PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY OF LONDON,

FROM THEIR COMMENCEMENT, IN 1665, *TO THE YEAR* 1800;

Abridged,

WITH NOTES AND BIOGRAPHIC ILLUSTRATIONS,

BY

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THE
PHILOSOPHICAL TRANSACTIONS
OF THE
ROYAL SOCIETY OF LONDON;
ABRIDGED.

XI. *Of the Influence of Cold on the Health of the Inhabitants of London.*

By Wm. Heberden, Jun., M. D., F. R. S. p. 279.

The extraordinary mildness of last Jan. (1796) compared with the unusual severity of the Jan. preceding, affords a peculiarly favourable opportunity of observing the effect of each of these seasons contrasted with each other. For of these 2 successive winters, one has been the coldest, and the other the warmest, of which any regular account has ever been kept in this country. Nor is this by any means an idle speculation, or matter of mere curiosity; for one of the first steps towards preserving the health of our fellow-creatures, is to point out the sources from which diseases are to be apprehended. And what may make the present inquiry more particularly useful, is that the result, as I hope clearly to make appear by the following statements, is entirely contrary to the prejudices usually entertained on this subject. During last Jan. nothing was more common than to hear expressions of the unseasonableness of the weather; and fears lest the want of the usual degree of cold, should be productive of putrid diseases, and I know not what other causes of mortality. On the other hand, "a bracing cold," and "a clear frost," are familiar in the mouth of every Englishman; and what he is taught to wish for, as among the greatest promoters of health and vigour.

Whatever deference be due to received opinions, it appears to me however from the strongest evidence, that the prejudices of the world are on this point at least unfounded. The average degrees of heat on Fahrenheit's thermometer kept in London during the month of Jan. 1795, was 23° in the morning, and 29°.4 in the afternoon. The average in Jan. 1796, was 43°.5 in the morning, and 50°.1 in the afternoon. A difference of above 20 degrees! And if we turn our attention from the comparative coldness of these months, to the corresponding healthiness of each, collected from the weekly bills of mortality, we shall find the result no less remarkable. For in 5 weeks between the 31st of Dec. 1794 and the 3d of Feb. 1795, the whole number of burials amounted to 2823; and in an equal period of 5 weeks between the 30th of Dec. 1795 and the 2d of Feb. 1796, to 1471. So that the excess of the mortality in Jan. 1795 above that of Jan. 1796, was not less than of 1352 persons. A number sufficient surely to awaken the attention of the most prejudiced admirers of a frosty winter. And though I have only stated the evidence of 2 years, the same conclusion may universally be drawn; as I have learned from a careful examination of the weekly bills of mortality for many years. These 2 seasons were chosen as being each of them very remarkable, and in immediate succession one to the other, and in every body's recollection.

It may not be impertinent to the objects of this Society, without entering too much into the province of medicine, to consider a little more particularly the several ways in which this effect may be supposed to be produced; and to point out some of the principal injuries which people are liable to sustain in their health from a severe frost. And one of the first things that must strike every mind engaged in this investigation, is its effect on old people. It is curious to observe among those who are said in the bills to die above 60 years of age, how regularly the tide of mortality follows the influence of this prevailing cause: so that a person used to such inquiries, may form no contemptible judgment of the severity of any of our winter months, merely by attending to this circumstance. Thus their number last Jan. was not much above $\frac{1}{5}$ of what it had been in the same month the year before. The article of asthma, as might be expected, is prodigiously increased, and perhaps includes no inconsiderable part of the mortality of the aged. After these come apoplexies and palsies, fevers, consumptions, and dropsies. Under the 2 last of which are contained a large proportion of the chronical diseases of this country; all which seem to be hurried on to a premature termination. The whole will most readily be seen at one view in the following table.

	Week ending.	Mean heat.		Whole N° of deaths.	Aged above 60.	Asthma.	Apoplexy and palsy.	Fever.	Consumption.	Dropsy.
		Morn.	Noon.							
1795.	6 Jan.	25°	29°	244	51	13	4	20	73	7
	13 Jan.	26	32	532	139	26	13	49	158	20
	20 Jan.	24	30	637	145	51	11	81	164	37
	27 Jan.	19	27	543	143	64	11	42	157	17
	3 Feb.	25	37	867	239	95	13	66	273	45
	Result	23	29 .4	2823	717	249	52	258	825	126
1796.	5 Jan.	40°	46	300	35	5	7	34	79	13
	12 Jan.	41	49	273	37	9	5	25	53	19
	19 Jan.	48	53	313	29	2	4	29	77	11
	26 Jan.	47	52	257	20	7	9	23	47	11
	3 Feb.	41	49	328	32	6	6	23	86	16
	Result	43 .5	50 .1	1471	153	29	31	134	342	70

Notwithstanding the plague, the remittent fever, the dysentery, and the scurvy, have so decreased, that their very name is almost unknown in London; yet there has, I know not how, arisen a prejudice concerning putrid diseases, which seems to have made people more and more apprehensive of them, as the danger has been getting less. It must in great measure be attributed to this, that the consumption of Peruvian bark in this country has, within the last 50 years, increased from 14,000 to above 100,000 lb. annually. And the same cause has probably contributed, from a mistaken mode of reasoning, to prepossess people with the idea of the wholesomeness of a hard frost. But it has in another place* been very ably demonstrated, that a long frost is eventually productive of the worst putrid fevers that are at this time known in London; and that heat does in fact, prove a real preventive against that disease. And though this may be said to be a very remote effect of the cold, it is not therefore the less real in its influence on the mortality of London. Accordingly a comparison of the numbers in the foregoing table will show that very nearly twice as many persons died of fevers in Jan. 1795, as did in the corresponding month of this year. I might go on to observe that the true scurvy was last year generated in the metropolis from the same causes extended to an unusual length. But these are by no means the only ways, nor indeed do they seem to be the principal ways, in which a frost operates to the destruction of great numbers of people. The poor, as they are worse protected from the weather, so are they of course the greatest sufferers by its inclemency. But every physician in London, and every apothecary, can add his testimony, that their business among all ranks of people never fails to increase, and to decrease, with the frost. For if there be any whose lungs are tender, any whose constitution has been impaired either by age, or by intemperance, or by disease; he will be very liable to have all his complaints increased, and all his infirmities aggravated by such a season. Nor must the young and active think themselves quite secure, or fancy their health will be confirmed by imprudently exposing themselves. The stoutest man may meet with impediments to his recovery from accidents otherwise inconsiderable; or may contract inflammations, or coughs, and lay the foundation of the severest ills. In a country where the prevailing complaints among all orders of people are colds, coughs, consumptions, and rheumatisms, no prudent man can surely suppose that unnecessary exposure to an inclement sky; that priding one-self on going without any additional clothing in the severest winter; that inuring oneself to be hardy, at a time that demands our cherishing the firmest constitution lest it suffer; that braving the winds, and challenging the rudest efforts of the season, can ever be generally useful to Englishmen. But if generally, and on the whole, it be inexpedient, then ought every one for himself to take care that he be not the sufferer. For many doctrines very importantly erroneous; many remedies either vain, or even noxious, are daily imposed on the world for want of attention to this great truth; that it is from general effects only, and those founded on extensive experience, that any maxim, to which each individual may with confidence defer, can possibly be established.

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* Observations on the Jail Fever, by Dr. Hunter, Med. Trans. vol. 3.—Orig.

XII. *An Analysis of the Carynthian Molybdate of Lead; with Experiments on the Molybdic Acid. To which are added some Experiments and Observations on the Decomposition of the Sulphate of Ammonia. By Charles Hatchett, Esq.* p. 285.

§ 1. The celebrated Scheele, in 1778, read before the Acad. of Sc. at Stockholm an essay, in which he proved, by a series of experiments, that the mineral called Molybdæna was composed of sulphur, and a peculiar metallic substance, which, like arsenic and tungsten, was liable by super-oxygenation to be converted into a metallic acid, which in its properties differed from every other that had been previously discovered *. The experiments of M. Pelletier †, Mr. Islmann ‡, and Mr. Hielm §, confirmed the discovery of the Swedish chemist; but the existence of this metallic substance was only known to be in that mineral which Scheele had examined, as no vestige of it had as yet been discovered in other bodies appertaining to the mineral kingdom. Mr. Jacquin, in 1781 and 1786, gave to the public an account, from the Abbé Wulfen, of a yellow sparry lead ore, found at Villach in Carinthia ||; and the Abbé Wulfen himself, in an elaborate work, written in the German language, and published in 1785, also described the above-mentioned lead ore, together with some experiments which had been made on it ¶. Nothing satisfactory however relative to its nature was exposed in these memoirs; and though the substance was indisputably proved to be an ore of lead, yet the mineralizing principle of it remained unknown.

In 1790, Mr. Heyer, of Brunswick, made some experiments on this ore; from which he inferred that it was composed of lead, combined with the tungstic acid **; and in the same year Mr. Klaproth communicated a similar account, which he had received from Mr. Salzwedel, of Frankfort sur Mayne ††. This substance was therefore universally believed to be a tungstate of lead, till that excellent chemist Mr. Klaproth undertook to subject it to a further examination; and as the experiments which I have made may be regarded as a continuation of those made by Mr. Klaproth, I think it necessary here to mention them.

Mr. Klaproth says, that by previous experiments he had found, that nitric acid much diluted did not attack the ore when cold; and therefore to separate it from the soluble matrix, he successively poured small quantities of the diluted acid on the ore till all effervescence had ceased, after which the ore was washed and dried. The nitric acid which had been employed was found to contain calcareous earth, and also a considerable quantity of red oxyde of iron, which on being dissolved in

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* Scheele's Essays, transl. by Dr. Beddoes, p. 227. † Journ. de Phys. Dec. 1785.

‡ Chem. Ann. von Crell, 1787, et Journ. de Phys. Oct. 1788. § Journ. de Phys. Mai, 1789.

|| Nic. Jos. Jacquin Misc. Austr. tom. 2, p. 139; et N. J. Jacquin Collect. tom. 1, p. 3. This ore is also described in Lithophyl. Bornianum, tom. 1, p. 90; et tom 2, p. 123.—Karsten in Mus. Lesk. tom. 2, p. 501.—Werner's Verziech. Band, 1, p. 128.—Raab's Catal. tome 2, p. 379.—Romé de l'Isle Cristal. tome 3, p. 387.—Widenmann's Handb. der Mineralogie, p. 864.

¶ Xavier Wulfens Abhand. vom Karnthnerischen Bleyspate. Wien, 1785.

** Chem. Ann. von Crell, 1790, p. 58. †† Ibid. 1790, p. 297.—Orig.

sulphuric acid, left a residuum of lead and siliceous earth *. Two drs. of the purified ore were mixed with an equal quantity of pot-ash, and afterwards exposed to the fire in a crucible. The mixture melted without intumescence. When cold, the mass was of a reddish colour, and the surface was covered with small scales. Water was poured on it, and the solution was saturated with nitric acid. The next day, the bottom of the glass was found covered with projecting crystals, about $\frac{1}{4}$ of an inch in length: these crystals were formed of small glittering rhomboidal plates, heaped one on the other. Their flavour was rather metallic. They quickly melted with the blow-pipe, on charcoal, without any increase of bulk, and became small round drops, which were immediately absorbed by the charcoal. When melted by the blow-pipe in a silver spoon, they appeared as small grains, of a greyish colour, which became streaked in cooling, and deposited a white powder during the operation.

When the phosphate of ammonia and soda was melted, and some of these crystals were added, they speedily dissolved, and communicated to the salt a green colour, more or less deep according to the quantity employed. They completely dissolved in distilled water, when heated. Prussiate of pot-ash with this solution produced a reddish-brown precipitate, not very dark. When some drops of muriatic acid were mixed with the solution of these crystals in water, and a small piece of tin was added, the liquor became of a deep blue. The solution of muriat of tin poured on the crystals produced the same effect.

Mr. Klaproth from these experiments concludes, that the crystals are the acidulous molybdate of pot-ash, especially as the crystals obtained from the filtrated solution of the molybdæna of Altenberg, detonated with nitre, and saturated with nitric acid, have the same properties. As in the above experiment the ore did not appear to have been completely decomposed, Mr. Klaproth mixed 2 drs. of the purified ore with 10 of carbonate of pot-ash, melted the whole in a crucible, and reduced it to powder, and dissolved it in water. The solution was filtrated, partially saturated with muriatic acid, and heated. A white precipitate fell, resembling curdled milk, which consisted of molybdic acid, and a still larger quantity of oxyde of lead. When dissolved in muriatic acid, the lead was precipitated in the state of muriate of lead. This precipitate being separated by a filter from the alkaline solution partially saturated with muriatic acid, the solution was then completely saturated with the same acid, and again became slightly turbid, and deposited a white precipitate, which resembled starch in cold water. This precipitate, after it had been washed and dried, was subjected to the same experiments as the above-mentioned crystals, and exhibited the same properties, excepting that it did not dissolve in distilled water till some drops of muriatic acid were added.

When the solution was evaporated in a glass basin, the rest of the oxyde of molybdæna was deposited in the form of a heavy citron-coloured powder. The white oxyde of lead on the filter through which the solution of the alkaline mass

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* Analyse Chimique du Plomb Spathique jaune de Carinthie An. de Chimie, 1791, p. 103.—Orig.

had passed, was found to be mixed with siliceous earth. When melted on charcoal, it did not entirely assume the metallic form, but a part changed into a small grain of transparent yellowish vitrified oxyde of lead. Mr. Klaproth observes, that in this experiment the presence of the siliceous earth prevented the complete reduction of the lead, in the same manner as when glass of lead, composed of 3 parts of oxyde of lead and 1 of siliceous earth, is melted on charcoal. He therefore dissolved this oxyde in weak nitric acid, separated the siliceous earth by a filter, and precipitated the lead by sulphuric acid. Mr. Klaproth however, doubts whether the greater part of the siliceous earth was not introduced, during the operation, by the action of the alkali on the crucible.

A drachm of the ore was digested with a considerable quantity of nitric acid, and a great part was dissolved. In the solution white flocculi were perceived, and were separated by a filter. When dried they were like a membrane, which became bluish when exposed to the light, and much resembled the molybdic acid obtained from molybdæna, by repeatedly distilling nitric acid from it. In the filtrated nitric solution there was much oxyde of molybdæna mixed with oxyde of lead. The lead was therefore precipitated by sulphuric acid, and afterwards the molybdæna by prussiate of pot-ash.

A drachm of the ore was digested with muriatic acid, and was completely dissolved, excepting a small residuum of siliceous earth. The solution was transparent, and without colour. In the course of some time it plentifully deposited crystals of muriated lead. When these crystals were separated, the solution was evaporated, and the interior of the basin was, during the evaporation, covered with a bluish saline crust, which dissolved and the colour disappeared when the vessel was shaken. The concentrated solution decanted from the muriate of lead, which had been precipitated during the evaporation, was of a deep blue, which disappeared when water was added. The solution was then saturated with alkali, and deposited a white precipitate, which consisted of molybdic acid, together with a small quantity of oxyde of lead.

According to these experiments Mr. Klaproth remarks, that the yellow sparry lead ore of Carinthia is composed of oxyde of lead and molybdic acid, and that this mineralogical novelty is the more remarkable, as it is the only one of the kind known at present. It is also worthy of notice, that the molybdæna changes its form according to the method employed to precipitate it from alkaline solutions; for it is obtained either in a crystalline form, or in that of a white powder, or in that of citron-coloured oxyde. When crystallized, it is soluble in acids, and in water; as a white powder it does not dissolve in water, unless a small quantity of muriatic acid be added; but in the state of the citron-coloured oxyde, it is insoluble in water as well as in the acids. Mr. Klaproth considers that this difference is occasioned by the presence of some alkali in the first 2 substances, so as to form an imperfect neutral salt with the molybdic acid, but that the yellow powder is in the state of a simple oxyde. This yellow colour probably occasioned the supposition that the lead was mineralized by the tungstic acid; but the blow-pipe is sufficient to distinguish them. The yellow oxyde of molybdæna loses the colour as soon as the point of the flame touches it, inclines to an olive colour, and melts into small grains, which are immediately absorbed by the charcoal. In the phosphate of ammonia and soda it dissolves, and communicates to it a green colour. On the contrary, the yellow oxyde of tungsten by ignition becomes blue or black, remaining refractory, and with phosphate of ammonia and soda it produces a sky-blue glass.

Mr. Klaproth concludes his paper by saying, that he could not exactly determine the proportions of the ingredients, as the quantity of the ore in his possession was not sufficient to make the necessary allowance for the solubility of the oxyde of lead in the alkalis, and especially that of the molybdic acid when in a state of combination. These experiments of Mr. Klaproth, certainly prove that this ore is a molybdate of lead; but as the quantity which he had was too small, either to make a greater number of experiments, or a regular analysis, I was induced to attempt a further investigation of it; and therefore in the course of the last summer I made the experiments and analysis which are described in this paper.

§ 2. *Characters of the Carinthian Molybdate of Lead.*—The molybdate of lead is found at Villach, in Carinthia *. The matrix is a lime-stone, of a pale brownish-grey colour, often more or less tinged with oxyde of iron. The ore is a heavy brittle substance, easily scratched with a knife, and of a yellow colour, varying from pale yellow to orange. The fracture is sparry.

The external lustre is like that of wax; and when crystallized, 2 of the faces of the crystals are commonly opaque, and of a pale yellow, but the remaining 4 faces or sides have a resinous appearance. It generally exhibits an appearance of crystallization, and the crystals, when perfect, afford various modifications between the octoëdral figure and the cube.

The specific gravity of a specimen, from which I had separated all the visible part of the matrix, was 5092, the temperature of the water being 60°, but when the ore was reduced to powder, and purified by diluted nitric acid, I found the specific gravity to be 5706. 1. When the ore was examined by the blow-pipe, it at first split and crackled as soon as the point of the flame touched it, but afterwards readily melted into a dark-coloured mass, in which were some shining globules of lead. 2. With borax it formed a brownish-yellow globule; but when it was in a small proportion, and heated by the interior flame, it occasionally produced a glass, which was greenish blue, and sometimes deep blue. 3. With phosphate of ammonia and soda it formed a sea-green glass, which in proportion to the quantity of the ore sometimes became deeper in colour, so as to be nearly of a deep blue.

Before making the following experiments, I reduced 8 oz. of the ore to a fine powder, and dissolved the matrix after the manner of Klaproth, by successively

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* It is said to have been sometimes found in Austria and Hungary, but I doubt if the nature of these ores is the same.—Orig.

pouring on the powder small quantities of nitric acid diluted with 6 parts of distilled water, after which I edulcorated and dried the residuum. The nitric acid used in this operation contained, as Klaproth has mentioned, calcareous earth, oxyde of iron, and oxyde of lead; but as prussiate of pot-ash produced a pale green precipitate, I suspected that some other metallic substance beside iron and lead was in the solution. I therefore added muriate of tin to a portion of it, which was immediately changed from a pale yellow to a pale blue, and showed that a small quantity of molybdic acid was present in the solution.

§ 3. *Molybdate of lead with water.*—I boiled 12 oz. of distilled water on 20 grs. of the purified ore in a glass matrass during 3 hours. The ore did not appear to be changed, nor did the water after it had passed the filter afford any trace of matter in solution. I believe therefore that the molybdate of lead is insoluble in water.

§ 4. As Mr. Klaproth had proved the action of the fixed alkalies on the molybdate of lead, in the dry way, I was desirous to know what effects they would produce in the humid way, and therefore made the following experiments.

Exper. 1. A. I boiled 4 oz. of strong lixivium of caustic pot-ash with 20 grs. of the purified ore, till there remained at the bottom of the matrass a dry mass, which was partly red, yellow, and green. I reduced this to powder, and poured distilled water on it, till the water came away without any taste. The alkaline solution was filtrated, and afterwards saturated with sulphuric acid. The liquor then became turbid, and deposited a small quantity of a white precipitate, which consisted of lead and some molybdic acid. This was separated by a filter, and prussiate of pot-ash being added to the clear liquor, precipitated a great quantity of molybdæna, in the state of a reddish-brown flocculent precipitate.

B. I took the residuum of the alkaline solution, which now was chiefly of a red colour, and appeared like minium, and poured nitric acid very largely diluted on it, till the whole was dissolved. I then precipitated the lead with sulphuric acid, and from the clear liquor which remained, I afterwards, by the means of prussiate of pot-ash, obtained a quantity of Prussian blue.

Exper. 2. A. 20 grs. of the purified ore were boiled with 4 oz. of a lixivium of carbonate of pot-ash. When all the water was evaporated, there remained a white saline mass, which was reduced to powder, and treated with distilled water as in the former experiment. A large quantity of a heavy white residuum remained on the filter. The clear solution was saturated as before with sulphuric acid, and a white precipitate, similar to that of the former experiment, was obtained. This was separated, and a copious precipitate of molybdæna was produced, on the addition of prussiate of pot-ash.

B. The white residuum was then edulcorated, and when diluted nitric acid was poured on it, it was dissolved with effervescence. From this solution I precipitated the lead by sulphuric acid, and afterwards the iron by prussiate of pot-ash. Ammonia, when digested on the ore, had not any effect. From these experiments it appears, that the molybdate of lead is decomposed by the fixed alkalies in the humid way, and that the component parts of the ore are lead and iron mineralized by the molybdic acid [*].

§ 5. *Molybdate of lead with sulphur.*—A mixture, composed of 50 grs. of the ore and 150 grs. of sulphur, was put into a small glass retort, to which a receiver was luted. The fire was then gradually raised till all the sulphur was driven over, and the bottom of the retort began to melt. The residuum was a black loose powder, which was greasy to the touch, and soiled the fingers like molybdæna. This black powder was digested in a strong heat with nitric acid, diluted with 3 parts of water. Nitrous fumes were discharged during the digestion, and the powder was dissolved, excepting a residuum of molybdic acid, which was of a greenish-yellow colour. The solution was diluted with an equal quantity of distilled water, and was filtrated. Sulphuric acid was then added till all the lead was precipitated; after which I obtained a brown precipitate by prussiate of pot-ash [†].

§ 6. *Molybdate of lead with carbonate of ammonia.*—A mixture, composed of 50 grs. of the ore and 220 grs. of dry carbonate of ammonia, was put into a glass retort, and was sublimed with a gentle heat. The molybdate of lead remained in the retort without having suffered any apparent alteration. The ammonia however had raised a small portion; for when it was dissolved in distilled water, and was saturated with an acid, prussiate of pot-ash produced a brown cloud.

§ 7. *Molybdate of lead sublimed with muriate of ammonia.*—*Exper.* 1. A mixture of 50 grs. of the molybdate of lead and 240 grs. of muriate of ammonia was sublimed. The sublimate was partly yellow, green, and blue; there was also some muriatic acid, and the residuum was a black powder [‡]. A. The sublimate was mixed with an equal weight of sulphur and distilled. The residuum of this was a black powder, resembling the mineral called molybdæna, and when distilled with nitric acid, afforded a citron-coloured oxyde.

B. A quantity of distilled water was boiled on the residuum of the first sublimation, by which a part was dissolved, and communicated a blue colour to the water. 1. Prussiate of pot-ash added to some of this blue liquor, produced a precipitate of Prussian blue. 2. Sulphuric acid added to another portion deepened the blue colour. 3. Lixivium of carbonate of soda precipitated some ochry matter. 4. And nitrate of silver was decomposed, and muriate of silver was precipitated.

C. Nitric acid diluted with 6 parts of water was then poured on the undissolved powder, and was digested on it in a sand-heat. The powder was nearly dissolved,

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[*] The alkalies, whether caustic or combined with carbonic acid, do not act in the humid way on molybdæna when mineralized by sulphur. Scheele's Essays, p. 230; and Mém. sur la Molybdène, par M. Pelletier, Journal de Phys. Dec. 1785, p. 437.—Orig.

[†] As the quantity of molybdic acid in the ore is much greater than that of iron, it is scarcely possible to discover the latter when they are precipitated together by prussiate of pot-ash.—Orig.

[‡] M. Sage has observed, that molybdæna with muriate of ammonia affords a blue sublimate. Journ. de Phys. 1788, p. 389.—Orig.

and the solution was colourless. 1. From this solution I precipitated sulphate of lead by the means of sulphuric acid. 2. With prussiate of pot-ash I obtained a brown precipitate of molybdæna; and 3. Muriate of tin turned another portion of it blue.

From these experiments it appears that the 1st solution contained iron, with some molybdic acid dissolved in muriatic acid; and the 2d solution contained molybdic acid and lead.

Molybdate of lead sublimed with muriate of ammonia.—Exper. 1. 125 grs. of the ore were mixed with 2 oz. of muriate of ammonia, and put into an earthen matrass, to which a head of stone-ware was fitted. The matrass was then exposed to a sufficient degree of heat, and when all was sublimed the vessels were separated. The black powder which remained was mixed with 2 oz. of muriate of ammonia, and again sublimed. This operation was repeated 3 times, after which nothing remained in the matrass. The sublimate, as before, was yellow, green, and blue. A. Distilled water was poured on the sublimate, so as to dissolve all of the saline part; but as the solution was turbid, it was poured on a filter, which collected a precipitate of a pale bluish-grey colour.

B. This precipitate after it had been edulcorated was boiled with distilled water, which was afterwards filtrated. 1. Prussiate of pot-ash precipitated some iron. 2. Muriatic acid was added to another portion, after which the prussiate produced a brown precipitate of molybdæna. 3. Muriate of silver was precipitated when nitrate of silver was dropped in.

C. I then boiled lixivium of carbonate of pot-ash on the undissolved part of the residuum, by which the greatest part was dissolved. The alkali was then saturated with muriatic acid, and prussiate of pot-ash being added, precipitated some molybdæna. On the small portion of the residuum which remained I poured diluted nitric acid. The solution was then filtrated, and I obtained a small quantity of sulphate of lead by the means of sulphuric acid. These experiments show that the residuum was composed of molybdic acid, iron, lead, and a small quantity of muriatic acid, which was produced from the muriate of ammonia during the sublimation.

D. I now took the solution A and divided it into 2 portions, to 1 of which I added 3 oz. of concentrated sulphuric acid, and evaporated the liquor to half the quantity. When cold it deposited a white saline matter, which for the greater part dissolved in water, leaving a small residuum which appeared to be muriate of lead. Pot-ash expelled some ammonia from a portion of the dry salt; and a precipitate was produced when muriate of barytes was added to the solution. This white saline matter was therefore a mixture of sulphate of ammonia with a small portion of muriate of lead.

The solution to which the sulphuric acid had been added was again evaporated to a considerable degree, and when cold it resembled a very thick mucilage of a pale yellow colour. It readily dissolved in water, and contained sulphuric acid in great excess. 1. Prussiate of pot-ash only changed the colour to pale green. 2. Carbonate of pot-ash expelled the ammonia, and a white precipitate like starch was formed, which was principally composed of molybdic acid and pot-ash.

E. To the 2d portion of the solution I added 3 oz. of concentrated nitric acid, and evaporated it nearly to dryness. A bright yellow matter was deposited, which I found to be molybdic acid combined with a portion of lead. There was also a small quantity of liquid, which I diluted with distilled water, and then precipitated some sulphate of lead by sulphuric acid. When this was separated I added prussiate of pot-ash, and obtained a quantity of Prussian blue.

§ 8. *Molybdate of lead with black flux.*—100 grs. of the ore were mixed with 4 times the weight of black flux. The mixture was then put into a crucible with a piece of charcoal over it; a cover was fitted to the crucible, and it was placed in a furnace in which a strong heat was kept up during an hour. When the crucible was cold and was broken, there did not appear any reguline button, but shining metallic particles were dispersed throughout the mass. It was then reduced to powder, and the largest particles were separated by washing, were dried on paper, and weighed 31 grs.; on examination they proved to be lead in the metallic state. Other particles were separated by a magnet, and the remainder consisted of a black powder.

A. Diluted nitric acid was poured on this black powder and dissolved it, excepting a small residuum, which consisted of siliceous earth with a little charcoal. 1. The solution was diluted with distilled water, and filtrated. 2. I then first separated a quantity of lead by sulphuric acid, and afterwards obtained some Prussian blue by prussiate of pot-ash.

B. The alkaline solution which had been formed when the melted mass was washed, was poured on a filter, and distilled water was added till it came away tasteless. The filtrated liquor was without colour: nitric acid was then added till the alkali was saturated. When about half of the requisite quantity of nitric acid was poured in, the liquor became pale blue, and as the quantity was increased it changed to green; and, lastly, when the nitric acid was added till it was in a small excess, the liquor was of a bright amber colour. 1. This solution, with prussiate of pot-ash, afforded a brown precipitate of molybdæna. 2. Muriate of tin changed the colour to a beautiful deep blue. 3. But sulphuric acid had no effect.

C. The amber-coloured solution was evaporated to dryness, and a saline mass of a bright citron-colour remained at the bottom of the vessel. As part of the yellow colouring matter appeared to be only mixed with the salt, I dissolved it in distilled water, and separated a quantity of a citron-coloured powder, which was the molybdic acid. The solution was twice again evaporated, and each time some molybdic acid was separated; but a part still remained intimately combined with the salt, so as with water always to produce the amber-coloured solution.

I now proceeded to examine the ore with the acids. As the results which I obtained when the ore was digested with nitric acid were the same as those mentioned by Mr. Klaproth, I shall not repeat them, but shall only observe, that it does not appear possible to decompose the ore completely by means of this acid.

§ 9. *Molybdate of lead with muriatic acid.*—240 grs. of the purified ore in fine powder were put into a glass matrass, with 3 oz. of pure muriatic acid. The matrass was then placed in a sand bath; in about an hour the whole was dissolved, excepting some muriate of lead, which I dissolved by pouring water on it. After this there only remained a very small residuum of siliceous earth.

A. The solutions were then added together, and formed a liquor which was transparent, and of a greenish-yellow colour. 1. Prussiate of pot-ash produced a copious precipitate of molybdæna, in the form of a reddish-brown flocculent matter. B. Lixivium of carbonate of pot-ash precipitated a yellowish-white matter, and turned the liquor to a deep blue. C. Carbonate of soda had the same effect. D. Solution of carbonate of ammonia produced a similar precipitate, and caused the liquor to become blue.

These precipitates were separately collected and washed on filters. When examined by the blow-pipe, all of them afforded a yellowish-green glass, with phosphate of ammonia and soda. These precipitates dissolved in diluted nitric acid with effervescence, and sulphuric acid precipitated sulphate of lead, after which Prussian blue was precipitated by prussiate of pot-ash, and the liquor became brown.

E. The blue solution, which consisted of the muriatic and molybdic acids combined with soda, was evaporated. When the liquor became hot, the colour changed from blue to pale yellow, and the evaporation was continued without any other perceptible alteration till the whole was become a dry concrete salt. I dissolved this salt in distilled water, and added muriatic acid, so as to be in a small excess. The liquor was then evaporated to half, and was set in a cool place. The following morning I found a quantity of crystallized muriate of soda at the bottom of the basin, covered with a white flocculent precipitate, which I collected and edulcorated on a filter. The rest of the liquor was repeatedly evaporated, till I had separated the greatest part of this white matter from the muriate of soda. The last portion of the liquor however still contained some molybdic acid, combined with the muriate of soda; for after it had been several times evaporated and again dissolved, it became blue when muriate of tin was added; or if muriatic acid was first poured in, prussiate of pot-ash produced a reddish-brown precipitate of molybdæna.

Experiments on the white precipitate.—1. It was not dissolved when water was boiled on it. 2. When digested with sulphuric or muriatic acid, the greatest part was dissolved, and prussiate of pot-ash produced a precipitate of a greenish-brown colour. 3. A small part became yellow when nitric acid was distilled from it. 4. The solutions of carbonate of pot-ash, soda, and ammonia, dissolved the greater part; and when these solutions were saturated with muriatic acid, prussiate of pot-ash produced precipitates like those of the acid solutions.

F. I next examined the blue solution, which consisted of the muriatic and molybdic acids combined with ammonia. It was first filtrated, and then gradually evaporated. When evaporated to half the original quantity, the colour was green, but towards the end of the operation it again became blue, and when evaporated to dryness, the residuum was a whitish salt, tinged in some parts with blue.

G. This salt was reduced to powder, and was put into a small glass retort, to which a receiver was fitted. I then placed the retort in a small open furnace, and gradually raised the fire till the bottom of it began to melt. The retort was now removed, and the contents examined. The receiver contained some water, and a small quantity of muriatic acid. Near the extremity of the beak of the retort was some muriate of ammonia, with some fuming muriatic acid, and the remainder of the tube was filled with a hard greyish-blue salt. In the retort was a black pulverulent residuum. I collected all of the blue salt, and again sublimed it, and again obtained muriatic acid, blue salt, and some of the black powder. The blue salt was composed of muriate of ammonia combined with the acid, or rather with a blue oxyde of molybdæna.

H. The black residuum was put into a glass retort, and some nitric acid being poured on it, it was exposed to a moderate heat. Nitrous fumes were discharged, and when the distillation had been repeated, I found the whole of this black powder converted into the citron-coloured molybdic acid. I had evident proof that in this experiment a portion of the muriate of ammonia was decomposed by each sublimation, and also that part of the molybdic acid was deprived of oxygen, and remained in the retort, if not in the state of metal, at least combined with so small a quantity of oxygen as to be nearly approaching to it.*

Molybdate of lead with muriatic acid.—Exper. 2. 1 dr. of the ore was digested with muriatic acid, and distilled water was added till the whole was dissolved, excepting a small residuum of siliceous earth. The solution was filtrated, and repeatedly evaporated, till muriate of lead was no longer separated. The muriate of lead, when edulcorated, I found to be perfectly free from any other substance.

I now saturated the acid solution, from which the muriate of lead had been separated, with solution of carbonate of ammonia, and obtained a pale yellow flocculent precipitate, which was well edulcorated. This precipitate immediately dissolved in very dilute nitric acid, and with sulphuric acid I precipitated a small portion of lead, after which, with prussiate of pot-ash, I separated a quantity of iron. The solution, when saturated with the ammonia, was deep blue, and was composed of muriatic acid, ammonia, and the blue oxyde of molybdæna, like that mentioned in the former experiment.

In the course of these experiments, I have observed that the full blue colour only takes place at the precise moment of saturation, and if the alkali is even added to a considerable excess, the colour does not suffer any further change; but if much water is first added, the blue colour does not appear; or if water is added after-

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* A black powder of a similar nature appears to have been obtained by Scheele, when he distilled the white molybdic acid with a small quantity of olive oil. Essays, p. 238.—Orig.

blue particles. The rest of the solution was then evaporated, and left a bright yellow mass at the bottom of the matrass.

The undissolved residuum of the nitric solution was then boiled with lixivium of pot-ash, which was afterwards saturated with muriatic acid, and became tinged with blue when prussiate of pot-ash was added. The residuum was now a black powder, which was edulcorated, and was immediately dissolved when nitric acid was poured on it; at the same time nitrous fumes were emitted. The solution was transparent, excepting that a few white particles were floating in it. It was then diluted, and filtrated. Prussiate of pot-ash turned it to a brownish green, which afterwards became brown. Lixivium of pot-ash precipitated a white flocculent matter; and caustic ammonia, added to a third portion, precipitated a quantity of iron.

As this precipitate had some remarkable properties, particularly in respect to the difficulty with which it was decomposed, I have been induced to mention the experiments made on it in a circumstantial manner. This precipitate appears from these experiments to be principally composed of iron, and some molybdic acid, together with a small portion of alkali and sulphuric acid. The intimate union between the iron and molybdic acid is apparently the cause which impedes the decomposition of this precipitate; but this can only be ascertained by future experiments on molybdate of iron.

I next examined the white precipitate which was deposited by the last evaporations, and found that, when distilled with nitric acid, it was converted into the yellow molybdic acid; and I was therefore convinced that this last portion of the white precipitate was the same neutral salt which I obtained in several other operations, and which has also been noticed by Scheele and Klaproth.[*] I now began to examine the blue solution B, which consisted of the sulphuric solution of molybdic acid, saturated with ammonia; but that the experiments made on this may be better understood, I shall first give an account of some experiments and observations which I have made on the sulphate of ammonia.

Experiments and observations on the sulphate of ammonia.—This neutral salt, which from Glauber, who first prepared it, was called the secret ammoniacal salt of Glauber, or vitriolic ammonia, has been very little examined; neither has it been applied to any useful purpose, though the inventor much recommended it in metallurgical operations. It has been long known that the fixed alkalies, lime, and barytes, when triturated with it, decompose it, by uniting with the acid. But the effects of heat on it do not appear to have been sufficiently observed. Macquer says, that it is demi-volatile, that it may be sublimed entire, and that it cannot be decomposed in close vessels without some intermediate substance.[†] Baumé makes use of nearly the same expressions.[‡] Bucquet says, that when it has lost the

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[*] Scheele observes, when molybdæna was detonated with nitre, and the mass afterwards dissolved in water, and saturated with sulphuric, nitric, or muriatic acid, that a white precipitate was produced, which was the acid of molybdæna combined with a portion of alkali. Essays, p. 231 and 240.—Orig.

[†] Dict. de Chimie, tom. 1, p. 111.—[‡] Chimie Exp. et Raisonnée, tom. 2, p. 83.—Orig.

water of crystallization, it becomes red-hot, and melts without being volatilized. Lastly, M. Fourcroy mentions it in the following manner: "As it contains much water of crystallization, it immediately liquifies by a very moderate heat; but by degrees it becomes dry, in proportion as the water of crystallization is dissipated. In this state it first becomes red-hot, and soon melts without being volatilized, according to Bucquet; but M. Baumé asserts that it is demi-volatile. In repeating this experiment I have observed, that in fact a part of this salt is sublimed, but a portion remains fixed in the vessel, and doubtless it is concerning this that Bucquet speaks*."

When so many eminent chemists concurred in nearly the same assertion, I was not a little surprized to observe, in some experiments made for a very different purpose, that the whole of the sulphate of ammonia was not only volatilized by heat, but also that the distillation of it was accompanied with some remarkable phenomena. I therefore diluted some very pure concentrated sulphuric acid with an equal quantity of distilled water, and having saturated it with ammonia, I gradually evaporated it till it became a concrete salt.

Exper. 1. I put 2 oz. of the salt into a glass retort, capable of containing 3 times the quantity, then fitted on a receiver without any luting, and placed the retort in a small open furnace over some lighted charcoal. The salt in the retort speedily liquified, and a small portion of water first came over; this was succeeded by a considerable quantity of alkaline gas, which continued to be produced during 15 or 20 minutes. On a sudden the vessels were filled with a thick white cloud, which on close inspection appeared to be composed of very minute glittering crystals. This cloud quickly disappeared, and was followed by a great quantity of sulphureous gas and water, the greatest part of which was condensed in the receiver; and the operation went on in this manner while any thing remained in the retort. During the distillation the matter in the retort was always liquid; and when the operation was finished, I found in the receiver a considerable quantity of sulphureous acid, with some ammonia in solution; and in the neck of the retort there was sublimed a portion of the undecomposed salt. From this experiment I was in a great measure convinced, not only that the neutral salt was decomposed, but that the ammonia was also in part resolved into its constituent principles.

Exper. 2. That I might however remove any doubt respecting this matter, I fitted a bent glass adopter to a retort, and to this added a double tubulated receiver, from which proceeded a bent tube, which passed under a glass jar filled with water and inverted. The former experiment was now repeated with this apparatus; and I had the satisfaction to observe, that when the sulphureous acid began to be produced, a quantity of gas at the same time displaced the water in the jar, and continued to pass into it till to the end of the operation. This gas I afterwards examined, and found that it possessed all the properties of the azotic gas†.

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* Elémens d'Hist. Nat. et de Chimie, tom. 2, p. 93.—Orig.

† This operation requires to be conducted with caution; for at the moment when the white cloud

I afterwards distilled 100 grs. of the yellow oxyde of iron, mixed with 200 grs. of sulphate of ammonia. Pure ammonia first came over, and afterwards some sulphureous acid. When the retort began to melt I removed it, and found the iron chiefly in the state of red oxyde, or colcothar, mixed with some sulphate of iron. When oxyde of zinc was used, the residuum was sulphate of zinc. Minium, when triturated with sulphate of ammonia, immediately decomposed it like lime, or the alkalies, and when distilled, the retort contained sulphate of lead. When native green oxyde of copper was distilled with sulphate of ammonia, the residuum was partly red oxyde of copper, with some sulphate of the same. But the ammonia came over in a concrete state, by reason of the carbonic acid contained in the green oxyde. The oxydes therefore of iron, zinc, lead, and copper decompose the sulphate of ammonia by combining with the acid.

I next mixed it with the yellow tungstic acid; but after the distillation, I found the tungstic acid unchanged, excepting that it had acquired a tinge of pale green. The ammonia and the sulphureous acid also came over in the same manner as when only the sulphate of ammonia was distilled. Lastly, I distilled 1 oz. of the sulphate of ammonia with 20 grs. of the yellow molybdic acid. During the distillation, the ammonia and sulphureous acid were produced in as great quantities as when the sulphate of ammonia was distilled by itself. But the molybdic acid remained in the retort, deprived of oxygen, in the form of a black blistered matter, which was again converted into the yellow acid when distilled with nitric acid.

From these experiments it appears, that the sulphate of ammonia is not, as many eminent chemists have imagined, incapable of being decomposed without some intermediate substance, but on the contrary, the whole of it can be raised, and a great part decomposed, whenever a proper degree of heat is applied; for then a certain portion of ammonia first comes over, so that the remainder is combined with acid in excess, and the hydrogen of the ammonia which remains unites with part of the oxygen of the sulphuric acid, and forms water, which passes into the receiver, accompanied by the acid, now become sulphureous acid, and by the azote in the state of gas. Various methods have long been in use to decompose ammonia. Metallic oxydes produce this effect; and Scheele particularly mentions, that if arseniate of ammonia is distilled, gas is produced, and the acid of arsenic is reduced to the metallic form, and as such is sublimed*.

appears, a vacuum takes place, occasioned by the alkaline gas, which previously filled the vessels, being neutralized by the sulphureous gas, which is then produced. It is necessary therefore, in about 10 or 15 minutes after the commencement of the operation, that the fire should be raised, and the azotic gas will then soon begin to pass into the jar. Some water will most commonly rush into the receiver, but if the capacity of this is not too small, there will not be time enough for the water to rise sufficiently high, so as to pass into the retort.—Orig.

[*] Essays, page 155. The same effects were also produced when acid of arsenic was sublimed with muriate of ammonia, p. 161.—Orig.

The decomposition of the nitrate of ammonia is also well known; and I have no doubt but that muriate of ammonia suffers a similar decomposition in a smaller degree each time that it is sublimed; for whenever I have had occasion to sublime muriate of ammonia, I have always found some fuming muriatic acid; and from whence could this be produced, but from a portion of the salt which was decomposed during the sublimation. The distillation of the triple salt, composed of molybdic acid, muriatic acid, and ammonia (§ 7 and 9,) places this in a stronger light; for whenever this salt was distilled, a certain portion of molybdæna was left in the retort deprived of oxygen, and muriatic acid was found in the receiver. Moreover, from several repetitions of this experiment, I am well convinced, that by a great number of sublimations the whole of the molybdæna might have been obtained in the proportion that the muriate of ammonia was decomposed.

When all these facts are considered, it appears more than probable that most, if not all, of the ammoniacal salts suffer different degrees of decomposition whenever they are treated in the dry way. As the molybdic acid was my principal object, I did not make all the experiments I could have wished on this neutral salt; neither have I as yet exactly determined the proportion of azotic gas produced from a certain quantity. I have found however, that the sublimed undecomposed part of the salt amounted to 183 grs. when an ounce of the salt had been distilled, and that the liquid in the receiver weighed 145 grs.; so that 152 grs. had escaped, which principally consisted of azotic gas, together with some sulphureous acid, and some alkaline gas, which had made their way out of the vessels during the operation.

Continuation of the experiments on the molybdate of lead.—From the effects which I observed to be produced when sulphate of ammonia was distilled with molybdic acid, I was induced to examine in a similar manner the blue solution B: but first I collected, washed, and dried the pale yellow precipitate which had been formed when the sulphuric solution of the molybdic acid was saturated with ammonia*. This precipitate, when dry, appeared of a deeper yellow, and easily dissolved in muriatic acid. Prussiate of pot-ash was then added to the clear solution, which precipitated the whole of the dissolved matter in the state of Prussian blue. The filtrated solution B was now evaporated till it became a dry concrete salt, the colour of which was pale greyish blue. I collected this salt, and having reduced it to powder, put it into a small glass retort, and having fitted on a receiver, I distilled it in the same manner as was employed with the sulphate of ammonia. The products which came over were also the same; and when the bottom of the retort began to be softened by the heat, I removed it, and found the residuum to be a

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Continuation of the experiments on the molybdate of lead.—From the effects which I observed to be produced when sulphate of ammonia was distilled with molybdic acid, I was induced to examine in a similar manner the blue solution B: but first I collected, washed, and dried the pale yellow precipitate which had been formed when the sulphuric solution of the molybdic acid was saturated with ammonia*. This precipitate, when dry, appeared of a deeper yellow, and easily dissolved in muriatic acid. Prussiate of pot-ash was then added to the clear solution, which precipitated the whole of the dissolved matter in the state of Prussian blue. The filtrated solution B was now evaporated till it became a dry concrete salt, the colour of which was pale greyish blue. I collected this salt, and having reduced it to powder, put it into a small glass retort, and having fitted on a receiver, I distilled it in the same manner as was employed with the sulphate of ammonia. The products which came over were also the same; and when the bottom of the retort began to be softened by the heat, I removed it, and found the residuum to be a

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black blistered matter. I then examined the sulphureous acid and sulphate of ammonia which had risen, but did not find any trace of molybdæna.

I next poured nitric acid diluted with an equal weight of distilled water on the black residuum in the retort, and distilled it. As soon as the acid began to be warm, nitrous fumes were discharged, and when the distillation had been repeated with a 2d portion of nitric acid, I found the whole of the black matter converted into a pale citron-coloured substance, which was the molybdic acid.

§ 11. *Analysis of the molybdate of lead.*—I put 250 grs. of the purified ore reduced to a fine powder into a glass matrass, and having poured on it 1 oz. of concentrated sulphuric acid, I digested it in a strong heat during an hour. When the solution was become cool and had settled, the acid was cautiously decanted from the powder, and distilled water was poured on till it came away tasteless. The same operation was repeated twice, so that 3 oz. of sulphuric acid were used. The acid solutions and washings were then filtrated, and were received in a large glass vessel.

I diluted the pale blue liquor with distilled water, in the proportion of 16 to 1, and afterwards gradually added ammonia till it was completely saturated. The liquor then became deep blue, and appeared turbid. When it had stood about 24 hours, a loose pale ochry precipitate subsided, and was collected on a filter, the weight of which had been noted*. This precipitate was edulcorated, and afterwards dried with the filter on the flat top of a tin vessel heated by boiling water, after which the weight of the precipitate was 4.2 grs. The colour of the dry precipitate was yellowish brown, and when dissolved in muriatic acid it was precipitated by prussiate of pot-ash in the state of Prussian blue.

I now poured part of the clear blue solution, which was composed of sulphuric and molybdic acid saturated with ammonia, into a glass retort, and when about half was evaporated, I continued to add the remainder of the liquor at different times till the whole was become a concrete salt. I then raised the fire and continued the distillation till all the sulphate of ammonia was decomposed or driven over: but as some of the sublimed salt was fixed in the neck of the retort, I turned the bottom of it upwards, and poured some distilled water into the neck, so as to wash out the salt; after this I increased the fire till the whole body of the retort was become red-hot†. The residuum in the retort was a black blistered mass, on which I poured 3 oz. of nitric acid diluted with an equal portion of water, and having distilled it, I repeated the operation, and thus converted the whole of the black

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* This is one of the many instances which prove the weak affinity between molybdæna and oxygen; for it is well known that pure ammonia precipitates iron from sulphuric acid, in a state nearly similar to martial æthiops; but in the present case the iron takes a considerable portion of oxygen from the molybdic acid at the moment that the acid menstruum is saturated by the ammonia, and it is therefore precipitated in the form of a yellowish-brown oxyde, while the molybdic acid being thus deprived of so large a quantity of oxygen, is converted into a blue oxyde which remains in solution.—Orig.

† To be certain that all of the ammoniacal salt is decomposed, it is absolutely necessary that the retort should be made red-hot.—Orig.

matter into the yellowish acid of molybdæna. When the retort was sufficiently cooled, I cut off the neck, and removed the powder, which weighed 95 grs.

I next proceeded to decompose the residuum left by the acid solution in the state of sulphate of lead; and having edulcorated it, I boiled it during an hour with 4 oz. of lixivium of carbonate of soda, then washed the powder, and poured on it nitric acid much diluted. The whole was dissolved, excepting a small portion of white powder, which was washed and dried on a filter by the heat of boiling water, and then weighed $\frac{7}{10}$ of a grain. This on examination, proved to be siliceous earth. I then exactly saturated the nitric solution with lixivium of pure or caustic soda, and having washed and dried the precipitate of lead, I exposed it in a porcelain crucible for a $\frac{1}{4}$ of an hour to a heat rather below red; after which it weighed 146 grs.

As I had found by a former experiment that a small portion of iron remained with the lead, I dissolved the 146 grs. in diluted nitric acid, and precipitated the lead by sulphuric acid. The solution was then filtrated, and being saturated with pure ammonia, I obtained a small quantity of oxyde of iron, which, when dried as before, weighed 1 gr.

By this analysis, 250 grs. of the ore yielded as annexed, and lost 3.1, which I am inclined to place principally to the account of the lead, as it is scarcely possible to decompose the sulphate of lead without some loss, occasioned by the action of the alkaline solution.

		Grs.	
Oxyde of lead		146	
Molybdic acid		95	
	Grs.		
Oxyde of iron	4 2 1	 5	2
and siliceous earth........		0	7
	grs.	246	9
The loss was therefore		3	1
		250	0

§ 12. *Experiments on the yellow molybdic acid, obtained by the analysis.*—A. When exposed to the blow-pipe on charcoal, it was melted by the exterior flame, and the sides of the charcoal were covered with small long crystals, which had a metallic lustre resembling silver*.

When the heat was continued, the whole was melted, and for the greater part absorbed by the charcoal, the edges of which became covered with a blue powder. When melted in a spoon of platina, some yellow powder was deposited near the edges, and a brownish yellow shining matter was formed, which became streaked in cooling. With borax it produced a brownish-yellow glass, but when the quantity of molybdic acid was small, the colour was sometimes blue when the globule was heated by the interior flame. With soda in the platina spoon it formed a brownish opaque matter. And with phosphate of ammonia and soda it formed a glass, which, in proportion to the quantity of molybdic acid, varied from a greenish blue to a deep blue.

B. 10 grs. of the yellow molybdic acid were boiled with 6 oz. of distilled water. About 3 grs. were dissolved, and the solution when filtrated was of a pale yellow

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* Scheele mentions a similar product obtained when molybdæna was exposed to the blow-pipe. Essays, p. 230.—Also by sublimation. Essays, p. 238.—And Mém. sur la Molybdène, par M. Pelletier. Journ. de Phys., Dec. 1785, p. 439.—Orig.

colour. It had scarcely any perceptible flavour, but turned litmus paper red. When prussiate of pot-ash was added to a portion of the solution, no apparent change was effected; I therefore added a small quantity of nitric acid, which produced a copious brown precipitate of molybdæna. The sulphuric and muriatic acids had the same effect, when poured into the solution, either before or after the addition of prussiate of pot-ash. With muriate of tin it changed to a beautiful deep blue.

Lead was precipitated from solution of nitrate of lead, in the form of a pale yellow precipitate, which was a regenerated molybdate of lead. Nitrate of barytes rendered the solution slightly turbid, but I did not find that the precipitate which subsided was soluble in cold water, as Scheele has mentioned*. The solution did not precipitate lime from nitric acid.

C. 10 grs. of the yellow molybdic acid were dissolved when digested with 1 oz. of concentrated sulphuric acid. The solution as it cooled became blue.† Prussiate of pot-ash produced a reddish-brown precipitate. Muriate of tin had no effect. When a portion of the solution was distilled to dryness, the yellow molybdic acid was left in its original state.‡ The remainder of the solution was saturated with lixivium of soda, by which the blue colour was heightened, and some white flocculent matter was precipitated. Prussiate of pot-ash added to part of this saturated solution did not precipitate the molybdæna, till the alkali was again supersaturated with an acid. Muriate of tin poured into the solution saturated with alkali, changed it to a deep blue; but when the alkali was again saturated with an acid the muriate of tin ceased to have any effect. The white flocculent matter which was precipitated when the solution was saturated with soda, was edulcorated and heated with nitric acid, by which it was converted into a yellow powder, similar to the molybdic acid which had been dissolved.

D. 10 grs. of the yellow molybdic acid, when digested in a strong heat with 1 oz. of concentrated muriatic acid, formed a pale yellowish-green solution. Prussiate of pot-ash precipitated the molybdæna. Muriate of tin had not any effect. A portion of the solution being distilled to dryness, left a greyish-blue residuum.§ I then saturated the remaining part of the solution with lixivium of pot-ash, by which the blue colour became more apparent and a much larger quantity of white flocculent matter was precipitated than when soda was employed. Prussiate of pot-ash did not affect this solution, till the alkali was again saturated with an acid. Muriate of tin was precipitated by the solution saturated with alkali, highly coloured with blue; but when the alkali was again saturated with an acid, the muriate of tin had no

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* Essays, p. 234.—Scheele does not mention the quantity of water which he employed.—Orig.

† Scheele observes that sulphuric acid dissolves a considerable quantity of molybdic acid, and that the solution as it cools becomes blue and thick; but when heated, the colour disappears, and returns again as the liquor cools. Essays, p. 235.—Orig.

‡ M. Pelletier says, that a small portion of molybdæna is raised by sulphuric acid when distilled with it; but I did not find it so with the molybdic acid.—Mém. sur la Molybdène, Journ. de Phys. Dec. 1785.—Orig.

§ Scheele has made the same observation. Essays, p. 235.—Orig.

effect.* Lastly, the white flocculent matter was boiled with nitric acid, and became like the molybdic acid before it was dissolved.

E. Nitric acid did not appear to have any effect on the molybdic acid when digested with it.

F. 2 oz. of lixivium of carbonate of pot-ash were poured on 10 grs. of the molybdic acid. In a few minutes carbonic acid was gradually expelled, and as the molybdic acid dissolved, a white flocculent matter was deposited. After it had stood some hours, the clear liquor was decanted from the residuum. Prussiate of pot-ash did not affect this solution. Some nitric acid was then dropped in, and produced a reddish-brown precipitate, which was re-dissolved till the acid was in some excess.

Muriate of tin, when added to a portion of the alkaline solution, was precipitated white, but when some muriatic acid was dropped in, the precipitate became blue. The white flocculent residuum, when treated with nitric acid as in the former experiments, was converted into the yellow molybdic acid. Another portion of the alkaline solution was evaporated to $\frac{1}{4}$, and in proportion as the evaporation advanced, some of the white flocculent matter was precipitated, but I did not obtain any crystals.

G. 2 oz. of lixivium of carbonate of soda were poured on 10 grs. of molybdic acid. In a few minutes carbonic acid was expelled, and when the molybdic acid was dissolved, a small quantity of white flocculent matter was precipitated. The clear solution was then poured from the residuum. Prussiate of pot-ash did not produce any precipitate till the alkali was saturated with an acid. The effects of muriate of tin were also the same as those mentioned in the former experiment. A part of the solution was evaporated to half, and the next morning I found crystals, which, though not very distinct, appeared to be in the form of four-sided tables with the angles truncated. These crystals dissolved in water without leaving any residuum, and when the solution was saturated with muriatic acid, the molybdic was precipitated by prussiate of pot-ash, as in the former experiments.

H. 2 oz. of carbonate of ammonia in solution were poured on 10 grs. of molybdic acid, which appeared to be more readily dissolved than when the fixed alkalies were employed. The solution appeared slightly turbid, but very little of it was precipitated. The effects produced by prussiate of pot-ash and muriate of tin were the same as in the preceding experiments. When a portion of the solution was distilled to dryness, part of the molybdic acid remained unchanged, but another part was deprived of oxygen, and appeared in the form of a dark grey powder. The remaining part of the solution was considerably evaporated; and the following day I found a

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* From the effects produced by muriate of tin on the molybdic acid dissolved in water, in acids, and in alkalies, it appears that it always tends to deprive the molybdic acid of a great part of its oxygen; and when water is the menstruum, the muriate of tin does this effectually; but when the molybdic acid is dissolved in sulphuric or muriatic acid, the muriate of tin has no effect, because, as I conceive, the oxygen is supplied by the acid menstruum. This seems the more evident from the effects produced by the muriate of tin when the excess of acid is saturated by an alkali.—Orig.

striated yellowish mass, which dissolved in water without leaving any residuum. This solution resembled the former in every respect.

§ 13. *Molybdic acid with sulphur.*—To remove every doubt concerning the nature of the yellow acid obtained by the analysis, I made the following experiment. I put 20 grs. of the yellow acid and 100 grs. of sulphur into a small glass retort, and continued the distillation till the bottom began to melt. The residuum was a black substance, which was greasy to the touch, stained the fingers black, communicated to them a shining metallic lustre, and had all the other properties of the mineral known by the name of Molybdæna. I afterwards distilled this black matter with nitric acid, which converted it into a yellow powder, similar in appearance and properties to the molybdic acid which had been originally employed.

§ 14. *General Observations.*—It has been proved in the course of this paper, that molybdate of lead can be decomposed in the humid way by the fixed alkalies, though these have no effect when boiled with molybdæna mineralized by sulphur.* The state of the molybdæna in the 2 substances appears to be the cause of this difference, for in the former it is oxygenated, but in the latter I conceive it to be in the state of metal. From the experiments of Scheele it also appears, that of all the known acids only 2 have any effect on the sulphurated molybdæna, and that these 2 are the nitric acid, and that of arsenic. The latter however seems rather to act on the sulphur than on the molybdæna; but the former communicates oxygen to both, so to convert the one into sulphuric and the other into molybdic acid.

The rapidity with which nitric acid oxygenates molybdæna, even to supersaturation, resembles the effects produced by the same acid on some other metallic substances, particularly tin; for in both cases the acid ceases to act as soon as the supersaturation with oxygen is effected; and on this account the nitric acid is unable to dissolve the molybdic acid. Before proceeding I must observe that whenever a solution of the molybdic acid becomes blue, or tending towards that colour, it is a certain sign that the molybdic acid has suffered a diminution of oxygen. A variety of facts which prove this, have been already brought forward in the different experiments contained in this paper; and I shall soon have occasion to mention others. Sulphuric acid can disolve a considerable quantity of molybdic acid; and the solution is always more or less of a blue colour according to the quantity dissolved; and the blue colour proves that the molybdic acid has parted with a portion of oxygen; but if the solution be heated, the blue colour disappears, and returns again when the liquor becomes cold.†

The cause of this I believe to be a change produced by heat in the respective degrees of affinity which prevail between the metallic base and oxygen, and between the base of the acid menstruum and oxygen; so that when the solution is heated, the affinity between the blue oxyde of molybdæna and oxygen is increased, and a

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* Scheele's Essays, p. 230; and Mém. sur la Molybdène, par M. Pelletier. Journ. de Phys. 1785, p. 437.—Orig.

† When lead or any other metal is present, the blue colour is permanent.—Orig.

portion of oxygen therefore quits the acid menstruum, and combines with the blue oxyde, which then becomes molybdic acid; but as soon as the heat is dissipated, the cause of this augmentation of affinity ceases, and the acid menstruum receives again the portion of oxygen from the molybdic acid, which then returns to the state of a blue oxyde; or if the heat is continued till the solution is distilled to dryness, the residuum is the molybdic acid exactly in the same state as it was before the solution was made; for the continuation of the heat enables it to retain the portion of oxygen requisite to constitute a metallic acid. I do not therefore believe that the total quantity of oxygen in the solution suffers alteration any further than that it is distributed in different proportions between the 2 acidifiable bases, sulphur and molybdæna, according to the temperature of the solution.

As the affinity between azote and oxygen is comparatively weak, the metal molybdæna effects a decomposition of the nitric acid, and acquires a sufficiency of oxygen to become molybdic acid. But as the affinity between sulphur and oxygen is greater than that of azote, and also under certain circumstances superior to molybdæna, the latter requires the assistance of heat to be able to retain a full portion of oxygen, and this increase of affinity lasts no longer than during the continuation of the heat. To corroborate this assertion, it will be proper to consider the effects of muriatic acid on that of molybdæna, especially as the affinity between the radical principle, or base of the muriatic acid, and oxygen, is known to be so great, that no chemist has as yet been able to effect a separation of the constituent principles.

It has been mentioned, that molybdic acid when dissolved in muriatic acid, also parts with some oxygen, and tinges the menstruum with a green colour. But heat does not enable it to take back the oxygen, for it augments the effects of the muriatic acid, which, when distilled, passes oxygenated into the receiver, and the molybdic acid is converted into a bluish grey oxyde.* These effects clearly prove, that heat in this case acts inversely to what it did when the nitric and sulphuric acids were the menstrua. For then the increase of affinity was between molybdæna and oxygen, but here it is in favour of the base of muriatic acid; so that by the continuation of heat, the muriatic acid carries with it into the receiver a certain portion of oxygen, which it has taken from the molybdic acid, and the latter is left in the state of an oxyde.

From this it appears that muriatic acid uniformly tends to deprive the molybdic acid of a certain quantity of oxygen, and that heat produces a contrary effect on this solution to that which it did on the one made with sulphuric acid; and heat and cold do not therefore produce a change of colour. I do not however believe that muriatic acid acts thus constantly on all those metals which can be dissolved by it; on the contrary, there is a muriatic solution much resembling the sulphuric solution of molybdic acid in the vicissitudes of colour which it exhibits by heat and cold.

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* Elemens d'Hist. Nat. et de Chimie, par M. de Fourcroy, tome 2, p. 439.—Orig.

The phenomena which heat produces on the solution of cobalt in muriatic or nitro-muriatic acid, called sympathetic ink, have long engaged the attention of chemists and others, but as yet great difficulties have occurred whenever an explanation has been attempted. There can be no foundation for the idea which some have had, that the green colour, which characters traced with this solution on paper assume when heated, is caused by a temporary crystallization of the salt, and the disappearance of the colour by a subsequent degree of deliquescence; because any quantity of the liquid becomes green when heated.

The effects caused by heat on the sulphuric solution of molybdic acid have therefore induced me to suspect a similar cause in the muriatic solution of cobalt; and I believe that heat and cold in like manner causes a temporary difference to take place in the proportions of oxygen existing in the acid menstruum and the oxyde; and this is the more confirmed when the acid is expelled by too great a degree of heat, for then the changes of colour are no longer to be observed. Heat, it is well known, assists the combination of oxygen with the metals, but I do not believe that the above-mentioned alternate effects of heat and cold have been as yet investigated. It is probable that these are not confined to the two instances which have been adduced, though in other solutions they may not be so apparent. The subject is certainly curious, and worthy of the attention of chemists, as it would reflect much light on the solutions of metals in general.

When the sulphuric or muriatic solutions of the molybdic acid are saturated with pot-ash or soda, they assume a very deep blue colour at the moment of saturation. The molybdæna is not however precipitated in the form of the blue oxyde, but for the greater part remains combined with the acid menstruum and the alkali, and thus forms a triple salt in solution, which differs considerably from another triple salt, which is slowly precipitated at the time of saturation in the form of a white flocculent matter, and is composed of the same 3 ingredients, but contains the oxyde in the largest proportion. Sometimes a 4th ingredient becomes added to the last mentioned white precipitate; for when iron is present in the sulphuric or muriatic solutions, it is precipitated by pot-ash or soda intimately combined with the other ingredients, and appears to render the decomposition of the precipitate very difficult. Though the triple salt which is in solution will pass many folds of paper without leaving any residuum, yet it is not permanent; for by repeated evaporations, the neutral salt resulting from the combination of the acid menstruum and the alkali becomes crystallized, and a white flocculent matter is separated, which does not contain iron like that which was precipitated when the acid solution was saturated with the alkali, but can be converted into the yellow molybdic acid by being distilled with nitric acid, which takes from it the small portion of the acid menstruum and the alkali required to constitute the triple salt.

It has already been observed, that nitric acid has no effect when immediately digested on molybdic acid, but I have found it otherwise when a 3d substance was present; and the effects were nearly the same whether this substance was a metal or an alkali. The portion of molybdic acid which I detected in the nitric acid employed to purify the ore, and Mr. Klaproth's experiments made with the same acid, prove the first, and the experiment mentioned in § 8 is a proof of the latter. The phenomena which appeared in the last experiment throw some light on the effects produced by nitric acid on molybdæna; for when the sulphuric and muriatic solutions of the molybdic acid were saturated with pot-ash or soda, they gradually changed to yellowish green, and so on to blue, in proportion as the alkali was added; but when nitric acid was added to the alkaline solution, the change of colour was exactly the reverse of the former, for the changes were then blue, green, and yellow, in proportion to the quantity of nitric acid.

The cause of these effects I conceive to be the different degrees of oxygenation of the molybdæna; for when the first portion of nitric acid was added, it rather combined with the alkali than with the molybdic acid, and the latter was therefore in some degree separated with a diminution of the original quantity of oxygen, and consequently appeared as the blue oxyde in solution. After this, the 2d portion of nitric acid began to oxygenate the blue oxyde, and therefore changed the colour of the solution to green; but the 3d addition of nitric acid acted immediately on the oxyde, turned the solution yellow, and when assisted by heat, caused a quantity of the yellow molybdic acid to be precipitated. The alkali however appears to have impeded the complete separation of the molybdic acid, and retained a part of it together with the nitric acid, so as to form a yellow triple salt. When the sulphuric and muriatic solutions of the molybdic acid are saturated with ammonia, triple salts are formed, which are different in their properties from those which have been described; for the triple salts produced by ammonia are permanent, and do not appear to be decomposed by evaporation.

When iron is present in the acid solution, sufficiently diluted, it is precipitated by ammonia free from molybdic acid, especially when the menstruum is the sulphuric acid. The affinity of the molybdic acid with muriate of ammonia is so great, that by sublimation it even in part quits lead to unite with it, and then forms the blue triple salt, from which the blue oxyde does not separate, but in proportion as it is deprived of oxygen by the gradual decomposition of the ammonia caused by repeated sublimations.

When the sulphuric solution saturated with ammonia is evaporated to a proper degree, the triple salt crystallizes in the usual figure of the sulphate of ammonia, but the colour is bluish green. When however the evaporation is continued to dryness, a pale greyish-blue salt is left, and by distillation this salt is decomposed, after the manner of the decomposition noticed in the sulphate of ammonia, and the molybdæna remains in the form of a black powder, deprived of oxygen.

I think it necessary here to observe, that when molybdæna is not in the metallic state, it appears to suffer 4 degrees of oxygenation. The first is the black oxyde; the 2d is the blue oxyde; the 3d is the green oxyde which, as it seems to be intermediate between an oxyde and an acid, I am inclined to call molybdous acid, according to the distinction made by the new Nomenclature; the last and 4th degree is the yellow acid, or that which is supersaturated with oxygen.

The affinity between molybdæna and oxygen is but weak, at least in respect to that portion which is required to constitute molybdic acid; for it has been proved, in the course of these experiments, that considerable changes are produced by a very small difference in the proportions of the acids or alkalies, and even by the degrees of heat. Scheele and Mr. Islmann have observed, that all of the metals, excepting gold and platina, are able in the humid way to deprive the molybdic acid of oxygen, so as to cause it to become blue; but here their effects appear to cease. M. Pelletier found that a solution of molybdic acid was turned blue when hydrogenous gas was passed through it. Mr. Klaproth has also remarked that light, under certain circumstances, changed the colour of molybdic acid to blue. And the effects of light appear in some measure to be connected with the following experiment.

I made a solution of the molybdic acid, by digesting sulphuric acid on molybdate of lead, and diluted it with an equal quantity of water. The solution was filtrated, and I then added a solution of hepar sulphuris till the brownish-red precipitate which was produced began to fall much paler. After this the liquor was filtrated, and was of a pale beer colour. I placed it accidentally in an open glass jar, on a shelf near a window, on which the sun shone during a great part of the day, and was surprized to observe that in about 2 days it began to assume a greenish tinge, which gradually became deeper; on the 3d day it was of a full green, on the 4th it had a tinge of blue, on the 5th the colour was greenish-blue, and on the 6th day it was changed to a beautiful deep blue. The solution continued all the time to be transparent; and though the vessel remained 4 weeks in the same situation, the blue colour suffered no further change. This solution much resembles that which Scheele discovered in the course of his experiments on manganese; but the apparent similar effects, I believe, are produced by opposite causes; for the changes of colour in the alkaline solution of manganese appear to be effected by the absorption of oxygen, but those of the molybdic solution are caused by a diminution of the inherent quantity.

The contrast of the causes which operate on the 2 solutions becomes the more evident when the effects which acids produce on them are considered; for when a few drops of an acid are added to the solution of manganese, the changes of colour are accelerated, not merely by the neutralization of the alkali and consequent precipitation of the manganese, but, as I conceive, by the accession of oxygen either immediately from the acid, or from the atmosphere, which the manganese is better able to absorb when the disengagement of it from the alkali is thus assisted by the addition of the acid. On the contrary, when nitric acid is dropped into the molybdic solution, the colour is immediately destroyed, in the same manner as in all the other blue solutions of molybdæna when oxygen is thus presented to them. The facility with which molybdæna parts with oxygen is evinced not merely in the humid way, for M. Pelletier found that molybdic acid yielded oxygen to arsenic when distilled with it, and that the latter was converted into a white oxyde. The same is also proved by my experiments on various oxydes distilled with sulphate of ammonia; for the molybdic acid was the only one which could thus be deprived of oxygen, not excepting the tungstic acid, which has been supposed much to resemble that of molybdæna.

XIII. Observations of the Diurnal Variation of the Magnetic Needle at Fort Marlborough, Sumatra. By John Macdonald, Esq. p. 340.

These observations were carefully made in a wooden building, having nothing of iron near it. They were taken 3 times mostly every day, at the hours of 7 or 8 in the morning, and at noon, and 4 or 5 afternoon; from June 27, 1794, till the middle of March 1795. The thermometer was commonly at about 80°.

It appears from these observations, that the diurnal east variation of the variation, increased from about 7 in the morning till 5 in the afternoon, and that it thence decreased till 7 in the morning. The whole variation was, on a medium, about 1° 8′ east; and the change in the variation, between morning and evening, 2 or 3′. It appears in general, that such diurnal variation of the variation as had been observed during thunder, is greater than it ought to have been, cæteris paribus. It has been remarked, that heat weakens the magnetic virtue, and that cold strengthens it. Supposing, with Dr. Halley, the existence of 4 magnetic poles, by blending this supposition with the above principle well ascertained, attempts have been made to account for the diurnal variation of the variation. The south-east magnetic pole being less heated in the morning, either by the sun or by subterraneous fire, than towards noon, and in the afternoon, and being at the same time, by passing through the meridian of Celebes, nearer Sumatra than the south-west magnetic pole, it draws to it in the morning the south end of the magnetic needle more powerfully than the other attracts; and consequently the diurnal variation of the variation ought to be, and actually is, less in the morning than in the afternoon. In the progress of the day, the south-east magnetic pole having become heated, and the south-west pole being at the same time less heated, attracts the south end of the magnetic needle more powerfully than the other does; and hence the east diurnal variation of the variation appears greater in the afternoon than in the morning. It is found in Europe, that this diurnal variation of the variation is greater in summer than in winter. This seems to point out heat acting on magnets in the earth, as its efficient cause. This was first observed in Europe in 1756, by Mr. Canton; and the results of the foregoing observations being diametrically opposite to his, with similar effects, afford not a small confirmation of the essential part of Halley's theory.

From the greatness of the angle of dip of the needle, we are led to suppose that the magnetic poles are fixed within the magnetic nucleus far within the earth's surface, and that some of these poles are more powerful in their action than others, from the variation observed in various places of the globe.

XIV. Discovery of some very Singular Balls of Stone, found in the Works of the Huddersfield Canal. By Mr. Benj. Outram, Engineer. p. 350.

The Huddersfield canal is to be carried through that chain of mountains which extends from the Peak of Derbyshire, in a northward direction through Yorkshire, &c. into and through a great part of Scotland, by pursuing from the navigation at Huddersfield a deep and narrow valley to Marsden, where it enters the north-eastern foot of one of these mountains, called Pule Hill, under which it is to be extended south-westwardly by a subterraneous cut or tunnel to the foot of Stand Edge Hill, or Brunn Clough, where it again excavates; and pursuing the bottom of a deep valley into Saddleworth, passes along the banks of the Tame to Ashton-under-Lyne, where it joins the canals that extend to Manchester, Stockport, Peak Forest, &c.

In the latter end of the year 1794, the miners employed by the canal company began to perforate the north-eastern foot at Pule Hill: the strata they first cut through consisted of a greyish coloured shale, the beds or laminæ of which did not lie quite horizontal, but dipped or declined a little to the westward. The strata continued regular till the workmen had perforated about 240 yards in length from the entrance of the hill, and were about 80 yards deep from the surface of the ground immediately over them, when they discovered on the north side of their work a fault, throw, or break of the strata, which was filled with shale, reared on the edge, and mixed with softer earth, and in some places with small lumps of coal. In continuing to pursue the direction of the tunnel, this fault occupied by degrees more of the space of the tunnel, for about 40 yards in length, when it nearly occupied the whole tunnel, which is near 4 yards in width: and at about 5 feet from its southern margin it contained a rib of lime-stone, near 4 feet thick in the bottom, but not quite so thick at the top of the tunnel; and on each side of this rib it contained balls of lime-stone promiscuously scattered, and of various sizes, from 1 ounce to upwards of 100 lbs. weight.

The rib and balls of lime-stone were first found at about 280 yards from the north-eastwardly end of the tunnel, where it is about 90 yards in perpendicular depth from the surface; and the workmen have now pursued the tunnel to near 350 yards from the entrance, and the rib of lime-stone and balls continue nearly the same; the rib has varied a little in thickness, and has not pursued a straight line; in one place it nearly left the tunnel to the northward, but in a few yards turned southward, to its former direction. The lime-stone of the rib is not perfectly pure, that in the balls is still less so, but it makes a good lime for cement. The balls when broken appear to be mixed with a kind of pyrites, in small particles, near their outward edges; their form is very peculiar, being similar in all their sizes; it is not perfectly globular, being flattened a little on two opposite sides, which appear to have been the poles when in a revolting state; and each ball is more or less furrowed in a latitudinal direction, as if, when revolving round its axis, and taking its fixed state from a more fluid one, it had met with some resisting substance.

Though the surface of this country is very rocky, it does not discover lime-stone any where within 20 miles of this tunnel; yet if the strata near the lime-stone at Buxton to the south, and at Clitheroe to the north, are examined, it will appear probable that the base of these hills is lime-stone at some depth, and this fault discovered in the shale probably extends from the lime-stone bed beneath; and the rib of lime-stone and balls which, with other mixed substances, fill up this crevice or fault, were probably thrown thither from the mass beneath, by the volcanic eruption which first occasioned this break in the strata, or by some subsequent eruption of the same kind.

XV. Account of the Earthquake felt in Various Parts of England, Nov. 18, 1795. *By Edw. Whitaker Gray, M. D., F. R. S.* p. 353.

This earthquake happened about 11 o'clock at night, of the day above-mentioned. It appears that the shock was felt as far to the north as Leeds, and as far to the south as Bristol. To the east it was felt as far as Norwich, and to the west as far as Liverpool. Its extent from north to south therefore was about 165 miles; and its extent from east to west about 175. In this latter direction, or rather from north-east to south-west, it may be said to have reached nearly across the island. The counties in which it has been perceived are, Somerset, Wilts, Oxfordshire, Buckinghamshire, Northamptonshire, Huntingdonshire, Norfolk, Lincolnshire, Leicestershire, Warwickshire, Gloucestershire, Herefordshire, Worcestershire, Staffordshire, Cheshire, Derbyshire, Nottinghamshire, Yorkshire, and Lancashire. To which may probably be added the counties of Rutland, Berks, Bedford, Cambridge, and Shropshire. I have not indeed met with any account of the earthquake from either of them; but, whoever will examine the situation of these counties, with respect to those above enumerated, will find it difficult to conceive that they were not, in some degree, affected by it.

The accounts of it from different places are nearly alike: the duration about 2 seconds. From Worcester the following account of it was sent by Dr. Johnstone of that city, in a letter dated Nov. 24; which may be considered a pretty good specimen of the several accounts. "The earthquake was chiefly felt by persons in bed, about 11 o'clock, or 5 minutes after, who describe the sensation to have been as if some person under the bed had heaved it up. That sensation was preceded, the instant before, by a noise which some call rumbling, and which others compare to the falling of tiles, though none fell from the houses where they lived. Many persons heard the windows and doors of their rooms rattle at the same time, which increased their alarm. Thunder and lightning had been observed some days before; and several persons, of a delicate state of health, passed the night of the 18th in a restless uneasy manner, without knowing why, though very much in the manner in which they used to be affected by thunder and lightning."

In Derbyshire the shock appears to have been very severe. A description of its effects, not only on the earth, but also under its surface, is contained in the two following letters from Mr. Wm. Milnes, of Ashover: the first is dated Nov. 20. "On Wednesday night, about a quarter past 11 o'clock, a severe shock of an earthquake was felt here. I felt it very sensibly; at first I heard a rumbling kind of noise, and immediately after it appeared as if some persons had violently forced into the room; the bed, and every thing else, shaking very much. The workmen in Gregory mine were so much alarmed by the noise, and the sudden gust of wind that attended it, as to leave their work; some expecting that the whole mass of bunnings above them, which contains many hundred tons weight of rubbish, had given way, and that they should be buried in the ruins; others, who were at work near the new shaft, supposed that the curb which supports the walling had given way, and the whole shaft had run in. Several chimnies were thrown down, and several families left their habitations; indeed such a general alarm was never known in this neighbourhood."

The gust of wind mentioned by Mr. Milnes being considered as a remarkable circumstance, he was desired to make some further inquiry concerning it: in consequence of which a second letter was received from him, dated Dec. 4, as follows. "I have examined all our miners separately, and, from the following circumstances, I think there cannot be a doubt but the wind which was felt in the mines, on the 18th of last month, rushed into the shafts from the surface. Those men who were at work in the pumps, which are a considerable depth below the waggon gates, and have no communication with them, did not feel the wind; but heard in the first place, a rushing rumbling kind of noise, which appeared to be at a distance, and to come nearer and nearer, till it seemed to pass over them, and die away, Those who were in the waggon gate which has a communication with the engine shaft, and the new shaft, felt a very strong current of wind: which, one man says, continued while he walked about 6 or 7 yards, and came along the gate, as if it came from the new shaft; he had no light, but, as he went along the gate, its sides, where he laid his hands, felt as if they were going to close in upon him.

"The only one who saw any appearance of light, on that evening, in this neighbourhood, was a person who lives with Mr. Enoch Stevenson, the miller, at Mill Town, He informs me that, as he and another man were returning from Tideswell, he saw, when he got on a piece of high land near Moor-hall, on the road to Chatsworth, an uncommon light; and, when looking towards Chesterfield, the sky appeared to be open for about the length of a mile, the colour pale red, and continued so while he awakened his fellow servant, who was asleep in the waggon, to show him the strangest flash of lightning that ever was seen. From his description, the range of it was from east to west; and so low in the horizon, that, had he not been on high ground, he could not have seen it."

XVI. Newton's Binomial Theorem Legally Demonstrated by Algebra. By the Rev. Wm. Sewell, A. M. p. 382.

Let m and n be any whole positive numbers; and $(1+x)^{\frac{m}{n}}$ a binomial to be expanded into a series, as $1 + \mathrm{A}x + \mathrm{B}x^2 + \mathrm{C}x^3 +$ &c. where A, B, C, D, &c. are the co-efficients to be determined.

Assume $v^m = (1+x)^{\frac{m}{n}} = 1 + \mathrm{A}x + \mathrm{B}x^2 + \mathrm{C}x^3 + \mathrm{D}x^4 +$ &c.

And $z^m = (1+y)^{\frac{m}{n}} = 1 + \mathrm{A}y + \mathrm{B}y^2 + \mathrm{C}y^3 + \mathrm{D}y^4 +$ &c.

Then will $v^n = 1 + x$, and $z^n = 1 + y$; $\therefore v^n - z^n = x - y$.

And $v^m - z^m = \mathrm{A} \times (x-y) + \mathrm{B} \times (x^2 - y^2) + \mathrm{C} \times (x^3 - y^3) +$ &c.

Consequently $\frac{v^m - z^m}{v^n - z^n} = \mathrm{A} + \mathrm{B} \times (x+y) + \mathrm{C} \times (x^2 + xy + y^2) +$ &c.

Now $v^m - z^m = (v - z) \times : v^{m-1} + v^{m-2}z + v^{m-3}z^2 +$ &c. . . . . z^{m-1}.

Also $v^n - z^n = (v - z) \times : v^{n-1} + v^{n-2}z + v^{n-3}z^2 +$ &c. . . . . z^{n-1}.

Therefore $\frac{v^m - z^m}{v^n - z^n}$ reduces to, and becomes $= \frac{v^{m-1} + v^{m-2}z + v^{m-3}z^2 + \&c. \ldots z^{m-1}}{v^{n-1} + v^{n-2}z + v^{n-3}z^2 + \&c. \ldots z^{n-1}}$ $= \mathrm{A} + \mathrm{B} \times (x+y) + \mathrm{C} \times (x^2 + xy + y^2) + \mathrm{D} \times (x^3 + x^2y + xy^2 + y^3) +$ &c.

The law is manifest; and it is also evident that the numerator and denominator of the fraction respectively, terminate in m and n terms. Suppose then $x = y$; then will $v = z$; and our equation will become $\frac{mv^{m-1}}{nv^{n-1}}$ or, $\frac{mv^{m-n}}{n} = \mathrm{A} + 2\mathrm{B}x + 3\mathrm{C}x^2 + 4\mathrm{D}x^3 +$ &c.

But $v^n = 1 + x$, therefore by multiplying we have $\frac{mv}{n} = \mathrm{A} + (\mathrm{A} + 2\mathrm{B})x + (2\mathrm{B} + 3\mathrm{C})x^2 + (3\mathrm{C} + 4\mathrm{D})x^3 +$ &c. Or $v^m =$ $(1+x)^{\frac{m}{n}} = \frac{n\mathrm{A}}{m} + \frac{n\mathrm{A} + 2n\mathrm{B}}{m}x + \frac{2n\mathrm{B} + 3n\mathrm{C}}{m}x^2 + \frac{3n\mathrm{C} + 4n\mathrm{D}}{m}x^3 +$ &c. Compare this with the assumed series, to which it is similar and equal, and it will be,

$$\frac{n\mathrm{A}}{m} = 1;\ \frac{n\mathrm{A} + 2n\mathrm{B}}{m} = \mathrm{A};\ \frac{2n\mathrm{B} + 3n\mathrm{C}}{m} = \mathrm{B};\ \text{\&c.} = \text{\&c.}$$

$$\therefore \mathrm{A} = \frac{m}{n};\ \mathrm{B} = \frac{m-n}{1.2.n}\mathrm{A};\ \mathrm{C} = \frac{m-2n}{1.2.3.n}\mathrm{B};\ \text{\&c.}$$

Therefore $(1+x)^{\frac{m}{n}} = 1 + \frac{m}{n}x + \frac{m \times m-n}{1.2.n^2}x^2 + \frac{m \times m-n \times m-2n}{1.2.3.n^3}x^3 +$ &c. the law is manifest, and agrees with the common form derived from other principles.

Schol. In the above investigation, it is obvious that unless m be a positive whole number, the numerator above-mentioned does not terminate: it still remains therefore to show, how to derive the series when m is a negative whole number. In this case, the expression $(v^m - z^m)$ assumes this form, $v^{\frac{1}{m}} - z^{\frac{1}{m}}$, or its equal $\frac{z^m - v^m}{v^m z^m}$, which divided by $v^n - z^n$, as before, gives

$$\frac{1}{v^m z^m} \times \frac{z^m - v^m}{v^n - z^n} = \frac{1}{v^m z^m} \times \frac{-(v - z) \times : v^{m-1} + v^{m-2}z + v^{m-3}z^2}{(v - z) \times : v^{n-1} + v^{n-2}z + v^{n-3}z^2 +}, \text{\&c.} = \frac{-1}{v^m z^m} \times$$

$\frac{v^{m-1} + v^{m-2}z + v^{m-3}z^2 + \&c.}{v^{n-1} + v^{n-2}z + v^{n-3}z^2 + \&c.} = (\text{when } v = z) - \frac{mv^{m-1}}{v^{2m} \times nv^{n-1}} = \frac{-mv^{-m-n}}{n}$, which is the same as the expression ($\frac{mv^{m-n}}{n}$) before derived, with only the sign of m changed. The remainder of the process being the same as before, shows that the series is general, or extends to all cases, regard being had to the signs. Q. E. D.

XVII. A Description of the Anatomy of the Sea Otter, from a Dissection made Nov. 15, 1795. *By E. Home, Esq., F. R. S., and Mr. Arch. Menzies.* p. 385.

The subjects from which the following description is taken, were procured from the natives on the west coast of America, near Queen Charlotte's Isles, by Mr. A. Menzies, surgeon in the navy, and naturalist to the expedition fitted out by government for making discoveries, under the direction of Capt. Vancouver. The sea otter is not confined to this particular situation; it was met with in the course of the voyage every where along the coast, from 30° to 62° north lat., and sometimes even 100 leagues out at sea. Two sea otters were examined, both of them males; one was a cub not old enough to leave the mother, the other appeared to be full grown.

A description of the external appearances.—The large one measured 4 feet 4 inches from the nose to the extremity of the tail. The body appears a little compressed, and is nearly of the same thickness throughout; its circumference is 2 feet 4½ inches. The colour of this animal varies in different subjects, but in general the head and neck are grey, or of a silver colour; the back, sides, legs, and tail, black and glossy; in some, the longest hairs are tipped with white, which gives them a beautiful greyish cast; the breast and belly also vary from a silver grey, to different shades of light brown. The long hairs shine with a brilliant gloss, but the short fur is exceedingly fine, soft, and thick set; and its colour is either a light chesnut-brown, or it has a silver hue, and a beautiful silky gloss. In the cub state, the hair is a long, coarse, shaggy fur, of a brown colour, destitute of any gloss; but as the animal grows up the fur becomes finer and more beautiful.

There are 2 nipples, one on each side of the sheath of the penis, nearer to the anus than to the external orifice of the sheath. The sheath of the penis does not project beyond the skin of the neighbouring parts; its external orifice is 7 inches from the anus, but the sheath itself extends 1½ inch further on under the skin of the belly; by which means the penis, when inclosed in it, has its point more effectually defended from injury. The head is somewhat compressed, and small for the size of the animal. The nose and upper lip are very muscular, and protrude about 1½ inch beyond the gums and lower lip. The eyes are small, and placed directly over the angles of the mouth, about ½ way between the ears and the tip of the nose. The ears are nearly naked, black, slightly notched at the ends, and about 1 inch long; they are 6 inches removed from the tip of the nose. The whiskers are in great number, they are white and strong, they arise from the upper lip on each side of the nose. There are a few weak long hairs on the eyebrows.

In the upper jaw there are 6 conical incisor teeth, regularly placed; of these the middle ones are the smallest. Two strong conical fangs, $\frac{3}{4}$ths of an inch long, measuring from the edge of the gums; on each side there are 2 small obtuse pointed teeth, of which that next the fang is much the smallest; and 2 broad molares with very irregular grinding surfaces. In the lower jaw there are 4 incisores, flatter than those in the upper; 2 fangs, shorter than the upper ones; and on each side 2 small teeth and 3 molares, similar in appearance to those in the upper jaw.

The fore legs are short and strong, with palmated feet; each foot has 5 toes. They are covered with a thick black fur, which has a fringe of the same colour round the edge of the sole of the foot, where the fur terminates. The hind legs, when stretched backwards, reach nearly to the end of the tail, and are well adapted for swimming, having 5 long wide-spreading palmated toes with claws, of which the innermost is the shortest; they measure across 8 inches, and are completely covered with fur, except a small spot under the extremity of each toe. The claws are of a light colour, and channelled on the under surface; those on the fore feet are small, and placed so far back that they seem of little use but as a defence for the upper part of the toe; those on the hind feet are stronger, and project beyond the toes. The tail is flat, and tapers to a sharp point; it is covered with a thicker short fur than any other part of the animal.

A description of the internal parts.—The panniculus carnosus, which lies immediately under the skin, is very strong, and extends over the greatest part of the body. The tongue is 4 inches long, and rounded at the end, in which there is a slight fissure, giving the tip a bifid appearance. The papillæ on its surface are soft; they are long towards the root, but less so near the tip. The os hyoides, thyroid, and cricoid cartilages are small for the size of the animal, and weak in their texture. The cricoid cartilage is not a circular ring, but made up of 2 equal parts, united anteriorly; their lower edge at this union forms an acute angle, the 2 sides pass a little down upon the trachea as they go round it; and the lower edge laps over the upper annular ring of the trachea. The thyroid gland is small, and divided into 2 parts. The epiglottis is short, and its edges are attached by means of a ligament to the inner side of the thyroid cartilage. The passage of the glottis is small. The rings of the trachea are circular, and disunited behind, so that their edges meet, and when pressed on, they lap over each other, being bevilled off for that purpose. Towards the bifurcation of the trachea, the space behind, which is not occupied by the cartilaginous ring, is much increased. This space is occupied by a muscle whose principal fibres are transverse. The trachea is very elastic in a longitudinal direction: 7 inches of its length being readily elongated to 10$\frac{1}{2}$, and immediately on being left to itself it contracts to its former state.

The lungs on the right side have 3 lobes, 2 large and one small azygos lobe; the lower lobe sends a process between the pericardium and diaphragm. On the left side there are 2 lobes. The lungs were completely empty, so as readily to sink in the spirits in which they had been preserved; the cells are very small, and so elastic that they are difficultly expanded, and readily collapse. The anterior mediastinum is of considerable breadth, but free from fat, consisting of nothing besides the duplicature of the pleura. In the fœtus there is a very large thymus gland, convex on its external surface, and concave on the other. The heart is inclosed in a thin pericardium, is rather short, free from fat on its external surface, and rounded at the apex. The ventricles have no communication between them, but the foramen ovale between the auricles remained open; the passage was however so oblique, that it must have acted as a valve; it admitted a crow quill. In the fœtus it was less oblique. The structure of the heart, and the valves of the aorta and pulmonary artery, are the same as in other animals. There were no remains of the canalis arteriosus. The aorta had nothing unusual in its appearance, but the vena cava descendens is very large; when slit open, its breadth is 5½ inches. The œsophagus is small for the size of the animal.

The stomach is bent on itself, the pylorus being on a line with the entrance of the œsophagus, and not at a great distance from it. The cardia does not project much into the left hypochondre; and that half of the stomach next the pylorus is much smaller than the other. The coats are thin. The internal surface is free from rugæ; the posterior portion is smooth, without any appearance of glandular structure; the anterior portion is more vascular and villous. At the pylorus there is an oval part of the internal surface of a dark colour, and rougher or more villous than the rest of the stomach, with a determined edge; the small end of the oval extends about ½ an inch beyond the pylorus into the duodenum; the larger end goes some way into the stomach, and extends chiefly over the posterior surface, also a little way beyond the great arch anteriorly, covering about ½ the breadth of this part of the stomach; it is nearly as long again as it is broad. This part is probably glandular; it was only seen in the young subject, which from the smallness of its size was more perfectly preserved, and its internal parts better fitted for anatomical examination. At the pylorus there is the usual thickened valvular appearance. The stomach was entirely empty, and in a very flaccid state.

The duodenum makes a considerable bend downwards on the right side before it crosses the spine, to become a loose intestine; there is no cœcum or difference of size in the intestines, they are all strung upon the mesentery till within 15 inches of the anus; this part of the gut crosses the spine above the root of the mesentery, and passes down to the anus. The intestines have no valvulæ conniventes; they were 52 feet long, which is 12 times the length of the animal. In a common otter, the intestines are only 3½ times the length of the animal. In a common otter 2 bags are found at the anus, but there are none in the sea otter. The mesentery is 7 inches broad, and its lower part, which may be called meso-rectum, is only 5 inches in breadth. The mesentery is thin, and has a great many blood-vessels which are accompanied with fat. There are no lymphatic glands on the general membrane, but a cluster of very large ones close to the root of the mesentery. The lacteals appear a little larger than in the human subject, but the circumstance of the animal having been 2 years in spirits, was very unfavourable for their examination. The omentum is a thin reticular membranous double bag, covering the whole of the intestines; it is attached anteriorly to the great curvature of the stomach, but not to the duodenum; posteriorly to the loins.

The liver is made up of 5 lobes, besides the lobulus Spigelii; 3 on the right of the falciform ligament, 2 on the left. The gall-bladder is found in the usual situation, is bent in the middle upon itself, and is 6 inches long. The cystic and hepatic ducts unite at the external surface of the duodenum, forming a common canal, or ductus communis choledochus, about 1½ inch long, of an oval shape, with an irregularly rugous internal surface, placed between the muscular coat and the internal membrane of the intestine; it opens into the duodenum by a projecting orifice 2½ inches from the pylorus. The vena portarum is very large, and the passage behind the ducts of the liver into the cavity of the little epiploon is also large. The pancreas is situated across the spine behind the stomach, it is not confined within the usual limits, but extends along the posterior membrane of the omentum. It is sub-divided into a number of small parts, of an oval shape, all at a certain distance from each other, united by blood-vessels resembling small leaves on the branch of a shrub. The little pancreas puts on the same appearance, covering the whole meso-duodenum, which is unusually broad. The duct of the pancreas is of the ordinary size, it opens into the duodenum by a separate orifice 1½ inch from the pylorus. In the common otter the pancreas has not this unusual sub-divided appearance, and the duct opens by a common orifice with those of the liver into the duodenum.

The receptaculum chyli is an oval bag, ¾ of an inch broad, from which 2 trunks go off to form the thoracic duct, each of them about ⅓th of an inch in diameter; these anastomose frequently in their course, so that there are always 2, sometimes 3, and even 4 trunks, running parallel to each other; the thoracic duct is 8 inches in length. The kidneys are conglomerated, 6 inches long, and 3 broad. The urinary bladder is pendulous and pyramidal, and the ureters open into it very near each other at the lower posterior part. The testicles are situated under the external skin on each side of the sheath of the penis, but have no pendulous scrotum. They are small, flat, and oval. The tunica vaginalis communicates with the cavity of the abdomen. The cremaster muscle is very strong. The vasa deferentia, as they pass behind the bladder, become a little convoluted, and open into the urethra at the caput gallinaginis. The penis, in the relaxed state, is 8 inches long, the bone 6 inches. The corpora cavernosa are small, but strong in their coats. The bone near its anterior end appears to be covered with a quantity of loose cellular substance; this in the erected state is filled with blood, and forms a large glans 6 inches in circumference, and 4 inches long; its anterior extremity is concave, and the end of the bone is seen in the centre. The penis, when erect, is 11 inches long. The erectores muscles are very strong. The globe of the eye is extremely small, and the optic nerve is small in the same proportion. Its internal parts were not in a state to bear examination. The articulation of the lower jaw admits of no motion forwards or laterally; it is a simple hinge an inch long, and very narrow. The condyle of the jaw is so much inclosed in the socket as to be with difficulty disengaged. The ribs are 14 in number, 9 true, and 5 spurious.

XVIII. Observations on some Ancient Metallic Arms and Utensils; with Experiments to determine their Composition. By George Pearson, M. D., F. R. S. p. 395.

Most of these articles, communicated by the president, Sir Joseph Banks, Bart. were found in Lincolnshire, in the bed of the river Witham, between Kirksted and Lincoln. Several of them were discovered when that river was scoured out in 1787 and 1788. The instruments were evidently made of what are commonly called brass, and iron. The brass instruments were allays of copper by tin; and the supposed iron implements were found to be steel. It may be proper here to observe, that brass is a term commonly used to denote any metallic composition the principal ingredient of which is copper; but the most accurate writers in chemistry use the term brass, with more precision, to denote only the compound of copper and zinc; and therefore (says Dr. P.) I shall employ it in this latter sense.

SECTION I. OF THE COPPER INSTRUMENTS.

§ 1. *Miscellaneous historical observations.*—The articles belonging to this head were 7 in number; namely, a lituus, a spear-head, a sauce-pan, a scabbard, and 3 celts.

1. The lituus.—This is represented, pl. 1, fig. 1. It is well known to have been a military musical instrument of the Romans. Several classical writers mention it, as Horace, ode i. Virg. Æn. l. vi. v. 162. Georg. l. iii. v. 182. It is supposed by judicious antiquaries to have been adopted from the barbarous nations; and that the figure of it was intended by them to resemble a snake, the principal object of their religious worship, and of the most sacred mysteries of the Druidical religion. If these remarks be true, they throw new light on the crooked staff of the augurs, which the lituus much resembles. It is accurately represented among the trophies which ornament the base of Trajan's column at Rome, erected in memory of his conquest of the Dacians and Sarmatians, and covered with bas-reliefs, describing the events of that war. The lituus is also found on the reverses of some Roman coins: see fig. 2.

The present specimen is imperfect, a little of both ends being broken off: it is however a very valuable relic, as there is little doubt that it is the only one known to be in any cabinet at this time in Europe. It has been neatly made. The parts which appear like joints are pieces which slide over the tube for ornament, or perhaps for holding the instrument more conveniently. It had the appearance of a brazen tube, from which a great part of a blackish coating has been rubbed off. It was evidently made of a plate of hammered metal of about $\frac{1}{30}$th of an inch thick. The juncture of the edges of the metal, the whole length of the tube, was preserved by means of a solder clumsily applied, by melting it withinside the tube.

This solder, which was readily melted out by a red-hot iron, was ascertained to be merely tin; for it afforded rapidly oxyde of tin by applying nitric acid: the cold saturated solution in muriatic acid afforded Cassius' precipitate on dropping into it nitro-muriate of gold; and it afforded no acetite of lead on digesting it in acetous acid. The black coating was easily scraped off with a knife, but the quantity of it was too small to enable me to determine whether it had been applied by art, or was the accidental effect of the mud or earth in which it had been buried for many ages. The ancients, as Pliny informs us, stained plates of one sort of copper, the æs coronarium, with ox-gall to make it look like gold; and the crowns and chaplets of public actors were made of copper so coloured *. It perhaps will not appear very improbable that the coating of the lituus was with this substance.

2. Fig. 2, represents a spear-head. In Sir Jos. Banks's collection there is a British spear-head of bone, a Norman one of iron, and a third, the article before us, of copper, which is believed with the greatest reason to be Roman workmanship †. This Roman spear-head is worthy of admiration and imitation, on account of its figure, weight, and size, as an offensive weapon. It is however made of cast metal, as appears from its rough surface, figure, texture, and grain. That it is made of bad metal will be made appear hereafter. It has not been hammered, but has been cast hollow to receive a wooden shaft, and in order to be light and save the expence of metal. It is evident from its figure, that it is of the very best conceivable form for piercing, and for inflicting the largest wound at the least expence of weight and bulk.

3. The sauce-pan is represented in fig. 3. From its form and the grain of its fracture, and its being one entire piece, it appears to have been made of cast metal. It is considered to be a piece of Roman workmanship. It is neatly and curiously grooved at the bottom, to admit the fire to penetrate to the contents more easily. On the handle is impressed, seemingly with a stamp, c. arat; which letters may possibly signify Caius Aratus, as the latter part of the stamp seems not to have made an impression. It appeared to have been tinned, but almost all the coating had been worn off. As it was said that it had been used by some boatmen, for some time after it had been found, it might have been tinned after it got into their possession. The art of tinning copper however was understood and practised by the Romans ‡, though it is commonly supposed to be a modern invention; so that it is not very improbable that this utensil was originally covered with tin by that people.

4. Fig. 4 represents the scabbard with a sword of iron within it, supposed to be either Danish or Saxon, being found in the Witham near the scite of Bardney

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* "Coronarium tenuatur in laminas, taurorumque felle rinctum, speciem auri in coronis histrionum præbet." Pliny, lib. 34, cap. 8.—† An instrument is described and represented by a figure in the Archæologia, vol. 9, fig. c, exactly like this spear-head, and it is deemed to be Roman.—‡ "Stannum illitum æneis vasis, saporem gratiorem facit, et compescit æruginis virus." Pliny, lib. xxxiv. cap. xvii.—Orig.

Abbey, destroyed by the Danes in 870, (Tanner's Not. Monast. p. 248.) In Strutt's Hoꞃꝺa Anᵹel-cẏnnan, where he describes the customs of our Saxon and Danish ancestors, such swords frequently occur; especially in the lives of the 2 Offas. Similar swords are also in the hands of the Danes, who are killing the abbot of Croyland on the shrine, as delineated by Dr. Stukeley, in the Phil. Trans. vol. 45, p. 597. The brass scabbard possesses some degree of elegance, and much accuracy of workmanship. It appears to have been originally covered with a bright blue varnish, but the quantity was much too small for ascertaining its nature. Exactly such a sword as this is represented at the side of a Danish soldier, in pl. 26, vol. 1, of Strutt's work just quoted. The sword within this scabbard was destroyed by rusting, and could not be drawn out. The pommel and guard had been broken off. There was a plate of open work, about 4 inches long, laid over one side, and near the top of the scabbard; and at the bottom, on one side, was a sort of joint; and on the other and opposite side was a bas-relief figure. The scabbard was made of hammered metal, and was perhaps about $\frac{1}{30}$ of an inch thick.

The next, and last 3 articles under the present head, are known to antiquaries by the name of Celts. They were probably instruments used by the ancient Britons, Gauls, or Celtæ. The learned do not agree whether the celts were Roman workmanship or not: nor to what particular uses they were applied. Accordingly some persons have supposed that they were the offensive weapons of our ancestors; and others have supposed that they were both offensive military weapons, and civil instruments; but the most probable opinion is, that they were merely domestic tools. Many of the celts are cast after the model of stone implements, which are confessedly ancient British or Celtic chopping instruments, and tools for making holes. Several of these stone implements, in Sir Jos. Banks's collection, correspond exactly with the figure and size of the celts. Great quantities of these instruments have been at different times discovered in England, as well as in Ireland, and some few in France. Sometimes they have been found in heaps, as if the owner had, and probably did throw them away by basket-fulls, as things of little value. It has been very ingeniously conjectured, that when the Romans came to Britain they found the inhabitants, especially to the northward, very nearly in the same state as that in which our late discoverers found the natives of the South Sea islands. The Britons parted with their valuable articles of food, rarities, and commerce, for metal tools made in imitation of their stone ones; but in time, finding themselves cheated by the Romans, who made these tools of bad metal, of the shape of the ancient British stone axe, as the inhabitants of Otaheite were by the use of base metals; they relinquished these tools when they became acquainted with those made of better metal, and according to the Roman patterns. Hence we see a reason for such great quantities of celts being found among the Celtic nations, and not among the Roman, excepting now and then a specimen, which may be considered as the tool or spoil of barbarian auxiliaries.

5. Fig. 5. represents a Celt, N°. 1, found on the peninsula of Ballrichen, within

the precincts of a Druidical grove, or dwelling, in Ireland. The same kind of celt is described in Wright's Louthiana, b. 14, p. 7, pl. 1; and also in the Archæologia, vol. 5, p. 113, by Dr. Lort. It weighed 1 and $\frac{1}{4}$ lb. Except at the edge it was nearly $\frac{3}{8}$ths of an inch in thickness. It was of a blackish colour, from oxyde of the metal and dirt on its surface.

6. Fig. 6, represents the Celt, N° 2. It was found in a field, by ploughing, in Cumberland. The celt in Dr. Lort's collection which most resembles this article is delineated by fig. 11, pl. 8, p. 113, vol. 5, of the Archæologia. The celt before us differs from that just referred to, in being grooved on both sides to receive a shaft or handle, instead of having a socket. It weighed nearly $\frac{3}{4}$ lb., and was about $\frac{5}{8}$ths of an inch thick, except at the edge. Its external appearance was like that of the former celt.

7. Fig. 7, represents the Celt, N° 3. It was much smaller than the 2 former, weighing only about 5 oz., but it resembled in shape, fig. 5.

In § 2. are described the external, or more obvious properties of these arms and utensils.

In § 3. the specific gravities of these arms and utensils are stated, being mostly contained between the limits 8.3 and 8.9.

§ 4. *Experiments with fire.*—(a) These old instruments melted at a lower temperature than that at which copper, or even some kinds of brass melt. Though I did not (says Dr. P.) succeed in determining precisely the temperature at which each of them fuses; it may be useful to relate the experiments made with that view.

(b) 100 grs. of each of the above 7 ancient metallic instruments, and the same quantity of copper, of pure silver, of allay of copper with $\frac{1}{8}$th of its weight of tin, of allay of copper with $\frac{1}{10}$th of its weight of tin, of allay of copper with $\frac{1}{20}$th of its weight of tin, of allay of 3 parts of copper with 1 of zinc, and of gun metal, were exposed each in separate coppels, under a muffle, to the greatest degree of fire I could produce in the best assay furnace. A pyrometer clay piece of Wedgwood's instrument was also put into each coppel. During 40 minutes exposure to fire, not one of the metals melted, except the pure silver*, and the allay with zinc: nor did any of them emit visible vapour, or inflame, except the allay with zinc; nor did any matter ooze out of any of the metals.

On cooling, it was found that the figure of the metals which had not been melted in the coppels was not altered, but they were changed, either totally or externally, into scoria-like black matter. The copper allayed with zinc was found to contain a nucleus of copper within a large proportion of black scoria and white oxyde of zinc. The celt metals were changed into scoriæ, including copper-like

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* In Wedgwood's scale it is stated, that pure silver melts at 28°, and Swedish copper at 27°. But every part of the furnace in the above experiments might not be of the same temperature for the same space of time: and perhaps the state of cohesion and figure of the metal exposed to fire may account for the difference in the degree noted by the pyrometer in my experiment, from that stated in the scale. For I am assured, by Mr. Thos. Wedgwood, that the degree of contraction is uniform among a number of pyrometer pieces, exposed in the same part of the furnace at the same time.—Orig.

metal. The other old metals were changed entirely into scoriæ. The copper allayed with $\frac{1}{20}$th of its weight of tin was changed into scoria containing a little copper; but the copper allayed with $\frac{1}{8}$th of tin was changed into scoria containing a little copper, seemingly allayed with a much smaller proportion of tin than before. The pyrometer pieces indicated degrees of fire, which varied between 18° and 21°. The pyrometer piece in the coppel which contained the silver, and also that in the coppel which contained the copper, denoted 20° of Wedgwood's scale, or about 3800° of Fahrenheit's scale.

(c) A thin plate of each of the old metals being exposed to the flame of a candle with the blow-pipe, a blue and green flame appeared, but soon disappeared, though the fire of the candle was applied so as to keep the metal red-hot. The same kind of blue and green flame was emitted from plates of these metals when they were exposed to fire in open crucibles, before they were melted; but it disappeared in a few seconds of time, though the fire was continued to be applied to keep the metal red-hot; nor was any such flame produced when the metal was melted in open vessels, or kept stirring when in a fluid state.

(d) Each of the ancient metals being melted in close vessels, was then exposed to the air, and stirred with an iron rod; but none of them emitted any blue flame, or white vapour, as was the case when brass was so treated. The following experiment, to determine whether the ancient metal instruments contained any gold or silver, was made, while I was present, by Mr. Bingley, Assay Master.

(e) 50 grs. of each of these metals, and as much gun metal, and also the same quantity of brass, were put into separate coppels, together with 150 grs. of lead, under the muffle of an assay furnace: 150 grs. of lead were also put alone, by way of test, into a separate coppel. The fire being kept up in the usual way, the brass emitted a blue flame, and began to melt, discharging at the same time white fumes: but soon after it was melted, the flame and white fumes disappeared. The ancient metals, and also the gun metal, afterwards melted, and without sending forth any flame, but a slight fume was seen when they were in fusion; which was particularly evident from the coppel containing the spear-head metal. This fume was not seen to arise from the coppel which contained lead only; but the Assayers observe it from charges of lead with silver, or lead with gold and silver, when much air is admitted.

' The process being finished, nothing was left in the coppels which contained lead only, and lead and brass, except a just visible particle of silver; but in the other coppels there remained about $\frac{1}{3}$ of the original quantity of the ancient metals, and of the gun metal: and therefore into each of these coppels 150 grs. of lead were again introduced. The process being performed a 2d time, every particle of metal was absorbed, excepting a just visible particle of silver in the coppels which contained the celt, N° 2, the metal of the scabbard, and the gun metal: but there was a much larger globule of silver in the coppel which contained the spear-head metal. As the only metal which appeared to contain more silver than the test itself, was the spear-head, and as it emitted more fume than the rest, I repeated the process on this metal. The process of cupellation the 2d time, as before, caused the appearance of the white fume, and afforded a residue of silver, as before, in greater quantity than that of the test. The silver was determined in the most accurate way to amount to the proportion of 15 grs. in a Troy pound of the spear-head metal. There was no gold in this silver, for it dissolved totally in nitric acid.

§ 5. *Experiments with nitric acid.*—(a) A polished piece of each of the ancient metals was just wetted with nitric acid. Fumes of nitrous acid arose, and the part wetted became white and corroded; as is the case when the nitric acid has been applied in this manner to the allay of copper by tin.

(b) On 300 grs. of each of the above metals, in a small retort, were poured 1800 gr. measures of nitric acid, purified by distillation from nitrate of silver, and of the specific gravity of 1350. The hydro-pneumatic apparatus being affixed, generally from 30 to 40 oz. measures of nitrous gaz came over in the cold, in the course of 2 to 3 days. In this time the whole, or at least the greatest part of the metal was oxydified and dissolved; there being a clear blue solution, with a copious white sediment, and sometimes a part of the undissolved metal. By means of the fire of a lamp, more gaz came over, which had been absorbed by the solution, and which also was afforded by the dissolution of the remaining metal. The whole quantity of nitrous gaz varied, with the same as well as with different metals, between 60 and 85 oz. measures; but either from my own inability to observe, or from the circumstances on which this variety depended being unknown, I cannot explain the reason of such differences in the result.

(c) After the solution (b) had stood several days, the clear blue liquor was decanted, and filtrated, from the white sediment: and pure water was poured on the filter repeatedly, till what passed through was colourless, and almost tasteless. The filtrated liquid was boiled to evaporate all but about 6 oz.; and it deposited, on standing, a small quantity of white sediment.

The white sediment, from the solution (b,) being dried, amounted to the following different quantities, from 300 grs. of each of the different metals, namely,

1. The sauce-pan, exclusive of a little dirty extraneous matter,	65 grs.	or 21½ per cent.
2. The spear-head, exclusive of a little dirty extraneous matter,	63 grs.	or 21 per cent.
3. The celt, N° 3	55 grs.	or 18½ per cent.
4. The lituus,	54 grs.	or 18 per. cent.
5. The scabbard,	48 grs.	or 16 per cent.
6. The celt, N° 1	42 grs.	or 14 per cent.
7. The celt, N° 2	42 grs.	or 14 per cent.

(d) The decanted and filtrated liquid (c,) being duly evaporated to crystallization, was found to contain nothing but nitrate of copper, and sometimes a very minute portion of white sediment; for it threw down nothing but prussiate of copper, on adding prussiate of soda; nor was any silver deposited on immersing in it bright copper wire; nor was any precipitation occasioned by adding muriatic acid, or muriate of soda, to the concentrated blue solution.

(e) The white sediment (c) was a light impalpably fine powder: it had a little metallic taste: it could not be melted with borax by flame with the blow-pipe, but was diffused through that salt, and rendered it opaque. This sediment dissolved totally, except a little mere dirt, by long digestion in muriatic acid, and immediately in this menstruum when caloric was applied to make it boil. This solution in muriatic acid did not throw down Cassius' precipitate on adding to it nitro-muriate of gold, but afforded a white deposit exactly like that which is made on adding nitro-muriate of gold to muriate of tin, made either by boiling tin in a large proportion of muriatic acid, or by dissolving oxyde of tin, made with nitric acid, in muriatic acid. The muriatic solution of the white sediment (c,) on adding prussiate of soda, afforded a precipitate exactly like that which appears on adding prussiate of soda to muriate of tin.

The white sediment (c) being mixed with tartar, on charcoal, the flame of a candle by the blow-pipe was directed upon it: by which treatment small silver-like globules were made to appear. These globules being collected, were digested in the cold, in so small a proportion of muriatic acid as could not dissolve the whole of the globules supposing them to be tin. They were gradually almost all dissolved, and nitro-muriate of gold being added, Cassius' precipitate was immediately deposited. But the metallic globules being dissolved by boiling in a large proportion of muriatic acid, no Cassius' precipitate was produced on adding nitro-muriate of gold; nor on adding it to tin dissolved by boiling in a large quantity of muriatic acid.

The preceding analytical observations and experiments will, on examination, perhaps be found to contain sufficient evidence to demonstrate that each of the ancient metallic instruments contains copper and tin; and they will also perhaps be found to prove, that these metals contain no other kind of metal, or other species of matter. But, in order to ascertain the proportion of the tin to the copper more accurately than I was able to do by analysis, and also in order to confirm or invalidate the evidence of analysis, I made the following synthetical observations and experiments.

§ 6. *Synthetical observations and experiments.—Exper.* 1. 50 grs. of tin were united by fusion with 1000 grs. of copper. The ingot of this allay of 20 parts of copper by 1 of tin, when polished, differed from the celt metals in shade of the same colour; these being much paler than this allay. It was a good deal harder, and not so tough as copper, but it was not so hard, and was more tough than the celt metals. Its fracture showed also a more open grain than the old metals, and more inclining to the peculiar red colour of copper, instead of the brown and grey, or slate colour of the ancient metals. With nitric acid it afforded, like the ancient metals, a blue liquor, and white deposit of oxyde of tin; but in much smaller proportion than any of them; not being more than 7 per cent.

Exper. 2. 100 grs. of tin were united by fusion with 1500 grs. of copper. This allay of 15 parts of copper with 1 of tin resembled the celt metals, N° 1 and N° 2, in colour, polished surface, grain of the fracture, and brown colour of the fracture; consequently the red colour of the copper was completely destroyed. It was not however so hard, though stronger than these celt metals; but was harder than the spear and the sauce-pan. The solution of this allay with nitric acid only differed from that in the former experiment in affording a more copious white deposit, namely, 10 per cent. of it in its dried state.

Exper. 3. 100 grs. of tin were melted with 1200 grs. of copper. This allay of 12 parts of copper by one of tin could scarcely be distinguished from the last described allay in the colour of the polished surface, nor was it so much closer grained or lighter coloured in its fracture as might have been expected; nor could I by the hammer distinguish it from that allay in point of hardness and strength. On the trial with the drill, it however betrayed a good deal more hardness. It was almost as hard as the celts, N° 1 and N° 2. With nitric acid it afforded a deposit of 11 per cent. of oxyde of tin.

Exper. 4. 100 grs. of tin were united by fusion with 1000 grs. of copper. This allay of copper with $\frac{1}{10}$ of its weight of tin was as pale coloured as the celts, N° 1 and N° 2, but not nearly so pale as the celt, N° 3. I could not distinguish this allay in the properties of hardness and strength from the 2 celts, N° 1 and N° 2, and the scabbard; but it was harder than the spear-head and sauce-pan, though not so brittle. Its fracture showed the same kind of rather open grain, and texture, as that of the celts, N° 1 and N° 2, before they were melted, but it was not so close grained as any of the ancient metals after fusion; and it differed from all of them in being of a lightish brown colour. The solution in nitric acid differed only from the former in affording a greater proportion of white deposit, namely, 13$\frac{1}{2}$ grs. per cent.

Exper. 5. 900 grs. of copper were melted with 100 grs. of tin: which allay of 9 parts of copper with 1 of tin differed very little from the former. By means of nitric acid this allay gave 17 grs. per cent. of oxyde of tin.

Exper. 6. 100 grs. of tin were melted with 800 grs. of copper. This allay of 8 parts of copper with 1 of tin was also scarcely distinguishable from the 2 former allays, in colour, strength, appearance of fracture, texture, and polish. With nitric acid this allay afforded 18$\frac{1}{2}$ grs. per cent. of oxyde of tin.

Exper. 7. 100 grs. of tin were melted with 700 grs. of copper. This allay of 7 parts of copper with 1 part of tin was evidently different from any of the former allays; being harder, more brittle, paler coloured, the fracture showing a much finer grain, and of a grey or somewhat slate colour. The grain therefore of this allay resembles in colour that of the celt, N° 3, the lituus, the spear-head, and the scabbard. It was especially like the lituus and the celt, N° 3, in the rather bright and silvery appearance of the fracture, instead of the dull slate colour of the spear-head and sauce-pan. On trial with the hammer, and the drill, it resembled exactly the lituus in brittleness and hardness. It was a little harder and more brittle than the celt, N° 3, and of course much more so than the other ancient metals. This allay, on solution in nitric acid, yielded 20 per cent. of oxyde of tin.

Exper. 8. 100 grs. of tin were melted with 600 grs. of copper. This allay of 6 parts of copper with 1 of tin was harder than any of the above allays: and perhaps it was harder and more brittle than any of the ancient metals. Its fracture exhibited a still finer, brighter, silvery, and more crystallized grain than any of the preceding allays. Nitric acid separated from this allay 22 per cent. of oxyde of tin.

Exper. 9. 100 grs. of tin were melted with 400 grs. of copper. This allay of 4 parts of copper with 1 of tin was about as hard and brittle as some sorts of bell-metal. Its fracture was still paler, finer grained, and silvery, than any of the preceding allays. Nitric acid separated from this allay 27 per cent. of oxyde of tin.

Exper. 10. 100 grs. of tin were melted with 300 grs. of copper. This allay of 3 parts of copper with 1 of tin, was much harder than any of the preceding ones. It was also much more brittle, the fractured surface was quite smooth, and without almost any grain at all. It was of a silvery hue, and resembled much an ingot of a melted bell; excepting that it was finer grained, and of a duller colour.

Exper. 11. 100 grs. of tin were melted with 200 grs. of copper. This allay of 2 parts of copper with 1 of tin, was as brittle almost as glass. The fracture showed no grain at all, being quite smooth. Its colour was more like that of silver than any other metal.

Exper. 12. The metal of which what are called brass guns are made, does not in general contain a grain of zinc. They are made of an allay of about 10 to 12 or 13 parts of copper, with one part of tin. I found that the shavings of 1 of these guns melted much more readily than copper. The ingot was not so hard, but tougher than any of the above ancient metals. It possessed nearly the same hardness and strength as the allay, in experiment 3, of 12 parts of copper by 1 of tin. The colour of the polished surface, and the grain and colour of the fractured surface, resembled pretty exactly that allay. Of course this gun metal is only a little less hard and brittle than the celts, N° 1 and N° 2, but it resembles them very exactly in the colour and texture of the grain. This gun metal afforded nearly 13 per cent. of oxyde of tin, by means of nitric acid.

Exper. 13. 20 grs. of tin and 10 grs. of zinc were melted with 800 grs. of copper. This allay of 80 parts of copper with 2 parts of tin, and 1 part of zinc, was a metal which had a very different aspect when polished, as well as when fractured, from either copper, or any of the above allays, or any of the ancient metals. For it had a rich yellowish or golden hue, and was nearly as tough, but a little harder than copper.

Exper. 14. 20 grs. of zinc were united by fusion with 800 grs. of copper. This allay of 40 parts of copper with 1 part of zinc, was of a yellowish golden hue, and of course was very different in its external appearance from the allays of copper by tin. Like the allay of experiments 1st and 13th, it was too soft, and, as the artists term it, clingy, to receive the impression of lines, figures, and letters, or for instruments in which holes are to be drilled. The solution of this allay in nitric acid was blue, like those of the preceding allays and old metals, but there was no white deposit.

Observation.—This is the proper place to observe that all the above allays, and the gun metal, melted at a lower temperature than copper does; and, as far as I could determine, the temperature of fusion decreases as the proportion of tin increases. The next experiments were made not only to satisfy myself, that if iron had been an ingredient in the ancient metals, it must have been made appear by the test employed; but also to determine the question made by some chemists, whether copper can be allayed by iron; and to show, as others have asserted, the allays of copper by iron, which were employed by the ancients. From the writings of many able chemists I was inclined to suppose, that a malleable uniform metal could not be composed of copper and iron, without the aid of an intermede. I therefore, in the first place, used tin as the intermede.

Some of these experiments next to be related may not be found immediately relative, but as they occurred in the course of investigation, and as I believe few experiments of the same kind have been published, perhaps they will be found useful.

Exper. 15. 2000 grs. of tin were melted with 1000 grs. of steel,* by keeping the 2 metals in a close crucible exposed to a pretty fierce fire of a melting furnace. An allay was produced of a uniform metallic mass, of the colour of pewter, of a very open grain, but uniform texture; which was as brittle, and not harder than certain kinds of old bad pewter.

Exper. 16. 1800 grs. of tin were melted with 600 grs. of steel. This allay of 3 parts of tin with 1 of steel was perfectly similar to the last allay of 2 parts of tin with 1 of steel, excepting that the allay of this experiment was not so hard, and was less brittle. Having thus prepared the steel for union with copper, by the medium of tin, I added to it copper.

Exper. 17. 600 grs. of the allay of exper. 15 were melted with 2400 grs. of copper. This allay of 12 parts of copper with 2 parts of tin, and 1 part of steel, resembled exactly the allay of 6 parts of copper with 1 of tin, in exper. 8, in the colour and grain of the fracture; in its polish, hardness, and brittleness. Its fracture was of course of a slate-coloured hue, or dark grey, somewhat crystallized and silvery. The fracture being inspected with a lens, the grain appeared finer or shorter than that of the allay of 6 parts of copper with 1 of tin.

The solution of this metal in nitric acid produced nitrous gaz, a blue solution, and a white deposit; as occurred in the dissolution of the ancient metals, § 5, p. 43, and of the allays of copper with tin, p. 44—49; but the result of the examination of this blue solution and white deposit was different from that of the ancient metals, and satisfied my mind completely, that if those metals had contained iron, it must have been detected.

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* The steel employed was part of a file. Steel was preferred to iron, because it is fusible, but iron is not.—Orig.

(a) The blue solution of this experiment being boiled, to carry off redundant acid, and evaporate about $\frac{3}{4}$ths of its water, prussiate of soda was added. A reddish-brown precipitation ensued, which resembled exactly that produced by adding this test to nitrate of copper.

(b) The white deposit of this experiment having been well edulcorated by pure water was wholly dissolved in muriatic acid. This solution differed from that of all the white deposits of the preceding experiments, in being of a reddish-brown colour, like dilute solution of muriate of iron, and especially in affording a copious precipitation of prussiate of iron by prussiate of soda. With nitro-muriate of gold however, this solution only produced a slight grey precipitation, as in the former experiments.

Exper. 18. 1000 grs. of the allay of experiment 15 were melted with 2000 grs. of copper. This allay of about 7 parts of copper with 2 parts of tin, and 1 part of steel, was an extremely hard metal, much harder than that of the last experiment; and it was very strong, but scarcely malleable. It took a beautiful polish, of a silvery colour. It was of a perfectly homogeneous texture. The grain of its fracture was extremely fine and uniform, and of a grey colour.

Exper. 19. 2000 grs. of copper were melted with 200 grs. of steel, in a close vessel, by keeping them exposed to a fierce fire in a wind-furnace for about 20 minutes. This allay of 10 parts of copper with 1 part of steel, was of a copper colour. The grain of its fracture was coarse, like that of copper. It was harder than copper, and less tough, but quite malleable. It was about as hard as the allay of 20 parts of copper by 1 of tin, and consequently was not nearly so hard as the softest of the ancient metals.

Exper. 20. 1000 grs. of copper with 500 grs. of a small round steel file were exposed to fire, as stated in the last experiment. On opening the crucible, part of the steel only was found to have been melted and united to the copper; but the other part of the steel which retained its form, was thoroughly impregnated or penetrated by copper; so that on breaking the part which had not been melted, and which was very brittle and porous, it was in appearance imperfectly metallized copper. The part of the allay which had been melted was not different from the allay of the last experiment, except that it was a little harder; being thought to be about as hard as brass.

Then follows a statement of the specific gravities of the different allays above described, which run variously between the limits 7.2 and 9.

§ 7. *Conclusions and remarks.*—1. The first conclusion, from the preceding observations and experiments is, that the ancient metal instruments examined consist principally of copper, as appears; 1st, from their external and obvious properties; particularly their colour, taste, malleability, and specific gravity: 2dly, from the whole of the metals, except a small deposit, yielding nitrate of copper with nitric acid: 3dly, from the synthetic experiments.

2. I conclude that these metal instruments contain tin; which metal was made

appear, by the experiments on the white deposit afforded on dissolution in nitric acid, § 5: and which also was made appear by the synthetic experiments, § 6.

3. The 3d conclusion is, that these metallic instruments consist of metal only, or at least of nothing else which can be detected by ordinary known modes of analysis: for they are all malleable, and uniform in their texture; which properties metals do not possess when they are mixed by fusion with extraneous substances hitherto discovered by analysis; except carbon in several metals, and siderite in iron only.

4. The 4th conclusion is, that these ancient instruments contain none of the metals but copper and tin: for, 1. They do not contain gold, silver, or platina, excepting silver in the spear-head, as appears from the experiment of cupellation, § 4, (e).—2. They do not contain lead: for that would have oozed out in the experiments of fusion and oxydation; and would have appeared in the grain of the fractures; as well as on adding muriate of soda, and muriatic acid, to the concentrated nitrate solution, § 5, (d).—3. They do not contain iron: for that would have been shown by the prussiate of soda, § 5, (d); as was proved by the synthetical experiment, § 6, exper. 17, (b).—4. They do not contain zinc: for that would have been shown by the blue flame and white flowers in exper. § 4, (c) (d); as well as by the yellow colour of the grain of the fracture, which was shown by the synthetical experiments, § 6, exper. 13 and 14.

5. Bismuth would have appeared on diluting the nitrate solution, § 5, (d).—6. Manganese would have been seen on concentrating by evaporation the nitrate solution, § 5, (c) (d).—7. Arsenic would have manifested itself by the brittleness and whiteness of the metals; by the smell and visible vapour on exposure to fire and air; and on examining the solution, § 5, (d), and the white deposit, § 5, (e).—8. Antimony would have produced more brittleness than these ancient metals possessed: a white vapour would have appeared on examining the white sediment with the blow-pipe, § 5, (e): as well as in the experiments in the assay-furnace, § 4, (b) (e); and a white precipitate would have fallen on diluting the muriatic solution of the white deposit from the nitrate solution, § 5, (e).—9. Cobalt would have been detected by the prussiate of soda; and by the colour of the oxyde, in the experiment in the assay furnace, § 4, (b); and it would have given brittleness to the ancient metal instruments.—10. It is not at all probable that nickel was present; but if it had been an ingredient, it most likely would have been betrayed by its greenish oxyde in the experiment, § 4, (b).—11. Molybdæna, and quicksilver may be mentioned for the sake of order, but it is utterly unreasonable to suppose them to be present, either naturally or by art; and evident appearances, or at least traces of them, must have occurred in the preceding experiments. As for the substances called tungsten, uranite, menackanite, and titanite *, we have not yet had sufficient evidence to prove their being peculiar metals; but from the properties which have been observed to belong to them, it is quite inconsistent with the pre-

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* A new metal, named Titanium, lately announced in the German Journals.—Orig.

ceding experiments and observations to suppose them to exist in the ancient metal instruments. It will be proper also to remark, that the only species of metals known till within the last 2 or 3 centuries, were gold, silver, quicksilver, iron, copper, lead, and tin. The oxydes of several of the brittle metals were known indeed to the Hebrews, Greeks, and Romans, and perhaps to several barbarous nations of great antiquity; but not one of them was used as an allay, except the oxyde of zinc to compose artificial orichalcum.

It appears that the metal of the spear-head contained silver; but though the presence of it was proved by a repeated decisive experiment, § 4, (e), the proportion of it was too small to alter sensibly the properties of the allay of copper with tin, and could not answer any useful purpose in such a compound. I therefore believe that the silver in this instance was not purposely added; but was an accidental or natural ingredient of the copper, used for making the metal of this spear-head. The Bishop of Llandaff made a few experiments on a celt, from which his lordship concludes that it seemed to contain zinc: for it emitted a blue flame, and a thick white smoke, on the first exposure of a piece to fire; but no such appearances were seen on the 2d exposure of the same piece to fire. Every person will readily give credit for the observations being accurately made; nor would I even refuse to admit the conclusion, that the celt examined by his lordship did contain zinc; but it is also just to observe, that a piece of copper, or of allay of copper, with tin, being exposed to fire in an open vessel, emits frequently a blue flame on a first, but not on a 2d exposure to fire soon after the first, § 4, (c); and if much air be admitted to the allay of copper with tin in fusion, a white smoke will also sometimes be seen; as was observed in the preceding experiment, § 4, (e).

I suspect that the blue flame from copper when first ignited, and which ceases on fusion, is produced by the inflammation of a little of the copper already combined with oxygen; for some oxydes of copper are so combustible, that if a small part of a given mass of them be ignited, the ignition will spread rapidly throughout the whole mass. Most probably celts were originally chopping tools, as we have shown in a former part of this paper, and therefore the addition of zinc to the allay of copper with tin would answer no useful purpose.

5. The 5th conclusion relates to the proportion of the copper and the tin to each other, in the ancient metals. I endeavoured to estimate the proportion of tin, by comparing the quantities of oxyde of tin obtained from the ancient metals, with the quantities of oxyde of tin obtained by the same means from allays of copper with known proportions of tin. It appears from the analysis of the allays of copper by tin, that the oxyde of tin afforded by the nitric acid solution is in the proportion of about 150 parts from every 100 parts of the metal tin, § 6, exper. 1st—9th. According to this datum the proportion of tin in the old metals is in the following proportions, or nearly so. 1. The sauce-pan contains of tin a little more than 14 per cent.; that is about 1 part of tin and 6 of copper,—2. The spear-head contains 14 per cent of tin; that is, somewhat less than 1 part of tin and 6

of copper.—3. The celt, N° 3, a little more than 12 per cent. of tin; that is, about 1 part of tin, and $7\frac{1}{2}$ parts of copper.—4. The lituus, nearly the same proportions of tin and copper as the celt, N° 3.—5. The scabbard, a little more than 10 per cent. of tin; that is, about 1 of tin and 9 parts of copper.—6. The celt, N° 1, a little more of tin than 9 per cent.; that is, about 1 of tin and 10 parts of copper.—7. The celt, N° 2, the same proportions of tin and copper, as in the celt, N° 1.

6. The last 2 conclusions are confirmed by the exact correspondence between the ancient metals and the allays of copper by tin, in external and obvious properties, § 2 and § 6; in specific gravities, § 3; and in chemical properties, § 5 and § 6. Allays of 5 to 18 parts of copper with 1 part of tin can generally be distinguished from such allays with the addition of a very small proportion of the other metals; by the colour of their polish, the colour and texture of their grain, their strength, their hardness, their malleability, and specific gravities; without the aid of chemical analysis. It is worthy of remark, that these allays of copper with tin are evidently different, in their colour and grain, from such allays with the addition of even $\frac{1}{40}$th of their weight of zinc, exper. 13th; and also from copper allayed by $\frac{1}{40}$th of its weight of zinc, exper. 14th.

The similarity of the properties of the ancient metals, and of the allays of 6 to 12 parts of copper with 1 of tin, is very evident. But with smaller proportions of tin we find the allays are softer, and the grain of their fractures more open than the ancient metals, experiment 1—4.; and with larger proportions of tin we find the allays harder, more brittle, paler, and closer in texture than the ancient metals, experiment 8—11. It is right, however to remark, that the property of hardness of the allays of copper by tin is, cæt. par. as the proportion of tin, or nearly so; which is not the case with some of the ancient metals; for the spear-head and sauce-pan contain rather more tin than an equal quantity of the lituus, § 5, (c), which is much harder than they, § 2. (c); and the spear-head and sauce-pan are nearly as soft as the celts, N° 1 and N° 2, § 2, (c), which contain the smallest proportion of tin of any of the old metals, § 5, (c).

The grain also of the fractures of the spear-head and sauce-pan, before melting, is much coarser, or open, than those of the other ancient metals which contain a smaller proportion of tin, § 2, (b): but it appears from the synthetic experiments that the grain becomes finer as the proportion of tin is increased, § 6, exper. 1—12. To account for these inconsistencies I must remark, that a minute quantity of extraneous unmetallic matter may be contained in metals; so minute indeed as to elude the most rigorous analysis, or at least not to be discoverable by the ordinary modes of examination; and which also may not render the metal at all unfit for most of the uses to which it is applied. For instance, good malleable iron may contain carbon, and even phosphate of iron or siderite; and metals in general may contain a very small proportion of oxygen, and yet be as useful as the purest metals. The best English copper is accounted less tough and ductile than Swedish copper. The purest English tin crackles when it is bent or chewed, but pure

Malacca tin has not this property. These differences of properties most probably depend on some extraneous matter; but in so small a proportion as to have hitherto eluded the research of analysis.

In the case before us, it is probable that a very minute proportion of extraneous matter was present in the spear-head and sauce-pan, especially as they were made of cast metal; which might be less hard, and less compact in texture, than an allay of pure metals, containing a smaller proportion of tin to the copper, and yet the allay might be less brittle than the cast metal. This extraneous matter may be oxygen, or sulphur, or earth, though in an imperceptible quantity, introduced during the fusion. The lituus is harder, and not more brittle than the spear-head and sauce-pan; though it contains less tin. It was made of a plate of metal which had been much hammered, and must therefore either originally have been made of purer metal than the spear-head and sauce-pan, or have been rendered purer by hammering. Perhaps metals in general are rendered purer, more uniform in texture, and more dense, by re-melting, than they were immediately after casting from the ore; or in the case of steel immediately after cementation; or in the case of allays after the fusion by which the union was effected. Accordingly, cast iron is rendered less brittle by repeated fusion; Mr. Huntsman's cast steel is made by merely re-melting steel which had been manufactured by cementation; and Mr. Mudge's speculum metal, an allay of copper by tin, was not uniform and sufficiently compact till it was re-melted. The specific gravity of the sauce-pan, and spear-head, was particularly increased by fusion, § 3, 2, 3; and their texture was rendered more uniform and compact, § 2.

The specific gravities of the ancient metals correspond, as nearly as should be expected, with their composition found by analysis; and agree sufficiently with the synthetic experiments, § 3 and § 6. I did not find that the specific gravity of the same metal, under known circumstances alike, was so nearly the same in all cases as is stated by most writers. In the preceding experiments, different parts of the same ingot varied more than is commonly supposed in point of specific gravity. The specific gravities of the ancient metals, after melting, varied between 8.5 and 8.8, or nearly so; and the specific gravities of the allays of 3 to 20 parts of copper with 1 of tin varied between about 8.5 and 8.9. These great specific gravities seem surprizing, because that of tin is only about 7.2, and of copper ingot about 8.420. But of all metallic combinations that of copper with tin produces perhaps the greatest increase of density. Aristotle made this observation long since *, and the fact is familiarly known to manufacturers of bell-metal. But it does not appear that the increase of specific gravity is so great as it is stated by Glauber. According to him, if 2 balls of copper and 2 balls of tin of the same dimensions be melted together, the compound will afford scarcely 3 balls of the same dimensions as each of the 4 balls; and yet the 3 balls will weigh as much as the 4 balls. —" Funde prædictos globulos in unum . . . . . . iterum effunde mixturam liquefactam in typum globulorum primorum, et non prodibunt iv sed vix iii numero glo-

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* Aristotle, ΠΕΡΙ ΓΕΝΕΣΕΩΣ ΚΑΙ ΦΘΟΡΑΣ ΤΟ Α. Κεφ. ι.—Orig.

buli, pondere iv globulorum reservato."—Glauber, de Furnis, pars iv. p. 67. 8vo. 1651.

The specific gravity of the allays of copper by tin, and the following experiment, show that the contraction in the dimensions of these 2 metals on combination, cannot be so great as stated by Glauber. I made 2 ingots of tin and 2 of copper, of nearly the same figure and dimensions. The specific gravity of the tin was 7.233, and that of the copper was 8.594. The absolute weight of these 4 ingots was 1730 grs. On combination by fusion, the compound afforded 3 ingots and $\frac{1}{3}$ of an ingot, of the same dimensions as the orginal ones; but those weighed only 1640 grs.; 90 grs. being wasted and adhered to the melting pot. The specific gravity of one ingot of this metallic combination was 8.340; and of another, 8.4. Consequently, after making the most reasonable allowance for the errors of the experiment, the contraction could not be $\frac{1}{4}$th of the sum of the bulks of the metals before fusion, according to Glauber; but it might be about $\frac{1}{8}$th.

7. I next observe, that the proportions of tin found in the ancient metals consist with the uses for which they were made. The principal uses of the allay of copper by tin are, to render copper less oxydable by water, or atmospheric air; to give hardness; to render it sonorous; to render it more fusible; to produce a close texture and whiteness for reflecting light; and to render copper less tough and clingy, or as the workmen say claggy.

Copper allayed with one of the smaller proportions of tin by manufacturers, is metal of which guns or cannon, improperly called brass guns, are made. Different proportions of these 2 metals are used at different manufactories; but I believe that this gun metal seldom contains less than 1 part of tin to 12 of copper, nor more than 1 part of tin to 9 of copper. Here as much strength, as is consistent with the preservation of the figure of the instrument during its use, is required: and if more tin were added, the gun would be liable to be fractured by the explosion; and if less were added, it would be liable to be bent.

Copper allayed with a somewhat larger proportion of tin than in gun metal in general, affords a metal sufficiently hard and strong for chopping tools, for many useful purposes. Of such proportions, namely, about 8 or 9 parts of copper and 1 part of tin, there is very little doubt all the ancient nations, who were acquainted with the allays of copper by tin, generally made their axes, hatchets, spades, chizzels, anvils, hammers, &c. These metals, united in these proportions, I believe would afford the best substitute known at this day for the instruments just mentioned, now commonly made of steel or iron. Accordingly, before the art of manufacturing malleable iron from cast iron was known at all, or at least practised extensively, that is, till within these last 4 or 500 years, the allays of copper by tin must have been very generally employed. The celts may be considered as specimens of the kind of metal tools in general use before the art of manufacturing iron in the manner just mentioned was discovered: for, as hath been remarked in a former part of this paper, the celts seem to have been generally neither more nor less than metal heads of hatchets, and axes, or other chopping tools. And it is no small confirmation of this opinion, that by analysis and synthesis we have found those metals to contain, in perhaps most instances, the proportions of tin which renders them most fit for the uses to which they were applied. This proportion being considered to be about 1 part of tin to 9 parts of copper.

Copper allayed with a larger proportion of tin than is generally contained in celt metal; that is, with $\frac{1}{6}$th or $\frac{1}{7}$th of its weight of tin, is fitter for cutting instruments, and piercing, boring, and drilling tools than celt metal; because it is harder, takes a finer edge, and yet is sufficiently strong on most occasions; nor do we possess at this day any metal, that I know, which is so fit for knives, swords, daggers, spears, drills, &c. as this allay, except iron and steel. The spear-head contains tin in the very proportion here mentioned; and if the metals had been pure, it would perhaps not have been possible to have made this instrument of any other metals, which were so proper, and at so small an expence. The sauce-pan also was made of allay of copper by tin in the proportions last mentioned; but as this instrument is sufficiently hard with less or without any tin, there seems to be no use from the addition of it. We may conjecture indeed, that as the sauce-pan was made of cast metal, the tin was made for the purpose of rendering the copper more fusible, and thus also for more easily casting forms of it. Perhaps also the tin was added to render the copper less readily oxydable, and for the colour of this composition.

Copper united with the proportions of tin last mentioned is very sonorous; but it is rendered much more so by still larger proportions of tin. I apprehend the sonorous property increases as the proportion of tin is increased, within certain limits; provided the allay possess sufficient strength not to be fractured by the necessary impulse. But as the brittleness increases with the increased proportion of tin, I believe not more than 1 part of tin is added to 3 parts of copper to compose the most sonorous metal which is manufactured, namely, bell-metal.* But this allay is too brittle to be beat out into a plate for making a trumpet; and accordingly the lituus, which has been made of hammered metal, contains only about 1 part of tin and 7$\frac{1}{2}$ parts of copper.

Copper is united with tin for the purpose merely of becoming more fusible, and of continuing longer fluid, or cooling more slowly while passing from the melted, or fluid state, to the solid state. Such metal is used for making statues, and casts of figures in general, and is called statuary metal,† and sometimes bronze. The proportions of the 2 metals are various; probably according to the colour proposed, and the size and figure of the cast; as well as on account of the price of the metals.

* The proportion of tin varies in bell-metal from $\frac{1}{3}$ to $\frac{1}{5}$th of the weight of the copper; according to the sound required, the size of the bell, and the impulse to be given.—Orig.

† The Greeks and Romans consumed vast quantities of copper in casts of figures. They added not only tin but lead to the copper. The proportions given by Pliny are 1 part of a mixture of equal quantities of lead and tin to 15 parts of copper. The use of the lead I do not understand, if it was not to save expence.—Orig.

A small proportion of zinc is sometimes added to allays of copper by tin; on some occasions, on account of colour, on others perhaps to render the copper still less oxydable and more fusible; and on other occasions, as I have found on inquiry, it is added from erroneous theory, or mere caprice. No one could tell me the use of zinc, which in some manufactories is added, in making gun metal.

Tin might be used also to render copper less clingy, or more brittle, for the purpose of writing on it, or marking it with lines and figures, as on mathematical instruments: but the allay with zinc is now preferred for these purposes, as I suppose on account of its being less hard than allays with tin, and yet sufficiently brittle; on account also of its golden colour; and also on account of its being still more difficultly oxydable by air and water.

The scabbard metal contained a rather larger proportion of tin than the celts, N° 1 and N° 2; namely, being $\frac{1}{10}$th of its weight. Copper allayed by zinc would have been sufficiently hard and strong, and on other accounts preferable to the allay of copper with tin. This is however one proof of the extensive use of this last composition among the ancients.

The art of allaying copper with an earth-like substance; which, within a little more than the last 50 years only, we have learned was an ore of a metal, namely, zinc; was known perhaps in the time of Aristotle, and certainly of Pliny; for the latter informs us, that this composition resembles orichalcum; and after his time it was called orichalcum. Thus the native and factitious orichalcum were confounded. The ancients do not appear to have used the allay of copper by zinc, except for mere ornaments, to resemble gold. It is much more extensively employed by the moderns, and the allay of copper with tin is less extensively used: 1st, because the former is cheaper than the allay of copper with tin; 2dly, because it is now generally understood that it preserves its colour longer; 3dly, because it is easier to work it into various forms, and especially for philosophical instruments; few of which were probably made by the ancients.

The composition in common use, which contains the greatest proportion of tin, is called speculum metal The requisites of this metal are compactness, uniformity of texture, whiteness, sufficient strength to prevent its cracking in cooling, and to bear polishing without breaking. Mr. Mudge found the whole of these properties attainable in the greatest degree, by a little less than 1 part of tin with 2 parts of copper. But for very large instruments, such as the 40-feet telescope of Dr. Herschel, the proportion of tin must be less than in small instruments, on account of the property of brittleness. The compound of equal weights of copper and tin is so brittle, that it is not easy to conceive to what useful purpose it can be applied. The allays of tin with copper, by which I mean those compounds of copper and tin in which the tin is in greater quantity than the copper, I believe, have not been examined. It is said, indeed, that tin allayed with a very small proportion of copper has been employed for tinning, to save much of the expence of tin; for a much thinner coat of this compound can be spread than of tin.

8. The next conclusion is founded on the experiments of the allays of copper with steel. It appears that copper may be united to steel without the intermede of any other metal; for a perfectly homogeneous compound was produced by melting 10 parts of copper with 1 of steel, § 6, exper. 19. As this allay was not harder than that of copper with $\frac{1}{20}$th of its weight of tin, and as it did not appear that a compact and uniform malleable metal could be composed of 2 parts of steel with 2 parts of copper, § 6, exper 20, I thought it unnecessary to make any more experiments with different proportions of copper and steel. For, 1st, granting that the allays of copper by steel are as hard, strong, and malleable as those of copper by tin, it is utterly improbable that the ancients should have used steel to harden copper; on account of the great scarcity and high price of steel comparatively with tin; and also on account of the difficulty of uniting copper with steel, and the facility of uniting copper with tin.

2dly. It appears that no allays of copper by steel can be made, which possess the hardness, strength, and malleability required; but which required properties we obtain by combinations of copper with tin, and with which most indubitably the ancients were well acquainted. Count Caylus has indeed told us, that the ancients had 2 methods of hardening copper; namely, by cementation, and by allaying it with iron. The first method he has not explained; nor is any method known of hardening copper without addition, except by hammering it; which it is well understood cannot produce the required hardness. As to the other method by allaying with iron, I think myself warranted in refusing the Count's single vague evidence; and in admitting the evidence of other plainly decisive experiments; which consist also with reasoning and analogy.

Philological, and antiquarian writers, in giving an account of the copper arms and utensils of the ancients, (as they found them much harder than copper, and that they were used for purposes to which copper would have been quite unfit; and as they saw that the ancients commonly used copper on most of those occasions in which we now use iron or steel); were led to imagine, that in ancient times there was an art understood of tempering copper, which had been subsequently lost.* If, instead of feigning such an hypothesis, these writers had examined by analysis the ancient implements which fell under their observation, I cannot doubt that they would have unravelled the mystery. Count Caylus himself had a glorious opportunity of ascertaining the composition of ancient copper instruments, when the 7 swords and hollow wheel were found at Genzac in 1751. If he had made but 2 adequate experiments, one to detect iron, and the other to detect tin, he would

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* "It appears, says Dr. Lort," in his paper on celts, "that the ancients had an art of tempering and hardening brass to a greater degree than is done at present, or perhaps than is necessary to be done."—Archæol. vol. 5, p. 187. With reluctance I must observe, that such an experienced inquirer as Dr. Priestley falls into the error of antiquaries, in asserting, that the ancients had a method, with which we are not well acquainted, of giving copper a considerable degree of hardness, so that a sword might be made of it with a pretty good edge. But Pauw tells us, that the Americans were in possession of the secret of giving a temper to copper equal to steel.—Orig.

have had a much better foundation for reasoning than that of a mere hypothesis, however ingenious and learned.*

There is not the least reason to suppose that the ancients added iron or steel to increase the hardness or strength of the allay of copper by tin; nor does it appear from the experiments with this mixture, exper. 17, and 18, that any advantage is to be expected from this addition; at least not for cutting instruments. I cannot confirm the opinion above delivered, that the common metal of the ancients for cutting instruments was the allay of copper with tin, by the experiments of other persons, excepting those of Mr. Dizé, in the Journal de Physique for 1790, p. 272. He had only 25 grs. of an ancient dagger to operate on. This small quantity however afforded tin and copper, as appeared on dissolution in nitric acid. But Mr. Dizé made several analytical experiments on 8 different sorts of coins, Greek, Roman, and Gallic. It appears from these experiments that these coins contained from $\frac{5}{12}$ of a grain to $24\frac{1}{3}$ grs. of tin in 100 grs. of each of the old metals. And it appears that these coins contained no other metal but copper and tin.

From the preceding experiments and observations we learn, that tin was infinitely more valuable to the ancients than it is to the moderns: without this metal, it is not easy to conceive how they could have carried on the practice, and invented the greater part of the useful arts. Tin was even of more importance to the ancients, than steel and iron are to the moderns; because allays of copper by tin would afford better substitutes for steel and iron, than any substitutes which the ancients, in all probability, could procure for allays of copper by tin. We see also the importance of Britain, in times more remote probably than those of which we have any record or tradition; being probably the only country that furnished the metal so necessary to the progress of civilization. If Mr. Locke had been acquainted with the properties of the allays of copper by tin, and of their extensive use in highly advanced states of civilization among the ancients, he would have known that iron was not the only metal by the use of which we are in possession of the useful arts, nor consequently is it "past doubt, that were the use of iron lost among us, we should in a few ages be unavoidably reduced to the wants and ignorance of the ancient savage Americans." In the barbarous state of its inhabitants, this island was known to the civilized nations of Europe, Asia, and Africa; and denominated in 2 of the most ancient languages, namely, the Phœnician and Greek, by terms which denote, the land of tin; for such, according to Bochart, is the import of Britain, a corruption of Barat-Anac, or Bratanac; and there is no doubt of the meaning of the Greek word Cassiterides.

I do not mean by these observation to represent, as authors in general have done, that the ancients were not acquainted with the art of manufacturing iron, or steel, till long after the common use of copper, or that they did not know the superior properties of iron and steel: on the contrary, if this were the proper place, I could show that iron, or at least steel, was manufactured, and its useful properties under-

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* See Recueil d'Antiq. Egypt. Etrusques, Greques et Romaines, tom. 1. 4to, 1761.

stood, as early as copper was known. But steel was got anciently from those ores only which yield it in a malleable state; as it is probably obtained at this day in India, and called wootz; and as it is also obtained in the northern Circars, and likewise by the Hottentots. As steel was the only state of iron anciently manufactured, it was too scarce, and much too dear for general use; and hence the extensive use of allays of copper by tin, the best substitutes for the malleable state of iron and steel.

SECTION II. OF THE STEEL ARMS.

§ 1. *A few miscellaneous observations.*—Of the ancient steel or iron arms and utensils in Sir Jos. Banks's collection, 4 articles only were selected for examination. One of these was the steel sword within the copper scabbard, described in sect. 1, § 1, 4. and represented by fig. 4.

1. A sword, fig. 8. Of a number of these weapons in the collection, this was the smallest. The great difference in their size and weight, it is observed, was probably intended to give every man, according to his strength and mode of fighting, an opportunity of suiting himself. The figure of the blade is particular, and seems very well contrived. The hollow in the middle of each side does not extend more than ⅔ds from the guard to the point; and terminates in a ridge, which must give great support and strength to the cutting part. The pommel and guard had been tinned, and part of the tin coating still remains on them. This weapon therefore affords a specimen of the mode of tinning iron practised by the ancients. The blade seems to have been varnished by black matter, which remains very brilliant and smooth. On one side is the inscription + BENVENUTUS +, and on the other + ME FECIT +, perfectly legible. From the crosses, we may conclude that the maker was a Christian; and from the name, that he was an Italian. The writing is in mixed characters, but it is probable that the artist exercised his trade of a sword cutler in the northern parts of Europe. We cannot however determine whether it be Danish, or Saxon. It was found in the river Witham, with a large quantity of other arms, in the neighbourhood of the scite of Bardney Abbey; and was brought up by an eel-spear, by a man who was fishing in that river, near Kirksted Wath, in 1788.

2. An axe. Its form is evident from the fig. 9. It was snipt a good deal, and several holes were worn in the middle, otherwise it was in a state of good preservation. It was found, with other axes, chopping instruments, and carpenters' tools, in the river Witham, in 1787 and 1788. This axe perfectly resembles that carried by the lictors in their fasces, in basso-relievos. Its form induces one to suppose, or indeed to believe, that it was made for parade rather than use; its edge being very thin, and immediately above it the blade being thicker; but behind the thick part, exactly where the strength of an axe ought to be placed, it is thinner than in any other part. It was therefore not well calculated for chopping. The weight of this axe was somewhat more than 1¼ lb. Its length from eye to edge was 7 inches, and the breadth was about 6 inches.

3. A dagger; its form is represented by fig. 11. It was made with great inge-

nuity and skill for answering the main purpose of it, that of piercing armour. It was found, together with another dagger, in Barling's Eau, near Short Ferry, in 1788.

4. A sword in its scabbard, fig. 4. I could not by any force draw it out of the scabbard. On breaking the scabbard, I found the sword destroyed by rust; but the guard and hilt were still in a metallic state, and the pommel had been broken off. I have already described this instrument in the account of the brass arms.

§ 2. *Chemical properties.*—1. The sword, fig. 8. (a) Being filed and polished, it was of the colour of steel. The blade was bent considerably before it was broken; and could not be broken without considerable force. Comparatively with soft steel, or malleable iron, it possessed little malleability. Under the hammer, file, and drill, it felt as hard as hardened steel. The snipt edges were hard, and strong enough to saw asunder the celts described in this paper. Its fractured surfaces showed a silvery kind of open grain, like steel which has been hardened by plunging it, when white hot, in cold water. The pommel and guard were much more malleable, and much less hard than the blade.

(b) The blade, when red-hot, was malleable, but much less so than our common steel. On cooling gradually it became less hard than before; but it was not so soft as our common annealed, or distempered steel. By plunging the distempered piece of the blade, when white hot, in cold water, it was restored to its original hardness. By plunging the pommel and guard when white hot in cold water, they were rendered much harder; and by again igniting them, and letting them part with their fire gradually, they became as soft as they were originally. (c) The specific gravity of the middle part of the blade, after filing off the coating, was 7.476.

(d) The dissolution of 300 grs. of the blade in sulphuric acid and water, yielded nearly the same quantity of hydrogen gaz as an equal quantity of our steel affords. During the dissolution the mixture became black, and a black froth appeared on its surface; and, after repose, there was a deposit of black matter. The solution, made boiling hot, was poured on a paper filter; and being filtrated, the filter was edulcorated, by repeatedly pouring on it pure water. The paper filter was stained black by the solution, and there was a small deposit of black matter in the apex of the cone of the filter. This black matter was carbon, in about the same proportion as our steel affords by the same treatment. The filtrated solution, on evaporation, was found to contain nothing but sulphate of iron.

(e) A little nitric acid being dropped on the polished surface of the blade; and also on the pommel and guard; a black spot was produced on the parts wetted. (f) The tinned part of the pommel being just wetted with nitric acid, it became white.

2. The axe, fig. 9. (a) Being polished, it appeared of almost a silvery whiteness. —It was harder than malleable iron, but was not so hard as hard steel, for it was easily filed, and bored through with the drill. It was also cut through, and the cut surface was smooth and uniform, and close, as if made of the purest metal. It bent a little, notwithstanding its form and thickness; and required a very smart

stroke with a heavy hammer to break it. The grain of the fractured part was like that of close-grained steel.—It was malleable both in its cold and ignited state.—It was almost as sonorous as bell-metal.

(b) By quenching in cold water when ignited to whiteness, it became harder, more brittle, and open grained; but it could not be made so hard as the sword, fig. 8. By igniting the piece so hardened, and letting it part with its fire gradually, it was rendered much less hard than it was originally. The artist who assisted me in examining this tool, observed that it was only made of steel for about an inch from the edge; but that the rest was iron; for he conceived it to be impossible to be all steel, on account of the eye for the wooden shaft. However, on filing different parts, and cutting the instrument, no seam could be discovered, where iron had been welded to steel; and every part appeared susceptible of induration and emollition, by the usual treatment of steel to produce these changes.

(c) The specific gravity, before hammering, was 7.802, and after hammering the same piece, it was 7.880. After ignition to whiteness and sudden quenching, the specific gravity was 7.384.—(d) 300 grs. of this metal dissolved in sulphuric acid and water, and afforded black matter and sulphate of iron; with the same phenomena as the dissolution of the sword, fig. 8, afforded. The black matter was carbon, in apparently the same proportion as was obtained from the dissolution of the sword, fig. 8.—(e) Several parts of this axe being just wetted with nitric acid, they became black spots, as is the case on so applying this acid to steel.

3. The dagger, fig, 11. (a) Being polished, it had the appearance of steel.—It was not so hard as the sword, fig. 8; but it was so very strong and tough, that it was with difficulty broken, and could be bent very considerably.—Its fractured, or rather torn surface was open grained, and crystallized,—It was more malleable when cold, than hardened steel usually is.

(b) In its ignited state it was very malleable. It was susceptible of induration and emollition, by the ordinary treatment to produce these changes in steel.—(c) The specific gravity of this dagger was 7.413.—(d) The dissolution in sulphuric acid and water afforded nothing but carbon and sulphate of iron, in about the same proportions as the dissolution of the sword, fig. 8.—(e) A black spot was produced by wetting this instrument with nitric acid.

4. The sword, fig. 4. (a) The hilt being polished, it appeared like steel. It was almost as hard as common hardened steel, and as malleable.—(b) The hilt was very malleable in its ignited state. It was hardened, but not considerably, by quenching in cold water when white hot. It was rendered softer, after being hardened, by ignition and gradually cooling.—(c) The specific gravity of the hilt was 7.647: and after ignition and quenching, it was 7.427.—(d) The nitric acid produced black spots when applied to the polished surfaces of this metal.—(e) The dissolution in sulphuric acid and water afforded nothing but carbon and sulphate of iron. The carbon was in smaller quantity from this hilt, than from the sword, fig. 8, and not more than from common steel.

§ 3. *Conclusions and remarks.*—1. It appears that all these instruments are of steel; because they consist of carbon and iron; because they are capable of induration by plunging them when ignited in a cold medium; and they are softened by ignition and gradual cooling; they have the colour, texture, hardness, brittleness, malleability when ignited, and specific gravity of many sorts of steel.

2. The sword, fig. 8, appears to be the hardest; and the dagger, fig. 11, the softest steel of the above instruments.

3. These steel instruments appear to have been tempered, at least in the parts destined for cutting and piercing.

4. The axe, fig. 9, being all steel, affords a proof that the ancients were not acquainted with the art of manufacturing soft malleable iron: nor consequently of welding it with steel; and that the only state of iron which they used, and could manufacture, was steel.

5. Though it is most probable that these steel instruments were made of steel got directly from the ore, they show that the ancients could render such steel very malleable in its ignited state; and free from extraneous matters, and particularly from oxygen.

6. The different degrees of hardness and brittleness of these instruments may reasonably be imputed to the different proportions of carbon which they contain; and to the different degrees of cold applied in tempering them; though the experiments were not made with such precision as to demonstrate the reality of these assigned causes.

7. It seems probable that the axe was tempered at a low temperature, and had been much hammered: hence its great specific gravity before hammering, and the little increase of its specific gravity by further hammering; and hence the great diminution of its specific gravity by quenching in its state of ignition to whiteness.

8. Iron and steel instruments are destroyed, commonly, by the oxygen of water, or oxygen of atmospherical air. The destruction of iron instruments is prevented by whatever prevents the union of the oxygen of these substances. On this principle the sword, fig. 8, was preserved by its varnish; but the other tools must have owed their preservation to their having been accidentally coated with earthy matter; which perhaps contained principally clay.

9. The destruction of the iron sword by oxygen within the copper scabbard; and the preservation of the part of it not in contact with the copper, is a good example of the action of copper and water united in destroying iron, the copper remaining entire. This effect of copper on the iron bolts and nails, in copper-bottomed ships, is a loss of the greatest magnitude. Fig. 10, is another sword of the same kind as fig. 8, in Sir Jos. Banks's collection.

XIX. On the Periodical Star α Herculis; with Remarks tending to establish the Rotatory Motion of the Stars on their Axes. To which is added a Second Catalogue of the Comparative Brightness of the Stars. By Wm. Herschel, LL.D., F. R. S. p. 452.

In my first catalogue of the comparative brightness of the stars, I announced α

Herculis as a periodical star. The precision of the characters introduced in that catalogue is such, that the smallest alteration in the lustre of the stars may be discovered, by a proper attention to their expressions: the variation in the light of α Herculis is however pretty considerable, and cannot easily be mistaken, when strictly compared to a proper standard. The star most conveniently situated for this purpose is κ Ophiuchi; and as I have had no reason, during the time of my observations, to doubt the uniformity of its lustre, I have made use of it in the comparisons; which seem to be sufficiently decisive, with regard to the periodical variations of the light of α Herculis. Other stars besides κ Ophiuchi have also been consulted; but the unsteadiness of their light would draw me into difficulties, which at present it will be proper to avoid. The observations are found to contain at least 4 regular changes of the alternate increase and decay of the apparent lustre of our new periodical star, deduced from a comparison of its brightness with that of κ Ophiuchi.

In order, from the table of observations, to obtain the time of the period, if we first take all the successive observations from Sept. 16, till Nov. 28, they show very clearly that the star has completely gone through all its changes. For, admitting a maximum of the light of α Herculis to have been Sept. 16, we find a minimum on Oct. 25; and a 2d maximum about Nov. 28. The period therefore is of somewhat more than 2 months duration. But as changeable stars are subject to temporary inequalities, which will render a determination of the length of a period, from a single series of changes, liable to considerable errors, we shall now take the assistance of the most distant observations. By an inspection of the table, we find again the first maximum to have been about Sept. 16, 1795; and the 4th the 14th of May, 1796. This being an interval of 241 days, in which 4 successive changes have been gone through, we obtain about 60¼ days for the duration of the period. In confirmation of this computation, the table shows that our periodical star was very faint in August 1795; bright about the middle of September; faint towards the end of October; bright the latter part of November; faint in December; bright in January, 1796; not observed in February; bright in March; faint in April; and lastly, bright again in May. This is just what should have happened according to the above determination, which, as we have seen, gives a period of 8 weeks, 4¼ days. Greater accuracy can only be obtained by future observations.

On the rotary motion of the stars on their axes.—The rotation of the fixed stars on their axes has been lately mentioned in a paper, where I could not have an opportunity to enter into the reasons why it ought to be admitted*. The rotatory motion of stars on their axes is a capital feature in their resemblance to the sun. It appears now, that we cannot refuse to admit such a motion, and that indeed it may be as evidently proved as the diurnal motion of the earth. Dark spots, or large portions of the surface, less luminous than the rest, turned alternately in certain directions, either towards or from us, will account for all the phenomena of

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* Phil. Trans. for the year 1795.—Orig.

periodical changes in the lustre of the stars, so satisfactorily, that we certainly need not look out for any other cause. Let us, however, take a review of any objections that might be made.

The periods in the change of the lustre of Algol, β Lyræ, δ Cephei, and η Antinoi, are short; being only 3, 5, 6, and 7 days respectively: those of ο Ceti, the changeable star in Hydra, and that in the neck of the Swan are long, amounting to 331, 394, and 497 days. Will not a doubt arise whether the same cause can be admitted to explain indiscriminately phenomena that are so different in their duration? To this it may be answered, that the whole force of the objection is founded on our very limited acquaintance with the state of the heavens. Hitherto we have only had 7 stars whose periodical changes have been determined. No wonder then that proper connections between their different periods were wanting. But let us now place α Herculis among the list, which is not less than 60 days in performing one return of its changes. Here we find immediately, that the step from the rotation of α Herculis to that of ο Ceti, is far less considerable than that from the period of Algol to the rotation of α Herculis; and thus a link in the chain is now supplied, which removes the objection that arose from the vacancy.

There is however another instance of a slow rotatory motion; and it is doubly instructive on this occasion. In a former paper it has been shown, that the 5th satellite of Saturn revolves on its axis in 79 days; this not only shows that very slow rotatory motions take place among the celestial bodies; but from the arguments that were brought to prove its rotation, which I believe no astronomer will oppose, we are led to apply the same reasoning to similar appearances among the fixed stars. A variation of light, owing to the alternate exposition of a more or less bright hemisphere of this periodical satellite, plainly indicates that the similar phenomenon of a changeable star, arises from the various lustre of the different parts of its surface, successively turned to us by its rotatory motion. The rotations of the sun and moon, and of several of the planets, become visible in a telescope by means of the spots on their surfaces; the remote situation and smallness of the 5th satellite of Saturn leave us without this assistance; but what we can no longer perceive, with our best optical instruments, we now supply by rational arguments. The change in the light of the satellite proves the rotation; and the rotation once admitted, proves the existence of spots, or less luminous regions on its surface, which at setting off were only hypothetical. In the same manner a still more extended similarity between the sun and the stars offers itself, by the spots that now must also be admitted to take place on their surfaces, as well as on that of the sun.

To return to the difficulty which has been started, it may be further urged, that there are some reasons to surmise that the 34 Cygni is a periodical star of 18 years return*; and that other stars seem very slowly to diminish their lustre, and may probably recover it hereafter. In answer to this, I remark that it will not be neces-

* Phil. Trans. for the year 1786, page 201.—Orig.

sary to remove objections to the rotatory motion of the stars, inferred from their very slowly changeable lustre, till they come properly supported by well ascertained facts. Many causes in the physical construction of the stars may occasion an accidental and gradual increase or decay of brightness, not subject to any regularity in its duration. But when settled periods can be ascertained, though they should be of the most extended duration, it will not be difficult to find other causes to explain them, without giving up the rotatory motion. When the biography of the stars, if I may be allowed the expression, is arrived to such perfection as to present us with a complete relation of all the incidents that have happened to the most eminent of them, we may then possibly not only be still more assured of their rotatory motion, but also perceive that they have other movements, such as nutations or changes in the inclination of their axes; which, added to bodies much flattened by quick rotatory motions, or surrounded by rings like Saturn, will easily account for many new phenomena that may then offer themselves to our extended views.

After this follows the 2d catalogue of the comparative brightness of the stars; not necessary to be here re-printed.

XX. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1795. *By Thos. Barker, Esq.* p. 483.

		Barometer.			Thermometer.						Rain.
					In the House.			Abroad.			
		Highest.	Lowest.	Mean.	Hig.	Low	Mean	Hig.	Low	Mean	Lyndon.
		Inches.	Inches.	Inches	°	°	°	°	°	°	Inch.
Jan.	Morn.	29.95	28.63	29.54	35½	25	30½	37	14	25	1.640
	Aftern.				36½	25½	31	44	20	28½	
Feb.	Morn.	30.17	28.49	13	44	28	34½	47	18	31	1.995
	Aftern.				45	29	35½	48	24½	34½	
Mar.	Morn.	29.82	28.15	30	45	33	40	47	20	36	2.101
	Aftern.				47	35	41½	54	32	43½	
Apr.	Morn.	29.71	28.80	31	50	41	45	51	36	42½	1.574
	Aftern.				54	41½	47½	62	41	51	
May	Morn.	29.99	29.18	69	64	42	52	59½	37	48½	0.404
	Aftern.				67	46½	55	74	48	60½	
June	Morn.	29.72	29.03	42	62½	50	55½	60	43	52	2.799
	Aftern.				67½	51	57	77	51	64	
July	Morn.	29.83	29.05	52	64½	55	58	62½	49	55	1.683
	Aftern.				66	56	59	82	55	65½	
Aug.	Morn.	29.82	29.13	49	68	58½	62½	69½	49	58	1.380
	Aftern.				71	59½	64½	84	60½	71	
Sept.	Morn.	29.99	29.18	63	68	56	62½	63	43	54½	0.057
	Aftern.				70½	56	65	78½	55	69	
Oct.	Morn.	29.70	28.67	17	62½	48	55	59½	39	50	4.536
	Aftern.				63½	50	56	69	48	58	
Nov.	Morn.	30.13	28.53	37	51	36½	44	48	24½	38½	1.850
	Aftern.				52	37	45	53½	32	43½	
Dec	Morn.	29.96	28.87	43	51	41½	46	52	33	42½	1.382
	Aftern.				51½	42	47	54	38	46½	
Means.				29.38			50			49	21.401

XXI. *Observations on the Changes which Blood undergoes, when Extravasated into the Urinary Bladder, and retained for some Time in that Viscus, mixed with the Urine. By Everard Home, Esq., F. R. S.* p. 486.

A gentleman, 71 years of age, in the spring 1795, found that in making water, the urine had the appearance of blood, and congealed into a solid mass as soon as received into the vessel. This complaint appeared to have arisen from the rupture of a vessel in one of the kidneys, for he had a pain in his loins, but none in the region of the bladder. He seemed to void no water, for the whole quantity which was expelled at any one time, amounting to about 4 oz. formed itself into a coagulum; next day he voided bloody water, which did not coagulate. This continued for 3 or 4 days, and then went entirely off. In the spring, 1796, he had a return of the same complaint. It came on in the evening of the 3d of April; on the 4th it was very violent; and in the afternoon there was a total suppression. A catheter was passed 6 or 7 times; but the oval holes near the end of the instrument were always filled with coagulated blood, and no urine could be drawn off. On the 5th, a larger catheter was passed, with small round holes, less likely to have the coagulum entangled in them, but no urine came away. In the evening it was introduced again, having its cavity completely lined with a flexible gum catheter, which was withdrawn as soon as the instrument was carried to the fundus of the bladder; and in this way 4 oz. of a bloody fluid were drawn off, which on exposure coagulated. On the morning of the 6th, a pint of bloody urine was drawn off; this operation was repeated 3 times in the 24 hours, and the same quantity was brought away each time.

On the 7th, the urine drawn off was less tinged with blood; and when it was allowed to stand, the upper part became tolerably clear. There was little change in the circumstances for 6 days; but on the 13th the urine drawn off was of a darker red colour, and in smaller quantity. On the 16th, the colour was more of a light brown, and after standing some time, a whitish powder was deposited; the urine drawn off in the morning on getting up, was nearly of the natural appearance, but that brought away in the course of the day, had a deeper tinge, and more of the white sediment, which evidently passed off only with the last part of the urine. On the 19th, the urine was tolerably clear, and the white sediment more completely separated, and in greater quantity. In the course of the night, while laying in bed, the patient voided naturally in many different attempts, 4 oz. of water, but could not make any when up. The urine now continued clear from any tinge, but no more passed without the catheter being introduced, till the 28th, when he again made some water naturally, but could not completely empty the bladder; on the 29th, the quantity which required being drawn off was less; and by the 5th of May he made water as usual, at which time the sediment began to diminish, and gradually disappeared. From the symptoms which have been stated, it appears that part of the blood which passed into the bladder from the kidney had remained there, and formed a coagulum, which coagulum gave a bloody tinge to the urine, and caused an inability to void it without assistance, till the coagulum was dissolved. With a view to ascertain how far this had been the case, and discover what changes the blood undergoes when placed in such circumstances, I instituted the following experiments. They were performed by Mr. Charles Grover, a very ingenious surgeon, at present house surgeon in St. George's hospital.

Exper. 1. 4 oz. of blood were drawn from the arm into a phial containing 4 oz. of fresh urine, and the phial was kept in the temperature of the human body; in 15 minutes the whole mixture formed a uniform firm coagulum, and appeared wholly composed of blood. This experiment was made to ascertain the probable time the blood would take to coagulate in the bladder.

Exper. 2. 6 oz. of blood were drawn from the arm into 6 oz. of fresh urine; in 15 minutes the whole mass became one solid coagulum. In 7 hours, 6 drs. of clear fluid were separated from it; this was poured off, and the same quantity of fresh urine was added; after standing 9 hours it was poured off; some red globules were mixed with it, but sunk to the bottom undissolved. The coagulum had fresh urine added to it 3 times a day, the former urine being previously poured off, and allowed to stand some hours for examination. For the first 5 days the coagulum appeared to undergo little change, except becoming smaller in size, and the urine poured off from it was tolerably clear, but on standing deposited a dark cloudy sediment. On the 6th day, the urine, when poured off from the coagulum, was of a dark red colour, and deposited a greater quantity of a dark coloured sediment, but on standing became tolerably clear. On the 9th day, the coagulum was reduced to the size of the original quantity of blood drawn from the arm. On the 13th day, the size of the coagulum was a good deal reduced; the urine poured off from it was still more tinged with the red globules; but when allowed to stand, the upper part became clear, and free from the red tinge, and the sediment had the appearance of a whitish powder. From this time the quantity of white sediment increased, and the size of the coagulum diminished. In its decrease from this period the loss was from its external surface, and nearly equally all round; what remained appearing like the nucleus of the original coagulum. On the 25th day, it was of the size of a large cherry, and on the 29th it entirely disappeared. Some red globules were very distinctly seen in the sediment along with the white powder. To see how far the changes the blood had undergone in this experiment depended on the peculiar properties of the urine, the following experiment was made, with blood and common water.

Exper. 3. 6 oz. of blood were drawn from the arm into 6 oz. of water. In a ¼ of an hour, the whole became one solid coagulum. In 12 hours, 6 oz. of a clear water, of a bright red colour, were separated, nor did it on standing deposit any sediment. This coagulum had fresh water added to it twice a day, and what was poured off was allowed to stand for examination. The coagulum on the 2d day began to break; on the 5th had a putrid smell; and in 18 days was almost entirely dissolved. The water poured off was of a bright red colour from the beginning to the end of the experiment, in consequence of the red globules being dissolved; it had a very offensive smell, but never deposited any white sediment; the coagulating lymph dissolving from putrefaction. As it is evident, from the result of the last experiment, that the coagulum remaining so long undissolved in the 2d experiment depended on its being mixed with the urine, I was desirous of knowing whether it was the urine incorporated with the coagulum, or that which surrounded it, which produced this effect. To determine this point I instituted the following experiment.

Exper. 4. 4 oz. of blood were drawn from the arm into a cup, and allowed to coagulate; 4 oz. more were drawn into a separate cup. From each of these equal portions of coagulum, at the end of 3 hours, 1 oz. of serum was separated, and poured off. To one of them fresh urine was added: to the other common water. The urine and water were changed night and morning. The water was tinged of a bright red colour throughout the whole experiment, and deposited no sediment. On the 8th day the coagulum was rather looser in its texture. On the 13th day it began to break, and by the 20th day it was nearly dissolved. The progress corresponding with that of the coagulum in experiment 3. The urine the 2d day of the experiment was clear, but the bottom of the basin was covered with red globules, undissolved. On the 5th day, the urine poured off was tinged of a bright red colour; similar to the water taken from the other coagulum; and after standing some hours a white sediment was deposited. On the 13th day it was looser in texture, and more dissolved than the coagulum in the water. It continued to tinge the urine of a bright red colour, and what was poured off deposited a white sediment in greater quantity. On the 18th, the coagulum was nearly dissolved; so that the coagulum immersed in the urine dissolved two days sooner than that in the water.

From this experiment we find, that it was the urine incorporated with the coagulum in experiment 2, that prevented the red globules from dissolving, and preserved the coagulum for so long a time, since these effects were not produced by urine while simply surrounding the coagulum. If we compare experiment 2, with the result of the case, they agree so entirely, that it leaves no doubt of the process carried on in the bladder being similar to that which took place out of the body. The patient was unable to make water for 24 days, though the passages readily admitted, during the whole of that time, an uncommonly large instrument, which could not have been the case had there been any obstruction in them; for 6 days more he voided it with difficulty, but afterwards made water very well. The coagulum out of the body was reduced in 25 days to the size of a cherry, and in 4 days more it was completely dissolved. The patient's urine became darker, from the red globules mixing with it in 9 days. In the experiment this took place in 5 days. The white sediment was first observed, in both instances, about the 12th day; it continued to be deposited till the patient got well, and to the end of the experiment.

That the blood is capable of uniting with a quantity of urine equal to itself, so as to form a firm coagulum; that the red globules do not dissolve in a coagulum so formed; that an admixture of urine prevents the blood from becoming putrid; and that the coagulating lymph breaks down into parts almost resembling a soft powder, are facts which I believe to be new;—they may however have been before ascertained, though I have not been acquainted with them. They are certainly not generally known, and one object of the present paper is to communicate them to others. These facts, considered abstractedly, may not appear of much importance; but when compared with what takes place in the living body, and found to agree with the process the blood undergoes in the urinary bladder, they become of no small value, since they enable us to account for the symptoms that occur in that disease, and lead to the most simple and effectual mode of relieving them.

XXII. On the Fructification of the Submersed Algæ. By Mr. Corrêa de Serra, F.R.S. p. 494.

The light which the prevailing spirit of inquiry and observation has thrown on the means of reproduction allowed by nature to vegetable beings, is not yet equally diffused over all of them. Those whose simpler organization seems, when examined, to want some of the parts which we are accustomed to consider as essential to generation, continue to the present moment more or less involved in darkness; and their fecundation, and means of reproduction, are still objects of doubt and inquiry. Among them the fuci, ceramiums, ulvæ, confervæ, all submersed algæ, are perhaps in the number of the less illustrated. It is probable that their peculiar way of living, which requires from nature a particular modification in the parts destined to reproduce them, as well as in the means of performing this operation, has been the principal cause of the perplexity of naturalists on this subject. They have either sought for things in their ordinary form, which nature furnishes to these plants under a different one, adapted to their circumstances; or they have thought that she deviates from her usual ways, when she only makes use of her stubborn versatility, enforcing the execution of her general plan, by the means which at first sight seem to make her deviate from it. In the present memoir I shall endeavour to ascertain what parts of these plants perform the sexual functions: and, in order to be clear, I will first relate in a few words, what has been observed and believed on this point, and proceed afterwards to the exposition of the opinions which observation and the strictest analogy induce me to hold on this subject.

Reaumur was the first naturalist who bestowed a proper attention on the fructification of the Fuci. Two elaborate memoirs of this great man are to be found, in the Parisian Transactions, for the years 1711 and 1712, in which he endeavours to persuade us, that the vesiculæ filled with small grains are the female part of the fructification in the fuci, and the filamentous hairs, which are found in different parts of the frons, the male organs. He examined 11 species of fuci, in 8 of which he found grains, and only in 6 the filamentous hairs. It is unlucky for his opinion, that these hairs have no antheræ, and still more unlucky that their existence has no relation at all to that of the vesicles which bear the grains, for they are persistent through all the life of the plant, without any remarkable alteration. Their situation besides is very unfit for the fecundation of the grains, except in the Fucus elongatus. Notwithstanding the weight of these objections, which he did not conceal, this otherwise sensible naturalist tried to the last to support by hypotheses, what he could not fairly prove by observations. His great name, joined to the general ascendancy which the sexual system gained a little after all over Europe, gave however a common currency to his opinion; and it was received, though in a wavering manner, by Linnæus himself, and, what is more surprizing, by the last of the Jussieus. These great men indeed gave it as the prevailing opinion of the day, not confirmed by any sanction of theirs. This was not the case of less profound botanists; for they, as the fashion then was, attempted to see stamina and antheræ, like the common ones, wherever they had not been observed before. I deem it unnecessary to stop a moment to consider the multitude of supposed stamina, which Donati, Griselini, and others, imagined they had found in Fuci, and Ulvæ, because it is at present clearly evinced that these fancied stamina are only organs of nutrition.

Cooler observation and reflection exploded at length all these dreams; but Gmelin, and Gærtner, the two greater among the naturalists who followed a different way of thinking, went perhaps too far on the opposite side. Gmelin convinced both by reason and observation of the inutility of the Reaumurian antheræ, and writing at a time when the recent publications on the hydræ, and on the aphides, had made it fashionable to find examples of multiplication of organized bodies without fecundation, determined to consider these plants as in the same predicament. Every one may see, in his Historia Fucorum, the elaborate discussion by which he endeavours to establish his opinion. It dazzles at first sight, but, on a candid examination, all his arguments, when deprived of the apparatus of science which accompanies them, may be reduced to the following; namely, that since the supposed male organs are not such in reality, and no others are to be found, the small grains which act as seeds are prolific, without receiving external fecundation. We shall see as we proceed, how groundless is the supposition on which this argument rests.

Gærtner, by far a deeper naturalist than the preceding, and keeping more closely to the ways of nature, would doubtless have given us the true and simple account of the fructification of the submersed algæ, if the specious Adansonian theory of aphrodite plants, and his own ideas of the perfect seed, had not in my opinion led him astray. The grains of the ceramiums and ulvæ, and the lateral internodia of the confervæ, he excludes from the number of seeds, and believes them to be gems of a particular kind, which he calls gongyli, or gemmæ carpomorphæ, consequently not standing in need of fecundation. The grains of the fuci he judges to be true seeds; but in this case he believes that the uterus performs the male functions, and that the plants, in respect to their fecundation, are aphroditæ, having only the apparatus fœmineus, et intimam utriusque sexus sub specie singuli copulam. Both he and Gmelin, forced by phenomena which they could not help observing, have been in some moments very near to what I conceive to be the truth, but have sacrificed it to preconceived opinions.

In the last year two English botanists, to whom science stands indebted for many excellent descriptions and figures of fuci, Major Velley, and Mr. Stackhouse, treated this same matter, the first at large, the second occasionally. Both have stated, with great ingenuity and candour, the many objections which attend the existing systems, and both declared themselves not fully satisfied with the present state of our knowledge on this subject. Mr. Stackhouse indeed seems to cherish hopes of future discoveries of the male organs, in what he calls the concealed fibrous fructification, the antheræ not seeming necessary to him, nor the farinaceous pollen *. Perfectly agreeing with him, in what respects the needlessness of a farinaceous pollen, I cannot accede to the other parts of his opinion. A membranaceous loculament, containing the pollen, is the only necessary part of the male apparatus in plants; the filaments and the fibrous texture are only the pedicles of it, and very far from being necessary, as the sessile antheræ of numberless fructifications clearly prove. If a fibrous concealed structure could be esteemed of any use, it was already found by Gmelin, in the seed-vessels of the true fuci, and elegantly described by Major Velley, in the Fucus Vesiculosus, and by Mr. Stackhouse himself, in the Fucus Siliquosus; but, even when magnified, it offers nothing more than simple tubular vessels, with frequent anastomoses, very remote indeed from the nature of a male apparatus.

Having stated the leading systems on the fructification of submersed algæ, I will next submit to this Society such opinions as the phenomena I have observed induce me to have about it. All these plants are furnished with grains, which are a temporary production, and by their falling give rise to new individuals of the same species. In the true fuci they are contained in a uterus, which has a temporary existence, and for their sake only, where they have a placentation, and are covered by a testa, or coat of their own. Nobody doubts that they are true seeds. The ceramiums and ulvæ have the same grains, as means of reproduction; and the confervæ also have them, though of a different shape. What then can be the reasons why these last are to be considered as gongyli and gemmæ carpomorphæ? The only argument adduced to deprive them of the nature of seeds are the 3 following. 1st. The grains of the ulvæ and ceramiums are solitary, not contained in a proper uterus, consequently without a placentation. They are, says Gærtner, part of the medulla of their mother, and their skin is part of the maternal one. 2d. They do not, in germinating, leave any coat behind. 3d. In the confervæ, whose grains have some likeness to fresh internodia, 2 or more of them very often coalesce, but give rise to only one individual.

All these reasons require to be candidly discussed; and I hope the result of the

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* Nereis Britannica, in the preface, and page 30.—Orig.

investigation will afford us many additional motives to believe them to be true seeds. The first of these objections cannot stand the test of close examination. The grains of the ceramiums, like those of the true fuci, fall at a proper period, which Gærtner calls senium, but which others will call maturity. If gently squeezed, they come forth from the little cavity where they are formed, and which they must leave when ripe. They come forth whole, and disengaged from the mother, and from every part of the frons; they have therefore a skin of their own. They are contained in a small uterus, proportionate to their size, which is of a temporary existence, and for them alone; where they are no doubt affixed by some placentation, from which, when they come to maturity, they are disengaged and fall. If we add to these considerations, that of their existing there enveloped in a soft juicy substance, all their difference from the seeds of the true fuci wholly disappears *; and a strong probability arises, that Gærtner's observations were made on dry specimens, as well as with a mind not only impartial to his preconceived theories.

The 2d reason is of a more specious nature, and requires serious attention. Animals are divided into oviparous and viviparous, and a generally received comparison points out the seeds as the eggs in plants, and the gems as correspondent to living-born fœtuses. We cannot conceive, says Gærtner, an egg where the animal, when coming forth, does not leave the shell behind; and in like manner we cannot conceive a seed where the coats are not left behind in the germination. The grains of the ulvæ, ceramiums, &c. according to him, do not leave any coat when they germinate, and are consequently gemmæ carpomorphæ. Every candid naturalist will easily acknowledge, that we are not possessed of observations sufficiently decisive to enable us to speak dogmatically, on phenomena so little obvious as the germination of these grains. But I will not contest the fact, I will only examine the principle. This general rule, of judging whether these grains be seeds or gems, by their leaving their coats in the germination, or not, is contradicted by nature, both in the instance of gems, and in that of eggs. All gems, properly so called, throw off their scaly hybernacula in the act of germinating. On the contrary, the eggs of frogs and toads leave no coat at all in their hatching, because they are possessed of none. Their very viscous albumen answers, in such an element as water, all the purposes the testa accomplishes in other eggs. Allowing Gærtner the exactness of his observation concerning our plants, the analogy between these submersed algæ and the aquatic oviparous quadrupeds would be striking, since both those plants and these animals are capable, from their structure, of being nourished by absorption; their embryos are hatched in the same element, and equally surrounded by a tenacious mucous substance, without any exterior coats. If the spawn of frogs be eggs, the grains of these plants must be seeds.

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* I have made mention of the ceramiums in this paper only to follow Gærtner through his objections. This genus, first made by Donati, adopted by Adanson, and Gærtner, has in reality scarcely any difference from the fuci.—Orig.

The 3d objection, very far from being an urgent one, is, I am persuaded, a capital reason to believe that the seeming lateral internodia of the confervæ, since they are capable of cohering 2 or more together, and produce only 1 individual, are true seeds, and not gems. The coherence of 2 living embryos, whether gems or seeds, may form monsters, but it is equally impossible, in both cases, that perfect individuals should regularly be formed by such coalition. Observation daily shows, that of 2 or more neighbouring gems or seeds, 1 may thrive by rendering the other abortive; but, in this case, gems never cohere, the abortive one falls. In seeds, on the contrary, not only the abortive coheres to the thriving one; but this abortion happens oftener in the several species of plants, in proportion as the seeds, by their situation, are apt to cohere. In some genera it is even a regular proceeding of nature, as in the dalea, lagœcia, hasselquistia, sapindus, ornitrophe, &c. These objections having been I hope satisfactorily answered, I do not hesitate to consider these grains of the submersed algæ to be, what obviously they seem, their effective seeds. The figure, formation, and temporary fall of these bodies, would never have left any room for the above doubts, if their fecundation had been easily accounted for. This point we must now proceed to investigate, and examine whether the mucous substance which surrounds these seeds can be considered as true pollen.

Pollen is by its nature immiscible with water, and specifically lighter than that fluid. In the aquatic plants, which have a farinaceous pollen, the buds of the flowers emerge from the surface of the water, and the fecundation is performed in the open air. The phenomena attending the blossom of the potamogetones, myriophylla, vallisneria, &c. are too well known to require a particular mention. In some aquatic plants, whose flowers have not the faculty of emerging from water in the period of fructification, but still are endowed with farinaceous pollen, nature has taken every precaution to defend it from that element. The flower of the zostera is situated, and its fecundation happens, in the interior cavity of the stem, which opens itself afterwards to let loose the fecundated seeds. The concave bases of the leaves in the isoetes, closely adhering to each other, and perhaps more so in the act of fecundation, forbid the entrance of water to the minute flowers situated within them. In the pilularia, and marsilea, whose flowers are exposed to inundations, the fecundation is performed in perfectly closed vessels. Even in plants living in the air, nature employs numberless well known contrivances, to shelter the farinaceous pollen from the contact of water in rainy seasons. The pollen, to be active in fecundation, needs not always be farinaceous. In most apocyneæ it is rather a fluid; in the orchideæ it is an aggregate of solid parts, of a ceraceous appearance; in some contortæ it is found in a solid or rather viscous state. In the pilularia, and marsilea, the particles of the pollen are kept in small bags of a mucous substance, compared by Bernard de Jussieu to dissolved gum. The original state in which it is found in every flower, before the act of fecundation, is that of a mucus, but it is perfectly active even in that state, since its particles, after becoming a little dry, if put into certain fluids, are seen, by the help of the microscope, acting in the same manner as when in the state of perfect farina.

The plants whose fructification lies unsheltered under water are very few in number; and such of them as have been hitherto thoroughly examined, (the ceratophyllum and the chara), have antheræ furnished with mucous pollen, not bursting in the fecundation. From the time of Dillenius it has been observed, that the submersed antheræ of the ceratophyllum never burst, but are found whole, though the seeds be ripe *. If squeezed, they shed a soft and pulpous matter, like that which is found in unripe antheræ. Dillenius suspected that the fructification of the chara being equally submersed, its antheræ and pollen would be of the same nature with the preceding, and observations have fully confirmed the conjectures of this great naturalist. The antheræ of the chara do not burst in fecundation; its pollen is mucus: the germen has no pistillum, and is probably fecundated through its receptaculum, as there exists in each internodium, according to modern observations †, a chain of vessels which twist round the antheræ and the fruit, and in which a circulation of humours is visible, at least in the period of fructification.

If pollen therefore, under the shape of farina, be unfit for fecundation in the water; if nature has taken a particular care to guard this operation from the presence of that element; if pollen can exist in an active state under a mucus appearance; and if the antheræ of perfectly submersed flowers are nothing else than closed vessels filled with mucus pollen; what doubt can we entertain, that the mucilaginous vesicles of the submersed algæ, which contain also their seeds, are antheræ, and very appropriate to the nature and situation of these plants? An observation made by Gleichen shows more clearly the propriety of such a fructification. The pollen of any flower, when put into water, in a very short time begins to move, and its particles agitate themselves in every direction, perfectly resembling the most lively animalcula. Their activity in this state lasts some time; but if the least quantity of salt be put into the liquor, death quickly ensues, from which they never more recover ‡. This inclosed mucilaginous fructification was therefore the only one which could ensure existence to vegetables living chiefly in sea-water, with which their mucus is found to be immiscible §.

A still more urgent consideration will, I hope, determine those who may hesitate to consider the mucus of these plants as pollen, and the vesicles which contain it as antheræ. The parts of fructification, in all plants, are temporary, and their existence is relative to their particular functions, and to each other. The moment the fecundation happens is a moment of crisis: henceforth the fecundated parts proceed to grow and perfect themselves, the fecundating ones change and decay. This is a general law of nature, to which we know no exception, nor can any be easily conceived to exist. We must remark, that there is an epoch when the mucous substance in the vesicles of the fuci suffers a material alteration, but the

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* Plantæ Gissenses, page 91.

† Corti, Osserv. Microscop. Lucca, 1774.

‡ Gleichen, Observ. Microscop. page 32.

§ Gmelin, Hist. Fucorum, page 27.—Orig.

grains proceed to their perfection. In the fucus vesiculosus this change of colour and consistency in the mucus is evident to common sight. It is still more evident in the fucus selaginoideus, where the temporary bright and vivid colour of the mucus, followed by a prompt decay after that period, has struck even those naturalists who most decidedly opposed the existence of male parts in these plants; and I am confident, from the steadiness with which nature adheres to her general plans, that proper observations will demonstrate the same in every species of those submersed algæ, and confirm what the forementioned analogies induce me to think, viz. That the vesicles of all these plants, whatever be their shape, if containing grains and mucus, are to be considered as hermaphrodite flowers; the grains they contain as their seeds, and the mucous substance as their pollen.

END OF THE EIGHTY-SIXTH VOLUME OF THE ORIGINAL.

I. The Croonian Lecture. In which some of the Morbid Actions of the Straight Muscles and Cornea of the Eye are explained, and their Treatment considered. By Everard Home, Esq. F. R. S. *Anno* 1797. *Vol.* LXXXVII. p. 1.

In my 2 former Lectures, to the R. S. on Vision, I confined myself to the adjustment of the eye for seeing objects at different distances. From the attention which I there necessarily paid to the natural actions of the muscles, and the structure of the cornea, I have been led to consider the effects which a diseased state of these parts will produce on the phenomena of vision.

That I may be understood in giving an account of the diseases that arise from morbid actions of the straight muscles of the eye, it will be necessary to explain the effects which their natural actions are intended to produce; for these are not confined to the separate, or combined actions of the muscles, but also vary according to the degree of their contraction. The first and most simple of these effects is that of moving the eye-balls in different directions. The 2d is that of making the motions of the 2 eyes correspond with such a degree of accuracy, that when an object is viewed with both eyes, the impressions from the object shall be made on corresponding parts of the retina of each eye. The 3d is that of compressing the eye-balls laterally, which renders the cornea more convex, and pushes forwards the crystalline lens, to adjust the eye to near distances. Distinct vision with 2 eyes depends upon these different actions of the straight muscles; an imperfection in any one of them, as it renders the organ unfit to perform its functions, must be considered as a disease. Three different diseases occur in practice, which appear to arise from morbid actions of the straight muscles. These are, an inability to see near objects distinctly; double vision; and squinting. I shall consider each of these separately.

Of the inability to see near objects distinctly.—As that action of the muscles which produces the adjustment of the eye to near objects, consists of the greatest degree of contraction usually exerted by them, it puts the fibres into a very uneasy state; which while in health they support with the utmost difficulty, and when affected by disease are unable to sustain: under these last circumstances near objects cannot be seen at all without considerable pain, and never distinctly, the eye not remaining a sufficient time adjusted for that purpose. I cannot better explain the nature of this disease, than by giving an account of the symptoms which occurred in the following case.

A gentleman 40 years of age, naturally short-sighted, of a delicate irritable habit from his infancy, being always soon tired by exercises that required muscular exertion, had the following affection of his eyes. His sight had been very perfect till he was 19 years of age; at that time he resided in a part of the country where the ground consisted chiefly of white chalk, which produced an unpleasant glare; and his constant amusement both by day-light and candle-light was drawing, which he frequently pursued so far as to fatigue his eyes. While thus employed his complaints had their origin. The first symptoms were that of being unable to look long at any object without pain, and feeling uneasiness when exposed to strong light. The eyes were apparently free from disease, having no unusual redness, nor any purulent, or watery discharge. The plan that was first adopted for his relief consisted in lowering the system, both constitutionally and locally; but this treatment rendered him more irritable, and made his eyes rather worse than before; he therefore, after a trial of 8 years, in different means of this kind, gave them entirely up. For the next 5 years, in which nothing was done to the eyes, the symptoms appeared to have been stationary; but at the end of that period, his mind suffering from an uncommon degree of anxiety, the complaints in his eyes were evidently rendered worse; this effect however depended solely on the state of mind, for as soon as he recovered from his distress, the eyes also returned to their former state. In this condition I first saw him in 1795, when his eyes had no external mark of disease, and were moved by the muscles in every direction without the smallest uneasiness. He could look at any thing at some distance, as the furniture in the room, the passing objects, &c. with perfect ease; but whenever he attempted to adjust the eyes to near objects, the effort gave so much pain, that though he succeeded in seeing them, he was almost immediately obliged to desist. Every attempt to write or read gave so much pain, that he became unable to do either; but as soon as the strain produced by such an effort was taken off, he was at ease. His disease therefore consisted in a want of power to adjust the eyes to near objects, for a sufficient length of time to render them distinct, which of course incapacitated him from reading or writing. The cause of this disease appears to be a morbid affection of the straight muscles of the eyes, which allows them to perform all their intermediate contractions as usual, but not the extreme degrees of contraction without considerable pain.

As these symptoms have not, I believe, been before accounted for in this way, it may appear to many who have not seen similar affections of other muscles, that the present opinion is rather theoretical than practical; it will therefore be satisfactory to illustrate this disease in the muscles of the eye, by examples of the same kind of morbid action in other muscles, more within the reach of common observation. The following instances all refer to the muscles of the fore-arm and hand, employed in actions with which every one is familiar, and show that these muscles are liable to be affected in the same manner as the muscles of the eye.

A gentleman, 46 years of age, naturally of an irritable habit, which had been much increased by a long residence in the East Indies, was, about 8 years ago, in a situation of great responsibility in that country. He was much engaged in writing, and previous to the sailing of a vessel for England, had, with a view to finish some dispatches of importance, written incessantly for a great many hours; the immediate effects of this exertion were simply fatigue, and stiffness in the muscles; but when he again attempted to employ the muscles in that action, he felt a nervous pain in the fore-arm, which was so severe as to oblige him to desist. This pain gave him considerable alarm, from the notion of its being of a paralytic nature, and many attempts were made to remove it. Recourse was had to electricity, and several other stimulating applications; but these always aggravated the symptoms, and they still continue. The circumstance in this case which is peculiarly applicable to my present purpose is, that the pain is only felt in the act of writing, the common motions of the fingers and thumb not giving the smallest uneasiness.

A gentleman about 46 years age, of a very irritable constitution, who had been in the habit of dealing cards for whole evenings together, was engaged in this employment one night for 6 hours; the weather was very warm, and he walked home in a state of perspiration, and went to bed. The window of his apartment, which faced the north, and was directly opposite to the foot of the bed, had been left open; the bed curtains were also undrawn. In the course of the night there was a sudden change in the weather from hot to cold, and the wind having shifted to the north, blew directly on his right arm, which was accidentally exposed. In the morning when he awoke his arm was in a very uneasy state. This however went off; but there was a pain in the muscles situated between the thumb and fore-finger, and those of the fore-arm, which continued, and gave him great uneasiness. As it was supposed to be paralytic, blisters were applied to the origin of the nerves at the shoulder, and a visit to Bath was agreed on as a necessary measure. The effects of the blister rather increased the complaint, which raised a doubt about its nature; and I found, on a careful investigation, that particular muscles only were affected, which suggested an inquiry into the use that had been made of them. This inquiry led to a discovery of the real nature of the complaint, as only those muscles used in dealing cards were particularly affected. They were not in pain while at rest, but were unable to bear the least action without considerable uneasiness. This was greater at some times, than others; and though a year has now elapsed since the complaint came on, it is not entirely removed.

One of the principal tavern-keepers in London was rendered very uneasy by a pain in the fore-arm, close to the elbow, which at times was very severe. On examining the parts, the pain was evidently not in the joint, but appeared to arise from an affection of the supinator brevis muscle, as the motion of that muscle gave pain. This I stated to him, but told him I was at a loss to find out in what way that part could have been injured; this was readily cleared up, when he informed me that the greatest pain he felt was in drawing claret corks, which he did with a jerk or sudden motion of the arm, and it was immediately after an exertion of this kind that he had first felt the complaint. It was clear from this account that this particular muscle had been strained, and was rendered unfit to bear any violent action.

These cases will be sufficient to explain that a muscle, or set of muscles, may be unable to perform those actions which require the greatest exertion, though capable of performing all the others. If then we consider the disease which causes the inability to see near objects as a strain on the muscles, and compare it with the same disease in other muscles, there will be no difficulty in accounting for the bad effects produced by every thing that irritates, or weakens the parts themselves, or the general habit: it will follow, that such a mode of practice should be laid aside, and those means adopted by which the parts can be soothed in their sensations, and quieted and strengthened in their actions, since in that way only the muscular fibres can possibly recover their tone.

Of double vision.—Many opinions have been advanced to account for the single appearance of objects when seen by both eyes. Dr. Reid of Glasgow, who has taken much pains on this subject, has treated it with much ingenuity and knowledge; and the opinion he has advanced, of objects appearing single when the impressions from the object are made on parts of the retina of the 2 eyes which correspond with each other, and double whenever that is not the case, is very strongly confirmed by the following observations on double vision.

There are 2 circumstances under which double vision takes place: one where the muscles of the eye do not correspond in their action, and therefore the 2 eyes do not bear equally on the object; the other where some change has taken place in the refracting media of one eye, which prevents the pencils of light from impressing the corresponding parts of the retina of both eyes. Instances of double vision produced by these 2 modes have fallen under my notice. It has been long ascertained by experiments, that when the eyes are not turned equally towards an object, it appears double, and the disease in the muscles which produces this effect is the subject which I now mean to consider. It will at the same time be proper to distinguish this kind of double vision from that which is produced by a change in the refracting media of the eye; and this will be best done by explaining the nature of those changes in consequence of which it occurs.

When one eye has had the crystalline lens extracted, the other remaining perfect, objects seen by both eyes will appear double. This is a fact which was noticed in a former lecture, in treating of the adjustment of the eye. At first it appeared difficult to account for the double vision, particularly as the 2 images were entirely separate from each other. It could not arise from the absence of the lens, as that would not alter the situation of the images on the retina; and the 2 images being of different dimensions on similar parts of the retina, would appear to be one before the other. As the operation of extracting the lens in no respect affects the muscles of the eye, the action of the muscles would be the same as before, and therefore could not contribute to produce this effect. The double vision in this instance appears to arise from the cornea of the eye which had undergone the operation being rendered flatter than the other, and giving a different direction to the rays of light, so as to form an image on a part of the retina not corresponding with the part impressed in the other eye.

If the crystalline lens be extracted from both eyes, and the person applies a convex glass to one eye only, and looks at an object, it will appear double; but if the convex glass is moved in different directions before the cornea, there will be found one situation in which it makes the object single. In this instance the corneas and muscles of the 2 eyes are under exactly the same circumstances; and when the centre of the convex glass is directly in the axis of vision, the image on the retina of that eye is formed on parts that correspond with those impressed in the other; but whenever the centre of the convex glass is out of the axis of vision this does not take place, and the object appears double. The experiments of which these observations are the result, were made on the eyes of a lady who had lost the sight of both, by opacities in the crystalline lenses; but by submitting to have the lenses extracted she recovered her sight, and had afterwards an uncommon degree of distinct vision; which made her a very favourable subject for experiments of this kind.

Having explained the 2 different modes by which double vision may take place in consequence of operations that render the refracting media of the eye imperfect, I shall now consider it when produced by a morbid action of the muscles. Several cases of this kind have come within my own knowledge, and I am induced to dwell on the subject, because some of them had been considered as arising from a defect in the organ, and erroneously treated. The fact has been long established by philosophers that a defect in the muscles may produce such a disease; but as other causes may also do the same, I believe that such a defect has not been practically considered, as one of the diseases of the eye; certainly not as a very common one, which undoubtedly it will be found.

The first case of this kind which led me to pay attention to the subject, was that of a friend, a lieutenant-colonel of engineers, who was in perfect health, shooting moor-game on his own estate in Scotland. He was very much surprized towards the evening of a fatiguing day's sport, to find all at once that every thing appeared double; his gun, his horse, and the road, were all double. This appearance distressed him exceedingly, and he became alarmed lest he should not find his way home; in this however he succeeded by giving the reins to his horse. After a night's rest the double vision was very much gone off; and in 2 or 3 days he went again to the moors, when his complaint returned in a more violent degree. He went to Edinburgh for the benefit of medical advice. The disease was referred to the eye itself, and treated accordingly; the head was shaved, blistered, and bled with leeches. He was put under a course of mercury, and kept on a very spare diet. This plan was found to aggravate the symptoms: he therefore, after giving it a sufficient trial, returned home in despair, and shut himself up in his own house. He gradually left off all medicine, and lived as usual. His sight was during the whole time perfectly clear, and at the same time near objects appeared single; at 3 yards they became double, and by increasing the distance they separated further from each other. When he looked at an object, it was perceived by a by-stander, that the 2 eyes were not equally directed to it. The complaint was most violent in the morning, and became better after dinner, when he had drank a few glasses of wine. It continued for nearly a year, and gradually went off.

The above account of the disease was given to me by the patient himself, who is an intelligent man, soon after his recovery. It was considered as a curious disease, and I had several conversations with Mr. Ramsden respecting it. The more we considered it, the more we were convinced that the disease had been entirely in the muscles; and this I explained to the patient at the time as my opinion. It is now about 8 years ago, and the gentleman has had no return of the disease; but for 2 or 3 years past has lost in a great measure the use of his lower extremities, being unable to walk alone.

Some time after the recovery of this gentleman, a house-painter, who had worked a good deal in white lead, was admitted a patient in St. George's Hospital, on account of a fever, attended with a violent head-ach. On recovering from the fever, he was very much distressed at seeing every thing double; and as the fever was entirely gone, he was put under my care for this affection of his eyes. On inquiry into his complaints, I found them exactly to correspond with the case I have just described, and therefore treated them as arising entirely from an affection of the muscles. I bound up one eye, and left the other open: he now saw objects single, and very distinctly, but looking at them gave him pain in the eye, and brought on head-ach. This led me to believe that I had erroneously tied up the sound eye; the bandage was therefore removed to the other eye, and that which had been bound up was left open. He now saw objects without pain, or the smallest uneasiness. He was thus kept with one eye confined for a week, after which the bandage was laid aside; the disease proved to be entirely gone, nor did it return in the smallest degree while he remained in the hospital. Rest alone had been sufficient to allow the muscles to recover their strength, and thus produced a cure.

A repetition of cases, I am very sensible, is not the most pleasing mode of conveying information, except to medical men. I have therefore selected those only which are absolutely necessary to explain the different phenomena of the diseased states of the eye at present under consideration. The cases brought forward with this view, are rather to be considered as the detail of so many experiments made in the investigation of the diseases, than as histories of particular patients. When muscles are strained or over-fatigued, to put them in an easy state, and confine them from motion, is the first object of attention; and this practice is no less applicable to the muscles of the eye, than to those of other parts.

Of squinting.—Whenever the motions of the 2 eyes differ from each other, whether in a less degree, so as to produce double vision, or in a greater, turning one eye entirely from the object, the disease has been called squinting. What I mean at present to consider under this head is, where the deviation of one of the eyes from the axis of vision is greater than that by which objects are made to appear double; so that in this view, double vision is an intermediate state between single vision with both eyes, and squinting. Squinting has been very generally believed to arise entirely from an inability in the muscles to direct the eye properly to the object. There is however probably no original defect in the muscles; certainly none sufficient to sanction such an opinion; since the muscles of a squinting eye have the power of giving it any direction, but cannot do it without some degree of effort. The defect therefore appears to be principally in the eye itself, which is too imperfect to assist the other in producing distinct vision. From this imperfection, the muscles have not the same guide to direct them as those of the other eye; and therefore, though perfectly formed, cannot make their actions exactly correspond with them.

In a squinting person, both eyes certainly do not see the object looked at. This is evident to a by-stander, who is able to determine, that the direction of one of the eyes differs so much from that of the other, that it is impossible for the rays of light from any object to fall on the retinas of both; and therefore, that one eye does not see the object. The same thing may be proved in another way: for since a small deviation in the direction of either eye from the axis of vision, produces double vision, any greater deviation must have the same effect, only increasing the distance between the 2 images, till it becomes so great that one eye is directed to the object. In squinting there is evidently a greater deviation from the axis of vision than in double vision, and the object does not appear double; it is therefore not seen by both eyes.

The circumstance of those who squint having an imperfect eye, is corroborated by all the well authenticated observations which have been made on persons who have a confirmed squint, which all agree in stating, that one of the eyes is too imperfect to see distinctly. From these observations, it would be natural to suppose that the loss of sight in one eye, should produce the appearance of squinting, which is by no means the case; for when that happens, the motions of the 2 eyes continue to correspond, though not exactly; but the deviation not equal to that which is met with in squinting; it is nearer to that which occurs in double vision.

The reason why the imperfect eye of a squinting person is directed from the object, while a blind one in its motions follows the other, is probably, that the indistinct vision of the imperfect eye prevents the muscles from directing it to the object with the same accuracy as those of the other do; this small deviation from the axis of vision renders the object double, and interferes with the vision of the perfect eye; and it is in the effort to get rid of the confused image that the muscles acquire a habit of neglecting to use the imperfect eye. It may also happen, when the eye is so imperfect as not to receive a correct image of any object, that it may have been neglected from the beginning. Distinct vision being at once obtained by the perfect eye, the end is answered, and the mind is never afterwards led to employ the other. The direction the eye takes under either of these circumstances is inwards, towards the nose, the adductor muscle being stronger and shorter, and its course more in a straight line, than any of the other muscles of the eye. That the eye, when not accurately directed to the object, produces confused vision, and is for that reason turned away, appears to be confirmed by the case of a patient, from whom I had extracted the crystalline lens. This man, at first, saw objects double, in a manner which extremely distressed him; but after some months he acquired the habit of neglecting to employ the imperfect eye, and no longer found any inconvenience.

The different degrees of squinting appear to be in proportion to the imperfection in the vision of the eye, and, in some instances, the person is capable of seeing distant objects with both eyes, and only squints when looking at near ones. The following case is of this kind. A young lady, 23 years of age, has been observed to squint from her infancy; this has not been considered by her friends as the consequence of any defect in her eyes, but as arising from the cradle in which she lay having been so situated, with respect to the light, as to attract her notice in one particular direction, so much as to occasion a cast in one eye. Her eyes are apparently both perfect; when she looks with attention at an object some yards distant she has no squint, but if her eyes are not engaged by any object, or a very near one, she squints to a considerable degree. On being asked if she saw objects distinctly with both eyes, she said certainly, but that one was stronger than the other. To ascertain the truth of this, I covered the strong eye and gave her a book to read; to her astonishment, she found she could not distinguish a letter, or any other near object. More distant objects she could see, but not distinctly. When she looked at a bunch of small keys in the door of a book-case, about 12 feet from her, she could see the bunch of keys, but could not tell how many there were.

To see how far the 2 eyes had the same focus, she was desired to look at an object in the field of a microscope; and it was found that she saw most distinctly with both eyes at the same focal distance, though the object was considerably more distinct to the perfect eye than to the other; so that the focuses of the 2 eyes were the same. I desired her to cover the perfect eye, and endeavour to acquire an adjustment of the other to near objects, by practising the use of that alone. At first she was unable to see at all with the imperfect eye, but in some weeks she has improved so much as to be able to work at her needle with it; this she cannot do long at any one time, the eye being soon fatigued and requiring rest, though without giving pain. She is unable to read with the imperfect eye. These trials have only been made in the course of 2 months, for a few hours in the day, and her friends think that she squints less frequently than she did. In this case it is probable that the imperfect eye never had acquired the power of adjustment to near objects; for as distinct vision seems necessary to direct the muscles in their actions, the perfect eye would require less practice to adjust itself than the other; and as soon as the near object became distinct to one eye, no information being conveyed to the mind of the failure in the other, all efforts to render its adjustment perfect would be at an end, and it would ever after be neglected, while the perfect eye was in use.

Squinting, according to these observations, appears to arise from the vision in one eye being obscure. It may however be acquired in a degree by children who have the lenses of their eyes of different focuses; or have one eye less perfect in its vision than the other, living constantly with those who do squint, and by imitation acquiring a habit of neglecting to use one eye. The power of squinting voluntarily may also be acquired at any age. This we find to be true in persons who look much through telescopes; they are led to apply the mind entirely to one eye, not seeing at all with the other. In this case the neglected eye will at first, from habit, follow the other; but in time, if frequently neglected, may lose this restraint, and be moved in another direction. Some astronomers, whose eyes have been much used in this way, are said to be able to squint at pleasure.

From this view of squinting, it takes place under the 3 following circumstances: where one eye has only an indistinct vision; where both eyes are capable of seeing objects, but the one less perfect in itself than the other; and where the muscles of one eye have acquired from practice a power of moving it independently of the other. When squinting arises from an absolute imperfection in the eye, there can be no cure. Where it arises from weakness only in the sight of one eye, it may in some instances be got the better of; but to effect the cure there is only one mode, which is that of confining the person to the use of the weak eye by covering the other; in this way the muscles, from constant use, will become perfect in the habit of directing the eye on the object, gain strength in that action, and acquire a power of adjusting the eye; when these are established in a sufficient degree, the other eye may be set at liberty. The time that will be necessary for the cure must depend on the degree of weakness of the sight, and the length of time the muscles have been left to themselves; for it is with difficulty they acquire an increased degree of action, after having been long habituated to a more limited contraction.

On the nature of the cornea, some of its diseases, and mode of treatment.—The cornea of the eye, as the name implies, has been considered of a cuticular nature. Haller compares it to the nails in a soft state, and believes that in its regeneration it resembles the epidermis. This opinion is founded on its want of sensibility, and having no vessels which carry red blood; the appearance it puts on when preserved in spirits, which is exactly similar to the nails at their roots, probably confirmed this supposition. As the cuticle is devoid of life, it is only under the influence of disease during its growth; once formed, it continues unchanged. The cornea, were it of the same nature, would be equally incapable of taking on new actions from disease, or any other cause; but we find, on the contrary, that it undergoes many changes, which exactly correspond with those which the living parts of an animal body go through when under the influence of disease; from which I am induced to consider it alive; and I find that many of the present teachers of anatomy are of the same opinion.

To prove that the cornea has life, it is necessary, as a previous step, to show, that being supplied with vessels which carry red blood, and having sensibility, are not essential to the possession of the living principle; for this purpose all that is required, is to demonstrate that there are living parts which have neither the one nor the other. Tendons and ligaments in a natural state are instances of this kind. That these parts are not supplied with red blood, is obvious to the eye of a common observer; no illustration will therefore be required to substantiate that proof. That they are not endowed with sensibility was, I believe, first taught by the late Dr. William Hunter,* who published the following account of it.† In a case where the last joint of the ring-finger had been torn off, half an inch of the tendon of the flexor muscle projected beyond the stump; this it was thought right to remove; and to ascertain whether it was possessed of sensibility, the following experiment was made: a piece of cord, the thickness of the tendon, was passed round the wrist and along the side of the finger, so as to project even with the end of the tendon; the man was told to turn away his head, and tell which of the 2 were cut through; the tendon was divided, and the man declared it was the string, not having felt the smallest degree of pain.

This proof is satisfactory; but that the cornea is possessed of life, by no means rests on any negative proofs; which I shall now endeavour to explain. The cornea in its structure is made up of membranous laminæ. One of these appears to be a portion of the tunica conjunctiva; but it is either so extremely thin, or so intimately connected with the lamina next to it, as not to admit of more than a very partial separation from it; another lamina, as I have shown in a former lecture, is a continuation of the tendons of the 4 straight muscles; but as both of these laminæ have the same properties as the other parts of the cornea, and are not to be distinguished from them, they must be considered in every respect as a part of it. The tunica conjunctiva and tendons, a continuation of which forms these anterior laminæ of the cornea, are allowed to be living parts, and the portions that make

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* This doctrine was first taught by Dr. Hunter, in the year 1746. Haller made experiments proving the same thing in 1750. † Medical Observ. and Inquir. Vol. 4, p 343.—Orig.

part of the cornea are not to be distinguished by their structure from the rest; we must therefore suppose them to be also composed of living parts. When the cornea is wounded it unites, like other living parts, by the first intention. If the wound is made by a clean cutting instrument the cicatrix is small; but if by a blunt instrument it is larger, extending farther into the neighbouring parts of the cornea, and a greater quantity of the coagulating lymph of the blood being required to procure the union.

Though the cornea, when divided in the operation for extracting the crystalline lens, commonly unites by the first intention, this union is in some cases attended with inflammation, which produces an opacity of the cornea; in other cases the inflammation exceeds the limits of adhesion, and the whole internal cavity of the eye proceeds to a state of suppuration. These stages of inflammation are only met with in parts possessed of life. It is true, that an injury may be committed to the cornea, such as a small piece of metal sticking in it, which from the indolent nature of its substance, shall remain there for months without producing inflammation; but an irritation of a less violent kind on the edge of the cornea, by which the tunica conjunctiva is also affected, will produce inflammation on that vascular membrane, which may extend itself on the cornea; for it is impossible that the vessels of the cornea, which naturally carry only lymph or serum, can be made to carry red blood, unless the irritation extends to some neighbouring part supplied with red blood.

That vessels carrying red blood have been met with on the cornea in a diseased state, is doubted by Haller; he does not altogether deny it, but the assertion, he says, requires proof, as he is not satisfied with the authorities of Petit and others whom he quotes on that subject. It is so common a thing, in inflammations of the eye, to have the branches of the arteries of the tunica conjunctiva continued on the cornea, that every practical surgeon must have met with it. In some intances of this kind, which have come immediately under my own care, I have examined these vessels with a magnifying glass, and have seen distinctly small arteries from the tunica conjunctiva, uniting on the cornea into a common trunk, larger than any of the branches that supplied it, and this trunk has sent off other branches distributed over the cornea. These vessels may, by some physiologists, be supposed to be continued on the lamina of the tunica conjunctiva, which is spread over the cornea; this however is not the case, as they pass behind it, and therefore belong as much to the lamina under them as that which is over them; and, in many instances of disease, vessels carrying red blood are met with in the substance of the cornea still deeper seated. This has been seen by Professor Richter,* who says, he has divided a thickened cornea, and the vessels in its substance have poured out red blood.

The cornea is not only capable of uniting by the first intention, inflaming, and

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* Richter, M. D., et Prof. Pub. Ordinar. Soc. Reg. Sc. Gotting. et Ac. Reg. Sc. Sueciæ Mem. in Novis Com. Soc. Reg. Gotting. T. vi. ad ann. 1775.—Orig.

suppurating, but when the inflammation is carried to a great height, a portion of its substance is sometimes removed by ulceration, and the ulcer so formed is filled up by coagulating lymph, which afterwards becomes cornea, acquiring the necessary property of transparency. This new formed part is weaker than the rest of the cornea, and commonly projects beyond it, forming one species of staphyloma; in the substance of the cornea, round the basis of the staphyloma, I have frequently seen vessels carrying red blood. From the opinion of the cornea being devoid of life, the opacities which are found to take place on it have been considered apart from common surgery, and entrusted to the care of men who are supposed to have made the diseases of the eye their particular study. According to this theory, the opacity was supposed to arise from a film of inanimate matter laid over the cornea, and on that idea very acrid and irritating applications were employed with the view of scraping it off, or destroying it, as powdered glass, powdered sugar, &c. and such applications being of service, confirmed the opinion which gave rise to the practice.

Having shown that the cornea is possessed of life, I shall now point out the parts of the body it resembles in structure, and to which it bears the greatest analogy, both in its healthy actions, and those arising from disease; and endeavour, by comparing them, to establish some general principle which will explain the beneficial effects of irritating applications in cases of inflammation and opacity of the cornea. The cornea, from some experiments and observations mentioned in a former lecture, appears to be similar in structure and use to the elastic ligaments. It has all the common properties of ligaments, those of elasticity and transparency being superadded. Like other ligaments it can be divided into laminæ, in a healthy state has no vessels carrying red blood, and is devoid of sensibility; when divided it readily admits of union, when inflamed acquires a great degree of sensibility, is slow in its powers of resolution, and when the inflammation subsides, the coagulating lymph deposited in the adhesive stage of the inflammation remains, producing an opacity which it is afterwards found difficult to remove.

All ligamentous parts, of which I consider the cornea to be one, are weak in their vital powers; this arises from their having no vessels carrying red blood; when they inflame, which is a state of increased action, they therefore require a different mode of treatment from the other parts of the body, whose vital powers are strong, in consequence of being largely supplied with red blood. The truly healthy inflammation requires an increased action in the parts affected; and if this, either from weakness or indolence, is not kept up, the inflammation does not go rapidly through its stages, but remains in a state between resolution and suppuration. In ligamentous structures the actions must therefore be roused and supported when under inflammation, to promote resolution, and prevent the parts from falling into an indolent diseased state. This is however attended with difficulty, and they too often become considerably thickened by a deposition of coagulating lymph during the adhesive state of inflammation, which in the cornea renders it opaque. The thickening of the parts remains after the inflammation is gone, and can only be removed by absorption, which is best effected by the application of very stimulating medicines. On these principles all ligamentous structures require a treatment peculiar to themselves, which may be illustrated both in inflammations of joints and of the cornea of the eye; the applications made use of with the greatest advantage in both cases being of a very stimulating kind.

The advantages attending this mode of treating the cornea were probably discovered by accident; and when they were ascertained, it established itself as a very general practice. It must however, in the hands of those who had no general principle to direct their practice, have been sometimes applied without benefit, and must sometimes have been injurious. It is an extremely curious circumstance, and probably the most so that can be met with in the history of medicine, that a local application should have been discovered to be of service in a particular disease 2513 years ago, that the same application, or those of a similar kind, should have been in very general use ever since, and in all that time no rational principle on which such medicines produced their beneficial effects should have been ascertained. This appears, from the following account, to have been the case with respect to stimulating applications to the cornea in a diseased state, and can only be accounted for by a want of knowledge of the structure of the parts, which is an argument of uncommon weight in favour of the study of anatomy.

In the Apocrypha we find, in the book of Tobit *, a very circumstantial account of an opacity of the cornea successfully treated by stimulating applications. It is there treated as a miracle, but we have the authority of Jerome, a father of the church, who wrote in the 4th century, to say, " the church reads the books of Tobit, &c. for examples of life and instruction of manners; we shall therefore consider the account which is given in extracts from the book of Tobit in that view."

Tob. chap. vi. ver. 2. " When Tobias went down to wash himself in the river Tigris, a fish leaped out of the river and would have devoured him. Ver. 4. The angel of the Lord told him to take out the gall, and put it up in safety. Ver. 6. Tobias asked the angel what was the use of the gall. Ver. 8. As for the gall, said the angel, it is good to anoint a man who hath whiteness in his eyes, and he shall be healed." Chap. xi. ver. 11. " Tobias took hold of his father, and strake of the gall in his father's eyes, saying, be of good hope, my father. Ver. 12. And when his eyes began to smart he rubbed them. Ver. 13. And the whiteness pealed away from the corners of his eyes, and when he saw his son he fell upon his neck †."

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* Tobit was of the tribe of Naphtali, in the city of Thisbe, in Upper Galilee; he was carried captive to Nineveh, after the extinction of the kingdom of Israel, by Enemasser, or Salmanessar, about the year of the world 3283. Gray's Key to the Old Test. and Apoc. p. 554.—Orig.

† Since this paper was read before the R. S., my friend Dr. Wells acquainted me with the following case, published in the Ann. Register for the year 1678. " One of the Paris newspapers gives an account of an extraordinary cure effected by the gall of a barbel, in a case of blindness, in substance as follows: A journeyman watchmaker, named Censier, having heard that the gall of a barbel was the remedy which Tobias employed to cure his father's blindness, resolved to try its effects on the widow Germain, his mother-in-law, whose eyes had for six months been afflicted with ulcers, and covered with a film, which rendered them totally blind: Censier having obtained the gall of that fish, squeezed the liquor out of it into a phial, and in the evening he rubbed it with the end of a feather into his mother's eyes. It gave her great pain for about ½ an hour, which abated by degrees, and her eyes watered very

In conversing with my friend Dr. Russell on the manner in which the Arabians treat inflammations and opacities of the cornea, he favoured me with the following account. "Respecting the practice of the Arabians in disorders of the eyes, I find nothing of consequence in my papers. An oculist among them is a distinct profession; and the collyria they apply are secret compositions, which pass hereditarily from father to son. The Arabian writers give a number of recipes, most of which are taken from Galen and the Greek physicians. One composition in Avicenna contains the gall of a crow, crane, partridge, goat, &c. At Aleppo, the gall of the sheet fish, silurus glanis of Linn. was in particular request; but it should be remarked, that they always add to the gall other ingredients, it being a material circumstance in that country, that a recipe should consist of a multitude of ingredients. What often struck me in their practice was the successful application of sharp or acrid remedies, at a time I should have been induced to make use of the mildest emollient applications."

From this account given by Dr. Russell there can be no doubt of gall having continued in use, as an application to the eye among the eastern nations, from the time of Tobit down to the present day. I have in the course of the last 3 years made many trials of the effects of gall, as an application to the cornea in a diseased state. I have used it pure, and diluted; and compared its effects with those of the unguentum hydrarg. nitrati, and the solution of the argentum nitratum; and find in old cases of opacity it is, in some instances, the best application. The gall of quadrupeds, in these trials, gave more pain than the gall of fish. The painful sensation was very severe for an hour or 2, and then went off. The beneficial effects it produces appear to be in proportion to the local violence at the time of its application.

To enter further into the practical part of the treatment for removing opacities from the cornea, would be foreign to the pursuits of the R. S., which I consider to be confined to the general principles of the different branches of science, and to collecting facts out of which new principles may be formed, or those already known better established. The practice of applying very stimulating applications to the cornea, has stood the test of 25 centuries, it can therefore require no support. The object of the present observations has been to explain the principle on which beneficial effects depend, a knowledge of which may serve as a guide to regulate our practice. It will guard us against using such medicines while the inflammatory action is increasing; it will lead us to adopt them the moment the inflammation

much: next morning she could not open them, the water as it were glueing her eyes up: he bathed them with pure water, and she began to see with the eye which had received the most liquor. He used the gall again in the evening; the inflammation dispersed, the white of her eyes became red, their colour returned by degrees, and her sight became strong. He repeated it a 3d time, with all the desired success. In short, she recovered her sight without any other remedy. The widow Germain is in her 53d year. She had been pronounced blind by the surgeons of the Hôtel-Dieu: and her blindness and cure have been attested by order of the lieutenant-general of police. She sees stronger and clearer now than before the accident." Annual Register, vol. xi. page 143.—Orig.

appears to be at a stand, and not postpone this practice till an indolent unhealthy state takes place, which too often terminates in opacities no applications can afterwards remove.

II. *Observations on Horizontal Refractions which affect the Appearance of Terrestrial Objects, and the Dip, or Depression of the Horizon of the Sea. By Jos. Huddart, Esq., F. R. S.* p. 29.

The variation and uncertainty of the dip, in different states of the air, taken at the same altitude above the level of the sea, was the occasion of turning my thoughts to this subject; as it renders the observed latitude incorrect, by giving an erroneous zenith distance of a celestial object. I have often observed that low lands and the extremity of head lands or points, forming an acute angle with the horizon of the sea, and viewed from a distance beyond it, appear elevated above it, with an open space between the land and the sea. The most remarkable instance of this appearance of the land I observed at Macao, for several days previous to a typhoon, in which the Locko lost her topmasts in Macao roads; the points of the islands and low lands appearing the highest, and the spaces between them and the sea the largest, I ever saw. I believe it arises, and is proportional to the evaporation going on from the sea; and in reflecting on this phenomenon, I am convinced that those appearances must arise from refraction, and that instead of the density of the atmosphere increasing to the surface of the sea, it must decrease from some space above it; and that evaporation is the principal cause which prevents the uniformity of density and refraction being continued, by the general law, down to the surface of the earth: and I am inclined to believe, though I mention it here as a conjecture, that the difference of specific gravity in the particles of the atmosphere may be a principal agent in evaporation; for the corpuscles of air, from their affinity with water, being combined at the surface of the fluid from expansion, form air specifically lighter than the drier atmosphere; and therefore float, or rise, from that principle, as steam from water; and in their rising (the surrounding corpuscles from the same cause imbibing a part of the moisture,) become continually drier as they ascend, yet continue ascending till they become equally dense with the air*. However, these conjectures I shall leave, and proceed to the following observations on refractions.

In the year 1793, when at Allonby, in Cumberland, I made some remarks on the appearance of the Abbey Head, in Galloway, which is at about 7 leagues distance from Allonby; and from my window, at 50 feet above the level of the sea at that time of tide, I observed the appearance of the land about the head as represented in pl. 2, fig. 1. There was a dry sand, x y, called Robin Rigg, between

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* Mr. Hamilton, in his very curious Essay on the Ascent of Vapours, does not allow of this principle, even as an assistant; though by a remark (page 15) he takes notice of those appearances in the horizon of the sea, and says they arise from a strong or unusual degree of refraction; the contrary of which I hope to illustrate in the course of this paper.—Orig.

me and the head, at the distance from my house of between 3 and 4 miles, over which I saw the horizon of the sea, HO; the sand at this time being about 3 or 4 feet above the level of the sea. The hummock d is a part of the head land, but appeared insulated or detached from the rest, and considerably elevated above the sea, with an open space between. I then came down about 25 feet, when I had the dry sand of Robin Rigg, xy, in the apparent horizon, and lost all that floating appearance seen from above, and the Abbey Head appeared every where distinct to the surface of the sand; this being in the afternoon, the wet or moisture on the sand was probably in a great measure dried up. I have reason therefore to conclude, that evaporation is the cause of a less refraction near the surface of the sea; and when so much so as to make an object appear elevated wholly above the horizon, as at d, there will from every point of this object issue 2 pencils of rays of light, which enter the eye of the observer; and that below the dotted line AB, parallel to the horizon of the sea HO, the objects on the land will appear inverted.

To explain this phenomenon, I shall propose the following theory, and compare it with the observations which I have made. Suppose HO, fig. 2, to represent the horizontal surface of the sea, and the parallel lines above it, the lamina or strata of corpuscles, which next the fluid are most expanded, or the rarest; and every lamina upwards increasing in density till it arrive at a maximum, and which I shall in future call the maximum of density, at the line DC, above which it again decreases in density ad infinitum. Though this in reality may be the case, I do not wish to extend the meaning of the word density farther, than to be taken for the refractive power of the atmosphere; that is, a ray of light entering obliquely a denser lamina to be refracted towards a perpendicular to its surface; and in entering a rarer lamina, the contrary; which laminæ being taken at infinitely small distances, the ray of light will form a curve, agreeable to the laws of dioptrics. In order to establish this principle in horizontal refractions, I traced over various parts of this shore at different times, when those appearances seemed favourable, with a good telescope, and found objects sufficient to confirm it; though it be difficult at that distance of the land to get terrestrial objects well defined so near the horizon, as will afterwards appear.

One day observing the land elevated, and seeing a small vessel at about 8 miles distance, from my window I directed a telescope to her, and thought her a fitter object than any other I had seen for the purpose of explaining the phenomena of these refractions. The telescope was 40 feet above the level of the sea. The boat's mast about 35 feet. The barometer at 29.7 inches, and Fahrenheit's thermometer at 54°. The appearance of the vessel, as magnified in the telescope, was as represented in fig. 3, and from the mast head to the boom was well defined. I pretty distinctly saw the head and shoulders of the man at the helm; but the hull of the vessel was contracted, confused, and ill defined: the inverted image began to be well defined at the boom, for I could not clearly perceive the man at the helm inverted, and from the boom to the horizon of the sea the sails were well defined, and I could see a small opening above the horizon of the sea, in the angle made by the gaff and mast; and had the mast been shorter by 10 feet, to the height of y, the whole would have been elevated above the horizon of the sea, and from y to d an open space. This drawing was taken from a sketch I took at the time, and represents the proportion of the inverted to the erect object, as near as I could take it by the eye, the former being about $\frac{2}{3}$ of the latter in height, and the same breadth respectively; though at one time during the observation, which was continued about an hour, I thought the inverted nearly as tall as the erect object. The day was fine and clear, with a very light air of wind, and I found very little tremor or oscillation in viewing her through the telescope.

Fig. 4 is laid down for explaining the above phenomena, in which A represents the window whence I viewed the vessel B; HO, the curved surface of the sea; CD parallel to HO, the height of the maximum of density of the atmosphere; the lines marked with the small letters aa, bb, cc, dd, the pencils of rays under their various refractions from the vessel to the eye, or object-glass of the telescope. The pencil of rays aa, from a point near the head of the main-sail, is wholly refracted in a curve convex upwards, being every where above the maximum of density; and the pencil of rays dd, which issues from the same point in the sail, and passes near the horizon of the sea at x, is convex upwards from the sail to w, where it passes the line of maximum of density, which is the point of inflection; there it becomes convex downwards, passing near the horizon at x to y, where it is again inflected, and becomes convex upwards from thence to the eye. The pencil of rays bb, from the end of the boom, passing nearly parallel to the horizon, and near the maximum of density, suffers very little deviation from a right line in the first part; but in ascending, from the curvature of the sea, will be convex upwards to the eye. The pencil of rays cc, from the same point in the boom, may have the small part to c convex upwards, from c to z it will be convex downwards, and from z to the eye convex upwards. From this investigation it appears, that 2 pencils of rays cannot pass from the same point, and enter the eye, from the law of refraction, unless one pencil pass through a medium which the other has not entered; and therefore the maximum of density was below the boom, and could not exceed 10 feet of height above the surface of the sea at the time these observations were made.

Respecting the hull of the vessel being confused, and ill defined in the telescope, as by fig. 3, it arises from the blending of the rays, from the different parts of the object, refracted through the 2 mediums; some parts of the hull appearing erect, and some inverted. Suppose the dotted line ii, fig. 4, an indefinite pencil of rays, passing from between the inverted and erect parts of the object, or the upper part of the hull of the vessel to the eye, for the lower part of the hull could not be observed: the objects cannot appear inverted, unless the angles at the eye aAc and aAd, exceed the angle aAi; for the intermediate space could only be contracted by the secondary pencils of rays. The lengths of the inverted, compared with the erect image of the sail, is as the sines of the angles at the eye aAi to iAd; and the angle at the eye aAd, made by the 2 pencils of rays from the same point near the head of the sail, must be double the angle aAi, when the inverted image is as tall as the erect. In this case the sines of the angles aAb, aAc, aAd, fig. 4, are proportional to the altitudes ab, ac, ad, in the magnified view of the vessel, fig. 3. Under this consideration, no inverted image of the sail will be formed, till the angle at the eye, made by the two refracted pencils of rays aa and dd, exceed the angle made by aa, and bb, the apparent height of the sail of the vessel; for were those angles equal, the inverted sail would only be contracted into the parallel of altitude of the boom b, and render the appearance confused, as in the hull of the vessel.

Respecting the existence of 2 pencils of rays entering the eye from every point of an object not more elevated than a, or less than i, fig. 3, in this state of the atmosphere, I cannot bring a stronger proof than that of the strength of a light when the rays pass near the horizon of the sea, proved by the following observations. Going down Channel about 5 years ago in the Trinity yacht, with several of the elder brethren, to inspect the light-houses, &c. I was told by some of the gentlemen, who had been on a former survey, that the lower light of Portland was not so strong as the upper light, at near distances, but that at greater distances it was much stronger. I suspected that this difference arose from the lower light being at or near the horizon of the sea, and mentioned it at the time; but afterwards had a good opportunity of making the observation. We passed the Bill of Portland in the evening, steering towards the Start, a fresh breeze from the northward and clear night; when we had run about 5 leagues from the lights, during which time the upper light was universally allowed to be the stronger, several gentlemen keeping watch to make observations on them, the lower light, drawing near the horizon, suddenly shone with double lustre. Mr. Strachan, whose sight is weak, had for some time before lost sight of both lights, but could then clearly perceive the lower light. I then went aloft, as well as others, but before I got half mast up, the lower light was weaker than the upper one; on coming down on deck, I found it again as strong as before. We proceeded on, and soon lost the lower light from the deck; and on drawing the upper light near the horizon, it like the former shone exceeding bright. I again went aloft, when it diminished in brightness; but from the mast head I could then see the lower light near the horizon as strong as before. This is in consequence of the double quantity of light entering the eye by the 2 pencils of rays from every point. To illustrate which, we compare the vessel, fig. 4, to a light-house built on the shore, and A the place of the observer; and having brought down the light so low as to view it in the direction aa, another light would appear in the horizon at x from the pencil dd; and had the vessel been still enough to have observed it at this time with a good glass, I doubt not but the 2 images might have been distinctly seen: as the light dropped, by increasing the distance, the 2 images would appear continually to approach each other, till blended with double light in one, and disappear at the altitude i, above the apparent horizon of the sea. But, as explained before, if the strength of evaporation did not separate by refraction the pencils aa and dd to a greater angle than double the angle that the lamps and reflectors appear under, the 2 images would be blended, and the strong appearance of light would be of shorter duration. The distance run from the lights, during the time each of the lights shone bright, would have been useful, but this did not occur at the time, nor have I had the like opportunity since. However, I recommend to the mariner to station people at different heights in looking out for a light, in order to get sight of it near the horizon, when it is always strongest.

Respecting the appearance of the Abbey Head before-mentioned, fig. 1, the dotted line AB represents the limit, or the lowest points of the land that can be seen over the sea; for, as above stated, all the objects appearing below this line, are the land above it inverted; and where the land is low, as at d and m, it must appear elevated above the horizon of the sea.

In fig. 5, let HO represent the curve of the ocean, and d the extreme top of the mount visible at A by the help of refraction; the dotted pencil of rays cc passing from d to the eye in some part a little below the maximum of density, where inversion begins; therefore no land lower than this can be seen; for any pencil from a point in the land lower than this, must in the refraction have a contrary flexure in the curve, and therefore pass above the observer. Let AD be a tangent to the curve at A; then the object d will appear to be elevated by refraction to D; also let AV be a tangent to the pencil AX at A; then the angle DAX will appear to be an open space, or between D and the horizon of the sea. Suppose a star should appear very near or over the mount d, as at *, 2 pencils would issue from every point of it, and form a star below as well as above the hummock d. There are always confused or ill defined images of the objects at the height of the dotted line, fig. 1, above the level of the sea, as before-mentioned; and instead of the points d ending sharp in that line, they appear blunted, and the Abbey Head is frequently insulated at the neck m.

I have viewed, from an elevated situation, a point or head land at a distance beyond the horizon of the sea, forming, as in fig. 6, a straight line AB, making an acute angle BAO with the horizon of the sea. Seeing the extreme point blunted and elevated, I descended; and though in descending the horizon cut the land higher, as at HO, HO, yet the point had always the same appearance as a, a, a, fig. 6, though the land is known to continue in the direction of the straight line AB to beneath the horizon, or nearly so, as viewed from the height above. If then from a low situation we view this head land through a telescope, the inclination of the surface AB to the horizon being known to be a straight line, it will appear as in fig. 7, the dotted line (at the height of the point where a perpendicular xy would touch the extreme of the land) being at the limit or lowest point of erect vision. And if a tangent to the curved appearance of the land ab, be drawn parallel to the inclined surface of the land AB, fig. 6, touching it at C, fig. 7, the point C will show the height of the maximum of density, where the pencil of the rays of light, from thence to the eye, approach nearest the sea; for pencils of rays from this land, taken at small distances from C, will form parallel curves, nearly, through the refracting mediums, and C will be the point of greatest refraction; for above C, as at B, the refraction somewhat decreasing, will appear below the line ab, or the parallel to the surface of the land, and the refractions decrease below the point C; for had they increased uniformly down to the surface of the sea, it would render the apparent angle of the point of land Z more acute than the angle cao, contrary to all observations.

Thus I have endeavoured to explain the phenomena of the distorted appearance of the land near the horizon of the sea, when the evaporation is great; and when at the least, I never found the land quite free from it when I used a telescope; and thence infer, that we cannot have any expectation to find a true correction for the effect of terrestrial refraction, by taking any certain part of the contained arc; for the points ZCB, fig. 7, will have various refractions, though they are at nearly the same distance from the observer. And if the observations are made wholly over land, if the ground rises to within a small distance of the rays of light in their passage from the object to the eye, as well as at the situation of the object and observer, the refractions will be subject to be influenced by the evaporation of rains, dews, &c. which is sufficiently proved by the observations of Colonel Williams, Captain Mudge, and Mr. Dalby, Phil. Trans. 1795. The appearances mentioned by them cannot be demonstrated on general principles, as they arise from evaporation producing partial refractions. In those general principles, it is supposed that the same lamina of density is every where at an equal distance from the surface of the sea, at least as far as the eye can reach a terrestrial object; but in the partial refractions, the lamina of the expanded or rarefied medium may be of various figures according to circumstances, which will refract according to the incidence of the rays, and affect the appearance of the land accordingly, which I have often seen to a surprizing degree. But my principal view is to show the uncertainty of the dip of the sea, and that the effect of evaporation tends to depress the apparent horizon at X, when the eye is not above the maximum of density; and hence the difficulty of laying down any correct formula for these refractions, while the law of evaporation is so little understood, which indeed seems a task not easy to surmount. The effect indicated by the barometer and thermometer is insufficient: and should the hygrometer be improved to fix a standard for moisture in the atmosphere, and show the variations near the surface of the ocean, which certainly must be taken into the account, evaporation going on quicker in a dry than a moist atmosphere, the theory might still be incomplete for correcting the tables of the dip. I shall therefore conclude this paper, by showing a method I used in practice, in order to obviate this error, in low latitudes.

When desirous to attain more accurately the latitude of any head land, &c. in sight, I frequently observed the angular distances of the sun's nearest limb from the horizons, on the meridian both north and south, beginning a few minutes before noon, and taking alternately the observations each way, from the poop, or some convenient part of the ship, where the sun and the horizon both north and south were not intercepted; and having found the greatest and least distances from the respective horizons, which was at the sun's passing the meridian, and corrected both for refraction, by subtracting from the least, and adding to the greatest altitude, the quantity given by the table; and also having corrected for the error of the instrument, and the sun's semi-diameter; the sum of these 2 angular distances, reduced as above, — 180°, is equal to double the dip, as by the following example.

The sun's declin. 4° 32′ 30″ north, and semi-diam. 15′ 58″, observed as follows:

	South.			North.		
The meridian dist. of the sun's nearest limb from the horizon of the sea.	78°	36′	30″ =	101°	1′	20″
Refraction per table	—	0	11 =	+	0	11
Distances corrected for refraction	= 78	36	19 =	101	1	31
Error of the sextant	+	1	32	+	1	32
Sun's semi-diameter	+	15	58	+	15	58
	78	53	49	101	19	1
½ diff. or the dip found	—	6	25	78	53	49
Altitude reduced	= 78	47	24	180	12	50
Zenith distance	= 11	12	36	180		
				Diff.	12	50
The sun's declination	N. = 4	32	30	½ =	6	25
				Dip.		
Latitude of the ship	N. = 15	45	6			

I regret that I cannot in this paper insert the dip which I have found in my observations; for I only retained the latitude of the ship determined by it, as is usual at sea; I generally rejected the error of the instrument, the dip, and semi-diameter, as they affect both observations with the same signs, and reduced the observation by the following method:

	South.			North.					
Sun's dist. as before	78°	36′	30″	101°	1′	20″			
Refraction	—	0	11	+	0	11			
Dis. corr. for refraction	78	36	19	101	1	31	101°	1′	31″
				+ 78	36	19			
			Sum	179	37	50			
Sum of s. diam. dip, and refraction = ½ diff.	+	11	5	180			+	11	5
	78	47	24	Diff.	22	10			
				½	11	5	101	12	36
	90						90		
The ½ dist. as before =	11	12	36	½ D. =			11	12	36

It may be observed, that neither the dip, semi-diameter, nor index error, can affect the zenith distance of the sun's centre; and the refraction being small near the zenith, the result must be true if the angles are accurately taken; and it is only necessary to observe, that when the sum of the distances is less than 180°, the half difference must be added to the distances, as by the last reduction. There is a difficulty in making this observation when the sun passes the meridian very near the zenith, as the change in azimuth from east to west is too quick to allow sufficient time; nor can it be obtained by the sextant when the sun passes the meridian more than 30 degrees from the zenith: for I never could adjust the back observation of the Hadley's quadrant with sufficient accuracy to be depended on.

III. Researches on the Chief Problems of Nautical Astronomy. By Joseph de Mendoza Rios, Esq., F. R. S. From the French. p. 43.

In these researches the author proposes to consider the chief problems of nautical astronomy in a general manner, and to give formulas for all the cases. He divides the subject into 2 parts. In the first part he comprises what relates to the determination of the latitude by 2 solar altitudes; also the calculation of the hour angle by the observed height of a star, and reversely the height by the hour angle. The subject of the 2d part is the reduction of the observed distances of the moon from the sun or a star, for determining the longitude.

In this paper Mr. Mendoza has collected together a great number of rules, or analytical formulas, for the purposes above specified, expressed chiefly by means of the versed sines of arcs; for which reason he first, by way of lemmas, adopted certain analytical expressions for the versed sines, coversed sines, and supversed sines of arcs, in terms of the sines and cosines of those arcs. He then enters on the subject of finding the latitude by observation. This, it is well known, is easily done by one observation only, at noon day. But as the weather and other convenient circumstances are not always or often suitable for this purpose, other methods have been devised for cases when the sun is out of the meridian of the place, and particularly that of observing 2 altitudes and interval of time between them, or the azimuth. Among the methods for these cases, Mr. M. more especially notices those of Peter Nunnez and Cornelius Douwes; which he investigates and transforms in many different ways.

Having noticed the formulas for the latitude, Mr. M. then, in the 2d part of the paper, in like manner, collects the various rules for the longitude of the place, according to what is called the lunar method, or that by the observed distance between the moon and the sun by day, or the stars by night. And here their chief excellencies consist in the reduction of the observed to the true distance of these luminaries, by a proper reduction on account of parallax and refraction; for which purpose various modes are given. But as the uses of these formulas are best shown by means of logarithmic and other tables, adapted expressly for the purpose, we cannot do better, for this purpose, than refer to the ingenious author's very elaborate collection of tables and rules, published in the year 1805, under the title, "A Complete Collection of Tables for Navigation and Nautical Astronomy," where the above formulas are applied to practice. To this paper is added another new method for the same purpose as this 2d part, by Mr. Cavendish, as follows.

Extract of a Letter from H. Cavendish, Esq. to Mr. Mendoza y Rios, Jan. 1795.

" The methods in which the whole distance of the moon and star is computed, particularly yours, require fewer operations than those in which the difference of the true and apparent places is found; but yet, as in the former methods, it is necessary either to take proportional parts, or to use very voluminous tables; I am much inclined to prefer the latter. This induced me to try whether a convenient method of the latter kind might not be deduced from the fundamental proposition used in your paper, and I have obtained the following, which has the advantage of requiring only short tables, and wanting only one proportional part to be taken, and I think seems shorter than any of the kind I have met with.

" Let h and H be the apparent and true altitude of the star; l and L the apparent and true altitude of the moon, g and G the apparent and true distance of the moon and star. Let the sine and cosine of $g = d$ and δ, the sine and cosine of $l = a$ and α, the sine and cosine of $h = b$ and β; and the sine of the actual and mean horizontal parallax $= p$ and π; and let the sine of $\mathrm{L} = a - m + pe$, and its cosine $= \alpha\ (1 + \mu - p\varepsilon)$ and let the sine of $\mathrm{H} = b - n$, and its cosine $= \beta\ (1 + \nu)$.

" Then the cosine of $\mathrm{G} = \delta\,(1 + \mu - p\varepsilon)\ (1 + \nu) + (a - m + pe)\ (b - n) - ab\ (1 + \mu - p\varepsilon)\ (1 + \nu)$, which equals $\delta + \delta\mu + \delta\nu - \delta p\varepsilon + \delta\mu\nu - \delta p\varepsilon\nu + ab - bm + bpe - an + nm - npe - ab - \alpha b\mu + abp\varepsilon - ab\nu - ab\mu\nu + ab\nu p\varepsilon = \delta + \delta\mu + \delta\nu - \delta p\varepsilon - bm - ba\mu + bpe + bap\varepsilon - an - ab\nu + nm - npe - ab\mu\nu + ab\nu p\varepsilon + \delta\mu\nu - \delta\pi\varepsilon\nu$.

" To make use of this rule, it must be considered that the quantity $\delta\mu\nu - \delta p\varepsilon\nu$ is so small that it may safely be disregarded; but $nm - npe - ab\mu\nu + ab\nu p\varepsilon$, if the altitudes are not more than 5°, may amount to about 12″, and therefore ought not to be neglected. The quantity $e + a\varepsilon$ also differs very little from 1, but is not quite equal to it. Let therefore a table be made under a double argument, namely, the altitudes of the moon and star, giving the value of .... $nm - n\pi e - ab\mu\nu + ab\nu\pi\varepsilon + b\pi e + ba\pi\varepsilon - b\pi$, answering to different values of these altitudes, which call A. Let a 2d table be made under a double argument, namely, the altitude of the star and the apparent distance of the moon and star, giving the value of $\delta\nu$, which call D. Let a 3d table be made with the observed altitude for argument, giving the logarithm of $am + a^2\mu$; and let this quantity, answering to the moon's altitude, be called M, and that answering to the star's altitude, N; observing that the same table will do for the moon and star; but a 4th table should be made for the sun, so as to include its parallax; and, lastly, let a 5th table be made, with the moon's altitude for argument, giving the logarithm of $\frac{\varepsilon}{a} = \frac{\mu}{\pi a}$, which call C. Then will cos.

$$\mathrm{G} = \delta - \delta ap\mathrm{C} - \frac{b\mathrm{M}}{a} - \frac{a\mathrm{N}}{b} + bp + \mathrm{D} - \mathrm{A}.$$

" It must be observed that $\delta ap\mathrm{C} = \delta p\varepsilon - \frac{\delta\mu p}{\pi}$, whereas it ought to equal $\delta p\varepsilon$

— $\delta\mu$; but μ cannot exceed 57″, and the horizontal parallax cannot differ from the mean by more than $\frac{1}{15}$ part of the whole; so that the error arising from thence cannot exceed 3″ or 4″. This small error however may be diminished by giving the quantity C for more than one horizontal parallax."

Addition to the foregoing Letter.—" I have procured tables of the above-mentioned kind to be computed, which are intended to be inserted in a work now printing by Mr. Mendoza y Rios. Allowance is made in them for the alteration of the refractive power of the atmosphere, which is done by 2 new tables; one giving the correction of the logarithms M and N, and the other the sum of the corrections of $\delta\mu$ and $\delta\nu$. Now it must be observed, that the quantities μ and ν vary only from 57″ to 51″; and therefore the corrections of $\delta\mu$ and $\delta\nu$, may, without any material error, be considered as the same at all altitudes; and therefore the sum of the corrections may be comprehended in a table, under a double argument, namely, the refractive power of the atmosphere and the apparent distance.

" In order to avoid as much as possible the inconvenience arising from using negative quantities, or giving different cases, the table D is continued to 125° of apparent distance, and the numbers in the table A are increased by 0.0003, so as to make them always positive; and to compensate this, the numbers in D are increased by 0.0002, and those in the correction of $\delta\mu + \delta\nu$ by 0.0001. It was found proper also to give the table C for 4 different values of horizontal parallax. The above tables are short, and do not require proportional parts to be taken. The only part of the work in which this is wanted, is in finding the angle answering to the natural cosine of the true distance. In finding the natural cosine of the apparent distance this is avoided, by neglecting the odd seconds in working the problem, and adding them to the result."

IV. On the Nature of the Diamond. By Smithson Tennant, Esq. F.R.S. p. 123.

Sir Isaac Newton having observed that inflammable bodies had a greater refraction, in proportion to their density, than other bodies, and that the diamond resembled them in this property, was induced to conjecture that the diamond itself was of an inflammable nature. The inflammable substances which he employed were camphire, oil of turpentine, oil of olives, and amber; these he called " fat, sulphureous, unctuous bodies;" and using the same expression respecting the diamond, he says, it is probably " an unctuous body coagulated." This remarkable conjecture of Sir Isaac Newton has been since confirmed by repeated experiments. It was found, that though the diamond was capable of resisting the effects of a violent heat when the air was carefully excluded, yet that on being exposed to the action of heat and air, it might be entirely consumed. But as the sole object of these experiments was to ascertain the inflammable nature of the diamond, no attention was paid to the products afforded by its combustion; and it still therefore remained to be determined whether the diamond was a distinct substance, or one of the known inflammable bodies. Nor was any attempt made to decide this question till M. Lavoisier, in 1772, undertook a series of experiments for this purpose. He exposed the diamond to the heat produced by a large lens, and was thus enabled to burn it in close glass vessels. He observed that the air in which the inflammation had taken place had become partly soluble in water, and precipitated from lime-water a white powder which appeared to be chalk, being soluble in acids with effervescence. As M. Lavoisier seems to have had little doubt that this precipitation was occasioned by the production of fixed air, similar to that which is afforded by calcareous substances, he might, as we know at present, have inferred that the diamond contained charcoal; but the relation between that substance and fixed air, was then too imperfectly understood to justify this conclusion. Though he observed the resemblance of charcoal to the diamond, yet he thought that nothing more could be reasonably deduced from their analogy, than that each of these substances belonged to the class of inflammable bodies.

As the nature of the diamond is so extremely singular, it seemed deserving of further examination; and it will appear from the following experiments, that it consists entirely of charcoal, differing from the usual state of that substance only by its crystallized form. From the extreme hardness of the diamond, a stronger degree of heat is required to inflame it, when exposed merely to air, than can easily be applied in close vessels, except by means of a strong burning lens; but with nitre its combustion may be effected in a moderate heat. To expose it to the action of heated nitre free from extraneous matters, I procured a tube of gold, which by having one end closed might serve the purpose of a retort, a glass tube being adapted to the open end for collecting the air produced. To be certain that the gold vessel was perfectly closed, and that it did not contain any unperceived impurities which could occasion the production of fixed air, some nitre was heated in it till it had become alkaline, and afterwards dissolved out by water; but the solution was perfectly free from fixed air, as it did not affect the transparency of lime-water. When the diamond was destroyed in the gold vessel by nitre, the substance which remained precipitated lime from lime-water, and with acids afforded nitrous and fixed air; and it appeared solely to consist of nitre partly decomposed, and of aerated alkali.

In order to estimate the quantity of fixed air which might be obtained from a given weight of diamonds, 2½ grs. of small diamonds were weighed with great accuracy, and being put into the tube with ¼ oz. of nitre, were kept in a strong red heat for about an hour and a half. The heat being gradually increased, the nitre was in some degree rendered alkaline before the diamond began to be inflamed, by which means almost all the fixed air was retained by the alkali of the nitre. The air which came over was produced by the decomposition of the nitre, and contained so little fixed air as to occasion only a very slight precipitation from lime-water. After the tube had cooled, the alkaline matter contained in it was dissolved in water, and the whole of the diamonds were found to have been destroyed. As an acid would disengage nitrous air from this solution as well as the fixed air, the quantity of the latter could not in that manner be accurately determined. To obviate this inconvenience, the fixed air was made to unite with calcareous earth, by pouring into the alkaline solution a sufficient quantity of a saturated solution of marble in marine acid. The vessel which contained them being closed, was left undisturbed till the precipitate had fallen to the bottom, the solution having been previously heated that it might subside more perfectly. The clear liquor being found, by means of lime-water, to be quite free from fixed air, was carefully poured off from the calcareous precipitate.* The vessel used on this occasion was a glass globe, having a tube annexed to it, that the quantity of the fixed air might be more accurately measured. After as much quicksilver had been poured into the glass globe containing the calcareous precipitate as was necessary to fill it, it was inverted in a vessel of the same fluid. Some marine acid being then made to pass up into it, the fixed air was expelled from the calcareous earth; and in this experiment, in which 2½ grs. of diamonds had been employed, occupied the space of a little more than 10.1 oz. of water. The temperature of the room when the air was measured, was at 55°, and the barometer stood at about 29.8 inches.

From another experiment made in a similar manner with 1 gr. and a half of diamonds, the air obtained occupied the space of 6.18 oz. of water, according to which proportion the bulk of the fixed air from 2 and ½ gr. would have been equal to 10.3 oz.

The quantity of fixed air thus produced by the diamond, does not differ much from that which, according to M. Lavoisier, might be obtained from an equal weight of charcoal. In the Memoirs of the French Academy of Sciences for the year 1781, he has related the various experiments which he made to ascertain the proportion of charcoal and oxygen in fixed air. From those which he considered as most accurate, he concluded that 100 parts of fixed air contain nearly 28 parts of charcoal and 72 of oxygen. He estimates the weight of a cubic inch of fixed air under the pressure and in the temperature above-mentioned, to be .695 parts of a grain. If we reduce the French weights and measures to English, and then compute how much fixed air, according to this proportion, 2½ grs. of charcoal would produce, we shall find that it ought to occupy very nearly the bulk of 10 oz. of water.

M. Lavoisier seems to have thought that the aerial fluid produced by the combustion of the diamond was not so soluble in water as that procured from calcareous substances. From its resemblance however, in various properties, hardly any doubt could remain that it consisted of the same ingredients; and I found, on combining it with lime, and exposing it to heat with phosphorus, that it afforded charcoal in the same manner as any other calcareous substance.

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* If much water had remained, a considerable portion of the fixed air would have been absorbed by it. But by the same method as that described above, I observed, that as much fixed air might be obtained from a solution of mineral alkali, as by adding an acid to an equal quantity of the same kind of alkali.—Orig.

V. A Supplement to the Measures of Trees, printed in the Phil. Trans. for 1759. *By Robert Marsham, Esq. F. R. S.* p. 128.

These measures were all taken by myself, except the 2d, of the ash in Scotland; and that I believe is fair. As that is the largest ash, and as thriving as any I had seen, I was desirous to procure a 2d measure of it. The measures, where there was no impediment, were taken at 5 feet from the earth, as the easiest height to run the line even, and a fair height for the bulk of the body. For most trees, at least oaks and chesnuts, are frequently found to be $\frac{1}{5}$ more in circumference at 1 foot than at 5. Where I have measures of more than one tree of the same kind, I give the largest and a smaller, to show the different proportion of the increase of their different sizes: and as trees standing single generally increase more than those in groves, I mark them with an S. and a G. as the difference is more than would be expected by those that think little of trees.

In 1719 I had about 2 acres sowed with acorns, and from 1729 to 1770 I planted oaks from this grove, always leaving the best plants standing for the future grove: but most of the transplanted trees are already larger than those that were not removed; the largest of which is now (1795) but 5 feet 6 inches 8 tenths in circumference; and the largest transplanted tree, which was planted in 1735, is 8 ft. 8 in. 7 tenths, viz. near 38 inches gained by transplanting in 60 years. And in beeches from seed, in 1733, the largest is now (1795) but 6 feet 9 inches; and the largest transplanted beech is 7 feet 5 inches 1 tenth, viz. 8 inches larger, though the transplanted beech is 8 years younger than that from the seed. This proves that it is better to plant a grove, than to raise one from the seed. The expence of planting is inconsiderable, and the planted trees are full as good and handsome; and many years are saved, besides the extra growth of planted trees. But this extra growth will not prove near so great in groves as in single trees. The first grove I planted from these acorns of 1719, was in 1731. In 1732 I made another grove from them; and in 1735 I planted a third grove from them; and in 1753 the last considerable number of plants were taken from the grove, and these are very good trees: so 34 years may be saved. But I would by no means advise the planting trees so large, as the trouble and expence will be too much, unless where a shelter or screen is wanted.

Whether a grove is to be raised from seeds, or planted, it is advisable to shelter it round; if from the seed, with such sorts as will grow quicker; and if by planting, with larger and taller trees. The soil in Norfolk is unfavourable to elms; therefore in planting I will venture to recommend hornbeams, as they may be planted large trees. I planted some hornbeams, rather large, in 1757, and disliking their situation, in 1792 I removed them when they were about 3 feet in circumference, and did not lose one tree; and they made shoots of near half a yard that year; but I ought to say I cut off their heads.

It may be observed, that if young oaks are unthriving, there is reason to hope they may be helped by cutting them down to a foot or 6 inches: for in 1750 I planted some oaks from my grove of 1719 into a poorer soil, and though they lived, they were sickly; so in 1761 I cut most of them down to 1 foot, and then by cutting off the side shoots, in 3 or 4 years led them into a single stem, and most of them are now thriving and handsome trees: it can hardly be seen where they were cut off, and some are 4 feet round: I have used the same method with unhealthy chesnuts, beech, hornbeam, and wych elm, with the same success.

The aggregate Increase in Circumference of different Trees, divided into tenths of Inches of their annual Growth.

	Dates.	Feet.	Inches.	10ths of In.	Feet.	Inches.	10ths of In.	Years.	10ths of In.
S. Oak, in the Holt Forest, by the Lodge	1759	34	0	2 +					
	1778	34	0	7 +					$\frac{1}{38}$
S. Oak, in Stratton, planted in 1580, at 4 feet	1760	15	2	9					
	1781	16	5	8	1	2	9	21	.. + 7
S. Oak, planted by me, in 1720	1742	2	11	2					
	1781	8	2	6	5	3	4	39	.... $16\frac{1}{4}$
S. Oak, acorn in 1719, and transplanted 1735	1756	3	6	0					
	1781	7	2	2	3	8	2	25	about $17\frac{2}{3}$
S. Wych elm, in Stratton Hollow, at 4 feet	1760	29	5	6					
	1780	29	10	0	0	4	4	20	.... $2\frac{1}{5}$
S. Wych elm by Bradly church, Suffolk	1754	25	5	4					
	1765	26	0	6	0	7	2	11	.... $6\frac{1}{2}$
G. Wych elm, in Stratton	1787	3	9	0					
	1795	4	6	0	0	9	0	8	.. + 11
S. Ash, in Benel church-yard N. of Dunbarton, Scotland	1768	16	9	0					
	1783	18	0	0	1	3	0	15	 10
S. Ash, in Stratton, planted after 1647	1742	9	10	5					
	1782	12	11	2	3	0	7	40	.. + 9
S. Ash, planted 1725, in very poor land	1769	5	5	0					
	1781	6	6	1	1	1	1	12	near 11
S. Chesnut, in Christ Church Park, by Ipswich	1763	15	8	5					
	1747	16	11	2	1	2	7	16	.. + 9
S. Chesnut, in Hevingham, Norfolk, planted 1610	1742	12	7	0					
	1781	14	11	2	2	4	2	39	near $7\frac{1}{2}$
S. Beech, in Christ Church Park, by Ipswich	1755	15	7	5					
	1763	15	10	6	0	3	1	8	near 4
S. Beech, in Stratton, seed 1741, washed and dried	1778	3	7	4					
	1781	4	4	4	0	9	0	3	 30
G. Beech, same age	1785	3	10	5					
	1795	5	1	5	1	3	0	10	 15
S. Plane, in Shottisham, Norfolk	1755	3	10	3					
	1774	7	9	2	3	10	9	19	.. + $24\frac{2}{3}$
S. Poplar, black, set in my father's time	1756	11	5	0					
	1768	12	2	4	0	9	4	12	near 8
S. Poplar, black, in Horstead, Norfolk	1750	6	1	0					
	1754	7	4	0	1	3	0	4	.... $37\frac{1}{2}$
S. Poplar, white Abele	1760	0	7	0					
	1781	4	3	5	3	8	5	21	.. + 21

	Dates.	Feet.	Inches.	10ths of In.	Feet.	Inches.	10ths of In.	Years.	10ths of In.
S. Willow	1756	5	0	0					
	1765	6	4	2	1	4	2	9	 18
G. Alder, in sandy soil	1759	2	0	4					
	1776	3	4	7	1	4	3	17	.... + 9½
S. Asp	1772	2	8	7					
	1781	4	2	0	1	5	3	9	.. + 19
G. Mountain ash	1759	2	2	7					
	1781	4	2	4	1	11	7	22	.. + 10¾
G. Birch	1759	2	10	4					
	1768	3	6	2	0	7	8	9	.... 8⅔
G. Horse-chesnut	1758	1	4	4					
	1779	3	0	2	1	7	8	21	near 9½
G. Lime, in sandy soil	1777	3	2	5					
	1783	3	9	0	0	6	5	6	near 11
G. Cedar, one foot high in 1748	1777	3	1	6					
	1795	6	1	5	2	11	9	18	almost 20
G. Silver fir, planted in 1746	1758	1	6	5					
	1781	4	10	6	3	4	1	23	near 18
G. Scotch fir, planted in 1735	1756	4	1	5					
	1781	6	8	0	2	6	5	25	.... 12⅕
G. Spruce fir, planted 1735	1756	3	4	9					
	1781	5	2	0	1	9	1	25	near 8½
S. Weymouth pine, planted in 1747	1756	1	4	1					
	1781	4	8	5	3	4	4	25	.. + 16
G. Pinaster, planted in 1738	1756	4	0	7					
	1762	4	11	5	0	10	8	6	 16
G. Larch, planted 1749	1758	1	5	2					
	1781	4	2	5	2	9	3	23	near 14½
S. Holly, from seed, by me, and transplanted	1749	1	10	4					
	1781	3	9	1	1	10	7	32	.... + 7
S. Hawthorn, by Hethel church, Norfolk, at 4 ft.	1755	9	1	0					
	1781	9	8	5	0	7	5	26	near 3

VI. On the Periodical Changes of Brightness of Two Fixed Stars. By Edward Pigott, Esq. p. 133.

The discoveries which at present I have the honour of laying before the R. S. are the periodical changes of brightness of 2 stars, one in Sobieski's Shield, the other in the Northern Crown. The constellation of Sobieski's Shield consists of a very few stars, and was formed by Hevelius, in honour of a king of Poland; the variable star that now appears in it was doubtless not noticed by him, as he has set down stars near it, which are by times much less conspicuous. It has nearly the same right ascension as the star *l*, and is about 1° more south. When at its full and least brightness, it attains in different periods, different degrees of brightness: I have never yet seen it of a greater magnitude than of the 5th, nor when at its least, less than the 7.8th. It completes all its changes in about 63 days, being 14 ± at its full brightness, without any perceptible change; 9 ± at its least, also without any perceptible change; 28 ± days decreasing from the middle of its full brightness to the middle of its least; and 35 ± increasing from the middle of its least brightness to the middle of its full. These results being deduced from only the few observations I have made, cannot of course be very accurate, but may easily and soon be corrected by comparing any future observation with those communicated in this paper.

From the observations in the journal the periodical changes were deduced as follows: The length of a single period of 67 days, being first settled, from a succession of observations between March and May, and of 69 between April and June, we may proceed to obtain a greater exactness from dates, thus:

				Days.
			Middle of its greatest brightness.	
1795, 1796,	Oct. June	1st 18	Interval of 4 periods, making the length of a single one	65¼
1795, 1796,	Oct. April	1 10	Interval of 3 periods, making the length of a single one	64
			Middle of its least brightness.	
1795, 1796,	Nov. May	6 10	Interval of 3 periods, making the length of a single one	62
1795, 1796,	Nov. March	6 4	Interval of 2 periods, making the length of a single one	59½
			A single period, on a mean	62¾

Had it been requisite to have given any preference to one of these 4 results, I should have chosen the 3d; not only on account of the exactness of the observations themselves, but particularly because the changes when near its least brightness are quicker; however, they all agree more satisfactorily than I think could be expected; still it must be remembered, that the mean period here determined is merely for this set of observations, it being yet unknown what kind of irregularities it is liable to; for while I am now writing, in the month of August, its changes seem different from those of the preceding 4 periods; and how these perturbations will terminate, cannot be settled in the present account, as I mean here to conclude it.

The other variable that I have discovered is, as already mentioned, in the Northern Crown. Its right ascension is 235° 2′ 51″, and declination 28° 49′½. This star, though not in Flamsteed's catalogue, is marked on Bayer's maps of the 6th magnitude. Several years ago, in 1783, 1784, and 1785, I suspected it to be changeable, which induced me to make the memorandums here copied in the journal, since which time I have often seen it, but not perceiving any alteration, the dates were neglected till the spring of 1795; I then had the satisfaction of finding my suspicions confirmed, it being invisible; but on the 20th of June, it appeared of the 9.10th magnitude, and went through its various changes as follows: in 6 weeks it had increased to its full brightness, the middle time of which was August 11th, 1795. At its full brightness it was of the 6.7th magnitude, and remained the same without any perceptible alteration for about 3 weeks: it then was 3½ weeks in decreasing to the 9.10th magnitude, and disappeared a few days after.

Having re-appeared in the following April, 1796, it was on the 7th of May again of the 9.10th magnitude, and increasing nearly in a similar manner as on the 20th of June the preceding year; which completes all its changes, and gives a period of 10½ months.

Very remarkable and perplexing it was, that just after I had made out the periods of these 2 variable stars, their changes should appear different from those before observed; the particulars concerning that in Sobieski's Shield have been noticed: as for this in the Northern Crown, it shows at present (being the computed time of its full brightness), great unsteadiness, more so I think than any of the variables whose periods have been settled with certainty; for having increased as before, with tolerable regularity, till it attained the 7.8th magnitude, it then kept wavering between those magnitudes, and is still so at the present time (August) that I am closing my account of it.

VII. Experiments and Observations, made with the View of ascertaining the Nature of the Gaz produced by passing Electric Discharges through Water. By George Pearson, M. D., F. R. S. p. 142.

§ 1. In the Journal de Physique for Nov., 1789, were published the very curious and interesting experiments of Messrs. Paets van Troostwyk and Deiman, which were made with the assistance of Mr. Cuthbertson, on the apparent decomposition of water by electric discharges. The apparatus employed was a tube 12 inches in length, and its bore ⅛ of an inch in diameter, English measure; which was hermetically sealed at one end, but before it was sealed, 1½ inch of gold or platina wire was introduced within the tube, and fixed into the closed end by melting the glass around the extremity of the wire. Another wire of platina, or of gold with platina wire at its extremity, immersed in quicksilver, was introduced at the open end of the tube, which extended to within ⅝ of an inch of the upper wire, which, as was just said, was fixed into the sealed extremity.

The tube was filled with distilled water, which had been freed from air by means of Cuthbertson's last improved air-pump, of the greatest rarefying power. As the open end of the tube was immersed in quicksilver, a little common air was let up into the convex part of the curved end of the tube, with the view of preventing fracture from the electrical discharge. The wire which passed through the sealed extremity was set in contact with a brass insulated ball; and this insulated ball was placed at a little distance from the prime conductor of the electrical machine. The wire of the lower or open extremity, immersed in quicksilver, communicated by a wire or chain with the exterior coated surface of a Leyden jar, which contained about a square foot of coating; and the ball of the jar was in contact with the prime conductor.

The electrical machine consisted of 2 plates of 31 inches in diameter, and was similar to that of Teyler. It had the power of causing the jar to discharge itself 25 times in 15 revolutions. When the brass ball and that of the prime conductor were in contact, no air or gaz was disengaged from the water by the electrical discharges; but on gradually increasing their distance from each other, the position was found in which gaz was disengaged; and which ascended immediately to the top of the tube. By continuing the discharges, gaz was discharged till it reached to nearly the lower extremity of the upper wire, and then a discharge occasioned the whole of the gaz to disappear, a small portion excepted, and its place was consequently supplied by water.

From my own experience I should venture to affirm, that a more particular and more accurate account than that published is requisite, to enable the student, or even the proficient, to institute the above experiment with success. Hence, during the 6 or 7 years which have elapsed since its publication, no confirmation has been published, except the experiment repeated by Mr. Cuthbertson for my satisfaction, as related in my work on the Chemical Nomenclature; though I have heard of many persons, and some of them experienced electricians and chemists, who have made the attempt. But by labouring with Mr. Cuthbertson, since he came to reside in London, I have learned the circumstances on which the success of the experiment depends; and I have received from him effectual aid in continuing a process, with the objects I had in view, the tediousness and even difficulties of which can only be conceived by those who have been engaged in the same pursuit. In the course of my experiments on this subject, Mr. Cuthbertson invented a new method of disengaging gaz from water, by means of the electrical discharges, namely, by means of uninterrupted or complete discharges; whereas the method of Mr. Van Troostwyk was by interrupted discharges. The rationale of the process according to these two methods, I apprehend, cannot be understood without an explanation; for I find books on electricity do not contain the necessary information.

In the experiment of Mr. Van Troostwyk, it must be considered, that if instead of water the tubes be filled with air, the whole of the charge of the Leyden jar will pass, at each explosion, from the upper to the under wire, and no interruption in the discharge will happen; but if they are filled with water, then an interrupted discharge may be caused: by which is meant, that a part of the change only passes at each explosion through the water from wire to wire, and with much diminished velocity. The residuary electricity in the Leyden jar is nearly one half, as may be accurately demonstrated. The reason of these differences must be assigned from the difference in point of density, elasticity, and conducting power, of the medium of water and of air. It must be added, that though water in large quantity is a good conductor, and air is not, yet water being here in very small quantity it proves a bad conductor; as is the case with the very best conductors. A cubic foot of water is only just capable of receiving, or letting pass through it, a full discharge from a jar of one foot of coated surface; and the quantity of water employed in this experiment not being $\frac{1}{100,000}$ part of a cubic foot, it is a very imperfect conductor; so that an interrupted discharge only can pass through the tube, without dispersing the whole of the water. But if the discharge be not seemingly as strong as the tube can bear without breaking, the gaz is not produced from it; and on this point hinges this extremely delicate process. The situation of the different parts of the apparatus for the interrupted discharge is shown by pl. 2, fig. 12.

To succeed by the method of the complete or uninterrupted discharge, the apparatus now to be described must be used, and the following rules must be observed. 1. A tube, fig. 13, is employed, about 4 or 5 inches in length, and its bore $\frac{1}{5}$ or $\frac{1}{6}$ of an inch in diameter. One end is mounted with a brass tube, fig. 14, and the other end is sealed at the lamp with a wire, about $\frac{1}{40}$ of an inch in thickness, fitted into it, as above described; which extends into the brass tube, so as to be almost in contact where the explosion is made. If the wire touches the brass tube, there will be no gaz produced. The tube being filled with water, and set in a cup of water, the discharge may be made into it, as in the above described process of Mr. Van Troostwyk; but here the insulated ball must be placed at a greater distance from the prime conductor, and a Leyden jar with only 50 square inches of coating will answer the purpose. In this way of making the experiment gaz is produced by each discharge, in the brass tube; and in much greater quantity, and with much less frequent accidents, and less trouble, than in the former method with the interrupted discharge. But the gaz obtained with this apparatus always contains a large proportion of atmospherical air, on account of the quantity of water, and more immediate and extensive communication of it with the atmosphere. By repeated discharges there is an impression made in the brass tube, in the part where the discharge passes through it, and at last a small hole is made in that part. On this account the same mounted tube cannot serve for producing a large quantity of gaz.

2. The other sort of apparatus, invented by Mr. Cuthbertson, is represented by fig. 15. At first it consisted of a glass tube $\frac{1}{4}$ an inch wide, and about 5 inches in length, mounted at 1 end with a brass funnel, and inverted in a brass dish; but afterwards the tube was blown funnel-wise at the end, as shown by fig. 16. The other end must have a wire, about $\frac{1}{40}$ of an inch thick, sealed into it at the lamp; which wire extends to nearly the bottom of the brass dish in which the tube stands. The exact distance between the end of the wire and brass dish must be found by trials; that which generally answered in my experiment was about $\frac{1}{20}$ of an inch. If it be properly arranged, gaz will be produced at each discharge. The Leyden jar used with this apparatus, must contain above 150 square inches of coating. The distance between the insulated ball and the prime conductor, at which the experiment succeeded, was commonly about $\frac{1}{4}$ an inch. If experiments be proposed in which electric discharges must be passed through water, or other fluids, for even a much longer time than was consumed in performing those referred to, or related in this paper; it may be an object to employ the wind, or perhaps the power of a horse, to turn the electrical machines; the expence of labourers being considerable.

§ 2. *Experiments.*—From my journal of the numerous experiments, made during the course of nearly 2 years, I shall select those which will serve to explain the nature of the process, and show the power of the plate electrical machines; and I shall particularly relate those experiments which afforded the most useful results concerning the nature of the gaz obtained.

1. *With interrupted discharges.—Exper.* A. About 1600 of these discharges, by means of a 34 inch single plate electrical machine, in nearly 3 hours, produced, from New River water taken from the cistern, and which had not been freed from air by the air-pump or boiling, a column of gaz $\frac{2}{3}$ of an inch in length and $\frac{1}{9}$ of an inch wide. On passing through this gaz, between the 2 wires of the tube in which it was produced, a single electric spark, its bulk was instantly diminished to $\frac{2}{3}$. In other experiments the bulk of gaz was only diminished to about $\frac{1}{2}$. And the result was the same with distilled water.

B. The experiment A being repeated several times, with distilled and New River water, freed from air by the air-pump or long boiling, the quantity of gaz just mentioned was obtained in about 4 hours. On passing an electric spark through this gaz, in the situation above-mentioned, its bulk was instantly diminished, in some cases $\frac{15}{16}$, and in others $\frac{19}{20}$.

C. 1600 interrupted discharges, by means of a 32 inch plate machine, produced, from New River water and distilled water freed from their air by the air-pump, a column of gaz about $\frac{3}{4}$ of an inch in length, and $\frac{1}{9}$ of an inch in diameter, in the space of 3 hours. It was reduced in bulk $\frac{19}{20}$ by passing through it a single electric spark.

D. 500 revolutions of the 32 inch plate machine, in $\frac{3}{4}$ of an hour, produced 600 interrupted discharges in river water, freed from air by the air-pump, by which a column of gaz, $\frac{1}{2}$ an inch in length and $\frac{1}{10}$ of an inch in diameter, was obtained. It was diminished, as usual, by an electric spark, $\frac{19}{20}$ of its bulk.

E. Nearly 4 days incessant labour, with the 32 inch plate machine, produced only 56.5488 cubes of gaz, of $\frac{1}{10}$ of an inch each; on account of the usual accidents during the process. The air had been exhausted, by setting the water under the receiver of the air-pump.

F. It was found that 6000 interrupted discharges produced about 3 inches in length of gaz, measured in a tube $\frac{3}{20}$ of an inch in width, from water out of which its air had been drawn by the air-pump.

G. It appeared, from many experiments, that the same unboiled water, or water from which the air had not been exhausted by the air-pump, which had repeatedly yielded gaz by passing through it electric discharges, always left a residue of gaz, which the electric spark did not diminish; and this residue was in nearly the same quantity, after 6 or 7 experiments, each of which afforded a column of gaz, $\frac{1}{2}$ an inch in length, and $\frac{1}{9}$ of an inch in diameter, as was left on passing the electric spark, through the gaz, afforded by the 3d or 4th experiment.

Hence it seems, that water is decompounded by the electric discharge, before the whole of the common or atmospherical air is detached from the water, by merely the impulse of each discharge. Yet I think it probable that, after the discharges have been passed through the same water for a certain time, the whole of the air contained in water will be expelled, and no gaz be produced, but that compounded by means of the electric fire from water; in which case, supposing the gaz so produced to be at last merely hydrogen and oxygen gaz, it will totally disappear on passing through it an electric spark. But I have never been able to determine this point; because the tubes were always broken after obtaining a few products, or long before it could reasonably be supposed the whole of the air of the water was expelled from it.

H. To the gaz obtained in the experiment E was added, over water, an equal bulk of almost pure nitrous gaz. Fumes of nitrous acid appeared, and the gaz examined was reduced almost $\frac{1}{3}$ of its bulk. A small bubble more of nitrous gaz being let up, no farther diminution took place. To this residue was added half its bulk of oxygen gaz, obtained from oxymuriate of pot-ash. This mixture of gazes having stood several days over well burnt lime and boiled quicksilver, an electric spark was passed through the mixture, over quicksilver; by which its bulk was instantly diminished $\frac{1}{4}$. But no moisture could be perceived on the sides of the tube, or on the quicksilver. The failure of the appearance of moisture was imputed to a bit of lime accidentally left in the tube, which was burst by the explosion and dispersed through the tube; or else the quantity of water produced was so small, comparatively with the residuary gaz, that the water was dissolved by it in the moment of its composition. For supposing water to have been compounded, it could not amount to the $\frac{1}{100}$ part of a grain; and the residuary gaz was at least 2000 times this bulk. That a quantity of water can be compounded, under the same circumstances as in this experiment, and be apparently dissolved in air, so as to escape observation, even with a lens, was proved by passing an electric spark through a mixture of hydrogen and oxygen gaz, well dried by standing over lime.

2. *With complete or uninterrupted discharges.*—The gaz obtained by the first described kind of apparatus, for the uninterrupted discharges, always left a residue of at least $\frac{1}{4}$ of its bulk on passing through it the electric spark; even when water was used, which had been freed from air by boiling, or the air-pump. This result will not appear surprizing, when it is considered how liable the water in this apparatus is to mix and absorb air during the experiment. However, this method would have been extremely valuable if the next other method had not been discovered; for gaz may be obtained by it with fewer accidents, and much more rapidly, than with the interrupted discharges. The apparatus is also much more easily fitted up, and is more simple. But I think it unnecessary to particularly relate any experiments, as they afforded the same results as those already described, and as those following experiments which were made with the apparatus before described, and shown by fig. 15, 16, 17.

Exper. 1. At 0^h 40^m P. M. began to produce discharges with a double plate 24 inch machine, in water taken from the cistern: and at 12^h 6^m P. M. of the same day, there had been written down 10200 discharges, each of which occasioned air to ascend from the bottom of the wire and brass cup. The quantity of air obtained was now apparently about $\frac{1}{4}$ of a cubical inch, and it occupied nearly $\frac{1}{2}$ the tube; the water in which was by this time very muddy. After standing till the day following at noon, when the process was again commenced, it did not appear that any of the gaz had been absorbed by the water over which it stood.

At 2^h 35^m P. M. began to produce discharges, and at 8^h P. M. had passed 6636; which, together with those of the preceding day, amounted to 16836. The tube was now $\frac{5}{8}$ full of gaz, and there seemed to be almost $\frac{1}{2}$ a cubical inch; for it was observed, that the gaz was this day yielded at double the rate it had been the day before. This was accounted for from the diminished pressure on the electric fire, by the tube containing gaz instead of water. At this time, namely, at 8^h P. M., I was surprized, on the passing of a discharge, by a vivid illumination of the whole tube, and a violent commotion within it; with, at the same time, the rushing up of water, instantly to occupy rather more than $\frac{5}{8}$ of the space which had been occupied by gaz. The residue of gaz was not diminished further by an electric spark; and to the test of nitrous gaz it appeared to be rather worse than atmospherical air, as it consisted of rather less than 1 part of oxygen, and 3 parts of nitrogen or azotic gaz.

It seemed as if the electrical discharge had kindled the oxygen and hydrogen gaz of the decompounded water, by flying from the bottom of the wire to the brass funnel; so that the fire returned into the tube where it passed through the gaz. Or the combustion might be occasioned by a chain of bubbles, reaching from the brass dish to the surface of the water in the tube, which was set on fire in its ascent, and thus produced combustion of the whole of the gaz of decompounded water. That this phenomenon was from the combustion here supposed, was in some degree proved by finding that the mixture of hydrogen and atmospherical air, under the same circumstances, was kindled in the same manner.

Exper. 2. With a double plate electrical machine, 24 inches in diameter, and a similar apparatus to that in the last experiment, 14600 discharges produced, at least, $\frac{1}{3}$ of a cubical inch of gaz. While I was measuring with a pair of compasses the quantity of gaz produced, the points of them being in contact with the part of the tube occupied by gaz, I was again surprized, on the passing of a discharge, by an illumination of the whole tube, and the rushing up, with considerable commotion, of water, to occupy about $\frac{2}{3}$ of the space filled by gaz. The residuary air was found, as in the former experiment, to be rather worse than atmospherical air.

It was concluded that the points of the compasses had attracted electrical fire from the wire to the sides of the glass, and so kindled the hydrogen and oxygen gaz of decompounded water. But to determine this question, I introduced into the same tube a mixture of 1 measure of oxygen and 2 measures of hydrogen gaz, to occupy nearly the same space in the tube as the gaz had occupied: then passing an electrical discharge through it, no combustion was excited; but on passing a discharge while the compasses were in contact with the tube, as just mentioned, an illumination and violent commotion were produced, with the rushing up of water, to leave only $\frac{1}{8}$ of the gaz as a residue. On repeating this experiment with 1 measure of atmospherical air and 2 of hydrogen gaz, combustion could not be excited; nor with 2 measures of atmospherical air and 1 of hydrogen; nor with 2 measures of hydrogen gaz and 1 of atmospherical air; but on adding to this last mixture 1 measure of oxygen gaz, the electrical discharge produced the phenomena of combustion just mentioned, with the rushing up of water, to occupy about $\frac{2}{3}$ of the space which was occupied by the gazes.

Exper. 3. Having passed 12000 discharges through water, with the apparatus of the preceding experiment, and thus obtained only $\frac{1}{5}$ of a cubical inch of gaz; and having observed that the quantity of gaz was not greater than it was when only 8000 discharges had been passed, and yet bubbles had been seen to be produced on each discharge as copiously, or more so, by the last 3 or 4000 discharges as before; I began to suspect that part of the gaz had been destroyed during the process, or had been absorbed. While I was considering how to account for this disappearance of gaz, and was at the same time looking at the tube through which the discharges were passing, I observed one of them to be attended with a diminution instantly, of about $\frac{1}{5}$ of the gaz produced, and with a slight commotion. I was now sure, from this phenomenon, and from the unequal augmentation of the bulk of the gaz at given times during the process, that combustion had been excited several times before; not only in the present experiment, but perhaps in the former ones, without observing it. I conceived that a gradual combustion also, very probably, took place in this process, by the kindling of bubbles of gaz in their ascent through the water. I now perceived that the discharges ought to be produced more slowly, or the tubes to be wider, to allow the bubbles to pass quite through the water, in order to avoid the accension of gaz during the process. My calculation also, that 35 to 40000 discharges were requisite to produce 1 cubical inch of gaz from water, containing its usual quantity of common air, was rendered much more vague by this accension, so often liable to be occasioned. To the gaz which remained in the tube in this experiment was added an equal bulk of nitrous gaz; the mixture diminished to 1.5; and on adding to the residue $\frac{1}{2}$ its bulk of oxygen gaz, and passing through it the electrical spark, no accension or diminution of bulk was produced. Hence all the hydrogen gaz and oxygen gaz, produced by the decomposition of the water, had been burnt during the process; the oxygen gaz thus detected being considered to be only that expelled from the water.

Exper. 4. By means of electrical discharges, with the apparatus used in the preceding experiment, I obtained gaz from New River water; letting it up into a reservoir as soon as about $\frac{1}{20}$ of a cubic inch was produced, till I had collected $\frac{1}{8}$ of a cubic inch. To this was added an equal bulk of nitrous gaz; on which the mixture diminished to 1.2; and on the addition of a little more nitrous gaz, no further diminution took place. To this residue $\frac{1}{2}$ its bulk of oxygen gaz was added, and this mixture of gazes being well dried by standing over lime and boiled quicksilver, an electric spark was passed through it, by which a diminution of $\frac{1}{6}$ of its bulk took place. A little dew was then seen on the sides of the tube where the quicksilver had risen; and, with the aid of a lens, the same appearance was perceived on the part of the tube containing the residue of gaz.

It may now be expected, that I should have made the experiments with this apparatus on distilled water freed from its air, not only by long boiling, or the air-pump, but by passing through it several hundred electrical discharges. It would also have been, to some persons, more satisfactory, if the experiments had been made on a larger scale, so as to have produced the combustion of a much larger quantity of gaz, and consequently have produced a greater quantity of water. As however I apprehend that the experiments contained in this paper, when well considered by competent judges, will be found to explain the nature of the gaz procured from water by electrical discharges; and as another very important subject demands my attention, the honour of more splendid and convincing experiments must be reserved for other inquirers. If the same sacrifices be made by them, which have been made in performing the present experiments, I think it is scarcely possible but that still further light concerning the composition of water should be obtained, as well as concerning oils, alcohol, acids, &c.; to the investigation of the composition of which, the mode of analysis and synthesis here indicated, may be applied.

§ 3. The following conclusions appear to me obvious and incontrovertible. The mere concussion by the electric discharges seems to extricate not only the air dissolved in water, which can be separated from it by boiling and the air-pump, but also that which remains in water, notwithstanding these means of extricating it have been employed. The quantity of this air varies in the same and in different waters, according to circumstances. New River water from the cistern yielded $\frac{1}{5}$ of its bulk of air, when placed under the receiver of Mr. Cuthbertson's most powerful air-pump; but in the same situation New River water, taken from a tub exposed to the atmosphere for a long time, yielded its own bulk of air. Hence the gaz produced by the first 1, 2, or even 300 explosions in water, containing its natural quantity of air, is diminished very little by an electrical spark.

The gaz or air, thus separable from water, like atmospherical air, consists of oxygen and nitrogen or azotic gaz; which may be in exactly the same proportions as in atmospherical air, for the water may retain one kind of gaz more tenaciously than the other; and on this account the air separated may be better or worse than atmospherical air, in different periods of the process for extricating it. The nature of the gaz, which instantly disappears on passing through it an electric spark, is shown by

(a) This very property of thus diminishing; and by the following properties;

(b) A certain quantity of nitrous gaz instantly disappeared, apparently composing nitrous acid, on being added to the gaz (a) H., exp. 4; oxygen gaz being added to the residue after saturation with nitrous gaz, and an electric spark being applied to the mixture of gazes, well dried, a considerable diminution immediately took place, and water was produced; exp. 4.

(c) Combustion from hydrogen and oxygen gaz took place, when the tube was about $\frac{3}{4}$ full of gaz, p. 108, exp. 1; which was confirmed by passing an electrical discharge, under the same circumstances, through a mixture of hydrogen and oxygen gaz, exp. 1.

(d) Combustion from hydrogen and oxygen gaz took place, when the points of the compasses were accidentally applied to the part of the tube containing gaz, exp. 2; which was confirmed by passing a discharge, under the same circumstances, through a mixture of hydrogen and oxygen gaz, while the points of the compasses were applied to the tube; exp. 2.

(e) The observations made of the kindling of gaz in small quantities, from time to time, during the process of obtaining it, particularly while it was ascending in chains of bubbles, or was adhering to the funnel of the tube, exp. 3, confirm the evidence in favour of this gaz being hydrogen and oxygen gaz.

The evidence contained under the heads (a)—(e), considered singly and conjunctively, I apprehend, must be admitted by the most rigorous reasoner, to be demonstrative that hydrogen and oxygen gaz were produced by passing electric discharges through water. With regard to the origin and mode of production of these 2 gazes, our present observations and experiments do not afford complete demonstrative evidence; but, though some hypotheses must be admitted, I conceive that the body of evidence we possess can afford a satisfactory interpretation of the phenomena.

Fig. 8, 9, 10, 11, pl. 2, represent the tubes used in producing gaz from water by the interrupted electric discharges. Fig. 12 represents the situation of the above tubes during the process of producing gaz from water. Fig. 13, 14, represent the tubes employed in producing gaz from water by the first method, with uninterrupted electric discharges. Fig. 15 shows the figure of the tube mounted with a brass funnel used in the 2d method of producing gaz from water by uninterrupted electric discharges. Fig. 16 represents the tube blown funnel-wise at the end, instead of being mounted with a brass funnel, as in fig. 15. Fig. 17 represents the situation of the tubes fig. 15 and 16 during the process of producing gaz by the uninterrupted electric discharges.

VIII. An Experimental Inquiry concerning Animal Impregnation. By John Haighton, M. D. p. 159.

From the experiments of De Graaf on rabbits, we learn, 1°. That the ovaries are the seat of conception. 2°. That 1 or more of their vesicles become changed. 3°. That the alteration consists in an enlargement of them, together with a loss of transparency in their contained fluid, and a change of it to an opaque and reddish hue. 4°. That the number of vesicles thus altered, corresponds with the number of fœtuses, and from these are formed the true ova. 5°. That these changed vesicles, at a certain period after they have received the stimulus of the male, discharge a substance, which being laid hold of by the fimbriated extremity of the fallopian tube, and conveyed into the uterus, soon assumes a visible vesicular form, and is called an ovum. 6°. That these rudiments of the new animal, which for a time manifested no arrangement of parts, afterwards begin to elaborate and evolve the different organs of which the new animal is composed. To these facts we may add, that the calyx or capsula which formed the parietes of the vesicles, thickens, by which the cavity is diminished. This cavity, together with the opening through which the fœtal rudiments escaped becomes obliterated, and from the parietes of these vesicles having acquired a yellowish hue, they are called corpora lutea.

But though some important facts are clearly ascertained, there are others still problematical. Physiologists are by no means agreed concerning the immediate cause of conception. All admit the necessity of sexual intercourse. They acknowledge too the necessity of some part of the female being affected by the direct contact of a fecundating fluid, but what the precise part is which must receive the stimulus, has hitherto been involved in mystery and doubt. Nor are they more unanimous respecting the state or condition of the substance that passes from the ovaries; whether at the time of its expulsion it has a circumscribed vesicular character, or whether it has no determined figure. De Graaf and Malpighi, in the last century, and some respectable physiologists of the present day, adopt the first opinion; Haller and some others favour the last.

The intention then of this essay is to explore the proximate cause of the impregnation of animals, and to trace with more accuracy the visible effects of it from their first appearance, till the rudiments of the fœtus are lodged in the uterus, and have assumed the proper characters of an ovum. As soon as these rudiments manifest that opaque spot, or "dim speck of entity," which is known to evolve the fœtus by regular and progressive steps; another stage of the inquiry then commences, viz. to trace the visible formation of the new animal through its whole course; but as this belongs rather to the economy of the fœtus than the mother, it is not intended to form any part of this paper. I perceive however, that I cannot investigate the question of the proximate cause of impregnation in a satisfactory way without first determining what are the evidences or proofs that impregnation has taken place: this then necessarily becomes a preliminary question. I therefore restrict my inquiry to the three following subjects. 1st. What are the evidences of impregnation? 2d. What is the proximate cause of impregnation? And, 3d. Under what form do the rudiments of the fœtus pass from the ovary to the uterus?

§ 1. *What are the evidences of impregnation?*—In order that I might bear evidence of the truth, that a female has conceived before there are any vestiges of a new animal, I examined with great attention the ovaries of some full-grown virgin rabbits, and found, as De Graaf has represented, that there entered into their composition a series of cells containing a transparent colourless fluid. But in none of them could I see any of those circumscribed substances, which, from their yellow colour, are called corpora lutea. But when similar observations were made on rabbits that had been impregnated at different periods, and the traces of those copora lutea were more or less evident, according to the interval of time that had elapsed; I may then say that no corpora lutea exist in virgin animals, and that whenever they are found, they furnish incontestable proof that impregnation either does exist, or has preceded. But a proper distinction between past and existing impregnation can be made only by tracing the phenomena of recent fecundation progressively, and noting the appearances in the different stages. I was therefore under the necessity of repeating with care several of De Graaf's experiments, that I might bear testimony to the truth of them, at least so far as the results coincided with my own.

Exper. Having therefore procured several virgin rabbits in a fit state for impregnation, I admitted one of them to the male. Twelve hours afterwards it was killed, and on examining the ovaries several of the vesicles evidently projected; they had lost their transparency, and were become opaque and red. When punctured, a fluid of the same colour escaped. I made sections through some of them; but at this early period the corpora lutea, which are formed by the thickening of the parietes of the vesicles, were not very evident. I therefore determined to examine them in a more advanced state.

Exper. Another rabbit being admitted to the male, I examined it 24 hours afterwards. The colour of the fluid contained in the vesicles was similar to that of the last experiment. The vesicles projected more evidently, and their thickened parietes manifesting the commencement of corpora lutea were become more apparent.

Exper. I inspected the ovaries of another rabbit 48 hours post coitum. At this period the vesicles seemed to be in the very act of bursting, and a semi-transparent substance, of a mucus-like consistence, was beginning to protrude from some of them; others indeed were less advanced. The fimbriated extremities of the fallopian tubes were preparing to receive their contents, as appeared by having quitted their usual position, and embraced the ovaries in such a degree, that only a small portion could be seen till the tubes were taken away. Sections being made into the thickened vesicles, the formation of corpora lutea appeared to have made further advances. From the appearance of an incipient rupture of the vesicles in this experiment, it was but reasonable to expect that their contents would soon have escaped; but as my views were directed to the formation of a corpus luteum, I deferred the next examination to a more distant time.

Exper. In 2 days and 12 hours after coition, I examined the ovaries of another rabbit. The fœtal rudiments had escaped; but the cavity of the ovarian vesicles had suffered but little diminution. Bristles were easily introduced by the ruptured orifices. In this experiment the advances towards the formation of a perfect corpus luteum were such as the period of examination would naturally lead us to expect. The contents of the vesicles having escaped, it was but reasonable now to look forward to a speedy obliteration of the cavity. I therefore examined these parts under similar circumstances on the 3d, 4th, and 5th day. In the last experiment there was but little vestige of cavity, consequently the corpora lutea might be considered as perfectly formed.

§ 2. *What is the proximate cause of impregnation?*—As the effect of sexual communication is so important, it cannot be indifferent to the design of nature, to what part of the uterine system the semen should be conveyed. It admits of no doubt that it either remains in the vagina, passes into the uterus, or else extends its course along the fallopian tubes to be applied to the surface of the ovaries, which it stimulates, and from which the new animal derives its existence; but whether it be one or other of these, has given birth to more physiological controversy, than perhaps any other operation of a living animal. Those who have entered the lists have ranged themselves either on the side of application of the semen to the ovaries by means of the tubes; or on that of the inutility of this process. These latter contend for an absorption of this fluid by the vagina, a peculiar excitement of the whole frame as a consequence, of which excitement the changes produced on the ovaries are to be considered the local effects. The advocates for the first opinion allege, that the semen has been seen both in the uterus and tubes, and quote as their authority the observations of Morgagni for the former, and Ruysch for the latter. When seen in this last situation, some have thought that it was conveyed thither by the muscular power of these parts in the manner of a peristaltic motion, beginning at the uterus and ending at the fimbriated termination of the tube; and when at this last, it was supposed that the semen was applied to the surface of the ovaries, and impregnated them by actual contact.

Though I shall prove that this hypothesis is altogether visionary, yet prima facie it is far from carrying with it the characters of absurdity. There is nothing repugnant to reason in contending for what analogy seems to favour, particularly when the subject is thought beyond the reach of demonstration or proof. And the analogy favourable to this opinion has probably been taken from the impregnation of frogs and toads, in which process we are told, on the authority of Roesel, Swammerdam, and Spallanzani, that the ova are impregnated by the male as they are passing from the body of the female; and that in water newts the ova are impregnated even without copulation. Now here is an appearance of contact between the fecundating fluid and the ova.

Again, on the other hand, the contact of semen with the ovaries has been thought improbable, from an analogy drawn from the vegetable kingdom; for admitting the Linnæan doctrine to be true, which contends for a necessity of sexual intercourse in vegetables, it would be difficult to demonstrate to the satisfaction of stern philosophers, that the pollen pervades the pistillum, and stimulates the contents of the pericarpium by contact, to the evolution of the germen. Such would deny the contact of semen. The advocates for either opinion then may avail themselves of analogies suited to their own mode of thinking. It may be said however, and with some colour of truth, that the latter analogy, as being more remote than the former, and as being founded on a principle which some have suspected to be gratuitous, should be received with caution and distrust. Before any deduction can be made from analogy concerning the means by which any important end is to be effected, we cannot examine the instruments performing such actions with an attention too nice or too minute. If we find nature employing different instruments, in different animals, to produce the same ultimate effect, I think it but fair to conclude, that the means used are essentially different; but the closer the resemblance in the instruments or organs, the nearer will the means approach. On this principle no conclusions can be drawn respecting the human species, from observations either on vegetables, or even on frogs, toads, and newts. Birds, as being impregnated by semen conveyed into the body, resemble human impregnation more than the former: but they differ so obviously in the mode of perfecting the fœtus from the ovum, that I hardly dare rest any thing on their general analogy. There is however a curious fact respecting them not altogether inapposite to this question, which is, the permanent effect of one coitus. I have read in the Abbé Spallanzani's dissertation, and elsewhere, that all the eggs which a hen will lay in 20 days will be impregnated at one coitus: and Mr. Cline tells me, that in Norfolk this matter is reduced to a certainty with respect to turkeys; and that even to a greater extent. There is certainly some difficulty in reconciling these facts to impregnation by contact of semen; but from the very obvious difference between oviparous and viviparous animals, I shall not press this argument further. Indeed it should always be impressed on the recollection of those who are labouring in the pursuit of truth, that arguments drawn from analogies, unless from those of the nearest relation, are better adapted to the purpose of illustration than of proof: and though they frequently find advocates in confident closet philosophers, they are received with deserved distrust by the more cautious practical physiologists.

Those who cannot admit the passage of semen by the tubes, do not neglect to take the advantage of some difficulties which their opponents have overlooked. They say, implicit confidence is not due to the observations of Morgagni and Ruysch, and that what appeared to them to be semen in the uterus and tubes, was nothing more than the mucus of the parts. They further invalidate the force of this argument by contrasting these solitary observations, with a numerous train of counterfacts; for in all the experiments made by Harvey, De Graaff, Haller, and others, it does not appear that semen was found beyond the vagina, except in one of Haller's experiments in a sheep, in which he saw semen in the uterus 45 minutes after coition. But this fact stands almost alone; and when placed in opposition to the many experiments attended with a contrary result, will weigh but little in the balance of impartial decision. Yet he rested much on this one fact, and adduced it in support of his opinion, that whenever impregnation happened, the semen passed into the uterus, and was retained; but when it returned from the vagina, then the animal remained unimpregnated. In this latter case, he supposes the semen had never passed beyond the vagina; for if it had, he says it would have been retained. This argument he thinks is unanswerable.

The insufficiency of this reasoning did not escape the penetration of his opponents; and the immense mass of counterfacts poured out against him, like an irresistible torrent, bore away the very foundation of his doctrine. This brings the advocates for the necessity of the contact of semen with the ovaries into a dilemma, from which they attempt to extricate themselves by contending, that fecundation does not require the application of semen to the ovaries in a palpable form; but that there is exhaled from it a subtile fluid in a vaporific state, called aura seminalis, and that the contact of this vapour is fully sufficient to impart to the ovaries their due quantity of stimulus. But the opinion, even thus qualified, has not passed without animadversion. There are some who cannot comprehend how the tubes should perform 2 motions in contrary directions, which they must do, if they first convey the aura seminalis to the surface of the ovaries, and afterwards return the rudiments of the fœtus into the uterus. Such a double action they think is repugnant to the economy of the part, but assign no reason for their opinion. They might with equal propriety deny the possibility of a peristaltic and inverted peristaltic motion of the intestines, or the opposite actions in the œsophagus of ruminant animals, though I am persuaded very few would acquiesce in their incredulity.

The difficulties which were opposed to the conveyance of the semen by the tubes, were, as we should suspect, intended to prepare the way for a different explanation; therefore physiologists, by a very natural transition of thought, were led to suppose that the presence of semen in the vagina alone was sufficient to account for impregnation. To give support to this opinion, cases were adduced, in which, from some anatomical peculiarities, it seemed almost impossible that the fecundating fluid could be conveyed into the uterus; and yet in several of these cases impregnation had really taken place. Those who hold the contrary opinion, either cavil at the accuracy of the statement, or draw a different conclusion; therefore to attempt conviction by these materials would be to engage in the service of forlorn hope. It remains then to try whether by a patient experimental investigation, we can make such an accession of new facts to our present stock of knowledge, as will enable us to undo this Gordian knot. This attempt naturally leads us to review the 2 points of the question, viz. Is the passage of the semen by the tubes to the ovaries, essential to impregnation? If not, what other means are employed? If it be true that the fecundating fluid must pass by the tubes to the ovaries before impregnation can take place, ought it not to follow, as a consequence, that if, from any cause, both these tubes be obliterated, the animal so affected would be barren? Or if the animal be multiparous, would not an obliteration on one side prevent conception in the corresponding ovary? Now I had some distant apprehensions, even before I made this experiment, that dividing both tubes would produce effects equivalent to an extirpation of both ovaries, which experience has since proved to be well founded; for it not only destroys the power of conception, but even the disposition for using the means.

Exper. Having procured a full grown virgin rabbit, which had betrayed signs of disposition for the male, I made an incision into the posterior part of each flank, exactly on the part where the tubes are situated. By means of my finger and a bent probe, I drew out a very small portion of the middle of the tube, and cut out about $\frac{1}{8}$ of an inch. The 2 ends were returned into their former situation, and the wound closed by what surgeons call the quill suture. The same operation was performed on the opposite side, and in a few days both wounds were healed. As soon as this rabbit appeared in health, it was admitted to the male, but the venereal appetite seemed to be entirely lost. Thinking it possible that its health was not perfectly restored, I kept it a month longer in a state of high feeding, and admitted it to the male a 2d time; but the same reluctance continued. I began now to suspect that the venereal appetite was irrecoverably gone: but as the season was cold, and of course unfavourable, it appeared proper to persevere in this plan till the genial influence of returning spring had produced its effect; but instead of discovering signs of restoration of the female character, it was evidently more averse. It was now killed and examined, the tubes adhered firmly to the loins at the part where they were divided, and at that part their canal was obliterated, so that neither quicksilver nor air could be made to pass. The ovaries were much smaller than they usually are in breeding rabbits; they appeared to have degenerated from their proper character; a circumstance probably the consequence of that destruction of the harmony of action in these parts, which subsists in the healthy state, which is essential to the views and intentions of nature, and for want of which harmony, the sexual indifference, approaching to aversion, was in this instance so remarkable.

In the relation of this experiment, it must be remembered, that a small portion of each tube was cut out, in order to obliterate the canal with greater certainty. It is not altogether indifferent to the present subject to know, whether this apathy depended on the removal of that portion, or whether it would have happened had there been nothing more than a mere division. Nor is it extraneous to inquire, whether a simple division of the tube is sufficient to obliterate it, because less violence is offered to the part, and of course the connection will be less disturbed.

Exper. Being furnished with another rabbit, in high breeding condition, I repeated the experiment, by making only a division of the tubes; in other respects every thing was conducted as before. The venereal appetite declined as evidently in this as in the former, and notwithstanding many solicitations from a very animated male, during the space of 3 months, it could never be excited. On dissection, it appeared that the tubes were as completely obliterated in this experiment as in the last, and the ovaries had equally degenerated. In the 2 preceding experiments neither of the rabbits had given any active proofs of fecundity, though they had marks of the venereal heat on them. I therefore changed my subject for one that had had young ones.

Exper. A healthy rabbit, which had lately been separated from her first litter, was made the subject of a repetition of the experiment. I took the opportunity of feeling for the ovaries, in order to have better evidence respecting their bulk, and by that means to form a juster comparison. The disposition to propagation declined as evidently in this animal as in the 2 former; and dissection equally evinced a change of the ovaries; for at the expiration of 3 months, they had lost nearly $\frac{1}{4}$ their size. Feeling but little encouragement to persevere in a repetition of these experiments, I determined to change the mode of inquiry, and to try the effect of a division of one tube only. From reasoning I was led to think, that if a division of both tubes destroyed the harmony of the generative system, a division of one only might permit that harmony to continue in some degree. I wished also, if possible, to have this point determined on a virgin rabbit, the better to guard against any deception which the remains of a former impregnation might occasion.

Exper. A full grown virgin rabbit had 1 of the tubes divided at a little distance from the extremity of the cornu uteri. The wound soon healed up, and its health was soon restored, but it betrayed no disposition for the male. I attributed it in part to the coldness of the season, for it was in the middle of Dec., 1794; but the effects of its inclemency were much moderated by having a fire in the room during the day. I kept her till the first of May; during this interval the male was frequently offered to her, but she always refused, except once in Feb.: it however was unproductive. From examination after death, it appeared that the divided tube was completely obliterated, but the other was sound: both ovaries were evidently shrunk, proving, in addition to my previous observations, that their actions had been languid.

The result of this experiment disappointed me much; for no reasoning à priori had led me to entertain the smallest suspicion that a mutilation of one side only could destroy the harmony of the whole uterine system. But my disappointment originated chiefly from the apprehension that this effect would be uniform, that it was the result of a determined law of the part; and if so, it formed an insuperable obstacle to my research. Its importance to my project was too great to be discouraged from a single obstacle; therefore in justice to my undertaking, I was in some measure compelled to push the inquiry to such an extent, as should enable me to say with precision, whether it is possible to impregnate an animal in the situation just described.

Exper. Two other rabbits full-grown and perfectly healthy were made the subject of a repetition of the last experiment. The male was offered to them several times during the space of 3 months. They generally refused him, yet received him 2 or 3 times each during this interval; but neither were impregnated. As the signs of degeneracy from their proper sexual character became daily more evident, they were devoted to anatomical inspection, and exhibited appearances in the ovaries like the former, but somewhat less in degree. The rabbit-keeper informing me that those which had already had a litter were more certain of breeding than those which had not; I determined to make a trial of one of this description, with a view to compensate for my former disappointment.

Exper. Being furnished with 1 of this kind, and from which the young had been taken away 3 weeks at the age of 10 weeks, which, together with the month of gestation, amounted in the whole to 4 months from the last conception, I made this the subject of the experiment. Now, at this distance of time, it is not very probable that the ovaries should retain very evident vestiges of the preceding conception: but as it was a point of too much importance to be left in doubt, I determined to satisfy myself by ocular examination, which, by a little management, was effected. The traces of corpora lutea were far from being evident, so that there was no danger of confounding them with any recent mark that might happen. The tube on one side was cut through as before, but to my unspeakable mortification this rabbit was as barren as the former, though tried several times during the space of 3 months. The generative organs were examined after death, and the appearances corresponded with those of former experiments.

In this case, as well as in a former, I had an opportunity of comparing the shrunk state of the ovaries after death, with the plump and healthy condition before the mutilation; and it affords an additional proof of that sympathetic connexion, or consent, between one part of the generative organs and another; and shows that in the production of a new animal, the co-operation of different parts is necessary; and further, that if the assistance of one part is wanting, the others, as if governed by a principle of intelligence, cease to continue their important work. But I was still in a state of suspense with regard to the end for which these experiments were instituted; and such an uninterrupted succession of failures on a point so essential to my present inquiries, I confess tended but little to animate me in the pursuit. I was beginning to suspect that the barrenness consequent to the division of only one of the tubes, was as determined a law in the economy of these parts, as it seemed to be in those cases where both tubes were cut through; and that nothing could prevent this sterility; but my contemplations were directed into another channel by the following experiment.

Exper. Having procured another rabbit, nearly under the same circumstances as the last, I operated precisely in the same mode, and had equal evidence too concerning the condition of the ovary. The result of this experiment was successful; for on admitting the male to her about 1 month from the operation, she betrayed no reluctance, and became impregnated. Ten days afterwards she was killed, and opened. Both ovaries retained their primitive plumpness, and manifested the evidences of impregnation. These evidences are the presence of corpora lutea, bearing the same precise characters as I have demonstrated in the former part of this essay. Those seated in the ovary of the mutilated side did not differ in any respect from the same bodies on the perfect side: but they were unattended with fœtuses; whereas in the perfect side, there were as many fœtuses as corpora lutea.

As this experiment had succeeded, I examined the divided tube with attention, to satisfy myself whether its canal was obliterated; and of this I had the clearest proof; for it would not allow quicksilver, nor even air to pervade it. Now here is matter for reasoning. Both ovaries, it seems, bear unequivocal proofs of impregnation, but fœtuses are found only on one side. Now, on what principles shall we explain these phenomena? It is certain that neither semen nor the aura seminalis could have touched the left ovary, and yet it bears the most unequivocal marks of recent impregnation. It must depend on some other cause than the actual contact of semen. But an important subject for investigation here presents itself. Why were there no fœtuses on the mutilated side; but only the corpora lutea? Is the application of the semen to the vagina or uterus sufficient to stimulate the ovaries to perform their first procreative operations, without enabling them to achieve any thing more? and does it require the permanent and active energies of this fluid, operating by direct contact on the surface of the ovaries, to produce the full measure of their effects? But as these are queries which cannot be answered from the mere reflections of the closet, I must engage anew in the business of experimental inquiry. But the first step that ought to be taken in the management of this question, is to give full confirmation to the above fact, by a repetition of the experiment; I therefore engaged a keeper of rabbits to procure me 6 in high breeding condition, as soon as possible.

Exper. Within the space of a month, 1 cut through the fallopian tube on 1 side in 6 rabbits. The season was warm, and consequently favourable for breeding. As soon as they recovered they were admitted to the male: but out of this number 2 only were impregnated; and the keeper assured me that one of them had never been impregnated before. When the success in these experiments is compared with that of the former, there was no cause for complaint. Of these 2 which succeeded, 1 had 3 corpora lutea and 3 fœtuses in the perfect side, with 2 corpora lutea and no fœtuses on the imperfect side. The other, which was the virgin rabbit, had 2 corpora lutea and 2 fœtuses on the perfect side, with 1 corpus luteum and no fœtus on the mutilated side. Having now 3 indisputable proofs of this important fact, I consider it a full answer to any objection that can be urged on the ground of accidental appearance; and that what has been stated above, must, under the circumstances described, be considered as a law of the part; viz. that the ovaries can be affected by the stimulus of impregnation, without the contact either of palpable semen, or of the aura seminalis.

But I cannot expect that any physiologist, prepossessed with the common notion of the contact of semen, will yield assent to my position, without subjecting it to a severe scrutiny, and exposing every possible objection to which it is liable. It certainly would not be unphilosophic to ask, why fœtuses were not found either in the ovarium, or in the tube between it and the obliterated part, agreeably to the assertion of Nuck, if, as I contend, the ovary was affected by impregnation? Again, a tenacious opponent might further avail himself of this apparent difficulty, by alledging, that if the tube had not been obliterated till after coition, the semen or its power might have affected the ovary by actual contact; and the product of conception might have been more complete. And in support of this idea, he might adduce the result of an experiment said to have been made by Nuck, in which he made an extra-uterine case in a bitch, by tying one of the tubes 3 days after coition.

These objections have at least speciousness to recommend them to our notice; but it is from experiment alone that we can determine whether they have any solidity. To the first difficulty I reply, that my experiments were not made under the same circumstances that Nuck's is said to have been; therefore, giving him full credit for what he has advanced, a similarity of result cannot be expected. But it is painful to me to differ from any writer of character in the statement of a fact, where the truth is equally accessible to us both; and notwithstanding the respect I willingly bear towards a name that has both acquired and deserved considerable reputation, I must confess that it appears to me highly problematical, whether this celebrated experiment be a reality, or only an ingenious device. But some facts, which it will be soon in order to relate, will show, I think very clearly, that I rest my suspicion on fair grounds. In the mean time I feel it incumbent on me to reply to the general principle of the objection, and to determine by experiment how far it is deserving attention.

Now, if there be any validity in the objection, it should necessarily follow, that if an opportunity was given for the semen to pass by the tubes to the ovaries; we might, by opening an animal at a proper time after coition, detect some disposition in the fimbriated extremities of the tubes to apply the semen, by first approaching, and afterwards embracing the ovaries; and this action ought, according to the common theory, to take place before the usual sign of conception is at all evident on those bodies, when the rabbit is somewhat apparent in 6 hours, but unequivocally marked in 12.

Again, admitting the probability of it, we are led to inquire by what power the semen can be conveyed to such a distant part. It must be either by the male, vi jaculationis, or by muscular power in the tubes, analogous to a peristaltic motion. If it were by the first mode, the conveyance would be instantaneous; but in the latter, some little time seems necessary to allow the tubes to be affected by the stimulus preparatory to their peristaltic action. Perhaps this question may receive some light from the sacrifice of a few animals, at different periods between the coitus and the first visible effects of impregnation; and I considered it by no means inapposite to the subject, to determine whether these conjectures were authorized by any visible changes, either in the condition or situation of the tubes. But the fruits of this inquiry will appear by the following experiments.

Expers. A female rabbit in high season was admitted to the male, and in a few minutes afterwards the ovaries and tubes were brought into view; but the fimbriæ were in their natural situation. As soon as proper rabbits could be procured, I repeated this experiment on 2 others, with precisely the same consequence. These facts militate strongly against the possibility of the conveyance of the semen to this part vi jaculationis, and demonstrably prove, as far as 3 facts can go, that if the moving power inheres in the female, it is not instantaneously exerted.

But are the powers of the fecundating fluid conveyed at any time by the tubes? This simple question betrayed me into the prosecution of experiments to a greater extent than I at first expected; for the result of several of them was unsatisfactory: but being once engaged in the question, I felt myself compelled to prosecute it, by examining these parts at different periods from the coitus to the manifestation of its effects. But I found from a regular series of observations made on different rabbits, at every hour between the 1st and the 9th, that the fimbriæ remained nearly in their usual situation; and the only difference I perceived in the last hours, was a greater turgescency of vessels, as is preparatory to some important action. I desisted from this inquiry at the 9th hour, because the ovaries now bore very evident marks of impregnation; and there appeared to have been no action in the tubes by which the semen could have been conveyed to them.

The impression which these experiments at first made on my mind was, I must confess, not altogether incongenial to my wish, inasmuch as they seemed to furnish a satisfactory answer to the question: but reflections when more at leisure abated my confidence, and in the end convinced me that my proofs did not exceed probability, so that there was still room for the suggestions of scepticism: and indeed it might be said with great propriety, that the tubes might have inclined towards the ovaries in the intervals of the hours above-mentioned, and have returned to their former situation, and thus have eluded my research. I think it but candid to acknowledge, that these last experiments do not prepare me to meet that objection.

These reflections suggested to me the expediency of constructing a plan of inquiry more apposite to the subject; and attended with experiments bearing more directly on the point at issue. Under this impression I determined to obliterate one of the tubes at different periods post coitum, and after the lapse of a sufficient length of time, to notice the effect. My particular view in this was to allow sufficient time for the arrival of the semen at the ovaries, supposing it to take place; so that if they were stimulated by an affusion of that fluid, either in a palpable or insensible form, here would be time allowed sufficient to produce its effect; and if in this mode fœtuses could be formed, while by obliterating the tube ante coitum nothing more than corpora lutea were seen, it furnished an argument of no inconsiderable force in favour of impregnation by immediate contact; but if, on the contrary, corpora lutea only were found, then such experiments would give additional force to the arguments stated in a former part of this section.

Exper. One of the tubes of a rabbit was divided $\frac{1}{2}$ an hour post coitum, and the wound closed as before. She was kept a fortnight, that I might know the result; but there were no marks of impregnation on either side. Though a failure of impregnation has been very common in experiments connected with the mutilation of these parts, I apprehend that the derangement in the present instance proceeded from some disturbance given to the procreative operations in their commencement, and therefore determined in the next trial to wait a few hours, the better to avoid this.

Exper. I repeated the operation on 2 other rabbits, in one at 4, and in the other at 6 hours after coition. On inspecting the parts at the end of a fortnight, the first was not impregnated, but the last was. In this there were 4 corpora lutea in the right side, answering to the same number of fœtuses in the cornu uteri of that side; but on the left or imperfect side, there were 3 corpora lutea without fœtuses. The corpora lutea on both sides were cut open, but not the slightest difference could be detected. Now, if the contact of the semen with the ovaries in any form be essential to impregnation, here has been an opportunity for such contact during the space of 6 hours; but it has not been sufficient to advance the procreative operations further than happened in those experiments where the tube had been divided before coition. Let us then for a moment suppose that the interval be lengthened, in order to allow a better opportunity for producing the full effects of impregnation, by exposing the ovary a longer time to the stimulus of the semen.

Exper. I cut through the left tube of another rabbit 12 hours post coitum, and examined the parts on the 15th day. There were 4 corpora lutea with the same number of fœtuses on the right side, and 3 corpora lutea without fœtuses on the left; so that 12 hours supposed exposure to semen, had made no sensible advances in the procreative operations on the mutilated side.

Exper. The same operation was repeated 24 hours post coitum. Corpora lutea were found in both ovaries, but fœtuses only on the perfect side. Now I observed in one of the experiments related in the former part of this essay, that the vesicles of the ovaries when examined 48 hours post coitum, were extremely prominent; they appeared as if going to burst: it is but reasonable then to admit, that at this time they must have received their full measure of stimulus; and if 1 of the tubes was divided in this state of things, the result would be more decisive.

Exper. The operation was repeated under the circumstances just described, and in 14 days the result was ascertained, viz. 3 corpora lutea and as many fœtuses on the perfect side, and 2 corpora lutea without fœtuses on the imperfect one. Now, what mode of reasoning ought we to adopt here? Has the mutilating process suspended the effect of that stimulus which impregnation had begun? and are those appearances in the ovaries, any thing more than incipient relapses into evanescence? Such really appears to be the state of things, and seems to mark in a decided manner, a sympathetic connexion between 1 part of the uterine system and another. And were I to adopt the language of a late celebrated physiologist, I should say "that the ovary on the imperfect side, feeling the inability of the tube to transmit its contents to the uterus, the proper receptacle, had suspended the usual operations of these parts, from a consciousness of their inutility."

This reasoning will probably appear not perfectly consentaneous to certain well established facts on the subject of extra-uterine fœtuses; for dissection has fully evinced the possibility of a fœtus being perfectly evolved, and of acquiring considerable bulk, either in the ovary, abdomen, or tube. I do not hesitate to acknowledge the full force of these facts; but I cannot admit that they subvert the principle I wish to establish from experiment; because I conceive there is an essential difference whether nature spontaneously dispenses with her usual modes, and attempts to effect her ultimate purpose by irregular means; or whether, proceeding in the ordinary course of her operations, she suffers an impediment which a physiologist may have produced to thwart her designs. In the first case, she may be provided with an expedient; in the last, she will probably be left without resource.

Here again we may notice the experiment mentioned by Nuck, which, though under similar circumstances, was attended with a different result. Some who feel themselves disposed to venerate his authority, will probably oppose his experiment to mine, and think it incumbent on me to account satisfactorily for the difference. I can by no means acknowledge such an obligation; for to confer validity on experiment by reasoning, is to invert the order of inquiry, and support facts by conjectures. It is sufficient for my credit to be able to adduce evidence of the truth of what I advance, and for this evidence I rely on my preparations.

The train of reasoning which I have lately pursued, led me to extend my inquiries into this particular question still further; and as in the last experiments the vesicles were known to be just on the point of bursting before the tube was cut through; the next step in the inquiry appeared to be, to determine the consequences of dividing the tube a short time after the rudiments of the fœtus had passed. Will the procreative operations be suspended, if the tube be cut through after the ovum is deposited in the uterus?

Exper. I repeated the operation on 2 rabbits, one of which had received the male 2 days and 18 hours, the other 2 days and 12 hours. I knew from my own experiments, as well as those of De Graaf, that the vesicles had discharged their contents before either of these periods. The examination of these at the usual time, proved that the actions of these parts suffer no interruption by a division of the tube made after the rudiments of the fœtus have been conveyed into the uterus; for there were corporea lutea in both ovaries, and fœtuses in both cornua uteri.

These experiments I think overturn, as far as experiment can, every argument which has hitherto been adduced to support the hypothesis, that the affusion of the semen on the ovaries, either in a sensible form or in that of aura seminalis, is essential to impregnation: for if the ovaries were susceptible of their proper excitement only by the contact of semen, by what accident has it happened that the effects of that excitement are not more obvious and further advanced in those experiments, where nothing was done to intercept its course for 48 hours, than in those where all communication between the uterus and ovary had been cut off before the means for impregnation had been employed? We should expect in the one case to find the full effects of impregnation, and in the other no traces of it would be seen; instead of which, the procreative actions are no further advanced where there has been an opportunity for the passage of the semen, than in those cases where the passage has been impossible. But if we defer the mutilation till the ovary has perfected its work, which it does in a rabbit in something more than 50 hours from the approach of the male, then the generative process is not disturbed, and the evolution of the fœtus goes on in the usual manner; for now all the different parts of the uterine system being in a condition to act, each performs its peculiar office.

1st. The semen by its presence stimulates either the vagina, os uteri, cavity of the uterus, or all of them.—2d. The impression made on these is propagated to the ovaries by consent of parts.—3d. One or more of the ovarian vesicles enlarges, projects, bursts, and discharges its contents.—4th. During this process in the ovary, the tube is undergoing a state of preparation for the purpose of embracing the ovary, and receiving the rudiments of the fœtus.—5th. This preparation consists in part of an increased turgescence of its vessels, and a consequent enlargement of its fimbriated extremity. When thus prepared, it approaches the ovary.—6th. After the tube has performed its office by a peristaltic motion, commencing at the fimbriæ, and terminating in the uterus, it gradually returns to its former situation and condition.—7th. While these different actions are going on in the appendages of the uterus, others not less important to the design of nature are instituted in the uterus itself: for the tunica decidua, where it is obvious, it is formed ready to secure firmness of connexion between the tender ovum and internal surface of the uterus, till a proper attachment by means of placenta can be effected.—8th. By way of guarding with additional security against a premature escape of the ovum, an apparatus, seated in the neck and mouth of the womb, now begins to develope its real structure, and perform its proper action, consisting in the secretion of a mucus-like substance, sufficient in quantity to fill completely the whole length of the neck, and by that means to seal up the communication between the cavity of the uterus and vagina.—9th. Nor does the care of nature for the preservation of the new animal terminate here; for while she is by various means forming and perfecting her work, at least as far as comes within the province of the uterine system, she is at the same time making preparation for its nourishment after birth, by instituting the proper secretion of the breasts.

When we take a reflected survey of these successive operations, I think it must appear, on tracing nature's steps through the different stages of this work, that they are the product of that law in the constitution which is called sympathy, or consent of parts. That the semen first stimulates the vagina, os uteri, cavity of the uterus, or all of them. By sympathy the ovarian vesicles enlarge, project, and burst. By sympathy the tubes incline to the ovaries, and having embraced them, convey the rudiments of the fœtus into the uterus. By sympathy the uterus makes the necessary preparation for perfecting the formation and growth of the fœtus. And, by sympathy the breasts furnish milk for its support after birth. Having now investigated this intricate question, I hope with some regularity; the design of this essay next leads me to consider the state or form of that substance which passes from the ovaries in consequence of impregnation.

§ 3. *What is the Form of that Substance which passes from the Ovaries in consequence of Impregnation?*

No sooner had the researches of the physiologists retraced the existence of the new-born animal to the ovaries, than their curiosity was excited to discover the form it assumed while resident in these bodies, and especially at that particular time when the fœtal primordia are about to escape from them. The analogous phenomena of oviparous animals, and the structure of the ovaries as described by De Graaf, concurred to favour an opinion, that in viviparous animals there existed ova in these bodies, and indeed from this very circumstance they received their name. But though several physiologists have concurred in this opinion, there has not been any strict coincidence respecting their state while in the ovary. Some have thought that the vesicles described by De Graaf were the true ova, and that these are the bodies that are expelled by impregnation. Others, with greater probability, have considered these vesicles as the apparatus destined by nature, under the influence of the proper stimulus, to form the ovum: and though at all times they contain a glairy kind of fluid, from the stimulus of impregnation this fluid becomes a small vesicle or ovum seated within the larger vesicle, which now becoming thickened, and acquiring a yellow colour, is called the corpus luteum: from this body the interior vesicle or ovum is protruded. Others again refuse assent to both these opinions, and contend that the substance extruded from the corpora lutea has no vesicular appearance; and though by some is has been called an ovum, yet that name is not applicable to it from any resemblance of figure, but rather from its agreement with an egg in being the substance in which the rudiments of the future animal are contained.

De Graaf contended that the primordia of the fœtus while in the ovary are vesicular, as appears in his work; in which, after describing the enlargement of the proper vesicles usually connected with his name, he says, "præterea aliquot post coitum diebus tenuiori substantia præditi sunt, et in sui medio limpidum liquorem membranâ inclusum continent, quo unâ cum membranâ foras propulso, exigua solum in iis capacitas superest." He is thererefore decidedly of opinion, that as soon as the product of conception becomes the subject of notice, it has a vesicular form, and this he thinks takes place at the end of the 3d day, though the substance passes from the ovaries several hours before this time. He seems rather to assert, that it passes in a vesicular form, than to prove it; for in 52 hours after the approach of the male, he found the ovarian vesicles were empty, though he could not now find the new vesicles either in the uterus or the tubes. But in 72 hours they were so evident, that he could distinguish with ease the 2 membranes of which they are formed, viz. the chorion and amnios; so that they cannot be very small at this time. Hence it would follow, that if on a repetition of this experiment on the 3d day no vesicles should happen to be found, it would not be from minuteness that they would escape observation; therefore should any one be disposed to search for them, he need not bend his sight, as if looking at microscopical objects.

Valisneri, on the contrary, searched for these eggs with great industry, accompanied with an ardent wish to find them; but though his experiments appear to have been judiciously conducted, he never succeeded. Haller also maintains, from a regular series of experiments made on sheep, whose term of utero-gestation is 5 months, that some days elapse between the escape of the substance from the ovaries, and the appearance of a circumscribed body in utero, which can properly be called ovum: and that this does not happen until 17 days from impregnation. In the mean time, nothing but irregular masses of mucus are found. The circumscribed form, at this time acquired, seems to depend on the formation of the fœtal membranes now bounding the contained mucus-like substance. This apparently homogeneous mass, on the 19th day undergoes a change of character; an opaque spot is seen within it, which subsequent observations prove to be the first evident marks of the evolution or formation of the fœtus. From this dim speck of animal existence we may observe a series of regular advances, from an inorganized mucus-like mass to the most beautiful and complicated machine in nature. But to trace her progressive steps through this important work, forms no part of the design of this dissertation.

The chief difference between De Graaf and Haller on this subject, consists in their opinions respecting the form of the substance that is passing from the ovaries, whether it is vesicular at this time or not; for in the subsequent processes they differ but little. No solution can be given of this question by force of reasoning; it is from experiment alone that we can receive conviction, notwithstanding the 2 contrary opinions that prevail. All that can be expected from an individual in such a case, is to add the result of his own labours to one side or the other, so that in the end the preponderance must depend on the weight of evidence.

The experiments I have made on this simple question do not allow me to incline to the side of De Graaf; for in the rabbit I have never found any thing in the uterus which had a regular circumscribed form earlier than the 6th day; and even then the substance was bounded by a covering so very tender, that it scarcely had firmness sufficient to support the figure. Before the 6th day, I have never seen any thing but irregular mucus-like masses in the uterus; but after this time the substance has firmness sufficient to admit of preservation in spirits, a specimen of which I have in my collection of preparations. This acquisition of figure does not depend so much on a difference of consistence, as on the formation of membranes inclosing this substance. These membranes when in a more advanced state of formation, are known by the names of chorion and amnios. The product of conception being arrived at this stage, may with some propriety be called an ovum, as it has acquired a determined figure; but the different constituent parts of it are not apparent at this early period; on the 10th day, in the rabbit, an opaque spot is seen in this ovum, which increasing daily in its bulk, progressively manifests the formation of the fœtus.

It is a little remarkable that in the rabbit, where the term of utero-gestation does not exceed 30 days, a 3d part of that time should be required to make that opaque spot obvious to the sight, while the remaining $\frac{2}{3}$ds should suffice to complete the formation of the fœtus. It appears as if it required a more elaborate exertion of the formative powers of these parts, to produce what might figuratively be called the nucleus of a fœtus, than to go on and complete the work. But this remark applies only to the rabbit; for in the human female, abortions at the 3d month clearly prove that the evolution of the fœtus has been perfected some time before. Such an obvious difference cannot fail to impress our minds with doubts and distrust, whenever we are drawing inferences from analogical reasonings: but to trace the formative process of nature through this work, and to compare her progressive advances in the different periods of utero-gestation, are foreign to the design of this essay.

IX. *Experiments in which, on the Third Day after Impregnation, the Ova of Rabbits were found in the Fallopian Tubes; and on the Fourth Day after Impregnation in the Uterus itself; with the first Appearances of the Fœtus. By Wm. Cruikshank, Esq.* p. 197.

The ancients imagined that the woman had her testicles, as well as the man, and her own semen. They taught, that in the coitus there was a mixture of the male and female semen in the uterus, and that from a process like fermentation between these 2 fluids, an embryo was produced. Lewenhoeck said the embryo belonged to the male; and saw, or thought he saw, animalcules in the male semen, resembling the animals to which they belonged. Spallanzani says, that the semen of male animals having no animalcules, impregnates as certainly as that of those which have them. This shows that those animalcules are not embryos. Steno, observing that there were round vesicles in the testicles of women, like the eggs of birds, called them ovaria, and said their structure was exactly similar to the ovaria of birds. After this the immortal Harvey broached the doctrine of "omnia ab ovo;" that all animals were produced from ova. "Nos autem asserimus, animalia omnia, et hominem ipsum, ex quibusdam ovis nasci."

The ova in the ovaria of rabbits are particularly described by De Graaf, whence Haller calls them ova Graffiana. But the ovaria of quadrupeds often contain vesicles of the hydatid kind; and it becomes difficult to distinguish between what are vesicles, and what are ova. The mark with me is this: the ova are inclosed in a capsule highly vascular from arteries and veins, carrying red blood. The hydatid vesicles are not vascular; at least their vessels carry no red blood. The calyx and the ovum, after impregnation, and even before it, in the state in which the quadruped is said to be hot, become black as ink, from the greater derivation of blood; and the ova resemble dark spots: they also come nearer the surface of the ovarium, so as to pout or project, at last, like the nipple in a woman's breast. Some hours after impregnation, the calyx and the coverings of the ovaria burst, and the ovum escapes; may fall into the general cavity of the abdomen, and form an extra-uterine fœtus; but almost always falls into the mouth of the fallopian tube, whose fimbriæ, like fingers, grasp the ovarium, exactly at the place where the ovum is to escape. What the appearance of the ovum was, when deprived of its calyx, or when descending the fallopian tube was not known. De Graaf discovered this in the fallopian tubes of rabbits, in the year 1672; and says, "minutissima ova invenimus, quæ licet perexigua, gemina, tamen, tunica amiciuntur;" and then adds, "hæc quamvis incredibilia, nobis demonstratu facillima sunt."

De Graaf had the fate of Cassandra, to be disbelieved even when he spoke the truth! Dr. Hunter had his doubts; and the great Haller, of whom I have always spoken in the language of Professor Marrhar, "cujus auctoritas apud me plus valet, quam auctoritas omnium aliorum anatomicorum simul sumptorum," positively denies their truth. His words are, "vix liceat admittere"—and afterwards, "denique, quod caput rei est, neque Hartmannus cum experimenta Graffiana iteravit; neque Valisnerus tot et tam variis in bestiis; neque ego in pene centum experimentis; neque nuperiorum anatomicorum quispiam, vesiculam, quales sunt in ovariis, post conceptionem, aut in tuba vidimus aut in utero!"

In the beginning of summer 1778, I was conversing with Dr. Hunter on this subject, and said, "I should like to repeat those experiments, now that lectures are over, and that I have the summer to myself."—"You shall make the experiments," said he, "and I shall be at all the expence." Accordingly he carried me to Chelsea, introduced me to a man who kept a rabbit warren, and desired him to let me have as many rabbits as I pleased. I made the experiments; and shall now lay a copy of my journal, then made, before this Society.

Exper. 1. May 30, 1778. I took a female rabbit, hot, as the feeders term it, that is, ready to be impregnated, and disposed to receive the male. This they find out, not by exposing her to the male, but by turning up the tail, and inverting part of the vagina: its orifice and internal surface are then as black as ink, from the great derivation of blood to these parts. Having run the point of a double-edged dissecting knife through the spinal marrow, between the atlas and dentata, she instantly expired. I preferred this method of killing her, because when the circulation stopped, the internal parts would be found, respecting vascularity, exactly as in the living body. On examination some time after, I found the internal parts of generation, exactly in the same state as the external; that is, as black as ink: the ovaria had, immediately under their external surfaces, a great number of black, round, bloody spots, somewhat less than mustard-seeds. These black spots are the calyces or cups which secrete the ova; they are extremely vascular; the ova themselves are transparent, and carry no visible blood vessels. These calyces, on the expulsion of the ova, enlarge and become yellow, projecting above the external surface of the ovaria, and form the corpora lutea; a certain mark of conception in all quadrupeds, and in women themselves, whether the embryo is visible or not. The use of the corpora lutea is not yet made out; but the orifice, through which the ovum bursts into the fallopian tube is often extremely manifest, and always has a ragged border, as lacerated parts usually have. The fallopian tubes, independent of their black colour, were twisted like wreathing worms, the peristaltic motion still remaining very vivid; the fimbriæ were also black, and embraced the ovaria (like fingers laying hold of an object) so closely, and so firmly, as to require some force, and even slight laceration, to disengage them.

Exper. 2. I opened a female rabbit 2 hours after she received the male: the black bloody spots, just mentioned, now projected much above the surfaces of the ovaria, some of the ruptured orifices were just visible; but in many of these spots there was not the least vestige of an orifice; whence I conclude that they heal very quickly in general. While the animal was yet warm, I injected the arterial system with size coloured with vermillion, whence every thing I had seen before became now more distinct, and the black spots, which I before conjectured to be congeries of vessels, were now proved to be so.

Exper. 3. I opened another female rabbit the 3d day after impregnation: that she was impregnated I could have no doubt, for I never knew impregnation fail if the female was hot, and the male had not been previously exhausted; besides the corpora lutea in the ovaria fully proved it: the appearances were the same as in the last, only the corpora lutea were larger; but though I examined the fallopian tubes in the sunshine, and with great care, I could not find any ova, neither in them nor in the horns of the uterus.

Exper. 4. I opened another female rabbit the 5th day after conception: the appearances were much the same as in the former animal, only the corpora lutea were increased in bulk, but there was not the least vestige of any ovum any where that I could discover. I was now ready to exclaim with Haller, "vix liceat admittere."

Exper. 5. I opened another female rabbit on the 8th day after she had admitted the male: the ova were in the cavity of the uterus, and projected through its substance about the size of a large garden pea; when I cut off the most superior part, and cut into the cavities of the ova, the liquor amnii escaped in a proportionate quantity: by their adhesions to the internal surface of the uterus they remained extended, not collapsing in the smallest degree; the fœtus was not visible; but I had often made the chick, in my experiments on the incubated egg, become visible, by dropping on the spot, where I knew it must be, a drop of distilled vinegar; by dropping the vinegar on the bottom of the little cups I had made, by cutting off the tops of the cells, the fœtus instantly became visible.

Exper. 6. Opened another, 9th day: fœtus contained within its amnion, floats in another fluid, between chorion and amnion, which are now at a considerable distance; this fluid jellies in proof spirit. Some corpora lutea have cavities, others none, nor the least appearance of orifice. The corpora lutea keep increasing as the fœtus increases, are of a sand-red colour, and very vascular.

Exper. 7. Opened a doe the 11th day after coitus: ova very little larger than the last, nor the fœtus: there were but 2 ova, though several corporea lutea. Some pellucid hydatids appeared hanging on the outside of the fallopian tubes. Could these be ova which had missed the passage? they were vascular: the heart of the fœtus was full of blood; the umbilical vessels very distinct, but no chord as yet, contrary to De Graaf.

Exper. 8. Opened a doe the 14th day: 7 corpora lutea in 1 ovarium, and 1 in the other; only 2 ova in the horns of the uterus, 1 in each; that in the horn next 1 of the ovaria with 1 corpus luteum was blighted, and the fœtus invisible, even with distilled vinegar; in the other it was increased proportionable to the time; the umbilical chord now for the first time distinct, and the tail detached from the under surface of the uterus; there was something unintelligible about the head, it was bifid on the side next the mouth, with a hole in each extremity; the intestines were now apparent, at least the rectum, as were the lower extremities.

Exper. 9. Opened a doe the 6th day complete: found the ova loose in the uterus, as described by De Graaf, and corresponding nearly to the corpora lutea, 6 in one horn and 4 in the other; the ova were transparent, and of different sizes; they were double, and contained each an internal vesicle; there was a spot on one side in most of them, which I conceived to be the intended point of adherence between them and the uterus; the internal vesicle was not equally in proportion to the external, but in some larger, in others less; I even suspect I saw something of the fœtus: a polypous excrescence in the uterus near the orifice of the fallopian tube, had detained 4 of the ova at that place; others were scattered in the uterus: just where one of these vesicles had become stationary a white vascular belt was beginning to form, and in the middle of this a cavity where the vesicle lay; the inner membrane I take to be amnion, the outer chorion.

Exper. 10. Opened a doe the 7th day: the ovaria were shrunk; there were something like 3 corpora lutea, but not distinct; there were 2 polypi or solid excrescences in the left horn of the uterus, but no ova.

Exper. 11. The day after a doe had received the male I made a small opening on the left side of the abdomen, got down upon the uterus just where the fallopian tube goes off, tied the left tube close to the uterus, with a view to intercept the ova. The result of this mentioned afterwards.

Exper. 12. Opened a doe the 7th day after coitus: ova all fixed and adhering to the uterus, even making a sensible swell in form of belts at different parts; the amnion appeared in some nearer the chorion than in others; the liquor between amnion and chorion very gelatinous, in many others less so. Saw nothing of fœtus.

Exper. 13. Opened a doe 8th day after coitus: there were about 10 or 11 ova; fœtus distinct in almost every 1, but not without the application of distilled vinegar for 2 or 3 minutes, and afterwards immersed in proof spirit; in some I found the brain, spinal marrow, and vertebræ, forming 2 columns at some distance; they afterwards gradually approached; for it was in one of the least forward that this was most evident.

Exper. 14. Opened a doe 21st day after the coitus: 5 vessels were seen going out of the navel in 1 of the fœtuses, besides the urachus; the omphalo-mesenteric artery was very distinct, and divided into 2 as it came to the mesentery; could not see the arachus or allantois well, nor the membrane to which the omphalo-mesenteric artery goes.

Exper. 15. Opened a doe the 5th day after coitus: found the ova loose in the uterus, to the number of 6; even these had a lesser coat in the inside, corresponding to amnion. None in the tubes.

Exper. 16. Opened, 14 days after the operation, the doe whose fallopian tube I tied. The uterus of the right side was the size of the 6th day; the ovarium and uterus had gone backward as to the process, and there was no appearance of fœtus; though placenta was very evident on the left side, there was no other appearance of conception in the uterus; no other placenta; the fallopian tube was very large, soft, and tender; the ovarium twice the size of that on the other side, red, and covered with extravasated coagulable lymph; there was an hydatid in the course of the tube, containing a clear fluid, but nothing like fœtus. I suspect that tying the tube prevented the ova on this side from coming out of the ovarium, and that though they rather increased in the ovarium, the process soon stopped; that the process went on however, in the other side for a few days, and then stopped likewise: there was universal inflammation about the uterus and colon of the left side, with great quantities of white extravasated coagulable lymph; there was water in the abdomen, and all the appearance of peritoneal inflammation. This process seems to give but little pain, for the animal at the time she was killed was eating and looking as usual.

Exper. 17. Opened a doe the 3d day after the coitus: the pouting parts of the corpora lutea very transparent before the uterus was touched; but as soon as the spermatic and hypogastric arteries were divided, in order to cut out the uterus, they all, as if struck with some shock like electricity, became opaque. The pouting part I believe is the ovum, and stands on the top of corpus luteum; it is very vascular, particularly at its basis, but as soon as perfect, or ready for expulsion, carries no red blood; it continues to grow of itself in utero, without adhering to the uterus for 2 or 3 days, then takes root, and becomes very vascular: nothing in the tubes or uterus.

Exper. 18. Opened another the 4th day in the morning; but it had not conceived, and was in the state of one hot.

Exper. 19. Opened 1 in the evening of the 4th day: the appearances were little different from those of the 5th morning; the ova were only less dispersed through the uterus, and all accumulated about the orifice of the tubes; the amnion was likewise closer to the chorion.

Exper. 20. Opened another at the end of the 3d day, or rather on the beginning of the 4th: the ovaria were dark brown; the fallopian tubes and uterus almost black, from the great quantity of blood derived to them at this time; I opened this uterus on the upper edge and in the body, so that the parts all remained turgid; the spermatics and hypogastrics not cut through; the corpora lutea were very vascular, an artery running across ramified from both sides, but particularly spent itself in the centre; the upper part of the corpus luteum, or centre, was a little concave, like the head of a turned small-pock, but no evident foramen: I believe the ova were gone out, but I could see nothing of them in the tubes nor uterus; the fimbriæ were more vascular than I ever saw them, and wholly covered the ovaria; the peristaltic motion of the tubes was very evident, and greater than ever I had seen it; the inner surface of the horns was graniform, with white spots; this I suppose decidua, or perhaps corpus glandulosum Everrahrdi. De Graaf saw the ova in the tubes this day.

Exper. 21. Opened a rabbit at 6½ days: ova in the horns of the uterus were just begun to fix, but did not adhere by vessels; they were very much enlarged compared with the 6th, and the side next the uterus had a round rough spot in it, now very conspicuous; the chorion and amnion were almost in contact with each other; they were easily turned out of the uterus, which embraced them every where loosely, but at the bottom; the corpora lutea now increased exceedingly in vascularity, and nourished by a large vessel running across the tubes; remarkably pale, as having done their duty; the graniform appearance on the uterus internally not observable as in the last.

Exper. 22. Opened a doe the 7th day complete after the coitus: turned out, but with difficulty, 1 of the ova a little larger than in the last; the substance of the uterus over these ova was become thin and transparent, so at first sight we might imagine it was the ovum naked, neither was this part so vascular as might have been expected, considering the principal change was going on here; the ovum burst the moment it was disengaged from the uterus; a gelatinous coagulable fluid issued out, but no appearance of fœtus even in the microscope.

Exper. 23. Opened another rabbit at the end of the 3d day: same appearance as in exp. 20: searched in vain for the ova on the right side; at last, by drawing a probe gently over the fallopian tube on the left side, before it was opened, more than an inch on the side next the uterus, I pressed out several ova, which seemed to come from about its middle, as I began the pressure there, and the ova did not appear till the very last; the amnion made a centre spot, and appeared small compared to the chorion; no ova in the uterus.

Exper. 24. Opened another at 3½ days: ovaria had the appearance as if the ova had not yet gone out; however, many of them were found in the uterus, and many in the tubes; I got about 6; others were lost, from the great difficulty in slitting up the fallopian tubes without bruising the ova with the fingers or with the point of scissars; there were 8 or 9 corpora lutea in one ovarium, and 2 only in the other; on the side of the 2 I only found 1 ovum, but twice as large as those on the other side. I observed that the redness of the uterus, depended on not losing much of the animal's blood; for when they had been so killed that much blood was lost, the fallopian tubes at least and ovaria were always pale.

Exper. 25. Opened another rabbit at 2½ days after the coitus: ovaria impregnated, but found no ova in the tubes, nor orifices in the corpora lutea.

Exper. 26. Opened one, 3d day complete: found about 6 or 7 ova in the fallopian tubes, near their end, or about an inch within the tube, on the side next the uterus: in the microscope the ovum appeared as having 3 coats; the middle one perhaps becomes allantois or membrana quarta.

Exper. 27. Opened again another at 2½ days: and though there were a great many corpora lutea, I could not discover any ova; they were probably too small to be perceived, for on the 3d day complete some of the ova were not perceptible, till they were put into a fluid, and viewed in the microscope.

Exper. 28. Opened 1 on the 3d day all but 2 hours: found six ova in 1 fallopian tube, and 7 in the other, which corresponded exactly to the number of corpora lutea in each ovarium; the ova had 3 membranes as before. The circles in the cicatricula of the hen's egg are perhaps similar to these. The ova seems to enlarge in their way down the tube, as a pea swells in the ground before it begins to take root; even in the uterus, for 2 days, they are either loose and unconnected by vessels, or the vessels are so small as not to be discovered by the microscope. The corpora lutea were flatter on the head than I had ever seen them before.

Exper. 29. I opened another at 8½ days: every thing more distinct and more advanced than on the 8th day; the heart now visible, and resembling much the appearance of the incubated egg in the 48th hour. There were 7 corpora lutea in the right ovarium, and but 4 ova in the right horn of the uterus; there were also 3 in the left ovarium, though but 2 ova in the left horn.

General conclusions.—1st. The ovum is formed in, and comes out of the ovarium after conception. 2dly. It passes down the fallopian tube, and is some days in coming through it. 3dly. It is sometimes detained in the fallopian tube, and prevented from getting into the uterus. 4thly. De Graaf saw 1 ovum only in the fallopian tube. I saw 13 in one instance, 5 in another, 7 in another, and 3 in another, in all 28. 5thly. The ovum comes into the uterus on the 4th day. 6thly. De Graaf did not see the fœtus till the 10th day; I saw it on the 8th. 7thly. These experiments explain what is seen in the human female. For,

A. I show a child, at lectures, which remained in the ovaria till it was the size of the 5th month; its fluids were all wasted, and its solids were hard and compressed into an oval form; it had the chorion and amnion, its chord and placenta.

B. I also have in my possession the uterus and ovaria of a young woman who died with the menses on her; the external membranes of the ovaria are burst at one place; whence I suspect an ovum escaped, descended through the tube to the uterus, and was washed off by the menstrual blood.

c. The ovum sometimes misses the fallopian tube, falls into the abdomen, and forms the extra-uterine fœtus; this sometimes grows to its full size, labour pains come on at the 9th month, the child may then be taken out alive by the Cæsarean section; or, dying and wasting, but not putrefying, may remain without much inconveniency to the mother for many years.

D. The ovum, though it has gone some way down the fallopian tube, may be arrested in its course and become stationary, and form what is called the fallopian tube case. A remarkable case of this kind is given by Dr. Hunter, in his book on the gravid uterus, where the tube burst, and the mother bled to death.

E. Lastly, the ovum comes into the uterus, where there is room for its enlargement, and a passage for its exit from the body.

Explanation of the Figures in Plate 3.

It was not thought necessary to delineate the whole uterus of the rabbit, as it exactly resembles the uterus of other quadrupeds, consisting of a vagina, common to 2 horns, 2 fallopian tubes, and 2 ovaries. Any one who wishes to see this, may see it in De Graaf's little book, tolerably well executed for the age in which he lived: but I am more concerned in his first appearances of the ova, than in his general anatomy of the uterus of the rabbit; and therefore proceed to explain the copy of a plate previously engraven, 19 years since.

The figures marked 3d day, are ova of the fallopian tube, found after impregnation on that day. The first 3 are of the natural size; the next 3 are magnified, in the simple microscope. In all of them the chorion and amnion are even now distinct, and in some of them the allantois, as I suspect.

The figures marked 3½ day, are ova still more advanced; similar to which I found many in the tubes, many in the horns of the uterus. The first 3 are of the natural size; the 2 following are magnified also in the simple microscope.

The figures marked 4th day, are more enlarged ova in the horns of the uterus, loose, not adhering, capable of being moved from one place to another, after these horns are opened, by the gentlest breath blown through a blow-pipe.

The figures marked 5th day, are ova of the 5th day; still loose in utero, and still capable of being blown with the gentlest breath from one part to another; they resemble the last in every thing, only that they are larger. The first 3 are of the natural size; the last 3 magnified, as the former ova.

The figures marked 6th day, are ova found in the horns of the uterus on that day; sensibly larger than the preceding; not adhering, even now, to the internal surface of the uterus, but exactly as the last in this respect. The first 4 are of the natural size, the last 3 magnified as before; but, as kept some years, the amnion has receded from the chorion to a considerable degree.

The figures marked 7th day, are ova of the 7th day: the first shows the ovum in its cell in the horn of the uterus, laid open; the next 3 are similar ova, taken out of their cells, and resembling the former; the last 3 are of the same period, and also removed from the uterus, but magnified by the same microscope as the preceding ova. They are seen after having been kept many years, and the secession of the amnion from the chorion is still more apparent and greater.

The figures marked 8th day: the first shows the fœtus now first visible to the naked eye by dropping distilled vinegar on it, in one of the cells of the uterus opened. A little above is seen a cell turgid and unopened; and below a cell half divided. The next 2 figures, in the same line with the fœtus mentioned, are fœtuses of the same period from other rabbits, magnified. They show the rudiments of the vertebræ, and the first appearance of the spinal marrow. The 3d in the same row is also magnified, it shows also the earlier appearances of the 2 hemispheres of the brain.

Of the figures marked 9th day, one shows the fœtus, now, for the first time, of itself visible to the naked eye, adhering near the tail to the placenta in the closest manner; the navel string as yet too short to be visible, as contrary to De Graaf as possible. The 2d shows the same fœtus magnified.

The figure N° 10 shows a fallopian tube, on one side of the uterus of the rabbit, with its fimbriated orifice opening into the abdomen; and its uterine orifice opening into the uterus; also the ovarium, and corpus luteum in it, projecting above the surface.

X. Letter from Sir Benjamin Thompson, Knt. Count of Rumford, F. R. S., to the Right Hon. Sir Joseph Banks, Bart., K. B., P. R. S., announcing a Donation to the Royal Society, for the Purpose of instituting a Prize Medal. p. 215.

"Sir,—Desirous of contributing efficaciously to the advancement of a branch of science which has long employed my attention, and which appears to me to be of the highest importance to mankind, and wishing at the same time to leave a lasting testimony of my respect for the R. S. of London, I take the liberty to request that the R. S. would do me the honour to accept of £1000 stock, in the 3 per cent. consolidated public funds of this country; which stock I have actually purchased, and which I beg leave to transfer to the President, Council, and Fellows of the Royal Society; to the end that the interest of the same may be by them, and by their successors, received from time to time for ever, and the amount of the same applied and given, once every 2d year, as a premium to the author of the most important discovery, or useful improvement, which shall be made and published by printing, or in any way made known to the public, in any part of Europe, during the preceding 2 years, on heat, or on light; the preference always being given to such discoveries as shall, in the opinion of the President and Council of the Royal Society, tend most to promote the good of mankind.

"With regard to the formalities to be observed by the President and Council of the Royal Society, in their decisions on the comparative merits of those discoveries, which in the opinion of the President and Council may entitle their authors to be considered as competitors for this biennial premium, the President and Council of the Royal Society will be pleased to adopt such regulations as they in their wisdom may judge to be proper and necessary. But in regard to the form in which this premium is conferred, I take the liberty to request, that it may always be given in 2 medals, struck in the same die, the one of gold, and the other of silver; and of such dimensions, that both of them together may be just equal in intrinsic value to the amount of the interest of the aforesaid £1000 stock during 2 years; that is to say, that they may together be of the value of £60 sterling.

"The President and Council of the Royal Society will be pleased to order such device or inscription to be engraved on the die they shall cause to be prepared for striking these medals, as they may judge proper. If, during any term of years, reckoning from the last adjudication, or from the last period for the adjudication of this premium, by the President and Council of the Royal Society, no new discovery or improvement should be made in any part of Europe, relative to either of the subjects in question, heat or light, which, in the opinion of the President and Council of the Royal Society, shall be of sufficient importance to deserve this premium; in that case, it is my desire that the premium may not be given, but that the value of it may be reserved, and being laid out in the purchase of additional stock in the English funds, may be employed to augment the capital of this premium; and that the interest of the same by which the capital may, from time to time, be so augmented, may regularly be given in money with the 2 medals, and as an addition to the original premium at each such succeeding adjudication of it. And it is further my particular request, that those additions to the value of the premium, arising from its occasional non-adjudications, may be suffered to increase without limitation.

" With the highest respect for the R. S. of London, and the most earnest wishes for their success in their labours for the good of mankind, I have the honour to be, &c.
London, 12th of July, 1796. (signed) " RUMFORD."
To Sir J. Banks, Bart., K.B., P.R.S. of London.

The Society hereupon resolved, that they accept of the donation, and accede to the conditions annexed to it by the Count; and also directed that a letter be written to the Count, acquainting him of this acceptance; returning him thanks for the liberal donation, and assuring him that the conditions annexed to it will be strictly adhered to.

Meteorological Journal, kept at the Apartments of the R. S., for the Year 1796. *By order of the President and Council.* p. 219.

1796.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	°	°	°	°	°	°	°	°	°	Inc.	Inc.	Inc.	°	°	°	Inc.
Jan.	56	36	47.3	55	38	47.5	62	51	57.2	30.32	29.00	29.72	86	73	79.3	2.128
Feb.	56	30	41.7	55.5	30.5	41.7	58.5	51	55.0	30.31	29.05	29.81	86	66	76.3	1.143
Mar.	60	26.5	41.0	59	27	41.4	60	47	54.0	30.44	29.50	30.03	84	58	70.7	0.074
April	70	36	50.9	68.5	39	51.4	64.5	55	59.8	30.32	29.08	30.04	82	59	70.4	0.302
May	65	39	52.7	64	44	54.0	63	57	60.4	30.22	28.94	29.73	85	63	71.4	2.301
June	80	45	58.8	78	49	59.8	68.5	59	62.2	30.31	29.44	29.96	83	59	69.7	0.536
July	77.5	44.5	61.2	76.5	50	62.0	67	60	64.1	30.18	29.37	29.79	86	61	71.2	1.904
Aug.	80	48.5	62.5	80	52	63.7	72	64	67.2	30.41	29.71	30.06	83	59	71.5	0.529
Sept.	79.8	45	61.9	78	46	61.4	72	61	66.1	30.28	29.46	29.96	88	65	75.1	1.541
Oct.	59	30	48.7	59	32	48.9	61	54.5	57.8	30.55	29.17	29.94	86	65	77.2	1.803
Nov.	57	29	42.2	57	29	42.2	60	50	54.3	30.29	29.18	29.83	88	68	80.0	1.209
Dec.	51.5	4	32.1	49	5	32.1	53	43	47.5	30.51	29.24	29.83	90	73	81.9	1.309
Whole year.			50.1			50.5			58.8			29.89			74.6	14.779

* The quicksilver in the basin of the barometer is 81 feet above the level of low water spring tides at Somerset-house.

XI. On the Action of Nitre on Gold and Platina. By Smithson Tennant, Esq., F.R.S. p. 219.

Gold, which cannot be calcined by exposure to heat and air, has been also considered as incapable of being affected by nitre. But in the course of some experiments on the diamond, an account of which has been communicated to the R. S., I observed, that when nitre was heated in a tube of gold, and the diamond was not in sufficient quantity to supply the alkali of the nitre with fixed air, a part of the gold was dissolved. From this observation I was induced to examine more particularly the action of nitre on gold, as well as to inquire whether it would produce any effect on silver and platina.

With this intention I put some thin pieces of gold into the tube together with nitre, and exposed them to a strong red heat for 2 or 3 hours. After the tube was taken from the fire the part of the nitre which remained, consisting of caustic alkali, and of nitre partially decomposed, weighed 140 grs.; and 60 grs. of the gold were found to have been dissolved. On the addition of water about 50 grs. of the gold were precipitated, in the form of a black powder. The gold which was thus precipitated was principally in its metallic state, the greater portion of it being insoluble in marine acid. The remaining gold, about 10 grs. in weight, communicated to the alkaline solution, in which it was retained, a light yellow colour. By dropping into this solution diluted vitriolic or nitrous acid, it became at first of a deeper yellow, but if viewed by the transmitted light, it soon appeared green, and afterwards blue. This alteration of the colour from yellow to blue arises from the gradual precipitation of the gold in its metallic form, which by the transmitted light is of a blue colour. Though the gold is precipitated from this solution in its metallic form, yet there seems to be no doubt that while it remains dissolved it is entirely in the state of calx. Its precipitation in the metallic state is occasioned by the nitre contained in the solution, which having lost part of its oxygen by heat, appears to be capable of attracting it from the calx of gold; for I found that if the calx of gold is dissolved by being boiled in caustic alkali, and a sufficient quantity of nitre which has lost some of its air by heat is mixed with it, the gold is precipitated by an acid in its metallic state.*

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* As the precipitation of gold in its metallic form, by nitre which has lost some of its oxygen has not, I believe, been noticed, it may not be improper to mention some of those facts relating to it which seem most entitled to attention. Nitre, which has been heated some time, precipitates gold in its metallic state from a solution in aqua regia, if it be diluted with water. If a solution of gold in nitrous acid be dropped into pure water, the calx of gold is separated, which is of a yellow colour; but if the water contain a very small proportion of nitre which has lost some of its air by heat, as 1 gr. in 6 oz., the gold is deprived of its oxygen, and becomes blue. The alkali of the nitre does not assist in producing this effect. Nitrous acid alone, which does not contain its full proportion of oxygen, occasions the same precipitation, unless it is very strong; and if a mixture of such strong nitrous acid, and of a solution of gold in nitrous acid, be dropped into water, the gold is deprived of its oxygen, and is precipitated of a blue colour. Two causes contribute to produce this effect on the addition of water. The adhesion of the calx of gold to nitrous acid is by that means weakened, and the oxygen is attracted

Having found that nitre would dissolve gold, I tried whether it would produce any effect on platina. It has been formerly observed that the grains of platina, in the impure state in which it is originally found, might, by being long heated in a crucible with nitre, be reduced to powder. Lewis, from his own experiments and those of Margraaf, thought that the iron only which is contained in the grains of platina was corroded by the nitre. But by heating nitre with some thin pieces of pure platina in a cup of the same metal, I found that the platina was easily dissolved, the cup being much corroded, and the thin pieces entirely destroyed. By dissolving the saline matter in water, the greater part of the platina was precipitated in the form of a brown powder. This powder, which was entirely soluble in marine acid, consisted of the calx of platina, combined with a portion of alkali, which could not be separated by being boiled in water. The platina which was retained by the alkaline solution communicated to it a brown-yellow colour. By adding an acid to it a precipitate was formed, which consisted of the calx of platina, of alkali, and of the acid which was employed.

Silver, I found to be a little corroded by nitre. But as its action on that metal was very inconsiderable, it did not appear to be deserving of a more particular examination.

XII. Experiments to determine the Force of Fired Gunpowder. By Benjamin Count of Rumford, F. R. S., M. R. I. A. p. 222.

Several eminent philosophers and mathematicians have, from time to time, employed their attention on this curious subject; and the modern improvements in chemistry have given us a considerable insight into the cause, and the nature of the explosion which takes place in the inflammation of gunpowder; and the nature and properties of the elastic fluids generated in its combustion. But the great desideratum, the real measure of the initial expansive force of inflamed gunpowder, so far from being known, has hitherto been rather guessed at than determined; and no argument can be more convincing to show our total ignorance on that subject, than the difference in the opinions of the greatest mathematicians of the age, who have undertaken its investigation. The ingenious Mr. Robins, who made a great number of very curious experiments on gunpowder, and who I believe has done more towards perfecting the art of gunnery than any other individual, concluded, as the result of all his inquiries and computations, that the force of the elastic fluid generated in the combustion of gunpowder is 1000 times greater than the mean pressure of the atmosphere.* But the celebrated mathematician Daniel Bernouilli determines its force to be not less than 10,000 times that pressure, or 10 times greater than Mr. Robins made it.

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more strongly to the imperfect nitrous acid, in consequence of their attraction for water when they are united.—Orig.

* Mr. Robins's experiments in proof of this, in his New Principles of Gunnery, are very simple; and sufficiently convincing to show that his number, 1000, is not very greatly wide of the truth in the small quantities he used. In fact it is gradually increased a little more, as the quantity of powder fired is greater, till the number increase to near 2000 times the pressure of the atmosphere.

Struck with this great difference in the results of the computations of these two able mathematicians, as well as with the subject itself, which appeared both curious and important, I many years ago set about making experiments on gunpowder, with a view principally of determining the point in question, namely, its initial expansive force when fired; and I have ever since occasionally, from time to time, as I have found leisure and convenient opportunities, continued these inquiries. In a paper printed in the year 1781, in the 71st vol. of the Philos. Trans., I gave an account of an experiment (No. 92), by which it appeared that, calculating even on Mr. Robins's own principles, the force of gunpowder, instead of being 1000 times, must at least be 1308 times greater than the mean pressure of the atmosphere. However, not only that experiment, but many others, mentioned in the same paper, had given me abundant reason to conclude that the principles assumed by Mr. Robins, in his treatise on gunnery, were erroneous; and I saw no possibility of ever being able to determine the initial force of gunpowder by the methods he had proposed, and which I had till then followed in my experiments. Unwilling to abandon a pursuit which had already cost me much pains, I came to a resolution to strike out a new road, and to endeavour to ascertain the force of gunpowder by actual measurement, in a direct and decisive experiment.

I shall not here give a detail of the numerous difficulties and disappointments I met with in the course of these dangerous pursuits; it will be sufficient briefly to mention the plan of operations I formed, in order to obtain the end I proposed, and to give a cursory view of the train of unsuccessful experiments by which I was at length led to the discovery of the truly astonishing force of gunpowder;—a force at least fifty thousand* times greater than the mean pressure of the atmosphere!

My first attempts were to fire gunpowder in a confined space, thinking, that when I had accomplished this, I should find means, without much difficulty, to measure its elastic force. To this end, I caused a short gun-barrel to be made, of the best wrought iron, and of uncommon strength; the diameter of its bore was ¾ of an inch, its length 5 inches, and the thickness of the metal was equal to the diameter of the bore, so that its external diameter was 2¼ inches. It was closed at both ends, by 2 long screws, like the breech-pin of a musket; each entering 2 inches into the bore, leaving only a vacuity of 1 inch in length for the charge. The powder was introduced into this cavity by taking out one of the screws, or breech-pins; which being afterwards screwed into its place again, and both ends of the barrel closed up, fire was communicated to the powder by a very narrow vent, made in the axis of one of the breech-pins for that purpose. The chamber, which was 1 inch in length, and ¾ of an inch in diameter, being about half filled with powder, I expected that when the powder should be fired, the generated elastic fluid,

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* It must seem strange to every pilosopher to be told that a force here said to be 50 thousand times greater than that of the atmosphere, should produce effects no greater than are experienced in projectiles. But more of this hereafter.

being obliged to issue out at so small an opening as the vent, which was no more than $\frac{1}{20}$ of an inch in diameter, instead of giving a smart report, would come ou with something like a hissing noise; and I intended, in a future experiment, to confine the generated elastic fluid entirely, by adding a valve to the vent, as in some of the experiments in my paper published in the 71st volume of the Philos. Trans. But on setting fire to the charge, instead of a hissing noise, I was surprized by a very sharp and a very loud report; and, on examining the barrel, I found the vent augmented to at least 4 times it former dimensions, and both the screws loosened.*

Finding, by the result of this experiment, that I had to do with an agent much more troublesome to manage than I had imagined, I redoubled my precautions. As the barrel was not essentially injured, its ends were now closed up by 2 new screws, which were firmly fixed in their places by solder, and a new vent was opened in the barrel itself. As both ends of the barrel were now closed up, it was necessary, in order to introduce the powder into the chamber, to make it pass through the vent, or to convey it through some other aperture made for that purpose. The method I employed was as follows: a hole being made in the barrel, about $\frac{2}{10}$ of an inch in diameter, a plug of steel was screwed into this hole; and it was in the centre or axis of the plug that the vent was made. To introduce the powder into the chamber, the plug was taken away. The vent was made conical, its largest diameter being inwards, or opening into the chamber; and a conical pin of hardened steel was fitted into it; which was intended to serve as a valve for closing up the vent, as soon as the powder in the chamber should be inflamed. To give a passage to the fire through the vent in entering the chamber, this pin was pushed a little inwards, so as to leave a small vacuity between its surface and the concave surface of the bore of the vent. But though all possible care was taken in the construction of this instrument, to render it perfect in all its parts, the experiment was as unsuccessful as the former: on firing the powder in the chamber, though it did not fill more than half its cavity, the generated elastic fluid not only forced its way through the vent, notwithstanding the valve, which appeared not to have had time to close, but it issued with such an astonishing velocity from this small aperture, that instead of coming out with a hissing noise, it gave a report nearly as sharp and as loud as a common musket. On examining the vent-plug and the pin, they were both found to be much corroded and damaged; though I had taken the precaution to harden them both before making the experiment.

I afterwards repeated the experiment with a simple vent, made very narrow, and lined with gold, to prevent its being corroded by the acid vapour generated in the combustion of the gunpowder; but this vent was found to be as little able to with-

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* This effect is nothing more than almost similar to what was experienced by Mr. Robins, viz. when the bullets were placed in the gun-barrel at some distance from the charge of powder: for then it was found that the barrel was greatly enlarged, like a blown bladder, just behind the bullet. An effect obviously produced by the elastic fluid acting there as a momentum, or percussive force.

stand the amazing force of the inflamed gunpowder as the others. It was so much and so irregularly corroded, by the explosion in the first experiment, as to be rendered quite unserviceable; and the barrel itself, notwithstanding its amazing strength, was blown out into the form of a cask; and though it was cracked, it was not burst quite asunder, nor did it appear that any of the generated elastic fluid had escaped through the crack.*

These unsuccessful attempts, and many others of a similar nature, of which it is not necessary to give a particular account, as they all tended to show that the force of fired gunpowder is in fact much greater than has generally been imagined, instead of discouraging me from pursuing these inquiries, served only to excite my curiosity still more, and to stimulate me to further exertions. These researches did not by any means appear to be merely speculative; on the contrary, I considered the determination of the real force of the elastic fluid generated in the combustion of gunpowder as a matter of great importance. The use of gunpowder is become so extensive, that very important mechanical improvements can hardly fail to result from any new discoveries relating to its force, and the law of its action. Most of the computations that have hitherto been made relative to the action of gunpowder, have been founded on the supposition that the elasticity of the generated fluid is as its density;† but if this supposition should prove false, all those computations, with all the practical rules founded on them, must necessarily be erroneous; and the influence of these errors must be as extensive as the uses to which gunpowder is applied.

Having found by experience how difficult it is to confine the elastic vapour generated in the combustion of gunpowder, when the smallest opening is left by which any part of it can escape, it occurred, that I might perhaps succeed better by closing up the powder entirely, in such a manner as to leave no opening whatever, by which it could communicate with the external air; and by setting the powder on fire, by causing the heat employed for that purpose to pass through the solid substance of the iron barrel used for confining it. Inorder to make this experiment, I caused a new barrel to be constructed for that purpose: its length was 3.45 inches, and the diameter of its bore $\frac{7}{10}$ of an inch; its ends were closed up by two screws, each 1 inch in length, which were firmly and immoveably fixed in their places by solder; a vacuity being left between them in the barrel 1.45 inch in length, which constituted the chamber of the piece; and whose capacity was nearly $\frac{6}{10}$ of a cubic inch. A hole, 0.37 of an inch in diameter, being bored through both sides of the barrel, through the centre of the chamber, and at right angles to its axis, 2 tubes of iron, 0.37 of an inch in diameter, the diameter of whose bore was $\frac{1}{10}$ of an inch, were firmly fixed in this hole with solder, in such a manner that while their internal openings were exactly opposite to each other, and on opposite sides of the chamber, the axes of their

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* This effect is to be explained also by the preceding note. † It might have been added, "when of the same temperature," as Mr. Robins and others have proved it to be.

bores were in the same right line. The shorter of these tubes, which projected 1.3 inch beyond the external surface of the barrel, was closed at its projecting end, or rather it was not bored quite through its whole length, $\frac{3}{10}$ of an inch of solid metal being left at its end, which was rounded off in the form of a blunt point. The longer tube, which projected 2.7 inches beyond the surface of the barrel on the other side, and which served for introducing the powder into the chamber, was open; but it could occasionally be closed by a strong screw, furnished with a collar of oiled leather, provided for that purpose. The method of making use of this instrument was as follows. The barrel being laid down, or held, in a horizontal position, with the long tube upwards, the charge, which was of the very best fine-grained glazed powder, was poured through this tube into the chamber. In doing this, care must be taken that the cavity of the short tube be completely filled with powder, and this can best be done by pouring in only a small quantity of powder at first, and then, by striking the barrel with a hammer, cause the powder to descend into the short tube. When, by introducing a priming-wire through the long tube, it is found that the short tube is full, it ought to be gently pressed together, or rammed down, by means of the priming-wire, to prevent its falling back into the chamber on moving the barrel out of the horizontal position. The short tube being properly filled, the rest of the charge may be introduced into the chamber, and the end of the long tube closed up by its screw.

More effectually to prevent the elastic fluid, generated in the combustion of the charge, from finding a passage to escape by this opening, after the charge was introduced into the chamber, the cavity of the long tube was filled up with cold tallow, and the screw that closed up its end, which was $\frac{1}{2}$ an inch long, and but a little more than $\frac{1}{10}$ of an inch in diameter, was pressed down against its leather collar with the utmost force. The manner of setting fire to the charge was as follows: a block of wrought iron, about $1\frac{1}{2}$ inch square, with a hole in it, capable of receiving nearly the whole of that part of the short tube which projects beyond the barrel, being heated red-hot, the end of the short tube was introduced into this hole, where it was suffered to remain till the heat, having penetrated the tube, set fire to the powder it contained, and the inflammation was thence communicated to the powder in the chamber.

The result of this experiment fully answered my expectations. The generated elastic fluid was so completely confined that no part of it could make its escape. The report of the explosion was so very feeble, as hardly to be audible: indeed it did not by any means deserve the name of a report, and certainly could not have been heard at the distance of 20 paces; it resembled the noise occasioned by the breaking of a very small glass tube. I imagined at first that the powder had not all taken fire, but the heat of the barrel soon convinced me that the explosion must have taken place, and after waiting near half an hour, on loosening the screw which closed the end of the long vent tube, the confined elastic vapours rushed out with considerable force, and with a noise like that attending the discharge of an air-gun. The quantity of powder used in the experiment was indeed very small, not amounting to more than $\frac{1}{8}$ part of what the chamber was capable of containing; but having so often had my machinery destroyed in experiments of this sort, I began now to be more cautious.

Having found means to confine the elastic vapour generated in the combustion of gunpowder, my next attempts were to measure its force: but here again I met with new and almost insurmountable difficulties. To measure the expansive force of the vapour, it was necessary to bring it to act on a moveable body of known dimensions, and whose resistance to the efforts of the fluid could be accurately determined; but this was found to be extremely difficult. I attempted it in various ways, but without success. I caused a hole to be bored in the axis of one of the screws, or breech-pins, which closed up the ends of the barrel just described, and fitting a piston of hardened steel into this hole, which was $\frac{2}{10}$ of an inch in diameter, and causing the end of the piston which projected beyond the end of the barrel to act on a heavy weight, suspended as a pendulum to a long iron rod, I hoped, by knowing the velocity acquired by the weight, from the length of the arc described by it in its ascent, to be able to calculate the pressure of the elastic vapour by which it was put in motion: but this contrivance was not found to answer, nor did any of the various alterations and improvements I afterwards made in the machinery render the results of the experiment at all satisfactory. It was not only found almost impossible to prevent the escape of the elastic fluid by the sides of the piston, but the results of apparently similar experiments were so very different, and so uncertain, that I was often totally at a loss to account for these extraordinary variations. I was however at length led to suspect, what I afterwards found abundant reason to conclude was the real cause of these variations, and of all the principal difficulties which attended the ascertaining the force of fired gunpowder by the methods I had hitherto pursued.

It has generally been believed, after Mr. Robins, that the force of fired gunpowder consists in the action of a permanently elastic fluid, similar in many respects to common atmospheric air; which being generated from the powder in combustion, in great abundance, and being in a very compressed state, and its elasticity being much augmented by the heat, which is also generated in the combustion, it escapes with great violence, by every avenue; and produces that loud report, and all those terrible effects, which attend the explosion of gunpowder. But though this theory is very plausible, and seems on a cursory view of the subject to account in a satisfactory manner for all the phenomena, yet a more careful examination will show it to be defective. There is no doubt but the permanently elastic fluids, generated in the combustion of gunpowder, assist in producing those effects which result from its explosion; but it will be found, I believe, on ascertaining the real expansive force of fired gunpowder, that this cause alone is quite inadequate to the effects actually produced; and that therefore the agency of some other power must necessarily be called in to its assistance.

Mr. Robins has shown, that if all the permanently elastic fluid generated in the combustion of gunpowder be compressed in the space originally occupied by the powder, and if this fluid so compressed be supposed to be heated to the intense heat of red-hot iron, its elastic force in that case will be 1000 times greater than the mean pressure of the atmosphere; and this, according to his theory, is the real measure of the force of gunpowder, fired in a cavity which it exactly fills. But what will become of this theory, and of all the suppositions on which it is founded, if I shall be able to prove, as I hope to do in the most satisfactory manner, that the force of fired gunpowder, instead of being 1000 times, is at least 50,000 greater than the mean pressure of the atmosphere?*

For my part, I know of no way of accounting for this enormous force, but by supposing it to arise principally from the elasticity of the aqueous vapour† generated from the powder in its combustion. The brilliant discoveries of modern chemists have taught us, that both the constituent parts of which water is composed, and even water itself, exist in the materials which are combined to make gunpowder; and there is much reason to believe that water is actually formed, as well as disengaged, in its combustion. M. Lavoisier, I know, imagined that the force of fired gunpowder depends in a great measure on the expansive force of uncombined caloric, supposed to be let loose in great abundance during the combustion or deflagration of the powder: but it is not only dangerous to admit the action of an agent whose existence is not yet clearly demonstrated, but it appears to me that this supposition is quite unnecessary; the elastic force of the heated aqueous vapour, whose existence can hardly be doubted, being quite sufficient to account for all the phenomena. It is well known that the elasticity of aqueous vapour is incomparably more augmented by any given augmentation of temperature, than that of any permanently elastic fluid whatever; and those who are acquainted with the amazing force of steam, when heated only to a few degrees above the boiling point, can easily perceive that its elasticity must be almost infinite when greatly condensed and heated to the temperature of red-hot iron; and this heat it must certainly acquire in the explosion of gunpowder. But if the force of fired gunpowder arises principally from the elastic force of heated aqueous vapour, a cannon is nothing more than a steam-engine on a peculiar construction; and on determining the ratio of the elasticity of this vapour to its density, and to its temperature, a law will be found to obtain, very different from that assumed by Mr. Robins, in his Treatise on Gunnery. What this law really is, I do not pretend to have determined with that degree of precision that I wished; but the experiments of which I am about to give an account will, I think, demonstrate in the most satisfactory manner, not only that the force of fired gunpowder is in fact much greater than has been imagined, but

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* We shall examine afterwards the fallacy of this assertion.

† On the contrary, the less quantity of moisture that is in the powder, or the drier it is made, the stronger it is always found to be.

also that its force consists principally in the temporary action of a fluid not permanently elastic, and consequently that all the theories hitherto proposed for the elucidation of this subject, must be essentially erroneous.

It would take up too much time, and draw out this paper to too great a length, to give an account in detail of all the experiments, and of the various observations I have had opportunities of making from time to time, relative to this subject. I shall therefore only observe at present, that the result of all my inquiries tended to confirm me more and more in the opinion, that the theory generally adopted relative to the explosion of gunpowder was extremely erroneous, and that its force is in fact much greater than is generally imagined. That the position of Mr. Robins, which supposes the inflammation and combustion of gunpowder to be so instantaneous "that the whole of a charge of a piece of ordnance is actually inflamed and converted into an elastic vapour before the bullet is sensibly moved from its place," is very far from being true; and that the ratio of the elasticity of the generated fluid, to its density, or to the space it occupies as it expands, is very different from that assumed by Mr. Robins. The rules laid down by Mr. Robins for computing the velocities of bullets from their weight, the known dimensions of the gun, and the quantities of powder used for the charge, may, and certainly do, very often give the velocities very near the truth; but this is no proof that the principles on which these computations are made are just; for it may easily happen, that a complication of erroneous suppositions may be so balanced, that the result of a calculation founded on them may yet be very near the truth; and this is never so likely to happen as when, from known effects, the action of the powers which produce them are computed. For it is not in general very difficult to assume such principles as, when taken together, may in the most common known cases answer completely all the conditions required. But in such cases, if the truth be discovered with regard to any one of the assumed principles, and it be substituted instead of the erroneous supposition, the fallacy of the whole hypothesis will immediately become evident.

It may perhaps with reason be asked, what the circumstances were which attended former experiments, which could justify so important a conclusion as that of the fallacy of the commonly received theory relative to that subject. To this I answer briefly, that in regard to the supposed instantaneous inflammation of the powder, on which the whole fabric of this theory is built, or rather of all the computations which are grounded on it, a careful attention to the phenomena which take place on firing off cannon led me to suspect, or rather confirmed me in my former suspicions, that however rapid the inflammation of gunpowder may be, its total combustion is by no means so sudden as this theory supposes. When a heavy cannon is fired in the common way, that is, when the vent is filled with loose powder, and the piece is fired off with a match, the time employed in the passage of the inflammation through the vent into the chamber of the piece is perfectly sensible, and this time is evidently shorter after the piece has been heated by repeated firing. With the same charge, the recoil of a gun, and consequently the velocity of its bullet, is greater after the gun had been heated by repeated firing than when it is cold. The velocity of the bullet is considerably greater when the cannon is fired off with a vent tube, or by firing a pistol charged with powder into the open vent, than when the vent is filled with loose powder. The velocity of 2, or 3, or more, fit bullets, discharged at once from a piece of ordnance, compared to the velocity of 1 single bullet discharged by the same quantity of powder, from the same cannon, is greater* than it ought to be according to the theory. Considerable quantities† of powder are frequently driven out of cannon and other fire-arms unconsumed. The manner in which the smoke of gunpowder rises in the air, and is gradually dissolved and rendered invisible, shows it to partake of the nature of steam. But not to take up too much time with these general observations, I shall proceed to give an account of experiments the results of which will be considered as more conclusive.

Having found it impossible to measure the elastic force of fired gunpowder with any degree of precision by any of the methods before-mentioned, I totally changed my plan of operations, and instead of endeavouring to determine its force by causing the generated elastic fluid to act on a moveable body through a determined space, I set about contriving an apparatus in which this fluid should be made to act, by a determined surface, against a weight, which by being increased at pleasure should at last be such as would just be able to confine it, and which in that case would just counterbalance and consequently measure its elastic force. The idea of this method of determining the force of fired gunpowder occurred to me many years ago; but a very expensive and troublesome apparatus being necessary in order to put it in execution, it was not till the year 1792, when, being charged with the arrangement of the army of his most Serene Highness the Elector Palatine, reigning Duke of Bavaria, and having all the resources of the military arsenal, and a number of very ingenious workmen at my command, with the permission and approbation of his most Serene Electoral Highness, I set about making the experiments which I shall now describe: and as they are not only important in themselves, and in their results, but as they are, I believe, the first of the kind that have been made, I shall be very particular in my account of them, and of the apparatus used in making them.

One difficulty being got over, that of setting fire to the powder without any communication with the external air, by causing the heat employed for that purpose to pass through the solid substance of the barrel, it only remained to apply such a weight to an opening made in the barrel as the whole force of the generated elastic fluid should not be able to lift, or displace‡; but in doing this many precautions were necessary. For, first, as the force of gunpowder is so very great,

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* Very little greater; and that only on account of a small portion of powder more fired.—† Very small quantities, in proportion to the whole charges.—‡ In this way of applying the force of the elastic fluid, it is to be observed that its first impulse acts as a momentum or percussive force, to which the opposed weight can be no measure, nor can be compared.

it was necessary to employ an enormous weight to confine it; for, though by diminishing the size of the opening, the weight would be lessened in the same proportion, yet it was necessary to make this opening of a certain size, otherwise the experiments would not have been satisfactory; and it was necessary to make the support or base on which the barrel was placed very massy and solid, to prevent the errors which would unavoidably have arisen from its want of solidity, or from its elasticity. In these experiments, first a solid block of very hard stone, 4 feet 4 inches square, was placed on a bed of solid masonry, which descended 6 feet below the surface of the earth. On this block of stone, which served as a base to the whole machinery, was placed the barrel of hammered iron, on its support of cast brass, or rather of gun-metal; which support was again placed on a circular plate of hammered iron, 8 inches in diameter, and $\frac{3}{4}$ of an inch thick, which last rested on the block of stone. The opening of the bore of the barrel, which was placed in a vertical position, and which was just $\frac{1}{4}$ of an inch in diameter, was closed by a solid hemisphere of hardened steel, whose diameter was 1.16 inch; and on this hemisphere rested a weight, employed for confining the elastic fluid generated from the powder in its combustion. This weight, which in some of the most interesting experiments was a cannon of metal, a heavy 24-pounder, placed vertically on its cascabel, being fixed to the timbers which formed a kind of carriage for it, was moveable up and down; the ends of these timbers being moveable in grooves cut in the vertical timbers, which being fixed below in holes made to receive them in the block of stone, and above by a cross piece, were supported by braces and iron clamps made fast to the thick walls of the building of the arsenal. This weight was occasionally raised and lowered in the course of the experiments, in placing and removing the barrel, by means of a very strong lever. The barrel was 2.78 inches long, and 2.82 inches in diameter, at its lower extremity, where it rested on its supporter, but something less above, being somewhat diminished, and rounded off at its upper extremity. Its bore, of $\frac{1}{4}$ of an inch in diameter, was 2.13 inches long, and it ended in a very narrow opening below, not more than 0.07 of an inch in diameter, and 1.715 inch long, forming the vent, if I may be permitted to apply that name to a passage which is not open at both ends, by which the fire was communicated to the charge. From the centre of the bottom of the barrel was a projection of about 0.45 of an inch in diameter, and 1.3 inch long, forming the vent tube. There was an iron ball, which being heated red-hot, and applied to the vent tube by means of a hole made in it for that purpose, fire was communicated through the solid substance of the vent tube to the contained powder, and thence to the charge.

The powder used in these experiments was of the best quality, being that kind called poudre de chasse by the French, and very fine grained. Care was taken to dry it very thoroughly, and the air of the room in which it was weighed out for use was very dry*. The weights employed for weighing the powder were German

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* Why all this care in dryness, if moisture was expected to increase its force?

apothecary's grains, 104.8 of which make 1000 grains Troy. But the weights used were reduced to pounds avoirdupois. The measures of length were all taken in English feet and inches. The experiments were all made in the open air, in the court-yard of the arsenal at Münich; and they were all made in fair weather, and between the hours of 9 and 12 in the forenoon, and 2 and 5 in the afternoon; but the barrel was always charged, and the extremity of the bore closed by a leather stopper, in the room where the powder was weighed. In placing the barrel on the block of stone, great care was taken to put it exactly under the centre of gravity of the weight employed to confine the generated elastic vapour. On applying the red-hot ball to the vent tube, and fixing it in its place by its lever which supported it, the explosion very soon followed.

When the force of the generated elastic vapour was sufficient to raise the weight, the explosion was attended by a very sharp and surprizingly loud report; but when the weight was not raised, as also when it was only a little moved, but not sufficiently to permit the leather stopper to be driven quite out of the bore, and the elastic fluid to make its escape, the report was scarcely audible at the distance of a few paces, and did not at all resemble the report which commonly attends the explosion of gunpowder. It was more like the noise which attends the breaking of a small glass tube than any thing else to which I can compare it. In many of the experiments in which the elastic vapour was confined, this feeble report attending the explosion of the powder was immediately followed by another noise, totally different from it, which appeared to be occasioned by the falling of the weight on the end of the barrel, after it had been a little raised, but not sufficiently to permit the leather stopper to be driven quite out of the bore. In some of these experiments, a very small part only of the generated elastic fluid made its escape: in these cases the report was of a particular kind, and though perfectly audible at some considerable distance, yet not at all resembling the report of a musket. It was rather a very strong, sudden hissing, than a clear, distinct, and sharp report. Though it could be determined with the utmost certainty by the report of the explosion, whether any part of the generated elastic fluid had made its escape, yet for still greater precaution, a light collar of very clean cotton wool was placed round the edge of the steel hemisphere, where it rested on the end of the barrel, which could not fail to indicate by the black colour it acquired, the escape of the elastic fluid, whenever it was strong enough to raise the weight by which it was confined sufficiently to force its way out of the barrel. After using a great variety of expedients, the best and most convenient method of closing the end of the bore, and defending the flat surface of the steel hemisphere from the corroding vapours, was found to be this: first, to cover the end of the bore with a circular plate of thin oiled leather; then to lay on this a very thin circular plate of hammered brass; and on this brass plate the flat surface of the hemisphere. When the elastic fluid made its escape, a part of the leather was constantly found to have been torn away, but never in more places than one; viz. always on one side only.

What was very remarkable in all those experiments in which the generated elastic vapour was completely confined, was the small degree of expansive force which this vapour appeared to possess after it had been suffered to remain a few minutes, or even only a few seconds, confined in the barrel: for, on raising the weight by means of its lever, and suffering this vapour to escape, instead of escaping with a loud report, it rushed out with a hissing noise hardly so loud or so sharp as the report of a common air-gun; and its efforts against the leathern stopper, by which it assisted in raising the weight, were so very feeble as not to be sensible. On examining the barrel however, this diminution of the force of the generated elastic fluid was easily explained; for what was undoubtedly in the moment of the explosion in the form of an elastic fluid, was now found transformed into a solid body as hard as a stone! It may easily be imagined how much this unexpected appearance excited my curiosity; but, intent on the prosecution of the main design of these experiments, the ascertaining the force of fired gunpowder, I was determined not to permit myself to be enticed away from it by any extraordinary or unexpected appearances, or accidental discoveries, however alluring they might be; and, faithful to this resolution, I postponed the examination of this curious phenomenon to a future period; and since that time I have not found leisure to engage in it. I think it right however, to mention in this place such cursory observations as I was able, in the midst of my other pursuits, to make on this subject; and it will afford me sincere pleasure, if what I have to offer should so far excite the curiosity of philosophers, as to induce some one who has leisure, and the means of pursuing such inquiries with effect, to precede me in the investigation of this interesting phenomenon; and as the subject is certainly not only extremely curious in itself, but bids fair to lead to other and very important discoveries, I cannot help flattering myself that some attention will be paid to it. I have said that the solid substance, into which the elastic vapour generated in the combustion of gunpowder was transformed, was as hard as a stone. This I am sensible is but a vague expression; but the fact is, that it was very hard, and so firmly attached to the inside of the barrel, and particularly to the inside of the upper part of the vent tube, that it was always necessary, in order to remove it, to make use of a drill, and frequently to apply a considerable degree of force. This substance, which was of a black colour, or rather of a dirty grey, that changed to black on being exposed to the air, had a pungent, acrid, alkaline taste, and smelt like liver of sulphur. It attracted moisture from the air with great avidity. Being moistened with water, and spirit of nitre being poured on it, a strong effervescence ensued, attended by a very offensive and penetrating smell. Nearly the whole quantity of matter of which the powder was composed, seemed to have been transformed into this substance; for the quantity of elastic fluid which escaped on removing the weight, was very inconsiderable: but this substance was no longer gunpowder; it was not even inflammable. What change had it undergone? what could it have lost? It is very certain that the barrel was considerably heated in these experiments. Was this occasioned by the caloric, disengaged from the powder in its combustion, making its escape through the iron? And is this a proof of the existence of caloric, considered as a fluid sui generis; and that it actually enters into the composition of inflammable bodies, or of pure air, and is necessary to their combustion? I dare not take upon me to decide on such important questions. I once thought that the heat acquired by a piece of ordnance in being fired, arose from the vibration or friction of its parts, occasioned by the violent blow it received in the explosion of the powder; but I acknowledge fairly, that it does not seem to be possible to account in a satisfactory manner for the very considerable degree of heat which the barrel acquired in these experiments, merely on that supposition.*

That this hard substance, found in the barrel after an experiment in which the generated elastic vapour had been completely confined, was actually in a fluid or elastic state in the moment of the explosion, is evident from hence, that in all those cases in which the weight was raised, and the stopper blown out of the bore, nothing was found remaining in the barrel. It was very remarkable that this hard substance was not found distributed about in all parts of the barrel indifferently, but more of it was always found near the middle of the length of the bore, than at either of its extremities; and the upper part of the vent tube in particular was always found quite filled with it. It should seem from hence, that it attached itself to those parts of the barrel which were soonest cooled; and hence the reason, most probably, why none of it was ever found in the lower part of the vent-tube, where it was kept hot by the red-hot ball by which the powder was set on fire.

I found by a particular experiment, that the gunpowder employed, when it was well shaken together, occupied rather less space in any given measure, than the same weight of water; consequently, when gunpowder of this kind is fired in a confined space which it fills, the density of the generated elastic fluid must be at least equal to the density of water.† The real specific gravity of the solid grains of gunpowder, determined by weighing them in air and water, is to the specific gravity of water, as 1.868 to 1.000. But if a measure, whose capacity is 1 cubic foot, hold 1000 ounces of water, the same measure will hold just 1077 ounces of fine grained gunpowder, such as was employed in these experiments; that is to say, when it is well shaken together. When it was moderately shaken together, its weight was exactly equal to that of an equal volume, or rather measure, of water. But it is evident that the weight of any given measure of gunpowder, must depend much on the forms and sizes of its grains. I shall add only one observation more, relative to the particular appearances which attended the experiments in which the

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* It seems probable however, that the heat may be accounted for on this principle, as, from the confined state of the barrel, the shock from the explosion must be sudden and extremely great.—† Not so: because only a part of the composition of powder, (about $\frac{1}{3}$), is changed into an elastic fluid. So that the density of the fluid is but about $\frac{1}{3}$ of the density of water.

elastic vapour generated in the combustion of gunpowder was confined, and that is, with regard to a curious effect produced on the inferior flat surface of the leathern stopper, where it was in contact with the generated elastic vapour. On removing the stopper, its lower flat surface appeared entirely covered with an extremely white powder, resembling very light white ashes, but which almost instantaneously changed to the most perfect black colour on being exposed to the air.

The sudden change of colour in this substance, on its being exposed to the air, has led me to suspect that the solid matter found in the barrel was not originally black, but that it became black merely in consequence of its being exposed to the air. The dirty grey colour it appeared to have immediately on being drilled out of the cavity of the bore, where it had fixed itself, seems to confirm this suspicion. An experiment made with a very strong glass barrel would not only decide this question, but would most probably render the experiment peculiarly beautiful and interesting on other accounts; and I have no doubt but a barrel of glass might be made sufficiently strong to withstand the force of the explosion. Whether it would be able to withstand the sudden effects of the heat, I own I am more doubtful; but as the subject is so very interesting, I think it would be worth while to try the experiment. Perhaps the apparatus might be so contrived as to set fire to the powder by the solar rays, by means of a common burning glass; but even if that method should fail, there are others equally unexceptionable, which might certainly be employed with success; and it is hardly possible to imagine any thing more curious than an experiment of this kind would be, if it were successful.

But to proceed to the experiments by which I endeavoured to ascertain the force of fired gunpowder. All the parts of the apparatus being ready, it was in the autumn of the year 1792 that the first experiment was made. The barrel being charged with 10 grains of powder (its contents when quite full amounting to about 28 grains), and the end of the barrel being covered by a circular piece of oiled leather, and the flat side of the hemisphere being laid down on this leather, and a heavy cannon, a 24 pounder, weighing 8081 lbs. avoirdupois, being placed on its cascabel in a vertical position on this hemisphere, to confine by its weight the generated elastic fluid, the heated iron-ball was applied to the end of the vent-tube; and I had waited but a very few moments in anxious expectation of the event, when I had the satisfaction of observing that the experiment had succeeded. The report of the explosion was extremely feeble, and so little resembling the usual report of the explosion of gunpowder, that the by-standers could not be persuaded that it was any thing more than a cracking of the barrel, occasioned merely by its being heated by the red-hot ball: yet, as I had been taught by the result of former experiments not to expect any other report, and as I found on putting my hand on the barrel that it began to be sensibly warm, I was soon convinced that the powder must have taken fire; and after waiting 4 or 5 minutes, on raising the weight which rested on the hemisphere, the confined elastic vapour rushed out of the barrel. On removing the barrel and examining it, its bore was found to be choaked up by the solid substance already described, and from which it was with some difficulty that it was freed, and rendered fit for another experiment. The extreme feebleness of the report of the explosion, and the small degree of force with which the generated elastic fluid rushed out of the barrel on removing the weight which had confined it, had inspired my assistants with no very favourable idea of the importance of these experiments. I had seen, indeed from the beginning, by their looks, that they thought the precautions I took to confine so inconsiderable a quantity of gunpowder as the barrel could contain, perfectly ridiculous; but the result of the following experiment taught them more respect for an agent, of whose real force they had conceived so very inadequate an idea.

In this 2d experiment, instead of 10 grains of powder, as in the former charge, the barrel was now quite filled with powder, and the steel hemisphere, with its oiled leather under it, was pressed down on the end of the barrel by the same weight as was employed for that purpose in the first experiment, viz. a cannon weighing 8081 lbs. In order to give a more perfect idea of the result of this important experiment, it may not be amiss to describe more particularly one of the principal parts of the apparatus employed in it, I mean the barrel. This barrel, which though similar to it in all respects, was not the same that has already been described, was made of the best hammered iron, and was of uncommon strength. Its length was 2¾ inches; and though its diameter was also 2¾ inches, the diameter of its bore was no more than ¼ of an inch, or less than the diameter of a common goose quill. The length of its bore was 2.15 inches. Its diameter being 2¾ inches, and the diameter of its bore only ¼ of an inch, the thickness of the metal was 1¼ inch; or, it was 5 times as thick as the diameter of its bore. The charge of powder was extremely small, amounting to but little more than $\frac{1}{10}$ of a cubic inch; not so much as would be required to load a small pocket pistol, and not $\frac{1}{10}$ part of the quantity frequently used for the charge of a common musket. I should be afraid to relate the result of this experiment, had I not the most indisputable evidence to produce in support of the facts. This inconsiderable quantity of gunpowder, when it was set on fire by the application of the red-hot ball to the vent-tube, exploded with such inconceivable force as to burst the barrel asunder in which it was confined, notwithstanding its enormous strength; and with such a loud report as to alarm the whole neighbourhood. It is impossible to describe the surprise of the spectators of this phenomenon. They literally turned pale with affright and astonishment, and it was some time before they could recover themselves. The barrel was not only completely burst asunder, but the 2 halves of it were thrown on the ground in different directions: one of them fell close by my feet, as I was standing near the machinery to observe more accurately the result of the experiment. Though I thought it possible that the weight might be raised, and that the generated elastic vapour would make its escape, yet the bursting of the barrel was totally unexpected by me. It was a new lesson to teach me caution in these dangerous pursuits.

Before giving an account of my subsequent experiments on this subject, I shall stop here for a moment to make an estimate, from the known strength of iron, and the area of the fracture of the barrel, of the real force employed by the elastic vapour to burst it. In a course of experiments on the strength of various bodies which I began many years ago, and an account of which I intend at some future period to lay before the R. S.,* I found, by taking the mean of the results of several experiments, that a cylinder of good tough hammered iron, the area of whose transverse section was only $\frac{3}{1600}$ of an inch, was able to sustain a weight of 119 lbs. avoirdupois, without breaking. This gives 63466 lbs. for the weight which a cylinder of the same iron whose transverse section is 1 inch, would be able to sustain without being broken. The area of the fracture of the barrel before mentioned was measured with the greatest care, and was found to measure very exactly $6\frac{1}{2}$ superficial inches. If now we suppose the iron of which this barrel was formed, to be as strong as that whose strength I determined, and I have no reason to suspect it to be of an inferior quality, in that case, the force actually employed in bursting the barrel must have been equal to the pressure of a weight of 412529 lbs. For the resistance or cohesion of 1 inch, is to 63466 lbs. as that of $6\frac{1}{2}$ inches to 412529 lbs.; and this force, so astonishingly great, was exerted by a body which weighed less than 26 grs. Troy, and which acted in a space that hardly amounted to $\frac{1}{10}$ of a cubic inch.

To compare this force exerted by the elastic vapour generated in the combustion of gunpowder, and by which the barrel was burst, to the pressure of the atmosphere, it is necessary to determine the area of a longitudinal section of the bore of the piece. Now the diameter of the bore being $\frac{1}{4}$ of an inch, and its length (after deducting 0.15 of an inch for the length of the leathern stoppers) 2 inches, the area of its longitudinal section turns out to have been $\frac{1}{2}$ an inch. And if now we assume the mean pressure of the atmosphere = 15 lbs. avoirdupois for each superficial inch, this will give $7\frac{1}{2}$ for that on a surface = $\frac{1}{2}$ inch, equal to the area of a longitudinal section of the bore of the barrel. But we have just found that the force actually exerted by the elastic vapour in bursting the barrel, amounted to 412529 lbs.; this force was therefore 55004 times greater than the mean pressure

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* Since writing the above, I have met with a misfortune which has put it out of my power to fulfil my promise to the R. S. On my return to England from Germany in October, 1795, after an absence of 11 years, I was stopped in my post-chaise in St. Paul's church-yard, in London, at six o'clock in the evening, and robbed of a trunk which was behind my carriage, containing all my private papers and my original notes and observations on philosophical subjects. By this cruel accident I have been deprived of the fruits of the labours of my whole life; and have lost all that I held most valuable. This most severe blow has left an impression on my mind, which I feel that nothing will ever be able entirely to remove.—Orig.

of the atmosphere! For it is as $7\frac{1}{2}$ lbs. to 1 atmosphere, so 412529 lbs. to 55004 atmospheres.*

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* At first glance such an experiment, and such enormous consequences and inferences deduced from it, may probably cause great amazement, and even astonishment, at the assertion of a force 55 times greater than it has hitherto been accounted. But on recollecting himself, after a little reflection, the real and calm philosopher will conclude that there must exist a fallacy in some part of this business, when he considers that an elastic fluid, of a strength less than the 50th part of that above deduced, has been found capable of producing all the experimented and known effects of military projectiles. If these effects, when applied to calculations on unerring mathematical and physical principles, be due to an elastic force of 1000 atmospheres only, or little more, where and what are the effects that must result from a force of 50 times as great? These ought to be 7 or 8 or more times as much as are always experienced in real practice. Where then must the fallacy lie? What can it be? There may be several causes of this illusion: but 2 especially suggest themselves to us, that seem fully adequate to have caused it. These are, the having measured the force of percussion by mere weight or pressure, to which it is not comparable; and the having supposed the small powder barrel to have broken in all parts of its fracture at once, instead of being rent or torn in succession from end to end, as it appears was actually and really the case. From the known rapidity with which gunpowder inflames and expands; it is manifest that its first action must be of the nature of a percussive force, especially when the opposing weight or resistance is at some distance from the charge. Hence it must be in vain to expect that a very heavy weight, when just lifted up by the elastic fluid, is to be considered as the measure of the strength of that fluid: for the two forces, being of different kinds, are quite incommensurable to each other, as much so as a surface and a solid are; the smallest momentum exceeding and overcoming the greatest pressure or mere dead weight; and the least body let fall from above, on one arm of a lever, moving or raising the other arm with the heaviest weight attached to it. The method of measuring the initial force therefore, of the explosion of the powder, by the bare moving of the heavy opposing weight, is a method that must be abandoned, as utterly unfit for the purpose. For nearly the same reason also we must reject the other method of measuring the force, by comparing the bursting of the barrel with the weight that by drawing breaks the rod of iron. For here again, not only is the iron drawn asunder by a weight, which is the proper measure of its cohesive force or strength, while the small charged barrel is acted on by the sudden or percussive force of the exploded powder; but moreover, the particles of the rod, at the fracture, or in the area of its section, do all yield and separate at the same time, while the sides of the small barrel are torn asunder from end to end in all its parts in succession, or one after another, in the manner that we easily tear a sheet of paper or piece of cloth, from end to end, which perhaps our utmost strength could not accomplish if it were closed or twisted all together. That this was the manner in which the barrel was broken and separated into 2 parts, we gather from the circumstances which Count R. has so minutely and commendably described: had he not given the description in so particular a manner, it might not have been so easy to detect the fallacy in this case. When cannon of hard or cast iron break on firing them off, the fracture is made in all its parts at once, and the broken pieces are projected and fly off with amazing velocities and to great distances, like so many balls, to the great danger and sometimes the destruction of the spectators. But when a barrel of soft or tough metal breaks, it is often after the manner of tearing or rending one part after another, and sometimes without entirely separating the barrel in pieces, the force of the explosion being spent before this is effected. Now that this was nearly the case in the present experiment, appears evident from the circumstances that attended it. The 2 pieces of the barrel were separated from each other with a small velocity, and one of them fell at the feet of the experimenter, standing near the machine, on purpose to watch and observe the effects of the explosion: a circumstance that shows the successive tearing of the metal, and that the force of the explosion was nearly quite spent just when the total separation was effected. And fortunate was it for Count R. that this was the case; for had the pieces been blown asunder rapidly, and separated with great velocity, he would probably not have escaped with life to make his report of these experi-

Thinking it might perhaps be more satisfactory to know the real strength of the identical iron of which the barrel used in the before-mentioned experiment was constructed, rather than to rest the determination of the strength of the barrel on the decision of the strength of iron taken from another parcel, and which very possibly might be of a different quality, since writing the above, I have taken the trouble to ascertain the strength of the iron of which the barrel was made, which was done in the following manner. Having the one half of the barrel still in my possession, I caused small pieces, 2 inches long, and about $\frac{1}{8}$ of an inch square, to be cut out of the solid block, in the direction of its length, with a fine saw; and these pieces being first made round in their middle by filing, and then by turning in a lathe with a very sharp instrument, were reduced to such a size as was necessary, in order to their being pulled asunder in my machine for measuring the strength of bodies. In this machine the body to be pulled asunder is held fast by 2 strong vices, the one fastened to the floor, and the other suspended to the short arm of a Roman balance, or common steel-yard; and in order that the bodies so suspended may not be injured by the jaws of the vices, so as to be weakened and to vitiate the experiments, they are not made cylindrical, but larger at their two ends where they are held by the vices, and from thence their diameters were gradually diminished towards the middle of their lengths, where their measures are taken, and where they never fail to break. As I had found by the results of many experiments which I had before made on the strength of the various metals, that iron, as well as all other metals, is rendered much stronger by hammering, I caused those pieces of the barrel which were prepared for these experiments to be separated from the solid block of metal, and reduced to their proper sizes, by sawing, filing, and turning, and without ever receiving a single blow of a hammer; so that there is every reason to believe that the strength of the iron, as determined by the experiments, may safely be depended on. The results of the experiments were as follow:

Experiments.	Diameter of the Cylinder at the Fracture.	Area of a transverse section of the Cylinder at the Fracture.	Weight required to break it. lbs. avoirdupois.	Weight required to break 1 inch of this iron. lbs. avoirdupois.
	Inch.	Inch.		
1	$\frac{50}{1000}$	$\frac{1}{509.29}$	123.18	62737
2	$\frac{60}{1000}$	$\frac{1}{353.68}$	182.	64366
3	$\frac{66}{1000}$	$\frac{1}{292.3}$	220.75	64526
4	$\frac{76}{1000}$	$\frac{1}{220.7}$	277.01	61063
			Number of experiments = 4)	452692
			Mean	63173

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ments. Thus then it appears, that the deduction of the enormous exaggeration of the initial force of the elastic fluid is entirely owing to the improper and unnatural mode of deducing it from a comparison with each other, of things of an incongruous and incommensurable nature. In such ways the most monstrous conclusions may be deduced preposterously from premises the most simple and certain in their own nature.

If now we take the strength of the iron of which the barrel was composed as here determined by actual experiments, and compute the force required to burst the barrel, it will be found equal to the pressure of a weight of 410624½ lbs. instead of 412529 as before determined. For it is the resistance or force of cohesion of 1 inch of this iron to 63173 lbs., as that of 6½ inches, the area of the fracture of the barrel, to 410624½ lbs. And this weight turned into atmospheres, in the manner above described, gives 54750 atmospheres for the measure of the force which must have been exerted by the elastic fluid in bursting the barrel. But this force, enormous as it may appear, must still fall short of the real initial force of the elastic fluid generated in the combustion of gunpowder, before it has begun to expand; for it is more than probable that the barrel was in fact burst before the generated elastic fluid had exerted all its force, or that this fluid would have been able to have burst a barrel still stronger than that used in the experiment.—But I wave these speculations in order to hasten to more interesting and more satisfactory investigations. Passing over in silence a considerable number of promiscuous experiments, which, having nothing particularly remarkable in their results, could throw no new light on the subject, I shall proceed immediately to give an account of a regular set of experiments, undertaken with a view to the discovery of certain determined facts, and prosecuted with unremitting perseverance.

These experiments were made by my directions under the immediate care of Mr. Reichenbach, commandant of the corps of artificers in the Elector's military service, and of Count Spreti, first lieutenant in the regiment of artillery. Though I was prevented by ill health from being actually present at all these experiments, yet being at hand, and having every day, and almost every hour, regular reports of the progress that was made in them, and of every thing extraordinary that happened, the experiments may be said with great truth to have been made under my immediate direction; and as the two gentlemen by whom I was assisted, were not only every way qualified for such an undertaking, but had been present, and had assisted me in a number of similar experiments which I had myself made, they had acquired all that readiness and dexterity in the various manipulations which are so useful and necessary in experimental inquiries; and I think I can safely venture to say that the experiments may be depended on. It would have afforded me great satisfaction to have been able to say that the experiments were all made by myself; and I had resolved to repeat them before I made them public, particularly as there appear to have been some very extraordinary and quite unaccountable differences in the results of those made in different seasons of the year; but having hitherto been prevented by ill health, and by other avocations, from engaging again in these laborious researches, I have thought it right not to delay any longer the publication of facts, which appear to me to be both new and interesting, as their publication may perhaps excite others to engage in their further investigation.

The principal objects I had in view in the following set of experiments were, first, to determine the expansive force of the elastic vapour generated in the combustion of gunpowder in its various states of condensation, and to ascertain the ratio of its elasticity to its density; and 2dly, to measure, by one decisive experiment, the utmost force of this fluid in its most dense state; that is to say, when the powder completely fills the space in which it is fired, and in which the generated fluid is confined. As these experiments were very numerous, and as it will be more satisfactory to be able to see all their results at one cursory view, I have brought them into the form of a general table. In this table, which does not stand in need of any particular explanation, may be seen the results of all these investigations. The dimensions of the barrel used in these experiments were as follow: Diameter of the bore at its muzzle = 0.25 of an inch. Joint capacities of the bore, and of its vent tube, exclusive of the space occupied by the leathern stopper, = 0.08974 of a cubic inch. Quantity of powder contained by the barrel and its vent tube when both were quite full, exclusive of the space occupied by the leathern stopper, 25.641 German apothecary's grains, = 24½ grains Troy.

The numbers expressing the charges of powder in thousandth parts of the joint capacities of the barrel and of its vent tube, were determined from the known quantities of powder used in the different experiments, expressed in German apothecary's grains, and the relation of these quantities to the quantity required to fill the barrel and its vent tube completely. Thus, as the barrel and its vent tube were capable of containing 25.641 apothecary's grains of powder, if we suppose this quantity to be divided into 1000 equal parts, this will give 39 of those parts for 1 grain; 78 parts for 2 grains; 390 for 10 grains, &c. For it is 25.641 to 1000, as 1 to 39 very nearly. As this method of expressing the quantities of powder shows at the same time the relative density of the generated elastic fluid, it is the more satisfactory on that account: it will also considerably facilitate the computations necessary in order to ascertain the ratio of the elasticity of this fluid to its density.

The elastic force of the fluid generated in the combustion of the charge of powder, is measured by the weight by which it was confined, or rather by that which it was just able to move, but which it could not raise sufficiently to blow the leathern stopper quite out of the mouth of the bore of the barrel. This weight in all the experiments, except those which were made with very small charges of powder, was a piece of ordnance, of greater or less dimensions, or greater or less weight, according to the force of the charge; placed vertically on its cascabel, on the steel hemisphere which closed the end of the barrel; and the same piece of ordnance, by having its bore filled with a greater or smaller number of bullets, as the occasion required, was made to serve for several experiments.

The weight employed for confining the generated elastic fluid, is expressed in the following table in pounds avoirdupois; but in order that a clearer and more perfect idea may be formed of the real force of its elastic fluid, I have added a column in which its force, answering to each charge of powder, is expressed in atmospheres. The numbers in this column were computed in the following manner. The diameter of the bore of the barrel at its muzzle being just ¼ of an inch, the area of its transverse section is 0.049088 of a superficial inch; and assuming the mean pressure of the atmosphere on 1 superficial inch equal to 15lbs. avoirdupois, this will give 0.73631 of a pound avoirdupois for that pressure on 0.049088 of a superficial inch, or on a surface equal to the area by which the generated elastic fluid acted on the weight employed to confine it; consequently the weight expressed in pounds avoirdupois, which measured the force of the generated elastic fluid in any given experiment, being divided by 0.73631, will show how many times the pressure exerted by the fluid was greater than the mean pressure of the atmosphere. Thus in the experiment, No. 6. where the weight which measured the elastic force of the generated fluid was = 504.8 lbs. avoirdupois, it is 504.8 ÷ .073631 = 685.6 atmospheres. And so of the rest.

I have said that the diameter of the bore of the barrel, used in the following experiments, was just ¼ of an inch at its muzzle, and this is strictly true, as I found on measuring it with the greatest care; but its diameter is not perfectly the same throughout its whole length, being rather narrower towards its lower end: yet the capacity of the barrel being known, and also the diameter of the bore of its muzzle, any small inequalities of the bore in any other part can in no wise affect the result of the experiments, as will be evident to those who will take the trouble to consider the matter for a moment with attention. I should not indeed have thought it necessary to mention this circumstance, had I not been afraid that some one who should calculate the joint capacities of the bore and of the vent tube from their lengths and diameters, finding their calculation not to agree with my determination of those capacities, as ascertained by filling them with mercury, might suspect me of having committed an error. The mean diameter of the bore of the barrel, as determined from its length and its capacity, turns out to be just 0.2281 of an inch; the diameter of the vent tube being taken equal to 0.07 of an inch, and its length 1.715 inch.

Table I. Experiments on the Force of fired Gunpowder.*

No. of the Experiment.	Time when the Experiment was made, 1793.			State of the Atmosphere.		The Charge of Powder.		Weight employed to confine the elastic Fluid.		General Remarks.
				Thermom.	Barometer.	In Apoth. gr.	In 1000 Parts of the Capacity of the Bore.	In lbs, avoirdupois.	In Atmospheres.	
No.		h.	m.	F.	Eng. In.	Grs.	Parts.	lbs.		
1	23d Feb.	9	0	31°	28.58	1	39	504.8		The generated elastic fluid was completely confined, the weight not being raised.
2		9	30	..	..	2	78	..		
3	25th	9	0	37°	28.56	3	117	..		Ditto.
4		10	15	..	..	4	156	..		Ditto, weight not raised.
5		10	30	..	..	5	195	..		Ditto, ditto.
6		11	0	..	..	6	234	..	685.6	Weight just moved.
7		3	0 PM	57°	28.37	1	39	14.16		In these 3 experiments the weight was raised with a report as loud as that of a pistol.
8		3	15	..	..	..	..	26.5		
9		3	30	..	..	..	..	38.9		
10		3	45	..	..	..	..	51.3		But just raised, report much weaker.
11		4	0	..	..	..	..	57.4	77.86	Weight hardly moved.
12	26th	9	0	34°	28.1	2	78	163.5		Not raised.
13		9	15	..	..	..	..	124		Raised with a loud report.
14		9	30	..	..	..	..	130.5		Ditto, the report weaker.
15		9	45	..	..	..	..	133		Ditto, the report still weaker.
16		10	0	..	..	..	..	134.2	182.3	Weight but just moved.
17		3	0	48°	28.31	3	117	186.3		Raised with a loud report.
18		3	15	..	..	..	..	198.7		Ditto, ditto.
19		3	30	..	..	..	..	204.8		Ditto, report weaker.
20		3	45	..	..	..	..	208.5		Raised, report weaker.
21		4	0	..	..	..	..	212.24	288.2	Weight just moved, no report.
22	27th	3	0	50°	28.36	4	156	269.2		Raised with a loud report.
23		3	15	..	..	..	..	274.13		Ditto, ditto.
24		3	30	..	..	..	..	277.9		Ditto, report less loud.
25		3	45	..	..	..	..	281.57	382.4	Weight just moved, no report.
26	28th	9	0	34°	28.32	5	195	319.68		Raised, loud report.
27		9	15	..	..	..	..	351.37		Ditto, ditto.
28		9	30	..	..	..	..	400.9		Ditto, ditto.
29		10	0	..	..	..	..	475.2		Not raised.
30		3	0	48°	28.35	..	..	443.5		Not raised.
31		3	15	..	..	..	..	425.65		Not raised.
32		3	30	..	..	..	..	419.46		Not raised.
33		3	45	..	..	..	..	413.27	561.2	Weight but just moved.
34	1st Mar.	9	0	34°	..	7	273	535.79		Raised with a loud report.
35		9	15	..	..	..	..	548.14		Ditto, ditto.
36		9	30	..	..	..	..	560.52		Ditto, ditto.
37		3	0	59°	28.34	..	..	572.9		Ditto, ditto.
38		3	15	..	..	..	..	585.28		Ditto, report weaker.
39		3	30	..	..	..	..	597.66	811.7	Weight just moved, no report.
40		3	45	..	..	8	312	690.52		Raised, report very loud.
41		4	0	..	..	..	..	752.42		Ditto, ditto.
42		4	15	..	..	..	..	783.37		Ditto, ditto.
43	2d	9	0	50°	28.32	..	..	876.22		Not raised.
44		9	15	..	..	..	..	845.19		But just raised, report weak.
45		9	30	..	..	..	..	857.64	1164.8	Weight just moved, no report.
46		9	45	..	..	9	351	961.65		Raised with a loud report.

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* The last preceding note is equally applicable to all the experiments in this table, as the percussive or initial force of the elastic fluid should be compared to, or measured by, the opposed weight or pressure.

Table I. Experiments on the Force of fired Gunpowder.

No. of the Experiment.	Time when the Experiment was made, 1793.			State of the Atmosphere.		The Charge of Powder.		Weight employed to confine the elastic Fluid.		General Remarks.
				Thermom.	Barometer.	In Apoth. gr.	In 1000 Parts of the Capacity of the Bore.	In lbs. avoirdupois.	In Atmospheres.	
No.		h.	m.	F.	Eng. In.	Grs.	Parts.	lbs.		
47	2d Mar.	10	0	50°	28.32	9	351	1209.4		Not raised.
48		10	30	..	..	..	..	1142.3	1551.3	Weight just moved, no report.
49		3	0	52°	28.33	10	390	1456.8		Not raised.
50		3	30	..	..	..	..	1329.9		Raised, loud report.
51	5th	9	0	32°	28.2	..	..	1387.5	1884.3	Weight just moved, no report.
52		9	15	..	..	11	429	1708.2		Not raised.
53		9	45	..	..	..	..	1646.2		Not raised.
54		10	15	..	..	..	..	1615.2		Raised, with a weak report.
55		10	45	..	..	..	..	1634	2219	Weight just moved, no report.
56	6th	9	0	36°	28.34	12	468	1943.3		Not raised.
57		9	30	..	..	..	..	1932.2		Not raised.
58		10	30	..	..	..	..	1907.4		Weight not raised.
59		11	0	..	..	..	..	1878.4		Raised with a loud report.
60		11	30	..	..	..	..	1895.1	2573.7	Weight just moved, no report.
61		3	0	42°	28.3	13	507	2142.7		Raised with a loud report.
62		3	15	..	..	..	..	2204.6		Ditto, ditto.
63		3	30	..	..	..	..	2266.5		Ditto, ditto.
64		3	45	..	..	..	..	2390.3		Raised, report weaker.
65		4	0	..	..	..	..	2422	3288.3	Weight just moved, no report.
66	9th	9	0	43°	28.31	14	546	3213		Not raised.
67		9	30	..	..	..	..	3093		Not raised.
68		10	0	..	..	..	..	2968		Not raised.
69		10	30	..	..	..	..	2846		Raised, with a loud report.
70		10	45	..	..	..	..	2908		Raised, report weaker.
71		11	0	..	..	..	..	2939		Ditto, report still weaker.
72		11	15	..	..	..	..	2951	4008	Weight just moved, no report.
73		11	30	..	..	15	585	3750		Not raised.
74		11	45	..	..	..	..	3508		Not raised.
75		12	15	..	..	..	..	3477	4722.5	Weight just moved, no report.
76	11th	9	0	43°	28.3	16	624	4037		The weight was raised with a loud report.
77		9	15	..	..	..	..	4284		Raised, loud report.
78		9	30	..	..	..	..	4532		Ditto, ditto.
79	4th Apr.	3	0	70°	28.2	..	..	5027		Ditto, ditto.
80		3	15	..	..	..	..	5138		Raised, report weaker.
81		3	30	..	..	..	..	5262		Not raised.
82		3	45	..	..	..	..	5220	7090	Weight just moved, no report.
83	5th	3	0	68°	28.3	17	663	8081		Not raised.
84		3	30	..	..	18	702	8081	10977	The weight was raised with a very sharp report, louder than that of a well loaded musket.
85		4	0	..	..	..	..	8700		The vent tube of the barrel was burst, the explosion being attended with a very loud report.

That a clear and satisfactory idea may be formed of the results of these experiments a figure is drawn, in which the given densities of the generated elastic fluid, or, which amounts to the same thing, the quantities of powder used for the charge, being taken on a line AB, from A towards B, the corresponding elasticities, as found by the experiments, are represented by ordinates or lines perpendicular to the line AB, at the points where the measures of the densities end. The curve line AD, drawn through the extremities of these ordinates, which must necessarily be regular, will, by bare inspection, give a considerable degree of insight into the nature of the equation which must be formed to express the relation of the densities to the elasticities; one principal object of these experimental inquiries. Now, putting the density $= x$, and the elasticity $= y$, the line AD will be the locus of the equation expressing the relation of x to y; and had Mr. Robins's supposition, that the elasticity is as the density, been true, x would have been found to be to y in a constant (simple) ratio, and AD would have been a straight line. But AD is a curve, which shows that the ratio of x to y is variable; and it is a curve convex towards the line AB, on which x is taken; and this circumstance proves that the ratio of y to x is continually increasing.

Though these experiments all tend to show that the ratio of y to x increases as x is increased, yet when we consider the subject with attention, we shall I think find reason to conclude that the exponent of that ratio can never be less than unity; and further, that it must of necessity have that value precisely, when, the density being taken infinitely small, or $= 0$, x and y vanish together. Supposing this to be the case, namely, that the exponent of the ultimate ratio of y to x is $= 1$, let the densities or successive values of x be expressed by the series of natural numbers, 0, 1, 2, 3, 4, &c. to 1000, the last term $= 1000$ answering to the greatest density; or when the powder completely fills the space in which it is confined; then, by putting $z =$ the variable part of the exponent of the ratio of y to x,

To each of the successive values of $x = 0, 1, 2, 3, 4$, &c.

The corresponding value of y will be accurately expressed by the equations.......... } $0^{1+z}, 1^{1+z}, 2^{1+z}, 3^{1+z}, 4^{1+z}$, &c.

For, as the variable part z, of this exponent, may be taken of any dimensions, it may be so taken at each given term of the series, or for each particular value of x, that the equation $x^{1+z} = y$, may always correspond with the result of the experiments; and when this is done,* the value of z, and the law of its increase as x increases, will be known; and this will show the relation of x to y, or of the elasticities of the generated fluid to their corresponding densities, in a clear and satisfactory manner. Without increasing the length of this paper still more, by giving an account in detail of all the various computations I made, in order, from the results of the experiments in the foregoing table, to ascertain the real value of z, and the rate at which it increases as x is increased, I shall content myself with merely giving the general results of these investigations, and referring for further information to the following table 2, where the agreement of the law founded on them, with the results of the foregoing experiments, may be seen.

Having, from the results of the experiments in table 1, computed the different values of z, corresponding to all the different densities, or different charges of

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* True, when this is done for every term or value of x: but the law of progression will not be known farther than the terms which are actually compared.

powder, from 1 grain, or 39 thousandth parts, to 18 grains, or 702 thousandth parts of the capacity of the barrel, I found that while the density of the elastic fluid $= x$, expressed in thousandth parts, is increased from 0 to 1000, or till the powder completely fills the space in which it is confined, the variable part z of the exponent of x, $(1 + z)$, is increased from 0 to $\frac{4}{10}$. And though some of the experiments, and particularly those which were made with large charges of powder, seemed to indicate that while x is increased with an equable or uniform motion, z increases with a motion continually accelerated; yet, as the results of by far the greatest number of the other experiments showed the velocity of the increase of z to be equable, this circumstance, added to some other reasons drawn from the nature of the subject, have induced me to assume the ratio of the increase of z to the increase of x as constant.

But if, while x increases with an equable velocity from 0 to 1000, z is increased with an equable velocity from 0 to $\frac{4}{10}$, then it is every where z to x as $\frac{4}{10}$ to 1000; or $1000\,z = \frac{4}{10}x$, and consequently $z = \frac{4x}{10000}$; and when x is $= 1$, it is $z = \frac{4}{10000} = 0.0004$; and when x is greater or less than 1, it is $z = 0.0004x$; and z being expunged, the general equation expressing the relation of x to y becomes $x^{1 + 0.0004x} = y$; and this is the equation which was used in computing the values of y, as expressed in the following table. In order that the elasticities might be expressed in atmospheres, the values of y, as determined by this equation, were multiplied by 1.841. If it be required to express the elasticity in pounds avoirdupois, then the value of y, as determined by the foregoing equation, being multiplied by 27.615, will show how many pounds avoirdupois, pressing on a superficial inch, will be equal to the pressure exerted by the elastic fluid in the case in question.

Table II. General Results of the Experiments in Table I. on the Force of Fired Gunpowder.

The charge of powder.		Value of the exponent $1+0.0004x$.	Computed elasticity of the generated fluid, or value of y, according to the theorem $x^{1+0.0004x} = y$.		Actual elasticity, as shown by the experiments.	Difference of the computed and the actual elasticities.
In grains.	In equal parts.		In equal parts.	In Atmospheres.	In Atmospheres.	In Atmospheres.
1	39	1.0156	41.294	76.822	77.86	+ 1.838
2	78	1.0312	89.357	164.506	182.30	+ 17.794
3	117	1.0468	146.210	269.173	228.2	− 40.973
4	156	1.0624	213.784	393.577	382.4	− 11.177
5	195	1.0780	294.209	541.640	561.2	+ 19.560
6	234	1.0936	389.919	717.841	685.6	− 32.241
7	273	1.1092	503.723	927.353	811.7	− 115.653
8	312	1.1248	638.889	1176.19	1164.8	− 12.390
9	351	1.1404	799.223	1471.37	1551.3	+ 79.930
10	390	1.1560	989.169	1821.06	1884.3	+ 63.240
11	429	1.1716	1213.91	2234.81	2219.	− 15.810
12	468	1.1872	1479.50	2723.77	2573.7	− 150.07
13	507	1.2028	1793.	3300.91	3283.3	− 17.61
14	546	1.2184	2162.69	3980.52	4008.	+ 27.48
15	585	1.2340	2598.18	4783.26	4722.5	− 60.76
16	624	1.2496	3110.73	5726.83	7090.	+ 1363.17
17	663	1.2652	3713.46	6836.46		
18	702	1.2808	4421.69	8140.34	10977.	+ 2836.66
19	741	1.2964	5253.3	9671.33		
20	780	1.3120	6229.14	11467.8		
25.641	1000	1.4000	15848.9	29177.9		

The agreement of the elasticities, computed from the theorem $x^{1 + 0.0004x} = y$, with the actual elasticities as they were measured in the experiments, may be seen in the foregoing table; but this agreement may be seen in a much more striking manner by a bare inspection of the figure before-mentioned; for the line AD in this figure having been drawn from the computed elasticities, its general coincidence with the line AC of the experiments, shows how nearly the computed and the actual elasticities approach each other. And when the irregularities of the line AC, which must be attributed to the unavoidable errors of the experiments, are corrected, these 2 curves will be found to coincide with much precision throughout a considerable part of the range of the experiments; but towards the end of the set of experiments, when the charges of powder were considerably increased, the elasticities seem to have increased faster than, according to the assumed law, they ought to have done. From this circumstance, and from the immense force the charge must have exerted in the experiment, when the barrel was burst, I was led to suspect that the elastic force of the fluid generated in the combustion of gunpowder, when its density is great, is still much greater than these experiments seem to indicate; and a further investigation of the subject served to confirm me in this opinion.

It has been shown that the force exerted by the charge in the experiment in which the barrel was burst could not have been less than the pressure of 54752 atmospheres; but the greatest force of the generated elastic fluid, when, the powder filling the space in which it is confined, its density is $= 1000$, on computing its elasticity by the theorem $x^{1 + 0.0004x} = y$, turns out to be only equal to 29178 atmospheres. In this computation the mean of the results of all the experiments in the foregoing set is taken as a standard to ascertain the value, expressed in atmospheres of y, and it is $y \times 1.841 = 29178$.

But if, instead of taking the mean of the whole set of experiments as a standard, we select that experiment in which the force exerted by the powder appears to have been the greatest, yet in this case even the initial force of fired gunpowder, computed by the above rule, would be much too small. In the experiment N° 84, when the charge consisted of 18 grains of powder, and the density or value of x was 702, a weight equal to the pressure of 10977 atmospheres was raised. Here the value of y ($= x^{1 + 0.0004x}$) is found to be ($702^{1.2808}$), $= 4421.7$; and to express this value of y in atmospheres, and at the same time to accommodate it to the actual result of the experiment, it must be multiplied by 2.4826; for it is 4421.7 (the value of y expressed in equal parts) to 10.977 (its value in atmospheres, as shown by the experiment), as 1 to 2.4826, and consequently $4421.7 \times 2.4826 = 10977$.

If now the value of y be computed on the same principles, when x is put $=$ 1000, it will turn out to be $y = 1000^{1 + 0.4} = 15849$; and this number expressed in atmospheres, by multiplying it by 2.4826, gives the value of $y = 39346$ atmospheres. This however falls still far short of 54752 atmospheres, the force the powder was actually found to exert when the charge filled the space in which it was confined. But in the 84th experiment, when 18 grains of powder were used, as the weight (8081 lbs. avoirdupois) was raised with a very loud report, it is more than probable that the force of the generated elastic fluid was in fact considerably greater than that at which it was estimated, namely, greater than the pressure of 10977 atmospheres.

But, without wasting time in fruitless endeavours to reconcile anomalous experiments, which probably never can be made to agree, I shall hasten to give an account of another set of experiments; the results of which, it must be confessed, were still more various, extraordinary, and inexplicable. The machinery having been repaired and put in order, the experiments were recommenced in July, 1793, the weather at that time being very hot. The principal part of the apparatus, the barrel, had undergone a trifling alteration: on refitting and cleaning it, the diameter of its bore at the muzzle was found to be a little increased, so that a weight equal to 8081 lbs. avoirdupois, instead of being equal to 10977 atmospheres, as was the case in the former experiments, was now just equal to the pressure of 9431 atmospheres. Though I was not at Munich when this last set of experiments was made, they however were undertaken at my request, and under my direction, and I have no reason to doubt of their having been executed with all possible care. They were all made by the same persons who were employed in making the first set; and as these experimenters may be supposed to have become expert in practice, and as they could not possibly have had any interest in deceiving me, I cannot suspect the accuracy of their reports.

Table III. Experiments on the Force of fired Gunpowder.

No. of the Experiment.	Time when the Experiment was made, 1793.			State of the Atmosphere.		The Charge of Powder.		Weight employed to confine the elastic Fluid.		General Remarks.
				Thermom.	Barometer.	In Apoth. grs.	In 1000 parts of the capacity of the bore.	In lbs. avoirdupois.	In atmospheres.	
No.		h.	m.	F.	Eng. In.	Grs.	Parts.	lbs.		
86	1st July	4	0	88°	28.37	17	663	8081	9431	The weight was raised with an astonishing loud report.
87		4	30	..	..	..	..	..		In these three experiments the weight was raised with a very loud report.
88		4	45	..	..	16	624	..		
89		5	0	..	..	15	585	..		
90		5	30	..	..	12	468	..		Weight not raised.
91		6	0	..	..	13	507	..	9431	Weight but just raised, report very weak.
92	2d	9	0	71°	28.38	..	..	..		Raised, loud report.
93		9	30	..	..	12	468	..		Raised, feeble report.
94		10	0	..	..	..	..	..	9431	Raised, report very feeble.
95		10	30	80°	..	$11\frac{7}{8}$	..	..		Just moved, no report.
96	3d	10	0	70°	28.55	12	468	..		Not raised.
97		10	30	..	..	13	507	..		Not raised.
98		11	0	75°	..	14	546	..	9431	Just raised, feeble report.
99	4th	9	0	70°	28.56	14	546	..		Not raised.
100		9	30	..	..	..	..	..		Not raised.
101		10	0	72°	..	15	585	..		The weight was raised, the report not very loud.

No. of the Experiment.	Time when the Experiment was made, 1793.	State of the Atmosphere.		The Charge of Powder.		Weight employed to confine the elastic Fluid.		General Remarks.
		Thermom.	Barometer.	In Apoth. grs.	In 1000 parts of the capacity of the bore.	In lbs. avoirdupois.	In atmospheres.	
No.	h. m.	F.	Eng. In.	Grs.	Parts.	lbs.		
102	4th July 10 30	72°	28.56	15½	585	..		Nearly as above.
103	8th 9 0	74°	28.42	..	..	..		Raised, and with an uncommonly loud report.
104	9 30	..	..	13	507	..		Raised, report very loud.
105	10 45	85°	..	12	468	..	9431	But just raised, the report very feeble.
106	17th 9 0	75°	28.4	..	..	..		Nearly as above.
107	9 45	..	..	..	..	..		Not raised.
108	10 30	..	..	11¼	..	..		Just moved, no report.
109	11 0	..	..	..	..	..		The same as above.

It appears from the foregoing table, that in the afternoon of the 1st of July, the weight (which was a heavy brass cannon, a 24 pounder, weighing 8081 lbs. avoirdupois), was not raised by 12 grs. of powder, but that 13 grs. raised it with an audible though weak report. That the next morning, July 2d, at 10 o'clock, it was raised twice by charges of 12 grs. That in the morning of the 3d of July, it was not raised by 12 grs., nor by 13 grs.; but that 14 grs. just raised it. That in the afternoon of the same day, 2 experiments were made with 14 grs. of powder, in neither of which the weight was raised; but that in another experiment, in which 15 grs. of powder were used, it was raised with a moderate report. That in the morning of the 8th July, in 2 experiments, one with 15 grs., and the other with 13 grs. of powder, the weight was raised with a loud report; and in an experiment with 12 grs., it was raised with a feeble report. And lastly, that in 3 successive experiments, made in the morning of the 17th of July, the weight was raised by charges of 12 grs. Hence it appears, that under circumstances the most favourable to the developement of the force of gunpowder, a charge, = 12 grs., filling $\frac{468}{1000}$ of the cavity in which it is confined, on being fired, exerts a force against the sides of the containing vessel equal to the pressure of 9431 atmospheres; which pressure amounts to 141465 lbs. avoirdupois on each superficial inch.

Mr. Robins makes the initial, or greatest force of the fluid generated in the combustion of gunpowder, namely when the charge completely fills the space in which it is confined, to be only equal to the pressure of 1000 atmospheres.* It appears however, from the result of these experiments, that even admitting the elasticities

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* The fact however is, that the initial force is really various, gradually increasing as the charge of powder is increased, on account of the superior heat of the explosion. Mr. Robins's experiments were made with only small quantities of powder; and hence he obtained for his initial force the pressure of 1000 atmospheres; but by increasing the charges considerably, it is found that the said force is gradually increased to near 2000 atmospheres; as appears by Dr. Hutton's Course of Mathematics, vol. 2, p. 352, 353, the 5th edition, anno 1807.

to be as the densities, as Mr. Robins supposes them to be, the initial force of this generated elastic fluid must be at least 20 times greater than Mr. Robins determined it; for $\frac{468}{1000}$, the density of the elastic fluid in the experiments in question, is to 1, its density when the powder quite fills the space in which it is confined, as 9431 atmospheres, the measure of its elastic force in the experiments in question, to 20108 atmospheres; which, according to Mr. Robins's theory respecting the ratio of the elasticities to the densities, would be the measure of its initial force. But all my experiments tend uniformly to prove, that the elasticities increase faster than in the simple ratio of the corresponding densities; consequently the initial force of the generated elastic fluid must necessarily be greater than the pressure of 20108 atmospheres. In one of my experiments which I have often had occasion to mention, the force actually exerted by the fluid must have been at least equal to the pressure of 54752 atmospheres. The other experiments ought no doubt to show, at least, that it is possible that such an enormous force may have been exerted by the charge made use of; and this I think they actually indicate.

In the first set of experiments, which were made when the weather was cold, though the results of them uniformly showed the force of the powder to be much less than it appeared to be in all the subsequent experiments, made with greater charges, and in warm weather, yet they all show that the ratio of the elasticity of the generated fluid to its density is very different from that which Mr. Robins's theory supposes;* and that this ratio increases as the density of the fluid is increased. Supposing, what on many accounts appears to be extremely probable, that this ratio increases uniformly, or with an equable celerity, while the density is uniformly augmented; and supposing further, that the velocity and limit of its increase have been rightly determined from the result of the set of experiments, table 1, which were made with that view; then, from the result of the experiments of which we have just been giving an account, in which 12 grs. of powder exerted a force equal to 9431 atmospheres, taking these experiments as a standard, we can, with the help of the theorem $x^{1+0.0004x} = y$, deduced from the former set of experiments, compute the initial force of fired gunpowder, thus:

The density of the elastic fluid, when 12 grs. of powder are used for the charge, being $= 468$, it is $468^{1.1872} = y = 1479.5$; and in order that this value of y may correspond with the result of the experiment, and be expressed in atmospheres, it must be multiplied by a certain co-efficient, which will be found by dividing the value of y expressed in atmospheres, as shown by the experiment, by the number

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* Though this circumstance, of the ratio of the elasticity to the density of the fluid, be here mentioned as if it related to the same thing in the 2 cases, they are yet however quite different: in these experiments it means the ratio of the charge of powder, or quantity of generated fluid in the *same* space, to its strength or elasticity; but in Mr. Robins it means also that the elasticity of the same quantity of fluid, when it occupies *different* spaces, as when following and impelling a ball along the barrel of a gun, is in the ratio of the decreasing densities, or inversely as those spaces. Besides, this property, in both these respects, is not merely a matter of supposition in Mr. Robins's theory, but the consequence of experimental proof, both by himself and other eminent members of the R. S.

here found indicating its value, as determined by computation. It is therefore $\frac{9431}{1479.5} = 6.3744$ for the value of this co-efficient, and this multiplied into the number 1479.5, gives 9431 for the value of y in atmospheres.

Again, the density being supposed = 1000 (or, that the charge of powder completely fills the cavity in which it is confined), in that case it will be $1000^{1+0.4} = y = 15849$, and this number being turned into atmospheres by being multiplied by the co-efficient above found (= 6.3744), gives 101021 atmospheres for the measure of the initial force of the elastic fluid generated in the combustion of gunpowder. Enormous as this force appears, I do not think it over-rated; for nothing much short of such an inconceivable force can, in my opinion, ever explain in a satisfactory manner the bursting of the barrel so often mentioned; and to this we may add, that, as in 7 different experiments, all made with charges of 12 grs. of powder, there were no less than 5 in which the weight was raised with a report, and as the same weight was moved in 3 different experiments in which the charge consisted of less than 12 grs., there does not appear to be any reason whatever for doubt with regard to the principal fact on which the above computation is founded.*

There is an objection however, that may be made to these decisions respecting the force of gunpowder, which, on the first view, appears of considerable importance; but on a more careful examination it will be found to have no weight. If the force of fired gunpowder is so very great, how does it happen that fire-arms and artillery of all kinds, which certainly are not calculated to withstand so enormous a force, are not always burst when they are used? I might answer this question by another, by asking how it happened that the barrel used in my experiments, and which was more than 10 times stronger in proportion to the size of its bore than ever a piece of ordnance was formed, could be burst by the force of gunpowder, if its force is not in fact much greater than it has ever been supposed to be? But it is not necessary to have recourse to such a shift to get out of this difficulty: there is nothing more to do than to show, which may easily be done, that the combustion of gunpowder is less rapid than it has hitherto been supposed to be, and the objection in question falls to the ground. Mr. Robins's theory supposes that all the powder of which a charge consists is not only set on fire, but that it is actually consumed and "converted into an elastic fluid before the bullet is sensibly moved from its place." I have already, in the former part of this paper, offered several reasons which appeared to me to prove that, though the inflammation of gunpowder is very rapid, yet the progress of the combustion is by no means so instantaneous as has been imagined. I shall now give an account of some experiments which put that matter out of all doubt.

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* Reasons, however, have been given in the foregoing notes, not only for doubt of such computations, but for proof of the fallacy of the principles on which they are made. One might have expected that the enormous number of 100,000 atmospheres might have staggered any philosopher!

It is a fact well known, that on the discharge of fire-arms of all kinds, cannon and mortars as well as muskets, there is always a considerable quantity of unconsumed grains of gunpowder blown out of them; and, what is very remarkable, and, as it leads directly to a discovery of the cause of this effect, is highly deserving of consideration, these unconsumed grains are not merely blown out of the muzzles of fire-arms; they come out also by their vents or touch-holes, where the fire enters to inflame the charge; as many persons who have had the misfortune to stand with their faces near the touch-hole of a musket, when it has been discharged, have found to their cost. Now it appears to me to be extremely improbable, if not absolutely impossible, that a grain of gunpowder actually in the chamber of the piece, and completely surrounded by flame, should, by the action of that very flame, be blown out of it, without being at the same time set on fire. But if these grains of powder are actually on fire when they come out of the piece, and are afterwards found at a distance from it unconsumed, this is, in my opinion, a most decisive proof, not only that the combustion of gunpowder is by no means so rapid as has generally been thought to be, but also, what will doubtless appear quite incredible, that if a grain of gunpowder, actually on fire, and burning with the utmost violence over the whole extent of its surface, be projected with a very great velocity into a cold atmosphere, the fire will be extinguished, and the remains of the grain will fall to the ground unchanged, and as inflammable as before.

This extraordinary fact was ascertained beyond all possibility of doubt by the following experiments. Having procured from a powder-mill in the neighbourhood of the city of Munich a quantity of gunpowder, all of the same mass, but formed into grains of very different sizes, some as small as the grains of the finest Battel powder, and the largest of them nearly the size of large pease, I placed a number of vertical screens of very thin paper, one behind another, at the distance of 12 inches from each other; and loading a common musket repeatedly with this powder, sometimes without, and sometimes with a wad, I fired it against the foremost screen, and observed the quantity and effects of the unconsumed grains of powder which impinged against it. The screens were so contrived, by means of double frames united by hinges, that the paper could be changed with very little trouble, and it was actually changed after every experiment. The distance from the muzzle of the gun to the first screen was not always the same; in some of the experiments it was only 8 feet, in others it was 10, and in some 12 feet. The charge of powder was varied in a great number of different ways, but the most interesting experiments were made with one single large grain of powder, propelled by smaller and larger charges of very fine-grained powder.

These large grains never failed to reach the screen; and though they sometimes appeared to have been broken into several pieces, by the force of the explosion, yet they frequently reached the first screen entire; and sometimes passed through all the screens, 5 in number, without being broken. When they were propelled by large charges, and consequently with great velocity, they were seldom on fire when they arrived at the first screen, which was evident not only from their not setting fire to the paper, which they sometimes did, but also from their being found sticking in a soft board, against which they struck, after having passed through all the 5 screens; or leaving visible marks of their having impinged against it, and being broken to pieces and dispersed by the blow. These pieces were often found lying on the ground; and from their forms and dimensions, as well as from other appearances, it was often quite evident that the little globe of powder had been on fire, and that its diameter had been diminished by the combustion, before the fire was put out on the globe being projected into the cold atmosphere. The holes made in the screen by the little globe in its passage through them, seemed also to indicate that its diameter had been diminished.

That these globes or large grains of powder were always set on fire by the combustion of the charge can hardly be doubted. This certainly happened in many of the experiments, for they arrived at the screens on fire, and set fire to the paper; and in the experiments in which they were projected with small velocities, they were often seen to pass through the air on fire; and when this was the case no vestige was to be found. They sometimes passed, on fire, through several of the foremost screens without setting them on fire, and set fire to one or more of the hindmost, and then went on and impinged against the board, which was placed at the distance of 12 inches behind the last screen. It is hardly necessary for me to observe, that all these experiments prove that the combustion of gunpowder is very far from being so instantaneous as has generally been imagined*. I will just mention one experiment more, in which this was shown in a manner still more striking, and not less conclusive. A small piece of red-hot iron being dropped down into the chamber of a common horse pistol, and the pistol being elevated to an angle of about 45 degrees, on dropping down into its barrel one of the small globes of powder, of the size of a pea, it took fire, and was projected into the atmosphere by the elastic fluid generated in its own combustion, leaving a very beautiful train of light behind it, and disappearing all at once, like a falling star. This amusing experiment was repeated very often, and with globes of different sizes. When very small ones were used singly, they were commonly consumed entirely before they came out of the barrel of the pistol; but when several of them were used together, some, if not all of them were commonly projected into the atmosphere on fire.

I shall conclude this paper by some observations on the practical uses and improvements that may probably be derived from these discoveries, respecting the great expansive force of the fluid generated in the combustion of gunpowder. As the slowness of the combustion of gunpowder is undoubtedly the cause which has pre-

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* It is not surprizing that the burning of these solid globes or lumps of gunpowder should be different from the same substance in the state of powder or small grains, its proper and useful state, in which the fire can pass freely between the grains. The common case, of comparatively slow burning, in a sky-rocket, when the powder is hard beaten together, nearly like the solid mass, is well known.

vented its enormous and almost incredible force from being discovered, so it is evident, that the readiest way to increase its effects is to contrive matters so as to accelerate its inflammation and combustion. This may be done in various ways, but the most simple and most effectual manner of doing it would, in my opinion, be to set fire to the charge of powder by shooting, through a small opening, the flame of a smaller charge into the midst of it. I contrived an instrument on this principle for firing cannon 3 or 4 years ago, and it was found on repeated trials to be useful, convenient in practice, and not liable to accidents. It also supersedes the necessity of using priming, of vent tubes, port-fires, and matches; and on that account I imagined it might be of use in the British navy. Whether it has been found to be so or not I have not yet heard *.

Another infallible method of increasing very considerably the effect of gunpowder in fire-arms of all sorts and dimensions, would be to cause the bullet to fit the bore exactly, or without windage, in that part of the bore at least where the bullet rests on the charge †: for when the bullet does not completely close the opening of the chamber, not only much of the elastic fluid generated in the first moment of the combustion of the charge escapes by the sides of the bullet, but, what is of still greater importance, a considerable part of the unconsumed powder is blown out of the chamber along with it, in a state of actual combustion, and getting before the bullet continues to burn on as it passes through the whole length of the bore, by which the motion of the bullet is much impeded. The loss of force arising from this cause is, in some cases almost incredible; and it is by no means difficult to contrive matters so as to render it very apparent, and also to prevent it. If a common horse pistol be fired with a loose ball, and so small a charge of powder that the ball shall not be able to penetrate a deal board so deep as to stick in it when fired against it from the distance of 6 feet; the same ball, discharged from the same pistol, with the same charge of powder, may be made to pass quite through one deal board, and bury itself in a 2d placed behind it, merely by preventing the loss of force which arises from what is called windage; as I have found more than once by actual experiment.

I have in my possession a musket, from which, with a common musket charge of powder, I fire 2 bullets at once with the same velocity that a single bullet is discharged from a musket on the common construction, with the same quantity of powder. And, what renders the experiment still more striking, the diameter of the bore of my musket is exactly the same as that of a common musket, except only in that part of it where it joins the chamber, in which part it is just so much

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* By many accurate experiments made by the Artillery at Woolwich, it has been found that firing the charge of powder in different parts of it, makes no sort of difference in the effects, whether it is fired in the middle, or at either end, or at the upper side or under side, &c.

† This is no new discovery or observation. It was particularly insisted on in the paper on the Force of Fired Gunpowder, in the Philos. Trans. anno 1778, p. 50, &c. And indeed, it is by taking advantage of this circumstance solely, that the ordnance called carronades have acquired such boasted effects.

contracted that the bullet which is next to the powder may stick fast in it. I ought to add, that though the bullets are of the common size, and are always considerably less in diameter than the bore, means are used which effectually prevent the loss of force by windage; and to this last circumstance it is doubtless owing, in a great measure, that the charge appears to exert so great a force in propelling the bullets. That the conical form of the lower part of the bore, where it unites with the chamber, has a considerable share in producing this extraordinary effect, is however very certain, as I have found by experiments made with a view merely to ascertain that fact. . . . .

I finish this paper by a computation, showing that the force of the elastic fluid generated in the combustion of gunpowder, enormous as it is, may be satisfactorily accounted for on the supposition that its force depends solely on the elasticity of watery vapour, or steam*. It has been shown by a variety of experiments made in England and in other countries, and lately by a well-conducted set of experiments made in France by M. de Betancour, and published in Paris under the auspices of the Royal Academy of Sciences, in the year 1790, that the elasticity of steam is doubled by every addition of temperature equal to 30 degrees of Fahrenheit's thermometer. Supposing now a cavity of any dimensions, equal in capacity to 1 cubic inch, for instance, to be filled with gunpowder, and that on the combustion of the powder, and in consequence of it, this space is filled with steam (and I shall presently show that the water, existing in the powder as water, is abundantly sufficient for generating this steam); if we know the heat communicated to this steam in the combustion of powder, we can compute the elasticity it acquires by being so heated.

Now it is certain that the heat generated in the combustion of gunpowder cannot possibly be less than that of red-hot iron. It is probably much greater, but we will suppose it to be only equal to 1000 degrees of Fahrenheit's scale, or something less than iron visibly red-hot in day-light. This is about as much hotter than boiling linseed oil, as boiling linseed oil is hotter than boiling water. As the elastic force of steam is just equal to the mean pressure of the atmosphere when its temperature is equal to that of boiling water, or to 212° of Fahrenheit's thermometer, and as its elasticity is doubled by every addition of temperature equal to 30 degrees of the same scale,† with the heat of $212^\circ + 30^\circ = 242^\circ$ its elasticity will be equal

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* Having, by a fallacious mode of comparison applied to his experiments, raised the force of fired gunpowder to a monstrous degree, the author now has recourse to a most preposterous invention to account for it, viz. moisture in the powder! which has always heretofore been found greatly to injure and depress the strength of that substance.

† We have not been able to discover that this rate of increase, in the strength of the steam, is according to the scale in Betancour's experiments. An account of these experiments, with the table of corresponding temperature and strength of the steam, may be seen at the end of Dr. Hutton's Philos. Dictionary, and may doubtless be found in other places in this country. Now in this table it does not appear that the law of the increase of strength is at all conformable to the law above-mentioned, or that the strength is doubled by the addition of any constant number of degrees of heat whatever. These

to the pressure of 2 atmospheres; at the temperature of $242^\circ + 30^\circ = 272^\circ$ it will equal 4 atmospheres;

at $272^\circ + 30^\circ = 302^\circ$ it will equal 8 atmospheres;
at $302 + 30 = 332$........ 16
at $332 + 30 = 362$........ 32
at $362 + 30 = 392$........ 64
at $392 + 30 = 422$........ 128
at $422 + 30 = 452$........ 256
at $452 + 30 = 482$........ 512
at $482 + 30 = 512$........ 1024
at $512 + 30 = 542$........ 2048
at $542 + 30 = 572$........ 4096
at $572 + 30 = 602$, (or 2 degrees above the heat of boiling linseed oil), its elasticity will be equal to the pressure of 8192 atmospheres, or above 8 times greater than the utmost force of the fluid generated in the combustion of gunpowder, according to Mr. Robins's computation. But the heat generated in the combustion of gunpowder is much greater than that of 602° of Fahrenheit's thermometer, consequently the elasticity of the steam generated from the water contained in the powder must of necessity be much greater than the pressure of 8192 atmospheres. Following up our computations on the principles assumed, (and they are founded on the most incontrovertible experiments) we shall find that,

at the temperature the elasticity will be equal to the pressure of
of $602^\circ + 30^\circ = 632^\circ$.............. 16384 atmospheres;
at $632 + 30 = 662$.............. 32768
at $662 + 30 = 692$.............. 65536

and at $692 + 30 = 722$, the elasticity will be equal to the pressure of 131072 atmospheres, which is 130 times greater than the elastic force assigned by Mr. Robins to the fluid generated in the combustion of gunpowder; and about $\frac{1}{6}$ part greater than my experiments indicated it to be.

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degrees of heat, in this table, are according to Reaumur's thermometer. A specimen of those numbers is contained in the annexed tablet; where the 1st column shows several degrees of strength of the steam, being in fact the number of French inches of the mercurial barometer that are balanced or sustained by the steam; in the 2d column are the corresponding degrees of heat of the steam, expressed by the degrees of Reaumur's thermometer; and in the 3d column are the differences of these degrees, which are not the same number or difference, but always increasing. Hence it appears, that while the numbers in the 1st column, denoting the strength of the steam, are always doubled, or nearly so, those in the 2d column, denoting the corresponding heat, are always increased by differences that are unequal. Were the calculation carried on, always doubling the elasticity for the constant addition of 30° of Fahrenheit's thermometer, till arriving at 1000° of heat, we should obtain the monstrous quantity of about 100 millions of atmospheres for the pressure of the elastic fluid!

Strength.	Heat.	Dif.
0.0346	2°	
0.0747	4	2
0.1508	7	3
0.3039	12	5
0.6283	19	7
1.2127	27	8
2.4401	37	10
4.8386	48	11
9.6280	60	12
19.433	73	13
39.697	87	14
77.359	102	15

But even here the heat is still much below that which is most undoubtedly generated in the combustion of gunpowder. The temperature which is indicated by 722° of Fahrenheit's scale, (which is only 122 degrees higher than that of boiling quicksilver, or boiling linseed oil), falls short of the heat of iron which is visibly red-hot in day-light by 355 degrees: but the flame of gunpowder has been found to melt brass, when this metal, in very small particles, has been mixed with the powder; and it is well known that to melt brass a heat is required equal to that of 3807 degrees of Fahrenheit's scale; 2730 degrees above the heat of red-hot iron, or 3085 degrees higher than the temperature which gives to steam an elasticity equal to the pressure of 131072 atmospheres. That the elasticity of steam would actually be increased by heat in the ratio here assumed, can hardly be doubted. It has absolutely been found to increase in this ratio in all the changes of temperature between the point of boiling water, I may even say of freezing water, and that of 280° of Fahrenheit's scale; and there does not appear to be any reason why the same law should not hold in higher temperatures.

A doubt might possibly arise with respect to the existence of a sufficient quantity of water in gunpowder, to fill the space in which the powder is fired, with steam, at the moment of the explosion; but this doubt may easily be removed. The best gunpowder, such as was used in my experiments, is composed of 70 parts in weight of nitre, 18 parts of sulphur, and 16 parts of charcoal; hence 100 parts of this powder contain $67\frac{3}{10}$ parts of nitre, $17\frac{3}{10}$ parts of sulphur, and of charcoal $15\frac{4}{10}$ parts. Mr. Kirwan has shown that in 100 parts of nitre there are 7 parts of water of crystallization; consequently, in 100 parts of gunpowder, as it contains $67\frac{3}{10}$ parts of nitre, there must be $4\frac{711}{1000}$ parts of water.

Now as 1 cubic inch of gunpowder, when the powder is well shaken together, weighs exactly as much as 1 cubic inch of water at the temperature of 55° F. namely 253.175 grs. Troy, a cubic inch of gunpowder in its driest state must contain at least $10\frac{927}{1000}$ grains of water; for it is 100 to 4.711, as 253.175 to 10.927. But besides the water of crystallization which exists in the nitre, there is always a considerable quantity of water in gunpowder, in that state in which it makes bodies damp or moist. Charcoal exposed to the air has been found to absorb nearly $\frac{1}{8}$ of its weight of water; and by experiments I have made on gunpowder, by ascertaining its loss of weight on being much dried, and its acquiring this lost weight again on being exposed to the air, I have reason to think that the power of the charcoal, which enters into the composition of gunpowder, to absorb water remains unimpaired, and that it actually retains as much water in that state, as it would retain were it not mixed with the nitre and the sulphur.

As there are $15\frac{4}{10}$ parts of charcoal in 100 parts of gunpowder, in 1 cubic inch of gunpowder = 253.175 grains Troy, there must be 38.989 grains of charcoal; and if we suppose $\frac{1}{8}$ of the apparent weight of this charcoal to be water, this will give 4.873 grains in weight for the water which exists in the form of moisture in 1 cubic inch of gunpowder. That this estimation is not too high, is evident from the following experiment. 1160 grains Troy of apparently dry gunpowder, taken from the middle of a cask, on being exposed 15 minutes in dry air, heated to the temperature of about 200°, was found to have lost 11 grains of its weight. This shows that each cubic inch of this gunpowder actually gave out $2\frac{4}{10}$ grains of water on being exposed to this heat; and there is no doubt but that at the end of the experiment it still retained much more water than it had parted with.

If now we compute the quantity of water which would be sufficient, when reduced to steam under the mean pressure of the atmosphere, to fill a space equal in capacity to 1 cubic inch, we shall find that either that contained in the nitre which enters into the composition of 1 cubic inch of gunpowder as water of crystallization, or even that small quantity which exists in the powder in the state of moisture, will be much more than sufficient for that purpose.

Though the density of steam has not been determined with that degree of precision that could be wished, yet it is quite certain that it cannot be less than 2000 times rarer than water, when both are at the temperature of 212°. Some have supposed it to be more than 10,000 times rarer than water, and experiments have been made which seem to render this opinion not improbable; but we will take its density at the highest possible estimation, and suppose it to be only 2000 times rarer than water. As 1 cubic inch of water weighs 253.175 grains, the water contained in 1 cubic inch of steam at the temperature of 212° will be $\frac{1}{2000}$ part of 253.175 grains, or 0.12659 of a grain. But we have seen that 1 cubic inch of gunpowder contains 10.927 grains of water of crystallization, and 4.873 grains in a state of moisture. Consequently the quantity of water of crystallization in gunpowder is 86 times greater, and the quantity which exists in it in a state of moisture is 38 times greater, than that which would be required to form a quantity of steam sufficient to fill completely the space occupied by the powder.

Hence we may venture to conclude, that the quantity of water actually existing in gunpowder, is much more than sufficient to generate all the steam that would be necessary to account for the force displayed in the combustion of gunpowder, supposing that force to depend solely on the action of steam, even though no water should be generated in the combustion of the gunpowder. It is even very probable that there is more of it than is wanted, and that the force of gunpowder would be still greater, could the quantity of water it contains be diminished. From this computation it would appear, that the difficulty is not to account for the force actually exerted by fired gunpowder, but to explain the reason why it does not exert a much greater force. But I shall leave these investigations to those who have more leisure than I now have to prosecute them.*

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* We have bestowed more than ordinary remarks on this paper, as the experiments are exceedingly curious, the observations ingenious, and the subject of great importance. The author has greatly merited the praise of the philosophical world for his ingenuity and uncommon exertions, and the minuteness with which he has related all the circumstances. But as we have often heard it remarked that Count R. did not always understand his own experiments, particularly in the case of the present paper;

XIII. A Third Catalogue of the Comparative Brightness of the Stars; with an Introductory Account of an Index to Mr. Flamsteed's Observations of the Fixed Stars contained in the 2d Volume of the Historia Cœlestis. To which are added, several Useful Results derived from that Index. By Wm. Herschel, LL. D., F. R. S. p. 293.

In my earliest reviews of the heavens, I was much surprized to find many of the stars of the British catalogue missing. Taking it for granted that this catalogue was faultless, I supposed them to be lost. The deviation of many stars from the magnitude assigned to them in that catalogue, for the same reason, I considered as changes in the lustre of the stars. Soon after however I perceived that these conclusions had been premature, and wished it were possible to find some method that might serve to direct us from the stars in the British catalogue, to the original observations which have served as a foundation to it. The labour and time required for making a proper index withheld me continually from undertaking the construction of it: but when I began to put the method of comparative brightness in practice, with a view to form a generated catalogue, I found the indispensible necessity of having this index recur so forcibly, that I recommended it to my sister to undertake the arduous task. At my request, and according to a plan which I laid down, she began the work about 20 months ago, and has lately finished it.

The index has been made in the following manner. Every observation on the fixed stars contained in the 2d volume of the Historia Cœlestis was examined first, by casting up again all the numbers of the screws, in order to detect any error that might have been committed in reading off the zenith-distance by diagonal lines. The result of the computation being then corrected by the quantity given at the head of the column, and, refraction being allowed for, was next compared with the column of the correct zenith-distance as a check. Every star was now computed by a known preceding or following star; and its place according to the result of the computation laid down in the Atlas Cœlestis, by means of proportional compasses. This was necessary, in order to ascertain the observed star: for the observations contain but little information on the subject; most of the small stars being without names, letters, or descriptions. The many errors in the names of the constellations affixed to the stars, and in the letters by which they are denoted, also demanded a more scrupulous attention; so that only their relative situation, examined by calculation, could ascertain what the stars really were which had been observed.

Every observed star being now ascertained, its number in the British catalogue was added in the margin at the end of the line of the observation; and a book with

and as we could not always agree in his deductions from them, especially in the most important particulars; we have found it our duty to the public to show, by our observations, our conviction that he has completely failed in the most important circumstances of the inquiry.

all the constellations and number of the stars of the same catalogue, with large blank spaces to each of them, being provided, an entry of the page where Flamsteed's observation is to be found, was made in its proper place. If the star observed was not in the British catalogue, it was marked as such in the margin of the observations; and being provided with another book of constellations and numbers, it was entered into the blank space belonging to some known preceding or following star, by which its place had been settled. The Greek and English letters used by Flamsteed, whether they were such as had been introduced before, or which he thought it expedient to add to them at the time of observation, were also entered in their proper places; and to complete the whole, the magnitude affixed to the stars was also joined to the entry made in the blank spaces of the index.

I have been so far particular in giving the method by which the index has been constructed, that it may appear what confidence ought to be given to the conclusions which will be drawn from its report. About 3 or 4 examples of its use, will completely show how the results, which will be mentioned, have been obtained. Suppose I wish to be informed of the particulars relating to the 13th Arietis. By the index I am referred, in the column allotted for that star, to 77 observations; and find that Flamsteed used the letter α 72 times, and that in 2 places he calls it a star of the 2d magnitude; the rest of the observations being without any estimation of its brightness. If it be required to know Flamsteed's observations on the 34th Tauri, which star is supposed to have been the Georgian planet, mistaken by Flamsteed for a small fixed star*; we find in our index, that on page 86, December 13, 1690, a star of the 6th magnitude was observed, which answers to the place of the 34th Tauri in the British catalogue; and that no other observation of the same star occurs in the 2d volume. In my catalogue of comparative brightness, the 34th Tauri is set down among the lost stars, it being no longer to be seen in the place where it was observed by Flamsteed.

If in my review of the heavens I cannot find 38 Leonis, and examine this index, I am at once informed that Flamsteed never observed such a star; and that of consequence it has been inserted in the British catalogue by some mistake or other. In many cases, these mistakes may be easily traced, as has been shown with regard to this star in my 2d catalogue of comparative brightness. See the note to 38 Leonis. When we wish to examine 90 Ceti in the heavens, and cannot find it, we are informed by our index, that 90 Ceti is the same star with 1 Eridani; and that consequently we are not to look out for 2 different stars.

We may now proceed to give some general results that are to be obtained from an inspection of our index. They are as follow. 111 stars inserted in the British catalogue have never been observed by Flamsteed. This will explain why so many stars in the heavens seem to have been lost. There are 39 stars in the same catalogue that want considerable corrections in right-ascension or polar-distance. In many it amounts to several degrees. 54 stars more, besides the 39 that are taken

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* See Astronomishes Jahr-Buch for 1789, page 202.—Orig.

from the erroneous stars in the catalogue, want corrections in the Atlas Cœlestis; several of them also of many degrees. 42 stars are set down, which must be reduced to 21; each going by 2 names in different constellations. 371 stars, completely observed both in right-ascension and zenith-distance, have been totally overlooked. 35 more, which have 1 of the 2, either right-ascension or polar-distance doubtful, have been omitted. 86 with only the polar-distance, and 13 with only the right-ascension, have also been unnoticed. About 50 more that are pointed out by pretty clear descriptions, are also neglected: so that on the whole between 5 and 6 hundred stars observed by Flamsteed, have been overlooked when the British catalogue was framed.

These additional stars will make a considerable catalogue, which is already drawn up and nearly finished by Miss Herschel, who is in hopes that it may prove a valuable acquisition to astronomers. Neither the index to Flamsteed's observations, nor the catalogue of omitted stars, were finished when my former 2 catalogues of comparative brightness were given; I shall therefore now select a few notes to be added to those which are at the end of these catalogues. They will contain such additional light as I have been enabled to gather from this newly acquired assistance.

Additional Notes to the Stars in the First Catalogue of the comparative Brightness of the Stars.

Aquarius.—25 Is the same star with 6 Pegasi. There are but 2 observations of it. The first is on page 57; Flamsteed calls it "in constellatione Pegasi sub capite." The 2d, on page 71, is described "in constellatione Aquarii trianguli in capite præcedens et borealis." Here we see that the double insertion in the catalogue is owing to the star's having been called by different names in the observations. See also Mr. Wollaston's catalogue, zone 88°.

27 Is the same with 11 Pegasi. There are 3 observations: the first places the star in the constellation of Pegasus, the 2 latter in that of Aquarius. See also Mr. Wollaston's catalogue for this star, and others of the same kind.

65 Has not been observed by Flamsteed; yet we find it inserted in my first catalogue, where its relative brightness is given. It should be considered that, in the first place, several stars, of which there are no observations in the 2d volume of Flamsteed's works, and which are yet inserted in the British catalogue, such for instance as θ and ι Draconis, are well known to exist in the heavens. Now whether they were put into the catalogue from observations that are not in the 2d volume, or taken from other catalogues, it so happens that observations of them cannot be found. Therefore the want of a former observation by Flamsteed is not sufficient to prove that a star does not exist. In the next place it should be recollected, that the method used to ascertain the stars in estimating their brightness, is not so accurate, as to point out with great precision the absolute situation of a star; and that consequently another star which happens to be not far from the place where the catalogue points out the star we look for, may be taken for it; especially when

there are no neighbouring stars of the British catalogue that may induce us to exert uncommon attention in ascertaining the identity of such a star. Mayer however has an observation of 65 Aquarii in his zodiacal catalogue, N° 932, which puts the existence of the star out of doubt.

72. As the star neither was observed by Flamsteed, nor does exist, we cannot admit the remark which Mr. Wollaston in his catalogue, zone 95°, has on Mayer's 939 star; where he supposes an error in declination of 3 degrees to have been committed, on a supposition of its being Flamsteed's 72.

80 Requires + 2^m in time in RA, and therefore is not the star I have given, which requires − 1^m 35^s.—104, Which is without RA in the British catalogue, has 3 complete observations, page 8, 70, and 331.

Aquila.—29 Is without RA. There is but one observation of Flamsteed, page 53, which has no time. The RA is given by M. de la Lande, in Mr. Bode's Jahr-Buch for 1796, page 163.—33 and 34, Which do not exist, were probably inserted by a mistake of 1 hour in the time of one of the observations on the 2 stars 68 and 69. In the zenith-distance, page 71 of Flamsteed's observation of 69 Aquilæ, for 53° read 55°.—40 and 43, which do not exist, were probably also inserted by the same mistake of 1 hour in the RA of 70 and 71.

Capricornus.—1 and 2 should be ξ^1 ξ^2. Flamsteed calls them so in his observations, and Mayer has also adopted the same letters in his catalogue N° 821 and 822.

Cygnus.—5 Is without RA in the British catalogue; but the star has not been observed by Flamsteed.—9 Is without RA; Flamsteed however has a complete observation of it, page 67.—24 Has no RA. The time observed by Flamsteed is only doubtful in the seconds. Its RA has been given in Mr. Bode's Jahr-Buch for 1797, page 163.—33 Has no RA. Flamsteed never observed this star; but it is 3 Cephei Hevelii.—38 Has no RA in the British catalogue; but as the defective and only observation of Flamsteed on page 75, which might be supposed to belong to 38, will agree better with 43, it follows that he never observed 38.—68 Has no RA. There is a complete observation by Flamsteed, page 75.—78 Has no time in Flamsteed's observations. It is N° 146 in de la Caille's catalogue.—79 Has no RA. Flamsteed has but one observation, which is without time. Mr. Bode gives it in his Jahr-Buch for 1797, page 163.

Hercules.—24 Is the same with 51 Serpentis.—28 Is the same with 11 Ophiuchi.—54. There is no observation of this star. The zenith-distance of 55 was taken twice April 8, 1703, (instances of which we find in several other stars,) which occasioned its being inserted as 2 stars.—63. There is no observation of this star, nor does it exist. The star of which the brightness is given in my catalogue, is at some distance from the place assigned in the British catalogue. Flamsteed observed a star, page 444, which will be N° 269 in Miss Herschel's manuscript catalogue. This, with an error in the calculation of the PD, probably occasioned the insertion of 63. And if this be the star, the PD of the British catalogue must be corrected

+ 3°.—71 Has never been observed by Flamsteed, nor does it exist. A small error, in the calculation of one of the 4 observations of 70, may have produced it.—80 and 81 were never observed. The 2 stars ν 24 and 25 Draconis, miscalled ι in Flamsteed's observations, page 55 and 175, with an error of PD, accounts for the insertion of these stars. See Mr. Bode's Jahr-Buch for 1787, page 194.—93. The PD is marked : : (doubtful,) in the British catalogue; but the observation of Flamsteed, page 520, is complete.

Pegasus.—6 Is the same star with 25 Aquarii.—11 Is the same star with 27 Aquarii.

Additional Notes to the Stars in the 2d Catalogue of the Comparative Brightness of the Stars.

Aries.—1 There is an observation of a star by Flamsteed, which being calculated with an error of 10^m of time in RA, would produce 1 Arietis; we may therefore correct the British catalogue RA + 10^m, and the star will be found to exist. In Miss Herschel's manuscript catalogue it is N° 143.—2 Is the same star with 107 Piscium.—38 is the same star with 88 Ceti. In 3 observations, page 85, 285, and 485, Flamsteed has called it Arietis; and on page 481 he has called it Ceti. See also Mr. Bode's Jahr-Buch for 1793, page 200.—50; By Flamsteed's observation, page 273, the catalogue requires — 1^m in time of RA.

Cassiopea.—3. The place in the catalogue by 2 observations of Flamsteed requires + 5$^m\frac{1}{4}$ of time in RA, and + 7′ of PD.—8 Is marked : :, but has 4 complete observations on page 140, 144, 145, and 147.—29; There is an observation of Flamsteed on page 144 which has produced this star, but the time of it requires a correction of + 6^m, and it will then belong to 32. That this correction should be used, will appear when we compare this observation with another on page 213. In both places a star, which is not inserted in the British catalogue, but which is N° 384 of Miss Herschel's manuscript catalogue, was taken at the same time. On page 144 it is "Duarum infra γ, versus polum, borealis. Simul fere transit, austrea;" and on page 213 we have "post transitum" for the new star, and "cum priore" for 32; and in both places the zenith-distance perfectly shows that they were the same stars: the 32d and a star south of it. And they are now both in the places where Flamsteed has observed them.—30: Flamsteed has no observation of this star. It is μ 21 Cassiopeæ Hevelii.—33: Flamsteed observed no RA of this star. It is θ 23 Cassiopeæ Hevelii.—34 Is wrong in the catalogue. By 2 observations of Flamsteed, page 144 and 521, it requires a mean correction of — 9^m of time in RA. In this case my double star III, 23, will no longer be φ 34 Cassiopeæ, but a star 9^m of time preceding φ; for it exists in the place where 34 is set in the Atlas, according to the erroneous catalogue, and is rather larger than Flamsteed's star φ.—35: The RA is marked : :. The single observation, page 207, has the time marked circiter, being probably set down to the nearest minute only; and by the same observation the PD requires + 20′.—47 Is also marked : :; but has one complete observation, page 149.—51: The observation of Flamsteed which produced this star should be corrected + 1 hour. This makes it 37 Cassiopeæ Hevelii.—52 and 53, By Flamsteed's observation page 208, should be the reverse in PD of what they are.

Cetus.—14: If we correct the British catalogue + 3° in PD, it will become a star observed by Flamsteed, which is N° 312 in Miss Herschel's manuscript catalogue.—26: Flamsteed has no observation of this star; but we find it in de la Caille's zodiacal catalogue, N° 10.—51 Is the same with 106 Piscium. Flamsteed has 23 observations of the star, and has always called it ν, except once on page 482, where it is without a letter, and where the constellation is marked Aquarii: now, as there was immediately following an observation of 54 Ceti, and Aquarius was evidently wrong, the star has been put in Cetus.—58: By Flamsteed's observation, page 358, the RA in the British catalogue requires a correction of — 3^m in time.—74: Flamsteed has no observation of this star, nor can I find it in any other catalogue. The place of it is so distant from other stars of the British catalogue, that my estimation of brightness may belong to some star not far from the situation assigned, and that the star of the British catalogue may not exist.—88 Is the same with 38 Arietis. See Bode's Jahr-Buch for 1793, page 200.

Eridanus.—44, In the British catalogue, is marked ::. The single observation of Flamsteed, page 153, is perfect, all but a difference of 5′ between the zenith-distance by the diagonal lines and by the screw.—45, Marked ::, has a complete observation, page 153.—68, Marked ::, has a complete observation, page 146.

Gemini.—50: There is no observation on this star. The star I have given is at a considerable distance from the place assigned by the British catalogue, so that in fact the star of the catalogue does not exist. It has been inserted in the British catalogue by a mistake in the calculation of a star which is about 1° 49′ more south. This will be N° 139 in Miss Herschel's manuscript catalogue, and it is probably the real intended 50 of Flamsteed. The expression of its brightness 41,50 of my catalogue will do very well for it.—70 and 71, By Flamsteed's observations should be called π^1 and π^2. Tycho and Hevelius also call 71 π.—72 and 73 have been inserted by a mistake in 64 and 65. See Bode's Jahr-Buch for 1788, page 175.—76: Flamsteed has no observation of this star. It is however Mayer's N° 310.—80 Is not π, but according to Flamsteed's observation quæ sequitur π; and has no letter.

Leo.—10 Is the same with 1 Sextantis.—25 does not exist in the place where the British catalogue gives it; but if we admit that it has been inserted by a mistake in the calculation of 10 Sextantis, it may be taken into the constellation of Leo, as a star inserted in 2 constellations; and it will then be "25 is the same with 10 Sextantis."—26. In Flamsteed's observations, page 299, the strias cochleæ give 26′ less than the lineas diagonales. The former are right; therefore the British catalogue must be corrected PD — 26′.—28: Flamsteed has no observation of this star. It was probably inserted by a mistake in calculating an imperfect observation of 11 Sextantis. If this be allowed, we then must say "28 is the same with 11

Sextantis.—66: Flamsteed has no observation of this star. There is a small star near the place where the British catalogue has given it, of which I have expressed the brightness; but as its situation is not exactly where it ought to be, my catalogue should have, "does not exist."—67 Is the same with 53 Leonis minoris.—71 May have been inserted by a mistake in one of the 3 observations of 73; setting the star north of θ instead of south.

Then follows a long list of the comparative brightness of a great many stars, not necessary to be here re-printed.

Notes to Andromeda.—1, By 3 observations of Flamsteed, page 130, 138, and 140, the polar-distance in the edition of 1725 requires + 9°.—40 is the same with 69 Piscium. Flamsteed observed it 5 times; twice among the stars of the constellation Pisces, and 3 times among those of Andromeda. See page 14, 134, 139, 149, and 210.—61, M. de la Lande says is lost. See Bode's Jahr-Buch for 1794, page 97; but as the star is now in its place, it may perhaps be changeable, and ought to be looked after.

Notes to Bootes.—47: The RA in the British catalogue is only given to the nearest degree, and Mr. Bode and Mr. Wollaston, in their catalogues, have left it out; but Flamsteed has 4 complete observations of it, on page 166, 168, 414, and 415, and the star is called k in all of them.

Notes to Cancer.—26 was not observed by Flamsteed. An observation on page 297 has occasioned the insertion of this star; but by correcting the time — 1^m, it will agree with 2 other observations of 22 Cancri on page 21 and 26. See Bode's Jahr-Buch for 1788, page 172.—56: This star has not been observed by Flamsteed, nor does it exist. Page 25 Flamsteed observed 55 Cancri with a memorandum, "Hæc habet comitem sequentem ad austrum;" which has probably occasioned the insertion of this star; but he had not then observed all the ϱ's, and might possibly mean to point out ϱ 53; which he afterwards observed on page 27. The stars are so near together that he might easily mistake sequens for præcedens ad austrum. Flamsteed in his observations calls 58 3d ϱ, 67 4th ϱ, and 70 5th ϱ; this shows that there is no authority for six ϱ's. See Bode's account of the same star in his Jahr-Buch for 1788, page 171.—71: "April 5, 1796, 71 Cancri is 15′ nearer to 78 and 15′ "farther from 68 than it is placed in Atlas."—73 and 74 have not been observed by Flamsteed, nor do they exist. How they came to be inserted, does not appear to be satisfactorily accounted for by Mr. Bode in his Jahr-Buch for 1788, page 172. He gives 4 observations of 62 and 63 Cancri; but Flamsteed has 13, and they are all perfect, except the last on page 564.

Notes to Cepheus.—15: "October 25, 1796: 15 Cephei consists of 2 stars. Both taken together for one, by the naked eye, give 14 . 15. In the telescope they are 14 —, 15 — 15."—18 has no time in Flamsteed's observations. "March 26, 1797. 18 is a very little preceding 19. It is $1\frac{1}{2}$° from 17. The stars 18, 20 and 19 are in a line which bends a little at 18 towards the preceding side."

Notes to Corona Borealis.—21, in the British catalogue requires a correction of

— 28^{m} 21^{s} in time of RA, and — 14′ 55″ in PD. In the place where it is marked in Atlas, according to the erroneous catalogue, is no star; but very unaccountably it is also marked in its right place in the same atlas. Flamsteed has 4 complete observations of it on page 167, 445, 477, and 478. Mr. Wollaston, not being acquainted with the existence of 21 Coronæ in its right place, supposes, zone 55°, that I have made a mistake in calling my double star VI. 18, very unequal; but in his corrections he gives the place of a star, as he calls it "near ν," which is the real second ν of Flamsteed; who very particularly describes it on page 167, "Duarum ad ν sequens et clarior;" and this is the double star I have given in my catalogue as 21 Coronæ.

Notes to Navis.—1: there is no observation of this star; but in Miss Herschel's manuscript catalogue N° 92 is a star 2° more south, which has probably been calculated wrong, and has given occasion for its insertion; correcting therefore the PD of 1 Navis + 2°, the expression of its brightness is as I have given it.—17: there is no observation of this star; but if we correct the PD + 3°, it will then agree with N° 238 in Miss Herschel's manuscript catalogue.—21, by Flamsteed's observation page 431, the PD of the British catalogue requires + 18′.

Notes to Orion.—12: Flamsteed never observed this star. It does not appear how it came to be inserted in the British catalogue.—26: Flamsteed never observed this star. An error of 20′ in PD in the calculation of one of the 4 observations of 25 Orionis, may have occasioned the insertion of it.—35 is marked : : in the British catalogue; but Flamsteed has 7 complete observations of this star; therefore the marks : : should be out.—63: There is no observation of this star; but supposing an error of + 2^{m} 14^{s} of time in RA, and of + 0′ 22″ in PD, it will then agree with N° 33 of Miss Herschel's manuscript catalogue. I have taken the comparative brightness of that star, supposing it to be 63.—64 and 65 have no observation by Flamsteed; but their insertion has been accounted for by Mr. Bode in his Jahr-Buch for 1793, page 195. He mentions Flamsteed's 2 observations on page 17 and 94. There is a 3d on page 292, which confirms what Mr. Bode says. The 64 of which I give the brightness, is not far from the place assigned to it in the British catalogue. It is N° 1 in Miss Herschel's manuscript catalogue. —76: There is no observation of this star. A mistake of 41′ in PD in calculating one of the 4 observations of 8 Monocreotis, might occasion its insertion.

XIV. An Account of the Means employed to obtain an Overflowing Well. By Mr. Benjamin Vulliamy. p. 325.

In beginning to sink this well, which has a diameter of 4 feet, the land springs were stopped out in the usual way, and the well was sunk and steined to the bottom. When the workmen had got to the depth of 236 feet, the water was judged not to be very far off, and it was not thought safe to sink any deeper. A double thickness of steining was made about 6 feet from the bottom upwards, and a borer of $5\frac{1}{4}$ inches diameter was used. A copper pipe of the same diameter with the borer was driven down the bore-hole to the depth of 24 feet, at which depth the borer pierced through the rock into the water; and by the manner of its going through, it must probably have broken into a stratum containing water and sand. At the time the borer burst through, the top of the copper pipe was about 3 feet above the bottom of the well: a mixture of sand and water instantly rushed in through the aperture of the pipe. This happened about 2 o'clock in the afternoon, and by 20 minutes past 3 the water of the well stood within 17 feet of the surface. The water rose the first 124 feet in 11 minutes, and the remaining 119 feet in 1 hour and 9 minutes. The next day several buckets of water were drawn out, so as to lower the water 4 or 5 feet; and in a short time the water again rose within 17 feet of the surface. A sound-line was then let down into the well in order to try its depth. To our great surprize the well was not found by 96 feet so deep as it had been measured before the water was in it; and the lead brought up a sufficient quantity of sand to explain the reason of this difference, by showing that the water had brought along with it 96 feet of sand into the well. Whether the copper pipe remained full of sand or not, is not easy to be determined; but I should rather be inclined to think it did not.

After the well had continued in the same state several days, the water was drawn out so as to lower it 8 or 10 feet; and it did not rise again by about a foot so high as it had risen before. At some days interval, water was again drawn out, so as to lower the water as before; which at each time of drawing rose less and less, till after some considerable time it would rise no more; and the water being then all drawn out, the sand remained perfectly dry and hard. I now began to think the water lost; and consequently that all the labour and expence of sinking this well, which by this time were pretty considerable, had been in vain. There remained no alternative but to endeavour to recover it by getting out the sand, or all that had been done would be useless; and though it became a more difficult task than sinking a new well might have been, yet I determined to undertake it, because I knew another well might also be liable to be filled with sand in the same manner that this was. The operation of digging was again necessarily resorted to, and the sand was drawn up in buckets till about 60 feet of it were drawn out, and consequently there remained only 36 feet of sand in the well: that being too light to keep the water down, in an instant it forced again into the well with the same violence it had done before; and the man who was at the bottom, getting out the sand, was drawn up almost suffocated, having been covered all over by a mixture of sand and water. In a short time the water rose again within 17 feet of the surface, and then ceased to rise, as before. When the water had ceased rising, the sounding-line was again let down, and the well was found to contain full as much sand as it did the first time of the water's coming into it.

Any further attempt towards recovering the water appeared now in vain; and most people would I believe have abandoned the undertaking. I again considered that the labour and the expence would be all lost by so doing; and I determined without delay to set about drawing the sand out through the water, by means of an iron box made for that purpose, without giving it time to harden as before. The labour attending this operation was very great, as it was necessary continually to draw out the water, for the purpose of keeping it constantly rising through the sand, and so to prevent the sand from hardening. What rendered this operation the more discouraging was, that frequently after having drawn out 6 or 7 feet of sand in the course of the day, on sounding the next morning the sand was found lowered only 1 foot in the well, so that more sand must have come in again. This however did not prevent me from proceeding in the same manner during several days, though with little or no appearance of any advantage arising from the great exertions we were making. After persevering however for some considerable time, we perceived that the water rose a little nearer to the surface, and I began to entertain some hopes that it might perhaps rise high enough to come above the level of the ground; but when the water had risen a few feet higher in the well, some difficulties occurred, occasioned by accidental circumstances, which very much delayed the progress of the work; and it remained for a considerable time very uncertain whether the water would run over the top of the well or not.

These difficulties being at length surmounted, we continued during several days the process before mentioned, of drawing out the sand and water alternately; and I had the satisfaction of seeing the water rise higher and higher, till at last it ran over the top of the well, into a temporary channel that conveyed it into the road. I then flattered myself that every difficulty was overcome; but a few days afterwards I discovered that the upper part of the well had not been properly constructed, and it became necessary to take down about 10 feet of brick-work. The water, which was now a continued stream, rendered this extremely difficult to execute. I began by constructing a wooden cylinder 12 feet long, which was let down into the well, and suspended to a strong wooden stage above, on which I had fixed 2 very large pumps, of sufficient power to take off all the water that the spring could furnish, at 11 feet below the surface. The stage and cylinder were so contrived as to prevent the possibility of any thing falling into the well; and I contrived a gage, by which the men on the stage could always ascertain to the greatest exactness the height of the water within the cylinder. This precaution was essentially necessary, to keep the water a foot below the work which was doing on the outside of the cylinder, to prevent the new work from being wetted too soon. After every thing was prepared, we were employed 8 days in taking down 10 feet of the wall of the well, remedying the defects, and building it up again; during which time 10 men were employed, 5 relieving the other 5, and the 2 pumps were kept constantly at work during 192 hours. By the assistance of the gage, the water was never suffered to rise on the new work till it was made fit to receive it. When the cylinder was taken out, the water again ran over into the temporary channel that conveyed it into the road.

The top ofthe well was afterwards raised 18 inches, and constructed in such a manner as to be able to convey the water 5 different ways at pleasure, with the power of being able to set any of these pipes dry at will, in order to repair them whenever occasion should require. The water being now entirely at command, I again resolved on taking out more sand, in order to try what additional quantity of water could be obtained. I cannot exactly ascertain the quantity of sand taken out, but the increase of water obtained was very great; as instead of the well discharging 30 gallons in a minute, the water was now increased to 46 gallons in the same time.

XV. Observations of the Changeable Brightness of the Satellites of Jupiter, and of the Variation in their Apparent Magnitudes; with a Determination of the Time of their Rotatory Motions on their Axes. To which is added, a Measure of the Diameter of the Second Satellite, and an Estimate of the Comparative Size of all the Four. By Wm. Herschel, LL.D., F.R.S. p. 332.

It may be easily supposed, when I made observations on the brightness of the 5th satellite of Saturn, by way of determining its rotation on its axis, and found that these observations proved successful, that I should also turn my thoughts to the rest of the satellites, not only of Saturn, but likewise of Jupiter, and of the Georgian planet. Accordingly I have from time to time, when other pursuits would permit, attended to every circumstance that could forward the discovery of the rotation of the secondary planets; especially as there did not seem to lie much difficulty in the way. For since I have determined, by observation, that the 5th satellite of Saturn is in its rotation subject to the same law that our moon obeys, it seems to be natural to conclude that all the secondary planets, or satellites, may probably stand in the same predicament with the two I have mentioned; consequently a few observations that coincide with this proposed theory, will go a good way towards a confirmation of it. I had another point in view when I made the observations contained in this paper. It was an attempt to avail myself of the abundant light and high powers of my various telescopes, to examine the nature and construction of the bodies of the satellites themselves, and of their real magnitudes. Here phenomena occurred that will perhaps be thought to be remarkable, and even inconsistent or contradictory. So far from attempting to lessen the force of such animadversions, I shall be the first to point out difficulties, in order that future observations may be made to resolve them.

Perhaps it would have been better to delay the communication of these observations, till I had continued them long enough to be able to account for things which at present must be left doubtful. But as, in final conclusions to be drawn from astronomical observations, we ought to take care not to be precipitate; so on the other hand I am perhaps too scrupulous in satisfying myself, and should probably require the observations of several years before I could venture to be decisive. It will also be seen by the dates of the first observations, that a further delay in the communication cannot be adviseable, since much information may possibly be gained by throwing open, to other observers, the road it will be eligible to take for a satisfactory investigation of the subject; especially as we have reason to congratulate ourselves on the spirit of observation, and increase of large instruments, that seem to have taken place in various parts of Europe. The observations from my journals are as follow.

OBSERVATIONS.

A remarkable conjunction of two satellites of Jupiter.—May 14, 1790, $11^h\ 30^m\ 10^s$, correct sidereal time. The 2d and 3d satellites of Jupiter are so closely in conjunction, that with a 7-feet reflector, charged with a magnifying power of 350, I cannot see a division between them. At $11^h\ 34^m\ 10^s$, the shadow of the 1st satellite is still on the disc of the planet.

Intenseness of light and colour of the satellites.—July 19, 1794, $17^h\ 12^m\ 47^s$, 7-feet reflector: the 1st satellite of Jupiter is of a very intense bright, white, shining light. It is brighter than the 2d or 4th. I speak only of the light, and not of the size.—The colour of the 4th satellite is inclining to red. In brightness it is very nearly, but not quite equal to the 2d.—10-feet reflector, power 170: the 3d satellite is just gone upon the body; before it went on, it appeared to be smaller than usual.—The 2d satellite is of a dull, ash-colour; not in the extreme, but rather inclining to that tint.—July 21, 1794, $16^h\ 56^m\ 45^s$; 10-feet reflector; power 170: the 3d satellite of Jupiter is round, large, and well defined. It is very bright, and its light is very white.—The 4th satellite is also round, large, and well defined. I estimate its magnitude in proportion to that of the 3d satellite to be as 4 to 5. Its light is not white, but inclined to orange.

Brightness and diameter distinguished.—July 26, 1794, $17^h\ 14^m\ 41^s$; 10-feet reflector; power 170: The 4th satellite is very dim: it is of a pale, dusky, reddish colour. The 2d satellite is of a bright, white colour. The 3d satellite is very bright, and white. The 1st satellite is very brilliant, and white. At $17^h\ 22^m\ 41^s$, the magnitudes with 240, were thus: the 3d satellite is the largest: the 2d satellite is the smallest. With 300, the 4th satellite is a very little larger than the 2d, though less bright. The 1st satellite is larger than either the 4th or 2d. With 400, the order of the magnitudes is 3 1 4 2. With the same power, the order of the light is 3 1 2 4.

Diameter of the second satellite by entering on the disc of the planet.—July 28, 1794, $17^h\ 25^m\ 40^s$; 10-feet reflector; power 170: the 2d satellite is nearly in contact with the following limb of Jupiter.—$17^h\ 29^m\ 40^s$, it seems to be very near the contact: with 300, very near the contact.—$17^h\ 30^m\ 40^s$, it seems to be in contact: it is brighter than that part of Jupiter where it enters.—$17^h\ 31^m\ 40^s$, it is more than half entered.—$17^h\ 33^m\ 40^s$, it seems to be nearly quite entered: its superior brightness makes it seem protuberant.—$17^h\ 34^m\ 40^s$, it is certainly quite entered.—$17^h\ 35^m\ 25^s$, I see a little of the disc of Jupiter on the outside of the satellite, equal to about $\frac{1}{4}$ of its diameter.—$17^h\ 39^m\ 40^s$, the 3d satellite is very bright, and of its usual colour.—The 4th satellite is faint, and also of its usual colour.—The 1st satellite is very bright, and its light is of the usual intenseness.

The magnitudes with 300.—The diameter of the 4th seems to be to that of the 3d, as 2 to 3; or perhaps more exactly, as 3 to 5.—The diameter of the 4th satellite exceeds that of the 1st a very little.—With 400. With this power the diameter of the 4th satellite certainly exceeds that of the 1st.—The diameter of the 4th is to that of the 3d, as 3 to 5.

July 30, 1794, $19^h\ 1^m\ 37^s$, 10-feet reflector; power 300: the 4th satellite of Jupiter is a little larger than the 1st: it is of its usual colour. The 2d is less than the 1st. The 3d is larger than the 4th.

July 31, 1794, $17^h\ 18^m\ 38^s$; 10-feet reflector; power 170: the 4 satellites of Jupiter are very favourably placed for my purpose. The 1st is less bright than the 2d: it is a very little larger than the 2d: the difference in the size is but barely visible. The light of the 2d is very intense and white. The light of the 3d is very intense and bright. The light of the 4th is dull; and seems to be inferior to the usual proportion it bears to the other satellites.—At $18^h\ 38^m\ 38^s$; with 300; the 4th satellite is larger than the 1st; the 2d satellite is a little larger than the 1st, or at least equal to it; the 3d is undoubtedly the largest. The order of the magnitudes therefore is, 3 4 2 1. My brother, Alexander Herschel, looked at the satellites, and estimated the order of their magnitudes exactly the same; though he was not present when I made the foregoing estimations.

August 1, 1794, $17^h\ 38^m\ 37^s$; 10-feet reflector; power 170: the light of all the 4 satellites is very brilliant, the evening being very fine.—With 300, the northmost and farther of the 2 satellites which are in conjunction, is the smaller: I suppose it to be the 2d. The southern and nearer of the 2 satellites in conjunction, is the next in size; I suppose it to be the 1st. The 4th satellite is a little larger than the larger of the 2 satellites which are in conjunction; but the difference is only visible with a great deal of attention. The 3d satellite is much larger than the 4th.

August 9, 1794, $17^h\ 56^m\ 32^s$; 10-feet reflector; power 170; the light of the 1st satellite is very intense and white. The light of the 2d satellite is also pretty intense and white. The light of the 3d satellite is neither so intense nor so white as that of the 1st. The light of the 4th is dull and of a ruddy tinge. With 300 and 400, the 2d is the least, and the 3d is the largest. I am in doubt whether the 4th or the 1st is largest; with 600, I suppose the 1st to be larger than the 4th.

September 30, 1795, $20^h\ 15^m\ 17^s$; 7-feet reflector; power 210; order of the magnitudes of the satellites of Jupiter 3 - 2 . 1 , 4; power 110; 3 - 2 , 1 . 4; with 460, 3 - 2 , 1 , 4.

October 2, 1795, $20^h\ 18^m\ 22^s$; 7-feet reflector; power 287; Jupiter's satellites 3 - - 2 1 , 4. The 2d and 3d satellites are not yet in conjunction.—At $20^h\ 43^m\ 22^s$. The conjunction between the 3d and 2d satellites is past. The distance between them is now one diameter of the 3d.

August 18, 1796, $18^h\ 47^m\ 21^s$; 7-feet reflector; power 287; the 4th satellite is less bright than the 1st; though the latter is so near the planet as to have its light overpowered by Jupiter, while the 4th is at a great distance; I mean light or brightness, not magnitude. The 1st is very bright.

September 15, 1796, $19^h\ 25^m\ 25^s$; 10-feet reflector; power 300; the 2d satellite of Jupiter is a little less than the 1st. The 3d is much larger than any of the rest.—With the power 600 the difference in the magnitude of the 1st and 2d satellites, with this power, is pretty considerable.

September 21, 1796, $19^h\ 24^m\ 5^s$; 10-feet reflector; power 600; the shadow of the 1st satellite is on one of the dark belts of Jupiter. In order to use very high powers with this telescope, I tried it on the double star ζ Aquarii with 1200. The air is very tremulous, but I see now and then the 2 stars of this double star very well defined. With the same power, the satellites of Jupiter are very large, but not so well defined as the above star.

The brightness of the satellites compared to the belts and disc of the planet.—The 1st satellite, which is lately come off the southern belt, is nearly of the same brightness with that belt; power 600. With 400, it is nearly as bright as the brighter part of the planet, or rather a mean between the belt and the planet. The 2d satellite is considerably bright; its colour is whiter than that of the 1st; it is however not so white as the colour of the bright part of Jupiter. The colour of the 4th satellite is as dingy as that of the belt; very much less bright and less white than that of the 2d. The brightness of the 3d satellite is not intense; its colour however is white, though not so white as the bright part of the planet.

September 24, 1796, $20^h\ 55^m\ 24^s$; 10-feet reflector; power 600; the 1st satellite of Jupiter is very bright, and of a white colour; it is also very large. The 2d satellite is faint and bluish; its light is not much brighter than that of the belt. The 3d satellite is pretty bright; its light is whitish; it seems to be comparatively less than it ought to be; or rather, its apparent smallness is owing to the uncommon largeness of the 1st. The 1st satellite, with 200, compared to the 3d, is proportionally larger than I have seen it before.

September 30, 1796, $20^h\ 8^m\ 4^s$; 10-feet reflector; power 600; the satellites of Jupiter are well defined, and the night is beautiful. The 3d satellite, in proportion to the 1st, is much larger than it was September 24; I ascribe the change to an apparent diminution of the 1st.—At $20^h\ 30^m\ 4^s$, the 1st satellite is evidently less in proportion to the 3d, than it was September 24. The 2d satellite is considerably bright; its light is whitish; much brighter than the belt, but not so bright as the bright part of the disc; its magnitude is less than that of the 4th; but its light is considerably superior. The 3d satellite is remarkably well defined; its light is considerably brighter than that of the belts. The magnitude of the 1st satellite exceeds that of the 2d; it is nearly equal to that of the 4th.—At $22^h\ 58^m\ 4^s$, appearances as before.

October 15, 1796, $21^h\ 23^m\ 42^s$; 10-feet reflector; power 600; the 2d satellite is uncommonly bright; its apparent magnitude is also larger than usual. The 4th satellite is very faint; it is not brighter than the belt, but is of a bluish, ruddy colour. The apparent magnitude of the 2d satellite, after long looking, is very nearly equal to that of the 1st; but at first sight it seems to be larger, owing to its superior brightness. The apparent diameter of the 2d satellite is certainly larger than that of the 4th.—At $23^h\ 55^m\ 42^s$, the light of the 1st satellite, compared to that of the 2d, is considerably increased since the last observation. It is now nearly as bright as the 2d.

October 16, 1796, $23^m\ 49^s$; 10-feet reflector; power 600; the 1st, 2d, and 3d satellites of Jupiter seem all considerably bright. The 3d is much larger than the 1st, and the 1st a little larger than the 2d. The intensity of the light seems to be pretty equal in all the 3; that of the 2d however is perhaps a little stronger than that of the 1st; for, notwithstanding its apparent less diameter, it seems to make as strong an impression as the 1st.

October 25, 1796, $21^h\ 44^m\ 48^s$; 10-feet reflector; power 600; the 1st satellite of Jupiter, compared to the 3d, is small. The 3d satellite is bright and large. The 2d is brighter than the 1st. Compared to its usual brightness and magnitude, it is very bright and small. The 1st satellite, compared to its usual brightness and magnitude, is faint and small. The air is so tremulous that the power of 600 is too high, and the necessary uniformity required in these observations will not permit a lower to be used. Perhaps one of 400 might be more generally employed; and it may be proper to use it constantly.

November 3, 1796, $23^h\ 55^m\ 47^s$; 10-feet reflector; power 600; the 4th satellite of Jupiter is large and bright. The 3d satellite is large and bright. The 1st satellite is pretty small, and not very bright. The 2d satellite is small, and considerably bright. The brightness and magnitude of each satellite refer to its own usual brightness and magnitude.

Before we can proceed to draw any conclusions from these observations, we ought to take notice of many causes of deception, and of various difficulties that attend the investigation of the brightness of the satellites. The difference in the state of the atmosphere between 2 nights of observation, cannot influence much our estimation of the brightness of a satellite, provided we adopt the method of comparative estimations. If we endeavour by much practice to fix in our mind a general ideal standard of the brightness of each satellite, we shall find the state of the atmosphere in different nights very much disposed to deceive us; but if we learn to acquire a readiness of judging of the comparative brightness of each satellite with respect to the other 3, we may arrive at much more precision, since the different disposition of the air will nearly affect all the satellites alike. But here, as we get rid of one cause of deception, we fall under the penalty of another. The situation of those very satellites to which we are to refer the light of the satellite under estimation, being changeable, permits us no longer to trust to their standard, without a full scrutiny of the causes that may have produced an alteration in them. In the foregoing observations it will also be seen, that I attempted to compare the intenseness of the light of the satellites with the different brightness of the disc of Jupiter; but these endeavours will always fail, on account of the little assurance we can have that the parts of the disc, setting aside its quick rotation, will remain for any time of the same lustre.

A very material difficulty arises from the magnifying power we use in our estimations. If it be a low one, such as for instance 180 (for a lower should not even be attempted), then we run the risk of being disappointed in bright nights by the sparkling of the brilliant light of the satellites. Besides, we cannot then see the bodies of them, and judge of their comparative magnitude, with the same power that we view their light. If we choose a higher magnifier, we shall be often disappointed in the state of the atmosphere, which will of course occasion an interruption in the series of our observation, of which the regular continuance is of the greatest consequence. If we change our power according to the state of the atmosphere, we introduce a far worse cause of confusion; for it will be next to impossible to acquire, for each magnifying power, an ideal standard of comparative brightness to which we can trust with confidence.

If the magnitudes are not attended to, and carefully contra-distinguished from the intenseness of light, we shall run into considerable error, by saying that a satellite is large, when we mean to express that it is bright. It is so common to call stars that are less bright than others, small, that we must be careful to avoid such ambiguities, when the condition of the satellites is under investigation. Nor is it possible to throw the size and light into one general idea, and take the first coup d'œil in looking at them, to decide about the general impression this compound may make. When our attention is forcibly drawn by a considerable power to the apparent size of the satellite we are looking at, its brightness can no longer be taken in that general way, but must be abstracted from size.

Let us now see what use may be drawn from the observations I have given. It appears in the first place very obviously, that considerable changes take place in the brightness of the satellites. This is no more than might be expected. A variegated globe, whether terraqueous like the earth, or containing regions of soil of an unequal tint, like that side of the moon which is under our inspection, cannot, in its rotation, present us with always the same quantity of light reflected from its surface. In the next place, the same observations point out what we could hardly expect to have met with; namely, a considerable change in the apparent magnitude of the satellites. Each of them having been at different times the standard to which another was referred, we cannot refuse to admit a change so well established, singular as it may appear.

The first of these inferences proves that the satellites have a rotatory motion on their axes, of the same duration with their periodical revolutions about the primary planet. The 2d either shows that the bodies of the satellites are not spherical, but of such forms as they have assumed by their quick periodical and slow contemporary rotatory motions, and which forms in future may become a subject for mathematical investigation; or it may denote, in case geometrical researches should not countenance a sufficient deviation from the spherical form, that some part of the discs of these satellites reflects hardly any light, and therefore in certain situations of the satellite makes it appear of a smaller magnitude than in others. Here then we see evidently that a considerable field for speculation, as well as observation, is opened to our view; and almost every attempt to enter on the work must seem premature, for want of more extended observations. However, from those that have been given, such as they are, I will show how far we may be authorised to say, that the satellites revolve on their axes in the same time that they perform a periodical revolution about the planet.

I shall take the usual method of throwing the observations of each satellite into a graduated circle. The zero of the degrees into which I suppose it divided, is in all observations assumed to be in the place of the geocentric opposition. In order to bring these observations to the circle, the places of the satellites have been calculated from my own tables of the mean motion in degrees, and according to epochs continually assumed from the geocentric conjunctions pointed out in the configurations of the Nautical Almanac; and the nearest of these conjunctions have been always used. This method is fully sufficient for the purpose, as greater precision in the calculation is not required. The observations extend from July 19, 1794, to November 3, 1796; and therefore include a period which takes in 470 rotations of the 1st satellite; 234 of the 2d; 116 of the 3d; and 50 of the 4th; that is, provided we admit that these rotations are performed in the same time as the satellites revolve in their orbits. In the following table are the calculated places of the satellites; the correct sidereal times, given with the observations, having been turned into mean time.

Positions of the 4 Satellites of Jupiter at the Time of the Observations.

Time of observation.	I	II	III	IV
1794.				
July 19^d .... 9^h 21^m	127°	346°	179°	46°
July 21 .... 8 57			278	89
July 26 .... 8 56	124	333	169	205
July 28 .... 8 59	171	176	270	248
July 30 .... 10 27	231	25	13	292
July 31 .... 8 40	59	118	60	312
Aug. 1 .... 8 56	265	221	111	334
Aug. 9 .... 8 42	83	310	152	138
1795.				
Sept. 30 .... 7 37	294	62	219	100
Oct. 2 .... 7 32	341	264	319	143
1796.				
Aug. 18^d .... 8^h 21^m	115	°	°	191°
Sept. 15 .... 7 44	36	328	198	
Sept. 21 .... 7 19	172	214	138	210
Sept. 24 .... 8 38	74	163	305	275
Sept. 30 .... 7 27	206	46	244	36
Oct. 15 .... 7 44	28	130		5
Oct. 15 .... 10 15	49			
Oct. 16 .... 10 39	256	243	334	
Oct. 25 .... 7 25	261	72	59	
Nov. 3 .... 9 0	306	270	151	58

It will be necessary now to explain in what manner, with the assistance of this table, the observations of the brightness and magnitudes of the satellites have been reduced to the expressions they bear in the 4 circles of the figures 1, 2, 3, 4, pl. 4. By way of uniformity I judged it would be best to reduce the estimations of magnitude to those of brightness; as it may be justly supposed that when a satellite is at any given time larger in proportion to another than it was at another time, it will also be brighter than it was at that other time, due regard being had to the light of the satellite to which its magnitude has been compared. To manage the space allotted to the figure advantageously, I have used the abbreviations formerly employed in my catalogue of nebulæ, VB, CB, B, PB, PF, F, CF, VF, for all the gradations of light that are necessary to express the brightness of the satellites at the time of observation. It will be easily remembered that B and F mean bright and faint; and p, c, v, stand for pretty, considerably, and very.

Now, when the observation mentions the brightness of the satellite, I place it in the figure as it is given. In that of the first, for instance, July 19, 1794, we find the satellite called very bright; I therefore set down in fig. 1, at 127°, VB. But where the brightness is not expressed, I have recourse to the comparative magnitude, if that can be had. By fig. 3, it appears that the 2d satellite is less subject to a change of brightness than either the 1st or 4th: it becomes, for that reason, a pretty good standard for the light of these other satellites. Therefore, in the observation of October 2, 1795, for instance, where the 1st satellite is described as undoubtedly less than the 2d, I set down very faint, or VF, at 341° of the circle of fig. 1; for in the observation of July 19, before-mentioned, when the satellite was called very bright, it was at the same time described as undoubtedly larger than the 2d. In this case, as regard must be had to the relative state of the satellite we refer to, the 4 figures will assist us in determining the condition of the light of the satellite we wish to admit as a standard.

In reducing the 2d satellite to the circle, I have generally used a reference to the magnitude of the 1st, where marks of brightness were wanting; and sometimes also to the magnitude of the 4th, and even of the 3d. The 3d satellite can hardly be ever compared to any but the 2d in magnitude; and this only in its degree of excess. The magnitude of the 4th satellite has been generally compared with that of the 1st; and also sometimes with that of the 2d. To make an application of the contents of the figures, will now require little more than a bare inspection of them.

The 1st satellite appears evidently to have a rotation on its axis that agrees with its revolution in its orbit. It cannot be supposed that, in the course of 470 revolutions, all the bright observations could have ranged themselves in one half of the orbit, while the faint ones were withdrawn to the other. The satellite appears in the middle of the duration of its brightness, when it is nearly half way between its greatest eastern elongation, in the nearest part of its orbit; or when advancing towards its conjunction. I have pointed out this circumstance by a division with dotted lines, and the words bright and faint, inserted within the circle, fig. 1. This satellite therefore revolves on its axis in $1^{d}\ 18^{h}\ 26^{m}.6$.

The 2d satellite, though much less subject to change, on account, as we may suppose, of having only a small region on its body which reflects less light than the rest; has nevertheless its rotation directed by the same law with the 1st. It will hardly be necessary to take notice of a single deviation which occurs at 163°, fig. 2; as, from the proximity of the satellite to the conjunction, a mistake in the estimation may easily take place. I generally made it a rule not to make allowance for the influence of the superior light of the planet; but it seems that we can hardly abstract sufficiently on such occasions. Two similar cases occur, in fig. 3, at 179; and fig. 4, at 5°. It is indeed not impossible but that occasional changes, on the bodies of the satellites themselves, may occasion some temporary irregularity of their apparent brightness: it will however not be necessary to make such an hypothesis, till we have better authority for it. The brighter side of this satellite is turned towards us when it is between the greatest eastern elongation and the conjunction. It revolves consequently on its axis in $3^d\ 18^h\ 17^m.9$.

The 3d satellite suffers but little diminution of its brightness, and is in full lustre at the time of both its elongations. It is however not impossible but that, after having recovered its light, on the return from the opposition, it may suffer a 2d defalcation of it in the nearest quadrant about half way towards the conjunction. The 2 independent observations at 151 and 152°, fig. 3, seem to give some support to this surmise. It revolves on its axis in $7^d\ 3^h\ 59^m.6$.

The 4th satellite presents us with a few bright views when it is going to its opposition, and on its return towards the greatest eastern elongation; but otherwise it is generally overcast. Its colour also is considerably different from that of the other 3; and it revolves on its axis in $16^d\ 18^h\ 5^m.1$.

It will not be amiss to collect into one view, all the observations that relate to the colour of the satellites. The 1st is white; but sometimes more intensely so than at others. The 2d is white, bluish, and ash-coloured. The 3d is always white; but the colour is of different intensity, in different situations. The 4th is dusky, dingy, inclining to orange, reddish and ruddy at different times; and these tints may induce us to surmise that this satellite has a considerable atmosphere.

I shall conclude this paper with a result of the observation of the diameter of the 2d satellite, taken by its entrance on the disc of the planet, July 28, 1794, and marked in fig. 2, at 176°. The duration by the observation is fixed at 4 minutes; in which time it passes over an arch in its orbit of 16′ 52″.9. Now as its distance from the planet is to its distance from the earth, so is 16′ 52″.9 to the diameter of the satellite; or the mean distance of the 2d satellite may be rated, with M. de Lalande, at 2′ 57″, or 177″. Then putting this equal to radius, we shall have the following analogy: Radius is to 177″, as the tangent of 16′ 52″.9 is to the angle, in seconds, which the diameter of the 2d satellite subtends when seen from the earth. And by calculation, this comes out 0″.87; that is less than $\frac{9}{10}$ of a second.

I have not been scrupulously accurate in this calculation, as the real distance of Jupiter at the time of observation should have been computed, whereas I have contented myself with the mean distance. Nor am I very confident that the angle of the greatest elongation, admitted to be 2′ 57″, is quite accurate; but I judged it unnecessary to be more particular, because the time of my observation in the beginning of the transit on the disc, I find was only taken down in whole minutes of the clock. The end however is more accurately determined, by the observation which was made 45^s after the immersion; when a part of the disc, equal to about $\frac{1}{4}$ of the diameter of the satellite, is said to be visible. It seems that observations of this kind, made with very good telescopes, charged with high powers, are capable of great precision. For the remark that a margin of Jupiter, equal to about $\frac{1}{4}$ of the diameter of the satellite, became visible in 45^s of time, adds great support to the accuracy of the observation of the foregoing 4 minutes; and at all events it is evidently proved, from the whole of the entrance on the disc, that the diameter of the satellite is less, by one half at least, than what from the result of the measures of former observers it has been supposed to be.

A method has also been used, of deducing the diameter of the satellites from the time they employ to immerge into the shadow of the planet; but this must be very fallacious, and ought not to be used.

I should not pass unnoticed the apparent magnitude of the satellites. The expressions that have been given of them may be collected into the following narrow compass: 1 , 4 , 2 4 ; 1 3 — , 4 ; 1 — 2 4 , 2 , 1 3 — — 4 ; 1 ; 2 1 , 4 , 2 3 — 2 , 1 , 4 3 — — 2 — 1 , 4 1 ÷ 24 . — 2 1 ⁝ 2 — 4 3 — — 1 , 2 2 — 1. From which we may conclude, that the 3d satellite is considerably larger than any of the rest; that the 1st is a little larger than the 2d, and nearly of the size of the 4th; and that the 2d is a little smaller than the 1st and 4th, or the smallest of them all.

XVI. Further Experiments and Observations on the Affections and Properties of Light. By Henry Brougham, Jun. Esq. p. 352.

I am first to unfold a new, and I think curious property of light, that may be indeed reckoned fourfold, as it holds, like the rest, equally with respect to refraction, reflexion, inflexion, and deflexion; thus preserving entire the same beautiful analogy in these 4 operations, which we have hitherto remarked. I shall then consider several phenomena connected either with this, or with the properties before described, and of which they afford some striking confirmations.

I. *Observation* 1. The sun shining strongly into my darkened chamber, I placed, at a small hole in the window-shutter, a prism with its refracting angle (of 65°) upwards, so that the spectrum was cast on a chart placed at right angles to the incident rays, and 4 feet from the prism. In the rays, parallel to the chart, and 2 feet from it, I placed a pin, whose diameter was $\frac{1}{30}$ of an inch, and fixed it so, that the axis of its shadow on the spectrum might be parallel to the sides of the spectrum. A set of images by reflexion was formed similar to those described for 1796, all inclining to the violet; but what I chiefly attended to at present was their shape. I had always observed that the part formed out of the red-making rays was broadest, and that the other parts diminished in breadth regularly towards the violet. I now delineated 1 or 2, at about 3 inches from the shadow; and though, from the pin's irregularities, the sides were by no means smooth, yet the general shape was in every pin, and with every prism used, nearly as represented in fig. 5, pl. 4, divided in the direction RA, according to the colours of the spectrum in which they were formed; ROBA was red, and the broadest; that is, RA was broader than OB, the confines of the red and orange; and GDEV was the violet, narrowest of all.

Observ. 2. Between the pin and the prism, $\frac{1}{10}$ of an inch from the pin, was placed a screen, through a small hole in which, of twice the pin's diameter, the rays of the spectrum passed, and were reflected into images by the pin; these were pretty distinct and well defined, when received on a chart $\frac{1}{2}$ a foot from the pin. They were oblong, having parallel sides and confused ends; they were wholly of the colour whose rays fell on the pin, unless when the white, mixed with those at the confines of the yellow and green, caused the images to be of all the colours. When the prism was turned round on its axis, so that different rays fell on the pin, the images changed their sizes as well as their positions; they were largest when red, and least when violet.

Observ. 3. In case it may be thought that the sides of the hole, through which the rays passed in observation 2, by inflecting, might dispose them, before incidence, into beams of different sizes, I removed the screen, and placed the pin horizontally, the axis of the shadow being now at right angles to that of the prismatic spectrum; and moving the prism on its axis, again I observed the contraction and dilatation of the images by reflexion, though now they were rather less distinct, from the greater size of the incident beam; and to show that there was both a change of size and of place, without any manner of deception, I placed one leg of a pair of compasses in a fixed point of the spectrum, and the other in the middle point of an image formed by the violet-making rays. The prism being then moved till the image became red, I again bisected it, and found its centre considerably beyond the point of the compasses, which was indeed evidently much nearer one end of the image than the other; besides, that the red image, when measured, was longer than the rest; and this satisfied me that there were 2 changes, one of place with respect to the fixed point, the other of size, with respect to the centre of the image. Lastly, as far as I could judge, the dilatation and contraction appeared even and uniform.

Observ. 4. I remarked that the fringes or images, by flexion, were always increased in size when formed out of red-making rays, and were less in every other colour, and least in violet, besides being moved farther from the edge of the shadow in the former rays than in the latter; and this agrees with an observation of Sir Isaac Newton, as far as he tried it, which was with respect to deflexion. In making several experiments with prisms, I hit on a very remarkable confirmation of this. I observed on each side of the spectrum 4 or 5 distinct fringes, like the images by reflexion, coloured in the order of the spectrum, but quite well defined at the edge, and even pretty distinct at the end; they were also much narrower than those images, but like them they inclined much to the violet, and were broadest in the red, becoming narrower by degrees, and narrowest of all in the violet. I moved the prism, and they disappeared; but when the prism was brought back to its former position, they also returned. I then observed the prism in open light, and saw that it had veins, chiefly opaque and white, running through it, and that there were several of these in the place where the light passed when the prism was held as before. But in case the inclination and shape of these images might be owing to the irregular order in which the veins were laid, I held another prism, which happened to have parallel veins; in many positions of this the fringes or images returned, not indeed always so regular nor always of the same kind; for some were confused and broader, formed, as I concluded from this and their position, by reflexion; others, made by transparent veins and air-bubbles, were also irregular, but inclined to the red, the violet being farthest from the perpendicular, and these were obviously caused by refraction; yet all agreed in this, that they were broadest in the red, and narrowest in the violet parts.

Observ. 5. I held, in the direct rays of the sun, at ½ an inch from the small hole in the window-shutter, a glass tube, free from scratches and opaque veins, but like most glass that is not finely wrought, having its surface of a structure somewhat fibrous. When this tube was slowly introduced into the light, and so held that none of the rays might be refracted, a streak, chiefly white, was seen, similar in shape and position to those described in the former paper. When narrowly inspected, it was found to contain many images by reflexion in it. But these were much diluted by the abundance of white light, reflected without decomposition in the manner above-mentioned. This streak lay wholly on one side of the tube; but I moved the tube onward a little, and another streak darted through the shadow, and extended all round on both sides: and now, when the tube was in the middle of the rays, there were 2 streaks on both sides, one a little separated from the other and continued through the shadow, the other on each side of the shadow; the former was evidently produced by refraction; it contained many images very like those by reflexion, only more vivid in the colours, which were all in the inverted order, the violet being outermost, and the rest nearest the point of incidence. Images similar to these are also producible on the retina, as mentioned before.

Observ. 6. I now placed a prism at the hole, and made the same images by refraction, out of homogeneal light. These inclined to the red, not, like images by reflexion, to the violet; but they were broadest in the red, and narrower towards the violet parts. In short, when viewed beside the images by reflexion, except in point of brightness and inclination, they differed from them in no respect. The first 3 experiments show, that when homogeneal light is reflected, some rays are constantly disposed into larger images than others are, that is, into images more distended in length, though of the same breadth. The 4th experiment shows, that the same takes place when light is inflected and deflected; and the last 2 show, that the same happens when the rays are refracted in a way similar, or analogous, to that in which the other images were produced by reflexion and flexion.

We are now to show, that this difference of size is not owing to the different reflexibilities and flexibilities of the rays. In order to this, we shall both demonstrate, and then prove by experience, "that inflexion and deflexion do not decompound heterogeneous rays, whose direction is such that they fall on the bending body." In fig. 6, let AB be the body; GH, EF, CD, the limits of its spheres of deflexion, inflexion, and reflexion, respectively; and let IP be a white ray of direct light entering at P the sphere of deflexion: through P draw LK at right angles to GH; IP will be separated into PR red, and PV violet, and the 5 other colorific rays according to their deflexibilities; at R and V draw the perpendiculars ST and QO; then the alternate angles PRT, RPL; and PVQ, VPL, are equal each to each. But TRP and QVP are the angles of incidence, at which the red and violet enter the sphere of inflexion: and RPL, VPL are the angles of deflexion of the red and the violet; therefore the difference of the latter 2, that is RPV, is likewise the difference of the 2 former. Suppose this difference equal to nothing; or that PV and PR are parallel; then rRS, the angle of the red's inflexion, will be less than vVO, the angle of the violet's inflexion, by the angle RPV: when not evanescent, add RPV to rRS; then rRS will be equal to vVO: that is, the divergence will be destroyed, and the rays enter the sphere of reflexion, parallel and undecompounded. It is evident therefore, that the effect arising from the different deflexibilities of the rays is destroyed by the equal and opposite effect produced by their different inflexibilities; and the same thing may in like manner be shown to happen in the return of the rays from the body after reflexion. But let the rays be so reflected, that they shall pass by the body without entering any more than one sphere of flexion; then they will be separated by their flexibilities, as we before described. It appears then, that if the rays of light were not differently reflexible, flexion could never produce the coloured images, by separating the compound light. And indeed this may be easily proved by fact. At 144 feet from the bending body, the greatest fringes by flexion are only half an inch in length, whereas the 4th or 5th images by reflexion are above half an inch at 1 foot from the reflecting surface: the one sort is therefore more than 144 times more distended than the other, whereas the flexion could, at the very farthest, only double them. Also the distinctness, and brightness, and regularity of the colouring, are quite different in the 2 cases; the supposed cause would neither account for the order of the colours, nor for their absence in common specular reflexion, and refraction through two prisms joined together with their angles the contrary ways. Lastly, if we suppose the images to be produced by flexion, and then reflected from the body, it would follow, that light incident on a prism should be decompounded, formed into several coloured images, and then refracted, the violet being least and the red most bent; all which is perfectly the reverse of what actually happens. I have multiplied the proof of this proposition perhaps beyond what is necessary; but its great importance to the whole theory I hope will plead my excuse.

Let us now suppose that a homogeneal beam passes through the spheres of flexion: it will follow that no divergence can take place from the bending power of the body; so that we have only to estimate the effect produced by the reflexion, and to inquire whether the different reflexibilities of the rays can cause the images to vary their sizes according as they are formed by different rays. In fig. 7, let AB be the body, CD the limit of its sphere of reflexion, and IP a beam of homogeneal rays, as red, incident at P and reflected to R, forming there the image Rr. It is evident that the greater reflexibility of the rays IP can only alter the position of the centre of Rr, making it nearer the perpendicular than the centre of an image formed by any other rays would be. But the greater length of Rr shows that a greater quantity of rays is reflected, or that the same quantity is spread over a greater space, and that in the following way. Let IFfi be a beam of violet-making rays entering ABCD, and reflected so as to form the image RV. The force exerted by AB decreasing according to some law (of which we are as yet ignorant) as the distance increases, is not sufficient to turn the rays back till they have come a certain length within ABCD. But for the same reason it turns back all that it does reflect before they come nearer than a certain distance; between these 2 limits therefore the rays are turned back. But the limits are not the same to all the rays; some begin to be turned at a greater distance from the body than others, and consequently are reflected to a greater distance from the middle ray of the incident beam. Thus if IFfi be changed to a red-making beam, it begins to be turned back at f. and the rays farthest from AB are reflected to r instead of to v, where they fell when IFfi was violet-making; not but that the same quantity of rays is reflected, the only difference is, that the most reflexible are reflected farthest from the body by their greater reflexibility, and farthest from each other by this other property. Exactly the same happens in the case of refraction, mutatis mutandis; but there seems to be a slight variation in the manner in which the different rays are disposed into images of different sizes by flexion. In this case also the bending body's action reaches farther when exerted on some rays than when exerted on others: but then, the direction of the rays not passing through the body, those which are farthest off and at too great a distance to be bent, never coming nearer, are not bent at all; and consequently as the least flexible rays are in this predicament at the smallest distance, and the most flexible not till the distance is greater, the images formed out of the former must be less than those formed out of the latter. This difference in the way in which the phenomenon appears, does not argue the smallest difference in the cause: it only follows from the different position of the rays, with respect to the acting body, in the 2 cases. I infer then from the whole, that different sorts of rays come within the spheres of flexion, reflexion, and refraction, at different distances, and that the actions of bodies extend farthest when exerted on the most flexible. It may perhaps be consistent with accuracy and convenience to give a name to this property of light; we may therefore say, that the rays of light differ in degree of refrangity, reflexity, and flexity, comprehending inflexity and deflexity. From these terms (uncouth as, like all new words, they at first appear) no confusion can arise, if we always remember that they allude to the degree of distance to which the rays are subject to the action of bodies. I shall only add an illustration of this property, which may tend to convey a clearer idea of its nature. Suppose a magnet to be placed so that it may attract from their course a stream of iron particles, and let this stream pass at such a distance that part of it may not be affected at all; those particles which are attracted may be conceived to strike on a white body placed beyond the magnet, and to make a mark there of a size proportional to their number. Let now another equal stream considerably adulterated by carbonaceous matter, oxygene, &c. pass by at the same distance, and in the same direction. Part of this will also be attracted, but not so far from its course, nor will an equal number be affected at all; so that the mark made on the white body will be nearer the direction of the stream, and of less size than that made by the pure iron. It matters not whether all this would actually happen, even allowing we could place the subjects in the situation described; the thing may easily be conceived, and affords a good enough illustration of what happens in the case of light.

Pursuant to the plan I before followed, I now tried to measure the different degrees of reflexity, &c. of the different rays; but though the measurements taken agreed in this, that the red images were much larger than the rest, and the green appeared by them of a middle size, yet they did not agree well enough (from the roughness of the images, and several other causes of error), to authorize us to conclude with any certainty "that the action of bodies on the rays is in proportion to the relative sizes of these rays." This however will most probably be afterwards found to be the case; in the mean time there is little doubt that the sizes are the cause of the fact.

II. Several phenomena are easily explicable on the principles just now laid down. 1. If a pin, hair, thread, &c. be held in the rays of the sun refracted through a prism, extending through all the 7 colours, a very singular deception takes place: the body appears of different sizes, being largest in the red and decreasing gradually towards the violet. This appearance seemed so extraordinary, that some friends who happened to see it as well as myself, suspected the body must be irregular in its shape. On inverting it however, the same thing took place; and on turning the prism on its axis, so that the different rays successively fell on the same parts, the visible magnitude of the body varied with the rays that illuminated it. The appearance is readily accounted for by the different reflexity of the rays, and follows immediately from obs. 2 and 3.

2. Sir Isaac Newton found that the rings of colours made by thin plates and by thick plates of glass, as he calls them, when formed of homogeneal light, varied in size with the rays that made them, being largest in the most flexible rays. I have

had the pleasure of observing several other sorts of rings, so extremely similar, and formed by flexion, that I can no longer doubt of this being also the cause of the phenomena observed by Newton. I shall first describe a species, to prove "that the colours by thick and thin plates are one and the same phenomenon, only differing in the thickness of the plates." Happening to look by candle-light on a round concave plate of brass, pretty well polished, so as to reflect light enough for showing an image of the candle, I was surprized to see that image surrounded by several waves of colours, red, green, and blue, disposed in pretty regular order. This was so uncommon in a metallic speculum, that I examined the thing very minutely by a variety of experiments; these I shall not particularly now describe, but give a general idea of their results.

It must be observed, for the sake of clearness, that in the following inquiries concerning the formation of rings or fringes, the diameter of a ring or fringe means the line passing through the centre of that ring, and terminated at both ends by the circumference; whereas the breadth means that part of the diameter intercepted between the limits of the ring, or the distance between its extreme colours, red and violet. In the 1st place, they were formed by the sun's light in the figure of rings surrounding the centre of the sphere to which the plate was ground, at greater distances increasing their breadths, the colours pretty bright, though inferior in brilliancy to those of concave specula. 2dly, The order of the colours was in all red outermost, and violet or blue innermost, with a greyish-blue spot in the common centre of the whole; and on moving the plate from the perpendicular position, the rings moved and broke exactly like those of specula. In the 3d place, homogeneal light made them of simple colours; they were broadest when red, narrowest when blue and violet. 4thly, They decreased in breadth from the centre; and I found, by a simple contrivance, that they were to each other in the very same ratio that the rays by specula follow. In the 5th place, I compared the general appearance of the 2 sorts by viewing them at the same time, and was struck with their general appearance, unless that these of specula were most vivid and distinct.

These things made me suspect that they were actually caused by the thin coat of gums with which the surface of the plate was varnished, called lacker. Accordingly I took it off with spirit of wine, and found the rings disappear; on lackering it again they returned; and in like manner I caused a well finished concave metal speculum to form the rings here spoken of, by giving it a thin coat of lacker. This is a clear proof that these rings were exactly the same with those of thick plates, to use Newton's expression, for the coat of gums is, when thin, pretty transparent, as may be seen by laying one on glass plates. But this coat is extremely thin, and cannot exceed the 200th part of an inch; so that the colours of thick plates are in fact the very same with those of thin plates, except that the 2 kinds are made by different sized plates. We cannot therefore distinguish them, any more than we do the spectrum made by a prism whose angle is 90° from that made by one whose angle is 20°. This kind of colours is not the only one I have observed of nearly the same kind with those of plates; we shall presently see another much more curious and remarkable.

III. In reflecting on the observations and conclusions contained in my former paper, several consequences seemed to follow, which appeared so new and uncommon, that I began to doubt a little the truth of the premises; but at any rate was resolved to examine more minutely how far these inferences might be consistent with fact: and I am happy in being able to announce the completeness of that consistency, even beyond my expectations. The chief consequences were the following. 1. That a speculum should produce, by flexion and reflexion, colours in its reflected light wherever it has the least scratch or imperfection on its surface. 2. That on great inclinations to the incident rays all specula, however pure and highly polished, should produce colours by flexion. 3. That they should also in the same case produce colours by reflexion. 4. That lenses, having the smallest imperfections, should produce by flexion colours in their refracted light. 5. That there should be many more than 3, or even 4 fringes by flexion, invisible to the naked eye. And, 6. That Iceland crystal should have some peculiarities with respect to flexion and reflexion; or if not, that some information should be acquired concerning its singular properties respecting refraction.

The manner in which the first of these propositions is demonstrated a priori, is evident from fig. 8, where CD is the reflecting surface, VO a concavity bearing a small ratio to CD, AO and AB rays proceeding to CD. The one, AB, will be separated into BR red, and BV violet, by deflexion from O, and will be reflected to r′v′, forming there the fringes. The other, AO, being reflected, will be separated into BX and BY, by deflexion from V, forming other fringes, XY, on the side of VO's shadow opposite to r′v′. Also when VO is convex, instead of concave, the like fringes will be produced by the rays being deflected in passing by its sides. Lastly, when VO is a polished streak, images by reflexion will be produced, as described in my former paper for 1796. The same passage will also show the reason why, on great inclinations, colours by reflexion should be produced. And the 2d proposition, with respect to flexion, follows from what has been demonstrated in this paper, it being that case where the rays either leave or fall on the speculum at such an inclination, as to come only within the sphere of inflexion, without being deflected. The 4th proposition is merely a simple case of flexion. And the last 2 require no illustration. I shall now relate how I inquired into the truth of these things a posteriori.

Observ. 1. Looking at a plane glass mirror exposed to the sun's light, I observed that up and down its surface there were minute scratches, called hairs by workmen, and that each of these reflected a bright colour, some red, others green, and others blue. On moving the mirror to a different inclination, or my eye to a different position with respect to the mirror, I saw the species of the colours change; the red, for instance, became green, and the green blue. I applied my eye close to the mirror, and received on it the light reflected from one hair. I observed several distinct images of the sun much distended and regularly coloured, just like those described above; the same appearances were observable in all specula, metal and glass, which had these hairs, and I never saw any metal one without some: their size is exceedingly small, not above $\frac{1}{1000}$ of an inch. Rubbing a minute particle of grease on the surface of the speculum, images were seen on the fibrous surface; and they always lay at right angles to that direction in which the grease was disposed by drawing the hand along it.

Observ. 2. Besides these polished hairs, many specula have fewer or more small specks and threads, rough and black. Perhaps every polished surface is studded with a number of small ones, invisible to the naked eye from the quantity of regular light which it reflects. I took, from a reflecting telescope, a small concave speculum not very well finished; its surface showed several specks to the naked eye, and many with a microscope. Its diameter was $\frac{37}{50}$ of an inch, its focal distance 2 inches, and the sphere to which it was ground 8 inches diameter. I placed it at right angles to the sun's rays, coming through a small hole $\frac{1}{42}$ of an inch diameter, into a very well darkened room; I then moved it vertically, so that the rays might be reflected to a chart 12 inches from the speculum, and consequently 10 from the focus: and though the focus appeared white and bright, yet on the chart the broad image was very different. It was mottled with a vast number of dark spots: these were of 2 sorts chiefly, circular and oblong. Of the former a considerable number were distinct and large, the rest smaller and more confused, but so numerous that they seemed to fill the whole image. None were quite black, but rather of a bluish grey, and the oblong ones had a line of faint light in the middle, just as is the case in shadows of small bodies. But the chief thing I remarked was the colours. Each oblong and round spot was bordered by a gleam of white, and several coloured fringes separated by small dark spaces. The fringes were exactly like those surrounding the shadows of bodies, of the same shape with the dark space, having the colours in the order, red on the outside, blue or violet in the inside; the innermost fringe was broadest, the others decreasing in order from the first. I could sometimes see 4 of them, and when made at the edge of the large image, I could indistinctly discern the lineaments of a 5th: when 2 of the spots were very near each other, their rings or fringes ran into each other, crossing.

Observ. 3. When the chart was removed to a greater distance, as 6 feet, the fringes were very distinct and large in proportion; also the smaller spots became more plain, and their rings were seen, though confusedly, from mixing with each other. When the speculum was turned round horizontally, so that its inclination to the incident rays might be greater, the distance of the chart remaining the same, by being drawn round in a circle, the spots and fringes evidently were distended in breadth. I have endeavoured to exhibit the sun's image, as mottled with fringes or rings and spots, in fig 9.

Observ. 4. I placed the speculum behind a screen with a hole in it, through which were let pass the homogeneal rays of the sun, separated by refraction through a prism; this being turned on its axis, the rays which fell on the speculum were changed; the fringes were now of that colour whose rays fell, and when the rays shifted, the fringes contracted or dilated, being broadest in the most flexible rays, and consequently in those whose flexity is greatest.

Observ. 5. The direct light falling on the speculum, and part of the reflected light on the horizontal white stage of a very accurate micrometer, I measured the breadth of the fringes, spots, &c. These, with the distance of the speculum from the window and micrometer, and the size of the sun's image, are set down in the following table, all reduced to inches and decimals.

	Inches.		Parts.
Distance of the speculum from the hole in the window shutter	.24	Breadth of the oblong dark spot	.0074
		Breadth of its first fringe	.0022
Distance of the speculum from the stage of the micrometer	18	Elliptic spot's transverse axis	.0116
		conjugate axis	.0068
Transverse axis of the sun's image	2.6	Breadth of its first fringe	.0034
Conjugate axis of the sun's image	1.4	Transverse axis of a larger elliptic spot	.013
Length of the oblong dark spot	.4	Conjugate axis of the same spot	.0076

In the image where these measures were taken, there were 7 other elliptic spots, a little less and nearly equal; all the others were much smaller and more confused.

Observ. 6. On viewing the surface of the speculum attentively in that place whence the rays formed the oblong and first mentioned elliptic spots, I saw a dark but very thin long scratch, and a dark dent, similar in shape to the dark spaces on the image; the dark spot measured less than $\frac{1}{350}$ of an inch; which makes its whole surface to the whole polished surface as 1 to 34225, supposing the former circular or nearly so. All these measures will be found to agree very well, for their smallness and delicacy: thus, the ratio last mentioned is nearly the same which we obtain by comparing the image and the spot; the like may be said of the two spots mentioned in the table, i. e. their axes are proportional. I now could produce what spots I pleased, by gently scratching the speculum, or by making lines, dots, &c. with ink, and allowing it to dry; for these last formed convex fibres, which produced coloured fringes as well as the concavities, agreeably to what was deduced a priori.

Observ. 7. The whole appearance which I have been describing bore such a close and complete resemblance to the fringes made round the shadows of bodies, that the identity of the cause in both cases could not be doubted. In order however to show it still further, I measured the breadths of 2 contiguous fringes in several different sets; the measurements agreed very well, and gave the breadth of the first fringe .0056, and of the 2d .0034; or of the first .0066, and of the 2d .0034. The ratio of the breadths by the first is 28 to 17; by the 2d 30 to 17; of which the medium is 29 to 17, and this is precisely the ratio of the 2 innermost fringes made by a hair, according to Sir Isaac Newton's measurement: the first being, according to him, $\frac{1}{170}$ of an inch; the 2d $\frac{1}{290}$ of an inch*. Further, the 2 in-

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* Optics, Book 3. Obs. 3.—Orig.

nermost rings made by plates have their diameters (not breadths) in the ratio of $1\frac{11}{16}$ to $2\frac{3}{8}$*, and the distance between the middle of the innermost fringes, made by a hair, on either side the shadow, is to the same distance in the 2d fringes, as $\frac{1}{38}$ to $\frac{2}{47}$; therefore the diameters of the first 2 rings made by the specks in the speculum, are as $\frac{209}{663}$ to $\frac{627}{1363}$; which ratio differs exceedingly little from that of $1\frac{11}{16}$ to $2\frac{3}{8}$, the ratio of the diameters of rings made by plates, either those called by Newton thick, or those which he names thin: for suppose this difference nothing, $2\frac{3}{8} \times \frac{209}{663} = 1\frac{11}{16} \times \frac{627}{1363}$; and the difference between these 2 products, now stated equal, is not much above $\frac{8}{9}$ in reality.

Observ. 8. The last thing worth mentioning in these phenomena was this: I viewed the fringes through a prism, holding the refracting angle upwards, and the axes parallel to that of the dark space; then moving it till the objects ceased descending, I saw in that posture the fringes much more distinct and numerous; for I could now see 5 with ease, and several more less distinctly. This led me to try more minutely the truth of the 5th proposition, with respect to the number of the fringes surrounding the shadows of bodies in direct light. Having produced a bright set of these by a blackened pin $\frac{1}{25}$ of an inch in diameter, I viewed them through a well made prism, whose refracting angle was only 30°, and held this angle upwards, when the fringes were on the side of the shadow opposite to me; I then moved the prism round on its axis, and when it was in the posture between the ascent and descent of the objects, I was much pleased to see 5 fringes plainly, and a great number beyond, decreasing in size and brightness till they became too small and confused for sight. In like manner those formed by a double flexion of 2 bodies, and those made out of homogeneal light, were seen to a much greater number when carefully viewed through the prism. And this experiment I also tried with all the species of fringes by flexion which I could think of.

Observ. 9. The same appearances which were occasioned by the metal speculum, might be naturally expected to appear when a glass one was used. But I also found the like rings or fringes of colours and spots in the image beyond the focus of a lens; nor was a very excellent one belonging to a Dollond's telescope free from them. The rings with their dark intervals resembled those floating specks so often observed on the surface of the eye, and called "muscæ volitantes," only that the muscæ are transparent in the middle, because formed by drops of humor: they will however be found to be compassed by rings of faint colours, which will become exceedingly vivid if the eyes be shut and slowly opened in the sun's light, so that the humour may be collected; they also appear by reflexion, mixed with the colours described in Phil. Trans. for 1796.

Observ. 10. The sun shining strongly on the concave metal speculum, placed at such a distance from the hole in the window that it was wholly covered with the light; on inclining it a little, the image on the chart was bordered on the inside with 3 fringes similar to those already described; on increasing the inclination these

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* Book 2. Parts 1 and 4.—Orig.

were distended, becoming very bright and beautiful; when the inclination was great, and when it was still increased, another set of colours emerged from the side next the speculum, and was concave to that side. Here I stopped the motion, and the image on both sides of the focus had 3 sets of fringes, and 4 fringes in each set; but when viewed through a prism, as before described, the numbers greatly increased, both the fringes and the dark intervals decreasing regularly. The appearance to the naked eye is represented in fig. 10, where ADC being the image, A and C are the sets of fringes at the edges, and B the 3d set, there being none at E and D the sides, since the light which illuminates these quarters comes not from the edges of the speculum in so great inclinations. I now viewed the surface of the speculum, and saw it, in the place answering to B in the image, covered with fringes exactly corresponding with those at B; and on changing the figure of that part of the speculum's edge between them and the sun, the fringes likewise had their figure altered in the very same way. On moving the speculum farther round, B came nearer to A in the image, according as the fringes on the speculum receded from that side which formed them; and before they vanished alike from the speculum and image, they mixed with the colours at A in the image, and formed in their motion a variety of new and beautiful compound colours: among these I particularly remarked a brown chocolate colour, and various other shades and tinges of brown and purple. Just before the fringes at B appeared, the space between A and C was filled with colours by reflexion, totally different in appearance from the fringes; but I could not examine them so minutely as I wished in this broad image, I therefore made the following experiment.

Observ. 11. At the hole in the window-shutter I held the speculum, and moved it to such an inclination that the colours by reflexion might be formed in the image; they were much brighter and far more distended than the fringes, and were in every respect like the images by reflexion in the common way, only that the colours were a little better and more regular. They were also seen on the speculum as the 3d set of fringes had before been in observ. 10; but by letting the rays fall on the half next the chart, and inclining that half very much, I could produce them, though less distinctly, by a single reflexion. I now held a plain metal speculum so, that the rays might be reflected to form a white image on a chart: On inclining the speculum much, I saw the image turn red at the edge; it then became a little distended; and lastly, fringes emerged from it well coloured, and in regular order, with their dark intervals. This may easily be tried by candle-light with a piece of looking-glass, and those who without much trouble would satisfy themselves of the truth of the whole experiment contained in this and the last observation, may easily do it in this way with a concave speculum; but the beauty of the appearance is hereby quite impaired. After this detail it is almost superfluous to add, that the fringes at B, fig. 6, are formed by deflexion from the edge of the speculum next the sun, and then falling on it are reflected to the chart; that the images by reflexion are either formed by the light being decompounded at its first reflexion, and then undergoing a second, or, in other instances, without this second reflexion; and that the other fringes are produced exactly as described above, from the necessary consequences of the theory. I shall only add, that nothing could have been more pleasing to me than the success of this experiment; not only because in itself it was really beautiful from its variety, but also because it was the most peremptory confirmation of what followed from the theory a priori, and in that point where the singularity of its consequences most inclined me to doubt its truth.

Let us now attend to several conclusions to which the foregoing observations lead, independently of the propositions, viz. the first 5, which they were made to examine.

1. We must be immediately struck with the extreme resemblance between the rings surrounding the black spots on the image made by an ill polished speculum, and those produced by thin plates observed by Newton; but perhaps the resemblance is still more conspicuous in the colours surrounding the image made by any speculum whatever, and fully described in observ. 10 and 11. The only difference in the circumstances is now to be reconciled. The rings surrounding the black spot on the top of a bubble of water, and those also surrounding the spot between 2 object glasses *, have dark intervals, exactly like those rings I have just now described, and the fringes surrounding the shadows of bodies; but these intervals transmit other fringes of the same nature, though with colours in the reverse order; from which Sir Isaac Newton justly inferred, that at one thickness of a plate the rays were transmitted in rings, and at another reflected in like rings. Now it is evident, that neither reflexibility nor refrangibility will account for either sort of rings, because the plate is far too thin for separating the rays by the latter, and because the colours are in the wrong order for the former; and also because the whole appearance is totally unlike any that refrangibility and reflexibility ever produce. To say that they are formed by the thickness of the plates, is not explaining the thing at all. It is demanded in what way? and indeed we see the like dark intervals and the same fringes formed at a distance from bodies by flexion, where there is no plate through which the rays pass. The state of the case then seems to be this: "when a phenomenon is produced in a particular combination of circumstances, and the same phenomenon is also produced in another combination, where some of the circumstances, before present, are wanting; we are intitled to conclude that the latter is the most general case, and must try to resolve the other into it." In the first place, the order of the colours in the Newtonian rings is just such as flexion would produce; that is, those which are transmitted have the red innermost, those which are reflected have the red outermost; the former are the colours arranged as they would be by inflexion, the latter as they would be by deflexion; and here by outermost and innermost must be understood relative position only, or position with respect to the thickness of the plate, not of the central spot. 2dly,

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* Optics, Book 11. P. 1.—Orig.

the thinnest plate makes the broadest ring, the diameter of the rings being in the inverse subduplicate ratio of the plate's thickness; just so is it with fringes by flexion; nearer the body the fringes are broadest, and their diameters increase in the same ratio with the diameters of the rings by plates whose thickness is uniform; each distance from the bending body therefore corresponds with a ring or fringe of a particular breadth, and the alternate distances correspond with the dark intervals: the question then is, what becomes of the light which falls on or passes at these alternate distances? In the case of thin plates, this light is transmitted in other rings; we should therefore be led to think that in the case of the light passing by bodies, it should be at one distance inflected, and at another deflected; and in fact the phenomena agree with this, for fringes are formed by inflexion within the shadows of bodies; they are separated by dark intervals; the fringes and the intervals without the shadow decrease in breadth according to the same law; so that the fringes and intervals within the shadow correspond with the intervals and fringes without, respectively. Nor will this explanation at all affect the theory formerly laid down; it will only, if found consistent with further induction, change the definite spheres of inflexion and deflexion into alternate spheres. At any rate, the facts here being the same with those described by Newton, but in different circumstances, teach us to reconcile the difference, which we have attempted to do, as far as is consistent with strictness; and what we have seen not only entitles us to conclude that the cause is the same, but also inclines us to look for further light concerning that cause's general operation: and I trust some experiments which I have planned, with an instrument contrived for the purpose of investigating the ratio of the bending power to the distances at which it acts, will finally settle this point.

II. Another conclusion follows from the experiments now related, viz. that we see the great importance of having specula for reflectors delicately polished; not only because the more dark imperfections there are on the surface, the more light is lost, and the more colours are produced by flexion (these colours would be mostly mixed and form white in the focus), but also because the smallest scratches or hairs, being polished, produce colours by reflexion, and these, diverging irregularly from the point of incidence, are never collected into a focus, but tend to confuse the image. Indeed it is wonderful that reflectors do not suffer more from this cause, considering the almost impossibility of avoiding the hairs we speak of: however, that they do actually suffer is proved by experience. I have tried several specula from reflecting telescopes, and found that though they performed very well, from having a good figure, yet from the focus, when they were held in the sun's light, several streaks diverged, and were never corrected; others had the hairs so small, that it was very difficult to perceive the colours produced by them, unless they fell on the eye. Glass concaves were freer from these hairs, but they were much more hurt by dark spots, &c. In general, the hairs are so small in well wrought metals, that they do little hurt; but when enlarged by any length of exposure to the light and heat in solar observations, they produce irregularities round the image. Such at least I take to be the explanation of the phenomenon, observed at Paris by M. de Barros during the transit of Mercury in 1743, and recorded in Phil. Trans. for 1753. But there is another more serious impediment to the performance of reflectors, and which it is to be feared we have no means of removing. In making the experiments of which the history has been given, on viewing attentively the surface of the speculum, every part of it was seen covered with points of colours, formed by reflexion from the small particles of the body. I never saw a speculum free in the least from these, so that the image formed in the focus must be rendered much more dim and confused by them, than it otherwise would be.

III. The last conclusion which may be drawn from these experiments, is a very clear demonstration in confirmation of what was otherwise shown, concerning the difference between coloured images produced by reflexion, and those made by flexion. This complete diversity is most evident in the experiments with specula, the colours produced by which, in the form of fringes and rings, ought, as well as the others described as images by reflexion in obs. 11, to be the same in appearance with those formed by pins; whereas no 2 things can be more dissimilar. It remains to examine the 6th proposition: for this purpose I made the following observations.

Observ. 1. Having procured a good specimen of Iceland crystal, I split it into several pieces, and chose one whose surface was best polished. I exposed this to a small cone of the sun's light, and received the reflected rays on a chart: nothing was observable in the image, more than what happens in reflexion from any other polished body. Some pieces indeed doubled and tripled the image, but only such as were rough on the surface, and consequently presented several surfaces to the rays: when smooth and well polished, a single image was all that they formed. The same happened when I viewed a candle, the letters of a book, &c. by reflexion from the Iceland crystal.

Observ. 2. I ground a small piece of Iceland crystal round at the edge, and gave it a tolerable polish here and there by rubbing it on looking-glass, and sometimes by a burnisher (it would have been next to impossible to polish it completely). I then placed the polished part in the rays near the hole in the window-shutter, and saw the chart illuminated with a great variety of colours by reflexion, irregularly scattered, as described above *; I therefore held the edge in the smoke of a candle and blackened it all over, then rubbed off a very little of the soot, and exposed it again in the rays. I now got a pretty good streak of images by reflexion, in no respect differing from those made in the common way. Nor could I ever produce a double set, or a single set of double images, by any specimen properly prepared, either on a chart by the rays of the sun, or on my eye by those of a candle.

Observ. 3. I ground to an even and pretty sharp edge 2 pieces of Iceland crystal, and placed one in the sun's rays. At some feet distance I viewed the fringes with which its shadow was surrounded, and saw the usual number in the usual order.

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* Phil. Trans. for 1796.—Orig.

I then applied the other edge so near that their spheres of flexion might interfere in the manner before described *, and thus the fringes might be distended: still no uncommon appearance took place; nor when other bodies were used with one edge of crystal, nor when polished pieces of different shapes and sizes were employed. The same things happened by candle-light, and by reflected homogeneal light. In short, I repeated most of my experiments on flexion with Iceland crystal, and found that they were not changed at all in their results.

Observ. 4. Having great reason to doubt the accuracy of an experiment tried by Mr. Martin, and in which, by a prism of Iceland crystal, he thought 6 spectra were produced, I was not much surprized to find, that a prism made by polishing the 2 contiguous sides of a parallelopiped of Iceland crystal produced only 2 equal and parallel images, in whatever position the prism was held. But though, from the imperfect account which Martin gives of this appearance, it was impossible to discover his error from his own words, yet chance led me to find out what most probably had misled him; for looking at a candle through the opposite sides of a specimen of Iceland crystal, I saw 4 coloured images, besides 2 white ones, of the candle. These were parallel to one another, and in the same line, as represented in fig. 11, where E represents the two regular images, G and F two others coloured very irregularly, and changing colours as the crystal was moved horizontally, sometimes appearing each two-fold, and its 2 parts of the same or different colours. A and B were regularly coloured, and evidently formed by refraction, and reflected back from the sides. On turning the crystal round, so that its position might be at right angles to its former position, the images moved round, and were in a line perpendicular to AB, as CD. All this happened in like manner in the sun's rays; and on viewing the specimen, I found it was split and broken in the inside, so as to be lamellated in directions parallel, or nearly so, to the sides; on these plates there were colours in the day-time by the light of the clouds: and it is evident that it was these fractures which caused the irregular images G and F, for other specimens showed no such appearance. I would therefore conclude, that Iceland crystal separates the rays of light into 2 equal and similar beams by refraction, and no more †.

As to the cause of the separation, I would hope that some information may be obtained from the experiments I have related: for from them it appears, that this singular property extends no farther than to the action of the particles of Iceland

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* Phil. Trans. for 1796, page 256.—Orig.

† Mentioning this account of Martin's mistake to Professor Robison, of this university, I was pleased to find a full confirmation of it. It was that excellent philosopher who showed the appearance to Martin; but he not understanding it, took the liberty of publishing the observation as his own, after first mangling it in such a way as to give him indeed some pretext for the appropriation. The Professor merely mentioned his having communicated it to Mr. Martin; how the latter used it we have shown in the text: the theory of the appearance is somewhat more complex than appears by my observations. I was therefore pleased to find that the Professor was in possession of the true account of it; which is however foreign to the present purpose.—Orig.

crystal on the particles of light in their passage through the body; and from obs. 4 it is further evident, that it is not owing to the different properties which Sir Isaac Newton conjectures the different sides of rays to have; for if this were the cause, when the rays pass between 2 pieces of crystal, an [illegible] would take place. Lastly, another fact, mis-stated by Bartolin * and Romé de Lisle †, shows, that the unusual refraction takes place within the body, while the other, like all refractions, begins at some small distance before the rays enter.

The writers just now quoted assert, that if the crystal be turned round so as to assume different positions, there is one in which the line appears single. The fact is very different, as follows. When the crystal is turned round, the unusual image moves round also, and appears above the other; the greatest distance between the 2 images is when they are parallel to the line bisecting one of the acute angles of the parallelogram through which the rays pass; when the images are parallel to a line bisecting one of the obtuse angles, they seem to coincide; but they will be found, if observed more nearly, to coincide only in part. Thus, in fig. 13, AB and CD are the 2 black lines at their greater distance, and their extremities A and C, B and D are even with each other; that is, the figure formed by joining A and C, B and D, is a rectangle. But in the other case, fig. 12, AB and CD being the lines, the space CB, equal in depth of colour to the real line on the paper, is the only place in which the lines or images coincide. The space AC of AB, and BD of CD are still of a light colour, and the 2 lines AB and CD do not coincide, by the difference AC or BD; that is, by the difference OP, the greatest distance; fig. 13. In short, the unusual line's extremities describe circles, in the motion of the crystal, whose centres are the extremities of the usual line, and whose radii are the greatest distance. From this it appears evident, that the unusual image is formed within the crystal, and turns round with the side of the particle, or rhomboidal mass of particles, which forms it. Further, it is evident that the power which produces the division of the incident light, is very different from common refraction, from the motion, and the effect taking place when the rays are perpendicular. Suspecting therefore, that it might be owing to flexion, I made the following experiment, which undeceived me.

Observ. 5. I covered one side of a specimen of Iceland crystal, 3 inches deep, with black paper, all but a small space $\frac{1}{30}$ of an inch in diameter, and placed a screen with a hole of the same size, 6 feet from the hole in the window-shutter of my darkened chamber, so that the rays might pass through the screen, and fall on a prism placed behind, to refract them into a small and well defined spectrum, which was received on a chart 2 feet from the prism. This spectrum I viewed through the crystal, and of course saw it doubled; but the 2 images were by no means parallel; the unusual one inclined to the red, and its violet was considerably farther removed from the violet of the other, than the 2 reds were from each other: which shows that the most refrangible or least flexible rays were farthest

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* Experimenta Crystalli.—Orig.

† Cristallographie, vol. i.—Orig.

moved from their course by the unusual action, and proves this to be very different from flexion *.

From all these observations this conclusion follows: that the remarkable phenomenon in question arises from an action very different from either refraction or flexion; and whose nature well deserves to be further considered. It may possibly belong to the particles of Iceland crystal, and in a degree to those of rock crystal, from the form and angles of the rhomboidal masses, of which these bodies are composed. Nor is this conjecture at all disproved by the fact, that glass shaped like these bodies wants the property; for we cannot mould the particles of glass, we can only shape large masses of these; whereas we cannot doubt that in crystallization the smallest masses assume the same form with the largest: but then other hypotheses may perhaps also account for the fact, such as atmospheres, electric fluid, &c. &c.; so that till further observations are made, we ought to rest contented with barely suggesting the query. In the mean time, reserving to a future opportunity some inquiries concerning the chemical properties of light, and the nature of the forces which bodies exert on it internally, I conclude at present with a short summary of propositions. But first, may I be permitted to express a hope, that what has been already attempted may prove acceptable to such as love to admire the beautiful regularity of nature, or more particularly to trace her operations, as exhibited in one of the most pleasing, most important, and most unerring walks of physical science.

Prop. 1. The sun's light consists of parts which differ in degree of refrangity, reflexity, inflexity, and deflexity; and the rays which are most flexible have also the greatest refrangity, reflexity, and flexity; or are most refrangile, reflexile, and flexile.—*Prop.* 2. Rays of compound light passing through the spheres of flexion and falling on the bending body, are not separated by their flexibility, either in their approach to, or return from the body.—*Prop.* 3. The colours of thin and those of thick plates are precisely of the same nature; differing only in the thickness of the plate which forms them.—*Prop.* 4. The colours of plates are caused by flexion, and may be produced without any transmission whatever.—*Prop.* 5. All the consequences deducible from the theory a priori are found to follow in fact.—*Prop.* 6. The common fringes by flexion, called hitherto the "3 fringes," are found to be as numerous as the others.—*Prop.* 7. The unusual image by Iceland crystal is caused by some power inherent in its particles, different from refraction, reflexion, and flexion.—*Prop.* 8. This power resembles refraction in its degree of action on different rays; but it resembles flexion within the body, in not taking place at a distance from it; in acting as well on perpendicular as on oblique rays; and in its sphere or space of exertion moving with the particles which it attends.

XVII. On Gouty and Urinary Concretions. By W. Hyde Wollaston, M. D., F. R. S. p. 386.

If in any case a chemical knowledge of the effects of diseases will assist us in the cure of them, in none does it seem more likely to be of service than in the removal of the several concretions that are formed in various parts of the body. Of these, one species from the bladder has been thoroughly examined by Scheele, who found it to consist almost entirely of a peculiar concrete acid, which, since his

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* When a candle or line is viewed through a deep specimen, the unusual image is tinged with colours. —Orig.

time, has received the name of lithic acid. In the following paper I purpose giving an account of the analysis of gouty concretions, and of 4 new urinary calculi.

The gouty matter, from its appearance, was originally considered as chalk, but from being found in an animal not known to contain or secrete calcareous earth uncombined with phosphoric acid, it has since been supposed to resemble earth of bones. Dr. Cullen has even asserted, that it is 'very entirely' soluble in acids. The assertion however, is by no means generally true, and I think he must probably have used the nitrous acid, for I find no other that will dissolve it. Another opinion, and I believe at this time the most prevalent, is, that it consists of lithic acid, or matter of the calculus described by Scheele. But this idea is not I believe founded on any direct experiments, nor is it, to my knowledge, more ably supported than by Mr. Forbes, who defends it solely by pathological arguments from the history of the disease. Had he undertaken an examination of the substance itself, he would have found that, instead of a mere concrete acid, the gouty matter is a neutral compound, consisting of lithic acid and mineral alkali; as the following experiments will prove.

(1) If a small quantity of diluted vitriolic acid be poured on the chalk-stone, part of the alkali is extracted, and crystals of Glauber's salt may be obtained from the solution. Common salt may still more easily be procured by marine acid. The addition of more acid will extract the whole of the alkali, leaving a large proportion of the chalk-stone undissolved; which exhibits the following characteristic properties of lithic matter. (a) By distillation it yields a little volatile alkali, Prussic acid, and an acid sublimate, having the same crystalline form as the sublimate observed by Scheele. (b) Dissolved in a small quantity of diluted nitrous acid it tinges the skin with a rose colour, and when evaporated leaves a rose-coloured deliquescent residuum. (c) It dissolves readily in caustic vegetable alkali, and may be precipitated from it by any acid, and also by mild volatile alkali; first as a jelly, and then breaking down into a white powder.

(2) In distillation of the chalk-stone the lithic acid is decomposed, and yields the usual products of animal substances, viz. a fetid alkaline liquor, volatile alkali, and a heavy fetid oil, leaving a spongy coal; which when burnt in open air fuses into a white salt, that does not deliquesce, but dissolves entirely in water, is alkaline, and when saturated with nitrous acid gives rhomboidal crystals. These characteristic properties prove it to be mineral alkali.

(3) Caustic vegetable alkali poured on the chalk-stone, and warmed, dissolves the whole, without emitting any smell of volatile alkali. From which it appears, that the volatile alkali obtained by distillation is a product arising from a new arrangement of elements, not so combined in the substance itself.

(4) Water aided by a boiling heat dissolves a very small proportion of the gouty concretion, and retains it when cold. The lithic acid thus dissolved in combination with the alkali, is rather more than would be dissolved alone; so that by addition of marine acid it may be separated. While the solution continues warm no preci-

pitate is formed; but as it cools, the lithic acid crystallizes on the sides of the vessel, in the same manner as the crystals called red sand do, when an acid is added to recent urine. The gouty concrete may be easily formed by uniting the ingredients of which I have found it to consist.

(5) If a fragment of lithic acid be triturated with some mineral alkali and a little warm water, they unite, and after the superfluous alkali has been washed out, the remainder has every chemical property of gouty matter. The acid will not sublime from it, but is decomposed (2) by heat: the alkali may be extracted by the vitriolic or marine (1), or indeed by most acids. The compound requires a large quantity of water for its solution (4), and while warm the solution yields no precipitate by the addition of an acid; but on its cooling the lithic crystals form, as in the preceding experiment. In each case the crystals are too small for accurate examination, but I have observed, that by mixing a few drops of caustic vegetable alkali to the solution previous to the decomposition, they may be rendered somewhat larger. At the first precipitation, the crystals from gouty matter were not similar to those of lithic acid; but by redissolving the precipitate in water with the addition of a little caustic vegetable alkali, and decomposing the solution as before, while hot, the crystals obtained were perfectly similar to those of lithic acid procured by the same means.

Such then are the essential ingredients of the gouty concretion. But there might probably be discovered, by an examination of larger masses than I possess, some portion of common animal fibre or fluids intermixed; but whatever particles of heterogeneous matter may be detected, they are in far too small proportion to invalidate the general result, that ‘gouty matter is lithiated soda.’ The knowledge of this compound may lead to a further trial of the alkalies which have been observed by Dr. Cullen to be apparently efficacious in preventing the returns of this disease (First Lines, DLVIII); and may induce us, when correcting the acidity to which gouty persons are frequently subject, to employ the fixed alkalies, which are either of them capable of dissolving gouty matter, in preference to the earths, termed absorbent, which can have no such beneficial effect.

Fusible calculus.—My next subject of inquiry has been a species of calculus, that was first ascertained to differ from that of Scheele by Mr. Tennant; who found that when urged by the heat of a blow-pipe, instead of being nearly consumed, it left a large proportion, fused into an opaque white glass, which he conjectured to be phosphorated lime united with other phosphoric salts of the urine, but never attempted a more minute analysis. Stones of this kind are always whiter than those described by Scheele, and some specimens are perfectly white. The greater part of them have an appearance of sparkling crystals, which are most discernible where 2 crusts of a laminated stone have been separated from each other. I lately had an opportunity of procuring these crystals alone, voided in the form of a white sand, and thence of determining the nature of the compound stone, in which these are cemented by other ingredients. The crystals consist of phosphoric acid, magnesia, and volatile alkali: the stone contains also phosphorated lime, and generally some lithic acid. The form of the crystals is a short trilateral prism, having one angle a right angle, and the other 2 equal, terminated by a pyramid of 3 or 6 sides.

(6) By heat the volatile alkali may be driven off from the crystals, and they are rendered opaque, or may be partially fused. The phosphorated magnesia may then be dissolved in nitrous acid; and by addition of quicksilver dissolved in the same acid, a precipitate of phosphorated quicksilver is obtained, from which the quicksilver may be expelled by heat, and the acid procured separate. By addition of vitriolic acid to the remaining solution, Epsom salt is formed, and may be crystallized, after the requisite evaporation of the nitrous acid, and separation of any redundant quicksilver.—(7) These crystals require a very large quantity of water for their solution, but are readily soluble in most if not all acids; viz. vitriolic, nitrous, marine, phosphoric, saccharine, and acetous; and when precipitated from them reassume the crystalline form.—(8) From the solution in marine acid, sal ammoniac may be obtained by sublimation.

(9) Though the analysis is satisfactory, the synthetic proof is, if possible, still more so. After dissolving magnesia in phosphoric acid, the addition of volatile alkali immediately forms the crystalline precipitate, having the same figure and properties as the original crystals.—(10) If volatile alkali be cautiously mixed with recent urine, the same compound will be formed; the first appearance that takes place when a sufficient quantity of alkali has been gradually added, is a precipitate of these triple crystals. These constitute the greater part of the fusible stone; so that a previous acquaintance with their properties is necessary, in order to comprehend justly the nature of the compound stone in which they are contained. The most direct analysis of the compound stone is effected by the successive action of distilled vinegar, marine acid, and caustic vegetable alkali.

(11) Distilled vinegar acts but slowly on the calculus when entire; but when powdered, it immediately dissolves the triple crystals, which may be again precipitated from it as crystals by volatile alkali; and if the solution has not been aided by heat, scarcely any of the phosphorated lime will be found blended with them. In one trial the triple crystals exceeded $\frac{6}{10}$ of the quantity employed; but it seemed unnecessary to determine the exact proportion which they bear to the other ingredients in any one instance, as that proportion must vary in different specimens of such an assemblage of substances not chemically combined. Marine acid, poured on the remainder, dissolves the phosphorated lime, leaving a very small residuum. This is soluble in caustic vegetable alkali entirely, and has every other property of mere lithic acid. The presence of volatile alkali in the compound stone may be shown in various ways.

(12) In the distillation of this stone, there arises, first volatile alkali in great abundance, a little fetid oil, and lithic acid. There remains a large proportion charred. Water poured on the remaining coal dissolves an extremely small quantity

of a salt, apparently common salt, but too minute for accurate examination. Distilled vinegar dissolves no part of it, even when powdered. Marine acid dissolves the phosphorated lime and phosphorated magnesia, leaving nothing but a little charcoal. From this solution vitriolic acid occasions a precipitate of selenite, after which triple crystals may be formed by addition of volatile alkali.—(13) Marine acid also acts readily on a fragment of the stone, leaving only yellowish laminæ of lithic acid. When the solution has been evaporated to dryness, sal ammoniac may be sublimed from it; and the 2 phosphorated earths are found combined with more or less of marine acid, according to the degree of heat applied. If the proportion of the earths is wished to be ascertained, acid of sugar will separate them most effectually, by dissolving the phosphorated magnesia, and forming an insoluble compound with the lime.

(14) Caustic vegetable alkali has but little effect on the entire stone; but if heated on the stone in powder, a strong effervescence takes place from the escape of alkaline air, and the menstruum is found to contain lithic acid precipitable by any other acid. Some phosphoric acid also, from a partial decomposition of the triple crystals, is detected by nitrated quicksilver.—(15) The triple crystals alone are scarcely fusible under the blow-pipe; phosphorated lime proves still more refractory; but mixtures of the 2 are extremely fusible, which explains the fusibility of the calculus. The appearance of the lithic strata, and the small proportion they bear to the other ingredients, shows that they are not an essential part, but an accidental deposit, that would be formed on any extraneous substance in the bladder, and which probably in this instance concretes during any temporary interval that may occur in the formation of the crystals.—I come now to what has been called

Mulberry calculus.—This stone, though by no means overlooked, and though pointed out as differing from other species, has not, to my knowledge, been subjected to any further analysis than is given, in the 2d vol. of the Med. Trans., by Dr. Dawson, who found that his lixivium had little or no effect on it; and in the Phil. Trans. by Mr. Lane, who, among other simple and compound stones, gives an account of the comparative effects of lixivium and heat on a few specimens of mulberry calculus, (viz. N° 7, 8, 9, 10); but neither of these writers attempted to ascertain the constituent parts. Though the name has been confined to such stones as, from their irregularity, knotted, surface, and dark colour, bear a distant resemblance to that fruit, I find the species, chemically considered, to be more extensive, comprehending also some of the smoothest stones we meet with; of which one in my possession is of a much lighter colour, so as to resemble in hue, as well as smoothness, the surface of a hemp-seed. From this circumstance it seems not improbable that the darkness of irregular stones may have arisen from blood voided in consequence of their roughness. The smooth calculus I find to consist of lime united with the acids of sugar and of phosphorus. The rougher specimens have generally some lithic acid in their interstices.

(16) Caustic vegetable alkali acquires a slight tinge from a fragment of this kind of stone, but will not dissolve it. When powdered it is thereby purified from any quantity of lithic acid that it may contain. Phosphoric acid will then dissolve out the phosphorated lime, and the remainder, after being washed, may be decomposedby the vitriolic acid. The affinity of this acid for a certain proportion of lime is superior even to that of acid of sugar; selenite is formed, and the acid of sugar may be crystallized, and by the form of its crystals recognized, as well as by every other property. It is easily soluble, occasions a precipitate from lime water, and from a solution of selenite, and with mineral alkali forms a salt that requires a large quantity of water for its solution.

(17) When the stone has been finely powdered, marine acid will slowly dissolve all but any small quantity of lithic matter which it may contain. After the solution has been evaporated to dryness, no part is then soluble in water, the marine acid being wholly expelled. When the dried mass is distilled with a greater heat, the saccharine acid is decomposed, and a sublimate formed, still acid and still crystallizable, but much less soluble in water, and which does not precipitate lime from lime water. After distillation, the remainder contains phosphorated lime, pure lime, and charcoal; and when calcined in the open air, the charcoal is consumed, and the whole reduced to a white powder. The 2 former may be dissolved in marine acid, which when evaporated to dryness will be retained only by the lime; so that water will then separate the muriated lime, and the phosphorated lime may afterwards be submitted to the usual analysis.

Bone-earth calculus.—Beside that of Scheele, and the 2 already noticed, there is also a 4th species of calculus, occasionally formed in the bladder, distinct in its appearance, and differing in its component parts from the rest; for it consists entirely of phosphorated lime. Its surface is generally of a pale brown, and so smooth as to appear polished; when sawed through, it is found very regularly laminated; and the laminæ in general adhere so slightly to each other, as to separate with ease into concentric crusts. In a specimen with which I was favoured by Dr. Baillie, each lamina is striated in a direction perpendicular to the surface, as from an assemblage of crystallized fibres. The calculus dissolves entirely, though slowly, in marine or nitrous acid, and, consisting of the same elements as earth of bones, may undergo a similar analysis, which it cannot be necessary to particularize. By the blow-pipe it is immediately discovered to differ from other urinary calculi: it is at first slightly charred, but soon becomes perfectly white, still retaining its form, till urged with the utmost heat from a common blow-pipe, when it may at length be completely fused. But even this degree of fusibility is superior to that of bones. The difference consists in an excess of calcareous earth contained in bones, which renders them less fusible. This redundant portion of lime in bones renders them also more readily soluble in marine acid, and may, by evaporation of such a solution, be separated, as in the last experiment on mulberry calculus. The remaining phosphorated lime may be re-dissolved by a fresh addition of marine acid; and being now freed from redundant lime, will, on evaporation of the marine acid, assume a crystalline form. As the laminated calculus contains no excess of lime, that will at once yield such crystals: their appearance will be described in the succeeding experiment.

Calculus from the prostate gland.—There is still another calculus of the urinary passages, though not of the bladder itself, which deserves notice, not from the frequency of its occurrence, but from having been supposed to give rise to stone in the bladder. I mean the small stones which are occasionally found in the prostate gland. Those that I have seen, and which, by favour of Mr. Abernethy, I have had an opportunity of examining, were from the size of the smallest pin's head to that of pearl barley, in colour and transparency like amber, and appeared originally to have been spherical; but from contiguity with others, some had flattened surfaces, so as at first sight to appear crystallized. These I find to be phosphorated lime in the state of neutralization, tinged with the secretion of the prostate gland.

(18) A small fragment being put into a drop of marine acid, on a piece of glass over a candle, was soon dissolved; and on evaporation of the acid, crystallized in needles, making angles of about 60° and 120° with each other. Water dropped on the crystals would dissolve no part of them; but in marine acid they would redissolve, and might be re-crystallized.—(19) Vitriolic acid forms selenite with the calcareous earth.—(20) By aid of nitrated quicksilver, phosphoric acid is readily obtained.

(21) When heated this calculus decrepitates strongly; it next emits the usual smell of burnt animal substances, and is charred, but will not become white though partially fused. It still is soluble in marine acid, and will in that state crystallize more perfectly than before. Hence I conclude, that these stones are tinged with the liquor of the prostate gland, which in their original state (18) somewhat impedes the crystallization. This crystallization from marine acid is so delicate a test of the neutral phosphorated lime, that I have been enabled by that means to detect the formation of it, though the quantities were very minute. The particles of sand which are so generally to be felt in the pineal gland, have this for their basis; for I find that after calcination they crystallize perfectly from marine acid. I have likewise met with the same compound in a very pure state, and soft, contained in a cyst under the pleura costalis.

On the contrary, ossifications, properly so called, of arteries and of the valves of the heart, are similar to earth of bones, in containing the redundant calcareous earth; and I believe also those of veins, of the bronchiæ, and of the tendinous portion of the diaphragm, have the same excess; but my experiments on these were made too long since for me to speak with certainty. To these I may also add the incrustation frequently formed on the teeth, which, in the only 2 specimens that I have examined, proved to be a similar compound, with a very small excess of lime.

Though I do not at present presume to draw conclusions with regard to the treatment of all the diseases in question, some inferences cannot pass unobserved. The sand from the pineal gland, from its frequency hardly to be called a disease, or when amounting to disease most certainly not known by its symptoms, would, at the same time, if known, be wholly out of the reach of any remedy. The calculi of the prostate are too rare perhaps to have been ever yet suspected in the living body, and are but indirectly worthy of notice. For if by chance one of them should be voided with the urine, a knowledge of its source would guard us against an error we might otherwise fall into, of proposing the usual solvents for urinary calculi.

The bone-earth calculus, though so nearly allied to the last, is still manifestly different, and cannot be supposed to originate from that source; but if ever the drinking of water impregnated with calcareous earth gave rise to a stone in the bladder, this would most probably be the kind generated, and the remedy must evidently be of an acid nature. With respect to the mulberry calculus, I fear that an intimate knowledge of its properties will leave but small prospect of relief from any solvent; but by tracing the source of the disease we may entertain some hopes of preventing it. As the saccharine acid is known to be a natural product of a species of oxalis, it seems more probable that it is contained in some other vegetables or their fruits taken as aliment, than produced by the digestive powers, or secreted by any diseased action of the kidneys. The nutriment would therefore become a subject of minute inquiry, rather than any supposed defect of assimilation or secretion.

When a calculus is discovered, by the evacuations, to be of the fusible kind, we seem to be allowed a more favourable prospect in our attempts to relieve: for here any acid that is carried to the bladder will act on the triple crystals, and most acids will also dissolve the phosphorated lime; while alkalies, on the contrary, would rather have a tendency to add to the disease. Though, from want of sufficient attention to the varieties of sediment from urine, and want of information with regard to the diversity of urinary calculi, the deposits peculiar to each concretion are yet unknown; it seems probable that no long course of observation would be necessary to ascertain with what species any individual may be afflicted.

The lithic, which is by far the most prevalent, fortunately affords us great variety of proofs of its presence. Particles of red sand, as they are called, are its crystals. Fragments also of larger masses, and small stones, are frequently passed; and it is probable that the majority of appearances in the urine called purulent, are either the acid itself precipitated too quickly to crystallize, or a neutral compound of that acid with one of the fixed alkalies. Beside this species, the fusible calculus has afforded decisive marks of its presence in the case which furnished me with my specimen of triple crystals; and by the description given by Mr. Forbes (in his Treatise on Gravel and Gout, ed. 1793, p. 65), of a white crystallized precipitate, I entertain no doubt that his patient laboured under that variety of the disease.

XVIII. Experiments on Carbonated Hydrogenous Gas; with a View to determine whether Carbon be a Simple or a Compound Substance. By Mr. Wm. Henry. p. 401.

The progress of chemical science depends not only on the acquisition of new facts, but on the accurate establishment, and just valuation, of those we already possess: for its general principles will otherwise be liable to frequent subversions; and the mutability of its doctrines will but ill accord with the unvaried order of nature. Impressed with this conviction, I have been induced to examine a late attempt to withdraw from its rank among the elementary bodies, one of the most interesting objects of chemistry. The inferences respecting the composition of charcoal, deduced by Dr. Austin from his experiments on the heavy inflammable air, (Philos. Trans., vol. 80), lead to changes so numerous in our explanations of natural phenomena, that they ought not to be admitted without the strictest scrutiny of the reasoning of this philosopher, and an attentive repetition of the experiments themselves. In the former, sources of fallacy may I think be easily detected; and in the latter, there is reason to suspect that Dr. Austin has been misled by inattention to some collateral circumstances. Several chemists however, of distinguished rank, have expressed themselves satisfied with the evidence thus produced in favour of the composition of charcoal; and among these it may be sufficient to mention Dr. Beddoes, who has availed himself of the theory of Dr. Austin, in explaining some appearances that attend the conversion of cast into malleable iron. Philos. Trans., vol. 81.

The heavy inflammable air, having been proved to consist of a solution of pure charcoal in light inflammable air, is termed, in the new nomenclature, carbonated hydrogenous gas. By repeatedly passing the electric shock through a small quantity of this gas, confined in a bent tube over mercury, Dr. Austin found that it was permanently dilated to more than twice its original volume. An expansion so remarkable could not, as he observes, be occasioned by any other known cause than the evolution of light inflammable air. When the electrified air was fired with oxygenous gas, it was found that more oxygen was required for its saturation than before the action of the electric fluid; which proves that, by this process an actual addition was made of combustible matter. The light inflammable air disengaged by the electrization, doubtless proceeded from the decomposition of some substance within the influence of the electric fluid, and not merely from the expansion of that contained in the carbonated hydrogenous gas; for had the quantity of hydrogen remained unaltered, and its state of dilatation only been changed, there would not, after electrization, have been any increased consumption of oxygen.

The only substances in contact with the glass tube and mercury, in these experiments, besides the hydrogen of the dense inflammable gas, were carbon and water; which last, though probably not a constituent of gases, is however copiously diffused through them. If the evolved hydrogen proceeded from the decomposition of the former of these 2 substances, it is evident that a certain volume of the carbonated hydrogenous gas must yield, after electrization, on combustion with oxygen, less carbonic acid than an equal volume of non-electrified gas; or, in other words, the inflammation of 20 measures of carbonated hydrogen, expanded by electricity from 10, should not afford so much carbonic acid as 10 measures of the unelectrified.

From the fact which has been before stated, respecting the increased consumption of oxygen by the electrified air, it follows, that in determining the quantity of its carbon by combustion, such an addition of oxygen should be made, to that necessary for the saturation of the gas before exposure to the electric shock, as will completely saturate the evolved hydrogen. For if this caution be not observed, we may reasonably suspect that the product of carbonic acid is diminished, only because a part of the heavy inflammable air has escaped combustion. It might indeed be supposed, that in consequence of the superior affinity of carbon for oxygen, the whole of the former substance, contained in the dense inflammable gas, would be saturated, and changed into carbonic acid, before the attraction of hydrogen for oxygen could operate in the production of water. But I have found that the residue, after inflaming the carbonated hydrogenous gaz with a deficiency of oxygen, and removing the carbonic acid, is not simply hydrogenous, but carbonated hydrogenous gas.

In the 2d, 5th, and 6th of Dr. Austin's experiments, in which the quantity of carbon, in the electrified gas, was examined by deflagrating it with oxygen, the combustion was incomplete, because a sufficiency of oxygen was not employed; and Dr. Austin himself was aware that, in each of them, "a small quantity of heavy inflammable air might escape unaltered." It is observable also, that the product of carbonic acid, from the electrified gas, increased in proportion as the combustion was more perfect. We may infer therefore, that if it had been complete, there would have been no deficiency of this acid gas, and consequently no indication of a decomposition of charcoal. A strong objection however is applicable to these, as well as to most of Dr. Austin's experiments, that the residues were not examined with sufficient attention. In one instance we are told, that the remaining gas was inflammable, and in another, that it supported combustion like vital air. I need hardly remark, that a satisfactory analysis cannot be attained of any substance, without the most scrupulous regard, not only to the qualities, but to the precise quantities of the products of our operators.

To the 8th and 9th experiments, the objection may be urged with additional weight, which has been brought against the preceding ones, that the quantity of oxygen, instead of being duly increased in the combustion of the electrified gas, was, on the contrary, diminished. Thus, in the 8th experiment, 2.83 measures of carbonated hydrogen were inflamed with 4.58 measures of oxygenous gas; but in the 9th, though the 2.83 measures were dilated to 5.16, and had therefore received a considerable addition of combustible matter, the oxygen employed was only 4.09. To the rest of Dr. Austin's experiments either one or both of the above objections are applicable.

The first and most important step therefore, in the repetition of these experiments, is to determine, whether the carbonated hydrogenous gas really sustains, by the process of electrization, a diminution of its quantity of carbon; because, should this be decided in the negative, we derive from the fact a very useful direction in ascertaining the true source of the evolved hydrogen. The following experiments were therefore made with a view to decide this question, and the error of Dr. Austin, in employing too little oxygen, was carefully avoided.*

Exper. 1. In a bent tube, standing inverted over mercury, 94.5 measures of carbonated hydrogenous gas, from acetite of pot-ash, were mixed with 107.5 of oxygen. The total, 202, was reduced by an explosion to 128.5, and was further contracted by lime-water to 54. A solution of hepar sulphuris left only 23 measures. The diminution by lime-water, viz. 74.5 measures, makes known to us the quantity of carbonic acid afforded by the combustion of 94.5 measures of carbonated hydrogenous gas: and the residue after the action of hepar sulphuris, viz. 23 measures, gives the proportion of azotic gas contained in the carbonated hydrogen; for the oxygenous gas employed, which was procured from oxygenated muriate of pot-ash, was so pure, that the small quantity used in this experiment could not contain a measurable portion of azotic gas.

Exper. 2. The same quantity of carbonated hydrogen was expanded, by repeated electrical shocks, to 188 measures. The addition of hydrogenous gas therefore amounted to 93.5. The gas thus dilated was fired, at different times, with 392.5 measures of oxygenous gas; and the residue, after these several explosions, was 203 measures. Lime water reduced it to 128.5, and sulphure of pot-ash to 19.5. In this instance, as in the former one, the product of carbonic acid is 74.5 measures. Finding, from the first experiment and other similar ones, that the carbonated hydrogenous gas, which was the subject of them, contained a very large admixture of azotic gas, I again submitted to distillation a quantity of the acetite of pot-ash, with every precaution to prevent the adulteration of the product with atmospherical air. Such an adulteration, I have observed, impedes considerably the dilatation of the gas, and for a time even entirely prevents it. This explains the failure, which

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* The apparatus employed in these experiments, was the ingenious contrivance of Mr. Cavendish, and is described in vol. 75, of the Philos. Trans. In dilating the gas, I sometimes used a straight tube, furnished with a conductor, in the manner of Dr. Priestley, (see his Experiments on Air, vol. 1, plate 1, fig. 16.) The bulk of the gases introduced, and their volume after the various experiments, were ascertained by a moveable scale, and by afterwards weighing the mercury which filled the tube to the marks on the scale; by which means I was spared the trouble of graduating the syphons. Each grain of mercury indicates 1 measure of gas; and though the smallness of the quantities submitted to experiment may be objected to, yet this advantage was gained, that the electrified gas could be fired at one explosion, as was done in the 4th, 6th, and 8th experiments. Errors, from variations of temperature and atmospherical pressure, were carefully avoided.—Orig.

some experienced chemists have met with, in their attempts to expand the carbonated hydrogenous gas by electricity. Gas which is thus vitiated, becomes however capable of expansion, after exposure to the sulphure of pot-ash.

Exper. 3. Carbonated hydrogen 340 measures were exploded with the proper proportion of oxygenous gas. The carbonic acid produced amounted to 380 measures, and the residue of azotic gas was 20 measures.—*Exper.* 4. The same quantity, when expanded to 690, gave on combustion 380 measures of carbonic acid, and 19.8 of azotic gas.—*Exper.* 5. 315 measures of carbonated hydrogen yielded 359 measures of carbonic acid, and 18.5 measures of azote.—*Exper.* 6. The same quantity, after expansion to 600, afforded the same products of carbonic acid and azotic gases.—*Exper.* 7 and 8. As much carbonic acid was obtained by the combustion of 408 measures of carbonated hydrogenous gas, expanded from 200, as from 200 measures of the non-electric fired gas; and the residues of azotic gas were the same in both cases.

It is unnecessary to state the particulars of several other experiments, similar to those above related, which were attended with the same results. They sufficiently prove that the action of the electric spark, when passed through carbonated hydrogenous gas, is not exerted in the decomposition of carbon; for the same quantity of this substance is found after as before electrization. Even granting that charcoal is a compound, the constituents of which are held together by a very forcible affinity, it does not appear likely that the agency of the electric shock, which seems, in this instance, analogous to that of caloric, should effect its decomposition under the circumstances of these experiments. For it is a known property of charcoal to decompose water, when aided by a high temperature; and its union with oxygen is a much more probable event, when this body is present, than a separation into its constituent principles. As an argument also, that water is the source of the light inflammable air in this process, it may be observed, that the dilatation in Dr. Austin's experiments could never be carried much farther than twice the original bulk of the gas*. This fact evidently implies that the expansion ceased only in consequence of the entire destruction of the matter, whose decomposition afforded the light inflammable air, and this substance could not be carbon, because Dr. Austin admits that a large portion, and I have shown that the whole of it still remains unaltered.

If the dilatation of the carbonated hydrogenous gas arose from the decomposition of water, the effect should cease when this fluid is previously abstracted. To ascertain whether this consequence would really follow, I exposed a portion of the gas, for several days before electrization, to dry caustic alkali. On attempting its

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* "After the inflammable air has been expanded to about double its original bulk," says Dr. Austin, "I do not find that it increases further by continuing the shocks. Conceiving that the progress of the decomposition was impeded by the mixture of the other airs with the heavy inflammable, I passed the spark through a mixture of the heavy inflammable air and light inflammable; but the expansion succeeded nearly as well as when the heavy inflammable was electrified alone." Phil. Trans. vol. 80, p. 52.—Orig.

expansion, I found that it could not be carried beyond $\frac{1}{6}$ the original bulk of the gas. By 160 very strong explosions it attained this small degree of dilatation, but 80 more produced not the least effect; though the former number would have been amply sufficient to have dilated the gas, in its ordinary state, to more than twice its original volume. A drop or 2 of water being admitted to this portion of gas, the expansion went on as usual; and I may here observe, that when a little water gained admission into the tube along with the gas, in any experiment, which often happened before I had acquired sufficient expertness in transferring the air from water to mercury, the dilatation went on with remarkable rapidity.

Carbonic acid gas, according to the discovery of M. Monge*, undergoes, when submitted to the electric shock, a change similar to that effected on the carbonated hydrogen; and the expansion has been shown, by Messrs. Landriani and Van Marum †, to be owing to the same cause, viz. the extrication of light inflammable air. The added gas, M. Monge ably contends, cannot proceed from any other source than the water held in solution by all aeriform bodies, the oxygen of which he supposes to combine with the mercury. That the decomponent of the water however, in the experiments which I have described, is not a metallic body, will appear highly probable when we reflect that there is present in them a combustible substance, viz. charcoal, which attracts oxygen much more strongly than metals; and the following experiments evince that the mercury, by which the air was confined, had no share in producing the phenomena.

Exper. 9. A portion of carbonated hydrogenous gas was introduced in a glass tube closed at one end, into which a piece of gold wire was inserted, that projected both within and without the cavity of the tube. The open end of the tube was then closed by a stopper perforated also with gold wire, so that electric shocks could be passed through the confined air, without the contact of any metal that has the power of decomposing water. On opening the tube with its mouth downwards, under water, a quantity of air immediately rushed out.—*Exper.* 10. The dilatation of the gas was found to proceed very rapidly when standing over water, and exposed to the action of the electric fluid, conveyed by gold conductors.

We have only therefore, in the 2 preceding experiments, one substance into contact with the gas which is capable of decomposing water, viz. charcoal. The union of this body with the oxygen of the water would be rendered palpable by the formation of carbonic acid; but Dr. Austin did not observe that any precipitation was occasioned in lime-water, by agitating it with the electrified gas. On passing up syrup of violets to the electrified air, with the expectation of its indicating the volatile alkali, as in the experiments of Dr. Austin, no change of colour took place, though the test was of unexceptionable purity. On examining however, whether any alteration of bulk had been produced in the air by the contact of this liquid, it appeared, that of 709 measures, 100 had been absorbed. Suspecting that the absorption was owing to the presence of carbonic acid, I introduced some lime-water

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* 29 Journal de Physique, 277.—† 2 Annales de Chimie, 273.—Orig.

to a volume of the expanded gas, amounting to 556 measures, when they were immediately reduced to 512. The contraction would probably have been still more remarkable if the gas had been further expanded before the admission of the liquid. The change in the lime-water was very trifling; but my friend Mr. Rupp, who witnessed this as well as several of the other experiments, and who is much conversant in the observation of chemical facts, was satisfied that, after a while, he saw small flocculi of a precipitate on the surface of the mercury. This contraction of bulk cannot be ascribed to any other cause than the absorption of carbonic acid; for besides the fact, that the colour of syrup of violets and of turmeric, which I also tried, were not affected by exposure to the electrified gas, I have this objection to the absorbed gas being ammonia, that no diminution, either of bulk or transparency, occurred on the admixture of muriatic acid gas with the electrified air; whereas ammonia would have been exhibited under the form of a neutral salt. When water was passed up to this mixture of the 2 gases, there was an absorption, not only of the muriatic gas, but of something more.

Conceiving that the demolition of charcoal, by the action of the electric fluid, was sufficiently proved by his experiments, Dr. Austin assigns the evolved hydrogen as 1 of its constituents, and the other he concludes to be azote. This inference however rests almost entirely on estimates, in which material errors may be discovered. Some of these it may be well to point out, for the satisfaction of such as have acquiesced in Dr. Austin's opinion. The carbonated hydrogenous gas submitted to Dr. Austin's experiments clearly appears, from his own account, to have been largely adulterated with azotic gas. One source of its impurity he has disclosed, by informing us that the gas "had been very long exposed to water*;" for Dr. Higgins has somewhere shown that the heavy inflammable air, after standing long over water, leaves a larger residue of azote, on combustion, than when recently prepared†. It is probable also, that the proportion of azote derived from the water, would increase with the time of its exposure; and thus a fertile source of error is suggested, which appears wholly to have escaped Dr. Austin's attention. In repeating his experiments, I was careful that comparative ones, on 2 equal quantities of the electrified and unelectrified gas, should be made, without the intervention of any time that could vary the proportion of azote in either of the gases.

To the 9th experiment, in which the quantity of azote seems to have been increased by electrization, I must repeat the objection, that a sufficiency of oxygenous gas was not used in the combustion. In the 8th experiment, 2.83 of the unelectrified air were fired with 4.17 oxygenous gas, and only 0.15 of the latter remained above what was sufficient for saturation; but in the 9th, though the 2.83 measures

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* 80 Phil. Trans. 54.—† Similar facts respecting the deterioration of other gases, by standing over water, may be seen in Dr. Priestley's Experiments on Air, vol. 1, p. 59, 158. I found that oxygenous gas, from oxygenated muriate of pot-ash, acquired, by exposure a few weeks to water, .125 its bulk of azotic gas.—Orig.

were expanded to 5.16, the quantity of oxygen employed was 0.08 less than in the former experiment; and it may therefore be presumed that a small quantity of inflammable air might escape unaltered, and might add apparently to the product of azote. In the 8th experiment also, the portion of oxygenous gas that was more than sufficient to saturate the carbonated hydrogen, would probably combine, in part, with the remaining azote, as in the experiments of Dr. Higgins* and Dr. Priestley†. But in the 9th, the quantity of oxygenous gas was hardly sufficient to saturate both kinds of inflammable air after electrization, and could not therefore diminish the azotic gas. When the proportion of oxygen is duly increased, and the inflammation of the electrified air is performed in small portions, there is no augmentation, but on the contrary a decrease of the quantity of the azote, as will appear on comparing the 1st and 2d of the experiments which I have related.

Two circumstances were observed, in the experiments of Dr. Austin, which have not been noticed in the preceding account of the repetition of them, viz. the appearance of a deposit from the carbonated hydrogenous gas during its electrization, and the formation of ammonia by the same process. In some experiments, which I made on the first portion of gas, both these facts were sufficiently apparent; but neither of them occurred on electrifying the gas which was afterwards procured. Suspecting that the cessation of them arose from the superior purity of the latter portion from azotic gas, I passed the electric shock through a mixture of carbonated hydrogen with about $\frac{1}{4}$ its bulk of azote, and thus again produced the precipitate, which would have been of a white colour, if it had not been obscured by minute globules of mercury, that were driven upwards by the force of the explosion. An infusion of violets was tinged green when admitted to the electrified gas; but the change of colour did not occur instantly, as happens from the absorption of ammoniacal gas; and required for its production that the liquid should be brought extensively into contact with the inner surface of the tube. From this effect on a blue vegetable colour, we may infer that the precipitate was an alkaline substance, and probably the carbonate of ammonia; but the quantity was much too minute to be the subject of more decisive experiment.

I shall conclude this memoir, with a brief summary of the facts that are established by the preceding experiments‡. Those included under the first head are deducible from the experiments of Dr. Austin. 1. Carbonated hydrogenous gas, in its ordinary state, is permanently dilated by the electric shock to more than twice its original volume; and as light inflammable air is the only substance we are acquainted with, that is capable of occasioning so great an expansion, and of exhibiting the phenomena that appear on firing the electrified gas with oxygen, we may ascribe the dilatation to the production of hydrogenous gas. 2. The hydro-

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* Experiments and Observations on acetous Acid, &c. p. 295.—† 79 Phil. Trans. 7.—‡ Since this paper was written I have extended the inquiry to phosphorated hydrogenous gas, which expands equally with the carbonated hydrogen; loses its property of inflaming when brought into contact with oxygenous gas; and affords evident traces of a production of phosphorous or phosphoric acid.—Orig.

genous gas evolved by this process does not arise from the decomposition of charcoal; because the same quantity of that substance is contained in the gas after, as before electrization. 3. The hydrogenous gas proceeds from decomposed water; because when this fluid is abstracted as far as possible from the carbonated hydrogenous gas, before submitting it to the action of electricity, the dilatation cannot be extended beyond $\frac{1}{8}$ its usual amount. 4. The decomponent of the water is not a metallic substance; because carbonated hydrogenous gas is expanded when in contact only with a glass tube and gold, a metal which has no power of separating water into its formative principles. 5. The oxygen of the water (when the electric fluid is passed through carbonated hydrogenous gas, that holds this substance in solution,) combines with the carbon, and forms carbonic acid. This production of carbonic acid therefore adds to the dilatation occasioned by the evolution of hydrogenous gas. 6. There is not, by the action of the electric matter on carbonated hydrogenous gas, any generation of azotic gas. 7. Carbon it appears therefore, from the united evidence of these facts, is still to be considered as an elementary body; that is, as a body with the composition of which we are unacquainted, but which may nevertheless yield to the labours of some future and more successful analyst.

XIX. Observations and Experiments on the Colour of Blood. By Wm. C. Wells, M.D., F.R.S. p. 416.

Dr. Priestley is, I believe, the only person who has hitherto attempted to show by what means common air brightens the colour of blood, which has been for some time exposed to it*. His opinion is, that the air produces this effect by depriving the blood of its phlogiston; for blood, according to the same author, is wonderfully fitted both to imbibe and to part with phlogiston, becoming black when charged with that principle, but highly florid when freed from it. Various arguments may be brought to prove that this opinion is erroneous, even on the admission of such a principle of bodies as phlogiston. It may be said, for instance, that it is contrary to the laws of chemical affinity, that the same mass should at one time convert pure into phlogisticated air, by giving out its phlogiston, and immediately after reconvert phlogisticated into pure air, by imbibing that principle; both which changes are supposed by Dr. Priestley to be induced by blood, on those airs. Again; it may be urged, that since the neutral salts, and the different alkalies, when saturated with fixed air, produce the same effect as common air on the colour of blood, if common air acts by attracting phlogiston, those other bodies must have a similar operation. But surely it cannot be thought, that the mild volatile alkali, which has been supposed by chemists to superabound with phlogiston, can yet attract it from blood. It appears to me however, unnecessary to bring any further arguments of this kind against the opinion of Dr. Priestley, since the following experiments will, I expect, be thought sufficient to show, in opposition to

* Phil. Trans. for 1776.—Orig.

what is taken for granted by him in the whole of his inquiry, that the alteration induced on the colour of blood, both by common air and the neutral salts, is altogether independent of any change effected by them on its colouring matter.

I infused a piece of black crassamentum of blood in distilled water, and immediately after covered the containing vessel closely, to prevent the access of air. Having obtained by this means a transparent solution of the red matter of blood, nearly free from serum and coagulable lymph, I exposed a quantity of it to the open air in a shallow vessel, and poured an equal quantity into a small phial, which was then well closed. When the 1st portion of the solution had been exposed to the air for several hours, I decanted it into a phial, of the same size and shape as that which contained the 2d portion, and having added to it as much distilled water as was sufficient to compensate the loss it had suffered by evaporation, I now compared the 2 together, and found them to be exactly of the same colour, with regard both to kind and degree. I afterwards poured 2 other equal quantities of the red solution into 2 phials of the same size and shape. To 1 I added a little of a solution of nitre in water, and to the other as much distilled water. On comparing the 2 mixtures together, I found that they also possessed precisely the same colour. Lastly, I cut a quantity of dark crassamentum of blood into thin slices, and exposed them to common air. When they became florid, I put them into a phial containing distilled water. I then took as much of the same crassamentum, which was still black, and infused it in an equal quantity of distilled water, contained in a phial similar in size and shape to the former. The 2 solutions which were thus obtained, 1 from florid blood, the other from black blood, were notwithstanding of precisely the same colour. These experiments were frequently repeated, and were attended with the same results, as often as I used certain precautions, which shall be mentioned hereafter, as the reasons for them will then be more readily understood than they can be at present. Assuming therefore as proved, that neither common air, nor the neutral salts (for all those I have tried are similar to nitre in this respect) change the colour of the red matter of blood; I shall now attempt to explain the manner in which those substances give, notwithstanding, to black blood a florid appearance; premising however some observations on the colours of bodies in general.

It was the opinion of Kepler *, that light is reflected without colour from the surfaces of bodies; which he says is easily proved, by exposing to the sun's light a number of cups filled with transparent liquors of different colours, and receiving the reflexions from them on a white ground in a dark place. Zucchius, who was younger than Kepler, but for some time his cotemporary, taught more explicitly †, that the colours of bodies depend, not on the light which is reflected from their anterior surfaces, but on that portion of it which is received into their internal parts, and is thence sent back through those surfaces. The following are some of the experiments on which he founded this doctrine. He exposed small round

* Paralipomena in Vitellionem, p. 23 et 436.—† Optica Philos. pars I, p. 278 et seq.—Orig.

pieces of transparent glass, tinged with various colours to the light of the sun, and received what was reflected from them on white paper, in a darkened part of his room. He then found, that each glass produced 2 luminous circles, which, when the paper was sufficiently remote, were entirely separate from each other; and that the circle which proceeded from the upper surface of the glass was altogether without colour, while that which arose from the under surface, was of the same colour as the glass exhibited, when held between the light and the eye. From these experiments Zucchius also concluded, first, that every coloured body must be in some degree transparent, since a body absolutely impenetrable to light, could only reflect the colours of other bodies, but possess none of its own; and 2dly, that all bodies, which appear coloured when seen by reflected light, must be in some measure opake; for as the light which is reflected from their surfaces comes untinged to the eye, if that part of it which penetrates their substance were afterwards to proceed in it without impediment, no colour could be exhibited by them.*

When Sir Isaac Newton began his experiments on light and colours, it was generally believed, that colours in opake bodies arise from some modification given to light, by the surfaces which reflect it. In opposition to one part of this opinion, our great philosopher maintained, that such bodies are seen coloured, from their acting differently on the different colorific rays, of which white light is composed; but having established this point beyond dispute, he seems to have admitted, without inquiry, that colours are produced at the surfaces of the opake bodies to which they belong. For his experiments do not necessarily lead to such a conclusion; on the contrary, they are not more consistent with it, than they are with the opinion of Kepler and Zucchius. This opinion indeed he appears not to have known; since he has taken for granted, what is contradicted by the experiments on which it is founded, that the tinging particles of transparent bodies reflect coloured light.†

The very splendour of Sir Isaac Newton's discoveries in optics, has probably done some injury to this branch of knowledge; for soon after they were made public, it became a common opinion, that the subject of light and colours had been exhausted by that great man, and that no writer on it before him was now worthy of being read. The former part of this opinion has long been generally acknowledged to be unjust; but the latter part of it is still maintained by many,

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* The works of Zucchius seem very little known, though they contain a considerable number of original experiments, and though it is probable that he was the inventor of the reflecting telescope. For he says (Pars 1, p. 126) it had occurred to him so early as 1616, that the same effect which is produced by the convex object-glass of a telescope, might be obtained by reflexion from a concave mirror; and that, after many attempts to construct telescopes with such mirrors, which proved fruitless from imperfections in their figure, he at length procured a concave mirror very accurately wrought, by means of which, and a concave eye-glass, he was enabled to prove his theory to be just. He does not mention at what precise time he constructed this telescope: but his book was printed in 1652, 11 years before the publication of the "Optica Promota" of James Gregory. I have not met with any account of Zucchius, in Montucla's or Priestley's histories; in the article "telescope," in the French Encyclopedia; or in any Biographical dictionary which I have consulted.——† Optics, book 1, part 2, prop. 10.—Orig.

among whom may be placed the learned Mr. Delaval. This gentleman has lately published, (in the Manchester Memoirs, vol. 2) a very elaborate treatise to prove, that the colours of opake bodies do not arise from the rays of light which they reflect from their anterior surfaces; but from that portion of it, which, having penetrated their anterior surfaces, is reflected by the opake particles which are diffused through their substance. But had the learned author not believed, that no European writer on colours, before Sir Isaac Newton, contained any valuable information on that subject, he would probably have discovered, that both Kepler and Zucchius had long ago maintained the very opinion which he now advances, and that they had built it on experiments similar to his own. The merit of the invention of this theory belongs therefore to the great Kepler; but still much praise is due to Mr. Delaval, both for reviving and confirming it; since, though it be not free from defects in some of its parts, it affords solutions of several optical difficulties, which, as far as I know, admit of an explanation from no other source. Among these I regard the phenomenon which is the subject of the present inquiry.

To show then, from the theory of Kepler, Zucchius, and Delaval, how common air and the neutral salts may brighten the appearance of blood, without producing any change on its colouring matter, I shall first suppose that all its parts have the same reflective power. The consequence will be, that a mass sufficiently thick to suffocate the whole of the light which enters it, before it can proceed to the posterior surface, and be thence returned through the first surface, must appear black; for the rays which are reflected from the first surface are without colour, and by hypothesis none can be reflected from its internal parts. In the next place, let there be dispersed through this black mass a small number of particles, differing from it in reflective power, and it will immediately appear slightly coloured; for some of the rays, which have penetrated its surface, will be reflected by those particles, and will come to the eye obscurely tinged with the colour, which is exhibited by a thin layer of blood, when placed between us and the light. Increase now by degrees the number of those particles, and in the same proportion as they are multiplied, must the colour of the mass become both stronger and brighter.

Having thus shown that a black mass may become highly coloured, merely by a considerable reflexion of light from its internal parts; if I should now be able to prove, that both common air and the neutral salts increase the reflexion of light from the internal parts of blood, at the same time that they brighten it, great progress would certainly be made in establishing the opinion, that the change of its appearance, which is occasioned by them, depends on that circumstance alone. But the following observations seem to place this point beyond doubt. I compared several pieces of crassamentum of blood, which had been reddened by means of common air and the neutral salts, with other pieces of the same crassamentum, which were still black, or nearly so; on which I found, that the reddened pieces manifestly reflected more light than the black. One proof of this was, that the minute parts of the former could be much more distinctly seen than those of the latter. Now this increased reflection of light, in the reddened pieces, could not arise from any change in the reflective power of their surfaces; for bodies reflect light from their surfaces in proportion to their density and inflammability; and neither of those qualities, in the reddened pieces of crassamentnm, can be supposed to have been augmented by common air, or a solution of a neutral salt in water. The increased reflexion must, consequently, have arisen from some change in their internal parts, by means of which much of the light which had formerly been suffocated, was now sent back through their anterior surfaces, tinged with the colour of the medium through which it had passed.

The precise nature of the change which is induced on blood by the neutral salts, is made manifest by the following experiment. I poured on a piece of printed card as much serum, rendered very turbid with red globules, as barely allowed the words to be legible through it. I next dropped on the card a little of a solution of nitre in water; when I observed, that wherever the solution came in contact with the turbid serum, a whitish cloud was immediately formed. The 2 fluids were then stirred together; on which the mixture became so opake, that the printed letters on the card could no longer be seen. I have not hitherto been able to devise any experiment which shows the exact change induced by common air; but it is evident that air must also, in some way, increase the opacity of blood, since it can by no other means increase the reflexion of light from the interior parts of that body.

This theory explains another fact respecting the colour of blood, which might otherwise seem unaccountable. If a small quantity of a concentrated mineral acid be applied to a piece of dark crassamentum, the parts touched by it will for an instant appear florid; but the same acid, added to a solution of the red matter in water, do nothing more than destroy its colour. On examining the crassamentum, a reason for this difference of effect is discovered; for the spots, on which the acid was dropped, are found covered with whitish films. From which it seems evident, that the acid had occasioned an increase of opacity in the crassamentum, more quickly than it had destroyed its colour; and that the red matter, from having been in consequence seen by a greater quantity of light, had in that short interval appeared more florid than formerly.

The change which, I think I have proved to take place in blood, when its colour is brightened by common air and the neutral salts, is similar to that which occurs to cinnabar in the making of vermillion. This pigment, it is known, is formed from cinnabar, merely by subjecting it to a minute mechanical division. But the effect of this division is, to interpose among its particles, an infinite number of molecules of air, which, now acting as opake matter, increase the reflection of light from the interior parts of the heap, and by this means occasion the whole difference of appearance observed between those 2 states of the same chemical body.

I expect however it will be said, in opposition to what I have advanced, that granting an increased reflection of light takes place from the interior parts of blood, in consequence of the application of common air and the neutral salts, still this is not a sufficient cause for the production of the colour which they occasion; for the colour of blood, after those substances have acted on it, is a scarlet, which, agreeably to the observation of Dr. G. Fordyce, (Elements of the Practice of Physic, p. 13) differs not only in brightness, but also in kind, from the ordinary colour of that fluid, which is a Modena red.

My answer is, that there are examples, besides that to which the objection is made, of dark blood appearing florid, merely from its colouring matter being seen by means of an increased quantity of light. One is afforded by rubbing a piece of the darkest crassamentum with a proper quantity of serum; for a mixture is thus formed, in a few seconds, possessing a colour similar to that which is given to crassamentum by common air. But here we certainly do nothing more than interpose, among the red globules, a number of the less dense particles of serum; which, in their present situation, act as opake matter, and consequently increase the internal reflections. A 2d example occurs, when we view, by transmitted light, the fine edges and angles of a piece of crassamentum in water; for, in this situation, their colour appears to be a bright scarlet, though all the other parts of the same mass are black. These facts seem sufficient to prove, that the immediate cause I have assigned for the production of the florid appearance in blood, which has been exposed to the action of common air and neutral salts, is adequate to the effect; but I shall advance a step farther, and show how the Modena red is converted into a scarlet. Blood, as I have found by experiment, is one of those fluids which Sir Isaac Newton has observed appear yellow, if viewed in very thin masses; book 1, part 2, prop. 10. When therefore a number of opake particles are formed in it, by the action of common air and the neutral salts, many of them must be situated immediately beneath the surface. The light reflected by these will consequently be yellow; and the whole effect of the newly-formed opake particles, on the appearance of the mass, will be the same, as if yellow had been added to its former colour, a Modena red. But Modena red and yellow are the colours which compose scarlet; Fordyce's Elements of the Practice of Physic, p. 14.

I shall now relate the cautions to be observed in making the experiments, which are described in the beginning of this paper. The first is, that the blood should be newly drawn, and the weather cool. For as the solution of the red matter is not to be filtred, but must become transparent by the gradual subsiding of whatever may render it turbid, if the blood be old, or the weather warm, it will often assume, before it be clear, a dark and purplish hue. When exposed in this state to the atmosphere in a broad and shallow vessel, its colour changes to a bright red, which however is not brighter than the proper colour of the solution. The dark purplish hue seems owing to some modification of sulphur; for the solution possessing it smells like hepatic air, particularly when agitated, and tarnishes silver held over it. Neutral salts produce no change on this colour.

The 2d caution is, that the neutral salts be not added to the red solution, except when perfectly transparent; for if it be not so, the salts will render it more turbid, and the mixture will appear brighter, if seen by reflected light. The last I shall note is, that the red solution ought to be poured gently from the vessel in which it has been made. If it be not, as it is a mucilaginous liquor, it is apt to entangle small particles of air, which by acting as opake matter, will for some time alter the appearance of the solution.

I proceed next to offer a few observations on the cause of the red colour of blood. It has of late been very generally supposed, that blood derives its colour from iron. As far as I know however, no other argument has been given in support of this opinion, than that the red matter is found to contain that metal. But there is certainly no necessary connection between redness and iron; since this metal exists in many bodies of other colours, and even in various parts of animals without colour, as bones and wool. More direct reasons however may be given for rejecting this opinion. 1. I know of no colour, arising from a metal, which can be permanently destroyed by exposing its subject, in a close vessel, to a heat less than that of boiling water. But this happens with respect to the colour of blood. 2. If the colour from a metal, in any substance, be destroyed by an alkali, it may be restored by the immediate addition of an acid; and the like will happen from the addition of a proper quantity of alkali, if the colour has been destroyed by an acid. The colour of blood, on the contrary, when once destroyed, either by an acid or an alkali, can never be brought back. 3. If iron be the cause of the red colour of blood, it must exist there in a saline state, since the red matter is soluble in water. The substances therefore, which detect almost the smallest quantity of iron in such a state, ought likewise to demonstrate its presence in blood; but on adding Prussian alkali, and an infusion of galls, to a very saturate solution of the red matter, I could not observe, in the former case, the slightest blue precipitate, or in the latter, that the mixture had acquired the least blue or purple tint.

On the whole it appears to me, that blood derives its colour from the peculiar organization of the animal matter of one of its parts; for whenever this is destroyed, the colour disappears, and can never be made to return; which would not I think be the case, if it depended on the presence of any foreign substance whatever. I shall conclude this paper with relating several miscellaneous facts respecting the colour of blood, and some conclusions which may be formed from them.

Dr. Priestley has mentioned, (Phil. Trans., 1776, p. 246) that the only animal fluid, besides serum, which he found to transmit the influence of common air to blood, was milk. But I have observed, that the white of an egg possesses the same property, notwithstanding its great tenacity. Now as serum contains an animal substance very similar to the white of eggs, it occurred to me as a question, whether, in transmitting the influence of air to blood, it acts by its salts only, or partly by means of the substance of which I have just spoken. I took therefore a quantity of urine, which is known to contain nearly the same salts as serum, and having added to it as much distilled water as rendered its taste of the same pungency as that of serum, I poured the mixture on a piece of dark crassamentum of blood.

I then put to another piece of the same crassamentum an equal quantity of serum, and exposed both parcels to the atmosphere. The result was, that the blood in the diluted urine did not become nearly so florid as that in the serum. I have found also, that a solution of sugar in water conveys the influence of air to blood; from which it seems probable, that milk owes its similar property to the saccharine matter which it contains. Black blood exposed to the atmosphere under mucilage of gum arabic, does not become florid.

It has been said, (Fordyce's Elements of the Practice of Physic, p. 14), that neither serum, nor solutions of the neutral salts, dissolve the red matter of blood. But this induction has been made from too small a number of experiments. For saturate solutions of all the neutral salts, which I have tried, will extract, though slowly, red tinctures from blood, some of which are very deep; and neither they, nor serum, added in any proportion to a solution of the red matter in water, alter its colour or transparency, except by diluting it. The following experiments however, will place this point in a clearer light.

I added 1 dr. of distilled water to 1 oz. of serum, and poured the mixture on a small piece of crassamentum. On an equal piece of crassamentum I poured 1 dr. of water, and after some time added 1 oz. of serum. Each parcel therefore contained the same quantity of crassamentum, serum, and water; but the crassamentum on which the mixture of serum and water had been poured, communicated no tinge to it; while the other piece, to which water had been first applied, and afterwards serum, gave a deep colour to the fluid above it. I made similar experiments with crassamentum, water, and a dilute solution of a neutral salt, which were attended with the same results.

Since then neither serum, nor a dilute solution of a neutral salt, will extract colour from blood, though they are both capable of dissolving the red matter, when separated by water from the other parts of the mass, it follows, in my opinion, that what are called the red globules consist of 2 parts, one within the other, and that the outer, being insoluble in serum or dilute solutions of neutral salts, defends the inner from the action of those fluids. It is remarkable, that microscopical observations led Mr. Hewson to the same conclusion, viz. that the red globules consist of 2 parts, (Hewson's works, vol. 3, p. 17) which, according to him, are an exterior vesicle, and an interior solid sphere. But the same writer, on the authority of other microscopic experiments, asserts that the vesicles are red. If they be so, there must exist 2 red matters in the blood, possessing different chemical properties; which is certainly far from being probable. The exterior part of the globule appears to be that ingredient of the blood on which common air and the neutral salts produce their immediate effect, when they render the whole mass florid; for I have shown that they do not act on the red matter itself, and I have not found that they occasion any change in coagulated lymph or serum. The only matter then which remains to be operated on, is that which I have mentioned. It seems evident also, from what has been just stated, that there exists an animal matter in the blood, different from the coagulable lymph, the coagulable part of the serum, the putrescent mucilage, and the red particles, which I believe are all the kinds it has hitherto been supposed to contain.

The microscopical observations of Mr. Hewson appear likewise to furnish a reason, why both water, and a saturate solution of a neutral salt, can extract colour from the red globules, though a mixture of those fluids be incapable of the same effect. For water applied to the red globules, separates the exterior vesicles from the red particles, which are therefore now open to the action of any solvent.* The addition however of a small quantity of a neutral salt to the water, enables the vesicles to preserve their shape, and to retain the inner spherules.† On the addition of a greater quantity of salt, the vesicles contract, and apply themselves closely to the red particles within.‡ Thus far Mr. Hewson's observations extend. Let it now be supposed that the vesicles contract still more, from a further addition of salt to the water; the consequence must be, that as the internal particles are incompressible, the sides of the vesicles will be rent, and their contents exposed to the action of the surrounding fluid. Both water and a strong solution of a neutral salt may therefore destroy the organization of the vesicles, though in different ways, and thus agree in bringing the red matter in contact with a solvent; while a mixture of those 2 fluids, namely, a dilute solution of a neutral salt, will, by hardening the vesicles, increase the defence of the red matter against the action of such substances as are capable of dissolving it. But all reasoning founded on experiments with microscopes ought perhaps to be regarded as, in great measure, conjectural.

XX. An Account of the Trigonometrical Survey, carried on in 1795, 1796, *by Order of the Marquis Cornwallis, Master General of the Ordnance. By Col. Edw. Williams, Capt. Wm. Mudge, and Mr. Isaac Dalby. Communicated by the Duke of Richmond, F. R. S.* p. 432.

According to the resolution expressed in the account of the trigonometrical survey, printed in the Phil. Trans. for the year 1795, we now communicate to the public, through the same channel, a further account of its progress. On referring to the above paper, it will be found that, for the prosecution of this undertaking, a design was formed of proceeding to the westward, with a series of triangles, for the survey of the coast. This intention has been carried into effect; and as the small theodolite, or circular instrument, announced in our former communication as then in the hands of Mr. Ramsden, was finished early in the summer of 1795, we are enabled to give a series of triangles extending, in conjunction with those before given, from the Isle of Thanet, in Kent, to the Land's End. In the composition of the following account, we have adhered to the plan adopted in the last, of giving the angles of the great triangles, with their variations; and we have, with as much

* Hewson's Works, vol. iii. p. 17.——† Ibid. p. 40.——‡ Ibid. p. 31.—Orig.

brevity as possible, inserted a narrative of each year's operations. This will be found however to extend only to the 1st part, or that containing the particulars of the survey in which the great instrument alone was used. The remaining contents of this portion of the work, are necessarily confined to the angles of the principal and secondary triangles, with the calculations of their sides, in feet; and likewise such data as have no connection with the computations of latitudes and longitudes.

The 2d part contains an account of a survey carried on in Kent, in the years 1795 and 1796, with the small instrument, for completing a map of the eastern and southern parts of that county, for the use of the Board of Ordnance, and the military commanders on the coast. In the 1st part will be found an article, for which we are indebted to Dr. Maskelyne, the Astronomer Royal. It contains his demonstration of M. de Lambre's formula, in the Connoissance des Temps of 1793, for reducing a distance on the sphere to any great circle near it, or the contrary. The practical rule thence derived, for reducing the angles in the plane of the horizon, to those formed by the chords, is very useful, and will considerably abridge the trouble which must necessarily arise in computing the chord corrections by any former method.

Some angles are next registered as taken at the best stations in Devonshire, and other points in the west of England, in the year 1795. The survey then extends farther westwards in the next year 1796. It is here observed that, to make observations for the purpose of hereafter determining the longitude and latitude of the Lizard, was a principal object in this year's operations; and as this headland seems to offer itself as very convenient for a station, it will be right to assign the reasons for not having chosen one upon it.

As no other spot but Hensbarrow Beacon could be found in that part of Cornwall proper for a station, it became necessary to fix on the Deadman, or Dodman, for another point in the series. From this place no part of the land within 4 miles of the Lizard can be seen, as the high ground about Black Head, which is to the eastward of the latter, is nearly in a line between them, and is also much higher than both. It will be perceived however, that no evil can result from the want of such a station, as the light-houses and the naval signal-staff at the Lizard, have been intersected from several stations. The precise spot on which Mr. Bradley made his observations in the year 1769, for ascertaining the longitude and latitude of this headland, was pointed out by the person having the care of the light-houses, who well remembered the common particulars relating to his operations: such measurements were made from the light-houses to this spot, as may enable us, at a future period, to compare the results from the data afforded by the trigonometrical operation, with those deduced from the astronomical observations made by the above gentleman. It may be also mentioned, that angles were at the same time taken at the western light-house and signal-staff, for the purpose of finding the situation of the Lizard Point.

We are now to speak of the most important business performed this year; that of making observations to determine the distance of the Scilly Isles from the Land's End. To do this as accurately as possible, it became necessary to find stations affording the longest base. The hill near Rosemergy, called the Watch, and the station near St. Buryan, are certainly the most advantageous places, because all the islands can be seen from both; but we could not avail ourselves of the former, as difficulties almost insuperable would have attended an attempt to get the instrument upon it. Another station was therefore selected, on Karnminnis, near St. Ives; a spot as well situated as the place spoken of, provided all the islands could be seen: this however does not prove to be the case, St. Martin's Day-Mark being the only object in the Scilly Islands visible from Karnminnis.

From the stations near the Land's End, Sennen, and Pertinney, as well as that above-mentioned, St. Buryan, St. Agnes' Light-house, and 2 objects in St. Mary's, were observed; and as the means by which all their distances are determined, except those of the Day-Mark, from the shortness of the bases, though the longest that could be found, are exceptionable, while we were engaged in that part of the operation now spoken of, the air was so unusually clear, that we could sometimes, with the telescope of the great theodolite, discover the soldiers at exercise in St. Mary's island. In the present and former seasons, such stations were selected and observed, as were judged to be proper for the future use of the small instrument; and as we had experienced, in the early stage of this survey, much delay and disappointment from the white lights not being always seen when fired on distant stations, we have since substituted lamps and staffs in their stead. The operations of the present year were continued till October, when the party returned to London.

A list is here given of the angles observed in this quarter in the year 1796. After which is set down the demonstration of M. de Lambre's Formula in the Connoissance des Temps of 1793, for reducing a distance on the sphere to any great circle near it, or the contrary; by Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal.

Put $\mathrm{A} =$ angle subtended by 2 terrestrial objects; $a =$ the same reduced to the horizon; H, h the 2 apparent altitudes: if either is a depression, it must be taken negative. By spherics, $c, \mathrm{A} = c, a \,.\, c, \mathrm{H} \,.\, c, h + s, \mathrm{H} \,.\, s, h$.

Put $\mathrm{A} = a + da$, where da signifies $\mathrm{A} - a$, and not their differential.

By trigonometry $c, \mathrm{A} = c, a \,.\, c, da - s, a \,.\, s, da = c, a \times (1 - vs, da) - s, a \,.\, s, da = c, a - c, a \times 2s^2, \frac{1}{2} da - s, a \,.\, s, da$ (by theorem above) $= c, a \,.\, c, \mathrm{H} \,.\, c, h + s, \mathrm{H} \,.\, s, h \because s, da + 2s^2, \frac{1}{2}da \,.\, 't, a = 't, a - 't, a \,.\, c, \mathrm{H} \,.\, c, h - s, \mathrm{H} \,.\, s, h \times$ cosec. $a = t', a - 't, a \times (\frac{1}{2}c, (\mathrm{H} - h) + \frac{1}{2}c, (\mathrm{H} + h) -$ cosec. $a \times (\frac{1}{2}c, (\mathrm{H} - h) - \frac{1}{2}c, (\mathrm{H} + h))$ because $t', a = \frac{1}{2}'t, \frac{1}{2}a - \frac{1}{2}t, \frac{1}{2}a$; and cosec. $a = \frac{1}{2}'t\frac{1}{2}a + \frac{1}{2}t, \frac{1}{2}a) = (\frac{1}{2}t', \frac{1}{2}a - \frac{1}{2}t, \frac{1}{2}a) \times (1 - \frac{1}{2}c, (\mathrm{H} - h) - \frac{1}{2}c, (\mathrm{H} + h)) - (\frac{1}{2}'t, \frac{1}{2}a + \frac{1}{2}t, \frac{1}{2}a) \times (\frac{1}{2}c, (\mathrm{H} - h) - \frac{1}{2}c, (\mathrm{H} + h)) = \frac{1}{2}'t, \frac{1}{2}a \times (1 - c, (\mathrm{H} - h)) - \frac{1}{2}t, \frac{1}{2}a \times (1 - c, (\mathrm{H} + h)) = \frac{1}{2}'t, \frac{1}{2}a \times vs, (\mathrm{H} - h) - \frac{1}{2}t, \frac{1}{2}a \times vs, (\mathrm{H} + h) = 't, \frac{1}{2}a \,.\, s^2, \frac{1}{2}(\mathrm{H} - h) - t, \frac{1}{2}a \,.\, s^2, \frac{1}{2}(\mathrm{H} + h)$.

Put $n = {}'t, \frac{1}{2}a \,.\, s^2, \frac{1}{2}(\text{H} - h) - t, \frac{1}{2}a \,.\, s^2, \frac{1}{2}(\text{H} + h)$.

We shall have

$s, da + 2s^2, \frac{1}{2}da \,.\, {}'t, a = n$;

and $s, da = n - 2s^2, \frac{1}{2}da \,.\, t', a$.

But $s, da = 2s, \frac{1}{2}da \,.\, c, \frac{1}{2}da$;

$\therefore s, \frac{1}{2}da = \dfrac{s, da}{2c, \frac{1}{2}da} = \dfrac{n - 2s^2, \frac{1}{2}da \,.\, {}'t, a}{2c, \frac{1}{2}da}$;

and $s, da = n - 2s^2, \frac{1}{2}da \,.\, t', a = n - 2t', a \left(\dfrac{n - 2s^2, \frac{1}{2}da \,.\, {}'t, a}{2c, \frac{1}{2}da}\right)^2$,

because $\left(\dfrac{n - 2s^2, \frac{1}{2}da \,.\, ta}{2c, \frac{1}{2}da}\right)^2 = \dfrac{n - 4n \,.\, s^2, \frac{1}{2}da \,.\, t', a + 4s^4, \frac{1}{2}da \,.\, {}'t^2, a}{4 \times (1 - s^2, \frac{1}{2}da)}$

$= \dfrac{n^2}{4} + \dfrac{n^2 s^2, \frac{1}{2}da}{4} - n \,.\, s^2, \frac{1}{2}da \,.\, {}'t, a - n \,.\, s^4, \frac{1}{2}da \,.\, {}'t, a$

$+ s^4, \frac{1}{2}da \,.\, {}'t^2, a = n - \frac{1}{2}n^2 \,.\, t', a - \frac{1}{2}n^2 \,.\, {}'t, a \,.\, s^2, \frac{1}{2}da$

$+ 2n \,.\, {}'t^2, a \,.\, s^2, \frac{1}{2}da + 2n't^2, a \,.\, s^4, \frac{1}{2}da - 2't^3, a \,.\, s^4 + \frac{1}{2}da$,

by substituting for $s, \frac{1}{2}da$ its near value n,

$= n - \frac{1}{2}n^2 t', a - \dfrac{n^4 t', a}{8} + \frac{1}{2}n^3 t^2, a + \frac{1}{8}n^{5\prime} t^2, a - \frac{1}{8}n^{4\prime} t^3, a$,

where the last term but one containing the 5th power of n may be rejected, as it has been omitted by M. de Lambre.

As da is always very small, the arc da in parts of the radius, unity, $= s, da$ in parts of the same radius, therefore $s, 1'' : 1'' :: s, da$ (in parts of radius unity) : $\dfrac{1}{s, 1''} \times s, da = da$ in seconds, $= \dfrac{1''}{s, 1''} \times (n - 2s^2, \frac{1}{2}da \,.\, {}'t, a) = \dfrac{1''}{s, 1''} \times (n - da \,.\, s, \frac{1}{2}da \,.\, {}'t, a) = \dfrac{1'' \times n}{s, 1''} - \dfrac{1'' \times da \,.\, s, \frac{1}{2}da \,.\, {}'t, a}{s, 1''}$; $\because$ if we put $n = \dfrac{1''}{s, 1''} \times t', \frac{1}{2}a \,.\, s, \frac{1}{2}(\text{H} - h) - t, \frac{1}{2}a \,.\, s^2, \frac{1}{2}(\text{H} + h)$, and $da =$ a number of seconds, we shall have $da = n - da \,.\, s, \frac{1}{2}da \,.\, {}'t, a$; and, for the most part, without any sensible error, $da = n - n \,.\, s, \frac{1}{2}n \,.\, {}'t, a$.

* Table 1 contains $\dfrac{1'' \times t, \frac{1}{2}a}{10000}$ and $\dfrac{1'' \times {}'t, \frac{1}{2}a}{10000}$; table 2 contains $10000 \times s^2, \frac{1}{2}(\text{H} \mp h)$. Table 3 contains the term $- n \,.\, s, \frac{1}{2}n \,.\, {}'t, a$. The argument on the side is a, and that on the top is n or the result found by the help of the first 2 tables. If this correction should be considerable, with the value of da, found after this correction has been applied, enter table 3, again at the top, and with a on the side as before; the number now found subtracted from n will give the correct value of da.

By the investigation, $da = \frac{1}{2}{}'t, \frac{1}{2}a \,.\, vs\,(\text{H} \mp h) - \frac{1}{2}t, \frac{1}{2}a \,.\, vs, (\text{H} \pm h) - vs, da \,.\, {}'ta$, where the upper or lower signs are to be used, according as the objects are on the same, or on contrary sides of the great circle to which they are referred; the 3d term will be negative or positive, according as a is less or more than 90°.† If da should come out negative, A will be less than a, or a greater than A. In the case

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* It does not appear what tables are here meant.

† Compute the two, which will give the approximate value of *da*, and make use of them in computing the 3d term; and join the 3 terms together according to their signs, which will give *da* still nearer; and, if this should prove considerable, compute the 3d term a 2d time with the new value of *ad*.—Orig.

of reducing a spheric angle to the angle between the chords, the spheric angle will be represented by a, and the angle between the chords by $\mathrm{A} = a + da$; and $da = \frac{1}{2}'t, \frac{1}{2}a \,.\, vs, (\mathrm{H} \backsim h) - \frac{1}{2}t, \frac{1}{2}a \,.\, vs, (\mathrm{H} + h) - vs, da \,.\, 't, a$ (if D, d represent the arcs to the chords) $= \frac{1}{2}'t, \frac{1}{2}a \,.\, vs, \frac{1}{2} (\mathrm{D} \backsim d) - \frac{1}{2}t, \frac{1}{2}a \,.\, vs, \frac{1}{2} (\mathrm{D} + d) - vs, da \,.\, 't, a$; $\mathrm{A} = a - (\frac{1}{2}t, \frac{1}{2}a \,.\, vs, \frac{1}{2} (\mathrm{D} + d) - \frac{1}{2}'t, \frac{1}{2}a \,.\, vs \frac{1}{2} (\mathrm{D} \backsim d) - vs, da \,.\, 't, a$; where the last term will change its sign to affirmative, if a is greater than 90°. If the answer is required in seconds, the correction must be multiplied by 206265, the number of seconds in an arc = radius. The calculation will be easily made by logarithms.

Practical rule.—The practical rule deduced from the above conclusions is the following, and given in the words of the Astronomer Royal. "To the constant logarithm 5.0134 add $\mathrm{L} \,.\, t, \frac{1}{2}a$ and $\mathrm{L} \,.\, vs\ (\mathrm{D} + d\,;)$ the sum diminished by 20 in the index is the logarithm of the first part of the value of da in seconds, which is always negative. To the constant logarithm 5.0134 add $\mathrm{L} \,.\, t', \frac{1}{2}a$, and $\mathrm{L} \,.\, vs, \frac{1}{2} (\mathrm{D} \backsim d)$, the sum diminished by 20 in the index, is the logarithm of the 2d part in seconds, which is always affirmative. These 2 joined together, according to their proper signs, will give the approximate value of da. To its logarithmic versed sine, add $\mathrm{L} \,.\, t', a$ and constant logarithm 5.0134, the sum, diminished by 20 in the index, will be the logarithm of the 3d part in seconds, which will be negative or affirmative, according as a is less or more than 90°. This applied according to its sign, to the approximate value of da, will give the correct value of da. If the 3d part comes out considerable, it should be computed anew with the last value of da. The value of da, finally corrected, applied to a, will give A, the angle between the chords."

In the application of the above rule, to the computation of such corrections as may be applied to the angles of any triangles in this survey, it is manifest that the last step may be entirely neglected, on account of the smallness of the approximate value of da, whose versed sine is one of the arguments. Being therefore confined to the use of the first 2 steps, the operation is very short. An example is here given in the computation of the correction for reducing the angle at Chanctonbury Ring in the 39th triangle, given in the last account, to that formed by the chords.

EXAMPLE.

Constant logarithm	5.0134	....	5.0134
Log. tang. $\frac{1}{2}a = 78° 56'$..........	10.7112	Log. co. tang. $\frac{1}{2}a$............	9.2887
Log. $vs \,.\, \frac{1}{2} (\mathrm{H} + h) = 19' 53''.5$ ..	5.2237	Log. $vs \,.\, \frac{1}{2} (\mathrm{H} - h) = 5' 53''.5$	4.1669
	0.9483 + .8″.88		−2.4690 + 0″.03

1st correction − 8.88
2d correction + 0.03

− 8.85 the correction required.

After this follows a calculation of the sides of the great triangles, carried on from the termination of the series, published in the Philos. Trans. of 1795, along the coasts of Dorset, Devon, and Cornwall, to the Land's End; not necessary to be here inserted. Then follows a series of observations of the angular elevations and depressions of the several stations, above and below each other, taken in minutes and seconds; from which is derived the following table of terrestrial refractions.

Between	Of the contained arc.	Between	Of the contained arc.
Maker and Kit Hill	$\frac{1}{7}$	Maker and Carraton Hill	$\frac{1}{15}$
Butterton and Kit Hill	$\frac{1}{8}$	Karnbonellis and the Deadman	$\frac{1}{15}$
Bindown and Lansallos	$\frac{1}{9}$	Karnbonellis and St. Agnes' Beacon	$\frac{1}{15}$
Nine Barrow Down and Black Down	$\frac{1}{10}$	Karnminnis and St. Buryan	$\frac{1}{15}$
Maker and Lansallos	$\frac{1}{10}$	Hensbarrow and Bodmin Down	$\frac{1}{15}$
Maker and the Bolt Head	$\frac{1}{10}$	Lansallos and Bodmin	$\frac{1}{15}$
Carraton Hill and Bindown	$\frac{1}{11}$	Butterton and the Bolt Head	$\frac{1}{15}$
Karnbonellis and St Buryan	$\frac{1}{11}$	Haldon and Charton Common	$\frac{1}{17}$
Maker and Bindown	$\frac{1}{12}$	Rippin Tor and Cawsand Beacon	$\frac{1}{17}$
Hensbarrow and the Deadman	$\frac{1}{12}$	Black Down and Bull Barrow	$\frac{1}{18}$
St. Agnes' Beacon and the Deadman	$\frac{1}{12}$	Black Down and Pilsden Hill	$\frac{1}{18}$
St. Agnes' Beacon and Karnminnis	$\frac{1}{12}$	Black Down and Charton Common	$\frac{1}{18}$
Dumpdon and Cawsand Beacon	$\frac{1}{13}$	Lansallos and Hensbarrow	$\frac{1}{18}$
Haldon and Cawsand Beacon	$\frac{1}{13}$	Rippin Tor and Haldon	$\frac{1}{19}$
Kit Hill and Bindown	$\frac{1}{13}$	Butterton and Furland	$\frac{1}{19}$
Carraton Hill and Hensbarrow	$\frac{1}{13}$	Butterton and Rippin Tor	$\frac{1}{21}$
Lansallos and the Deadman	$\frac{1}{13}$	Kit Hill and Carraton	$\frac{1}{26}$
Hensbarrow and St. Agnes' Beacon	$\frac{1}{13}$	Pilsden Hill and Charton Common	$\frac{1}{28}$
Karnbonellis and Karnminnis	$\frac{1}{14}$	Wingreen and Bull Barrow	$\frac{1}{31}$
Furland and Haldon	$\frac{1}{15}$	Lansallos and Carraton Hill	$\frac{1}{34}$
Butterton and Maker	$\frac{1}{15}$	Haldon and the Horizon of the Sea	$\frac{1}{11}$
Butterton and Carraton Hill	$\frac{1}{15}$	Pilsden Hill and the Horizon of the Sea	$\frac{1}{11}$

The mean refractions were found by the following rules. 1. Reduce the elevations, or depressions, to the place of the axis of the telescope at each station, by adding or subtracting, as the case may require, the angle at the place of observation, subtended by the vertical height between the object, whose elevation or depression was observed, and the axis of the telescope when at that station.* 2. Then, if both are depressions, subtract their sum from the contained arc, and half the remainder is the mean refraction. 3. If one is a depression and the other an elevation, take their difference. Then, if the depression is greater than the elevation, subtract the difference from the contained arc, and half the remainder is the mean refraction. But if the elevation is greatest, add the difference to the contained arc, and half the sum is the mean refraction.

Next follows a table containing the heights of the stations, viz.

Stations.	Heights. Feet.	Stations.	Heights. Feet.
Black Down	817	Bolt Head	430
Charton Common	582	Kit Hill	1067
Little Haldon	818	Bindown	658
Rippin Tor	1549	Carraton Hill	1208
Furland	589	Lansallos	514
Butterton	1203	Bodmin Down	649
Maker Heights	402	Hensbarrow Beacon	1026
Bull Barrow	927	The Deadman	379
Mintern Hill	891	St. Agnes' Beacon	599
Pilsden Hill	934	Karnbonellis	822
Dumpdon	879	Karnminnis	805
Cawsand Beacon	1792	St. Buryan	415

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* For example. At the station on Hensbarrow, the ground at Bodmin Down was depressed 31′ 27″: the distance of those stations is 47337 feet; and the axis of the telescope was 5½ feet above the ground:

After these occurs a long series of the secondary triangles, in which 2 angles only have been observed; unnecessary to be reprinted. Then follow triangles for ascertaining the distances of the Eddystone Light-house, from the Flagstaff of Plymouth garrison, and the Rame-head. The ball on the lantern of the light-house was observed from the stations on Butterton, Kit Hill, and Carraton Hill; and as much uncertainty has heretofore existed, with respect to a knowledge of its true distance from any point in the neighbourhood of Plymouth, observations were made on various arcs of the circle of the instrument, at the first 2 stations. Whence it results that, with the distance of the Eddystone Light-house from Kit Hill, and also that of the Flagstaff in Plymouth garrison from the same station, we find the distance from the Light-house to the Flagstaff = 73061 feet;* the observed angle being 29° 42′ 34″: and, computing with the data obtained from the last triangle, and the 223d, with the observed angle at Carraton Hill = 16° 22′ 1″, we get 49435 feet for the distance of the Eddystone Light-house from the building on Rame-head. It may be proper to observe, that the Eddystone Light-house is nearer to the Rame-head than to any other point on the coast. Next follow triangles for ascertaining the situations of the Lizard Light-houses; and the Lizard Point. Hence is inferred, that the distance from Karnbonellis to Pertinney 101474 feet.

From the last 2 triangles we obtain 79640 feet for the mean distance between the Lizard Signal-staff and the station on Karnbonellis. Computing with this distance, and also that from the Western Light-house to the same station, with the observed angle 0° 31′ 8″, we get 1857 feet for the distance between those objects.

For the purpose of ascertaining the situation of the Lizard point, 2 angles in the following triangle were observed with a sextant, viz.

Naval Signal-staff	77°	4′
Western Light-house	60	50
Lizard Point		

These, with the computed distance from the Signal-staff to the Light-house, give the distance of the Lizard Point from the Signal-staff 2419, Light-house 2700 feet. Hence the distance of the point from the station on Karnbonellis is 81085 feet, the angle at that station, between the Lizard Point and Western Light-house, being 1° 53′ 47″.

Then follow triangles for finding the distances of the Day-mark, St. Agnes' Light-house, and other objects in the Scilly Isles, from particular stations in the

therefore, as 47337 : radius :: 5½ feet : tang. 24″ the angle subtended by 5½ feet at that distance; which, taken from 31′ 27″, gives 31′ 3″ for the depression of the place of the axis, instead of the ground. Again, at Bodmin Down, the ground at Hensbarrow was elevated 23′ 57″, to which adding 24″, we have 24′ 21″ for the elevation of the place of the axis.—Orig.

* On referring to the late Mr. Smeaton's Narrative of the Building of the Eddystone Light-house, it will be found, that, from a trigonometrical process, founded on two bases measured on the Hoe, among other deductions, he concluded the distance between the above objects was 73464 feet; being 403 greater than the distance found by the above computation.—Orig.

west of Cornwall. Whence it is inferred, that the distance from the Day-Mark to Karnminnis, as obtained from the 309th triangle, is 190985 feet, and by the 310th, 190989 feet, which differs only 4 feet from the former; and by the 310th and 311th triangles, the difference of the distances from the same object, to the station on Pertinney, is 17 feet; which, allowing for the shortness of the bases, must be considered as trifling. We may presume therefore, that had not the Day-Mark been seen from Karnminnis, but from Sennen and Pertinney only, the observations from which the angles of the 311th triangle are derived, would have afforded the means of computing the distance with sufficient precision. In like manner the 312th and 313th triangles seem to prove, that the observations made to St. Agnes' Light-house were sufficiently accurate, as there is a difference only of 16 feet between the distances of the Light-house from Pertinney. The ball on the top of the Light-house was the object always observed; and the Day-Mark being pyramidical, we had the means of making the observations at the different stations to the same point of this building.

Of the distances of the objects in the Scilly Isles, intersected from the stations in the west of Cornwall, from Sennen steeple; the stone near the Land's End; and the Longship's Light-house. It is here observed, that as the observations made to the Day-Mark, and St. Agnes' Light-house, may be supposed sufficiently accurate; and the ball on the top of the Longship's Light-house was also observed under favourable circumstances, it will be proper to apply the corrections to the horizontal angles, in order to obtain those formed by the chords. Taking, therefore, Pertinney as the angular point, and computing with the following data, viz.

Station on Pertinney from ....
Day-Mark .... = 154551
St. Agnes' Light-house.. = 182207
Longship's Light-house.. = 27883
Feet. And

the angle at Pertinney, augmented for calculation, between the Longship's Light-house and
the Day-Mark ...... = 12° 17′ 30″
St. Agnes' Light-house = 6 15 25
We get the distance of

the Longship's Light-house from ....
the Day-Mark ...... = 127446 feet = 24.14
St. Agnes' Light-house = 154519 feet = 29.06
Miles.

Calculating also, with the distances of the two other objects in the Scilly Isles, and likewise those of Sennen steeple, and the stone near the Land's End from Pertinney, with the included angles at the same station, we get

		Feet.	Miles.
Sennen steeple from	Day-Mark	= 139521	= 26.43
	St. Agnes' Light-house	= 166255	= 31.49
	Flagstaff in St. Mary's	= 157912	= 29.95
	Windmill in St. Mary's	= 155299	= 29.41
Stone near the Land's End from	Day-Mark	= 135343	= 25.63
	St. Agnes' Light-house	= 162100	= 30.7
	Flagstaff in St. Mary's	= 153744	= 29.11
	Windmill in St. Mary's	= 151138	= 28.63

Of the Scilly Isles, Menawthen is the nearest to the Land's End, being about $1\frac{9}{10}$ miles eastward of the Day-Mark; and the cluster of rocks, called the Bishop and his Clerks, the most remote, being $3\frac{1}{3}$ miles west of St. Agnes' Light-house. Combining therefore the above particulars with those distances, we may conclude, that the nearest part of the Scilly Isles is about 24.7 miles from the Land's End, and the farthest nearly 34.

This account of the trigonometrical survey then relates to that carried on in Kent, in the years 1795 and 1796, with the small circular instrument. The instrument used in this survey was announced in the Philos. Trans. for 1795. It was made by Mr. Ramsden; and was about half the size of his large theodolite, or circular instrument, with which were taken the horizontal angles, but nearly similar to it in all its parts. This instrument, on account of its portable size, may very readily be taken to the tops of steeples, towers, &c. and is therefore extremely well adapted to the uses for which it was intended. Then follows a list of the situations of the stations on which observations were made with the small circular instrument, in the summer of the year 1795; with the triangles for determining the distances of the stations. As the station on the Keep of Dover Castle, in 1787, was directly over the steps of the Turret, a new point was chosen about 6½ feet from the former, where the instrument could stand conveniently: this new point is about 2.8 feet farther from Folkstone Turnpike, and 1 foot farther from Paddlesworth, than the point marking the old station. From General Roy's account of the trigonometrical survey in 1787, we have

Dover Castle from Folkstone Turnpike	31554.6	feet.
from Paddlesworth	42561.2	

Now, augmenting those distances in the proportion of 141747 to 141753 (see Phil. Trans. vol. 80, p. 595, and the vol. for 1795, p. 508), we get 31556, and 42563 feet; to which adding 2.8, and 1, respectively, we have

The new point on Dover Castle from Folkstone Turnpike	31558.8	feet.
from Paddlesworth	42564	

In order to obtain the distance between Waldershare and Dover Castle from those new sides, or distances, the three angles of the following triangle were very carefully taken.

1	Dover Castle	3° 49′ 16″	3° 49′ 15″ for computation.
	Folkstone Turnpike	36 6 31	36 6 30
	Hawkinge	140 4 16	140 4 15

The 3d angles of the next 2 triangles were not observed:

2	Hawkinge	44° 23′ 30″
	Dover Castle	73 53 44
	Waldershare	61 42 46
3	Dover Castle	62 24 7
	Paddlesworth (the station of 1787)	32 36 9
	Waldershare	84 59 44

	Feet.	
By the first 2 triangles, Dover Castle from Waldershare	23019.4	23020.5 mean distance.
From the latter	23021.5	
And Hawkinge from Dover Castle	28976.	
Waldershare	31616.	

Finally are deduced the distances of the objects intersected in the survey with the small circular instrument, from the meridian of Greenwich, and from the perpendicular to that meridian. Also their latitudes and longitudes. At Folkstone turnpike, the bearing of the station on Dover Castle in 1787, from the parallel to the meridian of Greenwich is 65° 52′ 46″ N.E. The new point on the Keep is $6\frac{1}{4}$ feet north-eastward from the old one, which will subtend an angle at Folkstone turnpike of about 38″; therefore the new station bears 65° 52′ 8″ N.E. The bearing of the centre of Tenterden steeple from Allington Knoll, is nearly the same as that of the station in 1787, or 85° 47′ 25″ S.W: but the distances of those stations (Folkstone turnpike and Allington Knoll), from the meridian of Greenwich, and its perpendicular, are augmented in the proportion of 141747 to 141753, for obtaining the distances in the 3d and 4th columns of the following table: Folkstone turnpike being 274979 and 137220; and Allington Knoll 219935 and 144038 feet, respectively, from the meridian, and its perpendicular.

Bearings and Distances of the Stations.

Bearings from the Parallels to the Meridian of Greenwich.					Distances from meri.	Distances from perp.
At Folkstone turnpike.	°	′	″		Feet.	Feet.
Dover	65	52	8	NE	303780	124318
Hawkinge	29	45	38	NE	276605	134376
At Dover.						
Paddlesworth	81	30	42	SW	262004	130553
Waldershare	36	24	53	NW	290114	105792
Ringswold	30	21	52	NE	315783	103830
At Waldershare.						
Shore	39	54	35	NE	317545	72997
Mount Pleasant	11	56	19	NE	302716	46190
Wingham	16	36	24	NW	279533	70315
Hardres	74	21	9	NW	248180	94046
Hawkinge	25	17	53	SW		
Ringswold	85	37	43	NE		
Near the shore.						
Ringswold	3	16	15	SW		

Bearings from the Parallels to the Meridian of Greenwich.					Distances from meri.	Distances from perp.
Near the shore.	°	′	″		Feet.	Feet.
Mount Pleasant	28	56	58	NW		
At Mount Pleasant.						
Wingham	43	51	31	SW		
Chislet	82	23	48	SW	272918	50168
At Wingham.						
Chislet	18	10	37	NW		
Hardres	52	52	37	SW		
Beverley Park	77	3	23	NW	247060	62852
At Beverley Park.						
Hardres	2	3	23	SE		
At Allington Knoll.						
Tenterden	85	47	25	SW		
Westwell Down	32	34	49	NW	192302	100797
Wye Down	2	2	48	NE	221269	106701
Brabourn Down	22	37	5	NE	230102	119636

Interior Objects.

At Dover.						
St. Radigund's Abbey	88	5	4	NW	287597	123777
Hougham steeple	68	19	22	SW	288341	130455
Gunston steeple	3	43	2	NW	303189	115226
St. Margaret's steeple	51	54	43	NE	315148	115408
South Foreland lighthouse	70	10	31	NE	315145	120721
At Waldershare.						
Barham windmill	61	0	4	NW	278573	93852
Elham windmill	10	14	39	SW	284430	137246
Upper Deal chapel	63	17	33	NE	317056	92237
Deal Castle	66	9	16	NE	321842	91768
Watch-house near the shore	85	2	37	SE	321314	108498
Sandown Castle	55	51	56	NE	321721	84365
Walmer steeple	73	8	30	NE	318338	97239
Ripple steeple	70	1	50	NE	302867	97534
Waldershare steeple	64	52	20	NE	295235	103390
Eastry steeple	23	30	46	NE	300393	82166
Ash steeple	4	44	29	NE	293069	70165
Minster steeple	11	24	56	NE	301155	51113
Woard steeple	34	11	33	NE	308967	78042
Sandwich highest steeple	26	19	14	NE	307187	71279
Wingham steeple	20	6	36	NW	278007	72725
Goodneston steeple	19	16	21	NW	281915	82343

At Waldershare.						
Littlebourn steeple	27	39	59	NW	278100	70860
Canterbury cathedral	49	51	48	NW	248198	60458
At Ringswold.						
Mongeham steeple	21	30	34	NW	311611	93243
Norbourn steeple	31	52	45	NW	307623	90710
Woodnesborough steeple	29	51	29	NW	299693	75800
Near the Shore.						
Ramsgate windmill	12	13	43	NE	321363	50817
St. Lawrence steeple	7	30	6	NE	320817	48148
At Mount Pleasant.						
Birchington steeple	20	17	12	NW	298729	35403
St. Nicholas steeple	78	0	9	NW	286391	42721
Stormouth steeple	65	26	52	SW	283143	55132
At Wingham.						
The South Reculver	8	3	15	NW	274346	33663
Hearne windmill	34	59	11	NW	260191	42679
Blean steeple	76	41	11	NW	241261	61259
Wickham steeple	77	21	44	NW	270923	68384
Bridge windmill	55	21	3	SW	260132	83723
Nackington steeple	69	29	17	SW	249854	81418
Chillingdon windmill	28	0	30	SE	286591	83586
Preston steeple	5	6	13	NW	278891	63124
Shottenden windmill	83	42	1	SW	212206	77748
Ickham steeple	89	45	57	SW	271533	70348

Bearings from the Parallels to the Meridian of Greenwich.					Distances from meri.	Distances from perp.	Bearings from the Parallels to the Meridian of Greenwich.					Distances from meri.	Distances from perp.
At Hardres.	°	′	″		Feet.	Feet.	On Westwell Down.	°	′	″		Feet.	Feet.
Harbledown steeple....	14	15	0	NW	241955	69535	Kennington steeple	50	34	14	SE	204869	111131
Sturry steeple	15	26	36	NE	256619	63499	At Allington Knoll.						
West Stone-street windmill	35	46	24	SW	236870	109743	Great Chart steeple....	52	21	16	NW	190572	121389
							Westwell steeple......	31	46	42	NW	194208	102510
Stelling windmill......	26	1	10	SW	242003	106700	Pluckley steeple	53	27	50	NW	173511	109641
On Westwell Down.							Eastwell steeple	25	17	49	NW	200945	103951
Ashford steeple	24	53	15	SE	200728	118961	Charing steeple	37	58	49	NW	182959	96677
Brook steeple	63	10	25	SE	219234	114417	Allington steeple	25	0	2	NE	221344	141017
Willsborough steeple ..	33	0	39	SE	206797	123109	Lymne steeple.........	81	23	44	SE	235914	146456
Kingsnorth steeple	12	49	1	SE	199037	130400	Mersham steeple......	22	32	14	NW	214269	130383
Shadoxhurst steeple....	7	20	54	SW	187830	135476	Monks-Horton steeple..	46	23	19	NE	237605	127204

Bearings and Distances of the Stations and Interior Objects, intersected in 1796.

At Goudhurst.							At Fairlight Down.						
Boughton Malherb	54	59	23	NE	159324	95480	Iden steeple...........	33	33	48	NE	168454	180711
Bidenden	88	49	3	NE	147431	131744	Brede steeple	13	48	32	NW	138116	197485
Hartridge.............	79	43	33	NE			At Allington Knoll.						
At Fairlight Down.							Stone Crouch	57	3	23	sw	176642	172082
Silver Hill	34	28	24	NW			Warehorn steeple......	72	50	14	sw	193071	152324

Interior Objects.

At Goudhurst							At Allington Knoll.						
Frittenden steeple	72	9	23	NE	135894	123079	Old Romney steeple ..	21	3	44	sw	207322	176759
Linton steeple	15	32	17	NE	117510	92425	New Romney steeple ..	4	41	50	sw	217098	178460
Chart Sutton steeple ..	36	16	23	NE	133757	95234	At Boughton Malherb.						
Sutton windmill	41	28	17	NE	138534	96169	Benenden steeple......	25	12	54	sw	129542	150187
Ulcomb steeple	47	18	3	NE	147633	94491	At Silver Hill.						
Headcorn windmill....	57	54	53	NE	140758	111015	Brasses windmill	40	7	4	SE	123521	187554
Staplehurst	51	49	3	NE	127216	116176	At High Nook.						
Cranbrook steeple	71	8	27	SE	123602	138488	New Church steeple ..	57	43	31	NW	214018	156687
At Fairlight Down.							Ivy Church steeple	82	52	46	sw	205170	168562
Rolvenden steeple	1	59	36	NE	145513	155271	St. Mary's steeple	78	53	12	sw	204756	170287
Beckley steeple	1	7	43	NE	144072	179830	At Lydd.						
Peasemarsh steeple	25	11	9	NE	158458	186395	Playden steeple	85	1	0	NW	169333	187207
Whittersham steeple ..	21	13	46	NE	162307	169704	At Westwell.						
Sandhurst steeple......	17	26	59	NW	127277	167613	Lenham steeple	63	25	45	NW	165089	87178
Winchelsea steeple	50	39	28	NE	164181	201501	Egerton steeple	86	38	14	sw	167621	102243
Icklesham steeple	41	12	28	NW	156031	204073	Smarden steeple	61	47	14	sw	157842	119273
At Allington Knoll.							Turret on Romden stables	56	18	54	sw	163521	119970
Bethersden	69	11	15	NW	173469	126373	At Stone Crouch.						
High Halden	81	47	1	NW	164672	136054	Appledore steeple	30	33	49	NE	182243	162595
Orleston steeple	86	50	21	sw	196655	145317	Snave steeple	65	22	1	NE	200828	160993
Woodchurch steeple ..	89	57	22	NW	177569	144000	Snargate steeple	66	35	7	NE	193068	164969
Warehorn steeple......	72	50	14	sw	193071	152324	East Guildford steeple..	6	54	4	sw	174746	187750
Brookland steeple......	42	19	2	sw	192410	174253							

Latitudes and Longitudes of Objects intersected in 1795.

Names of Objects.	Latitude.			Longitude east from Greenwich. In degrees.			In time.		Names of Objects.	Latitude.			Longitude east from Greenwich. In degrees.			In time.	
	°	′	″	°	′	″	m.	s.		°	′	″	°	′	″	m.	s.
The Belvidere in Waldershare park..........	51	11	13	1	15	39	5	2.6	St. Radigund's Abbey	51	7	56	1	14	44	4	58.9
									Hougham steeple	51	6	50	1	15	4	5	0.3
Ringswold, or Kingswold steeple	51	11	8	1	22	20	5	29.3	Gunston steeple	51	9	18	1	19	0	5	16
									St. Margaret's steeple	51	9	14	1	22	7	5	28.5
Upper Hardres steeple ..	51	13	1	1	4	45	4	19	South Foreland light-house	51	8	21	1	22	6	5	28.4
Chislet steeple	51	20	4	1	11	24	4	45.6	Barham windmill........	51	12	52	1	12	41	4	50.7

Names of Objects.	Latitude.			Longitude east from Greenwich. In degrees.			In time.	
	°	′	″	°	′	″	m.	s.
Elham windmill	51	5	44	1	14	1	4	56.1
Upper Deal Chapel	51	13	2	1	22	44	5	30.9
Deal Castle	51	13	5	1	23	59	5	35.9
Watch-house near the shore	51	10	21	1	23	46	5	35.1
Sandown Castle	51	14	18	1	23	59	5	35.9
Walmer steeple	51	15	29	1	23	8	5	32.5
Ripple steeple	51	12	12	1	19	0	5	16
Waldershare steeple	51	11	15	1	16	59	5	7.9
Eastry steeple	51	14	44	1	18	26	5	13.7
Ash steeple	51	16	44	1	16	34	5	6.3
Minster steeple	51	19	50	1	18	46	5	15.1
Woard steeple	51	15	23	1	20	41	5	22.7
Sandwich highest steeple	51	16	30	1	20	15	5	21
Wingham steeple	51	16	21	1	12	38	4	50.5
Goodneston steeple	51	14	45	1	13	26	4	53.7
Littlebourn steeple	51	16	40	1	11	1	4	44.1
Canterbury cathedral	51	18	26	1	4	53	4	19.5
Mongeham steeple	51	12	53	1	21	18	5	25.2
Norbourn, or Northbourn steeple	51	13	18	1	20	17	5	21.1
Woodnessborough, or Woodnesbor. steeple	51	14	47	1	18	16	5	13.1
Ramsgate windmill	51	19	49	1	24	4	5	36.3
St. Lawrence steeple	51	20	16	1	23	56	5	43.7
Birchington steeple	51	22	25	1	16	13	5	4.8
St. Nicholas steeple	51	21	15	1	14	57	4	59.8
Stourmouth, or Stormouth steeple	51	19	8	1	14	3	4	56.2
The South Reculver	51	22	47	1	11	50	4	47.3
Hearne windmill	51	21	20	1	8	6	4	32.4
Blean steeple	51	18	19	1	3	4	4	12.3
Wickham steeple	51	17	5	1	10	48	4	43.2
Ickham steeple	51	17	47	1	10	7	4	40.5
Bridge windmill	51	14	35	1	7	55	4	31.7
Nackington steeple	51	14	59	1	5	14	4	20.9
Chillingdon windmill	51	14	30	1	14	49	4	59.3
Preston steeple	51	17	55	1	12	54	4	51.6
Shottenden windmill	51	15	41	0	55	25	3	41.7
Harbledown steeple	51	16	58	1	3	13	4	12.9
Sturry steeple	51	17	55	1	7	5	4	28.3
West-Stone-street windm.	51	10	22	1	1	45	4	7
Stelling windmill	51	10	51	1	3	6	4	12.4
Ashford steeple	51	8	56	0	52	18	3	29.2
Brook steeple	51	9	38	0	57	8	3	48.5
Willsborough steeple	51	8	14	0	53	52	3	35.5
Kingsnorth steeple	51	7	3	0	51	49	3	27.3
Shadoxhurst steeple	51	6	14	0	48	53	3	15.5
Kennington steeple	51	10	12	0	53	17	3	33.2
Great Chart steeple	51	8	33	0	49	39	3	18.6
Westwell steeple	51	11	39	0	50	39	3	22.6
Pluckley steeple	51	10	30	0	45	14	3	0.9
Eastwell steeple	51	11	23	0	52	24	3	29.6
Charing steeple	51	12	37	0	47	44	3	10.9
Allington, or Aldington steeple	51	5	16	0	57	36	3	50.4
Lymne steeple	51	4	20	1	1	22	4	5.5
Mersham steeple	51	7	1	0	55	47	3	43.1
Monks Horton steeple	51	7	30	1	1	53	4	7.5

Latitudes and Longitudes of Objects intersected in 1796.

Names of Objects.	Latitude.			Longitude east from Greenwich. In degrees.			In time.	
Linton steeple	51	13	24	0	30	40	2	2.7
Sutton windmill	51	12	46	0	36	9	2	24.6
Chart Sutton steeple	51	12	56	0	34	54	2	19.6
Lenham steeple	51	14	13	0	43	6	2	52.4
Romden stables	51	8	49	0	42	36	2	50.4
Smarden steeple	51	8	57	0	41	8	2	44.5
Bethersden steeple	51	7	45	0	45	10	3	0.7
Rolvenden steeple	51	3	3	0	37	50	2	31.3
Beckley steeple	50	59	1	0	37	24	2	29.6
Bidenden steeple	51	7	3	0	38	23	2	33.5
Headcorn windmill	51	10	21	0	36	41	2	26.7
Ulcomb steeple	51	13	1	0	38	31	2	33
Staplehurst steeple	51	9	30	0	33	9	2	12.6
Cranbrook steeple	51	5	50	0	32	10	2	8.7
Egerton steeple	51	11	44	0	43	43	2	54.9
Frittenden steeple	51	8	20	0	35	24	2	21.6
Snargate steeple	51	1	23	0	50	10	3	20.7
Snave steeple	51	2	1	0	52	12	3	28.8
Warehorn steeple	51	3	27	0	50	13	3	20.9
Orleston steeple	51	4	36	0	51	10	3	24.7
Winchelsea steeple	50	55	26	0	43	34	2	54.3
Sandhurst steeple	51	1	3	0	33	4	2	12.3
Whittersham steeple	51	0	39	0	42	10	2	48.7
New Church steeple	51	2	42	0	55	38	3	42.5
Ivy Church steeple	51	0	45	0	53	18	3	33.2
St. Mary's steeple	51	0	29	0	53	11	3	32.7
East Guilford steeple	50	57	50	0	45	21	3	1.4
Appledore steeple	51	1	47	0	47	22	3	9.5
Old Romney steeple	50	59	25	0	53	50	3	35.3
New Romney steeple	50	59	7	0	56	22	3	45.5
Playden steeple	50	57	46	0	43	56	2	55.7
Brookland steeple	50	59	51	0	49	58	3	19.9
Iden steeple	50	58	50	0	43	43	2	54.9
Brede steeple	50	56	7	0	35	49	2	23.3
Benenden steeple	51	3	54	0	33	41	2	14.8
Brasses windmill	50	57	46	0	32	3	2	8.2
Icklesham steeple	50	55	1	0	40	29	2	42
Boughton Malherb steeple	51	12	51	0	41	34	2	46.3
Peasemarsh steeple	50	57	54	0	41	7	2	44.5
Woodchurch steeple	51	4	51	0	46	12	3	4.8
High Halden steeple	51	6	11	0	42	52	2	51.5

END OF THE EIGHTY-SEVENTH VOLUME OF THE ORIGINAL.

I. The Bakerian Lecture. Being Experiments on the Resistance of Bodies moving in Fluids. By the Rev. S. Vince, A. M., F. R. S., &c. Anno 1798. *Vol. LXXXVIII.* p. 1.

In a former paper on the motion of fluids, I stated the difficulties to which the theory is subject, and showed its insufficiency to determine the time of emptying vessels, even in the most simple cases; I also proved, by actual experiments, that in many instances there was no agreement between their results and those deduced from theory. The great difference between the experimental and theoretical conclusions, in most of the cases which respect the times in which vessels empty themselves through pipes, necessarily leads us to suspect the truth of the theory of the action of fluids under all other circumstances. In the doctrine of the resistances of fluids, we see strong reasons to induce us to believe, that the theory cannot generally lead us to any true conclusions. When a body moves in a fluid, its particles are struck by the body; and in our theoretical considerations, after this action, the particles are supposed to produce no further effect, but are conceived to be as it were annihilated. But in fact this cannot be the case; and what we are to allow for their effect afterwards, is beyond the reach of mere theoretical investigation. Whatever theory therefore we can admit, must be that which is founded on such experiments as include in them every principle which is subject to any degree of uncertainty. We must therefore have recourse to experiments, in order to establish any conclusions on which we may afterwards reason. In the paper above mentioned, I described a machine to find the resistances of bodies moving in fluids; since which time, I have made a variety of experiments with it, on bodies moving both in air and water, and I have every reason to be satisfied of its great accuracy. In this paper, I propose to examine the resistance which arises from the action of non-elastic fluids on bodies.

This subject divides into 2 parts: we may consider the action of water at rest on a body moving in it, or we may consider the action of the water in motion on the body at rest. We shall first give the result of our experiments in the former case, and compare them with the conclusions deduced from theory. Now the radius of the axis of the machine used in these experiments was 0.2117 in. the area of the 4 planes was 3.73 in. the distance of their centres of resistance from the axis was 7.57 in. and they moved with a velocity of 0.66 feet in a second. The first column of the following table exhibits the angles at which the planes struck the fluid; the 2d column shows the resistance by experiment, in the direction of their motion, in Troy ounces; the 3d column gives the resistance by theory, assuming the perpendicular resistance to be the same as by experiment; the 4th column shows the power of the sine of the angle to which the resistance is proportional.

Angle.	Experiment.	Theory.	Power.
10°	0.0112	0.0012	1.73
20	0.0364	0.0093	1.73
30	0.0769	0.0290	1.54
40	0.1174	0.0616	1.54
50	0.1552	0.1043	1.51
60	0.1902	0.1476	1.38
70	0.2125	0.1926	1.42
80	0.2237	0.2247	2.41
90	0.2321	0.2321	

The 4th column was thus computed: Let s be the sine of the angle to radius unity, r the resistance at that angle, and suppose r to vary as s^m; then $1^m : s^m ::$ $0.2321 : r$, hence, $s^m = \frac{r}{0.2321}$, and consequently $m = \frac{\log. r - \log. 0.2321}{\log. s}$; and, by substituting for r and s their several corresponding values, we get the respective values of m, which are the numbers in the 4th column. Now the theory supposes the resistance to vary as the cube of the sine; whereas, the resistance decreases from an angle of 90°, in a less ratio than that, but not as any constant power of the sine, nor as any function of the sine and cosine, that I have yet discovered. Hence, the actual resistance is always greater than that which is deduced from theory, assuming the perpendicular resistance to be the same; the reason of which, in part at least, is, that in our theory we neglect the whole of that part of the force which, after resolution, acts parallel to the plane; whereas, from the experiments which will be afterwards mentioned, it appears that part of that force acts on the plane; also, the resistance of the fluid which escapes from the plane, into the surrounding fluid, may probably tend to increase the actual resistance above that which the theory gives, in which that consideration does not enter; but, as this latter circumstance affects the resistance at all angles, and we do not know the quantity of effect which it produces, we cannot say how it may affect the ratio of the resistances at different angles.

In theory, the resistance perpendicular to the planes is supposed to be equal to the weight of a column of fluid, whose base = 3.73 in. and altitude = the space through which a body must fall to acquire the velocity of 0.66 feet; now that space is 0.08124 in. consequently the weight of the column = 0.1598 Troy oz.; but the actual resistance was found to be = 0.2321 oz. Hence, the actual resistance of the planes : the resistance in our theory :: 0.2321 : 0.1598, which is nearly as 3 : 2.

I am aware that experiments have been made on the resistances of bodies moving in water, which have agreed with our theory. An extensive set was instituted by D'Alembert, Condorcet, and Bossut, the result of which very nearly coincided with theory, so far as regards the absolute quantity of the perpendicular resistance. Their experiments were made on floating bodies, drawn upon the fluid by a force acting on them in a direction parallel to the surface of the fluid. There can be no doubt but that these experiments were very accurately made. The experiments here related were also repeated so often, and with so much care, and the results always agreed so nearly, that there can be no doubt but that they give the actual resistance to a very considerable degree of accuracy. In our experiments, the planes were immersed at some depth, in the fluid; in the other case, the bodies floated on the surface; and I can see no way of accounting for the difference of the resistances, but by supposing that, at the surface of the fluid, the fluid from the end of the body may escape more easily than when the body is immersed below the surface; but this I confess appears by no means a satisfactory solution of the difficulty. The resistances of bodies descending in fluids manifestly come under the case of our experiments.

Two semi-globes were next taken, and made to revolve with their flat sides forwards. The diameter of each was 1.1 inc. the distance of the centre of resistance from the axis was 6.22 inc. and they moved with a velocity of 0.542 feet in a second; and the resistance was found to be 0.08339 oz. by experiment. By theory, the resistance is 0.05496 oz.; hence, the resistance by experiment : the resistance by theory :: 0.08339 : 0.05496, agreeing very well with the above-mentioned proportion. But, when the spherical sides moved forwards with the same velocity, the resistance was 0.034 oz. Hence, the resistance on the spherical side of a semi-globe : resistance on its base :: 0.034 : 0.08339; but this is not the proportion of the resistance of a perfect globe to the resistance of a cylinder of the same diameter, moving with the same velocity, because the resistance depends on the figure of the back part of the body.

I therefore took 2 cylinders, of the same diameter as the 2 semi-globes, and of the same weight; and, giving them the same velocity, I found the resistance to be 0.07998 oz.; therefore the resistance on the flat side of a semi-globe : the resistance of a cylinder of the same diameter, and moving with the same velocity :: 0.08339 : 0.07998. This difference can arise only from the action of the fluid on the back side of the semi-globe, moving with its flat side forwards, being less than that on the back of the cylinder, in consequence of which the semi-globe suffered the greater resistance. The resistance of the cylinders, thus determined directly by experiment, agrees very well with the foregoing experiments. The resistance, cæteris paribus, varies as the square of the velocity very nearly, and may be taken so for all practical purposes, as I find by repeated experiments, made both on air and water, in the manner described in my former paper. Hence, for different planes, the resistance varies as the area × the square of the velocity. Now the resistance of the planes whose area was 3.73 inc. moving with a velocity of 0.66 feet in a second, was found to be = 0.2321 oz. Also, the area of the 2 cylinders was 1.9 inc. and their velocity was 0.542 feet in a second; to find therefore the resistance of the cylinders from that of the planes, we have $0.66^2 \times 3.73 : 0.542^2 \times 1.9 :: 0.2321$ oz. : 0.07973 oz. for the resistance on the cylinders, differing but very little from 0.07998 oz. the resistance found from direct experiment.

Now, to get the resistance on a perfect globe, we must consider, that when the back part is spherical, the resistance is greater than when it is flat, in the ratio of 0.08339 : 0.07998; hence the resistance on a globe : the resistance on a semi-globe in the same ratio; but the resistance on the semi-globe was 0.034 oz. hence, 0.07998 : 0.08339 :: 0.034 oz. : 0.0354 oz. the resistance of a globe; consequently, the resistance of a globe : the resistance of a cylinder of the same diameter, moving with the same velocity in water :: 0.0354 : 0.07998 :: 1 : 2.23.

We proceed next to compare the actual resistance of a globe with the resistance assumed in our theory. In the first place, the absolute quantity of resistance has been found to be greater than that which we use in theory, in the ratio of 0.2321 : 0.1598; but, by theory, the resistance of the globe : the resistance of the cylinder :: 1 : 2, or as 1.115 : 2.23; hence, by theory, we make the resistance of the globe too great, in the ratio of 1.115 : 1; and it is too small, from the former consideration, in the ratio of 0.1598 : 0.2321; therefore the actual resistance of the globe : the resistance in theory :: 0.2321 : 0.1598 × 1.115 :: 0.2321 : 0.1782, which is nearly in the ratio of 4 : 3. Thus far we have considered the resistance of bodies moving in a fluid; we come next to consider the action of a fluid in motion on a body at rest.

A vessel 5 feet high was filled with a fluid, which could be discharged by a stop-cock, in a direction parallel to the horizon. The cock being opened, the curve which the stream described was marked out on a plane set perpendicular to the horizon; and, by examining this curve, it was found to be a very accurate parabola, the abscissa of which was 13.85 inc. and the ordinate was 50 inc.; hence the latus rectum was 180.5 inc. $\frac{1}{4}$ of which is 45.1 inc. which is the space through which a body must fall to acquire the velocity of projection; hence that velocity was 189.6 inc. in a second. And here, by the bye, we may take notice of a remarkable circumstance. The depth of the cock below the surface of the fluid was 45.1 inc.; hence the velocity of projection was that which a body acquires in falling through a space equal to the whole depth of the fluid; whereas, through a simple orifice, the velocity would have been that which is acquired in falling through half the depth; the pipe of the stop-cock therefore increased the velocity of the fluid in the ratio of $1 : \sqrt{2}$, and gave it the greatest velocity possible; the length of the pipe was 3 inc. and the area of the section 0.045 inc.; also, the base of the vessel was a square, the side of which was 12 inches.

The area of the section of the pipe may be found very accurately, in the following manner. The vessel being kept constantly full, receive the quantity of fluid run out in any time t'', and then weigh it, by which we shall be able to get the quantity in cubic inches. Now if $v =$ the velocity of the fluid when it issues from the pipe, $a =$ the area of the section of the pipe, $l =$ the length of the cylinder of water run out, whose base $= a$, and $m =$ the quantity of fluid discharged in t''; then $v : l :: 1'' : t''$, hence, $l = vt$; but $al = m$; therefore $avt = m$; hence $a = \frac{m}{vt}$. In the present instance, $t = 20$, $m = 170.63$ cubic inches, $v = 189.6$; hence $a = 0.045$.

Let ABCD, fig. 1, pl. 5, be a solid piece of wood, on which are fixed 2 upright pieces, *rs*, *tu*; between these, a flat lever *eac* is suspended, in a perpendicular position, on the axis *xy*, and nicely balanced; and let *a* be a point directly against the middle of the axis, in a line perpendicular to the plane of the lever. This apparatus is placed against the stop-cock, at the distance of about 1 inch, and, when the water is let go, let us suppose the centre of the stream to strike the lever perpendicularly at *e*; take $ac = ae$, and, on the opposite side to that at which the stream acts, fasten a fine silk string at *c*, and bring it over a pulley *p*, and adjust it in a direction perpendicular to the plane of the lever, and, at the end which hangs down, fix a scale Q, the weight of which is to be previously determined. All the apparatus being thus adjusted, open the stop-cock, and let the fluid strike the lever, and put such weight into the scale as will just keep the lever in its perpendicular situation, and that weight, with the weight of the scale, must be just equivalent to the action of the fluid. Thus we get the perpendicular effect of the water. Now incline the plane of the lever at any angle to the direction of the stream, and adjust the string perpendicular to the plane, as before; then put such a weight into the scale as will keep the lever perpendicular to the horizon while the fluid acts on it, and you get that part of the effect of the fluid which acts perpendicular to the plane. In this manner, when the fluid acts oblique to the plane, we get the perpendicular part of the force. The 2d column of the annexed table shows this effect, by experiment, for every 10th degree of inclination shown in the first column; and the 3d column shows the effect, by theory, from the perpendicular force, supposing it to vary as the sine of inclination. It hence appears, that the resistance varies as the sine of the angle at which the fluid strikes the plane; the difference between the theory and experiment being only such as may be supposed to arise from the want of accuracy to which the experiment must necessarily be subject.

Angle.	Experiment.			Theory.		
	oz.	dwt.	gr.	oz.	dwt.	gr.
90°	1	17	12	1	17	12
80	1	17	0	1	16	22
70	1	15	12	1	15	6
60	1	12	12	1	12	11
50	1	18	10	1	18	17
40	1	4	10	1	4	2
30	0	18	18	0	18	18
20	0	12	12	0	12	19
10	0	6	4	0	6	12

Let us now first consider, what the whole perpendicular resistance by experiment is, when compared with that by theory. Now, by theory, the resistance is equal to the weight of a column of the fluid, whose base = 0.045 inc. and altitude = 45.1 inc. and the weight of that column is = 1 oz. 1 dwt. 10 gr. Hence, the resistance by theory : the resistance by experiment :: 1 oz. 1 dwt. 10 gr. : 1 oz. 17 dwt. 12 gr. :: 514 : 900. In the next place, let us examine what is this resistance, compared with the resistance of a plane moving in a fluid. We here prove, that the resistance of the fluid in motion acting on the plane at rest : the resistance by theory :: 900 : 514; and we have before proved, that the resistance by theory : the resistance of a plane body moving in a fluid :: 1598 : 2321; hence, the resistance of a fluid in motion on a plane at rest : the resistance of the same plane, moving with the same velocity, in a fluid at rest :: 900 × 1598 : 514 × 2321 :: 1438200 : 1192954 :: 6 : 5 nearly. Now we know that the actual effect on the plane must be the same in both cases; and the difference, I conceive, can arise only from the action of the fluid behind the body, in the latter case, there being no effect of this kind in the former case. For, in respect to the pressure before the body, that will probably be the same in both cases; for there is a pressure of the column of the spouting fluid, acting against the particles which strike the body at rest, similar to he action of the fluid before the body, on the particles which strike the body moving in the fluid. Hence, the resistance of the planes moving in the fluid, with the velocity here given, is diminished about $\frac{1}{5}$ part of the whole, by the pressure behind the body; but, with different velocities, this diminution must increase as the velocity increases.

The effect of that part of the force which acts perpendicular to the plane being thus established, we proceed next to examine, what part of the whole force which acts parallel to the plane, is effective. To determine which, the axis *wv*, fig. 2, was fixed perpendicular to the plane of the lever *abcd*, and the ends of the axis were conical, and laid in conical holes; and the thread from which the scale was hung was fixed to the end at *e*, and acted perpendicular to it, and the weight drew the lever in the direction *es*, contrary to that in which the fluid tends to move the lever, and it acted at the same perpendicular distance from the axis below, as the fluid acted above it. Let *xmz* be a line parallel to the horizon, when the lever is perpendicular to it, and which passes through the centre of the stream; and let *xmz* be also the direction of that part of the force which acts parallel to the plane. This apparatus being adjusted, the experiments were made for every 10th degree of inclination; and here a circumstance took place, for which I can give no satisfactory reason. Having gone through the experiments once, and noted the results, I repeated them; and, to my great surprise, I found all the 2d results to be very different from the first. The experiments were therefore repeated again, and the results were still different. Being certain that the experiments were very accurately made each time, I was totally at a loss to conjecture to what circumstance this difference of results was owing. By repeating however the experiments, and observing at what point of the line *xmz* the centre of the stream acted, I discovered that the effect varied by varying that point; that it was greatest when the stream struck the lever as near as it could to *x*; less when it struck it at the middle *m*; and least when it struck it as near as it could to z, though the stream acted at the same perpendicular distance from the axis in each case, and the parallel part of the force always acted in the line *xmz*. At the angles 80°, 70°, 60°, the fluid striking as near as it could to the edge z, gave the lever a motion, not in the direction *xmz*, but in the opposite direction *zmx*, as appeared by taking away the scale. I have therefore marked such results with the sign ——, the motion produced being then in a direction opposite to that which ought to have been produced, by that part of the force of the stream which acts parallel to the plane of the lever. The forces which are here set down, are those which take effect in a direction parallel to the plane of the lever, for every 10th degree of inclination; the perpendicular force being 1 oz. 17 dwt. 12 gr.

Inclin.		dwt.	gr.
	Edge *z*	..	..
80°	Middle *m*	3	3
	Edge *x*	10	17
	Edge *z*	..	..
70	Middle *m*	6	2
	Edge *x*	11	10
	Edge *z*	..	..
60	Middle *m*	7	9
	Edge *x*	11	22
	Edge *z*	0	17
50	Middle *m*	8	20
	Edge *x*	13	21
	Edge *z*	1	16
40	Middle *m*	8	6
	Edge *x*	13	15
	Edge *z*	3	20
30	Middle *m*	7	2
	Edge *x*	12	15
	Edge *z*	4	16
20	Middle *m*	6	0
	Edge *x*	11	12
10	Middle *m*	5	12

It is a remarkable circumstance, that the effect of the fluid at z increased regularly as the angle decreased; for, though I did not measure the negative effects, I could plainly perceive that that was the case; whereas, the effects at *m* and *x* increased to about the middle of the quadrant, and then decreased. At 10°, the obliquity was such, that the section of the stream extended very nearly from one side of the lever to the other. As it appears by experiment, that the velocity of the fluid flowing out of the vessel, was equal to the velocity which a body acquires in falling down the altitude of the fluid above the orifice, the square of the velocity must be in proportion to that altitude. To find therefore, in this case, whether the resistance varied as the square of the velocity, I let the water flow perpendicularly against the plane, fig. 1, at different depths, and I always found the resistances to be in proportion to the depths, and therefore in proportion to the square of the velocity, agreeing with what takes place when the body moves in the fluid.

II. Experiments and Observations, tending to show the Composition and Properties of Urinary Concretions. By Geo. Pearson, M. D., F. R. S. p. 15.

1. *Historical Observations.*—Urinary concretions have obtained their denominations, like most other things, from their obvious properties. Accordingly, in our language, they are popularly known by the names Stone and Gravel, or Sand, from their resemblance to the states of earth so named: and we find names of the same import in other languages, such as λιθος, (Aretæus;) λιθιασις, (Cælius Aurelianus;) ψαμμος, (Aretæus;) λιθιδια, (various authors;) Calculus, (Celsus and Pliny;) Sabulum, (various authors.) In other languages, and especially in those now spoken, it is unnecessary to notice names which have the same meaning.

The notion very generally entertained, of the nature of urinary concretions, consisted with the terms, till the last 20 years; though the experiments of Slare, Fred. Hoffman, and Hales, long before showed that these substances commonly consist of animal matter. Galen indeed imagined that φλεγμα, or viscid animal matter, is the basis of animal concretions; but in his days earth was believed to be the basis of animal matter. Alkaline medicines were however employed by the Greek physicians, in diseases from calculi.

The experiments of the alchemists also made it appear, that earth was only a part of the matter of concretions. It was probably the observation of the deposition and crystallization of saline bodies, which suggested the notion of urinary calculi being of the nature of tartar. Such was the opinion of Basil Valentine, and after him of Hochener, better known by the name of Paracelsus; but whether the latter adopted the denomination Duelech from its import, or from caprice, has not been explained. Van Helmont, a century after his prototype Paracelsus, being struck with the experiment in which he discovered the concretion of salts in distilled urine by alcohol, was led to depart from his adored master's opinion, with respect to the nature of calculi; though he acknowledges the merit of Paracelsus, in having discovered the solvent Ludus, a calcareous stone also called Septarium, which Van Helmont says is preferable to alkaline lixivium. He also says, that when the archeus spirit of urine meets with a volatile earthy spirit, and does not act in a due manner, a concretion will be formed; but in a healthy state, though all urine contains the matter of urinary calculi, no concretion can take place; because the archeus, or vital power of the bladder, counteracts its formation. As to the kind of earth composing calculi, the only distinction of earths, till about the last half century, was into absorbent and non-absorbent; but, since the absorbent earths were distinguished into calcareous earth, magnesia, and alumina or clay, the calcareous was considered to be the earth of urinary concretions; apparently however for no other reason but its obvious properties, and its extensive diffusion through the whole animal kingdom.

At length, viz. in 1776, the experiments of the wonderful Scheele were published in Sweden, but were scarcely known in this country till 1785. These experiments exploded the opinion of the earthy nature of calculi, and substituted that of their consisting of a peculiar acid, resembling the succinic, and of a gelatinous matter, without any earth. Afterwards about $\frac{1}{200}$ of their weight of lime was found by Bergman; which, for a cause now well known, had eluded the acuteness of Scheele. Though the experiments of Scheele were confessedly unquestionable, and were ably supported by the learned Bergman, some very eminent chemists, having obtained different results by their own experiments, adopted a different opinion of the composition of these concretions. The immortal, and ever to be deplored, Lavoisier supposed these substances to consist of acidulous phosphate of lime and animal matter, many of them being partially fusible; but still it was the unrivalled Scheele who discovered, that the urine of healthy persons contains superphosphate, or acidulous phosphate, of lime; and who also indicated the experiment which verified his opinion, that phosphate of lime is the basis of bone.

Experiments have been likewise made, for the most part in a rather desultory way, and most of them by persons but little practised in chemical inquiries, which at least afford evidence, that urinary concretions are very different, with respect to the proportion of the ingredients in their composition, and perhaps also in kind. M. Fourcroy, who however must not be classed with inexperienced chemists, I believe first obtained prussic acid by fire, and by nitric acid, from these concretions; and showed that they sometimes contain phosphate of ammonia and of soda; which may be dissolved out of them by water. M. Fourcroy also says, he found magnesia in the intestinal calculus of a horse; which calculus was a triple combination, of 1 part of phosphate of ammonia, 2 parts of magnesia, and 1 of water, besides traces of animal and vegetable matter. Dr. Link, in a very elaborate dissertation, published at Gottingen, in 1788, or urine and calculi, concludes that urinary concretions consist of phosphoric acid, lime, ammonia, oil, the bases of different kinds of gazes, with the acid sublimate of Scheele, though he did not succeed in obtaining it. It is a proof of Dr. Black's sagacity, that he was able to perceive, from a few experiments, that urinary concretions consisted of animal matter and the earth of bone, before the composition of this earth was demonstrated by Gahn.

In this historical sketch it should be noticed, that alkaline substances, though used by the Greek physicians, and afterwards by the alchemical physicians, appear to have been laid aside by the regular practitioners, for a century or 2 preceding their revival, by the famous Mrs. Stephens, in 1720. Her prescription brought into vogue the theory of these medicines operating by their causticity. The successful use, by Mr. Colborne, of pot-ash saturated with carbonic acid, according to the discovery of Bewley and Bergman, and the still further improvement in practice, from the use of soda, as well as pot-ash, super-saturated with carbonic acid, by the discovery of a peculiar method by Mr. Schweppe, have completely refuted the theory of the agency of alkalies on the principle of causticity. It appears, from the preceding brief history, as well as from the confession of the latest and best writers, that the experiments hitherto made, rather "afford indications of what remains to be done, than furnish demonstrations of the nature of animal concretions." It is also too obvious to need explanation, that more efficacious and innocent practice, in diseases of these concretions, can only be discovered by a further investigation of their properties. It is with this view, as well as for the sake of chemical philosophy, that I think it my duty to submit to the R. S. some of the observations I have made, in the course of inquiry on this subject.

The observations which I shall now offer, are principally on a substance, which my experiments inform me is very generally a constituent of both urinary and arthritic concretions. It is a substance obtained by dissolving it out of these concretions, by lye of caustic fixed alkali, and precipitating it from the solution by acids. In this way, Scheele separated this matter; but he did not consider its importance, nor of course at all investigate its properties. He does not even seem to have been aware that it was a distinct constituent part of the urinary concretion; for when he relates the experiment of precipitating matter from the nitric solution of calculus by metallic salts, no distinction is made between the precipitations in this experiment, and that in the former; yet we can now show, that in the one case the precipitate is a peculiar animal oxide, and in the other they are metallic phosphates. As Scheele obtained an acid sublimate, it has been imagined by some writers, that the precipitate by any acid, even by the carbonic, from the alkaline menstruum, was an acid; the same as that obtained by sublimation, and which, in the new system of chemistry, has been denominated lithic acid. The following experiments show that these substances are different species of matter.

2. *Exper.* 250 grs. of a white, smooth, laminated, urinary calculus, and the same quantity of a nut-brown one, with an uneven surface, both of which were of a roundish figure, were pulverized together*. 300 grs. of these pulverized

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* The object of these experiments being principally to investigate the properties of one of the constituent parts of urinary concretions, which part was previously determined by the test of nitric acid, to

calculi were triturated with 3 and ¼ oz. by measure, or 5 oz. by weight, of lye of caustic soda. The mixture became thick, and copiously emitted ammoniacal gaz. After digestion for a night, and then boiling, with the addition of 5 oz. of pure water, I obtained, by filtration, 5 oz. of clear colourless liquid. Boiling water was repeatedly poured on the strainer, till what passed through it was almost tasteless, and remained clear, on the addition of diluted sulphuric acid.

(a) The matter remaining on the strainer, being dried, was an impalpable, white, tasteless, heavy powder, which weighed 96 grs. (b) The 5 oz. of filtrated liquid, having been set apart, on standing, deposited a white, opaque, granulated, soap-like matter, from a colourless clear liquid. The liquid being decanted, the deposit was dried, and was then an opaque, brittle, soap-like matter, which dissolved readily in water, giving a clear but not viscid solution, and tasting weakly of soda. This soap-like matter weighed 280 grs. (c) The decanted liquor (b,) being mixed with the above filtrated liquors, on evaporation to 3 oz. afforded no deposit on standing, though it was a very heavy and soapy liquid to the feel; but, on adding diluted sulphuric acid gradually, till it ceased to become turbid, a sediment was deposited, which was a very light, white, impalpable powder, in weight, when dried, 26 grs. The liquid from which this powder was precipitated, being evaporated, afforded nothing but sulphate of soda, and a few grains of crystals, which seemed to be phosphate of soda. There was also a blackish matter, which burnt like horn, or other animal matter, and did not leave a pink or rose-coloured matter, on evaporating the solution of it in nitric acid to dryness, but left a carbonaceous residue; whereas, the white precipitate, so treated, afforded a beautiful pink matter.

(d) 250 grs. of the soap-like matter (b) being dissolved in 8 oz. of pure water; 1. A little of this solution, further diluted by 1 oz. of water, grew milky on adding a few drops of nitric acid, but became less so on standing. On adding more nitric acid, and heating it, the mixture became quite clear: by adding a few drops of lye of caustic soda, a very slight curdy appearance took place. 2. On adding, to the same diluted solution, a little of the diluted sulphuric or muriatic acid, milkiness ensued, and remained, though the acids were added till the mixture was extremely sour. On adding lye of caustic soda, much more than to saturate the superabundant acid, the mixture became clear again; and, on adding the acids a 2d time, the milkiness was reproduced. It was found that the milkiness could be produced and destroyed, or clearness be produced, by the alternate addition of the acid and alkali, for an unlimited number of times. If the nitric acid however was used, at length no milkiness could be induced. If carbonate of soda was added, instead of the caustic soda, the mixture could not be made clear. 3. Lime water was rendered turbid by this solution, but I neglected to examine the precipitated matter. 4. A little of the solution, with the addition of a few drops of concentrated nitric acid,

exist in both of them, it can be no objection to the experiments. that I made use of a mixture of 2 calculi.—Orig.

being evaporated to dryness, sometimes a pink, and at other times a blood red, or rose-coloured matter was left; which, by further application of fire, became black. 5. Carbonic acid, digested and shaken with this solution, did not render it turbid. 6. To the whole of the remaining solution was added diluted sulphuric acid, to saturate the alkali. On standing, a copious precipitate took place, from a clear liquid; which precipitate, being washed and dried, was a mass of very light, mica-like, whitish crystals, amounting to 123 grs. It was estimated that the solution used in the experiment 1.—5, would have produced 12 grs., and that the 30 grs. of soap-like matter, (b,) not decompounded, would have yielded about 14 grs. more.

(e) The precipitate, (d, 6,) 1. Had no taste, nor smell, and did not dissolve in the mouth. 2. About 1 part of it only dissolved in 800 parts of boiling water; which solution did not redden paper stained with turnsole, nor the solution and tincture of this test; neither did it change turnsole paper, reddened by acid, to a blue colour. On cooling, the greatest part of what had been dissolved was deposited, in a crystallized state, equally on the sides and bottom of the vessel. This crystallized matter had the properties above-mentioned (d.) Boiling water was found to dissolve a much greater proportion of urinary stone, and also of gravel, than of this precipitate. 3. Lye of mild pot-ash, or subcarbonate of pot-ash, being dropped into the solution (e, 2,) with its crystallized deposit, the crystals at first seemed to dissolve; but, on standing, a great part of the matter was deposited, and the liquid remained turbid. 4. The precipitate being boiled with lye of carbonate of soda, more seemed to be dissolved than in pure water; but the solution was not clear, and, on evaporating it nearly to dryness, and pouring cold water on it, on a paper strainer, scarcely any thing but the soda passed through with the water; the precipitate remaining behind on the paper. The result was the same, when this experiment was made with a lye of carbonate of ammonia. The result was also the same, with water in which red oxide of mercury had been boiled; which was also boiled with this precipitate, and filtrated after cooling. 5. A little of the precipitate being triturated with quick-lime, hot water was poured on it. The filtrated liquor gave the precipitate back again, on adding muriatic acid. 6. The precipitate exposed to flame, with the blow-pipe, turned black, emitted the smell of burning animal matter, and evaporated or burnt away without any signs of fusion; staining the platina spoon black. 7. Five grains of the precipitate, in ½ oz. of water, were left to stand in a warm room, during the months of August and Sept. last, without any signs of putrefaction appearing, or any obvious change taking place. 8. Twenty-four oz. of boiling water were saturated with the precipitate, and divided into 6 portions; from each of which, on cooling, most of it again precipitated. The first portion, on boiling with a little lye of carbonate of soda, the pneumatic apparatus being affixed, discharged no carbonic acid into lime water; but a transparent solution was produced, and on cooling very little was precipitated. The 2d portion was, in the same manner, boiled in a little lye of caustic soda; which gave a transparent solution on cooling, without any precipitation.

The 3d portion being boiled with lime water, very little more seemed to be dissolved than in pure water. The 4th portion being boiled with 4 grs. of sub-phosphate of lime, or calcined bone, no more seemed to be dissolved on account of this addition. Nor was more dissolved in the 5th portion, by the addition of 4 grs. of phosphate of lime, made by dropping phosphoric acid into lime water. And the result was the same with the 6th portion, to which were added 4 grs. of superphosphate of lime, made by adding phosphoric acid to lime water, so as just to make a clear solution, and then evaporating the solution.

9. Urine seemed to dissolve, or at least to suspend, a greater quantity of the precipitate than mere water; so likewise did water with a little sulphate of soda. 10. The precipitate did not render solution of hard soap at all curdy; but, on adding the precipitate to solution of sulphuret of pot-ash, it became very turbid. 11. The precipitate produced a strong effervescence, even in the cold, with nitric acid, but the fumes were not those of nitrous acid: there was a clear solution, which, on evaporation to dryness, afforded black matter, surrounded by a pink, or blood red margin. 12. The substance, with sulphuric acid, turned black, and emitted fumes copiously, which were scarcely those of sulphureous acid; and, on evaporation, a black mark only was left. 13. I first digested, and then boiled, in water, the precipitate with prussiate of iron; but the filtrated liquor afforded no precipitation with sulphate of iron.

14. Two dr., by measure, of nitric acid, of the specific gravity of 1.35, were poured on 7 grs. of the precipitate. A violent effervescence took place, which was soon succeeded by a complete solution. A few drops of this solution, being evaporated on glass, left a black mark, surrounded by a pink margin. A few drops of nitric acid being evaporated from this residue, nothing but a still less black mark, and a few red spots remained. Nitric acid being added a third time, nothing but a black mark, still smaller, remained; which entirely disappeared, on evaporating this acid from it a 4th time. I found that a few drops of this solution, so diluted that they did not contain the $\frac{1}{400}$, or even a much smaller part, of a grain of the precipitate, on evaporation, left a pink stain on glass. The whole of the rest of the solution was distilled in a very low temperature, so that a drop only fell about every half-minute, till a thick brownish sediment remained, with a red margin. A similar distillation was performed, with the distilled liquor, a 2d time, when there remained a little whitish thick matter. On a 3d distillation, as before, with the distilled liquor, towards the close white fumes arose, and about $\frac{1}{2}$ dr. of liquid, which now remained in the retort, being left to stand, prismatical crystals, decussating each other, were formed. They had a sharp taste, but were scarcely sour; were very soluble in the mouth, and evaporated in white fumes, leaving a very slight black stain.

15. Twenty grains of the precipitate were introduced into a tube, $\frac{1}{8}$ of an inch wide in the bore, sealed by melting at 1 extremity; which extremity was coated, and the tube was fitly bent for retaining sublimate, and collecting gaz. The tem-

perature, from the fire applied, was at first very low, but was gradually increased, so as to make the coated part, containing the charge, red-hot. 1st, the precipitate turned black, and a little water appeared. 2d, gaz came over, which had the smell of empyreumatic liquor cornu cervi. 3d, a brown sublimate appeared, and gaz as before, but also with prussic acid gaz. 4th, black matter, staining the tube, as if from tar, or animal oil. On cooling, there was found a residue, of nearly 3 gr., of pure carbon. The sublimate was principally carbonate of ammonia; the rest was animal oil. The gaz discharged was nearly $\frac{1}{2}$ its bulk, or 5 cubic inches by measure, carbonic acid; and the remaining 5 cubic inches were nitrogen gaz, containing prussic acid and empyreumatic oil.

I treated in the same manner, the same quantity of reddish crystals, deposited spontaneously from urine. The result was not very different from that of the former experiment. The gaz was more offensive, smelling like putrid urine, and the carbonaceous residue was more copious, and contained lime and phosphoric acid; at least the lixivium of it became white, on dropping into it oxalic acid; and it became slightly curdy, on adding lime water. I treated, in the same manner, some quite round and smooth concretions, of the size of black pepper seeds. The products were the same as the former, but the gaz was still more offensive, and in smaller quantity; and the carbonaceous matter was more copious. In the same way I subjected to experiment 20 gr. of a nut-brown light calculus, which I had previously ascertained to contain the matter above described, which was precipitated from caustic soda by acids. The products were of the same kind as the former; but I could find no trace of phosphoric acid in the residue, which I did of lime, and the gaz was less offensive. The carbonaceous residue was not in weight, 3 gr.

It will be proper, before I proceed further, to point out some of the more obvious conclusions from the above experiments. 1. It appears that at least $\frac{1}{2}$ of the matter of the urinary concretions subjected to the above experiments united to caustic soda, and was precipitated from it by acids. (2, *a* — *d.*) 2. This precipitate does not indicate acidity to the most delicate tests; (*e*, 2), and, as it is inodorous, tasteless, (*e*, 1), scarcely soluble in cold water, (*e*, 2), does not unite to the alkali of carbonate of pot-ash, of soda, or of ammonia, (*e*, 3, 4), nor to oxide of mercury, (*e*, 4), nor to the lime of lime water, (*e*, 8), nor decompound soap, (*e*, 10), or prussiate of iron, (*e*, 13), and, as its combination with caustic soda, resembles soap, more than any double salt known to consist of an acid and alkali, this precipitate does not belong to the genus acids. 3. As this precipitate could not be sublimed, without being decompounded, like animal matter, (*e*, 15), and also for the reasons mentioned in the last paragraph, it cannot be the same thing as the acid sublimate of Scheele, or the succinic acid. 4. As it does not appear to be putrescible, nor form a viscid solution with water, it cannot be referred to the animal mucilages. 5. On account of its manner of burning in the air, under the blow-pipe, (*e*, 6), and its yielding, on exposure to fire in close vessels, the distinguishing pucts of animal matter, especially ammonia and prussic acid, as well as on account of its affording a soap-like matter with caustic soda, this precipitate may be considered as a species of animal matter; and, from its composition being analogous to that of the substances called, in the new system of chemistry, animal oxides, it belongs to that genus. Its peculiar and specific distinguishing properties are, imputrescibility, facility of crystallization, insolubility in cold water, and that most remarkable property of all others, producing a pink or red matter, on evaporation of its solution in nitric acid.*

Having found the above precipitate to be an oxide, and not, as is commonly supposed, an acid, I thought it probable that, like other analogous oxides, it was acidifiable, and I suspected that I had really rendered it into the acid state, by the nitric acid; which, in the above experiments, (*e*, 14), had imparted oxygen to it, and so rendered it soluble, deliquescent, pungent, and volatile. This change also would account for the nitric solution not affording the precipitate. In order to obtain, for examination, an adequate quantity of this supposed acid, the following experiments were instituted, with the 3 acids, viz. the oxymuriatic, the nitro-muriatic, and the nitric, which can acidify oxides analogous to the present one.

Exper. 1. Twenty-five grains of the above animal oxide, (for so I will now venture to call it), and 3 oz. of nitric acid, of the specific gravity of 1.25, were put into a retort, and the hydro-pneumatic apparatus was adjoined. At a very low temperature, a clear solution was made. 1. Soon after the solution began to boil, 23 oz., by measure, of colourless gaz came over, which were succeeded, 2, by white fumes, filling the apparatus, and 23 oz. more of gaz. 3. A white sublimate ascended, and there was a strong smell of prussic acid. The sublimate was very readily washed out, being very soluble, and tasted pungent or sharp, but not sour. 4. The distillation being renewed, more white sublimate appeared, but only 3 oz. more of gaz came over; and then the retort only contained a dark-brown solid matter. The first portion of gaz, viz. 23 oz., consisted of about equal bulks of carbonic acid and atmospherical air. The 2d portion, viz. 23 oz., was $\frac{2}{3}$ of its bulk carbonic acid, and the rest nitrogen gaz. The 3d portion, or 3 oz., was atmospherical air, with a little carbonic acid.

Nitric acid was poured, in the same quantity as before, into the retort. An effervescence immediately took place, which was succeeded by a transparent solution. The distillation yielded gaz of the same kind as before, but in smaller quantity, with white fumes, and white sublimate. When only about 4 dr., by measure, of liquid remained in the retort, a little of it was evaporated; and, when reduced to a solid matter, it turned black, and took fire, leaving a carbonaceous residue; but, before this, a margin of beautiful pink matter appeared.

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* It is much to be wished that we possessed equally delicate tests of the other species of animal matter, which are confounded together, though, from their obvious properties, there is reason to believe that they are of very different kinds, as is the case with the matter of the brain, liver, voluntary muscles, mucus, &c. Mr. Hunter has discovered a distinguishing specific property of pus, and one is here indicated for the oxide of urinary concretions.—Orig.

Nitric acid was poured, as before, into the retort, for the 3d time, but very little gaz ascended, and much less white fumes than before. The distillation proceeded, till about 1 dr. measure of liquid remained in the retort: this being left to stand, prismatic crystals were formed in a very small quantity of liquid. These crystals did not taste sour, but sharp, and they reddened turnsole-paper. Adding a little soda to a part of them, to see whether I could form a neutral salt, I was surprized by the extrication of ammonia. To another portion of crystals, I added sulphuric acid, which disengaged nitric acid. A 3d portion of crystals, being exposed over a lamp, wholly evaporated, without leaving a mark behind. The remaining matter in the retort being examined, was found to be nitrate of ammonia. It was plain that the nitric acid had, by parting with oxygen to the carbon of the oxide, formed carbonic acid. The carbon being thus carried off, of course the nitrogen and hydrogen of the oxide uniting produce ammonia, which, uniting with the redundant nitric acid, composes nitrate of ammonia; but great part of the nitrate of ammonia was carried off in the vapour state, exhibiting white fumes, and sublimate, as above observed. The mode of making the experiments with the other acids was of course different from the former experiment.

Exper. 2. Twenty-five grains of the above animal oxide, and $\frac{1}{2}$ oz. of water, were put into a bottle capable of containing 3 pints; a stream of oxymuriatic acid gaz, from manganese and muriatic acid, was made to pass into the bottle, and on the charge, for 12 hours; and, for 24 hours more, oxymuriatic gaz kept issuing, but in smaller quantity, and circulating through the bottle. The oxide, by this time, was completely dissolved. On adding lime to a little of the solution of it, ammonia was disengaged; and, on adding sulphuric acid, there was a disengagement of oxymuriatic acid. On evaporation however I obtained nothing but muriate of ammonia, with which was mixed a little manganese. In this experiment, I could not doubt that the carbon had been carried off, in the state of carbonic acid, by the oxygen of the oxymuriatic acid; and thus ammonia was compounded, from the union of the 2 remaining constituent parts of the oxide, viz. the nitrogen and hydrogen. The oxymuriatic acid, united to the ammonia, parted with oxygen, and became muriatic acid during evaporation; hence, muriate of ammonia was formed.

Exper. 3. The above experiment was repeated, only the gaz was nitro-muriatic gaz, from a mixture of nitric and muriatic acids. The result was the same as in the last experiment, except that the product was a mixture of nitrate and muriate of ammonia.

I made other experiments of the same kind, but their results were so nearly the same as those above related, that I shall not give an account of them. By the unexpected issue of these experiments, all my hopes of acidifying the animal oxide were exploded; but I am indebted to that pursuit, for the curious discovery of the change of the most common basis of urinary concretions, the animal oxide, into ammonia and carbonic acid, by the oxygen of the above acids; which will be found extremely important, as it enables us to interpret many phenomena, in a variety of cases besides the present. It now appears, that the inflammation mentioned in 1 of the above experiments, and which also happened in several others, on evaporation of the nitric solution of the animal oxide, was from the nitrate of ammonia, the nitrum flammans of the old chemists, compounded in those experiments. This inflammation takes place sometimes on evaporation of nitric solutions, both of urinary concretions, and of urine itself evaporated to the state of soft extract, on account of the ammonia already existing in these substances. The composition of ammonia also explains the disappearance of the whole matter of some sorts of urinary concretions, a very small residue of black matter excepted, by repeated affusion and evaporation of nitric acid, from the solution of them in this menstruum.

It remains for me to give an account of the 96 grs. of powdery matter left on the paper-strainer, (*a*); which are the insoluble portion, in lye of caustic soda, of 300 grs. of urinary concretions. 1. A small portion of the insoluble matter, being exposed to flame with the blow-pipe, did not turn black, nor yield any smell of animal matter; but it became whiter, and I could just agglutinate the powder into one mass, though I was unable to render it fluid. 2. The filtrated liquid, from a little of the matter boiled in water, became very turbid and white with oxalic acid: with lime water was barely curdy; and it did not alter the colour of turnsole, or of violet juice. 3. The matter dissolved completely in muriatic acid, and also in nitric acid, without effervescence.

This nitric solution, having been evaporated, to carry off most of the free acid, instantly became very curdy on the addition of lime water. It became thick and white on adding sulphuric acid, yielding a copious precipitate of sulphate of lime. One portion of the supernatant liquor on this precipitate, on evaporation, afforded an extract-like matter; which readily melted, as phosphoric acid does when it is mixed with a little earthy matter. To the other portion of this supernatant liquor was added liquid caustic ammonia, producing a precipitate which afforded no sulphate of magnesia with sulphuric acid. From these experiments it appears, that the above 96 gr. of insoluble matter consisted of phosphate of lime. Accordingly, the 300 gr. of urinary concretions examined, appear to contain,

Peculiar animal oxide.	175 grs.
Phosphate of lime.	96
Ammonia, (and most probably phosphoric acid united to the ammonia), water, and common mucilage of urine, which were not collected and weighed, by estimation.	29
	300

I shall next relate some experiments, made in order to obtain the acid sublimate of Scheele, or lithic acid of the new system of chemistry. 100 grs. of an urinary concretion, which had been previously found to contain principally the above animal oxide, were introduced into a tube $\frac{1}{4}$ of an inch wide; which was sealed at one end by fusion, and which also was fitly bent for collecting sublimate, and obtaining gaz. The sealed end was coated and exposed to fire, first to a low temperature, and gradually to a very elevated one. 1. Gaz was discharged, which had the smell of burning bone. 2. Water appeared boiling immediately over the charge, which seemed to be burning, and was turned black. 3. Gaz was discharged, of the smell of empyreumatic liquor cornu cervi, and about $\frac{1}{2}$ a drachm of this liquor was in the upper part of the tube. 4. A brown sublimate of carbonate of ammonia appeared in the cold part of the tube; but in the hotter part, near the charge, was tar-like matter, and the gaz discharged had a very offensive smell of empyreumatic animal oil, with which was mixed that of prussic acid. The coated part of the tube was kept red-hot, for some time after gaz ceased to come over. The quantity of gaz amounted to 24 oz., by measure: it consisted of nearly 16 oz. of carbonic acid gaz, and the rest was air, with a larger proportion of nitrogen gaz than is contained in atmospheric air.

5. There was a residue of 30 grs., almost pure carbon; and 10 grs. of heavy black and brown matter, a little above the coated part of the tube. In this last-mentioned matter were many small white spicula. At about $\frac{1}{2}$ an inch above the carbonaceous residue, dark grey matter had been raised, which weighed 15 grs. This sublimed grey matter did not contain any ammonia, nor throw down any prussiate of iron, with sulphate of iron. It reddened turnsole paper and tincture. It dissolved in caustic soda; from which solution muriatic acid precipitated nothing; for, though on dropping it into the solution milkiness appeared, the liquid soon became clear again.

10 grs. of this sublimate dissolved in 4 oz. of boiling water; which being evaporated to $\frac{1}{2}$ an oz., there was, on cooling, a copious deposit of white spicula.* The sublimate had a sharp, but not sour taste. Being boiled in muriatic acid, and also in nitric, it did not dissolve at all; but remained, on evaporation to dryness, in the same state as before; and it must be particularly observed, that it left no red or pink matter, on evaporating the nitric acid from it. Sulphuric acid did not act on it in the cold; but, when heated, it dissolved it, without effervescence, from which solution nothing was precipitated by caustic soda; on evaporating it to dryness, black fumes arose, leaving behind only a black stain. This sublimed matter did not render lime water turbid. Boiled in muriatic acid, so as to carry off all but a very little free acid, on the addition of lime water there was no turbid appearance, but milkiness ensued on adding oxalic acid. The spicula, in the 10 grs. of sublimate above-mentioned, seemed to be of the same nature as the matter just described. The whole of this sublimate amounted, by estimation, to 18 grs.; and I apprehend it is the acid sublimate of Scheele. The sublimate of car-

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* From the deposition of these spicula by cooling, and from many of the following properties, they appear to be analogous to benzoic acid.—Orig.

bonate of ammonia amounted to 20 grs.; and it was black empyreumatic animal oil which stained the tube.

This experiment was repeated, on 120 grs. of a nut-brown, very light, urinary concretion. The result was not very different from that of the former experiment, except that the gaz contained a portion of hydrogen gaz. There were 30 grs. of the above described spicula, principally mixed with carbonaceous matter: they were light, and had only a very slight sharp and bitter taste. The experiment repeated a 3d time, with 80 grs. of urinary concretion, afforded 15 grs. of the white spicula above described, mixed with carbonaceous matter. These I found did dissolve in a large proportion of muriatic acid; which solution yielded them, on evaporation, in the same state as before. Under the flame applied by the blow-pipe, they first melted, and then evaporated, without any smell; leaving a slight black mark. Turnsole was reddened by these spicula. In a 4th experiment, I found the white spicula contained in the carbonaceous matter united, on boiling, with carbonate of soda, as well as with caustic soda; but, as before, muriatic acid precipitated nothing from the solution. These spicula could not be dissolved in nitric acid; nor did the solution of them in water become turbid with oxalic acid. Their taste was, as before, rather bitter and sharp than sour. A very suffocating smell issued forth, on breaking the tube used in this experiment, but it was not from sulphur, nor from prussic acid.

These experiments afford evidence of the wide difference between the animal oxide above described and the acid sublimate of Scheele.* If this conclusion be allowed to be just, it will be necessary to give a name to this urinary animal oxide. Agreeably to the principles of the new chemical nomenclature, the name should be lithic oxide. But the term lithic is a gross solecism; and I trust that philological critics will find the name ouric or uric oxide perfectly appropriate; for, if it be thought objectionable, on account of the existence of the matter in arthritic as well as urinary concretions, still philology will allow its admission, as in other similar cases, *κατ' εξοχην*; it being found in greater abundance, by far, in the urinary passages, than in other situations, and therefore falling under common observation as an ingredient of the urine. If however the term of lithic oxide, or any other denomination, shall obtain acceptance, I shall very willingly adopt it.

It requires no sagacity, in a person acquainted with the facts of the preceding experiments, to perceive that they are applicable to a variety of uses in chemical investigation, and in the practice of physic. The latter I of course take no notice of in this place; but, relative to the former uses, I shall particularly point out, that we are now able not only to detect, in the easiest manner, the presence of

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* From these experiments, it now appears very doubtful whether the lithic acid of Scheele exists as a constituent of urinary concretions, or is compounded, in consequence of a new arrangement taking place, of the elementary matters of the concretion, by the agency of fire; but it is demonstrated, that the urinary animal oxide is really a constituent part, and even a principal one, of almost all human urinary calculi.—Orig.

the minutest proportion of the above animal oxide in urinary concretions, and also in other substances, but even to determine its proportion to the other constituent parts, in the space of a few minutes, in most cases, and in all in a very little time, without any other apparatus than nitric acid, a round bottomed matrass or glass dish, and a lamp. By this method, I have, in a general way, examined above 300 specimens of concretions, of the human subject and other animals, principally urinary ones; and also many from other parts, particularly those from the joints. For these opportunities I am beholden to several professional gentlemen; whose willingness to furnish me with specimens, I shall have much satisfaction in acknowledging on a future occasion. At present, I must acknowledge my obligations to Mr. Heaviside, in whose museum I found between 700 and 800 specimens. The liberal possessor of this treasure offered me, what I could not have taken the liberty of requesting, viz. permisson to break off pieces from any of the articles, for experiment. Mr. Edward Howard did me the honour to take on himself the task of writing down the reports, and otherwise assisted me. At this time I shall only mention, 1. That out of 200 specimens of urinary calculi, not more than 6 did not contain the animal oxide above described, i. e. about 32 out of 33 contained it. 2. That the proportion of this oxide was very different; varying from $\frac{1}{200}$ exclusive of water, to $\frac{198}{200}$; but, for the most part, varying between $\frac{80}{200}$ and $\frac{140}{200}$.* 3. That the common animal mucilage of urine is frequently found in concretions, in very different proportions; but is perhaps never a principal constituent part of them. 4. That the above animal oxide was not found in the urinary concretions, or any other concretions, of any animal but the human kind. 5. That this animal oxide was found also in human arthritic calculi, but not in those of the teeth, stomach, intestines, lungs, brain, &c.

P. S. I think proper to subjoin a few experiments, made after the preceding paper was written, which afford evidence of the truth of some of my conclusions, and enable us to explain several properties of animal concretions.

1. *On a urinary concretion from a dog.*—This calculus may be said to be a great curiosity, for it is probably the only specimen in London. I owe the opportunity of examining it to Mr. H. Leigh Thomas, who met with it in the course of his dissections; and therefore we have unquestionable authority, that the concretion was really from the urinary bladder of a dog. It is worthy to be noticed, that the animal appeared to be in perfect health. This concretion is of an oval figure; is $3\frac{3}{4}$ inches in length, and 3 inches in breadth; is white as chalk; its surface is rough and uneven. Being sawed through longitudinally, no nucleus was found, nor was it laminated, but near the centre it was radiated, and contained shining spicula. In other parts it was, for the most part, compact and uniform in its texture. It weighed nearly $10\frac{1}{2}$ oz. Its specific gravity was found to be greater than

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* In some urinary concretions, the interior part contained this oxide, and the exterior parts had none of it. On the contrary, in other urinary concretions, the exterior part contained it, and the interior part did not.—Orig.

that of human urinary concretions, in general; which I have learned by experiments is also the case with urinary and intestinal concretions of other brute animals, especially with those of the horse. The specific gravity of the present calculus was 1.7. That of one from the urinary bladder of the human subject, of the sort called mulberry calculus, and which consisted almost entirely of uric oxide, was 1.609. That of another human urinary concretion, of the same composition as the former, but quite smooth, extracted by Mr. Ford, was 1.571.

1. The present calculus of the dog had no taste, nor smell, till exposed to fire. 2. Under the blow-pipe it first became black, and emitted the smell of common animal matter; it next smelt strongly of empyreumatic liquor cornu cervi, and, after burning some time, became inodorous, and white, and readily melted, like super-phosphate of lime. 3. On trituration with lye of caustic soda, there was a copious discharge of ammonia. 4. It dissolved on boiling in nitric acid: the solution was clear and colourless; and, on evaporation to dryness, left a residue of white bitter matter, which, under the blow-pipe, emitted weakly the smell of animal matter. 5. On distilling a mixture of 150 grs. of this concretion pulverized and 2½ pints of pure water, to 3 oz., the distilled liquid was found to contain nothing but a little ammonia. The 3 oz. of residuary liquid, being filtrated and evaporated, yielded 20 grs. of phosphate of ammonia, with a little animal matter; and the residuary undissolved matter amounted to 67 grs. 6. These 67 grs., being triturated with 4 oz. of caustic soda lye, discharged very little ammonia. On distilling this mixture to 1 oz., a very small proportion only of ammonia was found in the distilled liquid. The residuary oz. of alkaline liquid was filtrated, and mixed with the water of elutriation of the undissolved matter. One half of those liquids, on evaporation to dryness, afforded a dark brown matter, amounting to 20 grs., which consisted of phosphate of lime and animal matter. To the other half of the alkaline liquids was gradually added muriatic acid, which occasioned a deposit, in small proportion, of matter that dissolved in nitric acid, but which, on evaporation to dryness, left behind only a brownish matter, consisting of phosphate of lime and animal matter. 7. The residuary insoluble substance in caustic lye, (6), under the blow-pipe, first turned black, and then white, but could not be melted. By diluted sulphuric acid it was decompounded. On the addition of nitrate of mercury, to the filtrated liquid, it yielded phosphate of mercury; and with oxalic acid, it afforded oxalate of lime; but no sulphate of magnesia was found remaining after these precipitations were produced. These experiments fully demonstrate, that the above concretion of a dog contained none of the uric or lithic oxide above described, but that it consisted, principally at least, of phosphate of lime, phosphate of ammonia, and animal matter. The present instance leads me to explain the reason of the fusibility of calculi. This is demonstrated, by the above experiments, to depend on the discharge and decomposition of the ammonia of the phosphate of ammonia, during the burning away of the animal matter; hence the residuary phosphoric acid readily fuses, and, uniting to the phosphate of

lime, composes superphosphate of lime, a very fusible substance. The phosphate of ammonia being dissolved out by water, or caustic alkaline lye, the remaining matter is infusible, being phosphate of lime. A very hard, brittle, and blackish intestinal calculus of a dog, from Mr. Wilson, was found to be of greater specific gravity than human urinary calculi, and to have the same composition as that of the dog above described. This also was found to be the composition of a white, smooth, round, intestinal calculus of a horse, the specific gravity of which was 1.791. The same composition was discovered, on examining a very hard, grey, brittle, laminated, quadrilateral concretion, said to be from the urinary bladder, but which, I think, was more probably from the intestines, of a horse.

2. *On a calculus from the urinary bladder of a rabbit.*—This is also a curiosity, being the only instance I have seen. I am also indebted to Mr. Thomas for this specimen, which he very kindly sent me, fitted up as a preparation, included in the bladder itself. Mr. Thomas found this concretion, on dissecting a perfectly healthy and very fat rabbit. This specimen is spherical, and of the size of a small nutmeg. It is of a dark brown colour, has a smooth surface, is hard, brittle, and heavy. When broken, it appeared to consist of concentric laminæ. Its specific gravity was 2. 1. Under the blow-pipe it became black, and emitted the smell of animal matter while burning; at last it ceased to emit any smell; and urged with the intensest fire, showed no signs of fusibility. 2. It readily dissolved, with effervescence, like marble, in both muriatic and nitric acids, giving clear solutions. 3. The nitric solution (2) being evaporated partly to dryness, and partly to the consistence of extract, the dry residuary matter was white; and the extract-like matter, which was bitter, could not be fused under the blow-pipe; but, when brought to the state of a powder, its particles were made to cohere loosely together into one mass. 4. On dropping sulphuric acid into the muriatic solution (2), turbidness, and a copious white precipitation, immediately ensued, from the composition of sulphate of lime.

From these experiments it is warrantable to conclude, that the above urinary calculus of a rabbit consisted principally of carbonate of lime and common animal matter, with perhaps a very small proportion of phosphoric acid: it certainly contained no uric oxide. I examined, in the same manner, a concretion which was said to be from the stomach of a monkey; but I have not evidence of its origin equally satisfactory as that of the last 2 calculi. Its composition was found to be similar to that of the calculus of the rabbit, viz. carbonate of lime and animal matter. Its obvious properties were also the same, it was of the size of the largest nutmeg

3. *On urinary concretions of the horse.*—I examined several specimens in cabinets, said to be vesical calculi of the horse, and found none of them to contain the uric oxide above described; but that they consisted, as well as the calculi from the stomach and intestines of the same animal, of phosphate of lime, phosphate of ammonia, and common animal matter, which melted like superphosphate of

lime, after burning away the animal matter and ammonia. As these, and some other experiments, seemed to concur in establishing an important truth, I thought it necessary to examine a urinary concretion of a horse, which, from its figure and size, was unquestionably from the kidney of that animal; for I have found by experience, that one cannot depend entirely on the accounts in cabinets, nor indeed sometimes on the assertions of persons who collect specimens.

1. This concretion, which Dr. Baillie was so good as to give me, was of a blackish colour, was very brittle and hard, and had no smell or taste. It felt heavier than human urinary calculi. 2. Under the blow-pipe it became quite black, and emitted the smell, weakly, of common animal matter. It was reduced very little in quantity, and showed no appearances of fusibility, after being exposed for a considerable time to the most intense fire of the blow-pipe. 3. Muriatic acid dissolved this concretion, with effervescence, yielding a clear solution; which, on evaporation to dryness, left a black and bitter residue. 4. A little of the residue (3) being boiled in pure water, to the filtrated liquor superoxalate of pot-ash was added; which occasioned a very turbid appearance, and copious white precipitation. 5. Nitric acid also readily dissolved this concretion, with effervescence. The solution being evaporated, partly to dryness, and partly to the consistence of an extract, the dry residuary matter was white and bitterish, and the extract-like part showed no signs of fusibility under the intensest fire of the blow-pipe. 6. A little of the concretion, being triturated with lye of caustic soda, emitted no smell of ammonia.

From these experiments it appears, that this calculus, like the former one from a rabbit, consists of carbonate of lime and common animal matter. A renal calculus of a horse, in Mr. Heaviside's collection, appeared, on examination, to consist of carbonate of lime and common animal matter. Another specimen however of renal calculus of a horse, in the same collection, marked N° 3, was found to consist of phosphate of lime, phosphate of ammonia, and common animal matter. It was fused under the blow-pipe. The specimen marked N° 8, in the same collection, which was said to be a vesical calculus of a horse, appeared to consist of the 3 ingredients just mentioned.

I have met with 2 instances of a deposit of a prodigious quantity of matter in the urinary bladder of horses, which had not crystallized, or even concreted: it amounted, in 1 specimen, which was given to me by Dr. Marshall, to several pounds weight; and in the other, which is in the possession of Mr. Home, to about 45 lb. Its composition was principally carbonate of lime and common animal matter*. I have not found any instance of human urinary calculi of a similar composition to that of the rabbit, and those of horses above described, which consist of carbonate of lime and animal matter; and I believe that human urinary calculi very rarely occur of a similar composition to those of the dog and horses above-mentioned,

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* Since this paper was read, Mr. Blizard has been so attentive as to send me another specimen of the same kind of deposit as those here mentioned. It now appears probable. that such deposits frequently take place, though I believe they have not been noticed before.—Orig.

which were found to consist of phosphate of ammonia, phosphate of lime, and animal matter, without containing uric oxide. The difference in the constitution of urinary concretions may depend on the difference of the urinary organs of different animals, on the food and drink*, and on the various diseased and healthy states of the urinary organs.

I have not found the uric oxide in the urinary concretions of any phytivorous animal; but whether it would be formed in the human animal when nourished merely by vegetable matter, must be determined by future observations. In the mean time, it is warrantable to conclude from analogy that it would not, and the application of this fact to practice is obvious; but I now purposely avoid making any practical inferences, till I can, at the same time, state a number of facts I have collected, relative both to concretions and to the urine itself.

III. On the Discovery of Four Additional Satellites of the Georgium Sidus. The Retrograde Motion of its old Satellites Announced; and the Cause of their Disappearance at Certain Distances from the Planet Explained. By Wm. Herschel, LL.D., F.R.S. p. 47.

Having lately been much engaged in improving my tables for calculating the places of the Georgian satellites, I found it necessary to re-compute all my observations of them. In looking over the whole series, from the year of the first discovery of the satellites in 1787 to the present time, I found these observations so extensive, especially with regard to a miscellaneous branch of them, that I resolved to make this latter part the subject of a strict examination. The observations alluded to relate to the discovery of 4 additional satellites: to surmises of a large and a small ring, at rectangles to each other: to the light and size of the satellites; and to their disappearance at certain distances from the planet. In this undertaking, I was much assisted by a set of short and easy theorems I had laid down for calculating all the particulars respecting the motions of satellites; such as, finding the longitude of the satellite from the angle of position, or the position from the longitude: the inclination of the orbit from the angle of position and longitude: the apogee: the greatest elongation; and other particulars. Having also calculated tables for reduction; for the position of the point of greatest elongation; and for the distance of the apogee, or opening of the ellipsis; and also contrived an expeditious application of the globe for checking computations of this sort, I found many former intricacies vanish. By the help of these tables and theorems, I could examine the miscellaneous observations relating to additional satellites, on a supposition that their orbits were in the same plane with the 2 already known, and that the direction of their motion was also the same with that of the latter.

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* I found the stomach-concretion called Oriental Bezoar, to consist merely of vegetable matter; as did the intestinal concretion of a sheep.—Orig.

And here I take an opportunity to announce, that the motion of the Georgian satellites is retrograde. This seems to be a remarkable instance of the great variety that takes place among the movements of the heavenly bodies. Hitherto all the planets and satellites of the solar system have been found to direct their course according to the order of the signs; even the diurnal or rotatory motions, not only of the primary planets, but also of the sun, and 6 of their secondaries or satellites, now are known to follow the same direction; but here we have 2 considerable celestial bodies completing their revolutions in a retrograde order.

I return to the examination of the miscellaneous observations, the result of which has been of considerable importance, and will be contained in this paper. The existence of 4 additional satellites of our new planet will be proved. The observations which tend to ascertain the existence of rings not appearing to be satisfactorily supported, it will be proper that surmises of them should either be given up, as ill founded, or at least reserved till superior instruments can be provided, to throw more light on the subject. A remarkable phenomenon, of the vanishing of the satellites will be shown to take place, and its cause animadverted on. I shall now, in the first place, relate the observations on which these conclusions must rest for support, and afterwards join some short arguments, to show that my results are fairly deduced from them. For the sake of perspicuity, I shall arrange the observations under 3 different heads; and begin with those which relate to the discovery of additional satellites. A great number of observations on supposed satellites, that were afterwards found to be stars, or of which it could not be ascertained whether they were stars or satellites, for want of clear weather, will only be related.

Dr. H. first transcribes, from his journal, a number of reports and observations, of a miscellaneous nature, some indicating suspicions of satellites, but mostly showing only small fixed stars. These observations extend through several years, from Feb. 6, 1782, to March 25, 1797. On the whole Dr. H. makes these following remarks.

An interior satellite.—The observation of Jan. 18, 1790, says, " a supposed 3d satellite is about 2 diameters of the planet following." There is not the least doubt expressed about the existence of the satellite, or object in question, which therefore must be considered as ascertained. Now the angle of the greatest elongation of the Georgian satellites, by my new tables, at the time of observation, was 81° 33′ N. F. Therefore the angle of the apogee was 8° 27′ S. F.; and since, by observation, the satellite was " following," without any mention of degrees being made, we may admit it to have been not far from the parallel; suppose 11 or 12° S. F. In this case, the satellite would be in the apogee about the time of the 2d observation, at $7^h\ 57^m$; which says, " I cannot perceive the satellite." But it will be shown hereafter, when I come to treat of the vanishing of the satellites, that it would become invisible in this situation. Indeed without the supposition of the satellite's coming to the apogee, it might easily happen that the least change in the clearness of the air, during a time of $1^h\ 5'$, which elapsed between the first and second observation, might render an object invisible, which, as the first observation says, was "excessively faint, and could only be seen by glimpses."

From the observed distance, which is put at "2 diameters of the planet," we may conclude what would be the distance of its greatest elongation. For, 2 diameters from the disc of the planet give 2½ from the centre. Now, the distance of the apogee at this time, by my tables, was .64, supposing that of the greatest elongation 1; therefore we have the radius of its orbit $\frac{2\,.\,5 \times 4''\,.\,12}{.64} = 16''.1$.

This calculation is not intended to determine precisely the distance of the satellite, but only to show that its orbit is more contracted than that of the 1st, and that consequently it is an interior satellite.

If any doubt should be entertained about the validity of this observation, we have a second, and very striking one, of March 5, 1794; where an interior satellite was suspected south following the planet, at one-third of the distance of the 1st. March 4, when a description was made of the stars, as in figure 4, this satellite was not in the place where it was observed the 5th. And, by an examination of the same stars March 7, it appears, that even the smallest stars n m o, of the 5th, were seen in their former places, but not the satellite. The observation therefore must be looked upon as decisive with regard to its existence. If any doubt should arise, on account of the suspicion not being verified with 480, I must remark, that being used to such imperfect glimpses, it has generally turned out, even when I have given up as improbable the existence of a supposed satellite seen in that manner, that it has afterwards nevertheless been discovered that a small star remained in the place where the satellite had been suspected to be situated. From the assigned place of this satellite, at ⅓ of the distance of that of the first, it appears that this observation belongs to the interior satellite of Jan. 18, 1790, which has already been examined. The 1st satellite was this evening at its greatest elongation, ⅓ of which is about 11″. The apogee distance of a satellite whose greatest distance is 16″.1 would have been 6″.1 on the day of our observation; but not being come to the apogee, by many degrees, it could not be so near the planet.

For the sake of greater precision, let us admit that the satellite was exactly south following; that is 45° from the parallel, and 45 from the meridian; then, by calculation, a satellite whose orbit is at 16″.1 from the planet, would, in the situation now admitted, have been 7″.1 from its centre, which might coarsely be rated at ⅓ of the distance of the first. But the estimation of 11″ is probably more accurate than that in the 1st observation, where 2 diameters are given. And, by calculating from this quantity, we find that the greatest elongation distance of the satellite is 25″.5: now putting 2½ diameters in the first observation, instead of 2, the distance deduced from it will come out 19″.3; which is certainly an agreement sufficiently near to admit both observations to belong to the same satellite.

March 27, 1794, was a 3d observation, which will assist in supporting the 2 former. A glimpse of a satellite is mentioned, which was preceding the 1st, but nearer the planet. The positions of the 1st satellite the same evening was, by measuring, found to be 62°.1 N. F. which is still a considerable way from its greatest elongation ; but our new satellite preceded it, and was therefore more advanced in its orbit, or nearer its greatest distance: and yet the observation says, that it was not so far from the planet as the 1st; though this latter was in a more contracted part of its orbit. It follows therefore that this was also an interior satellite. Now, since we may allow these 3 observations to belong to the same, we ought not to make a distinction ; but admit, as sufficiently established, the existence of at least one interior satellite of our new planet.

An intermediate satellite.—March 26, 1794. A satellite was suspected, directly north of the planet. At first it could not be verified, but was seen perfectly well afterwards. It was supposed that probably it might be a star, but this was left undecided. The observation of March 27th however removes all doubt on the subject; as it fully affirms that the small star observed the 26th, at 11^h 32^m, was gone from the place in which it was the day before. Such strong circumstances are mentioned in confirmation, that we cannot hesitate placing this among the list of existing satellites. It was not the interior satellite of Jan. 18, 1790; for both the 1st and 2d known satellites were in full view March 26th ; and the observation places this new one in a line drawn from the planet continued through the 1st; with the remark, that it was a little farther from the planet than the 1st. The 2d was then near its greatest southern elongation, and we may perceive that the orbit of this new satellite is situated between the orbits of the other two.

We have a 2d observation of the same satellite March 27, 1794; where, among the glimpses of additional satellites at 11^h 41^m, is mentioned one in a place probably agreeing with the new satellite of March 26th, which, by its motion, must have been carried forward, so as to be where the observation of the 27th says it was, namely, a little farther off and after the 1st ; that is, at a little greater distance from the planet than the 1st, and not so far advanced in its orbit as that satellite. This amounts not only to an additional proof, but even announces the recognition of the satellite, and its motion in the course of one day.

An exterior satellite.—Feb. 9, 1790. A new satellite was seen, in a line with the planet and the 2d satellite. To convince us that this was not a fixed star, we have the observations of 2 other nights, the 11th and 12th of February, where the removal of it, from the place in which it was Feb. 9, is clearly demonstrated. As it was in a line continued from the planet through the 2d satellite, its orbit must evidently be of a greater dimension than that of the 2d; I shall therefore set it down as an exterior satellite. Most likely this satellite also was seen among the supposed satellites south of the planet, March 27, 1794; where we find mention made of some others south, at a good distance. In that case, this will make a 2d observation.

We have a 3d observation of the same new satellite, March 5, 1796; when a very small star was seen, in a place where the evening before there had been none; as appears by the configuration of the 5th of March. At the time of the observation, the planet was come to the longitude of the place where the star was perceived to be; which agrees with the idea of its having been brought to that situation by the planet. It may be objected, that the star could not be verified with a power of 600; but here we have more than a bare suspicion of the satellite, for the observation says, I had a pretty certain glimpse of it; and this appears also from the assigned place of the star at the intersection of 2 given lines. For, such a delineation could not have been made, without having perceived it with a considerable degree of steady vision. Its distance, to judge by the description, will agree sufficiently with the foregoing 2 observations of this exterior satellite.

The most distant satellite.—On Feb. 28, 1794, a star was perceived where on the 26th there was none. This star was larger than a very small star which was observed the 26th, not far from the place of the new supposed satellite; and a configuration having been made expressly, by way of ascertaining what stars might afterwards come into a situation where they could be mistaken for satellites, our new star or satellite would not have been omitted, when a smaller one very near it was scrupulously recorded. The motion of the planet in $3^h\ 3^m$, is mentioned as very visible. The place of the star, which was a new visitor this evening, was very particularly delineated, at $6^h\ 50^m$. From its situation, it is evident that the motion of the planet must have carried this star, if it was one of its satellites, towards a large star near it; in the light of which a dim satellite would be lost. This accordingly happened; for at $10^h\ 7^m$ and $10^h\ 21^m$ it was no longer visible. The direction of the planet's motion is plainly pointed out, by the place of the planet March 2d. With respect to the orbit of this satellite, it appears, from its situation near the apogee, where it was seen, that its distance was to that of the 2d satellite, which was then near its greatest elongation, as 8 to 5. And since the apogee distance, on the day of observation, was only .37, we have its greatest elongation as $\frac{8}{.37}$ to 5; that is, as 21.6 to 5, or above 4 to 1. From which we may conclude, that its orbit must lie considerably without the before-mentioned exterior satellite of Feb. 9, 1790.

We have a 2d observation of it March 27, 1794; which, though not very strong, yet adds confirmation to the former. For that evening, which was uncommonly fine, other satellites, south, at a good distance, were perceived. This must relate principally to our present satellite, which may certainly be said to be at a good distance from the planet, and which, by that time, was probably in the southern part of its orbit, and near its greatest elongation. There is a 3d observation, March 28, 1797, which probably also belongs to this satellite. For an exceedingly small near star, which is mentioned as not having been seen the 25th, when the delineation of the stars was made, will agree very well with the 2 former observations; and, being near the greatest elongation, the distance of this satellite is well pointed out, and agrees remarkably well with the calculation of the first observation of it.

The arrangement of the 4 new and the 2 old satellites together will be thus: 1st sat. the interior one of Jan. 18, 1790.—2d sat. the nearest old one of Jan. 11, 1787.—3d sat. the intermediate one of March 26, 1794.—4th sat. the farthest old one of Jan. 11, 1787.—5th sat. the exterior one of Feb. 9, 1790.—6th sat. the most distant one of Feb. 28, 1794.

Next follow observations and reports tending to the discovery of one or more rings of the Georgian planet, and the flattening of its polar regions. After which, Dr. H. makes these remarks on the foregoing observations: With regard to the phenomena which gave rise to the suspicion of one or more rings, it must be noticed, that few specula or object-glasses are so very perfect as not to be affected with some rays or inequalities, when high powers are used, and the object to be viewed is very minute. It seems however, from the observations of March 16, 1789, and Feb. 26, 1792, that the cause of deception, in this case, must be looked for elsewhere. It has often happened, that the situation of the eye-glass, being on one side of the tube, which brings the observer close to the mouth of it, has occasioned a visible defect in the view of a very minute object, when proper care has not been taken to keep out of the way; especially when the wind is in such a quarter as to come from the observer across the telescope. The direction of a current of air alone may also affect vision. Without however entering further into the discussion of a subject that must be attended with uncertainty, I will only add, that the observation of the 26th seems to be very decisive against the existence of a ring. When the surmises arose at first, I thought it proper to suppose, that a ring might be in such a situation as to render it almost invisible; and that consequently observations should not be given up, till a sufficient time had elapsed to obtain a better view of such a supposed ring, by a removal of the planet from its node. This has now sufficiently been obtained in the course of 10 years; for, let the node of the ring have been in any situation whatsoever, provided it be kept to the same, we must by this time have had a pretty good view of the ring itself. Placing therefore great confidence on the observation of March 5, 1792, supported by my late views of the planet, I venture to affirm, that it has no ring in the least resembling that, or rather those, of Saturn.

The flattening of the poles of the planet seems to be sufficiently ascertained by many observations. The 7-feet, the 10-feet, and the 20-feet instruments, equally confirm it; and the direction pointed out Feb. 26, 1794, seems to be conformable to the analogies that may be drawn from the situation of the equator of Saturn, and of Jupiter. This being admitted, we may without hesitation conclude, that the Georgian planet also has a rotation on its axis, of a considerable degree of velocity.

Dr. H. then states several reports and observations relating to the light and size of the Georgian satellites, and to their vanishing at certain distances from the planet; from which he deduces the following remarks on those observations. From the observations of Jan. 14, Feb. 10, March 6, 1787, and Feb. 13, 1792, it appears, that all very small stars, when they come near the planet, lose much of their lustre. Indeed, every observation that has been recorded before, of supposed satellites that have been proved to be stars afterwards, has fully confirmed this circumstance; for they were always found to be considerable stars, and their being mistaken for satellites was owing to their loss of light when near the planet. This would hardly deserve notice, as it is well known that a superior light will obstruct an inferior one; but some circumstances which attend the operation of the affections of light on the eye, when objects are very faint, are so remarkable, that they must not be passed over in silence. After having been used to follow up the satellites of Saturn and Jupiter, to the very margin of their planets, so as even to measure the apparent diameter of one of Jupiter's satellites by its entrance on the disk, I was in hopes that a similar opportunity would soon have offered with the Georgian satellites: not indeed to measure the satellites, but to measure the planet itself, by means of the passage of the satellite over its disk. I expected also to have settled the epochs of the satellites, from their conjunctions and oppositions, with more accuracy than I have yet been able to do, from their various positions in other parts of their orbits. A disappointment of obtaining these capital advantages deserves to have its cause investigated; but, first of all, let us cast a look on the observations.

The satellites, we may remark, become regularly invisible, when, after their elongation, they arrive to certain distances from the planet. In order to find what these distances are, we will take the first observation of this kind, as an example. Feb. 22, 1791, the first satellite could not be seen. Now, by my lately constructed tables, its longitude from the apogee, at the time of observation, was 204.5 degrees; that is, 24.5 degrees from the most contracted part of its orbit, on the side that is turned to us, which, as its opposite is called the apogee, I shall call the perigee. By my tables also for the same day, we have the distance of the apogee from the planet, which is .60; supposing the greatest elongation distance to be 1. This being given, we may find an easy method of ascertaining the distance of the satellite, when it is near the apogee or perigee: for it will be sufficiently true for our purpose to use the following analogy. Cosine of the distance of the satellite from the apogee or perigee is to the apogee distance from the planet, as the greatest elongation is to the distance of the satellite from the planet. When the ellipsis is very open, this theorem will only hold good in moderate distances from the apogee or perigee; but when it is a good deal flattened, it will not be considerably out in more distant situations: and it will also be sufficiently accurate to take the natural cosine from the tables to 2 places of decimals only. When this is applied to our present instance, we have .91 for the natural cosine of 24.5°; and the distance of the satellite from the planet will come out $\frac{.6 \times 33''}{.91} = 21''.8$. By this method, it appears that the satellite, when it could not be seen, was nearly 22″ from the planet.

We must not however conclude, that this is the given distance at which it will always vanish. For instance, the same satellite, though hardly to be seen, was however not quite invisible March 2, 1791. Its distance from the planet, computed as before, was then only $\frac{.6 \times 33''}{1} = 19''.8$. The clearness of the atmosphere, and other favourable circumstances, must certainly have great influence in observations of very faint objects; therefore, a computation of all the observations where the satellites were not seen, as well as a few others where they were seen, when pretty near the apogee or perigee, will be the surest way of settling the fact. The result of these computations show that both the satellites became always invisible when they were near the planet: that the 1st was generally lost when it came within 18″ of the planet, and the 2d at the distance of about 20″. In very uncommon and beautiful nights, the 1st has once been seen at 13″.8, and the 2d at 17″.3; but at no time have they been visible when nearer the planet.

I shall now endeavour to investigate the cause which can render small stars and satellites invisible at so great a distance as 18 or 20″. A dense atmosphere of the planet would account for the defalcation of light sufficiently, were it not proved that the satellites are equally lost, whether they are in the nearest half of their orbits, or in that which is farthest from us. But as a satellite cannot be eclipsed by an atmosphere that is behind it, a surmise of this kind cannot be entertained. Let us then turn our view to light itself, and see whether certain affections between bright and very bright objects, contrasted with others that take place between faint and very faint ones, will not explain the phenomena of vanishing satellites.

The light of Jupiter or Saturn, for instance, on account of its brilliancy, is diffused, almost equally, over a space of several minutes all round these planets. Their satellites also, having a great share of brightness, and moving in a sphere that is strongly illuminated, cannot be much affected by their various distances from the planets. The case then is, that they have much light to lose, and comparatively lose but little. The Georgian planet, on the contrary, is very faint; and the influence of its feeble light cannot extend far, with any degree of equality. This enables us to see the faintest objects, even when they are only a minute or 2 removed from it. The satellites of this planet are very nearly the dimmest objects that can be seen in the heavens; so that they cannot bear any considerable diminution of their light, by a contrast with a more luminous object, without becoming invisible. If then the sphere of illumination of our new planet be limited to 18 or 20″, we may fully account for the loss of the satellites when they come within its reach; for they have very little light to lose, and lose it pretty suddenly. This contrast therefore, between the condition of the Georgian satellites and those of the brighter planets, seems to be sufficient to account for the phenomenon of their becoming invisible.

We may avail ourselves of the observations that relate to the distances at which the satellites vanish, to determine their relative brightness. The 2d satellite appears generally brighter than the 1st; but, as the former is usually lost farther from the planet than the latter, we may admit the 1st satellite to be rather brighter than the 2d. This seems to be confirmed by the observation of March 9, 1791; where the 2d appeared to be smaller than the 1st, though the latter was only 25″ from the planet, while the other was 30″.8. The first of the new satellites will hardly ever be seen otherwise than about its greatest elongations, but cannot be much inferior in brightness to the other 2; and if any more interior satellites should exist, we shall probably not obtain a sight of them; for the same reason that the inhabitants of the Georgian planet perhaps never can discover the existence of our earth, Venus, and Mercury. The 2d new or intermediate satellite is considerably smaller than the 1st and 2d old satellites. The 2 exterior, or 5th and 6th satellites, are the smallest of all, and must chiefly be looked for in their greatest elongations.

Periodical revolutions of the new satellites.—It may be some satisfaction to know what time the 4 additional satellites probably employ in revolving round the planet. Now, as this can only be ascertained with accuracy by many observations, we must of course remain in suspense till a series of them can be properly instituted. But, in the mean time, we may admit the distance of the interior satellite to be 25″.5, as our calculation of the estimation of March 5, 1794, gives it; and from this we compute that its periodical revolution will be 5 days, 21 hours, 25 minutes.—If we place the intermediate satellite at an equal distance between the 2 old ones, or at 38″.57, its period will be 10 days, 23 hours, 4 minutes.—By the figure of Feb. 9, 1790, it seems that the nearest exterior satellite is about double the distance of the farthest old one; hence its periodical time is found to be 38 days, 1 hour, 49 minutes.—The most distant satellite, according to the calculation of the observation of Feb. 28, 1794, is full 4 times as far from the planet as the old 2d satellite; it will therefore take at least 107 days, 16 hours, 40 minutes, to complete one revolution.—It will hardly be necessary to add, that the accuracy of these periods depends entirely on the truth of the assumed distances; some considerable difference therefore may be expected, when observations shall furnish us with proper data for more accurate determinations.

IV. An Inquiry concerning the Source of the Heat which is excited by Friction. By Benjamin Count of Rumford, F. R. S., M. R. I. A. p. 80.

Being engaged lately in superintending the boring of cannon in the workshops of the military arsenal at Munich, I was struck with the very considerable degree of heat which a brass gun acquires, in a short time, in being bored; and with the still more intense heat, much greater than that of boiling water, as I found by experiment, of the metallic chips separated from it by the borer. From whence comes the heat actually produced in the mechanical operation above-mentioned? Is it furnished by the metallic chips which are separated by the borer from the solid mass of metal? If this were the case, then, according to the doctrine of latent heat, and of caloric, the capacity for heat of the parts of the metal, so reduced to chips, ought not only to be changed, but the change undergone by them should be sufficiently great to account for all the heat produced. But no such change had taken place: for I found, on taking equal quantities, by weight, of these chips, and of thin slips of the same block of metal, separated by means of a fine saw, and putting them, at the same temperature, that of boiling water, into equal quantities of cold water, viz. at the temperature of $59^\circ\frac{1}{2}$ F., the portion of water into which the chips were put was not, to all appearance, heated either less or more than the portion, in which the slips of metal were put. This experiment being repeated several times, the results were always so nearly the same, that I could not determine whether any, or what change, had been produced in the metal, in regard to its capacity for heat, by being reduced to chips by the borer.

Hence it is evident, that the heat produced could not possibly have been furnished at the expence of the latent heat of the metallic chips. But, not being willing to rest satisfied with these trials, however conclusive they appeared to me to be, I had recourse to the following still more decisive experiment. Taking a cannon, a brass six-pounder, cast solid, and rough as it came from the foundry, and fixing it horizontally in the machine used for boring, and at the same time finishing the outside of the cannon by turning, I caused its extremity to be cut off; and, by turning down the metal in that part, a solid cylinder was formed, $7\frac{3}{4}$ inches in diameter, and $9\frac{8}{10}$ inches long; which, when finished, remained joined to the rest of the metal, that which, properly speaking, constituted the cannon, by a small cylindrical neck, only $2\frac{1}{5}$ inches in diameter, and $3\frac{8}{10}$ inches long. This short cylinder, which was supported in its horizontal position, and turned round its axis, by means of the neck by which it remained united to the cannon, was now bored with the horizontal borer used in boring cannon; but its bore, which was 3.7 inches in diameter, instead of being continued through its whole length, 9.8 inches, was only 7.2 inches in length; so that a solid bottom was left to this hollow cylinder, which bottom was 2.6 inches in thickness.

The cylinder being designed for the express purpose of generating heat by friction, by having a blunt borer forced against its solid bottom at the same time that it should be turned round its axis by the force of horses, in order that the heat accumulated in the cylinder might from time to time be measured, a small round hole, 0.37 of an inch only in diameter, and 4.2 inches in depth, for the purpose of introducing a small cylindrical mercurial thermometer, was made in it, on one side, in a direction perpendicular to the axis of the cylinder, and ending in the middle of the solid part of the metal which formed the bottom of its bore.

The solid contents of this hollow cylinder, exclusive of the cylindrical neck by which it remained united to the cannon, were $385\frac{3}{4}$ cubic inches, English measure; and it weighed 113.13lb. avoirdupois; as I found, on weighing it at the end of the course of experiments made with it, and after it had been separated from the cannon with which, during the experiments, it remained connected.

Experiment 1.—This experiment was made in order to ascertain how much heat was actually generated by friction, when, a blunt steel borer being so forcibly shoved, by means of a strong screw, against the bottom of the bore of the cylinder, that the pressure against it was equal to the weight of about 10000lb. avoirdupois, the cylinder was turned round on its axis, by the force of horses, at the rate of about 32 times in a minute. At the beginning of the experiment, the temperature of the air in the shade, as also that of the cylinder, was just 60° F. At the end of 30 minutes, when the cylinder had made 960 revolutions about its axis, the horses being stopped, a cylindrical mercurial thermometer, whose bulb was $\frac{32}{100}$ of an inch in diameter, and 3¼ inches in length, was introduced into the hole made to receive it, in the side of the cylinder, when the mercury rose almost instantly to 130°.

To see how fast the heat escaped out of the cylinder, (in order to be able to make a probable conjecture respecing the quantity given off by it, during the time the heat generated by the friction was accumulating,) the machinery standing still, I suffered the thermometer to remain in its place near ¾ of an hour, observing and noting down, at small intervals of time, the height of the temperature indicated by it, as in the annexed tablet. Thus,

At the end of Min.	The heat, as shown by the thermometer, was
4	126°
5	125
7	123
12	120
14	119
16	118
20	116
24	115
28	114
31	113
34	112
37½	111
41	110

Having taken away the borer, I now removed the metallic dust, or rather scaly matter which had been detached from the bottom of the cylinder by the blunt steel borer, in this experiment; and, having carefully weighed it, I found its weight to be 837 grains Troy. Is it possible that the very considerable quantity of heat that was produced in this experiment (a quantity which actually raised the temperature of above 113lb. of gun-metal at least 70 degrees of Fahrenheit's thermometer, and which, of course, would have been capable of melting 6½lb. of ice, or of causing near 5lb. of ice-cold water to boil,) could have been furnished by so inconsiderable a quantity of metallic dust? and this merely in consequence of a change of its capacity for heat? As the weight of this dust, 837 grains Troy, amounted to no more than $\frac{1}{948}$th part of that of the cylinder, it must have lost no less than 948 degrees of heat, to have been able to have raised the temperature of the cylinder 1 degree; and consequently it must have been given off 66360 degrees of heat, to have produced the effects which were actually found to have been produced in the experiment!

But, without insisting on the improbability of this supposition, we have only to recollect, that from the results of actual and decisive experiments, made for the express purpose of ascertaining that fact, the capacity for heat, of the metal of which great guns are cast, is not sensibly changed by being reduced to the form of metallic chips, in the operation of boring cannon; and there does not seem to be any reason to think that it can be much changed, if it be changed at all, in being reduced to much smaller pieces, by means of a borer that is less sharp. If the heat, or any considerable part of it, were produced in consequence of a change in the capacity for heat of a part of the metal of the cylinder, as such change could only be superficial, the cylinder would by degrees be exhausted; or the quantities of heat produced, in any given short space of time, would be found to diminish gradually, in successive experiments. To find out if this really happened or not, I repeated the last-mentioned experiment several times, with the utmost care; but I did not discover the smallest sign of exhaustion in the metal, notwithstanding the large quantities of heat actually given off. Finding so much reason to conclude, that the heat generated in these experiments, or excited, as I would rather choose to express it, was not furnished at the expence of the latent heat or combined caloric of the metal, I pushed my inquiries a step further, and endeavoured to find out whether the air did, or did not, contribute any thing in the generation of it.

Exper. 2.—As the bore of the cylinder was cylindrical, and as the iron bar, to the end of which the blunt steel borer was fixed, was square, the air had free access to the inside of the bore, and even to the bottom of it, where the friction took place by which the heat was excited. As neither the metallic chips produced in the ordinary course of the operation of boring brass cannon, nor the finer scaly particles produced in the last-mentioned experiments by the friction of the blunt borer, showed any signs of calcination, I did not see how the air could possibly have been the cause of the heat that was produced; but, in an investigation of this kind, I thought that no pains should be spared to clear away the rubbish, and leave the subject as naked and open to inspection as possible. In order, by one decisive experiment, to determine whether the air of the atmosphere had any part, or not, in the generation of the heat, I contrived to repeat the experiment, under circumstances in which it was evidently impossible for it to produce any effect whatever. By means of a piston exactly fitted to the mouth of the bore of the cylinder, through the middle of which piston the square iron bar, to the end of which the blunt steel borer was fixed, passed in a square hole made perfectly air-tight, the access of the external air, to the inside of the bore of the cylinder, was effectually prevented. I did not find however, by this experiment, that the exclusion of the air diminished, in the smallest degree, the quantity of heat excited by the friction.

There still remained one doubt, which, though it appeared to be so slight as hardly to deserve any attention, I was however desirous to remove. The piston which closed the mouth of the bore of the cylinder, in order that it might be air-tight, was fitted into it with so much nicety, by means of its collars of leather, and pressed against it with so much force, that, notwithstanding its being oiled, it occasioned a considerable degree of friction, when the hollow cylinder was turned round its axis. Was not the heat produced, or at least some part of it, occasioned by this friction of the piston? and, as the external air had free access to the extremity of the bore, where it came in contact with the piston, is it not possible that this air may have had some share in the generation of the heat produced?

Exper. 3. A quadrangular oblong deal box, water-tight, $11\frac{1}{2}$ English inches long, $9\frac{4}{10}$ inches wide, and $9\frac{6}{10}$ inches deep, being provided, with holes or slits in the middle of each of its ends, just large enough to receive, the one, the square iron rod to the end of which the blunt steel borer was fastened, the other, the small cylindrical neck which joined the hollow cylinder to the cannon; when this box was put into its place was fixed to the machinery, in such a manner that its bottom being in the plane of the horizon, its axis coincided with the axis of the hollow metallic cylinder; it is evident, from the description, that the hollow metallic cylinder would occupy the middle of the box, without touching it on either side; and that, on pouring water into the box, and filling it to the brim, the cylinder would be completely covered, and surrounded on every side, by that fluid. And further, as the box was held fast by the strong square iron rod which passed, in a square hole, in the centre of one of its ends, while the round or cylindrical neck, which joined the hollow cylinder to the end of the cannon, could turn round freely on its axis in the round hole in the centre of the other end of it, it is evident that the machinery could be put in motion, without the least danger of forcing the box out of its place, throwing the water out of it, or deranging any part of the apparatus. Every thing being ready, I proceeded to make the experiment I had projected, in the following manner.

The hollow cylinder having been previously cleaned out, and the inside of its bore wiped with a clean towel till it was quite dry, the square iron bar, with the blunt steel borer fixed to the end of it, was put into its place; the mouth of the bore of the cylinder being closed at the same time, by means of the circular piston, through the centre of which the iron bar passed. The box was then put in its place, and the joinings of the iron rod, and of the neck of the cylinder, with the two ends of the box, having been made water-tight, by means of collars of oiled leather, the box was filled with cold water, (viz. at the temperature of 60°) and the machine was put in motion. The result of this beautiful experiment was very striking, and the pleasure it afforded me amply repaid me for all the trouble I had had, in contriving and arranging the complicated machinery used in making it. The cylinder, revolving at the rate of about 32 times in a minute, had been in motion but a short time, when I perceived, by putting my hand into the water, and touching the outside of the cylinder, that heat was generated; and it was not long before the water which surrounded the cylinder began to be sensibly warm. At the end of 1 hour, I found, by plunging a thermometer into the water in the box, (the quantity of which fluid amounted to 18.77 lb. avoirdupois, or $2\frac{1}{4}$ wine gallons), that its temperature had been raised no less than 47 degrees; being now 107° of Fahrenheit's scale. When 30 minutes more had elapsed, or 1 hour and 30 minutes after the machinery had been put in motion, the heat of the water in the box was 142°. At the end of 2 hours, reckoning from the beginning of the experiment, the temperature of the water was found to be raised to 178°. At 2 hours 20 minutes it was at 200°; and at 2 hours 30 minutes it actually boiled!

It would be difficult to describe the surprize and astonishment expressed in the countenances of the by-standers, on seeing so large a quantity of cold water heated, and actually made to boil, without any fire. Though there was, in fact, nothing that could justly be considered as surprizing in this event, yet I acknowledge fairly that it afforded me a degree of childish pleasure, which, were I ambitious of the reputation of a grave philosopher, I ought most certainly rather to hide than to discover. The quantity of heat excited and accumulated in this experiment was very considerable; for, not only the water in the box, but also the box itself, which weighed 15¼ lb., and the hollow metallic cylinder, and that part of the iron bar which, being situated within the cavity of the box, was immersed in the water, were heated 150° of Fahrenheit's scale; viz. from 60°, which was the temperature of the water, and of the machinery, at the beginning of the experiment, to 210°, the heat of boiling water at Munich. The total quantity of heat generated may be estimated with some considerable degree of precision, as follows:

Of the heat excited there appears to have been actually accumulated,	Quantity of ice-cold water which, with the given quantity of heat, might have been heated 180 degrees, or made to boil. In avoirdupois weight.
In the water contained in the wooden box, 18¾ lb. avoirdupois, heated 150°, namely, from 60° to 210° F.	15.2 lb.
In 113.13 lb. of gun-metal, the hollow cylinder, heated 150°; and, as the capacity for heat of this metal is to that of water as 0.1100 to 1.0000, this quantity of heat would have heated 12½ lb. of water the same number of degrees	10.37
In 36.75 cubic inches of iron, being that part of the iron bar to which the borer was fixed which entered the box, heated 150°; which may be reckoned equal in capacity for heat to 1.21 lb. of water.	1.01
N. B. No estimate is here made of the heat accumulated in the wooden box, nor of that dispersed during the experiment.	
Total quantity of ice-cold water which, with the heat actually generated by friction, and accumulated in 2h 30m, might have been heated 180°, or made to boil.	26.58

From the knowledge of the quantity of heat actually produced in the foregoing experiment, and of the time in which it was generated, we are enabled to ascertain the velocity of its production, and to determine how large a fire must have been, or how much fuel must have been consumed, in order that, in burning equably, it should have produced by combustion the same quantity of heat in the same time. In one of Dr. Crawford's experiments, (See his Treatise on Heat, p.321), 37 lb. 7 oz. troy, = 181920 gr., of water, were heated $2\frac{1}{10}$ degrees of Fahrenheit's thermometer, with the heat generated in the combustion of 26 grs. of wax. This gives 382032 grs. of water heated 1° with 26 grs. of wax; or $14693\frac{14}{26}$ grs. of water heated 1°, or $\frac{14693}{180} = 81.631$ grs. heated 180°, with the heat generated in the combustion of 1 gr. of wax. The quantity of ice-cold water which might have been heated 180°, with the heat generated by friction in the before-mentioned experiment, was found to be 26.58 lb. avoirdupois, = 188060 grs.; and, as 81.631 grs. of ice-cold water require the heat generated in the combustion of 1 gr. of wax, to heat it 180°, the former quantity of ice-cold water, namely 188060 grs., would require the combustion of no less than 2303.8 grs. = $4\frac{8}{10}$ oz. Troy, of wax, to heat it 180°.

As the experiment, N° 3, in which the given quantity of heat was generated by friction, lasted 2^h 30^m, = 150^m, it is necessary, for the purpose of ascertaining how many wax candles of any given size must burn together, in order that in the combustion of them the given quantity of heat may be generated in the given time, and consequently with the same celerity as that with which the heat was generated by friction in the experiment, that the size of the candles should be determined, and the quantity of wax consumed in a given time by each candle, in burning equably, should be known. Now I found by an experiment, made on purpose to finish these computations, that when a good wax candle, of a moderate size, $\frac{3}{4}$ of an inch in diameter, burns with a clear flame, just 49 grs. of wax are consumed in 30^m. Hence it appears, that 245 grs. of wax would be consumed by such a candle in 150^m: and that, to burn the quantity of wax, = 2303.8 grs., necessary to produce the quantity of heat actually obtained by friction in the experiment in question, and in the given time, 150^m, 9 candles, burning at once, would not be sufficient; for, 9 multiplied into 245, the number of grains consumed by each candle in 150^m, amounts to no more than 2205 grs.; whereas the quantity of wax necessary to be burnt, in order to produce the given quantity of heat, was found to be 2303.8 grs.

From the result of these computations it appears, that the quantity of heat produced equably, or in a continual stream, by the friction of the blunt steel borer against the bottom of the hollow metallic cylinder, in the experiment under consideration, was greater than that produced equably in the combustion of 9 wax candles, each $\frac{3}{4}$ of an inch in diameter, all burning together, or at the same time, with clear bright flames. As the machinery used in this experiment could easily be carried round by the force of one horse, (though, to render the work lighter, two horses were actually employed in doing it), these computations show further how large a quantity of heat might be produced, by proper mechanical contrivance, merely by the strength of a horse, without either fire, light, combustion, or chemical decomposition; and, in a case of necessity, the heat thus produced might be used in cooking victuals. But no circumstances can be imagined, in which this method of procuring heat would not be disadvantageous; for, more heat might be obtained by using the fodder necessary for the support of a horse, as fuel.

As soon as the last-mentioned experiment, N° 3, was finished, the water in the wooden box was let off, and the box removed; and the borer being taken out of the cylinder, the scaly metallic powder, which had been produced by the friction of the borer against the bottom of the cylinder, was collected, and, being carefully weighed, was found to weigh 4145 grs., or about 8⅔ oz. Troy. As this quantity was produced in 2½ hours, this gives 824 grs. for the quantity produced in half an hour. In the first experiment, which lasted only half an hour, the quantity produced was 837 grs. In the experiment N° 1, the quantity of heat generated, in half an hour, was found to be equal to that which would be required to heat 5lb. avoirdupois of ice-cold water 180°, or cause it to boil. According to the result of the experiment N° 3, the heat generated in half an hour, would have caused 5.31 lb. of ice-cold water to boil. But in this last-mentioned experiment the heat generated being more effectually confined, less of it was lost; which accounts for the difference of the results of the two experiments.

It remains for me to give an account of one experiment more, which was made with this apparatus. I found by the experiment N° 1, how much heat was generated when the air had free access to the metallic surfaces which were rubbed together. By the experiment N° 2, I found that the quantity of heat generated was not sensibly diminished when the free access of the air was prevented; and, by the result of N° 3, it appeared that the generation of the heat was not prevented, or retarded, by keeping the apparatus immersed in water. But as, in this last-mentioned experiment, the water, though it surrounded the hollow metallic cylinder on every side, externally, was not suffered to enter the cavity of its bore, being prevented by the piston, and consequently did not come into contact with the metallic surfaces where the heat was generated; to see what effects would be produced by giving the water free access to these surfaces, I now made the

Exper. 4. The piston which closed the end of the bore of the cylinder being removed, the blunt borer and the cylinder were once more put together; and the box being fixed in its place, and filled with water, the machinery was again put in motion. There was nothing in the result of this experiment that renders it necessary to be very particular in the account of it. Heat was generated, as in the former experiments, and to all appearance quite as rapidly; and there is no doubt but the water in the box would have been brought to boil, had the experiment been continued as long as the last. The only circumstance that surprized me was, to find how little difference was occasioned in the noise made by the borer in rubbing against the bottom of the bore of the cylinder, by filling the bore with water. This noise, which was very grating to the ear, and sometimes almost insupportable, was, as nearly as I could judge of it, quite as loud, and as disagreeable, when the surfaces rubbed together were wet with water, as when they were in contact with air.

By meditating on the results of all these experiments, we are naturally brought to that great question which has so often been the subject of speculation among philosophers; namely, what is heat?—Is there any such thing as an igneous fluid?—Is there any thing that can with propriety be called caloric? We have seen that a very considerable quantity of heat may be excited in the friction of two metallic surfaces, and given off in a constant stream or flux, in all directions, without interruption or intermission, and without any signs of diminution, or exhaustion. Then whence came the heat which was continually given off in this manner, in the foregoing experiments? Was it furnished by the small particles of metal, detached from the larger solid masses, on their being rubbed together? This, as we have already seen, could not possibly have been the case. Was it furnished by the air? This could not have been the case; for in 3 of the experiments the machinery being kept immersed in water, the access of the air of the atmosphere was completely prevented.

Was it furnished by the water which surrounded the machinery? That this could not have been the case is evident; first, because this water was continually receiving heat from the machinery, and could not, at the same time, be giving to, and receiving heat from, the same body; and 2dly, because there was no chemical decomposition of any part of this water. Had any such decomposition taken place, one of its component elastic fluids, most probably inflammable air, must, at the same time, have been set at liberty, and, in making its escape into the atmosphere, would have been detected; but though I frequently examined the water, to see if any air bubbles rose up through it, and had even made preparations for catching them, in order to examine them, if any should appear, I could perceive none; nor was there any sign of decomposition of any kind whatever, or other chemical process, going on in the water.

Is it possible that the heat could have been supplied by means of the iron bar to the end of which the blunt steel borer was fixed? or by the small neck of gun-metal by which the hollow cylinder was united to the cannon? These suppositions appear more improbable even than either of those before-mentioned; for heat was continually going off, or out of the machinery, by both these passages, during the whole time the experiment lasted. And, in reasoning on this subject, we must not forget to consider that most remarkable circumstance, that the source of the heat generated by friction, in these experiments, appeared evidently to be inexhaustible. It is hardly necessary to add, that any thing which any insulated body, or system of bodies, can continue to furnish without limitation, cannot possibly be a material substance: and it appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of any thing, capable of being excited, and communicated, in the manner the heat was excited and communicated in these experiments, except it be motion.

I am very far from pretending to know how, or by what means, or mechanical contrivance, that particular kind of motion in bodies, which has been supposed to constitute heat, is excited, continued, and propagated, and I shall not presume to trouble the Society with mere conjectures; particularly on a subject which, during so many thousand years, the most enlightened philosophers have endeavoured, but in vain, to comprehend. But, though the mechanism of heat should, in fact, be one of those mysteries of nature which are beyond the reach of human intelligence, this ought by no means to discourage us, or even lessen our ardour, in our attempts to investigate the laws of its operations. How far can we advance in any of the paths which science has opened to us, before we find ourselves enveloped in those thick mists which, on every side, bound the horizon of the human intellect? But how ample, and how interesting, is the field that is given us to explore! Nobody surely, in his sober senses, has ever pretended to understand the mechanism of gravitation; and yet what sublime discoveries was our immortal Newton enabled to make, merely by the investigation of the laws of its action! The effects produced in the world by the agency of heat, are probably just as extensive, and quite as important, as those which are owing to the tendency of the particles of matter towards each other; and there is no doubt but its operations are, in all cases, determined by laws equally immutable.

V. Observations on the Foramina Thebesii of the Heart. By Mr. John Abernethy, F.R.S. p. 103.

As the investigation of the resources of nature in the animal economy, for the maintenance of health, and the prevention of disease, cannot but be interesting to the philosopher as well as to the physician, I am therefore induced to submit to the R. S. the following observations. There is a remarkable contrivance in the blood vessels which supply the heart, not to be met with in any other part of the body, and which is of great use in the healthy functions of that organ, but which is particularly serviceable in preventing disease of a part so essential to life. A distended state of the blood vessels must always impede their functions, and consequently be very detrimental to the health of the part which they supply; but as the cavities of the heart are naturally receptacles of blood, a singular opportunity is afforded to its nutrient vessels, to relieve themselves when surcharged, by pouring a part of their contents into those cavities. Such appears to be the use of the foramina by which injections, thrown into the blood vessels of the heart, escape into the cavities of that organ; and which were first noticed by Vieussens, but, being more expressly described by Thebesius, generally bear the name of the latter author.

Anatomists appear to have been much perplexed concerning these foramina Thebesii; even Haller, Senac, and Zinn, were sometimes unable to discover them; which suggested an idea, that when an injection was effused into the cavities of the heart, the vessels were torn, and that it did not escape through natural openings. When these foramina were injected, they were found under various circumstances, as to their size and situation; and Haller observed, that the injection, for the most part, escaped into the right cavities of the heart. It also remains undetermined, whether these foramina belong both to the arteries and veins, or respectively to each set of vessels.

It is from an examination of these openings in diseased subjects, that a solution of such difficulties may probably be obtained. Whoever reflects on the circumstances under which the principal coronary vein terminates in the right auricle of the heart, will perceive that an impediment to the flow of blood through that vessel must occasionally take place; but the difficulty will be much increased, when the right side of the heart is more than ordinarily distended, in consequence of obstruction to the pulmonary circulation. Indeed it seems probable that such an obstruction, by occasioning a distended state of the right side of the heart, and thus impeding the circulation in the nutrient vessels of that organ, would as necessarily occasion corresponding disease in it, as an obstruction to the circulation in the liver occasions disease in the other abdominal viscera, were it not for some preventing circumstances, which I now proceed to explain.

Having been attentive to some very bad cases of pulmonary consumption, from a desire to witness the effects of breathing medicated air in that complaint, I was led to a more particular examination of the heart of those patients who died. In these cases, I found, that by throwing common coarse waxen injection into the arteries and veins of the heart, it readily flowed into the cavities of that organ; and that the left ventricle was injected in the first place, and most completely. When the ventricle was opened, and the effused injection removed, the foramina Thebesii appeared both numerous and large, and distended with the different coloured wax which had been impelled into the coronary arteries and veins. On 8 comparative trials, made by injecting the vessels of hearts taken from subjects whose lungs were either much diseased, or in a perfectly sound state; I found, that in the former, common injection readily flowed, in the manner which I have described, into all the cavities of the heart, but principally into the left ventricle; while, in many of the latter, I could not impel the least quantity of such coarse injection into that cavity.

This difference in the facility with which the cavities of the heart can be injected from its nutrient vessels, was observed by most anatomists, though they did not advert to the circumstances on which it depended. Haller's recital of his own observations, and of those of others on this subject, so well explain the facts which I have stated, that I shall take the liberty of quoting the passage, in order further to illustrate and authenticate them. He says, "Si per arterias liquorem injeceris, perinde in dextra auricula, sinuque et ventriculo dextro, et in sinu atque thalamo sinistro guttulæ exstillabunt; sæpe quidem absque mora, alias difficilius, et nonnunquam omnino, uti continuo dicemus, et mihi, et Senaco, et clarissimo Zinnio, nihil exsudavit."—Elem. Physiol. Tom. 1, p. 382.

As it seems right that the blood which had been distributed by the coronary arteries, and which must have lost, in a greater or less degree, the properties of arterial blood, should not be mixed with the arterial blood which is to be distributed to every part of the body, but ought rather to be sent again to the lungs, in order that it may re-acquire those properties; we therefore perceive why, in a natural state of the heart, the principal foramina Thebesii are to be found in the right cavities of that organ. However, as, even in a state of health, those cavities are liable to be uncommonly distended, in consequence of muscular exertion sometimes forcing the venous blood into the heart faster than it can be transmitted through the lungs, there seems to arise a necessity for similar openings on the left side; but these, in their natural state, though capable of emitting blood, and of relieving the plethora of the coronary vessels, are not of sufficient size to give passage to common waxen injections. Yet, when there is a distended state of the right cavities of the heart, which is almost certainly occasioned by a diseased state of the lungs, these foramina leading into the left cavities then become enlarged, in the manner that has been already described; and thus the plethoric state of the nutrient vessels of the heart, and the consequent disease of that important organ, are prevented. The preceding remarks will, I think, sufficiently explain the cause of the variety in the size and situation of these foramina, which also appear to belong both to the arteries and veins; because the injection which was employed was too coarse to pass from one set of vessels to the other, and yet the different coloured injections passed into the cavities of the heart unmixed.

There is yet another mode by which diseases of the heart, that would otherwise so inevitably succeed to obstruction in the pulmonary vessels, are avoided; and which I next beg leave to explain. Having formerly been much surprized to find the heart so little affected, when the lungs were greatly diseased, and observing, in one or two instances, that the foramen ovale was open, I was led to pay more particular attention to the state of that part; and I have found this to be almost a constant occurrence in those subjects where pulmonary consumption had for some time existed previous to the person's decease. I took notice of this circumstance 13 times in the course of one year; and in several instances the aperture was sufficiently large to admit of a finger being passed through it. Now, as the septum auricularum is almost constantly perfect in subjects whose lungs are healthy, I cannot but conclude, that the renewal of the foramen ovale is the effect of disease; nor will the opinion appear on reflection improbable; for the opening becomes closed by the membranous fold growing from one edge of it, till it overlaps the other, and their smooth surfaces being kept in close contact, by the pressure of the blood in the left auricle, they gradually grow together. But, should there be a deficiency of blood in the left auricle, and a redundance in the right, the pressure of the latter on this membranous partition, will so stretch and irritate the uniting medium, as to occasion its removal; and thus a renewal of the communication between the auricles will again take place.

From these observations it is natural to suppose, that in those men, or animals, who are accustomed to remain long under water, this opening will either be maintained or renewed; yet on this circumstance alone the continuance of their life does not depend; for we now have sufficient proof, that if the blood is not oxygenated in the lungs, it is unfit to support the animal powers. There is an experiment related by Buffon, the truth of which I believe has not been publicly controverted, and which tends greatly to misrepresent this subject. He says, that he caused a bitch to bring forth her puppies under warm water; that he suddenly removed them into a pail of warm milk; that he kept them immersed in the milk for more than half an hour; and that when they were taken out of it, all the 3 were alive. He then allowed them to respire about half an hour, and again immersed them in the warm milk, where they remained another half hour; and when taken out 2 were vigorous, but the 3d seemed to languish: this submersion was again repeated, without apparent injury to the animals.

This experiment is so directly contrary to what we are led to believe from all others, and also to the information derived from cases which frequently occur in the practice of midwifery, in which an interruption to the circulation through the umbilical chord occasions the death of the fœtus, as to make me suspect its truth: I was therefore induced to examine what would happen in a similar experiment. I did not indeed cause the bitch to bring forth her puppies in water; but immersed a puppy, shortly after its birth, under water which was of the animal temperature. It lost all power of supporting itself in about 60 seconds, and would shortly have perished, had I not removed it into the air. Neither could I, by repeating this experiment, so accustom the animal to the circulation of unoxygenated blood, as to lengthen the term of its existence in such an unnatural situation. I thought that a dog might have been made a good diver in this way; but having satisfied myself that this could not be done, without greatly torturing the animal, I did not choose to prosecute so cruel an experiment.

Young animals, indeed, retain their irritability for a considerable time, so that they move long after they have been plunged beneath water; and may even, on this account, recover after they are taken out. But the manner in which Buffon has related his experiment seems to imply, that the circulation of the blood, and other functions of life, were continued after the animals had been excluded from the air. I am convinced that the poor dog which was the subject of my experiment would have been beyond recovery in a few minutes. Those animals which are accustomed to remain long under water, probably first fill their lungs with air, which may, in a partial manner, oxygenate their blood during their submersion. The true statement of this subject may probably be, that the circulation of venous blood will destroy most animals in a very short space of time; but that custom may enable others to endure it, with very little change, for a longer period.

VI. Analysis of the Earthy Substance from New South Wales, called Sydneia or Terra Australis. By Charles Hatchett, Esq., F. R. S. p. 110.

§ 1. The late ingenious Josiah Wedgwood, Esq., F.R.S., published, in the Phil. Trans. for 1790, an account of some analytical experiments on a mineral substance from Sydney Cove, in New South Wales. This substance he describes as composed of a fine white sand, a soft white earth, some colourless micaceous particles, and also some which were black, resembling black mica, or black lead. Nitric acid did not appear to act on any part of this earthy substance; and even a portion on which sulphuric acid had been boiled to dryness, afforded afterwards, when edulcorated with water, only a few flocculi, which Mr. Wedgwood conceived to be aluminous earth. The muriatic acid, during digestion, seemed to act as little as the 2 preceding acids; but on water being poured in, to wash out the remaining portion, the liquor instantly became white as milk, with a fine white curdy substance intermixed; the concentrated acid having, in the opinion of the author, extracted something which the simple dilution with water precipitated. The remaining part was repeatedly digested with muriatic acid, and treated with water, as before, till the milky appearance was no longer produced.

The properties of this white precipitate, Mr. Wedgwood states to be as follows. 1°. It is only soluble in boiling concentrated muriatic acid. 2°. It is precipitated by water, in the form of a white earth; which may again be dissolved by boiling muriatic acid. 3°. When nitric acid is mixed with the muriatic solution of this earth, there is no appearance of a precipitate; not even when water is added, provided the nitric acid exceeds, or nearly approaches, the quantity of muriatic acid. 4°. The earth is precipitated by the alkalies. 5°. The muriatic solution does not crystallize by evaporation; but becomes a butyraceous mass, which soon liquefies on exposure to the air. 6°. The butyraceous mass is not corrosive to the taste; and is even less pungent than the combination of calcareous earth with the same acid. 7°. Heat approaching to ignition disengages the acid from the butyraceous mass, in white fumes, and a white substance remains. 8°. The white precipitated earth is fusible per se, in from 142° to 156° of Mr. Wedgwood's thermometer, and it is thus distinguished from all the other primitive earths. And, 9°. This precipitate cannot be reduced to a metallic state, when exposed to heat with inflammable substances.

From these properties, Mr. Wedgwood says, that though he cannot absolutely determine whether this substance belongs to the class of earths, or that of metallic substances, yet he is inclined to refer it to the former. Professor Blumenbach, of Gottingen, in his Manual of Natural History, published in 1791, also mentions that he had examined a portion of this earthy substance, by means of muriatic acid, after the manner of Mr. Wedgwood, and that he had obtained a slight precipitate by the addition of water. In consequence of these experiments, the mineralogists throughout Europe admitted the white precipitated substance to be a primitive earth; and we accordingly find, in all the systematical works on mineralogy published since the above-mentioned period, that it is arranged as a distinct genus, under the names of Sydneia, Australa, Terra Australis, and Austral Sand.

The extreme scarcity of this substance prevented the chemists in general from examining more minutely into the nature of this new primitive earth, till Mr. Klaproth, in the 2d volume of his Additions to the Chemical Knowledge of Mineral Bodies, gave to the public a memoir entitled, A Chemical Examination of the Austral Sand. In this memoir, Mr. Klaproth says, that he had received from Mr. Haidinger, of Vienna, 2 samples of this substance; one of which had a considerable quantity of black shining particles intermixed with it, which, though regarded by many as graphite or plumbago, he was inclined to believe to be eisenglimmer or micaceous iron ore. The other contained much less of these black or dark grey particles, and, as he considered it to be more pure than the former, he subjected it to the following experiments. 1. It was digested at 3 different times with concentrated muriatic acid, in a boiling heat, and the acid was afterwards filtrated through paper. The solution was then mixed by degrees with pure water, which did not however produce any precipitate, even when warmed. Carbonate of pot-ash caused some flocculi to fall, which, edulcorated and dried, weighed 3.25 grs. This precipitate was dissolved in diluted sulphuric acid, and left a small portion of siliceous earth; after which the solution, by evaporation, afforded crystals of alum.

2. The residuum of the muriatic solution was mixed with 3 times the weight of pot-ash, and exposed to a red heat. Muriatic acid was then poured on the mass, and the insoluble gelatinous residuum was edulcorated on a filter; and, after a red heat, weighed 19.50 grs., which proved to be siliceous earth. 3. The muriatic solution, with prussiate of pot-ash, afforded a blue precipitate; the ferruginous part of which was about $\frac{1}{4}$ gr. 4. The solution was then saturated with carbonate of pot-ash, and some alumine was precipitated; which, after a red heat, weighed 8.50 grs., and with sulphuric acid formed alum. Siliceous earth, alumina, and iron, appeared therefore to be the only ingredients of this substance; but, as Mr. Klaproth had no more than 30 grs. to examine, he could not extend his experiments.

From those above related, he is of opinion that the existence of this primitive earth may be much doubted, and that this doubt can only be removed in the course of time, by other analyses. Mr. Klaproth concludes his memoir by saying, that the substance examined by him was undoubtedly the genuine austral sand, as Mr. Haidinger had received it from Sir Jos. Banks, when he was in London.

Mr. Nicholson however, in the 9th N° of his Journal of Nat. Philos. &c. p. 410, published on the 1st of Dec. 1797, questions much, whether the substance examined by Mr. Klaproth was the same as that examined by Mr. Wedgwood; and, after having contrasted their experiments, says, "hence it seems fair to conclude that the 2 minerals were not the same, however this may have happened; and that the existence of the new fusible earth of Wedgwood stands on the same evidence as before, namely, his experiments, which have not yet been repeated, that I know of." Some of Mr. Nicholson's objections to the experiments of Mr. Klaproth, being founded principally on some difference in the external characters of the substance examined by him, and the one examined by Mr. Wedgwood, are such as very naturally occur; but the following pages will I believe prove, that Mr. Klaproth's experiments were made on that which might be justly regarded as the Sydneia or austral sand.

In 1796, Sir Jos. Banks, P. R. S. favoured me with a specimen of the Sydneia, which has been lately brought to England; a portion of this I soon after examined, in a cursory manner, by muriatic acid, but did not obtain any precipitate when water was added to the filtrated solution. On mentioning this circumstance, and expressing a desire to examine this substance with more accuracy, Sir Jos. Banks, not only permitted me to take specimens from different parts of the box which contained the earth already mentioned, but that every doubt might be obviated, gave me about 300 grs. which remained of the identical substance examined by Mr. Wedgwood. On these the following experiments were made; and, to distinguish them, I shall call the first, N° 1, and that examined by Mr. Wedgwood, N° 2.

§ 2. *Analysis of the Sydneia*, N° 1.—The Sydneia, N° 1, is in masses and lumps, of a pale greyish white, intermixed with a few particles of white mica, and also occasionally with some which are of a dark grey, resembling graphite or plumbago. It easily crumbles between the fingers, to a powder nearly impalpable, which has rather an unctuous feel. Small fragments of vegetable matter are also commonly found intermixed with it; and the general aspect is that of an earthy substance which has been deposited by water.

Exper. 1. 400 grs. were put into a glass matrass, and 1 quart of distilled water being added, the whole was boiled to $\frac{1}{4}$th. The liquor was then filtrated, and a portion being examined by the re-agents commonly used, afforded no trace of matter in solution. The remainder was then evaporated, without leaving any residuum.

Exper. 2. About 200 grs. of the earth, rubbed to a fine powder, were put into a glass retort, into which I poured 3 oz. of concentrated pure muriatic acid. The retort was placed in sand, and the acid was distilled, till the matter in the retort remained dry. 2 oz. of muriatic acid were again poured on it, and distilled as before, till $\frac{1}{4}$ remained. The whole was then put into a matrass, which was placed in an inclined position, so that when the earth had subsided, the liquor might be decanted, without disturbing the sediment. When it had remained thus for 12 hours, the acid was carefully poured into a glass vessel; but, as I observed that it was not so perfectly transparent as before it had been thus employed, I suffered it to remain 24 hours, but did not perceive any sediment. Half of this liquor was diluted with about 12 parts of distilled water, and after a few hours a very small quantity of a white earth subsided. This however did not appear to be a precipitate caused by a change in the chemical affinities, but rather an earthy matter which had been suspended in the concentrated acid, and afterwards deposited, when the liquor was rendered less dense by the addition of water. To ascertain this, I poured the remaining portion of the concentrated liquor on a filter of 4 folds: it passed perfectly transparent, and, though diluted with 24 parts of water, it remained unchanged, and as pellucid as before. I now filtrated the former portion, and added it to that already mentioned.

It was then evaporated to dryness, and left a pale brownish mass, which was dissolved again, by digestion, in the smallest possible quantity of muriacic acid. Water was added, in a very large proportion, to this solution, without producing any effect; I then, by prussiate of pot-ash, precipitated a quantity of iron, which was separated by a filter. The clear solution was then saturated with lixivium of carbonate of pot-ash, and a white precipitate was produced, which was collected and edulcorated. This, when digested with diluted sulphuric acid, was dissolved; and the superfluous acid being driven off by heat, boiling water was poured on the residuum, which completely dissolved it. To this solution some drops of lixivium of pot-ash were added, and by repeated evaporations the whole formed crystals of alum. From the above experiment it appeared, that the muriatic acid had only dissolved some alumina and iron; but, in order to satisfy myself more completely in respect to the component parts of this substance, I made the following analysis.

Analysis.—A. 400 grs. were put into a glass retort, which was then made red-hot during half an hour. Some water came over, and the earth afterwards weighed 380.80 grs., so that the loss amounted to 19.20 grs. The greater part of this loss was occasioned by the dissipation of the water imbibed by the earth; to which must be added, the loss of weight caused by the combustion of a small portion of vegetable matter.

B. The 380.80 grs. were rubbed to a fine powder, and being put into a glass retort, 1470 grs. of pure concentrated sulphuric acid were added. The retort was then placed in a small reverberatory, and the fire was gradually increased, till the acid was distilled over: it was then poured back on the matter in the retort, and distilled as before, till a mass nearly dry remained. On this, boiling distilled water was repeatedly poured, till it no longer changed the colour of litmus paper, and was devoid of taste. The undissolved portion was then dried, and made red-hot; after which it weighed 281 grs.

C. I now mixed the 281 grs. with 300 grs. of dry carbonate of pot-ash, and exposed the mixture to a strong read heat, in a silver crucible, during 4 hours. The mass was loose, and of a greyish white: it was softened with water, and being put into a retort, sulphuric acid was added to a considerable excess. The whole was then distilled to dryness, and when a sufficient quantity of boiling water had been added, it was poured on a filter, and the residuum was well washed; it was then made red-hot, and afterwards weighed 274.75 grs.

D. The solutions of B and C were added together, and were much reduced by evaporation. Pure ammonia was then employed to saturate the acid, and a copious loose precipitate, of a pale yellowish colour, was produced; which, collected, edulcorated, and made red-hot, weighed 103.70 grs.—E. The filtrated liquor of D was again evaporated, and carbonate of pot-ash being added, a slight precipitation of earthy matter took place; which, by the test of sulphuric acid proved to be some alumina which had not been precipitated in the former experiment: this weighed 1.20 grs.—F. The 103.70 grs. of D were completely dissolved when digested with nitric acid, excepting a small residuum of siliceous earth, which weighed 0.90 grs.—G. The nitric solution was evaporated to dryness, and a 2d portion of the same acid was added, and in like manner evaporated. The residuum was then made red-hot, and digested with diluted nitric acid, which left a considerable portion of red oxide of iron. The solution was again evaporated, and the residuum, being treated as before, again deposited some oxide of iron, much less in quantity than the former. The whole of the oxide was then heated with wax in a porcelain crucible, was taken up by a magnet, and weighed 26.50 grs.

H. The nitric solution of G was saturated with ammonia, and a loose white precipitate was formed; which edulcorated and made red-hot, weighed 76 grs.— I. These 76 grs. were dissolved when digested with diluted sulphuric acid; and, when the excess of acid had been expelled by heat, the saline mass was dissolved in boiling water. To this solution I added some lixivium of pot-ash, and by gradual and repeated evaporations, obtained the whole in regular octoedral crystals of alum.

K. The 274.75 grs. of C now alone remained to be examined. They appeared to consist of siliceous earth, mixed with the dark grey shining particles already mentioned; but, as I shall describe, in the following experiments, the process by which these were separated, I shall now only say that they amounted to 7.50 grs.— L. The earth with which the above-mentioned particles were mixed weighed 267.25 grs. This earth was white, and arid to the touch: when melted with 2 parts of soda, it formed a colourless glass; and with 4 parts of the same it dissolved in water, and formed a liquor silicum; it was therefore pure siliceous earth or silica. The substance here examined was composed therefore of the following ingredients.

		Grains.
Pure siliceous earth or silica	F.	0.90
	L.	267.25
Alumina	E.	1.20
	H.	76.
Oxide of iron	G.	26.50
Dark grey particles	K.	7.50
Water and vegetable matter	A.	19.20
		398.55

The foregoing analysis was repeated several times, and always with similar results; excepting, that as I had taken the specimens from different parts of a large quantity, I found that the proportions of the ingredients were not constantly the same: that of the siliceous earth, for example, was sometimes greater, and the alumina and iron proportionably less. Some specimens were also nearly or totally destitute of the dark grey shining particles; in short, every circumstance was such as might be expected from a mixed substance, which, from the nature of its formation, cannot have the ingredients in any fixed proportion*. As this substance agreed in its general characters, for the greater part, with that described by Mr. Wedgwood, and as it was indisputably brought from the same place, there appeared every reason to believe that the nature of both was the same; but, to obviate as much as possible any doubt or objection, I determined to repeat the experiments, and the analysis, on that portion which remained of the identical substance examined by Mr. Wedgwood, and which from that period had been reserved by Sir Jos. Banks.

§ 3. *Analysis of the Sydneia,* N° 2.—This substance, then consists of a white

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* The description given by Mr. Klaproth convinces me, that his experiments were made on a portion of this substance. Also, when Mr. Haidinger was in London, I gave him some of this earth for his collection; so that, whether Mr. Klaproth made his experiments on that which had been received by Mr. Haidinger from Sir Jos. Banks, or from myself, it is not less certain that he operated on that which might be regarded as the genuine Sydneia.—Orig.

transparent quartzose sand, a soft opaque white earth, some particles of white mica, and a quantity of dark lead-grey particles, which have a metallic lustre. The Sydneia, N° 2, appears chiefly to differ from N° 1, by being more arenaceous, and by a larger proportion of the dark grey particles. Many experiments, similar to those made on N° 1, already described, were made on this substance, with pure concentrated muriatic acid; but as none of these afforded any appearance of a precipitate by the means of water, I do not think it necessary to enter into a circumstantial account of them, and shall proceed therefore to the analysis.

A. 100 grs. were exposed to a red heat, in a glass retort, and, after ½ an hour, were found to have lost in weight 2.20 grs.—B. The 97.80 grs. which remained were mixed with 300 grs. of dry carbonate of pot-ash, and the mixture was exposed to a strong red heat, in a crucible of silver, during 3 hours. When cold, the mass was softened with water, and was put into a glass matrass. I then added 3 oz. of pure concentrated muriatic acid, and digested it for 2 hours in a strong sand heat. Boiling water was then added, and the whole being poured on a filter, the residuum was edulcorated, dried, and made red-hot; it then weighed 85.50 grs.—C. The filtrated solution was evaporated to ¼, and pure ammonia being added, a precipitate was formed, which, after a red heat, weighed 10.70 grs.—D. 1 oz. of muriatic acid was poured on the 10.70 grs., in a matrass, which was then heated. The whole of the 10.70 grs. was dissolved, excepting a small portion of siliceous earth, which weighed 0.30 gr.

E. The muriatic solution was then reduced by evaporation, to about ¼; to which was added a large quantity of distilled water, which did not however produce any change. I then gradually added a solution of pure crystallized prussiate of pot-ash, and heated the liquor till the whole of the iron was precipitated; after which, ammonia precipitated a loose white earth, which, edulcorated and made red-hot, weighed 7.20 grs. The iron precipitated by the prussiate may therefore be estimated at 3.20 grs.—F. The 7.20 grs. of the white earth were digested with sulphuric acid, and, after the excess of acid had been expelled by heat, boiling water was poured on the saline residuum. The solution was then gradually evaporated, with the addition of a small portion of lixivium of potash, and afforded crystals of alum, without a trace of any other substance.

G. I now proceeded to examine the 85.50 grs. of B. These appeared to consist of siliceous earth, or fine particles of quartz, mingled with a considerable quantity of the dark grey shining particles. Mr. Wedgwood was of opinion that these were a peculiar species of plumbago or graphite. Professor Blumenbach, on the contrary, regards them as molybdæna: and Mr. Klaproth believes them to be eisenglimmer or micaceous iron ore. When rubbed between the fingers, they leave a dark grey stain, and the feel is unctuous, like that of plumbago, or molybdæna: the traces which they make on paper also resemble those of the above-mentioned substances, but the lustre of the particles approaches nearer to that of molybdæna.

In order therefore to determine whether they consisted totally or partially of molybdæna, I put the 85.50 grs. into a small glass retort, and added 2 oz. of concentrated nitric acid. The retort was then placed in a sand heat, and the distillation was continued, till the matter remained dry. The acid was then poured back into the retort, and distilled as before; but I did not observe that the grey particles had suffered any change, nor were nitrous fumes produced, as when molybdæna is thus treated. To be more certain however, I digested pure ammonia on the residuum; and having decanted it into a matrass, I evaporated it to dryness, without perceiving any vestige of oxide of molybdæna, or indeed of any other substance.

It was evident therefore that molybdæna was not present; and as the general external characters and properties corresponded with those of plumbago, I was inclined to believe that these were particles of that substance, and not micaceous iron, as Mr. Klaproth imagined. To determine this, the following experiment was made.

H. 200 grs. of pure nitre in powder were mixed with the 85.50 grs., and the mixture was gradually projected into a crucible, made strongly red-hot. A feeble detonation took place at each projection; and, after a ¼ hour had elapsed, the crucible was removed. When cold, the mass was porous and white, without any appearance of the dark grey particles. Boiling water was poured on it, and the whole being put into a matrass, 1 oz. of muriatic acid was added, and digested with it in a sand heat. By evaporation it became gelatinous: it was then emptied on a filter, and being well washed, dried, and made red-hot, weighed 75.25 grs. The appearance of this was that of a white earth, arid to the touch. When melted with 2 parts of soda, a colourless glass was formed; and, with 4 parts of the same, it was soluble in water, and produced liquor silicum; it was therefore pure siliceous earth.

I. The filtrated liquor was saturated with ammonia, and, on being heated, a few brownish flocculi were precipitated, which, when collected and dried, weighed 0.40 grs. This precipitate was dissolved in muriatic acid, and was again precipitated by prussiate of pot-ash, in the state of Prussian blue. The liquor from which the flocculi of iron had been separated was then examined, by adding carbonate of pot-ash, and lastly, by being evaporated to dryness; but it no longer afforded any earthy or metallic substance: so that, by the process of detonation with nitre, the 85.50 grs. afforded 75.25 grs. of pure siliceous earth, with 0.40 gr. of iron; and, as the dark grey substance was destroyed, excepting the 0.40 gr. of iron abovementioned, and as 9.85 gr. of the original weight of 85.50 gr. were dissipated, there can be no doubt but that this substance, amounting to 10.25 grs., was carburet of iron or plumbago; especially as some experiments which I purposely made, on that from Keswick in Cumberland, were attended with similar results. It is also evident, that these particles could not be eisenglimmer or micaceous iron, as nitre has little or no effect on that substance, when projected into a heated crucible.

In a subsequent experiment on the same, the crucible was removed immediately after the last projection, and I then observed that an effervescence, with a disengagement of carbonic acid, took place, on the addition of the muriatic acid, as is usual when pure plumbago is decomposed by nitre, and that less of the gelatinous matter was formed by evaporation. The cause of this difference was evidently the duration of the red heat; for in the first instance the alkali developed by the decomposition of the nitre had time to unite with the siliceous earth, so as, when dissolved, to form liquor silicum; but in the 2d experiment a portion of alkali remained combined with the carbonic acid, produced by the carbon of the decomposed plumbago. The produce of 100 grs. by this analysis was as annexed. Mr. Wedgwood says, that sulphuric acid cannot dissolve the precipitated earth, and has but little effect on the mixed substance, even when distilled to dryness; but, from the preceding experiments, I had reason to believe that the aluminous earth and iron would be separated by reiterated distillation; I therefore repeated the analysis in the following manner.

		Grains.
Silica	D.	0.30
	H.	75.25
Alumina	F.	7.20
Oxide of iron	E.	3.20
Graphite or plumbago	I.	10.25
Water	A.	2.20
		98.40

Second analysis of the Sydneia, N° 2.—A. 100 grs. of the earth were put into a glass retort, on which 400 grs. of pure concentrated sulphuric acid were poured. The retort was placed in a small reverberatory, and the fire was continued till a dry mass remained. 400 grs. of the acid were again poured in, and distilled as before. On the dry mass, boiling water was poured, and the whole was then emptied on a filter, and edulcorated. The residuum, after a red heat, weighed 87.75 grs., and consisted of siliceous earth, mixed with some mica, and with particles of plumbago.

B. The filtrated solution, by ammonia, afforded a precipitate, which weighed 9.50 grs.; and, being examined, as in the former experiment, yielded 6.50 grs. of alumina, and 3 grs. of oxide of iron. The plumbago was separated from the siliceous matter, in the manner already described, and amounted to about 10 grs. By this analysis I obtained as annexed: it appears therefore that the Sydneian earth, when treated with sulphuric acid, is capable of being for the greater part decomposed; and Mr. Wedgwood probably did not succeed, because his process was in some respect different, or that the distillation was not sufficiently repeated. I have not thought it necessary to be more circumstantial in the account of this 2d analysis, as the operations were similar to those of the former.

	Grains.
Silica and mica	77.75
Alumina	6.50
Oxide of iron	3
Plumbago	10
	97.25

§ 4. These experiments prove, that the earthy substance called Sydneia or terra australis, consists of siliceous earth, alumina, oxide of iron, and black lead or graphite. The presence of the latter appears to be accidental, and it probably was mixed with the other substances at the time when they were transported, and deposited, by means of water; for this appears evidently to have been the case, from the general characters of this mixed earthy substance. The quartz and mica, which are so visible, indicate a granitic origin; and the soft white earth has probably been formed by a decomposition of feldt spar, such as is to be seen in many places, and particularly at St. Stephen's, in Cornwall. The granitic sand which covers the borders of the Mer de Glace, at Chamouni, in Savoy, also much resembles the terra australis, excepting that the feldt spar is not in a state of decomposition: in short, the general aspect, and the analysis, concur to prove, that the Sydneia has been formed by the disintegration and decomposition of granite, or gneiss.

Mr. Wedgwood's experiments are so circumstantial, that had I only examined the earth last brought to England, I should have supposed, with Mr. Nicholson, that I had operated on a different substance; but, as I had an opportunity to examine, by analysis, a portion of the same earth on which Mr. Wedgwood made his experiments, and as I received it from Sir Jos. Banks, the same gentleman who had furnished Mr. Wedgwood with it, no suspicion can be entertained about its identity.

Some of the experiments which I have related, and which prove that some of the finer earthy particles remained suspended in the concentrated muriatic acid, and were precipitated when the acid was diluted with water, appear in some measure to account for the mistake which has been made, in supposing that a primitive earth, before unknown, was present; but this alone will not account for many of the other properties mentioned by Mr. Wedgwood, such as, 1st. The repeated and exclusive solubility in the muriatic acid, and subsequent precipitation by water. 2d. The butyraceous mass which was formed by evaporation. And, 3dly. The degree of fusibility of the precipitated earth. These indeed I can by no means explain, but by supposing that the acids used by Mr. Wedgwood were impure. This supposition appears to be corroborated by a passage in Mr. Wedgwood's paper, where he says, "here the Prussian lixivium, in whatever quantity it was added, occasioned no precipitation at all, only the usual bluishness arising from the iron always found in the common acids." Now if, as it seems from this expression, Mr. Wedgwood employed the common acids of the shops, without having previously examined and purified them, all certainty of analysis must fall, as the impurity of such acids is well known to every practical chemist: but, whether this was the cause of the effects described by Mr. Wedgwood, I do not hesitate to assert, that the mineral which has been examined does not contain any primitive earth, or substance possessing the properties ascribed to it, and consequently, that the Sydneian genus, in future, must be omitted in the mineral system.

VII. *Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, for* 1796. *By Thos. Barker, Esq.* p. 130.

		Barometer.			Thermometer.						Rain.
					In the House.			Abroad.			
		Highest.	Lowest.	Mean.	Hig.	Low	Mean	Hig.	Low	Mean	
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.
Jan.	Morn.	29.77	28.47	29.17	50	41	45	52½	32	42½	1.955
	Aftern.				52	41½	46	55	34	47	
Feb.	Morn.	29.87	28.53	32	45½	38	42	49	31½	38	1.643
	Aftern.				51	34	42½	51½	32	43	
Mar.	Morn.	29.97	29.22	57	48	33	41	45	26	36	0.376
	Aftern.				51	34	42½	57	34	45	
Apr.	Morn.	29.88	28.67	59	59	44	50½	52	36	45	0.649
	Aftern.				61	45	52	72	45	57½	
May	Morn.	29.73	28.33	27	55	47	51	52	40	46	2.839
	Aftern.				56	48	52	65	48	56	
June	Morn.	29.84	29.05	47	59	50½	55	60	46	54	0.927
	Aftern.				70	52	57	80	56	67	
July	Morn.	29.73	28.91	32	67	50	58½	64½	49½	56½	5.646
	Aftern.				67½	54½	60½	77	59	68½	
Aug.	Morn.	29.95	29.23	60	65½	57	61	62½	49	56	1.120
	Aftern.				68½	58½	63	77	57	69	
Sept.	Morn.	29.83	29.03	50	65	53	59½	63	42½	54	1.891
	Aftern.				68	54	61	77	56	66	
Oct.	Morn.	30.07	28.75	45	55½	43½	49	59	29	43	1.320
	Aftern.				56	44½	50½	61½	41	51½	
Nov.	Morn.	29.86	28.69	36	51½	39½	43	52½	26	38	2.048
	Aftern.				53	39	43½	57	28½	43	
Dec.	Morn.	29.98	28.75	31	44½	28	34	48	14½	30	1.668
	Aftern.				46½	29½	35	49	25	34	
Whole year.				29.41			50			49	22.082

VIII. *Of some Endeavours to Ascertain a Standard of Weight and Measure. By Sir G. Shuckburgh Evelyn, Bart. F.R.S. and A. S.* p. 133.

Having for some years turned my thoughts to the consideration of an invariable and imperishable standard of weight and measure, as a thing, in a philosophical view, highly desirable, and likely to become extremely beneficial to the public, I had, so early as 1780, taken up the idea of a universal measure, whence all the rest might be derived, by means of a pendulum with a moveable centre of suspension, capable of such adjustments, as to be made to vibrate any number of times in a given interval; and, by comparison of the difference of the vibrations with the difference of the lengths of the pendulum, which difference alone might be the standard measure, to determine its positive length, if that should be thought preferable, under any given circumstances; by which means, all the difficulties arising in determining the actual centre of motion and of oscillation, which have hitherto so much embarrassed these experiments, would be obviated.

I made several computations of the probable accuracy that might be expected from such an experiment, and was satisfied with their result. But, not seeing clearly how such a pendulum could be connected to a piece of mechanism, to number the vibrations without affecting them, I dropped the idea for that time. I learnt however, some time afterwards, that Mr. John Whitehurst, a very ingenious person, had been in pursuit of the same object with better success, and had contrived a machine fully corresponding to his expectations and my wishes. This he afterwards explained to the world, in a pamphlet, entitled, "An Attempt to obtain Measures of Length, &c. from the Mensuration of Time, or the true Length of Pendulums;" published in 1787. Mr. Whitehurst having there done all that related to the standard measure of length, and suggested that of weight, it appeared to me that it remained only to verify and complete his experiments.

For this purpose, by the assistance of Dr. G. Fordyce, who, at Mr. Whitehurst's death, had purchased his apparatus, I was furnished with the very machine with which Mr. Whitehurst had made his observations. I also procured to be made, by Mr. Troughton, an excellent beam-compass or divided scale, furnished with microscopes and micrometer, for the most exact observations of longitudinal measure: as also a very nice beam or hydrostatic balance, sensible with the $\frac{1}{100}$ of a grain, when loaded with 6 lb. Troy at each end. Mr. Arnold made me one of his admirable time-keepers, in order to carry time from my sidereal regulator in my observatory, with which it was adjusted, to the room where I had fixed Mr. Whitehurst's pendulum; and who, having taken a journey from London into Warwickshire, was so good as to assist in the beginning of these experiments. Thus equipped, I went to work in the latter end of Aug. 1796, when the temperature was about 60°, first to examine the length of the pendulum; when, to my great mortification, I found that the thin wire, of which the rod consisted, was too weak to support the ball in a state of vibration; and that, after 15 or 20 hours action, it repeatedly broke. The same misfortune attended my trials with 3 other different sorts of wires that I had obtained from London. Whether this accident happened from any rust in the old wire, or from want of due temper in the new, or from its being too much pinched between the cheeks, I cannot tell: I can only observe, that all the wires that I used were considerably heavier, and therefore probably stronger, than what Mr. Whitehurst mentions, viz. 3 grains in weight for 80 inches in length; nay, mine proceeded as far as from 5 to 6 grains for that length, and yet I could never get it to support the ball during the whole period of my experiment. This being the case, and being in the country, far removed from the manufactory of this fine wire, I was reluctantly compelled to relinquish this part of the operation to some more favourable opportunity. In the mean while however, I thought it desirable to measure the difference of the lengths of Mr. Whitehurst's pendulum from his own observations; for, very fortunately, the marks that he had made on the brass vertical ruler of his machine were still visible; and this interval, which he calls "59.892 inches," I determined, on my divided scale made by Troughton, from Mr. Bird's standard, to be = 59.89358 inches, from a mean of

4 different trials in the temperature of 64°; that mean differing from the extremes only by .0003 inch.

By this examination, if I have not verified, I have at least preserved, Mr. Whitehurst's standard; and, for the present, I shall consider this measure of the difference of the length of the two pendulums, vibrating 42 and 84 times in a minute of mean time, as correct. On this presumption, I shall proceed to the examination of weight.

From the opinion of different skilful persons, with whom I have conferred, as well as from the result of my own considerations, I am inclined to believe there is hardly any body in nature, with which we are familiarly acquainted, that is of so simple and homogeneous a quality as pure distilled water, or so fit for the purposes of this inquiry; and I have concluded, that if the weight of any quantity of water, whose bulk had been previously measured by the above-mentioned scale, could be obtained, under a known pressure* and temperature of the atmosphere, we should be in possession of a general standard of weight.

With this view, I directed Mr. Troughton to make, in addition to the very sensible hydrostatic balance before-mentioned, a solid cube of brass, whose sides should be 5 inches; and also a cylinder of the same metal, 4 inches in diameter, and 6 high. From St. Thomas's hospital, by the favour of Dr. Fordyce, I procured 3 gallons of distilled water. With these I made the following observations; but, before I relate the experiments, I will describe the apparatus. Here Sir G. S. adds, the description of his instruments: these were, a beam-compass, or divided scale of equal parts, and a hydrostatic balance. A hollow sphere of brass, of 6 inches diameter, was also provided. Sir G. gives a minute detail of the curious experiments and observations he made to ascertain the exact figures, measures, dimensions, weights, and specific gravities of all these bodies; the results of all of these being as follow:

Of the brass cube, he measured very nicely the length of all the 12 linear edges; viz. as it stood on one end as a base, all the 4 linear sides of the top, all those of the bottom, and all the 4 upright edges: the medium of the first 4 was 4.98882; of the 2d, 4.98955; and of the 3d, 4.98925; all rather below the intended measure, 5 inches. The 3 mean dimensions multiplied all together, gave 124.18917 cubic inches, for the solid content of the cube.

In like manner, of the cylinder, the mean of several diameters taken at one end, was 3.99745 inches; at the other end, 3.99785; and the medium of several measures of length was 5.99502; which 3 dimensions multiplied all together, and by the circular factor .7854, gave 74.94823 cubic inches, for the solid content of the cylinder.

The diameter of the sphere was, by a medium of many measurements, found to

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* I do not here mean to infer any opinion respecting the compressibility of water; but only to say, that where water, or any thing else, is weighed in air, the density of that medium, as shown by the barometer and thermometer, must be known, in order to make allowances for it, if necessary.—Orig.

be 6.00745 inches; consequently the cube of this multiplied by .5236, gives 113.5194 cubic inches, for the solid contents of the sphere.

Now the results of the several weighings of the cube and cylinder, both in air and distilled water, as contained in the following synopsis.

	Cube.	Therm.	Cylinder.	Therm.	Barom.
	Inches.				Inches.
Contents (true to $\frac{2}{100000}$) in inches	124.18917	61°	74.94826	62°	
	grains.				
Weight in air, true to 0.02 grain........	32084.82	62	21560.05	62	29.00
Weight in water, true to 0.10 grain......	703.03	60.2	2553.22	60.5	29.47
Weight of an equal bulk of water, true to 0.12 grain, or $\frac{1}{190000}$..........	31381.79		19006.83		
Weight of a cubic inch of water, from these experiments................	252.694		253.600*		

The weight of the sphere in air was 28722.42 grains; and the same in water was 28673.51 grains; this divided by its content, 113.519, gives 252.587 grains, for the weight of a cubic inch of distilled water, by Mr. Troughton's weights;† the barometer being 29.74, and the thermometer 66.

Having, through the means of Mr. Whitehurst's observations, and of his own instrument, ascertained the length of his proposed standard, in the latitude of London, 113 feet above the level of the sea, under a density of the atmosphere corresponding to 30 inches of the barometer, and 60° of the thermometer, which is full as satisfactory, for all practical purposes, as if it had been done in vacuo, which I conceive to be nearly impossible; and having determined the weight of any given bulk of water, compared with this common measure; I believe it now only remains, to ascertain the proportion of this common measure and weight, to the commonly received measures and weights of this kingdom. The difference of the length of the 2 pendulums, from Mr. Whitehurst's observations, appearing to be 59.89358 inches, on Mr. Troughton's scale; and a cubic inch of distilled water, in a known state of the atmosphere, having been found to weigh 252.587 Troy grains, according to the weights of the same artist, it remains only to determine the proportions of these weights and measures, to those that have been usually, or

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* The weight of a cubic inch of common or rain water has been reckoned about 253 grains, sometimes = 253.33 grains, at others 253.18. But authors do not seem to have agreed in what they meant by common water, rain water, pump water, spring water, and distilled water; for occasionally they are all confounded, and made to pass for each other; and sufficient notice seems not to have been taken of the temperature to which these weights were assigned.—Orig.

† But, as will appear hereafter, these weights are too light, when compared with the standard in the House of Commons, by about 1 in 1523.92; the correction therefore, for this difference, would be = 0.165 grain, to be deducted from.. 252.587 grains.
— .165

And the weight of a cubic inch of distilled water, in grains of the parliamentary standard, will be.. } = 252.422

may be fitly considered as the standards of this kingdom; and herein a small discrepancy between themselves, in these authoritative standards, will have no influence on the general conclusion I propose to draw; which is, not so much to say what has been the standard of Great Britain, as what it shall be henceforward, and may be immutably so; and which shall differ but a very small quantity, and that an assignable one, from those that have been in use for 2 or 300 years past. By these means, no inconvenience would be produced from change of terms, or subdivisions of parts, or from sensible deviation from ancient practice: all that will be done, will be to render that certain and permanent, which has hitherto been fluctuating, or liable to fluctuation. To give effect and energy to these suggestions, is the province of another power.

The chief standards of longitudinal measure, are those preserved in the Exchequer; in the House of Commons; at the R. S.; and in the Tower. The first alone indeed bear legal authority, and have been in use for more than 200 years; the last is considered as a copy of them, and is not used for sizing generally. The other two are of modern date; and though they do not carry with them at present any statuteable authority, yet from the high reputation and acknowledged care of the artists who made them, the celebrated Mr. George Graham, and Mr. John Bird, they are doubtless entitled to very great respect; and are probably derived from a mean result of the comparisons of the old and discordant ones in the Exchequer. I shall begin with that of Mr. Graham, which contains also the length of the Tower standard laid down on it; will proceed then to Mr. Bird's, and finally conclude with those at the Exchequer.

May 5, 1797, I went to the apartments of the R. S., at Somerset-House, and made the following observations on Mr. Graham's* brass standard yard, made in 1742. This scale is about 42 inches long, and half an inch wide, containing 3 parallel lines engraven on it, on the exterior and ulterior of which are 3 divisions, expressing feet; with the letter E at the last division, and, by a memorandum, preserved with it in the archives of the Society, is said to signify English measure, as taken from the standard in the Tower of London. That with the letter F denoting the length of the half of the French toise; put on here, by the authority and under the inspection of the Royal Academy of Sciences, then subsisting at Paris, to whom it was sent in 1742, for the purpose of comparing the French and English measures. The middle line, marked Exch. of the three above-mentioned, denotes, as is supposed, the standard yard from the Exchequer.

This bar had been previously laid together with my scale divided by Mr. Troughton, for 24 hours, to acquire the same temperature; they were also of the same metal, and, by placing it under my microscopes, adjusted to the interval between 10 and 46 inches, I found the interval on the Tower standard exceed mine, by

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* This rod was not made by Mr. Graham, but, at his instance, procured by him from Mr. Jonathan Sisson, a celebrated artist of that time. See Phil. Trans., vol. 12.—Orig.

$$\left.\begin{array}{r} -0.00127 \\ .00135 \\ .00128 \\ \hline \text{Mean} = .00130 \end{array}\right\} = \text{the total length therefore } 36.00130 \text{ inches, the thermometer at } 60°.8.$$

The interval on the line marked Exch. was shorter than mine by

$$\left.\begin{array}{r} -.0066 \\ .0066 \\ .0068 \\ \hline .0067 \end{array}\right\} = \text{the total length} = 35.9933 \text{ inches, the therm. at } 60°.6.$$

And the Paris half-toise, which had been supposed by the Academy to be = 38.355 English inches, was found, compared with mine, to be

$$= \left.\begin{array}{r} 38.3561 \\ .3563 \\ .3559 \end{array}\right\} \text{Mean} = 38.3561 \text{ Inches.*}$$

$$\left.\begin{array}{lr} \text{The 1st of the preceding observations giving} \ldots & 36.0013 \\ \text{The 2d} \ldots & 35.9933 \\ \text{The mean length of Mr. Graham's standard becomes} \ldots & 35.9973 \end{array}\right\} \text{Inches.}$$

From the information in the report of a committee of the House of Commons, in 1758, I learnt that Mr. Bird's parliamentary standard had been in the custody of some of its officers, but of whom nobody knew: however, under the authority of the Speaker, who was so good as to furnish me with a room in his house, to make the comparisons in, I at last discovered this valuable original in the very safe keeping of Arthur Benson, Esq., Clerk of the Journals and Papers, and which I

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* Dr. Maskelyne says, this standard yard of Mr. Graham's was $\frac{1}{1000}$ inch longer on 3 feet than Mr. Bird's divided scale, which he generally made use of in all his operations of dividing; and, from one made conformably to this of Mr. Bird's, Mr. Troughton divided my scale of 60 inches. This remark seems to agree with my 1st and 3d comparison, but not with the intermediate one. See Phil. Trans. for 1768, p. 324.

As I am now on the subject of foreign measure, it may not be quite out of place to say a word on the length of the ancient Roman foot, which I am enabled to do with some precision. Some years ago, when I was in Italy, I had several opportunities of ascertaining the length of this measure, by actual examination of the Roman foot rules, of which I have met with 9, viz. 2 in the Capitol at Rome; 1 in the Vatican; 5 in the Museum at Portici, near Naples; and lastly, one in the British Museum, sent from Naples by Sir William Hamilton. They were all of brass, except one half foot of ivory, with a joint in the middle, resembling our common box or ivory rules: and, by reference to my journal kept at that time, I find the mean result from all the 9 rules, viz. by taking both the whole and the parts of each, (for they were divided into 12 inches, and also into 16ths, or digits), gave, for the length of the old Roman foot, in English inches, correspondent to Mr. Bird's measure, = 11.6063.

In confirmation also of this conclusion, and agreeably to the idea of Mons. De La Condamine, in the "Journal of his Tour to Italy," I took the dimensions of several ancient buidings, viz. the interior diameter of the temple of Vesta; the width of the arch of Severus; the door of the Pantheon; and the width of the base of the quadrilateral pyramid of Cestius, which, it is curious to observe, I found exactly 100 old Roman feet, and 125 feet high. This I do not remember to have seen noticed by any former traveller.

The mean result of these experiments gave me............ 11.617 English inches.
Ditto, as before, from the rules........................ 11.606 ditto.
The mean of the 2 modes of determination is............ 11.612 ditto.

I may add, that in the Capitol is a stone, of no very ancient date however, let into the wall, on which is engraven the length of several measures, from which I took the following: The ancient Roman foot, = 11.635 English inches. The modern Roman palm, = 8.82 inches. The ancient Greek foot, = 12.09 inches.—Orig.

believe had never seen the light for 35 years before. It is a brass rod or bar, about 39 inches long, and 1 inch square, inclosed in a mahogany frame, inscribed “ Standard  1758;” at each extremity of it is a gold pin, of about $\frac{1}{10}$ inch in diameter, with a central point, and these points are distant = 36 inches. It bears however no divisions; but there was found with it, in another box, a scale divided into 36 inches, with brass cocks at the extremities, for the purpose of sizing or gaging other scales or rules by. Besides these, I found another standard, in size, and in all respects, similar to the last, inscribed 1760, having been made for another committee, that sat in that year; this also was accompanied with a similar divided scale of 36 inches. These bars being too thick to be conveniently placed under the microscopes of my instrument, the interval of 36 standard inches was laid down on my scale with a beam-compass, 2 fine points made, and, compared with Troughton's divisions, was = 36.00023 inches; the thermometer being at 64°. I then examined the other standard, marked “ Standard, 1760,” and found it to agree exactly with that of 1758; at least it did not differ from it more than .0002 inch.

I was now to examine the old standards kept in the Exchequer: these, Mr. Charles Ellis, Deputy Chamberlain of the Tally Court at the receipt of the Exchequer, was so good as to supply me with; viz. the standard yard of the 30th of Eliz. 1588, and also the standard ell of the same date. These are what have been constantly used, and are indeed the only ones now in use, for sizing measures of length.* They are made of brass, about 0.6 inch square, and are very rudely divided indeed, into halves, quarters, 8ths, and 16ths; the lines being 2 or 3 hundredths of an inch broad, and not all of them drawn square, or at right angles to the sides of the bar, so that no accuracy could possibly be expected from such measures. However, the middle point of these transverse lines, between the sides of the bar, was taken as the intended original division; and these divisions, such as they were, were transferred, by a dividing knife, to the reverse side of my brass scale made by Mr. Troughton, the thermometer being at 63°; and, at my leisure afterwards, I found as follows. The ends of these venerable standards having been bruised a little, or rounded, in the course of so many years' usage, I conceived a tangent to be drawn to the most prominent part, which was about the centre or axis of the bar, and this point being referred to Troughton's scale, between 6 and 42 inches, the entire yard of 1588, measuring from one extremity to the other, was found to be shorter than this, by — .007 inch: but these comparisons will be better exhibited in a table, as follows.

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* There was also a standard yard of Henry VII., but of very rude workmanship indeed; now quite laid by, and at what time last used, no information remains; but of this more hereafter.—Orig.

Exchequer standard of 1588.	Differ. from Troughton.	Length in Inches.	Differ. on 36 inches.	Mean difference on 36 inches.
	Inch.			
Entire yard................	— .007	35.993	— .007	Inch. = + 0.015
$\frac{1}{2}$ yard, from 24 to 42 inch..	+ .063	18.063	+ .126	
$\frac{3}{4}$ yard, from 15 to 42 inch..	— .008	26.992	— .011	
$\frac{7}{8}$ yard, from $10\frac{1}{2}$ to 42 inch.	+ .022	31.522	+ .025	
$\frac{15}{16}$ yard, from $8\frac{1}{4}$ to 42 inch.	— .055	33.695	— .059	
Entire ell, from 2 to 47 inch.	— .036	44.964	— .029	= + 0.016 Viz. the Exchequer measure is by so much the longer, or about 1 in 2322.
$\frac{1}{2}$ ell, from 2 to $24\frac{1}{2}$ inch. ..	+ .032	22.532	+ .052	
$\frac{3}{4}$, from 2 to $35\frac{3}{4}$ inch......	+ .017	33.767	+ .018	
$\frac{7}{8}$, from 2 to 41.375 inch....	— .001	39.374	— .001	
$\frac{15}{16}$, from 2 to 44.1875 inch..	+ .051	42.239	+ .043	

It appears then, from this table, that the ancient standards of the realm differ very little from those that have been made by Mr. Bird, or Mr. Troughton, and consequently, even in a finance view, nothing need be apprehended, of loss in the customs, or excise duties, by the adoption of the latter.

I shall now endeavour to show the proportion of the weights that I have used, compared with the standards that were made by Mr. Harris, Assay Master of the Mint, under the orders of the House of Commons, in the year 1758. They are kept in the same custody with Mr. Bird's scales of length, and appear to have been made with great care, as a mean result from a great number of comparisons of the old weights in the Exchequer, which have been detailed at length in that report. Mr. Harris having been of opinion that the Troy pound was the best integer to adopt, as the standard of weight, I venture to conclude that this was the most accurate, and most to be depended on, of all the various weights and duplicates that he made for the use of this committee; for he made them of 1, 2, 4, 8, 16 lb. and of $\frac{1}{2}$, 1, 2, 3, 6 oz. It will therefore be sufficient for my purpose, to compare the 1 and 2 pounds Troy, and their duplicates, with the weights of Mr. Troughton. I did this, June 2d, 1797; the barometer being at 29.72 inches, and thermometer 67°. The result of which was, that

The mean weight of 2 lb. Troy, is......................	11527.70 grs.
And consequently 1 lb. becomes......................	5763.85
But, from the exam. of the 2 single pound weights, 1lb. is	5763.71
Therefore the mean of all is......................... =	5763.78
That is, Mr. Troughton's weights are too light by $\frac{3.78}{5760.00}$ = grain on 1000 grs., or 1 in 1523.92 grs.	0.6562

In conclusion, it appears then, that the difference of the length of two pendulums, such as Mr. Whitehurst used, vibrating 42 and 84 times in a minute of mean time, in the latitude of London, at 113 feet above the level of the sea, in the temperature of 60°, and the barometer at 30 inches, is = 59.89358 inches of the parliamentary standard; whence all the measures of superficies and capacity are deducible. That, agreeably to the same scale of inches, a cubic inch of pure distilled water, when the barometer is 29.74 inches, and thermometer at 66°, weighs

252.422 parliamentary grains; from whence all the other weights may be derived.

As a summary of what has been done, I hope it may now be said, that we have attained these 3 objects: 1st. An invariable, and at all times communicable, measure of Mr. Bird's scale of length, now preserved in the House of Commons; which is the same, or agrees within an insensible quantity, with the ancient standards of the realm. 2dly. A standard weight of the same character, with reference to Mr. Harris's Troy pound. 3dly. Besides the quality of their being invariable, without detection, and at all times communicable, these standards will have the additional property of introducing the least possible deviation from ancient practice, or inconvenience in modern use.

Before closing this paper, after having said so much on the subject of weights and measures, it may not be improper to add a few words on a topic which, though not immediately connected, has some affinity to it; I mean the subject of the prices of provisions, and of the necessaries of life, &c. at different periods of our history, and in consequence the depreciation* of money. Several authors have touched incidentally on this question, and some few have written professedly on it; but they do not appear to have drawn a distinct conclusion from their own documents. It would carry me infinitely too wide, to give a detail of all the facts I have collected: I shall therefore content myself with a general table of their results, deduced from taking a mean rate of the price of each article, at the particular periods, and afterwards combining these means, to obtain a general medium for the depreciation at that period; and lastly, by interpolation, reducing the whole into more regular periods, from the Conquest to the present time: and, however I may appear to descend below the dignity of philosophy, in such economical researches, I trust I shall find favour with the historian, at least, and the antiquary.

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* The various changes that have taken place, by authority, in different reigns, either in the weight or alloy of our coins, are allowed for in the subsequent table.—Orig.

A TABLE exhibiting the Prices of various Necessaries of Life, together with that of Day Labour, in sterling Money, and also in Decimals, at different Periods, from the Conquest to the present Time, derived from respectable Authorities; with the Depreciation of the Value of Money inferred from them. To which is added, the Mean Appreciation of Money, according to a Series of Intervals of 50 Years, for the first 600 Years; and, during the present Century, at shorter Periods, deduced by Interpolation.

Year of our Lord.	Wheat per Bushel.	THE PRICES OF VARIOUS ARTICLES AT DIFFERENT TIMES.																			
		Miscellaneous Articles.												Mean Depreciation from these 12 Articles.	Beef & Mutton, per lb.	Labour in Husbandry per day.	Depreciation of Money according to the Price of				
		Cattle in Husbandry.					Poultry.			Butter, per lb.	Cheese, per lb.	Ale, per gallon.	Small Beer, pr.gal.				Wheat	12 Miscellaneous Articles.	Meat.	Day Labour.	Mean of all.
		Horse.	Ox.	Cow.	Sheep.	Hog.	Goose.	Hen.	Cock.												
	s. d.	£ s. d.	£ s. d.	£ s. d.	£ s. d.	£ s. d.	s. d.	s. d.	s. d.	d.	d.	s. d.	d.		d. qr.	s. d.					
1050	0 2¼	1 17 6	0 7 6	0 6 0	0 1 3	0 2 0															
		89	20	37	29	36								42			10	42			26
1150	0 4½	0 12 5	0 4 8¼		0 1 8	0 3 0		0 3								0 2					
1250	1 7¾	1 11 0	1 0 7	0 17 0	0 1 7		1 0	0 3	0 4½												
1350	1 10½	0 18 4	1 4 6	0 17 2	0 2 7	0 2 6	0 9	0 2	0 3¾							0 3					
		43	66	106	61	45	75	24	31					56			100	56		75	77
1450	1 5		1 15 8	0 15 6	0 4 11½	0 5 1	0 6¼									0 3¾					
1550	1 10½	2 2 0	1 16 7	0 16 0	0 4 3¾	0 5 6	1 0	0 8¼	1 0	5	2	0 1½	1		1 0½	0 4					
		100	100	100	100	100	100	100	100	100	100	100	100	100			100	100	100	100	100
1600	4 0½											0 4	2		1 2	0 6					
1625	4 11						2 0		1 6							0 6½					
1650	5 6											0 4	2								
1675	4 6	5 10 0	3 6 0	2 17 0	0 11 0	0 14 0	3 0	1 3	1 3	4½	2	0 8	2½		1 3½	0 7½					
		250	184	345	256	254	300	182	125	90	100	530	250	239			246	239	166	188	210
1700	4 9½											0 10	3								
1720	4 4½											1 0	3		2 2	0 8					
1740	3 8	10 0 0	8 0 0	7 7 0	1 6 0	1 15 0	3 6	1 6	1 6	9	3½	1 0	3		3 0	0 10					
		476	437	884	602	634	350	218	150	180	175	800	300	434			197	434	266	250	287
1760	3 9¾	14 0 0	8 10 0	7 0 0	1 7 0	1 15 0	5 0	1 10	1 10	10	5½	1 2	3		4 2	0 11					
		667	465	874	626	634	500	266	183	200	262	930	300	492			203	492	400	275	342
1780	4 5½															1 2					
1795	7 10	19 0 0	16 8 0	16 8 0	1 18 0	5 8 0	3 0	1 6	1 6	11½	5	1 2½	2¾		5 3	1 5¼					
		904	890	2000	882	1960	300	218	150	230	250	969	275	752			426	752	511	436	531

Mean Appreciation by Interpolation.	
A. D.	
1050	26
1100	34
1150	43
1200	51
1250	60
1300	68
1350	77
1400	83
1450	88
1500	94
1550	100
1600	144
1650	188
1675	210
1700	238
1720	257
1740	287
1750	314
1760	342
1770	384
1780	427
1790	496
1795	531
1800 nearly.	562

* The small figures denote the price in decimals, whereof those for the year 1550 may be taken for the integer, viz. 100.

APPENDIX.—Since writing the preceding Memoir, I have had an opportunity of examining 3 other scales, divided into inches, or equal parts, of considerable authority in this country, having been executed by the late Mr. J. Bird. I have also compared the old standard in the Exchequer, of the time of Hen. 7th, and which is considered to be the most ancient authority of this sort now subsisting. The first of those scales belonged to the late General Roy, and was purchased by him at the sale of Mr. Short's, the celebrated optician; it was used by him in his operations of measuring a base line on Hounslow Heath. It was originally the property of Mr. G. Graham, has the name of Jon. Sisson engraven on it, but is known to have been divided by Mr. Bird, who then worked with old Mr. Sisson. It is 42 inches long, divided into 10ths, with a vernier of 100 at one end, and of 50 at the other, giving the subdivisions of 500ths and 1000ths, of an inch.

The 2d is in the possession of Alex. Aubert, Esq. and formerly belonged to Mr. Harris, of the Tower; contains 60 inches, divided into 10ths, with a vernier, like that of the preceding. It is 1 inch broad, and 0.2 thick. The 3d was presented by Alex. Aubert, Esq. and the late Adm. Campbell, Mr. Bird's executors, to the R. S., in whose custody it now remains. It consists of a brass rod, 92.4 inches long, 0.57 inch broad, and 0.3 inch thick; bearing a scale of 90 inches, or equal parts, each subdivided into 10, with a vernier at the commencement, being a scale of 100 divisions to 101 tenths. This has been called Mr. Bird's own scale, viz. made for his own use; and was the instrument with which it is said he laid off the divisions of his 8-feet mural quadrants. It is probable that Mr. Bird made many more of these scales, now in the hands of private persons.

In comparing General Roy's (Bird's) scale with Mr. Troughton's, I found 42 inches of the former were = 42.00010 inches on Troughton's; the thermometer 51°.7; 36 inches were consequently = 36.00008.

		Inches.
And 12 inches on the 1st foot were equal to the 12 inches from 12 to 24 on Troughton's scale	− .0003	= 11.9997
The 2d foot	+ .0006	12.0006
The 3d foot	− .0004	11.9996
The last foot	+ .0006	12.0006
The mean foot, therefore, in General Roy's scale, taken from 4 different feet, compared with Troughton's, between the 12th and 24th inch, is as 12 to		12.00012
That is, General Roy's scale is longest on 1 foot by so much, and longer on 3 foot by		.00036
And the greatest probable error from the inequality in the divisions is about		.0005
And the mean probable error about		.0003

Mr. Aubert's scale, compared with Mr. Troughton's, was as follows: 58 inches were equal to 57.9982 inches on Troughton's; thermometer at 51°.0; viz. Mr. Bird's measure was shortest .0018; or, shortest on 36 inches = .0012,

	Inches.	
And 12 inches, or 1st foot, on Mr. Aubert's	= 11.9999	on Mr. Troughton's scale, from 6 inches to 18 inches; the thermometer being at 50°.0.
2d foot,	= 12.0005	
3d foot,	11.9996	
4th foot,	12.0019	
5th foot,	12.0006	
Therefore the mean foot is	12.0005	

The greatest error in this scale appears to be about = .0012
And the mean probable error .... = .0006.

The Royal Society's scale, compared, was as follows: 58 inches on Mr. Bird's were equal to 57,99912 inches on Mr. Troughton's, thermometer 50°.5;

viz. Mr. Bird's measure was shortest .... .00088
Or shorter on 36 inches .... .00054
32 inches on the same were equal to 31.99967
viz. Mr. Bird's was shortest by .... .00033
Or, on 36 inches, by .... .00037
The mean of these two comparisons is ...... .00045

And, by so much, is Mr. Bird's scale shorter, in 3 feet, than Troughton's.

	Inches.	
And 12 inches, or 1st foot, of the Royal Society's scale, is ..	= 12.00013	on Troughton's scale; the thermometer at 51°.
2d foot of ditto	= 11.99957	
3d foot of ditto	= 12.00027	
4th foot of ditto	= 11.99990	
5th foot of ditto	= 12.00063	
6th foot of ditto	= 11.99823	
7th foot of ditto	= 12.00000	

The mean of these 7 feet is ...... = 11.99982
And the greatest error in these divisions = .0008
And the mean probable error .... = .0004.

Lest however it should be suspected that Mr. Troughton's scale, with which I have made these comparisons, is not sufficiently correct for this apparent preference, I will now give the result of my examination of that scale, from one end to the other. I set the microscopes to an interval of nearly 6 inches, correctly speaking, it was 6.00013 inches, taken from a mean of the whole scale; and, comparing this interval successively, I found as annexed.

	Inches.	Inches.	Error, or Difference from the mean.
viz. from	0 to 6	= 6.00025	+ .00012
	6 to 12	= 6.00013	.00000
	12 to 18	= 6.00020	+ .00007
	18 to 24	= 6.00000	− .00013
	24 to 30	= 6.00007	− .00006
	30 to 36	= 6.00033	+ .00020
	36 to 42	= 5.99980	− .00033
	42 to 48	= 6.00020	+ .00007
	48 to 54	= 6.00010	− .00003
	54 to 60	= 6.00023	+ .00010

Mean of all = 6.00013.

Whence it appears, that the greatest probable error, without a palpable mistake, in Mr. Troughton's divisions, is = .00033 inch; against which, the chance is 9 to 1; and the mean probable error = .00016; and that it is 4 to 1 the error doth not exceed $\frac{2}{10000}$ inch. This accuracy is about 3 times as great as that of Mr. Bird's scales, and about equal to that of the divisions of my equatorial instrument, made by Mr. Ramsden, in 1791.

I now proceed to the examination of the standard rod of Henry 7th, which is an octangular brass bar, of about ½ an inch in diameter, with one of the sides rudely divided, into halves, thirds, quarters, 8ths, and 16ths; and the first foot into inches. Each end is sealed with a crowned old English H, and hence is concluded to be of the time of King Henry 7th, viz. about 1490, but is now become wholly obsolete, since the introduction of the standard of Queen

Elizabeth; but such as it is, I have thought proper to examine it, and find as follows:

	Inches.	Difference.	Error, or difference on 3 feet.
On this rod, $\frac{1}{3}$, or the 1st foot, is equal to	11.973 on Troughton's.		
the 2d foot is	11.948		
the 3d foot is	12.047		
The mean foot is	11.989	— .011	— .033
$\frac{1}{2}$ yard, or 18 inches..................	= 17.946	— .054	— .108
$\frac{2}{3}$ yard, or 24 inches..................	= 23.921	— .079	— .118
$\frac{3}{4}$ yard, or 27 inches	= 26.937	— .063	— .084
$\frac{7}{8}$ yard, or 31$\frac{1}{2}$ inches	= 31.443	— .057	— .065
$\frac{15}{16}$ yard, or 33$\frac{3}{4}$ inches..............	= 33.665	— .085	— .091
Entire yard, or 36 inches............	= 35.966	— .034	— .034
And the mean yard	= 35.924		
		Mean	— .076

And by so much Mr. Troughton's measure is the longer.

And the probable error, in the divisions of this old standard, is about $\frac{3}{100}$ inch.

It may now be desirable to see the comparative lengths of these various standards and scales, reduced to one and the same measure, viz. Mr. Troughton's.

		Inches on Troughton's.	Difference.	Probable error in divisions.
36 inches, on a mean, of Hen. 7th's standard of 1490, are equal to ...		35.924	— .076	.03
——— of standard yard of Eliz. of 1588		36.015	+ .015	.04
——— of standard ell of ditto, of 1588		36.016	+ .016	.04
———*of yard-bed of Guildhall, about 1660		36.032	+ .032	
———*of ell-bed of ditto, about 1660		36.014	+ .014	
———*of standard of clock-makers' company, 1671		35.972	— .028	
———*of the Tower standard, by Mr. Rowley, about 1720		36.004	+ .004	
——— of Graham's standard, by Sisson, of 1742, viz. line E......		= 36.0013	+ .0013	
——— of ditto, ditto, viz. line EXCH.		= 35.9933	— .0067	
——— of Gen. Roy's (Bird's) scale	all made, probably, between the years 1745 and 1760.	= 36.00036	+ .00036	.0003
——— of Mr. Aubert's ditto, ditto		= 35.99880	— .00120	.0006
——— of Royal Society's ditto, ditto		= 35.99955	— .00045	.0004
——— of Mr. Bird's parliamentary standard, of 1758		= 36.00023	+ .00023	
——— of Mr. Troughton's scale, in 1796		= 36.00000	.00000	.0001

Hence it appears, that the mean length of the standard yard, taken from the first 7 instances in this table, agrees with the quantity assumed by Mr. Bird, or Mr. Troughton, to within $\frac{3}{1000}$ inch, but that the latter is the longest.

IX. A new Method of computing the Value of a Slowly Converging Series, of which all the Terms are Affirmative. By the Rev. John Hellins, F. R. S. p. 183.

1. The computing of the value of the series $ax + bx^2 + cx^3 + dx^4 +$ &c. ad infinitum, in which all the terms are affirmative, and the differences of the coefficients a, b, c, &c. are but small, though decreasing, quantities, and x is but little less than 1, is a laborious operation, and has engaged the attention of some eminent mathematicians, both at home and abroad, whose ingenious devices on the occasion entitle them to esteem. Of the several methods of obtaining the value of this series, which have occurred to me, the easiest is that which I am now to describe, by which the business is reduced to the summation of 2, or 3, or more

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* These four quantities are taken from Mr. Graham's account, in the Phil. Trans. vol. 42.—Orig.

series of this form, viz. $ax - bx^2 + cx^3 - dx^4$, &c. and one series of this form; viz. $px^n + qx^{2n} + rx^{3n} +$ &c. where n is $= 4, 8, 16, 32$, or some higher power of 2. The investigation of this method is as follows.

2. The series $ax + bx^2 + cx^3 + dx^4 + ex^5 + fx^6 +$ &c. is evidently equal to the sum of these two series, viz.

$$ax - bx^2 + cx^3 - dx^4 + ex^5 - fx^6, \text{ \&c.}$$
$$* + 2bx^2 * + 2dx^4 * + 2fx^6, \text{ \&c.}$$

of which, the value of the former is easily attainable, by the method so clearly explained, and fully illustrated, by Mr. Baron Maseres, in the Philos. Trans. for the year 1777; and the latter, though it be of the same form with the series first proposed, yet has a great advantage over it, since it converges twice as fast. On this principle then we may proceed to resolve the series $2bx^2 + 2dx^4 + 2fx^6 + 2hx^8 + 2kx^{10} + 2mx^{12} +$ &c. into the two following:

$$2bx^2 - 2dx^4 + 2fx^6 - 2hx^8 + 2kx^{10} - 2mx^{12}, \text{ \&c.}$$
$$* + 4dx^4 \quad * + 4hx^8 \quad * + 4mx^{12}, \text{ \&c.}$$

where again the value of the one may easily be computed; and the other, though it be of the same form with the series at first proposed, yet converges 4 times as fast. And in this manner we may go on, till we obtain a series of the same form with the series at first proposed, which shall converge 8, 16, 32, 64, &c. times as fast, and consequently a few terms of it will be all that are requisite.

An example, to illustrate this method, is here subjoined.

3. Let it be proposed to find the value of the series $x + \frac{x^2}{2} + \frac{x^3}{3} + \frac{x^4}{4} + \frac{x^5}{5} + \frac{x^6}{6} +$ &c. ad infinitum, when $x = \frac{9}{10}$.

4. In order to obtain the sum of this series, with the less work, it will be requisite to compute a few of the initial terms, as they stand. For, if we begin the operation with computing the value of $x - \frac{x^2}{2} + \frac{x^3}{3} - \frac{x^4}{4}$, &c. by the differential series before mentioned,* the values of D$'$, D$''$, D$'''$, &c. will be $\frac{1}{2}, \frac{1.2}{2.3}, \frac{1.2.3}{2.3.4}$, &c. respectively, i. e. $\frac{1}{2}, \frac{1}{3}, \frac{1}{4}$, &c. which is a series decreasing so very slowly, that the only advantage obtained by this transformation of the series is in the convergency of the powers of $\frac{x}{1+x}$ instead of the powers of x, which indeed is very great; for, x being $= \frac{9}{10}$, $\frac{x}{1+x}$ is $= \frac{9}{19}$; so that the new series $\frac{9}{19} + \frac{1}{2} \cdot (\frac{9}{19})^2 + \frac{1}{3} \cdot (\frac{9}{19})^3 + \frac{1}{4} \cdot (\frac{9}{19})^4 +$ &c. though $= \frac{9}{10} - \frac{1}{2} \cdot (\frac{9}{10})^2 + \frac{1}{3} \cdot (\frac{9}{10})^3 - \frac{1}{4} \cdot (\frac{9}{10})^4$, &c. yet converges more than 7 times as swiftly. But, if we begin the work by computing the

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* The theorem best adapted to this business is the following; viz. $ax - bx^2 + cx^3 - dx^4$, &c. $= \frac{ax}{1+x} + \frac{\text{D}'x^2}{(1+x)^2} + \frac{\text{D}''x^3}{(1+x)^3} + \frac{\text{D}'''x^4}{(1+x)^4} +$ &c. D$'$ being $= a - b$, D$'' = a - 2b + c$, D$''' = a - 3b + 3c - d$, &c. See Scriptores Logarithmici, vol. 3, p. 290, where b, c, d, &c. denote the same quantities that a, b, c, &c. do here.

first 8 terms of the series, as they stand, and then compute the value of $\frac{x}{9} - \frac{x^2}{10} + \frac{x^3}{11} - \frac{x^4}{12}$, &c. ad infinitum, by the same theorem, the values of D', D'', D''', &c. will be $\frac{1}{9.10}$, $\frac{2}{9.10.11}$, $\frac{2.3}{9.10.11.12}$, &c. which is a series decreasing, for a great number of its terms, much more swiftly than the powers of $\frac{9}{10}$, and, in the first 7 terms, much more swiftly than the powers of $\frac{9}{19}$. The value of the series $\frac{1}{9} \cdot \frac{9}{10} - \frac{1}{10} \cdot (\frac{9}{10})^2 + \frac{1}{11} \cdot (\frac{9}{10})^3 - \frac{1}{12} \cdot (\frac{9}{10})^4$, &c. is therefore = the series $\frac{1}{9} \cdot \frac{9}{19} + \frac{1}{9.10} \cdot (\frac{9}{19})^2 + \frac{2}{9.10.11} \cdot (\frac{9}{19})^2 + \frac{2.3}{9.10.11.12} \cdot (\frac{9}{19})^4 +$ &c. the first 7 terms of which converge above 14 times as swiftly as the other; or, in other words, the first 7 terms of it will give a result much nearer the truth than 100 terms of the other. And if, instead of the first 8 terms of the proposed series, the first 24 terms were computed, as they stand, and then the value of the series $\frac{x}{25} - \frac{x^2}{26} + \frac{x^3}{27} - \frac{x^4}{28}$, &c. by its equivalent, $\frac{1}{25} \cdot \frac{x}{1+x} + \frac{1}{25.26} \cdot \frac{x^2}{(1+x)^2} + \frac{2}{25.26.27} \cdot \frac{x^3}{(1+x)^3} + \frac{2.3}{25.26.27.28} \cdot \frac{x^4}{(1+x)^4} +$ &c. the rapid decrease of the coefficients $\frac{1}{25}$, $\frac{1}{25.26}$, $\frac{2}{25.26.27}$, &c. compounded with the decrease of the powers of $\frac{x}{1+x}$, (in the present case = the powers of $\frac{9}{19}$,) produces such a very swiftly converging series, that 8 terms of it will give the result true to 11 places of decimals. On these principles Mr. H. proceeds to compute the terms of these series, and to collect them together; in doing which he employs some ingenious contrivances, to methodize and facilitate the operations. After which the ingenious author concludes the whole process as follows.

The values of the several parts, into which the proposed series has been resolved, being now so far obtained that we have only to multiply each by its proper factor, viz. the numerical value of x^8, x^{16}, x^{32}, &c. and add the products together, to get its sum; this therefore is now to be performed. And, in this part of the calculation, several multiplications may be saved, and no larger factor than x^8 be used, by attending to the method described by Sir Isaac Newton, in his Tract De Analysi per Æquationes infinitas; p. 10 of Mr. Jones's edition of Sir Isaac's Tracts; or p. 270, vol. 1, of Bishop Horsley's edition of all his works. The manner in which this is to be done will appear, by collecting the several parts from the preceding articles, and exhibiting them in one view, thus:

$A + Bx^8 + Cx^{16} + (\alpha + D) \times x^{24} + (\beta + E) \times x^{32} + \gamma x^{40} + (\delta + F) \times x^{48}$ = the sum of the proposed series. Now,

1st. Calling $\delta + F$, z', and multiplying by x^8, we have $z' x^8 = 0{\cdot}26584,70242 \times 0{\cdot}43046,721 = 0{\cdot}11443,84267,9$.—2dly. Putting $\gamma + z' x^8 = z''$, and multiplying by x^8, we get $z'' x^8 = 0{\cdot}15172,24360,1 \times 0{\cdot}43046,721 = 0{\cdot}06531,15337,2$.—3dly. Putting $\beta + E + z'' x^8 = z'''$, and multiplying by x^8, we get $z''' x^8 =$

0·20827,01655,4 × 0·43046,721 = 0·08965,34770,9.—4thly. Putting $\alpha + \text{D} + z''' x^8 = z^{\text{iv}}$, and multiplying by x^8, we get $z^{\text{iv}} x^8 =$ 0·28046,43895.9 × 0.43046,721 = 0·12073,07232.90.—5thly. Putting $\text{C} + z^{\text{iv}} x^8 = z^{\text{v}}$, and multiplying by x^8, we get $z^{\text{v}} x^8 =$ 0·38085,19524,75 × 0·43046,721 = 0·16394,42774,05. — 6thly. Putting $\text{B} + z^{\text{v}} x^8 = z^{\text{vi}}$, and multiplying by x^8, we get $z^{\text{vi}} x^8 =$ 0·60806,50444,24 × 0·43046,721 = 0·26175,20631,73.

Lastly, to this product add A = 2·04083,30298,21, and we have the value of the series proposed .... = 2·30258,50929,94, which is true to twelve places of figures.

It may not be improper to remark, that this degree of accuracy is much greater than is requisite, even in astronomy; for which, as well as for most other purposes, 6 or 7 places of figures are sufficient: and to that degree of exactness the value of the proposed series might have been obtained, by less than a 4th part of the labour that has been taken above. But it was the intention to show, that the value of a very slowly converging series, of which all the terms are affirmative, may, by the method now described, be computed to 10 or 12 places of figures, in the space of a few hours.

Meteorological Journal, kept at the Apartments of the R. S. By order of the President and Council. p. 201.

1797.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	°	°	°	°	°	°	°	°	°	Inc.	Inc.	Inc.	°	°	°	Inc.
Jan.	49	25	37.3	49	25	37.7	56	45	51.2	30.50	29.52	30.09	90	69	85.1	0.960
Feb.	50	24.5	37.5	50	25	37.9	57	50	53.3	30.62	29.37	30.31	88	67	81.1	0.219
Mar.	54	27.5	39.9	54	29	40.2	59	51	54.3	30.42	29.44	29.94	86	60	76.6	0.777
April	65	34	47.4	65	35	47.8	62	55	57.8	30.13	29.10	29.77	87	63	77.3	1.859
May	79	34	53.8	78	40	55.4	68	56	61.5	30.33	29.38	29.89	90	61	75.1	1.436
June	73	40	57.5	73	45	58.6	65	59	61.8	30.29	29.36	29.86	85	64	74.3	4.223
July	85	48	65.8	84	55	66.7	74	62	67.4	30.25	29.51	29.96	83	64	74.6	1.288
Aug.	76	48	61.9	76	52	62.6	69	63	66.1	30.18	29.48	29.87	88	66	76.4	2.789
Sept.	71	42	56.9	69	45	57.5	67	60	62.2	30.14	29.04	29.75	90	65	79.3	4.061
Oct.	63	35	49.0	62	35	49.6	62	53	57.5	30.31	29.05	29.83	90	67	81.3	2.001
Nov.	57	27	43.3	57	27	43.4	59	49	55.0	30.48	29.14	29.92	91	73	85.0	1.473
Dec.	56	29	42.7	56	30	43.0	60	49	54.9	30.46	29.07	29.80	91	70	84.5	1.611
Whole year.			49.4			50.0			58.6			29.92			79.2	22.697

X. On the Stability of Ships. By Geo. Atwood, Esq. F. R. S. p. 201.

The stability of vessels, by which they are enabled to carry a sufficient quantity

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* The quicksilver in the basin of the barometer is 81 feet above the level of low water spring tides at Somerset-house.

of sail, without danger or inconvenience, is reckoned among their most essential properties; though the wind may, in one sense be said to constitute the power by which ships are moved forward in the sea, yet, if it acts on a vessel deficient in stability, the effect will be to incline the ship from the upright, rather than to propel it forward: stability is therefore not less necessary than the impulses of the wind are, to the progressive motion of vessels. This power has also a considerable influence in regulating the alternate oscillations of a ship in rolling and pitching; which will be smooth and equable, or sudden and irregular, in a great measure, according as the stability is greater or less at the several angles of inclination from the upright. From constantly observing that the performance of vessels at sea depends materially on their stability, both navigators and naval architects must, at all times, be desirous of discovering in what particular circumstances of construction this property consists, and according to what laws the stability is affected by any varieties that may given to their forms, dimensions, and disposition of contents; which are determined partly according to the skill and judgment of the constructor, and partly by adjustments after the vessel has been set afloat.

When a ship, or other floating body, is deflected from its quiescent position, the force of the fluid's pressure operates to restore the floating body to the situation from which it has been inclined. This force is distinctly described, in a treatise written by the most celebrated geometrician of ancient times, who uses the following argument for demonstrating the position in which a parabolic conoid will float permanently in given circumstances. To show that this solid will float with the axis inclined to the fluid's surface at a certain stated angle, depending on the specific gravity and dimensions of the solid, he demonstrates *, that if the angle should be greater than that which he has assigned, the fluid's pressure will diminish it; and that, if the angle should be less, the fluid's pressure will operate to increase it, by causing the solid to revolve round an axis which is parallel to the horizon. It is an evident consequence, that the solid cannot float quiescent with the axis inclined to the fluid's surface, at any angle except that which is stated. The force which is shown in this proposition, to turn the solid, so as to alter the inclination of the axis to the horizon, is the same with the force of stability; the quantity or measure of which, Archimedes does not estimate; nor was it necessary to his purpose, since the alteration of inclination required to establish the quiescent position, may be produced either in a greater or less time, without affecting his argument. It does not appear, that this method of determining the floating positions of bodies was afterwards extended to infer similar conclusions in respect to solids of any other forms, nor to determine any thing concerning the inclination or equilibrium of ships at sea, which require the demonstration, not only that a force exists, in given circumstances, to turn the vessel round an axis, but also the magnitude or precise measure of that force. M. Bouguer, in his treatise intitled "Traitè du Navire †," has investigated a theorem for estimating the exact measure of the stability of

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* Archimedes de iis quæ in humido vehuntur.—Orig.

† Livr. 2, sect. 2, chap. 8.—Orig.

floating bodies. This theorem, in one sense, is general, not being confined to bodies of any particular form ; but, in respect to the angles of inclination, it is restrained to the condition that the inclinations from the upright shall be evanescent, or, in a practical sense, very small angles. In consequence of this restriction, the rule in question cannot be generally applied to ascertain the stability of ships at sea; because the angles to which they are inclined, both by rolling and pitching, being of considerable magnitude, the stability will depend, not only on the conditions which enter into M. Bouguer's solution, but also on the shape given to the sides of the vessel above and beneath the water-line or section, of which M. Bouguer's theorem takes no account. But it is certain that the quantity of sail a ship is enabled safely to carry, and the use of the guns in rough weather, depend in a material degree on the form of the sides above and beneath the water-line; this observation referring to that portion of the sides only which may be immersed under, or may emerge above, the water's surface, in consequence of the vessel's inclination; for, whatever portion of the sides is not included within these limits, will have no effect on the vessel's stability, the centres of gravity, volume of water displaced, and other elements not being altered. By the water-section is meant, the plane in which the water's surface intersects the vessel, when floating upright and quiescent; and the termination of this section in the sides of the vessel is termed the water-line. A general theorem for determining the floating positions of bodies is demonstrated in a former paper, inserted in the Phil. Trans. for the year 1796, and applied to bodies of various forms: the same theorem is there shown to be no less applicable to the stability of vessels, taking into account the shape of the sides, the inclination from the upright, as well as every other circumstance by which the stability can be influenced. To infer, from this theorem, the stability of vessels in particular cases, the form of the sides, and the angle of inclination from the perpendicular, must be given. These conditions admit of great variety, considering the shape of the sides, both above the water-line and beneath it; for we may first assume a case, which is one of the most simple and obvious; this is, when the sides of a vessel are parallel to the plane of the masts, both above and beneath the water-line; or, secondly, the sides may be parallel to the masts under the water-line, and project outward, or may be inclined inward, above the said line ; or they may be parallel to the masts above the water-line, and inclined either inward or outward beneath it; some of these cases, as well as those which follow, being not improper in the construction of particular species of vessels, and the others, though not suited to practice, will contribute to illustrate the general theory. The sides of a vessel may also coincide with the sides of a wedge, inclined to each other at a given angle; which angle, formed at an imaginary line, where the sides, if produced, would intersect each other, may be situated either under or above the water's surface. To these cases may be added, the circular form of the sides, and that of the parabola. The sides of vessels may also be assumed to coincide with curves of different species and dimensions, some of which approach to the forms adopted in the practice of naval architecture, particularly in the larger ships of burden. And lastly, the shape of the sides may be reducible to no regular geometrical law; in which case, the determination of the stability, in respect to a ship's rolling, requires the mensuration of the ordinates of the vertical sections which intersect the longer axis at right angles; similar mensurations are also required for determining the stability, in respect to the shorter axis, round which a vessel revolves in pitching. In order to describe distinctly these several cases, the variation of the sections, both in form and magnitude, from head to stern of the vessel, has not been considered; the sections being supposed equal and similar figures, such as they in reality are, near the greatest section of a ship, growing smaller, and altering their form, toward the head and stern. But, before this alteration can be taken into account, it is necessary first to ascertain the stability corresponding to a vessel or segment, in which the sections are equal and similar figures; from which determination, the stability is inferred which actually exists, when the form and magnitude of the sections alter continually, from one extremity of the vessel to the other.

Mr. A. here first, by analytical investigations, obtains a general expression, or theorem, for the measure of a vessel's stability, in terms of its dimensions, and the angle to which it is inclined from the upright position. This general theorem he applies to several particular cases. As, first, when the sides of a vessel are parallel to the plane of the masts, both above and beneath the water-line. 2dly, when the sides of a vessel project outward above the water-line, and are parallel to the masts under the water-line. Case the 3d, when the sides of a vessel are inclined inward above the water-line, and are parallel to the plane of the masts under the water-line. Case 4, when the sides of a vessel project outwards, and at equal inclinations to the plane of the masts, both above and beneath the water-line. Case 5, when the sides of a vessel are inclined inward, and at equal angles of inclination to the plane of the masts, both above and beneath the water-line. Case 6, when the sides of a vessel coincide with the sides of an isosceles wedge, meeting, if produced, in an angle which is beneath the water's surface. Case 7, when the sides of a vessel coincide with the sides of a wedge, meeting, if produced, at an angle which is above the water's surface. Case 8, when the sides of a vessel are parallel to the masts above the water-line, and project outward beneath it. Case 9, when the sides of a vessel are parallel to the masts above the water-line, and are inclined inward beneath it. Case 10, when the sides of a vessel coincide with the surface of a cylinder, the vertical sections being equal circles. Case 11, when the vertical sections of a vessel are terminated by the arcs of a conic parabola. Case 12, still supposing the vertical sections of a vessel to be equal and similar figures; the figure being either a curve of the higher dimensions, or a curve not formed according to any geometrical law, of which the lengths of the ordinates, and of any other lines given in position, are supposed to be measurable, and given in quantity: the angle at which the vessel is inclined from the upright, and the other necessary conditions being known, it is required to find, by geometrical construction, a line which shall approximate nearly to the measure of the vessel's stability. This is accordingly determined in 2 different ways.

Case 13. The longer axis of a vessel is supposed to be divided into a given number of equal parts, and vertical sections to pass through the several points of division, intersecting the axis at right angles: the form and magnitude of each particular section being given, with the common distance between them, the positions of the centres of gravity of the vessel, and of the volume displaced, and the distance of the water-section from the keel, being known, it is required to construct the measure of the vessel's stability, when it is inclined from the upright through a given angle. This is accordingly effected in several instances; and application is then made to some numeral calculations on cases of certain vessels, of known figures and dimensions. After which are given some practical observations as follow.

The object of the preceding propositions, and inferences founded on them, has been rather to establish general principles, which may be of use in forming plans of construction, than to investigate what modes of construction are the most advantageous; a discussion more extensive than would be consistent with the subject here proposed to be considered, which relates to the stability of vessels only. The practice of navigation requires the co-operation of many qualities in vessels, the laws and powers of which, considered as acting either separately or conjointly, it is the employment of theory to investigate. In respect to the construction of ships, it is obvious that no one of the component qualities can be regulated, without paying attention to all the others; because, by increasing or diminishing any of the powers of action, the others are commonly more or less influenced. It has been shown, by the propositions demonstrated in these pages, that there are many practical methods by which the stability of vessels, at any given angle from the upright, may be augmented; a circumstance which gives to the constructor great choice of means for regulating this power, according to the particular service for which the ship is designed; for it is not every mode that will be advantageous. The several varieties of form and adjustment by which stability is increased, may be so unskilfully combined, that, in consequence of the very means used to obtain that essential quality, either the ship shall not steer well, or shall drift too much to leeward, or shall be liable to sudden and irregular motions in rolling, by which the masts are endangered; or those angular oscillations of the ship shall be performed round an axis situated so much beneath the water's surface, that the motion of rolling shall be excessive and laborious. It is the proper use of theory, or right principle, whencesoever derived, so to adapt the means to the end proposed, that the required stability shall be imparted, without producing inconveniences of any kind, or such only as are unavoidable, and are the least prejudicial: the same observation applies to the other qualities of vessels. By duly combining the whole, ships are constructed so as to fulfil the purposes of navigation.

XI. Some Optical Remarks, chiefly relative to the Reflexibility of the Rays of Light. By Mr. P. Prevost, Profess. of Philos. at Geneva, &c. From the French. p. 311.

The word reflexibility, says Mr. P., is taken in 2 different senses. 1. Newton means by it, that property of a ray of homogeneal light, by which the ray is reflected if it fall in a certain angle of incidence, and transmitted if it fall in a less angle: or, more simply, a disposition to be reflected, and not transmitted, at the limit which separates 2 refracting mediums. (Opt. l. 1, pt. 1, dif. 3). That philosopher thinks, that in this sense the reflexibility of the rays is not the same. He establishes by experiments, which he deems conclusive, that the most refrangible rays are also the most reflexible. So that, if a white beam fall in a certain angle on the obstructing surface, the violet ray will be reflected, while the other 6 will be still transmitted and refracted. But by augmenting the angle of incidence, we shall obtain successively the reflexion of all the rays, from the violet, which is the most reflexible, to the red, which is the least.—Mr. Brougham however (Philos. Trans., for 1796, p. 272), does not think those experiments of Newton conclusive, and on another experiment he establishes the contrary prop. viz. that all the rays have the same disposition to be reflected, while the angle of incidence is the same.

Mr. Brougham means by reflexibility, a disposition to be reflected near the perpendicular to a certain degree; or a property of a homogeneal ray, by which its angle of reflexion is to the angle of incidence in a certain ratio, which is not that of equality, except in some cases which he indicates. According to him, this ratio varies for each homogeneous ray. The ratio of equality takes place for the rays at the confines of the blue and green: the ratio of inequality for the others; and the most refrangible are the least reflexible. So that, for the red ray the angle of reflexion is less than the angle of incidence, but for the violet greater. Whereas Newton affirms, on the contrary, that the angle of reflexion is always equal to the angle of incidence.—Let us examine these opposite sentiments.

Of the 2 experiments by which Newton establishes the inequality of the reflexibility of the rays, it will suffice to notice that which Mr. B. attacks directly. Newton, in exper. 9, makes a white beam fall perpendicularly on the anterior face of a prism: then, turning the prism on its axis, he observed the reflexion which took place at its posterior face. He saw the violet reflected first; then the other rays, in the order of their refrangibilities, till the red which was reflected last. Hence he concludes, that the violet is reflected at a smaller angle of incidence than the red. In attacking this conclusion, Mr. Brougham says, " That the demonstration involves a logical error, appears pretty evident: when the rays, by refraction through the base of the prism used in the experiment, are separated into its parts, these become divergent, the violet and red emerging at very different angles, and these were also incident on the base at different angles, from the refraction of the side at which they entered; when therefore the prism is moved round on its axis, as described in the proposition, the base is nearest the violet, from the position of the rays by refraction, and meets it first; so that the violet being reflected as soon as it meets the base, it is reflected before any of the other rays, not from a different disposition to be so, but merely from its different refrangibility." Thus Mr. B. thinks that the reflexion of the violet ray precedes the red one, only because the refraction at the anterior surface forces the violet ray to attain the posterior surface sooner than it causes the red.

But, says Mr. P., it seems that the effect is here in the inverse sense of the cause. It is impossible that the author could wish to say that the eye can seize the interval of time between the arrival of the violet and red ray at the posterior face of the prism. Now, of the 2 rays, that which describes the shortest route, falls nearest the perpendicular let fall from the point of departure: and hence alone we may conclude that it falls in a less angle of incidence. From the premises it follows, that it is the red ray that must be first reflected, and not the violet. Now, let us first consider the position of the prism at the first moment, and such as the figure given by Newton in his Optics. The white beam FM, fig. 3, pl. 5, is perpendicular on AC: in this case it is not refracted at its immergence, but pursues the right line FM. At this point Newton represents the violet ray only, MN, reflected, while all the others, as MH, MI, are transmitted and refracted: at least, the violet alone is reflected entire.

Yet it is certain that he has failed, for obtaining this phenomenon, to endeavour, by turning the prism, to give it the degree of inclination that can make the experiment succeed: and Mr. B. is right in observing, that from thence the perpendicularism of the ray on the anterior face, AC, has ceased; that in consequence it has suffered a refraction, and that the several homogeneal rays have not followed a rectilineal route, as FM, and have not met the posterior face BC in equal angles. Let now A′B′C′, fig. 4, be the new position of the prism, taken by the rotation on its axis: then the ray FP will fall obliquely on A′C′, at the point P; so that the perp. PO will be towards the side A′, or within the angle A′PF. This is what results, 1st, from the end proposed by Newton, viz. of augmenting the angle of incidence on the posterior face; which angle would be too little to produce reflexion. 2. From Newton's precise expressions, when he says that the prism ABC is turned on its axis according to the order of the letters, A, B, C. The ray FP then, in fig. 4, will be refracted towards the perpendicular OP: but the most refrangible ray, the violet, will approach it nearest; and the least refrangible (the red) will approach it the least; as in the two lines PV, PR: so that the violet ray makes with the posterior face B′C′ an angle, PVC′, greater than the angle PRC formed by the red: but the angles of incidence, at the points V, R, are the complements of the angles PVC′, PRC′, respectively. It is certain then, that in virtue of the refraction at the anterior face, the violet ray meets the posterior face in a less angle of incidence than the red; consequently the former is in more favourable circumstances for reflexion than the latter; and is even reflected while the latter is not. It is therefore right to conclude that it is naturally more reflexible in the Newtonian sense.

Thus then the consideration introduced by Mr. B. (and which is very just), causes us to conclude à fortiori in favour of the Newtonian assertion. We might say indeed, that not only at the same incidence, the violet ray is reflected while the red is not, but we might even say that the phenomenon takes place though the incidence of the violet be more unfavourable for reflexion than that of the red. Therefore, finally, the rays differ in reflexibility in Newton's sense; and the most refrangible is the most reflexible. Mr. P. quotes also several other authors, all confirming the same proposition; and then proceeds thus.

But Mr. B. supports the contrary opinion by an experiment which he thus announces: "I held a prism vertically, and let the spectrum of another prism be reflected by the base of the former, so that the rays had all the same angle of incidence; then, turning round the vertical prism on its axis, when one sort of rays was transmitted or reflected, all were transmitted or reflected."* The complete discussion of this experiment, says Mr. P., would require long details; but suffice it to observe, that the plane of the vertical face, on which the refraction acts, cannot be so adjusted as to produce a same angle of incidence with all the rays of a spectrum at once; and, even supposing the thing possible, and executed in an instant, the rotation of the prism had changed that disposition, in altering unequally that angle, for different rays. We may consequently conceive a multitude of different results; among others, we can conceive that the angles of incidence of divers rays may be such, that Mr. B.'s observation will accord with Newton's sentiment, on their unequal reflexibility. But as Mr. B. does not enter into this detail, and gives only one result, it is to be presumed that he did not repeat or vary the experiment. He appears even not to give much importance to it, by the rapid manner in which he announces it. I think then, that it cannot as yet invalidate Newton's conclusions; and that it is still right to assert, in his sense, that the most refrangible rays are also the most reflexible.

As to the 2d question, or whether homogeneous rays differ in reflexibility in Mr. B.'s sense? In other words, with a same angle of incidence, whether the red ray forms an angle of reflexion less, and the violet an angle of reflexion greater, than the angle of incidence?

The fundamental experiment whence Mr. B. deduces this unequal reflexibility, in the sense of his definition, is this. A smooth and brilliant cylinder, of a very small diameter, or a metallic thread, having its convexity presented to a white ray, reflected a coloured spectrum; and, every thing being measured or calculated properly, he perceived that it was only the rays in the confines between the blue and

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* The same experiment, tried by Newton, gave him exactly the contrary result. Lect. Opt. Opuscul. tom. 2, p. 220.—Orig.

green that were reflected in an angle equal to the angle of incidence: the red were reflected at a less angle, the violet at a greater. Now the question reduces to this, to know if that experiment be conclusive in favour of Mr. B.s statement.

To ascertain this, it is material to recollect a principle laid down by Newton, and admitted by Mr. B., that the force producing reflexion, acts perpendicularly to the reflecting surface. From this principle it follows, that the reflexion produced by a plane surface, ought to act by a law hitherto admitted by all opticians. And this is true, whatever be the intensity of the repulsive force, and also, whatever be the velocity and the inclination of the incident ray: provided the ray be really incident, and move not parallel to the repulsing surface. Thus, according to a principle which is not contested, it appears that the reflexion cannot decompose the white light, when this is wholly reflected by a plane surface. This is perfectly conformable to what Mr. B. observes, that by no means can we succeed to effect this decomposition, in using plane surfaces, nor curve surfaces with a ray extremely small, and as it were evanescent. We may in fact, conceive that an element of the curve surface of a large ray, is a real plane, for a particle of light. The author indeed explains this phenomenon in another manner; but the fact, independent of all explanation, is not less certain and acknowledged. After some further remarks, Mr. P. concludes, from all that has been said, that homogeneous rays are not unequally reflexible in Mr. B.'s sense; in other words, that the law of reflexion admitted by Newton, is the true law of nature.

It follows from the preceding discussion that the violet rays reflect soonest, and the red most forcibly. Even when these 2 effects may have taken place in like circumstances, they would not be perhaps incongruous. We might conceive that the sphere of activity extends a little farther for the violet than for the red, but that it acts on these with more intensity. But it is essential to remark that these 2 effects take place in very different, and even opposite circumstances: and this indicates an important exception to Newton's assertion, on the unequal reflexibility of divers homogeneal rays. In the experiments by which this philosopher establishes it (exper. 9 and 10), reflexion acts in the denser medium, and therefore, by attraction. On the contrary, in Mr. B.'s experiments reflexion acts in the rarer medium, that is by repulsion.

Thus, on the one part, we see that the most refrangible rays, or the most attracted in the act of transmission, are also the most attracted in the act of reflexion; and, on the other part, we see that the least refrangible rays, or the least attracted in transmission, are the most repulsed, or the least attracted, in the act of reflexion. This appears to make an exception to the Newtonian law of unequal reflexibility; since this unequal reflexibility is proved by Newton only for the case where the ray moves in the densest medium. I do not recollect that Newton, or any other optician, till Mr. B., has treated the other case. The experiments of this last philosopher appear to me to indicate, at least indirectly, the Newtonian unequal reflexibility, for the case omitted by Newton, viz. where the ray moves in the rarest medium: and hence there follows, I think some disposition to believe, that this reflexibility is in the inverse sense of the other: which it was besides natural enough to expect.

Another question may be: Do the principles which explain reflexion, explain flexion?* To answer this question, there are 2 indispensable preliminaries: to recount the principles laid down for explaining reflexion; and to show with precision the laws of flexion. The principles admitted above are, 1. The repulsive force acting according to a direction perpendicular to the reflecting surface. 2. The red ray more repulsive than the violet; or, in general, the least refrangible rays more forcibly repelled than those that are more refrangible.

The laws of flexion, very well determined by Mr. B., may be thus stated: 1. The most inflexible ray is also the most deflexible. 2. The most refrangible ray is the least flexible. Thus, the red ray is at once more inflected, and more deflected, than the violet, in the same circumstances. The law of deflexion follows well from principles in what concerns this phenomenon alone. The red rays demonstrate here, as in reflexion, their greater force of repulsion. As to the law of inflexion, it does not follow from the principles which explain reflexion. We may say, that the most repulsive rays are also the most attractive. This prop. is admitted by Mr. B.; but the diverse refrangibility of the rays gives a quite contrary indication.

A 2d question is this: Are the principles acknowledged for reflexion reconcileable with those for refraction? Mr. P. replies yes: Since the rays have traversed the repulsive sphere, they enter the attractive: the red rays are indeed the most repulsive, but nothing hinders the violet from being the most attractive. We may indeed say, that these 2 facts are naturally connected, and that there is reason to expect that the rays least easy to repulse are most easy to attract. Now refraction shows them such: For, 1. Nothing is better proved in theoretic optics, than the prop. which establishes, that refraction is produced by an attraction perpendicular to the attracting surface. 2. Therefore also, the differences of refraction, and in particular the greater refrangibility of the violet rays, are produced by the same cause: a consequence confirmed by their superior reflexibility in the denser medium.

A 3d question is this: Can the principles of refraction explain those of reflexion? Mr. P. says no: the law of inflexion remains unexplained. In this law, the red rays seem the more attractive, while in refraction it is the contrary. "Are not the rays of light reflected, refracted, and inflected, by one and the same force, which displays itself differently in different circumstances?" Such is the question which Newton made at the beginning of this century, and which seems not yet to be resolved towards the end of it. It is true, that Mr. B. concludes that, "the rays of light are reflected, refracted, inflected, and deflected, by one and the same power, variously exerted in different circumstances." This is, says Mr. P., doubtless very probable, but not yet proved.

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* Mr. B. very properly names flexion, what most philosophers have hitherto called inflexion, and some others diffraction. And he distinguishes it into inflexion and deflexion: the former inclining the ray to the flecting body, and the 2d bending the ray from it.—Orig.

The only new analogy, and doubtless a very important one, discovered by Mr. B., among these 3 classes of phenomena, is that which results from the harmonic relations among the distinct parts of the coloured spectrums, produced by refraction, reflexion, and flexion. Can the spectrum by reflexion be calculated exactly, on the principles above laid down, in order to compare the result of this calculation with experiment? Its harmonic division is a relation detected between this phenomenon and that of refraction, which strengthens the opinion, already so probable, on the identity of the principle on which these 2 phenomena depend. I dare not go farther.

In weighing these considerations, we shall recollect the proposition advanced by Mr. B. viz. that "the reflexibility of the rays is inversely as their refrangibility." But we must observe the sense of this assertion. Newton made a ray to pass out of glass into air, by the plane face of a prism, under a known angle of incidence; and having observed the angle of refraction of the red and the violet rays, he found these angles and their sines as below:

Common angle of incidence	31° 15′ 0″	sine 50
Angle of refraction of the red	53 4 58	77
Angle of refraction of the violet	54 5 2	78

Mr. B. made a ray to fall on the convexity of a metallic fibre, in an unknown angle of incidence; and having observed the reflexion, he concluded the following angles and sines:

Angle of common incidence	77° 20′	sine 77½
Angle of reflexion of the red	75 50	77
Angle of reflexion of the violet	78 51	78

Here we indeed see, that the numbers 77 and 78 expressed in both places, are the limits, in the one case of refraction, in the other of reflexion; but we cannot conclude the inverse ratio of the quantities measured in the observation of these 2 phenomena: the disparity of circumstances in these 2 experiments opposing it. Mr. B. himself remarks the difference of incidence. But this is not the only one; and it is sufficient to recollect that, by the same incidence, the dispersion of the coloured rays varies according to the nature of the mediums, to destroy all idea of regular proportion, expressed in a precise and general manner, without regard to the diversity of mediums. The prop. asserted by Mr. B. then only means, that those rays which occupy the most space on the refracted spectrum, occupy less on the reflected one; and that both show the harmonic division. This is sufficient indeed to indicate some analogy, but not to found, without some other proof, the unity of principle.

It is in the same sense the prop. must be understood which establishes that "the flexibilities are as the reflexibilities directly, and as the refrangibilities inversely." And there is still much more disparity in the circumstances and in the results, as Mr. B. has taken care to remark. Thus, the harmonic division of the coloured spectrum furnishes an analogy still much weaker, in favour of the identity of a common principle, to which these 3 phenomena must be related. And yet it must be agreed that these feeble analogies favour the persuasion, and that the mind will not be satisfied until, by some new effort, flexion be re-united to the phenomena of reflexion and refraction, by the unity of principle.

Does our ignorance in the nature of the forces which produce these phenomena, particularly in the nature of the repulsive force, and the want of agreement still existing between the phenomena of flexion and the others, permit us to believe in the physical cause long since intimated by Newton, lately renewed, and even calculated, by Mr. B.; I mean the difference in the size of the particles which compose the different rays? Are we in the right to countenance as an hypothesis, Newton's theory of the fits of easy and difficult transmission? This theory is only a generalized expression of a well-observed fact. If the alternate transmissions and reflexions depend only on the thickness of the transparent plates, either the rays or the medium must alternate, and in equal tempuscules, in the opposite dispositions. From experiments more varied it will appear if the thickness alone influence it: it was with this view that the Abbé Mazeas undertook some experiments, which have not yet given however any result, but which we might hope to see followed with success, (Mem. des Sav. Etr., 1755). The theory of very thin transparent plates it appears was conceived by Newton at 27 years of age, and he did not publish it till 35 years later: for he alludes to it in his Lectures at Cambridge in 1669, and the 1st edit. of his Optics was in the year 1704. As much as it would be absurd to place even the most respectable authority on a parallel with reason, so far is it just to require a very attentive examination of any one so much reflected on.

I shall conclude by observing, that the explanation I have proposed, according to the Newtonian principles, of the phenomena observed by Mr. B., in the reflexion produced by a very small cylinder, does not injure the course he has taken to explain the colours of natural bodies. His idea and that of Newton, in that respect, are not in contradiction. It is not certain that the colours of natural bodies are only produced in one manner; but, that they be produced, reflexion must act on every particle of the bodies, in all sorts of angles. And I do not see that Mr. B. has succeeded in trying, under many various angles, the reflexion he has obtained by his small cylinders. It seems that he speaks only precisely of that where the angle of incidence was about 77°, and consequently a very large one.

XII. Of the Orifice in the Retina of the Human Eye, discovered by Prof. Soemmering. With Proofs of this Appearance being extended to the Eyes of other Animals. By Everard Home, Esq., F. R. S. p. 332.

Having received a particular account of this discovery, in a very obliging letter from Mr. Maunoir, an eminent surgeon at Geneva, which contains, I believe, the material information published by Mr. Soemmering; I shall transcribe that part of the letter, which is as follows: "The war being an obstacle to a free communication between England and the continent, you are not perhaps acquainted with a new discovery in the anatomy of the human eye, made by a professor of Mentz, Mr. Soemmering; permit me therefore to say something on the subject. He was dissecting, in the bottom of a vessel filled with a transparent liquid, the eyes of a young man who had been drowned, and was struck on seeing, near the insertion of the optic nerve on the retina, a yellow round spot, and a small hole in the middle, through which he could see the dark choroides, looking at the surface of the retina which covers the vitreous humour. He dissected other human eyes, and constantly, when the dissection was carefully made, found the hole of the retina seemingly at the posterior end of the visual radius, nearly 2 lines on the temporal side of the optic nerve, and the hole surrounded by the yellow zone, of above 3 lines in diameter. The hole of the retina is not directly seen, being covered with a fold of the retina itself. An anatomist of Paris dissected many eyes of quadrupeds and birds, and found the yellow spot and hole in no animal but the human kind. Should you think that nature has intended this hole to grow large when the eye is opposed to a strong light, and thereby cause a great part of the rays to fall on the choroid, and vice versa, when the eye is in darkness? And the want of such a construction in animals, is it owing to a greater power of augmenting or diminishing the pupil, than in men? If Messrs. Mariotte and Le Cat should come to life again, they would find, in that hole, the explanation of the phenomenon of the 2 cards, one disappearing at a certain distance from 1 eye, &c. which may be explained by saying, that where the optic nerve enters the ball, there is no choroid, and so no vision.

"I dissected some human eyes a short time after I had read the discovery, and found the spot, the ruga concealing it, and the yellow zone. The best way I think to see them, is to take off the half posterior part of the sclerotica, then the correspondent part of the choroid; both must be cut round the insertion of the optic nerve. The retina is to remain bare and untouched, sustaining alone the vitreous humour; then you may see the round spot, that reaches the optic nerve, and a fold of the retina, marking a diameter of the spot. Then, if you press the ball a little with your finger, so as to push the vitreous humour rather near the bottom of the eye, the ruga is unfolded, and you will see the hole perfectly round, of $\frac{1}{4}$ of a line in diameter, and its edges very thin. All this can be seen on the inside of the eye, but not so perfectly; and in that case you must make the observations in water."

Many months elapsed, after the receipt of this letter, before I could procure an eye in a proper state for observing this aperture in the retina; but, in the course of last month several opportunities offered, and I saw the appearance described by Mr. Maunoir very distinctly. The mode I adopted for examining the retina, was that of removing the transparent cornea; then taking away the iris, and wounding the capsule of the crystalline lens, so as to disengage the lens, without removing that part of the capsule which adheres to the vitreous humour; by which means, the retina remained undisturbed, and could be accurately examined, when a strong light was thrown into the eye. The aperture in the retina, surrounded by a zone with a radiated appearance, was distinctly seen, on the temporal side of the insertion of the optic nerve, and about $\frac{1}{6}$ of an inch distant from it, apparently a little below the posterior end of the visual radius. The aperture itself, in this view, was very small. After having viewed it in 2 different eyes, I took an opportunity of showing it to Sir Jos. Banks and Sir Charles Blagden, who both saw it with the same degree of distinctness.

At first, I believed it necessary to have a very fresh eye for demonstrating this aperture; but I have since found, that it is more readily seen in an eye 2 days after death; the zone, which is the most conspicuous part, being of a lighter colour the first day, than it is on the 2d. I have also succeeded in preserving the posterior part of the eye in spirits, without destroying the appearance of this aperture. I am induced to make this remark, by recollecting that a celebrated anatomist of Edinburgh denied, in his last publication, that the anterior lamina of the cornea can be separated from the others, as a continuation of the tendons of the 4 straight muscles of the eye, for no other reason than because he could not succeed in the demonstration of it; the failure probably arising from the eye not being sufficiently fresh to admit of such a separation. In separating the vitreous humour from the retina, I found a greater adhesion at this particular part; and, when the vitreous humour was removed, the retina was pulled forward, forming a small fold, in the centre of which was this aperture. This doubling was sometimes produced by endeavouring to cut through the vitreous humour, to disengage the crystalline and its capsule.

After having made the preceding observations on this singular appearance in the human eye, I found, in Dr. Duncan's Annals of Medicine for 1797, an account of a publication concerning it by Professor Reil, entitled, the plait, the yellow spot, and the transparent portion of the retina of the eye. After these are described separately, the following circumstances are mentioned. "Soemmering takes this appearance to be a real hole. Buzzi, on the contrary, thinks that it is merely a transparent and thin portion of the retina. Michaelis seems to agree with him. Reil and Meckel are rather in favour of the existence of an actual hole. Michaelis saw the plait more distinctly in fœtuses of 7 or 8 months, than in adults; and the transparent portion lay concealed within it, but the yellow spot was wanting: nor is it to be observed in the eyes of newly-born children. After the first year, it becomes somewhat yellow, and the depth of the colour increases with the age of the subject. Soemmering says that this spot is pale in children, bright yellow in young people, and becomes again pale in old age. Its degree of saturation seems to be intimately connected with the state of vision: it constantly diminishes, in proportion as vision is obstructed. Where one eye only is diseased, in it the yellow spot is wanting, and the plait is small and wrinkled; while in the sound one they are rather more distinct than usual. Michaelis discovered no vestige of these appearances in the eyes of dogs, swine, or calves."

Professor Reil's mode of dissecting the eye, to show the aperture and plait, is exactly similar to that mentioned in Mr. Maunoir's letter. It will appear, from the account of this orifice in the retina, which precedes these observations of Professor Reil, that the plait so particularly mentioned, is an artificial appearance, which takes place in the dissection of the eye, and arises from the circumstance of the vitreous humour adhering more firmly to the edge of this orifice, than to any other part of the retina; so that the smallest motion of the vitreous humour, in consequence of dividing it, or removing the choroid coat, produces a plait, by pulling forwards this portion of the retina. What is said of the colour of the yellow spot, and of the difference of opinion, whether it is a hole or a transparent portion of the retina, I shall consider more fully in another part of this paper.

After having ascertained the appearance of this aperture in the human eye, and found what appeared the best mode of seeing it, I determined to investigate this subject in the eyes of other animals. The monkey was the first animal which I procured for observation; being led, from previous knowledge in comparative anatomy, to believe that the structure of its eye must bear a very close resemblance to that of the human subject. The eye was examined immediately after the death of the animal, and was prepared in the same way that I have already described the human eye to have been for this purpose; so that the concave surface of the retina appeared in its most natural state, and the vitreous humour, being entire, kept it expanded, and free from rugæ. On the first view, nothing was to be seen but one dark surface, surrounding the entrance of the optic nerve. Two hours after death, the retina became sufficiently opaque to be distinguished, and immediately after, the orifice was visible, appearing to be an extremely small circular aperture, without any margin; but in ½ an hour more, the zone had formed, which, when very accurately examined in a bright light, had an appearance of four rays, at right angles, as expressed in pl. 5, fig. 7. Its situation, respecting the optic nerve, was precisely the same as in the human eye. As I considered this to be a fact of some importance, since it proved the aperture in the retina to be a part of the structure of the eye, generally, and not a peculiarity in the human eye, I requested Sir Jos. Banks, Sir Chas. Blagden, and Dr. Baillie, to examine it: to all of them it appeared very distinct. After having shown it to those gentlemen, and having an accurate drawing made of it, I preserved that portion of the eye in spirits; where the aperture in the retina can still be distinctly seen, but the radiated appearance is lost.

In the eye of a bullock, prepared in the same manner, I looked in vain for a similar appearance: if it existed, and bore any proportion to the size of the eye-ball, as it appears to do in the human eye and that of the monkey, it must have been very visible. The concave surface of the retina was examined in different lights, under a variety of circumstances, and by magnifying glasses of different powers, but still no aperture could be discovered. I was however very much struck, while looking at the optic nerve, to see something in the vitreous humour, in consequence of a person accidentally shaking the table, that had not been before observed. This proved to be a semi-transparent tube, resembling in its coats a lymphatic vessel, rising from the retina, close to the optic nerve, on the temporal side of its insertion, and coming directly forwards into the vitreous humour, in which it was lost, after being distinctly seen for $\frac{4}{20}$ths of an inch of its course. Its appearance is accurately delineated in fig. 8.

This tube is not so distinctly seen in the eye immediately on the animal's death, as some hours after; and is much more obvious in some eyes than in others. As the coats of the tube must be nearly the same in all eyes, this difference probably arises from its contents not always having the same degree of transparency. When the eye has been kept 24 hours after the animal's death, there is an appearance of a zone of a circular form, a shade darker than the rest of the eye, in which the optic nerve is included: when this zone, which is nearly $\frac{7}{20}$ths of an inch in diameter, is attentively examined, the tube I have described is exactly in the centre of it. The tube seems to be confined by the vitreous humour, while that humour is entire, and only to move along with the central part of it; and in some instances, when the vitreous humour is divided, the tube falls down. Its attachment at the retina appears stronger than its lateral connection with the vitreous humour; for when I coagulated the vitreous humour in spirits, and separated it from the retina, I found the tube was left with the retina, but on being touched was easily torn.

In the sheep's eye there is a similar tube, in exactly the same situation, respecting the optic nerve, but much shorter, and much less easily detected. It does not appear to be more than $\frac{1}{20}$th of an inch in length, before it is lost in the vitreous humour. After having seen the tube distinctly in 2 different eyes, and having had a drawing made of it, I looked for it in several others, without finding it: but, examining an eye from which the crystalline lens had not been removed, only an aperture made into the vitreous humour, by removing a portion of the ciliary processes along with the iris, the tube was distinctly seen. The weight of the lens probably pulled forward the vitreous humour, and kept the short tube erect, in its natural situation. In the sheep there is no appearance of a zone surrounding the tube.

These facts, though few in number, are sufficient to prove, that this orifice is not peculiar to the retina of the human eye; and that its situation in man and in the monkey is the same: in them, it is placed at some distance from the optic nerve; but in some other animals, its situation is close to that nerve, and it puts on the appearance of a tube, instead of an orifice. There is one circumstance which is curious, and which it will require further information on this subject to explain; the yellow zone, found in the human eye and that of the monkey, is not met with in any other animal which I have examined. Having stated the facts, and also the opinions of other anatomists, that have come to my knowledge, as well as my own observations, on this orifice in the retina of the human eye, discovered by Mr. Soemmering, and having added to these, several new facts respecting it in other animals, I shall draw some general conclusions from the whole, with a view to show that the conjectures which have been made, respecting its use, are probably erroneous. I shall afterwards point out several reasons for considering it as the orifice of a lymphatic vessel intended to carry off the vitiated parts of the vitreous humour and crystalline lens.

In the human subject, as no examination can be made for some considerable time after death, it is impossible to ascertain what is the real state of this orifice in the living eye, and what changes take place in it after death; we only learn, that the tinge of yellow surrounding the orifice is very slight, when the eye is examined recently, and that the next day it becomes much deeper. These points appear to be satisfactorily cleared up, by the examination that was made of the monkey's eye, as it was begun before the parts had lost the appearance belonging to them as living parts. In that state the retina was transparent, and no orifice could be seen; so that the orifice is rendered visible by remaining transparent, while the surrounding retina becomes opaque. This appears to decide the dispute between Messrs. Soemmering and Buzzi; for if this part does not undergo the change peculiar to the retina, we must consider the retina as wanting there. After the orifice is thus rendered visible, the yellow tinge is wanting, and does not take place for several hours, and even then is fainter than it becomes afterwards; which appears to be sufficient evidence, that this tinge is the effect of some change after death, and cannot therefore have any effect on vision.

The orifice has been supposed to account for a small object becoming invisible, when placed at a certain distance from the eye, and brought opposite a particular part of the retina. This however cannot be the case, as its situation in the retina does not correspond with the part opposed to the object, when rendered invisible. The orifice itself is probably too small to produce any defect in vision, as the trunks of the blood-vessels which ramify on the retina cover a larger space than this orifice, for a considerable extent, without obstructing the sight of any part of the object.

While my observations were confined to the human eye, I was led to consider this orifice as a lymphatic vessel, passing from the vitreous humour through the retina, but could bring no absolute proof of its being so. This opinion was strengthened by finding, that in the monkey, the orifice was only rendered visible when the retina became opaque; and it has since been corroborated, by a distinct tube being met with in the eyes of sheep and bullocks. That a change must be constantly taking place in the crystalline and vitreous humours, to preserve to them the necessary degree of transparency, can hardly be doubted; and that the absorbent vessels which perform that office should have one common trunk, which follows the course of the artery and vein, perfectly agrees with what takes place in other parts of the body. In the human eye, and that of the monkey, the artery is in the centre of the optic nerve; but that would have been too circuitous a course for the lymphatic vessel to follow, and by going through the retina, at some distance from the nerve, it can pass out of the orbit with the blood-vessels that go through the foramen lacerum orbitale inferius. In the bullock and sheep there is a plexus of vessels surrounding the optic nerve, and the tube dips down, close by the optic nerve, probably to accompany them.

From the observations made by Michaelis, of the yellow spot not being visible in fœtuses, or in infants under a year old, or in eyes that are blind, also of its being brighter in young people, and paler in old, it would appear, that it is only when the eye is capable of performing its functions, that there is any stain communicated to the retina. The drawings from which the figures are engraved, were made from preparations of the eye lying in water, with a strong light shining on the preparation. In making the drawings, the principal object was, procuring a distinct view of the parts surrounding the optic nerve; when this could be obtained, the situation of the eye itself was not attended to.

Fig. 5, pl. 5, is a transverse section of the human eye, immediately before the ciliary processes. The retina is viewed through the posterior portion of the capsule of the crystalline lens. a, The termination of the optic nerve. b, The aperture in the retina, discovered by Professor Soemmering.

Fig. 6, A longitudinal section of the left eye in the human subject, to show the relative situation of the aperture in the retina to the entrance of the optic nerve, and the mode in which it appears to project, when the vitreous humour is disturbed. a, The termination of the optic nerve. b, The aperture in the retina.

Fig. 7, A transverse section of the eye of a large monkey, to show the aperture in the retina: its situation is the same as in the human eye. The zone has the appearance of a star with 4 rays. a, The entrance of the optic nerve. b, The aperture in the retina.

Fig. 8, A transverse section of the eye of a bullock, to show that there is a semi-transparent tube projecting from the edge of the entrance of the optic nerve, into the vitreous humour. This tube is surrounded by a zone, with a distinct margin: it is situated on the temporal side of the optic nerve.

Fig. 9, A transverse section of the eye of a sheep, to show that there is a similar tube as in the bullock, in the same situation, but much shorter, and without the surrounding zone.

XIII. On a very Unusual Formation of the Human Heart. By Mr. J. Wilson, Surgeon. p. 346.

It is well known that the circulation of the blood throughout the body, and exposure of it to the atmospheric air in respiration, seem in most animals to be necessarily connected; but are not equally so in all. They are so much connected in the human subject, and in most quadrupeds, that after birth there is a double heart; viz. one for the circulation of the blood throughout the body, to be subservient to the various purposes of life and growth; the other for its circulation through the lungs, where it undergoes a change which is essential to its general circulation through the body: these 2 circulations, in the natural state, bear an exact proportion to each other. Instances however have occurred, even in the human subject, where this exact proportion has not been preserved; yet life has been prolonged for some years, but in a feeble and imperfect state. In some of these instances, the pulmonary artery has been smaller than usual, so that much less than the natural quantity of blood was exposed to the influence of the air in the lungs; in others the foramen ovale has not been closed, but a considerable communication has remained between the 2 auricles; and in others there has been a communication between the 2 ventricles, from a deficiency in the septum. The effect of all these deviations is the same, on the blood in the general circulation, viz. that a part of the blood is not exposed to the air in the lungs; so that it is less pure as it circulates over the body. A more remarkable deviation in the structure of the heart, than any to which I have just alluded, has been lately published by Dr. Baillie, in his Morbid Anatomy. In this heart, the aorta arose from the right ventricle, and the pulmonary artery from the left; the reverse of what ought, in the regular course of circulation, to have taken place; the veins being as usual; and no communication was found between the one vessel and the other, except through the remains of the ductus arteriosus, which was not larger than a crow quill, and a small part of the foramen ovale, which still continued open; yet this child lived for 2 months.

In the following case of monstrous formation of the heart, there is this very great singularity, that nature seems to have substituted, very exactly, the circulation which takes place in some amphibious animals, for that which is natural to the human species. The infant had arrived at its full time, and lived 7 days after its birth. Instead of the usual integuments, muscles, &c. a membranous bag appeared to protrude on the upper and fore part of the abdomen, extending from the last bone of the sternum some way below the middle of the belly, and outwards, so as to be nearly circular: the navel-string seemed to enter this membrane near its middle, and to wind superficially, for some little way, towards the left side; it then dipped into the abdomen, at the place where this membrane joined the usual coverings. Within this bag, the appearance of which was very nearly similar to that of the chorion and amnios which envelope the fœtus at birth, but thicker in consistence, a tumour was perceived, possessing considerable motion, from the nature of which, no doubt was entertained that it was the heart.

During the short period of the child's life, it was seen and examined by a number of professional men. On its death, the tumour was carefully opened by Mr. Morell, in the presence of Dr. Poignand; when the heart, as was previously suspected, appeared to be situated in the epigastric region of the abdomen, and to be imbedded, as it were, in a cavity formed on the superior surface of the liver. In this state, the child was sent to Dr. Baillie, by whose desire I injected the heart, and laid its principal vessels bare, so as to bring their uncommon distribution and course into view: a preparation of them still remains in Dr. Baillie's possession. A considerable part of the tendinous portion of the diaphragm appeared to be wanting, as well as the lower part of the pericardium, which is usually affixed to it. The thorax being laid open on each side of the sternum, the 2 pleuræ were seen passing from that bone to the spine, and covering the lungs as usual. The lungs appeared perfectly natural in colour, and nearly so in shape; but were larger and fuller than usual, in consequence of more room being afforded for them in the thorax, from the peculiar situation of the heart. In the space corresponding to the anterior mediastinum, was the thymus gland, considerably longer than in other children, and extending downwards the whole length of the sternum; behind this, was a peculiar arrangement of blood-vessels.

The heart, instead of consisting of 4 cavities, as in the natural structure, consisted of a single auricle and ventricle, which were each of them large in their size. A large arterial trunk arose from the ventricle, and ascended into the thorax, between the pleuræ, immediately behind the thymus gland: it soon divided into 2 large branches, 1 of which continued to ascend, forming the aorta; the other passed backwards, and proved to be the pulmonary artery. The aorta, having reached the common place of its curvature, formed it in the same manner as it usually does; sent off the vessels belonging to the head and upper extremities; descended before the vertebræ, and passed into the abdomen between the crura of the diaphragm. From the place where it began to form the arch, it was in no respect different from the aorta of any other infant, except that no bronchial artery was sent to the lungs, from it or any of its ramifications. The vessel which proved to be the pulmonary artery, almost immediately divided into 2 branches; one going to the lungs of the left, the other to the lungs of the right side. On measuring accurately the circumference of the aorta, where it separated from the original trunk, it was found to be exactly $1\frac{1}{4}$ inch. On measuring the circumference of the pulmonary artery, in the same manner, it was found to be $\frac{15}{16}$ of an inch; so that it was $\frac{5}{16}$ of an inch less than the aorta.

The vena cava inferior, having been partly surrounded by the substance of the liver, entered the lower and back part of the auricle. The subclavian vein of the right side crossed over to the left of the mediastinum, where it joined the left subclavian, and formed the vena cava superior. This passed down on the left of the ascending, and before the descending part of the aorta; it was then joined by a trunk formed by 2 large veins, which came out of the lungs, and which were situated immediately behind the pulmonary arteries: the union of this trunk with the vena cava superior was continued into a large vessel, which gradually expanded itself into the auricle. The vena azygos ascended on the left side; received some branches which passed under the aorta from the right, and then entered the upper and back part of the vena cava superior: there were no bronchial veins. From there being neither bronchial arteries nor veins, it would appear that the pulmonary arteries and veins, in addition to their usual offices, performed those of the bronchial vessels. The liver was not divided on its upper surface by the suspensory ligament, but had a considerable cavity scooped, as it were, out of its substance; which in shape was adapted to, and contained the heart: it was also, in some other particulars, rather different from its natural shape, but not sufficiently so to require being minutely described. The rest of the infant was not found to be dissimilar to any other.

It is a well-ascertained fact, that the blood receives a florid hue from the influence of the air on it in the lungs; and this change is supposed to be effected by the combination of a certain quantity of oxygen gas with it. In passing from the arteries to the veins, in every part of the body except the lungs, it loses the florid hue, and becomes darker: the florid blood is that which is employed for the purposes of supporting life. In the natural circulation, it is well known that the whole of the blood conveyed to, and circulating in, the pulmonary artery, is of a dark colour; and the whole of it, when returned by the pulmonary veins, is florid.

It is obvious, in the case which has been described, that there always must have been florid and dark-coloured blood, mixing and circulating in the arteries. It would seem also, on the first reflexion, that the quantity of dark-coloured blood would be the greatest, in the same proportion as the capacity of the aorta was larger than that of the pulmonary artery. It is therefore necessary to recollect, that a considerable proportion of the blood carried to the lungs was already florid or oxygenated; and also, that the lungs in this infant were larger in proportion, than in children of the same age: a smaller quantity of blood therefore was to be oxygenated, and a larger surface than usual was appropriated for this purpose. It appears also, from experiments, such as making a person breathe air in which there is a greater proportion of oxygen gas than in our atmosphere, that the blood can combine with more of it than it does in natural respiration: it therefore is not an improbable supposition, that a larger quantity was combined here. A small drawback must be allowed, for the quantity of oxygenated blood used in the support and secretions of the lungs, and which is usually conveyed to them by the bronchial artery; but this quantity is too small to require more than this slight observation of it. The blood also which passed to the lungs, must have been again conveyed to the heart sooner, from the shortness of its circuit; and must have entered the heart with a quicker or stronger current, than that blood which passed to, and was returned from, the more remote parts of the body; as, in this child, the pulmonary artery and aorta were filled by the contraction of the same ventricle. In the hearts of other children, some time after birth, the muscular fibres of the right side are much fewer in number than in the left.

If these circumstances are admitted as fact, viz. that the blood circulating through the lungs of this child was combined with a larger proportion of oxygen gas, and was returned in a quicker and stronger current into the auricle than that returned by the venæ cavæ, it seems reasonable to infer, that this blood, mixing and blending with the dark or unoxygenated blood, would render the whole nearly as much oxygenated as it usually is found in the left side of the heart, and in the aorta; therefore, that the blood circulating in the arteries of this child would be fully equal to the support of life. Previous to birth, this peculiarity of structure could not affect its health or growth, as the placenta then answers the purpose which the lungs do afterwards; and the single ventricle seemed as equal, from its size, to propel the blood on to the placenta, as both ventricles in the natural state are, by means of their communication through the ductus arteriosus.*

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* It is here not unworthy of remark, that the circulation in this child, after its birth, was in several

The inference which has been drawn seems further confirmed, from the colour and heat of this child, during life, being not perceptibly different from those of other children. In all those cases of malformation of the heart where the foramen ovale, or the ductus arteriosus, has continued open; or where the septum of the ventricles has been perforated, and the pulmonary artery small, and at the same time 2 ventricles, it has been observed, that the body had a livid colour, and in general that there was a deficiency of heat. From the particular inquiries which I made, concerning the heat and colour of this child, of the professional gentlemen who saw it during life, and of the nurse who attended and dressed it, I found that the heat, so far as could be judged by the feeling, for it was not tried by the thermometer, was in no respect different from that of other children; and that the colour of the skin was perfectly natural, except that, on the day on which it was born, and a short period before its death, the lips occasionally had something of a livid appearance; but that this did not last any time, as they were generally pale. This occasional lividness would happen to a child in that state, should the heart and circulation be in no way different from what they naturally are.

I could meet with no other remarkable circumstances, either in the history of the mother during pregnancy, or in the child after birth. It cried occasionally, like other children, but seemed weak, and in pain; it slept; it sucked heartily, even a few hours before its death, and had apparently healthy evacuations of urine and fæces. Its death can be satisfactorily accounted for, from another cause than the extraordinary formation of its heart and blood-vessels. The membranous covering on the fore part of the abdomen, did not appear to possess sufficient vascularity to retain its life after birth; for it immediately lost its living principle, and became putrid and mouldy in parts. Previous to the child's death, a process of separation had begun, between it and the living parts to which it was connected, and a line of inflammation was distinctly seen. Had this process been completed, and the slough thrown off, the heart would have been exposed; but, before this, the heart itself had inflamed; which was proved from its being found covered with a coat of coagulable lymph recently thrown out, and from this inflammation its death must have arisen.

Had the heart been covered with the usual parietes of the abdomen, it is probable, notwithstanding its situation, that this child might have lived in a tolerable state of health for years; but must constantly have been exposed to have its heart injured by some external accident, from its not being defended by the ribs and the sternum. The formation and disposition of the heart and vessels, in this child,

circumstances similar to the circulation in other children previous to that period. A child, before birth, may be said to have a single heart; as both the auricles communicate together, by means of the foramen ovale; and the pulmonary artery communicates with the aorta, by means of the ductus arteriosus. Hence, in the fœtal heart, the blood returned from the body, which is of a dark colour, and the blood returned from the placenta, which is florid, are poured into the same auricle; the blood which is sent to the placenta is therefore already in part oxygenated.—Orig.

resemble much those which are found in the frog, and some other amphibious animals; but this infant could not, like them, be amphibious. Those animals are extremely tenacious of life, so that they live some time, even after their heart and lungs are removed from their bodies; and as their circulation can go on without respiration, it is therefore not wonderful that they often live a considerable time without change of air. Life, in the human species, depends equally on both these actions; for death takes place, if either of them should stop. The circulation of the blood in this infant would have met with no impediment, had it been immersed in water; but, unless respiration went on, which in that state it could not do, the blood could undergo no change in the lungs; and this change is equally essential to the support of life, as the circulation of the blood.

XIV. On a Singular Instance of Atmospherical Refraction. By Wm. Latham, Esq., F. R. S., and A. S. p. 357.

July 26, about 5 o'clock in the afternoon, while sitting in my dining-room at this place, Hastings, which is situated on the Parade, close to the sea-shore, nearly fronting the south, my attention was excited by a great number of people running down to the sea side. On inquiring the reason, I was informed that the coast of France was plainly to be distinguished with the naked eye. I immediately went down to the shore, and was surprized to find that, even without the assistance of a telescope, I could very plainly see the cliffs on the opposite coast; which, at the nearest part, are between 40 and 50 miles distant, and are not to be discerned, from that low situation, by the aid of the best glasses. They appeared to be only a few miles off, and seemed to extend for some leagues along the coast. I pursued my walk along the shore to the eastward, close to the water's edge, conversing with the sailors and fishermen on the subject. At first they could not be persuaded of the reality of the appearance; but they soon became so thoroughly convinced, by the cliffs gradually appearing more elevated, and approaching nearer, as it were, that they pointed out, and named to me, the different places they had been accustomed to visit; such as, the Bay, the Old Head or Man, the Windmill, &c. at Boulogne; St. Vallery, and other places on the coast of Picardy; which they afterwards confirmed, when they viewed them through their telescopes. Their observations were, that the places appeared as near as if they were sailing, at a small distance, into the harbours.

Having indulged my curiosity on the shore for near an hour, during which the cliffs appeared to be at some times more bright and near, at others more faint and at a greater distance, but never out of sight, I went on the eastern cliff or hill, which is of a very considerable height, when a most beautiful scene presented itself to my view; for I could at once see Dengeness, Dover cliffs, and the French coast, all along from Calais, Boulogne, &c. to St. Vallery; and, as some of the fishermen affirmed, as far to the westward even as Dieppe. By the telescope, the French fishing-boats were plainly to be seen at anchor; and the different colours of the land on the heights, with the buildings, were perfectly discernible. This curious phenomenon continued in the highest splendour till past 8 o'clock, though a black cloud totally obscured the face of the sun for some time, when it gradually vanished. I was assured, from every inquiry I could make, that so remarkable an instance of atmospherical refraction had never been witnessed by the oldest inhabitant of Hastings, nor by any of the numerous visitors come to the great annual fair. The day was extremely hot. I had no barometer with me, but suppose the mercury must have been high, as that and the 3 preceding days were remarkably fine and clear. To the best of my recollection, it was high water at Hastings about 2 o'clock P. M. Not a breath of wind was stirring the whole of the day; but the small pennons at the mast-heads of the fishing-boats in the harbour were in the morning at all points of the compass. I was, a few days afterwards, at Winchelsea, and at several places along the coast; where I was informed, the above phenomenon had been equally visible. When I was on the eastern hill, the cape of land called Dengeness, which extends nearly 2 miles into the sea, and is about 16 miles distant from Hastings, in a right line, appeared as if quite close to it; as did the fishing-boats, and other vessels, which were sailing between the 2 places; they were likewise magnified to a great degree.

XV. Of a Tumour found in the Substance of the Human Placenta. By John Clarke, M.D. p. 361.

The structure of the egg of oviparous animals serves to elucidate the corresponding process in the viviparous; and though in many cases analogies are very inconclusive, yet in this the resemblance is so close, that the latter may be said to be demonstrated by the former. A certain temperature, nourishment, and the application of vital air, or oxygen, seem to be essential to the evolution of the young of oviparous animals. As the young are expelled from the mother, contained in the cavity of the egg, at a very early period of their existence, and as afterwards they have no connection whatever with her, these are supplied by various contrivances: and the mode of application has been very distinctly explained, by modern inquirers into the structure of eggs. Since then the same substances are to be produced, and supported, in viviparous as in oviparous animals, the conclusion is reasonable, that similar means should be employed to attain similar ends.

It is easy to conceive how warmth may be imparted to a fœtus situated in the uterus. The materials for nourishment, it receives from the placenta; but the precise manner in which they are supplied has not yet been discovered. Of the fact there can be no doubt, because there are many cases on record, in which there could be no other possible way by which support could be had. With respect to vital air, or oxygen, the young of all viviparous animals, while in the uterus, live in the same medium as fishes, and have a structure similar to gills, for the exposure of their blood to it; this structure is the placenta.

The heart of the fœtus is adapted to this mode of life, and in effect consists but of 1 auricle and 1 ventricle, as it is found to do in fishes. The junction between the 2 ventricles is attended with a great advantage, in performing the circulation through the placenta; where the length and convolution of the umbilical vessels, in some animals, offer a great resistance to the force of the heart, and render more exertion necessary. In the superior aorta, the circulation is carried on by the left ventricle alone; as the ductus arteriosus does not join the aorta, till after the latter has given off the carotid and subclavian branches. Vital air is communicated to the blood of the embryo, as it is to the blood of fishes. This, in its passage through the gills, is exposed to water, which is allowed by all to contain a large proportion of vital or oxygen gas, and returns thence fitted to answer the purposes of life.

In like manner, the blood of the mother, in the cells of the placenta, having received the essential part of this gas from her lungs, is applied to the capillary vessels of the umbilical arteries, which receive and transmit it to the embryo; the life of which so entirely depends on this communication, that an obstruction to the circulation through the placenta, for 2 or 3 minutes, will sometimes irrecoverably destroy it. The gills of fishes form a permanent part of their bodies, because they are designed to pass the whole of their lives in the same medium. This is not the case in the embryo of viviparous animals; which, after birth, is to change its situation for another, in which there is a direct exposure of the blood to atmospheric air. For this reason, the placenta, whose use is only temporary, is attached to the fœtus by a slender connection, which is soon dissolved after birth. I have thought it necessary to introduce the foregoing observations on the structure and functions of the placenta, in order to show that the principal use of it is to transmit, and apply respectively to each other, the blood of the fœtus, and that of its mother. No other action is carried on by the vessels of the fœtal portion of the placenta, as far as is yet known, than what has been described, unless so much as may be necessary for their own growth and nourishment.

The tumour which gave occasion to this paper is however an instance to prove, that these vessels are capable, like those in other parts, of forming solid organized matter; and that very considerable deviations from the ordinary structure of the placenta may exist, and be perfectly compatible with the life and health of the fœtus. Before the birth of a healthy child, an amazing quantity of liquor amnii was evacuated, which was by accident received in a vessel, and was found to amount to 2 gallons, Winchester measure. When the placenta came away, a hard solid body was found in its substance. It was preserved by Mr. Mainwaring, under whose care the case occurred, and was by him obligingly presented to me. Fine injection was thrown into the arteries and vein of the funis umbilicalis; when they were filled, they appeared to be enlarged thrice beyond their natural size.

The placenta, thus prepared, was subjected to examination. Its anterior surface was found to be covered with the amnion, behind which lay the chorion as usual. Some branches, both of the arteries and veins, coming from the funis, ramified in the common manner, forming the fœtal portion of the placenta. Others, of a very large size, not less than a swan's quill, were sent to the tumour; which was situated behind the chorion, and lay imbedded in the fœtal portion of the placenta. The general form of this tumour was oval; about 4½ inches long, and 3 inches broad; the thickness about 3 inches; and it weighed upwards of 7 oz. Its shape resembled that of a human kidney; 1 edge being almost uniformly convex, while the other, where the vessels approached it, was a little hollowed. The general character of the surface of the tumour was convexity; but in some parts of it there were slight indentations, more particularly in the course of the large vessels.

The whole of the tumour was inclosed in a firm capsule, in the substance of which the large vessels were contained, nearly in the same manner as they are found in the dura mater. In the interstices of the vessels, the capsule did not appear to be vascular; at least there were no vessels capable of carrying the injected matter. The blood-vessels, branching off from the funis to supply the tumour, partly went over one side, and partly over the other side of the tumour; ramifying as they ran, till, meeting at the convex edge of the tumour, they anastomosed very freely. From the large trunks on the surface, small branches were given off, penetrating into the substance, and supplying the whole tumour with blood. On making a section through the tumour, in the direction of its length, the consistence was found to be uniform, firm, and fleshy, very much resembling, in this respect, the kidney. The cut surface, on examination, had somewhat of a mottled appearance; some parts being highly vascular, while others were white and uninjected.

If the mere existence of such a tumour is not to be considered as a disease, there was no appearance of any morbid tendency in any part of it. The whole structure seemed to consist of a regularly organized matter throughout, supplied with vessels exclusively belonging to itself, and not passing to it from the surrounding parts, as is generally the case in diseased masses. Those who are inclined to consider every new appearance in the structure of parts as disease, may be disposed to include this under that appellation. But disease consists in such an alteration in the structure, or functions, of a part, as occasions the natural operations of it to be imperfectly performed, or entirely arrested. This tumour appears to have produced no such effect: all the common and known functions of the placenta were performed, notwithstanding the existence of this substance: the child had been as well nourished, and the benefits arising from the application of vital air or oxygen, to its blood, just as well supplied, as if the tumour had not existed.

It cannot be said of this, as it might of some tumours, that it would in time have shown marks of a morbid tendency, so as to have deranged the common actions of the placenta; because, when gestation terminates, the life, and all the uses of the placenta, are at an end. I am disposed, therefore, to consider this fleshy substance, as a solitary instance of a formative property in the vessels of the placenta; which they have not been hitherto generally known to possess.*

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* The placenta sometimes becomes converted into a mass of hydatids, connected to each other by small filaments; but this must be considered as a disease, inasmuch as the natural structure is destroyed,

There was a remarkable circumstance attending this case, which ought not to be lost sight of, viz. the extraordinary quantity of liquor amnii, which had been contained in the ovum. What connection there was between this and the tumour, cannot be absolutely explained from a single instance, as there did not seem to be any direct communication between the tumour and the cavity of the amnion. The whole of it lay, as has been before related, behind the chorion; so that between it and the cavity of the ovum there were 2 membranes interposed. In its organization, it had all the appearance of a glandular part, and was extremely vascular; but no duct could be found leading from it into the cavity of the ovum. Yet, though it may appear difficult to prove, that the quantity of liquor amnii depended on this substance, still, as it so considerably exceeded that which is found in common, or has ever been described, it is reasonably to be believed that it did so. The manner however by which the secreted fluid was conveyed from the tumour into the general cavity of the ovum, must still remain unaccounted for.

XVI. On the Roots of Equations. By James Wood, B. D. p. 369.

The great improvements in algebra, which modern writers have made, are chiefly to be ascribed to Vieta's discovery, that "every equation may have as many roots as it has dimensions." This principle was at first considered as extending only to positive roots; and even when it was found that the number might, in some cases, be made up by negative values of the unknown quantity, these were rejected as useless. It could not however long escape the penetration of the early writers on this subject, that in many equations, neither positive nor negative values could be discovered, which, when substituted for the unknown quantity, would cause the whole to vanish, or answer the condition of the question; in such cases, the roots were said to be impossible, without much attention to their nature, or inquiry whether they admit of any algebraical representation or not. As far as the actual solution of equations was carried, viz. in cubics and biquadratics, the imaginary roots were found to be of this form, $(a + \surd -b^2)$; and subsequent writers, from this imperfect induction, concluded in general, that every equation has as many roots, of the form $(a \pm \surd \pm b^2)$, as it has dimensions. In the present state of the science, this proposition is of considerable importance, and its truth ought to be established on surer grounds. The various transformations of equations, the dimensions to which they rise in their reduction, and the circumstances which attend their actual solution, are most easily explained, and most clearly understood by the help of this principle. Mr. Euler appears to have been the first writer who undertook to give a general proof of the proposition; but whatever may be thought of his reasoning in other respects, as he carries it no farther than to an equation of 4 dimensions, and it does not appear capable of being easily applied in other cases

and it directly interferes with the offices of the placenta, which no longer performs perfectly the functions for which it was designed. Nourishment and vital air are no longer supplied properly to the fœtus, which therefore commonly dies.—Orig.

it gives us no insight into the subject. Dr. Waring's observations on the proposition are extremely concise; and, to common readers, it will still be a matter of doubt, whether a quantity of any description whatever will, when substituted for x in the expression $x^8 - px^7 + qx^6 - \dots + w$, cause the whole to vanish.

In the investigation of the proof here offered, it became necessary to attend to the method of finding the common measure of 2 algebraic expressions; and to observe particularly, in what manner new values of the indeterminate quantities are introduced; and how they may again be rejected. It appears, that these values are necessary in the division; and, when they have been thus introduced, they enter every term of the 2d remainder, from which they may be discarded. This circumstance enables us, not only to determine the nature of the roots of every equation, but also affords a direct and easy method of reducing any number of equations to one, and obtaining the final equation in its lowest terms.

PROP. 1. To find a common measure of the quantities $ax^n + bx^{n-1} + cx^{n-2} + dx^{n-3} +$ &c. and $Ax^{n-1} + Bx^{n-2} + Cx^{n-3} + Dx^{n-4} +$ &c.

In order to avoid fractions, multiply every term of the dividend by A^2, the square of the co-efficient of the first term of the divisor, and after finding 2 terms in the quotient, the remainder is

$$(P) = (cA^2 - bBA + aB^2 - aCA)\, x^{n-2} + (dA^2 - bCA + aBC - aDA)\, x^{n-3} + \text{\&c.}$$

$$\text{Let } (cA - bB)\, A + (B^2 - CA)\, a = \alpha,$$
$$(dA - bC)\, A + (BC - DA)\, a = \beta,$$
$$(eA - bD)\, A + (BD - EA)\, a = \gamma, \text{ \&c.}$$

and the first remainder (P) is $\alpha x^{n-2} + \beta x^{n-3} + \gamma x^{n-4} +$ &c. proceed with this as a new divisor, and the next remainder (Q) will be $[(C\alpha - B\beta)\,\alpha + (\beta^2 - \alpha\gamma)\, A]\, x^{n-3} + [(D\alpha - B\gamma)\,\alpha + (\beta\gamma - \alpha\delta)\, A]\, x^{n-4} +$ &c.

Respecting this operation we may observe: 1. That were not every term of the first dividend multiplied by A^2, that quantity would be introduced by reducing the terms of the remainder (P) to a common denominator. 2. When $P = 0$, $Ax^{n-1} + Bx^{n-2} + Cx^{n-3} +$ &c. is a divisor of $A^2\,(ax^n + bx^{n-1} + cx^{n-2} + \text{\&c.})$; and therefore it is a divisor of $ax^n + bx^{n-1} + cx^{n-2} +$ &c. unless it be a divisor of A^2, which is impossible; consequently no alteration is, in this case, made in the conclusion, by the introduction of A^2.

3. When P does not vanish, then every divisor of P is a divisor of $Ax^{n-1} + Bx^{n-2} + Cx^{n-3} +$ &c. and of $A^2\,(ax^n + bx^{n-1} + cx^{n-2} + \text{\&c.})$; and therefore of $ax^n + bx^{n-1} + cx^{n-2} +$ &c. unless $A^2 = 0$, in which case the remainder, P, becomes $aB\,(Bx^{n-2} + Cx^{n-3} + \text{\&c.})$, every divisor of which is a divisor of $Bx^{n-2} + Cx^{n-3} +$ &c. whether it be a divisor of $ax^n + bx^{n-1} + cx^{n-2} +$ &c. or not. That is, there are 2 values of the indeterminate quantity A, which, if retained, will produce erroneous conclusions.

4. A^2 enters every term of the 2d remainder (Q), and the 2 values, before introduced, may therefore be again rejected. The co-efficient of the first term of this remainder is $(C\alpha - B\beta)\,\alpha + (\beta^2 - \alpha\gamma)\,.\,A$; and, by substituting for α, β and γ,

their values, and retaining only those terms in which A is not found, and those in which it is only of one dimension, we have

$$\begin{aligned}
\mathrm{C}\alpha &= -b\mathrm{BCA} + a\mathrm{CB}^2 \\
&\quad - a\mathrm{C}^2\mathrm{A} \\
-\mathrm{B}\beta &= +b\mathrm{BCA} - a\mathrm{CB}^2 \\
&\quad + a\mathrm{BDA} \\
\mathrm{C}\alpha - \mathrm{B}\beta &= -a\mathrm{C}^2\mathrm{A} + a\mathrm{BDA} \\
(\mathrm{C}\alpha - \mathrm{B}\beta)\,\alpha &= -a^2\mathrm{B}^2\mathrm{C}^2\mathrm{A} + a^2\mathrm{B}^3\mathrm{DA} \\
(\beta^2 - \alpha\gamma).\mathrm{A} &= a^2\mathrm{B}^2\mathrm{C}^2\mathrm{A} - a^2\mathrm{B}^3\mathrm{DA};
\end{aligned}$$

therefore, those parts of $(\mathrm{C}\alpha - \mathrm{B}\beta)\,\alpha + (\beta^2 - \alpha\gamma).\mathrm{A}$, in which A is of one dimension, and in which it is not found, vanish. In the same manner it appears, that A^2 enters every other term of the remainder Q.

5. If the remainder $\mathrm{Q} = 0$, then, by the 2d observation, the introduction of α^2 in the division, produces no error in the conclusion; and if Q do not vanish, α^2 will be found in every term of the 3d remainder, and may there be rejected; and so on. Thus we obtain the conclusion, without any unnecessary values of A, B, C, &c. or a, b, c, &c. 6. If the highest indices of x, in the original quantities, be equal, it will only be necessary to multiply the terms of the dividend by A, which may be rejected after the 2d division. If the difference of the highest indices of x be m, the terms of the dividend must be multiplied by A^{m+1}, the first quotient being carried to $m + 1$ terms. This quantity A^{m+1} will enter every factor in each term of the 2d remainder. 7. If it be necessary to continue the division, let

$$\begin{aligned}
(\mathrm{C}\alpha - \mathrm{B}\beta).\alpha + (\beta^2 - \alpha\gamma).\mathrm{A} &= m\mathrm{A}^2, \\
(\mathrm{D}\alpha - \mathrm{B}\gamma).\alpha + (\beta\gamma - \alpha\delta).\mathrm{A} &= n\mathrm{A}^2, \\
(\mathrm{E}\alpha - \mathrm{B}\delta).\alpha + (\beta\delta - \alpha\varepsilon).\mathrm{A} &= p\mathrm{A}^2, \\
\&\text{c.}
\end{aligned}$$

and the 3d remainder is $[(\gamma m - \beta n).m + (n^2 - mp).\alpha]\,x^{n-4} + [(\delta m - \beta p)\,m + (np - mq)\,\alpha]\,x^{n-5} + [(\varepsilon m - \beta q)\,m + (nq - mr)\,\alpha]\,x^{n-6} +$ &c. every term of which is divisible by α^2. The law of continuation is manifest.

PROP. 2. Two roots of an equation of $2m$ dimensions may be found by the solution of an equation of $m.(2m - 1)$ dimensions.

Let $x^{2m} + px^{2m-1} + qx^{2m-2} + rx^{2m-3} +$ &c. $= 0$; and, if possible, let v and z be so assumed that $v + z$, and $-v + z$, may be 2 roots of this equation; then,

$$\left.\begin{aligned}
x^{2m} &= v^{2m} \pm 2mzv^{2m-1} + 2m.\frac{2m-1}{2}.z^2v^{m-2} \pm \&\text{c.} \\
px^{2m-1} &= \phantom{v^{2m}} \pm pv^{2m-1} + (2m-1).pzv^{2m-2} \pm \&\text{c.} \\
qx^{2m-2} &= \phantom{v^{2m} \pm 2mzv^{2m-1} + (2m-1).pz} qv^{2m-2} \pm \&\text{c.} \\
rx^{2m-3} &= \phantom{v^{2m} \pm 2mzv^{2m-1} + (2m-1).pzv^{2m-2}} \pm \&\text{c.}
\end{aligned}\right\} = 0;$$

$$\text{conseq. } v^{2m} + \left.\begin{aligned} &2m.\frac{2m-1}{2}.z^2 \\ &+ (2m-1).pz \\ &+ q \end{aligned}\right\} v^{2m-2} + \&\text{c.} \left.\begin{aligned} &+ z^{2m} \\ &+ pz^{2m-1} \\ &+ qz^{2m-2} \\ &+ rz^{2m-3} \\ &+ \&\text{c.} \end{aligned}\right\} = 0; \text{ also}$$

$$\left.\begin{matrix}2mz\\+\ \ p\end{matrix}\right\}v^{2m-1}+\left.\begin{matrix}2m\,.\,\frac{2m-1}{2}\,.\,\frac{2m-2}{3}\,.\,z^3\\+\ (2m-1)\,.\,\frac{2m-2}{2}\,.\,pz^2\\+\ (2m-2)\,.\,qz\\+\ r\end{matrix}\right\}v^{2m-3}+\&c.\left.\begin{matrix}+\ 2mz^{2m-1}\\+\ (2m-1)\,.\,pz^{2m-2}\\+\ (2m-2)\,.\,qz^{2m-3}\\+\ (2m-3)\,.\,rz^{2m-4}\\+\ \&c.\end{matrix}\right\}v=0.$$

Assume $y = v^2$; and let the co-efficients of the terms of the former equation be 1, b, c, d, &c. and of the latter A, B, C, D, &c. then the equations become

$$y^m + by^{m-1} + cy^{m-2} + dy^{m-3} + \&c. = 0,$$
$$Ay^{m-1} + By^{m-2} + Cy^{m-3} + \&c. = 0.$$

These equations have a common measure of the form $y \pm z$, where z is expressed in terms of z and known quantities; and this common measure may be found, by dividing, as in prop. 1, till y is exterminated, and making the last remainder equal to 0.

Now, the first remainder is $((cA - bB) . A + B^2 - CA)\, y^{m-2} + ((dA - bC) . A + BC - DA)\, y^{m-3} + ((eA - bD) . A + BD - EA)\, y^{m-4}$ + &c.; or, by substitution, $\alpha y^{m-2} + \beta y^{m-3} + \gamma y^{m-4}$ + &c. and, in α, z rises to 6 dimensions; in β, to 8 dimensions; and, in γ, to 10 dimensions, &c. The 2d remainder is $((C\alpha - B\beta) . \alpha + (\beta^2 - \alpha\gamma) . A)\, y^{m-3} + ((D\alpha - B\gamma) . \alpha + (\beta\gamma - \alpha\delta) . A)\, y^{m-4}$ + &c.; or, by substitution, $mA^2y^{m-3} + nA^2y^{m-4}$ + &c. and, dividing by A^2, the dimensions of z in m, are 15; in n, 17, &c. Let π, $\varkappa$, ϱ, σ, τ, &c. be the dimensions of z in the first term of the 1st, 2d, 3d, 4th, 5th, &c. remainders; then $\pi = 6$, $\varkappa = 15$, $\varrho = 2\varkappa - \pi + 4$, $\sigma = 2\varrho - \varkappa + 4$, $\tau = 2\sigma - \varrho + 4$; &c. the increment of the $m - 1$ term of this series is $4m + 1$, and therefore the $m - 1$ term itself is $2m . (m - 1) + m$, or $m . (2m - 1)$. Now, in the $m - 1$ remainder, y does not appear, and in that remainder z rises to $m . (2m - 1)$ dimensions; if then this remainder be made equal to nothing, and a value of z determined, the last divisor, $y \mp z$, where z is some function of z, is known; and this is a common measure of the two equations $y^m + by^{m-1} + cy^{m-2}$ + &c. = 0, and $Ay^{m-1} + By^{m-2} + Cy^{m-3}$ + &c. = 0; consequently, $y \mp z = 0$; and $y = \pm z$; hence $\pm \sqrt{y}$, or v, $= \pm \sqrt{\pm z}$; therefore, by the solution of an equation of $m . (2m - 1)$ dimensions, 2 roots, $z \pm \sqrt{\pm z}$, of the original equation, are discovered.

Cor. 1. Since 2 roots of the proposed equation are $z + v$, and $z - v$, $x^2 - 2zx + z^2 - v^2 = 0$ is a quadratic factor of that equation.

Cor. 2. In the same manner that the 2 equations $y^m + by^{m-1} + cy^{m-2}$ + &c. = 0, and $Ay^{m-1} + By^{m-2} + Cy^{m-3}$ + &c. = 0, are reduced to one, may any 2 equations be reduced to one, and one of the unknown quantities exterminated; also the conclusion will be obtained in the lowest terms.

PROP. 3. Every equation has as many roots, of the form $a \pm \sqrt{\pm b^2}$, as it has dimensions.—*Case* 1. Every equation of an odd number of dimensions has, at least, one possible root; and it may therefore be depressed to an equation of an even number of dimensions.—*Case* 2. If the equation be of $2m$ dimensions, and m be an odd number, then $m\,.\,(2m - 1)$ is an odd number, and consequently z and v^2 (see prop. 2) have possible values; therefore the proposed equation has a quadratic factor, $x^2 - 2zx + z^2 - v^2 = 0$, whose co-efficients are possible; that is, it has 2 roots of the specified form; and it may be reduced 2 dimensions lower.

Case 3. If m be evenly odd, or $\frac{1}{2}m$ an odd number, then the equation for determining z, has either 2 possible roots, or 2 of the form $a \pm b\sqrt{-1}$, (case 2); and v^2 will be of the form $c \pm d\sqrt{-1}$; hence one value of the quadratic factor $x^2 - 2zx + z^2 - v^2 = 0$, will be of this form, $x^2 - (2a + 2b\sqrt{-1})\,x + \mathrm{AB} + \mathrm{CD}\sqrt{-1} = 0$; and another of this form, $x^2 - (2a - 2b\sqrt{-1})\,.\,x + \mathrm{AB} - \mathrm{CD}\sqrt{-1} = 0$; consequently $x^4 - 4ax^3 + (2\mathrm{AB} + 4a^2 + 4b^2)\,x^2 - (4a\mathrm{AB} + 4b\mathrm{CD})\,x + \mathrm{A}^2\mathrm{B}^2 + \mathrm{C}^2\mathrm{D}^2 = 0$, will be a factor of the proposed equation; and this biquadratic may be resolved into 2 quadratics, whose co-efficients are possible, and whose roots are therefore of the form specified in the proposition.

In the same manner the proposition may be proved, when $\frac{1}{4}m$, $\frac{1}{8}m$, $\frac{1}{16}m$, &c. is an odd number; and thus it appears that it is true in all equations.

Cor. 1. If v^2, or y, be positive, the roots of the quadratic factor $x^2 - 2zx + z^2 - v^2 = 0$, and therefore 2 roots of the proposed equation are possible. If $y = 0$, 2 roots are equal; and if y be negative, 2 roots are impossible.

Cor. 2. If a possible value of z be determined, and substituted in b, c, d, &c. the original equation will have as many pairs of possible roots as there are changes of signs in the equation $y^m + by^{m-1} + cy^{m-2} + \text{\&c.} = 0$; and as many pairs of impossible roots as there are continuations of the same sign.

XVII. General Theorems, chiefly Porisms, in the Higher Geometry. By Henry Brougham, Jun., Esq. p. 378.

The following are a few propositions that have occurred to me, in the course of a considerable degree of attention which I have happened to bestow on that interesting, though difficult branch of speculative mathematics, the higher geometry. They are all in some degree connected; the greater part refer to the conic hyperbola, as related to a variety of other curves. Almost the whole are of that kind called porisms, whose nature and origin is now well known; and, if that mathematician to whom we owe the first distinct and popular account of this formerly mysterious, but most interesting subject,* should chance to peruse these pages, he will find in them additional proofs of the accuracy which characterizes his inquiry into the discovery of this singularly beautiful species of proposition.

Though each of the truths which I have here enunciated is of a very general and extensive nature, yet they are all discovered by the application of certain principles or properties still more general; and are thus only cases of propositions still more extensive. Into a detail of these I cannot at present enter: they compose a system of general methods, by which the discovery of propositions is effected with cer-

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* See Mr. Playfair's paper, in vol. 3 of the Edinburgh Trans.—Orig.

tainty and ease; and they are, very probably, in the doctrine of curve lines, what the ancients appear to have prized so much in plain geometry; though unfortunately all that remains to us of that treasure, is the knowledge of its high value. I have not added the demonstrations, which are all purely geometrical, granting the methods of tangents and quadratures: I have given an example, in the abridged synthesis, of what I consider as one of the most intricate. It is unnecessary to apologize any further for the conciseness of this tract. Let it be remembered, that were each proposition followed by its analysis and composition, and the corollaries, scholia, limitations, and problems, immediately suggested by it, without any trouble on the reader's part, the whole would form a large volume, in the style of the ancient geometers; containing the investigation of a series of connected truths, in one branch of the mathematics, all arising from varying the combinations of certain data enumerated in a general enunciation.*

As a collection of curious general truths, of a nature, so far as I know, hitherto quite unknown, I am persuaded that this paper, with all its defects, may not be unacceptable to those who feel pleasure in contemplating the varied and beautiful relations between abstract quantities, the wonderful and extensive analogies which every step of our progress in the higher parts of geometry opens to our view.

PROP. 1. *Porism.* Pl. 5, fig. 10.—A conic hyperbola being given, a point may be found, such, that every straight line drawn from it to the curve, shall cut, in a given ratio, that part of a straight line passing through a given point which is intercepted between a point in the curve not given, but which may be found, and the ordinate to the point where the first mentioned line meets the curve.—Let X be the point to be found, NA the line passing through the given point N, and M any point whatever in the curve; join XM, and draw the ordinate MP; then AC is to CP in a given ratio.

Corol. This property suggests a very simple and accurate method of describing a conic hyperbola, and then finding its centre, asymptotes, and axes; or, any of these being given, of finding the curve, and the remaining parts.

PROP. 2. *Porism.*—A conic hyperbola being given, a point may be found, such, that if from it there be drawn straight lines to all the intersections of the given curve, with an infinite number of parabolas, or hyperbolas, of any given order whatever, lying between straight lines, of which one passes through a given point, and the other may be found; the straight lines so drawn, from the point found, shall be tangents to the parabolas, or hyperbolas.—This is in fact 2 propositions; there being a construction for the case of parabolas, and another for that of hyperbolas.

PROP. 3. *Porism.*—If, through any point whatever of a given ellipse, a straight line be drawn parallel to the conjugate axis, and cutting the ellipse in another point; and if at the first point a perpendicular be drawn to the parallel; a point may be found, such, that if from it there be drawn straight lines, to the innumerable intersections of the ellipse with all the parabolas of orders not given, but which may

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* See the celebrated account of ancient geometrical works, in the 11th book of Pappus.—Orig.

be found, lying between the lines drawn at right angles to each other, the lines so drawn from the point found, shall be normals to the parabolas at their intersections with the ellipse.

PROP. 4. *Porism.*—A conic hyperbola being given, if through any point of it a straight line be drawn parallel to the transverse axis, and cutting the opposite hyperbolas, a point may be found, such, that if from it there be drawn straight lines, to the innumerable intersections of the given curve with all the hyperbolas of orders to be found, lying between straight lines which may be found, the straight lines so drawn shall be normals to the hyperbolas at the points of section.

Scholium. The last 2 propositions give an instance of the many curious and elegant analogies between the hyperbola and ellipse; failing however when, by equating the axes we change the ellipse into a circle.

PROP. 5. *Local theorem.* Fig. 11.—If from a given point A, a straight line DE move parallel to itself, and another CS, from a given point C, move along with it round C; and a point I move along AB, from H, the middle point of AB, with a velocity equal to half the velocity of DE; then, if AP be always taken a 3d proportional to AS and BC, and through P, with asymptotes D′E′ and AB, a conic hyperbola be described; also with focus I and axis AB, a conic parabola be described; then the radius vector from C to M, the intersection of the two curves, moving round C, shall describe a given ellipse.

PROP. 6. *Theorem.*—A common logarithmic being given, and a point without it, a parabola, hyperbola, and ellipse, may be described, which shall intersect the logarithmic and each other in the same points; the parabola shall cut the logarithmic orthogonally; and if straight lines be drawn from the given point to the common intersections of the 4 curves, these lines shall be normals to the logarithmic.

PROP. 7. *Porism.*—Two points in a circle being given, but not in one diameter, another circle may be described, such, that if from any point of it to the given points straight lines be drawn, and a line touching the given circle, the tangent shall be a mean proportional between the lines so inflected. Or, more generally, the square of the tangent shall have a given ratio to the rectangle under the inflected lines.

PROP. 8. *Porism.* Fig. 12.—Two straight lines AB, AP, not parallel, being given in position, a conic parabola MN may be found, such, that if, from any point of it M, a perpendicular MP be drawn to the one of the given lines nearest the curve, and this perpendicular be produced till it meets the other line in B; and if from B a line be drawn to a given point C; then MP shall have to PB together with CB, a given ratio.

Scholium. This is a case of a most general enunciation, which gives rise to an infinite variety of the most curious porisms.

PROP. 9. *Porism.* Fig. 13.—A conic hyperbola being given, a point may be found, from which if straight lines be drawn to the intersections of the given curve with innumerable parabolas, or hyperbolas, of any given order whatever lying between perpendiculars which meet in a given point, the lines so drawn shall cut, in a given ratio, all the areas of the parabolas or hyperbolas contained by the peripheries and co-ordinates to points of it, found by the innumerable intersections of another conic hyperbola, which may be found.—This comprehends evidently 2 propositions; one for the case of parabolas, the other for that of hyperbolas. In the former it is thus expressed with a figure. Let EM be the given hyperbola; BA, AC, the perpendiculars meeting in a given point A: a point X may be found, such, that if XM be drawn to any intersection M of EM with any parabola AMN, of any given order whatever, and lying between AB and AC, XM shall cut, in a given ratio, the area AMNP, contained by AMN and AP, PN, co-ordinates to the conic hyperbola FN, which is to be found; thus, the area ARM shall be to the area RMNP in a given ratio.

PROP. 10. *Porism.*—A conic hyperbola being given, a point may be found, such, that if from it there be drawn straight lines, to the innumerable intersections of the given curve with all the straight lines drawn through a given point in one of the given asymptotes, the first mentioned lines shall cut, in a given ratio, the areas of all the triangles whose bases and altitudes are the co-ordinates to a 2d conic hyperbola, which may be found, at the points where it cuts the lines drawn from the given point.

PROP. 11. *Porism.*—A conic hyperbola being given, a straight line may be found, such, that if another move along it in a given angle, and pass through the intersections of the curve with all the parabolas, or hyperbolas, of any given order whatever, lying between straight lines to be found, the moving line shall cut, in a given ratio, the areas of the curves described, contained by the peripheries and co-ordinates to another conic hyperbola, that may be found, at the points where this cuts the curves described.

PROP. 12. *Porism.*—A conic hyperbola being given, a straight line may be found, along which if another move in a given angle, and pass through any point whatever of the hyperbola, and if this point of section be joined with another that may be found, the moving line shall cut, in a given ratio, the triangles whose bases and altitudes are the co-ordinates to a conic hyperbola, which may be found, at the points where it meets the lines drawn from the point found.

Scholium. I proceed to give 1 or 2 examples, wherein areas are cut in a given ratio, not by straight lines, but by curves.

PROP. 13. *Porism.* Fig. 14.—A conic hyperbola being given, if through any of its innumerable intersections with all the parabolas of any order, lying between straight lines, of which one is an asymptote, and the other may be found; an hyperbola of any order be described, except the conic, from a given origin in the given asymptote perpendicular to the axis of the parabolas, the hyperbola thus described shall cut, in a given ratio, an area, of the parabolas, which may be always found.

If from G, as origin, in AB, one of LM's asymptotes, there be described an hyperbola IC′, of any order whatever, except the first, and passing through M, a point where LM cuts any of the parabolas AM, of any order whatever, drawn from A a point to be found, and lying between AB and AC, an area ACD may be always found, (that is, for every case of AM and IC′,) which shall be constantly cut by IC′, in the given ratio of M : N; that is, the area AMN : NMDC :: M : N. I omit the analysis, which leads to the following construction and composition.

Constr. Let $m + n$ be the order of the parabolas, and $p + q$ that of the hyperbolas. Find φ a 4th proportional to $m + n$, $q - p$ and $m + 2n$; divide GB in A, so that AR : AG :: $q : p + \varphi$; then find π a 4th proportional to $\text{M} + \text{N}$, $\varphi + p$, and $q - p$, and γ a 4th proportional to q, AG, and $q - p$; and, lastly, θ a 4th proportional to the parameter* of LM, π and M. If, with a parameter equal to $\frac{m + n}{m + 2n} \times \theta - \frac{\text{M} + \text{N}}{\text{M}}$ of the rectangle τ . AN; and between the asymptotes AB, AC, a conic hyperbola be described, it shall cut the parabola in a point, the coordinates to which contain an area that shall be cut by IC′ in the ratio of M : N.

Demonstration. Because AG is divided in R, so that AR : AG :: $q : p + \varphi$, and that $\varphi : m + n :: q - p : m + 2n$, AR is equal to $p + \frac{\text{AG} \times q}{\frac{(m + n \times (q - p)}{m + 2n}}$; and, because LM is a conic hyperbola, the rectangle MS . RS, or MS . AP, or AP . (MP + AR) is equal to the parameter, or constant space, therefore this parameter is equal to $\text{AP} \times (\text{MP} + p + \frac{\text{AG} . q}{\frac{(m + n) . (q - p)}{m + 2n}})$.

Again, the space ACD is equal to $\frac{m + n}{m + 2n}$ of the rectangle AC . CD, since AD is a parabola of the order $m + n$; but by construction AC . CD is equal to $\frac{m + n}{m + 2n}$ of $(\theta - \frac{\text{M} + \text{N}}{\text{M}} . \tau . \text{AN})$; therefore, $\text{ACD} = \theta - \frac{\text{M} + \text{N}}{\text{M}} . \tau . \text{AN}$, of which θ : parameter of LM :: π : M, and $\pi : \text{M} + \text{N} :: \varphi + p : q - p$; therefore $\theta = \frac{\text{Par. LM} \times (\text{M} + \text{N})}{\text{M}\,(q - p)} \times (\frac{(m + n) \times (q - p)}{m + 2n} + p\,;)$ also $\tau : q :: \text{AG} : q - p$; consequently $\text{ACD} = \frac{\text{Par. LM} \times (\text{M} + \text{N})}{\text{M}\,(q - p)}$ multiplied by $(\frac{m + n \times q - p}{m + 2n} + p)$ and diminished by $\frac{\text{M} + \text{N}}{\text{M}} \times \text{AN} \times \frac{q . \text{AG}}{q - p}$; therefore, transposing $\frac{\text{Par. LM} \times (\text{M} + \text{N})}{\text{M} \times (q - p)} \times (\frac{m + n}{m + 2n} \times \frac{q - p}{1} + p)$ is equal to $\text{ACD} + \frac{\text{M} + \text{N}}{\text{M}} \times \text{AN} \times \frac{q . \text{AG}}{q - p}$; and par. LM will be equal to $\frac{(\text{ACD} + \frac{\text{M} + \text{N}}{\text{M}} \times \text{AN} \times \frac{q . \text{AG}}{q - p}) \times \frac{\text{M}}{q - p}}{(\frac{m + n}{m + 2u} \times \frac{q - p}{1} + p) \times (\text{M} + \text{N})}$, that is, $\frac{\frac{\text{M}}{\text{M} + \text{N}} \times (q - p) \times \text{ACD} + q . \text{AN} \times \text{AG}}{\frac{m + n}{m + 2n} \times (q - p) + p}$.

Now it was before demonstrated, that the parameter of LM is equal to $\text{AP} \times (\text{MP} + p + \frac{q . \text{AG}}{\frac{(p + n) . (q - p)}{m + 2n}})$. This is therefore equal to

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* i. e. The constant rectangle or space to which AP . SM is equal.—Orig.

$\dfrac{\dfrac{\mathrm{M}}{\mathrm{M+N}} \times (q-p) \times \mathrm{ACD} + q \,.\, \mathrm{AN} \times \mathrm{AG}}{\dfrac{m+n}{m+2n} \times (q-p) + p}$, multiplying both by $\dfrac{(m+n) \times (q-p)}{m+2n} + p$, we have $\dfrac{\mathrm{M}}{\mathrm{M+N}} \times (q-p) \times \mathrm{ACD} + q \,.\, \mathrm{AN} \times \mathrm{AG} = \mathrm{AP} \times (\mathrm{MP} \times (p + (\dfrac{(m+n) \times (q-p)}{m+2n}) + q \,.\, \mathrm{AG})$.

From these equals take $q \,.\, \mathrm{AG} \times \mathrm{AN}$, and there remains $\dfrac{\mathrm{M}}{\mathrm{M+N}} \times (q-p) \times \mathrm{ACD}$ equal to $\mathrm{AP} \times \mathrm{PM} \times (\dfrac{(m+n) \times (q-p)}{m+2n} + p) + q \,.\, \mathrm{AG} \times (\mathrm{AP} - \mathrm{AN})$; or, dividing by $q-p$, $\dfrac{\mathrm{M}}{\mathrm{M+N}} \times \mathrm{ACD} = \mathrm{AP} \times (\dfrac{m+n}{m+2n}) + \dfrac{p}{q-p}) \times \mathrm{PM} + \dfrac{q}{q-p} \times \mathrm{AG} \times (\mathrm{AP} - \mathrm{AN})$. Now, $\dfrac{m+n}{m+2n} \times \mathrm{AP} \times \mathrm{PM}$ is equal to the area APM; therefore the area APM together with $\dfrac{p}{q-p} \times \mathrm{AP} \,.\, \mathrm{PM}$, and $\dfrac{q}{q-p} \times \mathrm{AG} \times (\mathrm{AP} - \mathrm{AN})$, or APM with $\dfrac{p}{q-p} \times \mathrm{AP} \,.\, \mathrm{PM} - \dfrac{q}{q-p} \times \mathrm{AG} \times (\mathrm{AN} - \mathrm{AP})$, or $\mathrm{APM} + \dfrac{q}{q-p} \times \mathrm{AP} \,.\, \mathrm{PM} - \dfrac{q}{q-p} \times$ rect. PT, is equal to $\dfrac{\mathrm{M}}{\mathrm{M+N}} \times \mathrm{ACD}$. Now IC′ is an hyperbola of the order $p+q$; therefore its area is $\dfrac{p}{p-q} \times$ rect. GH . MH. But q is greater than p; therefore $\dfrac{p}{p-q}$ is negative, and $\dfrac{p \times \mathrm{GH} \,.\, \mathrm{HM}}{q-p}$ is the area MHKC′; and the area NTKC′ is equal to $\dfrac{p}{q-p} \times \mathrm{GT} \times \mathrm{TN}$; therefore MNTH is equal to (MHKC′ − NTKC′), or to $\dfrac{p}{q-p} \times (\mathrm{GH} \,.\, \mathrm{MH} - \mathrm{GT} \,.\, \mathrm{TN})$. From these equals take the common rectangle AT, and there remains the area MPN, equal to $\dfrac{p}{q-p} \times \mathrm{AP} \times \mathrm{MP} - \dfrac{q}{q-p} \times \mathrm{PT}$; which was before demonstrated to be, together with APM, equal to $\dfrac{\mathrm{M}}{\mathrm{M+N}} \,.\, \mathrm{ACD}$. Therefore MPN, together with APM, that is, the area AMN, is equal to $\dfrac{\mathrm{M}}{\mathrm{M+N}} \,.\, \mathrm{ACD}$; consequently AMN : ACD :: M : M + N; and (dividendo) AMN : NMDC :: M : N. An area has therefore been found, which the hyperbola IC′ always cuts in a given ratio. Therefore, a conic hyperbola being given, &c. Q. E. D.

Scholium. This proposition points out, in a very striking manner, the connection between all parabolas and hyperbolas, and their common connection with the conic hyperbola. The demonstration here given is much abridged; and, to avoid circumlocution, algebraic symbols, and even ideas, have been introduced: but by attending to the several steps, any one will easily perceive that it may be translated into geometrical language, and conducted on purely geometrical principles, if any numbers be substituted for m, n, p, and q; or if these letters be made representatives of lines, and if conciseness be less rigidly studied.

PROP. 14. *Theorem.*—A common logarithmic being given; if from a given point, as origin, a parabola, or hyperbola, of any order whatever be described, cutting in a given ratio a given area of the logarithmic; the point where this curve meets the logarithmic is always situated in a conic hyperbola, which may be found.

Scholium. This proposition is, properly speaking, neither a porism, a theorem, nor a problem. It is not a theorem, because something is left to be found, or, as Pappus expresses it, there is a deficiency in the hypothesis: neither is it a porism; for the theorem, from which the deficiency distinguishes it, is not local.

PROP. 15. *Porism.* Fig. 15.—A conic hyperbola being given; 2 points may be found, from which if straight lines be inflected, to the innumerable intersections of the given curve with parabolas or hyperbolas, of any given order whatever, described between given straight lines; and if co-ordinates be drawn to the intersections of these curves with another conic hyperbola, which may be found; the lines inflected shall always cut off areas that have to one another a given ratio, from the areas contained by the co-ordinates.—Let X and Y be the points found; HD the given hyperbola, FE the one to be found; ADC one of the curves lying between AB and AG, intersecting HD and FE; join XD, YD; then the area AYD : XDCB in a given ratio.

PROP. 16. *Porism.* Fig. 16.—If between 2 straight lines making a right angle, an infinite number of parabolas of any order whatever be described; a conic parabola may be drawn, such, that if tangents be drawn to it at its intersections with the given curves, these tangents shall always cut, in a given ratio, the areas contained by the given curves, the curve found, and the axis of the given curves.—Let AMN be one of the given parabolas; DMO the parabola found, and TM its tangent at M: ATM shall have to TMR a given ratio.

PROP. 17. *Porism.*—A parabola of any order being given; 2 straight lines may be found, between which if innumerable hyperbolas of any order be described; the areas cut off by the hyperbolas and the given parabola at their intersections, shall be divided, in a given ratio, by the tangents to the given curve at the intersections; and conversely, if the hyperbolas be given, a parabola may be found, &c. &c.

PROP. 18. *Porism.*—A parabola of any order $(m + n)$ being given, another of an order $(m + 2n)$ may be found, such, that the rectangle under its ordinate and a given line, shall have always a given ratio to the area (of the given curve) whose abscissa bears to that of the curve found a given ratio.

Example. Let $m = 1$, $n = 1$, and let the given ratios be those of equality; the proposition is this; a conic parabola being given, a semi-cubic one may be found, such, that the rectangle under its ordinate and a given line, shall be always equal to the area of the given conic parabola, at equal abscissæ.

Scholium. A similar general proposition may be enunciated and exemplified, with respect to hyperbolas; and as these are only cases of a proposition applying to all curves whatever, I shall take this opportunity of introducing a very simple, and I think perfectly conclusive demonstration, of the 28th lemma, Principia, book i. "that no oval can be squared." It is well known, that the demonstration which Sir Isaac Newton gives of this lemma, is not a little intricate; and, whether from this difficulty, or from some real imperfection, or from a very natural wish not to believe that the most celebrated desideratum in geometry must for ever remain a desideratum, certain it is, that many have been inclined to call in question the conclusiveness of that proof.

Let AMC be any curve whatever, (fig. 17,) and D a given line; take in *ab* a part *ap*, having to AP a given ratio, and erect a perpendicular *pm*, such, that the rectangle *pm* . D shall have to the area APM a given ratio; it is evident that *m* will describe a curve *amc*, which can never cut the axis, unless in *a*. Now because *pm* is proportional to $\frac{\text{APM}}{\text{D}}$, or to APM, *pm* will always increase ad infinitum, if AMC is infinite; but if AMC stops or returns into itself, that is, if it is an oval, *pm* is a maximum at *b*, the point of *ab* corresponding to B in AB; consequently the curve *amc* stops short, and is irrational. Therefore *pm*, its ordinate, has not a finite relation to *ap*, its abscissa; but *ap* has a given ratio to AP; therefore *pm* has not a finite relation to AP, and APM has a given ratio to *pm*; therefore it has not a finite relation to AP, that is, APM cannot be found in finite terms of AP, or is incommensurate with AP; therefore the curve AMB cannot be squared. Now AMB is any oval; therefore no oval can be squared. By an argment of precisely the same kind, it may be proved, that the rectification, also, of every oval is impossible. Therefore, &c. Q. E. D.

I shall subjoin 3 problems, that occurred during the consideration of the foregoing propositions. The first is an example of the application of the porisms to the solution of problems. The 2d gives, besides, a new method of resolving one of the most celebrated ever proposed, Kepler's problem; and the last exhibits a curve before unknown, at least to me, as possessing the singular property of a constant tangent.

PROP. 19. *Problem.* Fig. 18.—A common logarithmic being given; to describe a conic hyperbola, such, that if from its intersection with the given curve a straight line be drawn to a given point, it shall cut a given area of the logarithmic in a given ratio. The analysis leads to this construction. Let BME be the logarithmic, G its modula; AB the ordinate at its origin A; let C be the given point; ANOB the given area; M : N the given ratio: draw BQ parallel to AN; find D a 4th proportional to M, the rectangle BQ . OQ, and M + N. From AD cut off a part AL, equal to AC together with twice G; at L make LH perpendicular to AD, and between the asymptotes AL, HL, with a parameter, or constant rectangle, twice (D + 2 . AB . G) describe a conic hyperbola: it is the curve required.

PROP. 20. *Problem.* Fig. 19.—To draw, through the focus of a given ellipse, a straight line that shall cut the area of the ellipse in a given ratio.—*Const.* Let AB be the transverse axis, EF the semi-conjugate; E, of consequence, the centre; C and L the foci. On AB describe a semicircle. Divide the quadrant AK in the given ratio in which the area is to be cut, and describe the cycloid GMR, such, that the ordinate PM may be always a 4th proportional to the arc OQ, the rectangle AB × 2 FE, and the line CL; this cycloid shall cut the ellipse in M, so that, if MC be joined, the area ACM shall be to CMB :: M : N.

Demonstr. Let $\mathrm{AP} = x$, $\mathrm{PM} = y$, $\mathrm{AC} = c$, $\mathrm{AB} = a$, and $2\mathrm{EF} = b$; then, by the nature of the cycloid GMR, $-\mathrm{PM} : \mathrm{OQ} :: 2\,\mathrm{FE} \times \mathrm{AB} : \mathrm{CL}$, and $\mathrm{QO} = \mathrm{AO} - \mathrm{AQ} =$ by const. $\frac{\mathrm{M}}{\mathrm{M}+\mathrm{N}} \times (\mathrm{AK} - \mathrm{AQ})$; also, $\mathrm{CL} = \mathrm{AB} - 2\mathrm{AC}$, since $\mathrm{AC} = \mathrm{LB}$. Therefore, $-\mathrm{PM} : \frac{\mathrm{M}}{\mathrm{M}+\mathrm{N}} \times \mathrm{AK} - \mathrm{AQ} :: \mathrm{AB} \times 2\mathrm{EF} : \mathrm{AB} - 2\mathrm{AC}$; or $-y : \frac{\mathrm{M}}{\mathrm{M}+\mathrm{N}} \times \text{arc } 90^\circ -$ arc vers. sin. $x :: ab : a - 2c$; therefore $-y\,(a - 2c)$ or $+y\,(2c - a) = ab \times (\frac{\mathrm{M}}{\mathrm{M}+\mathrm{N}} \times \text{arc}\,.\,90^\circ - \text{arc v. s. } x)$, and by transposition $ab \times \text{arc}\,.\,\text{v. s. } x + y\,(2c - a) = \frac{ab\,.\,\mathrm{M}}{\mathrm{M}+\mathrm{N}} \times \text{arc } 90^\circ$. To these equals add $2y\,(x - x) = 0$, and multiply by -1; then will $ab \times \text{arc v. s. } x + (2x - a)\,y - 2y\,(x - c) = \frac{\mathrm{M}}{\mathrm{M}+\mathrm{N}} \times ab$ arc 90°, of which the 4th parts are also equal; therefore $\frac{ab \times \text{arc v. s. } x}{4} + \frac{(2x - a)\,y}{4} - \frac{y}{2}\,(x - c) = \frac{ab}{4} \times \frac{\mathrm{M}}{\mathrm{M}+\mathrm{N}} \times \text{arc } 90^\circ$. Now because AFB is an ellipse, $y^2 = \frac{b^2}{a^2} \times (ax - x^2)$, and $y = \frac{b}{a}\,\surd(ax - x^2)$; therefore $\frac{ab \times \text{arc v. s. } x}{4} + \frac{2x - a}{4} \times \frac{b}{a}\,\surd(ax - x^2) - \frac{y}{2}\,(x - c) = \frac{ab}{4} \times \frac{\mathrm{M}}{\mathrm{M}+\mathrm{N}} \times \text{arc } 90^\circ$. Multiply both numerator and denominator of the first and last terms by a; then will $\frac{b}{a} \times \frac{a^2}{4} \times \text{arc v. s. } x + \frac{2x - a}{4} \times \frac{b}{a}\,\surd(ax - x^2) - \frac{y}{2}\,(x - c) = \frac{b}{a} \times \frac{a^2}{4} \times \frac{\mathrm{M}}{\mathrm{M}+\mathrm{N}} \times \text{arc } 90^\circ$. Now the fluxion of an arc whose versed sine is x and radius $\frac{a}{2}$, is equal to $\frac{a\dot{x}}{2\,\surd(ax - x^2)}$, which is also the fluxion of the arc whose sine is $\surd\frac{x}{a}$ and radius unity; therefore $\frac{b}{a} \times (\frac{a^2}{4} \times \text{arc sin } \surd\frac{x}{a} + \frac{2x - a}{4} \times \surd(ax - x^2)) - \frac{y}{2}\,(x - c)$ is equal to $\frac{b}{a} \times \frac{a}{4} \times \frac{\mathrm{M}}{\mathrm{M}+\mathrm{N}} \times \text{arc } 90^\circ$; and, by the quadrature of the circle, $\frac{a^2}{4} \times \text{arc sin. } \surd\frac{x}{a} + \frac{2x - a}{4} \times \surd(ax - x^2)$, is the area whose abscissa is x; consequently the semicircle's area is $\frac{a^2}{4} \times \text{arc } 90^\circ$. But the areas of ellipses are to the corresponding areas of the circles described on their transverse axes, as the conjugate to the transverse; therefore $\frac{b}{a} \times (\frac{a^2}{4} \times \text{arc sin. } \surd\frac{x}{a} + \frac{2x - a}{4} \times \surd(ax - x^2))$ is the area whose abscissa is x, of a semi-ellipse, whose axes are a and b; and consequently $\frac{b}{a} \times \frac{a^2}{4} \times \text{arc } 90^\circ$ is the area of the semi-ellipse. Therefore the area $\mathrm{APM} - \frac{y}{2}\,(x - c)$ is equal to $\frac{\mathrm{M}}{\mathrm{M}+\mathrm{N}}$ of AMFB. But $\frac{y}{2}\,(x - c) = \frac{\mathrm{PM}}{2} \times (\mathrm{AP} - \mathrm{AC}) = \frac{\mathrm{PM}^2}{2} \times \mathrm{PC}$, is the triangle CPM; consequently, APM − CPM, or ACM, is equal to $\frac{\mathrm{M}}{\mathrm{M}+\mathrm{N}} \times \mathrm{AMFB}$; and $\mathrm{ACM} : \mathrm{AMFB} :: \mathrm{M} : \mathrm{M} + \mathrm{N}$; or (dividendo) $\mathrm{ACM} : \mathrm{CMFB} :: \mathrm{M} : \mathrm{N}$; and the area of the ellipse is cut in a given ratio by the line drawn through the focus. Q. E. D.

Of this solution it may be remarked, that it does not assume as a postulate the description of the cycloid; but gives a simple construction of that curve, flowing from a curious property, by which it is related to a given circle. This cycloid too gives, by its intersection with the ellipse, the point required, directly, and not by a subsequent construction, as Sir I. Newton's does. I was induced to give the demonstration, from a conviction that it is a good instance of the superiority of modern over ancient analysis; and in itself perhaps no inelegant specimen of algebraic demonstration.

PROP. 21. *Problem.* Fig. 20.—To find the curve whose tangent is always of the same magnitude.

Analysis. Let MN be the curve required, AB the given axis, SM a tangent at any point M, and let d be the given magnitude; then, SM . q . = SP . q . + PM . q. = d^2; or, $y^2 + \frac{\dot{y}^2 \dot{x}^2}{\dot{y}^2} = d^2$, and $\frac{\dot{x}^2}{\dot{y}^2} = \frac{d^2 - y^2}{y^2}$; therefore, $\dot{x} = \frac{\dot{y}}{y} \times \surd(d^2 - y^2)$. In order to integrate this equation, divide $\frac{\dot{y}}{y} \surd(d^2 - y^2)$ into its 2 parts, $\frac{d^2\dot{y}}{y\surd(d^2 - y^2)}$ and $\frac{-y\dot{y}}{\surd(d^2 - y^2)}$; to find the fluent of the former,

$$\frac{d^2\dot{y}}{y\surd(d^2 - y^2)} = \frac{d^2\dot{y}}{y} \times \frac{(1 + \frac{d}{\surd(d^2 - y^2)})}{d + \surd(d^2 - y^2)} = -d \times \frac{(-\frac{d\dot{y}}{y^2} - \frac{d^2\dot{y}}{y^2\surd(d^2 - y^2)})}{\frac{d + \surd(d^2 - y^2)}{y}}$$

$$= -\frac{d \times \text{fluxion of } \frac{d + \surd(d^2 - y^2)}{y}}{\frac{d + \surd(d^2 - y^2)}{y}};$$

therefore the fluent of $\frac{d^2\dot{y}}{y\surd(d^2 - y^2)}$ is $-d \times$ hyp. log. $\frac{d + \surd(d^2 - y^2)}{y}$, and the fluent of the other part, $\frac{-y\dot{y}}{\surd(d^2 - y^2)}$, is $+ \surd(d^2 - y^2)$; therefore the fluent of the aggregate $\frac{\dot{y}}{y} \surd(d^2 - y^2)$, is $\surd(d^2 - y^2) - d \times$ h. l. $\frac{d + \surd(d^2 - y^2)}{y}$, or $\surd(d^2 - y^2) + d \times$ h. l. $\frac{y}{d + \surd(d^2 - y^2)}$; a final equation to the curve required. Q. E. I.

I shall throw together, in a few corollaries, the most remarkable things that have occurred to me concerning this curve.

Corol. 1. The subtangent of this curve is $\surd(d^2 - y^2)$.

Corol. 2. In order to draw a tangent to it, from a given point without it; from this point as pole, with radius equal to d, and the curve's axis as directrix, describe a concoid of Nicomedes: to its interesections with the given curve draw straight lines from the given point; these will touch the curve.

Corol. 3. This curve may be described, organically, by drawing one end of a given flexible line or thread along a straight line, while the other end is urged by a weight towards the same straight line. It is consequently the curve of traction to a straight line.

Corol. 4. In order to describe this curve from its equation; change the one given above, by transferring the axes of its co-ordinates: it becomes (y being $=$ P′M and $x =$ AP′), $y = \sqrt{(d^2 - x^2)} + d \times \text{h. l.} \frac{x}{d + \sqrt{(d^2 - x^2)}}$; which may be used with ease, by changing the hyperbolic into the tabular logarithm. Thus then, the common logarithmic has its subtangent constant; the conic parabola, its subnormal; the circle, its normal; and the curve which I have described in this proposition, its tangent.

XVIII. Observations of the Diurnal Variation of the Magnetic Needle, in the Island of St. Helena; with a Continuation of the Observations at Fort Marlborough, in the Island of Sumatra. By John Macdonald, Esq. p. 397.

A short residence at St. Helena, arising from the sudden departure of the fleet to which the ship I was in belonged, has prevented the observations from being so numerous as I could wish. Their agreement however indicates that 58 observations are sufficient for affording such conclusions as philosophy may draw; and tends to confirm some inferences stated in a former paper, containing similar observations taken in the East Indies. By adding the mean of the morning and afternoon observations, at St. Helena, and taking the half, the general variation, in the month of November, 1796, appears to have been 15° 48′ 34½″ west: and by subtracting the medium diurnal afternoon variation, from the medium diurnal morning, the vibrating variation proves to be 3′ 55″. It appears that the magnetic needle is stationary from about 6 o'clock in the evening till 6 in the morning; when it commences moving, and the west variation increases, till it amounts to its maximum, about 8 o'clock; diminishing afterwards till it becomes stationary. Here the same cause seems to operate as at Bencoolen, with a modification of effect proportioned to the relative situation of the southern magnetic poles, and the places of observation. At the apartments of the R. S., this species of variation is found to increase, from 7 o'clock in the morning till 2 in the afternoon. If the variation is east, in the northern hemisphere in the East Indies, I conceive that the diurnal variation will increase towards the afternoon, remain some time stationary, and diminish before the succeeding morning: if the general variation is west in that quarter, the reverse may be the case. The quantity of the diurnal variation is greater in Britain than at St. Helena, or at Bencoolen. This will naturally arise from this country's being more contiguous to its affecting poles, than those islands, situated near the equator. It were to be wished, that observations were taken in as many situations as possible, similarly situated in the opposite hemispheres, on the lines of no variation. A greater degree of dip might be found, and conclusions might be deduced, that would tend considerably to illustrate this curious and interesting subject, as yet involved in conjecture and uncertainty. I frequently, while at Bencoolen, observed that the needle did not retain the same level, but was sometimes depressed, and sometimes elevated, 6 or 8 minutes. I paid little attention to this, ascribing it to a minute alteration in the position of the point of the socket over the pivot. I observed sometimes a similar difference of level in the position of the needle at St. Helena, without being able to account for it. It may be possible, that the dip of the needle is subject to a diurnal variation in its vertical movement. I have perused such publications as have appeared on magnetism for some time past: they state no theory of this obscure science, more rational, or satisfactory, than that left us by the celebrated Halley.

XIX. On the Corundum Stone from Asia. By the Rt. Hon. Cha. Greville, F. R. S. p. 403.

Having contributed to bring into notice the mineral substance from the East Indies which is generally called Adamantine spar, I beg leave to lay before the R. S. the following account of its history and introduction. About the year 1767 or 1768, Mr. Wm. Berry, an eminent engraver of stone, at Edinburgh, received from Dr. Anderson, of Madras, a box of crystals, with information of their being the material used in India, to polish all gems but diamonds. Mr. Berry found that they cut agate, cornelian, &c.; but in his minute engraving of figures, on seals, &c. the superior hardness of the diamond appeared preferable; and its dispatch compensated for the price: the crystals were therefore laid aside, as curiosities. Dr. Black ascertained their being different from other stones observed in Europe, and their hardness attached to them the name of Adamantine spar. Col. Cathcart sent me its native name, corundum, from India, with some specimens, given to him by Dr. Anderson, in 1784, which I distributed for analysis.

When the native name was obtained, it appeared, from Dr. Woodward's Catalogue of Foreign Fossils, published about 1719, that the same substance had been sent to him from Madras, by his correspondent Mr. Bulkley. In his first Cat. of Foreign Fossils, p. 6, ζ 17; "Nella Corivindum is found in fields where the rice grows: it is commonly thrown up by field rats, and used, as we do emery, to polish iron." Page 11, λ 13; "Tella Convindum, Fort St. George, Mr. Bulkley. 'Tis a talky spar, grey, with a cast of green: it is used to polish rubies and diamonds." In Dr. Woodward's additional Cat. of Foreign Fossils, published in 1725, p. 6, ζ 10: "Nella Corivendum is found by digging at the foot or bottom of hills, about 500 miles to the southward of this place. They use it as emery, to clean arms, &c. it serves also to grind rubies, by making it like hard cement, by the help of stick-lac mixed with it. East India. Mr. Bulkley."—These, with a few others in Woodward's Catalogues, are the only instances by which any author, prior to 1768, appears to have noticed this substance.

This information being unsatisfactory, and every appearance of the stone indicating it to be part of a stratum, I wrote repeatedly to friends in India, to ascertain if possible, the situation of the rock, and, if near the sea, to send a considerable quantity, as ballast, with a view of applying it to cut and polish granites, porphyry, and other stones, which the high price of cutting and polishing excluded from useful or ornamental work. But my inquiries at Madras were fruitless: by some I was assured it came from Guzarat. From Bombay I obtained no satisfactory information. At last, in 1793, I obtained a satisfactory account. Sir Chas. Oakley was disposed to oblige me: he was then Governor of Madras; and his success is due to the activity and judgment of Mr. Garrow. Mr. Garrow knew how difficult it was to avoid the causes of my failure, from every Hindoo being occupied by the duty of his cast; scarcely thinking on any thing else, and whenever his interest is concerned, being suspicious and reserved. Mr. Garrow, in the first place, ascertained the cast connected with corundum to be the venders of glass bangles; that they used it in their business, and sold it to all other casts. This cast of natives, at all times, had free access to every part of Tippoo's country; nor, until the districts about Permetty were ceded to the English, could it be procured in any other way. Mr. Garrow depended on his personal inspection; the particulars are contained in the following letter, communicated to me by Sir Chas. Oakley.

Sir Charles Oakley, Bart., Tritchinopoly, 10*th Nov.* 1792.

Sir,—" I derived so little satisfaction from the various accounts given me of the corundum, from the indifference of the natives to every subject in which they are not immediately interested, that I resolved to ascertain the particulars I wished to know, on the spot where the stone is found. The glassmen agreed in one material circumstance, that the place was not far from Permetty: in other particulars they disagreed, apparently with intention to mislead. It is near a fortnight since I dispatched a servant I could depend on to Permetty, with one of these people, who, on his arrival there, probably through fear of his cast, said he knew no further. My servant persevered, and informed me he had found the place I wished to see. I arrived at Permetty, by the route of Namcul, the 6th; and learning that the distance to the spot was about 3½ hours or 14 miles, I left Permetty in time to arrive there about sunrise the next morning. At this time no person but my servant was present, and from a continued excavation at different depths, from 6 to 16 feet, in appearance like a water-course, running in length about a mile and a half E. and W., over the brow of a very rising ground, I saw at once the place from which the stone was procured. The prodigious extent that at different times appears to have been dug up, with the few people employed, shows that it has been a business of ages.

" The ground through which the vein of excavation runs, and of course the mineral, commands one of the finest and most extensive prospects it is possible to conceive. The surface of the ground is covered with innumerable fine alabaster stones, and a variety of small shrubs, but not a tree sufficient to shelter my palanquin. There is not the appearance of an habitation within ¾ of a mile. The nearest village is called Condrastra Pollam. In this village are about 30 small thatched houses: among these are 5 families, who, in descent by prescriptive right, are the miners, and dig in the pits. The nearest place of any consequence, in Rennell's map, is Caranel, on the south side the Cavery. The distance of the pits from the river is above 4 miles; but the ground between prevent its being seen in a direct line. A fine view of the river is seen near Erode; which fort, as well as Sankerdroog, are plainly visible with the naked eye, as is also the Coimbitoor country, south and west of the river, to an immense extent.

" I procured, at Permetty, a cadjan from the Bramin manager to the head man of the pollam; which, on my arrival at the pits, I sent to him; and soon after 3 of the miners came from the pollam, with their implements, and families following with provisions. As they came up, they enquired of my servant how they were to address me, having never seen a European before. I followed them into a pit, in the line of the excavation, above 14 feet from the ground-level. The instrument they used is a heavy iron crow, ending in a broad point, with a straight wooden handle, clampt with iron. The soil they cut through is of different colours, but composed chiefly of a gritty granite; and at the depth of 7 feet are layers of a substance not unlike dried pitch, which crumbles into small flakes when taken out. With considerable labour, the miners, with the points of their crows, cut out several pieces of the strata, of some pounds weight each; and when a considerable quantity was broken off, it was carried up and crushed to pieces, with great force, by the iron crow. Among these broken lumps, the corundum stone is found; but in many of the pieces there was none. The mode of getting it made it difficult to get any with the stratum adhering to it; this however, after several trials, I obtained very perfect, and shall forward to Madras, with specimens of the strata at different depths. The stone is beyond all comparison heavier than the substance which encrusts it.

" It appears extraordinary how this stone, so concealed, should under such difficulties have been sought for, and applied to any purpose; and that the knowledge of the few people who dig for it, and who do so from father to son, is confined entirely to the finding the stone. For they told me they knew none of its uses, and that the labour was so hard, and their gain so small, that they would, through choice, rather work in the fields; that the sale of it from the spot is confined solely to the glass sellers, who vend it over the whole country, and who had, while I was there, above 40 parriar horses, bullocks, &c. ready in the pollam, to carry it to Tinnevelly, and the southern countries; through which track, if the stone is known in Europe, I apprehend it has found its way by means of the Dutch. The people on the spot declare it is to be got in no other situation or place whatever; and the stone-cutters tell me they can do nothing without it. It pays no duty, either where dug up or retailed. The colour of the stone is either very light brown or purplish, in the proportion of 20 to 1 of the latter; but in use no preference is given, and they are used equally. To an indifferent person, the most striking circumstance is its great weight.

" As the spot I have been speaking of now composes a part of the Company's territories, the most minute information on the subject may be acquired. I felt particular satisfaction in being the first European who was ever at the place; and I shall be much gratified if the account given meets with your approbation. I shall dispatch a load of the stone, in a day or 2, which I got at the pollam, with the charge of it. The distance from this place, by Namcul, is 84 miles.

"9 Tritchinopoly measures of the corundum stone weigh 50lb.

	P.	F.	C.
1½ Madras fanams per measure *	0	13	40
Cooley from thence to Tritchinopoly	0	28	40
Ditto from Tritchinopoly	1	13	40
Pagodas	2	10	40

"The stone is delivered by measures, and paid for at the pollam, in the gold fanam.

"I am, &c. EDWARD GARROW."

This letter contains very interesting topographical observations on the mine. The specimens sent were of one sort, of a greyish colour, with a shade of green. The entire crystals, which I selected among the broken ones, were of course few in proportion; but, with the addition of some distinct crystals, which Col. Cathcart and Capt. Colin Macauley had sent me, have been sufficient to ascertain the structure and form of the crystals, of which an analytical description will close this paper. I shall therefore now say nothing concerning their form, but proceed to give an account of the varieties of corundum stone, which I have obtained from India and China.

In the year 1786, Col. Cathcart sent me a small fragment of a stratified mass from Bengal, with this label; "Corundum, much inferior in price to that of the Coast." It is of a purplish hue; its fracture like compact sand-stone; and a confused crystallization appears in all parts of the stone, by fibres of a whiter colour, from which the light is reflected, as in feld-spar, &c. I have since obtained a larger lump of the stone, of the same texture, but rather paler in its purplish hue. Sir John Macgregor Murray informed me that it is called by the natives of Bengal, corone, and used for polishing stones, and for all the purposes of emery. Its specific gravity is 3.876.

Capt. Colin Macauley procured a lump of corundum from a sikuldar, (a polisher, a term most appropriate to polishers of steel,) in whose family it had been above 20 years employed, for grinding and polishing stones or gems. The use to which it had been so long devoted had occasioned grooves in its surfaces, which facilitated greatly the examination of its structure. It is about 5½ inches long, 3½ inches broad, and above 2 inches thick. On one of its broad surfaces are 2 oval grooves; one of them is 4 inches long, 1 broad, and ¾ of an inch deep. On the opposite side is a shorter oval groove, above 2½ inches long, 1½ inch broad, and 1 inch deep. In these grooves, the ends of the laminæ of the mass reflect the light, like the crystals. It serves as a specimen of the simple apparatus of an Indian lapidary. Stones polished in these grooves would be of the common

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* The above is the prime cost. I have been informed by correspondents, who purchased some in retail, that it was sold for about 6s. a pound, at Madras.—Orig.

India polish and form, en cabochon, which is often called tallow-drop, from the French lapidaries' term goutte de suif, convex, oval, or circular. A very small quantity of the corundum powder would be required, as the action of the powdered corundum and gems, on the lump of corundum, would, as appears from the depth of the grooves, wear away from it a supply of powder, for the operation of polishing. It appears to be a part of a larger mass, is of a purplish colour, and of the same laminated texture as the crystals of corundum; it has this peculiarity, there appear cracks, branching irregularly across the laminæ of the lump, which are filled with homogeneous matter, distinguished however by the superior purity which might be expected to arise from the degree of filtration required for its deposition in the fissures. Some of these cracks, which terminate on the surface, appear to have the same crystallized arrangement which characterizes the laminæ of corundum. The cracks not being in any degree influenced in their direction by the laminæ of the crystallized mass, it is probable they had not been consolidated, when they cracked: and, from this specimen, we may expect to find corundum cementing masses of stone, by the same process of stalactitical cementation by which quartz and calcedony connect great nodules and masses of siliceous stones.

In this specimen, I consider the veins as pure corundum, that is, having the same specific gravity, hardness, and texture as corundum crystals; and I found the whole lump possessed all the qualities of corundum, except its specific gravity, which amounted only to 2.785; and in this property it corresponded nearly with the matrix of the corundum crystals, or the vein in which corundum is before stated to be found; the specific gravity of which is 2.768. The texture of the matrix appears sometimes like adularia, and confusedly crystallized; often compact, like cipoline or primitive marble; sometimes sparry, sometimes granulated, and, on the outside of the vein, and near fissures, decomposed, and becoming opaque. In all its states, it scratches glass, but not rock crystal, possibly from want of adherence of its particles; and in this it differs from the substance of the above lump, which cuts glass and rock crystal with great facility. This lump, and the matrix of corundum, appeared to possess the same properties as corundum, when examined by the blow-pipe, with the different fluxes.

The matrix of corundum having sometimes an appearance like adularia and feldspar, I ascertained, by Mr. Hatchett's scales, the specific gravity of adularia to be 2.558, and of feldspar 2.555. The corundum, and the lighter corundum of the lump, cut adularia and feldspar; the latter effervesced, and combined with soda, which the former did not. It is therefore evident, that the matrix of corundum, or substance of the vein, is a distinct substance from adularia and feldspar, and nearly connected with corundum. The matrix or vein contains also a black substance, like shorl, which, on closer observation, appears to be hornblende. This substance Mr. Garrow had remarked to have the appearance of charcoal, and on that account he had attributed the formation of these strata to the agency of fire.

Other gentlemen, from the appearance of the matrix of corundum, have stated it to be a calcareous vein.

Mr. Garrow observed, that there ran through the strata in which the corundum was found, veins of a substance like dried pitch, apparently on their edge, which separated like a pack of cards. It is a brown micaceous substance, which in drying foliates, and shows a certain degree of regular arrangement of the component parts; in this case, the fragments of the folia subdivide, with some degree of regularity, into rhombs, whose angles are 60° and 120°: it is more smooth, and less flexible, than pure mica. These are all the sorts of corundum which I procured from India. I now proceed to the result of my inquiries in China.

I requested Capt. Cumming, in 1786, at that time commanding the Company's ship Britannia, to take a specimen of corundum to China, to ascertain its nature, and to obtain specimens, if possible, adhering to their matrix, and regularly crystallized. On his arrival at Canton, he collected the information I wished, with the good sense and zealous desire which he always exerts for his friends. He ascertained that the stone I inquired for, was in common use with the stone-cutters; and he brought me the stone, in its rude and in its pounded state, taking care to select the most regularly crystallized pieces, and others adhering to the rock. A stone-cutter was sawing rock crystal with a hand-saw, which he also brought to me; it is a piece of bamboo, slit, about 3 feet long, and 1¾ inch broad, thickened at the handle by a piece of wood, rivetted with 2 iron pins; having a lump of lead tied with a thong of split rattan, steadying an iron pin, on which the ends of a twisted iron wire is fastened, which, being stretched to the handle, is passed through a hole in the bamboo, with the superabundant wire; a wooden peg, being pressed into the hole, keeps the bow bent, and the wire stretched, and serves to coil the superfluous wire, till, by sawing the crystal, the stretched wire is worn, and requires to be renewed from the coil. The twisted wire answers the purpose of a saw, and retains the powder of corundum and water, which are used in this operation. Dr. Lind had before brought specimens similar to the above, from China.

From Sir Jos. Banks I obtained Dr. Lind's specimens, and some in powder, which Mr. Duncan, supercargo in China, had sent him, with the Chinese name, pou-sa. The matrix, being mixed with a red and white sparry substance and mica, is generally called red granite: but it appears to me of the same nature as the matrix of corundum from India. The white is more fibrous, and like cyanite; the red part of it is compact and opaque; other parts appear to foliate, and pure mica is in considerable patches, and generally adheres to the crystals. This corundum is of a darker brown, and is more irregular on the surface than the corundum of the coast, and is often mixed with black iron ore,* attractable by the magnet. It is

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* A small group, consisting of 3 or 4 octoedral crystals, presents the least common variety of this kind of iron ore: the edges of the octoedra being replaced by planes which almost cover the triangular planes. Romé de l'Isle. Cristallog. vol. 4, pl. 4, fig. 69.—Orig.

described as the 3d modification of the corundum crystal, in the analytical description which follows. The chatoyant or play of light, on these dark crystals, is very remarkable: some are of a bright copper colour; others exhibit the accident of reflection of light, which, in a polished state, gives varieties to the cat's eye, star-stone, sun-stone, &c.; which, as yet, are classed from such accident, without strict attention to their nature, which is various, and in general has not been ascertained.

These are the circumstances connected with the strata worth mentioning. The examination of corundum on which our present knowledge rests, is nearly that which an Indian mineralogist might derive of the history of feldspar, from a lump of Aberdeen granite, out of 1 or 2 different quarries. He might ascertain a few modifications of the crystal of feldspar, its fracture and matrix; but he would have no knowledge of the purest or more beautiful sorts which other quarries produce, in Scotland, at Baveno, at St. Gothard, and in Auvergne. I therefore think it essential to mention, that corundum, under circumstances favourable to its crystallization, becomes glassy in its fracture, and of various colours. I have not only observed, in crystals of corundum, specks of a fine ruby colour; but I have fragments of crystals, in texture and every respect like the colourless corundum, of a fine red colour. It is certain that we obtain from India, corundum which may pass for rubies. I have sent to India some of the corundum with small ruby specks, which were not sufficiently distinct or large either for measurement or analysis, in hopes of being enabled to ascertain correctly the form of Salam rubies found in corundum; in the mean time, I have the corundum of a fine red colour. Looking over some polished rubies from India, I selected one which appeared laminated like corundum, and had also the chatoyant or play of light on its laminæ, which formed an angle in the stone. The lapidary called it an oriental ruby. I altered the form of the cutting, so fortunately, that the reflected rays formed a perfect star; a phenomenon I had observed in the sapphire, and expected in corundum, but not in the octoedral ruby. The specific gravity of this stone, being 4.166, confirmed my opinion that it is one of the Salam rubies, so much esteemed by the natives on the coast or peninsula of India, which are found in the corundum vein. The specific gravity of a colourless sapphire, very little less opaque than corundum, forming also a perfect star, was 4.000: that of a deep blue sapphire, and of a star-stone, 4.035; all which I connect with the corundum; the specific gravity of a distinct crystal of which was 3.950; of a fragment of ruby-coloured corundum, 3.959; and of a fragment of corundum with vitreous lustre 3.954.

It may be objected, that Bergman has stated the variety of specific gravity in gems to be so great, as to leave no certain rule of judging by it of the species. He observed, that the topaz generally prevails in weight, being from 3.460 to 4.560; the ruby from 3.180 to 4.240; then the sapphire, from 3.650 to 3.940.* But in the preceding page he had said, "Analysi crystallorum, tam ejusdem quam diversæ

* De Terra Gemmarum. Bergm. Opusc. vol. 2, p. 104.—Orig.

figuræ, multum lucis scientia expectat. Illæ quarum antea compositionem explorare licuit, naturali forma per artem privatæ erant." It is not therefore an hypothesis unworthy of examination which I advance, that gems derived from the rectangled octoedra, whose specific gravity is above 3.300 to 3.800, will be found to be diamonds or octoedral rubies; and these will be easily distinguished from each other, by their lustre and hardness. Diamonds, whether red, yellow, blue, or white, being hardest, though their specific gravity will be less; viz. from 3.356 to 3.471, as I found among different diamonds in my collection: whereas the octoedral ruby was from 3.571 to 3.625, and inferior in hardness, not only to the diamond, but to the corundum; the specific gravity of which, in its different appearance of form and colour, I found to vary from 3.876 to 4.166; and I suppose it to be subject to a variation from 3.300 to 4.300: after which, the jargon will come, with a specific gravity of 4.600; easily distinguished also, by its crystallization, from the above-mentioned gems. The above specific gravities, Mr. Hatchett very obligingly assisted me in taking, with his accurate scales, in the temperature of 60°. It will not be understood that I depend entirely on the specific gravity; on the contrary, I connect this quality with crystallization: hardness is the next criterion; and analysis must separate the component parts, and demonstrate the analogy or identity of substances, or of compounds. The improvements of Mr. Klaproth's process are evident, by the comparison of his first analysis, and his last analysis, of corundum. In the first it consisted of

Corundum earth	68. 0
Siliceous earth	31.50
Iron and nickel	0.50
	100

By the last analysis of Klaproth, the corundum of the peninsula of India consisted of

Argillaceous earth	89.50
Siliceous earth	5.50
Oxide of iron	1.25
Loss	3.75
	100

The Corundum of China.

Argillaceous earth	84. 0
Siliceous earth	6.50
Oxide of iron	7.50
Loss	2. 0
	100

That the analysis of sapphire by Mr. Klaproth may be compared, it is here added.

Agillaceous earth	98.50
Calx of iron	1. 0
Calcareous earth	0.50
	100

Iron ore crystallized is often mixed with the Chinese corundum, as I have before stated, and may be considered as accidentally interposed, not combined. In the corundum of the coast, the greenish colour may indicate the combination of iron, as the blue colour does in the sapphire; and the proportion of iron in both is nearly alike. There then is the $\frac{5.50}{100}$ and $\frac{6.50}{100}$ of silex in corundum, evidently an integral part of the coarse corundum crystal, and not of the sapphire; but it will require an analysis of the vitreous or pellucid corundum, to decide that silex is a constituent part of corundum: there will then remain to account for the calcareous earth; and, having established its being a constituent part of the sapphire, the small proportion of $\frac{0.5}{100}$, cannot be expected to produce a very notable difference. It is not necessary to do more than thus to hint at what further analysis and examination of former experiments are required, to ascertain the analogy or identity of the sapphire and oriental ruby with corundum.

I have before stated, that I have corundum (which has the same texture and fracture as the common colourless corundum) of a ruby red, and also of sapphire blue, and of sapphire blue and white colours. I have sapphires, yellow and blue, white and blue, brown and greenish, and of a purplish hue; these I should consider as corundum, with fracture of vitreous lustre. Mr. Tranckell, who resides in Ceylon, and from whose communications I derived lately much information, had, about 5 years ago, a sapphire, the greater part blue, and the remainder of a pale ruby colour. I saw, in Romé de l'Isle's collection, at Paris, a small gem, which was yellow, blue, and red, in distinct spots, and he called it oriental ruby. M. de la Metherie, to avoid the confusion of the denomination oriental ruby with octoedral ruby, calls it a sapphire; with more correctness, I think, the above-mentioned gems should be classed as argillaceous, under the denomination of corundum.

I am not uninformed that corundum is said to be found in France. The Count de Bourbon is convinced, that the specimens mentioned in Crell's Journal, as having been found by him in a granite in the Forez, were corundum. M. Morveau also says, he found it in Bretagne; but the Abbé Hauy, in the Journal de Mines, N° 28, asserts, that the corundum found in France is titanite: he does not say whether this observation extends both to the corundum of Bretagne and that of the Forez. In the same manner I had observed, in the specimens which Mr. Raspe called jade, or a new substance, from Tiree, on the west coast of Scotland, a great resemblance to corundum; but having then only had a cursory view of the substance, I am indebted to Mr. Hatchett for the examination of a specimen of it which he had from Mr. Raspe's collection.

The Tiree stone resembles crystallized corundum of the coast, in texture and colour; it is also as refractory, when examined by the blow-pipe, with different fluxes. Its specific gravity is 3.049; consequently nearer the specific gravity of pure corundum than the above-mentioned lump, 2.785, and the matrix of corundum, 2.768. The Tiree stone will scratch glass readily, but not rock crystal; its hardness therefore corresponds with that of the matrix of corundum. The substance of the lump before described in page 359, cuts glass, and rock crystal, and the Tiree stone, readily. It will therefore be sufficient for me to say, that there is great probability corundum may be found in Great Britain, and on the continent of Europe, as well as in Asia; and the above slight assays may show, that observations on corundum, in its different states of purity, may lead to accurate distinction between substances hitherto imperfectly known, and will lead to a revision of the siliceous genus, by which the argillaceous genus may obtain its due pre-eminence in mineralogy.

When gems, by art, or by rolling in the beds of rivers, have been deprived of the angles of their crystals, they are unavoidably subjected to uncertain external characters, which even great practice cannot render certain; and hence the unwillingness of European jewellers to deal in coloured gems. I have some specimens of a sapphire-blue stone, India cut, very small and pellucid; they were purchased in India, as sapphires, and were supposed to be fluor by a lapidary in London, but are cyanite. The above could scarcely have happened, if the stones had been of sufficient size and value to require much examination, the weight and degree of hardness being exceedingly deficient. The colour therefore will not be a safe guide. The diamond, whether white, blue, red, yellow, or green, can be distinguished by its crystal, or by its specific gravity and hardness, or, when it is polished, by its lustre. Other stones which compose the order of gems, might equally depend on their crystallization, specific gravity, polish, and hardness, for a distinct arrangement. The near relation of argil, which Bergman gave to this order, is daily confirmed; and it will perhaps be to Mr. Klaproth, more than to any other existing chemist, that we shall owe our correct information on the subject of other gems, as we do on the subject of corundum.

Many of the varieties of corundum, particularly the coloured and transparent sorts, with their regular crystallizations, are yet desiderata. Many crystallized stones, from defect of colour, lustre, &c. are of little value in the market, such as, jargon, chrysolite, tourmaline; and an infinity of unnamed stones of Ceylon, Pegu, Siam, &c. would be valuable to the mineralogist, if obtained adhering to their strata, and in crystals whose external form is not obliterated. I have no doubt, when it is known how much such information will tend to illustrate the history of the earth, and particularly that of gems, that the spirit of inquiry, so laudably afloat in British India, will be directed to attain it. I have not heard of any metallic veins being found in corundum, unless a stone which Alonso Barba, lib. i, c. 13, describes should give an instance. "The chumpi, so called from its grey colour, is a stone of the nature of emery, and contains iron; it is of a dull lustre, difficult to work, because it resists fire long. It is found at Potosi, at Chocaya, and other places, with the minerals negrillos and rosicleres."

Having mentioned the varieties of crystallized and amorphous corundum, and the miscellaneous facts relative to my collection of that substance from India and China, it might be sufficient to give an icon of the crystal, and close a paper already prolix; but, having with satisfaction observed, within the last years, the science of mineralogy gaining ground in Great Britain, from the knowledge acquired by several gentlemen who have examined the mines, and formed personal acquaintance with the most experienced and learned men on the continent, and also from ingenious foreigners, who have communicated their observations on English fossils, and connected them with the most approved systems, it may perhaps be accepted as a sufficient apology for what follows, that I consider it as a desideratum to English mineralogists, to be invited to a preference of permanent characters, which the study of crystallization has collected, and which promises to be a certain method of ascertaining the laws by which elective attraction arranges and combines molecules of matter.

It is true, the progress of crystallography has been extremely slow, and different nations have contributed to its present improvement. It is rather remarkable, that the earliest treatise on metallurgy, of authority, was published in Italy, by Vanoccius Biringuccius, just before Agricola published his treatise in 1546 in Germany; and the first treatise on the structure of crystals I know of, is also from Italy, by Nicolas Steno, Prodromus Dissertationis de Solido intra Solidum naturaliter contento. Florentiæ, 1669, in 4to: a work of great merit. Louis Bourguet of Neufchatel, in his Lettres sur la Formation des Sels et des Crystaux; Amst. 1729, 12°, connected, by observation and measure, triangular and rhomboidal, and cubic and pyramidal tetraedal molecules, for all different substances. His contemporary, Maurice Antoine Capeller,* attempted to deduce a system from geometrical principles; and in this state did Linnæus find the subject, when he attempted to reduce the science of minerals to external characters, and crystallized bodies to salts.

None of the observations of Linnæus will prove useless to science; but his system alarmed the chemists and mineralogists, who rejected every other criterion than internal character from analysis, and the system of Cronstedt was preferred by general assent. By this means, a spirit of controversy deprived the chemist and lythologist of mutual assistance; and the general opinion was correct, on the supposition that a mixed system of chemical and external characters would be irreconcileable; but it has been admitted, even by those who most decidedly opposed Linnæus's system, that the best system of mineralogy should be founded on external and internal characters combined†. Among the few who ventured to profess their obligations, at the same time, to Linnæus and to Cronstedt, was Baron Born, whose abilities and character, in addition to his distinction as one of the counsellors of mines of his Imperial Majesty, obtained his enrollment among the Fellows of the R. S. He connected the intrinsic and extrinsic characters of minerals, in the Index Fossilium, which he published in 1772. In Sweden, Bergman's treatise on the forms of crystals, published in the Upsal Transactions, in 1773, was a more authoritative recommendation to the investigation of the principles of crystallization; and it can be of little importance for me to add, that since I have possessed the collection of Baron Born, in 1773, I have had every confirmation of the same opinion. The progress of chemistry and of crystallography, applied to mineralogy, has rendered the examination of strata, and of mines, a source of amusement as well as instruction; and the arrangement of interesting facts, in the chemistry and mechanism of nature, suits my occasional researches in geology, which, from variety of

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* Prodr. Crystallograp. &c. and Litteræ ad Scheuzerum, de Cryst. Generatione. Act. Nat. Cur. vol. 4, Append. p. 9.——† Nullum itaque est dubium, quin hujusmodi methodus mixta, quæ notis characteristicis tam extrinsecis quam intrinsecis simul combinatis, est superstructa, proxime ad naturalem accedens, maximam indicans symmetriam, reliquis sit præferenda methodis. J. G. Wallerius, de Syst. Min. rite condendo, §. 102.—Orig.

avocations and circumstances, have been very much interrupted. My acknowledgment of obligation to the learned who have made this progress in science, is the best recommendation I can give to others to examine their works. Those whose talents and time are devoted to the investigation of every mineral substance, can have no respite to their labour; minerals, in every state of their formation, perfection, and decomposition, as they occur in mines, must have their qualities immediately ascertained, and be reserved for profit, or thrown away on the heap. The practical miner could not, without external characters, make any progress. The valuable minerals are soon pointed out by assay, and their appearance remembered. The accuracy of selection depended, in all periods, much on the experience of the miners. It remained for Mr. Werner to give the utmost degree of accuracy which the irregular external characters can acquire, by fixing appropriate terms to all the characters which occur, and which the senses can discriminate. In 1774, he opened his system of external characters of minerals, and the perfection he has since given to it, has rendered it very general. The Leskean collection, arranged after Mr. Werner's method, has procured, in Mr. Kirwan, a powerful support to the introduction of that system in this country; and we have already some other valuable publications, to recommend and introduce other favourite systems of the Continent. It is therefore at this time the English mineralogist should be invited to examine, if not to prefer, permanent characters, so far as the progress of crystallography has collected them, or at least to give them a distinguished rank among external characters of bodies.

If prejudice too long has retarded the union of intrinsic and extrinsic characters, it has also occasioned a schism among the advocates of crystallography. Romé de l'Isle, in the year 1772, published the first edition of his Essay on Crystallography, which he states to be a supplement to Linnæus; and, by the assistance of a very few friends, he was enabled to increase the number of crystals in a degree to assume the appearance of a system. He told me, that the accuracy of his measurement of angles of minute crystals was the acquirement of great practice, but that the Count de Bourbon, after a short practice, attained equal correctness, and afforded him assistance, which he acknowledges in his 2d edition to have received, particularly by the discovery of crystals in Dauphiné, Auvergne, Franche-Comté, &c.

The Abbé Hauy, an accurate and patient observer, and a good mathematician, considered crystallography as founded on certain laws, reducible to demonstration by calculation. In the beginning, the differences of Bourguet and Capeller were not more pointed than those of Romé de l'Isle and the Abbé Hauy; but the progress of observation and calculation having demonstrated their mutual utility, the observer and measurer of crystals will now rest satisfied only when calculation confirms actual measurement. To the Abbé Hauy is also due a late scheme to simplify calculation, by expressing, according to algebraical formulæ, the different laws which determine the modifications of crystals. So far as they are the result of calculation and measurement, we may admit the laws of crystallization; for whenever the superposition, or subtraction, of simple or compound molecules on a nucleus, shall by calculation give a series of planes and angles, which corresponds exactly to the angles and planes measured on natural crystals, it will amount to no more nor less than a demonstration of the rule or arrangement of elective attraction by figures. These laws may be reduced to simple practice; for instance, the Abbé Hauy, by measuring the rhombic plane of corundum, found its 2 diagonals to be as 2 to 3; which gives to its acute angle 81° 47′ 10″, and to its obtuse angle 98° 12′ 50″; the same as martial vitriol*. The forms of fragments in corundum are all acute rhomboids. The cosine of the little angle in corundum is $\frac{1}{7}$ of the radius; but in calcareous spar the cosine is $\frac{1}{5}$ of the radius; in short $\frac{2}{5}$ of the radius; in the garnet $\frac{1}{3}$; and in rock crystal $\frac{1}{17}$.

Thus the application of general laws, to ascertain constant character, after they shall have been fully verified, may be very simple and general. It will not require perfect crystals; for when crystals separate into laminæ, which subdivide into fragments, and show the form or arrangement of their molecules, it is easy, from such fragments, to connect them with primitive crystal, and consequently with their class. It will be a great step, to obtain one regular and permanent external character. Attention to other characters will be necessary to ascertain the nature of the substance; and other external characters, such as irregular fracture, colour, &c. must be resorted to, where no permanent characters exist; but from their nature they are fallible, and in fact are seldom conclusive.

The progress of crystallography appearing to me of consequence to the progress of mineralogy, induced me to desire the Count de Bourbon, above-mentioned, one of the honourable victims to his allegiance to his King, to describe such crystals, in my collection, as showed the different known modifications of corundum; which will develope the theory of crystallization, so far as is consistent with the avowed object of this paper. The subject, I believe, has not hitherto been submitted to the consideration of the R. S. The translation of the Count de Bourbon's description has been carefully made to preserve its clearness, and I hope it will be favourably received by the Society, and make some amends for my tedious introduction. After it, I have added a table, connecting in one view the specific gravities of corundum, &c. herein mentioned, with those given by other authors.

An Analytical Description of the Crystalline Forms of Corundum, from the East Indies, and from China. By the Count de Bourbon.

The most usual form of corundum is a regular hexaedral prism, (pl. 6, fig. 1;) in general, the surface of the crystal is rough, with little lustre, owing to unfavourable circumstances under which it crystallized. The crystals of corundum hitherto found were not formed in cavities, where each crystal being insulated, its surface

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* This result is extracted from the Journ. de Phys.; but it appears, from the Journ. des Mines, N° 28, that the Abbé Hauy has since rectified this measure, and given 86° 26′ for the acute angle, and 93° 34′ for the obtuse angle.—Orig.

could preserve that smoothness and natural brilliancy which are common to all substances that freely assume a crystalline form. Like the crystals of feldspar which we meet with in the porphyroid granites, the corundum crystals have been enveloped, at the time of their crystallization, by the substance of the rock which was forming, at the same time with themselves, in an imperfect and confused crystalline mass; and the corundum crystal, before it had acquired its perfect solidity, necessarily received on its surface the impression of the different particles of the rock which enveloped them; this naturally renders the surface rough and dull. Crystals of feldspar found in the granitic porphyroid rocks, exhibit the same kind of appearance, from the same cause.

The corundum crystals are in general opaque, or at least they have only an imperfect transparency at the edges: when broken into thin fragments, the pieces are semi-transparent: when held between the eye and the light, and examined with a powerful lens, it will be perceived that their interior texture is rendered dull by an infinite number of small flaws crossing each other, much resembling the medullary part of wood, when viewed in the same manner. The degree of transparency of the small interstices which are between these flaws, is further evidence that this texture of small flaws occasions opacity, which augments in proportion to the thickness of the fragments. This kind of internal structure has also a very strong analogy with that of feldspar in granite and porphyry. The endeavour to split these crystals, in a direction either perpendicular or parallel to their axes, meets with a very considerable resistance: they may indeed be broken in these directions; but the rugged and irregular surface of the broken parts, clearly proves that the direction in which the crystalline laminæ have been deposited on each other, has not been followed. The regular hexaedral prism of these crystals, cannot therefore be considered as the form of the nucleus of the crystal; and consequently is not the primitive form of the crystals of this substance.

If, in order to discover the direction of the crystalline laminæ, a variety of crystals be examined, some will hardly fail to be met with, which, on their solid angles, formed by the junction of the sides of the prism with the planes of the extremities, present small isosceles triangles. These are sometimes greater and sometimes smaller, and form solid angles of 122° 34′, with the extreme planes of the crystal. They are in some instances real faces of the crystal; but most frequently they evidently are the effect of some violence on that part. The smoothness and brilliancy of these small faces, in the latter case, show that a piece has been detached in the natural direction of the crystalline laminæ. It is indeed much less difficult to separate a portion of the crystal at these angles, than at any other part; and, in following the natural direction of the faces, with a little patience and dexterity, all the crystalline laminæ may be detached, and progressively increase the size of the triangular face. This operation however cannot be done indiscriminately on all the solid angles of the crystals, but only on the alternate ones at the same extremity, and in a contrary direction to each other. As to the other angles, they may be broken, but it is impossible to detach them. When, instead of the solid angles of a hexaedral prism, small triangular planes are met with, (which frequently happens, whether caused by violence or otherwise,) they are always placed in the direction above-mentioned.

If, by following this indication of nature, we continue to detach the crystalline laminæ, we shall at last cause the form of the hexaedral prism to disappear totally, and, instead of it, a rhomboidal parallelopiped will be obtained, (fig. 2,) of which the plane angles at the rhombs will be 86° and 94°; the solid angles at the summit* will measure 84° 31′; and that taken at the re-union of the bases will be 95° 29′. We can split this parallelopiped only in a direction parallel to its faces; it will still consequently preserve the same form, which is that of the nucleus of this substance, and its primitive form.

It is, therefore, by a modification of the rhomboidical parallelopiped, (fig. 2,) that nature has formed the regular hexaedral prism (fig. 1,) which this substance presents. For if we conceive, that in any period whatever of the increase of the rhomboidal parallelopiped, a series of laminæ or crystalline plates has been deposited on all sides of the parallelopiped; and that these laminæ have all undergone a progressive decrease of 1 row of crystalline molecules, at the acute angle which tends to form the summit, and also along the sides of the opposite acute angle, (fig. 3 and 4,) there will necessarily result from the continuation of this superposition, to a certain period, an hexaedral prism, terminated by 2 triedral pyramids, placed in a contrary direction; and their planes or faces, which form a solid angle of 147° 26′, with the sides of the prism, will be either pentagonal, (fig. 3,) or triangular, (fig. 4.) They will also have, instead of a summit, an equilateral triangular plane, sometimes greater and sometimes smaller.

If the superposition continues, the equilateral triangular plane on the summit will become nonagonal, and there will remain no other traces of the primitive planes of the rhomboidal parallelopiped, than small isoceles triangular planes, (fig. 5:) if the superposition still continues, till the last crystalline lamina is reduced to a single molecule or point, no appearance of the rhomboidal parallelopiped will then remain; and the crystal resulting from this operation of nature will be a regular hexaedral prism, (fig. 1.) In the same manner, viz. by a decrease on the lower edges of the laminæ, the primitive rhomboidal parallelopiped of calcareous spar passes to a regular hexaedral prism of that substance; though more frequently it does so by a decrease on the lower angles of the laminæ.

When the laminæ of the corundum crystal have, during their superposition on the planes of the primitive rhomboidal parallelopiped, experienced a progressive decrease at 1 of their acute angles, and along the sides of the other, at the same

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* For greater clearness, this rhomboidal parallelopiped may be considered as being formed by the junction of 2 triedral pyramids, base to base; and the 2 solid angles (each of which is formed by the re-union of 3 of the acute angles on the planes of the rhomb) will then be considered as the summits of these pyramids.—Orig.

time, and in the same proportion, it is easy to conceive that the height of the hexaedral prism must be the same as that of the rhomboidal parallelopiped, on which it has been formed. The height BC (fig. 1,) must therefore bear the same proportion to the line AB, drawn through the middle of the 2 opposite sides of the planes on the extremities, as the whole height EF, of the rhomboidal parallelopiped, (fig. 2,) bears to the small diagonal GH, from one of the rhombs; that is, nearly as 6.45 to 5.

But though this exact proportion appears in a very great number of corundum crystals, yet we meet with some whose lengths are more or less considerable; and this is owing to different circumstances which have existed at the time of their crystallization. We may conceive, for instance, that if, before the progressive decrease of the crystalline laminæ, in the manner above-mentioned, the increase of the rhomboidal parallelopiped had taken place by a superposition of laminæ, in which the rows of crystalline molecules experienced a progressive decrease along the edges of the acute angle of the base only, (fig. 6,) and that, the sides of the prism having already acquired a certain length, the succeeding crystalline laminæ had experienced a decrease at the acute angle of the summit, the same regular hexaedral prism would have resulted from this process; but the proportion between the height and the line drawn from 2 of the opposite sides of the planes on the extremities, would have been much greater than that of 6.45 to 5; and consequently this prism would have been longer than that of the rhomboidal parallelopiped which served as its nucleus.

On the other hand, if the increase of the rhomboidal parallelopiped had taken place by a superposition of crystalline laminæ, decreasing at the acute angle of the summit, and some time after decreasing also along the sides of the acute angle of the base, (fig. 7,) the regular hexaedral prism resulting from this process would have been shorter, in proportion to the duration of the mode of decrease in the crystalline laminæ which were first deposited. There are some of the hexaedral prisms, in corundum crystals, which are so short, that they appear no more than segments. Calcareous spar offers the same phenomenon; as do likewise all the substances in which the hexaedral prism has any analogy of formation with that which we have here described.

It happens frequently, when the superposition of the crystalline laminæ does not go on equally on all the faces of the rhomboidal parallelopiped, that 1 or 2 only of the solid angles of the hexaedral prism, taken alternately, still show, by small isosceles triangular planes, some remains of the faces of the parallelopiped, while the others do not show any at all. Mr. Greville, in his collection of this substance, has a crystal of corundum on one side of which, only 2 of the planes of the rhomb have experienced an equal and perfect superposition, while there has been but a very small number of crystalline laminæ deposited on the 3d plane. Consequently, this crystal presents a regular hexaedral prism, one of whose solid angles is so much truncated, that the half of the plane of the end of the hexaedral prism disappears, (fig. 8;) and this cut or section forms an angle of 122° 34′, with the plane on the extremity. It is unnecessary to observe, that the regularity of the hexaedral prism, depends on that of the rhomboidal parallelopiped on which it is formed.

When, by detaching the laminæ from the alternate solid angles of the regular hexaedral prism, the planes resulting from this operation begin to run into each other, and the crystal begins to assume the form of the rhomboidal parallelopiped to which it owes its origin, we frequently see the surface of these new planes divided into an immense number of small rhombs, formed on them by the intersection of lines that are parallel to the sides, which belong to the rhomboidal form of the new faces: (fig. 9.) These lines are owing to the extremities of the laminæ which have been deposited on the inferior faces, corresponding with those on which we observe them; and they serve to corroborate still further, the demonstration we have given of the formation of the regular hexaedral prism in this substance. We frequently see small rhombs traced on the surface of the planes, on the ends of the hexaedral prism; (fig. 10.) This, no doubt, is occasioned also by the intersection of the laminæ, on the planes of the primitive rhomboidal parallelopiped. But these rhombs, formed by the re-union of lines that join in angles of 60° and 120°, instead of 86° and 94°, (like those we have seen traced on the faces which correspond with those of the rhomboidal parallelopiped,) form angles of 60° and 120°. It would therefore be an error to consider them as indications of the form of the elements of crystallization, as we are tempted to do from a simple inspection of the crystal. These same lines form equilateral triangles with each other, as may be seen in fig. 10.

The cause of these small equilateral triangles, which sometimes project a little over the planes on the ends of the prism, must now be obvious. If, during the superposition of the crystalline laminæ on all the planes of the rhomboidal parallelopiped, it has happened, from any cause whatever, that the laminæ deposited on the 3 faces of the same summit, have not fallen exactly on those which preceded them, or that they have experienced some deviation, or have not had the same decrease as all the others, at the angle of 86°, these triangles must necessarily occur; in the same manner it must be obvious why these small equilateral triangular projections are frequently placed on one of the sides of the crystal. The primitive form of the corundum crystal is therefore a rhomboidal parallelopiped, whose solid angle at the summit is 84° 31′, and that formed by the re-union of the bases is 95° 29′. The crystalline laminæ are rhombs of 86° and 94°: these, in my opinion, are double crystalline molecules; the single molecules I apprehend to be isosceles triangles, of 86°, at the angle of the summit, and of 47° at those of the base.* Though the rhomboidal parallelopiped of 86° and 94° is the primitive

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* I am at present preparing a work, in which I shall, if circumstances permit me to finish it, give the result of my observations, and my own opinion on this interesting part of mineralogy. I shall only observe here, that though double molecules, square and rhomboidal, are frequently formed in the

form of the corundum crystal, yet it is rare to meet with that substance under this perfect and determined form; and, in most mineral substances, it is more rare to meet with their primitive crystals than their different modifications. Among Mr. Greville's numerous specimens of corundum, I have met with only one which has this primitive form, and it is doubtful whether even this may not be a fragment.

The corundum crystal presents another modification, under which the regular hexaedral prism, instead of having 3 alternate solid angles at each of its ends, (on which solid angles are placed isosceles triangular planes, forming a solid angle of 122° 34′, with the planes at the extremities on which they are inclined), has also its angles supplied by isosceles triangular planes; but these planes, instead of 122° 34′, form solid angles of 160° 42′, with the said planes on the extremities. (See fig. 11 and 12). These new planes, which constitute a new modification of the primitive form of corundum, are the result of a different order in the decrease of the laminæ; which, in the primitive form, are deposited on the planes of its primitive rhomboid by single rows of crystalline molecules, and increase the planes which terminate the hexaedron: whereas, in this 2d modification, the decrease of molecules is by 2 rows, which gives a more obtuse inclination, and forms new planes. The surface is usually striated, parallel to the sides of the planes which terminate this crystal; an appearance always announcing imperfection in the crystallization, arising either from a change in the order of decrease or increase, or from a less perfect union of the crystalline laminæ. A section would show gradual risings or steps, as appears in fig. 14, which is a section of fig. 13, in the line ADB. These striæ are not to be confounded with those in numberless substances, as in tourmalines, schorls, &c. which arise from the longitudinal union of numberless distinct crystals. The crystal resulting from this new mode of decrease in the crystalline laminæ, will represent one or other of the varieties shown in fig. 11, 12, and 13, according to the period when such decrease has begun in the process of the crystallization; and, if it has begun very late, the new faces will only be small, nay almost imperceptible isosceles triangles, forming solid angles of 160° 42′, with the planes of the extremities of the prism, as in fig. 5; the measure of the angles however must be excepted.

If this regular mode of decrease had begun with the first crystalline laminæ which were deposited on the primitive rhomboidal parallelopiped, the hexaedral prism thence resulting would have been terminated by 2 very obtuse triedral pyramids, whose planes would have been rhombs; and they would have been placed in a contrary direction to each other, as may be seen in fig. 12, by the dotted lines. I have not met with this variety, but its existence may be supposed. It happens

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process of crystallization, yet the real form of the crystalline molecules seems to be triangular. By observing the progress of the rhomboidal parallelopiped, in its passage to the form of an hexaedral prism, (fig. 4 and 5), and by considering the prism terminated, it seems evident, that the last lamina which had been deposited, after the progressive decrease in the rows of crystalline molecules to one single molecule, must necessarily have been triangular.—Orig.

sometimes, that the crystallization has not been so perfect as to destroy every appearance of the faces of the primitive rhomboidal parallelopiped; in this case, there remains on the solid angle of 112°, formed by the junction of the new faces with the edges of the prism, a small isosceles triangle, as in fig. 13, which corresponds to those in fig. 5 of the preceding modification.

The crystals which explained the 2d modification, form also a part of Mr. Greville's collection: one in particular is highly worthy of notice; it is the most perfect crystal I have ever seen of this substance. The surface of the faces of the prism, though rough, is infinitely less so than that of the others, and much more brilliant. The planes on the ends have the usual polish of crystals; its colour is a pale red, and its transparency may be compared to that of wax. This substance presents a 3d modification, in which the hexaedral prism diminishes in diameter, as is apparent by comparing the diameters of its 2 ends; in some, it appears like a regular hexaedral pyramid truncated, as fig. 15. The crystals of this modification are usually irregular, and seldom admit of a certain measure of their angles; but among the numerous specimens in Mr. Greville's collection, I have been able to ascertain, in the greater part, that the hexagonal plane at the top forms angles of about 120° with the planes of the pyramid; and the hexagonal plane at the base forms angles of about 78° with the planes of the pyramid. In other instances, the form of the pyramid varies greatly; in some, the angle at the upper plane was 110°, and the angle at the base about 70°; in others, the angle at the upper plane was about 100°, and the one at the lower plane about 80°.

In these 3 varieties, the crystalline laminæ can be separated, as in the hexagonal prism, at the 3 solid alternate angles of each end, but in a contrary direction to each other. The planes which appear when the laminæ are detached regularly, form solid angles of 22° 34′, with the planes of the extremity: this arrangement is analogous to that of the hexaedral prism. The difference of form arises from the crystalline laminæ deposited on the planes of the primitive rhomboid, decreasing by more than one row of molecules, on the planes of one of the triedral pyramids of the rhomboid, and by less than one row, on the planes of its other pyramid. This general observation, on the manner in which this primitive crystal of corundum passes to the different varieties just mentioned, is the only one I have established with any great degree of certainty at present. Specimens with perfect crystals, whose angles may be measured with accuracy, will probably arrive from India, and give further demonstration, as to these and other varieties of modifications of corundum. We may conceive, that if, in this modification, the crystallization had ceased before the entire formation of the crystal, there would have remained small isosceles triangular planes, on 3 of the alternate solid angles, formed by the junction of the planes on the ends, with the edges of the truncated pyramid. These isosceles triangular planes resemble those we have seen in the first modification; (fig. 4 and 5), and form, in the same manner, solid angles of 122° 34′, with the planes on the ends of the prism. (Fig. 16).

Finally, if, during the formation of the crystal in this modification, it should happen that the laminæ deposited on the three planes of the rhomboidal parallelopiped, on the side where they undergo a greater decrease, do not undergo the decrease of one row of molecules at the acute angle of the summit, the crystal will be a real hexaedral pyramid, (fig. 17), whose acute angle at the summit, measured on the sides, will be nearly 24°, in one of the varieties; 40° for the most obtuse; and 20° for the most acute variety: the angle of their triangular planes, in the first instance, 13° 41′; in the 2d, 22° 20′; and 11° 28′ in the 3d. I have not seen any perfect pyramids; but in many the hexagonal plane terminating the pyramid is so small, that it renders its total suppression probable. This decrease necessarily produces a single pyramid, as above-mentioned; yet there are instances of crystals of corundum, belonging to the variety where the terminal planes make, with the planes of the pyramid, a solid angle of about 100°, in which, 2 pyramids of the same dimensions, having their summit replaced by a small hexagonal plane, are placed base to base. I have also observed, among the crystals of the obtuse variety above-mentioned, in Mr. Greville's collection, an instance of the decrease taking place by several rows, on 1 three-sided pyramid of the primitive rhomboid, and by single rows on the other. Consequently, the crystal is a short regular hexaedral prism, terminating on one end only by an hexaedral pyramid; the planes of which, as well as of the prism, are alternately broad and narrow, and almost perfect; its apex being replaced by a very small plane.

I shall conclude, by mentioning a variety of corundum, described by the Abbé Hauy, in the Journ. des Mines, N° 28; in which, the edges of the terminal planes of the hexaedral prism are replaced by planes which form an angle of 116° 31′, with the terminal planes; but, in the numerous collection of Mr. Greville, I have not seen this variety. One crystal had an appearance of such planes; but on examination it was clearly accidental. The authority of Hauy, in crystallography, is so great, that the existence of such modification ought not to be denied, without further examination; though I cannot in this instance adopt it: he derives this variety, which he calls subpyramidal, from a decrease of 3 rows of molecules, at the angles of the base of the 2 pyramids of the primitive rhomboid; and he seems to attribute the same formation to the pyramidal variety with double pyramid, which he supposes may exist. The primitive crystals, and the 1st and 2d modifications of corundum, are from the Peninsula of India. The 3d modification, or the pyramidal variety, is from China; nothing approaching this form being among the specimens which Mr. Greville received from the Peninsula of India.

The preceding observations, and particularly the last-mentioned modification of corundum, compared with the best descriptions of the sapphire, suggest the further examination of the degree of connection, if not of identity, of these oriental stones. In both, the hexaedral pyramids are usually incomplete in their apex, and they vary in acuteness. I have stated the degree in which the solid angles of the pyramid, taken as complete, vary in corundum, to be from 20° to 40°.

Romé de l'Isle states, that the sapphire varies from 20° to 30°. The Abbé Hauy (Journ. de Phys. Aug. 1793), mentions 2 varieties of the sapphire, one measuring at the solid angle of the pyramid 40° 6′, the other 57° 24′. I never saw a sapphire with so obtuse an angle as the last; but many, whose angle at the top, if the pyramid had been complete, would have been the same as that of the corundum. Besides the analogy between the crystals of corundum, and the sapphire, by the union of 2 hexaedral pyramids at their base, it also exists by the measure of their angles; and both substances are subject to the same irregularity, sometimes appearing as a single hexaedral pyramid, and sometimes as an hexaedral prism; moreover, the sapphire sometimes has on its solid angles, alternately, the same triangular planes, (fig. 5), and also the prominent triangles on the planes of the extremities, (fig. 10), which often appear in the crystals of corundum. The Abbé Hauy, in the Journ. de Phys., Aug., 1793, names this variety, Orientale Enneagone, which is represented fig. 18, and says, that the small triangular planes make, with the terminal planes, an angle of 122° 18′; and, in the description of the same triangular planes in the corundum, fig. 16, it appears, that these planes are the remains of the planes of the primitive rhomboid, and form, with the terminal planes, an angle of 122° 34′.

Perhaps the rhomboidal crystal, which Romé de l'Isle had given as 1 of the forms of the sapphire, should be restored to it. He had examined it at M. Jacquemin's, jeweller to the crown, (Cristallogr. 1. edit. p. 221,) and he suppressed it in his 2d edition, but often expressed to me his regret in having made the alteration. I have before me a letter from that celebrated naturalist, dated Sept. 1784, in which he inclosed, for my opinion, a copy of a letter he had received from Mr. Werner, with models of some crystals; among them, 2 called by him rubies; one a rhomboid, of which the angles of the summit are substituted by planes, (fig. 19), the other is precisely the same as fig. 3, 4, and 5, of the annexed plate 6.

The following is a translation of Romé de l'Isle's words: "The first of these rubies has exactly the same form as I have represented in plate 4, fig. 60, of my Cristallographie, viz. a rhomboidal parallelopiped, truncated at each of its obtuse angles, by an equilateral triangular plane. You will have a correct idea of the other crystal, if you suppose the crystal represented in pl. 4, fig. 87, truncated at each of the summits of its pyramids, by an equilateral triangular plane, as in the preceding modification, but deeper, and in so great a degree, that the 3 rhombic planes of each pyramid disappear, with the exception of 3 isosceles triangles; this modification differs from the first, only by the hexaedral prism, and the deeper truncature at the summits of the pyramids." It is therefore clear, that if the primitive rhomboid of corundum decreased only at the superior angles of its laminæ, it would exhibit exactly the first of these varieties of Mr. Werner's ruby, as in the annexed fig. 19.

As to the 2d variety of Mr. Werner's ruby, it is equally clear, if in fig. 87, referred to by Romé de l'Isle, represented by our fig. 20, no more of the pyramid was left than the 3 small triangles b, a, c, there would be precisely one of the forms of corundum before described, to which the annexed figure 5 belongs. It may perhaps be objected, that the laminæ appear to be parallel to the terminal planes, in the sapphire, and inclined, in the corundum. There are crystals of corundum, in which, very frequently, the laminæ appear parallel to the terminal plane; I was at first, and for some time, deceived by that appearance. In other corundum crystals, the laminæ appear to be parallel to the prismatic planes; and, to conclude the instances of analogy, the superposition of rhomboidal laminæ is sometimes observable in oriental rubies and sapphires. It was by this appearance, Mr. Greville was led to try the effect of cutting the forementioned stones en cabochon; by which a similar effect of triple reflexion which formed stars of 6 rays from a common centre, was produced in the oriental ruby, in the sapphire, and in the corundum.

Table of the Specific Gravity of the Corundum, Sapphire, Topaz, Ruby, and Diamond, on different Authorities.

Corundum.

Hatchett and Greville ...	2.768	*Matrix of corund. Coast.
H. and G.	2.785	*Lump of corund. Coast.
Klaproth	3.075	
Klaproth	3.710	
Blumenbach ..	3.808	
Brisson	3.873	
Hatchett & G.	3.876	*Corone. Bengal.
Lichtenberg ..	3.908	
Gross	3.935	
Hatchett & G.	3.950	*Crystal of corund. Coast.
H. and G. ...	3.954	*Ditto with vitreous cross fracture. Coast.
H. and G. ...	3.959	*Ruby-coloured. Coast.
H. and G.	3.959	*Chatoyant. China.
H. and G.	3.962	*Crystal. China.
Klaproth	4.180	

Sapphire.

Brisson	3.130	Brasilian; probably a topaz.
Bergman	3.650†	
Quist	3.800†	
Bergman.....	3.940†	
Klaproth	3.950†	
Bergman	3.974†	
Brisson	3.991†	White oriental.
Brisson	3.994†	
Blumenbach ..	3.994†	Blue.
Hatchett and G.	4.000†	*Greyish star-stone.
Werner	4.000†	
Blumenbach ..	4.010†	
Hatchett and G.	4.035†	Blue star-stone.
Brisson	4.076†	From Puy-en-Velay.
Blumenbach ..	4.083†	Crimson.
Hatchett and G.	4.083†	*Pale blue crystal.
Muschenbroeck	4.090†	
Blumenbach ..	4.100†	Yellow.
La Metherie ..	2.200†	
Quist	4.200†	

Topaz.

La Metherie ..	2.690	Siberia.
Bergman	3.460	
Werner	3.464	Light blue. Brazil.
Quist	3.500	
Werner	3.521	Eibenstocker.
Brisson	3.531	Red. Brazil.
Brisson	3.536	Dark yellow. Brazil.
Werner	3.540	Dark yellow. Brazil.
Brisson	3.548	Oriental.
Werner	3.556	Schneckensteiner.
Brisson	3.564	Schneckensteiner.
Brisson	4.010†	Oriental
Bergman	4.560†	

Ruby.

Bergman	3.180	
Muschenbroeck	3.180	
Quist	3.400	Spinel.
Blumenbach ..	3.454	Ceylon.

Ruby.

Quist	3.500	Brazil.
Brisson	3.531	Brazil.
Klaproth	3.570	
Hatchett and G.	3.571	*Octoedral crystal.
H. and G.	3.625	*Macle of octoedral cryst.
Blumenbach	3.645	
Blumenbach	3.760	
Brisson	3.760	
Hatchett and G.	4.166	Salam ruby. Coast. Star-stone.
Quist	4.200†	
Bergman	4.240†	

Diamond.

Hatchett and G.	3.356	Perfect crystal.
Wallerius	3.400	
Hatchett and G.	3.471	Aggregate crystal.
Cronstedt	3.500	
Muschenbroeck	3.518	
La Metherie	3.520	
Brisson	3.521	
Werner	3.600	

The mark * distinguishes the specimens in my collection, to which I have referred in the foregoing paper. The mark † distinguishes the stones which, from their specific gravity, I think belong to the genus of corundum. The generic name corundum, I am in the habit of giving to those sorts which have a sparry or a granulated fracture. When corundum has a vitreous cross fracture, I call it sapphire; and distinguish its varieties by their colours, white, red, blue, yellow, green; and by the accidental reflection of light from their laminæ: when in one direction, I call the sapphire chatoyant; when the reflection is compounded of rays which intersect each other, and appear to diverge from a common centre, I call them star-stones, as red, blue, or greyish star-stones, or star-sapphires.

XX. On the Chemical Properties that have been attributed to Light. By Benjamin Count of Rumford, F. R. S., M. R. I. A. p. 449.

In the 2d part of my 7th essay, on the propagation of heat in fluids, I have mentioned the reasons which had induced me to doubt of the existence of those chemical properties in light that have been attributed to it, and to conclude, that all those visible changes produced in bodies by exposure to the action of the sun's rays, are effected, not by any chemical combination of the matter of light with such bodies, but merely by the heat which is generated, or excited, by the light that is absorbed by them. As the decision of this question is a matter of great importance to the advancement of science, and particularly to chemistry, and as the subject is in many respects curious and interesting, it has often employed my thoughts in my leisure hours; and I have spent much time in endeavouring to contrive experiments, from the unequivocal results of which the truth might be made to appear. Though I have not been so successful in these investigations as I could wish, yet I cannot help flattering myself, that an account of the results of some of my late experiments will be thought sufficiently interesting to merit the attention of the R. S.

Having found that gold, or silver, might be melted by the heat, invisible to the sight, which exists in the air, at the distance of more than an inch above the point of the flame of a wax-candle, (see my 7th Essay, part 2, page 350,) I was curious to know what effect this heat woud produce on the oxides of those metals.

Exper. 1. Having evaporated to dryness a solution of fine gold in aqua regia, I dissolved the residuum, in just as much distilled water as was necessary in order that the solution, which was of a beautiful yellow colour, might not be disposed to crystallize; and, wetting the middle of a piece of white taffeta ribband, $1\frac{1}{2}$ inch wide, and about 8 inches long, in this solution, I held the ribband, with both my hands, stretched horizontally over the clear bright flame of a wax candle; the under side of the ribband being kept at the distance of about $1\frac{1}{2}$ inch above the point of the flame. The result of this experiment was very striking. That part of the ribband which was directly over the point of the flame, began almost immediately to emit steam in dense clouds; and in about 10 seconds a circular spot, about $\frac{3}{4}$ of an inch in diameter, having become nearly dry, a spot of a very fine purple colour, approaching to crimson, suddenly made its appearance in the middle of it, and, spreading rapidly on all sides, became, in 1 or 2 seconds more, nearly an inch in diameter. By moving the ribband, so as to bring, in their turns, all the parts of it which had been wetted with the solution to be exposed to the action of the current of hot vapour that arose from the burning candle, all those parts which had been so wetted, were tinged with the same beautiful purple colour.

This colour, which was uncommonly brilliant, passed quite through the ribband, and I found the stain to be perfectly indelible. I endeavoured to wash it out; but nothing I applied to it, and among other things I tried super-oxygenated marine acid, appeared in the smallest degree to diminish its lustre. The hue was not uniform, but varied from a light crimson to a very deep purple, approaching to a reddish brown. I searched, but in vain, for traces of revived gold, in its reguline form and colour; but though I could not perceive that the ribband was gilded, it had all the appearance of being covered with a thin coating of the most beautiful purple enamel, which, in the sun, had a degree of brilliancy that was sometimes quite dazzling.

Exper. 2. A piece of the ribband which had been wetted with the aqueous solution of the oxide, was carefully dried in a dark closet, and was then exposed dry, over the flame of a burning wax candle. The part of the ribband which had been wetted with the solution, and which on drying had acquired a faint yellow colour, was tinged of the same bright purple colour as was produced in the last-mentioned experiment, when the ribband was exposed wet to the action of the heat.*

Exper. 3. A piece of the ribband which had been wetted with the solution, and dried in the dark, was now wetted with distilled water, and exposed wet to the action of the ascending current of hot vapour which arose from the burning candle: the purple stain was produced as before, which extended as far as the ribband had been wetted with the solution, but no farther. I afterwards varied this experi-

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* We shall hereafter find reason to conclude, that the success of this experiment, or the appearance of the purple tinge, was owing to the watery vapour which existed in the hot current that ascended from the flame of the candle.—Orig.

ment in several ways, sometimes using paper, sometimes fine linen, and sometimes fine cotton cloth, instead of the silk ribband; but nearly the same tinge was produced, whatever the substance was that was made to imbibe the aqueous solution of the metallic oxide.

Similar experiments, and with similar results, were likewise made with pieces of ribband, fine linen, cotton, paper, &c. wetted in an aqueous solution of nitrate of silver; with this difference, however, that the tinge produced by this metallic oxide, instead of being of a deep purple, inclining to crimson, was of a very dark orange colour, or rather of a yellowish brown. In order to discover whether the purple tinge, in the experiments with the oxide of gold, was occasioned by the heat communicated by the ascending current of hot vapour, or by the light of the candle, I made the following experiment, the result of which, I conceive to have been decisive.

Exper. 4. A piece of ribband was wetted with the aqueous solution of the oxide of gold, and held vertically by the side of the clear flame of a burning wax candle, at the distance of less than half an inch from the flame. The ribband was dried, but its colour was not in the smallest degree changed. When it was held a few seconds within about $\frac{1}{8}$ of an inch of the flame, a tinge of a most beautiful crimson colour, in the form of a narrow vertical stripe, was produced. The heat which existed at that distance from the flame, on the side of it, where this coloured stripe was produced, was sufficiently intense, as I found by experiment, to melt very fine silver wire, flatted, such as is used in making silver lace.

Exper. 5. Two like pieces of ribband were wetted at the same time in the solution, and suspended, while wet, in 2 thin phials, A and B, of very transparent and colourless glass; the mouths of the phials being left open. Both these phials were placed in a window which fronted the south; that distinguished by the letter A being exposed naked to the direct rays of a bright sun; while B was inclosed in a cylinder of paste-board, painted black within and without, and closed with a fit cover, and consequently remained in perfect darkness. In a very few minutes, the ribband in the phial A began sensibly to change its colour, and to take a purple hue; and at the end of 5 hours it had acquired a deep crimson tint throughout. The phial B was exposed in the window, in its dark cylindrical cover, 3 days; but there was not the smallest appearance of any change of colour in the silk.

Exper. 6. Two small parcels of magnesia alba, in an impalpable powder, about half as much in each as could be made to lie on a shilling, were placed in heaps, in 2 china plates, A and B, and thoroughly moistened with the before-mentioned aqueous solution of the oxide of gold. Both plates were placed in the same window; the moistened earth in the plate A being exposed naked to the sun's rays; while that in the plate B was exactly covered with a tea-cup, turned upside down, which excluded all light. The magnesia alba in the plate A, which was exposed to the strong light of the sun, began almost immediately to change colour, taking a faint violet hue, which by degrees became more and more intense, and in a few hours ended in a deep purple ; while that in the plate B, which was kept in the dark, retained the yellowish cast it had acquired from the solution, without the smallest appearance of change.

Exper. 7. A small parcel of magnesia alba, placed on a china plate, having been moistened with the aqueous solution of the oxide of gold, and thoroughly dried in a dark closet, was now exposed in this dry state, to the action of the direct rays of a very bright sun. It had been exposed to this strong light above half an hour, before its colour began to be sensibly changed ; and at the end of 3 hours it had acquired only a very faint violet hue. Being now thoroughly wetted with distilled water, it changed colour very rapidly, and soon came to be of a deep purple tint, approaching to crimson.

Exper. 8. A piece of white taffeta ribband, which had been wetted with the solution, and thoroughly dried in the dark, was suspended in a clean dry phial of very fine transparent glass; and the phial, being well stopped with a dry cork, was exposed to the strong light of a bright sun. After the ribband had been exposed, in this manner, to the action of the sun's direct rays for about half an hour, there were here and there some faint appearances of a change of its colour; but it showed no disposition to take that deep purple hue which the ribband had always acquired, when exposed to the light in the preceding experiments. On taking the ribband out of the phial, and wetting it thoroughly with distilled water, and exposing it again, while thus wetted, to the sun's rays, it almost instantly began to change colour, and soon became of a deep purple tint ; but though I examined the surface of the ribband with the utmost care, and with a good lens, both during the experiment and after it, I could not perceive the smallest particle of revived gold, nor did I see any vestige remaining that appeared to indicate that any had in fact been revived. This experiment was repeated several times, and always with results which led me to conclude, what indeed was reasonable to expect, that light has little effect in changing the colour of metallic oxides, as long as they are in a state of crystallization.

The heat which is generated by the absorption of the rays of light must necessarily, at the moment of its generation at least, exist in almost infinitely small spaces; and consequently, it is only in bodies that are inconceivably small that it can produce durable effects, in any degree indicative of its extreme intensity. Perhaps the particles of the oxide of gold dissolved in water, are of such dimensions ; and it is very remarkable, that the colours produced, in some of my experiments on white ribbands, by means of an aqueous solution of the oxide of gold, are precisely the same as are produced from the oxide of that metal, by enamellers, in the intense heat of their furnaces.

As the colouring substance is the same, and as the colours produced are the same, why should we not conclude that the effects are produced in both these cases by the same means, that is to say, by the agency of heat ? or, in other words, and to be more explicit, by exposing the oxide in a certain temperature, at which it becomes disposed to vitrify, or to undergo a change in regard to the quantity of oxygen with which it is combined?

But the results of the following experiments afford still more satisfactory information, respecting the intensity of the heat generated in all cases where light is absorbed, and the striking effects which, under certain circumstances, it is capable of producing. The facility with which most of the metallic oxides are reduced, in the dry way, by means of charcoal, shows that, at a certain high temperature, oxygen is disposed to quit those metals, in order to form a chemical union with the charcoal, or at least with some one of its constituent principles, if it be a compound substance; and hence I concluded, that gold might be revived, in the moist way, by means of charcoal, from a solution of its oxide in water, were it possible, under such circumstances, to communicate to the charcoal, and to the oxide, at the same time, a degree of heat sufficient for that purpose. To see if this might not be done by means of light, I made, or rather repeated, the following very interesting experiment.

Exper. 9. Into a thin tube of very fine colourless glass, 10 inches long, and $\frac{6}{10}$ of an inch in diameter, closed hermetically at its lower end, I put as many pieces of charcoal, about the size of large peas, as filled the tube to the height of 2 inches; and, having poured on them as much of the aqueous solution of nitro-muriate of gold as nearly covered them, exposed the tube, with its contents, to the action of the direct rays of a very bright sun. In less than half an hour, small specks of revived gold, in all its metallic splendour, began to make their appearance here and there on the surface of the charcoal; and in 6 hours the solution, which at first was of a bright yellow colour, became perfectly colourless, and as clear and transparent as the purest water. The surface of the charcoal was in several places nearly covered with small particles of revived gold; and the inside of the glass tube, in that part where it was in contact with the upper surface of the contained liquid, was most beautifully gilded. This gilding of the tube was very splendid, when viewed by reflected light; but when the tube was placed between the light and the eye, it appeared like a thin cloud, of a greenish blue colour, without the smallest appearance of any metallic splendour. From the colour, and apparent density of this cloud, I was induced to conclude, that the gilding on the glass was less than one millionth part of an inch in thickness.

This interesting experiment was repeated 6 times, and always with nearly the same result. The gold was completely revived in each of them, and the solution left perfectly colourless; in most of the experiments however the sides of the glass were not gilded, all the revived gold remaining attached to the surface of the charcoal. In two of these experiments, I made use of pieces of charcoal which had been previously boiled several hours in a large quantity of distilled water, and which were introduced wet, and hot, into the tube, and immediately covered by the solution, to prevent them from imbibing any air; and, in different experiments, the solution was used of different degrees of strength. I plainly perceived that the experiment succeeded best, that is to say, that the gold was soonest revived, in those cases in which the solution was most diluted: one of the experiments however, and which succeeded perfectly, was made with the solution so much condensed, that it was nearly at the point at which it became disposed to crystallize *. On examining, with a good microscope, the particles of revived gold which remained attached to the surface of the charcoal, after it had been dried, I found them to consist of an infinite number of small scales, separated from each other; not very highly polished, but possessing the true metallic splendour, and a very deep and rich gold colour. The gold which attached itself to the inside of the glass tube, was in the form of a ring, about $\frac{1}{10}$ of an inch wide, badly defined however below, and adhered to the glass with so much obstinacy, as not to be removed by rincing out the tube a great number of times with water; it had, as has already been observed, a very high polish, when seen by reflected light. Those who enter into the spirit of these investigations, will easily imagine how impatient I must have been, after seeing the results of these experiments, to find out whether gold could be revived from this aqueous solution of its oxide by means of charcoal, without the assistance of light, and merely by such a degree of equal heat as could be given to it in the dark. To determine that important question, the following experiment was made.

Exper. 10. A cylindrical glass tube, $\frac{6}{10}$ of an inch in diameter, and 10 inches long, closed hermetically at its lower end, and containing a quantity of a diluted aqueous solution of the oxide of gold, mixed with charcoal in broken pieces, about the size of large peas, was put into a fit cylindrical tin case, which was nicely closed with a fit cover; and the glass tube, with its contents, so shut up in the dark, was exposed 2 hours, in the temperature of 210° of Fahrenheit's scale. On taking the glass tube out of its tin case, I found the solution perfectly colourless, and the revived gold adhering to the surface of the charcoal. On repeating the experiment, and using the solution nearly saturated with the oxide, the result was precisely the same; the solution being found perfectly colourless, and the revived gold adhering to the surface of the charcoal.

I own fairly, that the results of these experiments were quite contrary to my expectations, and that I am not able to reconcile them with my hypothesis, respecting the causes of the reduction of the oxide, in the foregoing experiments; but whatever may be the fate of this, or of any other hypothesis of mine, I hope and trust that I never shall be so weak as to feel pain at the discovery of truth, however contrary it may be to my expectations; and still less, to feel a secret wish to suppress experiments, merely because their results militate against any speculative opinions. It is proper I should observe, that the charcoal used in this last-

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* This agrees perfectly with the results of similar experiments made by the ingenious and lively Mrs. Fulhame. See her Essay on Combustion, page 124. It was on reading her book, that I was induced to engage in these investigations; and it was by her experiments, that most of the foregoing experiments were suggested.—Orig.

mentioned experiment had been boiled 2 hours in distilled water, by which means its pores had been so completely filled with that fluid, that the pieces of it that were used were specifically heavier than water, and sunk in it, to the bottom of the containing vessel. Having been so successful in my attempts to reduce the oxide of gold, by means of charcoal, in the moist way, I lost no time in making similar experiments with the oxide of silver.

Exper. 11. A solution of fine silver, in strong nitrous acid, was evaporated to dryness, and the residuum re-dissolved in distilled water. A portion of this solution, which was perfectly colourless, diluted with twice as much distilled water, was poured into a phial containing a number of small pieces of charcoal; and the phial, being well closed with a new cork stopple, was exposed to the action of the sun's rays. In less than an hour, small specks of revived silver began to make their appearance on the surface of the charcoal; and at the end of 2 hours these specks became very numerous, and had increased so much in size, that they were distinctly visible to the naked eye, at the distance of more than 3 feet. They were very white, and possessed the metallic splendour of silver in so high a degree, that when enlightened by the sun's beams, their lustre was nearly equal to that of very small diamonds. The phial, which was in the form of a pear, and about 1½ inch in diameter at its bulb, was very thin, and made of very fine colourless glass; the aqueous solution was also perfectly transparent and colourless; and, when the contents of the phial were illuminated by the direct rays of a bright sun, the contrast of the white colour of these little metallic spangles with the black charcoal to which they were fixed, and their extreme brilliancy, afforded a very beautiful and interesting sight. As the air had been previously expelled from the charcoal, by boiling it in distilled water, it was specifically heavier than the aqueous solution of the metallic oxide, and consequently remained at the bottom of the bottle.

Exper. 12. A phial, as nearly as possible like that used in the last experiment, and containing the same quantity of diluted aqueous solution of nitrate of silver, and also of charcoal, was inclosed in a cylindrical tin box, and exposed 1 hour to the heat of boiling water, in an apparatus used for boiling potatoes in steam, for the table. The result of this experiment was uncommonly striking. The surface of the charcoal was covered with a most beautiful metallic vegetation; small filaments of revived silver, resembling fine flatted silver wire, pushing out from its surface, in all directions! Some of these metallic filaments were above one-tenth of an inch in length. On agitating the contents of the phial, they were easily detached from the surface of the charcoal, to which they seemed to adhere but very slightly. These experiments were repeated several times, and always with precisely the same results. When the oxide of gold was reduced in this way, the revived metal appeared under the form of small scales, adhering firmly to the surface of the charcoal. May not the difference of the forms under which gold and silver are revived from their oxides, in this process, be owing to the difference of the specific gravities of those metals?

The following experiments, which were first suggested by an accident, were made with a view to investigate still further the causes of those effects which have been attributed to the supposed chemical properties of light. Having accidentally put away 2 small phials, each containing a quantity of aqueous solution of the oxide of gold and sulphuric ether, in each of which the ether had extracted the gold completely from the solution, as was evident by the yellow colour of the solution having been transferred to the ether, and the solution being left colourless; in one of the phials, which happened to stand in a window in which there was occasionally a strong light, though the direct rays of the sun never fell on it, I found, in about 3 weeks, that the oxide was almost entirely reduced; the revived gold appearing in all its metallic splendour, in the form of a thin pellicle, swimming on the surface of the aqueous liquor in the phial, and the colour of the ether which reposed on it having become quite faint; while no visible change had been produced in the contents of the other phial, which had stood in a dark corner of the room. As these appearances induced me to suspect, or rather strengthened the suspicions I had before conceived, that the separation of gold from ether, under its metallic form, when a solution of its oxide is mixed with that fluid, is always effected by a reduction of the oxide by means of light, I made the following experiment, with a view to the further investigation of that matter.

Exper. 13. Into a small pear-like phial, of very fine transparent glass, I put equal quantities of an aqueous solution of the muriatic oxide of gold and sulphuric ether; and the phial, which was about half filled, being closed with a good cork, well secured in its place, was exposed to the action of the direct rays of a bright sun. A pellicle of revived gold, in all its metallic splendour, began almost immediately to be formed on the surface of the aqueous liquid, and soon covered it entirely; and at the end of 2 hours the whole of the oxide was completely reduced, as was evident from the appearance of the ether, which became perfectly colourless. On shaking the phial the metallic pellicle, which covered the surface of the aqueous liquid, was broken into small pieces, which had exactly the appearance of leaf gold, possessing the true colour, and all the metallic brilliancy, of that metal. On suffering the phial to stand quiet, the aqueous liquor and the ether separated, and most of the broken pieces of the thin sheet of gold descended to the bottom of the phial: the remainder of them floated on the surface of the aqueous liquid; and the ether, as well as the aqueous liquid, appeared to be perfectly transparent and colourless. By the length of time which was required for the ether and the aqueous liquid to separate, I thought I could perceive that the ether had lost something of its fluidity; but as this was an event I expected, it is the more likely, on that account, that I was deceived, when I imagined I saw proofs of its having taken place. On removing the cork, after the contents of the bottle had been suffered to cool, there was no appearance of any considerable quantity of air, or other permanently elastic fluid, having been either generated or absorbed, during the experiment. Finding that the oxide of gold might be so completely and so expeditiously reduced, by means of ether, I conceived it might be possible to perform that chemical process, in the moist way, by means of essential oils; and this conjecture proved to be well founded.

Exper. 14. On a quantity of diluted aqueous solution of nitro-muriate of gold, in a small pear-like phial, about $1\frac{1}{2}$ inch in diameter at its bulb, was poured a small quantity of etherial oil of turpentine, just as much as was sufficient to cover the aqueous solution to the height of $\frac{2}{10}$ of an inch; and the phial, being well closed with a good cork, well secured, was exposed one hour to the heat of boiling water in a steam-vessel. The gold was revived, appearing in the form of a splendid pellicle, of a bright gold colour, which floated on the surface of the aqueous liquid. The oil of turpentine, which, at the beginning of the experiment, was as pale and colourless as pure water, had taken a bright yellow hue; and the aqueous fluid, on which it reposed, had entirely lost its yellow colour. On shaking the phial, its contents were intimately mixed; but, on suffering it to stand quiet, the oil of turpentine soon separated from the aqueous liquid, retaining its bright yellow hue, and leaving the aqueous liquid colourless.

On shaking the phial, before it had been exposed to the heat, and mixing its contents, and then suffering it to stand quiet, the oil of turpentine, on taking its place at the top of the aqueous solution, was not found to have acquired any colour; nor was the bright gold colour of the solution found to be at all impaired. When sulphuric ether was used, instead of the oil of turpentine, the effect was in this respect very different. To find out whether the oil of turpentine used in this experiment, and which had acquired a deep yellow colour, had lost that property by which it effected the reduction of the metallic oxide, I now poured an additional quantity of the aqueous solution of the oxide into the phial, and shaking the phial, exposed it with its contents to the heat of boiling water. After it had been exposed to this heat about 2 hours, I examined it, and found, that though a considerable quantity of gold had been revived, yet the aqueous liquid still retained a faint yellow colour. The oil of turpentine had acquired a deeper and richer gold colour, approaching to orange.

To the contents of the phial, I now added about half as much distilled water, and mixing the whole by shaking, I exposed the phial again, during 2 hours, to the heat of boiling water; when the remainder of the oxide was reduced, and the aqueous liquid left perfectly colourless. On repeating this experiment with oil of turpentine, and varying it, by using a solution of the oxide of silver, an aqueous solution of nitrate of silver, instead of that of gold, the result was nearly the same: the metal was revived, and the oil of turpentine acquired a faint greenish-yellow colour. I also revived the oxides of gold and silver with oil of olives, by a similar process, with the heat of boiling water. The oil of olives used in these experiments lost its transparency, and became deeply coloured; that used in the reduction of the oxide of silver, taking a very deep dirty brown colour, approaching to black; and that employed in reducing the oxide of gold, being changed to a yellowish-brown, with a purple hue. In the experiment with the oxide of silver, the inside of the phial, in the region where the oil reposed on the aqueous solution, was beautifully silvered, the revived metal forming a narrow metallic ring, extending quite round the phial; and in both experiments small detached pellicles of revived metal were visible in the oil, and adhered in several places to the inside of the phial, forming bright spots, in which the colour of the metal, and its peculiar splendour, were perfectly conspicuous.

Exper. 15. As carbon is one of the constituent principles of spirit of wine, as well as of essential oils and sulphuric ether, I thought it possible that I might succeed in the reduction of the oxide of gold, by mixing alcohol, with an aqueous solution of nitro-muriate of gold, and exposing the mixture, in a phial well closed, to the heat of boiling water; but the experiment did not succeed. By pouring upon this mixture a small quantity of oil of olives, and exposing it again to the heat of boiling water, the gold was revived.

Is it not probable, that the reason why the oxide was not reduced by alcohol, is the mobility of those elements, which ought to act on each other, in order that the effect in question may be produced? I have no doubt but the oxide would be reduced, could the alcohol be made to rest on the surface of the aqueous solution, without mixing with it. I wished to have been able to have collected and examined the elastic fluids, which probably were formed in most of the preceding experiments; but my time was so much taken up with other matters, that I had not leisure to pursue these investigations farther. In order to see what effects would be produced by the heat generated at the surface of an opaque body, of a nature different from those hitherto used in the reduction of the metallic oxides, and one that is little disposed to form a chemical union with oxygen, (magnesia alba,) when, being immersed in an aqueous solution of the oxide of gold, the rays of the sun were made to impinge on it, I contrived the following experiment.

Exper. 16. I took 4 small thin phials, A, B, C, and D, of very fine glass, and putting into each of them about 5 grains of dry magnesia alba, I filled the phial A nearly full with a saturated aqueous solution of the oxide of gold. I filled the phial B, in like manner, with some of the same solution, diluted with an equal quantity of distilled water; and the phials C and D were filled with the solution still further diluted. These phials, open or without stoppers, were exposed one whole day to the action of the direct rays of a bright sun, their contents being often well mixed together, during that time, by shaking.

The contents of all these phials changed colour, more or less, but they acquired very different hues. The contents of the phial A became of a very deep rich gold colour, approaching to orange, the earthy sediment being throughout of the same tint. The contents of the phial B, which were at first of a light straw colour, first changed to a light green, and then to a greenish blue. The phial having been suffered to stand quiet several days in an uninhabited room, in a retired part of the house, the solution became nearly colourless, and the sediment was found to be of a dirty olive colour. The colour of the contents of the phials C and D was changed nearly in the same manner; and having been suffered to stand quiet 2 or 3 days, to settle, the solution was found to be quite colourless, and the sediment to be deeply coloured. There was however a very remarkable difference in the hues of the 2 phials; that of the phial C being of a light greenish-blue; while that in the phial D was indigo, and of so deep a tint, that it might easily have been taken for black.

These appearances were certainly very striking, and well calculated to excite my curiosity; but I am so much engaged in public business, that it is not at present in my power to pursue these inquiries farther. I wish that what I have done may induce others, who have more time to spare, to devote some portion of their leisure to these interesting investigations.

XXI. To Determine the Density of the Earth. By H. Cavendish, Esq., F. R. S., and A. S. p. 469.

Many years ago, the late Rev. John Michell, of this Society, contrived a method of determining the density of the earth, by rendering sensible the attraction of small quantities of matter; but, as he was engaged in other pursuits, he did not complete the apparatus till a short time before his death, and did not live to make any experiments with it. After his death, the apparatus came to the Rev. Francis John Hyde Wollaston, Jacksonian Professor at Cambridge, who, not having conveniences for making experiments with it, in the manner he could wish, was so good as to give it to me. The apparatus is very simple; it consists of a wooden arm, 6 feet long, made so as to unite great strength with little weight. This arm is suspended in an horizontal position, by a slender wire 40 inches long, and to each extremity is hung a leaden ball, about 2 inches in diameter; and the whole is inclosed in a narrow wooden case, to defend it from the wind.

As no more force is required to make this arm turn round on its centre, than what is necessary to twist the suspending wire, it is plain, that if the wire is sufficiently slender, the most minute force, such as the attraction of a leaden weight a few inches in diameter, will be sufficient to draw the arm sensibly aside. The weights which Mr. Michell intended to use, were 8 inches diameter. One of these was to be placed on one side the case, opposite to one of the balls, and as near it as could conveniently be done, and the other on the other side, opposite to the other ball, so that the attraction of both these weights would conspire in drawing the arm aside; and when its position, as affected by these weights, was ascertained, the weights were to be removed to the other side of the case, so as to draw the arm the contrary way, and the position of the arm was to be again determined; consequently, half the difference of these positions would show how much the arm was drawn aside by the attraction of the weights. In order to determine from hence the density of the earth, it is necessary to ascertain what force is required to draw the arm aside through a given space. This Mr. Michell intended to do, by putting the arm in motion, and observing the time of its vibrations, from which it may easily be computed.*

Mr. Michell had prepared 2 wooden stands, on which the leaden weights were to be supported, and pushed forwards, till they came almost in contact with the case; but he seems to have intended to move them by hand. As the force with which the balls are attracted by these weights is excessively minute, not more than $\frac{1}{50000000}$ of their weight, it is plain that a very minute disturbing force will be sufficient to destroy the success of the experiment; and from the following experiments it will appear, that the disturbing force most difficult to guard against, is that arising from the variations of heat and cold; for if one side of the case be warmer than the other, the air in contact with it will be rarefied, and in consequence will ascend, while that on the other side will descend, and produce a current which will draw the arm sensibly aside.† As I was convinced of the necessity of guarding against this source of error, I resolved to place the apparatus in a room which should remain constantly shut, and to observe the motion of the arm from without, by means of a telescope; and to suspend the leaden weights in such manner, that I could move them without entering into the room. This difference in the manner of observing, rendered it necessary to make some alteration in Mr. Michell's apparatus; and as there were some parts of it which I thought not so convenient as could be wished, I chose to make the greatest part of it anew.

Fig. 1, pl. 7, is a longitudinal vertical section through the instrument, and the building in which it is placed: ABCDDCBAEFFE is the case; *x* and *x* are the two balls, which are suspended by the wires *hx* from the arm *ghmh*, which is itself suspended by the slender wire *gl*. This arm consists of a slender deal rod *hmh*, strengthened by a silver wire *hgh*; by which means it is made strong enough to support the balls, though very light.‡ The case is supported, and set horizontal, by 4 screws, resting on posts fixed firmly into the ground: 2 of them are represented in the figure, by s and s; the other 2 are not represented, to avoid confusion.

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* Mr. Coulomb has, in a variety of cases, used a contrivance of this kind for trying small attractions; but Mr. Michell informed me of his intention of making this experiment, and of the method he intended to use, before the publication of any of Mr. Coulomb's experiments.—Orig.

† M. Cassini, in observing the variation compass placed by him in the observatory, (which was constructed so as to make very minute changes of position visible, and in which the needle was suspended by a silk thread), found that standing near the box, in order to observe, drew the needle sensibly aside; which I have no doubt was caused by this current of air. It must be observed, that his compass-box was of metal, which transmits heat faster than wood, and also was many inches deep; both which causes served to increase the current of air. To diminish the effect of this current, it is by all means advisable to make the box, in which the needle plays, not much deeper than is necessary to prevent the needle from striking against the top and bottom.—Orig.

‡ Mr. Michell's rod was entirely of wood, and was much stronger and stiffer than this, though not much heavier; but, as it had warped when it came to me, I chose to make another, and preferred this form, partly as being easier to construct and meeting with less resistance from the air, and partly because, from its being of a less complicated form, I could more easily compute how much it was attracted by the weights.—Orig.

GG and GG are the end walls of the building. W and W are the leaden weights; which are suspended by the copper rods RrPrR, and the wooden bar *rr*, from the centre pin P*p*. This pin passes through a hole in the beam HH, perpendicularly over the centre of the instrument, and turns round in it, being prevented from falling by the plate *p*. MM is a pulley, fastened to this pin; and M*m* a cord wound round the pulley, and passing through the end wall; by which the observer may turn it round, and so move the weights from one situation to the other.

Fig. 2 is a plan of the instrument. AAAA is the case; SSSS the 4 screws for supporting it; *hh* the arm and balls, W and W the weights; MN, the pulley for moving them. When the weights are in this position, both conspire in drawing the arm in the direction *h*W; but, when they are removed to the situation *w* and *w*, represented by the dotted lines, both conspire in drawing the arm in the contrary direction *hw*. These weights are prevented from striking the instrument, by pieces of wood, which stop them as soon as they come within $\frac{1}{4}$ of an inch of the case. The pieces of wood are fastened to the wall of the building; and I find that the weights may strike against them with considerable force, without sensibly shaking the instrument.

In order to determine the situation of the arm, slips of ivory are placed within the case, as near to each end of the arm as can be done without danger of touching it, and are divided to 20ths of an inch. Another small slip of ivory is placed at each end of the arm, serving as a vernier, and subdividing these divisions into 5 parts; so that the position of the arm may be observed with ease to 100ths of an inch, and may be estimated to less. These divisions are viewed, by means of the short telescopes T and T, fig. 1, through slits cut in the end of the case, and stopped with glass; they are enlightened by the lamps L and L, with convex glasses, placed so as to throw the light on the divisions; no other light being admitted into the room. The divisions on the slips of ivory run in the direction W*w*, fig. 2, so that, when the weights are placed in the positions *w* and *w*, represented by the dotted circles, the arm is drawn aside, in such direction as to make the index point to a higher number on the slips of ivory; for which reason, I call this the positive position of the weights.

FK, fig. 1, is a wooden rod, which, by means of an endless screw, turns round the support to which the wire *gl* is fastened, and so enables the observer to turn round the wire, till the arm settles in the middle of the case, without danger of touching either side. The wire *gl* is fastened to its support at top, and to the centre of the arm at bottom, by brass clips, in which it is pinched by screws. In these 2 figures, the different parts are drawn nearly in the proper proportion to each other.

Before proceeding to the account of the experiments, it will be proper to say something of the manner of observing. Suppose the arm to be at rest, and its position to be observed, let the weights be then moved, the arm will not only be thus drawn aside, but it will be made to vibrate, and its vibrations will continue a great while; so that, in order to determine how much the arm is drawn aside, it is necessary to observe the extreme points of the vibrations, and thence to determine the point which it would rest at if its motion was destroyed, or the point of rest, as I shall call it. To do this, I observe 3 successive extreme points of a vibration, and take the mean between the 1st and 3d of these points, as the extreme point of vibration in one direction, and then assume the mean between this and the 2d extreme, as the point of rest; for, as the vibrations are continually diminishing, it is evident that the mean between 2 extreme points will not give the true point of rest. It may be thought more exact, to observe many extreme points of vibration, so as to find the point of rest by different sets of 3 extremes, and to take the mean result; but it must be observed, that notwithstanding the pains taken to prevent any disturbing force, the arm will seldom remain perfectly at rest for an hour together; for which reason, it is best to determine the point of rest, from observations made as soon after the motion of the weights as possible.

The next thing to be determined is the time of vibration, which is found in this manner: I observe the 2 extreme points of a vibration, and also the times at which the arm arrives at 2 given divisions between these extremes, taking care, as well as I can guess, that these divisions shall be on different sides of the middle point, and not very far from it. I then compute the middle point of the vibration, and by proportion find the time at which the arm comes to this middle point. I then, after a number of vibrations, repeat this operation, and divide the interval of time, between the coming of the arm to these 2 middle points, by the number of vibrations, which gives the time of 1 vibration.

To judge of the propriety of this method, we must consider in what manner the vibration is affected by the resistance of the air, and by the motion of the point of rest. Let the arm, during the first vibration, move from D to B, fig. 3, and during the 2d from B to *d*; B*d* being less than DB, on account of the resistance. Bisect DB in M, and B*d* in *m*, and bisect M*m* in *n*, and let *x* be any point in the vibration; then if the resistance is proportional to the square of the velocity, the whole time of a vibration is very little altered; but, if T is taken to the time of one vibration, as the diameter of a circle to its semi-circumference, the time of moving from B to *n* exceeds ½ a vibration, by $\frac{T \times Dd}{8Bn}$ nearly; and the time of moving from B to *m* falls short of ½ a vibration, by as much; and the time of moving from B to *x*, in the 2d vibration, exceeds that of moving from *x* to B, in the first, by $\frac{T \times Dd \times Bx^2}{4Bn^2 \times \sqrt{Bx \times x\delta}}$, supposing D*d* to be bisected in δ; so that, if a mean is taken, between the time of the first arrival of the arm at *x* and its returning back to the same point, this mean will be earlier than the true time of its coming to B, by $\frac{T \times Dd \times Bx^2}{8Bn^2 \sqrt{Bx \times x\delta}}$.

The effect of motion in the point of rest is, that when the arm is moving in the same direction as the point of rest, the time of moving from one extreme point of vibration to the other is increased, and it is diminished when they are moving in contrary directions; but if the point of rest move uniformly, the time of moving from one extreme to the middle point of the vibration, will be equal to that of moving from the middle point to the other extreme, and also, the time of 2 successive vibrations will be very little altered; and therefore the time of moving from the middle point of one vibration to the middle point of the next, will also be very little altered. It appears therefore, that on account of the resistance of the air, the time at which the arm comes to the middle point of the vibration, is not exactly the mean between the times of its coming to the extreme points, which causes some inaccuracy in my method of finding the time of a vibration. It must be observed however, that as the time of coming to the middle point is before the middle of the vibration, both in the first and last vibration, and in general is nearly equally so, the error produced from this cause must be inconsiderable; and, on the whole, I see no method of finding the time of a vibration which is liable to less objection.

The time of a vibration may be determined, either by previous trials, or it may be done at each experiment, by ascertaining the time of the vibrations which the arm is actually put into by the motion of the weights; but there is one advantage in the latter method, namely, that if there should be any accidental attraction, such as electricity, in the glass plates through which the motion of the arm is seen, which should increase the force necessary to draw the arm aside, it would also diminish the time of vibration; and consequently the error in the result would be much less, when the force required to draw the arm aside was deduced from experiments made at the time, than when it was taken from previous experiments.

Account of the Experiments.—In the first experiments, the wire by which the arm was suspended was $39\frac{1}{4}$ inches long, and was of copper silvered, one foot of which weighed $2\frac{4}{10}$ grains: its stiffness was such, as to make the arm perform a vibration in about 15 minutes. I immediately found indeed that it was not stiff enough, as the attraction of the weights drew the balls so much aside, as to make them touch the sides of the case; I chose however to make some experiments with it before I changed it. In this trial, the rods by which the leaden weights were suspended were of iron; for, as I had taken care that there should be nothing magnetical in the arm, it seemed of no signification whether the rods were magnetical or not; but, for greater security, I took off the leaden weights, and tried what effect the rods would have by themselves. Now I find, by computation, that the attraction of gravity of these rods on the balls, is to that of the weights, nearly as 17 to 2500; so that, as the attraction of the weights appeared, by the foregoing trial, to be sufficient to draw the arm aside by about 15 divisions, the attraction of the rods alone should draw it aside about $\frac{1}{10}$ of a division; and therefore the motion of the rods from one near position to the other, should move it about $\frac{1}{5}$ of a division.

The result of the experiment was, that for the first 15 minutes after the rods were removed from one near position to the other, very little motion was produced in the arm, and hardly more than ought to be produced by the action of gravity;

but the motion then increased, so that, in about a quarter or half an hour more, it was found to have moved $\frac{1}{2}$ or $1\frac{1}{2}$ division, in the same direction that it ought to have done by the action of gravity. On returning the irons back to their former position, the arm moved backward, in the same manner that it before moved forward. It must be observed, that the motion of the arm, in these experiments was hardly more than would sometimes take place without any apparent cause; but yet, as in 3 experiments which were made with these rods, the motion was constantly of the same kind, though differing in quantity from $\frac{1}{2}$ to $1\frac{1}{2}$ division, there seems great reason to think that it was produced by the rods. As this effect seemed to be owing to magnetism, though it was not such as I should have expected from that cause, I changed the iron rods for copper, and tried them as before; the result was, that there still seemed to be some effect of the same kind, but more irregular, so that I attributed it to some accidental cause, and therefore hung on the leaden weights, and proceeded with the experiments. It must be observed, that the effect which seemed to be produced by moving the iron rods from one near position to the other, was, at a medium, not more than one division; whereas the effect produced by moving the weight from the midway to the near position, was about 15 divisions; so that, if I had continued to use the iron rods, the error in the result thus caused, could hardly have exceeded $\frac{1}{30}$ of the whole.

In exper. 1, Aug. 5, the motions of the weights between the midway and positive positions were thus:

Motion on moving from midway to positive ...... = 14.32
From positive to midway .... = 14.1
Time of one vibration......................... = $14^{m}\ 55^{s}$.

It must be observed, that in this experiment, the attraction of the weights drew the arm from 11.5 to 25.8, so that, if no contrivance had been used to prevent it, the momentum thus acquired would have carried it to near 40, and would therefore have made the balls strike against the case. To prevent this, after the arm had moved near 15 divisions, I returned the weights to the midway position, and let them remain there, till the arm came nearly to the extent of its vibration, and then again moved them to the positive position, by which the vibrations were so much diminished, that the balls did not touch the sides; and it was this which prevented my observing the first extremity of the vibration. A like method was used when the weights were returned to the midway position, and in the 2 following experiments. The vibrations, in moving the weights from the midway to the positive position were so small, that it was thought not worth while to observe the time of the vibration. When the weights were returned to the midway position, I determined the time of the arm's coming to the middle point of each vibration, in order to see how nearly the times of the different vibrations agreed together. In great part of the following experiments, I contented myself with observing the time of its coming to the middle point of only the first and last vibration.

In the 2d experiment, Aug. 6, in like manner, the time of 1 vibration was $14^m\ 42^s$; and in the 3d experiment, Aug. 7, it was $14^m\ 46^s$.

These experiments are sufficient to show, that the attraction of the weights on the balls is very sensible, and are also sufficiently regular to determine the quantity of this attraction pretty nearly, as the extreme results do not differ from each other by more than $\frac{1}{10}$ part. But there is a circumstance in them, the reason of which does not readily appear, namely, that the effect of the attraction seems to increase, for half an hour, or an hour, after the motion of the weights; as in all the 3 experiments, the mean position kept increasing for that time, after moving the weights to the positive position; and kept decreasing, after moving them from the positive to the midway position. The first cause which occurred to me was, that possibly there might be a want of elasticity, either in the suspending wire, or something it was fastened to, which might make it yield more to a given pressure, after a long continuance of that pressure, than it did at first. To put this to the trial, I moved the index so much, that the arm, if not prevented by the sides of the case, would have stood at above 50 divisions, so that, as it could not move farther than to 35 divisions, it was kept in a position 15 divisions distant from that which it would naturally have assumed from the stiffness of the wire; or, in other words, the wire was twisted 15 divisions. After having remained 2 or 3 hours in this position, the index was moved back, so as to leave the arm at liberty to assume its natural position.

It must be observed, that if a wire is twisted only a little more than its elasticity admits of, then, instead of setting, as it is called, or acquiring a permanent twist all at once, it sets gradually, and when left at liberty it gradually loses part of that set which it acquired; so that if, in this experiment, the wire, by having been kept twisted for 2 or 3 hours, had gradually yielded to this pressure, or had begun to set, it would gradually restore itself, when left at liberty, and the point of rest would gradually move backwards: but though the experiment was twice repeated, I could not perceive any such effect.

The arm was next suspended by a stiffer wire: after which, in the next 2 experiments, being the 4th and 5th, the times of vibration were thus: viz. $7^m\ 2^s$ and $7^m\ 5^s$. In the 4th experiment, the effect of the weights seemed to increase on standing, in all the 3 motions of the weights, conformably to what was observed with the former wire; but in the last experiment the case was different; for though, on moving the weights from positive to negative, the effect seemed to increase on standing, yet on moving them from negative to positive, it diminished.

My next trials were, to see whether this effect was owing to magnetism. Now, as it happened, the case in which the arm was inclosed, was placed nearly parallel to the magnetic east and west, and therefore, if there was any thing magnetic in the balls and weights, the balls would acquire polarity from the earth; and the weights also, after having remained some time, either in the positive or negative position, would acquire polarity in the same direction, and would attract the balls; but when the weights were moved to the contrary position, that pole which before pointed to the north, would point to the south, and would repell the ball it was approached to; but yet, as repelling one ball towards the south has the same effect on the arm as attracting the other towards the north, this would have no effect on the position of the arm. After some time however, the poles of the weight would be reversed, and would begin to attract the balls, and would therefore produce the same kind of effect as was actually observed.

To try whether this was the case, I detached the weights from the upper part of the copper rods by which they were suspended, but still retained the lower joint, namely, that which passed through them; I then fixed them in their positive position, in such manner, that they could turn round on this joint, as a vertical axis. I also made an apparatus, by which I could turn them half way round, on these vertical axes, without opening the door of the room. Having suffered the apparatus to remain in this manner for a day, I next morning observed the arm, and having found it to be stationary, turned the weights half way round on their axes, but could not perceive any motion in the arm. Having suffered the weights to remain in this position for about an hour, I turned them back into their former position, but without its having any effect on the arm. This experiment was repeated on 2 other days, with the same result. We may be sure, therefore, that the effect in question could not be produced by magnetism in the weights; for if it was, turning them half round on their axes would immediately have changed their magnetic attraction into repulsion, and have produced a motion in the arm. As a further proof of this, I took off the leaden weights, and in their room placed two 10-inch magnets; the apparatus for turning them round being left as it was, and the magnets being placed horizontal, and pointing to the balls, and with their north poles turned to the north; but I could not find that any alteration was produced in the place of the arm, by turning them half round: which not only confirms the deduction drawn from the former experiment, but also seems to show, that in the experiments with the iron rods, the effect produced could not be owing to magnetism.

The next thing which suggested itself to me was, that possibly the effect might be owing to a difference of temperature between the weights and the case; for it is evident, that if the weights were much warmer than the case, they would warm that side which was next to them, and produce a current of air, which would make the balls approach nearer to the weights. Though I thought it not likely that there should be sufficient difference, between the heat of the weights and case, to have any sensible effect, and though it seemed improbable that, in all the foregoing experiments, the weights should happen to be warmer than the case, I resolved to examine into it, and for this purpose removed the apparatus used in the last experiments, and supported the weights by the copper rods, as before; and having placed them in the midway position, I put a lamp under each, and placed a thermometer with its ball close to the outside of the case, near that part which one of the weights approached to in its positive position, and in such manner that I could distinguish the divisions by the telescope. Having done this, I shut the door, and some time after moved the weights to the positive position. At first, the arm was drawn aside only in its usual manner; but in half an hour the effect was so much increased, that the arm was drawn 14 divisions aside, instead of about 3, as it would otherwise have been, and the thermometer was raised near $1\frac{1}{2}$°; namely, from 61° to $62\frac{1}{2}$°. On opening the door, the weights were found to be no more heated, than just to prevent their feeling cool to my fingers.

As the effect of a difference of temperature appeared to be so great, I bored a small hole in one of the weights, about $\frac{3}{4}$ of an inch deep, and inserted the ball of a small thermometer, and then covered up the opening with cement. Another small thermometer was placed with its ball close to the case, and as near to that part to which the weight was approached as could be done with safety; the thermometers being so placed, that when the weights were in the negative position, both could be seen through one of the telescopes, by means of light reflected from a concave mirror. Three other experiments were then made with this apparatus, viz. exper. 6 on Sept. 6, exper. 7 on Sept. 18, and exper 8 on Sept. 23; the results of which were as follow: viz.

Sept. 6. Motion of arm on moving weights from midway to $- = 3.03$
$-$ to $+ = 5.9$
Sept. 18. Motion of arm on moving weights from midway to $- = 3.15$
$-$ to $+ = 6.1$
Sept. 23. Motion of arm on moving weights from midway to $- = 3.13$
$-$ to $+ = 5.72$

In these 3 experiments, the effect of the weight appeared to increase from 2 to 5-10ths of a division, on standing an hour; and the thermometers showed, that the weights were 3 or 5-10ths of a degree warmer than the air close to the case. In the last 2 experiments, I put a lamp into the room, over night, in hopes of making the air warmer than the weights, but without effect, as the heat of the weights exceeded that of the air more in these 2 experiments than in the former.

On the evening of October 17, the weights being placed in the midway position, lamps were put under them in order to warm them; the door was then shut, and the lamps suffered to burn out. The next morning it was found, on moving the weights to the negative position, that they were $7\frac{1}{2}$° warmer than the air near the case. After they had continued an hour in that position, they were found to have cooled $1\frac{1}{2}$°, so as to be only 6° warmer than the air. They were then moved to the positive position; and in both positions the arm was drawn aside about 4 divisions more, after the weights had remained an hour in that position, than it was at first.

May 22, 1798. The experiment was repeated in the same manner, except that the lamps were made so as to burn only a short time, and only 2 hours were suffered to elapse before the weights were moved. The weights were now found to be scarcely 2° warmer than the case; and the arm was drawn aside about 2 divisions more, after the weights had remained an hour in the position they were moved to, than it was at first.

On May 23, the experiment was tried in the same manner, except that the weights were cooled by laying ice on them; the ice being confined in its place by tin plates, which, on moving the weights, fell to the ground, so as not to be in the way. On moving the weights to the negative position, they were found to be about 8° colder than the air, and their effect on the arm seemed now to diminish on standing, instead of increasing, as it did before; as the arm was drawn aside about 2½ divisions less, at the end of an hour after the motion of the weights, than it was at first.

It seems sufficiently proved therefore, that the effect in question is produced, as above explained, by the difference of temperature between the weights and case; for, in the 6th, 8th, and 9th experiments, in which the weights were not much warmer than the case, their effect increased but little on standing; whereas it increased much when they were much warmer than the case, and decreased much when they were much cooler. It must be observed, that in this apparatus the box in which the balls hang must be near the bottom of it, which makes the effect of the current of air more sensible than it would otherwise be, and is a defect which I intend to rectify in some future experiments.

After this were made 3 other experiments, with the weights first in the positive and then moved to the negative position; viz. exper. 9 on April 29, exper. 10 on May 5, and exper. 11 on May 6; the results of which were as follow: viz.

April 29. Motion of arm .... = 6.32
Time of vibration .... = $6^m\ 58^s$
May 5. Motion of arm .... = 6.15
Time of vibration .... = $6^m\ 59^s$
May 6. Motion of arm .... = 6.07
Time of vibration .... = $7^m\ 1^s$

In the foregoing 3 experiments, the index was purposely moved so that, before the beginning of the experiment, the balls rested as near the sides of the case as they could, without danger of touching it; for it must be observed, that when the arm is at 35, they begin to touch. In the following 2 experiments, the index was in its usual position. Next follow 3 more such experiments, by varying the positions from negative to positive, and the contrary; viz. exper. 12, 13, 14, on May 9, 25, 26 respectively; the results of which are as follow. viz.

Exper. 12. Motion of arm .... = 6.09
Time of vibration .... = $7^m\ 3^s$

Exper. 13. Motion of the arm on moving weights from − to + = 6.12
+ to − = 5.97
Time of vibration at .... + = $7^m\ 6^s$
− = 7 7

Exper. 14. Motion of arm by moving the weights from − to + = 6.27
+ to − = 6.13
Time of vibration at + = $7^m\ 6^s$
− = 7 6

In the next experiment 15, on May 27, the balls, before the motion of the weights, were made to rest as near as possible to the sides of the case, but on the contrary side from what they did in the 9th, 10th, and 11th experiments. The result as follows:

Exper. 15. Motion of the arm from ...... + 6.34
Time of vibration .... $7^m\ 7^s$

The following 2 experiments, 16, 17, on May 28 and 30, were made by Mr. Gilpin, who was so good as to assist me on the occasion. The results thus:

Exper. 16. Motion of the arm .... = 6.1
Time of vibration .... = $7^m\ 16^s$

Exper. 17. Motion of the arm on moving weights from − to + = 5.78
+ to − = 5.64
Time of vibration at .... + = $7^m\ 2^s$
− = 7 3

On computing the density of the earth from these experiments.—I shall first compute this, on the supposition that the arm and copper rods have no weight, and that the weights exert no sensible attraction, except on the nearest ball; and shall then examine what corrections are necessary, on account of the arm and rods, and some other small causes. The first thing is, to find the force required to draw the arm aside, which, as before said, is to be determined by the time of a vibration.

The distance of the centres of the 2 balls from each other is 73.3 inches, and therefore the distance of each from the centre of motion is 36.65, and the length of a pendulum vibrating seconds, in this climate, is 39.14; therefore, if the stiffness of the wire by which the arm is suspended is such, that the force which must be applied to each ball, in order to draw the arm aside by the angle A, is to the weight of that ball, as the arch of A to the radius, the arm will vibrate in the same time as a pendulum whose length is 36.65 inches, that is, in $\sqrt{\frac{36.65}{39.14}}$ seconds; and therefore, if the stiffness of the wire is such as to make it vibrate in N seconds, the force which must be applied to each ball, in order to draw it aside by the angle A, is to the weight of the ball, as the arch of $A \times \frac{1}{N^2} \times \frac{36.65}{39.14}$ to the radius. But the ivory scale at the end of the arm is 38.3 inches from the centre of motion, and each division is $\frac{1}{20}$ of an inch, and therefore subtends an angle at the centre, whose arch is $\frac{1}{766}$; therefore the force which must be applied to each ball, to draw the arm aside by 1 division, is to the weight of the ball, as $\frac{1}{766\,\text{N}^2}.\frac{36.65}{39.14}$ to 1, or as $\frac{1}{818\,\text{N}^2}$ to 1.

The next thing is, to find the proportion which the attraction of the weight on the ball bears to that of the earth on it, supposing the ball to be placed in the middle of the case, that is, to be not nearer to one side than the other. When the weights are approached to the balls, their centres are 8.85 inches from the middle line of the case; but, through inadvertence, the distance, from each other, of the rods which support these weights, was made equal to the distance of the centres of the balls from each other, whereas it ought to have been somewhat greater. In consequence of this, the centres of the weights are not exactly opposite to those of the balls, when they are approached together; and the effect of the weights, in drawing the arm aside, is less than it would otherwise have been, in the triplicate ratio of $\frac{8.85}{36.65}$ to the chord of the angle whose sine is $\frac{8.85}{36.65}$, or in the triplicate ratio of the cosine of $\frac{1}{2}$ this angle to the radius, or in the ratio of .9779 to 1.

Each of the weights weighs 2439000 grains, and therefore is equal in weight to 10.64 spherical feet of water; therefore its attraction on a particle placed at the centre of the ball, is to the attraction of a spherical foot of water on an equal particle placed on its surface, as $10.64 \times .9779 \times (\frac{6}{8.85})^2$ to 1. The mean diameter of the earth is 41800000 feet*; and therefore, if the mean density of the earth be to that of water as D to 1, the attraction of the leaden weight on the ball will be to that of the earth on it, as $10.64 \times .9779 \times (\frac{6}{8.85})^2$ to 41800000D :: 1 to 8739000D.

It is shown therefore, that the force which must be applied to each ball, in order to draw the arm 1 division out of its natural position, is $\frac{1}{818\,\text{N}^2}$ of the weight of the ball; and if the mean density of the earth be to that of water as D to 1, the attraction of the weight on the ball is $\frac{1}{8739000\text{D}}$ of the weight of that ball; therefore the attraction will be able to draw the arm out of its natural position by $\frac{818\,\text{N}^2}{8739000\text{D}}$ or $\frac{\text{N}^2}{10683\text{D}}$ divisions; and therefore, if on moving the weights from the midway to a near position the arm is found to move B divisions, or if it moves 2B divisions on moving the weights from one near position to the other, it follows that the density of the earth, or D, is $\frac{\text{N}^2}{10683\text{B}}$.

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* In strictness, we ought, instead of the mean diameter of the earth, to take the diameter of that sphere whose attraction is equal to the force of gravity in this climate; but the difference is not worth regarding.—Orig.

We must now consider the corrections which must be applied to this result; first, for the effect which the resistance of the arm to motion has on the time of the vibration: 2d, for the attraction of the weights on the arm: 3d, for their attraction on the farther ball: 4th, for the attraction of the copper rods on the balls and arm: 5th, for the attraction of the case on the balls and arm: and 6th, for the alteration of the attraction of the weights on the balls, according to the position of the arm, and the effect which that has on the time of vibration. None of these corrections indeed, except the last, are of much signification, but they ought not entirely to be neglected. As to the first, it must be considered, that during the vibrations of the arm and balls, part of the force is spent in accelerating the arm; and therefore, in order to find the force required to draw them out of their natural position, we must find the proportion which the forces spent in accelerating the arm and balls bear to each other.

Let EDC*edc*, fig. 4, be the arm; B and *b* the balls; C*s* the suspending wire. The arm consists of 4 parts: first, a deal rod D*cd*, 73.3 inches long; 2d, the silver wire DC*d*, weighing 170 grains; 3d, the end pieces DE and *ed*, to which the ivory vernier is fastened, each of which weighs 45 grains; and 4th, some brass work C*c*, at the centre. The deal rod, when dry, weighs 2320 grains, but when very damp, as it commonly was during the experiments, weighs 2400; the transverse section is of the shape represented in fig. 5; the thickness BA, and the dimensions of the part DE*ed*, being the same in all parts; but the breadth B*b* diminishes gradually, from the middle to the ends. The area of this section is .33 of a square inch at the middle, and .146 at the end; therefore, if any point x (fig. 4) be taken in *cd*, and $\frac{cx}{cd}$ be called x, this rod weighs $\frac{2400 \times .33}{73.3 \times .238}$ per inch at the middle; $\frac{2400 \times .146}{73.3 \times .238}$ at the end, and $\frac{2400}{73.3} \times \frac{.33 - .184\,x}{.238} = \frac{3320 - 1848\,x}{73.3}$ at x; and therefore, as the weight of the wire is $\frac{170}{73.3}$ per inch, the deal rod and wire together may be considered as a rod whose weight at $x = \frac{3490 - 1848\,x}{73.3}$ per inch.

But the force required to accelerate any quantity of matter placed at x, is proportional to x^2; that is, it is to the force required to accelerate the same quantity of matter placed at d, as x^2 to 1; and therefore, if *cd* be called l, and x be supposed to flow, the fluxion of the force required to accelerate the deal rod and wire is proportional to $\frac{x^2 l\dot{x} \times (3490 - 1848x)}{73.3}$, the fluent of which, generated while x flows from c to d, is $= \frac{l}{73.3} \times \frac{3490}{3} - \frac{1848}{4} = 350$; so that the force required to accelerate each half of the deal rod and wire, is the same as is required to accelerate 350 grains placed at d.

The resistance to motion of each of the pieces *de*, is equal to that of 48 grains placed at d; as the distance of their centres of gravity from C is 38 inches. The resistance of the brass work at the centre may be disregarded; and therefore the whole force required to accelerate the arm, is the same as that required to accelerate 398 grains placed at each of the points D and *d*.

Each of the balls weighs 11262 grains, and they are placed at the same distance from the centre as D and *d*; therefore the force required to accelerate the balls and arm together, is the same as if each ball weighed 11660, and the arm had no weight; and therefore, supposing the time of a vibration to be given, the force required to draw the arm aside, is greater than if the arm had no weight, in the proportion of 11660 to 11262, or of 1.0353 to 1.

To find the attraction of the weights on the arm, through *d* draw the vertical plane *dwb* perpendicular to D*d*, and let *w* be the centre of the weight, which, though not accurately in this plane, may without sensible error be considered as placed in it, and let *b* be the centre of the ball; then *wb* is horizontal and = 8.85, and *db* is vertical and = 5.5; let $wd = a$, $wb = b$, and let $\frac{dx}{dc}$, or $1 - x = z$; then the attraction of the weight on a particle of matter at *x*, in the direction *dw*, is to its attraction on the same particle placed at $b :: b^3 : (a^2 + z^2l^2)^{\frac{3}{2}}$, or is proportional to $\frac{b^3}{(a^2 + z^2l^2)^{\frac{3}{2}}}$, and the force of that attraction to move the arm, is proportional to $\frac{b^3 \times (1-z)}{(a^2 + z^2l^2)^{\frac{3}{2}}}$; but the weight of the deal rod and wire at the point *x*, was before said to be $\frac{3490 - 1848x}{73.3} = \frac{1642 + 1848z}{73.3}$ per inch; therefore, if *dx* flows, the fluxion of the power to move the arm is $= l\dot{z} \times \frac{1642 + 1848z}{73.3} + \frac{b^3 \times (1-\dot{z})}{(a^2 + z^2l^2)^{\frac{3}{2}}} = \dot{z} \times (821 + 924z)$

$$\times \frac{b^3 \times (1-z)}{(a^2 + l^2z^2)^{\frac{3}{2}}} = \frac{b^3\dot{z} \times (821 + 103z - 924z^2)}{(a^2 + l^2z^2)^{\frac{3}{2}}} = \frac{b^3\dot{z} \times (821 + 103z + \frac{924a^2}{l^2})}{(a^2 + l^2z^2)^{\frac{3}{2}}}$$

$$- \frac{924b^3\dot{z} \times (\frac{a^2}{l^2} + z^2)}{(a^2 + l^2z^2)^{\frac{3}{2}}}$$; which, as $\frac{a^2}{l^2} = .08$, is $= \frac{b^3\dot{z} \times (895 + 103z)}{(a^2 + l^2z^2)^{\frac{3}{2}}} - \frac{924b^3\dot{z}}{l^2\sqrt{(a^2 + l^2z^2)}}$. The fluent of this is

$$= \frac{895b^3z}{a^2\sqrt{(a^2 + l^2z^2)}} - \frac{103b^3}{l^2\sqrt{(a^2 + l^2z^2)}} + \frac{103b^3}{l^2a} - \frac{924b^3}{l^3} \log. \frac{lz + \sqrt{(a^2 + l^2z^2)}}{a}$$; and the force with which the attraction of the weight, on the nearest half of the deal rod and wire, tends to move the arm, is proportional to this fluent generated while z flows from 0 to 1, that is, to 128 grains.

The force with which the attraction of the weight on the end-piece *de* tends to move the arm, is proportional to $47 \times \frac{b^3}{a^3}$, or 29 grains; and therefore the whole power of the weight to move the arm, by means of its attraction on the nearest part of it, is equal to its attraction on 157 grains placed at *b*, which is $\frac{157}{11260}$, or .0139 of its attraction on the ball. It must be observed, that the effect of the attraction of the weight on the whole arm is rather less than this, as its attraction on the farther half draws it the contrary way; but as the attraction on this is small, in comparison of its attraction on the nearer half, it may be disregarded.

The attraction of the weight on the farther ball, in the direction *bw*, is to its attraction on the nearer ball :: $wd^3 : w\mathrm{D}^3$ :: .0017 : 1; and therefore the effect of the attraction of the weight on both balls, is to that of its attraction on the nearer ball :: .9983 : 1.

To find the attraction of the copper rod on the nearer ball, let *b* and *w*, fig. 6, be the centres of the ball and weight, and *ea* the perpendicular part of the copper rod, which consists of 2 parts, *ad* and *de*: *ad* weighs 22000 grains, and is 16 inches long, and is nearly bisected by *w*; *de* weighs 41000, and is 46 inches long; *wb* is 8.85 inches, and is perpendicular to *ew*. Now the attraction of a line *ew*, of uniform thickness, on *b*, in the direction *bw*, is to that of the same quantity of matter placed at *w* :: *bw* : *eb*; therefore the attraction of the part *da* equals that of $\frac{22000 \times wb}{db}$, or 16300, placed at *w*; and the attraction of *de* equals that of $41000 \times \frac{ew}{ed} \times \frac{bw}{be} - 41000 \times \frac{dw}{ed} \times \frac{bw}{bd}$, or 2500, placed at the same point; so that the attraction of the perpendicular part of the copper rod on *b*, is to that of the weight on it, as 18800 : 2439000, or as .00771 to 1. As for the attraction of the inclined part of the rod and wooden bar, marked Pr and *rr* in fig. 1, it may safely be neglected, and so may the attraction of the whole rod on the arm and farthest ball; therefore the attraction of the weight and copper rod, on the arm and both balls together, exceeds the attraction of the weight on the nearest ball, in the proportion of .9983 + .0139 + .0077 to 1, or of 1.0199 to 1.

The next thing to be considered, is the attraction of the mahogany case. Now it is evident, that when the arm stands at the middle division, the attractions of the opposite sides of the case balance each other, and have no power to draw the arm either way. When the arm is removed from this division, it is attracted a little towards the nearest side, so that the force required to draw the arm aside is rather less than it would otherwise be; but yet, if this force is proportional to the distance of the arm from the middle division, it makes no error in the result; for though the attraction will draw the arm aside more than it would otherwise do, yet, as the accelerating force by which the arm is made to vibrate is diminished in the same proportion, the square of the time of a vibration will be increased in the same proportion as the space by which the arm is drawn aside, and therefore the result will be the same as if the case exerted no attraction; but if the attraction of the case is not proportional to the distance of the arm from the middle point, the ratio in which the accelerating force is diminished is different in different parts of the vibration, and the square of the time of a vibration will not be increased in the same proportion as the quantity by which the arm is drawn aside, and therefore the result will be altered by it.

On computation, I find that the force by which the attraction draws the arm from the centre is far from being proportional to the distance, but the whole force is so small as not to be worth regarding; for, in no position of the arm does the attraction of the case on the balls exceed that of $\frac{1}{5}$th of a spheric inch of water, placed at the distance of 1 inch from the centre of the balls; and the attraction of the leaden weight equals that of 10.6 spheric feet of water placed at 8.85 inches, or of 234 spheric inches placed at 1 inch distance; so that the attraction of the case on the balls can in no position of the arm exceed $\frac{1}{1170}$ of that of the weight. The compuration is given in the Appendix.

It has been shown therefore, that the force required to draw the arm aside 1 division, is greater than it would be if the arm had no weight, in the ratio of 1.0353 to 1, and therefore is $= \frac{1.0353}{818\text{N}^2}$ of the weight of the ball; also, the attraction of the weight and copper rod on the arm and both balls together, exceeds the attraction of the weight on the nearest ball, in the ratio of 1.0199 to 1, and therefore is $= \frac{1.0199}{8739000\text{D}}$ of the weight of the ball; consequently D is really equal to $\frac{818\text{N}^2}{1.0353} \times \frac{1.0199}{8739000\text{B}}$, or $\frac{\text{N}^2}{10844\text{B}}$, instead of $\frac{\text{N}^2}{10683\text{B}}$, as by the former computation. It remains to be considered how much this is affected by the position of the arm.

Suppose the weights to be approached to the balls; let W, fig. 7, be the centre of one of the weights; M the centre of the nearest ball at its mean position, as when the arm is at 20 divisions; let B be the point which it actually rests at; and A the point which it would rest at, if the weight was removed; consequently AB is the space by which it is drawn aside by means of the attraction; and let Mβ be the space by which it would be drawn aside, if the attraction on it was the same as when it is at M. But the attraction at B is greater than at M, in the proportion of $\text{WM}^2 : \text{WB}^2$; and therefore $\text{AB} = \text{M}\beta \times \frac{\text{WM}^2}{\text{WB}^2} = \text{M}\beta \times (1 + \frac{2\text{MB}}{\text{MW}})$ very nearly.

Let now the weights be moved to the contrary near position, and let w be now the centre of the nearest weight, and b the point of rest of the centre of the ball; then $\text{A}b = \text{M}\beta \times 1 + \frac{2\text{M}b}{\text{MW}}$, and $\text{B}b = \text{M}\beta \times 2 + \frac{2\text{M}b}{\text{MW}} + \frac{2\text{MB}}{\text{MW}} = 2\text{M}\beta \times (1 + \frac{\text{B}b}{\text{MW}})$; so that the whole motion Bb is greater than it would be if the attraction on the ball was the same in all places as it is at M, in the ratio of $1 + \frac{\text{B}b}{\text{MW}}$ to 1; and therefore does not depend sensibly on the place of the arm, in either position of the weights, but only on the quantity of its motion, by moving them.

This variation in the attraction of the weight affects also the time of vibration; for suppose the weights to be approached to the balls, let W be the centre of the nearest weight; let B and A represent the same things as before; and let x be the centre of the ball, at any point of its vibration; let AB represent the force with which the ball, when placed at B, is drawn towards A by the stiffness of the wire; then, as B is the point of rest, the attraction of the weight on it will also equal AB; and when the ball is at x, the force with which it is drawn towards A, by the stiffness of the wire, is $= \text{A}x$, and that with which it is drawn in the contrary direction, by the attraction, $= \mathrm{AB} \times \frac{\mathrm{BW}^2}{\mathrm{W}x^2}$; so that the actual force by which it is drawn towards A is $= \mathrm{A}x - \frac{\mathrm{AB} \times \mathrm{WB}^2}{\mathrm{W}x^2} = \mathrm{AB} + \mathrm{B}x - \mathrm{AB} \times (1 + \frac{2\mathrm{B}x}{\mathrm{WB}}) = \mathrm{B}x - \frac{2\mathrm{B}x \times \mathrm{AB}}{\mathrm{WB}}$, very nearly. So that the actual force with which the ball is drawn towards the middle point of the vibration, is less than it would be if the weights were removed, in the ratio of $1 - \frac{2\mathrm{AB}}{\mathrm{WB}}$ to 1, and the square of the time of a vibration is increased in the ratio of 1 to $1 - \frac{2\mathrm{AB}}{\mathrm{WB}}$; which differs very little from that of $1 + \frac{\mathrm{B}b}{\mathrm{MW}}$ to 1, which is the ratio in which the motion of the arm, by moving the weights from one near position to the other, is increased.

The motion of the ball answering to 1 division of the arm, is $= \frac{36.35}{20 \times 38.3}$; and if $m\mathrm{B}$ be the motion of the ball answering to d divisions on the arm, $\frac{\mathrm{MB}}{\mathrm{WM}} = \frac{36.35d}{20 \times 38.3 \times 8.85} = \frac{d}{185}$; therefore the time of vibration, and motion of the arm, must be corrected as follows: If the time of vibration is determined by an experiment in which the weights are in the near position, and the motion of the arm, by moving the weights from the near to the midway position, is d divisions, the observed time must be diminished in the subduplicate ratio of $1 - \frac{2d}{185}$ to 1, that is, in the ratio of $1 - \frac{d}{185}$ to 1; but when it is determined by an experiment in which the weights are in the midway position, no correction must be applied.

To correct the motion of the arm caused by moving the weights from a near to the midway position, or the reverse, observe how much the position of the arm differs from 20 divisions, when the weights are in the near position: let this be n divisions, then if the arm at that time be on the same side of the division of 20 as the weight, the observed motion must be diminished by the $\frac{2n}{185}$ part of the whole; but otherwise it must be as much increased. If the weights are moved from one near position to the other, and the motion of the arm be $2d$ divisions, the observed motion must be diminished by the $\frac{2d}{185}$ part of the whole. If the weights are moved from one near position to the other, and the time of vibration be determined while the weights are in one of those positions, there is no need of correcting either the motion of the arm, or the time of vibration.

CONCLUSION.

The following Table contains the Result of the Experiments.

Experiment.	Mot. weight.	Mot. arm.	Do. corr.	Time vib.	Do. corr.	Density.
1	m. to +	14.32	13.42	m. s.	.	5.50
	+ to m.	14.1	13.17	14.55	.	5.61
2	m. to +	15.87	14.69	.	.	4.88
	+ to m.	15.45	14.14	14.42	.	5.07
3	+ to m.	15.22	13.56	14.39	.	5.26
	m. to +	14.5	13.28	14.54	.	5.55
4	m. to +	3.1	2.95		6.54	5.36
	+ to −	6.18	.	7.1	.	5.29
	− to +	5.92	.	7.3	.	5.58
5	+ to −	5.9	.	7.5	.	5.65
	− to +	5.98	.	7.5	.	5.57
6	m. to −	3.03	2.9	.	.	5.53
	− to +	5.9	5.71	7.4 by mean.	.	5.62
7	m. to −	3.15	3.03		6.57	5.29
	− to +	6.1	5.9			5.44
8	m. to −	3.13	3.00		.	5.34
	− to +	5.72	5.54	.	.	5.79
9	+ to −	6.32	.	6.58	.	5.10
10	+ to −	6.15	.	6.59	.	5.27
11	+ to −	6.07	.	7.1	.	5.39
12	− to +	6.09	.	7.3	.	5.42
13	− to +	6.12	.	7.6	.	5.47
	+ to −	5.97	.	7.7	.	5.63
14	− to +	6.27	.	7.6	.	5.34
	+ to −	6.13	.	7.6	.	5.46
15	− to +	6.34	.	7.7	.	5.30
16	− to +	6.1	.	7.16	.	5.75
17	− to +	5.78	.	7.2	.	5.68
	+ to −	5.64	.	7.3	.	5.85

From this table it appears, that though the experiments agree pretty well together, yet the difference between them, both in the quantity of motion of the arm and in the time of vibration, is greater than can proceed merely from the error of observation. As to the difference in the motion of the arm, it may very well be accounted for, from the current of air produced by the difference of temperature; but whether this can account for the difference in the time of vibration, is doubtful. If the current of air was regular, and of the same swiftness in all parts of the vibration of the ball, I think it could not; but as there will most likely be much irregularity in the current, it may very likely be sufficient to account for the difference.

By a mean of the experiments made with the wire first used, the density of the earth comes out 5.48 times greater than that of water; and by a mean of those made with the latter wire, it comes out the same; and the extreme difference of the results of the 23 observations made with this wire, is only .75; so that the extreme results do not differ from the mean by more than .38, or $\frac{1}{14}$ of the whole, and therefore the density should seem thus to be determined, to great exactness. It may indeed be objected, that as the result appears to be influenced by the current of air, or some other cause, the laws of which we are not well acquainted with, this cause may perhaps act always, or commonly, in the same direction, and so make a considerable error in the result. But yet, as the experiments were tried in various weathers, and with considerable variety in the difference of temperature of the weights and air, and with the arm resting at different distances from the sides of the case, it seems very unlikely that this cause should act so uniformly in the same way, as to make the error of the mean result nearly equal to the difference between this and the extreme; and therefore it seems very unlikely that the density of the earth should differ from 5.48 by so much as $\frac{1}{14}$ of the whole.

Another objection perhaps may be made to these experiments, namely, that it is uncertain whether, in these small distances, the force of gravity follows exactly the same law as in greater distances. There is no reason however to think that any irregularity of this kind takes place, until the bodies come within the action of what is called the attraction of cohesion, and which seems to extend only to very minute distances. With a view to see whether the result could be affected by this attraction, I made the 9th, 10th, 11th, and 15th experiments, in which the balls were made to rest as close to the sides of the case as they could; but there is no difference to be depended on, between the results under that circumstance, and when the balls are placed in any other part of the case.

According to the experiments made by Dr. Maskelyne, on the attraction of the hill Schehallien, the density of the earth is 4½* times that of water; which differs rather more from the preceding determination than I should have expected. But I forbear entering into any consideration of which determination is most to be depended on, till I have examined more carefully how much the preceding determination is affected by irregularities whose quantity I cannot measure.

APPENDIX.—*On the Attraction of the Mahogany Case on the Balls.*

The first thing is, to find the attraction of the rectangular plane *ckβb* (fig. 8,) on the point *a*, placed in the line *ac* perpendicular to this plane.

Let $ac = a$, $ck = b$, $cb = x$, and let $\frac{a^2}{a^2 + x^2} = w^2$, and $\frac{b^2}{a^2 + x^2} = v^2$, then the attraction of the line $b\beta$ on a, in the direction ab, is $= \frac{b\beta}{ab + a\beta}$; and therefore, if cb flows, the fluxion of the attraction of the plane on the point a, in the direction cb, is

$$= \frac{b\dot{x}}{\sqrt{a^2 + x^2} \times \sqrt{a^2 + b^2 + x^2}} \times \frac{x}{\sqrt{a^2 + x^2}} = \frac{-b\dot{w}}{w\sqrt{b^2 + \frac{a^2}{w^2}}} = \frac{-b\dot{w}}{\sqrt{b^2w^2 + a^2}} = \frac{-\dot{v}}{\sqrt{1 + v^2}},$$

the variable part of the fluent of which is $= -\log. v + \sqrt{1 + v^2}$, and therefore the whole attraction is $= \log. (\frac{ck + ak}{ac} \times \frac{ab}{b\beta + a\beta};)$ so that the attraction of the plane, in the direction cb, is found readily by logarithms, but I know no way of finding

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* The mean density of the earth by that experiment, has since been found to be nearly 5, or 5 times that of water, by taking the real density of the hill, instead of one that was assumed below the truth. See p. 420, vol. 14, of these abridgments.

its attraction in the direction *ac*, except by an infinite series. The two most convenient series I know, are the following;

First series. Let $\frac{b}{a} = \pi$, and let A = arc whose tang. is π, $B = A - \pi$, $C = B + \frac{\pi^3}{3}$, $D = C - \frac{\pi^5}{5}$, &c. then the attraction in the direction $ac = \sqrt{1 - w^2} \times (A + \frac{Bw^2}{2} + \frac{3Cw^4}{2.4} + \frac{3.5w^6}{2.4.6}$, &c.)

For the second series, let A = arc whose tang. $= \frac{1}{\pi}$, $B = A - \frac{1}{\pi}$, $C = B + \frac{1}{3\pi^3}$, $D = C - \frac{1}{5\pi^5}$, &c. then the attraction = arc $90^\circ - \sqrt{[(1 + v^2) \times (A - \frac{Bv^2}{2} + \frac{3Cv^4}{2.4} - \frac{3.5Dv^6}{2.4.6}}$, &c.)]

It must be observed, that the first series fails when π is greater than unity, and the 2d, when it is less; but if *b* is taken equal to the least of the 2 lines *ck* and *cb*, there is no case in which one or the other of them may not be used conveniently.

By the help of these series, I computed the following table.

	.1962	.3714	.5145	.6248	.7071	.7808	.8575	.9285	.9815	1.
.1962	.00001									
.3714	.00039	00148								
.5145	.00074	00277	00521							
.6248	00110	00406	00778	01183						
.7071	00140	00522	01008	01525	02002					
.7808	00171	00637	01245	01896	02405	03247				
.8575	00207	00772	01522	02339	03116	03964	05057			
.9285	00244	00910	01810	02807	03778	04867	06319	08119		
.9815	00271	01019	02084	03193	04368	05639	07478	09931	12849	
1.	00284	01054	02135	03347	04560	05975	07978	10789	14632	19612

Find in this table, with the argument $\frac{ck}{ak}$ at top, and the argument $\frac{cb}{ab}$ in the left hand column, the corresponding logarithm; then add together this logarithm, the logarithm of $\frac{ck}{ak}$, and the logarithm of $\frac{cb}{ab}$; the sum is logarithm of the attraction.

To compute from hence the attraction of the case on the ball, let the box DCBA, fig. 1, in which the ball plays, be divided into 2 parts, by a vertical section, perpendicular to the length of the case, and passing through the centre of the ball; and, in fig. 9, let the parallelopiped ABDE*abde* be one of these parts, ABDE being the above-mentioned vertical section; let *x* be the centre of the ball, and draw the parallelogram $\beta npm\delta x$ parallel to B*bd*D, and *xgrp* parallel to βB*bn*, and bisect $\beta\delta$ in *c*. Now the dimensions of the box, on the inside, are B*b* = 1.75; BD = 3.6; Bβ = 1.75; and βA = 5; whence I find, that if *xc* and βx be taken as in the 2 upper lines of the following table, the attractions of the different parts are as follows.

xc	.75	.5	.25
βx	1.05	1.3	1.55
Excess of attraction of D*drg* above B*brg*	.2374	.1614	.0813
——— *mdrp* above *nbrp*	.2374	.1614	.0813
——— *mesp* above *nasp*	.3705	.2516	.1271
Sum of these	.8453	.5744	.2897
Excess of attraction of B*bn*β above D*dm*δ	.5007	.3271	.1606
——— A*an*β above E*em*δ	.4677	.3079	.1525
Whole attraction of the inside surface of the half box	.1231	.0606	.0234

It appears therefore, that the attraction of the box on x increases faster than in proportion to the distance xc.

The specific gravity of the wood used in this case is .61, and its thickness is $\frac{3}{4}$ of an inch; therefore, if the attraction of the outside surface of the box was the same as that of the inside, the whole attraction of the box on the ball, when $cx = .75$, would be equal to $2 \times .1231 \times .61 \times \frac{3}{4}$ cubic inches, or, .201 spheric inches of water, placed at the distance of 1 inch from the centre of the ball. In reality, it can never be so great as this, as the attraction of the outside surface is rather less than that of the inside; and besides, the distance of x from c can never be quite so great as .75 of an inch, as the greatest motion of the arm is only $1\frac{1}{2}$ inch.

XXII. *An Improved Solution of a Problem in Physical Astronomy; by which, swiftly converging Series are obtained, which are useful in computing the Perturbations of the Motions of the Earth, Mars, and Venus, by their mutual Attraction. To which is added an Appendix, containing an easy Method of obtaining the Sums of many slowly converging Series which arise in taking the Fluents of binomial Surds, &c. By the Rev. John Hellins, F. R. S.* p. 527.

It was with much diffidence that I entered on a speculation which had engaged the attention of such learned men as Simpson, Euler, and La Grange. Considering the great abilities of these men, and the length of time which Euler, in particular, appears to have employed on the subject, all that I at first expected to effect was, to facilitate the summation of the slowly converging series by means of which they had computed the perturbations of the motions of the planets in their orbits, which arise from their actions on one another, by the force of gravity; and that this might be done by a method which I had some time before discovered, was evident, on inspecting their series. Here probably I should have stopped, had not Dr. Maskelyne put into my hands a sheet of paper, written by the late Mr. Simpson, which, though very ingenious, was by mistakes, which seem to have entered in transcribing it, rendered unintelligible to some eminent mathematicians who had perused it; in which state it had remained 36 years. On perusing this paper, the first thing that occurred to me was, a different method of finding the fluent, from that which had been used by Mr. Simpson; by which means, series converging by the powers of $\frac{1}{6}$ were obtained, while the series brought out the common way lost all convergency by a geometrical progression, and a computation by it was more difficult than the computation of the length of a quadrantal arch of the circle by the series $1 + \frac{1}{2.3} + \frac{3}{2.4.5} + \frac{3.5}{2.4.6.7}$, &c. I afterwards discovered the method of transforming that series which had lost all convergency by a geometrical progression, into another in which the literal powers decrease very swiftly; which is the improvement now offered.

In comparing the series here produced, for computing the values of A and B in the equation $(a - b \times \cos. z)^{-n} = \mathrm{A} + \mathrm{B} . \cos. z + \mathrm{C} . \cos. 2z + \mathrm{D} . \cos. 3z +$ &c. with those which have been published for that purpose, by Messrs. Euler and la Grange, it will appear, that those cases which were the most difficult to be computed by their methods, are the most easy by mine. For instance, if Venus's perturbation of the motion of the earth were to be computed, and vice versâ, the literal powers which have place in Euler's series, would be very nearly equal to the powers of $\frac{9}{10}$; the literal powers which have place in la Grange's series, would be nearly equal to the powers of $\frac{1}{2}$; and in the series now produced the literal powers would decrease somewhat swifter than the powers of $\frac{1}{36}$.

M. la Grange has indeed, by a very ingenious device, obtained a convergency in the numeral co-efficients of the series that he uses, which, for the first 5 terms of it, is nearly equal to the powers of $\frac{1}{4}$; but this convergency becomes less and less in every succeeding term, and the co-efficients approach pretty fast to a ratio of equality; so that, to obtain the sum of the series to 6 places of decimals, he proposes to compute the first 10 terms of it. The case in which those co-efficients have that convergency, is when n (which answers to his s,) is $= -\frac{1}{2}$, a case which does not often happen; however, from the values of A and B, when $n = -\frac{1}{2}$, he derives their values when $n = \frac{1}{2}, \frac{3}{2}$, &c. by another very ingenious device, worthy of that skill for which he is justly celebrated. But by the method now proposed the chief part of the convergency is in the literal powers; and such a difference in the numeral co-efficients, for a different value of n, does not take place.

For Mars's perturbation of the earth's motion, the literal powers by which the 3 different series converge, are nearly as follows:

$$\left.\begin{array}{l}\text{M. Euler's, ...}\\ \text{M. la Grange's, ...}\\ \text{The series now proposed,}\end{array}\right\}\text{by the powers of}\left\{\begin{array}{l}\frac{4}{5};\\ \frac{10}{23};\\ \frac{1}{20}.^{*}\end{array}\right.$$

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* For obtaining nearly the different rates of convergency of the literal powers in the 3 series, it will be sufficient to consider the distance of the 2 planets of which the perturbations are to be computed, as $= \sqrt{(\mathrm{RR} + rr - 2\mathrm{R}r \times c, z,)}$ where R and r denote their mean distances from the sun, of which R

If indeed the perturbation which arises from the action of Jupiter on the earth was to be computed, la Grange's series would be the best that has hitherto been published for the purpose, as the literal powers of it would, in that case, be nearly equal to the powers of $\frac{1}{27}$, while the literal powers in the new series would differ but little from those of $\frac{11}{24}$. So that, for computing the perturbation of each of these 3 planets, we now have series converging so very swiftly, that the first 4 terms are sufficient for the purpose. These indeed are the perturbations of motion, arising from the actions of the planets, which the inhabitants of this globe have most frequent occasion to compute. And since 2 of the 3 are most easily calculated by the method explained in the following pages, I am not without hopes that I have rendered an acceptable piece of service to astronomers in general, and more especially to those who are most intent on improving astronomical tables.

But it may be proper to remark, that the use of the new series is not confined to the computations just mentioned, but may successfully be used in computing the perturbations of the motions of other planets. For instance, in the computation of the perturbation of Saturn's motion by Jupiter, and vice versâ, the convergency of this series will be nearly by the powers of $\frac{1}{12}$, which is a swift rate of convergency. And, for the perturbation of the Georgium sidus by Saturn, and vice versâ, the series will converge nearly by the powers of $\frac{1}{9}$, which is also swiftly. And it is further to be remarked, that in the last instance, and indeed whenever the radii of the orbits of the 2 planets differ from each other in the ratio of 2 to 1, M. la Grange's series may be used with advantage, since the convergency of the first 5 terms of it will then be nearly by the powers of $\frac{1}{16}$; the numeral co-efficients of those terms converging as swiftly as the literal powers do in that case. And when the ratio of the 2 radii is greater than that of 2 to 1, his series will converge more swiftly.

An improved Solution of a Problem in Physical Astronomy, &c.

1. The perturbation of the motions of the planets in their orbits, by their actions on each other, is a curious phenomenon, which, while it affords to the philosopher a clear proof of the general attraction of matter, produces a problem of no small difficulty to the astronomer; viz. to compute the quantity by which a planet, so acted on, deviates from an ellipsis in its course round the sun: a pro-

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is the greater, and c, z the cosine of the angle of commutation. Then will M. la Grange's series converge by the powers of the quantity $\frac{rr}{\mathrm{RR}}$; and, since $\mathrm{RR} + rr = a$, and $2\mathrm{R}r = b$, in our notation, and the converging quantity in M. Euler's series is $(nn) = \frac{bb}{aa}$, it will be $= \frac{4\mathrm{R}^2r^2}{(\mathrm{RR} + rr)^2}$; and cc, by the powers of which the new series converges, is $= \frac{a - b}{a + b} = \frac{\mathrm{RR} - 2\mathrm{R}r + rr}{\mathrm{RR} + 2\mathrm{R}r + rr} = \frac{(\mathrm{R} - r)^2}{(\mathrm{R} + r)^2}$. See the Memoirs of the Royal Academy of Sciences and Belles-Lettres at Berlin, for 1781, p. 257; M. Euler's Institutiones Calculi Integralis, vol. 1, p. 186; and art. 4, in what follows.—Orig.

blem which has called forth the skill of several of the most learned philosophers and astronomers of the last and present age.

A preparatory step to the solution of this problem is, to find a convenient expression for the reciprocal of the cube, or rather of the n^{th} power, of the distance of any 2 planets. Such an expression was first given by Euler, in series proceeding by the cosines of the multiples, in arithmetic progression, of the angle of commutation; but the calculations of the first 2 co-efficients in it were very laborious, requiring the summation of series of the common form, which converged very slowly. Afterwards, other series were discovered by other authors, by which the same co-efficients might be computed with less labour; the best of which, that I have seen, appear to be those that were pointed out to me by Dr. Maskelyne, invented by la Grange, and published in the memoirs of the Royal Academy of Sciences at Berlin, for the year 1781. Yet the calculation of the first 2 co-efficients, A and B, for the perturbations of Mars, Venus, and the earth, by his method, is not shorter, if it be so short as by my method, to the investigation of which I now proceed.

PROB.—2. To determine the values of A, B, C, D, &c. in the equation $\frac{\dot{z}}{(a - b \, . \cos. z)^n} = \dot{z}\,(A + B \, . \cos. z + C \, . \cos. 2z + D \, . \cos. 3z$, &c.) z being the arch of a circle of which the radius is 1, and b less than a.

First, to find the co-efficient A.—3. The fluent of the right-hand side of this equation is $Az + B \, . \sin. z + \frac{1}{2}C \, . \sin. 2z + \frac{1}{3}D \, . \sin. 3z + \frac{1}{4}E \, . \sin. 4z$*, &c. which evidently vanishes when $z = 0$; and when $z = 3{\cdot}14159$, &c. the arch of 180°, it becomes barely $= Az$, the sines of z, $2z$, $3z$, &c. being then each $= 0$. Therefore if the fluent of the first side of the equation be taken, the increase of it, while z increases from 0 to $3{\cdot}14159$ &c. $= \pi$, will be $= \pi A$; and consequently A will be determined.

4. Now, to find the fluent of $\frac{\dot{z}}{(a - b \, . \cos. z)^n} = \frac{-\dot{x}}{\sqrt{(1 - xx)}\ (a - bx)^n}$, x being put = the cosine of z; in which expressions, while z increases from 0 to $3{\cdot}14159$, x will decrease from 1 to − 1. Therefore, to obtain a more convenient expression, put $vv = \frac{a - bx}{a + b}$; then, while x decreases from 1 to − 1, vv will increase from $\frac{a - b}{a + b}$ to $\frac{a + b}{a + b} = 1$; and we shall have the following equation: $\frac{-\dot{x}}{\sqrt{(1 - xx)}\ (a - bx)^n} = (a + b)^{-n} \times \frac{2\dot{v}v^{1-2n}}{\sqrt{(1 - vv)}\ \sqrt{(vv - cc)}}$; the fluent of which may be found when the value of n is given.

5. Now, the values of n with which astronomers are most concerned, are $\frac{3}{2}$ and $\frac{5}{2}$. Let therefore $\frac{3}{2}$ be written for n, and the radical quantity $\sqrt{(1 - vv)}$ be converted into series, then the last expression will be

$$= (a + b)^{-\frac{3}{2}} \times \frac{2\dot{v}v^{-2}}{\sqrt{(vv - cc)}} \left(1 + \frac{vv}{2} + \frac{3v^4}{2.4} + \frac{3.5v^6}{2.4.6} + \frac{3.5.7v^8}{2.4.6.8}, \text{\&c.}\right)$$

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* See Euler's Institutiones Calculi Integralis, vol. 1, p. 150—Orig.

$$= (a + b)^{-\frac{3}{2}} \times \begin{cases} \dfrac{2\dot{v}v^{-2}}{\surd(vv - cc)} \\ + \dfrac{\dot{v}}{\surd(vv - cc)} \left(1 + \dfrac{3vv}{4} + \dfrac{3.5v^4}{4.6} + \dfrac{3.5.7v^6}{4.6.8}, \&c.\right) \end{cases}$$

And the fluents of these several terms being taken, and collected together, then the whole multiplied by the common factor $(a + b)^{-\frac{3}{2}}$, the fluent sought will be

$$(a + b)^{-\frac{3}{2}} \times \begin{cases} \dfrac{2\surd(vv - cc)}{ccv} \\ + \alpha + \dfrac{3}{4}\beta + \dfrac{3.5}{4.6}\gamma + \dfrac{3.5.7}{4.6.8}\delta, \&c. \end{cases}$$

6. We must now inquire what value this series has when $z = 0$; in which case, x being $= 1$, vv is $= \frac{a - b}{a + b} = cc$. And it will appear that, with this value of vv, every term of the series vanishes, so that the fluent needs no correction. If, therefore, we compute the value of this series when $z = \pi$, i. e. when $x = -1$, and $vv = \frac{a + b}{a + b} = 1$, we shall have the value of $\text{A}\pi$, and consequently, A will be determined. But, with this value of v, the terms β, γ, δ, &c. lose all convergency by the geometrical progression, v, v^3, v^5, &c. and the computation of the value of the series, by the common method, would be more laborious than the computation of the quadrantal arch of the circle, by the series $1 + \frac{1}{2.3} + \frac{3}{2.4.5} + \frac{3.5}{2.4.6.7}$, &c. Here then we are stopped. But, by contemplating this series, expressed in terms of α and c, and by making various ingenious transformations, in this and the 7th, 8th, and 9th articles, Mr. H. at length obtains

$$\text{A} = \frac{1}{\pi(a + b)^{\frac{3}{2}}} \begin{cases} \dfrac{8 - 2cc}{8 - 5cc}\alpha \\ + \surd(1 - cc) \,.\, \left(\dfrac{2}{cc} + \lambda + \mu cc + \nu c^4\right) \end{cases}$$

10. The value of A when $n = \frac{3}{2}$ being now found, let us next investigate the value of it when $n = \frac{5}{2}$; which, for the sake of distinction, in a use to be made of it in a subsequent article, Mr. H. denotes by A′. Here, by proceeding in a way similar to the foregoing, in this article, and the 11th, 12th, 13th, the author at length obtains for the value of the arc in this case, the following form, viz.

$$\text{A}' = \frac{1}{\pi(a + b)^{\frac{5}{2}}} \times \begin{cases} \dfrac{96 - 23cc}{128 - 84cc}\alpha \\ + \surd(1 - cc) \,.\, \left(\dfrac{4 + 5cc}{3c^4} + \lambda' + \mu' cc + \nu' c^4\right). \end{cases}$$

2dly, *to find the Coefficient* B.

14. Multiply the equation in art. 2. by 2 cos. z $= 2x$, and it gives $\frac{2x\dot{z}}{(a - bx)^n} = \dot{z}$ (A × 2 cos. z + B × 2 cos. z × cos. z + C × 2 cos. z × cos. 2z + D × 2 cos. z × cos. 3z, &c.); which, because 2 cos. z × cos. mz is = cos. $(m - 1)$ z + cos. $(m + 1)$ z, will be $= \dot{z}$ (2A . cos. z + B (1 + cos. 2z) + C (cos. z + cos. 3z) + D (cos. 2z + cos. 4z), &c.).* And, by taking the fluents, we have $\int \frac{2x\dot{z}}{(a - bx)^n} = 2\text{A}$.

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* See Simpson's Miscellaneous Tracts, lemma i, p. 76.

$\sin. z + \text{B}z + \frac{1}{2}\text{B} . \sin. 2z + \text{C} (\sin. z + \frac{1}{3} \sin. 3z) + \text{D} (\frac{1}{2} \sin. 2z + \frac{1}{4} \sin. 4z)$, &c.; which equation, when $z = 3{\cdot}14159$, &c. $= \pi$, becomes $\int \frac{2x\dot z}{(a - bx)^n} =$ barely $\text{B}z = \text{B}\pi$, the sines of z, $2z$, $3z$, &c. being then $= 0$.

15. Now it appears, by the notation in art. 4, that $\frac{\dot z}{(a - bx)^n} = (a + b)^{-}{}_{n} \times \frac{2\dot v v^{1-2n}}{\sqrt{(1 - vv)}\ \sqrt{(vv - cc)}}$, and that $x = \frac{a - (a + b)\,vv}{b}$; we therefore have, by proper substitution,

$$\frac{2x\dot z}{(a - bx)^n} = \frac{-2x\dot x}{\sqrt{(1 - xx)}\ (a - bx)^n} = \left.\begin{matrix} \dfrac{2a}{b(a + b)^n} \times \dfrac{2\dot v v^{1-2n}}{\sqrt{(1 - vv)}\ (\sqrt{(vv - cc)}} \\ \dfrac{-2}{b\,(a + b)^{n-1}} \times \dfrac{2\dot v v^{3-2n}}{\sqrt{(1 - vv)}\ \sqrt{(vv - cc)}} \end{matrix}\right\},$$

of which 2 fluxions the fluents may be found, when n has any particular value.

16. First, let n be $\frac{3}{2}$; then the last expression in the preceding article becomes $\frac{2a}{b(a + b)^{\frac{3}{2}}} \times \frac{2\dot v v^{-2}}{\sqrt{(1 - vv)}\sqrt{(vv - cc)}} - \frac{2}{b(a + b)^{\frac{1}{2}}} \times \frac{2\dot v}{\sqrt{(1 - vv)}\sqrt{vv - cc)}}$. Now, the fluent of the affirmative part of this expression is evidently $= \frac{2a}{b} \times$ the fluent of the fluxion in art. 5, that is, $= \frac{2a}{b}\text{A}\pi$, and the negative part, by converting $\sqrt{(1 - vv)}$ into series, will become $\frac{-2}{b(a + b)^{\frac{1}{2}}} \times \frac{2\dot v}{\sqrt{(vv - cc)}} (1 + \frac{vv}{2} + \frac{3v^4}{2 . 4} + \frac{3 . 5v^6}{2 . 4 . 6}$, &c.); the fluent of which appears, by art. 5, to be $\frac{-2}{b(a + b)^{\frac{1}{2}}} (\alpha + \beta + \frac{3\gamma}{4} + \frac{3 . 5\delta}{4 . 6} + \frac{3 . 5 . 7\varepsilon}{4 . 6 . 8}$, &c.), which will vanish when $v = c$, and therefore needs no correction; and after further transformations and reductions, for the value of the co-efficient B, the ingenious author obtains the following expression:

$$\text{B} = \frac{2a}{b}\text{A} - \frac{2}{\pi b(a + b)^{\frac{1}{2}}} \times \left\{\begin{matrix} \dfrac{32 - 10cc}{16 - 9cc}\,\alpha \\ + \sqrt{(1 - cc)}\ (\rho + \sigma cc + \tau c^4), \end{matrix}\right.$$

which is its value when $n = \frac{3}{2}$.

17. We are next to find the value of this co-efficient, when $n = \frac{5}{2}$; which, for the sake of distinction, he denotes by B′. With this value of n, the fluxionary expression in art. 15, becomes

$\frac{2a}{b(a + b)^{\frac{5}{2}}} \times \frac{2\dot v v^{-4}}{\sqrt{(1 - vv)}\sqrt{(vv - cc)}} - \frac{2}{b(a + b)^{\frac{3}{2}}} \times \frac{2\dot v v^{-2}}{\sqrt{(1 - vv)}\sqrt{(vv - cc)}}$; which being compared with the fluxions in art. 5 and 10, it will appear that the fluent of the former part, when $v = 1$, is $= \frac{2a}{b}\text{A}'\pi$, and that the fluent of the latter part is $= \frac{-2}{b}\text{A}\pi$; which fluents, taken together, are, by art. 14, $= \text{B}'\pi$. Therefore we have $\text{B}' = \frac{2a}{b}\text{A}' - \frac{2}{b}\text{A} = \frac{2}{b}(\text{A}'a - \text{A})$.

3dly, *to find the Values of* C, D, E, *&c.*

18. The values of the co-efficients A and B being now found, corresponding to the values of n, $\frac{3}{2}$ and $\frac{5}{2}$, we might proceed in the same manner to find the value of C. For, if the equation in art. 2, be multiplied by $2 \cos. 2z$, and $\cos. (m - 2)z + \cos. (m + 2)z$ be written for $2 \cos. 2z \times \cos. mz$, it will become

$\frac{\dot{z} \times 2 \cos. 2z}{(a - b \times \cos. z)^n} = \dot{z}\,(2\text{A} \times \cos. 2z + \text{B}\,(\cos. z + \cos. 3z) + \text{C}\,(1 + \cos. 4z) + \text{D}$ $(\cos. z + \cos. 5z)$, &c.) And the sum of the fluents on the right-hand side, when $z = \pi$, will become barely $\text{C}z = \text{C}\pi$. Therefore, the fluent of the left-hand side of the equation, when $z = \pi$, will be $= \text{C}\pi$. The fluent of this fluxion, it is evident, will consist of 3 parts, the 1st and 2d of which, n being $= \frac{3}{2}$, are obviously attainable from the values of A and B above found in art. 9 and 16; and the 3d in series similar to those which have been given in the former part of this paper.

It is evident also that, if n be $= \frac{1}{2}$, all the 3 parts of this fluent are attainable from the values of the 2 co-efficients already found, and C′ would be $= -2\text{A}' + \frac{2}{b}\,(\text{B}'a - \text{B})$.

19. And in this manner may the other co-efficients, D, E, F, &c. be determined. And since the cosines of $3z$, $4z$, &c. are $= 4x^3 - 3x$, $8x^4 - 8x^2 + 1$, &c. respectively; and since $x = \frac{a - (a + b)vv}{b}$, it is evident that the numerator of the fraction into which the fluxion in the preceding article is to be multiplied, will be always of this form, viz. $p + qvv + rv^4 + sv^6$, &c.; from which it follows, that if the values of A′, A, $\underset{\prime}{\text{A}}$, &c. corresponding to n, $n - 1$, $n - 2$, &c. be computed, the values of C, D, E, F′, and all the rest, may be found in terms of A′, A, $\underset{\prime}{\text{A}}$, &c. with the co-efficients a and b. But, since the easiest method, that has come to my hands, of computing the values of C, D, E, &c. after A and B are found, is explained in M. Euler's Institutiones Calculi integralis, vol. 1, p. 181, I shall not pursue this method any further; but, having examined his process, and corrected the errors of the press which occur in it, now give the equations expressing the values of C, D, E, F, &c. which were obtained by that method.

20. For the sake of brevity, let $\frac{a}{b} = d$; then will the general values of C, D, E, F, &c. be expressed by these equations:

$$\text{C} = \frac{2n\text{A} - 2d\text{B}}{n - 2};\ \text{D} = \frac{(n + 1)\,\text{B} - 4d\text{C}}{n - 3};\ \text{E} = \frac{(n + 2)\,\text{C} - 6d\text{D}}{n - 4};\ \text{F} = \frac{(n + 3)\,\text{D} - 8d\text{E}}{n - 5},\ \text{\&c.}$$

where the law of continuation is very obvious. And the particular values of these letters, when $n = \frac{1}{2}, \frac{3}{2}, \frac{5}{2}$, will be as expressed in the following columns:

$n = \frac{1}{2}$	$n = \frac{3}{2}$	$n = \frac{5}{2}$
$\text{C} = \frac{4d}{3}\text{B} - \frac{4}{3}\text{A}$	$\frac{4d}{1}\text{B} - 6\text{A}$	$-4d\,\text{B} + 10\,\text{A}$
$\text{D} = \frac{8d}{5}\text{C} \quad \frac{3}{5} \quad \text{B}$	$\frac{8d}{3}\text{C} - \frac{5}{3}\text{B}$	$\frac{8d}{1}\text{C} - \frac{7}{1}\text{B}$
$\text{E} = \frac{12d}{7}\text{D} - \frac{5}{7}\text{C}$	$\frac{12d}{5}\text{D} - \frac{7}{5}\text{C}$	$\frac{12d}{3}\text{D} - \frac{9}{3}\text{C}$
$\text{F} = \frac{16d}{9}\text{E} - \frac{7}{9}\text{D}$	$\frac{16d}{7}\text{E} - \frac{9}{7}\text{D}$	$\frac{16d}{5}\text{E} - \frac{11}{5}\text{D}$
&c.	&c.	&c.

21. The solution of the problem being now finished, it may perhaps be satisfactory to the reader to see how the sums of the very slowly converging numerical

series, which arose in art. 7, 11, and 16, were obtained; the investigations of which, because they would have detained him too long from the immediate subject of this paper, if they had been inserted in it, are given in the following appendix.

An Appendix to the foregoing Paper, in which the method of obtaining the sums of the very slowly converging numerical series which are there used, and of many others of that kind which arise in the fluents of binomial surds, is explained and illustrated; and some observations, tending to facilitate and abridge the computations of the co-efficients A and B, are added.

1. As the sums of the very slowly converging numerical series, which arose in several articles of the preceding paper, are not exhibited in any book that has come to my hands, and as series of that kind frequently occur, I conceive that the following method of obtaining their sums will be acceptable to the lovers of mathematics in general, and particularly to those who have frequent occasion to use the sums of such series. And having observed, while considering the literal expressions in the preceding paper for the values of A and B, that others, no less accurate, might be derived from them, by which the arithmetical operations would be facilitated and abridged, I thought these observations might likewise be acceptable to those who are engaged in the theory of astronomy, and have inserted them also in this paper; which therefore consists of 2 principal parts, the summation of the slowly converging series, and the observations now mentioned.

1. *The Summation of the slowly converging Series.*—2. But, before beginning the investigation, it will be proper to premise a few particulars, an attention to which will shorten and facilitate the operations now to be performed.

1st. That $\frac{1-\surd(1-yy)}{1+\surd(1-yy)}$ being $= \frac{1-\surd(1-yy)}{1+\surd(1-yy)} \times \frac{1+\surd(1-yy)}{1+\surd(1-yy)}$, is $= (\frac{y}{1+\surd(1-yy)})^2$; from which it follows, that H. L. of $\frac{1-\surd(1-yy)}{1+\surd(1-yy)}$ is $= 2$ H. L. $\frac{y}{1+\surd(1-yy)}$.

2dly. That the fluxion of H. L. $\frac{2}{1+\surd(1-yy)}$ is $= \frac{\dot{y}}{y\surd(1-yy)} - \frac{\dot{y}}{y}$. For it is $=$ the fluxion of $-$ H. L. $(1+\surd(1-yy)) = \frac{y\dot{y}}{\surd(-yy)} \times \frac{1}{1+\surd(1-yy)}$; and if both numerator and denominator of this expression be multiplied by $1-\surd(1-yy)$, it will become $\frac{y\dot{y}}{\surd(1-yy)} \times \frac{1-\surd(1-yy)}{yy}$, which is $= \frac{\dot{y}}{y\surd(1-yy)} - \frac{\dot{y}}{y}$.

3dly. That the H. L. $\frac{2}{1+\surd(1-yy)}$ is therefore $= \int\frac{\dot{y}}{y\surd(1-yy)} - \int\frac{\dot{y}}{y} = \frac{yy}{2.2} + \frac{3y^4}{2.4.4} + \frac{3.5y^6}{2.4.6.6} + \frac{3.5.7y^8}{2.4.6.8.8}$, &c.

4thly. That Q being put $=\surd(1-yy)$, the fluxion of $\frac{Q}{y^n}$ will be $= \frac{\dot{y}}{Q}(\frac{-n}{y^{n+1}} + \frac{n-1}{y^{n-1}})$. For it will be $\frac{\dot{Q}}{y^n} - \frac{n\dot{y}Q}{y^{n+1}} = \frac{-y\dot{y}}{y^nQ} - \frac{n\dot{y}Q^2}{y^{n+1}Q} = \frac{-\dot{y}}{y^{n-1}Q} \quad \frac{n\dot{y}(1-yy)}{y^{n+1}Q} = \frac{-n\dot{y}}{y^{n+1}Q} + \frac{(n-1)\dot{y}}{y^{n-1}Q} = \frac{\dot{y}}{Q}(\frac{-n}{y^{n+1}} + \frac{n-1}{y^{n-1}})$.

5thly. That, when any quantities, as $\boxed{\dot{u}(\frac{f}{y^4} + \frac{g}{yy} + h)}$, are circumscribed by a parallelogram, it denotes that a substitution for these quantities has been made in the same equation in which it occurs, and consequently that they are no longer to be considered as part of that equation. This I have found to be better than cancelling, as it answers the same end without obliteration.

3. As it does not seem necessary to set down the operations of computing the sums of all the series which arose in the preceding paper, I shall make choice of the summation of those which, being the most difficult, are the most proper examples to illustrate this method.

It is well known that the expression

$$\frac{2\dot{y}y^{-5}}{\sqrt{(1-yy)}} \text{ is } = 2\dot{y}y^{-5} + \frac{2\dot{y}y^{-3}}{2} + \frac{2.3\dot{y}y^{-1}}{2.4} + \frac{2.3.5\dot{y}y}{2.4.6} + \frac{2.3.5.7\dot{y}y^{3}}{2.4.6.8} + \frac{2.3.5.7.9\dot{y}y^{5}}{2.4.6.8.10},$$

&c. from which equation we have

$$\frac{2\dot{y}y^{-5}}{(1-yy)} - 2\dot{y}y^{-5} - \dot{y}y^{-3} - \frac{3\dot{y}y^{-1}}{4} = \frac{3.5\dot{y}y}{4.6} + \frac{3.5.7\dot{y}y^{3}}{4.6.8} + \frac{3.5.7.9\dot{y}y^{5}}{4.6.8.10}, \text{ \&c.}$$

Now the fluents of the terms on the 1st side are

$$\left\{\begin{array}{l} \dfrac{-\sqrt{(1-yy)}}{2y^{4}} - \dfrac{3\sqrt{(1-yy)}}{4y^{2}} \boxed{+\dfrac{3}{4}\text{H. L. }\dfrac{y}{1+\sqrt{(1-yy)}}} \\ + \dfrac{1}{2y^{4}} + \dfrac{1}{2yy} \boxed{-\dfrac{3}{4}\text{H. L. }y} + \dfrac{3}{4}\text{H. L. }\dfrac{1}{1+\sqrt{(1-yy)}}; \end{array}\right.$$

on the 2d side, the fluents are $\frac{3.5yy}{4.6.2} + \frac{3.5.7y^{4}}{4.6.8.4} + \frac{3.5.7.9y^{6}}{4.6.8.10.6}$, &c. And, to find whether these 2 expressions are = each other, or have a constant difference, we may compute their numerical values, y being put = any small simple fraction, such as $\frac{1}{10}$, $\frac{1}{100}$, or $\frac{1}{1000}$, either of which values of y is a very convenient one for the purpose. But an easier method to discover the constant quantities which lie concealed in some of the terms on the first side, is to convert that side into series, by the binomial theorem; which will then be as follows:

$$\frac{-\sqrt{(1-yy)}}{2y^{4}} = -\tfrac{1}{2}y^{-4} + \tfrac{1}{4}y^{-2} + \tfrac{1}{16} + \tfrac{1}{32}y^{2} + \tfrac{5}{256}y^{4}, \text{ \&c.}$$

$$\frac{-3\sqrt{(1-yy)}}{4yy} = -\tfrac{3}{4}y^{-2} + \tfrac{3}{8} + \tfrac{3}{32}y^{2} + \tfrac{3}{64}y^{4}, \text{ \&c.}$$

$$+\frac{1}{2y^{4}} + \frac{1}{2yy} = +\tfrac{1}{2}y^{-4} + \tfrac{1}{2}y^{-2}$$

$$+\tfrac{3}{4}\text{H. L. }\frac{1}{1+\sqrt{(1-yy)}} = -\tfrac{3}{4}\text{H. L. }2 + \tfrac{3}{16}y^{2} + \tfrac{9}{128}y^{4}, \text{ \&c.}$$

The sum is $= \ast \quad \ast \quad + \frac{7}{16} - \frac{3}{4}\text{H. L. }2, + \frac{5}{16}y^{2} + \frac{35}{256}y^{4}$, &c. which evidently differs from the series on the 2d side by the constant quantity $\frac{7}{16} - \frac{3}{4}\text{H. L. }2$. We therefore have, by subtracting this constant quantity from the first side,

$$\left.\begin{array}{l} \dfrac{-\sqrt{(1-yy)}}{2^{4}y} - \dfrac{3\sqrt{(1-yy)}}{4yy} + \dfrac{3}{4}\text{H. L. }\dfrac{2}{1+\sqrt{(1-yy)}} \\ + \dfrac{1}{2y^{4}} + \dfrac{1}{2yy} - \dfrac{7}{16} \end{array}\right\} = \frac{3.5yy}{4.6.2} + \frac{3.5.7y^{4}}{4.6.8.4} + \frac{3.5.7.9y^{6}}{4.6.8.10.4}, \text{ \&c.}$$

which, when y becomes = 1, becomes

$$\left.\begin{array}{l} \ast \quad \ast \quad +\tfrac{3}{4}\text{H. L. }2 \\ \boxed{+\tfrac{1}{2}+\tfrac{1}{2}-\tfrac{7}{16}} + \tfrac{9}{16} \end{array}\right\} = \frac{3.5}{4.6.2} + \frac{3.5.7}{4.6.8.4} + \frac{3.5.7.9}{4.6.8.10.6}, \text{ \&c.}$$

4. If the equation of fluents in the preceding article be divided by y, and if $\frac{3.5y}{4.6.2} = \frac{5}{16}y$ be then taken from both sides of it, and u be written for H. L.

$\frac{2}{1+\sqrt{(1-yy)}}$, we shall have

$$\left.\begin{array}{l}\frac{-\sqrt{(1-yy)}}{2y^5}-\frac{3\sqrt{(1-yy)}}{4y^3}+\frac{3u}{4y}\\ +\frac{1}{2y^5}+\frac{1}{2y^3}-\frac{7}{16y}-\frac{5y}{16}\end{array}\right\}=\frac{3.5.7y^3}{4.6.8.4}+\frac{3.5.7.9y^5}{4.6.8.10.6}+\frac{3.5.7.9.11y^7}{4.6.8.10.12.8}\text{, \&c.}$$

And, if this equation be put into fluxions, and Q be written for $\sqrt{(1-yy)}$, for the sake of brevity, there will be $\left.\begin{array}{l}\frac{5}{2y^6}+\frac{9}{4y^4}+\frac{3}{4yy}\\ -\frac{4}{2y^4}-\frac{6}{4yy}\end{array}\right\}\frac{\dot{y}}{\mathrm{Q}}\boxed{+\frac{3\dot{u}}{4y}}-\frac{3u\dot{y}}{4yy}$

$$+\left(-\frac{5}{2d^6}+\frac{3}{2y^4}+\begin{array}{c}\frac{7}{16yy}\\ -\frac{3}{4yy}\end{array}-\frac{5}{16}\right)\dot{y}=\frac{3.5.7.3\dot{y}yy}{4.6.8.4}+\frac{3.5.7.9.5\dot{y}y^4}{4.6.8.10.6}+\frac{3.5.7.9.11.7\dot{y}y^6}{4\ 6.8.10.12.8}\text{, \&c.}$$

And this equation, more concisely expressed and divided by y, gives

$$\left(\frac{5}{2y^7}+\frac{1}{4y^5}-\frac{3}{4y^3}\right)\frac{\dot{y}}{\mathrm{Q}}-\frac{3u\dot{y}}{4y^3}$$
$$+\left(\frac{-5}{2y^7}-\frac{3}{2y^5}-\frac{5}{16y^3}-\frac{5}{16y}\right)\dot{y}=\frac{3.5.7.3\dot{y}y}{4.6.8.4}+\frac{3.5.7.9.5\dot{y}y^3}{4.6.8.10.6}+\frac{3.5.7.9.11.7\dot{y}y^5}{4.6.8.10.12.8}\text{, \&c.}$$

Now the fluent of the series on the 2d side of this equation is found, by the methods which have been long known, to be $\frac{3.5.7.3yy}{4.6.8.4.2}+\frac{3.5.7.9.5y^4}{4.6.8.10.6.4}+\frac{3.5.7.9.11.7y^6}{4.6.8.10.12.8.6}$, &c. and the fluent of the terms on the first side will be very easily obtained, by the following assumption, and attention to what was shown in art. 2 of this paper.

For the fluent of the terms on the first side of this equation, assume

$$\left(\frac{a}{y^6}+\frac{b}{y^4}+\frac{c}{y^2}\right)\mathrm{Q}+u\left(\frac{f}{yy}+g\right)$$
$$+\frac{p}{y^6}+\frac{q}{y^4}+\frac{r}{yy}+s\,\text{H. L. }y;$$ then will the fluxion of this expression be

$$\left.\begin{array}{r}\frac{-6a}{y^7}-\frac{4b}{y^5}-\frac{2c}{y^3}\\ +\frac{5a}{y^5}+\frac{3b}{y^3}+\frac{c}{y}\\ +\frac{f}{y^3}+\frac{g}{y}\end{array}\right\}\frac{\dot{y}}{\mathrm{Q}}\begin{array}{l}\boxed{+\dot{u}\left(\frac{f}{yy}+g\right)}\\ -\frac{2fu\dot{y}}{y^3}\end{array}$$

$$+\left.\left(\begin{array}{r}\frac{-6p}{y^7}-\frac{4q}{y^5}-\frac{2r}{y^3}+\frac{s}{y}\\ -\frac{f}{y^3}-\frac{g}{y}\end{array}\right.\right\}\dot{y},$$ which being put = the first side of the foregoing equation, there will arise as many simple equations for determining the co-efficients a, b, c, &c. as there are letters of that kind in the assumed fluent, from which their values will easily be found. The variable part therefore, of the fluent of the first side of the above equation, is

$\mathrm{Q}\left(\frac{-5}{12y^6}-\frac{7}{12y^4}-\frac{5}{16yy}\right)+u\left(\frac{3}{8yy}+\frac{5}{16}\right)+\frac{5}{12y^6}+\frac{3}{8y^4}-\frac{1}{32yy}$. Now, to discover the constant quantities which lie concealed in this expression, we must proceed as above in art. 3, whence is obtained,

$$\left.\begin{array}{l}\text{H. L. } 2\left(\frac{3}{8}+\frac{5}{16}\right)\\ +\frac{5}{12}+\frac{3}{8}-\frac{1}{32}-\frac{67}{192}\end{array}\right\}=\left.\begin{array}{l}\frac{11}{16}\text{H. L. } 2\\ \quad+\frac{79}{192}\end{array}\right\}=\frac{3\cdot5\cdot7\cdot3}{4\cdot6\cdot8\cdot4\cdot2}+\frac{3\cdot5\cdot7\cdot9\cdot5}{4\cdot6\cdot8\cdot10\cdot6\cdot4}+\frac{3\cdot5\cdot7\cdot9\cdot11\cdot7}{4\cdot6\cdot8\cdot10\cdot12\cdot8\cdot6},$$

&c. which is the value of another series of the preceding paper.

5. If the last literal equation be divided by y, and $\frac{3\cdot5\cdot7\cdot3y}{4\cdot6\cdot8\cdot4\cdot2}=\frac{105y}{512}$ be then taken from both sides, we shall have

$$\left.\begin{array}{l}\text{Q}\left(\frac{-5}{12y^7}-\frac{7}{12y^5}-\frac{5}{16y^3}\right)+u\left(\frac{3}{8y^3}+\frac{5}{16y}\right)\\ +\frac{5}{12y^7}+\frac{3}{8y^5}-\frac{1}{32y^3}-\frac{67}{192y}-\frac{105y}{512}\end{array}\right\}=$$

$$\frac{3\cdot5\cdot7\cdot9\cdot5y^3}{4\cdot6\cdot8\cdot10\cdot6\cdot4}+\frac{3\cdot5\cdot7\cdot9\cdot11\cdot7y^5}{4\cdot6\cdot8\cdot10\cdot12\cdot8\cdot6}+\frac{3\cdot5\cdot7\cdot9\cdot11\cdot13\cdot9y^7}{4\cdot6\cdot8\cdot10\cdot12\cdot14\cdot10\cdot8},$$ &c. which equation, in fluxions, gives

$$\frac{\dot{y}}{\text{Q}}\left(\frac{35}{12y^9}+\frac{5}{12y^7}-\frac{49}{48y^5}-\frac{5}{16y^3}\right)-u\dot{y}\left(\frac{9}{8y^5}+\frac{5}{16y^3}\right)$$
$$+\dot{y}\left(-\frac{35}{12y^9}-\frac{15}{8y^7}-\frac{9}{32y^5}+\frac{7}{192y^3}-\frac{105y}{512}\right)=$$
$$\frac{3\cdot5\cdot7\cdot9\cdot5\cdot3\dot{y}y}{4\cdot6\cdot8\cdot10\cdot6\cdot4}+\frac{3\cdot5\cdot7\cdot9\cdot11\cdot7\cdot5\dot{y}y^3}{4\cdot6\cdot8\cdot10\cdot12\cdot8\cdot6}+\frac{3\cdot5\cdot7\cdot9\cdot11\cdot13\cdot9\cdot7\dot{y}y^5}{4\cdot6\cdot8\cdot10\cdot12\cdot14\cdot10\cdot8},\ \&c.$$

Now the fluent of the fluxionary series on the 2d side of the equation being obviously the series $\frac{3\cdot5\cdot7\cdot9\cdot5\cdot3yy}{4\cdot6\cdot8\cdot10\cdot6\cdot4\cdot2}+\frac{3\cdot5\cdot7\cdot9\cdot11\cdot7\cdot5y^4}{4\cdot6\cdot8\cdot10\cdot12\cdot8\cdot6\cdot4}+\frac{3\cdot5\cdot7\cdot9\cdot11\cdot13\cdot9\cdot7y^6}{4\cdot6\cdot8\cdot10\cdot12\cdot14\cdot10\cdot8\cdot6}$, &c. we are next to take the fluent of the expression on the first side, and to correct it, that it may be = this series; which may be done as follows:

For the fluent sought, assume

$$\text{Q}\left(\frac{a}{y^8}+\frac{b}{y^6}+\frac{c}{y^4}+\frac{d}{yy}\right)+u\left(\frac{f}{y^4}+\frac{g}{yy}+h\right)+\frac{p}{y^8}+\frac{q}{y^6}+\frac{r}{y^4}+\frac{s}{yy}+t\,\text{H. L. }y,$$

and take the fluxion of this expression; then these fluxionary terms being put = to those on the first side of the preceding equation, there will arise several equations for determining the values of the letters, a, b, c, &c. and then the assumed fluent becomes,

$$\text{Q}\left(\frac{-35}{8\cdot12y^8}-\frac{95}{12\cdot16y^6}-\frac{75}{4\cdot8\cdot8y^4}-\frac{105}{8\cdot8\cdot8yy}\right)+u\left(\frac{9}{32y^4}+\frac{5}{32yy}+\frac{105}{8\cdot8\cdot8}\right)$$
$$+\frac{35}{8\cdot12y^8}+\frac{5}{16y^6}\quad*\quad-\frac{37}{4\cdot8\cdot12yy}\quad*,$$ which may be corrected in the manner shown in the 2 preceding articles, when it becomes

$$\text{Q}\left(\frac{-35}{8\cdot12y^8}-\frac{95}{12\cdot16y^6}-\frac{75}{16\cdot16y^4}-\frac{105}{8\cdot8\cdot8yy}\right)+u\left(\frac{9}{32y^4}+\frac{5}{32yy}+\frac{105}{8\cdot8\cdot8}\right)$$
$$+\frac{35}{8\cdot12y^8}+\frac{5}{16y^6}\quad*\quad-\frac{37}{4\cdot8\cdot12yy}-\frac{1023}{4096}=\text{ the series}$$
$$\frac{3\cdot5\cdot7\cdot9\cdot5\cdot3yy}{4\cdot6\cdot8\cdot10\cdot6\cdot4\cdot2}+\frac{3\cdot5\cdot7\cdot9\cdot11\cdot7\cdot5y^4}{4\cdot6\cdot8\cdot10\cdot12\cdot8\cdot6\cdot4}+\frac{3\cdot5\cdot7\cdot9\cdot11\cdot13\cdot9\cdot7y^6}{4\cdot6\cdot8\cdot10\cdot12\cdot14\cdot10\cdot8\cdot6},\ \&c.$$

which, when $y=1$, becomes

$$\left.\begin{array}{l}\text{H. L. } 2.\left(\frac{9}{32}+\frac{5}{32}+\frac{105}{8\cdot8\cdot8}\right)\\ +\frac{35}{8\cdot12}+\frac{5}{16}-\frac{37}{4\cdot8\cdot12}-\frac{1023}{4096}\end{array}\right\}=\frac{4067}{12288}+\frac{329}{512}\text{ H. L. } 2$$

$$= \frac{3.5.7.9.5.3}{4.6.8.10.6.4.2} + \frac{3.5.7.9.11.7.5}{4.6.8.10.12.8.6.4} + \frac{3.5.7.9.11.13.9.7}{4.6.8.10.12.14.10.8.6}, \text{ \&c.}$$

which is the value of another series of the foregoing paper.

II. Observations, tending to facilitate and abridge the numerical computations of A *and* B *in the preceding paper.*—6. The radical factor $\surd(1 - cc)$, in the literal expressions of the values of A and B, may be taken away, by multiplying the other factors by its equivalent $1 - \frac{cc}{2} - \frac{c^4}{8} - \frac{c^6}{16}$, &c. in consequence of which, other expressions will be obtained, better adapted to the purpose of numerical calculation. This will appear by the following operations. The product of $\surd(1 - cc) \times$ the other factor in the expression of the value of A, in art. 9 of the preceding paper, viz. $\frac{2}{cc} + \lambda + \mu cc + \nu c^4$, will give $\frac{2}{cc} + e + fcc + gc^4$, &c. where, e, f, and g, are $= \lambda - 1$, $\mu - \frac{1}{2}\lambda - \frac{1}{4}$, and $\nu - \frac{1}{2}\mu - \frac{1}{8}\lambda - \frac{1}{8}$, respectively; in numbers $= 0{\cdot}1931472$, $0{\cdot}1036802$, and $0{\cdot}0687064$, respectively. And this expression, which is evidently more simple than the former, is somewhat nearer than that to the value of the whole series.

7. In like manner, the product of the 2 factors in the value of A′, in art. 13, viz. $\frac{4}{3c^4} + \frac{5}{3cc} + \lambda' + \mu' cc + \nu' c^4$, will be $= \frac{4}{3c^4} + \frac{1}{cc} + h + icc + kc^4$, &c. which expression also is more simple than that from which it is derived, while its accuracy is not less, as is pretty evident on inspection. And that the numerical values of h, i, and k, are very easily attainable from the values of λ', μ', and ν', given above in art. 3, 4, and 5, of this paper, is very obvious.

8. And the product of the 2 factors in the value of B, in art. 16, may also be exchanged for a more convenient expression, by a like process.
viz. $\rho + \sigma cc + \tau c^4 \times 1 - \frac{cc}{2} - \frac{c^4}{8}$, &c. $= \rho + lcc + mc^4$, &c. which expression also is more accurate than that from which it is derived, as well as more simple.

9. The numerical calculation of the other member also, in which α enters, may be facilitated and abridged, by the following considerations. If c be put for the sine of an angle, radius being 1, then will $1 + \surd(1 - cc)$ be the versed sine of the supplement of that angle, and $\frac{c}{1 + \surd(1 - cc)}$ will be $=$ the tangent of half that angle; from which it follows, that the reciprocal of this quantity, viz. $\frac{1 + \surd(1 - cc)}{c}$, is $=$ the co-tangent of half the angle of which the sine is c. The common logarithm of $\frac{1 + \surd(1 - cc)}{c}$ may therefore be taken out from a table of common logarithms, and then converted into an hyperbolic logarithm, by table 37 of Dodson's Calculator, or by table 7 of Dr. Hutton's Logarithms.

10. An expression of this kind, $\frac{p \pm rcc}{q \pm scc}$, when c is the only variable quantity, consisting of several figures, and r and s are likewise long numbers, will be much better adapted to the use of logarithms, when put in this form, $\frac{r}{s} \times \frac{p \div r \pm cc}{q \div s \pm cc}$; because the multiplications of r and s into cc, or additions of their logarithms and

taking out two numbers, are by this means exchanged for the addition of the constant logarithm of $\frac{r}{s}$: the quotients $\frac{p}{r}$ and $\frac{q}{s}$, once found, being constant numbers. Thus, the numerical value of even $\frac{8-2cc}{8-5cc}$, where r and s are single figures, is more easily obtained by $\frac{4}{10} \times \frac{4-cc}{1{\cdot}6-cc}$, than by the former expression.

11. But it will appear on trial, that the arithmetical value of any 3 terms, $p' + q'cc + r'c^4$, in which p', q', and r', are constant quantities, and cc consists of 5 or 6 places of figures, may, in general, be more easily obtained by logarithms, than the arithmetical value of $\frac{r}{s} \times \frac{p \div r \pm cc}{q \div s \pm cc}$. And since the difference of the values of these 2 expressions is inconsiderable in the present case, I shall make no further use of the fractional expression; but observe, that the logarithm of $q'cc$, in the other expression, being found, the logarithm of $r'c^4$ will be had, by adding to it the logarithm of $\frac{r'}{q'}cc$; for $q'cc \times \frac{r'}{q'}cc = r'c^4$. And, since the logarithms of the numbers which stand in the places of q' and $\frac{r'}{q'}$ may be taken out and reserved for use, and the logarithms of cc and α, once found, will serve for all the terms in which these quantities occur, it will appear by an example, that neither many logarithms, nor many numbers corresponding to logarithms, need be taken out of other tables, in computing the value of A or B.

12. It will now be proper, since the literal expressions of the values of A and B have been exchanged for others which are more convenient, to bring the new equations together in one view, and then give an example of the numerical calculations by them. It appears, by art. 9, 10, 13, 16, and 17 of the preceding paper, and 6, 7, and 8 of this, that

$$1.\ \mathrm{A} = \frac{1}{\pi(a+b)^{\frac{3}{2}}} \times \begin{cases} \frac{2}{cc} + e + fcc + gc^4 \\ \quad + \alpha + \frac{3}{8}\alpha cc + \frac{3.5}{8.8}\alpha c^4. \end{cases}$$

$$2.\ \mathrm{A}' = \frac{1}{\pi(a+b)^{\frac{1}{2}}} \times \begin{cases} \frac{4}{3} + \frac{1}{cc} + h + icc + kc^4 \\ \quad + \frac{3}{4}\alpha + \frac{3.5}{4.12}\alpha cc + \frac{3.5.21}{4.12.32}\alpha c^4. \end{cases}$$

$$3.\ \mathrm{B} = \frac{2a}{b}\mathrm{A} - \frac{2}{\pi b(a+b)^{\frac{1}{2}}} \times \begin{cases} \rho + lcc + mc^4 \\ + 2\alpha + \frac{1}{2}\alpha cc + \frac{9}{2.16}\alpha c^4. \end{cases}$$

$$4.\ \mathrm{B}' = \frac{2}{b}(\mathrm{A}'a - \mathrm{A}).$$

In which equations, the values of the coefficients are as follow:

$e = 0{\cdot}1931472$, $h = 0{\cdot}0823604$, $\rho = 1{\cdot}3862944$,
$f = 0{\cdot}1036802$, $i = 0{\cdot}0551502$, $l = 0{\cdot}3465736$,
$g = 0{\cdot}0687064$, $k = 0{\cdot}0408309$, $m = 0{\cdot}1793226$.

The constant numbers which will be wanted, in computing the arithmetical values of A and B, are those denoted by e, h, and ρ, which are given in the preceding article. Mr. H. then sets down the work of an example, in a very neat manner, viz. for the 2 planets Venus and the earth.

XXIII. Of a Substance found in a Clay-pit; and of the Effect of the Mere of Diss, on various Substances immersed in it. By Mr. Benj. Wiseman, of Diss, Norfolk. Communicated by John Frere, Esq., F. R. S. With an Analysis of the Water of the said Mere. By Charles Hatchett, Esq., F. R. S. p. 567.

This substance was found near Diss, in a body of clay, from 5 to 8 feet below the surface of the soil. All the pieces lay nearly in an horizontal direction; and varied in size, from 2 or 3 oz., to as many lb. The colour of the substance, when taken fresh from the clay-pit, was like that of chocolate; it cuts easily, and has the striated appearance of rotten wood. The pieces were of no particular form; in general, they were broad and flat, but I do not recollect to have met with a piece that was more than 2 inches in thickness: it breaks into laminæ, between which are the remains of various kinds of shells. The specific gravity of this substance, dried in the shade, is 1.588; it burns freely, giving out a great quantity of smoke, with a strong sulphureous smell. By a chemical analysis, which I cannot consider as very accurate, 100 gr. appear to contain,

Of inflammable matter, including the small quantity of water contained in the substance	41.3 grs.
Of mild calcareous earth	20.0
Of iron	2.0
Of earth, that appears to be silex	36.7
	100

On the effect of the Mere of Diss, on various substances.

Observing several years ago, that flint stones taken out of the Mere of Diss were incrusted with a metallic stain, I was induced to make some experiments, in order to discover the nature or composition of this metallic substance. Nitrous acid readily removes it, dissolving a part, and leaving a yellowish powder, which, washed and filtered, was found to be sulphur. Vegetable fixed alkali precipitated from the nitrous acid a ferruginous coloured powder, which was iron.

With a view to determine what length of time was necessary for the formation of this metallic stain on flint stones, or other substances, I inclosed in a brass wire net the following articles: flint stones, calcareous spar, common writing slate, a piece of common white stone ware, and a piece of black Wedgwood-pottery. After remaining in the water from the summer of 1792 to August, 1795, the flints and Wedgwood-ware had acquired the metallic stain in a slight degree, and the slate had assumed a rust colour; the other substances appeared not to be at all altered. I was greatly surprized to find the copper wire that held the net, surrounded with a metallic coating of a considerable thickness; it was of a deep lead colour, and of a granulated texture. When taken from the wire, and ground in a mortar, it had a black appearance, interspersed with very hard shining particles. The wire was evidently eroded, and this substance deposited in the place of the copper that was decomposed, somewhat similar to the decomposition of iron in cupreous waters. By repeated chemical analysis of this substance, 100 gr. contain, of copper, 70; of sulphur, 16.6; of iron, 13.3 gr.

I have never met with an account of the decomposition of copper, in waters impregnated with iron, in any chemical work; and as iron appears to have a greater affinity to the vitriolic acid than copper has, as is constantly evinced in the neighbourhood of copper mines, it appears an anomaly in chemistry, that I am not adept enough in the science to account for.

[The President and Council, thinking the effects of the water of Diss Mere deserving of further inquiry, desired Mr. Wiseman would send some of the said water, for the purpose of examination. Mr. Wiseman accordingly sent a quantity of the water, accompanied by the other substances described in the following letter to the President, dated Diss, May 29, 1798.]

"As the Society have expressed a wish, through Mr. Frere, to have some of the water in which the copper wire was deposited, which Mr. Frere, at my request, laid before the Society, I have sent 2 gallons of the water of Diss Mere, (N° 1), with a small quantity of copper cuttings, (N° 2), which laid in the same water, a few feet from the side, and 6 feet in depth, from the 7th of February, 1797, to the 20th of the present month, May, 1798. The pieces of copper, when laid in, weighed 3051 gr.; when they were taken out, and washed from the mud that lightly adhered to them, preserving and weighing the scaly matter that came off, they weighed 2944 grs., indicating a loss of 107 gr. Examining the pieces of copper, the same evening they were taken out of the water, I observed a number of small crystals formed on some of them, in the form of pyramids joined at their bases; these crystals lost their shining appearance, by the evaporation of the water of crystallization, in the warmth of the succeeding day. Whether they will be preserved in a journey of nearly 100 miles, is perhaps doubtful. N° 3 contains 2 pieces of copper, on which the crystals were most abundant. N° 4 contains a small quantity of the substance formed on the copper, that came off in washing and in weighing it.

The town of Diss is principally situated on the N.N.E. and E. sides of this piece of water. The land runs pretty steep on the W. and N. of it, to the height of 40 or 50 feet: on the S.E., the ground comes within a few feet of the level of it. The soil of the upper part of the town is a stiff blue clay; that of the lower part, to the S.E., a black sand, beneath which it is a moor. The water in the higher parts of the town is good; in the lower parts, it is a chalybeate, of which a specimen is sent, (N° 5). N° 6 contains a quantity of flint stones, taken from the S.E. side of the Mere, where the water is shallow; many of which are strongly marked with the metallic stain, which they acquire by lying in this water a few years.

The Mere contains about 8 acres, and is of various depths, to 24 feet: from its situation with respect to the town, it may naturally be supposed to contain a vast quantity of mud, as it has received the silt of the streets for ages. In summer, the water turns green; and the vegetable matter that swims on its surface, when exposed to the rays of the sun, affords vast quantities of oxygen gas. I cannot help considering this process as having a considerable agency in the corrosion, and in the formation of the metallic crust on the copper deposited in this water. Some of this vegetable matter will be found, in the water sent to the Society."

[The water, and other substances described in the foregoing letter, were delivered to Mr. Hatchett, who had been previously requested, by the President and Council, to examine them. The result of his examination is as follows:]

Analysis of the Water of the Mere of Diss. By Chas. Hatchett, Esq.

The substances sent by Mr. Wiseman are as follow: Some copper wire, with a blackish grey incrustation. Water from Diss Mere, (marked N° 1). Copper cuttings, covered with a blackish crust, similar to that on the copper wire, (marked N° 2). Some cuttings similar to those above-mentioned (marked N° 3). The paper, N° 4, contained some of the black crust, detached from the cuttings. N° 5, a quart bottle, containing some water from the lower part of the town of Diss, and called, by Mr. Wiseman, a chalybeate water. N° 6, some flints, taken from the S.E. side of the Mere, where the water is shallow, and having, as Mr. Wiseman terms it, a metallic stain.

My first experiments were made on the incrustation of the copper wire, mentioned in Mr. Wiseman's first letter. This incrustation was easily detached from the wire, and being reduced to powder, was digested with nitro-muriatic acid, in a gentle heat: a green solution was formed, and there remained a residuum, of a pale yellow, which proved to be sulphur. The solution being diluted with 2 parts of distilled water, was supersaturated with pure ammonia, by which, a few brown flocculi of iron were precipitated. The supernatant liquor was blue; and being evaporated, and re-dissolved by sulphuric acid, the whole was precipitated by a plate of polished iron, in the state of metallic copper. The component parts of this coating were therefore copper, and a very small portion of iron combined with sulphur. I could not extend these experiments, as the whole quantity of the coating that I was able to collect, amounted only to $3\frac{1}{4}$ gr.*

The next experiments were made on the black crust of N° 2, 3, and 4. This I found to be exactly the same as that formed on the copper wire; viz. it consisted of copper combined with sulphur, and a very small portion of iron.

I next examined the water of Diss Mere, (N° 1) and I was at length led on, step by step, to make a regular analysis of the fixed ingredients. Before making the analysis, I examined this water with certain re-agents, and remarked the following properties. 1. The water of Diss Mere has a yellowish tinge, and the flavour is rather saline; but it has not any perceptible odour. 2. Prussiate of pot-ash did not produce any effect. 3. Acetite of lead produced a slight white precipitate. 4. Nitrate of silver formed one, very copious. 5. Tincture of galls had not any

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* The copper wire, when the coating was removed, was perfectly flexible, and the surface did not appear unequal or corroded: this is commonly the case under such circumstances; for, when sulphur has combined superficially with a metal, the compound is observed to separate easily, so as to leave the metal underneath not injured in quality, and very little, if at all, affected in appearance. Those who diminish silver coin, make use of the following method. They expose the coin to the fumes of burning sulphur, by which a black crust of sulphurated silver is soon formed, which, by a slight but quick blow, comes off like a scale, leaving the coin so little affected, that the operation may sometimes be repeated twice or thrice, without much hazard of detection, if the coin has a bold impression.—Orig.

effect. 6. Muriate of barytes caused a slight precipitate. 7. Ammonia, pot-ash, and oxalic acid, severally produced precipitates, when added to different portions of this water.

Analysis.—A. 300 cubic inches of the water, by a gentle evaporation, left a pale brown scaly substance, which weighed 58 gr. B. These 58 gr. were digested in alcohol, without heat, during 24 hours, and afforded a solution, which, by evaporation, yielded muriate of lime, slightly tinged by marshy extract, 18 gr. C. 6 oz. of distilled water were then poured on the residuum, and, with repeated stirring, remained during 24 hours. By evaporation this afforded muriate of soda, with a very small portion of sulphate of soda; in all, 10 gr. D. What remained was boiled in 800 parts of distilled water; and the solution, being evaporated, left of selenite 1.70 gr. E. The undissolved portion now weighed 25 gr., and was digested with diluted muriatic acid: a great part was dissolved, with much effervescence, and, being filtrated, afforded, by ammonia, of alumina 1.50 gr. From this I afterwards separated a very minute quantity of iron, by means of prussiate of pot-ash. F. Carbonate of soda was then added to the liquor, and precipitated carbonate of lime 21 gr. G. The insoluble residuum weighed 3.50 gr.; and proved to be principally carbon, produced by decomposed vegetable matter, with a very small quantity of siliceous earth. The result of this analysis was, therefore,

	Grains.
B. Muriate of lime	18
C. Muriate of soda, with a very small portion of sulphate of soda	10
D. Selenite	1 70
E. Alumina, with a portion of iron too small to be estimated	1 50
F. Carbonate of lime	21
G. Carbon, with a little siliceous earth	3 50
	55 70
Loss	2 30
	58 0

It is worthy of notice, that the iron present was in so very small a quantity, as not to be detected by any test, till it had been separated in conjunction with the alumina.

The water N° 5, from Mr. Wiseman's account, does not appear to have been concerned in producing the effects which he has observed, and the quantity was too small to be subjected to a regular analysis, I noted however what follows: 1. It has a very strong hepatic flavour and smell. 2. A plate of polished silver, put into it, became black in a few hours. 3. It became faintly bluish with prussiate of pot-ash, after standing 5 or 6 hours. 4. Tincture of galls produced a faint purple cloud. 5. Solution of acetite of lead afforded a brown precipitate. 6. Nitrate of silver produced the same. 7. Pot-ash, and ammonia, caused a precipitate; but that of the former was the most copious. 8. Oxalic acid produced a precipitate. 9. Muriate of barytes had also a slight effect. The water N° 5 cannot therefore be considered as a chalybeate, the quantity of iron contained in it being scarcely perceptible; but it appears to be a water containing some hepatic gas, together with substances similar to those contained in N° 1.

From the above experiments it is evident, that the water N° 1 does not contain any of the component parts of the crust formed on the copper wire and cuttings, though it is certain that the incrustation took place during the immersion of those bodies; but before mentioning my ideas on this subject, I shall give an account of some experiments made on the flints, N° 6. These were coated with a yellowish shining substance, which appeared to be pyrites; and as the flints could not have contributed any metallic substance to form this coating, I was enabled by their means to ascertain, whether the copper of the crust, formed on the wire and cuttings, had been furnished by the pieces of copper, or by any thing in the vicinity of the water. 1. I poured nitro-muriatic acid on some of the flints, in a matrass, so as completely to cover them. The coating was rapidly dissolved, with much effervescence; and when the flints appeared perfectly uncoated, and in their usual state, I decanted the liquor. 2. A yellow matter subsided, which proved to be sulphur. 3. Prussiate of pot-ash produced Prussian blue; and the remaining part of the solution, being supersaturated with ammonia, afforded an ochraceous precipitate of iron. The supernatant liquor did not become blue, as when copper is present, nor was the smallest trace of it afforded by evaporation. Martial pyrites is therefore the only substance deposited on bodies immersed in the water of Diss Mere; and the copper of the crust, formed on the wire and cuttings, was furnished by those bodies.

It is proved by the analysis, that the water of Diss Mere does not hold in solution any sulphur, and scarcely any iron; it has not therefore been concerned in forming the pyrites; but it appears that the pyritical matter is formed in the mud and filth of the Mere; for Mr. Wiseman says in his letter, that "the Mere has received the silt of the streets for ages." Now it is a well-known fact, that sulphur is continually formed, or rather liberated, from putrefying animal and vegetable matter, in common sewers, public ditches, houses of office, &c. &c.; and this most probably has been the case at Diss. And, if sulphur, thus formed, should meet with silver, copper, or iron, it will combine with them, unless the latter should be previously oxidated. The sulphur has therefore, in the present case, met with iron, in, or approaching, the metallic state, and has formed pyrites; which, while in a minutely divided state, or progressively during formation, has been deposited on bodies, such as the flints, when in contact with the mud.

But an excess of sulphur appears to be present; for when copper is put into the Mere, the sulphur readily combines with it; and at the same time a small portion of iron appears to unite with the compound of copper and sulphur, possibly by the mere mechanical act of precipitation. The incrustation on the copper wire and cuttings is, in every property, similar to that rare species of copper ore, called by the Germans Kupfer schwärtze, (cuprum ochraceum nigrum); and I consider it as absolutely the same. In respect to the martial pyrites on the flints, there can be no hesitation; and as in these 2 instances there were evident proofs of the recent formation of ores in the humid way, I was desirous to ascertain the effect on silver. I therefore wrote to Mr. Wiseman, to request that he would take the trouble to make the experiment: and received from him the following answer, dated Diss, 8th Sept. 1798, accompanied by the specimens.

"SIR.—Immediately on the receipt of your letter, (27th July) I laid some silver plate, and silver wire, into the Mere; the whole weighed 235.6 gr. I took it out on Thursday last, (Sept. 6th) and, after cleaning it carefully from mud and weeds, I find it weighs 242.8 gr.; an increase of 7.2 gr. The silver plate you will find much tarnished, in some parts almost black; the wire is in many places fairly incrusted, which crust on the pressure of the fingers, comes off in thin scales. The whole appearance of the silver strongly indicates the presence of sulphur, which I have no doubt abounds in every part of the Mere. The peculiar smell of the mud gives reason to suppose, that a great deal of hepatic air is produced; which probably uniting with the iron held in solution in the water of the Mere, may account for the martial pyrites found on the flints. By what affinity the copper wire, laid in this water, is attacked, I am not chemist enough to determine. I have begun a set of experiments, with the view of producing the same effects on copper wire by artificial means; but whether I shall succeed, I am not able at present to say.

BENJ. WISEMAN."

P. S.—By experiments I have lately made, I find hepatic gas precipitates carbonate of iron in the form of a black flocculent matter: 71 parts of which are iron, and 29 sulphur.

The silver plate I found, as Mr. Wiseman has mentioned, much tarnished, and in many places almost black, but I could not detach any part of it. I succeeded better with the wire, and collected a small portion of a black scaly substance, which, as far as the smallness of the quantity would allow it to be ascertained, was sulphuret of silver; and was similar, in every respect, to the sulphurated or vitreous ore of silver, called by the Germans glasertz. This effect on the silver was to be expected; and I recollect to have read, not many months ago, in one of the foreign journals, that Mr. Proust had examined an incrustation, of a dark grey colour, formed in the course of a very long time, on some silver images, in a church at, I believe, Seville. This incrustation he found to be a compound of silver with sulphur, or, in other words, vitreous silver ore. The same principle is the cause of the tarnish which silver plate contracts with so much ease, particularly in great cities; for this tarnish is principally a commencement of mineralization on the surface, produced by the sulphureous and hepatic vapours dispersed throughout the atmosphere, in such places.

To Mr. Wiseman's observations we are much indebted, as they make known the recent and daily formation of martial pyrites, and other ores, under certain circumstances. It is not to be supposed that such effects are local, or peculiar to Diss Mere; on the contrary, there is reason to believe that similar effects, on a larger scale, have been, and are now, daily produced in many places. The pyrites in coal mines have probably in great measure thus originated. The pyritical wood also may thus have been produced; and by the subsequent loss of sulphur, and oxidation of the iron, this pyritical wood appears to have formed the wood-like iron ore which is found in many parts, and particularly in the mines on the river Jenisei, in Siberia. In short, when the extensive influence of pyrites in the mineral kingdom, caused by the numerous modifications of it, in the way of composition and decomposition, is considered, every thing which reflects light on its formation becomes interesting; and I cannot but regard as such, the effects which Mr. Wiseman has observed in the Mere of Diss. CHARLES HATCHETT.

XXIV. A Catalogue of Sanscrita Manuscripts, presented to the R. S. by Sir Wm. and Lady Jones. By Charles Wilkins, Esq. F. R. S. p. 582.

1. a. Mahá-bhárata.* A poem in 18 books, exclusive of the part called Raghuvansa; the whole attributed to Crishna Dwaipáyana Vyása; with copious notes by Nila-canta. This stupendous work, when perfect, contains upwards of 100,000 metrical verses. The main subject is the history of the race of Bhárata, one of the ancient kings of India, from whom that country is said to have derived the name of Bhárata-varsha; and more particularly that of 2 of its collateral branches, distinguished by the patronymics, the Cauravas and the Pauravas, so denominated from 2 of their ancestors, Curu and Puru, and of their bloody contentions for the sovereignty of Bhárata-varsha, the only general name by which the Aborigines know the country we call India, and the Arabs and Persians Hind and Hindostan. But, besides the main story, a great variety of other subjects is treated of, by way of introduction and episode. The part entitled Raghu-vansa, contains a distinct history of the race of Crishna. The Mahá-bhárata is so very popular throughout the East, that it has been translated into most of its numerous dialects; and there is an abridgment of it in the Persian language, several copies of which are to be found in our public libraries. The Gitá, which has appeared in an English dress, forms part of this work; but, as it contains doctrines thought too sublime for the vulgar, it is often left out of the text, as happens to be the case in this copy. Its place is in the 6th book, called Bhishma-parva. This copy is written in the character which, by way of pre-eminence, is called Dévanágari. Ly J.

1. b. Ditto. Another copy, without notes, written in the character peculiar to the province of Bengal, in which the Brahmans of that country are wont to transcribe all their Sanscrita books. Most of the alphabets of India, though they differ very much in the shape of their letters, agree in their number and powers, and are capable of expressing the Sanscrita, as well as their own particular language. This copy contains the Gitá, in its proper place. Ly J.

2. a. Rámáyana. The adventures of Ráma, a poem in 7 books, with notes, in the Dévanágari character. There are several works with the same title, but this, written by Válmici, is the most esteemed. The subject of all the Rámáyanas is the same: the popular story of Ráma, surnamed Dásarathi, supposed to be an incarnation of the god Vishnu, and his wonderful exploits to recover his beloved Sitá out of the hands of Rávana, the gigantic tyrant of Lancá. Ly J.

2. b. Ditto. Another copy, in the Bengal character, without notes, by Válmici. Ly J.

2. c. Ditto. A very fine copy, in the Dévanágari character, without notes; but unfortunately not finished, the writer having been reduced to a state of insanity, by habitual intoxication. S. W. J.

3. a. Sri Bhágavata. A poem in 12 books, attributed to Crishna Dwaipáyana Vyása, the reputed author of the Mahá-bhárata, and many other works; with notes by Sridhara Swámi. Dévanágari character. It is to be found in most of the vulgar dialects of India, and in the Persian language. It has also appeared, in a very imperfect and abridged form, in French, under the title of Bagavadam, translated from the Támul version. The chief subject of the Bhágavata is the life of Crishna; but, being one of that species of composition which is called Purána, it necessarily comprises 5 subjects, including that which may be considered the chief. The Brahmans, in their books, define a Purána to be "a poem treating of 5 subjects: primary creation, or creation of matter in the abstract; secondary creation, or the production of the subordinate beings, both spiritual and material; chronological account of their grand periods of time, called Manwantaras; genealogical rise of families, particularly of those who

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* The Sanscrita words are spelt according to the method practised by Sir William Jones, in his works.—Orig.

have reigned in India; and lastly a history of the lives of particular families." There are many copies of this work in England. L^y J.

3. b. Ditto. Another copy, in the Bengal character, without notes. L^y J.

3. c. Ditto. Another copy, on palm leaves, in the Bengal character. S. W. J.

4. Agni Purána. This work, feigned to have been delivered by Agni, the god of fire, contains a variety of subjects, and seems to have been intended as an epitome of Hindu learning. The poem opens with a short account of several incarnations of Vishnu; particularly in the persons of Ráma, whose exploits are the theme of the Rámáyana, and of Crishna, the material offspring of Vasudéva. Then follows a history of the creation; a tedious dissertation on the worship of the gods, with a description of their images, and directions for constructing and setting them up; a concise description of the earth, and of those places which are esteemed holy, with the forms of worship to be observed at them; a treatise on astronomy, or rather astrology; a variety of incantations, charms, and spells, for every occasion; computation of the periods called Manwantaras; a description of the several religious modes of life, called A'srama, and the duties to be performed in each of them respectively; rules for doing penance; feasts and fasts to be observed throughout the year; rules for bestowing charity; a dissertation on the great advantages to be derived from the mystic character OM! with a hymn to Vasishta. The next subject relates to the office and duties of princes; under which head are given rules for knowing the qualities of men and women; for choosing arms and ensigns of royalty; for the choice of precious stones; which are followed by a treatise on the art of war, the greatest part of which is wanting in this copy. The next head treats of worldly transactions between man and man, in buying and selling, borrowing and lending, giving and receiving, &c. &c. and the laws respecting them. Then follow certain ordinances, according to the Véda, respecting means of security from misfortunes, &c. and for the worship of the gods. Lists of the 2 races of kings, called the Suryavansa, and the Chandravansa; of the family of Yadu, and of Crishna; with a short history of the 12 years war, described in the Mahá-bhárata. A treatise on the art of healing, as applicable to man and beast, with rules for the management of elephants, horses, and cows; charms and spells for curing various disorders; and the mode of worshipping certain divinities. On the letters of the Sanscrita alphabet; on the ornaments of speech, as applicable to prose, verse, and the drama; on the mystic signification of the single letters of the Sanscrita alphabet; a grammar of the Sanscrita language, and a short vocabulary. The work is divided into 353 short chapters, and is written in the Bengal character. L^y. J.

5. Cálica Purána. A mythological history of the goddess Cálí, in verse, and her adventures under various names and characters; a very curious and entertaining work, including by way of episode, several beautiful allegories, particularly one founded on the motions of the moon. There seems to be something wanting at the end. Bengal character, without notes. L^y. J.

6. a. Váyu Purána. This work, attributed to Váyu the god of wind, contains, among a variety of other curious subjects, a very circumstantial detail of the creation of all things celestial and terrestrial, with the genealogy of the first inhabitants; a chronological account of the grand periods called Manwantaras, Calpas, &c.; a description of the earth, as divided into Dwipas, Varshas, &c., with its dimensions in Yojanas; and also of the other planets, and fixed stars, and their relative distances, circumferences of orbits, &c. &c. Written in the Dévanágari character. L^y. J.

6. b. Ditto. A duplicate in the Dévanágari character. L^y. J.

7. Vrihan Náradiya Purána. This poem, feigned to have been delivered to Sanatcumára, by the inspired Nárada, like others of the Puránas, opens with chaos and creation; but it treats principally of the unity of God, under the title of Mahá Vishnu; arguing, that all other gods are but emblems of his works, and the goddesses, of his powers; and that the worshipping of either of the triad, creator, preserver, or destroyer, is, in effect, the worshipping of him. The book concludes with rules for the several tribes, in their spiritual and temporal conduct through life. It is a new copy, in the Bengal character, and, for a new copy, remarkably correct. L^y. J.

8. Náradiya Purána. This poem treats principally on the worship of Vishnu, as practised by Rukmingada, one of their ancient kings. Dévanágari character. S. W. J.

9. a. Bhavishyóttara Purána. The 2d and only remaining part. The subject is confined to religious ceremonies. Dévanágari character. S. W. J.

9. b. Ditto. With an index. Dévanágari character. L^{y}. J.

10. Gita-góvinda. A beautiful and very popular poem, by Jayadéva, on Crishna, and his youthful adventures. Bengal character. L^{y}. J.

11. a. Cumara Sambhava. An epic poem on the birth of Cártica, with notes, by Calidása. Dévanágari character. The notes are separate. L^{y} J.

11. b. Ditto. A duplicate of the text only, in the Bengal character. L^{y} J.

12. Naishadha. The adventures of Nala; a poem, with notes. Bengal character. L^{y} J.

13. Bhatti. A popular heroic poem, in the Bengal character. L^{y} J.

14. Raghu-vansa. The race of Crishna, a poem by Calidas, with notes. Dévanágari character. L^{y} J.

15. Vrihatcathá. Tales in verse, by Somadéva. Dévanágari character. L^{y} J.

16. Singhásána. The throne of Rájá Vicramáditya; a series of instructive tales, supposed to have been related by 32 images which ornamented it. Dévanágari character. It has been translated into Persian. L^{y} J.

17. Cathá Saritságara. A collection of tales by Somadéva. Dévenágari character. L^{y} J.

18. Suca Saptati. The 70 tales of a parrot. Dévanágari character. S. W. J. The Persians seem to have borrowed their Tuti-náma from this work.

19. Rasamanjari. The analysis of love, a poem, by Bhánudatta Misra. Dévanágari character. L^{y} J.

20. Sántisataca. A poem, in the Bengal character. L^{y} J.

21. Arjuna Gitá. A dialogue, something in the manner of the Bhagavat Gitá. Dévanágari character. L^{y} J.

22. Hitópadésa. Part of the fables translated by C. W. Written in the Bengal character. L^{y} J.

23. Brahmá Nirupana. On the nature of Brahmá. Dévanágari character. Imperfect. L^{y} J.

24. Méghaduta. A poem. Bengal character. L^{y} J.

25. Tantra Sára. On religious ceremonies, by Crishnánanda Battácharya. Bengal character. S. W. J.

26. Sahasra Náma. The 1000 names of Vishnu. Dévanágari character. S. W. J.

27. Cirátárjuniya. A poem, in the Bengal character. L^{y} J.

28. Siddhánta Sirómani. A treatise on geography and astronomy, by Bháscarácharya. Dévanágari character. S. W. J.

29. Sangita Nárâyana. A treatise on music and dancing. Dévanágari character. S. W. J.

30. Vrihadáranyaca. Part of the Yajur Vèda, with a gloss, by Sancara. Dévanágari character. L^{y} J.

31. Niructi, or Nairucta. A gloss on the Véda. Dévanágari character. L^{y} J.

32. Aitaréya. A discourse on part of the Véda. Dévanágari character. L^{y} J.

33. Chandasi. From the Sáma Véda. Dévanágari character. L^{y} J.

34. Mágha Tícá. A comment on some other work. Dévanágari character. L^{y} J.

35. Rájaballabha. De materia Indorem medicâ; by Náráyanadasá. Bengal character. L^{y} J.

36. Hatha Pradipaca. Instructions for the performance of the religious discipline called Yóga; by Swátmaráma. Bengal character. L^{y} J.

37. a. Mánava Dharma Sástra. The institutes of Mănŭ, translated into English by S. W. J. under the title of " Institutes of Hindu Law, or the Ordinances of Menu." Dévanágari character. Incorrect. L^{y} J.

37. b. Ditto. Duplicate in the Dévanágari character. Very incorrect. L^{y} J.

38. Mugdha-bódha-ticá. A commentary on the Mugdha-bódha, which is a Sanscrita grammar, peculiar to the province of Bengal, by Durgá Dása. Bengal character. Four vols. L^{y} J.

39. Sáraswati Vyácarana. The Sanscrita grammar called Sáraswati. (That part only which treats of the verb.) Dévanágari character. L^{y} J.

40. Sárâvali. A grammar of the Sanscrita language. Incomplete. Bengal character. S. W. J.

41. Siddhânta Caumudi. A grammar of the Sanscrita language, by Panini, Cátyáyana, and Patanjali; with a duplicate of the first part, as far as compounds. Dévanágari character. L^{y}. J.

42. a. Amara Cósa. A vocabulary of the Sanscrita language, with a grammatical comment. Not perfect. Dévanágari character. L^{y}. J.

42. b. Ditto. The botanical chapter only, with a comment. Dévanágari character. Ly. J.

42. c. ditto. The whole complete. Bengal character. S. W. J.

43. Médini Cósa. A dictionary of the Sanscrita language. Dévanágari character. Ly. J.

44. Viswapracása Cósa. A dictionary of the Sanscrita language; by Mahéswara. Dévanágari character. Ly. J.

45. Sabda Sandarbha Sindu. A dictionary of the Sanscrita language; by Cásinátha Sarman. It appears from the introduction, that it was compiled expressly for the use of S. W. J. The learned author is, at present, head professor in the newly-established college at Varanásí. Dévanágari character. Two vols. folio. Ly. J.

46. Vénisanhára. A drama, Sanscrita and Prácrita, in the Bengal character. Ly. J.

47. Mahá Nátaca. A drama, Sanscrita and Prácrita, in the Bengal character. Ly. J.

48. Sacuntalá. A drama, Sanscrita and Prácrita, in the Bengal character. This is the beautiful play which was translated into English by S. W. J. but not the copy he used for that purpose. Ly. J.

49. Málati and Mádhava. A drama, Sanscrita and Prácrita, in the Bengal character. Ly. J.

50. Hásyárnava. A farce, Sanscrita and Prácrita, in the Bengal character. Ly. J.

51. Cautuca Sarvaswam. A farce, Sanscrita and Prácrita, in the Bengal character. Ly. J.

52. Chandrábhishéca. A drama, Sanscrita and Prácrita. Bengal character. Ly. J.

53. Ratnávali. A drama, Sanscrita and Prácrita. Bengal character. Ly. J.

54. Vicramórvasi. A drama, Sanscrita and Prácrita. Bengal character. Ly. J.

55. Manavicágnimitra. A drama, Sanscrita and Prácrita. Bengal character. Ly. J.

56. A catalogue of Sanscrita books, on various subjects. Dévanágari character. Ly. J.

N. B. Those articles in the above catalogue, marked S. W. J., were presented by Sir William Jones; and those marked Ly. J. by Lady Jones. A catalogue of the Persian and Arabic MSS., presented by them, will be given in a future volume.

END OF THE EIGHTY-EIGHTH VOLUME OF THE ORIGINAL.

I. The Croonian Lecture. Being Experiments and Observations on the Structure of Nerves. By Ev. Home, Esq., F. R. S. An. 1799, *Vol. LXXXIX.* p. 1.

Having had the honour of laying before the R. S. several lectures on the actions of different parts of the organ of vision, the prosecution of the same inquiry has led to some observations on the internal structure of the optic nerve, which will be explained in the present lecture. On the first view, the structure of nerves may appear an improper subject; but, when their offices and connection with muscles are maturely considered, any knowledge respecting them will be allowed an important acquisition towards the investigation of muscular motion. In bringing forward an account of newly-acquired facts, the most natural, and therefore the most satisfactory method is, to begin with the circumstances, which led to their detection. This at present becomes the more proper, as the experiments which brought the subject of nervous structure under consideration, were made on the eye, and were in some measure connected with the observations contained in the former lectures: they were instituted with a view to ascertain the cause of the luminous appearance frequently observed in the cat's eye.

The illumination so conspicuous in the eye of the cat, and of many other animals, when seen in an obscure light, has attracted the attention of every common observer. Philosophers also have paid particular attention to it, and have endeavoured to investigate the cause. On this subject there have been 2 opinions: one, that the illumination arises from the external light collected in the eye, and reflected; the other, that there is a quantity of light generated in the organ itself. Professor Bohn, at Leipsick, made experiments which proved, that when the external light is wholly excluded, none can be seen in the cat's eye. These experiments were favourable to the first opinion; but the brightness of the illumination is so great, that it appeared to exceed any effect which could be produced through the medium of the retina; so that some other source of light was thought necessary to account for the phenomenon: this circumstance gave support to the 2d opinion.

To determine which of the 2 opinions was just, several experiments were instituted, under the direction of Mr. Ramsden, who also assisted in making them. The truth of Professor Bohn's experiments was readily ascertained; it therefore only became necessary to inquire, whether the external light was of itself capable of producing so great a degree of illumination as that seen in the cat's eye. This was attended with difficulty; for, when the apartment, in which the experiments were made, was so much darkened that nothing but the illumination from the eye was visible, the animal, by change of posture, or some other means, almost immediately deprived the observers of all light from that source. This was found to be the case, whether the cat, the tiger, or the hyena, was the subject of the experiment. On the other hand, when the light in the room was sufficient for the animal itself to be seen, the illumination in the eye was more obscure, and appeared to arise from the external surface of the iris.

As the difficulties which occurred in making observations on the illuminated state of the eye in the living animal were so great, an attempt was made to repeat, as nearly as possible, the experiments after death. In doing so it was found, that a strong light thrown on the cornea illuminated the iris, as it had done in the living eye; but when the cornea was removed, this illumination disappeared. The iris was then dissected off, and the lucid tapetum completely exposed to view; the reflexion from which was extremely bright; the retina proving no obstruction to the rays of light, but appearing equally transparent with the vitreous humour and crystalline lens.

From these experiments it appeared evident that no light is generated in the eye; the illumination being wholly produced by the concave bright-coloured surface of the tapetum, collecting the rays of the external light, concentrated by the cornea and crystalline lens, and reflecting them through the pupil. When the iris is completely open, the degree of brilliancy is the greatest; but, when the iris is partly contracted, which it always is when the external light is increased, then the illumination is more obscure, and appears to come from the iris; a part of the light reflected from the tapetum being thrown back, by the concave surface of the cornea, on the anterior surface of the iris, giving it a bright shining appearance. The influence which the will of the animal has over this luminous appearance seems altogether to depend on the contraction and relaxation of the iris. When the animal is alarmed or first disturbed, it naturally dilates the pupil, and the eye glares; when it is appeased or composed, the pupil contracts, and the light in the eye is no longer seen. The most material information that has been gained in this investigation, is the transparent state of the retina in the eye during life; the opaque membranous appearance which it puts on in the dead body not being natural to it, but a change which takes place in consequence of death. This fact is almost all that is necessary to explain the luminous appearance in the eyes of cats.

That neither Baron Haller nor Fontana had an adequate idea of the transparency of the retina, will appear from the following expressions respecting it, taken from their works: Haller describes it in the following words: "Membranam crassam quidem, sed mollisimam, pellucidam utique, quando recens oculus inspicitur, ut per eam sub aquis choroideam videas; tamen ex flavo subcineream."* So that, though he calls it transparent, he says it is of a yellowish ash-colour. Fontana's expressions are, " Cette insensibilité de la rétine, à la lumière, en tant que lumière, dérive-telle de ce que les nerfs sont encore trop gros, et ne sont pas bien découverts des tissus cellulaires? ou de ce que la pulpe de la rétine est trop amoncelée, et empêche les rayons de lumière d'arriver jusqu'à ces mêmes nerfs?"†

In considering the use of the lucid tapetum, it was an idea of the late Mr. Hunter's, that the retina received a double stroke from the rays of light which entered the eye; one in passing to the tapetum, the other in returning from it. This very ingenious opinion had some difficulties opposed to it, while the retina was supposed capable of obstructing the rays of light even in the smallest degree, as they could not be equably transmitted, so as to affect every part of the membrane alike. But the retina being ascertained to be absolutely transparent, these objections are entirely removed, and there can be no doubt that the rays of light, in those eyes which have a lucid tapetum, must remain on the retina as long again as in the eyes of other animals; since the time required to strike on the tapetum, and return, must be twice as much as is necessary for passing through the retina, to reach the nigrum pigmentum, where they are lost. This may appear to be a consideration of little consequence, as the velocity of light is so great, and the continuance of impression necessary for distinct vision is that produced by a successive flow of similar rays of light from the object; it may however be all that is necessary for the purpose.

The retina being found perfectly transparent, when the eye is examined in a recent state, led to the eye that the internal structure of the optic nerve, when examined in the same state, might also be transparent. To ascertain this point, the following experiment was made: The posterior $\frac{1}{2}$ of a cat's eye, while in a very recent state, was immersed in a basin of water, and examined. The tapetum appeared very bright, the retina not having acquired sufficient opacity to become

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* Elementa Physiologiæ, tom. 5, p. 385.-Orig. † Sur le Venin de la Vipere, 1781, vol. 2, p. 219.-Orig.

visible: the entrance of the optic nerve was a very white spot, which seemed to be opaque; but, when small pieces of coloured paper were alternately placed between the outside of the eye, and the bottom of the basin, their colour was distinctly seen in the cavity of the eye, through the substance of the optic nerve; so that, at this part, the internal structure of the nerve has a degree of transparency. This appeared to be a newly-discovered fact; and, to ascertain whether it was really so, the works of several physiological writers were consulted, but nothing was found which gave an idea that their authors had the smallest knowledge of it. This semi-transparent state of the internal parts of the optic nerve, while recent, led naturally to the examination of its substance, by means of magnifying glasses; and notwithstanding the failure of so many men of superior abilities, in this intricate inquiry, it held out the hope of meeting with some success.

The principal theories which have been formed respecting the structure of nerves, which have been taken notice of by Fontana, as they all differ from the observations which will be stated in the present paper, it may not be improper to mention the heads of each of them, so as to bring into one point of view, all the knowledge that has been acquired on the subject. Torre found the medullary substance of the brain, spinal marrow, and nerves, to be a mass of transparent globules, swimming in a transparent fluid. When the parts were magnified 1000 times, the globules appeared largest in the brain, and smaller in the spinal marrow; they had no regular order: but in the nerves the globules were placed in lines, so as to give the appearance of fibres. In examining the optic nerve, the parts were magnified 120 times. Prochaska considered the nerves to be composed of globules, united by a transparent elastic cellular membrane, and disposed in straight lines, resembling fibres. Fontana found the primitive structure of nerves to consist of transparent cylinders, which, when united, formed the nerve: the manner of their being disposed is not mentioned. The objects were magnified 700 times, to show this appearance. Dr. Monro considered the nerves as made up of spiral fibres; but afterwards found that what he had described was entirely an optical deception. In his last work, he says, "The optic nerves have, in their whole course, less appearance of a fibrous structure than perhaps any other pair of nerves in the human body." Other authors may have written on this subject, and may have made observations on the structure of nerves, but want of leisure must be an excuse for my not having come to a knowledge of them.

It is scarcely necessary to mention, that parts of an animal body are not fitted for being examined by glasses of a great magnifying power; and wherever they are shown 100 times larger than the natural size, no dependence can be placed on their appearance. In making the following microscopical experiments on the internal structure of the optic nerve, great care was taken to avoid the errors of former inquirers. The microscope used was a single one; the focal length of the lens was about $\frac{35}{100}$ of an inch, so that the object was magnified about 23 times; and, that the results of the experiments might be as free from optical deceptions as the present state of our knowledge in this branch of science will admit, no appearance is described which Mr. Ramsden was not satisfied of having distinctly seen. The experiments performed with the single microscope were repeated with a double one, made by Mr. Ramsden, which magnified the object about 40 times; but in the double microscope the appearances were indistinct, the reflexion from the different glasses having thrown a confused glare on the moist surface of the nerve. This circumstance led Mr. Ramsden to object to the use of compound microscopes, and to consider them as unfit for viewing objects of this kind.

For the following reasons, the optic nerve of the horse was selected, as the most proper for the experiment. It is of a large size, and several inches in length. It is readily procured in a recent state; as there are places in London where horses are allowed to be killed, and regular days in the week are fixed for that purpose. That the examination of the nerve might be made as soon as possible after the animal's death, permission was procured from the man who superintends the killing of horses, to allow Mr. Clift to make the necessary experiments on the spot, the moment the horses were killed. Mr. Clift is the person entrusted with the care of keeping in order the late Mr. Hunter's collection in comparative anatomy, and is well qualified, from his anatomical knowledge, and a familiarity in looking at organized parts through magnifying glasses, for an examination of this kind. These experiments were afterwards repeated by Mr. Ramsden and myself. From this mode of conducting them, the chances of error were few; since the person who first observed the appearances had no previous opinions on the subject; and Mr. Ramsden was better able than any other person, to correct such optical errors as might deceive Mr. Clift or myself.

The first experiments were made on transverse sections of the nerve. One, near its termination in the eye, was placed on glass, and exhibited in the microscope the following appearances: it was evidently composed of 2 parts, 1 opaque, the other transparent. The opaque portions were nearly circular in their shape, about 600 in number, and touched each other; the interstices between them were transparent. When the opaque parts were attentively examined in a favourable light, and the nerve was in a recent state, they were found to be made up of a great number of smaller portions, each of which appeared to be also opaque. To see this subdivision of parts required some attention, and in many sections it could not be perceived. The cause of the difficulty seemed to be, the softness and tenacity of the substance divided, which therefore spread itself over the surface, giving it a uniform appearance: but towards the circumference of the nerve, where the parts were cut obliquely, and some of them torne, the subdivision was very distinct. It was first observed by Mr. Clift, in several different sections; and was afterwards seen very distinctly, both by Mr. Ramsden and myself, in a nerve examined about 2 hours after death.

Having repeated these experiments 6 or 7 times, on different days, so as to ascertain the accuracy of the results, the next object was, to determine whether the nerve had the same structure in its whole course. For this purpose, transverse sections were examined in different parts of the nerve, near the brain towards the middle, and nearer the eye: of these experiments the following are the results. In all the sections, the nerve appeared to be made up of the same substances; but the size and number of the opaque parts differed very much. They have been stated, near the eye, to be 600; about the middle of the nerve, they were 150; and, near the brain, between the origin and union of the 2 nerves, they were only about 40. As they became larger, they were less regular in their shape, and had less of a circular form; nor were they uniform, some appearing very large, with 1 or 2 smaller placed between them.

After having succeeded in this examination of the nerve transversely, an attempt was made to investigate its structure in a longitudinal direction. To do this, a portion of the nervous pulp had its coat, formed by the dura mater, along with a thin vascular membrane which lines it, carefully removed for about an inch in length; the external surface of the pulp was then examined with a magnifying glass; the structure was evidently fasciculated, but the fasciculi did not run parallel to each other; they seemed to unite together and separate again, in such a manner that any one of them could not be traced for half an inch in length, without being lost in the neighbouring part. When thin sections were examined in the field of the microscope, they put on the same appearance: this was equally the case, whether the part examined was near the centre or circumference of the nerve. The fasciculi were largest in that part of the nerve near the brain, and smallest towards the eye. Great pains were taken to ascertain whether the fasciculi were made up of continued fibres, or of small parts unconnected, which, from their position, gave that appearance; but every observation that was made was in proof of their being continued fibres.

From these experiments, the internal structure of the optic nerve appears to be made up in the following manner: At its origin from the brain, it consists of 30 or 40 fasciculi or bundles of extremely small opaque pulpy fibres, the interstices between which are filled with a transparent jelly. As the nerve goes farther from the brain, the fasciculi form smaller ones of different sizes. This is not done by a regular subdivision, but by a few fibres going off laterally from several large fasciculi, and being united, forming a smaller one: some of the fasciculi so formed, which are very small, unite again into one. In this way, the fasciculi gradually diminish in size, and increase in number, till they terminate in the retina. Near the eye, where the fasciculi are most numerous, the substance of the nerve has a considerable degree of transparency, from the number of transparent interstices between them; but this is less the case nearer the brain, where the interstices are fewer. In the optic nerve of the cat, the structure is the same as in the horse; but, from the smallness of the parts, less fitted for investigation. Near the eye, its internal substance is more transparent than the corresponding part in the horse.

To see how far this structure was peculiar to the optic nerve, similar experiments were made on the internal substance of the 5th and 7th pair of nerves, near their origin at the brain, and the structure was found to be the same. In these last mentioned nerves, the interstices between the fasciculi were smaller than in the optic nerve, rendering their transverse sections less transparent; from which it is natural to suppose, that the internal parts of the optic nerve are not so compact as in other nerves, and therefore it is better fitted for examination.

These experiments show, that the nerves do not consist of tubes conveying a fluid, but of fibres of a peculiar kind, different from every thing else in the body, with which we are acquainted. The course of these fibres is very curious; they appear to be constantly passing from one fasciculus to another, so as to connect all the different fasciculi together by a mixture of fibres. This is different from the course of blood-vessels, lymphatics, or muscular fibres: the only thing similar to it, is in the formation of nervous plexuses; which leads to the idea of its answering an essential purpose, respecting the functions of the nerves.

II. Observations on an Unusual Horizontal Refraction of the Air; with Remarks on the Variations to which the Lower Parts of the Atmosphere are sometimes Subject. By the Rev. S. Vince, A.M., F.R.S. Being the Bakerian Lecture. p. 13.

The uncertainty of the refraction of the air near the horizon has long been known to astronomers, the mean refraction varying by quantities which cannot be accounted for from the variations of the barometer and thermometer; on which account, altitudes of the heavenly bodies, which are not more than 5° or 6°, ought never to be made use of when any consequences are to be deduced from them. The cause of this uncertainty is probably the great quantities of gross vapours, and exhalations of various kinds, which are suspended in the air near the earth's surface, and the variations to which they are subject; causes of which we have no instruments to measure the effects they produce, in refracting the rays of light. In general, the course of a ray passing through the atmosphere, is that of a curve which is concave towards the earth, the effect of which is to give an apparent elevation to the object; and thus the heavenly bodies appear above the horizon, when they are actually below it; but it will not alter the position of their parts, in respect to the horizon, that is, the image of the highest part of the object will be uppermost, and the image of the lowest part will be undermost. The figures however of the sun and moon, when near the horizon, will suffer a change, in consequence of the refraction of the under limb being greater than that of the upper; from which they assume an elliptical form, the minor axis of which is perpendicular to the horizon, and the major axis parallel to it. But a perpendicular object, situated on the surface of the earth, will not have its length altered by refraction, the refraction of the bottom being the same as that of the top. These are the effects which are produced on bodies at or near the horizon, in the common state of the atmosphere, by what I shall call the usual refraction.

But, besides the usual refraction which affects the rays of light, the atmosphere over the sea is sometimes found to be in a state which refracts the rays in such a manner as to produce other images of the object, which we will call an effect from an unusual refraction. In the Phil. Trans. for 1797, Mr. Huddart has described some effects of this kind, which he has accounted for by supposing that, from the evaporation of the water, the refractive power of the air is not greatest at the surface of the sea, but at some distance above it; and this will solve, in a very satisfactory manner, all the phenomena which he has observed. But effects very different from those which have been described by Mr. Huddart are sometimes found to take place. These I had an opportunity of observing at Ramsgate last summer, on August the first, from about half an hour after 4 o'clock in the afternoon till between 7 and 8. The day had been extremely hot, and the evening was very sultry; the sky was clear, with a few flying clouds. I shall describe the phenomena as I observed them with a terrestrial telescope, which magnified between 30 and 40 times; they were visible however to the naked eye. The height of the eye above the surface of the water, at which most of the observations were made, was about 25 feet; some of them however were made at about 80 feet from the surface; and it did not appear that any of the phenomena were altered by varying the height of the eye, the general effect remaining the same.

The first unusual appearance observed, was that which is represented in pl. 8, fig. 1. Directing my telescope at random, to examine any objects which might happen to be in view, I saw the top of the masts of a ship A, above the horizon xy, of the sea, as shown in the figure; at the same time also, I discovered in the field of view, 2 complete images, B, C, of the ship in the air, vertical to the ship itself, B being inverted, and C erect, having their hulls joined. The phenomenon was so strange, that I requested a person present to look into the telescope, and examine what was to be seen in it, who immediately described the 2 images, as observed by myself; indeed they were so perfect, that it was impossible we could differ in our description. On this I immediately took a drawing of the relative magnitudes and distances of the ship and its images, which at that time were as represented in the figure, as near as it was possible for the eye to judge; and it was very easy to estimate them to a very considerable degree of accuracy. As the ship was receding from the shore, less and less of its masts became visible; and, continuing my observations, in order to discover whether any, or what variations might take place, I found that as the ship descended, the images B, C, ascended; but as the ship did not sink below the horizon, I had not an opportunity of observing at what time, and in what order, the images would have vanished, if the ship had so disappeared.

Being desirous of seeing whether the same effect was produced on the other ships which were visible, I directed my telescope to another ship A, fig. 2, whose hull was just in the horizon xy; when I observed a complete inverted image B, the mainmast of which just touched that of the ship itself. In this case, there was no 2d image as before. The ship A moving on the horizon, B continued to move with it, without any variation in its appearance.

The next ship I directed my telescope to, was so far on the other side of the horizon xy, as just to prevent its hull from being seen, as is represented by A, fig. 3. And here I observed only an inverted image of part of the ship; the image y of the topsail, with the mast joining that of the ship, the image x of the top a of the other mast, and the image z of the end c of the bowsprit, only appearing at that time. These images would suddenly appear and disappear very quickly after each other; first appearing below, and running up very rapidly, showing more and less of the masts at different times, as they broke out; resembling, in the swiftness of their breaking out, the shooting out of a beam of the aurora borealis. As the ship was descending on the other side of the horizon, I continued my observations on it, in order to discover what changes might take place; when I found, that as it continued to descend, more of the image gradually appeared, till at last the image of the whole ship was completed, with their main-masts touching each other; and, on the ship descending lower, the image and the ship separated; but I observed no 2d image, as in the first case; a 2d image however might probably have appeared, if the ship had continued to descend.

On moving my telescope along the horizon, in order to examine any other ships which might be in sight, I observed, just at the horizon xy, in fig. 4, the top a of the mast of a ship; and here an effect was observed which had not been before discovered; for there was an inverted image B, vertical to a, an erect image c, both of them very perfect and well defined, and an image vw of the sea between them, the water appearing very distinctly. As the ship was coming up towards the horizon, I continued to observe it, in order to discover the variations which might follow, and found, that as the ship approached the horizon, the image c gradually disappeared, and at last it vanished; after that, the image vw of the sea disappeared; and during this time the image B descended; but the ship did not rise so near to the horizon as to bring the main-masts together. Had I directed my telescope to the same point of the horizon a little sooner, I should have seen the 2 images before the ship itself was visible. In fact, the images were visible when the whole ship was actually below the horizon; for, from the very small part of the mast which was at first visible, that part must then have been below the horizon, and appeared above it by the usual refraction; the altitude of a, above the horizon, having then been much less than the increase of altitude which arises from the common horizontal refraction. The discovery of ships in this manner might, in some cases, be of great importance; and on such occasions it might be worth while to appoint proper persons to make observations for that purpose.

The cliffs at Calais being very visible, I directed my telescope towards them, in order to examine whether there was any thing unusual in their appearance; when I observed an image of the cliffs, above the cliffs themselves, together with an image of the sea separating them, as is represented in fig. 5; in which, xy represents the horizon of the sea, AB the cliffs, ab their image, and vw the image of the sea between them: the depth of ab was much less than that of AB. It is probable however, that vw might not be the image of the sea immediately adjoining to the cliffs, but a partial elevation of the sea at some distance from them; and that the image vw might intercept some part of the image ab, which would otherwise have been visible; we must not therefore conclude that the image ab, so far as it appeared, was less than the corresponding part of the object. From the memorandums which I made at the time of observation, I do not find that I examined the appearance of the cliff AB, and its image ab; which, had there at that time been any striking marks in them, would have determined whether the object and its image were of the same magnitude. The image ab was however erect; the boundaries on the top of AB and ab agreeing together. Having examined this for some time, and taken a drawing of the appearance, during which I could discover no variation, I directed my telescope to other objects; and on turning it again to the same cliffs, after the space of about 6 or 7 minutes, the images ab and vw were vanished; but examining them again soon after, the images were again visible, and in every respect the same as they appeared before. A short time after they disappeared, and did not appear any more.

Soon after the above appearances, I observed a ship C, with the hull below the horizon xy, passing by the same cliffs AB; an inverted image D of which appeared against the cliffs, as represented in fig. 6. The ship was in motion, and remained at the same distance on the other side of the horizon: I continued my observations on it till it had passed the cliffs for a considerable distance, but there was no change of appearance. The cliffs were illuminated by the sun, and appeared very distinctly; but there was no image above, as in the last case. Continuing to observe the same cliffs AB, fig. 7, I soon after discovered 2 partial elevations m, n, of the sea, by the unusual refraction; they changed their figures a little, and disappeared in the place where they first appeared, and were equally distinct in every part.

About this time, I observed a very thick fog coming on the horizon from the other side, rolling on it with a prodigious velocity; curling as it went along, like volumes of smoke sometimes out of a chimney. This appeared several times. I conclude therefore, that there was a considerable fog on the other side of the horizon. The last phenomenon observed was that represented in fig. 8; where xy represents the horizon, ab 2 partial elevations of the sea, meeting at c, and continued to d; e, another partial elevation of the sea, of which kind I observed several, some of which moved parallel to the horizon, with a very great velocity. I conjecture therefore, that these appearances were, in part at least, caused by the fog on the other side of the horizon. For though I did not at the same time see the motion of these images and that of the fog, yet from memory I judged the motions to be equal: and they were also in the same direction. A fog which, by producing an unusual refraction, might form these images, would, by its motion, produce a corresponding motion of the images.

I have here described all the different phenomena which I observed from the unusual refraction, of most of which I saw a great many instances. Every ship which I observed on the other side of the horizon of the sea, exhibited phenomena of the kind here described, but not in the same degree. Of 2 ships which, in different parts, were equally sunk below the horizon, the inverted image of one would but just begin to appear, while that of the other would represent nearly the whole of the ship. But this I observed in general, that as the ship gradually descended below the horizon, more of the image gradually appeared, and it ascended; and the contrary when the ships were ascending. On the horizon, in different parts, one ship would have a complete inverted image; another would have only a partial image; and a 3d would have no image at all. The images were in general extremely well defined; and frequently appeared as clear and sharp as the ships themselves, and of the same magnitude. Of the ships on this side of the horizon, no phenomena of this kind appeared. There was no fog on our coast; and the ships in the Downs, and the South Foreland, exhibited no uncommon appearances. The usual refraction at the same time was uncommonly great; for the tide was high, and at the very edge of the water I could see the cliffs at Calais a very considerable height above the horizon; whereas they are frequently not to be seen in clear weather from the high lands about the place. The French coast also appeared both ways, to a much greater distance than I ever observed it at any other time; particularly towards the east, on which part also the unusual refraction was the strongest.

During the remainder of my stay at Ramsgate, which was about 5 weeks, I continued daily to examine all the ships in sight; but I discovered no phenomena similar to those above described. The phenomenon of the ship observed by Mr. Huddart, differed altogether from those above described, as the inverted image which he observed was below the ship itself. An appearance of this kind I observed on August the 17th, about half an hour after 3 in the afternoon, of which fig. 9 is a representation. The real ship is represented by A, and the image by B; er, mv, the hulls; st the flag, and wx its image, just touching it, with the sea xy below. Between the 2 hulls, some faint dark spots and lines appeared, but I could not discover what they were the representatives of. The vessel, at the time of this appearance, was not quite come up to the horizon; and as it approached it, the image gradually diminished, and totally disappeared when the ship arrived at the horizon.

It remains now, that we inquire into the causes which might produce the very extraordinary effects above related. From the phenomena, we are immediately led to the nature of the path of the rays of light to produce them; and we may conceive, that the air may possibly be in such a state as will account for the unusual track which they must have described, For, let bz, fig. 10, be the surface of the sea; ab an object; E the place of the eye; arE, bsE, the progress of 2 rays, by the usual refraction, from the extreme parts of the object to the eye; to these curves draw the tangents Ea′, Eb′, and a′ b′ will be the image of the object, as usually formed. Now if we take the case represented in fig. 4, let a″ b″ represent the inverted image, and a‴ b‴ the erect image; join a″E, the a‴E, and b″E, b‴E, and these lines must respectively be the directions of the rays entering the eye from a and b, in order to produce the images a″ b″ and a‴ b‴; hence these lines must be tangents at E, to the curves which are described by the rays of light; let therefore anE, amE, bvE, bwE, be the curves described. We have therefore to assign a cause which may bring rays passing above the rays arE, bsE, to the eye at E. Now if there were no variation of the refractive power of the air, a ray of light passing through it would describe a straight line; therefore the curvature of a ray of light passing through the atmosphere, depends on the variation of the refractive power of the air. If therefore we suppose the air lying above arE, to vary quicker in its refractive power than the air through which arE passes, the curvature of a ray proceeding above that of arE, will be greater than the curvature of arE; and on this principle we may conceive that a ray may describe the curve anE: and, in like manner, if a quicker variation of refractive power should take place above the curve anE, than in that curve, a 3d ray may describe the curve amE. The same may be said for the rays bvE, bwE, diverging from b. The alterations of the refractive power may arise, partly from the variations of its density, and partly from the variations of its moisture; and the passage of the rays through the boundary of the fog may there suffer a very considerable refraction; for, from the motion of the fog, and that of the images above-mentioned, I have no doubt that the fog was a very considerable agent in producing the phenomena. When all the causes co-operate, I can easily conceive that they may produce the effects which I have described. If the cause should not operate in the tract of air through which the curves anE, bvE pass, but should operate in the tract through which amE, bwE pass, an erect image would be visible, but there would be no inverted image; and should it operate in the latter case, but not in the former, there would be only an inverted image.

As the phenomena are very curious, and extraordinary in their nature, and have not, that I know of, been before observed, I have thought proper to lay a description of them, with all the attending circumstances, before the R. S. They appear to be of considerable importance; as they lead us to a knowledge of those changes to which the lower parts of the atmosphere are sometimes subject. If, when these phenomena appear, a vessel, furnished with a barometer, thermometer, and hygrometer, below, and also at the top of the mast, were sent out to pass below the horizon and return again, and an observer at land, having like instruments, were to note, at certain intervals, the situation and figure of the images, it might throw further light on this subject, and lead to useful discoveries respecting the state of the atmosphere, from a conjunction of the causes which affect these instruments.

III. *Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland,* 1797. *With some Remarks on the Recovery of injured Trees. By Thos. Barker, Esq.* p. 24.

		Barometer.			Thermometer.						Rain.
					In the House.			Abroad.			
		Highest.	Lowest.	Mean.	Hig.	Low	Mean	Hig.	Low	Mean	
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.
Jan.	Morn.	29.99	29.01	29.58	46	33	39	48	26	37	1.319
	Aftern.				48	36	40	51	29	39	
Feb.	Morn.	30.07	28.84	50	47	37	42	46½	25	34	0.076
	Aftern.				48	38½	43½	52½	32	42½	
Mar.	Morn.	29.90	29.03	47	46	38	41½	46	29½	35½	0.908
	Aftern.				47	39	43	53	36½	44½	
Apr.	Morn.	29.70	28.63	31	51½	42½	46	52	36	42½	2.882
	Aftern.				52½	43½	47	59	37	49	
May	Morn.	29.86	28.44	38	67	44	53	64	38	50½	2.528
	Aftern.				69	44½	54½	76	41½	59½	
June	Morn.	29.85	28.93	41	60	52	57	63	39½	52½	4.221
	Aftern.				62½	53½	58	76	43½	63	
July	Morn.	29.80	29.05	49	70½	57	63	66	54	61	3.075
	Aftern.				72½	59	65½	83	60	72½	
Aug.	Morn.	29.72	29.05	35	64	58	60½	64	49	58	2.415
	Aftern.				65½	59	62	76	62	68	
Sept.	Morn.	29.67	28.67	28	62	51½	56	58	43	50	4.792
	Aftern.				64	54	57	70½	51	60	
Oct.	Morn.	29.83	28.57	37	55½	41½	49½	55	31½	44	1.143
	Aftern.				57½	42½	50	63	42½	52	
Nov.	Morn.	29.98	28.50	43	49	34	43½	51½	24½	38½	1.620
	Aftern.				49½	34½	44	53	30	44	
Dec.	Morn.	29.93	28.60	27	49½	36	42	53	29	38½	2.875
	Aftern.				49½	36½	42½	53	32	42	
Whole year.				29.40			50			49	27.854

On the recovery of injured trees.—About the year 1788 or 1789, a Lucombe oak was planted, the top of which might be about 6 feet high, but was broken off in coming down. In spring, the tree put out at the highest 2 buds, but much better at some lower ones. In 1791, the highest 2 buds again put out, yet, as before, very indifferently; however, a lower bud, about 5 feet high, put out a strong shoot, about 15 inches long; but, as side branches are apt to do, it did not grow upright, but slanting. In winter I fixed that strong shoot upright, by tying it to another shoot, which came out of the opposite side of the tree; and in 1792 it made a very strong, straight, and upright shoot, 3 feet 9 inches long, and as thick as a moderate finger, and has continued thriving ever since. The tree is now about 18 feet high, and 11 inches in girth at the bottom; it has been pretty well cleared of boughs about half way up, and may perhaps, by degrees, be cleared several feet higher.

In the winter of 1789, an ash tree was cut down, which, falling against a young oak, as thick as my leg, beat it down. I had the oak cut close to the ground, and in 1790 it put out a number of shoots, which grew that year, and 1791. In 1792, I chose out the best shoot, trained it up as straight as I could, and beat down the rest of the shoots to the ground, under the hedge, to weaken them, and encourage the best shoot, which I intend to be the tree. It has since grown strong, is pretty straight, has been pruned, and may I believe by degrees be cleared to a good height, for the leading bud is strong and upright. It is now (in August, 1798) about 14½ feet high, and about 6½ inches in girth at the bottom.

IV. Additions to a Paper, on the Subject of a Child with a Double Head. By Evd. Home, Esq. F. R. S. p. 28.

In the year 1790, I laid before the R. S. an account of a child with a double head, illustrated by drawings, which is honoured with a place in the Phil. Trans., vol. 80, (Abridgt. vol. 16, page 663). Since that time, Mr. Dent, the gentleman who sent over from India the double skull, which was shown at the meeting when the paper was read, has returned to England. Among his drawings there are 2 portraits of the double head, taken by Mr. Devis, an artist of considerable merit, who was on a visit at Mr. Dent's house, in Bengal, when the child was brought there alive, to be shown as a curiosity. These drawings give a more faithful representation of the appearance of the double head, than the engravings annexed to the former paper, and at the same time exhibit a striking likeness of the child's features.

Mr. Dent's observations, in addition to those already in the possession of the Society, are the following. The child was a male. Its father was a farmer at Mundul Gaut, in the province of Bardwan, who told Mr. Dent, that it was more than 4 years old at the time of its death *. The mother, who was 30 years of age, had 3 children, all naturally formed; and her 4th child was the subject of the present paper. Mr. Dent endeavoured to discover whether any imaginary cause had been assigned by the parents, for the unnatural formation of the child; but the mother declared, that no circumstance whatever, of an uncommon nature, had occurred: she had no fright, met with no accident, and went through the period of her pregnancy exactly in the same way as she had done with her other children. The body of the child was uncommonly thin, appearing emaciated from want of due nourishment. The neck of the superior head was about 4 inches long; and the upper part of it terminated in a hard, round, gristly tumour, nearly 4 inches in diameter. The front teeth had cut the gums, in the upper and under jaws of both heads. When the child cried, the features of the superior head were not always affected; and when it smiled, the features of the superior head did not sympathize in that action.

In preparing the skull, which unpleasant operation Mr. Dent was obliged, from the prejudices of his servants, to superintend, he found that the dura mater belonging to each brain, was continued across, at the part where the 2 skulls joined,

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* In the former account, the child is said to have been about 2 years old at that time.—Orig.

so that each brain was invested, in the usual way, by its own proper coverings; but the dura mater which covered the cerebrum of the upper brain, adhered firmly to the dura mater of the lower brain: the 2 brains were therefore separate and distinct, having a complete partition between them, formed by a union of the duræ matres. When the contents of the double skull were taken out, and this union of the duræ matres more particularly examined, a number of large arteries and veins were seen passing through it, making a free communication between the blood-vessels of the 2 brains. This is a fact of considerable importance, as it explains the mode in which the upper brain received its nourishment. Before these observations were communicated by Mr. Dent, it was natural to suppose that the 2 brains had been united into 1 mass; as it was difficult to imagine in what way the upper brain could be supplied with blood.

V. Observations on the Manners, Habits, and Natural History, of the Elephant. By Jno. Corse, Esq. p. 31.

Since the remotest ages, the elephant, on account of his size, his sagacity, and his wonderful docility, has attracted the notice, and excited the admiration, of philosophers and naturalists, both ancient and modern: and few travellers into Asia, or Africa, have omitted giving some account of him. A residence however of more than 10 years, in Tiperah, a province of Bengal, situated at the eastern extremity of the British dominions in Asia, where herds of elephants are taken every season, afforded me frequent opportunities of observing, not only the methods of taking them, but also the habits and manners of this noble animal. From the year 1792 till 1797, the elephant hunters were entirely under my direction; so that I had it in my power to institute such experiments as I thought likely to discover any particulars, not formerly known, in the natural history of the elephant. Soon after my arrival at Tiperah, while informing myself of the methods of taking wild elephants, I had occasion to observe, that many errors, relative to the habits and manners of that useful animal, had been stated in the writings of European authors, and countenanced by some of the most approved writers.

The elephant has been declared to possess the sentiment of modesty in a high degree; and, by some, his sagacity was supposed to excite feelings for the loss of liberty, so acute, as to cause him to refuse to propagate his species while in slavery, lest he should entail on his progeny a fate similar to his own; while others have asserted, that he lost the power of procreation in the domestic state. So circumstanced, I was desirous of taking advantage of my situation, and of making such experiments and observations, as might tend to render more perfect the natural history of this useful animal. Early in the year 1789, I gave an account of the methods then used for taking and training wild elephants, to the Asiatic Society in Calcutta, which was published in vol. 3, of their researches; and the following experiments and observations, made since that period, on the natural history of the elephant, will not, I hope, prove unworthy the attention of the R. S.

The young of the elephant, at its birth, is about 35 inches high; and, as a knowledge of its progressive growth forms the best criterion by which we can judge of the age of this animal, I shall here note down some observations made on this subject, till the elephant has attained its full size; for, after this period, till signs of old age appear, I do not know any marks by which a tolerable guess can be made of the number of its years, unless we could examine the teeth accurately; and even then there would be much uncertainty. Very erroneous notions have been entertained, with respect to the size of elephants, in different parts of India; for which reason, I have collected such facts as were likely to ascertain their general height. The following observations, of the gradual increase of growth, were made on a young elephant of Mr. Stephen Harris, which was accurately measured from time to time, and on a female elephant of my own, till I left Tiperah.

Mr. Harris's elephant, at its birth, Oct. 16, 1789, was 35 inches high. In 1 year he grew 11 inches, and was 3 feet 10 inches high. In the 2d year he grows 8 inches. In the 3d, 6. In the 4th, 5. In the 5th, 5. In the 6th, 3½. In the 7th, 2½. And was then 6 feet 4 inches high. Except during his 4th and 5th years, this measurement shows a gradual decrease in the proportion of growth for every year; and there was no opportunity of tracing the growth of this elephant further than its 7th year.

Another elephant, 6 feet 9 inches high, at the time she came into my possession, was supposed to be 14 years old; but as the accuracy of the hunters cannot be depended on, it will be proper to take Mr. Harris's elephant, whose age is exactly known, as a standard; and, judging from its annual increase, this will lead us to consider the elephant, at the time I received her, to be only 11 years old; giving a period of 4 years, for the addition of 5 inches. I have made a greater allowance of time, on account of this elephant being a female, and Mr. Harris's a male, which there is much reason to believe grows faster. During the next 5 years, before she was covered, she grew only 6 inches; but, what is extremely curious, while pregnant, she grew, in 21 months, 5 inches; and, in the following 17 months, though again pregnant, she grew only ½ an inch; at which time, she was sent from Comillah, as I was then preparing to leave India. At this time, she was about 19 years old, and had perhaps attained her full growth. Her young one was then (Nov. 1796) not 20 months old; yet he was 4 feet 5½ inches high, having grown 18 inches since his birth; which is the greatest progressive growth in the elephant that I have known.

These observations, when applied to the general growth of elephants, are to be taken with some allowance; since, during the state of the first pregnancy, there is so great an irregularity in the growth of female elephants, as alone occasions considerable difficulty, even supposing the progressive growth nearly equal in the species. It is probable, however, that this is not by any means equal: for, as elephants vary greatly in size, and as males are generally much taller than females, we must conclude they either grow faster, or are longer in attaining their full growth*. But it may be safely asserted, that elephants, like most quadrupeds, propagate their species before they have acquired their full growth. Many females have been known, when taken while pregnant, to have grown several inches higher before delivery; and, as it has been stated, that the female elephant on which my observations were made, could not exceed 16 years when she received the male, it is probable the wild female elephants are in heat before that period.

If from the above data, it may be allowed to form a probable conjecture, elephants attain their full size between 18 and 24 years of age. The height of the elephant, I believe, has been generally much exaggerated. In India, the height of females is, in general, from 7 to 8 feet; and that of males, from 8 to 10 feet, measured at the shoulder. I have never heard of more than one elephant, on good authority, that much exceeded 10 feet: this was a male, belonging to Asoph ul Dowlah, the late vizier of Oude. His dimensions, as communicated by Mr. Cherry, then resident at Lucknow, were as follow, measured on the 18th of June, 1796: from foot to foot, over the shoulder 22 feet 10½ inches. From the top of the shoulder, perpendicular height 10 feet 6 inches. From the top of the head, when set up, as he ought to march in state 12 feet 2 inches. From the front of the face to the insertion of the tail 15 feet 11 inches.

Capt. Sandys, of the Bengal establishment, showed me a list of about 150 elephants, of which he had the management during the late war with Tippoo Sultaun, in Mysore, and not one of them was 10 feet, and only a few males 9½. I was very particular in ascertaining the height of the elephants employed at Madras, and with the army under Marquis Cornwallis, where there were both Ceylon and Bengal elephants; and I have been assured, that those of Ceylon were neither higher, nor superior, in any respect, to those of Bengal; and some officers assert, that they were considerably inferior, in point of utility. The Madras elephants have been said to be from 17 to 20 feet high; but, to show how much the natives of India are inclined to the marvellous, and how liable Europeans themselves are to mistakes, I will relate a circumstance that happened to myself.

Having heard, from several gentlemen who had been at Dacca, that the Nabob there had an elephant about 14 feet high, I was desirous to measure him; especially as I had seen him often myself, during the year 1785, and then supposed him to be above 12 feet. After being at Tiperah, and having seen many elephants caught, in the years 1786, 1787, and 1788, and finding all of them much inferior in height to what I supposed the Nabob's elephant, I went to Dacca in 1789, determined to see this huge animal measured. At first, I sent for the mahote or driver, to ask some questions concerning this elephant; he assured me he was from

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* A male elephant, belonging to the Cudwah Rajah till he was above 20 years of age, continued to increase in height, and was supposed not to have attained his full size, when I left Tiperah: he was then about 22 years old.—Orig.

10 to 12 cubits, that is, from 15 to 18 feet high; but added, he could not, without the Nabob's permission, bring me the elephant to be examined. Permission was accordingly asked, and granted: I had him measured exactly, and was rather surprized to find he did not exceed 10 feet in height. The Company's standard, for serviceable elephants, is 7 feet and upwards, measured at the shoulder, in the same manner as horses are. At the middle of the back, they are considerably higher; the curve or arch of which, particularly in young elephants, makes a difference of several inches. After an elephant has attained his full growth, it is a sure sign of old age when this curve becomes less; and still more so, when the back is flat, or a little depressed. A partial depression of the spine is however no uncommon occurrence, even in very young elephants; and I am convinced it happens from external injury. I have been surprized to see the violence used, in herds of wild elephants just taken, by the large elephants, both male and female, putting the projecting part of the upper jaw, from which the tusks grow out, on the spine of the young ones, and pressing them to the ground, while they roared from pain.

It has been stated, that the sagacity of the elephant is so great, and his memory so retentive, that when once he has received an injury, or been in bondage, and afterwards escapes, it is not possible, by any art, again to entrap him. Great as my partiality is for this noble animal, whose modes of life and general sagacity I have had so many opportunities of observing, yet a regard to truth compels me to mention some facts, which contradict that opinion. The following history of an elephant taken by Mr. Leeke*, of Longford Hall, Shropshire, contains many interesting particulars on this subject. The elephant was a female, and was taken at first, with a herd of many others, in 1765, by Rajah Kishun Maunick†, who, about 6 months after, gave her to Abdoor Rezah, a man of some rank and consequence in the district. In 1767, the Rajah sent a force against this Abdoor Rezah, for some refractory conduct, who, in his retreat to the hills, turned her loose into the woods, after having used her above 2 years, as a riding elephant. In Jan. 1770, she was retaken by the Rajah; but, in April, 1771, she broke loose from her pickets, in a stormy night, and escaped to the hills. On Dec. 25, 1782, she was driven by Mr. Leeke's elephant hunters into a keddah‡; and the day following, when Mr. Leeke went to see the herd that had been secured, this elephant was pointed out to him by the hunters, and particularly by a driver who had had charge of her for some time, and well recollected her. They frequently called to her by name; to which she seemed to pay some attention, by immediately looking towards them, when her name, Jugget-Peāuree, was repeated; nor did she appear like the

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* He was then the Resident of Tiperah, and took some pains to ascertain the facts here mentioned.—† The Rajah is the principal Zemindar in the province of Tiperah, paying the usual revenue for his lands in the low country; but in the hills he is an independent sovereign, has the power of life and death over his subjects, a mint, and other insignia of sovereignty.—— ‡ The inclosure in which elephants are secured. Vide Asiatic Researches, vol. 3, art. "Method of catching Elephants."—Orig.

wild elephants, which were constantly running about the keddah in a rage, but seemed perfectly reconciled to her situation.

From Dec. 25 to Jan 13, a space of 18 days, she never went near enough the outlet, or roomee, to be secured; from a recollection perhaps of what she had twice before suffered*. Orders however had been given, not to permit her to enter the outlet, had she been so inclined, as Mr. Leeke wished to be present when she was taken out of the keddah. On Jan. 13, 1783, Mr. Leeke went out, when there were only herself, another female, and 8 young ones, remaining in the inclosure. After the other female had been secured, by means of the koomkees† sent in for that purpose, the hunters were ordered to call Juggut-Peāuree. She immediately came to the side of the ditch, within the inclosure; on which, some of the drivers were desired to carry in a plaintain tree, the leaves of which she not only took from their hands, with her trunk, but opened her mouth, for them to put a leaf into it, which they did, stroking and caressing her, and calling to her by name. Mr. Leeke, seeing the animal so tame, would not permit the hunters to attempt tying her; but ordered one of the trained elephants to be brought to her, and the driver to take her by the ear, and order her to lie down. At first she did not like the koomkee to go near her, and retired to a distance, seemingly angry; but when the drivers, who were on foot, called to her, she came immediately, and allowed them to stroke and caress her, as before; and in a few minutes after permitted the trained females to be familiar. A driver, from one of these, then fastened a rope round her body, and instantly jumped on her back; which at the moment she did not like, but was soon reconciled to it. A small cord was next fastened round her neck, for the driver to put his feet in, who, seating himself on the neck, in the usual manner, drove her about the keddah, the same as any of the tame elephants. After this, he ordered her to lie down, which she instantly did; nor did she rise till she was desired. He fed her from his seat, gave her his stick to hold, which she took with her trunk, and put into her mouth, kept, and then returned it, as she was directed, and as she formerly had been accustomed to do. In short, she was so obedient, that had there been more wild elephants in the keddah, to tie, she would have been useful in securing them. Mr. Leeke himself then went up, took her by the ear, and bade her lie down; a command she instantly obeyed. I have known several other instances of elephants being taken a 2d time; and was myself a witness both of the escape and retaking of one, as related in the following account.

In June 1787, Jâttra-Mungul, a male elephant, taken the year before, was travelling, in company with some other elephants, towards Chittigong, laden with a tent and some baggage, for our accommodation on the journey. Having come

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* When elephants were secured in the outlet from the keddah, they bruised themselves terribly. Vide Asiatic Researches, vol. 3.——† Koomkees are female elephants, trained for the purpose of securing wild ones, and more particularly those large males which stray from the woods, named goondahs. Vide Asiatic Researches, vol. 3.—Orig.

upon a tiger's track, which elephants discover readily by the smell, he took fright, and ran off to the woods, in spite of the efforts of his driver. On entering the wood, the driver saved himself, by springing from the elephant, and clinging to the branch of a tree under which he was passing; when the elephant had got rid of his driver, he soon contrived to shake off his load. As soon as he ran away, a trained female was dispatched after him, but could not get up in time to prevent his escape; she however brought back his driver, and the load he had thrown off, and we proceeded, without any hope of ever seeing him again. Eighteen months after this, when a herd of elephants had been taken, and had remained several days in the inclosure, till they were enticed into the outlet, there tied, and led out in the usual manner, one of the drivers, viewing a male elephant very attentively, declared he resembled the one which had run away. This excited the curiosity of every one, to go and look at him; but when any person came near, the animal struck at him with his trunk, and in every respect appeared as wild and outrageous as any of the other elephants. At length, an old hunter coming up and examining him narrowly, declared he was the very elephant that had made his escape about 18 months before. Confident of this, he boldly rode up to him, on a tame elephant, and ordered him to lie down, pulling him by the ear at the same time. The animal seemed quite taken by surprize, and instantly obeyed the word of command, with as much quickness as the ropes, with which he was tied, permitted; uttering, at the same time, a peculiar shrill squeak through his trunk, as he had formerly been known to do; by which he was immediately recognized, by every person who had ever been acquainted with this peculiarity.

Thus we see that this elephant, for the space of 8 or 10 days, during which he was in the keddah, and even while he was tying in the outlet, appeared equally wild and fierce as the boldest elephant then taken; so that he was not even suspected of having been formerly taken, till he was conducted from the outlet. The moment however he was addressed in a commanding tone, the recollection of his former obedience seemed to rush upon him at once; and without any difficulty he permitted a driver to be seated on his neck, who, in a few days, made him as tractable as ever. These, and several other instances which have occurred, clearly evince, that elephants have not the sagacity to avoid a snare into which they have, even more than once, fallen.

The general idea, that tame elephants would not breed, has doubtless prevented trials being made, to ascertain whether, under particular circumstances, this supposed reluctance could be overcome. I was however convinced, from observation, as well as from some particular facts, that elephants had their seasons in which they were in heat; I shall therefore first mention the circumstances which induced me to attempt breeding from tame elephants, and then relate the success of the experiments instituted for this purpose.

The circumstances to which I allude, happened in Jan. 1790, at a keddah near Comillah the capital of Tiperah. Messrs. Henry Buller and Geo. Dowdeswell, of

Chittigong, being then on a visit at Comillah, accompanied me and several others, to see a herd of elephants which had been lately taken. Our visitors then proposed a trial being made, of tying the wild elephants immediately, in the keddah, in the manner practised at Chittigong, instead of waiting till they were enticed, one after another, into the narrow outlet, there to be secured, and led out in the usual manner*. This mode they recommended so earnestly, from a conviction of its superior utility†, that Mr. John Buller, to whom the keddah belonged, assented to the trial being made, and gave orders for the trained females, and proper assistants, to go directly within the inclosure. Having but few trained females present, it was judged advisable to send in a fine male elephant, taken many years before, and thoroughly broke in, to assist them, as well as to keep the herd in awe. He had no sooner entered the inclosure, and been brought near the herd, than, discovering one of the females to be in heat, impelled by desire, and eager to cover her, he dashed through the herd, regardless of the orders and severe discipline of the driver, and had nearly accomplished his purpose. The driver, being alarmed for his own safety, exerted in vain all his strength, to turn him, and bring him from among the wild elephants; but the drivers of the trained females, coming speedily to his assistance, soon surrounded this furious animal, and separated him from the herd. In resentment however of his disappointment, he attacked a small koomkee, with such violence as completely overturned her and her rider; and had he not been of a particular species, called mucknah, which have only small tusks, he most probably would have transfixed, and killed her on the spot: fortunately, neither she nor her driver received any considerable hurt. This accident prevented the trial being then made, to tie the wild elephants in the manner proposed.

Reflecting on the disobedience shown by an elephant remarkably docile, and which had been domesticated for many years, when his passions were excited, and recollecting also, that a wild elephant had covered a female, in Feb. 1778, before many spectators, just after the herd had been secured in the inclosure, I was assured in my own mind, that it was not from any sense of modesty, either wild or tame elephants did not gratify their passions in public; but no opportunity offered of prosecuting this inquiry, till 1792. Having then taken on myself the management of the elephant hunters, a very fine male was caught in Nov.: he was both

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* Vide Asiatic Researches, vol. 3, article, "Method of catching wild Elephants;" where this process is particularly described.——† Though fully convinced of this, I could not bring the hunters to adopt the Chittigong method, till the year 1794. After this, during the last 3 years I remained at Tiperah, I did not lose 1 elephant in 20; whereas, by the former method, of tying them in the roomee, near one-third of those taken died in less than a year, in consequence of the hurts they received from their violent efforts to get free, before they could be properly secured. The natives of Tiperah, and indeed of most parts of India, are extremely attached to old customs; and it was with the utmost difficulty I prevailed on the hunters to deviate from the practice of their ancestors, though the method recommended was followed at Silhet, as well as at Chittigong. The method was, simply to surround a herd, in the first convenient place, with a ditch and palisade; and when this was finished, to send in the koomkees, and proper persons to tie the wild elephants on the spot, and then conduct them, one by one, through an opening in the palisade, from the keddah, as soon as they were tied.—Orig.

young and handsome, and also of a most docile disposition; I therefore promised his driver a considerable gratuity, if he would get him into high order, so that I might have an opportunity of bringing his procreative powers to trial, with a tame female. In March 1793, the driver of a favourite female elephant informed me, that she had then signs of being in heat; and that, if the male and she were kept together, and highly fed, an intimacy would probably soon take place. They were therefore shortly after this brought near to Comillah, where a spacious shed was erected for their accommodation.

In the day, they went out together, to feed; they also brought home a load of such succulent food as their drivers and attendants could collect. After their return, they stood together, slept* near each other, and every opportunity was granted them to form a mutual attachment. In the evening, they had each from 10 to 12 lb. of rice soaked in water, to which a little salt was added; and, from the middle of May till the latter end of June, some warm stimulants, such as onions, garlic, turmeric, and ginger, were added to their usual allowance of rice. Long before this however a partiality had taken place, as was evident from their mutual endearments, and caressing each other with their trunks; and this without ceremony, before a number of other elephants, as well as their attendants. Near the end of June, I was satisfied the male would not, even to regain his freedom, quit the object of his regard; I therefore ordered the keepers to picket the female, by one of her fore-legs only, in the house where they stood, but to leave the male at full liberty. Fearful however of hurting their supposed delicacy, and thinking the nearness and sight of the attendants might possibly give umbrage to their modesty, I desired them to remain quiet in a little hut, erected on the outside of the building appropriated to the elephants, where they could see equally well as if nearer.

On the evening of June 28, 1793, the male was let loose from his pickets; and soon after he covered the female without any difficulty, though before this she never could have received the male, being taken when very young, about 5½ years prior to this period. The male was then led quietly to his stall; but early on the morning of the 29th he became so troublesome, that the drivers, in order, as they said, to quiet him, but partly I suspect to indulge their own curiosity, permitted him to cover her a 2d time; which he readily did, before the usual attendants, as well as a number of other spectators. After this, the driver brought me a particular account of the whole process. Though much pleased with the success of the experiment, yet I was rather chagrined he had not given me notice, that I might have been myself an eye-witness; and therefore told him, he should not receive the promised reward, till I had satisfied myself of the fact.

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* It is always a good sign, when an elephant lies down to sleep, within a few months after he is taken; as it shows him to be of a good temper, not suspicious, but reconciled to his fate. Elephants, particularly goondahs, have been known to stand 12 months at their pickets, without lying down to sleep; though they sometimes take a short nap standing.—Orig.

About 2 in the afternoon of the same day, I was desired to repair to the place where the elephants stood, as the male had been trying to get nearer the female. On this I proceeded to the spot, with my friend Capt. Robt. Burke Gregory: when we arrived, I ordered the male to be freed from his shackles; and, after some toying, and a few mutual caresses, we had the satisfaction to see him cover the female. When the male mounted, he placed 1 of his fore-legs on each side of her spine, with his feet turned to, and pressing against her shoulders, and his trunk over her forehead; supporting himself firmly in this situation, during coition, which he continued nearly the same time, and in the same manner, as a horse with a mare.

The female remained perfectly still, during the coitus. When the male had finished, he stood quietly by her side, while she caressed him with her trunk; and as they then appeared well pleased, and gentle as usual, I went up and patted them both, as I had formerly been accustomed to do, without the smallest apprehension. In the evening they were brought home to be fed; and though only a few hours had elapsed since his last embrace, the male seemed inclined to make another attempt; to which I would have consented, to gratify a crowd of people then present, had I not now learned, that he had covered the female in the open plain, about 10 in the morning, when going out for food, in spite of the exertions of the drivers and attendants; at least so they alleged, in excuse for having permitted it, contrary to my orders. As he had already covered 4 times in about 16 hours, I was afraid a further indulgence might be prejudicial, and therefore would not permit it; especially as Mr. Imhoff, to whom he then belonged, was absent. That gentleman however returned 2 days after; but when the 2 elephants were brought together, in order that Mr. Imhoff's curiosity might be indulged with so novel a sight, the female, being no longer in heat, was so uncivil as to give the male a kick in the face, when he was using what she then thought improper liberties; nor did she afterwards permit him to cover her, though, when standing together, they mutually indulged in a few caresses.

During the time they were kept together, the male never showed signs of his passions being excited, by any exudation from the ducts of the glands near his temples; which is generally considered as the sign of a male elephant being peculiarly ready for the female. This however I am inclined to believe is a vulgar error; as not one of the male elephants I have seen cover, in a domestic state, nor any of the males which were caught singly, or rather entrapped, by their desire to have connexion with the tame females, had at those times the smallest appearance of such an exudation. Had this happened in any 1 instance, during my residence in Tiperah, I think I must have known it; for when this exudation takes place, the elephant has a dull heavy look, and it is dangerous for strangers to go near him. I have seen elephants in this situation, after they had been many years caught; but though they were then said to have their passions excited, I have never known one to cover during the continuance of this exudation: nor have elephants, so far as I have been able to observe, any particular seasons of love, like horses and cattle. Of 5 instances of elephants covered at Tiperah, one received the male in Feb., another in April, a 3d in June, a 4th in Sept., and the 5th in Oct. Besides these, an attempt was made by a tame male, to cover, in the month of Jan. a wild female, then in heat*. When the female is in heat, the parts of generation show it, by an unusual fulness of the labia; and if she is placed near a male, she endeavours by caresses to excite his desires†.

After the female had been covered by the male, as has been just related, there being then no other female ready, he was placed with an elephant which had had a young one about 4 years before this, and some months ago was reported to have been put in heat. It was thought, after some trial, that she was likely to permit him to cover, as she caressed him occasionally, and roused his passions; but she would not allow him to gratify his desire. The drivers, tired of this coyness, and stimulated perhaps by the hopes of another gratuity, were so brutal as to tie her, and let the male make an attempt upon her, while tied. His attempt however was to no purpose; though he continued his efforts till he appeared to be quite exhausted. This being told me, I severely reprimanded the people; and ordered the female to be left at full liberty to reject or receive the male, as she might think proper. Here however was positive proof, that the male would have effected his purpose by force, when he found he could not obtain it in any other way. He remained at Comillah till Oct. 1793, without my being able to procure a female that was in heat; he was then sent to Calcutta.

I now became extremely solicitous about the health of the female which was covered in June; and gave particular directions not to overheat her, but merely to give her as much food and exercise as were likely to keep her in the best condition, as she was now known to be pregnant. In 3 months after she was covered, she became fuller, her flesh softer, and her breasts began to swell. These marks of her being with young were so evident to the driver, that he mentioned them of his own accord; which convinced me that an elephant, 3 months after conception, may be known by the keepers to be pregnant. She had always been a favourite, from having been the gift of my worthy and respected friend Mr. John Buller‡, as well as for her gentle and docile disposition; and now I had hopes of her going her full time. She was 7 feet 3 inches high, when covered; but after this increased so fast, not in bulk only, but also in height, as to exceed 7 feet 8 inches, before she brought forth. On the 16th of March, 1795, she produced a fine male; just

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* Many pregnant females are taken every year at Tiperah, and produce young ones in the different months: this clearly shows that there are no particular seasons during which the females are in heat.—† It may be proper to observe, that the penis of a full grown elephant is from 2 feet 4 to 2 feet 6 inches in length, and from 14 to 16 inches in circumference. I caused the penis of 2 males to be measured, after their passions were excited, in order to ascertain the real size. On some occasions, I have seen the penis absolutely touch the ground, when the elephant has been walking; but it must be recollected, that the hind legs of an elephant are very short, in proportion to his size.——‡ Now one of the Members of the Board of Revenue, at Calcutta.—Orig.

20 months and 18 days after she was first covered. The young one was $35\frac{1}{4}$ inches high; and had every appearance of having arrived at its full time, being the largest I had known produced in Tiperah.

We have many young produced every year, by the females which are taken while breeding, and these seldom exceed 34 inches; this however may be owing to the weak and reduced state the mothers are brought to, while breaking in. The young of the elephant, at least all those I have seen, begin to nibble and suck the breast soon after birth; pressing it with the trunk, which by natural instinct they know will make the milk flow more readily into the mouth, while sucking. Elephants never lie down to give their young ones suck; and it often happens, when the dam is tall, that she is obliged for some time to bend her body towards her young, to enable him to reach the nipple with his mouth; consequently, if ever the trunk was used to lay hold of the nipple, it would be at this period, when he is making laborious efforts to reach it with his mouth, but which he could always easily do with his trunk, if it answered the purpose. In sucking, the young elephant always grasps the nipple, which projects horizontally from the breast, with the side of his mouth. I have very often observed this; and so sensible are the attendants of it, that with them it is a common practice to raise a small mound of earth, about 6 or 8 inches high, for the young one to stand on, and thus save the mother the trouble of bending her body every time she gives suck, which she cannot readily do when tied to her picket.

Tame elephants are never suffered to remain loose; as instances occur of the mother leaving even her young, and escaping into the woods. Another circumstance deserves notice: if a wild elephant happens to be separated from her young, for only 2 days, though giving suck, she never afterwards recognizes or acknowledges it. This separation sometimes happened unavoidably, when they were enticed separately into the outlet of the keddah. I have been much mortified at such unnatural conduct in the mother; particularly when it was evident the young elephant knew its dam, and, by its plaintive cries and submissive approaches, solicited her assistance.

Here it may be observed, that a female was believed to have gone 21 months and 3 days; being supposed to have been covered on Jan. 13, 1788, some days before she was driven into the inclosure. When I made particular inquiry as to the real time she was taken, the daroga, or superintendant of the hunters, said it was in Jan.; but the dydars, or principal hunters, declared she was among the herd taken in Feb. following, and was probably the same elephant Mr. Buller, Captain Hawkins, and many others, saw covered on the 9th and 10th of that month. Perhaps some days prior to this she might have been covered in the woods, before she was brought into the inclosure; but as a herd was taken in each of those months, and not kept separate, and 2 years had nearly elapsed before I thought of making any inquiry, it was impossible for me to determine in which of those months she was really taken; and the only motive I then had for endeavouring to ascertain this point, was to form some probable conjecture as to the period of an elephant's gestation, which has now been ascertained, in the instance before related.

Early in Sept. 1795, the female that had been covered, and had bred under my own observation, was known to be in heat; this was less than 6 months after bringing forth. Learning, at the same time, that the Rajah of Cudwah, a principal Zemindar of the province, had a very large male that had been in the family near 20 years, from the time he was about 5 years old, I sent a messenger, requesting the elephant might be sent to Comillah, which request the Rajah immediately complied with. To prevent any interruption from the numbers of spectators, the elephants were put into a small inclosure, on the 17th of Sept.; the female was picketed by 1 leg, and the young one, to which she was giving suck, was tied to a tree at some distance, fearing, if permitted to run about, he might receive some injury. After a few caresses from the female, the male at length effected his purpose, and covered her twice the same evening. As the intention of the male elephant's visit was known in the district, and a few days had elapsed since the 2 elephants were brought together, in order to make them acquainted, the number of spectators was greater than on any other similar occasion. She was afterwards covered, several times, on the 20th of the same month; the male, in this case, being admitted after an interval of 3 days, though formerly, in June, 1793, she refused him when only 2 had elapsed. She again proved with young; and, in Nov. 1796, being myself in a bad state of health, and under the necessity of returning to Europe, I sent her to Lucknow, together with her young one, at the request of my friend captain David Lumsden: though she was then very big, she was still giving suck. About a month before that period, I got my friend, Mr. Stephen Harris, to permit a female of his to be covered; the same which had, in 1793, rejected the attempts of the male to cover her contrary to her inclination. Another messenger was dispatched to Cudwah, for the Rajah's elephant, which was again sent to Comillah. He covered her repeatedly, on the 14th, 15th, and 16th of Oct. 1796, before many Europeans, as well as natives; and, the last time he covered her, it was evidently contrary to her inclination; so that, in fact, he used force to effect his purpose, and held her so firmly, that the marks of the nails of his fore-feet were deeply imprinted on her shoulders.

Having mentioned a sufficient number of instances, to prove the ability, as well as the inclination of the elephant, to propagate his species in a domestic state, and that without any signs of modesty, and having ascertained the period of gestation to be 20 months and 18 days, it may be necessary to observe, that it is a difficult matter to bring a male, which has been taken about the prime of life, into good condition to act as a stallion; for, being naturally bolder, and of a more ungovernable disposition, than the female, he is not in general easily tamed, till reduced very low; and it requires considerable time, as well as much expence and attention, before he can be brought into such high order as is requisite. He must also be of a gentle temper, and disposed to put confidence in his keeper; for he will not readily have connexion with a female, while under the influence of fear or distrust. Of this I have seen many instances; nor do I recollect one male elephant in ten, which had been taken after having attained his full growth, much disposed to have connexion with a female. This is a most convincing proof, that those males which are taken early in life, and have been domesticated for many years, more readily procreate their species than elephants taken at a later period. In their wild state however they show no reluctance; for, besides all the males that are entrapped, from their desire to have connexion with the trained females which, though not in heat, are carried out to seduce them, several instances have occurred, of wild elephants covering, immediately after being taken, in the keddah.

On the 3d of April, 1795, a very fine male elephant covered a female twice, in the midst of the herd, and before all the hunters. On the 4th, I saw him attempting to cover a third time, when he was suddenly disturbed, by the noise the hunters made to drive away some of the herd which had come too near the palisade. In consequence of this interruption, he threw down first 1 and then another small elephant, and gored them terribly with his tusks, though they came between him and the female only for their protection: he had, before this, killed 4, and wounded many others. When the poor animals were thrown down, conscious of their impending fate, they roared most piteously; but notwithstanding their prostrate situation, and submissive cries, he unfeelingly and deliberately drove his tusks through, and transfixed them to the ground; yet none of the large elephants, not even the dams of the sufferers, came near to relieve them, or seemed to be sensibly affected. This savage animal had been then confined 4 days in the inclosure, along with the herd, on a very scanty allowance of food, and could have but very little hope of escaping; yet here his passions were stronger than his fears. It was on account of this savage disposition that the hunters had asked permission to shoot him, before I had either seen him or the herd, and thence judged he was a goondah*, that had lately joined. Having never before known any elephant killed willingly, in the keddah, by the larger males, and having no idea that he would commit such terrible havock, I unluckily refused to grant their request, being desirous to save so stately an elephant. When the palisade was finished, I got him tied, and led out;

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* From this instance, as well as many concurring circumstances, I am convinced that these goondahs generally leave the herd of their own accord, and join it when they think proper, or are induced to it from a female being in heat; yet it has been supposed, that they are driven from the herd, at an early period of life, by their seniors This appears improbable, as it is not often that very large males are taken with a herd of elephants; for, depending on their own strength, they stray singly, or in small parties, from the woods into the plains, and even to the villages; and it is in these excursions they are taken, by means of the trained females. As these goondahs are much larger, and stronger, than the males generally taken with the herd, it is not probable they would submit to be driven from it, unless at an early period. I have seldom seen, in a herd of elephants, a male so large as may be commonly met with among two or thee goondahs; but if these last were driven from the herd when young, the very reverse would be observed.—Orig.

but, not brooking restraint, he languished about 40 days, after he was secured, and then died.

In the course of this narrative, I have, in general, related only such particulars concerning the elephant as came within my own knowledge, and which were either not known, or not published. To enter into a particular history of the elephant was not my intention; and though the procreation of tame elephants has been proved, yet the expence incurred by breeding them, may deter others from making attempts of this kind. But it opens a field of curious inquiry to the naturalist; and, now that the facility with which it may be done is ascertained, it suggests itself as a mode by which the breed of elephants may be improved, in size, strength, and activity. In this way, any expence which might be incurred, would more than repay itself, in the future benefits to be derived from a superior breed of elephants.

VI. On the Decomposition of the Acid of Borax or Sedative Salt. By Lawrence de Crell, M. D., F. R. S., &c. From the German. p. 56.

The salt called borax, so useful in various manufactures and arts, and hitherto imported only from Thibet and Persia, or in small quantities from Tranquebar, has ever excited the attention of natural philosophers. This attention was principally directed to the acid contained in it, called sedative salt; its other component part, the alkaline salt, (soda or natron), being better known, and found in many other natural productions, either alone, or in conjunction with other acids. The acid above-mentioned has hitherto been discovered only by Hofer, in the lagone of Castelnuovo; by Martinovich, in the petroleum of Gallicia, mixed with alkaline earth; and by Mr. Westrumb, near Luneburg. The scarcity of this acid, and its being found only in the substances and situations above-mentioned, occasioned a supposition, in the minds of those who minutely observe and examine the course of nature, that it is not a simple substance, but is formed afresh from a variety of other substances, previously decomposed, by a singular coincidence of operative causes; and consequently that it belongs to compounds.

Numerous have been the experiments made by chemists, who supposed they had formed this salt by composition: some described experiments, which they declared to have succeeded with them, though they always failed, when attempted by others; from which Leonhardi concludes, that nothing more can be expected from any similar attempts to produce sedative salt. I was surprized that these chemists had never, so far as I knew, examined the subject by the way of analysis, and endeavoured to decompose the sedative salt already formed by nature. Indeed no great hopes of success could be entertained, as daily experience shows, that though this salt be kept fluid, in the hottest fire, for many hours together, till it becomes a vitrified substance, yet when it is afterwards dissolved in distilled water, the solution is complete, without any residuum, and it then shoots into crystals of precisely the same salt as before. Notwithstanding all this, when I reflected, that borax is generated only in certain climates of the east, and that its acid is found only in particular substances and situations, as has been already mentioned, I could not but suppose the latter to be the produce of a new formation. This being premised, I considered maturely in what manner the decomposition of this new and extraordinary compound might be attempted. Admitting the composition to be formed by the coalition of a number of different substances, it seemed not improbable that an acid, penetrating into and dissolving the whole mass, would rather associate with some than with others of its various component parts, and thus produce a separation or change of the latter. Besides, as the sedative salt, strong as its operation is, in a high degree of heat, on almost all neutral salts, has but a faint taste of acid, it might be supposed, that its acid is contained within some unknown species of earth, intimately combined; or within some sort of inflammable matter; or, according to a phrase used in the new system, there might be a deficiency of acid matter; that therefore some more powerful acid would probably separate and dissolve the earthy particles, destroy or change the inflammable matter, or impart the acid it might be supposed to want.

My choice, among the different acids, was fixed on that particular one, which, though not always quick in its operation, never fails to penetrate deep into all soluble substances, is nearly related to all inflammable bodies, and possesses an abundance of acid matter: I mean the oxygenated muriatic acid, prepared with manganese. In the application of this menstruum, I resolved to follow the practice established by the constant experience of both ancient and modern chemists; which has taught us, that difficult decompositions of parts closely united, are more easily effected by a gentle, long continued, digestive heat, and repeated distillation of the same menstruum, than by a heat which is more violent, and operates more quickly. I first made some preliminary experiments, in order to judge what probability there might be of success.

Exper. 1. I poured 1¼ oz. of the above-mentioned acid on 2 dr. of sedative salt, in a retort, to which I adapted a proper receiver, and then placed the mixture in a gentle digestive heat, of from 140° to 200° of Fahrenheit. The fluid was distilled over very slowly, and the salt was dry on the 3d day. The salt in the retort seemed unchanged; nor had the marine acid lost any thing of its usual smell.—*Exper.* 2. I poured the distilled fluid out of the receiver on the same salt, and exposed them to the same degree of heat as before. The salt again became dry on the 3d day, but there was yet no appearance of any change.—*Exper.* 3. I repeated the same process a 3d time. I now perceived during the distillatory digestion, several bright yellow spots on the salt, as it ascended the sides of the retort, resembling well-formed ammoniacal flowers of iron; more of which I discovered after the entire exhalation of the fluid.—*Exper.* 4. The above change induced me to repeat the distillation; and I then perceived, not only as many, but a much greater number of bright yellow spots, some of which were even much darker in colour, and approaching to brown. A change had now evidently taken place, which change increased on every repetition of the process; I therefore judged I might follow this direction with confidence. But, with a view to use the greatest accuracy and precaution in my proceedings and observations, I resolved to begin my work over again. First, I procured some ounces of sedative salt, which had been obtained from borax by means of vitriolic acid; and then prepared 2 quarts of the above-mentioned oxygenated muriatic acid, by distilling 3 parts of muriatic acid with 1 part of the purest manganese, in the usual manner; this I preserved in a cool dark place. Thus, the substances used in the following experiments, were always of the same nature.

Exper. 5. I poured 3 oz. of the oxygenated muriatic acid on $\frac{1}{2}$ oz. of the sedative salt, in a white glass tubulated retort. I used such a retort, that, in frequently pouring back the distilled fluid, I might not have to lute afresh the several vessels, after every distillatory digestion. For the same reason also, I chose a tubulated receiver, the tube of which gradually terminated in a point, in shape of a funnel. This tube passed into a phial, placed in such a manner that all the fluid passing into the receiver dropped immediately into the phial, the joinings of which were closed with bladder. To close the tube of the retort, I did not think it right to use a waxed cork, though it closes very tight, because it might be corroded; and also because the vapours, dropping from the cork, might carry some fat and oily matter back into the retort. For the same reason, I would not use any greasy lute; but closed the joints of the glass stopper, which fitted remarkably close, with a ring of fine sealing-wax, closely pressed on it, but which could easily be disengaged, after my work was done, while the retort was still warm: and as I was even afraid of an oily lute about the joints of the receiver, I closed them up with a ring of very fine white clay, which I fitted to them as exactly as possible, by pressure; letting it stand several days to dry, and then carefully filling up all the cracks. Having made this previous arrangement, and put the above-mentioned ingredients together, I suffered them to remain cold for 24 hours; at the end of which, the salt was not entirely dissolved, but, on the application of heat, the whole became a clear fluid.* The degree of heat in the sand was from 180° to 240°, by which the fluid evaporated very slowly. During this operation there ascended, or rather crept up the sides of the retort, a considerable quantity of salt, in very loose flowers, rising pretty high above the fluid, increasing by degrees, and chiefly occupying that half of the retort which received a greater degree

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* This appeared to me so striking, that I endeavoured to obtain a confirmation of it. I made a similar mixture, in the same proportions, which was not dissolved so long as it remained cold; but was dissolved by heat. When the solution cooled, a small part of the salt, and a larger as the cold increased, precipitated, which was dissolved again by a fresh application of heat. But with the degree of heat I employed, no more than 1 part of salt would dissolve in 6 parts of the acid.—Orig.

of heat than the other; but never the opposite or colder half. In 4 days, the fire being extinguished towards the evening of the last, the fluid had evaporated, so as to leave the salt apparently dry. After cooling for some time, the bladder on the phial was moistened by water, and the vessels were separated; the sealing-wax also having been removed, and the stopper taken out, the distilled fluid was poured back, through a glass funnel, on the salt, without disturbing the lute.

Exper. 6. As soon as the fluid was added, the salt at the bottom began by degrees to dissolve: that on the sides of the retort did the same, after it was heated, but soon began to form again: the solution appeared of a yellowish hue. In general however, the whole experiment took the same course as in exper. 5, and the smell, both of the salt and the fluid, seemed to be unchanged. The only difference was, that the former did not appear like salt, the crystallization on the sides excepted, and in single detached crystals, but something like a white, uniform, spongy, and as it were earthy mass. The fluid was now again taken from the phial, as in exper. 5, and poured back on the salt.

Exper. 7, 8, and 9. During the 3d distillation, bright yellow spots began to appear on the white flowers; and after the salt at the bottom had become dry, similar spots appeared on it, particularly on the lower surface. The fluid was again, for the 4th time, poured on the salt, and distilled; when the yellow spots and flowers increased in number. This was also the case in the 5th distillation.

Exper. 10. The fluid obtained by the last experiment, which had changed a little in smell, and had acquired a particular scent, almost as if some sebacic acid had combined with the muriatic, was poured on the salt as before. The number of yellow spots, which had also become of a darker hue, was considerably increased. The salt had now been exposed, ever since the 5th exper. for 32 days, to the digestive distillation; and the intermediate time between each distillation had been longer or shorter, in proportion to the degree of heat, and to the time of kindling and extinguishing the fire. As I now found that business of importance would prevent me from continuing my labours for some months, I poured 2 other ounces of the muriatic acid on the salt, besides the fluid so often drawn off by distillation, and left the mixture at rest.

Exper. 11, 12, 13, 14. When my business was finished, I again undertook the distilling of the mixture, which had been so long digesting in the cold, for the 7th time, and obtained the same results as in exper. 10. Nor was there much difference observed in the 12th, 13th, and 14th experiments.

Exper. 15. I now poured the fluid obtained by the 14th exper. on the salt, which had acquired more and more yellow spots, brighter in hue, and then proceeded as before, till the salt became dry; on which, when the retort was cool, I poured 1 oz. 3 dr. of the muriatic acid, in addition, and allowed the mixture to digest gently for some days. *Exper.* 16. In this 12th distillation, there appeared a large quantity of flocculent sublimate, looking almost like branches, hanging down, and in many places of a yellow colour; it extended even into the neck of the retort, and almost covered the interior aperture of the tube.—*Exper.* 17. The 13th distillation produced the same phenomena. On the lowermost surface of the mass of salt, many light-brown spots appeared, as soon as the fluid was so much evaporated that no more of it could be seen on the salt.—From all these circumstances, I now believed the mass of salt, by a digestion of 22 days, and 7 distillations, from exper. 11 to 17, that is, by a digestion of 54 days, and 13 distillations, in the whole, to be so far decomposed, as to admit of a separation of some of its constituent parts. I therefore supposed I might leave off applying only digestive warmth, and proceed to a greater degree of heat.

Exper. 18. Having poured out the fluid obtained by exper. 17, and replaced the phial, I increased the degree of heat. By this the retort became quite obscured, first by fumes, and afterwards by a quantity of white sublimate, attaching itself to all its sides, which however had not the appearance of common sedative salt. As I increased the heat, the sublimate became dark in colour; afterwards became black and frothy: and at length ran down the sides of the retort, in different places, like thick oil of hartshorn, the retort being almost wholly blackened by it.—*Exper.* 19. While the retort was still warm, I poured into it the fluid obtained by exper. 17, having first warmed it a little; when, almost in the same instant, a very agreeable phenomenon took place. Crystals, perfectly white, shot forth suddenly, and all at once, from every part of the black mass, covering the sides of the retort. The distillation being continued, these crystals were at length dissolved, and entirely removed. The supernatant fluid was, as usual, almost colourless. When the mass of salt appeared dry, the fire was increased, as in exper. 18, and the same appearances as above related took place: first, the sublimate appeared white, then black, frothy, and flowing down the sides.—*Exper.* 20. I proceeded, as in exper. 19, to pour back the distilled fluid. Instantly a number of the whitest crystals shot forth from the black ground, forming small groups; but the retort was cracked.—*Exper.* 21. I therefore took all the vessels asunder, and shook the retort well, till whatever hung on its sides was dissolved; then distilled the fluid in another retort, till the mass of salt appeared quite dry. I now put the retort into a crucible, surrounded it with sand, fitted another receiver to it, and placed the crucible in an open fire. First, some sublimate was produced, towards the neck of the retort, but which vanished as the heat increased, and then a small portion of fluid, hardly more than a dram, or a dram and a half, which appeared to smell a little of the sebacic acid. At the bottom of the retort was a blackish mass, a, and likewise some sublimate, b, which, by its varied appearance, seemed to be of a two-fold nature.

Exper. 22. The residuum taken out of the broken retort had a spongy appearance, and swam on water; it had a blackish colour, and weighed 3 dr. 10 gr. Being exposed to the air, the blackish colour became lighter, and inclining to grey. When digested in 16 parts of distilled water, in the usual temperature, for 2½ days, it did not all sink to the bottom; and after being digested with heat for 20 hours, it was not entirely dissolved: that part which sank, was of a blackish brown. More water was then added, and it was made to boil for 2 hours; it was afterwards placed on a paper filter, the weight of which was previously ascertained, and edulcorated with boiling distilled water, till at last a proportion of 26 parts of water to the substance had been used. After all the fluid, α, had passed through, and the filter, with the residuum, had been dried in a heat of 212°, for an hour and a half, the residuum, β, weighed, exclusive of the filter, 19 gr.—*Exper.* 23. The fluid, α, obtained by exper. 21, was suffered to evaporate gradually, and yielded 3 dr. 10 gr. of a white transparent salt.—*Exper.* 24. This salt, obtained by exper. 23, was put into a small retort, and exposed, in a crucible filled with sand, to an open fire. It became of a blackish-brown colour, yielded some sublimate, a, about 5 gr., a small portion of fluid, b, and a blackish-brown residuum, c, which became lighter in colour, on being exposed to the air.—*Exper.* 25. The fluid, b, of exper. 24, smelled like marine acid, and precipitated nitrate of lead.—*Exper.* 26. The residuum, c, of exper. 24, by the addition of some water, became whiter, and was dissolved; more water having been added, it was digested with heat, by which the matter was dissolved. The solution being afterwards filtered, I obtained 2 dr. 4 gr. of white salt: the residuum on the filter weighed 4 gr.—*Exper.* 27. This salt, exper. 26, was again exposed to the fire; when it yielded from 20 to 30 drops of acid liquor, 4 gr. of sublimate, and a residuum which, being dissolved, yielded 1 dr. 33 gr. of salt, and left 2½ gr., c, on the filter.

The same salt, obtained by *Exper.* 26, being distilled, became of a brownish grey colour; and, besides a few drops of fluid, yielded not quite 2 gr. of sublimate. On treating the residuum with water, it yielded 68 gr. of salt, and there were not quite 2 gr. left on the filter.

Exper. 28. On treating these 68 grs. of salt in the same manner, they yielded a few drops of fluid, and 2 gr. of sublimate: after filtration, there remained 48 gr. of salt, and a residuum of hardly 1½ gr.—*Exper.* 29. The same salt, treated in the same manner, yielded a few drops, and a little sublimate; and after filtration, 35 gr. of salt, and a residuum of hardly 1 gr.—*Exper.* 30. On treating these 35 grs. of salt in the same way, they yielded, besides a very small quantity of fluid, and of sublimate, 24 gr. of salt, and about ¾ gr. of residuum.—As I now discovered that the quantity of salt was continually decreasing, and some coal separating from it, I thought it superfluous to endeavour to decompose the above 24 gr. any further.

Exper. 31. The residuum, β, of *Exper.* 22, was light, blackish, and like coal. I now poured common concentrated muriatic acid on 3 grs. of it, and digested the mixture for 42 hours, in a considerable degree of heat, but no dissolution was apparent. I then added smoking nitrous acid, and digested it for 24 hours, till it boiled, without any apparent dissolution. I added about 2 gr. of sugar, but without effect, except that its colour became yellowish. I now boiled the fluid, till it all evaporated in reddish-yellow vapours: there remained a very black, thick, glutinous mass, smelling like burnt sugar. Having added 3 oz. of water, the greatest part of the blackish matter rose to the surface, and the water appeared only a little tinged. The fluid part indeed became brown by boiling; but after rest and subsidence, it again got clear. I filtered it, a; then poured 2 oz. more distilled water on the residuum, and, after digesting, boiling, and filtering, added the filtered fluid, b, to the former, a. After this treatment, there remained 2 gr. of residuum, c.

Exper. 32. Having caused the fluid a, b, of exper. 31, to evaporate, it yielded a salt greyish-yellow mass, which very quickly attracted the moisture of the air. Being again dissolved in water, and saturated with pot-ash, a considerable quantity of whitish earth was precipitated, very much resembling talc.

Exper. 33. The residuum, c, of exper. 31, which, besides its insolubility and lightness, had much of the external appearance of coal, was now thrown on melted nitre, and it deflagrated. I placed a 2d crucible with melted nitre close to it, and having, at the same moment, thrown into one the above-mentioned residuum, and into the other a quantity of common charcoal pulverized, I could not observe the smallest difference in effect. Very little difference was also apparent, as to the residuum, β, of exper. 22, c, of exper. 24, and that of the following experiments.

Exper. 34. To obviate the objection, that sedative salt alone would perhaps deflagrate with melted nitre, I made that experiment also, but in vain. Not the smallest deflagration took place, even when both were melted together for many hours.—*Exper.* 35. Another objection may be made, namely, that in distilling the muriatic acid from manganese, part of the latter had passed over with the acid; and, in the frequent distillations of the sedative salt, had been deposited on it, and thus deflagrated. But, on throwing fresh pulverized or solid manganese, either such as is usually sold, or quite pure, heated to redness, into melted nitre, not the smallest deflagration took place.

Exper. 36 to 50. Instead of the interrupted heat used in the foregoing experiments, I now exposed $\frac{1}{2}$ oz. of the salt, with 3 oz. of the oxygenated muriatic acid, to a continued heat of between 200° and 300° of Fahrenheit. The fluid had nearly evaporated in 24 hours. I changed the phial, towards the close of the operation, for another, that the former might be gently heated, and the fluid by that means be poured back, with the greater safety, on the warm salt, through the tube of the retort. In this manner, during an uninterrupted fire of 14 days, the acid was 14 times distilled, and returned on the salt. On the 3d day, yellow spots appeared. On the 4th, some particles of oil or fat were discovered, swimming on the surface of the fluid in the phial; which particles, after cooling and emptying the phial, adhered to its sides, so as to obscure its transparency.

More or less of these oily particles were discovered in every successive operation; and the oily matter, adhering to the inside of the glass, increased considerably.

Exper. 51. When the fluid was distilled, the receiver was changed, and the fire increased. A considerable quantity of sublimate was obtained, pretty white in colour, as was also the surface of the mass of salt at the bottom of the retort; but lower down it was almost of a light ash-grey. After the sublimate ceased to arise, I diminished the fire.

Exper. 52. On the mass of the former experiment, I poured the fluid, obtained by exper. 49, and continued a gentle digestion. I very soon perceived something rising towards the surface, and swimming on it: after some hours, it appeared to be a thick wrinkled skin, like fat, or a skin of mould, increasing in size, till it covered the whole surface. White spots of sublimate appeared on it, but it did not sink. It assumed gradually a fine lemon colour, and some yellow matter, though not in large quantity, ascended the sides of the retort. The fluid having been gently distilled, and the receiver changed, I placed the retort in an open fire; on which, more sublimate soon appeared; but, not long after, it all vanished, and the retort lost its transparency. The mass contained in it began to rise, first gently, and then violently, especially in the centre, in large frothy bubbles. The distillation was finished, after obtaining 1 dr. of fluid, and when the frothy bubbling had ceased. The retort being broken, that part where the bubbling had been strongest, was found to be black; the upper surface being covered with a thin greyish matter, under which a solid, compact, and almost vitrified substance appeared. On this I poured water, and dissolved it in the usual manner; filtered it, let it evaporate, and treated it as described above, exper. 22—30.

Exper. 53. I obtained a white salt, a, and some coal, b, which deflagrated briskly with nitre, in nearly the same proportions as throughout the series of experiments described from exper. 20 to 33, which I will not repeat, on account of the little variety observed in them; one of them however deserves to be distinguished from the rest.—*Exper.* 54. I put 6 gr. of the coal, b, of exper. 53, in 3 dr. of common muriatic acid, and digested them for 2 days, till the acid had gradually evaporated. I then added 1 dr. of the same acid, with 1 scruple of nitric acid, and when they had evaporated, boiled the residuum full half an hour in distilled water. By this process, I obtained a red solution; and, having saturated it with mild alkali, a sort of skin rose to the surface, with some small pieces of a fat slippery substance, a. A considerable quantity of loose earth, b, was also precipitated, of a light brown colour.—*Exper.* 55. On throwing the floating pieces, a, of exper. 54, into a solntion of caustic alkali, they dissolved; the solution had a reddish-brown colour.—*Exper.* 56. With the same solution of caustic alkali, I covered the light brown earth of exper. 54. As the solution changed its colour to a reddish brown, the earth gradually became perfectly white.—*Exper.* 57. To observe the affinity of other acids to the sedative salt, I poured 6 dr. of nitrous acid on 2 dr. of the salt, with 10 dr. of the oxygenated fore-mentioned muriatic acid; digested the mixture, and distilled it, in 24 hours, with a gentle heat. On the fluid swam a white compact substance, and some small particles of the same kind lay at the bottom, which however rose, on the application of heat, and swam about with the rest.

Exper. 58 to 63. I poured the whole distillation back on the salt, and, by means of a digesting heat, again drew off a fluid, which appeared covered with a thin fat skin. I then poured the fluid back; distilled it again, and thus repeated the process 3 times more. No phenomenon particularly remarkable appeared, except that the thin fat skin became more inconsiderable, and at last seemed almost to vanish.

Exper. 64. The salt separated from the fluid, by the gentle distillation in exper. 63, emitted now, by the force of additional heat, dark red vapours, as is usual in strong nitrous acid. When the distillation was at an end, the retort was exposed to an open fire; but, during this operation, no black matter appeared; nor was any coal separated from the mass, on dissolving it in distilled water.*—*Exper.* 65. I now tried the effect of a mixture of 4 dr. of strong vitriolic acid, and 12 dr. of the muriatic acid, repeating the usual digestion and distillation 6 times. I shall pass over other circumstances, and only mention, that after the 6th distillation of the fluid, a stronger heat, and at length an open fire, was applied; but hardly any fluid was produced, though the fire was so violent, that the whole mass appeared to be melted down into one uniform compact substance.—*Exper.* 66. The vessels having cooled, the mass was of a light milky colour throughout, without the least mixture of brown or black, or any other indication of coal.† Being some time exposed to the air, it became moist, and for a long time attracted much water, which I caused to run off. At last it remained pretty dry; but the mass seemed to have diminished, by at least $\frac{1}{4}$ part.

Here I shall stop, for the present, in the description of my experiments; which sufficiently tend to prove, in a general way, the decomposition of sedative salt, and to show, that one of its component parts is inflammable matter, which may be converted into coal. I obtained of true coal, mixed with some earth, exper. 33 and 54, according to the above-described experiments, (exper. 22, 26—30), $30\frac{3}{4}$ gr. in the whole; and by other experiments, often repeated, in general, $1\frac{1}{2}$ gr. more or less. Every other substance liable to be changed into coal, as gum, tartar, sugar, &c. suffers this change by a gentle heat, and deflagrates with nitre, in the degree of heat necessary to melt the former. But sedative salt can bear a red heat for many hours, without showing any signs of becoming coal, of burning, or of deflagration. Astonishing phenomenon! What menstruum preserves it so securely against the assault of force, in a dissolved state, and yet suffers itself to be

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* Here the nitrous acid seemed to destroy, and carry off, the inflammable matter, sooner than it could become coal; as it had before occasioned the oily and fat substance to vanish, in the beginning of this experiment.—Orig.

† Perhaps here also the remark contained in the former note holds good: yet I am rather of opinion, that the vitriolic acid did not operate with sufficient strength to separate the component parts.—Orig.

separated from it by more gentle means? What power exists here, to protect the inflammable particles, which afterwards turn to coal, so effectually against a degree of heat which nothing else can resist? Of what nature is the salt obtained in conjunction with the coal? These are all questions which excite great interest, but which are not easily answered. How far I have been successful in resolving them, some subsequent Essays will show; which I shall have the honour to lay before the R. S., as soon as I shall have sufficiently repeated the experiments I have already made.

VII. A Method of Finding the Latitude of a place, by means of Two Altitudes of the Sun, and the Time Elapsed between the Observations. By the Rev. W. Lax, A. M., Lowndes's Prof. of Astronomy, Cambridge. p. 74.

I hope the following method of determining the latitude, by means of 2 altitudes of the sun and the time elapsed between the observations, will be found not less convenient for nautical purposes than the rules which are commonly employed. But I would rather recommend it in those cases where rigid accuracy is required, and the astronomer is provided with no better instrument for taking the sun's altitude, than a Hadley's sextant of the most improved construction. The process will be neither difficult nor tedious; and, if the observations are made with proper exactness, I conceive the latitude will generally be obtained within a few seconds of the truth.

In the spherical triangle, whose sides are the complements of the latitude, declination, and altitude, let z represent the angle at the pole, and t its tangent; Z the azimuth, and T its tangent; L the latitude, and λ its cosine, radius being unity; then, if the altitude and declination remain constant, we shall have $\dot{L} = \lambda T \dot{z}$, and consequently $\dot{L}$ will vary as $T\dot{z}$, when the increment of λ, compared with λ itself, is inconsiderable. Hence, if the abscisse of the curve ABCD, fig. 1, be always proportional to z, and its ordinate to T, the area GB, intercepted between any 2 of these ordinates may represent the increment of the latitude corresponding to the increment of the time EG. Let *abcd*, fig. 2, be another curve, whose abscisse *ae* is always equal to AE in the preceding figure, but whose ordinate *eb* is proportional to t, the tangent of the hour-angle; then will the area *gb* vary as GB, at small distances from the meridian, and of course may represent the increment of the latitude. Now, to prove this, we have only to show that T and t, when both are small, bear to each other a given ratio. Let S and Σ be the sine and cosine of the azimuth; s and σ the sine and cosine of the angle at the pole; then will $\frac{\dot{T}}{T} = \dot{z} \cdot \frac{1 + T^2}{T}$, and $\frac{\dot{t}}{t} = \dot{z} \cdot \frac{1 + t^2}{t}$; $\dot{Z} = \frac{\dot{S}}{\Sigma}$, and $\dot{z} = \frac{\dot{s}}{\sigma}$. But since the complements of the declination and altitude remain constant, while the latitude is made to vary $\dot{S}$ will be to $\dot{s}$ as S to s; and

Fig. 1.

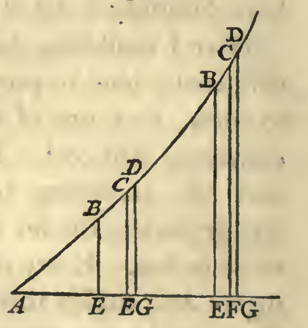
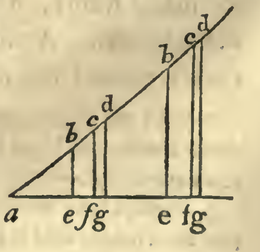


Fig. 2.



therefore $\frac{\dot{T}}{T} : \frac{\dot{t}}{t} :: \frac{S}{\Sigma} \times \frac{1 + T^2}{T} : \frac{s}{\sigma} \times \frac{1 + t^2}{t} :: 1 + T^2 : 1 + t^2$ :: the square of the secant of the azimuth : the square of the secant of the hour-angle, which may be considered as a ratio of equality, when the angles are very small. The fluxions therefore of the tangents are as the tangents themselves; and consequently they must always preserve the same ratio towards each other. Let us now suppose that an altitude of the sun is taken at the distance *ae* from the meridian, but that, in consequence of an error in the assumed latitude, the calculated time is *ag* ; and that, with a lat. differing from the former by 1 minute, we compute again, and the time is found equal to *af* ; then will the area *gc* be to *gb* as 1′ to the whole error in latitude. Let another altitude be taken at the distance *a*e from noon, and let the times computed with the two different latitudes that were employed before be *a*g and *a*f; then, in this case likewise, the area gc will be to the area gb, as 1′ to the error in latitude. Now the latter curve is the "figura tangentium," whose quadrature is given by Cotes, in his Harmonia Mensurarum, and the expression for which is extremely simple. For the fluxion of the area is $= \frac{t\dot{t}}{1 + t^2}$, and the area itself $= \log. (1 + t^2)^{\frac{1}{2}} =$ log. secant of the angle at the pole. The difference of the log. secants, or log. cosines, will of course be equal to the area intercepted between the tangents which correspond to them.

Hence a table might easily be constructed with a double argument,—the distance from noon, and the variation in time arising from the different suppositions of latitude,—which might immediately exhibit the logarithm of the area corresponding to any particular base *eg* supposed to be given. A 2d table might have for its argument the difference of the logarithms of the area *gb* and the area *gc*, which is also conceived to be known, and discover at once, in degrees, minutes, and seconds, the correction to be made in the assumed latitude. This correction, as it appears from a comparison of the signs of $\dot{L}$ and $\dot{z}$ in the equation $\dot{L} = \lambda T \dot{z}$, must be added or subtracted, according as the distance from noon obtained by computation is too great or too little, when the azimuth is less than 90 degrees; but the contrary, when the azimuth exceeds a right angle. Tables of the above description shall be constructed, if this method be received with approbation; and, in the mean time, it is proposed to subjoin a short specimen which is already completed.

I have presumed that we are able to determine *eg*, the error of time arising from an error in the assumed latitude, at either of the observations; and hence it becomes necessary, before we can avail ourselves of the principles which have been laid down, to point out the manner in which this may be accomplished. The clock gives us the whole interval between the observations, supposed to be made on different sides of the meridian, equal to *ae* + *a*e, and by computation we obtain *ag* + *a*g, and then we deduce *eg* + eg the whole error in time. Now the area *gb* is equal to gb; and therefore, if we make a rough division of the whole error, without any regard to accuracy, in the inverse ratio of the hour-angles at the two observations, and, entering the first table with these times, mark the area corresponding to each other at their respective distances from noon, and increase the one and diminish the other equally, till we get the areas of the same magnitude, this, we may conclude, is the proper value of each.

If the table were constructed to every second of time, we might ascertain these logarithmic areas merely from inspection; but, as it will be advisable to confine it within narrower limits, we shall sometimes find it necessary, as in other tables, to deduce their ultimate value by the rule-of-three. When we have increased one portion of time and diminished the other, till the difference of their corresponding areas becomes a minimum, we must divide this difference between them in the proportion of their respective increments in the next interval of time, and subtract or add the part assigned to each, according as it is greater or less than the other. The table however might easily be carried to such an extent, that exactness in this division could never be required; but, on the contrary, it would be quite sufficient, when the hour-angles were nearly equal, to add the arms together, and take half the sum for the value of each.

From these principles may be deduced the following practical rule for determining the latitude of a place. When the sun comes within 15 degrees of the meridian, in the morning, let his altitude be taken, and the time of the observation be accurately marked; and let another altitude be taken after he has passed the meridian, while his distance from it is less than 15°; and let the time of this observation likewise be noted. Then, with the supposed latitude of the place, compute the times corresponding to each of the altitudes in terms of the log. cosine of the hour-angle, and take the difference of the intervals, as shown by the clock, and determined by calculation, and divide it between the observations in the manner explained above. Compute the log. cosine of the hour-angle a 2d time, with the greatest altitude and the latitude increased or diminished by a minute, according as it appears, from a comparison of the intervals, to have been too little or too great; and take the difference between this log. cosine and that which resulted from the first operation, when the same altitude was employed. Having thus obtained the two areas *gb* and *gc*, we must subtract their logarithms from each other, and with their difference entering the 2d table we shall find the degrees, minutes, and seconds, by which the assumed latitude is to be increased or diminished.

It will be needless perhaps to suggest that, in the higher latitudes we may extend the limits above specified a few degrees farther from the meridian, without offering any material violence to the theory, as it has hitherto been explained; and that, on the other hand, when the declination and latitude are nearly equal, and of the same denomination, it will be expedient to confine our observations within a much shorter distance from noon. But it will afterwards be demonstrated that, whatever be the magnitude of the hour-angles, or however nearly the latitude and declination may approach towards each other, we can always secure, with very little additional trouble, an exact conclusion.

We may remark that the latitude, determined in this manner, will be nearly equivalent, in point of accuracy, to the mean result of 2 meridian altitudes. For we know that the increment of latitude : increment of altitude :: radius : cosine of azimuth; and since the cosine of a small angle differs so little from the radius, this may be considered, within the limits which have been prescribed, as a ratio of equality. If therefore one altitude of the sun were taken, and we could ascertain the error in time arising from an error in the assumed latitude, without the aid of a 2d observation, the latitude would be discovered with nearly the same precision as if it had been deduced from the meridian altitude. But, by means of a 2d observation made on a different side of noon, we obtain a 2d error in time of the same kind; and this being added to the former, and their sum divided in a just proportion between the 2 observations, the same effect will be produced, with respect to the accuracy of the result, as if 2 latitudes had been deduced from meridian altitudes, and a mean between them had been taken.

I might perhaps be allowed to say more; for I am satisfied, from experience, that I can take an altitude of the sun with greater exactness, when he is in any other situation, than when he is on the meridian. If we could ascertain, within a few seconds, or even within a minute, the time when he attains his greatest altitude, there would then be no reason why an observation should not be made with the same degree of certainty in this, as in other cases; but we are generally obliged to keep our eye stedfastly fixed, for several minutes, on the 2 images, and it is well known that, in such circumstances, the best eyes are apt to be deceived. Besides, it is impossible to preserve the contact of the limbs by perpetually moving the index, while the sun continues to ascend so very slowly. We are compelled to wait till they are evidently separated, and then, by one turn of the screw, to bring them into contact again, which must necessarily be a source of some inaccuracy. It is for the first of these reasons that, in taking an altitude of the sun, when he is near the meridian, I have found it advisable, not, in the usual manner, to bring the images almost to touch each other, and then to wait till they actually do so, but to bring them at once into contact, with such a degree of velocity as would make them sensibly overlap, or separate while the clock beats a second.

But I consider it as one of the principal advantages of this method, that we can avail ourselves of any number of altitudes, and of course approximate as near as we please to a true conclusion with so little additional labour. If there be an equal number of observations made on each side of the meridian, we must combine them together by pairs, according to the preceding instructions, and thus determine the different logarithmic values of *gb*. Having then added them all together, and taken a mean among them, we have only to compute a single incremental area *gc* with any of the altitudes and the lat. varied 1 minute, and subtracting its log. from the mean log. value of *gb*, we shall obtain a very accurate correction of the assumed latitude. But if there be more observations on one side of the meridian than on the other, when all the pairs have been united, and the areas resulting from them found, we may combine the supernumerary observations on either side with any of those which are made on the opposite side. The fittest however for this purpose, is the observation which is made at the least distance from the meridian. I should hope further, the practical astronomer will think it a circumstance of some moment, that the principal part of the work consists in finding the time, an operation which he is obliged so frequently to perform. Any of the 3 methods which are usually adopted on this occasion might easily be applied to the tables which have been described; but I shall venture to recommend a different rule, which I conceive to be better adapted to our purpose than any of the others, and to which the directions before given had a particular reference.

Let a be the sine of the altitude; γ the cosine of the hour-angle; d the sine, δ the cosine, and r the tangent of declination; l the sine, λ the cosine, and s the tangent of the latitude. Then $\gamma = \frac{a - dl}{\delta\lambda} = \frac{a}{\delta\lambda} - \frac{dl}{\delta\lambda} = \frac{ars}{\delta\lambda rs} - rs = (\frac{a}{dl} - 1) . rs$, when radius is unity, but $= (\frac{am^3}{dl} - m^2) . \frac{rs}{m^3}$, when radius is $m = \frac{rs}{m^3} \times$ into the square of the tangent of the arc whose secant is $\sqrt{\frac{am^3}{dl}}$. Hence we deduce the following rule for determining the log. cosine of the angle at the pole. From the log. sine of the altitude increased by 3 times the log. radius, subtract the sum of the log. sines of the latit. and declination; take half of the remainder, and, considering it as the log. secant of an arc, find the log. tangent corresponding; multiply this by 2, and add the log. tangents of the latit. and declin. and reject thrice the log. radius; the sum will be the log. cosine of the angle required. But, when the declin. and latit. are of different denominations, it is evident that our expression becomes $(m^2 + \frac{am^3}{dl}) . \frac{rs}{m^3}$, which is equal to $\frac{rs}{m^3} \times$ into the square of the secant of the arc whose tangent is $\sqrt{\frac{am^3}{dl}}$. In this case therefore, having found the log. value of $\frac{am^3}{dl}$, and divided it by 2, we must consider the quotient as the log. tangent of an arc, whose log. secant being taken, we are to proceed as in the former case.

The advantages of this rule are obvious. We obtain the angle in terms of the log. cosine; and consequently, when we have calculated the 2d time with the new latit. we have only to subtract one result from the other, and we immediately determine the area corresponding to the difference of the times. Besides, in the 2d computation, fewer of the elements are changed by this rule, than by any of those which are usually employed; and this is a consideration of much importance. But, if we are disposed to adopt the following method of ascertaining the incremental area *gc*, this advantage will be found still greater. Let us resume the expression $\gamma = \frac{a - dl}{\delta\lambda}$, and we shall have $\dot{\gamma} = \frac{1}{\delta} \times \frac{-d\lambda\dot{l} - a\dot{\lambda} + dl\dot{\lambda}}{\lambda\lambda^1}$ (λ^1 being the succeeding value of λ) $= \frac{1}{\delta} \times \dfrac{\frac{d\lambda^2\dot{\lambda}}{l} - a\dot{\lambda} + dl\dot{\lambda}}{\lambda\lambda^1} = \frac{d\lambda^2\dot{\lambda} - al\dot{\lambda} + dl^2\dot{\lambda}}{\delta l\lambda\lambda^1} = \frac{d\lambda\dot{\lambda}}{\delta l\lambda^1} - \frac{a - dl}{\delta\lambda} \times \frac{\dot{\lambda}}{\lambda^1} =$ $(\frac{r}{s} - \gamma) \cdot \frac{\dot{\lambda}}{\lambda^1} =$ (taking $\dot{\lambda}$ positive instead of negative, as it ought to be when $\dot{l}$ is positive), $(\gamma - \frac{r}{s}) . \frac{\dot{\lambda}}{\lambda^1}$; and consequently $\frac{\dot{\gamma}}{\gamma} = (1 - \frac{r}{s\gamma}) \cdot \frac{\dot{\lambda}}{\lambda^1}$, when radius is unity; but $= (m^2 - \frac{rm^3}{s\gamma}) . \frac{\dot{\lambda}}{\lambda^1 m^2}$, when radius is m. Now $\frac{\dot{\gamma}}{\gamma}$ may be considered as the increment of the hyperbolic log. of γ, and therefore, with its proper modulus, may represent the area which is the object of our investigation. We may suppose the other side of the equation to be the square of the cosine of the arc whose sine is $\sqrt{\frac{rm^3}{s\gamma}}$ × into the increment of the hyperbolic log. of λ^1, divided by the square of the radius; and if, instead of taking this log. with the hyperbolic, we take it with Briggs's modulus, we must then consider $\frac{\dot{\gamma}}{\gamma}$ as the increment of the log. of γ, according to the same system. But $\frac{\dot{\lambda}}{\lambda^1}$ being equal to $\frac{si}{m^2}$ (when i is only 1′), it will vary as s; and therefore, if its value be determined according to Briggs's system, when s is equal to radius, and be denominated v, its value in any other case will be expressed by $\frac{vs}{m}$.

Hence, to obtain the log. of the area *gc*, the quantity with which we are immediately concerned, we must find the log. value of $\frac{rm^3}{s\gamma}$, and divide it by 2; we must then take out the log. cosine of the arc whose log. sine is equal to the quotient; and, having multiplied it by 2, we must add the product to the constant log. of v (3.1015), and the log. tangent of the supposed latitude, rejecting thrice the log. radius. But if $\sqrt{\frac{rm^3}{s\gamma}}$ be greater than radius, which must necessarily be the case when the azimuth is greater than a right angle, we must then consider $\frac{rm^3}{s\gamma} - m^2$ as the square of the tangent of the arc whose secant is $\sqrt{\frac{rm^3}{s\gamma}}$, observing in other respects the directions before given. The quantities r and s are both employed in the first computation, from the result of which we also obtain γ; and consequently this operation will not be attended with much trouble.

The above instructions, it is manifest, are given on the supposition of r and s having the same sign; but if the declin. and latit. should not be of a similar denomination, then will our expression become $(m^2 + \frac{rm^3}{s\gamma}) \cdot \frac{vs}{m^3}$, and we must consider $m^2 + \frac{rm^3}{s\gamma}$ as the square of the secant whose corresponding tangent is $\sqrt{\frac{rm^3}{s\gamma}}$. With this exception, the process will be the same as when the tangents r and s are both affirmative.

The preceding formula naturally suggests to us another method of finding the log. area *gc*; and as some perhaps may think this more eligible than either of the former, I shall take the liberty of explaining it. When the latit. is given, the area GC, it is obvious, must invariably preserve the same magnitude at all distances from the meridian; and consequently the area *gc*, which is proportional to it, must likewise remain constant. If therefore we can ascertain this area when the hour-angle is supposed to vanish, we may employ it when the sun is at any distance from noon. Let us now conceive the declin. to be equal to nothing; then will our expression for the area *gc* become $\frac{\dot{\lambda}}{\lambda'}$; and consequently, (since the tangents of the azimuth and hour-angle vanish in the ratio of their sines, or of the sines of the opposite sides in the triangle alluded to before), we shall have the area GC $= \frac{\dot{\lambda} m}{\lambda' l}$, when the sun is on the meridian. But this area is always the same when the latitude is given, whatever be the sun's declination, and therefore may always be represented by $\frac{\dot{\lambda} m}{\lambda' l} = \frac{vs}{m} \times \frac{m}{l} = \frac{vm}{\lambda}$; and the area *gc* will be generally expressed by $\frac{vm}{\lambda} \times \frac{\text{cos. of merid altit.}}{\text{cos. of declin.}}$, when the hour-angle does not exceed the limits which have been recommended. Hence, if we add together the constant log. 3.1015, the log. radius, and the log. cosine of the merid. altitude, and from their sum subtract the log. cosines of the latit. and declin. we shall obtain the log. value of *gc*.

It will be necessary perhaps to meet an objection, which some may be inclined to urge, against the method of deducing the hour-angle in terms of the cosine, when this angle is very small. But it should be recollected, that with the angle itself we have no immediate concern, the accuracy of our conclusion depending entirely on the accuracy with which the area corresponding to any particular increment of time can be determined. Now this area, whatever be the sun's distance from the meridian, will be nearly proportional to the increment of the latit. and consequently its magnitude is totally unconnected with that of the hour-angle. A given error in the quantity which expresses this area will equally affect our conclusion, whether the angle be 2, or whether it be 20 degrees. But let us inquire what effect will actually be produced, by admitting an error of half an unit in each of the log. cosines whose difference is equal to the area *gc*; and of course in some instances, an error of an unit in the area itself, on any particular supposition of latit. and declin. We have only to ascertain the ratio which this area bears to unity: for the same ratio will the correction of the latit. bear to the error in our result. If the latit. for instance, be 50°, and the declin. 10°, on the same side of the equator with the latit. then, radius being unity, $\dot{z}$, the increment of the hour-angle, will equal $\frac{\dot{\text{L}}}{\lambda\text{T}} = \frac{\dot{\text{L}}}{\cos. 50^\circ \times \frac{\text{S}}{\Sigma}} =$ (g being the sine of the hour-angle, and α the cos. of the altit.) $\frac{\cdot}{\cos. 50^\circ} \times \frac{\sqrt{(\alpha^2 - g^2\delta^2)}}{g\delta} = 11'$ nearly, when z is 5°; and we have seen that, at any other distance from the meridian, the incremental area will be of the same magnitude. Hence, subtracting the log. cosine of 5° from that of 5° 11′, we get the difference equal to 1238; and consequently the error in our approximation will be to the error in the assumed latit. as 1 : 1238, when the log. cosines are carried to 7 places of decimals. But, when the zenith distance of the sun, at his greatest altit. is very small, and there is also a considerable uncertainty with respect to the latit. this error will probably become of more importance, and we may find it necessary to guard against it. Now it is manifest that, by diminishing the multiple which the area *gb* is of the area *gc*, exactly in the same proportion we shall diminish this error.

Mr. L. further adverts to the methods of correcting or avoiding certain small inaccuracies, which however may commonly be omitted, or obviated; he then proceeds: I have hitherto supposed that this method is only to be adopted, when the sun, at each observation, is within 15° of the meridian; or, to speak more accurately, when both the azimuth and the hour-angle are so small, that we may consider their tangents as bearing a given ratio to each other; and indeed these limits should never be transgressed, when it can possibly be avoided; for we have seen that, whatever be the method employed, the smaller the hour-angle, the greater is the exactness with which the lat. is determined. Sometimes however it will be impossible to make both, or perhaps either of our observations within the distance recommended; but even in these cases our rule may be conveniently applied. It has already been demonstrated that we can never be subject to any material error in consequence of the inequality of the areas *gb* and gb, except when the zenith-distance of the sun, at his meridian altitude, is very small; and for this case an effectual remedy has been provided.

Before concluding the theory, it may be observed, that though I have directed the altitudes to be taken on different sides of the meridian, it is by no means requisite that we should invariably adhere to this precept. We have seen the reason indeed why it is expedient, in most instances to prefer this method, being generally calculated to produce a much greater degree of exactness in the result. This however is not always the case; for, if one of the observations be made beyond the distance originally prescribed, it is of little importance whether the 2d altitude be taken on the same side of the meridian, or not. But it will sometimes be impossible to make the observations on different sides of noon; and hence it becomes necessary to inquire in what manner the real latitude may be discovered in these circumstances. The clock gives us the interval between the observations equal to ae — *ae*; and by computation we find *ag* and ag, and thence we deduce *eg* — eg, the difference between the errors in time. Having then assumed, without any regard to accuracy, 2 portions of time, corresponding to the 2 observations, whose difference is the same as the difference between the errors before determined, and which are to each other in the inverse ratio of the hour-angles, we must increase or diminish them both equally, till we get the areas in the first table of the same magnitude, and then we may conclude that we have obtained the proper value of each. The directions which have been given for the prevention of errors in the former case, when the altitudes are taken on different sides of the meridian, are very easily accommodated to the present; and it would therefore be superfluous to bestow any further consideration on them.

From a review of the inaccuracies to which this method, in particular cases, may be liable, it appears that none of them can ever be of sufficient importance to affect the mariner. If he only computes the time with each of the altitudes and the latitude by account, and an incremental area with the greatest altitude and the former latitude varied 10′, the correction will generally be deduced within much less than a second, and in the most unfavourable circumstances within a minute of the truth. But the astronomer, in every instance, even when the latitude and declination are nearly equal and of the same kind, by adopting the precautions which have been recommended, may be assured of a result perfectly exact. If however he should entertain any doubts on this point, he might easily compute a second value of the incremental area with the latitude already determined; and this, it is evident, would necessarily produce a conclusion not less accurate than if it were obtained from the direct method.

The most satisfactory way of proving the utility of this rule, will be to suppose a particular latitude and declination; with these to compute the altitudes, when the sun is at 2 given distances from the meridian; and thence to deduce the latitude, by an application of our own principles. And for this purpose, Mr. L. here gives 4 several examples, the calculation of which he gives, at length; and then adds the following remarks.

1. All the altitudes that are taken on the same side of noon, tend only to correct the error which may be supposed to exist in the greatest of these altitudes, and can have no effect in removing any inaccuracy to which the greatest altitude on the other side of the meridian may be subject. Hence we must take more than one altitude on each side of noon, if we are desirous of obtaining a very exact conclusion.

2. When some of the observations are made in the morning, and others in the afternoon, the smaller the hour-angle, in every instance, the more favourable it will be for our purpose. But if we cannot procure an altitude on each side of the meridian, we ought to make one observation when the hour-angle is as large as possible, and with this all the rest should be separately combined. We must be cautious however not to let the sun be too near the horizon, lest the apparent altitude should be affected by the uncertainty of the refraction.

3. If the clock were to furnish us with the true time, we might combine together any 2 observations made within the proper limits, without applying to the first table, and deduce a very exact correction. Should there even be a small error in the supposed time, we might still proceed in the same manner, without being liable to any material inaccuracy, provided the difference between the hour-angles was not very considerable. The error indeed occasioned by adopting this method of finding the 2 areas, and taking a mean between them for the value of *gb*, may easily be determined in any particular case. If t and t' be the respective tangents of the smaller and greater hour-angles, and $\dot{t}$ their difference; $\dot{z}$ the error of the clock in minutes of time; and $\dot{z}$ the whole error in the time computed with the

greater altitude; then will the error in the result be to the whole correction of the lat. ($\dot{\text{L}}$) $:: \frac{t'\dot{z} - t\dot{z}}{2} : t\dot{z} :: \frac{t\dot{z}}{2} : \frac{t\dot{\text{L}}}{15\text{T}\lambda} :: \frac{t\dot{z}}{2} : \frac{l\gamma\delta - d\lambda}{15\lambda\gamma\delta} \times \dot{\text{L}} :: 7.5 \times t\dot{z} : s - \frac{r}{\gamma}\dot{\text{L}}$. This error will consequently be equal to $\frac{7.5t\dot{z}}{s - \frac{r}{\gamma}}$; and hence it appears that, if we had pursued this method in the last example, and there had been an error of a minute in the time given by the clock, there would not have been an error of a single second in the conclusion.

4. If the time were determined by equal altitudes, and one of them were to be employed in computing the area *gb*, it is manifest that we should entirely exclude the error which has just been considered. It would be necessary however, in order to correct by the 2d observation, any inaccuracy that may have occurred in reading off at the first, to move the index, and then bring it apparently to the same position again, before we proceeded to take the 2d altitude. A 5th example is then calculated according to these remarks; after which is given a short specimen of the tables before mentioned.

The process observed in this table will require very little explanation. The first column contains the observed double altitudes, as they were read off from the sextant; the 2d contains the corresponding times given by the clock; the third contains the times from noon determined, with sufficient exactness, by taking half the interval between the first and last observations, (in which the altitudes are equal,) and supposing this to be the time elapsed between the first observation and the sun's reaching the meridian. The fourth column is formed of the log. cosines of the hour-angles taken immediately from the argument of the first table; and the last column exhibits the latitudes deduced from every two corresponding altitudes, and is intended to show the agreement between these results.

It is not necessary that we should employ the tables in this operation: for we may take the log. cosines of the hour-angles from Taylor's Logarithms, after the time is reduced into degrees, minutes, and seconds; and, by dividing the area $gb = 7537$, by the area $gc = 203$, (the natural number belonging to the log. 3.3081,) we shall obtain the correction required.

VIII. A Fourth Catalogue of the Comparative Brightness of the Stars. By Wm. Herschel, LL.D., F.R.S. p. 121.

After the usual list of the stars, are added the following notes.

Notes to Auriga.—γ Is β Tauri.

ε Is 32 Camelopardali.

κ "Oct. 5, 1798. The time of this star, in the observation of Flamsteed, vol. 2, p. 189, is marked ::; but it cannot be much out, as the star seems to be in the place assigned to it by the British catalogue."

61. The RA in the Atlas Cœlestis requires a correction of — 42′.

Notes to Draco.—*i* Is 87 Ursæ.

ι and θ Were never observed by Flamsteed, but are in La Caille's Catalogue of northern stars.

η M. de la Lande says the star is not to be found. See Mr. Bode's Ast. Jahr-Buch for 1795, page 198. I observed this star in a sweep of the heavens, June 2, 1788. Its brightness was estimated Sept. 11, 1795; Sept. 24, 1796; Sept. 30, 1796; and Dec. 28, 1798; so that, if M. de la Lande is sure no cloud intervened when he looked for it, we may suspect it to be a changeable star.

A. The British catalogue requires + 30′ in RA.

(ψ^2) The expression "35 — $\overline{40 + 41}$" in my estimation of brightness, means that, with the naked eye, the star is a very little brighter than 40 and 41 together, taken as one star. For they are so near each other, that the eye alone cannot distinguish them from a single star. The British catalogue gives them 3′ farther asunder than they ought to be according to Flamsteed's observation, p. 463. See also Mr. Bode's Ast. Jahr-Buch for 1785, p. 173.

b The estimation "40 — 41" was made with a 7-feet reflector, power 460.

υ Does not exist. Flamsteed has no observation of it. My double star II, 31, called 56 Draconis, is a star situated between 59 and 50, about 1½ degree from 59 towards 50.*

σ (62) Does not exist. Flamsteed has no observation of it; but, if an error of 2 hours be supposed in the calculation of one of the observations of 31 Draconis, it will account for the insertion of this star.

ϱ (72) Does not exist. There is an observation, p. 173, which produced it; but, if we admit an error of 3^m in time in that observation, it will then belong to 71.

Notes to Lynx.—7. Does not exist in the place pointed out by the British catalogue; but, in Flamsteed's observations, p. 286, its time is marked ::, and there is probably some considerable error.

20, 21, 22. The place of these stars in the heavens does not seem to agree with their situation in the Atlas.

30 Is 58 Camelopardali.

35. Flamsteed has no observation of this star; but, as it is marked 7m in the British catalogue, and has a line allotted to it, my Atlas and stars have been numbered so as to take it in; and the numbers I have used with double stars and other

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* When I say 1½ degree from 59 towards 50, it is to be understood that I express myself in degrees of a great circle. I have always used the same method of description in my catalogues of double stars; and, as these objects were pointed out for being viewed with telescopes of great magnifying power, which are generally not fixed, and therefore can give no right ascension, I am rather surprized to find that, in a catalogue published not many years ago, the author has taken my degrees of a great circle for degrees of right ascension. For instance, the double star IV, 82, where, in pointing out its place, I say, "above ¾ degree following the 16 Cephei, in a line parallel to β and α Cassiopeæ," is placed in the zone from 15 to 19° of that author's catalogue, only 2′ 47″.5 of time following 16 Cephei, when it ought to have been at least 10′ or 11′ following. I take this opportunity to mention that, in general, the same author's account of my double stars is extremely erroneous.—Orig.

objects where the stars in Lynx after the 35th are concerned, must be reckoned accordingly.

37 "Dec. 4, 1796, This star is nearer to 25 than it is marked in the Atlas." The RA should be corrected + 1°.

Notes to Lyra.—β (10) This is one of our periodical stars discovered by Mr. Goodricke; its period is about 6 days 9 hours. The greatest variation of its light, as far as I have observed, is from "10 . 14 to $\overline{6 + 7}$; 10." The expression $\overline{6 + 7}$ is borrowed from algebra, and is always to be understood as has been explained in the note to 35 Draconis.

ϱ (16) The British catalogue requires a correction of — 9° in PD; and this star will then agree with 12 Lyræ Hevelii.

ι (19) The British catalogue requires a correction of + 8° in PD.

Notes to Perseus.—5 Flamsteed has no observation of this star; but there is a star exactly in the place pointed out by the British catalogue.

10 Does not exist. Flamsteed never observed it.

q (12) "Sept. 5, 1798. This star, which has no time in Flamsteed's observations, is placed a little too forward; or requires about + 10′ in RA."

14. "Sept. 4, 1798. The time of this star is marked doubtful by Flamsteed, p. 214; but it seems to be in the situation where the British catalogue places it.'

15 Is lost. Flamsteed observed it Jan. 17, 1693, p. 186; but it is not to be seen in the place pointed out by that observation. See Bode's Ast. Jahr-Buch for 1794, p. 97.

19 Does not exist. There is an observation in p. 185, which has produced this star, but it belongs to 18; for the star is lettered τ, and a memorandum says, "Post transitum." See also Bode's Ast. Jahr-Buch for 1788, p. 172.

s (24) "Sept. 4, 1798. The place of this star in the British catalogue wants a correction of + 56′ in PD, and — 45′ in RA."

β (26) Is a periodical star. It has been noticed in the last century as subject to change, by Montanari and Maraldi; but its being periodical was discovered by Mr. Goodricke, in 1783, who fixed the time of its change at $2^d\ 20^h\ 48^m\ 56^s$. I have seen it when brightest, "6 Arietis , 26 — 23", and when most diminished, "26 , 25".

o′ (38) Sept. 5, 1798. The British catalogue requires nearly + 2° in RA, and — 13′ in PD; at least there is no other star that can be taken for it."

n (42) "Sept. 4, 1798. The British catalogue requires a correction of + 13 in PD."

Notes to Sextans.—1 Is 10 Leonis. 10 Is 25 Leonis. 11 Is 28 Leonis.

12 "March 17, 1797. This star is misplaced in the British catalogue; the PD should be corrected + 1°."

28 "March 21, 1797. This star is misplaced in the British catalogue, and requires a correction of + 20′ in RA, and + 1° in PD."

29 "March 21, 1797. The PD of this star in the British catalogue requires + 1°."

Notes to Taurus.—3 Does not exist. Flamsteed never observed it.

8 "Jan. 10, 1796. This star does not exist. Flamsteed has no observation of it. There is a star about 9m not far from the place."

9. "Dec. 28, 1798. This star is lost." M. de la Lande says it is not to be found. See Mr. Bode's Ast. Jahr-Buch for 1795, p. 198. Flamsteed has 2 complete observations of it, p. 86, and p. 506. We can hardly admit what Mr. Bode suggests, that this star, like the rest, has found its way into the British catalogue by some error of writing, or of calculating the observations; it will therefore be advisable to look for a future re-appearance of it, as it may prove to be a periodical or changeable one.

n (15) Does not exist. Flamsteed has no observation of it.

34 The estimation "39 — 34" belongs to a star very nearly in the place where, according to Flamsteed's observation, 34 should be; but, as we know by calculation that the Georgian planet was about the situation where, the 13 of Dec. 1690, Flamsteed observed the supposed 34th, there can be no doubt but that he must have seen it, and taken it for a fixed star. The magnitude, 6m, which he assigned to 34, agrees perfectly well with the lustre of the planet, compared with other stars which the same author has marked 6m; and, with his telescope, he could not have the most distant suspicion of its being any other object than a fixed star of about the 6th magnitude.

40. "March 4, 1796. The RA in the Atlas requires a correction of about + 20′."

55. In the British catalogue, the PD requires — 8′.

56. The RA in the British catalogue requires — 15′.

82 Does not exist. Flamsteed did not observe this star, unless we admit a correction of the British catalogue — 1° 5′ in PD.

99. Flamsteed has no observation of this star; but, as there is one in the heavens, about a degree more north, the British catalogue requires probably a correction of — 1° in PD.

100. This star is lost. Flamsteed settled its place, p. 369, and the observation seems to be a very good one.

103. Flamsteed has no observation of this star. How it came to be inserted in the British catalogue does not appear. I have given it as a double star v, 114, and here also estimated its brightness; but it must be remembered that my estimations do not strictly ascertain the place of objects. If therefore 103 does not exist, my double star, as well as the one here estimated, must be some star not far from the place assigned to 103 in the British catalogue.

β (112) Is 23 Aurigæ.

118 The Atlas should be corrected — 30′ in RA.

124 Does not exist; unless we admit a correction of + 1° 4′ in RA of the British catalogue.

138 Does not exist; but, as there is no time in Flamsteed's observation of this star, it is probably misplaced in the British catalogue, for there are several considerable stars in the neighbourhood.

Note to Triangulum.—d (1) "Nov. 2, 1798." This star, which has the time and zenith distance in Flamsteed's observations doubtful, seems to be nearly in the place where the British catalogue gives it. It should perhaps be a few minutes more north.

IX. On a Submarine Forest, on the East Coast of England. By Joseph Correa de Serra, LL.D., F.R.S. and A. S. p. 145.

It was a common report in Lincolnshire, that a large extent of islets of moor, situated along its coast, and visible only in the lowest ebbs of the year, was chiefly composed of decayed trees. These islets are marked in Mitchell's chart of that coast, by the name of clay huts; and the village of Huttoft, opposite to which they principally lie, seems to have derived its name from them. In the month of Sept. 1796, I went to Sutton, on the coast of Lincolnshire, in company with Sir Jos. Banks, to examine their extent and nature. The 19th of the month, being the first day after the equinoctial full moon, when the lowest ebbs were to be expected, we went in a boat, at half past 12 at noon, and soon after set foot upon 1 of the largest islets then appearing. Its exposed surface was about 30 yards long, and 25 wide, when the tide was at the lowest. A great number of similar islets were visible round us, chiefly to the eastward and southward; and the fishermen, whose authority on this point is very competent, say, that similar moors are to be found along the whole coast, from Skegness to Grimsby, particularly off Addlethorpe and Mablethorpe. The channels dividing the islets were, at the time we saw them, wide, and of various depths; the islets themselves ranging generally from east to west in their largest dimension.

We visited them again in the ebbs of the 20th and 21st; and, though it generally did not ebb so far as we expected, we could notwithstanding ascertain, that they consisted almost entirely of roots, trunks, branches, and leaves of trees and shrubs, intermixed with some leaves of aquatic plants. The remains of some of these trees were still standing on their roots; while the trunks of the greater part lay scattered on the ground, in every possible direction. The bark of the trees and roots appeared generally as fresh as when they were growing; in that of the birches particularly, of which a great quantity was found, even the thin silvery membranes of the outer skin were discernible. The timber of all kinds, on the contrary, was decomposed and soft, in the greatest part of the trees; in some however it was firm, especially in the knots. The people of the country have often found among them very sound pieces of timber, fit to be employed for several economical purposes.

The sorts of wood which are still distinguishable, are birch, fir, and oak. Other woods evidently exist in these islets, of some of which we found the leaves in the soil; but our present knowledge of the comparative anatomy of timbers, is not so far advanced as to afford us the means of pronouncing with confidence respecting their species. In general, the trunks, branches, and roots of the decayed trees, were considerably flattened; which is a phenomenon observed in the Surtarbrand or fossil wood of Iceland, and which Scheuchzer remarked also in the fossil wood found near the lake of Thun, in Switzerland. The soil to which the trees are affixed, and in which they grew, is a soft greasy clay; but, for many inches above its surface, the soil is entirely composed of rotten leaves, scarcely distinguishable to the eye, many of which may be separated, by putting the soil in water, and dexterously and patiently using a spatula, or a blunt knife. By this method I obtained some perfect leaves of ilex aquifolium, which are now in the herbarium of Sir Jos. Banks; and some other leaves which, though less perfect, seem to belong to some species of willow. In this stratum of rotten leaves, we could also distinguish several roots of arundo phragmites.

These islets, according to the most accurate information, extend at least 12 miles in length, and about a mile in breadth, opposite Sutton shore. The water without them, towards the sea, generally deepens suddenly, so as to form a steep bank. The channels between the several islets, when the islets are dry, in the lowest ebbs of the year, are from 4 to 12 feet deep; their bottoms are clay or sand, and their direction is generally from east to west. A well dug at Sutton, by Josh. Searby, shows that a moor of the same nature is found under ground, in that part of the country, at the depth of 16 feet; consequently, very nearly on the same level with that which constitutes the islets. The disposition of the strata was found to be as follows:

Clay	16 feet.
Moor, similar to that of the islets	from 3 to 4 ditto.
Soft moor, like the scowerings of a ditch bottom, mixed with shells and silt	20 ditto.
Marly clay	1 foot.
Chalk rock	from 1 to 2 feet.
Clay	31 yards.
Gravel and water; the water has a chalybeate taste.	

In order to ascertain the course of this subterraneous stratum of decayed vegetables, Sir Jos. Banks directed a boring to be made, in the fields belonging to the R. S., in the parish of Mablethorpe. Moor, of a similar nature to that of Searby's well, and of the islets, was found, very nearly on the same level, about 4 feet thick, and under it a soft clay.

The whole appearance of the rotten vegetables we observed, perfectly resembles, according to the remark of Sir Jos. Banks, the moor which, in Blankeney fen, and in other parts of the East fen in Lincolnshire, is thrown up in the making of banks; barks, like those of the birch tree, being there also abundantly found. This moor extends over all the Lincolnshire fens, and has been traced as far as Peterborough, more than 60 miles to the south of Sutton. On the north side, the moory islets, according to the fishermen, extend as far as Grimsby, situated on the south side of the mouth of the Humber; and it is a remarkable circumstance, that in the large tracts of low lands which lie on the south banks of that river, a little above its mouth, there is a subterraneous stratum of decayed trees and shrubs, exactly like those we observed at Sutton; particularly at Axholme isle, a tract of 10 miles in length, by 5 in breadth; and at Hatfield chase, which comprehends 180,000 acres. Dugdale* had long ago made this observation, in the first of these places; and De la Pryme† in the second. The roots are there likewise standing in the places where they grew; the trunks lie prostrate. The woods are of the same species as at Sutton. Roots of aquatic plants and reeds are likewise mixed with them; and they are covered by a stratum of some yards of soil, the thickness of which, though not ascertained with exactness by the above-mentioned observers, we may easily conceive to correspond with that which covers the stratum of decayed wood at Sutton, by the circumstance of the roots being according to Mr. Richardson's observations,‡ only visible when the water is low, where a channel was cut, which has left them uncovered.

Little doubt can be entertained of the moory islets of Sutton being a part of this extensive subterraneous stratum, which, by some inroad of the sea, has been there stripped of its covering of soil. The identity of the levels; that of the species of trees; the roots of these affixed, in both, to the soil where they grew; and, above all, the flattened shape of the trunks, branches, and roots, found in the islets, which can only be accounted for by the heavy pressure of a superinduced stratum, are sufficient reasons for this opinion. Such a wide spread assemblage of vegetable ruins, lying almost in the same level, and that level generally under the common mark of low water, must naturally strike the observer, and give birth to the following questions. 1. What is the epoch of this destruction? 2. By what agency was it effected? In answer to these questions, I will venture to submit the following reflections.

The fossil remains of vegetables hitherto dug up in so many parts of the globe, are, on a close inspection, found to belong to 2 very different states of our planet. The parts of vegetables, and their impressions, found in mountains of a cotaceous, schistous, or even sometimes of a calcareous nature, are chiefly of plants now existing between the tropics, which could neither have grown in the latitudes in which they are dug up, nor have been carried and deposited there by any of the acting forces under the present constitution of nature. The formation indeed of the very mountains in which they are buried, and the nature and disposition of the materials which compose them, are such as we cannot account for by any of the actions and re-actions which, in the actual state of things, take place on the surface of the earth. We must necessarily recur to that period in the history of our planet, when the surface of the ocean was at least so much above its present level, as to cover

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* History of Embanking and Draining. Chap. 27.——— † Philos. Trans. vol. 22, p. 980.——— ‡ Philos. Trans. vol. 19, p. 528.—Orig.

even the summits of these secondary mountains which contain the remains of tropical plants. The changes which these vegetables have suffered in their substance, is almost total; they commonly retain only the external configuration of what they originally were. Such is the state in which they have been found in England, by Llwyd; in France, by Jussieu; in the Netherlands, by Burtin; not to mention instances in more distant countries. Some of the impressions or remains of plants found in soils of this nature, which were, by more ancient and less enlightened oryctologists, supposed to belong to plants actually growing in temperate and cold climates, seem, on accurate investigation, to have been parts of exotic vegetables. In fact, whether we suppose them to have grown near the spot where they are found, or to have been carried thither from different parts, by the force of an impelling flood, it is equally difficult to conceive, how organized beings, which, in order to live, require such a vast difference in temperature and in seasons, could live on the same spot, or how their remains could, from climates so widely distant, be brought together to the same place, by one common dislocating cause. To this ancient order of fossil vegetables belong whatever retains a vegetable shape, found in or near coal mines, and, to judge from the places where they have been found, the greater part of the agatized woods. But, from the species and present state of the trees which are the subject of this Memoir, and from the situation and nature of the soil in which they are found, it seems very clear that they do not belong to this primeval order of vegetable ruins.

The 2d order of fossil vegetables, comprehends those which are found in strata of clay or sand; materials which are the result of slow depositions of the sea or of rivers, agents still at work under the present constitution of our planet. These vegetable remains are found in such flat countries as may be considered to be of a new formation. Their vegetable organization still subsists, at least in part; and their vegetable substance has suffered a change only in colour, smell, or consistence; alterations which are produced by the developement of their oily and bituminous parts, or by their natural progress towards rottenness. Such are the fossil vegetables found in Cornwall, by Borlase; in Essex, by Derham; in Yorkshire, by De la Pryme, and Richardson; and in foreign countries, by other naturalists. These vegetables are found at different depths, some of them much below the present level of the sea, but in clayey or sandy strata, evidently belonging to modern formation, and have, no doubt, been carried from their original place, and deposited there by the force of great rivers or currents, as it has been observed with respect to the Mississippi.* In many instances however, these trees and shrubs are found standing on their roots, generally in low or marshy places, above, or very little below, the actual level of the sea.

To this last description of fossil vegetables, the decayed trees here described certainly belong. They have not been transported by currents or rivers; but, though standing in their native soil, we cannot suppose the level in which they are found,

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* La Coudreniere sur les Depots du Mississippi. Journ. de Phys. vol. 21, p. 230.—Orig.

to be the same as that in which they grew. It would have been impossible for any of these trees and shrubs to vegetate so near the sea, and below the common level of its water: the waves would cover such tracts of land, and hinder any vegetation. We cannot conceive that the surface of the ocean has ever been lower than it now is, on the contrary, we are led by numberless phenomena to believe, that the level of the waters in our globe is much below what it was in former periods; we must therefore conclude, that the forest here described grew in a level high enough to permit its vegetation; and that the force, whatever it was, which destroyed it, lowered the level of the ground where it stood.

There is a force of subsidence, particularly in soft ground, which, being a natural consequence of gravity, slowly though perpetually operating, has its action sometimes quickened and rendered sudden by extraneous causes, for instance, by earthquakes. The slow effects of this force of subsidence have been accurately remarked in many places; examples also of its sudden action are recorded in almost every history of great earthquakes. The shores of Alexandria, according to Dolomieu's observations, are a foot lower than they were in the time of the Ptolemies. Donati, in his natural history of the Adriatic, has remarked, seemingly with great accuracy, the effects of this subsidence at Venice; at Pola, in Istria; at Lissa, Bua, Zara, and Diclo, on the coast of Dalmatia. In England, Borlase has given, in the Phil. Trans. vol. 48, p. 62, a curious observation of a subsidence, of at least 16 feet, in the ground between Sampson and Trescaw islands, in Scilly. The soft and low ground between the towns of Thorne and Gowle, in Yorkshire, a space of many miles, has so much subsided in latter times, that some old men of Thorne affirmed, "that whereas they could before see little of the steeple of Gowle, they now see the churchyard wall."* The instances of similar subsidence which might be mentioned, are innumerable.

This force of subsidence, suddenly acting by means of some earthquake, seems the most probable cause to which the actual submarine situation of the forest we are speaking of may be ascribed. It affords a simple easy explanation of the matter; its probability is supported by numberless instances of similar events; and it is not liable to the strong objections which exist against the hypothesis of the alternate depression and elevation of the level of the ocean; an opinion which, to be credible, requires the support of a great number of proofs, less equivocal than those which have hitherto been urged in its favour, even by the genius of a Lavoisier.†

The stratum of soil, 16 feet thick, placed above the decayed trees, seems to remove the epoch of their sinking and destruction, far beyond the reach of any historical knowledge. In Cæsar's time, the level of the North Sea appears to have been the same as in our days. He mentions the separation of the Wahal branch of the Rhine, and its junction to the Meuse; noticing the then existing distance from that junction to the sea; which agrees, according to D'Anville's inquiries,‡ with

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[*] Gough's edition of Camden's Britannia, t. 3, p. 35.——[†] Mém. de l'Acad. de Paris, 1789, p. 351. ——[‡] D'Anville Notice des Gaules. p. 461.—Orig.

the actual distance. Some of the Roman roads constructed by order of Augustus, under Agrippa's administration, leading to the maritime towns of Belgium, still exist, and reach the present shore.* The descriptions which Roman authors have left us, of the coasts, ports, and mouths of rivers, on both sides of the North Sea, agree in general with their present state; except in the places ravaged by the inroads of this sea, more apt, from its form, to destroy the surrounding countries, than to increase them.

An exact resemblance exists between maritime Flanders and the opposite low coast of England, both in point of elevation above the sea, and of internal structure and arrangement of their soils. On both sides, strata of clay, silt, and sand, often mixed with decayed vegetables, are found near the surface; and in both, these superior materials cover a very deep stratum of bluish or dark-coloured clay, unmixed with extraneous bodies. On both sides, they are the lowermost part of the soil, existing between the ridges of high lands,† on their respective sides of the same narrow sea. These two countries are certainly coeval; and whatever proves that maritime Flanders has been for many ages out of the sea, must, in my opinion, prove also, that the forest we are speaking of was long before that time destroyed, and buried under a stratum of soil. Now it seems proved, from historical records, carefully collected by several learned members of the Brussels Academy, that no material change has happened to the lowermost part of maritime Flanders, during the period of the last 2000 years.‡

I am therefore inclined to suppose the original catastrophe which buried this forest, to be of a very ancient date; but I suspect the inroad of the sea which uncovered the decayed trees of the islets of Sutton, to be comparatively recent. The state of the leaves and of the timber, and also the tradition of the neighbouring people, concur to strengthen this suspicion. Leaves and other delicate parts of plants, though they may be long preserved in a subterraneous situation, cannot remain uninjured, when exposed to the action of the waves and of the air. The people of the country believe, that their parish church once stood on the spot where the islets now are, and was submerged by the inroads of the sea; that, at very low water, their ancestors could even discern its ruins; that their present church was built to supply the place of that which the waves washed away; and that even their present clock belonged to the old church. So many concomitant, though weak testimonies, incline me to believe their report, and to suppose that some of the stormy inundations of the North Sea, which in these last centuries have washed away such large tracts of land on its shores, took away a soil resting on clay, and at last uncovered the trees which are the subject of this paper.

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* Nicol. Bergier. Hist. des grands Chemins des Romains. Ed. de Bruxelles. vol. 2, p. 109.—Orig.

† These ridges of high lands, both on the British and Belgic side, must be very similar to each other, since they both contain parts of tropical plants in a fossil state. Cocoa nuts, and fruits of the areca, are found in the Belgic ridge. The petrified fruits of Sheppey, and other impressions of tropical plants, on this side of the water, are well known.——‡ Vide several papers in the Brussels Mémoires; also Journ. de Phys. t. 34, p. 401.—Orig.

Meteorological Journal, kept at the Apartments of the R. S. By order of the President and Council. p. 157.

1798.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	°	°	°	°	°	°	°	°	°	Inc.	Inc.	Inc.	°	°	°	Inc.
Jan.	53	28	39.6	53	29	40.1	59	49	53.5	30.52	28.96	29.94	90	73	82.8	1.105
Feb.	54	24	39.9	54	24	40.1	58	49	54.2	30.76	29.23	30.11	90	71	82.2	0.693
Mar.	58	30	42.9	58	30	42.9	61	50	55.4	30.37	29.18	29.93	90	68	79.8	0.333
April	69	30	51.6	69	31	52.7	66	53	59.8	30.38	29.27	29.96				0.517
May	76	43	56.5	75	46	57.3	66	58	60.6	30.44	29.11	30.00	69	30	51.4	1.621
June	86	47	64	86	51	64.8	71	62	65.8	30.42	29.65	30.07	69	32	50.1	0.960
July	78	51	63.9	76	54	64.4	72	64	65.9	30.17	29.36	29.80	74	38	55.8	2.879
Aug.	83	52	65.6	82	55	63.9	72	66	68.5	30.35	29.70	30.09	70	41		1.525
Sept.	76	44	58.9	76	45	59.2	70	58	64.2	30.26	28.97	29.78	73	37		2.437
Oct.	64	32	51.8	63	33	52.4	63	57	60.7	30.39	29.16	29.90	82	45		3.428
Nov.	60	25	41.6	60	25	42.4	62	48	55.4	30.27	28.69	29.58	93	57		3.056
Dec.	50	11	35.2	50	14	35.5	57	38	50.3	30.58	29.27	29.90	95	53		0.857
Whole year.			51.0			51.3			59.5			29.92				19.411

X. The Dissection of an Hermaphrodite Dog. With Observations on Hermaphrodites in general. By Everard Home, Esq. F. R. S. p. 157.

Instances of animals being brought forth, whose organs of generation are preternaturally formed, sometimes occur, and have been commonly called hermaphrodites; this term however should be confined to those only in which there is a mixture of the male and female organs in the same animal. Examples of this kind have been rarely noticed; they have been met with at very distant periods of time, and confined to too few species of animals, to afford extensive opportunities for collecting observations respecting them. To this cause must be attributed the little information that has been acquired on so curious and interesting a subject.

Monstrous productions, having a mixture of the male and female organs, and which deserve the name of hermaphrodites, appear to arise most frequently in neat cattle; they are now generally known, and have been called free-martins. This compound animal attracted the attention of the late Mr. Hunter; and a paper of his, containing a description of the organs of generation of different free-martins, to show that they are by no means uniformly the same, or partake equally of the parts belonging to both sexes, is published in the Phil. Trans. vol. 69. To add to these dissections an account of similar formation of the organs of generation in a dog, is the intention of the present paper. The subject having already been considered an object deserving the attention of the R. S., is an inducement for bringing forward new facts, and observations which have been made respecting them.

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* The quicksilver in the basin of the barometer is 81 feet above the level of low water spring tides at Somerset-house.

The causes of monstrous productions of every kind are at present equally unknown, but it is highly probable that they are very similar; and, when once they have been brought into action, it would be reasonable to suppose, that the influence should be continued to several young, in succession; this is however by no means the case, for, of all the monstrous productions that have come under my observation, none of them have been either immediately preceded or followed, by a monster of the same, or of any other kind. In the neat cattle, the free-martin is most commonly met with where there are twins; one is a free-martin, and the other is always a perfectly formed male. In the human species, there have been instances of mothers having alternately a perfect and a monstrous child; so that these observations lead to the idea, that monstrous productions do not follow immediately on each other; that they sometimes alternate; but are commonly, as in the child with the double head, an account of which has been laid before this Society, only one in a family, of which the others are perfectly formed. From Mr. Hunter's observations we learn, that in all the instances of free-martins which he examined, none had the complete organs of the male and female, but partly the one and partly the other, forming a mixture of both; and, what is deserving of notice, the ovaria and testicles in all of them, were too imperfect to perform their functions.

There is much reason to believe, that no instance of an hermaphrodite, in the strict sense of the word, has ever occurred in the more perfect quadrupeds, or in the human species; for, when we consider the bones of the pelvis, to which the organs of generation are connected, it is difficult to conceive in what way the complete parts of the male and female could be placed, distinct from each other; and no instance of its having happened is to be found, in any record which can be depended on.

As much has been said by authors, respecting hermaphrodites, particularly in our own species, and histories of them have a place even in the Phil. Trans., it may not be improper to explain the different kinds of monstrous production, which have been frequently mistaken for a complete mixture of male and female organs. This inquiry into the subject of hermaphrodites, I shall pursue in the following order: 1°, examine into such malformations of the male, as led to the belief of the persons being hermaphrodites. 2°, such malformations in the female, as have led to the same conclusion. 3°, such males as, from a deficiency in their organs have not the character and general properties of the male, and may be called neuters. 4°, those in which there is a real mixture of the organs of both sexes, though not sufficiently complete to constitute double organs; which I believe to be the nearest approach towards an hermaphrodite that has been met with in the more perfect animals; and it is extremely in favour of this opinion, that every account I have met with in authors, may be referred to one or other of these heads.

Baron Haller, who has laboured this subject with his usual perspicuity, has collected in 1 point of view, the histories of reputed hermaphrodites, from almost every author that preceded him*; and his conclusions are in confirmation of what is now advanced. In considering the malformations of the male organs of generation, which put on the appearance of both the male and female organs, I cannot better illustrate the description, than by taking up the cases mentioned in Cheselden: one is a Negro, the other a European. From an examination of the engravings of that work †, no superficial observer would harbour a doubt of their being complete hermaphrodites; and the opinion of Dr. Douglas, which is annexed, in favour of the existence of the female organs, strengthens Cheselden's authority. In these cases however there is much reason to believe, that the parts were entirely those belonging to the male, only very much distorted by an imperfection of the scrotum, which was divided into 2 separate bags, with a deep slit between them, resembling very much the labia pudendi, and the opening into the vagina; over these hung down the penis: the imperfection of the septum of the scrotum extended to the canal of the urethra. This is not unlike the fissure in the hare-lip being continued through the bony palate, a circumstance often met with. The under surface of the penis was attached, through its whole length, to the two bags containing the testicles, looking like a preternatural clitoris, to which it bore a more perfect resemblance, from the absence of the urethra. The urine passed through a preternatural termination of the urethra in the perinæum, and came out externally, in the space between the testicles, which formed an enlarged aperture, that had been mistaken for a narrow vagina, in consequence of its allowing an instrument to pass to some distance, by conducting it to the bladder.

Haller dissected a ram, in which the parts had been supposed to be those of an hermaphrodite. He found the animal to be a male, with the imperfections above mentioned; and, on comparing the dissection with many instances which have been stated by different writers, both in the human species and in quadrupeds, he considered them all to have been similar in the conformation of the parts of generation. Such malformation of the parts in the male, is particularly deserving of attention, as it is that which, more than any other, has been mistaken for a mixture of those of both sexes. It often occurs in different degrees of imperfection; and in some instances can be materially diminished by the assistance of the surgeon, though the greater number of cases are beyond the reach of art.

It may be supposed, that so great an imperfection in the structure of the penis, is necessarily attended with others in the more essential organs of generation; I shall therefore give an instance to the contrary. In a case of this kind, in which the canal was continued to the external orifice at the glans penis, the deficiency of the urethra behind the scrotum was so great that every attempt to close the aperture necessarily left in perinæo proved ineffectual; and, under these circumstances, the person married. When he had connexion, the emission was complete,

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* Comment. A. Haller, de Hermaph. Comment. Soc. Reg. Scient. Coll. Tom. 1.—Orig.

† Cheselden's Anatomy of the Human Body, 8vo.—Orig.

which proved that the testicles were perfect; but the semen always passed out at the perinæum. The late Mr. Hunter was consulted, to remedy, if possible, this inconvenience, and enable the person to beget children. After the failure of several modes of treatment which were adopted, Mr. Hunter suggested the following experiment. He advised that the husband should be prepared with a syringe fitted for the purpose, previously warmed; and that, immediately after the emission had taken place, it should be taken up by the syringe, and injected into the vagina, while the female organs were still under the influence of the coitus, and in the proper state for receiving the semen. This experiment was actually made, and the wife proved with child. On a subject of this kind it is proper to speak with caution; but, from all the attending circumstances, no doubt was entertained by Mr. Hunter, or the husband, that the impregnation was entirely the effect of the experiment. Spallanzani's experiments on this subject, on animals, were made several years after this proposal of Mr. Hunter's had been attended with success.

In the female, there are 2 malformations of the organs of generation, which give an appearance to the external parts, tending to mislead the judgment respecting the sex. One is, an enlargement of the clitoris; which is stated by authors to grow to an immoderate size in warm climates, and to resemble a penis. In cold climates, instances of this kind do not occur; and even in hot countries they are now rarely met with to such an extent. The accounts that have been given we must suppose are much exaggerated; for it is scarcely to be believed, that any enlargement which the clitoris is liable to, can give it a sufficient likeness to a penis, to be productive of any mistake. The most remarkable instance of this kind, that has come to my knowledge, was a Negress, who was purchased by General Melville, in the island of Dominica in the West Indies, about the year 1774. She was of the Mandingo nation, 24 years of age; her breasts were very flat; she had a rough voice, and masculine countenance. The clitoris was 2 inches long, and in thickness resembled a common sized thumb; when viewed at some distance, the end appeared round, and of a red colour, but on a closer examination, was found to be more pointed than that of a penis, not flat below, and having neither prepuce nor perforation; when handled, it became half erected, and was then fully 3 inches long, and much larger than before: when she voided her urine, she was obliged to lift it up, as it completely covered the orifice of the uretha. The other parts of the female organs were found to be in a natural state.

Dr. Clark, who has favoured me with this account, from his own examination, mentions, that among the Negro women of the Mandingo and Ibbo nations, a large clitoris is very common; and in several instances which came under his observation, in the course of his practice in midwifery, in the island of Dominica, the clitoris was 1 inch long, and thick in proportion; but attended with no other preternatural appearance. The case above-mentioned, while it proves that the clitoris is sometimes of a very extraordinary size, also shows, that when so enlarged, it is unconnected with any mixture of the male organs.

The other malformation is a protrusion of the internal parts, which may be considered a prolapsus uteri, and therefore more a disease than an original malformation; it is probable however, that if the parts had been perfectly formed, and acquired their due size, this change of their situation could not happen. The womb, thus displaced, has put on an appearance resembling a penis; and has been actually mistaken for one, even by medical men of character, who examined the parts. The following case of this kind came under my own observation. A French woman had a prolapsus uteri at an early age, which increased as she grew up; the cervix uteri was uncommonly narrow, and at the time I saw her, when she was about 25 years old, projected several inches beyond the external opening of the vagina; the surface of the internal parts, from constant exposure, had lost its natural appearance, and resembled the external skin of the penis; the orifice of the os tincæ was mistaken for the orifice of the urethra. This woman was shown as a curiosity in London; and, in the course of a few weeks, made 400l. I was induced by curiosity to visit her, and on the first inspection discovered the deception; which, though very complete to a common observer, must have been readily detected by any person intimately acquainted with anatomy. To render herself still more an object of curiosity, she pretended to have the powers of a male. As soon as the deception was found out, she was obliged to go away.

The history of an hermaphrodite is published in the 16th vol. of the Phil. Trans., which proves to be exactly similar to this, as is sufficiently ascertained by the menses flowing regularly through the orifice of the supposed penis. The French physicians were however so perfectly convinced of her manhood, that they made her change her dress, and learn a trade. To this she readily submitted; and the account says, she could perform very well the functions of a man, but not those of the other sex. This woman also was French.

It is probable, that the most common imperfection in the male organs of generation, is a defect in the structure of the testicle; that organ remaining in its fœtal state, and never becoming fitted to perform its functions. When this happens, the person cannot be considered of the masculine gender, but of the neuter; having, properly speaking, no sex. Such persons, in their general external form, have neither the true character of a man, nor that of a woman. These neuters are more common than is generally believed: they vary in their external appearance; some being an exact medium between the male and female, and others having a greater resemblance to the one or the other sex; which bias may be the result of turn of mind, occupation, or other circumstances. Probably, only those whose form is very like females, attract the notice of common observers, so as to have their defects discovered.

The following instances of children with male organs having remained neuters, in consequence of the testes being imperfectly formed, and incapable of producing that influence on the constitution which stamps it with the character of the sex, have come under my own observation. A marine soldier, aged 23, in the year

1779, was admitted a patient into the Royal Naval Hospital at Plymouth, under my care. He had been there only a few days, when a suspicion arose of his being a woman, which induced me to examine into the circumstances. He proved to have no beard; his breasts were fully as large as those of a woman at that age; he was inclined to be corpulent; his skin uncommonly soft, for a man; his hands fat and short; his thighs and legs very much like those of a woman; the quantity of fat on the os pubis, resembled the mons veneris; the penis was unusually small, as well as short, and not liable to erections; the testicles not larger in size than we commonly find them in the fœtal state; and he had never felt any passion for women. In this case, the testicles had been imperfectly formed, and the constitution was deprived of that influence which it naturally receives from them. In addition to this imperfection in the organs of generation, he was weak in his intellects, and in his bodily strength.

The 2 following cases show a still greater degree of imperfection in the male organs. A woman near Modbury, in Devonshire, the wife of a day-labourer, had 3 children: the first was considered to be an hermaphrodite; the 2d was a perfectly formed girl; and the 3d an hermaphrodite, similar to the first. Having heard this account, I visited the cottage, in the year 1779, and made the following observations on the imperfectly formed children. The eldest was 13 years of age, of a most uncommon bulk, which appeared to be almost wholly composed of fat; his body, round the waist, was equal to that of a fat man, and his thighs and legs in proportion; he was 4 feet high; his breasts as large as those of a fat woman; the mons veneris loaded with fat; no penis; a præputium $\frac{1}{4}$th of an inch long; and under it the meatus urinarius, but no vagina. There was an imperfect scrotum, with a smooth surface, without a raphe in the middle, but in its place, an indented line; it contained 2 testicles, of the size they are met with in the fœtus. He was very dull and heavy, almost an idiot, but could walk and talk: he began to walk at a year and $\frac{1}{2}$ old. The younger one was 6 years old, uncommonly fat, and large for his age; more an idiot than the other, not having sense enough to learn to walk, though his limbs were not defective. The external parts of generation differed in nothing from those just described, except in the prepuce being an inch long. He had a supernumerary finger on each hand, and a supernumerary toe on each foot

It is curious that the mother of these 2 children, so like in their imperfections, should have had a perfectly formed child between them; and it leads me to mention, that the Polish dwarf, Count Boruwlaski, who was in England in 1786, stated, that in his family the children had been alternately dwarfs, of which there were 2, he and his sister; the intermediate child having grown to the common size. The immense accumulation of fat, and the uncommon size of these children, accords with the disposition to become fat, so commonly met with in the free-martin.

The species of malformation of the organs of generation in which there is really a mixture of parts, or an evident attempt towards it, is less common than those we have mentioned. Mr. Hunter has given several instances of it in the neat cattle, where the mixture of male and female organs was in different degrees. In 2 free-martins, imperfect testicles were found in the situation of the ovaria; and, in a 3d, an appearance like both testicles and ovaria was met with, close together, in the situation of the ovaria. He also gives the dissection of an hermaphrodite ass; in which there were substances resembling both testicles and ovaria in the abdomen. Mr. Hunter never met with an instance of this kind in the dog; and I have not found one in any record which I have examined. I shall therefore state the following history of a case which has fallen under my own observation, as it proves, that a mixture of the generative organs sometimes occurs, in a species of animal in which it had not been before met with; and, as the dog is more domesticated than almost any other quadruped, the occurrence must be rare indeed, otherwise it could not have escaped notice.

A favourite dog of Lord Besborough's, which had lived in the family for many years, was observed to have no teats, and never to have been in heat, though, to appearance a perfectly formed bitch in all other respects: those circumstances being made known to Sir Jos. Banks, he requested, that when the animal died, it might be sent to him. This happened last summer; and I had an opportunity of examining the organs of generation, which exhibited the following appearances. There was not the smallest appearance of teats on the skin of the belly: so that, in this particular, it differed both from the male and female; nor was there the least trace of any thing like the gland of the breast, under the skin. The clitoris was very large, being three-quarters of an inch long, and half an inch broad; the orifice of the meatus urinarius was unusually large, as if it was intended for a common passage to the bladder and vagina; so that the external parts were only the clitoris, meatus urinarius, and rectum. Internally, in the situation of the ovaria, were 2 imperfectly formed small testicles, distinguished to be such by the convolutions of the spermatic artery; from these passed down an impervious chord, or vas deferens, not thicker than a thread, to the posterior part of the bladder, where they united into one substance, which was nearly 2 inches long, and terminated behind the meatus urinarius. The other parts of the animal were naturally formed. When the testicles were cut into, they appeared to have no regular glandular structure. In this animal, the clitoris was the only part of the female organs that was completely formed. What rendered the parts a decided mixture of male and female organs was, the testes being in the place appropriated for the ovaria, and the ligamentous substance, to which the vasa deferentia were connected, somewhat resembling an impervious vagina. The clitoris, in this instance, could not be considered as an imperfect penis, since the bone, the distinguishing mark of the dog's penis, was wanting.

In Haller's account of hermaphrodites, before-mentioned, there is the history of a kid, in which there was a mixture of male and female organs, illustrated by an engraving. They were very similar to those of this dog: the imperfect testicles were in the same situation; but there was a pervious canal or vagina, that divided, like the uterus, into 2 horns, which extended to the testicles; there were also vesiculæ seminales. In the Memoirs of the Royal Acad. of Sciences of Paris for 1720, there is a very accurate description, by M. Petit, of a similar mixture of organs in the human species. The person had wholly the character of a man, but was of a delicate constitution: he was a soldier, and died of his wounds. The appearance of the penis is passed over; but the scrotum, not containing testicles, drew M. Petit's attention; and, in the dissection, he found testicles in the situation of the ovaria, attached to 2 processes, continued from an imperfect vagina, but having vasa deferentia, which passed, in the usual manner, to the vesiculæ seminales. The vagina communicated with the urethra, between the neck of the bladder and the prostate gland.

A case of mixed organs is mentioned in Dr. Baillie's Morbid Anatomy: the person was 24 years of age, had the breasts of a woman, and no beard. The clitoris and meatus urinarius had the natural appearance, but there were no nymphæ, and the labia pudendi were unusually pendulous, containing a testicle in each of them. The vagina was nearly 2 inches long, and terminated in a blind end. She never menstruated, and had a masculine appearance. This appears to have been the reverse of the case mentioned by M. Petit: in this, the external parts were those of the female, in which were contained the testicles; while, in the other, the internal parts were those of the female, with the testicles attached to them.

There is still another mixture of the organs of the female with those of the male, which is probably the most rare in its occurrence; this is, an hermaphrodite bull, probably a free-martin, partaking so much of the bull as to have the male organs capable of propagating the species, and an udder capable of secreting milk. The glands which secrete milk, though in themselves not organs of generation, entirely belong to them, and form a part of the female character, sufficiently obvious to connect them intimately with the present subject. That an animal not a perfect female, should have glands which secrete milk, or indeed that an animal truly female, without having had young, should give milk, is so extraordinary, that even written evidence respecting it requires confirmation to entitle it to credit; in this respect, the following fact must be considered as perfectly satisfactory.

An instance of an hermaphrodite bull, whose udder secreted milk, occurred lately in Poland. The animal came into the possession of Mr. Brookes, who procured it near Grodno, in the year 1796, and carried it to St. Petersburg, where it died in the following year; unfortunately, no examination was made after death. While the animal was at St. Petersburg, both Dr. Rogerson and Dr. Rogers had opportunities of examining it with a considerable degree of accuracy; and the following account is taken from their description, with which I have been favoured by Dr. Rogerson, who is now in London. The animal was under the usual size of neat cattle, and is stated by Mr. Brookes to have been about 15 years old; it was in a weakly state, and Mr. Brookes told Dr. Rogerson, that he had much difficulty in making it bear the journey from Grodno, a distance of about 800 miles, and was obliged to give it the most nourishing diet, which was principally ground malt. In its general appearance, the male character predominated, particularly in the head, horns, neck, shoulders, and organs of generation. The flanks and hind-quarters had a greater resemblance to those of the cow. The penis was of the ordinary size, and had the common appearance; the preputium had the tuft of hair at the orifice, as in the bull. The urine was ejected through the penis. It had an udder in the common situation, which was smaller, and more globular, that that of the cow, and its teats were less pendulous. Dr. Rogers found one of the testicles, by pressing on the udder, but was unable to detect the other. There was an external orifice in the situation of the vagina, but so small as not to appear capable of receiving more than the point of the fore finger. Dr. Rogerson thinks, from its appearance, that it never could have admitted the male, much less have brought forth a calf, which had been asserted, but without any proof whatever. Mr. Brookes, who is now in this country, admits that it had never received the male, or brought forth young, while in his possession; but asserts that it had several times covered the female, and had begot 5 calves. This assertion, Dr. Rogerson thinks highly deserving of credit. The udder contained milk capable of giving cream, but the quantity was very small. When Dr. Rogerson was present, only 1 oz. could be procured; but he was told that at other times a tea-cupful was drawn. Mr. Brookes states, that he saw an English pint milked at one time.

As the teats of the bull are in the same situation as those of the cow, it became an object of inquiry, whether any males of that tribe of animals, that were not hermaphrodites, had ever been known to give milk; and I find there are two instances recorded in the Phil. Trans. of wethers having given suck.

One is on the testimony of the Rev. Dr. Doddridge, who states that a lamb was nourished by the milk, and when the teats were pressed, milk came out *. The other is on the testimony of Mr. Kirke, of Cookridge, in Yorkshire. He mentions, that Sir William Lowther had a lamb suckled by a wether. The lamb sucked during the whole summer, and after it was weaned, milk could be pressed from the teats: each side of the udder was the size of a hen's egg. This account is dated Sept. 28th, 1694. He gives a 2d relation, in Nov., stating that the udder was reduced in size, but there was still some milk in it, and no appearance of the animal being an hermaphrodite †. A case is also recorded in the Phil Trans., vol. 41, p. 813, of a man giving suck to a child 2 months old; this however is not stated with sufficient accuracy to allow any stress being laid on it, though it would have been improper not to have noticed it in this place.

In considering the influence of the testicles on the constitution of the male, which is rendered so evident by contrasting it with those cases in which the testicles are imperfect, it leads to a supposition, that the ovaria may have a similar in-

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* Phil. Trans. vol. 45, p. 502.—Orig.

† Phil. Trans. vol. 18, p. 263.—Orig.

fluence on the constitution of the female; and that, when the ovaria are imperfectly formed, or when testicles are substituted for them, though the external parts are decidedly female, the person may grow up, deprived of that feminine character which the constitution would have acquired, if the ovaria had been capable of producing their influence on the body. To this cause may be attributed the unnatural bias which some women have shown, to pass through life in the character of men. The circumstance of some women, after the time of breeding is over, at which period the influence of the ovaria may be considered as lost to the constitution, approaching nearer to the male in appearance, and acquiring a beard; also the female pheasant and duck *, in several instances, at the same period of their life, acquiring the feathers which distinguish the male, so as to be mistaken for males, is in favour of such an opinion.

The histories of monsters which have superfluous parts, as that of the child with the double head †, and all others of the same kind, lead to the opinion of 2 or more fœtuses having been contained in 1 ovum, similar to 2 yolks in 1 egg; and that, from some circumstances having taken place in utero, certain parts of 1 of the fœtuses were prevented from coming to perfection, and were absorbed; while those that remained became connected to the other fœtus. When monsters are imperfect, there is no difficulty in accounting for any organ, or other structure, not having been completely formed; but, that the ovaria should be wanting, and their place supplied by testicles, is not to be explained on the same principle. The testicles being substituted for the ovaria, and the ovaria themselves entirely wanting, is probably the most curious circumstance that is met with, in the structure of these hermaphrodites; and as many important discoveries in the animal economy have been suggested from the examination of monstrous productions, it naturally leads to the inquiry, whether there is any thing in the original formation of the parts, which can account for so strange an occurrence. The only mode in which it can be explained, as far as I am able to judge, appears to be the following. By supposing the ovum, previous to impregnation, to have no distinction, but to be so formed as to be equally fitted to become a male or female fœtus; and that is

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* The following account of a duck of this kind was sent me by Mr. Rumball, surgeon, at Abingdon, in Berkshire. The duck was bred by Mr. Cator, of Norwood, in Surrey, in the year 1781. It continued to lay, and to hatch its young, till the year 1789; when the curled feathers, peculiar to the drake, made their appearance in its tail. From this period, she not only left off laying, but frequently attempted to tread the other ducks, both in the water, and on the ground; and they courted her in return. This was particularly observed on the 19th of August, 1791, when she trod a duck in the water, and fell off on her side, as drakes usually do; and they both began washing themselves immediately after, as is customary on these occasions. She never afterwards suffered a drake to come near her. Though the plumage changed, the voice continued the same, which is very different from that of the drake. This circumstance first attracted Mr. Rumball's notice, and made him doubt of its being really a drake. On the 14th of Oct. 1793, at the request of Mr. Rumball, this duck was sent to Mr. Hunter, and died on the 18th, 2 days after Mr. Hunter's death. On examination, the organs of generation were those of a perfect duck. The skin is stuffed, and preserved in Mr. Hunter's collection.—Orig.

† Phil. Trans. vol. 80, p. 296, and vol. 89, p. 28.—Orig.

the process of impregnation which marks the distinction, and conduces to produce either testicles or ovaria, out of the same materials. The following circumstances are in favour of this opinion. The testicles and ovaria are formed originally in the same situation, though the testicles, even before the fœtus has advanced to the 8th month, are to change their situation, to a part at a considerable distance. The clitoris, in fœtuses under 4 months, is so large as to be often mistaken for a penis. Preparations to show the size of the clitoris at this age are preserved in Mr. Hunter's collection; and M. Ferrien mentions it, with a view to explain an erroneous opinion which prevailed in France *, that the greater number of miscarriages between 3 and 4 months, have been remarked to be males; which mistake arose from the above circumstance. The clitoris originally appears therefore equally fitted to be a clitoris or penis, as it may be influenced by the ovarium or testicle.

In considering this subject, it is curious to observe the number of secondary parts, which appear so contrived that they may be equally adapted to the organs of the male or female. In those quadrupeds whose females have mammæ inguinales, the males have also teats in the same situation; so that the same bag which contains the testicles of the male, is adapted to the mammæ of the female. In the human species, which have the mammæ pectorales, the scrotum of the male serves the purpose of forming the labia pudendi of the female, and the preputium makes the nymphæ. The male has pectoral nipples, as well as the female; and in many infants, milk, or a fluid analogous to it, is secreted; which proves the existence of a glandular structure under the nipple. This circumstance, when added to the instances already related, of an hermaphrodite bull, and of wethers giving suck, affords a strong presumption that the rudiments of the mammæ exist in the male, and in some few instances have been brought to perfection, either by an original mixture of organs, early emasculation, or other changes with which we are at present unacquainted.

If it is allowed that the sex is impressed on the ovum at the time of impregnation, it may in some measure account for the free-martin occurring when 2 young are to be impressed with different sexes, at one impregnation; which must be a less simple operation, and therefore more liable to a partial failure, than when 2 or any greater number of ova are impressed with the same sex. It may also account for twins being most commonly of the same sex; and when they are of different sexes, it leads us to inquire whether the female, when grown up, has not in some instances less of the true female character than other women, and is not incapable of having children. It is curious, and in some measure to the purpose, that in some countries, nurses and midwives have a prejudice, that such twins seldom breed. This view of the subject throws some light on those cases where the testicles are substituted for the ovaria; since, whenever the impregnation fails in stamping the ovum with a perfect impression of either sex, the part formed will neither be an

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* Mem. de l'Acad. Royal des Sciences de Paris, 1767, p. 330.—Orig.

ovarium nor a testicle, sometimes bearing a greater resemblance to the one, sometimes to the other; and may according to circumstances, either remain in the natural situation proper to the testicle, whether it is the scrotum of the male, or the labia pudendi of the female.

XI. An Inquiry concerning the Weight ascribed to Heat. By Benjamin Count of Rumford, F. R. S., M. R. I. A., &c. p. 179.

The various experiments which have hitherto been made with a view to determine the question so long agitated, relative to the weight which has been supposed to be gained, or to be lost, by bodies on their being heated, are of a nature so very delicate; and are liable to so many errors, not only on account of the imperfections of the instruments made use of, but also of those, much more difficult to appreciate, arising from the vertical currents in the atmosphere, caused by the hot or the cold body which is placed in the balance, that it is not at all surprizing that opinions have been so much divided, relative to a fact so very difficult to ascertain. It is a considerable time since I first began to meditate on this subject, and I have made many experiments with a view to its investigation; and in these experiments, I have taken all those precautions to avoid errors, which a knowledge of the various sources of them, and an earnest desire to determine a fact which I conceived to be of importance to be known, could inspire; but though all my researches tended to convince me more and more, that a body acquires no additional weight on being heated, or rather, that heat has no effect whatever on the weights of bodies, I have been so sensible of the delicacy of the inquiry, that I was for a long time afraid to form a decided opinion on the subject.

Being much struck with the experiments recorded in the Transactions of the R. S. vol. 75, made by Dr. Fordyce, on the weight said to be acquired by water on being frozen; and being possessed of an excellent balance, belonging to his most Serene Highness the Elector Palatine Duke of Bavaria; early in the beginning of the winter of the year 1787,—as soon as the cold was sufficiently intense for my purpose, I began to repeat those experiments, in order to convince myself whether the very extraordinary fact related might be depended on; and with a view to removing, as far as was in my power, every source of error and deception, I proceeded in the following manner. Having provided a number of glass bottles, of the form and size of what in England is called a Florence flask, blown as thin as possible, and of the same shape and dimensions, I chose out from among them 2, which, after using every method I could imagine of comparing them together, appeared to be so much alike as hardly to be distinguished.

Into one of these bottles, which I shall call A, I put 4107.86 grains Troy of pure distilled water, which filled it about half full; and into the other B, I put an equal weight of weak spirit of wine; and, sealing both the bottles hermetically, and washing them, and wiping them perfectly clean and dry on the outside, I suspended them to the arms of the balance, and placed the balance in a large room, which for some weeks had been regularly heated every day by a German stove, and in which the air was kept up to the temperature of 61° of Fahrenheit's thermometer, with very little variation. Having suffered the bottles, with their contents, to remain in this situation till I conceived they must have acquired the temperature of the circumambient air, I wiped them afresh, with a very clean dry cambric handkerchief, and brought them into the most exact equilibrium possible, by attaching a small piece of very fine silver wire to the arm of the balance to which the bottle which was the lightest was suspended. Having suffered the apparatus to remain in this situation about 12 hours longer, and finding no alteration in the relative weights of the bottles, they continuing all this time to be in the most perfect equilibrium, I now removed them into a large uninhabited room, fronting the north, in which the air, which was very quiet, was at the temperature of 29°, F.; the air without doors being at the same time at 27°; and going out of the room, and locking the door after me, I suffered the bottles to remain 48 hours undisturbed, in this cold situation, attached to the arms of the balance as before.

At the expiration of that time, I entered the room, using the utmost caution not to disturb the balance, when, to my great surprize, I found that the bottle A very sensibly preponderated. The water which this bottle contained was completely frozen into one solid body of ice; but the spirit of wine, in the bottle B, showed no signs of freezing. I now very cautiously restored the equilibrium, by adding small pieces of the very fine wire of which gold lace is made, to the arm of the balance to which the bottle B was suspended, when I found that the bottle A had augmented its weight by $\frac{1}{35904}$ part of its whole weight at the beginning of the experiment; the weight of the bottle with its contents having been 4811.23 grains Troy, (the bottle weighing 703.37 grains, and the water 4107.86 grains), and it requiring now $\frac{134}{1000}$ parts of a grain, added to the opposite arm of the balance, to counterbalance it.

Having had occasion just at this time to write to my friend, Sir Charles Blagden, on another subject, I added a postscript to my letter, giving him a short account of this experiment, and telling him how "very contrary to my expectation" the result of it had turned out; but I soon after found that I had been too hasty in my communication. Sir Charles, in his answer to my letter, expressed doubts respecting the fact; but, before his letter had reached me, I had learned from my own experience, how very dangerous it is, in philosophical investigations, to draw conclusions from single experiments.

Having removed the balance, with the 2 bottles attached to it, from the cold into the warm room, which still remained at the temperature of 61°, the ice in the bottle A gradually thawed; and, being at length totally reduced to water, and this water having acquired the temperature of the surrounding air, the 2 bottles, after being wiped perfectly clean and dry, were found to weigh as at the beginning of the experiment, before the water was frozen. This experiment being repeated, gave nearly the same result, the water appearing, when frozen, to be heavier than in its fluid state; but some irregularity in the manner in which the water lost the additional weight which it had appeared to acquire on being frozen, when it was afterwards thawed, as also a sensible difference in the quantities of weight apparently acquired in the different experiments, led me to suspect, that the experiment could not be depended on for deciding the fact in question; I therefore repeated it, with some variations and improvements; but, before I give an account of my further investigations relative to this subject, it may not be amiss to mention the method I pursued for discovering whether the appearances mentioned in the foregoing experiments might not arise from the imperfections of my balance; and it may likewise be proper to give an account, in this place, of an intermediate experiment which I made, with a view to discover, by a shorter route, and in a manner less exceptionable than that above-mentioned, whether bodies actually lose, or acquire, any weight, on acquiring an additional quantity of latent heat.

My suspicions respecting the accuracy of the balance arose from a knowledge, which I acquired from the maker of it, of the manner in which it was constructed. The 3 principal points of the balance having been determined, as nearly as possible, by measurement, the axes of motion were firmly fixed in their places, in a right line, and the beam being afterwards finished, and its 2 arms brought to be in equilibrio, the balance was proved by suspending weights, which before were known to be exactly equal, to the ends of its arms. If with these weights the balance remained in equilibrio, it was considered as a proof that the beam was just; but if one arm was found to preponderate, the other was gradually lengthened, by beating it on an anvil, till the difference of the lengths of the arms was reduced to nothing, or till equal weights, suspended to the 2 arms, remained in equilibrio; care being taken before each trial to bring the 2 ends of the beam to be in equilibrio, by reducing, with the file, the arm which had been lengthened.

Though, in this method of constructing balances, the most perfect equality in the lengths of the arms may be obtained, and consequently the greatest possible accuracy, when used at a time when the temperature of the air is the same as when the balance was made, yet as it may happen, that in order to bring the arms of the balance to be of the same length, one of them may be much more hammered than the other, I suspected it might be possible that the texture of the metal forming the 2 arms might be rendered so far different, by this operation, as to occasion a difference in their expansions with heat; and that this difference might occasion a sensible error in the balance when, being charged with a great weight, it should be exposed to a considerable change of temperature.

To determine whether the apparent augmentation of weight, in the experiments above related, arose in any degree from this cause, I had only to repeat the experiment, causing the 2 bottles A and B to change places on the arms of the balance; but as I had already found a sensible difference in the results of different repetitions of the same experiment, made as nearly as possible under the same circumstances, and as it was above all things of importance to ascertain the accuracy of my balance, I preferred making a particular experiment for that purpose. My first idea was, to suspend to the arms of the balance, by very fine wires, 2 equal globes of glass, filled with mercury, and, suffering them to remain in my room till they should have acquired the known temperature of the air in it, to have removed them afterward into the cold, and to have seen if they still remained in equilibrio, under such difference of temperature; but, considering the obstinacy with which moisture adheres to the surface of glass, and being afraid that, somehow or other, notwithstanding all my precautions, one of the globes might acquire or retain more of it than the other, and that by that means its apparent weight might be increased; and having found by a former experiment, of which I have already had the honour of communicating an account to the R. S., that the gilt surfaces of metals do not attract moisture; instead of the glass globes filled with mercury, I made use of 2 equal solid globes of brass, well gilt and burnished, which I suspended to the arms of the balance, by fine gold wires.

These globes, which weighed 4975 grains each, being wiped perfectly clean, and having acquired the temperature (61°) of my room, in which they were exposed more than 24 hours, were brought into the most scrupulous equilibrium, and were then removed, attached to the arms of the balance, into a room in which the air was at the temperature of 26°, where they were left all night. The result of this trial furnished the most satisfactory proof of the accuracy of the balance; for, on entering the room, I found the equilibrium as perfect as at the beginning of the experiment. Having thus removed my doubts respecting the accuracy of my balance, I now resumed my investigations relative to the augmentation of weight which fluids have been said to acquire on being congealed.

In the experiments which I had made, I had, as I then imagined, guarded as much as possible against every source of error and deception. The bottles being of the same size, neither any occasional alteration in the pressure of the atmosphere during the experiment, nor the necessary and unavoidable difference in the densities of the air in the hot and in the cold rooms in which they were weighed, could affect their apparent weights; and their shapes and their quantities of surface being the same, and as they remained for such a considerable length of time in the heat and cold to which they were exposed, I flattered myself that the quantities of moisture remaining attached to their surfaces, could not be so different as sensibly to affect the results of the experiments. But, in regard to this last circumstance, I afterwards found reason to conclude that my opinion was erroneous.

Admitting the fact stated by Dr. Fordyce, and which my experiments had hitherto rather tended to corroborate than to contradict, I could not conceive any other cause for the augmentation of the apparent weight of water, on its being frozen, than the loss of so great a proportion of its latent heat as that fluid is known to evolve when it congeals; and I concluded, that if the loss of latent heat added to the weight of one body, it must of necessity produce the same effect on another, and consequently, that the augmentation of the quantity of latent heat must, in all bodies, and in all cases, diminish their apparent weights. To determine whether this is actually the case or not, I made the following experiment. Having provided 2 bottles, as nearly alike as possible, and in all respects similar to those made use of in the experiments above-mentioned, into one of them I put 4012.46 grains of water, and into the other an equal weight of mercury; and, sealing them hermetically, and suspending them to the arms of the balance, I suffered them to acquire the temperature of my room, 61°; then, bringing them into a perfect equilibrium with each other, I removed them into a room in which the air was at the temperature of 34°, where they remained 24 hours. But there was not the least appearance of either of them acquiring, or losing, any weight.

Here it is very certain, that the quantity of heat lost by the water, must have been very considerably greater than that lost by the mercury; the specific quantities of latent heat in water and in mercury, having been determined to be to each other as 1000 to 33; but this difference in the quantities of heat lost, produced no sensible difference on the weights of the fluids in question. Had any difference of weight really existed, had it been no more than one millionth part of the weight of either of the fluids, I should certainly have discovered it; and had it amounted to so much as $\frac{1}{7000000}$ part of that weight, I should have been able to have measured it; so sensible, and so very accurate, is the balance which I used in these experiments.

I was now much confirmed in my suspicions, that the apparent augmentation of the weight of the water on its being frozen, in the experiments before related, arose from some accidental cause; but I was not able to conceive what that cause could possibly be,—unless it were, either a greater quantity of moisture attached to the external surface of the bottle which contained the water, than to the surface of that containing the spirits of wine,—or some vertical current or currents of air, caused by the bottles, or one of them not being exactly of the temperature of the surrounding atmosphere. Though I had foreseen, and, as I thought, guarded sufficiently against these accidents, by making use of bottles of the same size and form, and which were blown of the same kind of glass, and at the same time; and by suffering the bottles, in the experiments, to remain for so considerable a length of time exposed to the different degrees of heat and of cold, which alternately they were made to acquire; yet, as I did not know the relative conducting powers of ice and of spirit of wine with respect to heat; or, in other words, the degrees of facility or difficulty with which they acquire the temperature of the medium in which they are exposed; or the time taken up in that operation; and consequently was not absolutely certain as to the equality of the temperatures of the contents of the bottles at the time when their weights were compared, I determined now to repeat the experiments, with such variations as should put the matter in question out of all doubt. I was the more anxious to assure myself of the real temperatures of the bottles and of their contents, as any difference in their temperatures might vitiate the experiment, not only by causing unequal currents in the air, but also by causing, at the same time, a greater or less quantity of moisture to remain attached to the glass.

To remedy these evils, and also to render the experiment more striking and satisfactory in other respects, I proceeded in the following manner. Having provided 3 bottles A, B, and C, as nearly alike as possible, and resembling in all respects those already described; into the first, A, I put 4214.28 grains of water, and a small thermometer, made on purpose for the experiment, and suspended in the bottle in such a manner that its bulb remained in the middle of the mass of water; into the 2d bottle, B, I put a like weight of spirit of wine, with a like thermometer; and into the bottle C I put an equal weight of mercury. These bottles, being all hermetically sealed, were placed in a large room, in a corner far removed from the doors and windows, and where the air appeared to be perfectly quiet; and being suffered to remain in this situation more than 24 hours, the heat of the room (61°) being kept up all that time with as little variation as possible, and the contents of the bottles A and B appearing, by their inclosed thermometers, to be exactly at the same temperature, the bottles were all wiped with a very clean dry cambric handkerchief; and being afterwards suffered to remain exposed to the free air of the room a couple of hours longer, in order that any inequalities in the quantities of heat, or of the moisture attached to their surfaces, which might have been occasioned by the wiping, might be corrected by the operation of the atmosphere by which they were surrounded, they were all weighed, and were brought into the most exact equilibrium with each other, by means of small pieces of very fine silver wire, attached to the necks of those of the bottles which were the lightest.

This being done, the bottles were all removed into a room in which the air was at 30°, where they were suffered to remain, perfectly at rest and undisturbed, 48 hours; the bottles A and B being suspended to the arms of the balance, and the bottle C suspended, at an equal height, to the arm of a stand constructed for that purpose, and placed as near the balance as possible, and a very sensible thermometer suspended by the side of it. At the end of 48 hours, during which time the apparatus was left in this situation, I entered the room, opening the door very gently, for fear of disturbing the balance; when I had the pleasure to find the 3 thermometers, viz. that in the bottle A, which was now inclosed in a solid cake of ice, that in the bottle B, and that suspended in the open air of the room, all standing at the same point, 29° F, and the bottles A and B remaining in the most perfect equilibrium. To assure myself that the play of the balance was free, I now approached it very gently, and caused it to vibrate; and I had the satisfaction to find, not only that it moved with the utmost freedom, but also, when its vibration ceased, that it rested precisely at the point from which it had set out.

I now removed the bottle B from the balance, and put the bottle C in its place; and I found that that likewise remained of the same apparent weight as at the beginning of the experiment, being in the same perfect equilibrium with the bottle A as at first. I afterwards removed the whole apparatus into a warm room, and causing the ice in the bottle A to thaw, and suffering the 3 bottles to remain till they and their contents had acquired the exact temperature of the surrounding air, I wiped them very clean, and comparing them together, I found their weights remained unaltered. This experiment I afterwards repeated several times, and always with precisely the same result; the water, in no instance, appearing to gain, or to lose, the least weight, on being frozen, or on being thawed; neither were the relative weights of the fluids in either of the other bottles in the least changed, by the various degrees of heat, and of cold, to which they were exposed. If the bottle were weighed at a time when their contents were not precisely of the same temperature, they would frequently appear to have gained, or to have lost, something of their weights;—but this doubtless arose from the vertical currents which they caused in the atmosphere, on being heated or cooled in it; or to unequal quantities of moisture attached to the surfaces of the bottles;—or to both these causes operating together.

As I knew that the conducting power of mercury, with respect to heat, was considerably greater than either that of water, or that of spirit of wine, while its capacity for receiving heat is much less than that of either of them, I did not think it necessary to inclose a thermometer in the bottle C, which contained the mercury; for it was evident, that when the contents of the two other bottles should appear, by their thermometers, to have arrived at the temperature of the medium in which they were exposed, the contents of the bottle C could not fail to have acquired it also, and even to have arrived at it before them; for the time taken up in the heating or in the cooling of any body is, cæteris paribus, as the capacity of the body to receive and retain heat directly, and as its conducting power inversely. The bottles were suspended to the balance by silver wires, about 2 inches long, with hooks at the ends of them; and, in removing and changing the bottles, I took care not to touch the glass. I likewise avoided on all occasions, and particularly in the cold room, coming near the balance with my breath, or touching it, or any part of the apparatus, with my naked hands.

Having determined that water does not acquire or lose any weight, on being changed from a state of fluidity to that of ice, and vice versâ, I shall now take my final leave of a subject which has long occupied me, and which has cost me much pains and trouble; being fully convinced, from the results of the above-mentioned experiments, that if heat be in fact a substance, or matter,—a fluid sui generis, as has been supposed,—which, passing from one body to another, and being accumulated, is the immediate cause of the phenomena we observe in heated bodies, (of which, however, I cannot help entertaining doubts), it must be something so infinitely rare, even in its most condensed state, as to baffle all our attempts to discover its gravity. And if the opinion which has been adopted by many of our ablest philosophers, that heat is nothing more than an intestine vibratory motion of the constituent parts of heated bodies, should be well founded, it is clear that the weights of bodies can in no wise be affected by such motion. It is, no doubt, on the supposition that heat is a substance distinct from the heated body, and which is accumulated in it; that all the experiments which have been undertaken, with a view to determine the weight which bodies have been supposed to gain, or to lose, on being heated or cooled, have been made; and on this supposition, but without however adopting it entirely, as I do not conceive it to be sufficiently proved, all my researches have been directed.

The experiments with water, and with ice, were made in a manner which I take to be perfectly unexceptionable;—in which no foreign cause whatever could affect the results of them;—and the quantity of heat which water is known to part with, on being frozen, is so considerable, that if this loss has no effect on its apparent weight, it may be presumed that we shall never be able to contrive an experiment by which we render the weight of heat sensible. Water, on being frozen, has been found to lose a quantity of heat amounting to 140 degrees of Fahrenheit's thermometer; or, which is the same thing, the heat which a given quantity of water, previously cooled to the temperature of freezing, actually loses, on being changed to ice, if it were to be imbibed and retained by an equal quantity of water, at the given temperature, that of freezing, would heat it 140 degrees, or would raise it to the temperature of (32° + 140) 162° of Fahrenheit's thermometer, which is only 60° short of that of boiling water; consequently, any given quantity of water, at the temperature of freezing, on being actually frozen, loses almost as much heat as, added to it, would be sufficient to make it boil. It is clear, therefore, that the difference in the quantities of heat contained by the water in its fluid state, and heated to the temperature of 61° F, and by the ice, in the experiments before mentioned, was at least nearly equal to that between water in a state of boiling, and the same at the temperature of freezing.

But this quantity of heat will appear much more considerable, when we consider the great capacity of water to contain heat, and the great apparent effect which the heat that water loses on being frozen would produce, were to be imbibed by, or communicated to any body whose power of receiving and retaining heat is much less. The capacity of water to receive and retain heat,—or what has been called its specific quantity of latent heat,—has been found to be that of gold as 1000 to 50,—or as 20 to 1; consequently, the heat which any given quantity of water loses on being frozen,—were it to be communicated to an equal weight of gold, at the temperature of freezing, the gold, instead of being heated 162°, would be heated 140 × 20 = 2800°, or would be raised to a bright red heat.

It appears therefore to be clearly proved, by my experiments, that a quantity of heat equal to that which 4214 grs., or about 9¾ oz. of gold, would require to heat it from the temperature of freezing water to be red-hot, has no sensible effect on a balance capable of indicating so small a variation of weight as that of $\frac{1}{1000000}$ part of the body in question; and if the weight of gold is neither augmented nor lessened by one millionth part, on being heated from the point of freezing water to that of a bright red heat, I think we may very safely conclude, that all attempts to discover any effect of heat on the apparent weights of bodies, will be fruitless.

XII. Experiments on the Fecundation of Vegetables. By Thomas Andrew Knight, Esq. p. 195.

In this paper Mr. K. gives an account of some experiments on plants, which prove the existence of super-fœtation in the vegetable world, and which seem likely to conduce to improvements in agriculture.

The breeders of animals, (says Mr. K.,) have very long entertained an opinion that considerable advantages are obtained by breeding from males and females not related to each other. Though this opinion has lately been controverted, the number of its opposers has gradually diminished; and I can speak from my own observation and experience, that animals degenerate, in size at least, on the same pasture, and in other respects under the same management, when this process of crossing the breed is neglected. The close analogy between the animal and vegetable world, and the sexual system equally pervading both, induced me to suppose, that similar means might be productive of similar effects in each; and the event has, I think, fully justified this opinion. The principal object I had in view, was to obtain new and improved varieties of the apple, to supply the place of those which have become diseased and unproductive, by having been cultivated beyond the period which nature appears to have assigned to their existence. But, as I foresaw that several years must elapse, before the success or failure of this process could possibly be ascertained, I wished, in the interval, to see what would be its effects on annual plants. Among these, none appeared so well calculated to answer my purpose as the common pea; not only because I could obtain many varieties of this plant, of different forms, sizes, and colours; but also, because the structure of its blossom, by preventing the ingress of insects and adventitious farina, has rendered its varieties remarkably permanent. I had a kind growing in my garden, which, having been long cultivated in the same soil, had ceased to be productive, and did not appear to recover the whole of its former vigour, when removed to a soil of a somewhat different quality; on this, my first experiment, in 1787, was made. Having opened a dozen of its immature blossoms, I destroyed the male parts, taking great care not to injure the female ones; and, a few days afterwards, when the blossoms appeared mature, I introduced the farina of a very large and luxuriant grey pea into one half of the blossoms, leaving the other half as they were. The pods of each grew equally well; but I soon perceived, that in those into whose blossoms the farina had not been introduced, the seeds remained nearly as they were before the blossoms expanded, and in that state they withered. Those in the other pods attained maturity, but were not in any sensible degree different from those afforded by other plants of the same variety; owing, I imagine, to the external covering of the seed, as I have found in other plants, being furnished entirely by the female. In the succeeding spring, however, the difference became extremely obvious; for the plants from them rose with excessive luxuriance, and the colour of their leaves and stems clearly indicated, that they had all exchanged their whiteness for the colour of the male parent: the seeds produced in autumn were dark gray. By introducing the farina of another white variety, or in some instances by simple culture, I found this colour was easily discharged, and a numerous variety of new kinds produced, many of which were, in size, and in every other respect, much superior to the original white kind, and grew with excessive luxuriance, some of them attaining the height of more than 12 feet. I had frequent occasion to observe, in this plant, a stronger tendency to produce purple blossoms, and coloured seeds, than white ones; for when I introduced the farina of a purple blossom into a white one, the whole of the seeds in the succeeding year became coloured; but when I endeavoured to discharge this colour, by reversing the process, a part only of them afforded plants with white blossoms; this part sometimes occupying one end of the pod, and being at other times irregularly intermixed with those which, when sown, retained their colour. It may perhaps be supposed, that something might depend on the quantity of farina employed; but I never could discover in this, or in any other experiment in which superfœtation did not take place, that the largest or smallest quantity of farina afforded any difference in the effect produced.

The dissimilarity I observed in the offspring afforded by different kinds of farina, in these experiments, pointed out an easy method of ascertaining whether superfœtation, the existence of which has been admitted among animals, could also take place in the vegetable world. For, as the offspring of a white pea is always white, unless the farina of a coloured kind be introduced into the blossom, and as the colour of the gray one is always transferred to its offspring, though the female be white, it readily occurred, that if the farina of both were mingled, or applied at the same moment, the offspring of each could be easily distinguished. My first experiment was not altogether successful; for the offspring of 5 pods, the whole which escaped the birds, received their colour from the coloured male. There was however a strong resemblance to the other male, in the growth and character of more than one of the plants; and the seeds of several, in the autumn, very closely resembled it in every thing but colour. In this experiment, I used the farina of a white pea, which possessed the remarkable property of shrivelling excessively when ripe; and in the 2d year I obtained white seeds, from the gray ones above-mentioned, perfectly similar to it. I am strongly disposed to believe, that the seeds were here of common parentage; but I do not conceive myself to be in possession of facts sufficient to enable me to speak with decision on this question. If however the female afford the first organized atom, and the farina act only as a stimulus, it appears by no means impossible, that the explosion of 2 vesicles of farina, at the same moment, taken from different plants, may afford seeds, as I have supposed, of common parentage; and, as I am unable to discover any source of inaccuracy in this experiment, I must believe this to have happened.

Another species of superfœtation, if I have justly applied that term to a process in which 1 seed appears to have been the offspring of 2 males, has occurred to me so often, as to remove all possibility of doubt as to its existence. In 1797, the year after I had seen the result of the last-mentioned experiment, having prepared a great many white blossoms, I introduced the farina of a white and that of a gray pea, nearly at the same moment, into each; and as, in the last year the character of the coloured male had prevailed, I used its farina more sparingly than that of the white one; and now almost every pod afforded plants of different colours. The majority however were white; but the characters of the 2 kinds were not sufficiently distinct to allow me to judge with precision, whether any of the seeds produced were of common parentage or not. In the last year, I was more fortunate: having prepared blossoms of the little early frame pea, I introduced its own farina, and immediately afterwards that of a very large and late gray kind, and I sowed the seeds thus obtained in the end of the last summer. Many of them retained the colour and character of the small early pea, not in the slightest degree altered, and blossomed before they were 18 inches high; while others, taken from the same pods, whose colour was changed, grew to the height of more than 4 feet, and were killed by the frost, before any blossoms appeared. It is evident, that in these instances superfœtation took place; and it is equally evident, that the seeds were not all of common parentage. Should subsequent experience evince, that a single plant may be the offspring of 2 males, the analogy between animal and vegetable nature may induce some curious conjecture, relative to the process of generation in the animal world.

In the course of the preceding experiments, I could never observe that the character, either of the male or female, in this plant, at all preponderated in the offspring; but, as this point appeared interesting, I made a few trials to ascertain it. And, as the foregoing observations had occurred in experiments made principally to obtain new and improved varieties of the pea, for garden culture, I chose, for a similar purpose, the more hardy varieties usually sown in the fields. By introducing the farina of the largest and most luxuriant kinds into the blossoms of the most diminutive, and by reversing this process, I found that the powers of the male and female, in their effects on the offspring, are exactly equal. The vigour of the growth, the size of the seeds produced, and the season of maturity, were the same, though the one was a very early, and the other a late variety. I had, in this experiment, a striking instance of the stimulative effects of crossing the breeds; for the smallest variety, whose height rarely exceeded 2 feet, was increased to 6 feet; while the height of the large and luxuriant kind was very little diminished. By this process it is evident, that any number of new varieties may be obtained; and it is highly probable, that many of these will be found better calculated to correct the defects of different soils and situations, than any we have at present; for I imagine that all we now possess, have in a great measure been the produce of accident; and it will rarely happen, in this or any other case, that accident has done all that art will be found able to accomplish.

The success of my endeavours to produce improved varieties of the pea, induced me to try some experiments on wheat; but these did not succeed to my expectations. I readily obtained as many varieties as I wished, by merely sowing the different kinds together; for the structure of the blossom of this plant, unlike that of the pea, freely admits the ingress of adventitious farina, and is thence very liable to sport in varieties. Some of those I obtained very excellent; others very bad; and none of them permanent. By separating the best varieties, a most abundant crop was produced; but its quality was not quite equal to the quantity, and all the discarded varieties again made their appearance. It appeared to be an extraordinary circumstance, that in the years 1795 and 1796, when almost the whole crop of corn in the island was blighted, the varieties thus obtained, and these only, escaped, in this neighbourhood, though sown in several different soils and situations.

My success on the apple, as far as long experience and attention have enabled me to judge from the cultivated appearance of trees which have not yet borne fruit, has been fully equal to my hopes. But, as the improvement of the fruit was the first object of my attention, no probable means of improvement, either from soil or aspect, were neglected. The plants however which I obtained from my efforts to unite the good qualities of 2 kinds of apple, seem to possess the greatest health and luxuriance of growth, as well as the most promising appearance in other respects. In some of these, the character of the male appears to prevail; in others, that of the female; and in others, both appear blended, or neither is distinguishable. These variations, which were often observable in the seeds taken from a single apple, evidently arise from the want of permanence in the character of this fruit, when raised from seed.

The results of similar experiments on another fruit, the grape, were nearly the same as of those on the apple, except that, by mingling the farina of a black and a white grape, just as the blossoms of the latter were expanding, I sometimes obtained plants, from the same berry, so dissimilar, that I had good reason to believe them the produce of superfœtation. By taking off the cups, and destroying the immature male parts, as in the pea, I perfectly succeeded in combining the characters of different varieties of this fruit, as far as the changes of form and autumnal tints, in the leaves of the offspring, will allow me to judge.

Many experiments, of the same kind, were tried on other plants; but it is sufficient to say that all tended to evince, that improved varieties of every fruit and esculent plant may be obtained by this process, and that nature intended that a sexual intercourse should take place between neighbouring plants of the same species. The probability of this will I think be apparent, when we take a view of the variety of methods which nature has taken to disperse the farina, even of those plants in which it has placed the male and female parts within the same empalement. It is often scattered by an elastic exertion of the filaments which support it, on the first opening of the blossom; and its excessive lightness renders it capable of being carried to a great distance by the wind. Its position within the blossom, is generally well adapted to place it on the bodies of insects; and the villous coat of the numerous family of bees is not less well calculated to carry it. I have frequently observed, with great pleasure, the dispersion of the farina of some of the grasses, when the sun had just risen in a dewy morning. It seemed to be impelled from the plant with considerable force; and, being blue, was easily visible, and very strongly resembled, in appearance, the explosion of a grain of gunpowder. An examination of the structure of the blossoms of many plants, will immediately point out, that nature has something more in view, than that its own proper males should fecundate each blossom; for the means it employs are always those best calculated to answer the intended purpose. But the farina is often so placed, that it can never reach the summit of the pointal, unless by adventitious means; and many trials have convinced me that it has no action on any other part of it. In promoting this sexual intercourse between neighbouring plants of the same species, nature appears to have an important purpose in view; for, independent of its stimulative power, this intercourse certainly tends to confine within more narrow limits, those variations which accidental richness or poverty of soil usually produces. It may be objected, by those who admit the existence of vegetable mules, that, under this extensive intercourse, these must have been more numerous; but my total want of success, in many endeavours, to produce a single mule plant, makes me much disposed to believe that hybrid plants have been mistaken for mules; and to doubt, with all the deference I feel for the opinions of Linnæus and his illustrious followers, whether nature ever did, or ever will, permit the production of such a monster. The existence of numerous mules in the animal world, between kindred species, is allowed; but nature has here guarded against their production, by impelling every animal to seek its proper mate; and among the feathered tribe, when, from perversion of appetite, sexual intercourse takes place between those of distinct genera,* it has, in some instances at least, rendered the death of the female the inevitable consequence. But in the vegetable world there is not any thing to direct the male to its proper female: its farina is carried, by winds and insects, to plants of every different genus and species; and it therefore appears to me, as vegetable mules certainly are not common, that nature has not permitted them to exist at all.

I cannot dismiss this subject, without expressing my regret, that those who have made the science of botany their study, should have considered the improvement of those vegetables which, in their cultivated state, afford the largest portion of subsistence to mankind and other animals, as little connected with the object of their pursuit. Hence it has happened, that while much attention has been paid to

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* This is said to be the case with the drake and the hen.—Orig.

the improvement of every species of useful animal, the most valuable esculent plants have been almost wholly neglected. But when the extent of the benefit which would arise to the agriculture of the country, from the possession of varieties of plants which, with the same extent of soil and labour, would afford even a small increase of produce, is considered, this subject appears of no inconsiderable importance. The improvement of animals is attended with much expence, and the improved kinds necessarily extend themselves slowly; but a single bushel of improved wheat or peas, may in 10 years be made to afford seed enough to supply the whole island; and a single apple, or other fruit-tree, may within the same time be extended to every garden in it. These considerations have given rise to the foregoing observations; for it was much my wish to have ascertained first, whether in any instance a single plant can be the offspring of 2 male parents. The decision of that question must of necessity have occupied 2 years, and must therefore be left to the test of future experiment.

XIII. Observations on the Different Species of Asiatic Elephants, and their Mode of Dentition. By John Corse, Esq. p. 205.

Before entering on this new and curious subject, it may be proper to premise a few general observations on the various zats, or casts, of the Asiatic elephant, and on the tusks; as the form and size of these give a diversity of appearance, which may be considered as forming varieties of the same species of elephant. Both males and females are divided into 2 casts, by the natives of Bengal, viz. the koomareah* and the merghee†; and this without any regard to the appearance, shape, or size of the tusks in the male, as these serve merely to characterize some varieties in the species. The koomareah is a deep-bodied, strong, compact elephant, with a large trunk, legs short, but thick, in proportion to the size of the animal. The merghee cast, when full grown, is generally taller than the former, but has not so compact a form, nor is he so strong, or so capable of bearing fatigue; his legs are long, he travels fast, has a lighter body, and his trunk is both short and slender, in proportion to his height. A large trunk is always esteemed a great beauty in an elephant; so that the koomareah is preferred, not only for this, but for its superior strength, by which it can undergo greater fatigue, and carry heavier loads, than the merghee.

As there appears however no predilection in any of these elephants to have connection with his own particular kind, from an indiscriminate intercourse several varieties are produced, partaking of the qualities of their respective progenitors. This mixed breed is in greater or less estimation, in proportion as it partakes of the qualities of the koomareah, or merghee cast. A breed from a pure koomareah

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* Koomareah signifies of a princely race; being derived from koomārah, a prince, or king's son.—
† Merghee, properly mrigee, from mrigah, a deer, or hunting, signifies an elephant used in hunting; or it is so called from its slender make.—Orig.

and merghee is termed sunkareah*, or mergha-bauliah†; but a further mixture or crossing of the breed renders it extremely difficult for the hunters to ascertain the variety. Besides the koomareah, merghee, and sunkareah breeds, several varieties are generally to be found in the same herd; but the nearer an elephant approaches to the true koomareah species, the more he is preferred, especially by the natives, and the higher price he will consequently bear. Europeans are not so particular, and will sometimes prefer a merghee female for hunting and riding on, when she is known to have remarkably good paces, and to be of a mild and tractable disposition.

The elephants for the service of the Hon. East India Company, are generally taken in the province of Chittigong and Tiperah; but, from what I have heard, those to the southward of Chittigong, in the Burmagh territories and kingdom of Pegu, are of a superior breed. In confirmation of this opinion, I may observe, that the elephants taken to the south of the Goomty river, which divides the province of Tiperah from east to west, are generally better than those taken to the north of that river; and though elephants are taken at Pilibet, as far north as latitude 29°, in the Vizier of Oude's territories, yet the Vizier, and the officers of his court, give those taken in Chittigong and Tiperah a decided preference, being much larger and stronger than the Pilibet elephant, Till the year 1790, Tiperah was a part of the Chittigong province; and so sensible was the Bengal government of the superiority of the southern elephants, for carrying burdens, enduring fatigue, and being less liable to casualties, that in the late contracts for supplying the army with those useful animals, the contractor was bound not to send any elephant to the military stations, taken north of the Chittigong province.

Hence we may conclude the torrid zone to be the natural clime, and the most favourable for producing the largest, the best, and the hardiest elephant; and that when this animal migrates beyond the tropics, the species degenerates. On the coast of Malabar, elephants are taken as far north as the territories of the Coorgah Rajah; but these are much inferior to the Ceylon elephant, and, from this circumstance, the report of the superiority of the Ceylon elephant to all others has probably originated. Most of the accounts we have had respecting the Asiatic elephant, have been given by gentlemen who resided many years ago on the coast of Malabar or Coromandel; where, at that time, they had but few opportunities of seeing the Chittigong or Pegu elephant.

After premising these general observations, I may here observe, that elephants have 2 tusks, in the upper jaw only; but those in some of the females are so small as not to appear beyond the lip, while in others they are almost as large as in one variety of the male, named mooknah‡. Elephants have no incisores or cutting

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* Sunkareah signifies a mixed breed, from sunkarah, a mixture.——† Mergha-bauliah signifies for the most part merghee; that is, partaking more of their cast than of the koomareah.——‡ Probably from mookh, the mouth or face.—Orig.

teeth; and the grinders are so much alike in males and females, that one description will serve for both. The largest tusks, from which the best ivory is supplied, are taken from that species of male named dauntelah*, in consequence of his large tusks, and whose countenance, from this circumstance, is the most opposite in appearance, to that of the mooknah; which, as before observed, is hardly to be distinguished, by his head, from a female elephant. Though there is a material difference in the appearance of a mooknah and a dauntelah, as well as in the value of the tusks, yet, if they are of the same cast, size, and disposition, and perfect, that is, free from any defect or blemish, there is scarcely any difference in their price.

An elephant is said to be perfect, when his ears are large and rounded, not ragged, or indented at the margin; his eyes of a dark hazle colour, free from specks; the roof of his mouth, and his tongue, without dark or black spots of any considerable size; his trunk large, and his tail long, with a tuft of hair reaching nearly to the ground. There must be 5 nails on each of his fore-feet, and 4 on each of the hind ones; his head well set on, and carried rather high. The arch or curve of his back rising gradually from the shoulder to the middle, and thence descending to the insertion of the tail; and all his joints firm and strong. There are several other points, of less consequence, which are noticed by the natives as well as Europeans. The dauntelah is generally more daring, and less manageable, than the mooknah; for this reason, till the temper and disposition of the two species are ascertained, Europeans will prefer the mooknah; but the natives, who are fond of show, generally take their chance, and prefer the dauntelah; which, when known to be of a mild and gentle disposition, will always be preferred, both by Europeans and natives.

The varieties between the mooknah and dauntelah are considerable, and for these there are appropriate names, according as the form of the tusks varies from the projecting horizontal, but rather elevated, curve of the pullung daunt† of the perfect dauntelah, to the nearly straight tusks of the mooknah, which point directly downwards. When a dauntelah has never had but 1 tusk, and this of the pullung sort, he is said to be a goneish or ganesa‡, and will sell to the Hindoo princes for a very high price, to be kept in state, and worshipped as a divinity. I have seen elephants apparently of this kind; but when accurately examined, the tusk wanting appeared to me to have been lost by accident, so that I cannot say I ever saw a male which had originally only 1 tusk.

A 2d variety of the dauntelah is, when the large tusks point downwards, projecting only a little way beyond the trunk; he is then said to have soor or choor

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* Dauntelah signifies toothy; having large or fine teeth.——† Pullung signifies a bed or cot, and daunt, teeth; and, from the tusks projecting so regularly, and being a little curved and elevated at the extremities, the natives suppose a man might lie on them at his ease, as on a bed.——‡ Ganesa is the name of the Hindoo god of wisdom, who is represented with a head like an elephant's, with only 1 tooth. See Asiatic Researches, vol. 1, art. On the Gods of Greece, Italy, and India.—Orig.

daunt*. A 3d variety is the puttel-dauntee, whose tusks are straight, like those of the mooknah, only much longer, and thicker. A 4th variety is the ankoos-dauntee†, where 1 tusk grows nearly horizontal, like the pulling-daunt, and the other like the puttel-daunt. Besides these, the elephant-keepers notice other varieties, which are less distinct. All these tusks, in the male, are fixed very deep in the upper jaw; and the root or upper part, which is hollow and filled with a core, goes as high as the insertion of the trunk, round the margin of the nasal opening to the throat; which opening is just below the protuberance of the forehead.

Through this opening the elephant breathes, and by its means he sucks up water into his trunk; between it and the roots of the tusks there is only a thin bony plate. The first or milk tusks of an elephant never grow to any size, but are shed between the 1st and 2d year, when not 2 inches in length. These, as well as the first grinders, are named by the natives dood-kau-daunt, which literally signifies milk teeth. The tusks which are shed have a considerable part of the root or fang absorbed before this happens. The time at which the tusks cut the gum, varies considerably. I have known a young one get his tusks when about 5 months old; whereas the tusks of another did not cut the gum till he was 7 months old. Those tusks which are deciduous are perfect, and without any hollow in the root, in a fœtus which is come to its full time; at this period, the socket of the permanent tusk begins to be formed, on the inner side of the deciduous tusk. A young elephant had shed 1 of his milk tusks on the 6th of Nov. 1790, when near 13 months old, and the other on the 27th of Dec., when above 14 months old: they were merely 2 black-coloured stumps, when shed; but 2 months afterwards the permanent ones cut the gum, and on the 19th of April, 1791, they were 1 inch long, but black and ragged at the ends. When they became longer, and projected beyond the lip, they soon were worn smooth, by the motion and friction of the trunk. Another young elephant did not shed his milk tusks till he was 16 months old; which proves that there is considerable variety in the time at which this happens. The permanent tusks of the female are very small, in comparison with those of the male, and do not take their rise so deep in the jaw; but they use them as weapons of offence, in the same manner as the male named mooknah, that is, by putting their head above another elephant, and pressing their tusks down into the animal. These tusks are never shed, and sometimes grow to a very large size in the male. The largest I have known in Bengal, did not exceed 72 lb. avoirdupois: at Tiperah, they seldom exceed 50 lb.; but both these weights are very inferior to that of the tusks brought from other places to the India House, where I have seen some near 150 lb. each. From what part of Asia they came, I

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* Soor or choor-daunt signifies hogs' teeth; from the tusks having some distant resemblance to those in the lower jaw of the hog.—— † Ankoos signifies a crook, and is particularly applied to the weapon the drivers use to govern the elephant, to which these irregular tusks bear some resemblance.—Orig.

could not learn, but suspect they were imported from Pegu to Calcutta, and thence to London.

The African elephant is said to be smaller than the Asiatic; yet I am credibly informed, by the ivory dealers in London, that the largest tusks generally come from Africa, and are of a better texture, and less liable to turn yellow, than the Indian ivory, after being manufactured. This probably is owing to the tusks having laid longer in Africa, before they were imported, than those brought from Asia. In the latter country, most of the tusks exported are taken from elephants immediately after their death; whereas the Africans find many teeth in the desert places which have been frequented by this animal. The intense heat of a vertical sun will undoubtedly render the ivory firmer and harder, if the tusks happen to lie on the scorching sand, or in any other dry situation. The increase of the tusk arises from circular layers of ivory, applied internally, from the core on which they are formed, similar to what happens in the growth of the horns of some animals. When the tusks of the living elephant are sawn through, and the remaining portion exposed some months to the air, this structure is clearly shown. If the period in which one of these circular layers is completed could be ascertained, this might lead us to fix, with tolerable precision, the age of an elephant, by counting the circles in each tusk. Cutting off a portion of the tusks of a living elephant is a common practice; it is done with a view to make the tusks grow thicker, when they are too long and slender, and also sometimes for the sake of uniformity, when they grow in a wrong direction.

In describing the structure of the grinders, it must be observed, that a grinder is composed of several distinct laminæ or teeth, each covered with its proper enamel; and that these teeth are merely joined to each other by an intermediate softer substance, acting like a cement. I accordingly use the words teeth, strata, layers, and laminæ, as synonimous, when speaking of the structure of the grinders. The structure of the grinders, even from the first glance, must appear very curious, being composed of a number of perpendicular laminæ, which may be considered as so many teeth; each covered with a strong enamel, and joined to one another by the common osseous matter. This, being much softer than the enamel, wears away faster by the mastication of the food; and in a few months after some of these teeth cut the gum, the enamel remains considerably higher, so that the surface of each grinder soon acquires a ribbed appearance, as if originally formed with ridges. These strata, when first formed, have no firm attachment to each other, but always appear separate and distinct, when contained in their bony sockets within the jaw, after their membranes and soft parts are destroyed. Before any part of a grinder cuts the gum, there is a bony crust formed above the enamel, which gives a smoothness to the grinding surface. But, after the grinders cut the gum, and the convex surface has been worn down a little by the trituration of the food, each lamina appears to have been formed on several points*, which are covered

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* This appearance has been observed by Patrick Blair, M. D., F. R. S., who, in his Osteographia

by a strong enamel. There are from 4 to 8 of these points, joined together by the common bony matter, which fills up the space between the enamelled portions. When the grinder is farther advanced in the mouth, its foremost laminæ are gradually worn down by the mastication of the food; and these enamelled points or denticuli disappear, one after another, till the enamel at last runs quite across the tooth, surrounding the central part on which it was formed, and taking the irregular indented plaited shape of the lamellæ. This bony centre on which the enamel is formed, is harder than the matter which joins the teeth together, does not wear so fast, and consequently remains higher.

The number of teeth of which a grinder is composed, varies from 4 to 23, according as the elephant advances in years; so that a grinder or case of teeth, in full grown elephants, is more than sufficient to fill one side of the mouth; in proportion however as the foremost layers are worn away, the succeeding ones come forward, to supply their places. The denticuli of which each layer or tooth is composed, are much larger, and fewer in number, in old than in young elephants; in consequence of this, the same number of laminæ generally fills the jaw of a young or of an old elephant; and from 3 till 50 years, there are from 10 to 12 teeth or laminæ in use, in each side of either jaw, for the mastication of the food. When several of the anterior teeth of which a grinder is composed have been completely formed, and each tooth covered with its proper enamel, they become firmly united, beginning at the fore part, by the intervention of the common bony matter, which gradually fills up the interstices between them. When the bodies of several of the anterior laminæ have been connected together the inferior edge of each becomes united, in the same manner, to the one next it, till the whole are thus gradually joined, and form a grinder or case of teeth.

As soon as the anterior part of the grinder is thus firmly united, the fangs or roots are next added: these at first appear in the form of a thin curtain or lamella of bone, extending backwards, along some of the anterior laminæ, at their lower edges. A fang common to the 3 anterior teeth, first begins to be formed by the ossification shooting across from each side, in a circular direction, at the anterior portion of the first, and the posterior part of the 3d lamina. These join and become longer, assuming a conical shape: the hollow is gradually filled up by successive layers of the substance of the tooth, as the fang lengthens, till at last it becomes solid. This however does not happen, till the 3 layers to which the fang is attached are nearly worn away. When its ossification is almost completed, another process begins to take place, which is, the absorption of the fang from its external surface. By the time that the anterior layers of the grinders are completely

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Elephantina, published in 1713, calls it digitations. The above work, which was put into my hands by my friend Dr. Alex. Monro, jun., since this paper was written, contains some useful information. The ingenious author had, in several particulars, a tolerable idea of the formation and structure of the grinders; yet, far from suspecting a regular succession of them, he attempts to prove such succession to be impossible. He is equally erroneous in many other respects.—Orig.

worn down, both the fangs and the alveolar processes begin to be absorbed. Their places are gradually supplied by the next laminæ of the grinder, and their fangs coming forward in a constant succession. When the last tooth of a grinder has advanced sufficiently in the jaw, to supply the place of its predecessor, the anterior tooth of the next succeeding grinder comes forward, to supply its place.

From the peculiar manner in which the grinders are supplied from behind, but never from beneath, a preceding grinder, as is the case in the human species, and in most other animals, it must appear evident, that an elephant can never shed his teeth; but, from this regular succession, he may, at one period, have only a single grinder in each side of either jaw; at another there may be one and part of a succeeding grinder; even a still greater variety in the appearance of the grinders will take place, according as the anterior one is more or less worn away, and the waste supplied by its successor. In this manner, the growth of new teeth, to compose a succeeding grinder, and the ossification and formation of the fangs, are constantly going on, in regular succession; so that, after the 2d year, the mouth of the elephant is constantly filled with as many laminæ of the grinders on each side as it can hold. While the grinders thus advance forward in the mouth, in regular succession, the alveolus of each advances along with them; and as the anterior fangs are absorbed, the same process is going on in the alveoli. In the partition between each alveolus there is a communication, which in young elephants is larger than in those farther advanced in years; and it is probable, that this canal or sinus between the different alveoli, admits the passage of an elongation of the membrane, from the anterior to the posterior grinder.

The time requisite for the complete formation of 1 of these cases of teeth, constituting a grinder, varies from 2 to 6 or 8 years; and when an elephant has attained its full size, a considerable number of the anterior laminæ must be worn away, and the fangs absorbed, before the posterior ones can be sufficiently advanced to cut the gum. From the curved line in which the grinders of the upper jaw advance, it must be evident, that some of the anterior laminæ must be obliterated before the last can come into use: this may be made to appear more clearly, by drawing lines parallel to the surface of the grinder of the upper jaw; yet there are 10 of the posterior ones that cannot come into action, till the same number of their predecessors are worn away in regular succession. Before this could have happened, several years must have elapsed, during which the posterior laminæ would have been completed; for, in the present state, the 3 aftermost layers are not even now attached to each other, or to the rest which are anterior; the membrane between, and connecting these laminæ, not being ossified at the time of the animal's death. In this grinder, there are 23 laminæ, which is the greatest number I have seen.

In the lower jaw, the same circumstances take place: the teeth of the grinders rise by the addition of their fangs, force their way through the alveoli, and cut the gum, as they advance forward in the jaw. The grinding surface has rather a on-cave form, to adapt itself to that of the grinder in the upper jaw. The number of layers does not always correspond with those of the grinder in the upper jaw; but, like them, consists of from 4 to about 23 teeth or laminæ. In both jaws, the alveoli are firmly attached, anteriorly and laterally, to the bony plates of which the jaw is composed; but at the posterior part these alveoli are separate from the jaw, and have only a membranous attachment. The alveoli terminate in an apex or point, and become thicker and stronger, as the elephant advances in years. In the lower jaw, the portion of the alveolus which is attached to the inner plate, is thick and spongy; and through the under part of this spongy substance there is a pretty large foramen, for transmitting the blood-vessels and nerves which supply the teeth and lower jaw. The alveolus of the grinder advances in the same manner in the lower as in the upper jaw; and as the fangs are absorbed it is absorbed also. In proportion as the fangs or roots are added to the grinder, it rises through the alveolus, and cuts the gum; at the same time the bottom of the alveolus, in which the grinder is formed, becomes more spongy, and shoots up between the fangs, firmly embracing them, and thus preventing the grinder from being shaken or disturbed by the trituration of the food. As the grinders of the upper and under jaws wear away, their roots are lengthened, and become more solid, by the internal addition of new matter, till the cavity is entirely filled up. This lengthening of the roots is necessary, to give that portion of the grinder in use sufficient firmness in the jaw, as well as to keep the surface at a proper level above the gum. When the anterior teeth are worn down to the roots, these, with the sockets, begin to be absorbed, to make room for their successors, which are coming forwards.

The shape of a grinder of the lower jaw is very different from that of one of the upper: in the latter, the grinder advances from behind straight forwards, and the back part has a very convex shape; whereas the lower grinder advances rather in a bent or curved direction, adapting itself to the shape of the jaw. The surface of this grinder is somewhat of a concave figure, adapted to the form of the corresponding grinder in the upper jaw. The upper and lower grinders, and the section of a grinder, show, in the clearest manner, the progress of ossification in the roots, and the manner in which the different teeth are joined. In a young elephant, soon after birth, the milk grinders, with their roots are completely formed; and even the succeeding or 2d set of grinders have the roots partly added to some of the anterior teeth, which are soon to cut the gum; but the posterior layers are then without roots. Farther back in the jaw, the 3d grinder, which is composed of about 13 teeth, has no appearance of roots; nor have the different teeth any connection with each other, except by the common membranes. When these are destroyed, the teeth or rudiments of a succeeding grinder can be easily separated from each other. At this period, the enamel of the third grinder has not been formed, but only the substance of the teeth, which it afterwards covers, adapting itself to the irregularities of the surface. When a grinder is considerably worn down, these irregularities of the central lamellæ are evident, from the enamel of each tooth being indented and puckered, as it were, all round. Having thus attempted to explain, in a clear and satisfactory manner, the progressive growth and regular succession of the grinders, I will next point out the periods in which I conceive these respective changes to take place. Here, however, I am in considerable doubt and uncertainty; but will fairly state the circumstances which first drew my attention particularly to this subject, as well as the grounds on which my conclusions have been made. In Nov. 1795, I sent a couple of elephants' heads, through my friend Mr. Fairlie, of Calcutta, to D. Scott, Esq., of Upper Harley-street, to be placed by him in some public museum*. In my letter, dated the 17th of that month, I mentioned the most remarkable peculiarities of these heads, and particularly the grinders; but at the same time made this remark, " there is only 1 tooth in each side of either jaw, till an elephant attains its full growth." On examining afterwards the heads of some younger elephants, I perceived I had made a mistake, and that there was not always only one grinder in each side of the jaw. This want of uniformity in the appearance of the grinders of young elephants, of the same size, and nearly of the same age, showed me my mistake, and puzzled me a good deal; nor did I perceive any means whereby I could satisfactorily and rationally account for it, till I had carefully compared a number of heads, of different ages, with each other.

To effect this, I immediately began to collect the heads of such elephants as died at Tiperah, with the size and qualities of which I was perfectly acquainted: in the course of the year 1796, I procured above 30 heads, and, beginning with the youngest of these, I arranged them as nearly as possible according to their respective ages. As it may be satisfactory to many members of the R. S., to learn the means by which I was enabled to collect the heads of so many elephants, whose heights and qualities I had accurately ascertained, I shall just observe, that between the beginning of Nov. 1795, and the 1st of April, 1796, there were 4 herds of elephants taken in Tiperah. Three of these herds were taken under my immediate inspection: the 4th, consisting of about 50 elephants, was taken by the Rajah's hunters, but was afterwards so terribly neglected, and almost starved to death, that I was requested by the Rajah to take them under my management; to this I consented, and his servants were ordered to obey implicitly my directions. In consequence, however, of the former ill treatment the elephants had received, above half of them died in the course of a few months; these, with some other casualties, enabled me to form the numerous collection above-mentioned.

The elephants from which the heads were taken being well known to me, I was enabled to form a tolerable estimate of the ages of several of them; those young ones whose ages are particularly specified, were brought forth after their dams were secured. After arranging and comparing the heads with each other, I endeavoured to ascertain the different periods necessary for the formation of the grinders, in

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* These were afterwards sent to the Right Hon. Sir Jos. Banks, Bart., and by him to the British Museum, where they now are.—Orig.

young and old elephants, and thence to draw some conclusions, respecting the progress of dentition in this useful animal. The first set of grinders, or milk teeth, begin to cut the gum 8 or 10 days after birth; and the grinders of the upper jaw appear before those of the lower one. Though this happens at first, yet in a few months the grinders in the lower jaw come forward faster than those of the upper, as I have observed in the heads of several elephants. In about 6 weeks, the first set of grinders can be easily felt, consisting of 4 teeth, viz. one on each side of either jaw; and as young elephants begin to eat grass, or some soft succulent food, before they are 3 months old, we may conclude, that the first set of grinders* have then completely cut the gum, and that dentition is not attended with any symptoms of pain, or irritation, in the system. The milk grinders are not shed, as the tusks are, but are gradually worn away, during the time the 2d set are coming forward; and as soon as the body of the grinder is nearly worn away, the fangs begin to be absorbed.

I have not been able to ascertain the exact time when the 2d set of grinders make their appearance, as I could never get an elephant to open his mouth in such a manner as to permit me to examine his teeth accurately; but when the elephant is about 2 years old, the 2d set are completely in use. At this period, the 3d set begin to cut the gum. From the end of the 2d to the beginning of the 6th year, the 3d set come gradually forward, as the jaw lengthens, not only to fill up this additional space, but also to supply the place of the 2d set, which are, during the same period, gradually worn away, and their fangs absorbed. From the beginning of the 6th to the end of the 9th year, the 4th set of grinders come forward, to supply the gradual waste of the 3d set. After this period, several other sets are produced. In what time these succeeding grinders come forward, in proportion to their predecessors, I have not been able to ascertain; but from the data already given, I conclude, that every succeeding grinder takes at least a year more than its predecessor to be completed; consequently, that the 5th, 6th, 7th, and 8th set of grinders (a further succession I have not been able to trace) will take from 5 to 8 years, and probably much longer, each set, before the posterior lamina has cut the gum. The milk grinders consist each of 4 teeth or laminæ; the 2d set of grinders of 8 or 9 laminæ; the 3d set of 12 or 13; the 4th set of 15; and so on, to the 7th or 8th set, when each grinder consists of 22 or 23, which is the greatest number I have observed.

All these circumstances considered, I may venture to affirm, that the formation of the teeth and mode of dentition, in the elephant, has but little analogy with those of any other quadruped; nature having, by a peculiar and wonderful contrivance, and in the most convenient manner, supplied this animal with a regular succession of teeth, till he attains a very advanced period of life. An advantage which, as far as we know, no other quadruped possesses. The mode in which the

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* By a set, I mean 4, one grinder in each side of either jaw.—Orig.

elephant's grinders are originally formed, my short stay at Tiperah did not allow me sufficient opportunities to investigate; but, since my return to England, I have had frequent conversations with my friend Mr. Home on that subject, who, from an examination of the teeth brought home by me, and some preparations in the late Mr. Hunter's collection, has been able to prosecute the subject with considerable success. His observations will be laid before the R. S., immediately after the present paper, as a continuation of the same subject.

XIV. On the Structure of the Teeth of Graminivorous Quadrupeds; particularly those of the Elephant and Sus Æthiopicus. By E. Home, Esq., F.R.S. p. 237.

When Mr. Corse put into my hands his observations on the elephant's teeth, and showed me the teeth themselves in their different stages of growth, in illustration of what he had advanced on the subject, I very readily engaged in the prosecution of so curious an investigation. I examined several specimens of elephants' teeth, preserved in spirit, while in a growing state, which are deposited in Mr. Hunter's collection of comparative anatomy, and compared them with the teeth in Mr. Corse's possession. From these 2 sources, I was enabled to procure every information that was required, to explain the structure of the elephant's teeth, and to point out the general principle on which all teeth are formed, that have the enamel intermixed with the substance of the teeth; a subject, as far as I am acquainted, not hitherto investigated. To make my observations on the structure of the complex tooth of the elephant intelligible to the R. S., it appears necessary to mention, generally, the mode in which the more simple teeth of the human species, and of carnivorous animals, are formed: this knowledge will render the account of such additional parts as are met with in those of the elephant, more easily understood.

The teeth of carnivorous animals are formed from a vascular pulp, of the same shape with the future tooth, on the external surface of which the substance of the tooth begins to grow, and increases till it is completely formed. The pulp is inclosed by a capsule, the cavity of which, while the tooth is growing, is filled with a viscid fluid, similar to the synovia of joints; and this fluid, by the absorption of the thinner parts, becomes inspissated to a proper state for crystallization, so as to form the enamel, which adheres to the surface of the tooth. Teeth formed in this way, are composed of 2 parts, of dissimilar texture: one, the enamel, which is striated; the other, the substance of the tooth, which is laminated, like ivory, being more compact than common bone, and less so than the enamel; but differing from both in the mode of its formation. Bones are formed in 2 different ways: those that are cylindrical, have cartilage for their basis; those that are flat, either cartilage or membrane; but in no instance in the body are they formed on a pulp. The substance of the tooth must therefore be considered as distinct from bone, and may be ranked, both from its structure and mode of formation, as a species of ivory.*

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* The tusks of the elephant are formed on a pulp, similar to teeth. Tumors are sometimes met with

The teeth of the elephant differ from those just described, in being composed of a great many flattened oval processes; these, while growing, are detached; but when completely formed, their bases unite together, and make the body of the tooth, to which the fangs are afterwards added; and as the fangs are lengthened, the tooth rises in the jaw. This is what may be considered as the tooth itself, being composed of the same materials as the teeth of carnivorous animals; but in addition there is another substance, which unites all the processes together laterally, into one mass; this is softer than the substance of the tooth, and on examination proves to be similar, in its texture and formation, to common bone. As teeth have been hitherto considered of the same texture with common bone; it is probable that nothing but the 2 substances being united in the same mass, could have led me to the discovery of their differing materially from each other. It will therefore be proper to explain, in this place, the circumstances which first gave me the present view of the subject.

To obtain an accurate knowledge of the different parts of the elephant's tooth, a longitudinal section was made, of one that was full grown. This section exposed the lateral connection between the different processes, and the intermediate substance which unites them into one mass; it also showed the mode in which the processes are continued into the body of the tooth and fangs. That the internal structure might be made more distinct, the surface of this section was polished very highly, which led to the discovery of the processes of the tooth having a more compact texture than the intermediate substance; for though both had the same appearance after being sawn, the processes bore a polish, which the other did not,* and were laminated, like ivory; while the other parts were porous, like the internal structure of common bone. This led me to examine preparations of the elephant's teeth, in a growing state, preserved in spirit, which explained the mode of growth of these 2 substances to be different. In these preparations it was found, that the processes of the tooth, which may be called ivory, were all formed on so many portions of one common pulp, which had its origin in the jaw; and that the intermediate substance, which may be called bone, was formed on a species of ligament situated immediately under the gum, from which membranous elongations extended into the spaces between the processes of the tooth.

This structure of tooth is not peculiar to the elephant, but common to the teeth of all animals whose food requires to be ground, or much bruised, before it is

in the frontal sinuses of the human body, having a perfect resemblance to ivory; they have their origin in the bony cavity of the sinus, and extend themselves into the orbit of the eye. Of these, I have seen 2 instances, and was unable, at the time, to account for them; but am now induced to believe they were formed on vascular excrescences, growing from the lining of the sinuses, similar in their organization to the pulps above-mentioned.—Orig.

* A portion of the jaw itself bore the same degree of polish as the intermediate substance of the tooth. The cells in the elephant's skull are no part of its common structure; they communicate freely with the cavity of the tympanum, and are therefore appendages to the organ of hearing, which I shall explain more fully on some future occasion.—Orig.

swallowed. In the elephant's tooth, from the largeness of its size, the parts are more distinct, and more readily contrasted with each other; but in other animals, even those of a small size, as the sheep, the different structures are readily detected. It is singular that this structure should have escaped the accurate investigation of the late Mr. Hunter; particularly as the formation of the teeth was one of the first objects he employed himself on; and he continued to pursue it to the end of his life, marking the varieties which occur in different animals. The cause of his overlooking it was the following: in making preparations of horses' teeth, to show the figured appearance on the grinding surface, he rendered them black by means of fire, which did not affect the enamel, so that the white lines of the enamel were beautifully distinct on the black ground; but the bony part and the substance of the tooth were equally coloured, and had a uniform appearance. The examination of these preparations led him to believe, that the horse's tooth consisted of only 2 substances, the tooth itself and the enamel. Under this impression, Mr. Hunter examined the growing teeth of the horse, and found the pulp rising from the jaw, and the vascular membranes passing down from the gum, into the spaces between the portions of pulp; he was therefore led to conclude, that the pulp was for the formation of the tooth, and that the membranes which came from the gum were for the formation of the enamel.

Having so fully explained, in the elephant's tooth, the real uses of these 2 parts, it is not necessary to say more in refutation of this opinion, which is published in Mr. Hunter's work on the teeth; but, in justice to the correctness of his other observations, I shall subjoin his account of the circumstances under which the enamel of the human teeth is formed, taken from the same work. He says, "the pulps are surrounded by a membrane, which is not connected with them, except at their root, or surface of adhesion. This membrane adheres, by its outer surface, all round the bony cavity in the jaw, and also to the gum, where it covers the alveoli. When the pulp is very young, as in the fœtus of 6 or 7 months, this membrane is pretty thick and gelatinous. We can examine it best in a new-born child, and we find it made up of 2 lamellæ, an external and an internal: the external is soft and spongy, without any vessels; the other is much firmer, and extremely vascular, its vessels coming from those that are going to the pulp and body of the tooth. While the teeth are within the gum, there is always a mucilaginous fluid, like the synovia in joints, between this membrane and the pulp of the tooth."* This mucilaginous fluid, I have already asserted, deposits the enamel; which is further confirmed by the following experiments and observations. The complex tooth of the elephant, being composed of 3 different structures, each of which has a peculiar process for its formation, led to an inquiry whether the materials themselves were different, or only differently arranged. To investigate this, Mr. C. Hatchett, from a zeal to promote the pursuits of science by which he is distinguished, oblig-

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* Natural History of the Human Teeth, by John Hunter, p. 86.—Orig.

ingly gave his assistance, and made some experiments, the results of which are as follow. It is to be understood, that a complete analysis was never intended to be made; as neither Mr. H.'s time admitted of it, nor did it appear necessary for the object of the present inquiry.

Exper. 1. Some enamel, rasped into a fine powder, was put into a matrass, and, pure muriatic acid being added, the whole was suffered to remain without the application of heat during 1 hour; in the course of this time, the enamel was completely dissolved, with a gentle effervescence. To this solution, some sulphuric acid was gradually added, till all precipitation had ceased: the precipitate was separated by a filter, and was found to be selenite. The filtrated liquor, by evaporation, afforded a small additional quantity of selenite, which was also separated; after which, the liquor, being evaporated, became thick and viscid. This, when diluted with water, precipitated lime from lime-water, in the state of phosphate. To another portion, solution of acetite of lead was added, and caused an immediate precipitation of a white matter, which, when dried and sprinkled on burning charcoal, produced a light and smell like phosphorus; it was soluble in nitrous acid, and was thus to be distinguished from muriate or sulphate of lead.

Exper. 2. Some of the raspings of enamel were dissolved by digestion in nitric acid, and when the solution had been diluted and filtrated, it was saturated with carbonate of ammonia. The precipitate thus produced was collected, and edulcorated in a filter. The small excess of carbonate of ammonia, in the filtrated liquor, was saturated with acetous acid; after which, the phosphoric acid was precipitated, by solution of acetite of lead. On examining the first precipitate, or that produced by the carbonate of ammonia, it was found that it was still composed of lime, combined with a portion of phosphoric acid, instead of carbonic acid, which might have been supposed. To effect therefore a complete separation of the 2 ingredients, lime and phosphoric acid, acetous acid was poured on the precipitate, by which it was immediately dissolved. The whole of the phosphoric acid was then separated from this solution, by acetite of lead; after which, lest any lead should be present, the liquor was saturated with pure or caustic ammonia, and the lead was separated by a filter; lastly, the lime which remained dissolved was precipitated, in the state of carbonate, by carbonate of ammonia.

The enamel has been supposed, not a phosphate but a carbonate of lime. This error may have arisen from its solubility in acetous acid or distilled vinegar; but the effects of the acetous acid are, in every respect, the same on powdered bone as on the enamel. Consequently, when enamel, or bone, is put into a glass matrass containing acetous acid, placed in a sand bath, the portion which is dissolved, is not, as has been supposed, carbonate but phosphate of lime; for if to the filtrated solution nitrate or acetite of lead is added, a precipitate is produced, of phosphate of lead, in the same manner as when nitrate or acetite of lead is added to urine. This mode of treating substances supposed to contain phosphoric acid, as bone, &c. Mr. Hatchett has found of great utility; because, by this means, he can detect phosphoric acid, when the substance is in too small a quantity to be examined in any other manner. Similar experiments, on the substance of teeth formed on pulps, and on common bone, afforded similar results.

Mr. Hatchett considers lime and phosphoric acid to be the essentially constituent principles of these 3 different structures; and any difference that is met with, only seems to be that which would constitute species of the same genus, similar to what is found in the mineral kingdom, under lime-stone, marble, and calcareous spar; these differ only by a small change in the proportions of their constituent principles, and by a different arrangement of their integrant particles. The head of a human thigh bone was found, some years since, with a thin crust of highly-polished enamel, similar in some respects to that of the teeth, on a portion of its surface, an inch and half in length, and an inch in breadth; the cartilage having been previously removed by disease. This uncommon appearance, at the time, could not be accounted for; but the fore-mentioned observations, on the formation of the enamel of the teeth, appeared to throw some light on it; and Mr. H., at my request, made the following experiment, to determine whether the synovia, in a healthy state, contains phosphate of lime.

960 gr. of synovia, by a gradual evaporation, afforded 21 gr. of a substance which resembled dried glue. This being collected was put into a small porcelain crucible, which, placed in a larger crucible, was exposed to a red heat, during nearly an hour. The matter in the porcelain crucible was much reduced in bulk, and appeared like a glazing, thinly spread on those parts of the crucible which had been in contact with it in its former state. Boiling distilled water was digested on the matter in the crucible, for some time. This water afterwards afforded, with acetite of lead, a copious precipitate of phosphate of lead; but no appearance of lime could be obtained. On the residuum in the crucible, acetous acid was digested, which was afterwards divided into 2 portions. To one of these, solution of acetite of lead was added, and as before afforded a plentiful precipitation of phosphate of lead. To the other portion was added oxalic acid, by which a small quantity of a precipitate was obtained, which was an oxalate of lime. Phosphate of lime is therefore present in synovia, though but in a small quantity; and as, from these experiments, there is reason to believe, that more phosphoric acid was obtained than was requisite to saturate the lime, it seems probable, that part of it was combined, in the synovia, either with soda or ammonia; and this accounts for the part dissolved by the distilled water.

M. Margueron, in the Annales de Chimie, (vol. 14, p. 123,) estimates the proportion of water in 288 gr. of the synovia of an ox at 232 gr. The other ingredients therefore amount to 56 gr.: but by evaporation Mr. Hatchett obtained, from 960 gr. of synovia, only 21 gr. of residuum; which proves that the proportion of water is much greater; for 56 to 288 is as 1 to 5.14; but 21 to 960 is as 1 to 45.71. It is possible, that the proportion of water to the other ingredients may not always be the same. M. Margueron also probably estimated the albuminous matter, &c.

in a moist state; for, without one of these suppositions it is impossible to reconcile such a very great difference. By these experiments of Mr. Hatchett, phosphate of lime was ascertained to be present in the synovia; which though in a very small quantity in the natural state of that fluid, explains the mode by which the crust of enamel on the head of the thigh bone could be produced, when by a morbid action of the parts, the quantity of phosphate was preternaturally increased.

A mixture of bony matter with the enamel and the substance of the tooth is a structure, as has been mentioned, not confined to the elephant: it is common to all truly graminivorous quadrupeds. But the whole number of grinding teeth belonging to each side of the jaw being confined in a case of bone, so as to form 1 large grinding surface, and the teeth being pushed forward from behind, instead of a 2d set being formed immediately under the fangs of the first, as in other animals, are peculiarities not met with in any teeth hitherto described, except those of the elephant.

These peculiarities have however been ascertained, in the course of the present inquiry, to belong to the Sus Æthiopicus; a skull of which, with the teeth, is preserved in Mr. Hunter's collection. The particular species to which it belonged was determined, by its exact similarity to a skull, without the grinding teeth, in the British Museum, marked, in Dr. Solander's hand-writing, Sus Æthiopicus, from Guinea. As it has been ascertained by Dr. Solander to come from Guinea, there is reason to hope so curious a species of the hog will attract the notice of naturalists, and be the means of perfect specimens being introduced into this country. From the appearance of the teeth in the perfect skull, the animal had probably arrived at its full growth, and only 1 grinder remained on each side of the jaw, consisting of 7 different processes, cased with bone, similar to those of the elephant. The grinding surface of those processes which had their points worn down sufficiently to show a transverse section, exposed 3 oval portions of tooth, surrounded by enamel, inclosed in bone; which is more like the tooth of the African elephant than the Asiatic, and makes another variety of form of these processes.

The tusks of the Sus Æthiopicus are uncommonly large, and in their structure resemble those of the elephant. The skull was shown to Sir Jos. Banks, whose readiness to forward the labours of those who engage in the pursuits of science, by liberally communicating to them his own knowledge of the subjects connected with their inquiries, is sufficiently known to the members of the R. S. He identified the species of the genus to which the skull belonged, in the manner abovementioned; and, by an accurate search among the skulls of animals deposited in the British Museum, discovered a small head in a dried state, which, when properly macerated and cleaned, proved to be that of a young Sus Æthiopicus, whose teeth were in a growing state, and enable me to explain all the necessary circumstances, respecting this curious mode of dentition. The grinding teeth in this young head are distinct from each other, and 4 in number, on each side of the jaw. That which is most anterior is the smallest, and has a grinding surface only equal in extent to that of 1 of the processes contained in the large tooth of the full-grown animal: the second has a grinding surface equal to that of 2 such processes: the 3d is still larger, its surface being equal to that of 3 processes. These 3 teeth, in their general appearances, resemble those of the common hog; they have also the same kind of fangs; their only peculiarity is, the enamel being intermixed with the substance of the tooth, but without any bony matter surrounding it. The 4th or last tooth is very different from the others, and exactly resembles that found in the large head, only that this is in a growing state. It is composed of 7 processes, united together; these are in different stages of growth, fitting them to come forward in succession, similar to those of the elephant. The 2 first have their grinding surface worn smooth: the points of the next 2 have recently cut the gum; and the other 3 are still concealed in the jaw, not being completely formed; of the last of these the first rudiments only are to be seen. This large tooth, which may be considered to be a 2d set of teeth, as the concealed processes enlarge, advances forwards, pushing the other teeth before it: the most anterior of these, as soon as its body is worn away, has its fangs removed by absorption, and drops out: the same thing takes place with the 2d and 3d; and in this way room is made for the large 1 to supply the place of all the others.

These peculiarities in the teeth of the Sus Æthiopicus, led to the examination of the teeth of the other species of the same genus, all of which appear to resemble the human grinders, only that the last in the jaw has a broader grinding surface than the rest, which is common to most quadrupeds. It is worthy of remark, that the number in each side of the jaw in the common hog is 7; in the Pecary, 6; in the Babyroussa, 5; and in the Sus Æthiopicus, till a certain age, 4. It is curious, that one species of a genus should differ so widely from all the others, in respect to its teeth; and should be allied to the elephant in the structure of its tusks, the mode of formation of the grinding teeth, and the manner in which they succeed each other. From these circumstances it appears that the Sus Æthiopicus, in a natural state, is supplied with a different kind of food from that of other hogs, and is an animal of greater longevity.

On comparing the internal structure of the elephant's tooth with that of the horse, cow, and sheep, it was found, that they were similar, in having an intermixture of bone with the substance of the tooth, but that they differed materially from each other in the proportions and situations of the bony portions. Each of these animals having the grinding surface of their teeth adapted for particular kinds of food, the parts composing that surface are variously combined, so as to answer the purpose for which the teeth are intended. In all of them, the mode of growth is the same; the substance of the tooth is first formed, and the bony part is afterwards adapted to the irregularities of that surface. In the horse's grinding teeth, the processes are 2 in number; and, in an early stage of their growth, they appear, as well as those of the elephant, to be separate teeth; they differ however extremely in their shape, forming irregular cylindrical tubes, the central part of which is filled up by the projecting membranes from the gums, that are to be changed for bone. The division of the tooth into 2 parts, is very distinct in the shedding teeth, but not in the 2d set or permanent teeth. These 2 portions of bone in the middle of the tooth have frequently a hole in them, probably the passage of a blood-vessel, never completely filled up, and the food getting into it, as the tooth is worn down, considerably increases its size. Besides which, there is a portion of bony substance surrounding a great part of the outside of the tooth.

In the cow's grinding teeth, there are 2 portions of bony substance in the middle of the tooth, as in the horse, in shape of crescents, and a very small portion in the hollows on the outside of the circumference of the tooth; but none on the projecting parts. In the grinding teeth of the sheep, the middle portions of bone are similar to those of the cow, but on a much smaller scale; there is no portion of bone on the outside of the tooth. It is not to be wondered at, that there is so great a variety in the grinding surfaces of the teeth of different genera of graminivorous quadrupeds, each no doubt adapted to the kind of food they are in a state of nature destined to live on, since there is even a variation between the teeth of the African and Asiatic elephants. In the African elephant, the processes of which the tooth is composed are not flattened ovals, as they have been described in the Asiatic, but are in the form of an oblong square or parallelopipedon, so that, in the middle line of the tooth, the processes are in contact with each other, though at no other part; by this means, the middle line of the tooth is the hardest; the whole surface therefore does not wear regularly, as in the Asiatic elephant, but with a ridge in the middle.

Having, by the foregoing observations, established a well marked characteristic distinction between the teeth of truly carnivorous and truly graminivorous quadrupeds, I was desirous of knowing how far this general rule applied to quadrupeds at large, and if it did not, in what animals the teeth were differently formed. The teeth of the hippopotamus and rhinoceros are found to differ in their structure from those above described, partaking in some measure of the properties of both, and forming 2 very curious links in the chain of regular gradation between the one and the other. The grinding teeth of the hippopotamus are made up of the substance of the tooth and enamel only, having no portion of bone mixed with the other parts; but, what is I believe peculiar to them, the enamel pervades the substance of the tooth to a considerable depth, so as to be intermixed with it. The grinding teeth of the rhinoceros have a peculiarity of a very different kind: they also are only composed of the substance of the tooth and enamel; but the tooth is so formed as nearly to surround a middle space, which, were it filled up with bone, would make a truly graminivorous tooth, not unlike those above described. This middle space is left open, and becomes filled up with the masticated food, which falls into it, and cannot afterwards be readily removed; so that the grinding surface will be always kept irregular, and in a still greater degree than in any of the other teeth which have been described. It is highly probable that there are many other varieties in the structure of the grinding teeth of quadrupeds, but these will be sufficient to illustrate the general principles on which such varieties depend.

*XV. On the Quantity of Tanning Principle and Gallic Acid contained in the Bark of various Trees.** *By George Biggin, Esq.* p. 259.

The bark of trees contains the astringent principle, called gallic acid, and also that principle which has a peculiar affinity to the matter of skin, and which, from the use to which it is applied, is called the tanning principle. But in the present mode of tanning bark is applied in mass to the skins; consequently both principles are applied. It remains for examination, whether both principles are useful in the process of tanning: for, if they are not both useful, probably one is detrimental. To a nobleman, whose zeal on every occasion by which the sciences or arts may receive illustration or improvement is eminently conspicuous, and to whose public energy, as well as private friendship, I feel myself much indebted, to his Grace the Duke of Bedford, I owe the means of prosecuting some experiments on this subject. His Grace, by collecting a variety of barks at Woburn, gave me an opportunity of making some experiments to ascertain the quantity of tanning principle and gallic acid each bark contained. For that purpose, I made use of the following methods, according to the principles laid down by M. Seguin.

By dissolving an ounce of common glue in 2 lb. of boiling water, I procured a mucilaginous liquor, which, as it contains the matter of skin in solution, is a test for the tanning principle. By a saturated solution of sulphate of iron, I obtained a test for the gallic acid. I then took 1 lb. of the bark I meant to try, ground as for the use of tanners, and divided it into 5 parts, each part being put into an earthen vessel. To 1 part of this bark I added 2 lb. of water, and infused them for 1 hour. Thus I procured an infusion of bark, which I poured on the 2d part of the bark, and this strengthened infusion again on the 3d part, and so on, to the 5th. But as a certain portion of the infusion will remain attached to the wood of the bark, after the infusion is poured or drawn off, I added a 3d lb. of water to the first part, and then followed up the infusion on the several parts, till the 3 lb. of water, or so much of them as could be separated from the bark, were united in the 5th vessel; from which I generally obtained 1 pint of strong infusion of bark.† To a certain quantity of this infusion I added a given measure of the solution of glue; which formed an immediate precipitate, that may be separated from the infusion by filtering paper. When dried, it is a substance formed by the chemical union of the matter of skin with the tanning principle, and is in fact a powder of leather. By

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* This inquiry has been further prosecuted by Mr. Davy in a paper inserted in the Phil. Trans. for the year 1803.

† The specific gravity of this infusion was ascertained by an hydrometer whose gradations are inverse to those of a spirit hydrometer.—Orig.

saturating the infusion with the solution of glue, the whole of the tanning principle may be separated by precipitation.

For the gallic acid.—To the pound of bark left in the earthen vessels, and already deprived of its tanning principle by these quick infusions, I added a given quantity of water, to procure a strong infusion of the gallic acid, which requires a longer time, say 48 hours. This infusion, when obtained pure,* affords little signs of the presence of the tanning principle, when tried by the test of the solution of glue; but, with the solution of sulphate of iron, it gives a strong black colour, the common black dye, which differs in density, according to the quality of the bark: this may be further proved by boiling a skain of worsted in the dye, by which the gradations of colour will be very perceptibly demonstrated. Having thus obtained a point of comparison; by making a similar infusion, under similar circumstances, of any bark, or vegetable substance, and paying strict attention to the specific gravity of the infusion, the quantity of precipitate of leather, and the density of colour produced by given quantities of one or the other test, the result will be, a comparative statement of the respective powers of any bark, or vegetable substance. This comparative statement I conceive to be sufficient for all commercial purposes. As oak bark is the usual substance employed in the trade of tanning, if a quantity of tanning principle is found to be contained in any other bark or vegetable, the commercial utility of that bark or vegetable may be determined, by comparing its quantity of tanning principle and price with those of oak bark.

For an accurate chemical analysis, I have tried a variety of acids, and simple and compound affinities; and having pursued the above experiments, at the same time that I was employed on some in dying, I found the muriate of tin (the method of using which is described by Mr. Proust in the Annales de Chimie), very convenient. A solution of it, being added to the infusion of bark, forms a precipitate with the tanning principle, leaving the gallic acid suspended: the precipitate is of a fawn colour, and is composed of tanning principle and oxydated tin. By these means, I have been enabled to form a comparative scale of barks; which however I do not produce as accurate. Oak bark, in its present state, as procured for commercial purposes, differs very much in quality, from accidental circumstances: the season of the year in which it is collected occasions a still more important difference, consequently the scale now produced must be very imperfect; but I am of opinion, that by the pursuits of scientific men who may be inclined to investigate this subject more fully, a very accurate scale may hereafter be formed. In the following scale, I have taken Sumach as the most powerful in the comparative statement; leaving however a few degrees, for a supposed maximum of tanning principle, which I reckon 20.

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* It is hardly possible, from the intimate connection of the 2 principles, to separate them entirely by infusion: in the infusion of tanning principle, there will always exist a little gallic acid; and, in an infusion of gallic acid, a little tanning principle will commonly be present, unless the infusion of gallic acid is very weak, and procured by a 3d or 4th watering.—Orig.

Scale of Barks.

Bark of	Gallic acid, by colour.	Tanning principle, by hydrometer.	Tanning principle, (in grs.), from ½ a pint of infusion and 1 oz. of solution of glue.
Elm*	7	2.1	28
Oak, cut in the winter.	8	2.1	30
Horse chesnut	6	2.2	30
Beech	7	2.4	31
Willow (boughs)	8	2.4	31
Elder	4	3.0	41
Plum-tree	8	4.0	58
Willow (trunk)	9	4.0	52
Sycamore	6	4.1	53
Birch	4	4.1	54
Cherry-tree	8	4.2	59
Sallow	8	4.6	59
Mountain ash	8	4.7	60
Poplar	8	6.0	76
Hazel	9	6.3	79
Ash	10	6.6	82
Spanish chestnut	10	9.0	98
Smooth oak	10	9.2	104
Oak, cut in spring	10	9.6	108
Huntingdon or Leicester willow	10	10.1	109
Sumach	14	16.2	158

It is to be observed, that the barks do not keep any respective proportion in the quantity of gallic acid and tanning principle contained in each; which is an evidence of the distinctness of principle, and may perhaps open a new field for saving oak-bark in dying, as the willows, sallow, ash, and others, produce a very fine black. It is also worthy of observation, that the quantities of gallic acid and tanning principle do not differ in equal proportions, between the winter and spring felled oaks. This fact may lead to the discrimination of the proper time for cutting; which is probably when the sap has completely filled and dilated that part of the vegetable intended for use. This will make a difference in the season of cutting oak, elm, and other trees, shrubs, &c. Leaves should be taken when arrived at their full size, and then dried under cover; for, as the tanning principle is so soluble, and the substance that contains it so thin, in a leaf, the dew alone might dissolve it. Finally, as the gallic acid does not seem to combine with the matter of skin, and as its astringency will corrugate the surface, we may I think conclude, that its presence in tanning is not only useless, but detrimental.

XVI. On the Resolution of Algebraic Equations: attempting to distinguish particularly, the Real Principle of every Method, and the True Causes of the Limitations to which it is subject. By Giffin Wilson, Esq. p. 265.

1. The practical management of algebraic equations, as far as respects the solution of problems depending on them, is well understood; but their general theory, being considered as an abstruse and purely speculative subject, is no where, that I have seen, so fully analysed, as with all the assistance to be derived from the application of the principles of combination, it appears it might be.—2. The difficulties under which the higher branches of algebra still labour are generally known. No degree of equations beyond the 2d, is yet perfectly resolved: cubics present frequently an irreducible case: biquadratics have, by several methods, been reduced

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* The infusion of the elm was so loaded with mucilage, that it was with difficulty I could separate the tanning principle, or try the specific gravity.—Orig.

to cubics; but no formula exhibiting to the eye the actual resolution of a biquadratic has yet appeared; and for the 5th degree, and all upwards, not even a clue which promises a general resolution has been struck out, by the continued labour and ingenuity of mathematicians for several centuries.

3. This failure in the chain, beginning at the 3d degree, and its breaking off entirely after the 4th, have been very puzzling and mortifying circumstances to the cultivators of algebra. Having in the first degrees proceeded on apparently very general principles, and made a seeming progress towards a general resolution of equations, it is provoking to find it suddenly interrupted, not to be resumed by any contrivance. Various causes have been assigned for so remarkable a difficulty: but the generality of those causes, as commonly given, do not reach the principle. It has been usual for operators, when they found their methods fail, to look back till they could detect some inconsistence or impossibility in their work, and to suppose the difficulty explained, by pointing out the period at which such an error is made. The power and richness of the algebraic calculus affords numerous ways of compassing the same thing, and, as all of them fail when applied to this object, there is necessarily a point in every one of them, at which some inconsistence or impossibility is introduced: thence, a number of different causes may be imagined. In Dr. Waring's Meditationes Algebraicæ, p. 182, may be seen several concurrent reasons assigned, why the methods there shown, and Dr. Waring's own, undoubtedly the most general of any of them, since it proceeds on one principle to the 5th degree, cannot apply further: but all reasons drawn from the data of any particular method, like that commonly given for the imperfection in Cardan's Rule, which I shall examine hereafter, though very just in themselves, cannot be conclusive: they indisputably show why the precise method to which they respectively apply must fail; but that does not exclude the expectation that some other, founded on different principles, may succeed. The question therefore recurs: Is there not some paramount fundamental reason for this general failure? If there can be shown to be any thing in the nature of abstract quantity, which governs the several orders of quantities from which equations are framed, and leads directly to the distinctions and limitations practice discovers, that will reach the difficulty at its source, and afford the satisfaction desired.

4. I think, that by turning the course of our inquiry rather to examine how we come to succeed at all, in resolving any degree of equations, than why our success is so limited, the true principle on which their resolution must depend will appear; and with what probability, and by what means, if possible, we may expect to render our methods more perfect. With this idea, I shall take a concise view of the nature and resolution of equation in general; pointing out the common difficulty, and by what circumstances that difficulty is, in certain cases, lessened or removed; confining myself always to the principle of each step, and a strict analysis of the result, avoiding all detail of mere operation; and, without pretending to much novelty on a subject already so beaten, I persuade myself, such an inves-

tigation will lead to some conclusions which have not been remarked, and which are both curious and important.

On the Resolution of Equations in General.

5. Equations, in that part of algebra which treats of their general resolution, are usually considered to be reduced to one general form, for the greater convenience of comparing them, i. e. to their lowest rational dimension, with unity always for the co-efficient of the highest power of the unknown quantity; in which state, every simple equation is already resolved. The resolution of all other degrees is the finding the simple equations of which they are compounded: but to do this in a general manner, it is evident we must seek, instead of the particular equations themselves directly, a general expression representing them all; which general expression is called the formula of resolution, such as, the common quadratic resolution, or that given for cubics by Cardan's Rule.

6. These formulæ, properly speaking, are rather the reversion of an equation, than the resolution of it: for though the unknown quantity be evolved or reduced to a simple dimension, the known parts are necessarily involved or affected with a surd at least as high as the dimension of the equation, in order to exhibit the proper number of correspondent values belonging to the unknown quantity in an equation of that degree. Thus, the equation, $x^2 - px + q = 0$, and its common resolution $x = \frac{p \pm \surd(p^2 - 4q)}{2}$, are both the same quadratic; only, under the first form, the unknown quantity, being of the dimension of the 2d degree, has 2 values; whereas in the 2d form it has only 1, and the double value is transferred, by the quadratic surd, to the known parts on the opposite side of the equation. Thus also, the equation $x^3 - qx + r = 0$, and the Cardanic formula belonging to it $x = \sqrt[3]{[-\frac{r}{2} + \surd(\frac{r^2}{4} - \frac{q^3}{27})]} + \sqrt[3]{[-\frac{r}{2} - \surd(\frac{r^2}{4} - \frac{q^3}{27})]}$ are, in the same manner, the same cubic merely reverted. But as equations are usually denominated from the dimension of the unknown quantity, these resolutions are commonly deemed simple equations: they may in this view be defined to be, the simple equations that the original quadratic, cubic, or other higher given equation, contained in power, since they express the nature and form of a quantity which, by involution or reverting the operation, re-produces it; as the root of any power, being re-involved, returns to the power from which it was extracted. This fixed and visible connection between the equation and the general formula for its roots, throws a beauty and elegance into the method of pure algebraic resolution, which none of the others, such as the method of divisors, and all the contrivances for approximation, can pretend to. For, when by any of those methods we have obtained one or more separate roots, the relation to the original equation is no longer perceivable; but here the chain is perfect. The equation leads to the resolution: the resolution embraces at once all the correspondent roots; and, when re-involved, proves the operation, by reproducing the original equation. Thus, for example, if $x^2 - 5x + 4 = 0$, and it be perceived, or found by any conjectural method, that

unity is one of the roots of that equation, there is no discernible connection between the simple equation expressing $x = 1$, and the original equation; no transformation of one will produce the other. This latter equation $x = 1$, though truly expressing a numerical root of the former, is no more a resolution of it than of the equations $x^2 - 6x + 5 = 0$, $x^2 - 7x + 6 = 0$, or any other of the infinite number of equations of which unity is a root; whereas the algebraic resolution of $x^2 - 5x + 4 = 0$, viz. $x = \frac{5 \pm \sqrt{(25 - 16)}}{2}$, which equally expresses 1, and 4 the other root, needs only to be cleared of its radical, to show itself but another form of the same equation; and gives $x^2 - 5x + 4 = 0$, as at first.

7. This view of the algebraic resolution of an equation shows, that it does not so much aim at giving us the roots themselves, as the basis or common principle of their artificial combination in the equation to which it applies; pointing out some form of a perfect power, of which they may be conceived to be the correspondent natural roots. From which it follows, that if the transformation required to be made in the given equation be possible, or such as can really be effected, the resolution will be real; for every real power has some real root: but that if, on the contrary, the power into which the equation is conceived to be transformed be merely imaginary, the resolution must be so too; for all the roots of an imaginary power are themselves imaginary. It doth not therefore depend on the nature of the roots of the equation themselves, but on the form which the equation must assume to become a perfect power, to determine whether the resolution be real or imaginary: so that the nature of the resolution, and that of the roots of an equation may be very different, as we know is frequently the case; particularly in the resolution of cubic equations by Cardan's Rule, where, when the roots are real, the resolution is almost always imaginary. This has seemed to surprize and perplex some writers very much, who have treated it as at best a paradox, if not a contradiction,* but surely without cause; for, as the formula affects only to be an ideal representation of the mechanism or structure of a perfect power answering to the given affected equation, it may be expected to be clear or complicated, real or imaginary, not as the roots themselves are simple and real, but as the principle of their union, of which only it is truly the index, is near or remote: it merely shows the central point of their combination, which, like the centre of gravity, suspension, or any other power, may not actually exist in any of the bodies whose motions it governs, but in some imaginary point without, and remote from them all. Had the nature of the algebraic resolution of an equation been considered in this light, and the forms to which they are proposed to be reduced, been compared with the original forms of the roots in the given equation, no surprize or appearance of paradox could have arisen in the matter; but it must have been clearly perceivable what cases would admit of real, and what only of imaginary resolutions, as will be shown hereafter. I have dwelt the longer on the nature of

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* Vide Playfair on the Arith. of Impossibles, Phil. Trans. 1778, p. 318; Dr. Hutton on Cubic Equations, ditto, 1780, p. 387; and Mr. Baron Maseres, Script. Logarith. vol. 2, p. 246.—Orig.

the algebraic resolution of an equation, because it is a very curious subject, about which many errors and inconsistencies have been fallen into, though hardly any direct examination of it is to be found in any of our books. It is the sole method of obtaining a complete general answer to any problem. It makes algebra consistent with itself and sufficient to solve its own difficulties, without foreign aid, from series or other branches; and, in all cases where any general ulterior use is to be made of the resolution of an equation, is the only method that avails at all.

8. In order to obtain this general resolution, the common methods have been, without considering the nature of the roots, to attempt some universal reduction in the forms of equations; as, 1st. The destroying their intermediate terms, and converting them into pure powers. Or, 2dly. The discovering some constant complement which will always raise them to the nearest perfect power. In both which cases, the resolution will afterwards be nothing more than simple extraction of the proper root. Or, 3dly. The assuming some convenient formula with indeterminate co-efficients; and, by assigning their values properly, adapting it to every case.

It would be going to too great a length, to give distinct examples here, of the application of these methods. Numerous instances of each of them are given in the common books of algebra, which usually treat them as separate and distinct from each other; but the fact is, they are all in truth the same. Whoever tries them separately, will find, however variously they seem to set out, they lead precisely to the same conclusion, and fail precisely in the same points. A quadratic, whether resolved by completing the square, or by expunging the 2d term: a cubic, whether resolved by Cardan's rule, or by completing the cube, or by assuming a resolution, as suggested in Dr. Waring's Meditationes Algebraicæ, p. 179, 180, present the same formula of resolution, and the same limitations and irreducible cases. And the reason is easily found. To complete the requisite power, according to the index of the equation, or to destroy the intermediate terms, occasions an alteration in just the same number of terms; it is only the particular relation they are required to bear to each other that is varied. In the one case, they are all to be equal, or equal to nothing; in the other, to correspond respectively with the known law of the binomial theorem, which gives the unciæ of a regular power. Both depend on the practicability of a more general problem, of which they are but specific cases; viz. the problem "to give the co-efficients of an equation any general determinate relation." If that were practicable, and it were possible to mould them so as to establish a general relation between them, or any required number of them, it is easy to perceive, that the particular relation must be a secondary consideration; and that, wherever the same number of terms are to be acted on, the same means that might make them equal, might give them any other proportion at pleasure.

9. However, of all these methods, and any other of the kind, it is to be observed, that the principle is demonstrably a false assumption. For, if it be once admitted that the construction of equations, and the laws of the successive co-effi-

cients received ever since Vieta's time, be true; or that all equations are formed invariably in the same manner, from the continual multiplication of the simple equations of their roots, which experience confirms without any exception;* it follows, that the nature of the roots must infallibly govern that of the equation derived from them; that the same form of equation can only be produced by the same forms of roots; and therefore, before all sorts of equations can be made into pure or perfect powers, or be given any other general shape, it must be shown, that all quantities are capable of taking the forms required to produce equations of that sort, which will presently be seen to be impossible. If those who have lost their time and labour in vain endeavours to improve these general methods, had, instead of involving themselves in a labyrinth of substitution and process, on the chance of some means of simplification presenting itself, considered before-hand the probability of success, the imperfection of Cardan's rule would never have appeared a paradox, nor the interruption of all further progress by it have given room for surprize. They must have seen, that no equation beyond a quadratic can admit of a real extinction of its intermediate terms. In the general equation $x^n - px^{n-1} + qx^{n-2} - rx^{n-3} + sx^{n-4}$ &c. $= 0$, p being the sum of the roots, and q the sum of their combinations in pairs, by Sir I. Newton's theorem for finding the sums of the powers of the roots, $p^2 - 2q$ will be the sum of their squares; and therefore, if both p and q vanish, the sum of the squares of the roots must vanish also; which can never happen with real quantities. Besides this, in attempting to destroy many intermediate terms at once, we know by experience, that the equations which become incidentally necessary to be solved, rise to a much higher dimension than the given equation; so that our labour, in this respect, defeats itself.

10. Nor will these difficulties be avoided, if we abandon the idea of a general resolution, and attempt to work out the roots separately: though the number of co-efficients is always sufficient to afford a distinct equation of each root, and therefore, by the common principles of indeterminate equations, will clearly determine them all; and would also find them, if the equations afforded by the co-efficients were all of the same degree; but they rise successively, and, from the drawing them together, in order to expunge the several unknown quantities, the index of the reducing equation increases so as to defeat the operation. To show this, let us recur to the general equation before given, $x^n - px^{n-1} + qx^{n-2} - rx^{n-3} + sx^{n-4} = 0$; suppose its n roots to be represented by a, b, c, d, &c. n;† then, by the construc-

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* Some algebraists, affecting to reject the use of negative quantities, have been compelled to dispute the generally received theory of the construction of equations; but they have not been able to suggest any other.—Orig.

† The nature of the roots is not material in this place; whether affirmative or negative, real or imaginary, they have just the same operation in forming the co-efficients of the equation. I have however throughout chosen, wherever I could, to give examples capable of being tried by real and affirmative roots; and, for that purpose, have uniformly made the signs of the co-efficients alternately affirmative and negative.—Orig.

tion of equations, we have n distinct equations from the several co-efficients in succession; viz.

$$a + b + c + d \text{ \&c.} \ldots\ldots + n \ldots\ldots\ldots\ldots \text{ in number } n = p,$$

$$ab + ac + ad \text{ \&c.} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots n \times \frac{n-1}{2} = q,$$

$$abc + abd \text{ \&c.} \ldots\ldots\ldots\ldots\ldots\ldots n \times \frac{n-1}{2} \times \frac{n-2}{3} = r,$$

$abcde$ &c. n, or the product of them all, being the co-efficient of the last term. Now, as we have n equations, and n indeterminate quantities, it is evident, that by employing each equation successively to determine one quantity, the whole will be determined. But the equations are not all of the same degree: the first, is a simple equation: the 2d, being composed on one side wholly of products by two is in degree a quadratic: the 3d, for the same reason, a cubic: and so on. If the first of these equations be used to determine a, we shall have $a = p - b - c - d$ &c. $- n$; inserting that value for a in the 2d equation, it becomes the quadratic $pb - b^2 + pc - c^2 + pd - d^2 - bc - bd$ &c. $= q$. If that quadratic be solved to determine b, and the values of a and b be inserted in the 3d equation, it becomes the cubic c^3 &c. .. $= r$. Now, the quadratic having 2 roots, its solution will have introduced a quadratic surd. Before therefore we can proceed to employ the 3d equation to determine c, it must be squared to clear it of that surd, and of course will then rise to the 6th degree. The solution of such a dimension, if admitted for the present to be equally possible, must introduce higher radicals; and, by the intrusion of these superfluous roots at every stage, our labour increases, instead of diminishing. This is the difficulty alluded to before; and, as we have appropriated already all our subordinate equations, we have nothing to oppose it. It therefore seems hopeless, to expect to make any general impression on indeterminate equations, without more help, beyond the mere knowledge of the constitution of the co-efficients.

11. This difficulty however is wholly removed by the least circumstance that discloses any particular relation among the co-efficients of an equation, independent of the general law of their construction. This of course, whenever it occurs, furnishes new conditions and means of comparing the terms. Every particularity in the co-efficients that gives specific varieties to the forms of equations, must, from the nature of their construction, have its source in some particular relation between 2 or more of the roots, and therefore, as far as that relation extends, detects them infallibly. The observation of the forms and relations of the co-efficients under different species of equations, and the correspondent inferences to be drawn, as to the connection of their roots, would form a curious and very useful part of a complete treatise on the whole doctrine of equations, which is a work much wanted. The most striking of these relations will be obvious, or familiar, to the reader who has at all considered the nature of the subject; such as, that equations deficient in every alternate term arise from pairs of equal roots with opposite signs, $\pm a$, $\pm b$ &c.; that those whose terms on both sides the middle term are alike, which are

generally called recurring equations, arise from pairs of roots, of which each pair contains a quantity and its reciprocal, a, $\frac{1}{a}$, b, $\frac{1}{b}$, &c.; together with Maclaurin's demonstration of the particularities of the co-efficients when an equation has equal roots.* And the extent to which these notices might easily be carried, from observations of the effects of the different sorts of proportion, and all other relations, is prodigious. But my present concern is merely with the result, supposing from any means a relation to be previously discovered affecting any number of the roots. For example,—suppose, in the above given equation, $x^n - px^{n-1} + qx^{n-2} - rx^{n-3} + sx^{n-4}$ &c. $= 0$, whose roots we called a, b, c, d, &c. . . . n, we happened to know that 2 of the number, a and b, were equal; then, since they might both be expressed by the same character, the n roots of the equation might now be represented by only $n - 1$ distinct characters; and therefore, of the subordinate equations derived from the construction of the co-efficients, 2 might be employed to determine one root, a and b being equal, the equation furnished by the value of the co-efficient p, and also that furnished by the co-efficient q, may be both together used to determine the same quantity. But, if any quantity a be a root of an equation, the simple equation $x - a = 0$ must be a divisor of that equation;† therefore here $x - a$ must be a common divisor of the 2 equations furnished by p and q, and consequently may be found, without resolving either of them, by continual division or subtraction, according to the ordinary rule for finding the common measure.‡

12. Any other relation from the knowledge of which one character may be made to represent two or more roots, evidently answers the same end. Indeed all relations of that kind may be converted into equality itself, by taking, instead of the given equation, some other properly derived from it. Thus if, instead of a and b being the same, b had been supposed the negative of a, or $-a$, and then, instead of the former equation, that of the squares of the roots were taken, the relation would be made equality; for a and $-a$ have the same square. If arithmetical proportion was known to be the relation of any number of the roots, by taking the equation of their differences, it would also be converted into equality.

13. If 3 or more roots, or any number of parcels of roots, are known to be related, and their common relation be used to represent them, of course the number

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* Vide Maclaurin's Algebra, chap. 4, p. 162, et infra.——† Vide Sanderson's Algebra, vol. 2, p. 679, 680, art. 432, and all algebras on the method of divisors.—Orig.

‡ Vide Sanderson's Algebra, quarto ed. vol. 1, p. 86, 87, 88, where the rule is well given; and Maclaurin's Algebra, p. 2, cap. 4, p. 162; or Mr. Hellins's Essay on the Reduction of Equations having equal roots. But of the last it should be observed, that some qualification must be made to the assertion, that the reduction may be carried on till a simple equation is obtained. In cases where there is only one pair of roots equal, that proposition is undoubtedly true; but, if 2, 3, or more pairs of roots are equal, the reduction can only be carried down to a quadratic, cubic, &c. for, every pair of equal roots being equally to be found by the method, of course the final or resulting equation must be of a dimension as great as their number.—Orig.

of distinct characters to be determined will proportionably be diminished: and as the number of subordinate equations furnished by the co-efficients remains always the same, while the dimension of the proposed equation is unaltered, more of them may be used together to discover the related roots, and their investigation be proportionably facilitated. This single observation, in the hands of a skilful analyst, is sufficient for the reduction, if not the solution, of any particular numeral equation whatever, and the more so the larger its dimension: for, from the endless variety of relations numbers bear to each other, hardly any set of them can occur, as the co-efficients of an equation, or perhaps exist, that, on being compared, do not exhibit some peculiarity, of greater or less extent, sufficient to afford a clue to the correspondent relation in their roots. And if no such clue is immediately given by the equation itself, taking the equation of the differences or sums in pairs, or of the squares, &c. of the roots, will soon find one. But, as peculiarities of that sort, though never so frequent, may be deemed always accidental, and evidently no general method can be founded on them, even where the co-efficients are given, it may be asked, how any use can be made of them in cases of indeterminate equations?

14. To this I answer, that there are some properties of quantities that depend only on the index of the equation, without any regard to the value of its co-efficients; or, in other words, there are some peculiar properties which merely depend on the number of any set of quantities, abstracted from all consideration of their nature and values. For example, 2 quantities a and b have their differences the same quantity $a - b$, only taken both affirmatively and negatively, $a - b$ and $b - a$; when squared, these differences become equal; $a^2 - 2ab + b^2$ is the square of both: therefore, let the quantities themselves be chosen as they may, the equation of the squares of their differences must have both equal roots, and consequently be reducible by the reasoning in art. 11, 12, and 13. Again, 3 quantities, however distinct in themselves, give a set of differences marked with a peculiar relation, any 2 of them being equal to the 3d; a, b, c, being 3 quantities, $(a - b) + (b - c) = a - c$. Also, if the 3 quantities be so chosen originally as to have their sum equal to nothing, one of them must necessarily be equal in magnitude to the sum of the remaining two; and therefore, whether taken simply or summed in pairs, their relative magnitudes must remain the same. Again, 4 quantities, of any sort whatever, may be pursued to a constant relation, though somewhat more remote, and grounded on very different causes; viz. a, b, c, d, being 4 quantities, from their combinations by pairs, ab, ac, ad, bc, bd, cd, 6 in number, added together two by two, thus, $ab + cd, ac + bd, ad + bc$, 3 quantities are formed, sufficiently distinguished from the group of similar combinations to be found separately, as will be shown hereafter. And also, if the 4 quantities are originally so taken as to have their sum equal to nothing, their sums in pairs, though 6 in number, will be reduced to 3 in effect; for, if $a + b + c + d = 0$, by transposition, $a + b = -c - d$, $a + c = -b - d$, $a + d = -b - c$, i.e. 3 of the 6 must be merely the negatives of the other 3; which relation, if they are squared, will become equality, so

that the number of distinct squares will be only 3. These properties, though without any order or connection, and confined merely to particular ranks or numbers of quantities, being general to all possible or imaginable quantities of those classes, afford methods general, as to those degrees, but without producing any result really general to equations at large.

15. Having shown that an indeterminate general equation cannot be resolved by any of the methods whose principle is yet known, because they are all grounded on the assumption of some particularity, either inherent in the roots, or universally communicable to them, which, so far from being general, is seldom found, and absolutely incompatible with many sorts of roots; that the difficulty is in all cases the same,—the intrusion of superfluous roots and higher radicals; that a relation of any kind, when known, obviates that difficulty, as far as it extends; and that some orders of quantities have generally a constant and necessary relation, more or less remote, I proceed to examine, more minutely, the application of these observations to the several degrees of equations to which they materially apply.

Of the Resolution or Reduction of Equations of particular Degrees.

16. In examining those degrees of equations which submit to be resolved, I shall observe the same order as before; i. e. first consider the power of obtaining a general formula, or complete resolution; and, if that is not attainable directly, inquire by what general means the roots can be separately investigated, and what new forms they have taken, or what different functions of them are used in the operation.

17. If we resume the general indeterminate equation $x^n - px^{n-1} + qx^{n-2} - rx^{n-3}$ &c. $= 0$, and assign the progressive values 2, 3, 4, &c. to the index n, in the first case it will become the quadratic $x^2 - px + q = 0$. Now as this equation has 2 roots, in order to obtain a general formula for its resolution, the first step that suggests itself is, to inquire what is necessary to construct a general representation of 2 quantities in a simple equation. Two quantities are known to be generally expressed by means of their sum and difference; that half their sum added to half their difference gives the greater, and the same quantities subtracted, the less. The sum being always the co-efficient of the 2d term of the equation, is given in all cases, and here the difference is readily found; for, the square of the difference of any 2 quantities differs from the square of their sum, by a constant quantity, viz. 4 times their product or the co-efficient of the 3d term. If a and b be called the roots of the equation $x^2 - px + q = 0$; then

$$p = a + b, \text{ and } p^2 = a^2 + 2ab + b^2;$$

$$q = ab, \text{ and } -4q = \quad -4ab;$$

$$\text{whence } a^2 - 2ab + b^2 = (a - b)^2 = p^2 - 4q \left\{ \begin{array}{l} \text{the square} \\ \text{of the dif-} \\ \text{ference.} \end{array} \right.$$

The difference itself is therefore $\sqrt{(p^2 - 4q)}$. And now, being possessed of the parts required to construct a general representation of the 2 quantities, we can at once complete the formula of general resolution of equations of this degree, viz. $x = \frac{1}{2}p \pm \frac{1}{2}\sqrt{(p^2 - 4q)}$. This, as before observed in art. 6, is however the same quadratic, only reverted; for, the quadratic surd it contains is frequently incapable of further reduction. Therefore, generally speaking, the degree of the equation is not altered; only the place of the index, which being first affixed to the unknown quantity, is now transferred to the known ones. But yet this resolution is, in all cases, equally true and direct; for, involving no other radical than belongs to the degree it relates to, it faithfully exhibits the nature of the roots, and is always rational or real, or not, according as they are so.

18. If, instead of seeking, a priori, the formula of resolution, we attempt to find the roots simply, we may instantly trace a constant connection between them, or at least between their differences; which, however the quantities are varied, are always related in the same manner, being $a - b$ and $-a + b$, the same quantity with different signs, and consequently their squares precisely the same. From which it appears that the equation of those differences will always want the 2d term, or be a pure quadratic; and that of their squares will be a perfect binomial square, having both roots equal; which roots may therefore, by the reasoning in art. 11, be certainly found. But the inference is just the same as before: the equation is not lowered in degree; the equal relation is brought no nearer than between the squares of the differences; and, when they are found, the same quadratic surd must be used to arrive at the roots themselves. This formula of resolution $x = \frac{1}{2}p \pm \frac{1}{2}\sqrt{(p^2 - 4q)}$, is the same given for quadratics in every algebra; but it is not usually remarked, or perhaps understood, that the whole operation, however varied in appearance by setting about to complete the square, as it is called, or to destroy the 2d term, is merely employed to obtain the difference of the roots; that, on analysing the formula, the part under the vinculum is always that difference and nothing else, and why it must be so.

19. Next, let $n = 3$, and the equation be the complete cubic $x^3 - px^2 + qx - r = 0$. If we make it our first step here, as in the last case, to inquire what is necessary to construct a general representation of 3 numbers in simple equations, we shall find it must consist of the same parts, the sum and the differences: but, as the differences increase in number, to show the order in which they are taken, and the law they observe progressively, I shall subjoin a general table of the simple representation of the different orders of quantities. As in every equation the sum of the roots is always given, I shall, for greater simplicity in the table, suppose it always to vanish. If then there be a series of general equations, beginning with a quadratic, and proceeding upwards with progressive indexes, in all of which the coefficient of the 2d term p be taken $= 0$, and A be supposed a difference of the roots of the first, A and B 2 of the differences of the roots of the 2d, A, B, C, 3 differences of those of the 3d, and so on; in taking of which differences, no other caution is necessary than that they should be similarly situated, viz. all derived by comparing the same individual root with the remaining ones, as if a be taken as a root,

and $a - b$ be the first difference, $a - c$, $a - d$, $a - e$ &c. . . . . a n, having all the same antecedent letter, whose number will always be $n - 1$, must be the rest; then, the table will be as follows:

Table of the simple Representation of the Roots of Equations of progressive Indexes.

In quadratics,

$$x = \pm \frac{A}{2}.$$

In cubics,

$$x = \left\{\begin{array}{l}\frac{A+B}{3}\\[4pt] \frac{-A-B}{2.3}\end{array}\right. + \left\{\begin{array}{l}+\frac{A-B}{2}\\[4pt] -\frac{A+B}{2}.\end{array}\right.$$

In biquadratics,

$$x = \left\{\begin{array}{l}\frac{A+B+C}{4}\\[4pt] \frac{-A-B-C}{3.4}\left\{\begin{array}{l}+\frac{A-B}{3}-\frac{B+C}{3}\\[4pt] +\frac{A-C}{3}+\frac{B-C}{3}\\[4pt] -\frac{A+B}{3}-\frac{A+C}{3}.\end{array}\right.\end{array}\right.$$

In the 5th degree,

$$x = \left\{\begin{array}{l}\frac{A+B+C+D}{5}\\[4pt] \frac{-A-B-C-D}{5}\left\{\begin{array}{l}+\frac{A-B}{4}-\frac{B+C}{4}-\frac{B+D}{4}\\[4pt] +\frac{A-C}{4}+\frac{B-C}{4}-\frac{C+D}{4}\\[4pt] +\frac{A-D}{4}+\frac{B-D}{4}+\frac{C-D}{4}\\[4pt] -\frac{A+B}{4}-\frac{A+C}{4}-\frac{A+D}{4}.\end{array}\right.\end{array}\right.$$

In the (nth) degree, or generally,

$$x = \left\{\begin{array}{l}\frac{A+B+C+D+E\ \&c. \ldots\ (n-1)\ \text{in number}}{n}\\[6pt] \frac{-A-B-C-D-E\ \&c.}{(n-1)\times n}\left\{\begin{array}{l}+\frac{A-B}{n-1}-\frac{B+C}{n-1}\ \&c.\ (n-2)\ \text{in number}\\[4pt] +\frac{A-C}{n-1}+\frac{B-C}{n-1}\ \&c.\\[4pt] +\frac{A-D}{n-1}+\frac{B-D}{n-1}\ \&c.\\[4pt] +\frac{A-E}{n-1}+\frac{B-E}{n-1}\ \&c.\end{array}\right.\end{array}\right.$$

20. The inspection of the table shows us, that in all cases, to construct a general simple representation of any number of quantities, and consequently to construct a direct resolution of their equation, we must first find a certain number of their differences; but we have no general means of separating particular differences from the rest; and the whole number of differences increases in a proportion so much greater than the number of quantities, that the former difficulty recurs, the pre-

vious steps involve higher dimensions than the original equation. The original index being n, that of the equation of the difference of the roots is $n \times (n - 1)$. However, from the nature of differences, being taken both affirmatively and negatively, all equations formed from them must, as observed of quantities of that sort in art. 11, be universally deficient in every alternate term, which brings their equation to the form of equations of only half their own index, or $n \times \frac{1}{2}(n - 1)$: but in this case their differences are 6, and their equation, with that consideration, is reduced no lower than a cubic form, which is the same degree with the proposed equation: therefore it does not appear that we can be enabled, a priori, to determine the formula of any direct resolution of this case.

21. Let us then try to trace some relation which may convert some or all of the roots, or some regular function of them, into equal quantities; when, the equation of that function having equal roots, of course those roots will be separately deducible, as shown in art. 11, 12. In art. 14, we may remember that 2 particularities were mentioned to belong to 3 quantities, viz. that their differences were so related as to be every 2 of them equal to the 3d; and that, if the quantities themselves have their sum equal to nothing, 2 of them also must equal the 3d, and their magnitude be respectively the same, whether they are taken simply, or summed in pairs. To avail ourselves of both these properties, let us suppose the 2d term to be expunged from the given equation, which we know may always be effected, its form will then be $x^3 - qx + r = 0$ *, and the sum of its roots equal to nothing. Let a and b be two of its roots, the 3d will therefore be $-a-b$; take their sums by 2, $-a$, $-b$, $a+b$; take their differences, $2a+b$, $a+2b$, $a-b$, and their negatives, which may be divided into 2 sets whose sum is nothing, like that of the roots, viz. $\left\{\begin{array}{ccc} a-b, & a+2b, & -2a-b \\ -a+b, & -a-2b, & 2a+b \end{array}\right\}$. So that, from the given equation we derive 3 others, which make a set of 4 exactly similar.

1st. $x^3 - qx + r = 0$, the given equation.

2d. $x^3 - qx - r = 0$, that of its roots summed in pairs.

$\left.\begin{array}{l} \text{3d. } x^3 - 3qx + \sqrt{(4q^3 - 27r^2)} = 0, \\ \text{4th. } x^3 - 3qx - \sqrt{(4q^3 - 27r^2)} = 0, \end{array}\right\}$ 2 similar equations, formed by dividing $x^6 - 6qx^4 + 9q^2x^2 - (4q^3 + 27r^2) = 0$, the equation of the differences, into 2 wanting the 2d term.

22. Now, leaving these considerations for a moment, let us speculate on the further reduction of the equation. If, instead of the present form $x^3 - qx + r = 0$, q could be supposed to vanish as well as p, a still more powerful additional relation would be given to the roots; for, the equation being then a pure cubic, $x^3 = \pm r$, its roots would obviously be the cube roots of r, and all cube roots are alike. If r be a cube, and $\sqrt[3]{r}$ be one of its roots, the remaining two are $\frac{-1+\sqrt{-3}}{2} \times \sqrt[3]{r}$ and $\frac{-1-\sqrt{-3}}{2} \times \sqrt[3]{r}$, let r be any quantity whatever, real or

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* Besides expunging p, the sign of q has been changed; because, in cases of real roots, it will invariably become negative on destroying the 2d term. Vide note in p. 536.—Orig.

imaginary. But it is clear, from what has been before observed in art. 9, that tis reduction is not generally possible, since it supposes 2 contiguous intermediate terms to vanish together, which real roots do not admit of: it must therefore be effected by means of some imaginary assumption. Those who are conversant in the use of impossible quantities, will at once perceive, that the addition or subtraction, (which in surd quantities is always the same thing, as they are equivocal in sign,) of the imaginary surd $\sqrt{-\frac{1}{3}q}$ to each root of the equation, will infallibly cause q to vanish, but the new roots $\begin{cases} a + \sqrt{-\frac{1}{3}q}, \\ b + \sqrt{-\frac{1}{3}q}, \end{cases} - a - b + \sqrt{-\frac{1}{3}q}$, so formed, would not have their sum equal to nothing; and therefore, in destroying the 3d term, the 2d would be revived, so that nothing would be gained.

23. To understand how this difficulty is ever removed, let us examine particularly some equation that wants both 2d and 3d terms, and observe accurately the constitution of its roots. The simplest of the kind is the pure cubic $x^3 = 1$, whose roots are 1, $\frac{-1 \pm \sqrt{-3}}{2}$; but, to avoid fractions in the roots, let us take $x^3 = 8$, whose roots are $(2, - \pm \sqrt{-3}$. Distinguishing the real and the imaginary parts, the real are $2, -1, -1$; the imaginary are $\pm \sqrt{-3}$ or $\pm 3\sqrt{-\frac{1}{3}}$, which are the differences of the real parts, multiplied by the imaginary surd $\sqrt{-\frac{1}{3}}$. It appears therefore, that the roots of a pure cubic are compounded of the roots of some affected cubic, added to their differences drawn into the imaginary surd $\sqrt{-\frac{1}{3}}$. The real parts, $2, -1, -1$, are the roots of the cubic equation $x^3 - 3x + 2 = 0$. The imaginary, of the equation $x^3 + 3x + * = 0$, or the roots of the similar equation of the differences of the former, viz. $x^3 - 9x + * = 0$, drawn into the $\sqrt{-\frac{1}{3}}$; and from their addition are formed the roots of the pure cubic $x^3 = 8$. In constructing which, it is material to observe, that each root of the first equation is joined to the difference of the remaining pair; but it may be remembered, that 3 quantities, whose sum is nothing, are the same when summed in pairs, i. e. each is, in quantity, the sum of the other 2, therefore each difference is in fact added to the sum of the same quantities; and if the question were proposed to reduce the equation $x^3 - 3x + 2 = 0$ to a pure cubic, the rule furnished by this example would be, to find the equation of the sum of its roots in pairs, which, by the last article, is $x^3 - 3x - 2 = 0$; to find the similar equation of their differences, $x^3 - 9x + * = 0$; and, to find the equation produced by the quantities formed from the addition of the roots of the one to those of the other multiplied into the imaginary surd $\sqrt{-\frac{1}{3}}$. The equation last found would however, be of the dimension of the 9th power, at least: for, the addition of each root of the 2d equation to every separate root of the first, produces a separate quantity: thus,

$$\begin{array}{l} 2, -1, \quad - \quad 1 \\ \text{being the roots of the 1st.} \ldots \\ 0, +\sqrt{-3}, -\sqrt{-3} \\ \text{those of the 2d.} \ldots\ldots\ldots \end{array} \left\{ \begin{array}{lll} 2 + 0 & -1 + 0, & -1 + 0, \\ 2 + 3\sqrt{-\frac{1}{3}}, & -1 + 3\sqrt{-\frac{1}{3}}, & -1 + 3\sqrt{-\frac{1}{3}} \\ 2 - 3\sqrt{-\frac{1}{3}}, & -1 - 3\sqrt{-\frac{1}{3}}, & -1 - 3\sqrt{-\frac{1}{3}} \end{array} \right\}.$$

will be the 9 quantities formed by their addition. But we have a decisive clue to distinguish some from the rest; for we know, that if we find the equation of the cubes of those quantities, it must have 3 equal roots; for every time the sum of 2 of the roots of the 1st equation meets its own difference, it will constitute a cube root of 8, and therefore the equation $x^3 - 8 = 0$ will be 3 times contained in the resulting equation of cubes. That equal root being discovered by the method of finding equal roots, so often alluded to before, reduces the equation $x^3 - 3x + 2 = 0$ to the pure cubic $x^3 = 8$.

24. The instance in the last article, of the reduction of the equation $x^3 - 3x + 2 = 0$ to a pure cubic, by means of the equation $x^3 + 3x = 0$, evidently depends on the co-efficient of the 2d term vanishing; and also, that of the 3d term being the same in both, but of opposite signs. For, the roots of the one, in their combinations by 2, producing -3, and those of the other $+3$, of course destroy each other; and as the sums of both equal nothing, when added together their sum will still be nothing; so that no new 2d term can arise, as in art. 22. If we now return to the considerations in art. 21, where we showed how to derive from every cubic equation $x^3 - qx + r = 0$, wanting the 2d term, a similar equation $x^3 - 3qx + \sqrt{(4q^3 - 27r^2)} = 0$, being the equation of 3 of the differences of the roots of the former, so arranged as to want the 2d term also, we may perceive that, to render the 3d term the same in both, we need only divide the roots of the latter by $\sqrt{3}$, or, which is the same thing, multiply them into the $\sqrt{\frac{1}{3}}$. For, the equation $x^3 - 3qx + \sqrt{(4q^3 - 27r^2)} = 0$, when its roots are multiplied by the $\sqrt{\frac{1}{3}}$, becomes $x^3 - qx + \frac{\sqrt{(4q^3 - 27r^2)}}{3\sqrt{3}} = 0$ *. If, by the same reason, they had been multiplied by the $\sqrt{-\frac{1}{3}}$, it would be $x^3 + qx + \frac{\sqrt{(4q^3 - 27r^2)}}{3\sqrt{-3}} = 0$; where the sign of the co-efficient of x is opposite to that of q in the given equation. Therefore the roots of the equation $x^3 - qx + r = 0$, and that of its differences, multiplied into the imaginary surd $\sqrt{-\frac{1}{3}}$, viz. $x^3 + qx + \frac{\sqrt{(4q^3 - 27r^2)}}{3\sqrt{-3}} = 0$, will, by being added together, according to the method in the last article, lead to a reduction of that equation to a pure cubic; i. e. the equation formed from their addition will have 3 roots, whose cubes are the same.

25. The analysis of the pure cubic gives us the following general properties, belonging to any set of those equations whose sum is nothing. Viz. 1st. if 3 such quantities, a, b, $-a-b$, be added in pairs, and 3 of their differences be also taken so as to have their sum nothing, $a - b$, $a + 2b$, $-2a - b$; if then each sum be formed into a binomial, by joining to it its correspondent difference, multiplied by the imaginary surd $\sqrt{-\frac{1}{3}}$, the quantities so formed, viz. $a + b + (a - b)\sqrt{-\frac{1}{3}}$, and $-a + (a + 2b)\sqrt{-\frac{1}{3}}$, and $-b - (2a - b)\sqrt{-\frac{1}{3}}$ will have the same cube.

Now let the equation of the 3 quantities a, b, $-a-b$, be $x^3 - qx + r = 0$;

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* Vide Sanderson's Algebra, vol. 2, p. 688; and Hale's Analysis, p. 146.—Orig.

then, by the construction of equations, this equation may be reduced to a pure cubic of this form $x^3 = 4 \times (-r + \frac{\sqrt{(4q^3 - 27r^2)}}{3\sqrt{-3}})$, which, when cleared of its irrational quadratic surd, becomes $x^6 + 8rx^3 + 16r^2 = \frac{-64q^3 + 432r^2}{27}$ or $x^6 + 8rx^3, + \frac{64q^3}{27} = 0$; or, dividing its roots by 2, to reduce it still lower, $x^6 + rx^3 + \frac{q^3}{27} = 0$, the common reducing equation obtained by Cardan's rule.

Example 2*d*. Let 1, 2, -3, the roots of the equation $x^2 - 7x + 6 = 0$, be taken; the quantity $24 - \frac{80}{3\sqrt{-3}}$ is the common cube of the 3 binomials.

Exam. 3*d*. Let -1, -4, $+5$, the roots of the equation $x^3 - 21x - 20 = 0$, which are the differences used in the last example, be next taken; the cube comes out $-\frac{80}{3} + 24\sqrt{-3}$, or exactly the reverse of the former. Now the cube $24 - \frac{80}{3}\sqrt{-\frac{1}{3}}$, when its equation $x^3 = 24 - \frac{80}{3}\sqrt{-\frac{1}{3}}$ is made rational, gives the quadratic formed equation of the 6th degree, $x^6 - 48x^3 - + 576 = -\frac{6400}{27}$; or, transposing all the terms to one side, and dividing it by 2, to reduce it, as before, $x^6 - 6x^3 + \frac{343}{27} = 0$; the same equation that results from the common methods.

2dly. The differences of the 3 differences $a - b$, $a + 2b$, $-2a - b$, are $3a$, $3b$, $3(a + b)$, or merely 3 times the original quantities. Therefore, had the differences themselves been taken as original quantities, and binomials being formed from them, according to the directions before observed, those binomials, and the ultimately resulting cubes, would differ from the former, in nothing essential but the place of the surd. The differences which were affected with it before, would now be clear; and the quantities themselves, or, which is the same thing, their sums in pairs, be affected with it. However, as these latter are to be multiplied by 3, that multiplication will destroy the fraction when they come again to be multiplied by the surd $\sqrt{-\frac{1}{3}}$, since $3 \times \sqrt{-\frac{1}{3}} = \sqrt{-3}$. Therefore the same end, as to reducing the equation, will be obtained, whether, after adding the sums of the roots in pairs to their respective differences, we multiply the sums by $\sqrt{-3}$, or divide the differences by it.

3dly. If any cubic equation, wanting the 2d term, be transformed into the equation of that function of its roots, formed of the cubes of the binomials arising from joining the sum of each pair of roots to its correspondent difference drawn into the imaginary fractional surd $\sqrt{-\frac{1}{3}}$, or each difference to its correspondent sum drawn into the surd $\sqrt{-3}$, the transformed equation will have among its roots 3 equal cubes; by finding which, according to the methods of finding equal roots, the equation is reduced to a pure cubic.

4thly. The roots of a cubic equation may be all real; or only one of them real, and the remaining 2 imaginary. If only one be real, they will be of this form $a - \frac{a \pm b\sqrt{-1}}{2}$; and, by taking their sums and differences according to the rule, and multiplying the latter into the $\sqrt{-\frac{1}{3}}$, one of the resulting binomials will be real, and the other 2 imaginary: the cube produced by them will therefore be real. When all the roots are real, if 2 be equal, one difference necessarily vanishes; therefore the imaginary factor will only appear about the 2 that remain; and here again the cube produced will be real. But if all the roots are real, and unequal, their sums and differences will all be real: whence all the binomials will involve the imaginary surd; which constitutes the irreducible case.

To give examples of this, let, 1st. $x^3 - 2x + 4 = 0$, a cubic equation, whose roots are 2, and $-1 + \surd - 1$, and $-1 - \surd - 1$; the binomials constructed by taking their sums and differences as before, will be

$$\left.\begin{array}{l} 2 - (1 - \surd - 1) = 1 + \surd - 1 \\ 2 + (1 - \surd - 1) = 3 - \surd - 1 \end{array}\right\}$$

$$\left.\begin{array}{l} 2 - (1 + \surd - 1) = 1 - \surd - 1 \\ 2 + (1 + \surd - 1) = 3 + \surd - 1 \end{array}\right\}$$

$$\left.\begin{array}{l} -(1 + \surd - 1) - (1 - \surd - 1) = \quad -2 \\ -1 + \surd - 1 + 1 + \surd - 1 = \quad 2\surd - 1 \end{array}\right\},$$

which last binomial $-2 + 2\surd - 1$, when the latter quantity $2\surd - 1$ is drawn into the imaginary surd $\surd - \frac{1}{3}$, becomes $-2 - \frac{2}{\surd 3}$, a real quantity.

2dly. Let $x^3 - 3x + 2 = 0$ be proposed, whose roots have been, in art. 23, shown to be 2, -1, -1. Here

$$\left.\begin{array}{l} 2 - 1 = +1 \\ 2 + 1 = +3 \end{array}\right\} \quad \left.\begin{array}{l} 2 - 1 = +1 \\ 2 + 1 = +3 \end{array}\right\} \quad \left.\begin{array}{l} -1 - 1 = -2 \\ -1 + 1 = \quad 0 \end{array}\right\}.$$

This latter binomial must evidently remain real, since the difference into which the imaginary factor was to have been drawn vanishes. 3dly. Let $x^3 - 7x + 6$ be given, whose roots are 1, 2, -3. The binomials derived from these have been before given, in the 2d example to the first section of this article; and the cube they produce shown to be $24 - \frac{80}{3}\surd - \frac{1}{3}$, the cube root of which cannot be extracted; it being from the quadratic surd, it involves, in truth, not a cube, but a truncate 6th power in a cubic shape: and when, to remove its equivocal state, it is made rational, shows itself to be properly the 6th power equation $x^6 - 6x^3 + \frac{343}{27} = 0$, as before demonstrated.

26. This is the common reduction of a cubic equation, to one of the 6th degree, but in form a quadratic, obtained, by clearing of its quadratic surd, the pure cubic formed by either of the 2 sets of binomials before described; and this is the only reduction of it yet discovered. Perhaps the method called Cardan's rule is the shortest mode of effecting this reduction; but I am not aware, that the real principle on which it is founded has been any where fully analysed and explained, except in the foregoing investigation of it. The ordinary expositions of it certainly disclose nothing of the principle, and are even in many respects faulty; for they treat it as the effect of a supposition or lucky conjecture, when, in fact, there is no supposition or lucky conjecture made; a regular clue, furnished by certain demonstrable peculiarities in some functions of this order of quantities, being pursued, till such a relationship among the roots may be inferred, as may be converted

into equality at some known period. They also fail to account for the most striking part of the result; the irreducibility happening uniformly in cases where it has been supposed least to be expected, i. e. when the roots are real; which they refer to a particular limitation in one of the steps taken, when it is, in truth, of much deeper origin than any particular method, being the necessary consequence of the constitution of the cube power.

27. The result of these observations on cubic equations shows, that directly they are not resolvable, i. e. they cannot, like quadratics, be always brought to a mere extraction of their correspondent root: that however, by means of the peculiarities inseparable from the number of 3 quantities, a relation is discoverable, which inevitably gives equal roots to the equation of the cubes of a particular function of them; but that, that function involves sometimes a quadratic surd which was not in the roots themselves, but arose from the form necessary to be given them; that the equal relation not taking place in any case, till the cube of that function, and in some cases not being rational, till the square of that cube, the equation is not lowered in degree, by the operation, but rather increased.

28. Let $n = 4$, and the equation becomes the general biquadratic $x^4 - px^3 + qx^2 - rx + s = 0$, the number of differences are 12; we cannot therefore hope to obtain a direct simple resolution. But, in art. 14, two peculiarities belonging to sets of 4 quantities were pointed out, from which it is easy to obtain a reduction of the equation to a cubic form. The first peculiarity there mentioned, was shown to subsist among the sums of the combinations of the roots in pairs. If a, b, c, d, be supposed the roots of the given equation, and their combinations by two, ab, ac, ad, bc, bd, cd, be summed in pairs, though the number of quantities so formed are no fewer than 30, yet there is an evident distinction observable among them; for in some, (the first 6,) no letter occurs twice. If, therefore, instead of simply requiring the sums of the combinations of the roots in pairs, that function of the roots had been required, consisting of the sums of these combinations, into the forming of which no root enters twice, only 6, out of the whole number of combinations of the kind, would answer that condition; and those six would be the same 3 repeated, for $ab + cd$, and $cd + ab$ &c. are the same quantities. So that the 3 quantities $ab + cd$, $ac + bd$, $ad + bc$, would be the functions required, and all of the kind that can be made. Now there is no proposition in the theory of equations more certain, than that the equation of any regular function of the roots may always be found by means of the known values of the co-efficients*. As there are but 3 functions in this case, the resulting equation must consequently be a cubic; and, by taking the several combinations of the quantities $ab + cd$, $ac + bd$, $ad + bc$, we may obtain their equation, viz. $x^3 - qx^2 + (pr - 4s) x - p^2s + 4qs - r^2 = 0$. Therefore the finding the equation of that function of the roots of a biquadratic which arises from its combinations by 2 summed in pairs, so however that no root shall occur twice in any such sum, reduces the biquadratic to a cubic.

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* Waring's Med. Algeb. cap. 1, p. 1, et infra.—Orig.

29. Another peculiarity of 4 quantities is also given in art. 14, i. e. that if taken originally so far as to have their sum equal to nothing, the 6 quantities formed from their sums in pairs, will be the same 3 quantities taken both affirmatively and negatively. Then we know, by the reasoning in art. 11, that the equation of those quantities, though of the 6th degree, will want every alternate term, or be of a cubic form; accordingly the equation of the function of the roots formed by summing them in pairs, is $(x - \frac{1}{2}p)^6 - (2q + \frac{3}{4}p^2) \times (x - \frac{1}{2}p)^4 + (q^2 - 4s - qp^2 + pr + \frac{3}{16}p^4) \times (x - \frac{1}{2}p)^2 - \frac{1}{8}p^3 - \frac{1}{2}p^4 + r)^2 = 0$*, which when p is supposed to vanish, becomes $x^6 - 2qx^4 + (q^2 - 4s)\, x^2 - r^2 = 0$.

30. These 2 methods, one applying to the biquadratic equations complete in their terms, and the other to those from which the 2d term has been expunged, are all that have yet been discovered; and, notwithstanding the number of different methods attributed to different writers, which from their manner of setting out appear distinct, they will all be found to resolve themselves, in principle, into one of these. Dr. Hutton's Mathematical Dictionary, under the article Biquadratic Equations, gives 4 methods; viz. Ferrari's, Des Cartes's, Euler's, and Simpson's; to which may be added another by Dr. Waring†, and perhaps many more. They proceed on a variety of different contrivances; but when analysed, and the real object gained is viewed apart from the process that led to it, Ferrari's, which is the oldest, and does not require the extinction of the 2d term, will be seen to produce the cubic $x^3 - qx^2 + (pr - 4s)\, x - p^2s + 4qs - r^2 = 0$; and Des Cartes's, which supposes the 2d term to be first destroyed, terminates in the cubic-formed equation of the 6th degree, $x^6 - 2qx^4 + (q^2 - 4s)\, x^2 - r^2 = 0$. The rest produce cubics, or cubic-formed 6th powers, whose roots are some parts or multiples of this last; except Waring's method, which does not expunge the 2d term, and therefore produces a cubic whose roots are part of the first. But, whether the resulting equation be that of the function, formed by summing the combinations by 2 of the roots in pairs, or summing the roots themselves in pairs, or the equation of the halves, or quarters, or doubles, trebles, &c. of those functions, is immaterial; no new function is employed, no other principle put in action, than what is derived from the general properties of this degree of quantities here explained.

31. Biquadratics being generally thus reducible to cubics, of course, by resolving those cubics, distinguishing what function their roots are of the roots of the original biquadratic, they may all be found; and, for practical utility, there is no preference to be made of either of the 2 methods; for the first, though a real cubic, being formed from products of the roots, it requires a quadratic equation to obtain them after the cubic is resolved; whereas the 2d, though an equation of the 6th power, being formed from simple addition of the roots, gives them at once. But as both these cubics necessarily have all their roots real, when those of the biquadratic are so, and the resolution of cubics is in that case imaginary, it

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* Waring's Medit. Algeb. p. 138.——† Ibid. p. 133; and the Appendix to Dr. Hutton's Dictionary.—Orig.

follows that no biquadratic having all its roots real, can admit of a real solution by either of these methods.

32. The formula expressing the actual resolution of a biquadratic has not been given; the writers on algebra going no further than to point out the cubics by means of which such a resolution may be obtained. To be sure, such a formula would be very long, and, till the imperfection in the cubic resolution, which must make a large part of it, can be removed, embarrassed with radicals, so as to be of little practical use; but it would be a valuable accession to the theoretical part of algebra, to have the analysis of this degree carried as far as that of the preceding, by developing every part of the functions that enter into the resolution, so as to be able to compose it at once, or make a complete reduction of the equation, without the intervention of any other steps.

33. Let n be taken = 5, or any higher number. Here the number of differences is increased to 20; and the higher we go the more they increase, so that a direct simple resolution is out of the question. Nor are we yet acquainted with any peculiarity attending 5, or any higher number of quantities, on which we can ground a relation to effect a reduction of any sort; therefore no method is known for equations of this and the higher orders. Whether any may ever be discovered, it is not easy to pronounce: if the reasoning from art. 8 to art. 15, of this paper, be correct, there can be no chance, until some peculiar property of quantities of this class can be hit on. It is perhaps a discouraging presumption against the existence of any such property, that no art or labour has hitherto afforded the least clue to lead to one; but, on the other hand, it is impossible to tell what general properties of quantity may remain to be discovered; and, from the great distance the peculiarities of the degrees we have treated of lie from the surface, and their total want of order or connection with each other, it may be justly expected that those of the higher degrees may lie still more detached and remote, beyond any efforts that have yet been made on the subject. The proper method to proceed seems therefore to be, abandoning all projects for the general resolution of equations, to investigate regularly the abstract properties of each separate order or number of quantities, turning them into all shapes, sifting all their combinations, and constructing and examining the equations of different complex functions of them, in order to see if latent peculiarities be not to be traced out in some of them. Wherever any distinguishing property is found, it will, by the principles here explained, infallibly lead to some method for the degree to which it belongs; and whoever may be fortunate enough to discover any such property, in 5, 6, or any higher order of quantities, will have the honour of removing the important and hitherto impenetrable barrier, which has so long obstructed the further improvement of algebra.

XVII. On different Sorts of Lime used in Agriculture. By Smithson Tennant, Esq., F. R. S. p. 305.

It is said, that in the neighbourhood of Doncaster, there are 2 kinds of lime employed in agriculture, which are supposed to differ materially in their effects. One of these, procured near the town, it is necessary to use sparingly, and to spread very evenly over the land; as a large proportion of it, instead of increasing, diminishes the fertility of the soil; and that wherever a heap of it is left in one spot, all vegetation was prevented for many years. Fifty or 60 bushels on an acre, were considered to be as much as could be used with advantage. The other sort of lime, obtained from a village near Ferry-bridge, though considerably dearer' from the distant carriage, is more frequently employed, on account of its superior utility. A large quantity is never found to be injurious; and the spots which were entirely covered with it, instead of being rendered barren, become remarkably fertile. The different properties ascribed to these 2 kinds of lime were so very distinct, that it seemed probable they could not be imaginary; and it therefore appeared to be worth the trouble of ascertaining them more fully, and of attempting to discover the nature of the ingredients from which the difference arose. For this purpose, I procured some pieces of each sort of limestone, and first tried what would be their effect on vegetables, in their natural state, by reducing them to coarse powder, and sowing in them the seeds of different plants. In both kinds the seeds grew equally well, and nearly in the same manner as they would in sand, or any other substance which affords no nourishment to vegetables. Pieces of each sort of stone were then burnt to lime; and after they had been exposed for some weeks to the air, that their causticity might be diminished, some seeds were sown in them. In the kind of lime which was found most beneficial to land, almost all the seeds came up, and continued to grow, as long as they were supplied with water; and the roots of the plants had many fibres, which had penetrated to the bottom of the cup in which they grew. On examining the composition of this sort of lime, it proved to consist entirely of calcareous earth. By its exposure to the air for about 3 months, it was found to have absorbed $\frac{4}{5}$ths of the fixed air required to saturate it. In the other kind, a few only of the seeds grew, and the plants produced from them had hardly any stalks or roots, being formed almost entirely of the 2 seed-leaves, which lay quite loose on the surface. This sort of lime, being spread on a garden soil, to the thickness of about $\frac{1}{10}$th inch, prevented nearly all the seeds which had been sown from coming up, while no injury was occasioned by common lime used in the same manner. On examining the composition of this substance, which was so destructive to the plants, it was discovered to contain 3 parts of pure calcareous earth, and 2 of magnesia. The quantity of fixed air which it had absorbed, by being exposed for about the same time as the pure lime just mentioned, was only $\frac{42}{100}$ths of that combined with it before it was burnt.

As it seemed probable, that the magnesia contained in this lime was the cause of its peculiar properties, the following experiments were made, to determine the effects of that substance on the growth of vegetables. Some seeds, chiefly of colewort, which were preferred from their growing quickly, were sown in uncalcined magnesia; but, though they sprouted, the leaves never rose above the surface, and the plants were entirely without roots: nor did they appear to grow better in magnesia which had been washed in water containing fixed air. Calcined magnesia was however much more destructive, as the seeds would not come up in it. To compare its effects on vegetables with those of lime, each of these earths was mixed, in different proportions, with sand, in small cups, in which seeds were then sown. The lime was obtained from marble; and, before it was put into the sand, was made to fall to powder, by being moistened with water. In a mixture of 4 oz. of sand with 3 or 4 gr. of calcined magnesia, it was a long time before the seeds came up, and the plants had hardly any roots or stalks; and with 10 gr. or more of magnesia, there was no appearance of vegetation. Thirty or 40 gr. of lime did not retard the growth of the seeds more than 3 or 4 gr. of magnesia, and the injurious effects were not so lasting. The lime, by absorbing fixed air, soon lost its destructive properties; so that, after keeping these mixtures 4 or 5 weeks, seeds were found to grow in that with 40 gr. of lime, nearly as well as pure sand; but, in that with 5 gr. of magnesia, they produced only the seed-leaves, as before described. It was necessary occasionally to break in pieces the sand which had so much lime, as it would otherwise have been too hard to admit the seeds to penetrate through it. Plants will bear a much larger proportion of magnesia in vegetable soil than in sand: with 20 gr. however of calcined magnesia, in as much soil as was equal in bulk to 4 oz. of sand, the seeds produced only the seed-leaves, without roots; and with about 40 gr. they were entirely prevented from coming up.

In countries where the magnesian lime is employed, it was said that the barrenness of any spot on which a heap of it had been laid, would continue for many years. To learn how far it could by time be deprived of its injurious qualities, I procured some pieces of mortar made of this species of lime, from 2 houses, 1 of which had been built 3, and the other 8 years: they were taken from the outside of the building, where they had been exposed to the air. After they were reduced to powder, seeds were sown in them. Only a few came up, these produced merely the seed-leaves, without any roots. As plants would grow in the limestone from which this species of lime was formed, though not in the mortar made from it, I wished to know what proportion of the fixed air originally contained in the limestone, had been absorbed by the mortar. For this purpose, a piece of it was finely powdered, to render it of a uniform quality: it was then tried how much of this powder and of the limestone would saturate the same quantity of acid: by this means, I ascertained the proportions of limestone and mortar containing equal quantities of the magnesian lime. The fixed air being obtained from them in those proportions, and measured in an inverted vessel, with quicksilver, it was found that the mortar which had been exposed 3 years had absorbed 43, and that of 8 years, only $\frac{47}{100}$ths of the quantity originally contained in the limestone. I was not able to obtain any mortar which had been made earlier, though it might deserve to be known how much fixed air it was ultimately capable of absorbing.

Common mortar which had been exposed to the air for a year and three-quarters, had regained $\frac{63}{100}$ths of its full quantity of fixed air. As the preceding experiments were tried during the winter, in a room warmed by fire, perhaps under circumstances more favourable to vegetation, the same quantity of magnesia would not be equally pernicious.

Magnesian limestone may be easily distinguished from that which is purely calcareous, by the slowness of its solution in acids, which is so considerable, that even the softest kind of the former is much longer in dissolving than marble. From this property of the magnesian limestone, there appeared to be reason for suspecting that the kind of marble which had been called Dolomite, from M. Dolomieu, who first remarked its peculiarity in dissolving slowly, might also be similar in its composition. An analysis of this substance was lately given in the Journal de Physique, but this is probably erroneous; for, on examining three specimens, they were found to consist of magnesia and calcareous earth, like the magnesian limestone; so that it ought, no doubt, to be considered as the same species of stone, but in a state of greater purity. The pieces of Dolomite were from different places; one of them being found among the ruins of Rome, where it is thought to have come from Greece, as many statues of Grecian workmanship are made of it, and no quarries of a similar kind are known in Italy; the 2d was said to have been thrown up by Mount Vesuvius; and the 3d was from Iona, one of the western islands of Scotland. In many kinds of common marble, small particles and veins may be observed, which are a long time in dissolving. These, on examination, I discovered to contain a considerable proportion of magnesia; but, as they were probably not quite free from the surrounding marble, I did not ascertain the quantity precisely. The crystallized structure which may generally be observed in the magnesian limestone, seems to show that it has not been formed by the accidental union of the 2 earths, but must have resulted from their chemical combination. The difficulty of dissolving it, may also arise from the attraction of the different component parts to each other. The mortar formed from this kind of lime, is as soluble in acids as common marble; and the substances of which it consists are easily separated. The magnesia may be taken from it by boiling it in muriated lime, and lime is precipitated by it from lime water; but neither of these effects can be produced by the stone, before it is calcined.

Magnesian limestone is probably very abundant in various parts of England. It appears to extend for 30 or 40 miles, from a little south-west of Worksop, in Nottinghamshire, to near Ferry-bridge, in Yorkshire. About 5 or 6 miles farther north there is a quarry of it, near Sherburn; but whether this is a continuation from the stratum near Ferry-bridge, I have not learnt. From some specimens which were sent me, I find that the cathedral and walls of York are made of it. I have not been able to learn whether there were any shells in the limestone of the tract of country before mentioned. In Mr. Marshall's account of the agriculture of the midland counties, he speaks of the lime made at Breedon, near Derby, as destructive to vegetables, when used in large quantities. I therefore procured some pieces of it, and they were discoverd to contain nearly the same proportion of magnesia as that before described. In this quarry, the stone is frequently crystallized in a rhomboidal form; and petrified shells, not calcareous, but similar in composition to the stone itself, are sometimes, but very rarely, found in it. This substance seems to be common in Northumberland. In the 3d vol. of the Annals of Agriculture, Dr. Fenwick, of Newcastle, observes, that the farmers of that country divide limes into hot and mild. The former of these is no doubt magnesian, as it has similar effects on the soil; and he remarks that it is not so easily dissolved in acids as the latter. At Matlock, in Derbyshire, the 2 kinds are contiguous to each other; the rocks on the side of the river where the houses are built being magnesian, and on the other, calcareous. The magnesian rock appears also to be incumbent on a calcareous stratum; for, in descending a cave formed in this rock, a distinct vein of common limestone may be observed, which contains no magnesia. The latter stratum is very full of shells; but though there are some also in the magnesian rock, yet they are very rare. In the following tables, containing the analysis of various specimens, some other places are mentioned where this substance is found, but of which I received no further information.

After it was known that the magnesian marble and limestone consisted of the 2 earths, their proportion was attempted to be discovered, by trying how much gypsum and Epsom salt could be obtained, by means of vitriolic acid, from a certain weight of each specimen. When the superfluous vitriolic acid had been evaporated by heat, the Epsom salt was separated from the gypsum by water. The result of these trials is expressed in the following table.

	Dry gypsum.	Dry Epsom salt.
5 gr. of limestone from Breedon gave	3.9	3.15
———————— Matlock	3.95	2.9
———————— Worksop	3.8	3.0
———————— York	3.8	3.1
3 gr. of calcareous spar and 1 gr. of calcined magnesia gave	3.9	2.7

As the preceding method of estimating the quantities of magnesia and calcareous earth is liable to considerable error, I afterwards examined them in the following manner, which seems capable of great exactness. Twenty-five grains of each substance were dissolved by marine acid, in a cup of platina, and after the solution was evaporated to dryness, it was made red-hot for a few minutes. The mass remaining in the cup, which consisted of muriated lime, and of the magnesia freed from the acid, was washed out with water, and poured into a phial. There was then added to it a known quantity of diluted marine acid, somewhat more than was sufficient to re-dissolve the magnesia, and, after the solution, a certain weight of calcareous spar, part of which would be dissolved by the superfluous acid. By the quantity of spar remaining undissolved, it was learnt how much acid was required to dissolve the magnesia. The iron and argillaceous earth contained in some specimens, were precipitated by the spar, and therefore could not occasion any error.

The calcareous spar however dissolved more slowly where there was argillaceous earth, as it became coated with it; but this incrustation was occasionally removed, and, in all the experiments, the spar was left in the solution till it suffered no further diminution. For this purpose it was necessary to keep them slightly warm for some days, during which time the phials were generally closed, to prevent any escape of the acid.

The first experiment in the following table was made on known quantities of magnesia and calcareous earth, to try the accuracy of the process. For this purpose also, the 2d was repeated on a piece of limestone, previously powdered, to render every part of it of the same quality. The first column shows the quantity of calcareous spar which might have been dissolved by the acid required to take up the magnesia. The 2d shows the corresponding quantities of magnesia in 25 gr. of each substance. The 3d expresses the quantity of lime. This was inferred by subtracting the weight of the magnesia, and of the iron and clay, from 13.2 gr., the weight of the whole quantity of earth in 25 gr. of limestone. This is probably not very incorrect, as, in 2 specimens which differed most in the proportion of magnesia and lime, the weight of the 2 earths was nearly the same.

A piece of Dolomite, from Rome, was wrapped in a thin leaf of platina, that no part of it might be lost, and, being then exposed to a strong heat, left of earth 52.9 per cent.

Dolomite from Mount Vesuvius	52.8
Breedon limestone	52.4
Calcareous spar left of lime	55.8

In 3 of the experiments also the calcareous earth was precipitated by mineral alkali; and the quantity of it being tried by that of the marine acid required to dissolve it, it corresponded very nearly with that set down. A quantity of marine acid which would dissolve 15 gr. of calcareous spar, would also dissolve 5.5 of calcined magnesia, and 2.5 gr. of spar; so that, 12.5 gr. of spar required the same quantity of acid as 5.5 gr. of magnesia. The magnesia used was very pure, and made red-hot immediately before it was weighed.

Substances examined.	Quantity of spar which the acid, required to take up the magnesia, would have dissolved.	Quantity of magnesia.	Quantity of lime.	Iron and clay.
Mixture of 5.5 gr. of magnesia and 14 gr. of calcareous spar	12.5	5.5	7.8	.0
25 gr. of Breedon limestone, previously powdered	11.53	5.071	7.929	.2
25 gr. from part of the same powder	11.56	5.082	7.913	.2
25 gr. of Dolomite from Rome	12.2	5.37	7.73	.1
——— Dolomite from Iona	10.1	4.4	7.8	1.0 { Insoluble substance.
——— Vesuvian Dolomite	10.38	4.565	8.575	.06
A 2d experiment, from part of the same Vesuvian Dolomite	10.03	4.411	8.849	.06
25 gr. of magnesian limestone from Wansworth, near Doncaster	12.75	5.61	7.34	.25
—— Thorpe arch	10.95	4.84	7.8	.6
—— Matlock	12.5	5.5	7.388	.31
—— York Minster	11.	4.84	8.26	.1
—— Worksop	11.6	5.104	7.496	.6
—— Sherborn	11.5	5.08	7.56	.56
—— Westminster-hall	10.1	4.44	8.37	.4

XVIII. *Experiments and Observations on Shell and Bone. By Charles Hatchett, Esq. F. R. S.* p. 315.

When shells were examined, they were immersed in acetous acid, or nitric acid diluted, according to circumstances, with 4, 5, 6, or more parts of distilled water; and the solution was always made without heat. The carbonate of lime was precipitated by carbonate of ammonia, or of potash; and phosphate of lime, if present, was previously precipitated by pure or caustic ammonia. If any other phosphate, like that of soda, was suspected, it was discovered by solution of acetite of lead. Bones and teeth were also subjected to the action of the acetous, or diluted nitric and muriatic acids. The dissolved portion was examined by the above-mentioned precipitants; and, in experiments where the quantity of the substance would permit, the phosphoric acid was also separated by nitric or sulphuric acid. The phosphoric acid thus obtained, was proved, after concentration, by experiments which, being usually employed for such purposes, are too well known to require description. It is necessary moreover to observe, that as the substances examined were very numerous, and my principal object was to discover the most prominent characters in them, I did not, for the present, attempt in general to ascertain minutely the proportions, so much as the number and quality, of their respective ingredients.

The greater part, if not all, of marine shells, appear to be of 2 descriptions, in respect to the substance of which they are composed. Those which will be first noticed, have a porcellaneous aspect, with an enamelled surface, and when broken are often in a slight degree of a fibrous texture. The shells of the other division have generally, if not always, a strong epidermis, under which is the shell, principally or entirely composed of the substance called nacre, or mother of pearl. Of the porcellaneous shells, various species of voluta, cypræa, and others of a similar nature, were examined. Of the shells composed of nacre or mother of pearl, I selected the oyster, the river muscle, the haliotis iris, and the turbo olearius.

Experiments on porcellaneous shells.—Shells of this description, when exposed to a red heat in a crucible, about a quarter of an hour, crackled and lost the colours of their enamelled surface; they did not emit any apparent smoke, nor any smell like that of burnt horn or cartilage. Their figure remained unchanged, excepting a few flaws; and they became of an opaque white, tinged partially with pale gray, but retained part of their original gloss. The shells which had not been exposed to fire, whether entire or in powder, dissolved with great effervescence in the various acids; and the solution afterwards remained colourless and transparent. But the shells which had been burned, on being dissolved, deposited a very small quantity of animal coal; by which the presence of some gluten was denoted, though the proportion was too small to be discovered in the solution of the shells which had not been burned. The various solutions were filtrated, and were examined by pure ammonia and acetite of lead; but I never obtained any trace of phosphate of lime, nor of any other combination of phosphoric acid. The carbonate of lime was afterwards precipitated by carbonate of ammonia; and from many experiments it appeared, that porcellaneous shells consist of carbonate of lime, cemented by a very small portion of animal gluten.

Previous to the experiments on shells composed of nacre or mother of pearl, I examined some patellæ from Madeira. When these were exposed to a red heat in a crucible, there was a perceptible smell, like that of horn, hair, or feathers. The proportion of carbonic matter deposited by the subsequent solution, was more considerable than that of the shells above-mentioned; and the proportion of carbonate of lime, relative to their weight, was less. When the recent shells were immersed in very dilute nitric acid, the epidermis was separated, the whole of the carbonate of lime was dissolved, and a gelatinous substance, nearly liquid, remained; but without retaining the figure of the shell, and without any fibrous appearance. These shells evidently therefore contain a larger proportion of a more viscid gelatinous substance than those before mentioned; but the solution, separated from the gelatinous substance, afforded nothing but carbonate of lime.

Experiments on shells composed of nacre, or mother of pearl.—When the shell of the common oyster was exposed to a red heat, the effects were the same as those observed in the patellæ, and the solution of the unburnt shell was similar, only the gelatinous part was rather of a greater consistency. A species of the river muscle was next subjected to experiment. This, when burned in a crucible, emitted much smoke, with a strong smell of burnt cartilage or horn; the shell throughout became of a dark gray, and exfoliated. By solution in the acids, a large quantity of carbonic matter was separated; and much less of carbonate of lime was obtained, from a given weight of the shell, than from those already mentioned. On immersing an unburnt shell in dilute nitric acid, a rapid solution and effervescence at first took place, but gradually became less, so that the disengagement of the carbonic acid gas was to be perceived only at intervals. At the end of 2 days, I found nearly the whole of the carbonate of lime dissolved; but a series of membranes, retaining the figure of the shell, remained, of which the epidermis constituted the first. In the beginning, the carbonate of lime was readily dissolved, because the acid menstruum had an easy access; but after this, it had more difficulty to insinuate itself between the different membranes, and of course the solution of the carbonate of lime was slower. During the solution, the carbonic acid gas was entangled, and retained in many places between the membranes, so as to give to the whole a cellular appearance.

The haliotis iris, and the turbo olearius, resembled this muscle, excepting that their membranaceous parts were more compact and dense. These shells, when deprived by an acid menstruum of their hardening substance, or carbonate of lime, appear to be formed of various membranes, applied stratum super stratum. Each membrane has a corresponding coat or crust of carbonate of lime; which is so situated, that it is always between every 2 membranes, beginning with the epidermis, and ending with the last formed internal membrane. The animals which inhabit these stratified shells, increase their habitation by the addition of a stratum of carbonate of lime, secured by a new membrane; and as every additional stratum exceeds in extent that which was previously formed, the shell becomes stronger n proportion as it is enlarged; and the growth and age of the animal become denoted by the number of the strata which concur to form the shell.

Though the haliotis iris and the turbo olearius are composed of the true mother of pearl, I was induced to repeat the foregoing experiments on some detached pieces of mother of pearl, such as are brought from China. These experiments I need not describe, as the results were precisely the same. I must however observe, that the membranaceous or cartilaginous parts of these shells, as well as of the pieces of mother of pearl, retained the exact figure of the shell, or piece, which had been immersed in the acid menstruum; and these membranaceous parts distinctly appeared to be composed of fibres placed in a parallel direction, corresponding to the configuration of the shell. The same experiments were made on pearls; which proved to be similar in composition to the mother of pearl; and, so far as their size would enable me to discern, they appeared to be formed by concentric coats of membrane and carbonate of lime; by this structure they much resemble the globular calcareous concretions, found at Carlsbad and other places, called pisolithes. The wavy appearance and iridescency of mother of pearl, and of pearl, are evidently the effect of their lamellated structure and semi-transparency; in which, in some degree, they are resembled by the lamellated stone called adularia.

When the experiments on the porcellaneous shells, and on those formed of mother of pearl are compared, it appears that the porcellaneous shells are composed of carbonate of lime, cemented by a very small portion of gluten; and that mother of pearl and pearl do not differ from these, except by a smaller proportion of carbonate of lime; which, instead of being simply cemented by animal gluten, is intermixed with, and serves to harden, a membranaceous or cartilaginous substance; and this substance, even when deprived of the carbonate of lime, still retains the figure of the shell. But between these extremes there will probably be found many gradations; and these we have the greater reason to expect, from the example afforded by the patellæ, which have been lately mentioned. Some few experiments were made on certain land shells; and in the common garden snail I thought that I discovered some traces of phosphate of lime; but, as I did not find any in the helix nemoralis, it may be doubted whether the presence of phosphate of lime should be considered as a chemical character of land shells.*

Experiments on the covering substance of crustaceous marine animals.†—As I was

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* Some experiments which I have lately made on the cuttle-bone of the shops, have proved, that the term bone is here misapplied, if the presence of phosphate of lime is to be regarded as the characteristic of bone; for this substance, in composition, is exactly similar to shell, and consists of various membranes hardened by carbonate of lime, without the smallest mixture of phosphate.——† Under this head I have included my experiments on echini, star-fish, crabs, lobsters, &c.—Orig.

not acquainted with any experiments by which the chemical nature of the substance which covers crustaceous marine animals had been determined, I was desirous to ascertain in what respect it was different from shell, and I began these experiments on 3 species of the echinus, with which I had been favoured by the Right Hon. President. I was the more inclined to begin with the echini, because naturalists do not appear to be perfectly agreed, whether to call them testaceous or crustaceous animals. Klein, who has written a work on echini, after having noticed the various opinions of Rondelet, Rumphius, and others, determines that they are to be regarded as testaceous animals. But Linnæus was of the contrary opinion, as appears from his definition of the echinus. "Corpus subrotundum, crusta ossea tectum, spinis mobilibus sæpius asperum."

Now, as the experiments above related had proved, that the shells of marine animals were composed of carbonate of lime, without any phosphate, I thought it very possible, that the covering of the crustaceous animals might, in some respect, be different, and if so, I should thus, by chemical characters, be enabled to ascertain the class to which the echinus was to be referred. Of the 3 echini which were examined, one had small spines; the 2d had large obtuse spines; and the 3d was of a very flat form. Portions of these echini were separately immersed in acetous, muriatic, and diluted nitric acid, by each of which they were completely dissolved, with much effervescence; depositing, at the same time, a thin outer skin or epidermis. The transparency of the solutions was also disturbed by a portion of gluten, which remained suspended, and communicated a brownish colour to the liquors. The solutions in acetous and diluted nitric acid were filtrated; after which, from the acetous solution of each echinus, I obtained a precipitate of phosphate of lead, by the addition of acetite of lead; and, having thus proved the presence of phosphoric acid, I saturated the nitric solutions with pure ammonia, by which a quantity of phosphate of lime was obtained, much inferior however in quantity, to the carbonate of lime, which was afterwards precipitated by carbonate of ammonia. The composition of the crust of the echinus is therefore different from that of marine shells; and, by the relative proportions and nature of the ingredients, it approaches most nearly to the shells of the eggs of birds; which, in like manner, consist of carbonate, with a small proportion of phosphate of lime, cemented by gluten.

It remained now to examine the composition of those substances which are decidedly called crustaceous; but previous to this, some experiments were made on the asterias or star-fish, of which I took the species commonly found on our coasts, and known by the popular name of five fingers, asterias rubens. The asterias is thus described by Linnæus. "Corpus depressum, subtus sulcatum: crusta coriacea, tentaculis muricata." When the asterias was immersed in the acids, a considerable effervescence was produced, and a thin external stratum was dissolved; after which it remained in a perfectly coriaceous state, and complete, in respect to the original figure. The dissolved portion, being examined by the usual precipitants, proved to be carbonate of lime, without any mixture of phosphate; but in another species of the asterias, which had 12 rays, asterias papposa, I discovered a small quantity of phosphate of lime, I am therefore induced to suspect that, in the different species of the asterias, nature makes an imperfect attempt to form shell on some and a crustaceous coating on others; and that a series of gradations is thus formed between the testaceous, the crustaceous, and the coriaceous marine animals.

It was now requisite to ascertain if phosphate of lime is a component part of the substance which covers the crustaceous marine or aquatic animals, such as the crab, lobster, prawn, and crayfish. Pieces of this substance, taken from various parts of those animals, was at different times immersed in acetous, and in diluted nitric acid; those which had been placed in the diluted nitric acid, produced a moderate effervescence, and in a short time were found to be soft and elastic, of a yellowish-white colour, and like a cartilage which retained the original figure. The same effects were produced by acetous acid, but in a less degree; in the latter case also the colouring matter remained, and was soluble in alcohol. All the solutions, both acetous and nitric, afforded carbonate and phosphate of lime, though the former in the larger proportion. There is reason to conclude therefore, that phosphate of lime, mingled with the carbonate, is a chemical characteristic which distinguishes the crustaceous from the testaceous substances; and that the principal difference in the qualities of each, when complete, is caused by the proportion of the hardening substances, relative to the gluten, by which they are cemented; or by the abundance and consistency of the gelatinous, membranaceous, or cartilaginous substance, in and on which, the carbonate of lime, or the mixture of carbonate and phosphate of lime, has been secreted and deposited. And, as the presence of phosphate of lime, mingled with carbonate appears to be a chemical character of crustaceous marine animals, there is every reason to conclude that Linnæus did right not to place the echini among the testaceous ones.

The presence of phosphate of lime, in the substance which covers the crustaceous marine animals, appears to denote an approximation to the nature of bone, which, not only by the experiments of Mr. Gahn, but by the united testimony of all chemists, has been proved principally to consist, as far as the ossifying substance is concerned, of phosphate of lime. This consideration therefore induced me to repeat the above experiments, on the bones of various animals. It is scarcely necessary to mention the usual effects of acids on bones steeped in them, as they are known to every physiologist and anatomist. In every operation of this nature, the ossifying substance, which is principally phosphate of lime, is dissolved, and a cartilage or membrane, of the figure of the original bone, remains; so that the first origin of bones appears to be by the formation of a membrane or cartilage, of the requisite figure, which, when the subsequent secretion of the ossifying substance takes place, is penetrated by it, and thus becomes more or less converted into the state of bone. It is also known, that the nature of the bone is more influenced by the greater or less predominance of the membranaceous or cartilaginous part, than by any other cause. It is not therefore for me to add any thing to this part; and in respect to the substance which is the cause of ossification, little also requires to be mentioned, for this, as before observed, is known principally to consist of phosphate of lime. I shall only therefore briefly mention the results of certain experiments.

The bones of fish, such as those of the salmon, mackerel, brill, and skate, afforded phosphate of lime; and the only difference was, that the bones of these fish appeared in general to contain more of the cartilaginous substance, relative to the phosphate of lime, than is commonly found in the bones of quadrupeds, &c. The different bones also of the same fish were various in this respect; and the bones about the head of the skate only differed from cartilage, by containing a moderate proportion of phosphate of lime. It is at present believed that phosphate, with some sulphate of lime, constitutes the whole of the ossifying substance; and perhaps the formation of bone from cartilage depends only on the phosphate of lime; but whether this is the case or not, it is fit that I should notice a 3d substance, which constantly occurred in the course of my experiments.

When human bones, or teeth, as well as those of quadrupeds and fish, whether recent or calcined, were exposed to the action of acids, an effervescence, though at times but feeble, was produced. This circumstance at first I did not particularly notice, but the following experiments excited my attention. After the phosphate of lime had been precipitated from the solutions of various teeth and bones, by pure ammonia, I observed that a 2d precipitate, much smaller in quantity, was obtained by the addition of carbonate of ammonia. This 2d precipitate dissolved in acids, with much effervescence, during which carbonic acid was disengaged; and selenite was formed by adding sulphuric acid. The solution of this precipitate did not contain any phosphoric acid; nor did the liquor from which the precipitate had been separated afford any trace of it. This precipitate was therefore carbonate of lime; but I still was not certain that it existed, as such, in the teeth and bones.

Though regular and comparative analyses of the bones of different animals have not hitherto been made, yet, by the experiments of Messrs. Gahn, Scheele, Macquer, Fourcroy, Berniard, and the Marquis de Bullion, it has been proved, that phosphate of lime is the principal ossifying substance of bones in general, and that this is accompanied by a small proportion of some saline substances, and by sulphate of lime. I was therefore desirous to ascertain, whether the carbonate of lime, which I had obtained by the above-mentioned experiments, had been produced from the sulphate of lime decomposed by the alkaline precipitant, or whether the greater part had not existed in the bones, in the state of carbonate.

Each of the solutions in nitric acid afforded a precipitate with nitrate of barytes; but the quantity of sulphuric acid thus separated, appeared by far too small to be capable of saturating the whole of the carbonate of lime obtained from an equal quantity of the solution. To prove therefore the presence of the carbonic acid, and the consequent formation of carbonate of lime, portions of the various teeth and bones were immersed, at separate times, in muriatic acid; and the gas produced was received in lime-water, by which it was speedily absorbed, and a proportionate quantity of carbonate of lime was obtained. Though it appears, that the principal effects during ossification are produced by phosphate of lime, yet we here see, that not only sulphate, but also some carbonate of lime, enters the composition of bones; and it is not a little curious to observe, that as the carbonate of lime exceeds in quantity the phosphate of lime in crustaceous marine animals, and in the egg shells of birds, so in bones it is vice versa. It is possible, when many accurate comparative analyses of bones have been made, that some may be found composed only of phosphate of lime; and that thus, shells containing only carbonate of lime, and bones containing only phosphate of lime, will form the 2 extremities of the chain.

I shall now make a few remarks on the enamel of teeth. When a tooth coated with enamel is immersed in diluted nitric or muriatic acid, a feeble effervescence takes place, and the enamel is completely dissolved; so also is the bony part, but the cartilage of that part is left, retaining the shape of the tooth. Or, if a tooth in which the enamel is intermixed with the bony substance, is plunged in the acid, the enamel and the bony part are dissolved, in the same manner as before; that is to say, the enamel is completely taken up by the acid, while the tooth, like other bones, remains in a pulpy or cartilaginous state, having been deprived of the ossifying substance. Consequently, those parts which were coated or penetrated by lines of enamel, are diminished in proportion to the thickness of the enamel which has been thus dissolved; but little or no diminution is observed in the tooth.* Mr. Hunter has noticed this; and, speaking of enamel, says, " when soaked in a gentle acid, there appears no gristly or fleshy part with which the earthy part had been incorporated."† Now, when the difference which has been lately stated, between porcellaneous shell and mother of pearl, is considered, it is not possible to avoid the comparing of these to enamel and tooth. When porcellaneous shell, whole or in powder, is exposed to the action of acids, it is completely dissolved, without leaving any residuum.

Enamel is also completely dissolved, in the like manner. Porcellaneous shell and enamel, when burnt, emit little or no smoke, nor scarcely any smell of burnt horn, or cartilage. Their figure, after having been exposed to fire, is not materially changed, except by cracking in some parts: their external gloss partly remains, and their colour at most becomes gray, very different from what happens to mother of pearl, or tooth. In their fracture they have a fibrous texture; and in short the only essential difference between them appears to be, that porcellaneous shell consists of carbonate of lime, and enamel of phosphate of lime, each being cemented by a small portion of gluten.

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* I have also observed, that when raspings of enamel are put into diluted nitric or muriatic acid, they are dissolved without any apparent residuum; but when raspings of tooth or bone are thus treated, portions of membrane or cartilage remain, corresponding to the size of the raspings.——† Nat. Hist. of the Human Teeth, p. 35.—Orig.

In like manner, if the effects produced by fire and acid menstrua, on shells composed of mother of pearl, and on the substance of teeth and bone, are compared, a great similarity will be found; for, when exposed to a red heat, 1°. They smoke much, and emit a smell of burnt cartilage, or horn. 2°. They become of a dark grey or black colour. 3°. The animal coal thus formed is of difficult incineration. 4°. They retain much of their original figure; but the membranaceous shells are subject to exfoliate.* 5°. These substances, pearl, mother of pearl, tooth, and bone, when immersed in certain acids, part with their hardening or ossifying substances, and then remain in the state of membrane or cartilage. 6°. When previously burned, and afterwards dissolved in acids, a quantity of animal coal is separated, according to the proportion of the gelatinous, membranaceous, or cartilaginous substance, and according to the duration of the red heat. And lastly, the acid solutions of these substances, by proper precipitants, afford carbonate of lime, in the one case, and phosphate of lime principally, in the other, in a proportion relative to the membrane or cartilage with which, or on which, the one or the other had been mixed, or deposited.

As porcellaneous shell principally differs from mother of pearl, only by a relative proportion between the carbonate of lime and the gluten or membrane, in like manner, the enamel appears only to be different from tooth or bone, by being destitute of cartilage, and by being principally formed of phosphate of lime, cemented by gluten. The difference, in the latter case, seems to explain why the bones and teeth of animals fed on madder become red, when, at the same time, the like colour is not communicated to the enamel; for it appears probable, that the cartilages which form the original structure of the teeth and bones, become the channels by which the tinging principle is communicated and diffused. These comparative experiments prove, that there is a great approximation in the nature of porcellaneous shell and the enamel of teeth, and also in that of mother of pearl and bone; and if a shell should be found composed of mother of pearl coated by the porcellaneous substance, it will resemble a tooth coated by the enamel, with the difference of carbonate being substituted for phosphate of lime.

Some experiments on cartilaginous substances have in a great measure convinced me, that membranes and cartilages (whether destined to become bones by a natural process, as in young animals, or whether they become such by morbid ossification, as often happens in those which are aged), do not contain the ossifying substance, or phosphate of lime, as a constituent principle. I mean by this, that I believe the portion of phosphate of lime found in cartilaginous and horny substances to be simply mixed as an extraneous matter; and that when it is absent, membrane, cartilage, and horn, are most perfect and complete. The frequent presence of phosphate of lime in cartilaginous substances, is not a proof of its being 1 of their constituent principles, but only that it has become deposited and mixed with them, in proportion to the tendency they may have to form modifications of bone; or

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* This is a natural consequence, arising from their structure.—Orig.

according to their vicinity with such membranes or cartilages as are liable to such a change. If horns are examined, few I believe will be found to contain phosphate of lime in such a proportion as to be considered an essential ingredient. I would not be understood to speak here of such as stag or buck horn, for that has every chemical character of bone, with some excess of cartilage; but I allude to those in which the substance of the horn is distinctly separate from the bone, and which, like a sheath, covers a bony protuberance which issues from the os frontis of certain animals.[*]

Horns of this nature, such as those of the ox, the ram, and the chamois, also tortoise shell, afford, after distillation and incineration, so very small a residuum, of which only a small part is phosphate of lime, that this latter can scarcely be regarded as a necessary ingredient. By some experiments made on 500 gr. of the horn of the ox, I obtained, after a long continued heat, only 1.50 gr. of residuum; and of this, less than half proved to be phosphate of lime. 78 gr. of the horn of the chamois afforded only 0.50 of residuum; and 500 gr. of tortoise shell yielded not more than 0.25 of a gr., of which, less than half was phosphate of lime. Now it must be evident, that so very small a quantity cannot influence the nature of the substances which afforded it; and the same may be said of synovia, 480 gr. of which did not yield more than 1 gr. of phosphate of lime. This substance is undoubtedly various in its proportions, in all these and other animal substances, arising probably from the age and habit of the animal which has produced them; but I believe that I may, at least, venture to place some confidence in the foregoing experiments, as several others, made since the above was written, have tended to confirm them.[†]

In the course of making the experiments which have been related, I examined the fossil bones of Gibraltar, as well as some glossopetræ or shark's teeth. The latter afforded phosphate and carbonate of lime; but the carbonate of lime was visibly owing principally to the matter of the calcareous strata which had inclosed these teeth, and which had insinuated itself into the cavities left by the decomposition of the original cartilaginous substance. The bones of the Gibraltar rock also consist principally of phosphate of lime; and the cavities have been partly filled by the carbonate of lime which cements them together. Fossil bones resemble bones which, by combustion, have been deprived of their cartilaginous part; for they retain the figure of the original bone, without being bone in reality, as one of the

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[*] Nature seems here to have made an analysis or separation of horn from bone.—Orig.

[†] These experiments were repeated on bladder, which I chose in preference to any other membrane, as not being liable to ossification, and therefore likely to contain very little or no phosphate of lime. 250 gr. of dry hogs' bladder, after incineration, left a residuum the weight of which did not exceed $\frac{1}{10}$ of a gr. This was dissolved in diluted nitric acid; and, on adding pure ammonia, some faint traces of phosphate of lime were observed. Now as 250 gr. of bladder did not afford more than $\frac{1}{10}$ of a gr. of residuum, of which only a part consisted of phosphate of lime, there is much reason to regard this experiment as an additional proof, that phosphate of lime is not an essential ingredient of membrane.—Orig.

most essential parts has been taken away. Now such fossil or burnt bones can no more be regarded as bone, than charcoal can be considered as the vegetable of which it retains the figure and fibrous structure. Bones which keep their figure after combustion, resemble charcoal made from vegetables replete with fibre; and cartilaginous bones which lose their shape by the same cause, may be compared to succulent plants which are reduced in bulk and shape in a similar manner.

From these last experiments, I much question if bodies consisting of phosphate of lime, like bones, have concurred materially to form strata of limestone or chalk; for it appears to be improbable that phosphate is converted into carbonate of lime, after these bodies have become extraneous fossils. The destruction or decomposition of the cartilaginous parts of teeth and bones in a fossil state, must have been the work of a very long period of time, unless accelerated by the action of some mineral principle; for after having, in the usual manner, steeped in muriatic acid the os humeri of a man brought from Hythe, in Kent, and said to have been taken from a Saxon tomb, I found the remaining cartilage nearly as complete as that of a recent bone. The difficult destructibility of substances of a somewhat similar nature, appears also from the mining implements formed of horn, which are not unfrequently found in excavations of high antiquity.

XIX. A Catalogue of Oriental Manuscripts presented to the Royal Society by Sir Wm. and Lady Jones., By Charles Wilkins, Esq., F. R. S. p. 335.

Continued from page 430 of this vol.

SANSCRITA.

57. Gitá and Dharmánusàsana. Two extracts from the Mahá-bhárata, with beautiful drawings; written in the Dévanágari character. S. W. J.
58. Raghuvansa. The Children of the Sun, a poem, by Cálidás. Bengal character. S. W. J.
59. Prabódha Chandródaya. The Rising Moon of Knowledge, a drama, by Césava Misra. Bengal character. S. W. J.

CHINESE.

60. Con Fu Tsu. The works of Confucius, vol. 2, 3, 4, 5, 6. S. W. J.*
61. Tahia Su Shuw. A commentary. S. W. J.
62. Shung Lon Su Shuw. A commentary. S. W. J.
63. Hor Lon Su Shuw. A commentary. S. W. J.
64. Shung Morng Su Shuw. A commentary. S. W. J.
65. Hor. Morng. Su Shuw. A commentary. S. W. J.
66. Shi Kin. A book of Chinese odes. L. J.
67. Lon Yu. A grammar of the Chinese Language. L. J.
68. A dictionary. Chinese and Latin. L. J.

PERSIAN.

69. Zafar Námeh. A most elegant history of Taimur; written in the Niskh character. L. J.
70. Towárikh i Gujarát. A history of the province of Guzerat. L. J.
71. Tárikh i Bahádersháhi. A history of the Emperor Baháder Sháh. L. J.
72. Tárikh i Jenáhcushá. The history of Nadir Sháh, by Mirza Mahádi Khán. L. J.
73. Narrative of the proceedings of Scindia, and the confederates. L. J.
74. Jehángír Námeh. The history of Jehángir Sháh. L. J.

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* From No. 60 to 67, inclusive, are printed from blocks. No. 68 is manuscript.—Orig.

75. Mujmel ut Tarikh i Nádiri. An Abridgment of the History of Nádir Sháh. L^y. J.
76. History of Hindostan, by Gholam Hussain. S. W. J.
77. Behar i Danish. The tales of Ináyetulla. L^y. J.
78. Bostán i Khyáal. The Garden of Imagination, an historical romance, in 8 vols. L^y. J.
79. Jámay ul Hecáyet. A collection of tales; written in the Niskh character. S. W. J.
80. a. Sháh Námeh. The heroic poem of Ferdosi. L^y. J.
80. b. Ditto. In 4 vols. S. W. J.
81. a. Masnavi. A poem, by Jalal ud Din, surnamed Rumi. L^y. J.
81. b. Ditto. Six vols. S. W. J.
81. c. Ditto. First book only. L^y. J.
81. d. Ditto. A commentary on the first book. L^y. J.
81. e. Ditto. A commentary on the first book. L^y. J.
81. f. Ditto. A table of contents of the first book. L^y. J.
82. a. Culyát i Jámi. The works of the poet Jámi. S. W. J.
82. b. Ditto. The miscellaneous poems of Jámi. L^y. J.
83. Yusuf wa Zuleyca. A poem, by Jámi. L^y. J.
84. a. Culyát i Nizámi. The works of the poet Nizámi. S. W. J.
84. b. Ditto. The 5 poems of Nizámi. L^y. J.
85. Culyát i Anwari. The works of the poet Anwári. S. W. J
86. Dewan i Khosru. The odes of Khosru. S. W. J.
87. Dewán i Saib. The odes of Saib. S. W. J.
88. Dewán i Arfi. The odes of Arfi. S. W. J.
89. Dewán i Cásim. The odes of Cásim. L^y. J.
90. Dewán i Jámi. The odes of Jámi.
91. Asrár, or, Ishak Námeh. Secrets, or, the history of love: a poem. L^y. J.
92. Miscellaneous Poems. Chiefly by Arfi.
93. Mujma uz Záya. On the art of poetry. L^y. J.
94. Mekhzen i Asrár. The treasury of secrets: a poem, by Nizámi. L^y. J.
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97. Miscellaneous, prose and verse. By Arfi, and others. S. W. J.
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103. The Grammatical Introduction to the Farhang i Jehangiri. L^y. J.
104. Fowáyed i Ghaniya. A short treatise on Persian and Hindu grammar. L^y. J.
105. A dictionary of the Persian language. No title. L^y. J.
106. Tohfit ul Hind. A miscellaneous treatise on the literature, &c. of the Hindus. Enriched with marginal notes by Sir W. J. S. W. J.
107. a. Sri Bhágavata. A translation of N° 3. L^y. J.
107. b. Ditto. With drawings. L^y. J.
108. Rámáyana. A translation of N° 2. L^y. J.
109. Anwári Soheili. A Persian version of the Hitópadésa, by Husain Vaiz, surnamed Cashifi.
110. Arjuna Gitá. Translation of the Gitá. L^y. J.
111. Siva Purána. Translation from the Sanscrita. L^y. J.
112. Rága Darpana. A treatise on Hindu music. Translated from the Sanscrita. L^y. J.
113. Párijátaka. A treatise on Hindu music. Translated from the Sanscrita, by Roshin Zamir, in the reign of Aurungzeb. L^y. J.
114. Hazár Dharpad. A treatise on vocal music, according to the Hindus. L^y. J.

115. Shams ul áswát. The sun of sounds. A treatise on Hindu music. L^y. J.

116. Cefáyet ut Tálím. A treatise on astronomy, by Mahommed, son of Masawad Mahommed. L^y. J

117. Lowaih ul Kamar. A treatise on astronomy. L^y. J.

118. Resálah Sharífah. A treatise on astronomy. L^y. J.

119. A treatise on astronomy, with tables, in the Niskh character. L^y. J.

120. Sharah i Zíj i Merza Ulagh Bég. A commentary on the tables of Ulagh Bég. L^y. J.

121. Sharah i Elm i Hayát. A commentary on the science of astronomy. L^y. J.

122. Miscellaneous loose sheets on astronomy. L^y. J.

123. Tála Námeh and Sharah Tálá. Two treatises on fortune-telling. L^y. J.

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135. Kitáb ul Biyua. A law tract, translated from the Arabic. L^y. J.

136. Miscellaneous fragments.

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137. a. Al Kudûri. Institutes of Mahommedan law, by Abul Hasan A'hmed, of Bagdad, surnamed Al Kudûri, of which the Hadáyah is a comment. L^y. J.

137. b. Ditto. L^y. J.

138. Hedáyah. A comment on Al Kuduri, by Burhán ud Din ul Marghináni. L^y. J.

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143. Sacardán us Sultán. A treatise on various mystical subjects, in 7 chapters, by Shékh Ibn i Hajalah. L^y. J.

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145. a. Kámus. A dictionary of the Arabic language. S. W. J.

145. b. Ditto. L^y. J.

146. Al Khuláset. A grammar of the Arabic language. L^y. J.

147. Two treatises on Arabic grammar. L^y. J.

148. A treatise on Arabic grammar. L^y. J.

149. A dictionary of the Arabic language. L^y. J.

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151. A treatise on geometry, with tables.

152. Al Mutálab ul Hasani. Propositions in theology. L^y. J.

153. Hamasah. Ancient Arabian poems, collected by Abu Timmám. Copied from a manuscript traced on oiled paper. S. W. J.

154. Al Motanábi. The poems of Abu Taib, surnamed Al Motanábi. S. W. J.
155. Dewán i Ali. The poems of Ali. S. W. J.
156. Dewán ul A'shak. A book of poems. S. W. J.
157. Sharah i akàyad i Mulá Saduddin. A commentary on the Akáyad, by Saduddin. S. W. J.
158. Sharah ul Moalakát. A commentary on the Moalakát. L'. J.
159. Sharah ul Mobárak. Another commentary on the Moalakát. L'. J.
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163. Behr ul Basít. L'. J.
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165. Moruj uz zeheb wa maaden ul Jóher. An historical and geographical work, by Abul Hassan, surnamed Masaudi. S. W. J.
166. Hariri. The moral discourses of Haríri. S. W. J.
167. An Arabic manuscript, traced on oiled paper. Probably that copy of the ancient Arabian poems, called Hamasah, mentioned N° 153. L'. J.
168. A new copy of a manuscript, in sheets. No name. L'. J.

HINDOSTANI.

169. Gulistan. Translated from the Persian. S. W. J.
170. A commentary on the Grunt'ha, the religious institution of the Sic'hs, in the Nágari character. L'. J.

END OF THE EIGHTY-NINTH VOLUME OF THE ORIGINAL.

I. The Croonian Lecture. On the Structure and Uses of the Membrana Tympani of the Ear. By Everard Home, Esq. F.R.S. Anno 1800. *Vol. XC.* p. 1.

The principal object of the present lecture is to communicate a discovery of the structure of the membrana tympani; which, in some respects, affords a new and very curious instance of the application of muscular action, and may conduce to account for certain phenomena in the sense of hearing, in a more satisfactory manner than has hitherto been proposed. The membrana tympani has always been considered as a common membrane, which, by means of muscles belonging to the malleus being stretched or relaxed, became fitted, in its various degrees of tension, to convey the vast variety of external sounds to the internal organ. Its shape, situation, and office, have procured it the name of drum of the ear; and the muscles of the malleus having been deemed sufficient for bracing and unbracing it, less attention was bestowed on the structure of the membrane itself: to which may be added, that in the human ear, and generally in the ear of quadrupeds, the membrane is so extremely small and thin, and in its situation so peculiarly confined, as not to be got at for inspection but with much difficulty.

The case is different in the elephant, where this membrane is so very large, that the parts of which it is composed are readily distinguished: they are even conspicuous to the naked eye; and muscular fibres are seen passing along the membrane, in a radiated manner, from the bony rim which surrounds it, towards the handle of the malleus, to which the central part of the membrane is firmly attached. This discovery in the elephant having led to that of a similar construction in the human membrana tympani, it may not be improper to relate the circumstances by which I became engaged in the investigation of the organ of hearing in that animal. Three different opportunities have occurred of dissecting the elephant in London, by the deaths of those which had been presented to his Majesty, and were kept at the King's stables at Pimlico. One of them was given to the late Dr. Hunter; one to his brother Mr. J. Hunter; and the third to Sir Ashton Lever.

From my being connected with Mr. J. Hunter's pursuits in comparative anatomy, I was employed throughout the whole of these dissections, and became extremely desirous of examining the internal parts of the ear, the structure of that organ in the human body having at a very early period particularly engaged my attention;* but neither Dr. Hunter nor his brother could be prevailed on to sacrifice so large a portion of the skull as was necessary for the purpose. When Mr. Corse arrived from Bengal, last year, and mentioned his having brought over a number of skulls of elephants, in order to show the progress of the formation of their grinding teeth,† the desire to examine the organ of hearing in that animal recurred to me so strongly, that I requested to have one of the skulls for that purpose, and Mr. Corse very readily and obligingly complied with my request. After having examined the organ in the dried skull, the want of the membrana tympani, and of the small bones, made the information thus received of a very unsatisfactory nature, and increased the desire of seeing these parts in the recent head. In considering how this could be done, I recollected a mutilated elephant's head, preserved in spirits, which had been sent to Mr. Hunter; but which, from the multiplicity of his engagements, had remained neglected in the cask at the time of his death, and in the following year was dried, to show the proboscis, that it might not be altogether spoiled.

On examining this dried head, the bones had been so much broken, that one of the organs of hearing was altogether wanting; the other however was fortunately entire; and the membrana tympani and small bones, having been little disturbed in the drying of the parts, remained nearly in their natural situation. The membrana tympani, and every other part of the organ, were found to be much larger in proportion than in other quadrupeds, or in man; differing in this respect from the eye

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* In 1776, I injected the cochlea and semicircular canals of the human ear with a composition of wax and rosin. This was done by placing the temporal bone in the receiver of an air-pump, the upper part of which was in the form of a funnel, rendered airtight by a cork being fitted into its neck, and surrounded with bees' wax. After the air had been exhausted, the hot injection, poured into the funnel, melted the wax, and the cork was pulled out by means of a string previously attached to it; the injection immediately rushed into the receiver, and was forced, by the pressure of the atmosphere, into the cavities of the temporal bone.——† On this subject, a very ingenious paper has been since published by him, in the Phil. Trans. for 1799.—Orig.

of the elephant, which is unusually small, when compared with the enormous bulk of the animal. The membrane was found of an oval form; the short diameter of the oval rather more than an inch in length; the long diameter an inch and $\frac{7}{20}$. In the human ear, the membrana tympani is nearly circular; the longest diameter is $\frac{8}{20}$ of an inch; the shortest $\frac{7}{20}$. As the membrane in the elephant exceeds that of the human ear in thickness as much as in extent, which is as the squares of their diameters, or in the proportion of 135 to 14, it is natural to conclude that the muscular fibres which are to stretch the one, must greatly exceed in strength those capable of producing the same degree of tension in the other.

From this statement, the muscular structure in the human membrana tympani will necessarily be so much less distinct than in the elephant, as scarcely to be visible to the naked eye, and will easily be overlooked by the most attentive observer, who is not directed by some previous information to examine it under the most favourable circumstances; but when these are attended to, it can be perceived without the aid of glasses. If the membrana tympani of the human ear be completely exposed on both sides, by removing the contiguous parts, and the cuticular covering carefully washed off from its external surface; then, by placing it in a clear light, the radiated direction of its fibres may be easily detected. If a common magnifying glass be used, they are rendered nearly as distinct as those of the elephant appear to the naked eye; their course is exactly the same; and they differ in nothing but in being formed on a smaller scale. When viewed in a microscope magnifying 23 times, the muscular fibres are beautifully conspicuous, and appear uniformly the same throughout the whole surface, there being no central tendons, as in the diaphragm; the muscular fibres appear only to form the internal layer of the membrane, and are most distinctly seen when viewed on that side. In examining this membrane in different subjects, the parts were frequently found in a more or less morbid state. In one instance the membrane was found loaded with blood-vessels, was less transparent than usual, and was united by close adhesion to the point of the long process of the incus. In another instance, there was a preternatural ossification adhering to it, at a small distance from the end of the handle of the malleus.

As muscles in general are supplied with blood-vessels in proportion to the frequency of their action, it is an object of importance to determine the vascularity of the membrana tympani. On this subject my own want of information has been amply supplied by Dr. Baillie, who, in a communication on this subject, showed me a preparation of the membrane, in which the vessels had been most successfully injected with coloured wax. In this preparation, the most beautiful of the kind I ever saw, the vessels in their distribution resembled those of the iris, and were nearly half as numerous; they anastomosed with each other in a similar manner, and their general direction was from the circumference to the handle of the malleus; from near this handle, a small trunk sent off branches, in a radiated manner, which anastomosed with those which had an opposite course. This correspondence, in the number and distribution of blood-vessels, between the membrana tympani and the iris, is a strong circumstance in confirmation of that membrane being endowed with muscular action.

In the horse, the membrana tympani is smaller than in man; its long diameter is $\frac{8}{20}$ of an inch; the short one $\frac{6}{20}$; and it is almost quite flat, while in man it is concave, which makes the difference of extent considerably exceed the difference in the diameters. In the horse, the fibrous structure is not visible to the naked eye; it is even indistinctly seen when viewed through a common magnifying glass; but in a microscope it is very visible, and in every other respect agrees in structure with the membrane in the human ear, and in that of the elephant. In birds, the membrana tympani is larger in proportion than in the quadruped, and more circular in its shape. In the goose, it is $\frac{6}{20}$ of an inch in its longest diameter, and $\frac{5}{20}$ in its shortest diameter. In the turkey $\frac{7}{20}$ by $\frac{5}{20}$. It is thinner in its coats in birds than in the horse, and to the naked eye has no appearance of fibres; but when viewed in a microscope, there is a visible radiated structure, not very unlike the wire marks on common writing paper.

In a former Lecture on the Structure of Muscles*, in which a comprehensive view was taken of the subject, it was stated, that the organization necessary for muscular contraction could exist in an apparent membrane, and that a fasciculated structure was only necessary when muscular action was to be enabled to overcome resistance. The coats of the tænia hydatigena were mentioned as an instance of the first; and the human heart as the most complex of the 2d. In comparing the membranæ tympani of different animals, they afford a beautiful illustration of the truth of this position. In birds, where from the smallness of its size the resistance is very trifling, the membrane is very similar to the coat of an hydatid, only still thinner. In the elephant, fibres forming fasciculi are very distinct. The membrane of the horse, and that of the human ear, form the intermediate gradations. The knowledge of a muscular structure in the membrana tympani, enables us to explain many phenomena in hearing, which have not hitherto been accounted for in a satisfactory manner. It is principally by means of this muscle that accurate perceptions of sound are communicated to the internal organ, and that the membrana tympani is enabled to vary the state of its tension, so as to receive them in the quick succession in which they are conveyed to it. In the human ear, and in that of birds, the radiated fibres of the membrana tympani have their principal attachment to the extremity of the handle of the malleus, which is nearly in the centre of the membrane. In the membrane of the elephant, which is oval, the attachment to the handle of the malleus is at some distance from the centre. In the horse, deer, and cat, which have the membrane still more oval than the elephant, the handle of the malleus is situated in the long axis of the membrane, with its extremity extending beyond the centre, reaching nearer to the circumference;

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* Phil. Trans. for 1795.—Orig.

and the fibres of the radiated muscle are not only attached to its end, but also laterally to nearly the whole length of its handle.

This oval form of the membrana tympani, in those quadrupeds, and the very extensive attachment of the fibres of the radiated muscle to the handle of the malleus, may be the reason why their ears are not equally fitted to hear inarticulate sounds, as the ears of birds and of man. Should this radiated muscle of the membrana tympani, which is probably the smallest in the body that has a distinct action, be thought too insignificant to have an office of so much consequence assigned to it, let it be remembered, that the size of muscles is no indication of their importance, but only of the resistance to be overcome by their action; and that the more delicate actions are performed universally in the body by very small muscles, of which the iris in the eye furnishes a very conspicuous example.

Before the mode in which the radiated muscle adapts the membrana tympani to different sounds can be explained, it is necessary that the more important parts of the organ should be enumerated, and the use commonly assigned to each of them pointed out. In man and the more perfect quadrupeds, this organ consists of the following parts: the membrana tympani, situated between the external passage and the cavity of the tympanum; 4 small bones, which form a chain across the tympanum, connecting the membrana tympani with another membrane lining the foramen ovale, which opens into the vestibulum, a more internal part of the organ of hearing. The bones are, the malleus, which is united to the membrana tympani by a portion of its handle, and to the 2d bone or incus by its head. The incus, which is connected to the malleus by a capsular ligament, forming a regular joint, the surfaces of the bones being covered with cartilage, but they have only a tremulous motion on each other. The incus is also attached to the side of the cavity of the tympanum, where the mastoid cells open, by a ligament on which it moves backward and forward: it is united by its long process to the orbicular bone, which is the smallest in the body, and connects the incus to the 4th bone or stapes, which has its base applied to the foramen ovale, or opening leading into the cavity of the vestibulum. The cavity of the tympanum, in which these bones are situated, communicates with the external air by means of the Eustachian tube, so that there is always air behind the membrana tympani.

The malleus has 3 muscles, by which it is moved; one of them is called the tensor, from its pulling the malleus inwards, and tightening the membrana tympani: the other 2 act in an opposite direction, and relax the membrane; the largest of these is called the obliquus, and is the antagonist of the tensor muscle; the other is very small, and is called the lexator. The stapes has 1 muscle, which acts on it by bringing its basis closer to the foramen ovale. The vestibulum, which is completely separated from the tympanum, by the membrane that lines the foramen ovale, communicates freely with the cochlea and semicircular canals; but these cavities are filled with a watery liquor, and have no communication, as the tympanum has, with the external air. This fact was ascertained in the horse, by the following experiment, repeated several times. The organ of hearing was separated from the skull immediately after death, and the cavity of the tympanum exposed; the parts were then immersed in water, and the stapes removed; by which means, the membrane of the foramen ovale was destroyed, but no globule of air was seen to escape through the water*.

The following uses have generally been assigned to the parts now mentioned. The membrana tympani was supposed to be adapted to receive impressions, by the combined action of the tensor and laxator muscles varying the degree of its tension, so as to bring it in unison with different sounds: these impressions were conducted, by the chain of bones, to the vestibulum, cochlea, and semicircular canals; in which cavities, particularly the cochlea, they were supposed to undergo some modification, before they were impressed on the nerves spread on the linings of these cavities. The function of modifying impressions of sound was assigned to the cochlea, partly from the delicacy of its internal structure, supposed to resemble a musical instrument, and partly from there being no other part of the organ apparently suited for repeating the variety of delicate sounds which pass into the ear: the changes that could be produced on the membrana tympani by the muscles of the malleus, being considered as incapable of answering that purpose.

This slight sketch of the organ of hearing, and of the uses, as they are generally understood, of the different parts, will enable me to point out, with more clearness, what parts of the theory appear defective, and what improvements may be made on it. It is true that the membrana tympani is stretched and relaxed by the action of the muscles of the malleus, but not for the purpose alleged in the commonly received theory. It is stretched, in order to bring the radiated muscle of the membrane itself into a state capable of acting, and of giving those different degrees of tension to the membrane which empower it to correspond with the variety of external tremors: when the membrane is relaxed, the radiated muscle cannot act with any effect, and external tremors make less accurate impressions. The membrana tympani, with its tensor and radiated muscles, may be not unaptly compared to a monochord, of which the membrana tympani is the string; the tensor muscle the screw, giving the necessary tension to make the string perform its proper scale of vibrations; and the radiated muscle acting on the membrane like the moveable bridge of the monochord, adjusting it to the vibrations required to be produced. The combined effects of the action of these muscles give the perceptions of grave and acute tones; and, in proportion as their original conformation is more or less perfect, so will their actions be, and consequently the perceptions of sound which they communicate.

This mode of subdividing the motions of the membrana tympani between 2 sets of muscles, allotting a portion to each, is not peculiar to this part. A remarkable

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* This experiment was made by Mr. Clift, who superintends Mr. Hunter's collection, and who has afforded me material assistance in the different parts of this investigation.—Orig.

instance of it appears in the rapid movements of the fingers, in performing several actions, and particularly in playing on a musical instrument. In all such rapid motions, the fingers are bent to a certain degree by the long muscles that lie on the fore-arm, to the tendons of which a set of smaller muscles are attached, called lumbricales. These last are unable to produce any effect on the fingers, till elongated in consequence of the action of the long muscles in bending the other joints; the lumbricales then become capable of bending the fingers a little more, and of acting with great rapidity. It is a curious circumstance, that a similar application of muscles should be employed to fit the fingers to produce a quick succession of sounds, and to enable the ear to be impressed by them.

From the explanation given of the adjustment of the membrana tympani, the difference between a musical ear and one which is too imperfect to distinguish the different notes in music, will appear to arise entirely from the greater or less nicety with which the muscle of the malleus renders the membrane capable of being truly adjusted. If the tension be perfect, all the variations produced by the action of the radiated muscle will be equally correct, and the ear truly musical; but if the first adjustment is imperfect, though the actions of the radiated muscle may still produce infinite variations, none of them will be correct: the effect, in this respect, will be similar to that produced by playing on a musical instrument which is not in tune. The hearing of articulate sounds requires less nicety in the adjustment, than of inarticulate or musical ones: an ear may therefore be able to perceive the one, though it is not fitted to receive distinct perceptions from the other.

The nicety or correctness of a musical ear being the result of muscular action, renders it in part an acquirement; for, though the original formation of these muscles in some ears renders them more capable of arriving at this perfection in their action, early cultivation is still necessary for that purpose; and it is found that an ear, which on the first trials seemed unfit to receive accurate perceptions of sounds, shall, by early and constant application, be rendered tolerably correct, but never can attain excellence. There are organs of hearing in which the parts are so nicely adjusted to each other, as to render them capable of a degree of correctness in hearing sounds which appears preternatural. Children who during their infancy are much in the society of musical performers, will be naturally induced to attend more to inarticulate sounds than articulate ones, and by these means acquire a correct ear, which, after listening for 2 or 3 years to articulate sounds only, would have been attained with more difficulty. This mode of adapting the ear to different sounds, appears to be one of the most beautiful applications of muscles in the body; the mechanism is so simple, and the variety of effects so great.

Several ways in which the correctness of hearing is affected by the wrong actions of the muscles of the tympanum, that appeared to be inexplicable, can be readily accounted for, now that the means by which the membrane adjusts itself are understood. The following are instances of this kind.—*Case* 1. A gentleman 33 years of age, who possessed a very correct ear, so as to be capable of singing in concert, though he had never learned music, was suddenly seized with a giddiness in the head, and a slight degree of numbness in the right side and arm. These feelings went off in a few hours, but on the 3d day returned, and for several weeks he had returns of the same sensations. It was soon discovered that he had lost his musical ear; he could neither sing a note in tune, nor in the smallest degree perceive harmony in the performance of others. For some time he himself thought he had become a little deaf, but his medical attendant was not sensible of that in conversation. On going into the country, he derived great benefit from exercise and sea-bathing. Twenty months after the first attack, he was capable of singing a Scotch air with tolerable exactness, though he could not sing in concert. He continued to improve in his health, and in the course of 2 or 3 years completely recovered his ear for music. In this case, there appeared to be some affection of the brain, which had diminished the actions of the tensor muscles of the membranæ tympani, through the medium of the nerves which regulate their actions; this gradually went off, and the muscles recovered their former action.

Case 2. A young lady was seized with a frenzy, which lasted for several years. Previous to her derangement, she was incapable of singing in tune, from the want of an ear for music; but in the course of her madness she frequently, to the astonishment of her relations, sung a tune with tolerable correctness. This case is the reverse of the former; and, as it arose from a directly contrary affection of the brain, may be considered as the result of an unusual degree of action in the tensor muscles, giving the membrane a more correct adjustment than it had before.

Case 3. An eminent music master, after catching cold, found a confusion of sounds in his ears. On strict attention he discovered, that the pitch of 1 ear was ½ a note lower than that of the other; and that the perception of a simple sound did not reach both ears at the same instant, but seemed as 2 distinct sounds, following each other in quick succession, the last being the lower and weaker. This complaint distressed him for a long time, but he recovered from it without any medical aid. In this case, the whole defect appears to have been in the action of the radiated muscle, exerted neither with the same quickness nor force in 1 ear as in the other, so that the sound was half a note too low, as well as later in being impressed on the organ. This affection of the muscle of the membrana tympani is very similar to an affection of the straight muscles of one of the eyes, producing double vision, which I have noticed in a former lecture, when treating of the wrong actions of that organ*.

In endeavouring to explain the uses of the more internal parts of the ear, considerable advantage may be derived from classing them in 2 divisions; namely, those which are formed for the purpose of receiving impressions conveyed through the medium of liquid or of solid substances; and those adapted to receive impressions made by the impulses of an elastic fluid, as the common air. This can be done correctly. Fish, which are formed to hear in water, can have only the

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* Vide Phil. Trans. for 1797.—Orig.

parts belonging to the first division; while all the parts found in the ears of birds and quadrupeds, that are not met with in fish, must belong to the 2d. In fish, the organ consists of a vestibulum and 3 semicircular canals, and these are met with in all fish. In some genera there is an external opening, and substances of a hard nature are found lying loose in the vestibulum: these however cannot be considered as essential parts of the organ, from their not being common to fish in general. Birds have the vestibulum and semicircular canals in common with fish, but they have also a membrana tympani; a slender bone connecting that membrane with the vestibulum; and an Eustachian tube. In birds, the membrana tympani is convex externally, being pushed forwards by the end of the slender bone above-mentioned. In quadrupeds and man, besides the vestibulum and canals met with in fish, the membrana tympani, the bone connecting it with the vestibulum, and the Eustachian tube, found in birds, there is a cochlea. The membrana tympani is either flat or concave externally; the bony connection between it and the vestibulum is made up of several bones, supplied with muscles to move them in different directions.

The parts which compose the organ of hearing in fish, must be intended for receiving impressions conveyed through water: those additional parts met with in birds, and the still greater additions which are found in the quadruped and man, must be intended by nature for rendering more perfect the impressions conveyed to the ear through the medium of the external air. Fish, from the structure of the organ, can only hear sounds which agitate the water immediately in contact with the head of the fish; so that the impulse is conveyed, without interruption, from the liquid in which they live, to the organ of hearing. Man is capable of hearing in a similar manner to fishes, when a communication of solid parts is kept up between the sounding body and the bones of the skull: experiments of this kind must have been made by many members of the R. S. One of the most common is, applying a watch to the forehead, and stopping the ears, which does not prevent the ticking from being heard: the sound is still more distinct when the watch is applied to the mastoid process. Here, as the sound can neither pass through the meatus externus, nor by the Eustachian tube, while the mouth is kept shut, it evidently must be conducted through the bones of the skull. When the sound produced by boiling water is brought to the ear, by one end of an iron rod resting on the side of the kettle and the other kept in contact with the teeth, the sound is conducted in the same way, though in this case it has by some been supposed to pass through the Eustachian tube. In this mode of hearing, the vestibulum and semicircular canals are probably the only parts of the organ which are necessary to convey the impression to the expansion of the auditory nerve.

In hearing in air, the use of the membrana tympani in man and quadrupeds has already been explained. Its office in birds is precisely the same; but as in birds this membrane has no tensor muscle to vary its adjustment, but is always kept tense by the pressure of the end of the slender bone, the scale in birds cannot descend so low as in the human ear; and the intervals in their scale will be more minute, in consequence of the slightest tremor communicated by the action of the radiated muscle to one end of the slender bone being immediately conducted to the internal organ; while in the human ear it has to pass from one bone to another, before it arrives at the vestibulum. The cochlea has been considered by all physiologists as one of the most intricate and curious parts of the ear, and on that account had a most important office assigned to it. This however is now to be transferred to the membrana tympani; and, on attentive consideration of the subject, it will appear impossible for the cochlea to be of any use in modulating sounds, since the ear is only intended to convey impressions received from external bodies; hence, no impression can be communicated to the cochlea, which has not been transmitted by the membrana tympani. But if all the varieties of sound are repeated by the membrana tympani, no modulation in the cochlea is required; and when it is considered that the cochlea contains water, instead of air, the effect on every part will be found to be simultaneous. That the cochlea is neither absolutely necessary to fit the organ to be impressed by sounds communicated through air, nor to render it what is termed a musical ear, is sufficiently proved by that part being wanting in birds, whose organ is particularly adapted to inarticulate sounds. Some birds, particularly bulfinches, can be taught to sing various airs, though it will be always in high notes.

If it should be found that birds hear less accurately than quadrupeds, it will favour the idea that the great delicacy of structure of the cochlea, is intended to render the nerves which are spread on it more readily impressed by weak tremors, than those in either the vestibulum or semicircular canals. The cochlea and semi circular canals must be considered as 2 of the most important parts of the ear; their peculiar forms are no doubt adapted to some essential purposes; but what are the precise advantages derived from their particular shape, is at present unknown. There is however much ground to believe, that a more extensive knowledge in comparative anatomy, joined with future observations, may clear up this very curious and obscure part of the physiology of the organ of hearing. In the elephant, the small bones, the cochlea and semicircular canals, are larger than those in the human ear, nearly in the same proportion with the increased size of the membrana tympani. In that animal, there is a very remarkable peculiarity; which is, a cellular structure occupying the upper and posterior part of the skull, inclosed between the 2 tables communicating by a considerable aperture with the cavity of the tympanum, and lined by a similar membrane: the cells communicate freely with each other at their lower extremities, but not near the upper, forming irregular cylinders, placed in a converging direction, towards the cavity of the tympanum. There is no middle bony septum, separating the cells of the skull belonging to one ear from those which open into the other, but a ready communication between them. On the anterior part of the skull there is also a similar cellular structure, only much smaller, which communicates with the nose, but is entirely separate and distinct from that which forms an appendage to the organ of hearing.

That the elephant hears better than other animals, is generally asserted by those who have had opportunities of making observations on the subject. As this opinion has been advanced by men who had no knowledge in anatomy, and had no previous theory to bias their judgment, it is deserving of credit. The organ of hearing being now found more perfect, and formed on a larger scale than in any other animal with which we are acquainted, considerable weight is given to this opinion. Mr. Corse, who resided many years at Tiperah, in Bengal, and paid particular attention to the manners and habits of elephants, concurs in the belief of their hearing being more acute than that of man. The following circumstances are mentioned by him in proof of it. A tame elephant, who was never reconciled to have a horse moving behind him, though he expressed no uneasiness if the horse was within his view, either before or on one side, could distinguish the sound of a horse's foot at a distance, some time before any person in company heard it: this was known by his pricking up his ears, quickening his pace, and turning his head from side to side. A tame female elephant, which had a young one, was occasionally sent out with other elephants for food, without the young one being allowed to follow. She was not in the habit of pining after her young one, unless she heard its voice; but frequently, on the road home, when no one could distinguish any sound whatever, she pricked up her ears, and made a noise expressive of having heard the call of her young. This having occurred frequently, attracted Mr. Corse's notice, and made him, at the time the female elephant used these expressions, stop the party, and desire the gentlemen to listen; but they were unable to hear any thing till they had approached nearer to the place where the young one was kept.

The foregoing observations, the object of which has been to prove that the membrana tympani of the ear has a muscular structure, have already exceeded the limits of a lecture, which prevents us from going further at present into the consideration of this very curious and important organ. The general analogy between the uses of its different parts and those of the organ of vision, and the similar variations of their actions when under the influence of disease, furnish materials which, on some future occasion, may be laid before the R. S.

II. On the Method of determining, from the Real Probabilities of Life, the Values of Contingent Reversions in which Three Lives are involved in the Survivorship. By Wm. Morgan, Esq., F. R. S. p. 22.

The several papers, says Mr. M. which I have had the honour of communicating to the R. S., on the doctrine of contingent reversions, contain the greater number of those cases in which 3 lives are concerned in the survivorship. With the view of completing this subject, I have been induced to investigate the remaining problems; and, having succeeded in the solution of them, I hope the following will not be considered as an improper addition to my former communications.

PROBLEM 1. To determine the value of a given sum, payable on the death of A or B, should either of them be the 1st or 2d that fails, of the 3 lives, A, B, and C. —*Solution.* In this case, the payment of the given sum must certainly take place on the extinction of the joint lives of A and B, independent of C, and therefore the value of the reversion will be $= \frac{\mathrm{S}\,.\,(r-1)\,.\,(\mathrm{V}-\mathrm{AB})}{r}$.*

PROB. 2. To determine the value of a given sum, payable on the decease of A or B, should either of them be the 2d or 3d that shall fail, of the 3 lives, A, B, and C.—From the analytical solution of this problem, which is of considerable extent, it is at length deduced, that the value required will be generally denoted by S into $\frac{r-1}{r} \times (\mathrm{V} + \mathrm{ABC} - \frac{\mathrm{A}+\mathrm{B}+\mathrm{C}}{2}) + \frac{\mathrm{AC}}{2r} - \mathrm{AB} - \frac{\varkappa}{2c} \times (\mathrm{AK} + \mathrm{BK} - 2\mathrm{ABK}) + \frac{\beta}{2b} \times (\mathrm{AF} - \mathrm{AFC}) + \frac{m}{2br} \times (1 + \mathrm{AP} + d\,.\,\frac{\mathrm{PT}-\mathrm{APT}}{c})$.

But, it is observed that, unless A and B are very nearly of the same age, and both older than C, this rule will not be sufficiently accurate. If B be the oldest of the 3 lives, the annuities A, AC, and AK, should be continued only for as many years, x, as are equal to the difference between the age of B and that of the oldest life in the table of observations. Let those annuities be respectively denoted by A', $\mathrm{A}'\mathrm{C}'$, and $\mathrm{A}'\mathrm{K}'$; also let φ denote the probability that C survives B, q the number of persons living opposite to the age of A at the end of x years, then will the value of S, after x years, be $= \frac{\varphi q}{ar^x} \times \frac{r-1\,.\,\mathrm{V}-\mathrm{A}^x}{r}$, and the whole value of the reversion will be $=$ S into $\frac{r-1}{r} \times (\mathrm{V} + \mathrm{ABC} - \frac{\mathrm{A}'+\mathrm{B}+\mathrm{BC}}{2}) \times \frac{\mathrm{A}'\mathrm{C}'}{2r} - \mathrm{AB} - \frac{\varkappa}{2c} \times (\mathrm{A}'\mathrm{K}' + \mathrm{BK} - 2\mathrm{BK}) + \frac{\beta}{2b} \times (\mathrm{AF} - \mathrm{AFC}) + \frac{m}{2br} \times (1 + \mathrm{AP} + d.\,\frac{\mathrm{PT}-\mathrm{APT}}{c}) + \frac{\varphi q}{ar^x} \times \frac{r-1\,.\,\mathrm{V}-\mathrm{A}^x}{r}$; A^x denoting the value of an annuity on a life x years older than A.

If C be the oldest of the 3 lives, let $a - s$, $s - t$, $t - u$, &c. be substituted for their equals a', a'', a''', &c, and $c - c'$, $c - (c' + c'')$, $c - (c' + c'' + c''')$, &c. for their equals e, f, g, &c. then will the value of the reversion be found $=$ S into $\frac{r-1}{r} \times (\mathrm{V} - \frac{1}{2}(\mathrm{A} + \mathrm{B}) + \mathrm{ABC}) + \frac{\mathrm{BC}+\mathrm{AC}}{2r} - \mathrm{AB} + \frac{\beta\,.\,(\mathrm{AF}-\mathrm{AFC})}{2b} + \frac{\alpha\,.\,(\mathrm{HB}-\mathrm{HBC})}{2a} - \frac{s}{2ar} \times (1 + \mathrm{NC}) - \frac{m}{2br} \times (1 + \mathrm{PC}) + \frac{ms}{abr} \times (1 + \mathrm{NPC})$.

If the lives be all equal, the value, according to the first rule, will be $=$ S into $\frac{r-1}{r} \times (\mathrm{V} - \mathrm{C} - \frac{1}{2}\mathrm{CC} + \mathrm{CCC}) + \frac{\mathrm{CC}}{2r} - \mathrm{CC} + \frac{\varkappa}{2c} \times (\mathrm{KCC} - \mathrm{KC}) + \frac{d}{2cr} \times (1 + \mathrm{CT}) + \frac{dd}{2ccr} \times (\mathrm{TT} - \mathrm{CTT})$; and, according to the 2d rule, it will be $=$ S into $\frac{r-1}{r} \times (\mathrm{V} - \mathrm{C} + \mathrm{CCC}) + \frac{\mathrm{CC}}{r} - \mathrm{CC} + \frac{\varkappa\,.\,(\mathrm{CK}-\mathrm{CCK})}{c} - \frac{d}{cr} \times (1 + \mathrm{CT}) + \frac{dd}{ccr} \times (1 + \mathrm{CTT})$. If these expressions be resolved into their respective series, the value in

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* The same symbols are uniformly retained in this, as in my last 2 papers on the subject. See Phil. Trans., vol. 81, p. 247.—Orig.

each case will be found $= \text{s}\,.\,\frac{r-1}{r} \times (\text{v} - \text{c} - \text{cc} + \text{ccc})$, which is known to be the true value, from self-evident principles.

But the solution of this problem may be obtained by the assistance of the 1st problem in my last paper, for the year 1794, supposing, instead of a given sum, it were required to know the value of the reversion of a given estate. For since the possession of this estate is an event which must certainly take place, and the only point to be determined is the time in which it will probably happen, it is obvious that no event can postpone the possession, but the contingency of C's being the 2d that fails, of the 3 lives. If therefore the sum of the values of an annuity on the life of B after A, provided A should die before C, and of an annuity on the life of A after B, provided B should die before C, both found by the problem just mentioned, be subtracted from the whole value of the reversion after the joint lives of A and B, the remainder will be the value required. Let X and Y respectively denote the annuities found by problem 1st, Phil. Trans. vol. 84, then will the general rule expressing the value of an estate be $= \text{v} - \text{AB} - (\text{X} + \text{Y})$, and consequently of a given sum $= \frac{\text{s}\,.\,(r-1)}{r} \times (\text{v} - \text{AB} - (\text{X} + \text{Y}))$, which, when the lives are equal, may be reduced, as in the former cases, to $\frac{\text{s}\,.\,(r-1)}{r} \times (\text{v} - \text{c} - \text{cc} + \text{ccc})$.

PROB. 3. To determine the value of an estate, or of a given sum, after the decease of A or B, should either of them be the first or last that shall fail, of the 3 lives, A, B, and C.

Solution. The reversion of the estate in this problem, like that in the preceding one, cannot be prevented ultimately from taking place; and there is only the single contingency of C's being the first that fails, of the 3 lives, which can postpone the possession of it after the extinction of the joint lives. The whole value therefore of the reversion, after the joint lives of A and B, must in this case be lessened by the sum of the values of an annuity on A's life after B, provided B should survive C; and of an annuity on B's life after A, provided A should survive C, both found by the 2d problem in my last paper, of the year 1794. Let these two values be respectively denoted by W and Z, then will the general rule expressing the value of an estate be $= \text{v} - \text{AB} - (\text{W} + \text{Z})$, and the value of a given sum $= \frac{\text{s}\,.\,(r-1)}{r} \times (\text{v} - \text{AB} - (\text{W} + \text{Z}))$. When the lives are all equal, the value of the reversion, by substituting the values of W and Z, becomes $= \text{v} + \text{cc} - \text{c} - \text{ccc}$, or $\frac{\text{s}\,.\,(r-1)}{r} \times (\text{v} + \text{cc} - \text{c} - \text{ccc})$, according as it consists of an estate, or a given sum.

PROB. 4. To determine the value of a given sum, payable on the death of A, should his life be the 1st or 2d that fails, and should B's life, if it fail, become extinct before the life of C.

Solution. In this case, the payment of the given sum can only be prevented by the contingency of C's dying before A, and therefore its value is immediately found by the solution of the 2d problem in my first paper on this subject, in the year 1788, being no more than "the value of a given sum on the death of A, should C survive him."

PROB. 5. To determine the value of a given sum, payable on the death of A, should his life be the 2d or 3d that fails, and should B's life, when it fails, become extinct before the life of C.—The analytical investigation gives for the general value in the solution of this problem, S into $\frac{(r-1).(\mathrm{V}-\mathrm{A})}{2r} - \frac{(r-1).(\mathrm{BC}-\mathrm{ABC})}{6r} - \frac{\varkappa}{3c} \times (\mathrm{BK} - \mathrm{ABK} + \frac{\beta.(\mathrm{FK}-\mathrm{AFK})}{2b}) + \frac{m.(\mathrm{PC}-\mathrm{APC})}{3br} - \frac{d}{6cr} \times (\mathrm{BT} - \mathrm{ABT} - \frac{m.(\mathrm{PT}-\mathrm{APT})}{b}) + \frac{\beta}{2b} \times (\frac{\mathrm{FC}-\mathrm{AFC}}{3} - \mathrm{F} + \mathrm{AF}) + \frac{\mathrm{B}-\mathrm{C}+\mathrm{AC}-\mathrm{AB}}{2r} + \frac{\varkappa.(\mathrm{K}-\mathrm{AK})}{2c}$.

PROB. 6. To determine the value of a given sum, payable on the death of A, should his life be the first or last that shall fail, of the 3 lives; and should B's life, if it fail, become extinct before the life of C.—In like manner, the solution of this problem gives, for the value required, the general expression, S into $\frac{r-1}{2r} \times (\mathrm{V} - \mathrm{A} - \mathrm{ABC}) + \frac{\mathrm{AC}}{2} - \frac{\mathrm{AB}}{2r} + \frac{md}{2bcr} \times (1 + \mathrm{APT}) - \frac{d}{2cr} \times (1 + \mathrm{AT} + \mathrm{BT} - \mathrm{ABT}) - \frac{\beta\varkappa}{2bc} \times \mathrm{AFK} + \frac{\beta}{2b} \times (\mathrm{AF} + \mathrm{FC} - \mathrm{AFC})$.

PROB. 7. To determine the value of a given sum, payable on the death of A, B, and C, provided C shall die after one life in particular, A.—The expression for the solution in this case, is S into $\frac{r-1}{2r} \times (\mathrm{BC} - \mathrm{B} - \mathrm{ABC} + \frac{\mathrm{BC}}{2} - \frac{\mathrm{AB}}{2r} + \frac{m}{2br} \times (1 + \mathrm{AP} - \frac{d.(1+\mathrm{APT})}{c}) - \frac{\varkappa}{2c} \times (\mathrm{BK} - \mathrm{ABK}) + \mathrm{R}$; R denoting the value of S, by the 3d problem in the first paper, on the contingency of C's dying after A, anno 1788. This general rule gives the true value of the reversion, when B is the oldest of the 3 lives. But, when C is the oldest of the 3 lives, the general rule will be = S into $\frac{r-1}{2r} \times (\mathrm{BC} - \mathrm{B}' - \mathrm{ABC}) + \frac{\mathrm{BC}}{2} - \frac{\mathrm{A'B'}}{2r} + \frac{m}{2br} \times (1 + \mathrm{A'P'} - \frac{d.(1+\mathrm{APT})}{c} - \frac{\varkappa}{2c} \times (\mathrm{BK} - \mathrm{ABK}) + \mathrm{R} + \frac{\mu p.(r-1).(\mathrm{V}-\mathrm{B}^x)}{br^{x+1}}$; — x denoting the difference between the ages of C and of the oldest person in the table; p the number of persons living at the age of B after x years; B′, A′B′, and A′P′, the values of annuities on those single and joint lives for x years; and μ the probability that C dies after A, in the paper anno 1794.

The foregoing problems, together with those which have been investigated in my former papers, adds Mr. M., comprehend, as far as I can perceive, all the different cases of survivorship between 3 lives. The great number of contingencies on which these reversions depend, must necessarily render the solutions intricate, and consequently the general rules complicated and laborious. It would not however be a difficult task to abridge these rules very considerably, without destroying their accuracy in any great degree; but this would be foreign to my purpose in these papers, which has uniformly been confined to the investigation of the correct values of the different reversions. Nor do I think that such an abridgment is necessary, as the operations of even the longest of the present rules, may be completed in very nearly as short a time as the inaccurate approximations which have hitherto been employed for the same purpose. It may not be improper to observe, that the solutions in these papers are not only the first which have ever been deduced, in the case of 2 and 3 lives, from just principles and the real probabilities of life; but that, as to many of the problems, not even an attempt has ever been made to approximate to the value of the reversion. Being now possessed of correct solutions of all the cases in which 2 and 3 lives are involved in the survivorship, we are possessed of all that is really useful, and therefore I feel the greater satisfaction in closing my inquiries on this subject. For, in regard to contingencies depending on 4 or more lives, the cases are not only much too numerous and intricate to admit of a solution, but they occur so seldom in practice, as to render the entire investigation of them, were it even possible, a matter of little or no importance.

III. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, for 1798. *By Thos. Barker, Esq.* p. 46.

		Barometer.			Thermometer.						Rain.
					In the House.			Abroad.			
		Highest.	Lowest.	Mean.	Hig.	Low	Mean	Hig.	Low	Mean	
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.
Jan.	Morn.	30.01	28.47	29.44	46½	33	40½	49	28	36½	1.028
	Aftern.				47½	34	41	49	30	40½	
Feb.	Morn.	30.19	28.70	58	49½	35½	41½	50	23½	34½	1.542
	Aftern.				50	37	43	54	32½	43	
Mar.	Morn.	29.84	28.71	44	51½	40	43	48½	28	38	0.532
	Aftern.				53	41½	44	58	36½	44½	
Apr.	Morn.	29.88	28.66	48	58	39	51	52½	29	46	1.321
	Aftern.				61	41	51½	68	44½	57	
May	Morn.	30.00	28.75	56	61½	51	55	62½	44	52	1.892
	Aftern.				64	51½	56½	71	52½	61½	
June	Morn.	29.98	29.11	61	68	56	62	69	51	59½	0.950
	Aftern.				70	57½	64	84	60	72	
July	Morn.	29.73	28.91	33	66	57	61	67	54	59	2.942
	Aftern.				69	59½	63	79	60½	70	
Aug.	Morn.	29.90	29.25	62	66	60	63	64½	51	58½	1.942
	Aftern.				70	61½	65	80	62½	70	
Sept.	Morn.	29.77	28.48	32	66	52	58½	62½	44	53	2.814
	Aftern.				68½	51	60½	76	48	63½	
Oct.	Morn.	29.87	28.70	39	57½	44	52	55	32	47	3.030
	Aftern.				60	44	53	63	43½	55	
Nov.	Morn.	29.75	28.21	09	52	35½	42½	53	27	38	2.546
	Aftern.				53	36	43½	57	33	43	
Dec.	Morn.	30.18	28.80	43	42	21½	36	42½	15½	31½	1.396
	Aftern.				43	23½	36½	45	13	35	
Mean of all.				29.44			51			52	20.938

IV. On the Power of Penetrating into Space by Telescopes; with a Comparative Determination of the Extent of that Power in Natural Vision, and in Telescopes of various Sizes and Constructions; illustrated by Select Observations. By Wm. Herschel, LL. D., F. R. S. p. 49.

It will not be difficult to show that the power of penetrating into space by telescopes is very different from the magnifying power, and that, in the construction of instruments, these 2 powers ought to be considered separately. In order to conduct our present inquiry properly, it will be necessary to examine the nature of luminous bodies, and to enter into the method of vision at a distance. Therefore, to prevent the inaccuracy that would unavoidably arise from the use of terms in their common acceptation, I shall have recourse to algebraic symbols, and to such definitions as may be necessary to fix a precise meaning to some expressions which are often used in conversation, without much regard to accuracy. By luminous bodies I mean, in the following pages, to denote such as throw out light, whatever may be the cause of it: even those that are opaque when they are in a situation to reflect light, should be understood to be included; as objects of vision they must throw out light, and become intitled to be called luminous. However, those that shine by their own light may be called self-luminous, when there is an occasion to distinguish them. The question will arise, whether luminous bodies scatter light in all directions equally; but, till we are more intimately acquainted with the powers which emit and reflect light, we shall probably remain ignorant on this head. I should remark, that what I mean to say, relates only to the physical points into which we may conceive the surfaces of luminous bodies to be divided; for, when we take any given luminous body in its whole construction, such as the sun or the moon, the question will assume another form, as will appear hereafter.

That light, flame, and luminous gases are penetrable by the rays of light, we know from experience*; it follows therefore, that every part of the sun's disc cannot appear equally luminous to an observer in a given situation, on account of the unequal depth of its luminous atmosphere in different places†. This regards only bodies that are self-luminous. But the greatest inequalities in the brightness of luminous bodies in general, will undoubtedly be owing to their natural texture; which may be extremely various, with regard to their power of throwing out light more or less copiously. Brightness I ascribe to bodies that throw out light; and those that throw out most are the brightest. It will now be necessary to establish certain expressions for brightness in different circumstances. In the first place, let us suppose a luminous surface throwing out light, and let the whole quantity of light thrown out by it be called L. Now, since every part of this surface throws out light, let us suppose it divided into a number of luminous physical

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* In order to put this to a proof, I placed 4 candles behind a screen, at $\frac{3}{4}$ of an inch distance from each other, so that their flames might range exactly in a line. The first of the candles was placed at the same distance from the screen, and just opposite a narrow slit, $\frac{2}{3}$ of an inch long, and $\frac{1}{4}$ broad. On the other side of the screen I fixed up a book, at such a distance from the slit that, when the first of the candles was lighted, the letters might not be sufficiently illuminated to become legible. Then, lighting successively the 2d, 3d, and 4th candles, I found the letters gradually more illuminated, so that at last I could read them with great facility; and by the arrangement of the screen and candles, the light of the 2d, 3d, and 4th, could not reach the book, without penetrating the flames of those that were placed before them.——† See the paper on the Nature and Construction of the Sun, Phil. Trans. for 1795, page 46.—Orig.

points, denoted by N. If the copiousness of the emission of light from every physical point of the luminous surface were equal, it might in general be denoted by c; but, as that is most probably never the case, I make C stand for the mean copiousness of light thrown out from all the physical points of a luminous object. This may be found in the following manner. Let c express the copiousness of emitting light, of any number of physical points that agree in this respect; and let the number of these points be n. Let the copiousness of emission of another number of points be c', and their number n'. And if, in the same manner, other degrees of copiousness be called c^2, c^3, &c. and their numbers be denoted by n^2, n^3, &c. then will the sum of every set of points, multiplied by their respective copiousness of emitting light, give us the quantity of light thrown out by the whole luminous body. That is, $L = cn + c'n' + c^2n^2$, &c.; and the mean copiousness of emitting light, of each physical point, will be expressed by $\frac{cn + c'n' + c^2n^2, \&c.}{N} = C$. It is evident that the mean power, or copiousness of throwing out light, of every physical point in the luminous surface, multiplied by the number of points, must give us the whole power of throwing out light, of the luminous body. That is $CN = L$.

I ought now to answer an objection that may be made to this theory. Light, as has been stated, is transparent; and, since the light of a point behind the surface of a flame will pass through the surface, ought we not to take in its depth, as well as its superficial dimensions? In answer to this, I recur to what has been said with regard to the different powers of throwing out light, of the points of a luminous surface. For, as light must be finally emitted through the surface, it is but referring all light arising from the emission of points behind the surface, to the surface itself, and the account of emitted light will be equally true. And this will also explain why it has been stated as probable, that different parts of the same luminous surface may throw out different quantities of light. Since therefore the quantity of light thrown out by any luminous body is truly represented by CN, and that an object is bright in consequence of light thrown out, we may say that brightness is truly defined by CN. If however there should at any time be occasion for distinction, the brightness arising from the great value of C, may be called the intrinsic brightness; and that arising from the great value of N, the aggregate brightness; but the absolute brightness, in all cases, will still be defined by CN.

Hitherto we have only considered luminous objects, and their condition with regard to throwing out light. We proceed now to find an expression for their appearance at any assigned distance; and here it will be proper to leave out of the account, every part of CN which is not applied for the purpose of vision. L representing the whole quantity of light thrown out by CN, we shall denote that part of it which is used in vision, either by the eye or by the telescope, by l. This will render the conclusions that may be drawn hereafter more unexceptionable; for, the quantity of light l being scattered over a small space in proportion to L, it may reasonably be considered as more uniform in its texture; and no scruples about its inequality will take place. The equation of light, in this present sense therefore, is $\text{CN} = l$. Now, since we know that the density of light decreases in the ratio of the squares of the distances of the luminous objects, the expression for its quantity at the distance of the observer D, will be $\frac{l}{\text{D}^2}$.

In natural vision, the quantity l undergoes a considerable change, by the opening and contracting of the pupil of the eye. If we call the aperture of the iris a, we find that in different persons it differs considerably. Its changes are not easily to be ascertained; but we shall not be much out in stating its variations to be chiefly between 1 and 2 tenths of an inch. Perhaps this may be supposed under-rated; for the powers of vision, in a room completely darkened, will exert themselves in a very extraordinary manner. In some experiments on light, made at Bath, in the year 1780, I have often remarked, that after staying some time in a room fitted up for these experiments, where on entering I could not perceive any one object, I was no longer at a loss, in half an hour's time, to find every thing I wanted. It is however probable that the opening of the iris is not the only cause of seeing better after remaining long in the dark; but that the tranquillity of the retina, which is not disturbed by foreign objects of vision, may render it fit to receive impressions such as otherwise would have been too faint to be perceived. This seems to be supported by telescopic vision; for it has often happened to me, in a fine winter's evening, when, at midnight, and in the absence of the moon, I have taken sweeps of the heavens, of 4, 5, or 6 hours duration, that the sensibility of the eye, in consequence of the exclusion of light from surrounding objects, by means of a black hood which I wear on these occasions, has been very great; and it is evident, that the opening of the iris would have been of no service in these cases, on account of the diameter of the optic pencil, which in the 20 feet telescope, at the time of sweeping, was no more than .12 inch. The effect of this increased sensibility was such, that if a star of the 3d magnitude came towards the field of view, I found it necessary to withdraw the eye before its entrance, in order not to injure the delicacy of vision acquired by long continuance in the dark. The transit of large stars, unless where none of the 6th or 7th magnitude could be had, have generally been declined in my sweeps, even with the 20 feet telescope. And I remember, that after a considerable sweep with the 40 feet instrument, the appearance of Sirius announced itself, at a great distance, like the dawn of the morning, and came on by degrees, increasing in brightness, till this brilliant star at last entered the field of view of the telescope, with all the splendour of the rising sun, and forced me to take the eye from that beautiful sight. Such striking effects are a sufficient proof of the great sensibility of the eye, acquired by keeping it from the light. On taking notice, in the beginning of sweeps, of the time that passed, I found that the eye, coming from the light, required near 20^{m}, before it could be sufficiently reposed to admit a view of very delicate objects in the telescope; and that the observation of a transit of a star of the 2d or 3d magnitude would disorder the eye again, so as to require nearly the same time for the re-establishment of its tranquillity.

The difficulty of ascertaining the greatest opening of the eye, arises from the impossibility of measuring it at the time of its extreme dilatation, which can only happen when every thing is completely dark; but, if the variation of a is not easily to be ascertained, we have, on the other hand, no difficulty to determine the quantity of light admitted through a telescope, which must depend on the diameter of the object-glass, or mirror; for its aperture A may at all times be had by measurement. It follows therefore, that the expression $\frac{a^2l}{D^2}$ will always be accurate for the quantity of light admitted be the eye; and that $\frac{A^2l}{D^2}$ will be sufficiently so for the telescope. For it must be remembered, that the aperture of the eye is also concerned in viewing with telescopes; and that consequently, whenever the pencil of light transmitted to the eye by optical instruments exceeds the aperture of the pupil, much light must be lost. In that case, the expression A^2l will fail; and therefore in general, if m by the magnifying power, $\frac{A}{m}$ ought not to exceed a.

As I have defined the brightness of an object to the eye of an observer at a distance, to be expressed by $\frac{a^2l}{D^2}$, it will be necessary to answer some objections that may be made to this theory. Optical writers have proved, that an object is equally bright at all distances. It may therefore be maintained against me, that since a wall illuminated by the sun will appear equally bright, at whatever distance the observer be placed that views it, the sun also, at the distance of Saturn, or still farther from us, must be as bright as it is in its present situation. Nay it may be urged, that in a telescope, the different distance of stars can be of no account with regard to their brightness, and that we must consequently be able to see stars which are many thousands of times farther than Sirius from us; in short, that a star must be infinitely distant not to be seen any longer. Now, objections such as these, which seem to be the immediate consequence of what has been demonstrated by mathematicians, and which yet apparently contradict what I assert in this paper, deserve to be thoroughly answered. It may be remembered, that I have distinguished brightness into 3 different kinds. Two of these, which have been discriminated by intrinsic and absolute brightness, are, in common language, left without distinction. In order to show that they are so, I might bring a variety of examples from common conversation; but taking this for granted, it may be shown that all the objections I have brought against my theory have their foundation in this ambiguity. The demonstrations of opticians, with regard to what I call intrinsic brightness, will not oppose what I affirm of absolute brightness; and I shall have nothing further to do than to show that what mathematicians have said, must be understood to refer entirely to the intrinsic brightness, or illumination of the picture of objects on the retina of the eye: from which it will clearly follow, that their doctrine and mine are perfectly reconcileable; and that they can be at variance only when the ambiguity of the word brightness is overlooked, and objections, such as I have made, are raised where the word brightness is used as absolute, when we should have kept it to the only meaning it can bear in the mathematicians' theorem.

The first objection I have mentioned is, that the sun, to an observer on Saturn, must be as bright as it is here on earth. Now by this cannot be meant, that an inhabitant standing on the planet Saturn, and looking at the sun, should absolutely receive as much light from it as one on earth receives when he sees it; for this would be contrary to the well-known decrease of light at various distances. The objection therefore can only go to assert, that the picture of the sun, on the retina of the Saturnian observer, is as intensely illuminated as that on the retina of the terrestrial astronomer. To this I perfectly agree. But let those who would go farther, and say that therefore the sun is absolutely as bright to the one as to the other, remember that the sun on Saturn appears to be 100 times less than on the earth; and that consequently, though it may there be intrinsically as bright, it must here be absolutely 100 times brighter.

The next objection relates to the fixed stars. What has been shown in the preceding paragraph, with regard to the sun, is so entirely applicable to the stars, that it will be very easy to place this point also in its proper light. As I have assented to the demonstration of opticians with regard to the brightness of the sun, when seen at the distance of Saturn, provided the meaning of this word be kept to the intrinsic illumination of the picture on the retina of an observer, I can have no hesitation to allow that the same will hold good with a star placed at any assignable distance. But I must repeat, that the light we can receive from stars is truly expressed by $\frac{a^2l}{D^2}$; and that therefore their absolute brightness must vary in the inverse ratio of the squares of their distances. Hence I am authorised to conclude, and observation abundantly confirms it, that stars cannot be seen by the naked eye, when they are more than 7 or 8 times farther from us than Sirius; and that they become, comparatively speaking, very soon invisible with our best instruments. It will be shown hereafter, that the visibility of stars depends on the penetrating power of telescopes, which I must repeat falls indeed very short of showing stars that are many thousands of times farther from us than Sirius; much less can we ever hope to see stars that are all but infinitely distant.

If now it be admitted that the expressions we have laid down are such as agree with well-known facts, we may proceed to vision at a distance; and first with respect to the naked eye. Here the power of penetrating into space, is not only confined by nature, but is also occasionally limited by the failure in brightness of luminous objects. Let us see whether astronomical observations, assisted by mathematical reasoning, can give us some idea of the general extent of natural vision. Among the reflecting luminous objects, our penetrating powers are sufficiently ascertained. From the moon we may step to Venus, to Mercury, to Mars, to Jupiter, to Saturn, and last of all to the Georgian planet. An object seen by reflected light at a greater distance than this, it has never been allowed us to perceive; and it is indeed much to be admired, that we should see borrowed illumination to the amazing distance of more than 18 hundred millions of miles; especially when that light, in coming from the sun to the planet, has to pass through an equal space, before it can be reflected, by which it must be so enfeebled as to be above 368 times less intense on that planet than it is with us, and when probably not more than one-third part of that light can be thrown back from its disc.*

The range of natural vision with self-luminous objects, is incomparably more extended, but less accurately to be ascertained. From our brightest luminary, the sun, we pass immediately to very distant objects; for, Sirius, Arcturus, and the rest of the stars of the first magnitude, are probably those that come next; and what their distance may be, it is well known, can only be calculated imperfectly from the doctrine of parallaxes, which places the nearest of them at least 412530 times farther from us than the sun. In order to take a 2d step forwards, we must enter into some preliminary considerations, which cannot but be attended with considerable uncertainty. The general supposition, that stars, at least those which seem to be promiscuously scattered, are probably one with another of a certain magnitude, being admitted, it has already been shown in a former paper,† that after a certain number of stars of the first magnitude have been arranged about the sun, a farther distant set will come in for the 2d place. The situation of these may be taken to be, one with another, at about double the distance of the former from us.

By directing our view to them, and thus penetrating one step farther into space, these stars of the 2d magnitude furnish us with an experiment that shows what phenomena will take place, when we receive the illumination of 2 very remote objects, equally bright in themselves, of which one is at double the distance of the other. The expression for the brightness of such objects, at all distances, and with any aperture of the iris, according to our foregoing notation, will be $\frac{a^2 l}{D^2}$; and a method of reducing this to an experimental investigation will be as follows. Let us admit that α Cygni, β Tauri, and others, are stars of the 2d magnitude, such as are here to be considered. We know, that in looking at them and the former, the aperture of the iris will probably undergo no change; since the difference in brightness, between Sirius, Arcturus, α Cygni, and β Tauri, does not seem to affect the eye so as to require any alteration in the dimensions of the iris; a therefore becomes a given quantity, and may be left out. Admitting also, that the latter of these stars are probably at double the distance of the former, we have D^2 in one case 4 times that of the other; and the 2 expressions for the brightness of the stars, will be l for those of the 1st magnitude, and $\frac{1}{4}l$ for those of the 2d.

The quantities being thus prepared, what I mean to suggest by an experiment is, that since sensations, by their nature, will not admit of being halved or quartered, we come thus to know by inspection what phenomenon will be produced by the 4th part of the light of a star of the 1st magnitude. In this sense, I think we must

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* According to Mr. Bouguer, the surface of the moon absorbs about ⅔ of the light it receives from he sun. See Traité d'Optique, p. 122.——† Phil. Trans. for the year 1796, p. 166, 167, 168.—Orig.

take it for granted, that a certain idea of brightness, attached to the stars which are generally denominated to be of the 2d magnitude, may be added to our experimental knowledge; for, by this means, we are informed what we are to understand by the expressions $\frac{a^2l}{\odot^2}$, $\frac{a^2l}{(\text{Sirius})^2}$, $\frac{a^2l}{(\beta\,\text{Tauri})^2}$.[*] We cannot wonder at the immense difference between the brightness of the sun and that of Sirius; since the first 2 expressions, when properly resolved, give us a ratio of brightness of more than 170 thousand millions to 1; whereas the latter 2, as has been shown, give only a ratio of 4 to 1. What has been said will carry us, with very little addition, to the end of our unassisted power of vision to penetrate into space. We can have no other guide to lead us a 3d step than the same before-mentioned hypothesis; in consequence of which however it must be acknowledged to be sufficiently probable, that the stars of the 3d magnitude may be placed about 3 times as far from us as those of the 1st. It has been seen, by my remarks on the comparative brightness of the stars, that I place no reliance on the classification of them into magnitudes; but in the present instance, where the question is not to ascertain the precise brightness of any one star, it is quite sufficient to know that the number of the stars of the first 3 different magnitudes, or different brightnesses, answers, in a general way, sufficiently well to a supposed equally distant arrangement of a 1st, 2d, and 3d set of stars about the sun. Our 3d step forwards into space, may therefore very properly be said to fall on the pole star, on γ Cygni, ε Bootis, and all those of the same order. As the difference, between these and the stars of the preceding order, is much less striking than that between the stars of the 1st and 2d magnitude, we also find that the expressions $\frac{a^2l}{(\beta\,\text{Tauri})^2}$, and $\frac{a^2l}{(\text{Polaris})^2}$, are not in the high ratio of 4 to 1, but only as 9 to 4, or $2\frac{1}{4}$ to 1.

Without tracing the brightness of the stars through any farther steps, I shall only remark, that the diminution of the ratios of brightness of the stars of the 4th, 5th, 6th, and 7th magnitudes, seems to answer to their mathematical expressions, as well as, from the first steps we have taken, can possibly be imagined. The calculated ratio, for instance, of the brightness of a star of the 6th magnitude, to that of one of the 7th, is but little more than $1\frac{1}{3}$ to 1; but still we find by experience, that the eye can very conveniently perceive it. At the same time, the faintness of the stars of the 7th magnitude, which require the finest nights, and the best common eyes to be perceived, gives us little room to believe that we can penetrate much farther into space, with objects of no greater brightness than stars. But, since it may be justly observed, that in the foregoing estimation of the proportional distance of the stars, a considerable uncertainty must remain, we ought to make a proper allowance for it, and, in order to see to what extent this should go, we must make use of the experimental sensations of the ratios of brightness we have now acquired, in going step by step forward; for, numerical ratios of

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[*] The names of the objects, ☉, Sirius, β Tauri, are here used to express their distance from us. —Orig.

brightness, and sensations of them, as has been noticed before, are very different things. And since, from the foregoing considerations, it may be concluded, that as far as the 6th, 7th, or 8th magnitude, there ought to be a visible general difference between stars of one order and that of the next following, I think, from the faintness of the stars of the 7th magnitude, we are authorized to conclude, that no star, 8, 9, or at most 10 times as far from us as Sirius, can possibly be perceived by the natural eye.

The boundaries of vision, however, are not confined to single stars. Where the light of these falls short, the united lustre of sidereal systems will still be perceived. In clear nights, for instance, we may see a whitish patch in the sword-handle of Perseus, which contains small stars of various sizes, as may be ascertained by a telescope of a moderate power of penetrating into space. We easily see the united lustre of them, though the light of no one of the single stars could have affected the unassisted eye. Considerably beyond the distance of the former must be the cluster discovered by Mr. Messier, in 1764; north following H Geminorum. It contains stars much smaller than those of the former cluster; and a telescope should have a considerable penetrating power, to ascertain their brightness properly, such as my common 10-feet reflector. The night should be clear, in order to see it well with the naked eye, and it will then appear in the shape of a small nebula. Still farther from us must be the nebula between η and ζ Herculis, discovered by Dr. Halley, in 1714. The stars of it are so small that it has been called a nebula; and has been regarded as such, till my instruments of high penetrating powers were applied to it. It requires a very clear night, and the absence of the moon, to see it with the natural eye. Perhaps, among the farthest objects that can make an impression on the eye, when not assisted by telescopes, may be reckoned the nebula in the girdle of Andromeda, discovered by Simon Marius, in 1612. It is however not difficult to perceive it, in a clear night, on account of its great extent.

From the powers of penetrating into space by natural vision, we proceed now to that of telescopes. It has been shown, that brightness, or light, is to the naked eye truly represented by $\frac{a^2 l}{D^2}$; in a telescope therefore, the light admitted will be expressed by $\frac{A^2 l}{D^2}$. Hence it would follow, that the artificial power of penetrating into space should be to the natural one as A to a. But this proportion must be corrected by the practical deficiency in light reflected by mirrors, or transmitted through glasses; and it will in a great measure depend on the circumstances of the workmanship, materials, and construction of the telescope, how much loss of light there will be sustained.

In order to come to some determination on this subject, I made many experiments with plain mirrors, polished like my large ones, and of the same composition of metal. The method I pursued was that proposed by Mr. Bouguer, in his Traité d'Optique, page 16, fig. 3; but I brought the mirror, during the trial, as close to the line connecting the two objects as possible, in order to render the reflected rays nearly perpendicular. The result was, that out of 100 thousand incident rays, 67262 were returned; and therefore, if a double reflection takes place, only 45242 will be returned. Before this light can reach the eye, it will suffer some loss in passing through the eye-glass; and the amount of this I ascertained, by taking a highly polished plain glass, of nearly the usual thickness of optical glasses of small focal lengths. Then, by the method of the same author, page 21, fig. 5, I found, that out of 100 thousand incident rays, 94825 were transmitted through the glass. Hence, if 2 lenses be used, 89918; and with 3 lenses, 85265 rays will be transmitted to the eye. Then by compounding we shall have, in a telescope of my construction with one reflection, 63796 rays, out of 100 thousand, come to the eye. In the Newtonian form, with a single eye lens, 42901; and with a double eye-glass 40681 will remain for vision. There must always remain a considerable uncertainty in the quantities here assigned; as a newly-polished mirror, or one in high preservation, will give more light than another that has not those advantages. The quality of metal also will make some difference; but if it should appear by experiments, that the metals or glasses in use will yield more or less light than here assigned, it is to be understood that the corrections must be made accordingly.

We proceed now to find a proper expression for the power of penetrating into space, that we may be enabled to compare its effects, in different telescopes, with that of the natural eye. Since then the brightness of luminous objects is inversely as the squares of the distances, it follows, that the penetrating power must be as the square roots of the light received by the eye. In natural vision therefore, this power is truly expressed by $\sqrt{a^2 l}$; and, since we have now also obtained a proper correction x, we must apply it to the incident light with telescopes. In the Newtonian and other constructions, where 2 specula are used, there will also be some loss of light on account of the interposition of the small speculum; therefore, putting b for its diameter, we have $\mathrm{A}^2 - b^2$ for the real incident light. This being corrected as above, will give the general expression $\sqrt{(xl \times (\mathrm{A}^2 - b^2))}$ for the same power in telescopes. But here we are to take notice, that in refractors, and in telescopes with 1 reflection, b will be $= 0$, and therefore is to be left out. Then, if we put natural light $l = 1$, and divide by a, we have the general form $\frac{\sqrt{(x . (\mathrm{A}^2 - b^2))}}{a}$ for the penetrating power of all sorts of telescopes, compared to that of the natural eye as a standard, according to any supposed aperture of the iris, and proportion of light returned by reflexion, or transmitted by refraction.

In the following investigation we shall suppose $a = 2$ tenths of an inch, as being perhaps nearly the general opening of the iris, in star-light nights, when the eye has been some moderate time in the dark. The value of the corrections for loss of light will stand as has been given before.

We may now proceed to determine the powers of the instruments that have been used in my astronomical observations; but, as this subject will be best explained by a report of the observations themselves, I shall select a series of them for that purpose, and relate them in the order which will be most illustrating. First, with regard to the eye, it is certain that its power, like all our other faculties, is limited by nature, and is regulated by the permanent brightness of objects, as has been shown already, when its extent with reflected light was compared to its exertion on self-luminous objects. It is further limited on borrowed light, by the occasional state of illumination; for when that becomes defective at any time, the power of the eye will then be contracted into a narrower compass; an instance of which is the following. In the year 1776, when I had erected a telescope of 20 feet focal length, of the Newtonian construction, one of its effects by trial was, that when towards evening, on account of darkness, the natural eye could not penetrate far into space, the telescope possessed that power sufficiently to show, by the dial of a distant church steeple, what o'clock it was, notwithstanding the naked eye could no longer see the steeple itself. Here I only speak of the penetrating power; for though it might require magnifying power to see the figures on the dial, it could require none to see the steeple. Now the aperture of the telescope being 12 inches, and the construction of the Newtonian form, its penetrating power, when calculated according to the given formula, will be $\frac{1}{2}\sqrt{(.429 \times (120^2 - 15^2))} = 38.99$, A, *b*, and *a*, being all expressed in tenths of an inch.* From the result of this computation it appears, that the circumstance of seeing so well, in the dusk of the evening, may be easily accounted for, by a power of this telescope to penetrate 39 times farther into space than the natural eye could reach, with objects so faintly illuminated.

This observation completely refutes an objection to telescopic vision, that may be drawn from what has also been demonstrated by optical writers; namely, that no telescope can show an object brighter than it is to the naked eye. For, in order to reconcile this optical theory with experience, I have only to say, that the objection is entirely founded on the same ambiguity of the word brightness that has before been detected. It is perfectly true, that the intrinsic illumination of the picture on the retina, which is made by a telescope, cannot exceed that of natural vision; but the absolute brightness of the magnified picture by which telescopic vision is performed, must exceed that of the picture in natural vision, in the same ratio in which the area of the magnified picture exceeds that of the natural one; supposing the intrinsic brightness of both pictures to be the same. In our present instance, the steeple and clock-dial were rendered visible by this increased absolute brightness of the object, which in natural vision was 15 hundred times inferior to what it was in the telescope. And this establishes beyond a doubt, that telescopic vision is performed by the absolute brightness of objects;

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* I have given the figures, in all the following equations of the calculated penetrating powers, in order to show the constructions of my instruments to those who may wish to be acquainted with them.—Orig.

for, in the present case, I find by computation, that the intrinsic brightness, so far from being equal in the telescope to that of natural vision, was inferior to it in the ratio of 3 to 7.

The distinction between magnifying power, and a power of penetrating into space, could not but be felt long ago, though its theory has not been inquired into. This undoubtedly gave rise to the invention of those very useful short telescopes called night-glasses. When the darkness of the evening curtails the natural penetrating power, they come in very seasonably, to the relief of mariners that are on the look-out for objects which it is their interest to discover. Night-glasses, such as they are now generally made, will have a power of penetrating 6 or 7 times farther into space than the natural eye. For, by the construction of the double eye-glass, these telescopes will magnify 7 or 8 times; and the object-glass being $2\frac{1}{2}$ inches in diameter, the breadth of the optic pencil will be $3\frac{1}{8}$ or $3\frac{4}{7}$ tenths of an inch. As this cannot enter the eye, on a supposition of an opening of the iris of 2 tenths, we are obliged to increase the value of a, in order to make the telescope have its proper effect. Now whether nature will admit of such an enlargement becomes an object of experiment; but, at all events, a cannot be assumed less than $\frac{A}{m}$. Then, if x be taken as has been determined for 3 refractions, we shall have $\frac{\sqrt{.853 \times 25^2}}{a} = 6.46$ or 7.39.

Soon after the discovery of the Georgian planet, a very celebrated observer of the heavens, who has added considerably to our number of telescopic comets and nebulæ, expressed his wish, in a letter to me, to know by what method I had been led to suspect this object not to be a star, like others of the same appearance. I have no doubt but that the instrument through which this astronomer generally looked out for comets, had a penetrating power much more than sufficient to show the new planet, since even the natural eye will reach it. But here we have an instance of the great difference in the effect of the 2 sorts of powers of telescopes; for, on account of the smallness of the planet, a different sort of power, namely, that of magnifying, was required; and, about the time of its discovery, I had been remarkably attentive to an improvement of this power, as I happened to be then much in want of it for my very close double stars.* On examining the nebulæ which had been discovered by many celebrated authors, and comparing my observations with the account of them in the Connoissance des Temps for 1783, I found that most of those which I could not resolve into stars with instruments of a small penetrating power, were easily resolved with telescopes of a higher power of this sort; and that the effect was not owing to the magnifying power I used on these occasions, will fully appear from the observations; for when the closeness of the stars was such as to require

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* Magnifying powers of 460, 625, 932, 1159, 1504, 2010, 2398, 3168, 42 4, 5489, 6450, 6652, were used on ε Bootis, γ Leonis, α Lyræ, &c. See Cat. of double stars, Phil. Trans. vol. 72, page 115, and 147; and vol. 75, page 48.—Orig.

a considerable degree of magnifying as well as penetrating power, it always appeared plainly, that the instrument which had the highest penetrating power resolved them best, provided it had as much of the other power as was required for the purpose.

Sept. 20, 1783, I viewed the nebula between Flamsteed's 99th and 105th Piscium, discovered by Mr. Mechain, in 1780. " It is not visible in the finder of my 7-feet telescope; but that of my 20-feet shows it."—Oct. 28, 1784, I viewed the same object with the 7-feet telescope. " It is extremely faint. With a magnifying power of 120, it seems to be a collection of very small stars: I see many of them." At the time of these observations, my 7-feet telescope had only a common finder, with an aperture of the object-glass of about $\frac{3}{4}$ of an inch in diameter, and a single eye-lens; therefore its penetrating power was $\frac{1}{2}\sqrt{.899 \times 7.5^2} = 3.56$. The finder of the 20-feet instrument, being achromatic, had an object-glass 1.17 inch in diameter; its penetrating power therefore was $\frac{1}{2}\sqrt{.85 \times 11.7^2}. = 4.50$.

Now, that one of them showed the nebula and not the other, can only be ascribed to space-penetrating power, as both instruments were equal in magnifying power, and that so low as not to require an achromatic object-glass to render the image sufficiently distinct. The 7-feet reflector evidently reached the stars of the nebula; but its penetrating and magnifying powers are very considerable, as will be shown presently. July 30, 1783, I viewed the nebula south preceding Flamsteed's 24 Aquarii, discovered by Mr. Maraldi, in 1746. " In the small sweeper,* this nebula appears like a telescopic comet."—Oct. 27, 1794, the same nebula with a 7-feet reflector. " I can see that it is a cluster of stars, many of them being visible." If we compare the penetrating power of the 2 instruments, we find that we have in the first $\frac{1}{2}\sqrt{(.41 \times (42^2 - 12^2))} = 12.84$; and in the latter $\frac{1}{2}\sqrt{(.43 \times (63^2 - 12^2))} = 20.25$. However, the magnifying power was partly concerned in this instance; for, in the sweeper it was sufficient to separate the stars properly.

March 4, 1783, with a 7-feet reflector, I viewed the nebula near the 5th Serpentis, discovered by Mr. Messier, in 1764. " It has several stars in it; they are however so small that I can but just perceive some, and suspect others." May 31, 1783, the same nebula with a 10-feet reflector; penetrating power $\frac{1}{2}\sqrt{(.43 \times (89^2 - 16^2))} = 28.67$. " With a magnifying power of 250, it is all resolved into stars: they are very close, and the appearance is beautiful. With 600, perfectly resolved. There is a considerable star not far from the middle;

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* The small sweeper is a Newtonian reflector, of 2 feet focal length; and, with an aperture of 4.2 inches, has only a magnifying power of 24, and a field of view 2° 12′. Its distinctness is so perfect, that it will show letters at a moderate distance, with a magnifying power of 2000; and its movements are so convenient, that the eye remains at rest while the instrument makes a sweep from the horizon to the zenith. A large one of the same construction has an aperture of 9.2 inches, with a focal length of 5 feet 3 inches. It is also charged low enough for the eye to take in the whole optic pencil; and its penetrating power, with a double eye-glass, is $\frac{1}{2}\sqrt{(.41 \times (92^2 - 21^2))} = 28.57$.—Orig.

another not far from one side, but out of the cluster; another pretty bright one; and a great number of small ones." Here we have a case where the penetrating power of 20 fell short, when 29 resolved the nebula completely. This object requires also great magnifying power to show the stars of it well; but that power had before been tried, in the 7-feet, as far as 460, without success, and could only give an indication of its being composed of stars; whereas the lower magnifying power of 250, with a greater penetrating power, in the 10-feet instrument, resolved the whole nebula into stars.

May 3, 1783, I viewed the nebula between η and ρ Ophiuchi, discovered by Mr. Messier, in 1764. " With a 10-feet reflector, and a magnifying power of 250, I see several stars in it, and make no doubt a higher power and more light will resolve it all into stars. This seems to be a good nebula for the purpose of establishing the connection between nebulæ and clusters of stars in general."—June 18, 1784, the same nebula viewed with a large Newtonian 20-feet reflector; penetrating power $\frac{1}{2}\sqrt{(.43 \times (188^2 - 21^2))} = 61.18$; and a magnifying power of 157. " A very large and very bright cluster of excessively compressed stars. The stars are but just visible, and are of unequal magnitudes: the large stars are red; and the cluster is a miniature of that near Flamsteed's 42d Comæ Berenices. RA $17^h\ 6^m\ 32^s$; PD 108° 18′." Here, a penetrating power of 29, with a magnifying power of 250, would barely show a few stars; when, in the other instrument, a power 61 of the first sort, and only 157 of the latter, showed them completely well.

July 4, 1783, I viewed the nebula between Flamsteed's 25 and 26 Sagittarii, discovered by Abraham Ihle, in 1665. " With a small 20-feet Newtonian telescope, power 200, it is all resolved into stars, that are very small and close. There must be some hundreds of them. With 350, I see the stars very plainly; but the nebula is too low in this latitude for such a power."—July 12, 1784, I viewed the same nebula with a large 20-feet Newtonian reflector; power 157. " A most beautiful extensive cluster of stars, of various magnitudes, very compressed in the middle, and about 8′ in diameter, besides the scattered ones, which do more than fill the extent of the field of view *: the large stars are red; the small ones are pale red. RA $18^h\ 23^m\ 39^s$; PD 114° 7′." The penetrating power of the first instrument was 39, that of the latter 61; but, from the observations, it is plain how much superior the effect of the latter was to that of the former, notwithstanding the magnifying power was so much in favour of the instrument with the small penetrating power.

July 30, 1783. With a small 20-feet Newtonian reflector, I viewed the nebula in the hand of Serpentarius, discovered by Mr. Messier, in 1764. " With a power of 200, I see it consists of stars. They are better visible with 300. With 600, they are too obscure to be distinguished, though the appearance of stars is still preserved. This seems to be one of the most difficult objects to be resolved.

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* This field, by the passage of an equatorial star, was 15′ 3″.—Orig.

With me, there is not a doubt remaining; but another person, in order to form a judgment, ought previously to go through all the several gradations of nebulæ which I have resolved into stars."—May 25, 1791, I viewed the same nebula with a 20-feet reflector of my construction, having a penetrating power of $\frac{1}{2}\sqrt{.64 \times 188^2} = 75.08$. "With a magnifying power of 157, it appears extremely bright, round, and easily resolvable. With 300, I can see the stars. It resembles the cluster of stars taken at $16^h\ 43^m\ 40^s$ *, which probably would put on the same appearance as this, if it were at a distance half as far again as it is. RA $17^h\ 26^m\ 19^s$; PD 93° 10′." Here we may compare 2 observations; one taken with the penetrating power of 39, the other with 75; and, though the former instrument had far the advantage in magnifying power, the latter certainly gave a more complete view of the object.

The 20-feet reflector having been changed from the Newtonian form to my present one, I had a very striking instance of the great advantage of the increased penetrating power, in the discovery of the Georgian satellites. The improvement, by laying aside the small mirror, was from 61 to 75; and whereas the former was not sufficient to reach these faint objects, the latter showed them perfectly well. March 14, 1798, I viewed the Georgian planet with a new 25-feet reflector. Its penetrating power is $\frac{1}{2}\sqrt{.64 \times 240^2} = 95.85$; and having just before also viewed it with my 20-feet instrument, I found that with an equal magnifying power of 300, the 25-feet telescope had considerably the advantage of the former.

Feb. 24, 1786, I viewed the nebula near Flamsteed's 5th Serpentis, which has been mentioned before, with my 20-feet reflector; magnifying power 157. "The most beautiful extremely compressed cluster of small stars; the greatest part of them gathered together into one brilliant nucleus, evidently consisting of stars, surrounded with many detached gathering stars of the same size and colour. RA $15^h\ 7^m\ 12^s$; PD 87° 8′."—May 27, 1791, I viewed the same object with my 40-feet telescope; penetrating power $\frac{1}{2}\sqrt{.64 \times 480^2} = 191.69$; magnifying power 370. "A beautiful cluster of stars. I counted about 200 of them. The middle of it is so compressed that it is impossible to distinguish the stars." Here it appears, that the superior penetrating power of the 40-feet telescope enabled me even to count the stars of this nebula. It is also to be noticed, that the object did not strike me as uncommonly beautiful; because, with much more than double the penetrating, and also more than double the magnifying power, the stars could not appear so compressed and small as in the 20-feet instrument: this very naturally must give it more the resemblance of a coarser cluster of stars, such as I had been in the habit of seeing frequently.

The 40-feet telescope was originally intended to have been of the Newtonian construction; but, in the year 1787, when I was experimentally assured of the vast

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* The object referred to is No. 10, of the Connoissance des Temps for 1783, called "Nebuleuse sans etoiles." My description of it is, "A very beautiful, and extremely compressed, cluster of stars: the most compressed part about 3 or 4′ in diameter. RA. $16^h\ 46^m\ 2^s$; PD 93° 46′."—Orig.

importance of a power to penetrate into space, I laid aside the work of the small mirror, which was then in hand, and completed the instrument in its present form. "Oct. 10, 1791, I saw the 4th satellite and the ring of Saturn, in the 40-feet speculum, without an eye-glass." The magnifying power on that occasion could not exceed 60 or 70; but the great penetrating power made full amends for the lowness of the former; notwithstanding the greatest part of it must have been lost for want of a greater opening of the iris, which could not take in the whole pencil of rays, for this could not be less than 7 or 8 tenths of an inch.

Among other instances of the superior effects of penetration into space, I should mention the discovery of an additional 6th satellite of Saturn on the 28th of August, 1789; and of a 7th, on the 11th of September, in the same year; which were first pointed out by this instrument. It is true that both satellites are within the reach of the 20-feet telescope; but it should be remembered, that when an object is once discovered by a superior power, an inferior one will suffice to see it afterwards. I need not add, that neither the 7 nor 10-feet telescopes will reach them; their powers, 20 and 29, are not sufficient to penetrate to such distant objects, when the brightness of them is not more than that of these satellites. It is also evident, that the failure in these latter instruments, arises not from want of magnifying power; as either of them has much more than sufficient for the purpose. Nov. 5, 1791, I viewed Saturn with the 20 and 40-feet telescopes. "20-feet: The 5th satellite of Saturn is very small. The 1st, 2d, 3d, 4th, 5th, and the new 6th satellite, are in their calculated places." "40-feet: I see the new 6th satellite much better with this instrument than with the 20-feet. The 5th is also much larger here than in the 20-feet; in which it was nearly the same size as a small fixed star, but here it is considerably larger than that star." Here the superior penetrating power of the 40-feet telescope showed itself on the 6th satellite of Saturn, which is a very faint object; as it had also a considerable advantage in magnifying power, the disc of the 5th satellite appeared larger than in the 20-feet. But the small star, which may be said to be beyond the reach of magnifying power, could only profit by the superiority of the other power.

Nov. 21, 1791, 40-feet reflector,; power 370. "The black division on the ring is as dark as the heavens about Saturn, and of the same colour." "The shadow of the body of Saturn is visible on the ring, on the following side; its colour is very different from that of the dark division. The 5th satellite is less than the 3d: it is even less than the 2d." 20-feet reflector; power 300. "The 3d satellite seems to be smaller than it was the last night but one. The 4th satellite seems to be larger than it was the 19th. This telescope shows the satellites not nearly so well as the 40-feet." Here the magnifying power being nearly alike, the superiority of the 40-feet telescope must be ascribed to its penetrating power.

The different nature of the two powers above-mentioned being thus evidently established, I must now remark that, in some respects, they even interfere with each other; a few instances of which I shall give. August 24, 1783, I viewed the nebula north preceding Flamsteed's 1 trianguli, discovered by Mr. Messier, in 1764. "7-feet reflector; power 57. There is a suspicion that the nebula consists of exceedingly small stars. With this low power it has a nebulous appearance; and it vanishes when I put on the higher magnifying powers of 278 and 460."—Oct. 28, 1794, I viewed the same nebula with a 7-feet reflector. "It is large, but very faint. With 120 it seems to be composed of stars, and I think I see several of them; but it will bear no magnifying power." In this experiment, magnifying power was evidently injurious to penetrating power. I do not account for this on the principle that by magnifying we make an object less bright; for when opticians have also demonstrated that brightness is diminished by magnifying, it must again be understood as relating only to the intrinsic brightness of the magnified picture; its absolute brightness, which is the only one that concerns us at present, must always remain the same*. The real explanation of the fact I take to be, that while the light collected is employed in magnifying the object, it cannot be exerted in giving penetrating power. June 18, 1799, I viewed the planet Venus with a 10-feet reflector. "Its light is so vivid that it does not require, nor will it bear, a penetrating power of 29, neither with a low nor with a high magnifying power." This is not owing to the least imperfection in the mirror, which is truly parabolical, and shows with all its aperture open, and a magnifying power of 600, the double star γ Leonis in the greatest perfection. "It showed Venus perfectly well defined with a penetrating power as low as 14, and a magnifying power of 400, or 600." Here, penetrating power was injurious to magnifying power; and that it necessarily must be so, when carried to a high pitch, is evident; for, by enlarging the aperture of the telescope, we increase the evil that attends magnifying, which is, that we cannot magnify the object without magnifying the medium. Now since the air is very seldom of so homogeneous a disposition as to admit to be magnified highly, it follows that we must meet with impurities and obstructions, in proportion to its quantity. But the contents of the columns of air through which we look at the heavens by telescopes, being of equal lengths, must be as their bases, that is, as the squares of the apertures of the telescopes; and this is in a much higher ratio than that of the increase of the power of penetrating into space. From my long experience in these matters, I am led to apprehend, that the highest power of mag-

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* This may be proved thus. The mean intrinsic brightness, or rather illumination, of a point of the picture on the retina, will be all the light that falls on the picture, divided by the number of its points; or $c = \frac{l}{N}$. Now since, with a greater magnifying power m, the number of points N increases as the squares of the power, the expression for the intrinsic brightness $\frac{l}{N}$, will decrease in the same ratio; and it will consequently be in general $N \propto m^2$, and $\frac{l}{N}$ or $c \propto \frac{1}{m^2}$; that is, by compounding $cN \propto \frac{m^2}{m^2} = l = 1$; or absolute brightness a given quantity. M. Bouguer has carefully distinguished intrinsic and absolute brightness, when he speaks of the quantity of light reflected from a wall, at different distances. Traité d'Optique, page 39 and 40.—Orig.

nifying may possibly not exceed the reach of a 20 or 25-feet telescope; or may even lie in a less compass than either. However, in beautiful nights, when the outside of our telescope is dropping with moisture discharged from the atmosphere, there are now and then favourable hours, in which it is hardly possible to put a limit to magnifying power. But such valuable opportunites are extremely rare; and, with large instruments, it will always be lost labour to observe at other times.

As I have hinted at the natural limits of magnifying power, I shall venture also to extend my surmises to those of penetrating power. There seems to be room for a considerable increase in this branch of the telescope; and, as the penetrating power of my 40-feet reflector already goes to 191.69, there can hardly be any doubt but that it might be carried to 500, and probably not much farther. The natural limit seems to be an equation between the faintest star that can be made visible, by any means, and the united brilliancy of star-light. For, as the light of the heavens, in clear nights, is already very considerable in my large telescope, it must in the end be so increased, by enlarging the penetrating power, as to become a balance to the light of all objects that are so remote as not to exceed in brightness the general light of the heavens. Now if P be put for penetrating power, we have $\sqrt{\frac{P^2a^2}{x}} = A =$ 10 feet 5.2 inches, for an aperture of a reflector, on my construction, that would have such a power of 500.

But, to return to our subject; from what has been said before, we may conclude, that objects are viewed in their greatest perfection when, in penetrating space, the magnifying power is so low as only to be sufficient to show the object well; and when, in magnifying objects, by way of examining them minutely, the space-penetrating power is no higher than what will suffice for the purpose; for, in the use of either power, the injudicious overcharge of the other, will prove hurtful to perfect vision.

It is remarkable that, from very different principles, I have formerly determined the length of the visual ray of my 20-feet telescope on the stars of the milky way, so as to agree nearly with the calculations that have been given.* The extent of what I then figuratively called my sounding line, and what now appears to answer to the power of penetrating into space, was shown to be not less than 415, 461, and 497 times the distance of Sirius from the sun. We now have calculated that my telescope, in the Newtonian form, at the time when the paper on the Construction of the Heavens was written, possessed a power of penetration, which exceeded that of natural vision 61.18 times; and, as we have also shown, that stars at 8, 9, or at most 10 times the distance of Sirius, must become invisible to the eye, we may safely conclude, that no single star above 489, 551, or at most 612 times as far as Sirius, can any longer be seen in this telescope. Now the greatest length of the former visual ray, 497, agrees nearly with the lowest of these present numbers, 489; and the higher ones are all in favour of the former computation; for

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* Phil. Trans. vol. 75, p. 247, 248.—Orig.

that ray, though taken from what was perhaps not far from its greatest extent, might possibly have reached to some distance beyond the apparent bounds of the milky way: but if there had been any considerable difference in these determinations, we should remember that some of the data by which I have now calculated are only assumed. For instance, if the opening of the iris, when we look at a star of the 7th magnitude, should be only $\frac{1}{10}$ of an inch and a half, instead of 2, then *a*, in our formula, will be = 1.5; which, when resolved, will give a penetrating power of 81.58; and therefore on this supposition, our telescope would easily have shown stars 571 times as far from us as Sirius; and only those at 653, 734, or 816 times the same distance, would have been beyond its reach. My reason for fixing on $\frac{2}{10}$, rather than a lower quantity, was, that I might not run a risk of over-rating the powers of my instruments. I have it however in contemplation, to determine this quantity experimentally, and perceive already, that the difficulties which attend this subject may be overcome.

It now only remains to show, how far the penetrating power, 192, of my large reflector, will really reach into space. Then, since this number has been calculated to be in proportion to the standard of natural vision, it follows, that if we admit a star of the 7th magnitude to be visible to the unassisted eye, this telescope will show stars of the 1342d magnitude. But, as we did not stop at the single stars above-mentioned, when the penetration of the natural eye was to be ascertained, so we must now also call the united lustre of sidereal systems to our aid in stretching forwards into space. Suppose therefore a cluster of 5000 stars to be at one of those immense distances, to which only a 40-feet reflector can reach, and our formula will give us the means of calculating what that may be. For, putting s for the number of stars in the cluster, and D for its distance, we have $\frac{\sqrt{x\mathrm{A}^2\mathrm{s}}}{a} = \mathrm{D}$;* which, on computation, comes out to be above 11¾ millions of millions of millions of miles! A number which exceeds the distance of the nearest fixed star, at least 300000 times.

From the above considerations it follows, that the range for observing, with a telescope such as my 40-feet reflector, is indeed very extensive, we have the inside of a sphere to examine, the radius of which is the immense distance just now assigned to be within the reach of the penetration of our instruments, and of which all the celestial objects visible to the eye, put together, form as it were but the kernel, while all the immensity of its thick shell is reserved for the telescope. It follows, in the next place, that much time must be required for going through so extensive a range. The method of examining the heavens, by sweeping over space, instead of looking merely at places that are known to contain objects, is the only one that can be useful for discoveries. In order therefore to calculate how long a time it must take to sweep the heavens, as far as they are within the reach of my 40-feet telescope, charged with a magnifying power of 1000, I have had recourse to my journals, to find how many favourable hours we may annually hope for in

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* D = 11,765475,948678,678679 miles.—Orig.

this climate. It is to be noticed, that the nights must be very clear; the moon absent; no twilight; no haziness; no violent wind; and no sudden change of temperature; then also, short intervals for filling up broken sweeps will occasion delays; and, under all these circumstances, it appears that a year which will afford 90, or at most 100 hours, is to be called very productive.

In the equator, with my 20-feet telescope, I have swept over zones of 2°, with a power of 157; but an allowance of 10 minutes in polar distance must be made, for lapping the sweeps over each other where they join. As the breadth of the zones may be increased towards the poles, the northern hemisphere may be swept in about 40 zones: to these we must add 19 southern zones; then, 59 zones, which, on account of the sweeps lapping over each other about 5^m of time in right ascension, we must reckon of 25 hours each, will give 1475 hours. And, allowing 100 hours per year, we find that, with the 20-feet telescope, the heavens may be swept in about 14 years and $\frac{3}{4}$. Now the time of sweeping, with different magnifying powers, will be as the squares of the powers; and, putting p and t for the power and time in the 20-feet telescope, and $P = 1000$ for the power in the 40, we shall have $p^2 : t :: P^2 : \frac{tP^2}{p^2} = 59840$. Then, making the same allowance of 100 hours per year, it appears that it will require not less than 598 years, to look with the 40-feet reflector, charged with the above-mentioned power, only one single moment into each part of space; and even then, so much of the southern hemisphere will remain unexplored, as will take up 213 years more to examine.

V. A Second Appendix to the Improved Solution of a Problem in Physical Astronomy, inserted in the Philos. Trans. for 1798, *containing some further Remarks, and Improved Formulæ for computing the Co-efficients* A *and* B; *by which the Arithmetical Work is considerably Shortened and Facilitated. By the Rev. John Hellins, B. D., F. R. S., &c.* p. 86.

It was shown, in art. 9, of the first appendix, how the common logarithm of the fraction $\frac{1 + \sqrt{(1 - cc)}}{c}$, when c is expressed in numbers, may be taken out from the best log. tables. Yet that method of obtaining the value of α, easy as it is, requires, first, a search in the table for the angle of which c is the sine, and generally a proportion for the fractional parts of a second; then, a division of the degrees, minutes, and seconds contained in that angle, by 2; and, thirdly, another search for the logarithmic tangent of half the angle, and another proportion to find the fractional parts of a second. Mr. H. was therefore desirous of finding some easier and shorter method of performing the whole business, without the use of any trigonometrical tables, in which time is required, not only in searching for logarithms, but also in making proportions for the fractional parts of a second; and, after some consideration, he discovered that which is here explained. This method then, together with some further observations made for facilitating and abridging the work of computing the values of the letters A and B, used in the former paper, make up the contents of this paper.

The H.L. $\frac{1+\surd(1-cc)}{c}$, which was denoted by α, both in the solution of the problem and in the appendix, is $=$ H.L. $\frac{2}{c}$, $-\frac{cc}{22}-\frac{3c^4}{2.44}-\frac{3.5c^6}{246.6}$, &c. and if, for the sake of distinction, the Roman letter a be put for H.L. $\frac{2}{c}$, we shall have $\alpha=\mathrm{a}-\frac{cc}{4}-\frac{3c^4}{32}$, &c. (of which series, the first 3 terms are sufficient for the present purpose); and this value of α being written for it in the expression $\alpha\times(1+\frac{3}{8}cc+\frac{3.5}{8.8}c^4)$, which occurs in the first theorem in art. 12, of the first appendix, we have $(1+\frac{3}{8}cc+\frac{3.5}{8.8}c^4)\times(\mathrm{a}-\frac{cc}{4}-\frac{3}{4.8}c^4)$; that is, by actual multiplication,

$$\begin{array}{l} \mathrm{a}+\frac{3}{8}\mathrm{a}cc+\frac{3.5}{8.8}\mathrm{a}c^4 \\ \qquad -\frac{1}{4}cc-\frac{3}{16}c^4. \end{array}$$

Now the terms $-\frac{1}{4}cc$ and $-\frac{3}{16}c^4$ may very easily be added to the terms fcc and gc^4, i.e. to $0{\cdot}1036802\,cc$ and $0{\cdot}0687064\,c^4$, which will then become $-0{\cdot}1463198\,cc$, and $-0{\cdot}1187936\,c^4$; and, by denoting the co-efficients of these new terms by the Roman letters $-\mathrm{f}$ and $-\mathrm{g}$ respectively, the first theorem in the art. beforementioned, or the value of A, is

$$\mathrm{A}=\frac{1}{\pi(a+b)\frac{3}{2}}\times\left\{\begin{array}{l}\frac{2}{cc}+e-\mathrm{f}cc-\mathrm{g}c^4 \\ \quad+\mathrm{a}+\frac{3}{8}\mathrm{a}cc+\frac{3.5}{8.8}\mathrm{a}c^4.\end{array}\right.$$

The expression $\alpha(\frac{3}{4}+\frac{3.5}{4.12}cc+\frac{3.5.21}{4.12.32}c^4)$, which occurs in the value of A′, in art. 12, of the first appendix, is $=$

$$\left\{\begin{array}{l}\frac{3}{4}+\frac{3.5}{4.12}cc+\frac{3.5.21}{4.12.32}c^4 \\ \times\mathrm{a}-\frac{1}{4}cc\;-\frac{3}{4.8}c^4\end{array}\right\}=\left\{\begin{array}{l}\frac{3}{4}\mathrm{a}+\frac{3.5}{4.12}\mathrm{a}cc+\frac{3.5.21}{4.12.32}\mathrm{a}c^4 \\ \qquad-\frac{3}{16}cc\qquad-\frac{19}{8.16}c^4.\end{array}\right.$$

Here again the terms $-\frac{3}{16}cc$ and $-\frac{19}{8.16}c^4$ may very easily be added to the terms icc and kc^4, i. e. to $0{\cdot}0551502\,cc$ and $0{\cdot}0408309\,c^4$, and we have the two new terms $-0{\cdot}1323498\,cc$ and $-0.1076091\,c^4$. Let the coefficients of these two new terms be denoted by the Roman letters $-\mathrm{i}$ and $-\mathrm{k}$ respectively, and the 2d theorem in art. 12 of the first appendix becomes

$$\mathrm{A}'=\frac{1}{\pi(a+b)\frac{5}{2}}\times\left\{\begin{array}{l}\frac{4}{3c^4}+\frac{1}{cc}+h-\mathrm{i}cc-\mathrm{k}c^4 \\ \qquad\qquad+\frac{3}{4}\mathrm{a}+\frac{3.5}{4.12}\mathrm{a}cc+\frac{3.5.21}{4.12.32}\mathrm{a}c^4.\end{array}\right.$$

The product of $\alpha(2+\frac{1}{4}cc+\frac{9}{16}c^4)$, which is found in the 3d theorem of the art. before referred to, is $=$

$$\left\{\begin{array}{l} 2 + \frac{1}{2}cc + \frac{9}{16}c^4 \\ \times\ \mathrm{a} - \frac{1}{4}cc - \frac{3}{4.8}c^4 \end{array}\right\} = \left\{\begin{array}{l} 2\mathrm{a} + \frac{1}{2}\mathrm{a}cc + \frac{9}{16}\mathrm{a}c^4 \\ \quad - \frac{1}{2}cc - \frac{5}{16}c^4. \end{array}\right.$$

Here likewise, the terms $-\frac{1}{2}cc$ and $-\frac{3}{16}c^4$ may be added to $0{\cdot}3465736cc$ and $0{\cdot}1793226c^4$, which are $= lcc$ and mc^4 respectively; the co-efficients of which being denoted by the Roman letters l and m, the 3d theorem in the art. before referred to becomes

$$\mathrm{B} = \frac{2a}{b}\mathrm{A} - \frac{2}{\pi b\,(a+b)^{\frac{1}{2}}} \times \left\{\begin{array}{l} \varrho - \mathrm{l}cc - \mathrm{m}c^4 \\ + 2\mathrm{a} + \frac{1}{2}\mathrm{a}cc + \frac{9}{2.16}\mathrm{a}c^4. \end{array}\right.$$

These new forms, to which the theorems are now brought, it is evident are no less convenient, and on examination they will be found no less accurate, than the original ones; and that the common logarithm of $\frac{2}{c}$, and consequently the hyperbolic logarithm of it, is much more easily and expeditiously obtained than the common logarithm of $\frac{1+\sqrt{(1-cc)}}{c}$, is too obvious to need a description; and therefore it follows, that a computation by these new formulæ will be easier and shorter than by those in the first appendix. Mr. H. besides mentions some other subordinate expedients for facilitating the computations; he then adds as follows.

Having now described these short and easy methods of computing the values of a, b, and c, and of deriving the other logarithmic terms from them, and having introduced a new and more compendious notation of several of the terms in each of the first 3 theorems, it will be proper next to exhibit those theorems in this improved state, and, after that, to give an example or 2 of computing by them.

$$1.\ \mathrm{A} = \frac{1}{\pi\,(a+b)^{\frac{3}{2}}} \times \left\{\begin{array}{l} \frac{2}{cc} + e - \mathrm{f}cc - \mathrm{g}c^4 \\ + \mathrm{a} + \frac{3}{8}\mathrm{a}cc\ (= \mathrm{b}) + \frac{3.5}{8.8}\mathrm{a}c^4\ (= \mathrm{c}). \end{array}\right.$$

$$2.\ \mathrm{A}' = \frac{1}{\pi\,(a+b)^{\frac{5}{2}}} \times \left\{\begin{array}{l} \frac{4}{3c^4} + \frac{1}{cc} + h - \mathrm{i}cc - \mathrm{k}c^4 \\ \quad + \mathrm{s} - \frac{\mathrm{a}}{4} - \frac{\mathrm{b}}{6} - \frac{\mathrm{c}}{8}. \end{array}\right.$$

$$3.\ \mathrm{B} = \frac{2a}{b}\mathrm{A} - \frac{2}{\pi b\,(a+b)^{\frac{1}{2}}} \times \left\{\begin{array}{l} \quad \varrho - \mathrm{l}cc - \mathrm{m}c^4 \\ + \mathrm{s} + \mathrm{a} + \frac{\mathrm{b}}{3} + \frac{\mathrm{c}}{5}. \end{array}\right.$$

The ingenious author then concludes with an example or two, computed in numbers by these theorems.

VI. Account of a Peculiarity in the Distribution of the Arteries sent to the Limbs of Slow-moving Animals; with some other Similar Facts. By Mr. Anthony Carlisle, Surgeon. p. 98.

The lemur tardigradus, in its injected state, accompanies this paper; and, for the kind of preparation, the vessels are filled with more than ordinary success. The arteries alone are injected; and the peculiarity of their arrangement is to be observed in the axillary arteries, and in the iliacs. These vessels, at their entrance into the upper and lower limbs, are suddenly divided into a number of equal-sized cylinders, which occasionally anastomose with each other. They are exclusively distributed on the muscles; while the arteries sent to all the parts of the body, excepting the limbs, divide in the usual arborescent form; and even those arteries of the limbs which are employed on substances not muscular, branch off like the common blood-vessels. I counted 23 of these cylinders, parallel to each other, about the middle of the upper arm; and 17 in the inguinal fasciculus.

This fact appeared at first too solitary for the foundation of any physiological reasoning; but having since had an opportunity of prosecuting the inquiry among animals of similar habits and character, I have been encouraged to hope that the result may eventually assist in the elucidation of muscular motion. The bradypus tridactylus, or great American Sloth, has a similar distribution of the arteries of its limbs to that already described in the lemur tardigradus. The communications of these vessels with each other are more frequent than in the lemur tardigradus, and their number is considerably greater. I counted 42 separate cylinders on the superficies of the brachial fasciculus; and from the bulk of the fasciculus I estimate that there were 20, or more, concealed in the middle. The lower extremity has its arteries less divided, and they are of larger diameter. I observed only 34 branches in the middle of the thigh; and the first series of ramifications were larger than the subsequent ones. May not this have some relation to the greater distance of the lower limb from the heart? The extremely slow movements of the bradypus tridactylus are sufficiently known among natural historians.

The bradypus didactylus has its arterial system distributed in some degree like the tridactylus; but the brachial artery in the upper limb is much less subdivided; and in the lower limb the arteries of the plexus afterwards divide a few times in the arborescent form. It may be worthy of remark, that this correspondence of arrangement, in the arteries of the lesser Sloth, bears a striking analogy with the structure and habits of the large American Sloth; the movements of the bradypus didactylus being universally represented quicker than those of the bradypus tridactylus.

The Lemur Loris was next examined, and its arterial system was found to resemble those already described; but, as the animal had been preserved in very strong spirit, the vessels were so corrugated as not to admit of injection. The 2 bradypi were injected with quicksilver. The natural history of the Lemur Loris appears not to be very well ascertained; but it is a slow-moving animal, and has been confounded with the species called tardigradus, though doubtless a much more agile creature. In all the quadrupeds before mentioned, the other blood-vessels, as well as the nerves, presented the common appearances. The size of the heads, and the interior capacity of the skulls, both in the bradypus tridactylus and the lemur tardigradus, seemed smaller in proportion than is usual among animals, so that the quantity of brain must be less than ordinary.

The effect of this peculiar disposition of the arteries, in the limbs of these slow-moving quadrupeds, will be that of retarding the velocity of the blood. It is well known, and has been explained by various writers, that the blood moves quicker in the arteries near the heart, than in the remote branches; and also, that fluids move more rapidly through tubes which branch off suddenly from large trunks, than if they had been propelled for a considerable distance through small-sized cylinders; besides the frequent communications in the cylinders of the bradypus tridactylus must produce eddies, which will retard the progress of the fluid. From these and a variety of other facts, it will appear, that one effect on the animal economy, connected with this arrangement of vessels, must be, that of diminishing the velocity of the blood passing into the muscles of the limbs. It may be difficult to determine, whether the slow movement of the blood sent to these muscles be a subordinate convenience to other primary causes of their slow contraction, or whether it be of itself the immediate and principal cause. The facts at present ascertained, relative to muscular motion, do not authorize me to treat decidedly of the share which the vascular system holds in the operation of muscular contraction. Certain it is, that a larger proportion of arteries is sent to the muscles of quadrupeds, than to the ordinary substances; and the extreme redness of these organs shows that their capillaries are of large diameter. A greater degree of redness is also observable in those muscles, of the same animal, which are most frequently called into action. The habits of life among the tardigrade animals, give occasion for the long continued contraction of some muscles in their limbs: these creatures are represented clinging to the boughs of trees, and remaining thus, without locomotion, for several hours. The powers which require so long a time to determine the contraction of a series of muscles, are probably no less slow in restoring the parts to their former condition; or, if the restoration is to be affected by antagonist muscles under the same circumstances, then the flexion and extension of every part of the limbs will correspond, as to time.

I have not met with any arrangement of blood-vessels analogous to those described, except in the carotid artery of the Lion. May not this peculiarity be subservient to the long continued exertion of the muscles of his jaws, while holding a powerful animal, such as a horse or buffalo, and thus enable him to retain his prey, till it is wearied out by ineffectual struggles? I believe also, that those animals which chew the cud have a plexus of arteries in the neck, analogous to the rete mirabile: but this fact has not yet been verified in all the ruminating quadrupeds; and the effect of these arrangements seems rather to operate as sluices to the arteries of the masticating muscles, than directly as the means of retarding the velocity of their fluids. It is however necessary to examine these subjects more accurately*.

P. S. The Maucauco which Mr. John Symmons lately possessed, was sufficiently quick in the movements of its head to snap a person's finger, when touched incautiously; and the motion of its jaw, when chewing, was not slower than in other animals. A Maucauco of the same species, kept among the wild beasts in the Tower, was very apt to bite those who, calculating the movements of its head by those of its limbs, approached within the length of its neck: the chewing of this animal was similar to that of a cat.

VII. Outlines of Experiments and Inquiries respecting Sound and Light. By Thos. Young, M. D., F. R. S. p. 106.

It has long been my intention to lay before the R. S. a few observations on the subject of sound; and I have endeavoured to collect as much information, and to make as many experiments, connected with this inquiry, as circumstances enabled me to do; but the further I have proceeded, the more widely the prospect of what lay before me has been extended; and as I find that the investigation, in all its magnitude, will occupy the leisure hours of some years, or perhaps of a life, I am determined, in the mean time, lest any unforeseen circumstances should prevent my continuing the pursuit, to submit to the Society some conclusions which I have already formed from the results of various experiments. Their subjects are, 1. The measurement of the quantity of air discharged through an aperture. 2. The determination of the direction and velocity of a stream of air proceeding from an orifice. 3. Ocular evidence of the nature of sound. 4. The velocity of sound. 5. Sonorous cavities. 6. The degree of divergence of sound. 7. The decay of sound. 8. The harmonic sounds of pipes. 9. The vibrations of different elastic fluids. 10. The analogy between light and sound. 11. The coalescence of musical sounds. 12. The frequency of vibrations constituting a given note. 13. The vibrations of chords. 14. The vibrations of rods and plates. 15. The human voice. 16. The temparement of musical intervals.

1. *Of the quantity of air discharged through an aperture.*—A piece of bladder was tied over the end of the tube of a large glass funnel, and punctured with a hot needle. The funnel was inverted in a vessel of water; and a gage, with a graduated glass tube, was so placed as to measure the pressure occasioned by the different levels of the surfaces of the water. As the air escaped through the puncture, it was supplied by a phial of known dimensions, at equal intervals of time; and according to the frequency of this supply, the average height of the gage was such as is expressed in the first table. It appears, that the quantity of air discharged by a given aperture, was nearly in the subduplicate ratio of the

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* There is a rete mirabie in the genus bos, and in some of the cervi which I have seen; but of these and the other pecora a fuller description will be given in a future paper.—Orig.

pressure; and that the ratio of the expenditures by different apertures, with the same pressure, lay between the ratio of their diameters and that of their areas. The 2d, 3d, and 4th tables show the result of similar experiments, made with some variations in the apparatus. It may be inferred, from comparing the experiments on a tube with those on a simple perforation, that the expenditure is increased, as in water, by the application of a short pipe.

TABLE 1.

A	B	C
.00018	.25	3.9
.00018	.58	11.7
.10018	1.	15.6
.001	.045	7.8
.001	.2	15.6
.001	.7	31.2
.004	.35	46.8

A is the area, in square inches of an aperture nearly circular. B, the pressure in inches. C, the number of cubic inches discharged in one minute.

All numbers throughout this paper, where the contrary is not expressed, are to be understood of inches, linear, square, or cubic.

TABLE 2.

A	B	C
.07	1.	2000.
.07	2.	2900.

A is the area of the section of a tube about 2 inches long. B, the pressure. C, the quantity of air discharged in a minute by estimation.

TABLE 3.

A	B	C	D
.0064	1.15	.2	46.8
.0064	10.	.45	46.8
.0064	13.5	.35	31.2
.0064	13.5	.7	46.8

A is the area of the section of a tube. B, its length. C, the pressure. D, the discharge in a minute.

TABLE 4.

A	B	C
.003	.28.	46.8

A is the area of an oval aperture, formed by flattening a glass tube at the end: its diameters were .025 and .152. B, the pressure. C, the discharge.

2. *Of the direction and velocity of a stream of air.*—An apparatus was contrived for measuring, by means of a water-gage communicating with a reservoir of air, the pressure by which a current was forced from the reservoir through a cylindrical tube; and the gage was so sensible that, a regular blast being supplied from the lungs, it showed the slight variation produced by every pulsation of the heart. The current of air issuing from the tube was directed downwards, on a white plate, on which a scale of equal parts was engraved, and which was thinly covered with a coloured liquid; the breadth of the surface of the plate laid bare was observed at different distances from the tube, and with different degrees of pressure, care being taken that the liquid should be so shallow as to yield to the slightest impression of air. The results are collected in tables 5 and 6, and are exhibited to the eye in plate 9, figs. 1 to 12. To measure with greater certainty and precision, the velocity of every part of the current, a 2d cavity, furnished with a gage, was provided, and pieces perforated with apertures of different sizes were adapted to its orifice: the axis of the current was directed as accurately as possible to the centres of these apertures, and the result of the experiments, with various pressures and distances, are inserted in tables 7, 8, 9. The velocity of a stream being, both according to the commonly received opinion and to the experiments already related, nearly in the subduplicate ratio of the pressure occasioning it, it was inferred, that an equal

pressure would be required to stop its progress, and that the velocity of the current, where it struck against the aperture, must be in the subduplicate ratio of the pressure marked by the gage. The ordinates of the curves in figs. 13 to 23, were therefore taken reciprocally in the subduplicate ratio of the pressure marked by the 2d gage to that indicated by the first, at the various distances represented by the abscisses. Each figure represents a different degree of pressure in the first cavity. The curve nearest the axis, is deduced from observations in which the aperture opposed to the tube was not greater than that of the tube itself; and shows what would be the diameter of the current, if the velocities of every one of its particles in the same circular section, including those of the contiguous air, which must have acquired as much motion as the current has lost, were equal among themselves. As the central particles must be supposed to be less impeded in their motion than the superficial ones, of course the smaller the aperture opposed to the centre of the current, the greater velocity ought to come out, and the ordinate of the curve the smaller; but where the aperture was not greater than that of the tube, the difference of the velocities at the same distance was scarcely perceptible. When the aperture was larger than that of the tube, if the distance was very small, of course the average velocity came out much smaller than that which was inferred from a smaller aperture; but where the ordinate of the internal curve became nearly equal to this aperture, there was but little difference between the velocities indicated with different apertures. Indeed, in some cases, a larger aperture seemed to indicate a greater velocity: this might have arisen in some degree from the smaller aperture not having been exactly in the centre of the current; but there is greater reason to suppose, that it was occasioned by some resistance derived from the air returning between the sides of the aperture and the current entering it. Where this took place, the external curves, which are so constructed as that their ordinates are reciprocally in the subduplicate ratio of the pressure observed in the 2d cavity, with apertures equal in semidiameter to their initial ordinate, approach, for a short distance, nearer to the axis than the internal curve: after this, they continue their course very near to this curve. Hence it appears, that no observable part of the motion diverged beyond the limits of the solid which would be formed by the revolution of the internal curve, which is seldom inclined to the axis in an angle so great as 10°. A similar conclusion may be made, from observing the flame of a candle subjected to the action of a blow-pipe: there is no divergency beyond the narrow limits of the current; the flame on the contrary, is every where forced by the ambient air towards the current, to supply the place of that which it has carried away by its friction. The lateral communication of motion, very ingeniously and accurately observed in water by Professor Venturi, is exactly similar to the motion here shown to take place in air; and these experiments fully justify him in rejecting the tenacity of water as its cause: no doubt it arises from the relative situation of the particles of the fluid, in the line of the current, to that of the particles in the contiguous strata, which is such as naturally to lead to a communication of motion nearly in a parallel direction; and this may properly be termed friction. The lateral pressure which urges the flame of a candle towards the stream of air from a blow-pipe, is probably exactly similar to that pressure which causes the inflection of a current of air near an obstacle. Mark the dimple which a slender stream of air makes on the surface of water; bring a convex body into contact with the side of the stream, and the place of the dimple will immediately show that the current is inflected towards the body; and if the body be at liberty to move in every direction, it will be urged towards the current, in the same manner as, in Venturi's experiments, a fluid was forced up a tube inserted into the side of a pipe through which water was flowing. A similar interposition of an obstacle in the course of the wind, is probably often the cause of smoky chimneys. One circumstance was observed in these experiments, which it is extremely difficult to explain, and which yet leads to very important consequences: it may be made distinctly perceptible to the eye, by forcing a current of smoke very gently through a fine tube. When the velocity is as small as possible, the stream proceeds for many inches without any observable dilatation; it then immediately diverges at a considerable angle into a cone, fig. 24; and, at the point of divergency, there is an audible and even visible vibration. The blow-pipe also affords a method of observing this phenomenon: as far as can be judged from the motion of the flame, the current seems to make something like a revolution in the surface of the cone, but this motion is too rapid to be distinctly discerned. When the pressure is increased, the apex of the cone approaches nearer to the orifice of the tube, figs. 25, 26; but no degree of pressure seems materially to alter its divergency. The distance of the apex from the orifice is not proportional to the diameter of the current; it rather appears to be the greater the smaller the current, and is much better defined in a small current than in a large one. Its distance in one experiment is expressed in table 10, from observations on the surface of a liquid; in other experiments, its respective distances were sometimes considerably less with the same degrees of pressure. It may be inferred, from the numbers of tables 7 and 8, that in several instances a greater height of the first gage produced a less height of the 2d: this arose from the nearer approach of the apex of the cone to the orifice of the tube, the stream losing a greater portion of its velocity by this divergence than it gained by the increase of pressure. At first sight, the form of the current bears some resemblance to the vena contracta of a jet of water: but Venturi has observed, that in water an increase of pressure increases, instead of diminishing, the distance of the contracted section from the orifice. Is it not possible, that the facility with which some spiders are said to project their fine threads to a great distance, may depend on the small degree of velocity with which they are thrown out, so that, like a minute current, meeting with little interruption from the neighbouring air, they easily continue their course for a considerable time?

TABLE 5.

A	1.	2.	3.	3.8
B	C	C	C	C
1.	.1	.1	.1	
2.	.12	.12	.2	
3.	.17	.25	.3	
4.	.2	.4	.4	
5.	.25	.5		
6.	.3	.52		
7.	.35	.54	.5	
8.	.37	.56		
9.	.39	.58		
10.	.40	.6	.6	.5
15.		.7		
18.	.50			
20.				

TABLE 6.

A	1.	2.
B	C	C
1.	.1	.1
2.	.13	
3.	.2	.2
4.	.25	.3
6.	.3	.4
7.	.35	.5
10.	.35	.6
15.	.35	.7
20.	.35	.7

TABLE 7.

A	.5	
B	.06	.15
C	D	D
.1	.083	
.2	.16	
.3	.25	.1
.4	.35	
.5	.45	
.6	.53	.2
.7	.6	
.8		.3
1.	.5	
1.2	.4	.4
1.5	.6	
2.	.67	.55
4.	1.3	1.
8.		2.
9.	.3	
14.	.5	

The diameter of the tube in tab. 5, is 0.7. A is the distance of the liquid from the orifice. B, the pressure. C, the diameter of the surface of the liquid displaced.

In tab. 6, the diameter of the tube, .1, A, B, and C, as in tab. 5.

In tab. 7, the diameter of the tube .06. A is the distance of the opposite aperture, from the orifice of the tube. B, the diameter of the aperture. C, the pressure, indicated by the first gage. D, the height of the second gage.

TABLE 8.

A	.5				1.				2.				4.			
B	.06	.15	.3	.5	.06	.15	.3	.5	.06	15.	3.	5.	.06	.15	.3	.5
C	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D
.1	.05	.05			.03				.017							
.2	.1	.1			.12	.08	.02		.034							
.5	.2	.22			,1	.00			.00							
1.	.32	.36	.1		.17	.1	.1	.05	.04							
2.	.52	.6	.2		.28	.22	.21	.08	.07							
3.	.8	.9	.3		.4	.36	.32	.12	.12		.1	.1				
4.	1.1	1.2	.4		.58	.52	.42	.16	.18		.15	.14				
5.		1.5	.5		.8	.68	.52	.2	.23		.2	.18	.04		.04	.05
6.		1.7	.6		1.	.83	.63	.25	.3		.25	.22	.05		.05	.06
7.		1.9	.7		1.2	1.	.75	.3	.35		.3	.26	.06		.06	.07
8.		2.1	.8		1.5	1.2	.88	.34	.4		.34	.3	.07		.07	.07
9.		2.3	.9		1.7	1.4	1.	.37	.45		.37	.34	.08		.08	.08
10.		2.6	1.		1.9	1.6	1.1	.4	.5		.4	.37	.09		.09	.09

Diameter of the tube .1. A, B, C, and D, as in table 7.

TABLE 9.

A	1.15				3.3			4.
B	.15	.3	.5	1.	.06	.15	1.	.06
C	D	D	D	D	D	D	D	D
.5	.1	.1	.1					
1.	.2	.2	.2					
2.	.4	.35	.34	.13	.1	.1	.125	
3.	.6	.5	.5	.2	.15	.15	.18	.1

TABLE 10.

A	B
.4	6.
.8	3.
1.2	1.5
1.8	1.
2.	.5
4.	.0

In tab. 9, the diameter of the tube .3. A, B, C, and D, as in tab. 7.

In tab. 10, A is the pressure, B the distance of the apex of the cone from the orifice of a tube, .1 in diameter.

3. *Ocular Evidence of the Nature of Sound.*—A tube about the 10th of an inch in diameter, with a lateral orifice half an inch from its end, filed rather deeper than the axis of the tube, fig. 27, was inserted at the apex of a conical cavity containing about 20 cubic inches of air, and luted perfectly tight; by blowing through the tube, a sound nearly in unison with the tenor c was produced. By gradually increasing the capacity of the cavity as far as several gallons, with the same mouth-piece, the sound, though faint, became more and more grave, till it was no longer a musical note. Even before this period a kind of trembling was distinguishable; and this, as the cavity was still further increased, was changed into a succession of distinct puffs, like the sound produced by an explosion of air from the lips; as slow, in some instances, as 4 or 3 in a second. These were undoubtedly the single vibrations, which, when repeated with sufficient frequency, impress on the auditory nerve the sensation of a continued sound. On forcing a current of smoke through the tube, the vibratory motion of the stream, as it passed out at the lateral orifice, was evident to the eye; though, from various circumstances, the quantity and direction of its motion could not be subjected to exact mensuration. This species of sonorous cavity seems susceptible of but few harmonic sounds. It was observed, that a faint blast produced a much greater frequency of vibrations than that which was appropriate to the cavity: a circumstance similar to this obtains also in large organ pipes; but several minute observations of this kind, though they might assist in forming a theory of the origin of vibrations, or in confirming such a theory drawn from other sources, yet, as they are not alone sufficient to afford any general conclusions, are omitted at present, for the sake of brevity.

4. *On the Velocity of Sound.*—It has been demonstrated, by M. De La Grange and others, that any impression whatever communicated to one particle of an elastic fluid, will be transmitted through that fluid with a uniform velocity, depending on the constitution of the fluid, without reference to any supposed laws of the continuation of that impression. Their theorem for ascertaining this velocity is the same as Newton has deduced from the hypothesis of a particular law of continuation: but it must be confessed, that the result differs somewhat too widely from experiment, to give us full confidence in the perfection of the theory. Corrected by the experiments of various observers, the velocity of any impression transmitted by the common air, may, at an average, be reckoned 1130 feet in a second.

5. *Of Sonorous Cavities.*—M. De la Grange has also demonstrated, that all impressions are reflected by an obstacle terminating an elastic fluid, with the same velocity with which they arrived at that obstacle. When the walls of a passage, or of an unfurnished room, are smooth and perfectly parallel, any explosion, or a stamping with the foot, communicates an impression to the air, which is reflected from one wall to the other, and from the 2d again towards the ear, nearly in the same direction with the primitive impulse: this takes place as frequently in a

second, as double the breadth of the passage is contained in 1130 feet, and the ear receives a perception of a musical sound, thus determined in its pitch by the breadth of the passage. On making the experiment, the result will be found accurately to agree with this explanation. If the sound is predetermined, and the frequency of vibrations such as that each pulse, when doubly reflected, may coincide with the subsequent pulse proceeding directly from the sounding body, the intensity of the sound will be much increased by the reflection; and also in a less degree, if the reflected pulse coincides with the next but 1, the next but 2, or more, of the direct pulses. The appropriate notes of a room may readily be discovered by singing the scale in it; and they will be found to depend on the proportion of its length or breadth to 1130 feet. The sound of the stopped diapason pipes of an organ is produced in a manner somewhat similar to the note from an explosion in a passage; and that of its reed pipes to the resonance of the voice in a room: the length of the pipe in one case determing the sound, in the other, increasing its strength. The frequency of the vibration does not at all immediately depend on the diameter of the pipe. It must be confessed, that much remains to be done in explaining the precise manner in which the vibration of the air in an organ pipe is generated. M. Daniel Bernoulli has solved several difficult problems relating to the subject; yet some of his assumptions are not only gratuitous, but contrary to matter of fact.

6. *On the Divergence of Sound.*—It has been generally asserted, chiefly on the authority of Newton, that if any sound be admitted through an aperture into a chamber, it will diverge from that aperture equally in all directions. The chief arguments in favour of this opinion are deduced from considering the phenomena of the pressure of fluids, and the motion of waves excited in a pool of water. But the inference seems to be too hastily drawn: there is a very material difference between impulse and pressure; and, in the case of waves of water, the moving force at each point is the power of gravity, which, acting primarily in a perpendicular direction, is only secondarily converted into a horizontal force, in the direction of the progress of the waves, being at each step disposed to spread equally in every direction: but the impulse transmitted by an elastic fluid, acts primarily in the direction of its progress. It is well known, that if a person calls to another with a speaking trumpet, he points it towards the place where his hearer stands: and I am assured by a very respectable member of the R. S., that the report of a cannon appears many times louder to a person towards whom it is fired, than to one placed in a contrary direction. It must have occurred to every one's observation, that a sound such as that of a mill, or a fall of water, has appeared much louder after turning a corner, when the house or other obstacle no longer intervened; and it has been already remarked by Euler, on this head, that we are not acquainted with any substance perfectly impervious to sound. Indeed, as M. Lambert has very truly asserted, the whole theory of the speaking trumpet, supported as it is by practical experience, would fall to the ground, if it were demonstrable that

sound spreads equally in every direction. In windy weather it may often be observed, that the sound of a distant bell varies almost instantaneously in its strength, so as to appear at least twice as remote at one time as at another. Now if sound diverged equally in all directions, the variation produced by the wind could never exceed $\frac{1}{10}$ of the apparent distance; but, on the supposition of a motion nearly rectilinear, it may easily happen that a slight change in the direction of the wind may convey the sound, either directly or after reflection, in very different degrees of strength, to the same spot. From the experiments on the motion of a current of air, already related, it would be expected that a sound, admitted at a considerable distance from its origin through an aperture, would proceed, with an almost imperceptible increase of divergence, in the same direction; for, the actual velocity of the particles of air, in the strongest sound, is incomparably less than that of the slowest of the currents in the experiments related, where the beginning of the conical divergence took place at the greatest distance. Dr. Matthew Young has objected, not without reason, to M. Hube, that the existence of a condensation will cause a divergence in sound: but a much greater degree of condensation must have existed in the currents described than in any sound. There is indeed one difference between a stream of air and a sound; that, in sound, the motions of different particles of air are not synchronous: but it is not demonstrable that this circumstance would affect the divergency of the motion, except at the instant of its commencement, and perhaps not even then in a material degree; for, in general, the motion is communicated with a very gradual increase of intensity.

7. *On the Decay of Sound.*—Various opinions have been entertained respecting the decay of sound. M. De la Grange has published a calculation, by which its force is shown to decay nearly in the simple ratio of the distances; and M. Dan. Bernoulli's equations for the sounds of conical pipes lead to a similar conclusion. The same inference would follow from a completion of the reasoning of Dr. Helsham, Dr. Mat. Young, and Mr. Venturi. It has been very elegantly demonstrated by Maclaurin, and may also be proved in a much more simple manner, that when motion is communicated through a series of elastic bodies increasing in magnitude, if the number of bodies be supposed infinitely great, and their difference infinitely small, the motion of the last will be to that of the first in the subduplicate ratio of their respective magnitudes; and since, in the case of concentric spherical laminæ of air, the bulk increases in the duplicate ratio of the distance, the motion will in this case be directly, and the velocity inversely, as the distance. But, however true this may be of the first impulse, it will appear, by pursuing the calculation a little further, that every one of the elastic bodies, except the last, receives an impulse in a retrograde direction, which ultimately impedes the effect of the succeeding impulse, as much as a similar cause promoted that of the preceding one: and thus, as sound must be conceived to consist of an infinite number of impulses, the motion of the last lamina will be precisely equal to that of the first; and, as far as this mode of reasoning goes, sound must decay in the

duplicate ratio of the distance. Hence it appears, that the proposal for adopting the logarithmic curve for the form of the speaking trumpet, was founded on fallacious reasoning. The calculation of M. de la Grange is left for future examination; and it is intended, in the mean time, to attempt to ascertain the decay of sound as nearly as possible by experiment: should the result favour the conclusions from that calculation, it would establish a marked difference between the propagation of sound and of light.

8. *On the Harmonic Sounds of Pipes.*—In order to ascertain the velocity with which organ pipes of different lengths require to be supplied with air, according to the various appropriate sounds they produce, a set of experiments was made, with the same mouth-piece, on pipes of the same bore, and of different lengths, both stopped and open. The general result was, that a similar blast produced as nearly the same sound as the length of the pipes would permit; or at least that the exceptions, though very numerous, lay equally on each side of this conclusion. The particular results are expressed in table 11, and in fig. 28. They explain how a note may be made much louder on a wind instrument by a swell, than it can possibly be by a sudden impression of the blast. It is proposed, at a future time, to ascertain by experiment, the actual compression of the air within the pipe under different circumstances; from some very slight trials, it seemed to be nearly in the ratio of the frequency of vibrations of each harmonic.

TABLE 11.

OPEN.

A	B	C	D	E	F
4.5		0.7	8.8	$\overline{\overline{\overline{d}}}$※	1
	4.1	8.8			2
9.4		0.3	0.9	$\overline{\overline{f}}$	1
	0.8		8.0		2
	2.0		18.0		3
	5.0	8.0	20.0		4
	16.5	18.0			5
	19.0	20.0			6
16.1		0.4	1.0	$\overline{\overline{g}}$※	2
	0.8	1.0	2.2		3
	1.2	2.2	4.7		4
	2.2	4.7	11.5		5
	3.4		13.5		6
	4.0		15.0		7
	6.5	10.0			8
20.5		0.6	0.8	$\overline{\overline{b}}$	3
		0.8	1.9		4
	1.1	1.9	5.7		5
	4.5	5.7			8

STOPPED.

A	B	C	D	E	F
4.5		0.3	1.8	$\overline{\overline{d}}$	1
	1.2	1.7	10.0		3
	5.0	9.0			5
9.4		0.2	0.4	$\overline{f}$	1
		0.45	1.6		3
	1.1	1.6	8.5		5
	7.0	8.0			7
16.1		0.4	0.6	$\overline{\overline{d}}$※	3
	0.6	0.65	1.1		5
	0.9	1.1	2.4		7
	1.6	2.4	4.9		9
	2.5	4.8	9.0		11
	6.0	7.0			13
20.5		0.8	1.1	$\overline{\overline{\overline{c}}}$※	7
	1.0	1.1	3.8		9
	1.8		3.8		11
	3.2	3.8	12.		17
		12.		0	00

A, is the length of the pipe from the lateral orifice to the end. C, the pressure at which the sound began. B, its termination, by lessening the pressure; D, by increasing it. E, the note answering to the first sound of each pipe, according to the German method of notation. F, the number showing the place of each note in the regular series of harmonics. The diameter of the pipe was .35; the air duct of the mouth-piece measured, where smallest, .25 by .035; the lateral orifice .25 by .125. The apparatus was not calculated to apply a pressure of above 22 inches. Where no number stands under C, a sudden blast was required to produce the note.

9. *On the Vibrations of different Elastic Fluids.*—All the methods of finding the velocity of sound agree in determining it to be, in fluids of a given elasticity, reciprocally in the subduplicate ratio of the density: hence, in pure hydrogen gas it should be $\sqrt{13} = 3.6$ times as great as in common air; and the pitch of a pipe should be a minor 14th higher in this fluid than in the common air. It is therefore probable that the hydrogen gas used in Professor Chladni's late experiments was not quite pure. It must be observed, that in an accurate experiment of this nature, the pressure causing the blast ought to be carefully ascertained. There can be no doubt but that, in the observations of the French academicians on the velocity of sound, which appear to have been conducted with all possible attention, the dampness and coldness of the night air must have considerably increased its density: hence the velocity was found to be only 1109 feet in a second; while Derham's experiments, which have an equal appearance of accuracy, make it amount to 1142. Perhaps the average may, as has been already mentioned, be safely estimated at 1130. It may here be remarked, that the well-known elevation of the pitch of wind instruments, in the course of playing, sometimes amounting to half a note, is not, as is commonly supposed, owing to any expansion of the instrument, for this should produce a contrary effect, but to the increased warmth of the air in the tube. Dr. Smith has made a similar observation, on the pitch of an organ in summer and winter, which he found to differ more than twice as much as the English and French experiments on the velocity of sound. Bianconi found the velocity of sound, at Bologna, to differ at different times, in the ratio of 152 to 157.

10. *Of the Analogy between Light and Sound.*—Ever since the publication of Sir Isaac Newton's incomparable writings, his doctrines of the emanation of particles of light from lucid substances, and of the formal pre-existence of coloured rays in white light, have been almost universally admitted in this country, and but little opposed in others. Leonard Euler indeed, in several of his works, has advanced some strong objections against them, but not sufficiently powerful to justify the dogmatical reprobation with which he treats them; and he has left that system of an ethereal vibration, which after Huygens and some others he adopted, equally liable to be attacked on many weak sides. Without pretending to decide positively on the controversy, it is conceived that some considerations may be brought forwards, which may tend to diminish the weight of objections to a theory similar to the Huygenian. There are also one or two difficulties in the Newtonian system, which have been little observed. The first is, the uniform velocity with which light is supposed to be projected from all luminous bodies, in consequence of heat, or otherwise. How happens it that, whether the projecting force is the slightest transmission of electricity, the friction of 2 pebbles, the lowest degree of visible ignition, the white heat of a wind furnace, or the intense heat of the sun itself, these wonderful corpuscles are always propelled with one uniform velocity? For, if they differed in velocity, that difference ought to produce a different refraction.

But a still more insuperable difficulty seems to occur, in the partial reflection from every refracting surface. Why, of the same kind of rays, in every circumstance precisely similar, some should always be reflected, and others transmitted, appears in this system to be wholly inexplicable. That a medium resembling, in many properties, that which has been denominated ether, does really exist, is undeniably proved by the phenomena of electricity; and the arguments against the existence of such an ether throughout the universe have been pretty sufficiently answered by Euler. The rapid transmission of the electrical shock shows that the electric medium is possessed of an elasticity as great as is necessary to be supposed for the propagation of light. Whether the electric ether is to be considered as the same with the luminous ether, if such a fluid exists, may perhaps at some future time be discovered by experiment; hitherto I have not been able to observe that the refractive power of a fluid undergoes any change by electricity. The uniformity of the motion of light in the same medium, which is a difficulty in the Newtonian theory, favours the admission of the Huygenian; as all impressions are known to be transmitted through an elastic fluid with the same velocity. It has been already shown that sound, in all probability, has very little tendency to diverge: in a medium so highly elastic as the luminous ether must be supposed to be, the tendency to diverge may be considered as infinitely small; and the grand objection to the system of vibration will be removed. It is not absolutely certain, that the white line visible in all directions on the edge of a knife, in the experiments of Newton and of Mr. Jordan, was not partly occasioned by the tendency of light to diverge. Euler's hypothesis of the transmission of light, by an agitation of the particles of the refracting media themselves, is liable to strong objections; according to this supposition, the refraction of the rays of light, on entering the atmosphere from the pure ether which he describes, ought to be a million times greater than it is. For explaining the phenomena of partial and total reflection, refraction, and inflection, nothing more is necessary than to suppose all refracting media to retain, by their attraction, a greater or less quantity of the luminous ether, so as to make its density greater than that which it possesses in a vacuum, without increasing its elasticity; and that light is a propagation of an impulse communicated to this ether by luminous bodies: whether this impulse is produced by a partial emanation of the ether, or by vibrations of the particles of the body, and whether these vibrations are, as Euler supposed, of various and irregular magnitudes, or whether they are uniform, and comparatively large, remains to be hereafter determined. Now, as the direction of an impulse transmitted through a fluid, depends on that of the particles in synchronous motion, to which it is always perpendicular, whatever alters the direction of the pulse, will inflect the ray of light. If a smaller elastic body strike against a larger one, it is well-known that the smaller is reflected more or less powerfully, according to the difference of their magnitudes: thus, there is always a reflection when the rays of light pass from a rarer to a denser stratum of ether; and frequently an echo when a sound strikes against a cloud. A greater body striking a smaller one, propels it, without losing all its motion: thus, the particles of a denser stratum of ether do not impart the whole of their motion to a rarer, but, in their effort to proceed, they are recalled by the attraction of the refracting substance with equal force; and thus a reflection is always secondarily produced, when the rays of light pass from a denser to a rarer stratum. Let AB, pl. 9. fig. 29, be a ray of light falling on the reflecting surface FG; cd the direction of the vibration, pulse, impression, or condensation. When d comes to H, the impression will be, either wholly or partly, reflected with the same velocity as it arrived, and EH will be equal to DH; the angle EIH to DIH or CIF; and the angle of reflection to that of incidence. Let FG, fig. 30, be a refracting surface. The portion of the pulse IE, which is travelling through the refracting medium, will move with a greater or less velocity in the sub-duplicate ratio of the densities, and HE will be to KI in that ratio. But HE is, to the radius IH, the sine of the angle of refraction; and KI that of the angle of incidence. This explanation of refraction is nearly the same as that of Euler. The total reflection of a ray of light by a refracting surface, is explicable in the same manner as its simple refraction; HE, fig. 31, being so much longer than KI, that the ray first becomes parallel to FG, and then, having to return through an equal diversity of media, is reflected in an equal angle. When a ray of light passes near an inflecting body, surrounded, as all bodies are supposed to be, with an atmosphere of ether denser than the ether of the ambient air, the part of the ray nearest the body is retarded, and of course the whole ray inflected towards the body, fig. 32. The repulsion of inflected rays has been very ably controverted by Mr. Jordan, the ingenious author of a late publication on the Inflection of Light. It has already been conjectured by Euler, that the colours of light consist in the different frequency of the vibrations of the luminous ether: it does not appear that he has supported this opinion by any argument; but it is strongly confirmed, by the analogy between the colours of a thin plate and the sounds of a series of organ pipes. The phenomena of the colours of thin plates, require, in the Newtonian system, a very complicated supposition, of an ether, anticipating by its motion the velocity of the corpuscles of light, and thus producing the fits of transmission and reflection; and even this supposition does not much assist the explanation. It appears, from the accurate analysis of the phenomena which Newton has given, and which has by no means been superseded by any later observations, that the same colour recurs whenever the thickness answers to the terms of an arithmetical progression. Now this is precisely similar to the production of the same sound, by means of a uniform blast, from organ-pipes which are different multiples of the same length. Supposing white light to be a continued impulse or stream of luminous ether, it may be conceived to act on the plates as a blast of air does on the organ-pipes, and to produce vibrations regulated in frequency by the length of the lines which are terminated by the two refracting surfaces. It may be objected that, to complete the analogy, there should be tubes, to answer to the organ-pipes: but the tube of an organ-pipe is only necessary to prevent the divergence of the impression, and in light there is little or no tendency to diverge; and indeed, in the case of a resonant passage, the air is not prevented from becoming sonorous by the liberty of lateral motion. It would seem, that the determination of a portion of the track of a ray of light through any homogeneous stratum of ether, is sufficient to establish a length as a basis for colorific vibrations. In inflections, the length of the track of a ray of light through the inflecting atmosphere may determine its vibrations: but, in this case, as it is probable that there is a reflection from every part of the surface of the surrounding atmosphere, contributing to the appearance of the white line in every direction, in the experiments already mentioned, so it is possible that there may be some 2d reflection at the immediate surface of the body itself, and that, by mutual reflections between these 2 surfaces, something like the anguiform motion suspected by Newton may really take place; and then the analogy to the colours of thin plates will be still stronger. A mixture of vibrations, of all possible frequencies, may easily destroy the peculiar nature of each, and concur in a general effect of white light. The greatest difficulty in this system is, to explain the different degree of refraction of differently coloured light, and the separation of white light in refraction: yet, considering how imperfect the theory of elastic fluids still remains, it cannot be expected that every circumstance should at once be clearly elucidated. It may hereafter be considered how far the excellent experiments of Count Rumford, which tend very greatly to weaken the evidence of the modern doctrine of heat, may be more or less favourable to one or the other system of light and colours. It does not appear that any comparative experiments have been made on the inflection of light by substances possessed of different refractive powers; undoubtedly some very interesting conclusions might be expected from the inquiry.

11. *On the Coalescence of Musical Sounds.*—It is surprizing that so great a mathematician as Dr. Smith could have entertained for a moment, an idea that the vibrations constituting different sounds should be able to cross each other in all directions, without affecting the same individual particles of air by their joint forces: undoubtedly they cross, without disturbing each other's progress; but this can be no otherwise affected than by each particle's partaking of both motions. If this assertion stood in need of any proof, it might be amply furnished by the phenomena of beats, and of the grave harmonics observed by Romieu and Tartini; which M. De la Grange has already considered in the same point of view. In the first place, to simplify the statement, let us suppose, what probably never precisely happens, that the particles of air, in transmitting the pulses, proceed and return with uniform motions; and, in order to represent their position to the eye, let the uniform progress of time be represented by the increase of the absciss, and the distance of the particle from its original position, by the ordinate, fig. 33 to 38. Then, by supposing any 2 or more vibrations in the same direction to be combined, the joint motion will be represented by the sum or difference of the ordinates. When 2 sounds are of equal strength, and nearly of the same pitch, as

in fig. 36, the joint vibration is alternately very weak and very strong, producing the effect denominated a beat, pl. 10, fig. 43, B and C; which is slower and more marked, as the sounds approach nearer to each other in frequency of vibrations; and, of these beats there may happen to be several orders, according to the periodical approximations of the numbers expressing the proportions of the vibrations. The strength of the joint sound is double that of the simple sound only at the middle of the beat, but not throughout its duration; and it may be inferred, that the strength of sound in a concert will not be in exact proportion to the number of instruments composing it. Could any method be devised for ascertaining this by experiment, it would assist in the comparison of sound with light. In pl. 9, fig. 33, let P and Q be the middle points of the progress or regress of a particle in 2 successive compound vibrations; then, CP being = PD, KR = RN, GQ = QH, and MS = SO, twice their distance, 2RS = 2RN + 2NM + 2MS = KN + NM + NM + MO = KM + NO, is equal to the sum of the distances of the corresponding parts of the simple vibrations. For instance, if the 2 sounds be as 80 to 81, the joint vibration will be as 80.5, the arithmetical mean between the periods of the single vibrations. The greater the difference in the pitch of 2 sounds, the more rapid the beats, till at last, like the distinct puffs of air in the experiments already related, they communicate the idea of a continued sound; and this is the fundamental harmonic described by Tartini. For instance, in pl. 9, fig. 34—37, the vibrations of sounds related as 1 to 2, 4 to 5, 9 to 10, and 5 to 8, are represented; where the beats, if the sounds be not taken too grave, constitute a distinct sound, which corresponds with the time elapsing between 2 successive coincidences, or near approaches to coincidence; for, that such a tempered interval still produces a harmonic, appears from pl. 9, fig. 38. But, besides this primary harmonic, a secondary note is sometimes heard, where the intermediate compound vibrations occur at a certain interval, though interruptedly; for instance, in the coalescence of 2 sounds related to each other as 7 to 8, 5 to 7, or 4 to 5, there is a recurrence of a similar state of the joint motion, nearly at the interval of $\frac{5}{15}$, $\frac{4}{12}$, or $\frac{3}{9}$ of the whole period; hence, in the concord of a major 3d, the 4th below the key note is heard as distinctly as the double octave, as is seen in some degree in pl. 9, fig. 35; AB being nearly $\frac{2}{3}$ of CD. The same sound is sometimes produced by taking the minor 6th below the key note; probably because this 6th, like every other note, is almost always attended by an octave, as a harmonic. If the angles of all the figures resulting from the motion thus assumed be rounded off, they will approach more nearly to a representation of the actual circumstances; but, as the laws by which the motion of the particles of air is regulated, differ according to the different origin and nature of the sound, it is impossible to adapt a demonstration to them all; however, if the particles be supposed to follow the law of the harmonic curve, derived from uniform circular motion, the compound vibration will be the harmonic instead of the arithmetical mean; and the secondary sound of the interrupted vibrations will be more accurately formed, and more strongly marked, pl. 10, figs.

41, 42; the demonstration is deducible from the properties of the circle. It is remarkable, that the law by which the motion of the particles is governed, is capable of some singular alterations by a combination of vibrations. By adding to a given sound other similar sounds, related to it in frequency as the series of odd numbers, and in strength inversely in the same ratios, the right lines indicating a uniform motion may be converted very nearly into figures of sines, and the figures of sines into right lines, as in pl. 9, figs. 39, 40.

12. *Of the Frequency of Vibrations contituting a given Note.*—The number of vibrations performed by a given sound in a second, has been variously ascertained; first, by Sauveur, by a very ingenious inference from the beats of 2 sounds; and since, by the same observer and several others, by calculation from the weight and tension of a chord. It was thought worth while, as a confirmation, to make an experiment suggested, but coarsely conducted, by Mersennus, on a chord 200 inches in length, stretched so loosely as to have its single vibrations visible; and, by holding a quill nearly in contact with the chord, they were made audible, and were found, in one experiment, to recur 8.3 times in a second. By lightly pressing the chord at $\frac{1}{8}$ of its length from the end, and at other shorter aliquot distances, the fundamental note was found to be $\frac{1}{6}$ of a tone higher than the respective octave of a tuning-fork marked c: hence the fork was a comma and a half above the pitch assumed by Sauveur, of an imaginary c, consisting of 1 vibration in a second.

13. *Of the Vibrations of Chords.*—By a singular oversight in the demonstration of Dr. Brook Taylor, adopted as it has been by a number of later authors, it is asserted, that if a chord be once inflected into any other form than that of the harmonic curve, it will, since those parts which are without this figure are impelled towards it by an excess of force, and those within it by a deficiency, in a very short time arrive at or very near the form of this precise curve. It would be easy to prove, if this reasoning were allowed, that the form of the curve can be no other than that of the axis, since the tending force is continually impelling the chord towards this line. The case is very similar to that of the Newtonian proposition respecting sound. It may be proved, that every impulse is communicated along a tended chord with a uniform velocity; and this velocity is the same which is inferred from Dr. Taylor's theorem; just as that of sound, determined by other methods, coincides with the Newtonian result. But, though several late mathematicians have given admirable solutions of all possible cases of the problem, yet it has still been supposed, that the distinctions were too minute to be actually observed; especially, as it might have been added, since the inflexibility of a wire would dispose it, according to the doctrine of elastic rods, to assume the form of the harmonic curve. The theorem of Euler and De la Grange, in the case where the chord is supposed to be at first at rest, is in effect this: continue the figure each way, alternately on different sides of the axis, and in contrary positions; then, from any point of the curve, take an absciss each way, in the same proportion to the length of the chord as any given portion of time bears to the time of 1 semi-

vibration, and the half sum of the ordinates will be the distance of that point of the chord from the axis, at the expiration of the time given. If the initial figure of the chord be composed of 2 right lines, as generally happens in musical instruments and experiments, its successive forms will be such as are represented in plate 10, figs. 47, 48: and this result is fully confirmed by experiment. Take one of the lowest strings of a square piano-forte, round which a fine silvered wire is wound in a spiral form; contract the light of a window, so that, when the eye is placed in a proper position, the image of the light may appear small, bright, and well defined, on each of the convolutions of the wire. Let the chord be now made to vibrate, and the luminous point will delineate its path, like a burning coal whirled round, and will present to the eye a line of light, which, by the assistance of a microscope, may be very accurately observed. According to the different ways by which the wire is put in motion, the form of this path is no less diversified and amusing, than the multifarious forms of the quiescent lines of vibrating plates, discovered by Professor Chladni; and is indeed in one respect even more interesting, as it appears to be more within the reach of mathematical calculation to determine it; though hitherto, excepting some slight observations of Busse and Chladni, principally on the motion of rods, nothing has been attempted on the subject. For the present purpose, the motion of the chord may be simplified, by tying a long fine thread to any part of it, and fixing this thread in a direction perpendicular to that of the chord, without drawing it so tight as to increase the tension: by these means, the vibrations are confined nearly to one plane, which scarcely ever happens when the chord vibrates at liberty. If the chord be now inflected in the middle, it will be found, by comparison with an object which marked its quiescent position, to make equal excursions on each side of the axis; and the figure which it apparently occupies will be terminated by 2 lines, the more luminous as they are nearer the ends, plate 10, fig. 49. But if the chord be inflected near one of its extremities, fig. 50, it will proceed but a very small distance on the opposite side of the axis, and will there form a very bright line, indicating its longer continuance in that place; yet it will return on the former side nearly to the point from which it was let go, but will be there very faintly visible, on account of its short delay. In the middle of the chord, the excursions on each side the axis are always equal; and beyond the middle, the same circumstances take place as in the half where it was inflected, but on the opposite side of the axis; and this appearance continues unaltered in its proportions, as long as the chord vibrates at all: fully confirming the non-existence of the harmonic curve, and the accuracy of the construction of Euler and La Grange. At the same time, as M. Bernoulli has justly observed, since every figure may be infinitely approximated, by considering its ordinates as composed of the ordinates of an infinite number of trochoids of different magnitudes, it may be demonstrated, that all these constituent curves would revert to their initial state, in the same time that a similar chord bent into a trochoidal curve would perform a single vibration; and this is in some respects a convenient and compendious method of considering the problem. But when a chord vibrates freely, it never remains long in motion, without a very evident departure from the plane of the vibration; and, whether from the original obliquity of the impulse, or from an interference with the reflected vibrations of the air, or from the inequability of its own weight or flexibility, or from the immediate resistance of the particles of air in contact with it, it is thrown into a very evident rotatory motion, more or less simple and uniform according to circumstances. Some specimens of the figures of the orbits or chords are exhibited in plate 10, fig. 44. At the middle of the chord, its orbit has always 2 equal halves, but seldom at any other point. The curves of fig. 46, are described by combining together various circular motions, supposed to be performed in aliquot parts of the primitive orbit: and some of them approach nearly to the figures actually observed. When the chord is of unequal thickness, or when it is loosely tended and forcibly inflected, the apsides and double points of the orbits have a very evident rotatory motion. The compound rotations seem to demonstrate to the eye the existence of secondary vibrations, and to account for the acute harmonic sounds which generally attend the fundamental sound. There is one fact respecting these secondary notes, which seems entirely to have escaped observation. If a chord be inflected at $\frac{1}{2}$, or $\frac{1}{3}$, or any other aliquot part of its length, and then suddenly left at liberty, the harmonic note which would be produced by dividing the chord at that point is entirely lost, and is not to be distinguished during any part of the continuance of the sound. This demonstrates, that the secondary notes do not depend on any interference of the vibrations of the air with each other, nor on any sympathetic agitation of auditory fibres, nor on any effect of reflected sound on the chord, but merely on its initial figure and motion. If it were supposed that the chord, when inflected into right lines, resolved itself necessarily into a number of secondary vibrations, according to some curves which, when properly combined, would approximate to the figure given, the supposition would indeed in some respects correspond with the phenomenon related; as the co-efficients of all the curves supposed to end at the angle of inflection would vanish. But whether we trace the constituent curves of such a figure through the various stages of their vibrations, or whether we follow the more compendious method of Euler to the same purpose, the figures resulting from this series of vibrations are in fact so simple, that it seems inconceivable how the ear should deduce the complicated idea of a number of heterogeneous vibrations, from a motion of the particles of air which must be extremely regular, and almost uniform; a uniformity which, when proper precautions are taken, is not contradicted by examining the motion of the chord with the assistance of a powerful magnifier. This difficulty occurred very strongly to Euler; and La Grange even suspects some fallacy in the experiment, and that a musical ear judges from previous association. But, besides that these sounds are discoverable to an ear destitute of such associations, and, when the sound is produced by 2 strings in imperfect unison, may be verified by counting the number of their beats, the experiment already related is an undeniable proof that no fallacy of this kind exists. It must be confessed, that nothing fully satisfactory has yet occurred to account for the phenomena; but it is highly probable that the slight increase of tension produced by flexure, which is omitted in the calculations, and the unavoidable inequality of thickness or flexibility of different parts of the same chord, may, by disturbing the isochronism of the subordinate vibrations, cause all that variety of sounds which is so inexplicable without them. For, when the slightest difference is introduced in the periods, there is no difficulty in conceiving how the sounds may be distinguished; and indeed, in some cases, a nice ear will discover a slight imperfection in the tune of harmonic notes; it is also often observed, in tuning an instrument, that some of the single chords produce beating sounds, which undoubtedly arise from their want of perfect uniformity. It may be perceived that any particular harmonic is loudest, when the chord is inflected at about $\frac{1}{3}$ of the corresponding aliquot part from one of the extremities of that part. An observation of Dr. Wallis seems to have passed unnoticed by later writers on harmonics. If the string of a violin be struck in the middle, or at any other aliquot part, it will give either no sound at all, or a very obscure one. This is true, not of inflection, but of the motion communicated by a bow; and may be explained from the circumstance of the successive impulses, reflected from the fixed points at each end, destroying each other: an explanation nearly analogous to some observations of Dr. Matthew Young on the motion of chords. When the bow is applied not exactly at the aliquot point, but very near it, the corresponding harmonic is extremely loud; and the fundamental note, especially in the lowest harmonics, scarcely audible: the chord assumes the appearance, at the aliquot points, of as many lucid lines as correspond to the number of the harmonic, more nearly approaching to each other as the bow approaches more nearly to the point, pl. 10, fig. 51. According to the various modes of applying the bow, an immense variety of figures of the orbits are produced, fig. 45, more than enough to account for all the difference of tone in different performers. In observations of this kind, a series of harmonics is frequently heard in drawing the bow across the same part of the chord: these are produced by the bow; they are however not proportionate to the whole length of the bow, but depend on the capability of the portion of the bowstring, intercepted between its end and the chord, of performing its vibrations in times which are aliquot parts of the vibration of the chord: hence it would seem, that the bow takes effect on the chord but at one instant during each fundamental vibration. In these experiments, the bow was strung with the 2d string of a violin: and, in the preparatory application of resin, the longitudinal sound of Chladni was sometimes heard; but it was observed to differ at least a note in different parts of the string.

14. *Of the Vibrations of Rods and Plates.*—Some experiments were made, with the assistance of a most excellent practical musician, on the various notes produced by a glass tube, an iron rod, and a wooden ruler; and, in a case where the

tube was as much at liberty as possible, all the harmonics corresponding to the numbers from 1 to 13, were distinctly observed; several of them at the same time, and others by means of different blows. This result seems to differ from the calculations of Euler and Riccati, confirmed as they are by the repeated experiments of Chladni; it is not therefore brought forward as sufficiently controverting those calculations, but as showing the necessity of a revision of the experiments. Scarcely any note could ever be heard when a rod was loosely held at its extremity; nor when it was held in the middle, and struck $\frac{1}{7}$ of the length from one end. The very ingenious method of Chladni, of observing the vibrations of plates by strewing fine sand over them, and discovering the quiescent lines by the figures into which it is thrown, has hitherto been little known in this country: his treatise on the phenomena is so complete, that no other experiments of the kind were thought necessary. Glass vessels of various descriptions, whether made to sound by percussion or friction, were found to be almost entirely free from harmonic notes; and this observation coincides with the experiments of Chladni.

15. *Of the Human Voice.*—The human voice, which was the object originally proposed to be illustrated by these researches, is of so complicated a nature, and so imperfectly understood, that it can be on this occasion but superficially considered. No person, unless we except M. Ferrein, has published any thing very important on the subject of the formation of the voice, before or since Dodart; his reasoning has fully shown the analogy between the voice and the voix humaine and regal organ-pipes: but his comparison with the whistle is unfortunate; nor is he more happy in his account of the falsetto. A kind of experimental analysis of the voice may be thus exhibited. By drawing in the breath, and at the same time properly contracting the larynx, a slow vibration of the ligaments of the glottis may be produced, making a distinct clicking sound; on increasing the tension, and the velocity of the breath, this clicking is lost, and the sound becomes continuous, but of an extremely grave pitch: it may, by a good ear, be distinguished 2 octaves below the lowest A of a common bass voice, consisting in that case of about 26 vibrations in a second. The same sound may be raised nearly to the pitch of the common voice; but it is never smooth and clear, except perhaps in some of those persons called ventriloquists. When the pitch is raised still higher, the upper orifice of the larynx, formed by the summits of the arytænoid cartilages and the epiglottis, seems to succeed to the office of the ligaments of the glottis, and to produce a retrograde falsetto, which is capable of a very great degree of acuteness. The same difference probably takes place between the natural voice and the common falsetto: the rimula glottidis being too long to admit of a sufficient degree of tension for very acute sounds, the upper orifice of the larynx supplies its place; hence, taking a note within the compass of either voice, it may be held, with the same expanse of air, 2 or 3 times as long in a falsetto as in a natural voice; hence too, the difficulty of passing smoothly from the one voice to the other. It has been remarked, that the larynx is always elevated when the sound is acute: but this elevation is only

necessary in rapid transitions, as in a shake; and then probably because, by the contraction of the capacity of the trachea, an increase of the pressure of the breath can be more rapidly effected this way, than by the action of the abdominal muscles alone. The reflection of the sound thus produced from the various parts of the cavity of the mouth and nostrils, mixing at various intervals with the portions of the vibrations directly proceeding from the larynx, must, according to the temporary form of the parts, variously affect the laws of the motion of the air in each vibration, or, according to Euler's expression, the equation of the curve conceived to correspond with this motion, and thus produce the various characters of the vowels and semi-vowels. The principal sounding board seems to be the bony palate: the nose, except in nasal letters, affords but little resonance; for the nasal passage may be closed, by applying the finger to the soft palate, without much altering the sound of vowels not nasal. A good ear may distinctly observe, especially in a loud bass voice, besides the fundamental note, at least 4 harmonic sounds, in the order of the natural numbers; and the more reedy the tone of the voice, the more easily they are heard. Faint as they are, their origin is by no means easy to be explained. This observation is precisely confirmed, in a late dissertation of M. Knecht, published in the musical newspaper of Leipsic. Perhaps by a close attention to the harmonics entering into the constitution of various sounds, more may be done in their analysis than could otherwise be expected.

16. *Of the Temperament of Musical Intervals.*—It would have been extremely convenient for practical musicians, and would have saved many warm controversies among theoretical ones, if 3 times the ratio of 4 to 5, or 4 times that of 5 to 6, had been equal to the ratio of 1 to 2. As it happens to be otherwise, it has been much disputed in what intervals the imperfection should be placed. The Aristoxenians and Pythagoreans were in some sense the beginners of the controversy. Sauveur has given very comprehensive tables of a great number of systems of temperament; and his own now ranks among the many that are rejected. Dr. Smith has written a large and obscure volume, which, for every purpose but for the use of an impracticable instrument, leaves the whole subject precisely where it found it. Kirnberger, Marpurg, and other German writers, have disputed with great bitterness, almost every one for a particular method of tuning. It is not with any confidence of success, that one more attempt is made, which rests its chief claim to preference, on the similarity of its theory to the actual practice of the best instrument-makers. However we estimate the degree of imperfection of 2 tempered concords of the same nature, it will appear, that the manner of dividing the temperament between them does not materially alter its aggregate sum; for instance, the imperfection of a comma in a major-third, occasions it to beat very nearly twice as fast as that of half a comma. If indeed the imperfection were great, it might affect an interval so materially as to destroy its character; as, in some methods of temperament, a minor 3d diminished by 2 commas approaches more nearly to the ratio 6 to 7, than to 5 to 6; but, with this limitation, the sum

of harmony is nearly equal in all systems. Hence, if every one of the 12 major and minor 3ds occurred equally often in the compositions which are to be performed on an instrument, it would be of no great consequence, to the sum of the imperfections, among which of the 3ds they were divided: and even in this case the opinion of the best practical authors is, that the difference of character produced by a difference of proportions in various keys, would be of considerable advantage in the general effect of modulation. But when it is considered, that on an average of all the music ever composed, some particular keys occur at least twice as often as others, there seems to be a very strong additional reason for making the harmony the most perfect in those keys which are the most frequently used; since the aggregate sum of all the imperfections which occur in playing, must by this means be diminished in the greatest possible degree, and the diversity of character at the same time preserved. Indeed in practice this method, under different modifications, has been almost universal; for though many have pretended to an equal temperament, yet the methods which they have employed to attain it have been evidently defective. It appears to me, that every purpose may be answered, by making C to E too sharp by a quarter of a comma, which will not offend the nicest ear; E to G※, and A^b to C, equal; F※ to A※ too sharp by a comma; and the major 3ds of all the intermediate keys more or less perfect, as they approach more or less to C in the order of modulation. The 5ths are perfect enough in every system. The results of this method are shown in table 12. In practice, nearly the same effect may be very simply produced, by tuning from C to F, B^b, E^b, G※, C※, F※ 6 perfect 4ths; and C, G, D, A, E, B, F※, 6 equally imperfect 5ths, pl. 10, fig. 52. If the unavoidable imperfections of the 4ths be such as to incline them to sharpness, the temperament will approach more nearly to equality, which is preferable to an inaccuracy on the other side. An easy method of comparing different systems of temperament is exhibited in fig. 53, which may easily be extended to all the systems that have ever been invented.

TABLE 12

A	
C	50000
B	53224
B^b	56131
A	59676
G※	63148
G	66822
F※	71041
F	74921
E	79752
E^b	83810
D	89304
C※	94723
C	100000

B			
1	C	+	.0013487
2	G, F		.0019006
3	D, B^b		.0024525
4	A, E^b		.0034641
5	E, A^b		.0044756
6	B, C※		.0049353
7	F※		.0053950

C			
1	A, E	−	.0023603
2	D, B		.0029122
3	G, F※		.0034641
4	C, C※		.0044756
5	F, G※		.0049353
6	B^b, E^b		.0053950

D			
1	E^b, G※, C※, F※	−	.0000000
2	F, B^b, E, B		.0004597
3	C, G, D, A		.0011562

A shows the division of a monochord corresponding to each note, in the system proposed. B, the logarithm of the temperament of each of the major 3ds. C, of the minor 3ds. D, of the 5ths; C and D being both negative.

Thus I have endeavoured to advance a few steps only in the investigation of some very obscure but interesting subjects. As far as I know most of these observations are new; but if they should be found to have been already made by any other person, their repetition in a connected chain of inference may still be excusable. I am persuaded also, that at least some of the positions maintained are incontrovertibly consistent with truth and nature; but should further experiments tend to confute any opinions here suggested, I shall relinquish them with as much readiness as I have long since abandoned the hypothesis which I once took the liberty of submitting to the R. S., on the functions of the crystalline lens.

Explanation of the Figures in Plates 9 *and* 10.—Figs. 1—6; The section of a stream of air from a tube .07 inch in diameter, as ascertained by measuring the breadth of the impression on the surface of a liquid. The pressure impelling the current, was in fig. 1, 1 inch. Fig. 2, 2. Fig. 3, 3. Fig. 4, 4. Fig. 5, 7. Fig. 6, 10.

Figs. 7—12; A similar section, where the tube was .1 in diameter, compared with the section as inferred from the experiments with 2 gages, which is represented by a dotted line. From this comparison it appears, that where the velocity of the current was small, its central parts only displaced the liquid; and that where it was great, it displaced, on meeting with resistance, a surface somewhat greater than its own section. The pressure was in fig. 7, 1. Fig. 8, 2. Fig. 9, 3. Fig. 10, 4. Fig. 11, 7. Fig. 12, 10.

Figs. 13—20; A, the half section of a stream of air from a tube .1 in diameter, as inferred from experiments with 2 water gages. The pressure was in fig. 13, .1. Fig. 14, .2. Fig. 15, .5. Fig. 16, 1. Fig. 17, 3. Fig. 18, 5. Fig. 19, 7. Fig. 20, 10. The fine lines, marked B, show the result of the observations with an aperture .15 in diameter opposed to the stream; C with .3; and D with .5.

Figs. 21—23; A, the half section of a current from a tube .3 in diameter, with a pressure of .5, of 1, and of 3. B shows the course of a portion next the axis of the current, equal in diameter to those represented by the last figures.

Fig. 24; The appearance of a stream of smoke forced very gently from a fine tube. Fig. 25 and 26, the same appearance when the pressure is gradually increased. Fig. 27; see section 3. Fig. 28, the perpendicular lines over each division of the horizontal line show, by their length and distance from that line, the extent of pressure capable of producing, from the respective pipes, the harmonic notes indicated by the figures placed opposite the beginning of each, according to the scale of 22 inches parallel to them. The larger numbers, opposite the middle of each of these lines, show the number of vibrations of the corresponding sound in a second.

Figs. 29—33; see section 10. Fig. 34, the combination of 2 equal sounds constituting the interval of an octave, supposing the progress and regress of the particles of air equable. Figs. 35, 36, 37, a similar representation of a major 3d, major tone, and minor 6th. Fig. 38, a 4th, tempered about 2 commas. Fig. 39, a vibration of a similar nature, combined with subordinate vibrations of the same kind in the ratios of 3, 5, and 7. Fig. 40, a vibration represented by a curve of which the ordinates are the sines of circular arcs increasing uniformly, corresponding with the motion of a cycloidal pendulum, combined with similar subordinate vibrations in the ratios of 3, 5, and 7.

Figs. 41 and 42; Two different positions of a major 3d, composed of similar vibrations, as represented by figures of sines. Fig. 43; a contracted representation of a series of vibrations. A, a simple uniform sound. B, the beating of 2 equal sounds nearly in unison, as derived from rectilinear figures. C, the beats of 2 equal sounds, derived from figures of sines. D, a musical consonance, making by its frequent beats a fundamental harmonic. E, the imperfect beats of 2 unequal sounds. Fig. 44, various forms of the orbit of a musical chord, when inflected, and when struck. Fig. 45, forms of the orbit, when the sound is produced by means of a bow. Fig. 46, epitrochoidal curves, formed by combining a simple rotation or vibration with other subordinate rotations or vibrations. Figs. 47 and 48, the successive forms of a tended chord, when inflected and let go, according to the construction of La Grange and Euler. Fig. 49, the appearance of a vibrating chord which had been inflected in the middle, the strongest lines representing the most luminous parts Fig. 50, the appearance of a vibrating chord, when inflected at any other point than the middle. Fig. 51, the appearance of a chord, when put in motion by a bow applied nearly at $\frac{1}{3}$ of the length from its end. Fig. 52, the method of tuning recommended for common use.

Fig. 53; A comparative view of different systems of temperament. The whole circumference represents an octave. The inner circle L is divided into 30103 parts, corresponding with the logarithmical parts of an octave. The next circle R shows the magnitude of the simplest musical and other ratios. Q is divided into 12 equal parts, representing the semitones of the equal temperament described by Zarlino, differing but little from the system of Aristoxenus, and warmly recommended by Marpurg and other late writers. Y exhibits the system proposed in this paper as the most desirable; and P the practical method nearly approaching to it, which corresponds with the 11th method in Marpurg's enumeration, except that, by beginning with C instead of B, the practical effect of the temperament is precisely inverted. K is the system of Kirnberger and Sulzer; which is derived from 1 perfect 3d, 10 perfect and 2 equally imperfect 5ths. M is the system of mean tones, the sistema participato of the old Italian writers, still frequently used in tuning organs, approved also by Dr. Smith for common use. S shows the result of all the calculations in Dr. Smith's harmonics, the system proposed for his changeable harpsichord, but neither in that nor any other form capable of practical application.

VIII. *On the Effects which take place from the Destruction of the Membrana Tympani of the Ear. By Mr. Astley Cooper.* p. 151.

Anatomists have endeavoured to ascertain, by experiments on quadrupeds, the loss of power which the organ of hearing would sustain by perforating the membrana tympani: dogs have been made the subject of these trials; but the results have not been clear or satisfactory. Mr. Cheselden had conceived the design of making the human organ itself the subject of direct experiment; and a condemned criminal was pardoned, on condition of his submitting to it; but, a popular outcry being raised, the idea was relinquished. Though denied the aid of experiment, the changes produced by disease frequently furnish a clue equally satisfactory.

It often happens, that some parts of an organ are destroyed by disease, while others are left in their natural state; and hence, by the powers retained by such organ, after a partial destruction, we are enabled to judge of the functions performed by those parts when the whole was in health. Guided by this principle, I have made the human ear the subject of observation, and have endeavoured to ascertain the degree of loss it sustains in its powers by the want of the membrana tympani; a membrane which has been generally considered, from its situation in the meatus, and its connection with the adjacent parts by a beautiful and delicate structure, as essentially necessary to the sense of hearing; but which, as appears by the following observations, may be lost, with little prejudice to the functions of the organ.

Mr. P——, a medical student aged 20, applied to me, in the winter of 1797, while he was attending a course of anatomical lectures, requesting my opinion on the nature of a complaint in his ear, which had long rendered him slightly deaf. On inquiring into the nature of the symptoms which had preceded, and of those which now accompanied the disease, he informed me, that he had been subject from his infancy to pains in the head, and was attacked, at the age of 10, with an inflammation and suppuration in the left ear, which continued discharging matter for several weeks: about 12 months after the first attack, symptoms of a similar kind took place in the right ear, from which also matter issued for a considerable time. The discharge in each instance was thin, and extremely offensive to the smell; and, in the matter, bones or pieces of bones were observable. The immediate consequence of these attacks was a total deafness, which continued for 3 months; the hearing then began to return, and, in about 10 months from the last attack, was restored to the state in which it at present remains.

Having thus described the disease and its symptoms, he gave me the following satisfactory proof of each membrana tympani being imperfect. Having filled his mouth with air, he closed the nostrils, and contracted his cheeks: the air, thus compressed, was heard to rush through the meatus auditorius, with a whistling noise, and the hair hanging from the temples became agitated by the current of air which issued from the ear. To determine this with greater precision, I called for a lighted candle, which was applied in turn to each ear, and the flame was agitated in a similar manner. Struck with the novelty of these phenomena, I wished to have many witnesses of them, and therefore requested him, at the conclusion of the lecture on the organ of hearing, to exhibit them to his fellow students; with which request he was so obliging as to comply. It was evident from these experiments, that the membrana tympani of each ear was incomplete, and that the air issued from the mouth, by the Eustachian tube, through an opening in that membrane, and escaped by the external meatus.

To determine the degree in which the membrana tympani had been injured, I passed a probe into each ear, and found that the membrane on the left side was entirely destroyed; since the probe struck against the petrous portion of the temporal bone, at the interior part of the tympanum, not by passing through a small opening; for, after an attentive examination, the space usually occupied by the membrana tympani was found to be an aperture, without one trace of membrane remaining. On the right side also, a probe could be passed into the cavity of the tympanum; but here, by conducting it along the sides of the meatus, some remains of the circumference of the membrane could be discovered, with a circular opening in its centre, about $\frac{1}{4}$ of an inch in diameter. From such a destruction of this membrane, partial indeed in one ear, but complete in the other, it might be expected that a total annihilation of the powers of the organ would have followed: but the deafness was inconsiderable. This gentleman, if his attention were exerted, was capable, when in company, of hearing whatever was said in the usual tone of conversation; and it is worthy of remark, that he could hear with the left ear better than with the right, though in the left no traces of the membrana tympani could be perceived. When attending the anatomical lectures also, he could hear, even at the most distant part of the theatre, every word that was delivered; though, to avoid the regular and constant exertion which it required, he preferred placing himself near the lecturer. I found however, that when a note was struck on the pianoforte, he could hear it only at $\frac{2}{3}$ds of the distance at which I could hear it myself;

and he informed me, that in a voyage to the East Indies, while others, when ships were hailed at sea, could catch words with accuracy, his organ of hearing received only an indistinct impression. But the most extraordinary circumstance in this case is, that the ear was nicely susceptible of musical tones; for he played well on the flute, and had frequently borne a part in a concert. I speak this from the authority of his father, who is a judge of music, and plays well on the violin: he told me, that his son, besides playing on the flute, sung with taste, perfectly in tune.

The slight degree of deafness of which Mr. P—— complained, was always greatly increased by his catching cold: an effect which seems to have arisen from the meatus being closed by an accumulation of the natural secretion of the ear; for it frequently happened to him, after he had been some time deaf from cold, that a large piece of hardened wax, during a fit of coughing, was forced from the ear, by the air rushing from the mouth through the Eustachian tube, and his hearing was instantly restored. From bathing too, he suffered inconvenience, unless his ears were guarded against the water, by cotton being previously forced into the meatus. When this precaution was neglected, the water, as he plunged in, by rushing into the interior parts of the ears, occasioned violent pain, and brought on a deafness, which continued till the cause was removed, that is, till the water was discharged: but he had acquired the habit of removing it, by forcing air from the mouth through the ear.

In a healthy ear, when the meatus auditorius is stopped by the finger, or is otherwise closed, a noise similar to that of a distant roaring of the sea is produced: this arises from the air in the meatus being compressed on the membrana tympani. In the case here described, no such sensation was produced: for, in Mr. P.'s ear, the air, meeting with no impediment, could suffer no compression; since it found a passage, through the open membrane, to the mouth, by means of the Eustachian tube. Mr. P—— was liable to the sensation commonly called the teeth being on edge, in the same degree as it exists in others; and it was produced by similar acute sounds, as by the filing of a saw, the rubbing of silk, &c. Its occurring in him seems to disprove the idea which has been entertained of its cause; for it has been thought, that the close connection of the nerve called the chorda tympani with the membrana tympani, exposed it to be affected by the motions of the malleus; and that, as it passes to nerves connected with the teeth, they would suffer from the vibratory state of the nerve, produced by the agitations of the membrane. But in this case, as the membrane was destroyed on that side on which the sensation was produced, some other explanation must be resorted to; and I see no reason why this effect should not be referred to that part of the auditory nerve which lines the labyrinth of the ear, which, being impressed by acute and disagreeable sounds, would convey the impression to the portio dura of the same nerve, and to the teeth with which that nerve is connected. The external ear, though 2 distinct muscles are inserted into it, is capable, in its natural state, of little motion; however, when an organ becomes imperfect, every agent which can be employed to increase its powers is called into action; and, in the case here described, the external ear had acquired a distinct motion upward and backward, which was observable whenever Mr. P—— listened to any thing which he did not distinctly hear. This power over the muscles was so great, that when desired to raise the ear, or to draw it backwards, he was capable of moving it in either direction.

This case is not the only one of this description which has come under my observation; for another gentleman, Mr. A—, applied to me under a similar complaint, but in one ear only, proceeding from suppuration, and producing the same effects. This gentleman has the same power of forcing air through the imperfect ear; suffers equally from bathing, if the meatus auditorius be unprotected; and feels, even from exposure to a stream of cold air, very considerable pain. The only difference I could observe was, that in Mr. A.'s case, the defect of hearing in the diseased organ was somewhat greater than in the former; for though, when his sound ear was closed, he could hear what was said in a common tone of voice, yet he could not distinguish the notes of a piano-forte at the same distance: a difference which might have in part arisen from the confused noise which is always produced by closing the sound ear; or because, as he heard well on one side, the imperfect ear had remained unemployed, and consequently had been enfeebled by disuse.

From these observations it seems to follow, that the loss of the membrana tympani in both ears, far from producing total deafness, occasions only a slight diminution of the powers of hearing. Anatomists who have destroyed this membrane in dogs, have asserted, that at first the effect on the sense of hearing was trivial; but that after a few months a total deafness ensued. Haller also has said, that if the membrane of the tympanum be broken, the person becomes at first hard of hearing, and afterwards perfectly deaf. But in these instances the destruction must have extended further than the membrana tympani; and the labyrinth must have suffered from the removal of the stapes, and from the consequent discharge of water contained in the cavities of the internal ear; for it has been very constantly observed, that when all the small bones of the ear have been discharged, a total deafness has ensued.

It is probable, that in instances in which the membrana tympani is destroyed, the functions of this membrane have been carried on by the membranes of the fenestra ovalis and fenestra rotunda: for as they are placed over the water of the labyrinth, they will, when agitated by the impressions of sound, convey their vibrations to that fluid in a similar manner, though in somewhat an inferior degree, to those which are conveyed by means of the membrana tympani and the small bones which are attached to it; and thus, in the organ of hearing, each part is admirably adapted, not only to the purpose for which it is designed, but also as a provision against accident or disease; so that, whenever any particular part is destroyed, another is substituted for it, and the organ, from this deprivation, suffers but little injury in its functions. It seems that the principal use of the membrana tympani is, to modify the impressions of sound, and to proportion them to the powers and expectation of the organ. Mr. P— had lost this power for a considerable period after the destruction of the membrane; but in process of time, as the external ear acquired the additional motions I have described, sounds were rendered stronger or weaker by them. When therefore he was addressed in a whisper, the ear was seen immediately to move; but when the tone of voice was louder, it then remained altogether motionless.

Some additional Remarks to the foregoing, on the Mode of Hearing in Cases where the Membrana Tympani has been destroyed. By Evd. Home, Esq. p. 159.

After having communicated to the R. S. the curious facts contained in Mr. Cooper's paper, which prove that the organ of hearing is capable of receiving all the different impressions of sound, when the membrana tympani has been destroyed, it may not be improper to explain, from the observations contained in a former paper on this subject, in what manner this may take place. It is there stated, that any vibrations communicated directly to the bones of the skull, are as accurately impressed on the organ, as through the medium of the membrana tympani. The office of that membrane is therefore to afford an extended surface, capable of receiving impressions from the external air, and of communicating them to the small bones of the ear; which a membrane would be incapable of doing, unless it had a power of varying its tension, to adapt it to different vibrations.

In the above cases, in which this membrane, the malleus, and the incus, had been destroyed, it would appear that the stapes was acted on by the air received into the cavity of the tympanum, and communicated the impressions immediately to the internal organ. This not happening for some months after the membrane was destroyed, probably arose from the inflammation of the tympanum confining the stapes, and rendering its vibrations imperfect. That sounds can be communicated with accuracy by the bones of the skull, to the internal organ, when received from solid or liquid substances, has long been well understood. That the membrana tympani is incapable of perfectly answering this purpose, when sounds are propagated through air, has been a generally received opinion; to refute which, was the object of my former paper. That in cases in which the membrana tympani has been destroyed, the air is capable of acting with sufficient force on the stapes to communicate vibrations to it, and to produce on the internal organ the necessary effect for perfect hearing, is completely ascertained by Mr. Cooper's observations.

IX. *Experiments and Observations on the Light which is Spontaneously emitted, with some Degree of Permanency, from various Bodies. By Nath. Hulme,* M. D., F. R. S. and A. S.* p. 161.

The discoveries which have been made with respect to light, as it proceeds im-

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* Dr. Hulme was a native of Yorkshire, where he was born, in 1732; and he died in the Charter-

mediately from the sun, are many and important; but the observations on that species of light which is spontaneously emitted from various bodies, are not only few in number, but in general very imperfect. The author is therefore desirous of drawing the future attention of the philosopher more particularly to this subject, and of communicating his own experiments and observations on it, to the R. S. By the spontaneous emission of this light, the author wishes to distinguish it from all kinds of artificial phosphorus; which, as he apprehends, differ essentially, in some of their properties, from that light of which he means to treat. And, by its adhesion to bodies with some degree of permanency, he distinguishes it from that transient sort of light which is observable in electricity, in meteors, and in other lucid emanations. The light which is the subject of this paper, he therefore discriminates by the name of spontaneous light.

The substances from which such light is emitted, are principally the following. Marine animals, both in a living state, and when deprived of life. As instances of the first may be mentioned, the shell-fish called pholas, the medusa phosphorea, and various other mollusca. When deprived of life, marine fishes in general seem to abound with this kind of light. The hon. Mr. Boyle commonly obtained light, for his use, from the whiting, as appears from many parts of his works: Dr. H. procured his fish light chiefly from the herring and the mackerel. The flesh of quadrupeds has also been observed to emit light. Instances of this are mentioned by Fabricius ab Aquapendente; by T. Bartholin; by Mr. Boyle; and by Dr. Beale; for which, see T. Bartholin, de luce animalium, p. 183; Boyle's works, vol. 3, p. 304; Phil. Trans. vol. 11, p. 599. In the class of insects are many which emit light very copiously, particularly several species of fulgora or lantern-fly, and of lampyris or glow-worm; also the scolopendra electrica; and a species of crab, called cancer fulgens. Rotten wood is well known to emit light spontaneously. Peat earth also has the same property. Of the effects of the latter, a remarkable instance is related in Plot's Nat. Hist. of Staffordshire, p. 115.

The place where the following experiments were made, was a dark wine-vault, which, for distinction's sake, the author calls the laboratory. The heat of this laboratory varied, throughout the year, from about 40 degrees of temperature to 64°. The thermometer made use of was that of Fahrenheit. The weight is always to be supposed that called Troy weight. The liquid measure employed, was that used for wine in this country: the ounce containing 8 dr. avoirdupois; and the pint,

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house, London, in 1807. It would seem that he studied physic at Edinburgh, as we find published there an inaugural dissertation, "De Scorbuto," 1765, afterwards reprinted in 1768, both in Latin and English. He published also a treatise on the Puerperal Fever, Lond. 1772. Dr. H. practised medicine during a long course of years in London, with considerable reputation, and was elected physician to the Charter-house, in 1774, which appointment he held to the time of his death. He lost his life in consequence of the chimney of his house being blown down; when, getting up through the trap-door to the roof, to see what damage had been done, he fell from the steps, so as to cause a violent concussion of the brain, which in a few days terminated fatally.

16 oz. The water used in general for the experiments, was pure spring water, drawn up from under ground by means of a pump; and it was always employed cold, unless otherwise expressed.

SECTION 1. *The Quantity of Light emitted by Putrescent Animal Substances, is not in Proportion to the Degree of Putrefaction in such Substances as is commonly supposed; but, on the contrary, the greater the Putrescence, the less is the Quantity of Light emitted.*

The truth of this proposition is proved by 7 different experiments, made partly on dead herrings and partly on dead mackerel. The light which they emitted diminished after the 2d or 3d day, and was nearly extinct on the 4th or 5th day.

N. B. In experiments of this kind, for the production of light, the fishes should always be gutted, the roes taken out; and the scales, if any, carefully removed. As the roes are likewise very productive of light, they should be preserved.

Obser. 1. These experiments clearly prove, that light begins to be emitted by marine fishes, before any signs of putrefaction appear: they likewise demonstrate, that as soon as a great degree of putrescence has taken place, the luminous property of the fishes is destroyed, and the light extinguished.

Obser. 2. In the instance of light proceeding spontaneously from animal flesh, recorded by Aquapendente, the flesh emitted light before any sensible putrescence had taken place, the meat being hung up in the larder for use. In that also mentioned by Bartholin, in 1641, the flesh must have been fresh and sweet, for it was not intended to be dressed till the next day. Mr. Boyle, in his report of light issuing from flesh, expressly says, that neither he, nor any of those who were about him, could perceive in it any offensive smell, whence to infer any putrefaction; the meat being judged very fresh, and well conditioned, and fit to be dressed. And lastly Dr. Beale, in his account of a luminous neck of veal, says, that when it was dressed, on Feb. the 27th, some of the neighbours, who saw it shining, were invited to eat of it, and all esteemed it as good as they had ever tasted; that a part of it was kept for Feb. 28th and 29th, in which time it lost nothing of its sweetness.

Obser. 3. Whenever I wish to obtain a plentiful supply of light from fishes, for the purpose of experiments, I always endeavour to procure the freshest that can be had: long experience and frequent disappointments have taught me to adopt such a precaution.

SECTION 2. *The Light here treated of is a constituent Principle of some Bodies, particularly of Marine Fishes, and may be separated from them, by a peculiar Process; may be retained, and rendered permanent for some Time. It seems to be incorporated with their whole Substance, and to make a Part of it, in the same manner as any other constituent Principle.*

Exper. 1. A fresh herring was split, or divided longitudinally, by a knife, into 2 parts. Then, about 4 dr. of it, being cut across, were put into a solution, composed of 2 dr. of Epsom salt or vitriolated magnesia, and 2 oz. of cold spring water drawn up by the pump. The liquid was contained in a wide-mouthed 3 oz. phial, which was placed in the laboratory. On carefully examining the liquid, on the 2d evening after the process was begun, I could plainly perceive a lucid ring (for the phial was round) floating at the top of the liquid, the part below it being dark; but on shaking the phial, the whole at once became beautifully luminous, and continued in that state. On the 3d evening, the light had again risen to the top; but the lucid ring appeared less vivid, and on shaking the phial as before, the liquid was not so luminous as on the preceding night.—*Exper.* 2. The same experiment was repeated. On the 2d night, the liquid, being agitated, was very luminous; on the 3d, not so lucid; and on the 4th the light was extinguished.—*Exper.* 3. With sea salt or muriated natron ½ dr., and 2 oz. of water. On the 2d night, the liquid, when agitated, was dark; on the 3d, lucid; on the 4th, very luminous; on the 5th, it began to lose light; on the 6th, it continued to decrease; and on the 7th it was quite gone. Neither the liquid, nor the herring, had contracted any putrid smell. *Exper.* 4. With sea-water 2 oz. On the 2d night, dark; on the 3d, 4th, and 5th, luminous; on the 6th, nearly extinct; and on the 7th, totally. The piece of herring, when taken out and examined, was remarkably sweet.

Exper. 5. Roe of herring,* with Epsom salt 2 dr., and water 2 oz. On the 2d night, the liquid was pretty luminous; on the 3d and 4th, still luminous; and on the 5th its light was extinct—.*Exper.* 6. With Glauber's salt or vitriolated natron 2 dr., to 2 oz. of water. On the 2d night, when the phial was shaken, as usual in all these experiments, the liquid was pretty luminous; on the 3d, less so; and on the 4th the light was scarcely visible.—*Exper.* 7. With sea-water 2 oz. On the 2d night, dark; on the 3d, the liquid was moderately luminous; on the 4th and 5th, it had extracted much light; and on the 7th it was still shining. After this process, both the roe and the sea-water remained perfectly sweet.

The Flesh of Mackerel.—*Exper.* 8. With Epsom salt 2 dr., and water 2 oz. On the 2d night, the liquid was finely illuminated; on the 3d, a similar appearance; on the 4th, a diminution of light; on the 5th, it continued lucid in a small degree; and on the 6th the light was extinguished.

Roe of Mackerel.—*Exper.* 9. With Epsom salt 2 dr., and water 2 oz. On the 2d night, the liquid, when agitated, was exceedingly bright; on the 3d, the same; and on the 4th and 5th, still lucid.

The Tadpole.—*Exper.* 10. It occurred to my mind, in 1797, to try what effect a saline menstruum would have on the tadpole. Accordingly, I procured some tadpoles on the 10th of June, and put 6 of them into a solution of 2 dr. of Glauber's salt in 2 oz. of water. On the 11th, in the evening, the menstruum was dark; on the 12th, after shaking the phial, I was agreeably surprized to find it impregnated with light; on the 13th, the light was so abundant as to float on the top of the menstruum; on the 14th, the same phenomenon appeared; on the

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* The quantity used in each experiment was about ½ drams.—Orig.

15th and 16th, it was still present; on the 17th, the lucidness began to diminish; on the 18th, it was faint; and on the 19th it had vanished.—*Exper.* 11. June 11th, 6 other tadpoles were dropped into a solution of 1 dr. of common salt in 3 oz. of water. On the 12th and 13th, the menstruum was dark; on the 14th, it had extracted from the tadpoles a very beautiful bright light; on the 15th, the menstruum was exceedingly luminous; on the 16th and 17th, nearly the same: the light then gradually faded, so that on the 21st it was merely visible; and on the 22d it disappeared.—*Exper.* 12. On the 21st of June, the above 2 experiments were repeated; when the tadpoles remained in the menstruums till the 27th, but no light was emitted. What was the cause of this failure in these 2 last experiments? Was it 10 days' increased growth of the animal, which was taken from the same pond, that made the difference?—*Exper.* 13. The above experiments were repeated, when the tadpole had just put on the state of a frog, but without producing any lucid appearance.

The Light is incorporated with the whole Substance of Marine Fishes.

Exper. 14. A fine fresh herring, being gutted, was divided longitudinally into 2 parts, both of which were hung up, by pieces of string, in the laboratory. On the 2d night, they were very lucid on the skinny side, but not on the fleshy or inward part; on the 3d, the fleshy or central parts of the fish were thickly covered with a rich azure light; on the 4th, they continued exceedingly luminous; and on the 5th and 6th they were still lucid. It is surprizing to think what a profusion of light was emitted from the interior substance of this single fish.—*Exper.* 15. A similar experiment was made with a mackerel, and with similar effects. These 2 experiments were frequently repeated.—*Exper.* 16. But the soft-roe, of both the herring and the mackerel, abounds more with light than even the flesh. When it is in its most luminous state, which generally happens about the 3d or 4th night, it will sometimes shine so very splendidly, as to appear like a complete body of light. It is remarkable that the hard-roe in general does not emit so much light as the soft-roe. When the roes were used, they were laid on plates, and deposited in the laboratory.

Obser. 1. The above experiments clearly prove, as I apprehend, that this light is a constituent principle of marine fishes: and that it is separated, by the menstruum employed on this occasion, in the same way that the principles of any other body are separated, by the menstruum fitted to decompose it. They likewise show, that it is not partially but wholly incorporated with every part of their substance, and makes a part of it, in the same manner as any other constituent principle.

Obser. 2. Light is probably the first constituent principle that escapes, after the death of marine fishes. The experiments of the first §. teach us that it appears soon after death, even in fishes which, to the eye, seem quite fresh and sweet; or at least long before any sensible putrescence takes place. And we have seen that the flesh and roes, infused in the saline menstruums, continued to emit light for several days, without undergoing any apparent putrefactive change.

Obser. 3. The experiments likewise render it probable, that no offensive putrefaction takes place in the sea, after the death of such myriads of animals as must needs daily perish in the vast ocean, quite contrary to what happens on land; and that the flesh of marine fishes remains pretty sweet for some time, and may become wholesome food for many kinds of those which still remain alive. An eminent instance this, of the wisdom of the Creator, in the construction of the aqueous part of the world, which comprehends, by far, the greatest portion of the terraqueous globe, and is the most replete with animal life!

SECTION 3. *Some Bodies or Substances have a Power of extinguishing spontaneous Light, when it is applied to them.*

Expers. The luminous matter proceeding from the herring and the mackerel, was quickly extinguished when mixed with the following substances: 1. Water alone. 2. Water impregnated with quick-lime. 3. Water impregnated with carbonic acid gas. 4. Water impregnated with hepatic gas. 5. Fermented liquors. 6. Ardent spirits. 7. Mineral acids, both in a concentrated and diluted state. 8. Vegetable acids. 9. Fixed and volatile alkalis, when dissolved in water. 10. Neutral salts: viz. saturated solutions of Epsom salt, of common salt, and of sal ammonia. 11. Infusions of chamomile flowers, of long pepper, and of camphor, made with boiling-hot water, but not used till quite cool. 12. Pure honey, if used alone.

SECTION 4. *Other Bodies or Substances have a Power of preserving spontaneous Light for some Time, when it is applied to them.*

Exper. 1. Some luminous matter scraped from the herring, was mixed with a solution of 2 dr. of Epsom salt in 2 oz. of cold pump water: after shaking very well for some time the phial which contained them, the whole liquid became richly impregnated with light, and continued shining above 24 hours. This experiment was frequently repeated, and with the same effect.—*Exper.* 2. Two dr. of Glauber's salt and 2 oz. of water being mixed with herring light, the solution was quickly made very lucent, and remained so till the succeeding evening.—*Exper.* 3. Mackerel-light, being mixed with 2 dr. of Rochelle salt or tartarized natron, and 2 oz. of water, caused the fluid to be very luminous.—*Exper.* 4. Two dr. of soda phosphorata and 2 oz. of water, mixed with herring-light, formed a very lucent fluid, which retained the light for a long time.—*Exper.* 5. Herring-light, with 1 dr. of saltpetre or nitrated kali, and 2 oz. of water, made the solution pretty luminous.—*Exper.* 6. Half a dr. of common salt dissolved in 2 oz. of water, with the addition of mackerel-light, composed a very shining mixture, which retained its splendour for the space of a day or 2. The same effect was produced by herring light.—*Exper.* 7. Two oz. of sea-water, being agitated with the light of a mackerel, soon obtained a brilliant illumination. The sea-water preserved its luminousness for several days. The experiment was successfully repeated.—*Exper.*

8. Two dr. of pure honey, that had not been clarified, or exposed to heat, were dissolved in 2 oz. of water; and, after the admission of some mackerel-light, and shaking the phial, the solution was fully impregnated with light, which was visible the next evening.—*Exper.* 9. Two dr. of purified or refined sugar being dissolved in 2 oz. of water, and mixed with the shining matter of a herring, the fluid acquired a great degree of lucidness. The same effect took place when the experiment was made with soft brown sugar.

Obser. These experiments enable us to take light, and diffuse it through water, so as to render the whole liquid most brilliantly luminous, or in other words, to impregnate water with light. By these means, the light is so extended in its surface, and combined in such a manner, as to become exceedingly convenient and useful for various other experiments.

SECTION 5. *When spontaneous Light is extinguished by some Bodies or Substances, it is not lost, but may be again revived in its former Splendour, and that by the most simple Means.*

Exper. 1. June 1, 1795, the following experiments were made, to know what was the best proportion of Epsom salt to water, in order to produce the most luminous liquid. Some shining matter was taken from a mackerel, and mixed with a solution of 7 dr. of the salt in 1 oz. of water; and its light was immediately extinguished. The same effect ensued, but in a less degree, with a solution of 6, and one of 5 dr. In a solution of 2 dr., in the same quantity of water, the liquid was luminous; but much more so when only 1 dr. of salt was used. Observing the extinction of light to take place, as above, in the more saturated solutions, while the diluted solutions were luminous, it occurred to endeavour to discover what became of the extinguished light, in the former case, and whether it might not be revived by dilution. For this purpose, I took the solution of 7 dr. of salt in 1 oz. of water, in which the lucid matter from a mackerel had been extinguished, and diluted it with 6 oz. of cold pump water; when, to my great astonishment, light in a moment burst out of darkness, and the whole liquid became beautifully luminous! This revived light remained above 48 hours, that is, as long as other light in general does, which has never been extinguished. Hence, it had lost nothing of its vivid luminous powers by its extinction.—*Exper.* 2. The last experiment was then reversed. A solution of 1 dr. of Epsom salt in 1 oz. of water, was brilliantly illuminated with mackerel light. Then, 6 dr. of the salt were put into this luminous liquid; and after shaking the phial very well for a little time, to promote the solution of the salt, the light was totally extinguished. But the same light was again recovered by the addition of 6 oz. of water. In this manner the light may be frequently extinguished, and as often revived. In one instance, the same light, by a repetition of this method, was made to undergo 10 extinctions.—*Exper.* 3. A good quantity of herring-light being mixed with a solution of 4 dr. of common salt, in 2 oz. of water, was immediately extinguished. Then 14 oz. of cold pump water were added; when the whole liquid was at once finely illuminated. The next evening it appeared still very lucid; as also on the succeeding night.—*Exper.* 4. The experiment was reversed. Half a dr. of the salt being dissolved in 2 oz. of water, had herring-light mixed, so as to be made very luminous. On the addition of 2 dr. more of the salt, the lucidness was instantly destroyed; but the light was again recovered, by pouring 8 oz. of cold water on the extinguished luminous fluid. The revived light was very vivid the next evening.—*Exper.* 5. Two oz. of sea-water were illuminated with mackerel-light, and then extinguished by adding 2 dr. of common salt. The light was again restored, by diluting the solution with 8 oz. of cold spring water.

N. B. If the illuminated liquid be uncommonly brilliant, it may sometimes require more salt to extinguish the light completely, than is here specified; in that case, the measure of water for dilution must be always calculated in exact proportion to the weight of salt employed.

SECTION 6. *Spontaneous Light is rendered more vivid by Motion.*

The truth of this proposition is proved by 2 experiments in which the liquid became more luminous on being shaken or stirred.

SECTION 7. *Spontaneous Light is not accompanied with any Degree of sensible Heat, to be discovered by a Thermometer.*

The truth of this proposition is proved by 5 different experiments made on dead herrings, dead mackerel, rotten wood, &c.

SECTION 8. *The Effects of Cold on Spontaneous Light.*

The Light of Fishes.—*Exper.* 1. Five small gallipots, containing 3 pieces of soft-roe of herring, and 2 of the herring itself, all very luminous, were placed in a frigorific mixture, composed of snow and sea-salt; in about an hour and a half the light was quite extinct, and the bodies totally frozen. The gallipots were then removed into a vessel of cold water, that their contents might be gradually thawed; which being done, they all recovered their pristine luminous state. The pieces were afterwards observed to shine during 3 succeeding nights.—*Exper.* 2. A small phial, containing 3 or 4 drams of liquid impregnated with light, was placed in a frigorific mixture. As the liquid froze, its lucidness gradually diminished; and when it quite congealed the light perfectly disappeared. The phial was then taken out, and put into cold water, at about 49° temperature, that the ice might be gradually liquified; after which, the whole fluid became as luminous as before.

The Light of shining Wood.—*Exper.* 3. A fragment of shining wood was put into a small wide-mouthed phial, which was plunged into a frigorific mixture. As the cold affected the wood, the light gradually faded, and at last was totally imperceptible. The phial was then taken out, and placed in water at about 62°; by this change of temperature, the frozen wood gradually thawed, and then regained its former lustre.

The Light of Glow-worms.—*Exper.* 4. A small phial, containing a luminous dead glow-worm, was exposed to the cold of the frigorific mixture; as the coldness penetrated the phial, the light diminished, and at length was totally extinct.

But, by placing the phial in water at about 62°, the glowing property of the insect soon returned. In this experiment, the glow-worm was evidently congealed; for it adhered to the side of the glass, and was covered with a hoar-frost. This experiment was frequently repeated, and with the same result.

Obser. By these experiments we learn, that cold extinguishes spontaneous light in a temporary manner, but not durably, as the substances of the 3d §. do; because the light revived again in its full splendour, as soon as it was exposed to a moderate temperature.

SECTION 9. *The Effects of Heat on Spontaneous Light.*

The Light of Fishes.—Exper. 1. One side of a luminous herring was held before the fire, for a short space of time, but so as to receive its heat very strongly. It was then conveyed into the laboratory; when that side which had been exposed to the fire was found quite dark, but the other continued still luminous. The fish was preserved till the next evening, but the extinguished light did not re-appear.—*Exper.* 2. A whole herring, finely shining, was thrown into a quantity of boiling-hot water, and the light was immediately extinguished: after keeping it there for some time, it was taken out, but the light did not revive.

The Light of shining Wood.—Exper. 3. A piece of shining wood; its light being very faint, was put into tepid water at about 90 degrees of temperature, and it became in a short time much more lucid. Another piece, at 96°, was rendered beautifully luminous.—*Exper.* 4. A pretty thick piece of shining wood was put into a gallipot, and sunk under water by means of a weight, together with a thermometer, at the temperature of 64°. Boiling-hot water was then added by spoonfuls; and the light, at first, was rendered much more vivid, but soon after began to decrease, and was apparently extinct at about 110°. I say apparently, because on the next evening the light had somewhat revived; which shows, that the heat of 110° was not sufficient to extinguish totally all the light inherent in this piece of wood.—*Exper.* 5. Finding that 110° of heat did not wholly extinguish the light of shining wood, a good many fragments, of different sizes, were then submitted to the power of boiling water, and detained therein for some time, in order that the heat might penetrate them thoroughly. The effect was, that the light became quickly extinct, and did not, as before, re-appear on the following evening.

The Light of Glow-worms.—Exper. 6. A dead shining glow-worm was put on 2 ounces of water, contained in a wide-mouthed phial, at the temperature of 58°. The phial was then sunk, about 2 or 3 inches deep, in boiling-hot water; and, as the heat communicated itself to the contents of the phial, the light of the glow-worm became much more vivid.—*Exper.* 7. Another lucid dead glow-worm was put into warm water, at 114°, to see if that degree of heat would extinguish the light; but, on the contrary, its glowing property was augmented. All the water was then poured off, yet the insect continued to shine for some length of time.—*Exper.* 8. The effect of that heat which is obtained from dry solid bodies by friction, was next tried on the light of the glow-worm. Two living glow-worms were put into a one-ounce phial, with a glass stopple; and though they were perfectly dark at the time, yet, if the phial was briskly rubbed with a silken or linen handkerchief, till it became pretty warm, it seldom failed to make them display their light very finely. This experiment was very frequently repeated. It had the same illuminating effect on the light of a dead glow-worm.—*Exper.* 9. The complete influence of 212 degrees of heat was now applied to the light of a glow-worm, by pouring on one when dead, but in a luminous state, some boiling water. Its light was instantly extinguished, and did not revive. The experiment was repeated, and with the same result.

Any of the saline Solutions mentioned in the 4th Section, being impregnated with luminous matter, and left some time at rest, are rendered more lucid by a moderate Degree of Heat.

This is proved by 3 different experiments.

Their Light is extinguished by a great Degree of Heat.

This is also proved by 3 separate experiments. The luminous property was destroyed by a degree of heat from 96 to 100.

If much Heat be applied to the Bottom of a Tube filled with illuminated Liquid, which has been some Time at rest, the Light will descend in luminous Streams, from the Top of the Tube to the Bottom, and be gradually extinguished.

Exper. 16. A glass cylindrical tube, closed at one end, being 9 inches long, with a bore of $1\frac{1}{10}$ inch, when used, was put into a gallipot $3\frac{1}{2}$ inches deep, and $3\frac{1}{2}$ wide, which held about 12 oz. of boiling water, and was placed in another larger vessel, to receive the overflowing water on the immersion of the tube. The tube being filled over night with some very luminous liquid, was placed in the laboratory till the next evening. The light had then ascended plentifully to the top of the fluid, the rest being dark, and, taking the circular shape of the tube, formed a very lucid ring. The vessels with the boiling-hot water were then carried into the dark laboratory; and the tube being gently and carefully placed, without shaking, in the gallipot, the light was, generally in about $\frac{1}{2}$ a minute, seen plainly to descend in streams from the top to the bottom, illuminating the whole fluid in its descent in a beautiful manner, and then was gradually extinguished. The extinction of the light began at the top of the tube, and ended at the bottom.—*Exper.* 17. The experiment was also made with a tube 19 inches high, $\frac{1}{2}$ an inch in bore, having several curvatures, and sealed hermetically at its lower end. Both the extremities were made straight for a few inches; the one to be immersed in the water, and the other to prevent the liquid running out. The luminous ring being formed as above-mentioned, the tube was put into the gallipot of boiling-hot water; in a short time the light began to descend from the top, and came waving down, in a pleasing manner, to the bottom of the tube in the hot water, and then was by degrees extinguished. The whole length of the tube, including the curvatures, was 26 inches.

The most eligible solutions for this curious experiment, are those made with

Epsom salt, Glauber's salt, sea-salt, and sal ammonia: if either of the 2 former be used, the proper proportion is, 1 dr. of salt to each oz. of water; if either of the 2 latter, 15 gr. to each oz. of water will be sufficient.

N. B. The experimentalist, before he views the descent of the light in the tube, should always remain in the dark for some little time, in order to get rid of all extraneous light adhering to the organs of vision, and to accommodate the eye to darkness.

SECTION 10. *The Effects of the Human Body, and of the Animal Fluids, on Spontaneous Light.*

The living Body.—Exper. 1. On touching the luminous matter of fishes, the light adhered to the fingers and different parts of the hands; remained very lucid for some little time, and then gradually disappeared. But the same kind of matter being applied to pieces of wood, stone, and the like, of the same temperature as the laboratory, continued luminous on these substances for many hours.—*Exper.* 2. A piece of red blotting-paper, about 1 inch square, and 4 times doubled, was finely illuminated by matter from a herring, and applied to the upper part of the inside of the thigh. After the expiration of 15 or 20 minutes, it was taken off; when the light was quite extinguished. The experiment was repeated several times, and with the same effect. Another piece of the like paper was illuminated at the same time, and placed in the laboratory; where it retained its light above 48 hours.—*Exper.* 3. A piece of shining wood was placed on the palm of the hand, and inclosed there for some time; on inspection, it was found to be more lucid than before. Many trials of this kind were made, with the like success.—*Exper.* 4. A dead glow-worm, being but slightly luminous, was breathed on several times; and its light increased both in magnitude and brightness. The experiment was frequently repeated, with the same result.

Animal Fluids.—Blood. Exper. 5. A person having received a contusion, but otherwise in health, was bled. The next day some herring-light was mixed with about 2 oz. of the crassamentum or red coagulated part of the blood, by stirring them well together with a knife: it caused it to be slightly luminous, but the light was not of long duration. Nearly the same result followed the mixture of lucid matter with the recent crassamentum of persons labouring under inflammatory diseases, as the pleurisy and rheumatism.—*Exper.* 6. But when mixed with crassamentum that had been kept for some time, and become black and somewhat offensive to the smell, the light seemed to be more quickly extinguished.—*Exper.* 7. A singular phenomenon happened several times, on mixing fish-light with putrescent bloody serum. It would not incorporate, but was ejected in globules, like quicksilver when rubbed with any unctuous substance, and afterwards adhered to the side of the vessel in which the mixture was made, in the form of a lucid ring.—*Exper.* 8. The luminous matter of a herring was mixed with about 2 oz. of pure serum, from the healthy subject of the 5th experiment: it soon became finely illuminated, and retained its shining appearance for a long time, whenever it was stirred or agitated.—*Exper.* 9. The recent serum, drawn from patients afflicted with inflammatory complaints, was illuminated pretty much in the same manner as in the 8th experiment; and often retained light above 48 hours.

Urine.—Exper. 10. Mackerel-light being mixed, by strong agitation, with some fresh urine from a healthy person, a glimpse of light was retained at first, and then was gradually extinguished. But stale and pungent urine, being incorporated with luminous matter, had a still greater extinguishing effect.

Bile.—Exper. 11. Some bile, taken from a person who died of a suppression of urine, had herring-light mixed with it, which soon became extinct. Another trial was made with a different bile, and with the same result.

Milk.—Exper. 12. Human milk not being easily obtained, some mackerel-light was incorporated, by agitation, with 2 oz. of fresh cow's milk, which was thus rendered finely luminous, and continued shining above 24 hours. Fresh cream also retained some light; though it was not so visible as with milk, owing probably to its thickness. But, when either milk or cream turn sour, they contract a very extinguishing property. A quart of milk was kept 5 days, in a moderately cool place, in June; by that time it was changed into a mixture somewhat resembling curds and whey, that is, into a soft smooth coagulated part, and a very thin one, both which were acidulous. Some fine mackerel-light was mixed with 2 oz. of each of them, in separate phials, and they extinguished it immediately.

X. Experiments for Decomposing the Muriatic Acid. By Mr. Wm. Henry. p. 188.

One of the first objects, in the analysis of a compound body, should be its complete separation from all other substances, which, by their presence, may tend to introduce uncertainty into the results of the processes that are employed. But it is seldom that a simplicity so desirable can be attained in the objects of chemical research; for, agreeably to a known law of affinity, the last portions of any substance are separated with peculiar difficulty; the force of attraction appearing to increase, as we recede from the point of saturation. In a liquid state, the muriatic acid is a totally unfit subject for analytic experiment; for, in the strongest form under which it can be procured, it still contains a large proportion of water. This watery portion, besides the complexity which it introduces into the results of experiments, prevents any combustible substance that may be applied, from acting on the truly acid part; because that class of bodies, having less difficulty in attracting oxygen from the water than from the acid, will necessarily take it from the former source. The state of gas therefore is the only one in which the muriatic acid can become a proper object of analysis.

In the series of experiments on this gas, which I am now about to describe, I employed the electric fluid, as an agent much preferable to artificial heat. This mode of operating enables us to confine accurately the gases submitted to experiment; the phenomena that occur during the process may be distinctly observed and the comparison of the products with the original gases may be instituted with great exactness. The action of the electric fluid itself, as a decomponent, is extremely powerful; for it is capable of separating from each other, the constituent parts of water, of the nitric and sulphuric acids, of the volatile alkali, of nitrous gas, and of several other bodies, whose components are strongly united. I began therefore with examining attentively the effects of the electric fluid on the muriatic acid gas, without admixture*.

§ 1. *On the Effects of Electricity on Muriatic Acid Gas.*

When strong electrical shocks were passed through a portion of muriatic acid gas, confined in a glass tube over mercury, the following appearances took place. The bulk of the gas, after 20 or 30 shocks, was considerably diminished; and a white deposit appeared on the inner surface of the tube, which considerably obscured its transparency. In some instances, both the contraction and deposit were much more remarkable than in others. The gas which issued from muriate of soda, soon after the affusion of sulphuric acid, and while the charge was yet warm, exhibited these appearances in an eminent degree. Of this gas, 307 measures were reduced, by 20 shocks, to 227, or were contracted nearly $\frac{1}{4}$. Gas from the same materials, after they had continued working for some hours, was diminished, by similar treatment, only about a 12th. These effects therefore it seemed probable depended in some measure on the presence of moisture; and I accordingly found that muriatic acid gas, after more than a week's exposure to muriate of lime, brought into contact with it immediately after cooling from a state of fusion, was scarcely diminished at all; and that the deposit, though it still occurred, was less copious in quantity. This deposit was not, like corrosive sublimate, soluble in water; but had every property of the less saturated salt, calomel. The mercury by which the muriatic acid was confined, was therefore evidently oxidated; and to the combination of a part of the gas with the oxide thus produced, the diminution of bulk was doubtless to be ascribed. But it was uncertain from whence this oxygen was derived. It might either result from the decomposition of the acid gas, or of the water chemically combined with it. The following experiments were therefore made, to determine this point.

Exper. 1. Through 1457 measures of muriatic acid gas, 300 electrical shocks were passed. There remained, after the admission of water, 100 measures of permanent gas, or not quite 7 from each 100 of the original gas, which on trial, appeared to be purely hydrogenous.—*Exper.* 2. Of the gas, dried by muriate of lime, 176 measures received 120 shocks. The residue of hydrogenous gas amounted to 11 measures, or rather more than 6 per cent.

These experiments, and other similar ones, made on comparative portions of

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* The gases submitted to the action of electricity, in the following experiments, were confined in straight glass tubes of various diameters, armed at the sealed end with a conductor of gold, or platina, but generally of the latter metal. The shocks were as strong as could be given without breaking the tubes, which, notwithstanding every precaution, were often shattered by the force of the explosion. Each measure of gas is equal to the bulk occupied by 1 gr. of mercury.—Orig.

muriatic acid gas, in its recent state, and after exposure to muriate of lime, convinced me that it was impossible, by this method, wholly to deprive the muriatic gas of water. The recent gas however, when electrified in smaller quantity than in exper. 1, gave a larger proportion of hydrogenous gas; which shows, that some portion of its moisture was removed by exposure to muriate of lime. In order, if possible, to procure the gas perfectly dry, another mode of preparing it was resorted to. Alum and common salt were first well calcined, separately, to expel their water of crystallization, and, being then mixed, were distilled together in an earthen retort. The gas proceeding from these materials was received over dry mercury; but though only the last portion that came over was reserved for experiment, it still, after the usual electrization, afforded a product of hydrogenous gas.

In the course of the preceding experiments, I observed that the diminution of the muriatic acid gas stopped always at a certain point, beyond which it could not be carried by continuing the shocks. Gas also, which had been thus treated, when transferred to another tube, and again electrified, did not exhibit any further deposit. It became interesting therefore to know, whether the production of hydrogenous gas had a similar limitation; because the decision of this question would go far towards ascertaining its source. If the evolved hydrogenous gas arose from the decomposition of the acid, it might be expected to be produced, as long as any acid remained undecomposed. But if water were the origin of this gas, it would cease to be evolved, when the whole of the water contained in the gas had been resolved into its constituent principles.

Exper. 3 and 4. Into 2 separate tubes, I passed known quantities of muriatic acid gas. Through the one portion 200 discharges were taken; and through the other 400. On comparing the quantities of hydrogenous gas produced, it proved to bear exactly the same proportion, in each tube, to the gas originally submitted to experiment. Hence it may be inferred, that the hydrogenous gas, evolved by electrifying the muriatic acid, has its origin, not from the acid, but from the water which is intimately attached to it. The agency of the electric fluid appears also, from the following experiments, to be exerted, not only in disuniting the elements of water, but in promoting the union of the evolved oxygen with muriatic acid.

Exper. 5. A mixture of common air and muriatic acid gas, in the proportion of 143 of the former to 116 of the latter, was rapidly diminished by electrical shocks; 30 of which reduced the whole to 111*. The remainder consisted of muriatic acid and azotic gases, with a small proportion of oxygenous gas. The deposit formed on the tube was of the same kind as before, but much more abundant.

Exper. 6. The same appearances were occasioned, much more remarkably, by electrifying muriatic acid with oxygenous gas; and the contraction continued till the mercury rose so as to touch the extremity of the platina conductor. At each

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* This experiment suggests an additional reason, to that already given, for the greater diminution of the first, than of the subsequent portions of muriatic acid gas; for the former may be presumed to have been much more adulterated than the latter, with the atmospherical air of the vessels.—Orig.

explosion, a dense white cloud was seen in the tube, which soon settled on its inner surface, and was of exactly the same chemical composition as the one already described. Nitrous gas and muriatic gas, when electrified together, underwent a similar change.

In order to ascertain whether the mercury by which the gases were confined, in the above experiments, had any influence on their results, they were repeated in an instrument made, purposely for the occasion, by Mr. Cuthbertson, of London. It consisted of a glass tube, ground at each end, with the view of receiving 2 stoppers, each perforated with platina wire, which projected into the cavity of the tube. When the stoppers were in their places, the extremities of the wires were at the distance of about half an inch; and, by properly disposing the apparatus, electrical shocks might be passed through any gas or mixture of gases, with the contact only of glass and platina.

Exper. 7. In this tube I electrified the muriatic acid gas, and then admitted to it an infusion of litmus. The sudden destruction of its colour evinced the formation of oxygenated muriatic acid. Not the smallest deposit appeared on the tube.

Exper. 8 and 9. The same phenomenon took place, when an infusion of litmus was brought into contact with a mixture of common air and muriatic acid, and of oxygenous gas and muriatic acid, after electrization in this instrument; oxygenated muriatic acid being produced in both cases.

The above facts prove, that the combination of oxygen with muriatic acid, in these experiments, is not occasioned by a pre-disposing affinity in the mercury to combine with oxygenated muriatic acid; but that the electric fluid serves actually as an intermedium, in combining the muriatic acid with oxygen. From the relation of these experiments it appears, that not the smallest progress had been made by them, towards the decomposition of the muriatic acid. I resolved therefore to attempt its analysis, in a similar manner, with the aid of combustible gases.

§ 2. *Effects of electrifying the Muriatic Acid Gas with inflammable Substances.*

In a memoir read before the R. S., and inserted in their Trans. for 1797, I have shown, that when electrical shocks are passed repeatedly through a confined portion of carbonated hydrogenous gas, the water held in solution by the gas is decomposed by the carbon, which forms a constituent part of it; that carbonic acid is formed; and an addition made of hydrogenous gas. Hence the bulk of the carbonated hydrogen gas is considerably enlarged by this process; which shows, by its results, that the affinity of carbon for oxygen is rendered much more powerful and efficient by the electric fluid. I have since found that other oxygenated substances are decomposed, by electrifying them with carbonated hydrogen gas. Nitrous gas, for example, is speedily destroyed by this process, and carbonic acid and azotic gases are obtained.

Every attempt to decompose the muriatic acid must be founded on the presumption that it is an oxygenated substance; as those bodies promise to be the most successful agents that possess a strong affinity for oxygen. Now, of all known bodies, charcoal most strongly attracts oxygen; I have therefore repeatedly attempted the destruction of this acid, by passing it over red-hot charcoal. But in a series of experiments, which I made some time since, with this view, in conjunction with Mr. Rupp, we soon found reason to be dissatisfied with the difficulty and uncertainty of this process. An immense production of hydrogenous gas took place; but it was not easy to determine whether it had its origin from real acid, or from water. Our experiments however, though insufficient to furnish decisive proof, induced us to believe that it had the latter origin.

It next occurred to me, that the comparative affinities of the muriatic radical, whatever it may be, and of charcoal, for oxygen, would be elegantly and satisfactorily ascertained, by electrifying together the carbonated hydrogen and muriatic gases. If the muriatic acid be capable of decomposition by carbon, it might be expected to be destroyed by this process; and the exact quantity of acid decomposed, and the nature and quantity of the products would thus be easily determined. I electrified therefore, the muriatic acid and carbonated hydrogen gases, with the most scrupulous attention to the phenomena and results. That the electric fluid might not be misapplied, in decomposing the water of the carbonated hydrogen gas, it was kept more than a week, before use, over quick-lime, introduced to it while yet hot.

Exper. 10. Of this carbonated hydrogenous gas, 186 measures were expanded, by 130 shocks, to 211; that is, the gas was increased about $\frac{1}{8}$ its bulk.

Exper. 11. Of the same gas, 84 measures were mixed with 116 of muriatic acid gas, dried by muriate of lime. By 120 shocks, the mixture was a little dilated. After the admission of a drop or two of water, there remained 91 measures; i. e. the addition of permanent gas was 7 measures, or about as much as might have been expected from the muriatic gas alone.

Exper. 12. Eighty-three measures of dry carbonated hydrogenous gas, with 89 of muriatic acid gas, received 200 shocks. The permanent residue, after the admission of water, was 101 measures: the addition therefore amounted to 18. Of the added 18, 6 may be accounted for by the decomposition of the water of the muriatic gas, and 10 by that of the carbonated hydrogenous gas. There remain therefore only 2 measures that can be supposed to be produced from the muriatic acid gas; a quantity too small to afford grounds for supposing them to arise from decomposed acid.

Exper. 13. Dry carbonated hydrogenous gas .... 132 measures,
—— mixed with dry muriatic gas 108
making 240
by 200 shocks, expanded to .... 268.

Part of this gas was then transferred to another tube, and the proportion of permanent gas ascertained. Through the remainder, 150 additional shocks were passed, before the amount of the gas thus evolved was determined. In both, it bore exactly the same proportion to the original gas; which shows, that by continuing the electrization, no further effects were produced.

A great variety of similar experiments convinced me, that by electrifying together the carbonated hydrogenous and muriatic gases, not the smallest progress was made towards the decomposition of the latter. All that was thus effected, consisted in the decomposition of the water of the 2 gases, by the carbon of the combustible gas; and when this was completely accomplished, no further effect ensued from continuing the electrization. The generation of carbonic acid was proved by the following experiment.

Exper. 14. To a mixture of carbonated hydrogen and muriatic gases, after having received above 100 shocks, a drop of water was admitted, which absorbed the muriatic acid. The liquid was then taken up by blotting-paper; and the residuary gas, being transferred into another tube, was brought into contact with a solution of pure barytic earth. The precipitation of this solution evinced the presence of carbonic acid.

It was desirable however that the effects should be ascertained, of electrifying together pure muriatic acid and pure carbonated hydrogenous gas, both perfectly free from water. Now, from the experiments related in § 1, it appears highly probable, that a complete purification from moisture is produced, in both gases, by the action of the electric fluid; all the water they before contained being thus decomposed. In the following experiments therefore the 2 gases were separately electrified, before they were submitted to this process conjointly.

Exper. 15. To a portion of muriatic acid, diminished by the action of electricity from 144 to 121 measures, 27 measures of carbonated hydrogenous gas, expanded as far as possible, were added, and 200 shocks passed through the mixture. The addition of permanent gas amounted to 14 measures; 10 of which may be traced to the muriatic acid, and were evolved by its separate electrization. The remaining 4 measures, which remain to be accounted for, are too small a quantity to be ascribed to the decomposition of the acid.

Exper. 16. To a quantity of carbonated hydrogenous gas, which had received 400 shocks, and occupied the space of 212 measures, were added 232 of muriatic acid, through which 200 shocks had been previously passed. The electrization of the mixture was next continued, till 800 discharges had taken place. On examining the mixture of gases, during this operation, no change whatever took place; and after its close no more muriatic acid had disappeared, than would have been deficient after the first electrization; nor was there any further production of permanent gas.

Exper. 17. The same result was obtained, by electrifying together 280 measures of carbonated hydrogenous gas, previously expanded by 600 shocks, and 114 of muriatic acid, after 400 shocks. The additional discharge through this mixture, of 1000 shocks, did not evince the smallest progress towards the decomposition of the muriatic acid.

Exper. 18. In the naturally moist state of these gases it follows, from the 14th experiment, that carbonic acid is produced by electrifying them in conjunction. It appeared of some importance to ascertain whether, after a previous decomposition of their moisture, carbonic acid would continue to be generated. But the electrified carbonated hydrogenous gas itself contains carbonic acid, which, unless removed, would render the result of the experiment undecisive. This was accomplished by passing up, to a portion of electrified gas, a bubble or two of dry ammoniacal gas, which, uniting with the carbonic acid, would condense any portion of it that might be present. The remainder was transferred into another tube; and to this carbonated hydrogenous gas, perfectly deprived both of moisture and carbonic acid, muriatic acid gas, previously electrified, was added, and electrical shocks were passed through the mixture. A drop of water was then admitted; and the residuary gas, after having been dried, was transferred into another tube. On passing up barytic water, not the smallest trace of carbonic acid could be discovered.

From the preceding experiments, the following conclusions may be deduced.

1. The muriatic acid gas, in the driest state in which it can be procured, still contains a portion of water. From a calculation founded on the experiments described in § 1, the grounds of which are too obvious to require being stated, it follows, that 100 cubical inches of muriatic gas, after exposure to muriate of lime still hold in combination 1.4 grain of water. 2. When electrical shocks are passed through this gas, the watery portion is decomposed. The hydrogen of the water, uniting with the electric matter, constitutes hydrogenous gas, and the oxygen unites with the muriatic acid; which last, acting on the mercury, composes muriate of mercury. 3. The electric fluid serves as an intermedium, in combining oxygen with muriatic acid. 4. The really acid portion of muriatic gas does not sustain any decomposition by the action of electricity. 5. When electric shocks are passed through a mixture of carbonated hydrogen and muriatic acid gases, the water held in solution by these gases is decomposed by the carbon of the compound inflammable gas; and carbonic acid and hydrogenous gases are the result. 6. When all the water of the two gases has been decomposed, no effect ensues from continuing the electrization; or, if the water of each gas has been previously destroyed, by electrifying them separately, no further effect ensues from electrifying them conjointly. 7. Since therefore carbon, though placed under the most favourable circumstances for abstracting from the muriatic acid, and combining with its oxygen, evinces no such tendency, it may be inferred, that if the muriatic acid be an oxygenated substance, its radical has a stronger affinity for oxygen than charcoal possesses.

Though the first impressions excited in my mind by the total failure of the above experiments, in accomplishing one of the greatest objects of modern chemistry, have induced me for some time to withhold them from the society, I am satisfied by reflection that this communication is not without expediency. The means employed in attempting the analysis of the muriatic acid, were such as, after mature deliberation, appeared to me most to promise success; and the experiments were attended with a degree of labour, which can only be estimated by those who have been engaged in similar pursuits; not one-third of those which were really made having been described, in the foregoing account of them. It may spare therefore to others a fruitless application of time and trouble, to be made acquainted with what I have done; and the collateral facts, which have presented themselves in the inquiry, are perhaps not without curiosity or value.

From the result of these experiments, I apprehend all hope must be relinquished, of effecting the decomposition of the muriatic acid, in the way of single elective affinity. They furnish also a strong probability, that the basis of the muriatic acid is some unknown body; for, no combustible substance with which we are acquainted, can retain oxygen, when submitted, in contact with charcoal, to the action of electricity, or of a high temperature. The analysis of this acid must, in future, be attempted with the aid of complicated affinities. Thus, in the masterly experiment of Mr. Tennant, phosphorus, which attracts oxygen less strongly than charcoal, by the intermediation of lime decomposes the carbonic acid. Yet, led by the analogy of this fact, its discoverer found that a similar artifice did not succeed in decomposing the muriatic acid. "As vital air," he observes, "is attracted by a compound of phosphorus and calcareous earth, more powerfully than by charcoal, I was desirous of trying their efficacy on those acids which may from analogy be supposed to contain vital air, but which are not affected by the application of charcoal. With this intention, I made phosphorus pass through a compound of marine acid and calcareous earth, and also of fluor acid and calcareous earth, but without producing in either of them any alteration. Since the strong attraction which these acids have for calcareous earth tends to prevent their decomposition, it might be thought, that in this manner they were not more disposed to part with vital air than by the attraction of charcoal: but this however does not appear to be the fact. I have found, that phosphorus cannot be obtained by passing marine acid through a compound of bones and charcoal when red-hot. The attraction therefore of phosphorus and lime for vital air, exceeds the attraction of charcoal, by a greater force than that arising from the attraction of marine acid for lime."*

By means similar to those employed in attempting the analysis of the muriatic acid, I tried to effect that of the fluoric acid. When electrified alone, in a glass tube coated internally with wax, it sustained a diminution of bulk, and there remained a portion of hydrogenous gas. But neither in this mode, nor by submitting it, mixed with carbonated hydrogenous gas, to the action of electricity, was any progress made towards its analysis. These experiments however render it probable, that the fluoric acid, like the muriatic, is susceptible of still further oxygenation, in which state it becomes capable of acting on mercury. The carbonic acid, on the contrary, appears not to admit of different degrees of oxygenation. When the electric shock has been repeatedly passed through a portion of this

* Phil. Trans., vol. 81, p. 184.—Orig.

acid gas, its bulk is enlarged, and a permanent gas is produced, which is evidently a mixture of oxygenous and hydrogenous gases; for when an electrical spark is passed through the gas that remains after the absorption of the carbonic acid by caustic alkali, it immediately explodes. These results even take place on electrifying carbonic acid from marble, previously calcined in a low red-heat, to expel its water, and then distilled in an earthen retort.*

XI. On a New Fulminating Mercury. By Edw. Howard, Esq. F. R. S. p. 204.

§ 1. The mercurial preparations which fulminate, when mixed with sulphur, and gradually exposed to a gentle heat, are well known to chemists: they were discovered, and have been fully described, by Mr. Bayen.† MM. Brugnatelli and Van Mons have also produced fulminations by concussion, as well with nitrate of mercury and phosphorus, as with phosphorus and most other nitrates.‡ Cinnabar also is among the substances which, according to MM. Fourcroy and Vauquelin, detonate by concussion with oxymuriate of pot-ash.§ Mr. Ameilon had, according to Mr. Berthollet, observed, that the precipitate obtained from nitrate of mercury by oxalic acid, fuses with a hissing noise ||

§ 2. But mercury, and most if not all its oxides, may, by treatment with nitric acid and alcohol, be converted into a whitish crystallized powder, possessing all the inflammable properties of gunpowder, as well as many peculiar to itself.—I was led to this discovery, by a late assertion, that hydrogen is the basis of the muriatic acid: it induced me to attempt to combine different substances with hydrogen and oxygen. With this view, I mixed such substances with alcohol and nitric acid, as I thought, by pre-disposing affinity, favour as well as attract an aeid combination, of the hydrogen of the one, and the oxygen of the other. The pure red oxide of mercury appeared not unfit for this purpose; it was therefore intermixed with alcohol, and nitric acid was affused on both. The acid did not act on the alcohol, so immediately as when these fluids are alone mixed together, but first gradually dissolved the oxide: however, after some minutes had elapsed, a smell of ether was perceptible, and a white dense smoke, much resembling that from the liquor fumans of Libavius, was emitted with ebullition. The mixture then threw down a dark-coloured precipitate, which by degrees became nearly white. This precipitate I separated by filtration; and observing it to be crystallized in small acicular crystals, of a saline taste, and also finding a part of the mercury volatilized in the white fumes, I must acknowledge I was not altogether without hopes that muriatic

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* Messrs. Landriani and Van Marum (Annales de Chimie, tom. 2, p. 270), obtained only hydrogenous gas, by electrifying the carbonic acid gas. But the conductor of their apparatus was an iron one; which metal would combine with the oxygen of the water, and prevent it from appearing in a gaseous state. In my experiments, the conductors were of platina.—Orig.

† Opuscules Chimique de Bayen, tom. 1, p. 346, and note in p. 344. ‡ Annales de Chimie, tom. 27, p. 74 and 79. § Ibid. tom. 21, p. 238. || This fact has been misrepresented, in the introduction to a work entitled The chemical Principles of the metallic Arts, by W. Richardson, surgeon, F. A. S., Sc. (p. 57).—Orig.

acid had been formed, and united to the mercurial oxide. I therefore, for obvious reasons, poured sulphuric acid on the dried crystalline mass, when a violent effervescence ensued, and, to my great astonishment, an explosion took place. The singularity of this explosion induced me to repeat the process several times; and finding that I always obtained the same kind of powder, I prepared a quantity of it, and was led to make the following series of experiments.

§ 3. I first attempted to make the mercurial powder fulminate by concussion; and for that purpose laid about 1 gr. of it on a cold anvil, and struck it with a hammer, likewise cold: it detonated slightly, not being, as I suppose, struck with a flat blow; for, on using 3 or 4 gr., a very stunning disagreeable noise was produced, and the faces both of the hammer and the anvil were much indented. Half a grain or 1 gr., if quite dry, is as much as ought to be used on such an occasion. The shock of an electrical battery, sent through 5 or 6 gr. of the powder, produces a very similar effect: it seems indeed that a strong electrical shock generally acts on fulminating substances like the blow of a hammer. Messrs. Fourcroy and Vauquelin found this to be the case with all their mixtures of oxymuriate of pot-ash.*

To ascertain at what temperature the mercurial powder explodes, 2 or 3 gr. of it were floated on oil, in a capsule of leaf tin; the bulb of a Fahrenheit's thermometer was made just to touch the surface of the oil, which was then gradually heated till the powder exploded, as the mercury of the thermometer reached the 368th degree.

§ 4. Desirous of comparing the strength of the mercurial compound with that of gunpowder, I made the following experiment, in the presence of my friend Mr. Abernethy. Finding that the powder could be fired by flint and steel, without a disagreeable noise, a common gunpowder proof, capable of containing 11 gr. of fine gunpowder, was filled with it, and fired in the usual way: the report was sharp, but not loud. The person who held the instrument in his hand felt no recoil; but the explosion laid open the upper part of the barrel, nearly from the touch-hole to the muzzle, and struck off the hand of the register, the surface of which was evenly indented, to the depth of 0.1 of an inch, as if it had received the impression of a punch.—The instrument used in this experiment being familiarly known, it is therefore scarcely necessary to describe it; suffice it to say, that it was of brass, mounted with a spring register, the moveable hand of which closed up the muzzle to receive and graduate the violence of the explosion. The barrel was $\frac{1}{2}$ an inch in caliber, and nearly $\frac{1}{2}$ an inch thick, except where a spring of the lock impaired $\frac{1}{2}$ its thickness.

§ 5. A gun belonging to Mr. Keir, an ingenious artist of Camden-town, was next charged with 17 gr. of the mercurial powder, and a leaden bullet. A block of wood was placed at about 8 yards from the muzzle, to receive the ball, and the gun was fired by a fuse. No recoil seemed to have taken place; as the barrel was

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* Annale de Chimie, tom. 21, p. 239.—Orig.

not moved from its position, though it was in no ways confined. The report was feeble: the bullet, Mr. Keir conceived, from the impression made on the wood, had been projected with about half the force it would have been by an ordinary charge, or 68 gr., of the best gunpowder. We therefore re-charged the gun with 34 gr. of the mercurial powder; and, as the great strength of the piece removed any apprehension of danger, Mr. Keir fired it from his shoulder, aiming at the same block of wood. The report was like the first in § 4, sharp, but not louder than might have been expected from a charge of gunpowder. Fortunately, Mr. Keir was not hurt, but the gun was burst in an extraordinary manner. The breech was what is called a patent one, of the best forged iron, consisting of a chamber 0.4 of an inch thick all round, and 0.4 of an inch in caliber; it was torn open and flawed in many directions, and the gold touch-hole driven out. The barrel, into which the breech was screwed, was 0.5 of an inch thick; it was split by a single crack 3 inches long; but this did not appear to me to be the immediate effect of the explosion, I think the screw of the breech, being suddenly enlarged, acted as a wedge on the barrel. The ball missed the block of wood, and struck against a wall, which had already been the receptacle of so many bullets, that we could not satisfy ourselves about the impression made by this last.

§ 6. As it was pretty plain that no gun could confine a quantity of the mercurial powder sufficient to project a bullet, with a greater force than an ordinary charge of gunpowder, I determined to try its comparative strength in another way. I procured 2 blocks of wood, very nearly of the same size and strength, and bored them with the same instrument to the same depth. The one was charged with $\frac{1}{4}$ oz. of the best Dartford gunpowder, and the other with $\frac{1}{4}$ oz. of the mercurial powder; both were alike buried in sand, and fired by a train communicating with the powders by a small touch-hole. The block containing the gunpowder was simply split into 3 pieces: that charged with the mercurial powder was burst in every direction, and the parts immediately contiguous to the powder were absolutely pounded, yet the whole hung together, whereas the block split by the gunpowder had its parts fairly separated. The sand surrounding the gunpowder was undoubtedly most disturbed: in short, the mercurial powder appeared to have acted with the greatest energy, but only within certain limits.

§ 7. The effects of the mercurial powder, in the last experiments, made me believe that it might be confined, during its explosion, in the centre of a hollow glass globe. Having therefore provided such a vessel, 7 inches in diameter, and nearly half an inch thick, mounted with brass caps, and a stop cock, (see pl. 11, fig. 1), I placed 10 gr. of the mercurial powder on very thin paper, laid an iron wire 149th of an inch thick across the paper, through the midst of the powder, and, closing the paper, tied it fast at both extremities, with silk, to the wire. As the inclosed powder was now attached to the middle of the wire, each end of which was connected with the brass caps, the packet of powder became, by this disposition, fixed in the centre of the globe. Such a charge of an electrical battery was then sent along the wire, as a preliminary experiment had shown me would, by making the wire red-hot, inflame the powder. The glass globe withstood the explosion, and of course retained whatever gases were generated; its interior was thinly coated with quicksilver in a very divided state. A bent glass tube was now screwed to the stop-cock of the brass cap, which being introduced under a glass jar standing in the mercurial bath, the stop-cock was opened. Three cubical inches of air rushed out, and a 4th was set at liberty when the apparatus was removed to the water-tub. The explosion being repeated, and the air all received over water, the quantity did not vary. To avoid an error from change of temperature, the glass globe was, both before and after the explosion, immersed in water of the same temperature. It appears therefore, that the 10 gr. of powder, produced 4 cubical inches only of air.

To continue the comparison between the mercurial powder and gunpowder, 10 gr. of the best Dartford gunpowder were in a similar manner set fire to in the glass globe: it remained entire. The whole of the powder did not explode, for some complete grains were to be observed adhering to the interior surface of the glass. Little need be said of the nature of the gases generated during the combustion of gunpowder: they must have been, carbonic acid gas, nitrogen gas, sulphureous acid gas, and, according to Lavoisier,* perhaps hydrogen gas. As to the quantity of these gases, it is obvious that it could not be ascertained; because the 2 first were, at least in part, speedily absorbed by the alkali of the nitre, left pure after the decomposition of its nitric acid.

§ 8. From the experiments related in the 4th and 5th §, in which the gunpowder proof and the gun were burst, it might be inferred, that the astonishing force of the mercurial powder is to be attributed to the rapidity of its combustion; and, a train of several inches in length being consumed in a single flash, it is evident that its combustion must be rapid. From the experiments of the 6th and 7th §, it is sufficiently plain that this force is restrained to a narrow limit; both because the block of wood charged with the mercurial powder was more shattered than that charged with the gunpowder, while the sand surrounding it was least disturbed; and also because the glass globe withstood the explosion of 10 gr. of the powder fixed in its centre: a charge I have twice found sufficient to destroy old pistol barrels, which were not injured by being fired when full of the best gunpowder. It also appears, from the last experiment, that 10 gr. of the powder, produced by ignition 4 cubical inches only of air; and it is not to be supposed that the generation, however rapid, of 4 cubical inches of air, will alone account for the described force; neither can it be accounted for by the formation of a little water, which, as will hereafter be shown, happens at the same moment: the quantity formed from 10 gr. must be so trifling, that I cannot ascribe much force to the expansion of its vapour. The sudden vaporization of a part of the mercury, seems to me a principal cause of this immense, yet limited force; because its limi-

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* See Lavoisier, Traité elementaire, p. 527.—Orig.

tation may then be explained, as it is well known that mercury easily parts with caloric, and requires a temperature of 600 degrees of Fahrenheit, to be maintained in the vaporous state. That the mercury is really converted into vapour, by ignition of the powder, may be inferred from the thin coat of divided quicksilver, which, after the explosion in the glass globe, covered its interior surface; and also from the quicksilver with which a tallow candle, or a piece of gold, may be evenly coated, by being held at a small distance from the inflamed powder. These facts certainly render it more than probable, though they do not demonstrate, that the mercury is volatilized; because it is not unlikely that many mercurial particles are mechanically impelled against the surface of the glass, the gold, and the tallow.

As to the force of dilated mercury, Mr. Baumé relates a remarkable instance of it, as follows. "Un alchymiste se présenta à Mr. Geoffroy, et l'assura qu'il avoit trouvé le moyen de fixer le mercure par une opération fort simple. Il fit construire six boîtes rondes en fer fort épais, qui entroient les unes dans les autres; la derniere étoit assujettie par deux cercles de fer qui se croisoient en angles droits. On avoit mis quelques livres de mercure dans la capacité de la première: on mit cet appareil dans un fourneau assez rempli de charbon pour faire rougir à blanc les boîtes de fer; mais, lorsque la chaleur eut pénétré suffisamment le mercure, les boîtes creverent, avec une telle explosion qu'il se fit un bruit épouvantable: des morceaux de boîtes furent lancés avec tant de rapidité, qu'il y en eut qui passerent au travers de deux planchers; d'autres firent sur la muraille des effets semblables à ceux des éclats de bombes."* Had the alchemist proposed to fix water by the same apparatus, the nest of boxes must, I suppose, have likewise been ruptured; yet it does not follow that the explosion would have been so tremendous; indeed it is probable that it would not, for if, as Mr. Kirwan remarked to me, substances which have the greatest specific gravity, have likewise the greatest attraction of cohesion, the supposition that the vapour of mercury exceeds in expansive force the vapour of water, would agree with a position of Sir Isaac Newton, that those particles recede from each other with the greatest force, and are most difficultly brought together, which on contact cohere most strongly.†

§ 9. Before attempting to investigate the constituent principles of this powder, it will be proper to describe the process and manipulations which, from frequent trials, seem to me best calculated to produce it. 100 gr., or a greater proportional quantity, of quicksilver, not exceeding 500 gr.,‡ are to be dissolved, with heat, in a measured ounce and a half of nitric acid.§ This solution being poured cold on 2 measured oz. of alcohol,‖ previously introduced into any convenient glass vessel, a moderate heat is to be applied till an effervescence is excited. A white fume then

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* Chymie expérimentale et raisonnée, tom. 2, p. 393. Paris, 8°, 1773. † Newton's Optics, p. 372, 4th ed. Lond. 1730. ‡ The reason of this limitation is not on account of any danger attending the process; but because the quantities of nitric acid and alcohol required for more than 500 gr., would excite a degree of heat detrimental to the preparation. § Of the specific gravity of about 1.3. ‖ Of the specific gravity of about .849.—Orig.

begins to undulate on the surface of the liquor; and the powder will be gradually precipitated, on the cessation of action and re-action. The precipitation is to be immediately collected on a filter, well washed with distilled water, and carefully dried in a heat not much exceeding that of a water bath. The immediate edulcoration of the powder is material, because it is liable to the re-action of the nitric acid; and while any of that acid adheres to it, it is very subject to the influence of light. Let it also be cautiously remembered, that the mercurial solution is to be poured on the alcohol.

I have recommended quicksilver to be used in preference to an oxide, because it seems to answer equally, and is less expensive; otherwise, not only the pure red oxide, but the red nitrous oxide and turpeth may be substituted; neither does it seem essential to attend to the precise specific gravity of the acid, or the alcohol. The rectified spirit of wine and the nitrous acid of commerce, never failed to produce a fulminating mercury. It is indeed true, that the powder prepared without attention, is produced in different quantities, varies in colour, and probably in strength. From analogy, I am disposed to think the whitest is the strongest; for it is well known, that black precipitates of mercury approach the nearest to the metallic state. The variation in quantity is remarkable; the smallest quantity I ever obtained from 100 gr. of quicksilver being 120 gr., and the largest 132 gr. Much depends on very minute circumstances. The greatest product seems to be obtained, when a vessel is used which condenses and causes most ether to return into the mother liquor; besides which, care is to be had in applying the requisite heat, that a speedy, and not a violent action be effected. 100 gr. of an oxide are not so productive as 100 gr. of quicksilver. As to the colour, it seems to incline to black, when the action of the acid on the alcohol is most violent, and vice versâ.

§ 10. I need not observe, that the gases which were generated during the combustion of the powder in the glass globe, were necessarily mixed with atmospheric air; the facility with which the electric fluid passes through a vacuum, made such a mixture unavoidable. The cubical inch of gas received over water was not readily absorbed by it: and as it soon extinguished a taper, without becoming red, or being itself inflamed, barytes let up to the 3 cubical inches received over mercury, when a carbonate of barytes was immediately precipitated. The residue of several explosions, after the carbonic acid had been separated, was found, by the test of nitrous gas, to contain nitrogen or azotic gas; which does not proceed from any decomposition of atmospheric air, because the powder may be made to explode under the exhausted receiver of an air-pump. It is therefore manifest that the gases, generated during the combustion of the fulminating mercury, consist of carbonic acid and nitrogen gases.

§ 11. The principal re-agents which decompose the mercurial powder, are the nitric, the sulphuric, and the muriatic acids. The nitric changes the whole into nitrous gas, carbonic acid gas, acetous acid, and nitrate of mercury. I resolved it into these different principles, by distilling it pneumatically with nitric acid:

this acid, on the application of heat, soon dissolved the powder, and extricated a quantity of gas, which was found, by well known tests, to be nitrous gas mixed with carbonic acid gas. The distillation was carried on till gas no longer came over. The liquor of the retort was then mixed with the liquor collected in the receiver, and the whole saturated with pot-ash; which precipitated the mercury in a yellowish-brown powder, nearly as it would have done from a solution of nitrate of mercury. This precipitate was separated by a filter, and the filtrated liquor evaporated to a dry salt, which was washed with alcohol. A portion of the salt being refused by this menstruum, it was separated by filtration, and recognized by all its properties, to be nitrate of pot-ash. The alcoholic liquor was likewise evaporated to a dry salt, which, on the affusion of a little concentrate sulphuric acid, emitted acetous acid, contaminated with a feeble smell of nitrous acid, owing to the solubility of a small portion of the nitre in the alcohol.

§ 12. The sulphuric acid acts on the powder in a remarkable manner, as already has been noticed. A very concentrate acid produces an explosion nearly at the instant of contact, on account, I presume, of the sudden and copious disengagement of caloric from a portion of the powder which is decomposed by the acid. An acid somewhat less concentrate likewise extricates a considerable quantity of caloric, with a good deal of gas; but, as it effects a complete decomposition, it causes no explosion. An acid diluted with an equal quantity of water, by the aid of a little heat, separates the gas so much less rapidly, that it may with safety be collected in a pneumatic apparatus. But, whatever be the density of the acid, provided no explosion be produced, there remains in the sulphuric liquor, after the separation of the gas, a white uninflammable and uncrystallized powder, mixed with some minute globules of quicksilver.

To estimate the quantity, and observe the nature, of this uninflammable substance, I treated 100 gr. of the fulminating mercury with sulphuric acid a little diluted. The gas being separated, I decanted off the liquor as it became clear, and freed the insoluble powder from acid, by edulcoration with distilled water; after which I dried it, and found it weighed only 84 gr.; consequently had lost 16 gr. of its original weight. Suspecting, from the operation of the nitric acid in the former experiment, that these 84 gr., with the exception of the quicksilver globules, were oxalate of mercury, I digested them in nitrate of lime, and found my suspicion just. The mercury of the oxalate united to the nitric acid, and the oxalic acid to the lime. A new insoluble compound was formed; it weighed, when washed and dry, 48.5 gr. Carbonate of pot-ash, separated the lime, and formed oxalate of pot-ash, capable of precipitating lime-water, and muriate of lime; though it had been depurated from excess of alkali, and from carbonic acid, by a previous addition of acetous acid. That the mercury of the oxalate in the 84 gr., had united to the nitric acid of the nitrate of lime, was proved by dropping muriatic acid into the liquor from which the substance demonstrated to be oxalate of lime had been separated; for a copious precipitation of calomel instantly ensued.

The sulphuric liquor, decanted from the oxalate of mercury, was now added to that with which it was edulcorated, and the whole saturated with carbonate of pot-ash. As effervescence ceased, a cloudiness and precipitation followed; and the precipitate, being collected, washed, and dried, weighed 3.4 gr.: it appeared to be a carbonate of mercury. On evaporating a portion of the saturated sulphuric liquor, I found nothing but sulphate of pot-ash; nor had it any metallic taste: There then remains, without allowing for the weight of the carbonic acid united to the 3.4 gr., a deficit from the 100 gr. of mercurial powder, of 12.6 gr., which I ascribe to the gas separated by the action of the sulphuric acid. To ascertain the quantity, and examine the nature, of the gas so separated, I introduced into a very small tubulated retort, 50 gr. of the mercurial powder, and poured on it 3 dr., by measure, of sulphuric acid, diluted with an equal quantity of water, and extricated the gas with the assistance of a gentle heat. I first received it over quicksilver, the surface of which, during the operation, partially covered itself with a little black powder.*

The gas, by different trials, amounted from 28 to 31 cubical inches; it at first appeared to be nothing but carbonic acid, as it precipitated barytes water, and extinguished a taper, without being itself inflamed, or becoming red. But, on letting up to it liquid caustic ammonia, there was a residue of from 5 to 7 inches of a peculiar inflammable gas, which burnt with a greenish-blue flame. When I made use of the water-tub, I obtained, from the same materials, from 25 to 27 inches only of gas, though the average quantity of the peculiar inflammable gas was likewise from 5 to 7 inches; therefore the difference of the aggregate product, over the 2 fluids, must have arisen from the absorption, by the water, of a part of the carbonic acid in its nascent state. The variation of the quantity of the inflammable gas, when powder from the same parcel is used, seems to depend on the acid being a little more or less dilute. With respect to the nature of the peculiar inflammable gas, it is plain to me, from the reasons I shall immediately adduce, that it is no other than the gas, in a pure state, into which the nitrous etherized gas can be resolved, by treatment with dilute sulphuric acid.

The Dutch chemists have shown,† that the nitrous etherized gas can be resolved into nitrous gas, by exposure to concentrate sulphuric acid, and that, by using a dilute, instead of a concentrate acid, a gas is obtained which enlarges the flame of a burning taper, so much like the gaseous oxide of azote, that they mistook it for that substance, till they discovered that it was permanent over water, refused to detonate with hydrogen, and that the fallacious appearance was owing to a mixture of nitrous gas with an inflammable gas. The inflammable gas separated from the powder answers to the description of the gas which at first deceived the Dutch chemists; 1st, in being permanent over water; 2dly, refusing to detonate with hydrogen; and, 3dly, having the appearance of the gaseous

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* I cannot account for this appearance.

† Journal de Physique, p. 250, Oct. 1794.—Orig.

oxide of azote, when mixed with nitrous gas. The gas separable by the same acid, from nitrous etherized gas, and from the mercurial powder, have therefore the same properties. Every chemist would thence conclude, that the nitrous etherized gas is a constituent part of the powder, had the inflammable and nitrous gas, instead of the inflammable and carbonic acid gas, been the mixed product extricated from it by dilute sulphuric acid. It however appears to me, that nitrous gas was really produced by the action of the dilute sulphuric acid; and that, when produced, it united to an excess of oxygen present in the oxalate of mercury.

To explain how this change might happen, I must premise that my experiments have shown me, that oxalate of mercury can exist in 2, if not in 3 states. 1st. By the discovery of Mr. Ameilon already quoted, the precipitate obtained by oxalic acid, from nitrate of mercury, fuses with a hissing noise. This precipitate is an oxalate of mercury, seemingly with excess of oxygen. Mercury dissolved in sulphuric acid and precipitated by oxalic acid, and also the pure red oxide of mercury digested with oxalic acid, give oxalates in the same state. 2dly. Acetate of mercury precipitated by oxalic acid, though a true oxalate is formed, has no kind of inflammability. I consider it as an oxalate with less oxygen than those above-mentioned. 3dly. A solution of nitrate of mercury boiled with dulcified spirit of nitre, gives an oxalate more inflammable than any other: perhaps it contains most oxygen.

The oxalate of mercury remaining from the powder in the sulphuric liquor, is not only always in the same state as that precipitated from acetate of mercury, entirely devoid of inflammability, but contains globules of quicksilver; consequently it must have parted with even more than its excess of oxygen; and if nitrous gas was present, it would of course seize at least a portion of that oxygen. It is true, that globules of quicksilver may seem incompatible with nitrous acid; but the quantity of the one may not correspond with that of the other, or the dilution of the acid may destroy its action. As to the presence of the carbonic acid, it must have arisen either from a complete* decomposition of a part of the oxalate; or, admitting the nitrous etherized gas to be a constituent principle of the powder, from a portion of the oxygen, not taken up by the nitrous gas, being united with the carbon of the etherized gas.

§ 13. The muriatic acid digested with the mercurial powder, dissolves a portion of it, without extricating any notable quantity of gas. The dissolution evaporated to a dry salt, tastes like corrosive sublimate; and the portion which the acid does not take up, is left in the state of an uninflammable oxalate.

§ 14. These effects all tend to establish the existence of the nitrous etherized gas, as a constituent part of the powder; and also corroborate the explanation I have ventured to give, of the action of the sulphuric acid. A measured $1\frac{1}{2}$ oz. of nitrous acid, holding 100 gr. of mercury in solution, and 2 measured ounces of

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* Inflammable oxalate of mercury, made to fuse in a retort connected with the quicksilver tube, gives out carbonic acid gas.—Orig.

alcohol, yield 90 cubical inches only of gas: whereas, without the intervention of mercury, they yield 210 inches. On the whole, I trust it will be thought reasonable to conclude, that the mercurial powder is composed of the nitrous etherized gas, and of oxalate of mercury with excess of oxygen. 1st. Because the nitric acid converts the mercurial powder entirely into nitrous gas, carbonic acid gas, acetous acid and nitrate of mercury. 2dly. Because the dilute sulphuric acid resolves it into an uninflammable oxalate of mercury, and separates from it a gas resembling that into which the same acid resolves the nitrous etherized gas. 3dly. Because an uninflammable oxalate is also left, after the muriatic acid has converted a part of it into sublimate. 4thly. Because it cannot be formed by boiling nitrate of mercury in dulcified spirit of nitre; though a very inflammable oxalate is by this means produced. 5thly. Because the difference of the product of gas, from the same measures of alcohol and nitrous acid, with and without mercury in solution, is not trifling; and, 6thly. Because nitrogen gas was generated during its combustion in the glass globe.

Should my conclusions be thought warranted by the reasons I have adduced, the theory of the combustion of the mercurial powder will be obvious to every chemist. The hydrogen of the oxalic acid, and of the etherized gas, is first united to the oxygen of the oxalate, forming water;* the carbon is saturated with oxygen, forming carbonic acid gas; and a part, if not the whole of the nitrogen of the etherized gas, is separated in the state of nitrogen gas; both which last gases, it may be recollected, were after the explosion present in the glass globe. The mercury is revived, and I presume thrown into vapour; as may well be imagined, from the immense quantity of caloric extricated, by adding concentrate sulphuric acid to the mercurial powder. I will not venture to state with accuracy, in what proportions its constituent principles are combined. The affinities I have brought into play are complicated, and the constitution of the substances I have to deal with not fully known. But, to make round numbers, I will resume the statement, that 100 gr. of the mercurial powder lost 16 gr. of its original weight, by treatment with dilute sulphuric acid: 84 gr. of mercurial oxalate, mixed with a few minute globules of quicksilver, remained undissolved in the acid. The sulphuric liquor was saturated with carbonate of potash, and yielded 3.4 gr. of carbonate of mercury. If 1.4 gr. should be thought a proper allowance for the weight of carbonic acid in the 3.4 gr. I will make that deduction, and add the remaining 2 gr. to the 84 gr. of mercurial oxalate and quicksilver; I shall then have, of oxalate and mercury ...... 86 gr.
and a deficit, to be ascribed to the nitrous etherized gas and excess of oxygen 14

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It may perhaps be proper to proceed still further, and recur to the 48.5 gr. separated by nitrate of lime from the 84 gr. of mercurial oxalate and globules of quick-

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* Drops of water were observed on the internal surface of the globe, the day after several explosions had been produced in its centre.—Orig.

silver, in § 11th. These 48.5 gr. were proved to be chiefly oxalate of lime; but they contained also a minute inseparable quantity of mercury, almost in the state of quicksilver, formerly part of the 84 gr. from which they were separated. Had the 48.5 gr. been pure calcareous oxalate, the quantity of pure oxalic acid in them would, according to Bergmann,* be 23.28 gr. Hence, by omitting the 2 gr. of mercury in the 3.4 gr. of carbonate, 100 gr. of the mercurial powder might have been said to contain, of pure oxalic acid 23.28 gr.; of mercury 62.72 gr.; and of nitrous etherized gas and excess of oxygen 14 gr. But, as the 48.5 gr. were not pure oxalate, inasmuch as they contained the mercury they received from the 84 gr. from which they were generated by the nitrate of lime, some allowance must be made for the mercury successively intermixed with the 84 gr. and the 48.5 gr.

In order to make corresponding numbers, and allow for unavoidable errors, I shall estimate the quantity of that mercury to have amounted to 2 gr. which I must of course deduct from the 23.28 gr. of oxalic acid. I shall then have the following statement:

That 100 gr. of the fulminating mercury ought to contain, of pure oxalic acid		21.28 gr.
Of mercury formerly united to the oxalic acid	60.72	
Of mercury dissolved in the sulphuric liquor	2	
And of mercury left in the sulphuric liquor after the separation of the gases	2	
Total of mercury		64.72
Of nitrous etherized gas and excess of oxygen		14.
		100.

Since 100 gr. of the powder seem to contain 64.72 gr. of mercury, it will be immediately inquired, what becomes of 100 gr. of quicksilver, when treated as directed, in the description of the process for preparing the fulminating mercury. It has been stated, in § 9, that 100 gr. of quicksilver produce, under different circumstances, from 120 to 132 gr. of mercurial powder; and, if 100 gr. of this powder contain 64.72 gr. 120 gr. or 132 gr. must, by parity of reasoning, contain 78.06 gr. or 85.47 gr.; therefore 13.34 gr. or 20.75 gr. more of the 100 gr. are immediately accounted for; because 64.72 gr. + 13.34 gr. = 78.06, and 64.72 gr. + 20.75 gr. = 85.47 gr. The remaining deficiency of 21.94 gr. or 14.53 gr. which with the 78.06 gr. or 85.47 gr. would complete the original 100 gr. of quicksilver, remains partly in the liquor from which the powder is separated, and is partly volatilized in the white dense fumes, which in the beginning of this paper I compared to the liquor fumans of Libavius. The mercury cannot, in either instance, be obtained in a form immediately indicative of its quantity; and a series of experiments to ascertain the quantities in which many different substances can combine with mercury, is not my present object. After observing, that the mercury left in the residuary liquor can be precipitated in a very subtle dark powder, by carbonate of potash, I shall content myself with examining the nature of the white fumes.

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* Bergmann, de Acido Sacchari. Opuscula. tom. 1, § 6. p. 248. Leipzig, 1788.—Orig.

§ 15. It is clear that these white fumes contain mercury: they may be wholly condensed in a range of Woulfe's apparatus, charged with a solution of muriate of ammonia. When the operation is over, a white powder is seen floating with ether on the saline liquor, which, if the bottles are agitated, is entirely dissolved. After the mixture has been boiled, or for some time exposed to the atmosphere, it yields to caustic ammonia a precipitate, in all respects similar to that which is separated by caustic ammonia from corrosive sublimate.—I would infer from these facts, that the white dense fumes consist of mercury, or perhaps oxide of mercury, united to the nitrous etherized gas; and that when the muriate of ammonia containing them is exposed to the atmosphere, or is boiled, the gas separates from the mercury; and the excess of nitrous acid, which always comes over with nitrous ether, decomposes the ammoniacal muriate, and forms corrosive mercurial muriate or sublimate. This theory is corroborated, by comparing the quantity of gas estimated to be contained in the fulminating mercury, with the quantities of gas yielded from alcohol and nitrous acid, with and without mercury in solution; not to mention that more ether, as well as more gas, is produced without the intervention of mercury; and that, according to the Dutch chemists, the product of ether is always in the inverse ratio to the product of nitrous etherized gas. Should a further proof be thought necessary, of the existence of the nitrous etherized gas in the fulminating mercury, as well as in the white dense fumes, it may be added, that if a mixture of alcohol and nitrous acid holding mercury in solution, be so dilute, and exposed to a temperature so low, that neither ether nor nitrous etherized gas are produced, the fulminating mercury, or the white fumes, will never be generated: for, under such circumstances, the mercury is precipitated chiefly in the state of an inflammable oxalate. Further, when we consider the different substances formed by an union of nitrous acid and alcohol, we are so far acquainted with all, except the ether and the nitrous etherized gas, as to create a presumption, that no others are capable of volatilizing mercury, at the very low temperature in which the white fumes exist, since during some minutes they are permanent over water of 40° Fahrenheit.

§ 16. Hitherto, as much only has been said of the gas which is separated from the mercurial powder by dilute sulphuric acid, as was necessary to identify it with that into which the same acid can resolve the nitrous etherized gas; I have further to speak of its peculiarity.* The characteristic properties of the inflammable gas, seem to me to be the following: 1st. It does not diminish in volume, either with oxygen or nitrous gas. 2dly. It will not explode with oxygen by the electric shock, in a close vessel. 3dly. It burns like hydrocarbonate, but with a bluish green flame. And, 4thly. It is permanent over water. (§ 12.) It is of course either not formed, or is convertible into nitrous gas, by the concentrate nitric and muriatic acids; because, by those acids, no inflammable gas was extricated from the powder.

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* It must be first noticed, that it is never pure when obtained from the nitrous etherized gas; nor am I aware how it is to be purified, unless the nitrous gas could be taken from it, without being converted into nitrous acid; for, by that acid, it would probably be itself converted into nitrous gas.—Orig.

Should this inflammable gas prove not to be a hydrocarbonate, I shall be disposed to conclude, that it has nitrogen for its basis: indeed, I am at this moment inclined to that opinion, because I find that Dr. Priestley, during his experiments on his dephlogisticated nitrous air, once produced a gas which seems to have resembled this inflammable gas, both in the mode of burning, and in the colour of the flame. After the termination of the common solution of iron in spirit of nitre, he used heat, and got, he says,* "such a kind of air as I had brought nitrous air to be, by exposing it to iron, or liver of sulphur; for, on the first trial, a candle burned in it with a much enlarged flame. At another time, the application of a candle to air produced in this manner, was attended with a real though not a loud explosion; and, immediately after this, a greenish coloured flame descended from the top to the bottom of the vessel in which the air was contained. In the next produce of air, from the same process, the flame descended blue and very rapid, from the top to the bottom of the vessel."

These greenish and blue coloured flames, descending from the top to the bottom of the vessel, are precisely descriptive of the inflammable gas separated from the powder. If it can be produced with certainty by the repetition of Dr. Priestley's experiments, or should it by any means be got pure from the nitrous etherized gas, my curiosity will excite me to make it the object of future research; otherwise, I must confess, I shall feel more disposed to prosecute other chemical subjects: for, having reason to think that the density of the acid made a variation in the product of this gas, and having never found that any acid, however dense, produced an immediate explosion, I once poured 6 dr. of concentrate acid on 50 gr. of the powder. An explosion, nearly at the instant of contact, was effected: I was wounded severely, and most of my apparatus destroyed. A quantity also of the gas I had previously prepared, was lost by the inadvertency of a person who went into my laboratory, while I was confined by the consequences of this discouraging accident. But should any one be desirous of giving the gas a further examination, I again repeat, that as far as I am enabled to judge, it may with safety be prepared, by pouring 3 dr. of sulphuric acid diluted with the same quantity of water, on 50 gr. of the powder, and then applying the flame of a candle till gas begins to be extricated. The only attempt I have made to decompose it, was by exposing it to copper and ammonia; which, during several weeks, did not effect the least alteration.

§ 17. I shall now conclude, by observing, that the fulminating mercury seems to be characterised by the following properties. It takes fire at the temperature of 368 Fahrenheit; explodes by friction,† by flint and steel, and by being thrown into concentrate sulphuric acid. It is equally inflammable under the exhausted receiver of an air-pump, as surrounded by atmospheric air; and it detonates loudly, both by the blow of a hammer, and by a strong electrical shock. Notwithstanding the

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* Priestley on Air, vol. 2, p. 88. Birm. 1790.——† Consequently it should not be inclosed in a bottle with a glass stopper.—Orig.

composition of fulminating silver, and of fulminating gold, differ essentially from that of fulminating mercury, all 3 have some similar qualities. In tremendous effects, silver undoubtedly stands first, and gold perhaps the last. The effects of the mercurial powder and of gunpowder, admit of little comparison. The one exerts, within certain limits, an almost inconceivable force: its agents seem to be gas and caloric, very suddenly set at liberty, and both mercury and water thrown into vapour. The other displays a more extended but inferior power: gas and caloric are, comparatively speaking, liberated by degrees; and water, according to Count Rumford, is thrown into vapour.* Hence it seems, that the fulminating mercury, from the limitation of its sphere of action, can seldom if ever be applied to mining; and, from the immensity of its initial force, cannot be used in fire-arms, unless in cases where it becomes an object to destroy them; perhaps where it is the practice to spike cannon it may be of service, because I apprehend it may be used in such a manner as to burst cannon, without dispersing any splinters.

The inflammation of fulminating mercury by concussion, offers nothing more novel or remarkable than the inflammation, by concussion, of many other substances. The theory of such inflammations has been long since exposed by the celebrated Berthollet, and confirmed by Messieurs Fourcroy and Vauquelin: yet I must confess I am at a loss to understand, why a small quantity of mercurial powder made to detonate by the hammer, or the electric shock, should produce a report so much louder than when it is inflamed by a match, or by flint and steel. It might at first be imagined, that the loudness of the report could be accounted for, by supposing the instant of the inflammation, and that of the powder's confinement between the hammer and anvil, to be precisely the same; but when the electrical shock is sent through or over a few grains of the powder, merely laid on ivory, and a loud report is the consequence, I can form no idea of what causes such a report.

The operation by which the powder is prepared, is perhaps one of the most beautiful and surprising in chemistry; and it is not a little interesting to consider the affinities which are brought into play. The superabundant nitrous acid of the mercurial solution, must first act on the alcohol, and generate ether, nitrous etherized gas, and oxalic acid. The mercury unites to the last 2 in their nascent state, and relinquishes fresh nitrous acid, to act on any unaltered alcohol. The oxalic acid, though a predisposing affinity seems exerted in favour of its quantity, is evidently not formed fast enough to retain all the mercury; otherwise, no white fumes, during a considerable period of the operation, but fulminating mercury alone, would be produced.

Should any doubt still be entertained of the existence of the affinities which have

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* See Phil. Trans. for 1797, p. 222. The hard black substance mentioned by the Count, as remaining after the combustion of gunpowder, must, I believe, have been an alkaline sulphuret, mixed chiefly with sulphite and carbonate of potash. The conjecture that it is white when first formed, is certainly just, as my experiment with the glass globe evinced.—Orig.

been called predisposing or conspiring, a proof that such affinities really exist, will I think be afforded, by comparing the quantity of oxalic acid which can be generated from given measures of nitrous acid and alcohol, with the intervention of mercury, and the intervention of other metals. For instance, when 2 measured ounces of alcohol are treated with a solution of 100 gr. of nickel in a measured ounce and a half of nitrous acid, little or no precipitate is produced; yet, by the addition of oxalic acid to the residuary liquor, a quantity of oxalate of nickel, after some repose, is deposited. Copper affords another illustration: 100 gr. of copper, dissolved in a measured $1\frac{1}{2}$ oz. of nitrous acid, and treated with alcohol, yielded me about 18 gr. only of oxalate; though cupreous oxalate was plentifully generated, by dropping oxalic acid into the residuary liquor. About 21 gr. of pure oxalic acid seem to be produced, from the same materials, when 100 gr. of mercury are interposed. (See § 14). Besides, according to the Dutch paper, more than once referred to, acetous acid is the principal residue after the preparation of nitrous ether. How can we explain the formation of a greater quantity of oxalic acid, from the same materials, with the intervention of 100 gr. of mercury, than with the intervention of 100 gr. of copper, otherwise than by the notion of conspiring affinities, so analogous to what we see in other phenomena of nature?

I have attempted, without success, to communicate fulminating properties, by means of alcohol, to gold, platina, antimony, tin, copper, iron, lead, zink, nickel, bismuth, cobalt, arsenic, and manganese; but I have not yet sufficiently varied my experiments, to enable me to speak with absolute certainty. Silver, when 20 gr. of it were treated with nearly the same proportions of nitrous acid and alcohol as 100 gr. of mercury, yielded, at the end of the operation, about 3 gr. of a gray precipitate, which fulminated with extreme violence. Mr. Cruickshank had the goodness to repeat the experiment: he dissolved 40 gr. of silver in 2 oz. of the strongest nitrous acid diluted with an equal quantity of water, and obtained, by means of 2 oz. of alcohol, 60 gr. of a very white powder, which fulminated like the gray precipitate above described. It probably combines with the same principles as the mercury, and of course differs from Mr. Berthollet's fulminating silver, alluded to in p. 662. I observe, that a white precipitate is always produced in the first instance, and that it may be preserved, by adding water, as soon as it is formed; otherwise, when the mother liquor is abundant, it often becomes gray, and is re-dissolved.

P. S. Since the preceding pages were written, I have been permitted by Lord Howe, Lieut. General of the Ordnance, to make the following trials of the mercurial powder, at Woolwich, in conjunction with Col. Blomefield, and Mr. Cruickshank.

Exper. 1. From the manner in which the screw of the gun-breech, mentioned in § 5, had acted on the barrel, it was imagined, that by bursting an iron case, exactly fitted to the bore of a cannon, its sudden enlargement might make many flaws, and split the piece, without dispersing any splinters. In conformity to this opinion, a cast iron case was constructed, with a cylindrical chamber, of equal length and diameter, calculated to hold 3¼ oz. Troy of the mercurial powder. The case, being firmly screwed together, was charged through its vent-hole, and introduced into a 12-pounder carronade, the bore of which it exactly fitted. The powder was then inflamed, with proper precautions. The gun remained entire, but the case divided; the portion forming the upper surface of the chamber, was expelled in one mass; that adjoining the breech, which constituted the rest of the chamber, was cracked in every direction, and in part crumbled; yet it was so wedged into some indentations which the explosion had made in the sides of the piece, that the fragments were not removed without great labour.

Exper. 2. Another cast iron case was prepared, of the same size as the former, with a chamber also cylindrical, but wrought in a transverse direction, and of a greater length than diameter; the thickness of metal at each extremity not being more than ¼ of an inch. This case was filled with nearly 5 oz. Troy of the mercurial powder, and placed in the same carronade. Three 12-pound shot were next introduced, and brought into close contact with the upper surface of the case, as well as with each other. The gun a 2d time withstood the explosion: the case was divided across the middle of the chamber, into 2 equal parts; that adjoining the breech was, as in the former experiment, much flawed, and left immoveable; that nearest to the muzzle was also much flawed, but driven out with the shot. All the 3 shot were broken; the 2 lower being divided into several pieces, and the upper 1 cracked through the centre.

The report was so feeble, in both experiments, that an inattentive person, I am confident, would not have heard it at the distance of 200 yards.

Exper. 3. It was found so difficult to extract the fragments of the case remaining in the carronade, after the last experiment, that a channel was drilled through them, to the vent-hole of the piece. It was then charged with 6 oz. Troy of the mercurial powder, made up as a cartridge, which did not occupy above ½ of the diameter of the bore. A wad was placed over the powder, dry sand superadded, to fill all vacuities, and the gun filled to the muzzle with 2 12-pound shot. A block of wood was set at a small distance, to receive the impression of the shot, and the powder was inflamed as usual. The carronade still resisted. One of the shot was split into 2 pieces; and the block of wood was driven to a considerable distance, but not penetrated by the shot above the depth of 1 inch. The report was somewhat louder than the former ones. In all the 3 instances, a considerable recoil evidently took place. I presume therefore, that in the first experiment related in § 5, there must have been a recoil, though too trifling to be observed; and in the instances where the gun and the proof were burst, it was not so much to be expected.

Exper. 4. Finding that the carronade, from the great comparative size of its bore to that of its length, required a larger quantity of mercurial powder to burst it than we were provided with, we charged a ½-pounder swivel with 1½ oz. avoirdupois of the mercurial powder, (the service charge of gunpowder being 3 oz.)

and a ⅛-pound shot between 2 wads. The piece was destroyed from the trunnions to the breech, and its fragments thrown 30 or 40 yards. The ball penetrated 5 inches into a block of wood, standing at about a yard from the muzzle of the gun; the part of the swivel not broken, was scarce, if at all, moved from its original position.

Exper. 5. 1 oz. avoirdupois of the mercurial powder enclosed in paper, was placed in the centre of a shell 4.4 inches in diameter, and the vacant space filled with dry sand. The shell burst by the explosion of the powder, and the fragments were thrown to a considerable distance. The charge of gunpowder employed to burst shells of this diameter, is 5 oz. avoirdupois.

Exper. 6. A sea grenade, 3.5 inches diameter, charged like the shell in the last experiment, was burst into numerous fragments, by ¼ oz. avoirdupois of the mercurial powder. The fragments were projected with but little force, and only to the distance of 8 or 10 yards. The charge of gunpowder required for grenades of this size, is 3 oz.

Exper. 7. A sea grenade, of the same diameter as the last-mentioned, and charged in the like manner, with ⅛ oz. avoirdupois, or 57¼ gr., of the mercurial powder, was split into 2 equal pieces, which were not thrown 10 inches asunder. The report in the last 4 experiments was very sharp, but not loud in proportion.

It seems, from the manner in which the swivel was burst, in the 4th experiment, that a smaller charge would have been sufficient for the purpose. We may therefore infer, both from this instance and from the 2d experiment made with the gun, in § 5, that any piece of ordnance might be destroyed, by employing a quantity of the mercurial powder equal in weight to half the service charge of gunpowder; and from the 7th and last experiment we may also conclude, that it would be possible so to proportion the charge of mercurial powder to the size of different cannons, as to burst them without dispersing any splinters. But the great danger attending the use of fulminating mercury, on account of the facility with which it explodes, will probably prevent its being employed for that purpose.

In addition to the other singular properties of the fulminating mercury, it may be observed, that 2 oz. inflamed in the open air, seem to produce a report much louder than when the same quantity is exploded in a gun capable of resisting its action. Mr. Cruickshank, who made some of the powder, by my process, remarked that it would not inflame gunpowder. In consequence of which, we spread a mixture of coarse and fine grained gunpowder on a parcel of the mercurial powder; and after the inflammation of the latter we collected most, if not all of the grains of gunpowder. Can this extraordinary fact be explained by the rapidity of the combustion of fulminating mercury? or is it to be supposed, as gunpowder will not explode at the temperature at which mercury is thrown into vapour, that sufficient caloric is not extricated during this combustion?

From the late opportunity I have had of conversing with Mr. Cruickshank, I find that he has made many accurate experiments on gunpowder; and he has permitted me to state, that the matter which remains after the explosion of gunpowder, consists of potash united with a small proportion of carbonic acid, sulphate of potash, a very small quantity of sulphuret of potash, and unconsumed charcoal. That 100 gr. of good gunpowder yield about 53 gr. of this residuum, of which 3 are charcoal. That it is extremely deliquescent, and when exposed to the air soon absorbs moisture sufficient to dissolve a part of the alkali; in consequence of which, the charcoal becomes exposed, and the whole assumes a black or very dark colour. Mr. Cruickshank likewise informs me, that after the combustion of good gunpowder under mercury, no water is ever perceptible.

References to the Figures of the Glass Globe, &c. mentioned in § 7.—A, in pl. 11, fig. 1, is a ball or globe of glass, nearly half an inch thick, and 7 inches in diameter. It has 2 necks, on which are cemented the brass caps B, C, each being perforated with a female screw, to receive the male ones D, E; through the former a small hole is drilled; the latter is furnished with a perforated stud or shank G. By means of a leather collar H, the neck C can be air-tightly closed. When a portion of the powder is to be exploded, it must be placed on a piece of paper, and a small wire laid across the paper, through the midst of the powder: the paper being then closed, is to be tied at each end to the wire, with a silken thread, as shown at I. One end of this wire is to be fastened to the end of the shank G, and the screw D inserted to half its length into the brass cap B; the other end of the wire, a, by means of the needle K, is to be drawn through the hole F. The screw E being now fixed in its place, and the wire drawn tight, it is to be secured, by pushing the irregular wooden plug L into the aperture of the screw D, taking care to leave a passage for air. The stop-cock M, the section of which is shown at N, is now to be screwed on to the part D, which is made air-tight by the leather collar b. The glass tube O is bent, that it may more conveniently be introduced under the receiver of a pneumatic apparatus. P shows the manner of connecting the glass tube with the stop-cock.

Meteorological Journal, kept at the Apartments of the R. S. By order of the President and Council. p. 239.

1799.	Six's Therm. without			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	°	°	°	°	°	°	°	°	°	Inc.	Inc.	Inc.	°	°	°	Inc.
Jan.	50	20	35.1	50	23	35.6	55	41	49.0	30.43	29.25	29.98	86	61	79.1	0.949
Feb.	56	18	38.2	56	22	38.7	60	42	51.0	30.26	28.88	29.70	92	57	75.2	2.235
Mar.	56	28	39.3	56	28	39.4	62	49	53.6	30.23	29.31	29.84				0.433
April	59	28	44.1	56	30	44.7	58	47	54.3	30.23	28.75	29.62				1.671
May	70	36	52.4	70	40	53.2	62	54	58.7	30.38	29.33	29.84				1.749
June	77	43	58.1	77	49	59.4	67	58	62.1	30.41	29.18	30.04				0.552
July	77	48	62.3	77	52	63.1	68	62	64.9	30.18	29.22	29.82				2.913
Aug.	73	47	60.4	72	51	61.1	66	62	63.3	30.12	29.26	29.81	78	45	59.8	2.209
Sept.	72	44	56.4	71	46	57.2	67	60	62.1	30.40	29.04	29.82	83	45	63.9	2.824
Oct.	63	35	49.6	63	35	49.7	63	55	59.5	30.37	29.34	29.80	88	53	69.4	2.191
Nov.	58	32	44.7	58	32	45.0	60	53	56.1	30.40	28.82	29.87	87	55	71.9	1.587
Dec.	50	17	34.3	50	17	34.7	57	43	50.4	30.54	29.19	29.93	85	60	71.1	0.349
Whole year.			47.9			48.5			57.1			29.84				19.662

* The quicksilver in the basin of the barometer is 81 feet above the level of low water spring tides at Somerset-house.

XII. On Double Images caused by Atmospherical Refraction. By Wm. H. Wollaston, M. D., F. R. S. p. 239.

In some of the last volumes of the Philos. Trans. there have been related many instances of strong atmospherical refraction, by which, objects seen near the horizon have appeared inverted, and the horizon itself either elevated or depressed. Mr. Huddart first took notice of a distinct image, inverted beneath the object itself; and, in the Philos. Trans. for 1797, has described several such appearances, accompanied with an optical explanation, where he shows that the lowest strata of the air were at the time endued with a weaker refractive power, than others at a small elevation. In the volume for 1799, Mr. Vince has given an instance, fig. 1, where erect, as well as inverted images were visible above, instead of beneath, the objects themselves; and, by tracing the progress of the rays of light, in a manner similar to Mr. Huddart's, concludes that these phenomena arose from "unusual variations" of increasing density in the lower strata of the atmosphere. In the volume for 1795, Mr. Dalby mentions having seen "the top of a hill appear detached, for the sky was seen under it." In this case, as well as in the preceding, it is probable that inversion took place, and that the lower half of the portion detached was an inverted image of the upper, as the sky could not be seen beneath it, but by an inverted course of the rays.

Since the causes of these peculiarities of terrestrial refraction have not received so full an explanation as might be wished, I have endeavoured, 1st. To investigate theoretically the successive variations of increasing or decreasing density to which fluids in general are liable, and the laws of the refractions occasioned by them. 2dly. To illustrate and confirm the truth of this theory, by experiments with fluids of known density. And lastly, to ascertain, by trial on the air itself, the causes and extent of those variations of its refractive density, on which the inversions of objects, and other phenomena observed, appear to depend. The general laws may be comprised in three propositions.

Prop. 1. If the density of any medium varies by parallel indefinitely thin strata, any rays of light moving through it in the direction of the strata, will be made to deviate during their passage, and their deviations will be in proportion to the increments of density where they pass. For each ray will be bent towards the denser strata, by a refracting force proportioned to the difference of the densities above and below the line of its passage; and as their velocities are the same, and therefore the times of action of the forces equal, the deviations will be as the refracting forces, i. e. as the increments of density.

Prop. 2. When 2 fluids of unequal density are brought into contact, and unite by mutual penetration; if the densities at different heights be expressed by ordinates, the curve which terminates these ordinates will have a point of contrary flexure. For the straight lines da, rn, pl. 11, fig. 2, which terminate the ordinates rx, dy, of uniform density, will be parallel, and, if not united by contrary curvature, some straight line of union, as ao, must be supposed. But, from whatever cause the first line ao is inferred, by the same cause other intermediate lines mp, tq, &c. will be produced, and curves defm, mtsr, will be ultimately formed, having a point of contrary flexure at m. The form of the curve does not appear to admit of accurate investigation, nor is it of importance to the subsequent reasoning if wholly unknown. We may however form some judgment of its nature; for whether the densities depend on different specific gravities of different fluids, or on unequal temperatures of different portions of the same fluid, the curves will be nearly alike. In each of these cases, to whatever small distance pc, fig. 3, the mutual attraction is sufficient to occasion intimate union of the fluids, the density mn of the mixture will be an arithmetic mean; and, for the same reason, at any intermediate smaller distances, there will be a series of arithmetic means ef, gh, &c. interposed, and the line ao, uniting the ordinates, will be straight. By progressive effect of this attraction, and successive interpolations, in fig. 2, curves defm, rstm, will be formed; of which the straight lines mp, tq, &c. are tangents. The attracting distances np, oq, &c. are subtangents; and if it be admitted that these are every where equal, the curves so produced are logarithmic, and the increment of the ordinate greatest at m, where they meet.

Prop. 3. If parallel rays pass through a medium varying according to the preceding proposition, those above the point of contrary flexure will be made to diverge, and those below the same point will converge, after their passage through it. For, since the deviation of each ray depends on the increment of density where it passes, and since the increment of density is greatest at the point of contrary flexure, any rays, as ab, fig. 4, passing near to that point, will be refracted more towards the denser medium than those at cd, which move in a higher stratum, and will diverge from them, but will be refracted towards and meet those at ef, which pass nearer to the denser medium, where the increments of density are also less.

Cor. Hence, adjacent portions of the converging rays will form a focus, beyond which they will diverge again; and the varied medium will produce effects similar to those caused by a medium of uniform density*, having a surface similar to the curve of densities, since convergence or divergence will be produced, according as the curve of densities is convex or concave; consequently, by tracing backwards, to the extremities of an object, the progress of the visual rays, or axes of the pencils received by the eye, it will be manifest that, any object seen through the inclined concave part rm, fig. 6, would appear elevated, erect, and somewhat diminished. An object seen through md, where it is convex and inclined, would be elevated; and if situated beyond the focus of visual rays from the eye, it would appear in-

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* In the varied medium, bc and bm, fig. 5, the corresponding increments of the abscissa and ordinate, are to each other as radius to the tangent of the angle c. Therefore the tangent of deviation, which is as the increment of the ordinate, varies as the tangent of the angle c. So also, in the uniform medium, since the sines of refraction and incidence are in a given ratio, their differences will bear a given ratio to either of them; and when the angles are small, the tangent of deviation will vary as the tangent of incidence, or as the tangent of the angle c, which is equal to it.—Orig.

verted. The magnitude would depend on the relative distances of the eye and object. Below the point d, where the curve terminates, vision would be direct, so that an object might be situated so as to be seen in all the 3 ways at the same time, direct at o, inverted at I, and erect again at A.

I consider the foregoing propositions as applicable to all cases of varying density, whether occasioned by mutual solution of different fluids, or partial rarefaction of the same fluid; and by trial of various fluids, however different in density, or even in viscidity, I find that the refractions observe a law agreeable to the theory, as will appear by the following experiments.

Exper. 1. Into a square phial containing a small quantity of clear syrup, I put about an equal quantity of water, in such a way that it floated on the surface of the syrup, without mixing. For a short time, the stratum of union was so thin that nothing could be distinctly seen through it. But soon, by mutual penetration of the water and the syrup, the effects represented at A, fig. 7, were produced. Through the syrup, a word written on a card placed behind was seen erect, and in its place; through the adjacent variable medium, an inverted image was visible above the true place; and also above that a 2d image of the same object appeared erect. When these appearances are first discernible, the variations of density are so great, that the object to be looked at may be in contact with the phial; but when the variations of density become more gradual, and so the focus more distant, any object so near is only elongated, and requires to be removed an inch or 2, to be seen inverted.

Exper. 2. Over the surface of the water, in the same phial, I next put about the same measure of rectified spirit of wine. At the stratum where the water and spirit united, the appearances were the same; but since the refractive power of spirit exceeds that of water, the true place of the object was seen uppermost; the inverted and erect images are below. Fig. 7, B. When an oblique line der is viewed through any variable medium so made, it appears bent into different forms, according to its situation with respect to focal distance. If it be at the distance of the principal focus, one point of it is dilated into a vertical line, as lm. Fig. 8, A. If beyond that focus, the portion lm is inclined backwards, being an inverted image of dl; and mn is another image of the same portion seen erect. Fig. 8, B. On this account, it becomes a convenient object for ascertaining the state of any medium under examination.

In these experiments, the appearances continue many hours, even with spirit of wine; with syrup, 2 or 3 days; with acid of vitriol, 4 or 5; with a solution of gum arabic, much longer; but though their disposition to unite is so different, yet the appearances produced are the same in all. The refraction is greatest nearly in the plain of original contact of the fluids, diminishing from thence both upwards and downwards. The exact rate of diminution above or below this point, I had no means of measuring, with the accuracy that would be requisite for determining the nature of the curves of density formed according to the first proposition. But the truth of the 2d proposition appeared capable of confirmation by experiment; for the deviation of a ray is there said to depend on the increment of density, and time of the ray's passage, jointly; accordingly the deviation caused by a given increment should be in proportion to the extent of the medium.

In order to try what effect a greater extent of medium would produce.

Exper. 3. I made a rectangular glass vessel, of which the sides were in the ratio of 10 to 30.6; and, having put into it some clear syrup, with water on its surface, I measured the greatest refractions through it in both directions, and found them in the ratio of 10 to about 29. In another vessel, of which the sides were as 10 to 40.4, the refractions were, on an average, as 1 to 4. Being now fully satisfied of the effect of different fluids, I made the following experiment, by which it appears, that the variations of density occasioned by difference of temperature between adjacent strata of the same fluid, follow a similar law.

Exper. 4. Having put some cold water into a square vessel, I covered its surface with a piece of writing paper perforated with a few small holes, and then filled the vessel cautiously with boiling water. The paper nearly prevented any mixture of the hot and cold water; but, by floating gradually up, left them to communicate their heat by contact alone. While they were in this state, I examined the appearance of remote objects through the varied medium, and found, that when my eye was removed 4 or 5 feet from the vessel, the effects were the same as in the preceding experiments with different fluids; above any object viewed through the cold water, I could distinguish 2 images of it, the one inverted, the other erect, as usual; but these appearances did not continue more than 5 or 6 minutes.

Having thus established, by experiments sufficiently varied, that the contiguity of 2 fluids of unequal density is capable of occasioning all the appearances that have been observed, I shall proceed to show by what means the air may be made to exhibit similar phenomena.

Exper. 5. I heated a common poker red-hot, and held it so as to look along the side of it, at a paper 10 or 12 feet distant. The rarefaction occasioned by it caused a perceptible refraction, to the distance of about $\frac{3}{8}$ of an inch from the side of the poker. A letter seen more distant from it appeared as usual; within that distance there was a faint image of it reversed; and still nearer to the poker was a 2d image direct, and as distinct as the object itself, but somewhat smaller, as in fig. 9, in which a section of the atmosphere surrounding the poker is represented. At the bottom and sides it is nearly circular; but upwards the circular form is lost in undulations, occasioned by the rapid ascent of the rarefied air. The greatest deviation produced in this case measured about $\frac{1}{2}$ a degree.

Exper. 6. By a red-hot bar of iron, 30 inches long, the refractions were much greater, the extreme deviations amounting to full $1\frac{1}{4}$ degree.

The refractions observed in these experiments may, at first view, be thought greater than could be caused by difference of temperature alone, being in one instance more than double the greatest horizontal refraction of the heavenly bodies;

in which case, as the rays enter from a vacuum, the greatest possible effect of the atmosphere might be expected. But it must be remembered, that when a star appears in the horizon, its rays intersect the superior strata of the atmosphere at an inclination of several degrees, and that they pass but once through the variations from rarity to density; but, on the contrary, that in the experiments with red-hot iron, the rays may pass actually in the direction of the strata, and that they are refracted, not only by their entrance from the denser into the rarer medium, but the effect is doubled, since the refraction caused by their emergence is equal to that produced by their incidence.

Though a stratum of air, heated by these means to so great a degree, affords an erect, as well as an inverted, image of objects seen through it, the more moderate warmth communicated to it from bodies heated by the action of the sun on them, seems insufficient to produce both images; but the inverted image may generally be seen, when the sun shines on a brick wall, or other dark-coloured surface. While the eye of the observer is placed nearly in a line with the wall, if another person, at 30 or 40 yards distance, extends any object towards the wall, an image similar to it will appear to come out to meet it. It would be difficult to ascertain with accuracy the degree of rarefaction capable of showing this appearance, but it may be of some use to future observers, to mention the different degrees of heat which I observed. In one instance, a thermometer in contact with the wall, stood at 96°; but, at $\frac{3}{8}$ of an inch distance, 82°. One morning, when the sun shone bright, I examined the temperatures and refraction produced at the surface of a deal bar painted green, about 8 feet long. A small thermometer in contact with the bar, rose to 96°; at $\frac{1}{4}$ of an inch distance, it stood at 73°. The refraction at the same time exceeded 20 minutes.

To explain why red-hot iron occasions 2 images, while solar heat produces but 1, I imagine that the intense heat in the former case rarefies the air for some small distance uniformly, and thus affords the same series of variations as between other fluids of uniform density; but that, in the latter, the heat is conveyed off as fast as it is generated; so that, as there is no extent of medium uniformly rare, the densities corresponding to the concave portion rm, fig. 6, of the curve before described, do not take place, but the phenomena occasioned by the convex part md are alone produced. It must be remarked, that the vertical position of the surface contributes greatly to increase the effect; for since the heated air rises in the direction of the surface, its ascent has in this case no tendency to blend it with the adjacent denser strata, and hence very different degrees of density take place in the thickness of $\frac{1}{4}$ of an inch; so that, as the increments of density are great, the refractions will be proportionally so; but where the heated surface is horizontal, the ascent of the rarefied air into the superincumbent denser strata renders the variations far more gradual; consequently a heated surface of far greater extent must be requisite, to produce equal refraction.

However, over extensive plains, when the sun shines, some degree of inversion is very frequently to be seen; but the inverted images are rarely well defined, unless over a very even surface. One of the best situations for this purpose, is over a level open road, with a gentle breeze blowing across it. A current of air brings a cool stratum more closely in contact with the heated surface; and, since refraction depends on the increment or difference of density in a given small space, a very moderate breeze will thus render inversion more perceptible; but a strong wind will reduce the temperature of the surface, and may make the heated stratum too thin for any object to be seen through it from a distance. In one instance, when I saw a refraction of about 9 minutes, at the distance of $\frac{1}{3}$ of a mile, a thermometer in the sand was 101°; at 4 inches above, 82°; and at 1 foot above, 76°.

Over water the evenness of the surface is favourable to the production of such appearances; but, since the action of the sun is weak on a body so transparent, a far greater extent of surface is requisite to produce any perceptible inversion. Being at Bognor one bright morning, when the sea was calm, I had an opportunity of observing the appearance of Selsea Bill, about 6 miles distant. The whole extent of coast, when viewed with a pocket telescope magnifying about 16 times, appeared inverted from one end to the other; and the lower part of a brick house on the shore, was seen as distinct as the house itself. I judged the quantity of refraction, in this case, to be about 2 minutes of a degree. This state of atmosphere appears to be not very uncommon; for, at Shanklin Chine, in the Isle of Wight, a few days preceding, similar appearances were visible in several directions, but I neglected to make any estimate of the quantity of refraction. In the instance of the inverted vessel seen by Mr. Huddart, (Phil. Trans. for 1797, fig. 3) at the distance of 8 miles, the refraction seems to have been about 3′. All the appearances described by him, I am inclined to think, arose from difference of temperature alone. He offers a conjecture, that evaporation might occasion the lower strata of the atmosphere to have a weaker refractive power; but, from the following experiments, it seems to have a contrary effect.

Exper. 7. I took a plate of glass, and, while looking along the surface, I poured on it a small quantity of ether. A line on the opposite wall, appeared instantaneously elevated many minutes, and at times above $\frac{1}{2}$ a degree. This fluid being the most volatile, and most soluble in the atmosphere, of any known liquid, produces the greatest effect; since the cold, during evaporation, conspires with the ether dissolved in the air, to increase the refractive power. Rectified spirit of wine also produces, from the same cause, a very considerable effect.

Exper. 8. By moistening a board, 5 feet in length, with alcohol, and observing the elevation of an object viewed over its surface, I found the refraction to be about 15′.

Exper. 9. I next made a similar experiment with water itself. Of this, the effect was barely visible, when tried in the same way; but, by means of a surface of 10 feet, and by viewing a luminous point at a greater distance, the refraction became evident, and the object elevated above 3 minutes. In the course of these experiments, I tried whether confining the saturated atmosphere, by boards on each side, would vary the effect, and found the refraction in all cases much lessened; and, when water was used, it became imperceptible; but as soon as the boards were removed, and a free current allowed to pass across, the full effect was again produced. The reason of this difference appears to be, that the quicker evaporation increases the degree of cold, and the current brings greater differences of density contiguous. This state of rapid evaporation will fully account for the phenomenon witnessed by Mr. Latham, who has described (in Phil. Trans. for 1796,) an extraordinary elevation of the opposite coast of France, so as to be seen from the beach at Hastings, and other parts of Sussex. There is a fact of the same kind stated by De la Lande, (Astron. tom. 2,) who says that the mountains of Corsica, though at the distance of more than 100 miles, are occasionally visible from Genoa. It is probably owing to the same cause, that other objects have been sometimes seen, at such distances that we should expect them to be intercepted by the curvature of the earth; for it is evident, that whenever the evaporation over each mile of surface occasions a refraction of about 1 minute, the rays receive a curvature equal to that of the ocean, so that its surface will appear flat, and the spherical form of the earth will not obstruct horizontal vision of objects at any distance.

It still remained to explain the phenomena seen by Mr. Vince, as I had not hitherto made an atmosphere capable of exhibiting images inverted, as well as elevated, by increased density. For, in the refractions produced in the 7th, 8th, and 9th experiments, by evaporation at an exposed surface, I observed the effect was always greatest in contact with the evaporating surface; any lower point a, fig. 10, appeared brought nearer to a higher point c, by the pencils of rays from a being more refracted at b, than the pencil from c was refracted at d. Therefore, any rays passing from the eye at e, as a point, through b and d, would be made to diverge to a and c; consequently visual rays could not, under these circumstances, intersect each other, and no objects could appear inverted. But, whenever the lowest strata of the air becomes saturated with moisture, the variations between the saturated stratum and the incumbent atmosphere of the common density, will follow a law similar to what is found at the confines of other fluids of unequal density; hence, inversion will become visible, as there will be a point below which the increment of density will decrease, and where the refractions will consequently be less, though through a denser medium.

Exper. 10. To produce these appearances, I procured a trough of thin deal, 5 feet long, 1 inch wide, with sides 2½ inches high, and closed the extremities of it with glass. A section of it is given in fig. 11. When the bottom was wetted with ether, the greatest refraction was, at intervals, more than ¾ of an inch from the bottom of the trough; and beneath this height I saw a 2d image inverted, when my eye was removed to 14 or 15 feet distance, and the object at about 70 feet. The focus seemed at the same time to be about 9 feet distant. There was not depth enough of uniformly saturated atmosphere for the object itself to be seen through it, but its true place, compared with that of the images, is represented at a.

Exper. 11. When I made use of rectified spirit in the same apparatus, I had also sufficient proof that the laws of evaporation would admit of such appearances being produced; for the same object now appeared curved downwards, as in fig. 12, so that rays nearer to the bottom were manifestly less refracted than such as passed at some distance above. A degree of convergency must therefore have been produced, though the distance at which the rays would meet, was beyond that of my eye, and circumstances would not admit of my removing beyond 35 feet.

The evaporation of water could not be expected to produce any sensible effect of this kind, in so short a space; but, in a view of some miles extent, there can be no doubt, from the foregoing experiments, that evaporation from the surface of the sea, in such a state of the atmosphere as would allow the lower strata to be saturated, is capable of occasioning all the phenomena which have been described, and probably was the cause of those which Mr. Vince observed. Since heat alone tends to depress objects, and evaporation produces apparent elevation, it is probable, that in the instance of refraction related by Mr. Dalby, Phil. Trans. for 1795, the heat of the sun was the principal agent, and that the moisture rather tended to counteract than assist its action. Simple inversion may generally be seen, when the sun shines on a dry even road of $\frac{1}{3}$ or $\frac{1}{2}$ mile extent; but when the ground has been wet, I have very rarely seen it, and have even failed of discerning it, when the heat has been sufficient to raise a steam from the ground. The following experiment shows that it is not to be expected but by very great extent of surface.

Exper. 12. I placed a dark-coloured board in the sunshine, and, having examined the refraction along its surface, I made a wet line along it, with a sponge dipped in boiling water. Notwithstanding this additional heat, the refraction, in the direction of the wet line, was far less than over the rest of the board, though I took care to observe the effect before the surface could be cooled again by evaporation. I should therefore expect the depression of the horizon at sea, where the refraction occasioned by heat must always be counteracted by evaporation, never to exceed a few minutes; and that any one in a situation commanding a view of the sea, by attention to the various degrees of the dip of the horizon under different circumstances, might soon form some estimate of the proper allowance to be made, for brightness of the sun at the time of an astronomical observation, or for difference of temperature between the sea and air.

Having now examined the several peculiarities of refraction which I proposed for consideration, I shall, in few words, recapitulate the purport of the foregoing pages. According to the theory here given, there appear to be 2 opposite states of the atmosphere, either of which may occasion objects to be seen doubled or tripled, since both increase and decrease of its density; when partial, produce similar effects. It has been explained, 1st. Why air heated by the moderate warmth of the sun's rays, occasions objects to appear doubled and inverted. 2dly. Why rarefaction, by a higher degree of heat, gives an additional image, which is not inverted. 3dly. In what state of evaporation the increase of the air's density brings distant objects into view by unusual elevation. 4thly. Under what circumstances evaporation may also produce an inverted image less elevated. And it is probable, that the same reasoning will afford a ready explanation to other varieties of terrestrial refraction, that may have been, or may hereafter be observed.

XIII. Investigation of the Powers of the Prismatic Colours to Heat and Illuminate Objects; with Remarks, that prove the Different Refrangibility of Radiant Heat. To which is added, an Inquiry into the Method of Viewing the Sun advantageously, with Telescopes of Large Apertures and High Magnifying Powers. By Wm. Herschel, LL. D., F. R. S. p. 255.

It is sometimes of great use in natural philosophy, to doubt of things that are commonly taken for granted; especially as the means of resolving any doubt, when once it is entertained, are often within our reach. We may therefore say, that any experiment which leads us to investigate the truth of what was before admitted on trust, may become of great utility to natural knowledge. Thus, for instance, when we see the effect of the condensation of the sun's rays in the focus of a burning lens, it seems to be natural to suppose, that every one of the united rays contributes its proportional share to the intensity of the heat produced; and we should probably think it highly absurd, if it were asserted that many of them had but little concern in the combustion, or vitrification, which follows, when an object is put into that focus. It will therefore not be amiss to notice what gave rise to a surmise, that the power of heating and illuminating objects, might not be equally distributed among the variously coloured rays.

In a variety of experiments I have occasionally made, relating to the method of viewing the sun, with large telescopes, to the best advantage, I used various combinations of differently-coloured darkening glasses. What appeared remarkable was, that when I used some of them, I felt a sensation of heat, though I had but little light; while others gave me much light, with scarce any sensation of heat. Now, as in these different combinations the sun's image was also differently coloured, it occurred to me, that the prismatic rays might have the power of heating bodies very unequally distributed among them; and, as I judged it right in this respect to entertain a doubt, it appeared equally proper to admit the same with regard to light. If certain colours should be more apt to occasion heat, others might, on the contrary, be more fit for vision, by possessing a superior illuminating power. At all events, it would be proper to recur to experiments for a decision.

Experiments on the Heating Power of Coloured Rays.—I fixed a piece of pasteboard, AB, pl. 11, fig. 13, in a frame mounted on a stand, CD, and moveable on 2 centres. In the pasteboard, I cut an opening, mn, a little larger than the ball of a thermometer, and of a sufficient length to let the whole extent of one of the prismatic colours pass through. I then placed 3 thermometers on small inclined planes, EF: their balls being blacked with japan ink. That of No. 1 was rather too large for great sensibility. No. 2 and 3 were 2 excellent thermometers, which my highly esteemed friend Dr. Wilson, late Professor of Astronomy at Glasgow, had lent me for the purpose: their balls being very small, made them of exquisite sensibility. The scales of all were properly disengaged from the balls. I now placed the stand, with the framed pasteboard and the thermometers, on a small plain board, GH; that I might be at liberty to move the whole apparatus together, without deranging the relative situation of the different parts. This done, I set a prism, moveable on its axis, into the upper part of an open window, at right angles to the solar ray, and turned it about till its refracted coloured spectrum became stationary, on a table placed at a proper distance from the window.

The board containing the apparatus was now put on the table, and set in such a manner as to let the rays of one colour pass through the opening in the pasteboard. The moveable frame was then adjusted to be perpendicular to the rays coming from the prism; and the inclined planes carrying the 3 thermometers, with their balls arranged in a line, were set so near the opening, that any one of them might easily be advanced far enough to receive the irradiation of the colour which passed through the opening, while the rest remained close by, under the shade of the pasteboard. By repeated trials, I found that Dr. Wilson's No. 2 and mine, always agreed in showing the temperature of the place where I examined them, when the change was not very sudden: but that mine would require 10 minutes to take a change, which the other would show in 5. No. 3 never differed much from No. 2.

1*st Exper.* Having arranged the 3 thermometers in the place prepared for the experiment, I waited till they were stationary. Then, advancing No. 1 to the red rays, and leaving the other 2 close by, in the shade, I marked down what they showed at different times, as annexed. This, in about 8 or 10 minutes, gave 6¾ degrees, for the rising produced in my thermometer, by the red rays, compared to the 2 standard thermometers.

No. 1.	No. 2.	No. 3.
43½	43½	43¼
48	43½	43½
49½	43¼	43¼
49¾	43¼	43¼
50	43¼	3¼

2*d Exper.* As soon as my thermometer was restored to the temperature of the room, which I hastened, by applying it to a large piece of metal that had been kept in the same place, I exposed it again to the red rays, and registered its march, along with No. 2 as a standard, which was as annexed. Hence, in 10 minutes, the red rays made the thermometer rise 7 degrees.

No. 1.	No. 2.
45	45
48	45
51	45
51	44½
51	44

3*d Exper.* Proceeding in the same manner as before, in the green rays I had as annexed. Therefore in 10 minutes, the green rays occasioned a rise of 3¼ degrees.

N° 1.	N° 2.
43	43
45½	43
46	43
46	42¾
46	42¾

4th Exper. I now exposed my thermometer to the violet rays, and compared it with N° 2. Here we have a rising of 2°, in 10 minutes, for the violet rays.

N° 1.	N° 2.
44	44
44	44
44¾	43½
45	43

5th Exper. I now exposed Dr. Wilson's thermometer N° 2 to the red rays, and compared its progress with N° 3. Here the thermometer, exposed to red, rose in 5 minutes 2¾ degrees.

N° 2.	N° 3.
44	44
46	44
46½	43¾
46½	43¾

6th Exper. In red rays again. And here the thermometer, exposed to red, rose in 5 minutes 4 degrees.

N° 2.	N° 3.
44	44
46	44
46½	43½
47	43½
47	43

7th Exper. In green rays. This made the thermometer rise, in the green rays, 1½ degree.

N° 2.	N° 3.
43½	43½
44½	43½
44½	43

8th Exper. Again in green rays. Here the rising, by the green rays, was 2 degrees.

N° 2.	N° 3.
43	43
44½	42¾
44¾	42¾

From these experiments, we are authorised to draw the following results. In the red rays, my thermometer gave 6¾ degrees in the 1st, and 7 degrees in the 2d, for the rising of the quicksilver: a mean of both is 6⅞. In the 3d experiment, we had 3¼ degrees, for the rising occasioned by the green rays; from which we obtain the proportion of 55 to 26, for the power of heating in red to that in green. The 4th experiment gave 2° for the violet rays; and therefore we have the rising of the quicksilver in red to that in violet, as 55 to 16. A sufficient proof of the accuracy of this determination we have in the result of the last 4 experiments. The rising for red rays in the 5th, is 2¾; and in the 6th, 4°: a mean of both is 3⅜. In the 7th experiment, we have 1½, and in the 8th, 2° for the rising in green: a mean of these is 1¾. Therefore, we have the proportion of the rising in red to that in green, as 27 to 11, or as 55 to 22.4. We may take a mean of the result of both thermometers, which will be 55 to 24.2, or more than 2¼ to 1, in red to green; and about 3½ to 1, in red to violet.

It appears remarkable, that the most sensible thermometer should give the least alteration, from the exposure to the coloured rays. But since, in these circumstances, there are 2 causes constantly acting different ways; the one to raise the thermometer, the other to bring it down to the temperature of the room, I suppose, that on account of the smallness of the ball in Dr. Wilson's N° 1, which is but little more than ⅛ of an inch, the cooling causes must have a stronger effect on the mercury it contains than they can have on mine, the ball of which is half an inch. More accuracy may hereafter be obtained, by attending to the circumstances of blacking the balls of the thermometers, and their exposure to a more steady and powerful light of the sun, at greater altitudes than it can be had at present; but the experiments which have been related, are quite sufficient for my present purpose; which only goes to prove, that the heating power of the prismatic colours, is very far from being equally divided, and that the red rays are chiefly eminent in that respect.

Experiments on the Illuminating Power of Coloured Rays.—In the following examination of the illuminating power of differently-coloured rays, I had 2 ends in view. The first was, with regard to the illumination itself; and the next, with respect to the aptness of the rays for giving distinct vision; and, though there did not seem to be any particular reason why these 2 should not go together, I judged it right to attend to both. The microscope offered itself as the most convenient instrument for this investigation; and I thought it expedient to view only opaque objects, as these would give me an opportunity to use a direct prismatic ray, without running the risk of any bias that might be given to it, in its transmission through the colouring particles of transparent objects.

Exper. 1. I placed an object that had very minute parts, under a double microscope; and, having set a prism in the window, so as to make the coloured image of the sun stationary on the table where the microscope was placed, I caused the differently-coloured rays to fall successively on the object, by advancing the microscope into their light. The magnifying power was 27 times. In changing the illumination, by admitting a different colour, it always becomes necessary to re-adjust the instrument. It is well known, that the different refrangibility of the rays will sensibly affect the focal length of object-glasses; but, in compound vision, such as in a microscope, where a very small lens is made to cast a lengthened secondary focus, this difference becomes still more considerable. By an attentive and repeated inspection, I found that my object was very well seen in red; better in orange, and still better in yellow; full as well in green; but to less advantage in blue; indifferently well in indigo, and with more imperfection in violet

This trial was made on one of the microscopic objects generally prepared for transparent vision: but, as I used it in the opaque way, I thought that others might be chosen which would answer the purpose better; and, in order to give some variety to my experiments, and to see the effect differently coloured substances might have on the rays of light, I provided the following materials to be viewed. Red paper; green paper; a piece of brass; a nail; a guinea; black paper. Having also found that a higher power might be used, with sufficient convenience for the rays of light to come from the prism to the object, I made the microscope magnify 42 times.

The appearance of the nail in the microscope, is so beautiful, that it deserves to be noticed; and the more so, as it is accompanied with circumstances that are very favourable for an investigation, such as that which is under our present consideration. I had chosen it on account of its solidity and blackness, as being most likely to give an impartial result, of the modifications arising from an illumination by differently-coloured rays; but, on viewing it, I was struck with the sight of a bright constellation of thousands of luminous points, scattered over its whole extent, as far as the field of the microscope could take it in. Their light was that of the illuminating colour, but differed considerably in brightness; some of the points being dim and faint, while others were luminous and brilliant. The brightest of them also admitted of a little variation in their colour, or rather in the intensity of the same colour; for, in the centre of some of the most brilliant of these lucid appearances, their light had more vivacity, and seemed to deviate from the illuminating tint towards whiteness, while on and near the circumference it appeared to take a deeper hue. An object so well divided by nature, into very minute and differently-arranged points, on which the attention might be fixed, in order to ascertain whether they would be equally distinct in all colours, and whether their number would be increased or diminished by different degrees of illumination, was exactly what I wanted; nor could I think it less remarkable, that all the other objects I had fixed on, besides many more which have been examined, such as copper, tin, silver, &c. presented themselves nearly with the same appearance. In the brass, which had been turned in a lathe, the luminous points were arranged in furrows; and in tin they were remarkably beautiful. The result of the examination of my objects was as follows.

Exper. 2. Red paper. In the red rays, I view a bright point near an accidental black spot in the paper, which serves me as a mark; and I notice the space between the point and the spot: it contains several faint points. In the orange rays, I see better. The bright point I now perceive is double. In the yellow rays, I see the object still better. In the green rays, full as well as before. In the blue rays, very well. In the indigo rays, not quite so well as in the blue. In the violet rays, very imperfectly.

Exper. 3. Green paper. Red. I fix my attention on many faint points, in a space between 2 bright double points. Orange; I see those faint points better. Yellow; still better. Green; as well as before: I see remarkably well. Blue; less bright, but very distinct. Indigo; not well. Violet; bad.

Exper. 4. A piece of very clean turned brass.

R. I remark several faint luminous points between 2 bright ones. The colour of the brass makes the red rays appear like orange. O. I see better, but the orange colour is likewise different from what it ought to be; however, this is not at present the object of my investigation. Y. I see still better. G. I see full as well as before. B. I do not see so well now. I. I cannot see well. V. Bad.

Exper. 5. A nail. R. I remark 2 bright points, and some faint ones. O. Brighter than before; and more points visible: very distinct. Y. Much brighter than before; and more points and lines visible: very distinct. G. Full as bright: and as many points visible: very distinct. B. Much less bright; very distinct. I. Still less bright: very distinct. V. Much less bright again: very distinct.

Exper. 6. I viewed a guinea, at 9 feet 6 inches from the prism; and adjusted the place of the object in the several rays, by the shadow of the guinea: If this be not done, deceptions will take place. R. 4 remarkable points: very distinct. O. Better illuminated: very distinct. Y. Still better illuminated: very distinct: The points all over the field of view are coloured; some green; some red; some yellow; and some white, encircled with black about them. Between yellow and green is the maximum of illumination. Extremely distinct. G. As well illuminated as the yellow: very distinct. B. Much inferior in illumination: very distinct. I. Badly illuminated: distinct. V. Very badly illuminated: I can hardly see the object at all.

Exper. 7. The nail again, at 8 feet from the prism. R. I attended to 2 bright points, with faint ones between them almost all the points in the field of view are red: very distinct. O. I see all the points better: they are red, green, yellow, and whitish, with black about them: very distinct. Y. I see better: more bright points, and more faint ones: the points are of various colours: very distinct. G. I see as well: the points are mostly green, and brightish-green, inclining to white: very distinct. B. Much worse illuminated: very distinct. I. Badly illuminated: very distinct. V. There is hardly any illumination.

Exper. 8. The nail again, at 9 feet 6 inches from the prism, by way of having the rays better separated. R. Badly illuminated: the bright points are very distinct. O. Much better illuminated: the bright points very distinct. Y. Still better illuminated: all points extremely distinct. G. As well illuminated, and equally distinct. B. Badly illuminated: the bright points are distinct; but the others are not so. I. Very badly illuminated: I do not see distinctly; but I believe it to be for want of light. V. So badly illuminated that I cannot see the object; or at least but barely perceive that it exists.

Exper. 9. Black paper at 8 feet from the prism. R. The object is hardly visible: I can only see a few faint points. O. I see several bright points, and many faint ones. Y. Numberless bright and small faint points. Between yellow and green, is the maximum of illumination. G. the same as the yellow. B. Very indifferently illuminated; but not so bad as in the red rays. I. I cannot see the object. V. Totally invisible.

From these observations, which agree uncommonly well, with respect to the illuminating power assigned to each colour, we may conclude, that the red-making rays are very far from having it in any eminent degree. The orange possess more of it than the red; and the yellow rays illuminate objects still more perfectly. The maximum of illumination lies in the brightest yellow, or palest green. The green itself is nearly equally bright with the yellow; but, from the full deep green, the illuminating power decreases very sensibly. That of the blue is nearly on a par with that of the red; the indigo has much less than the blue; and the violet is very deficient.

With regard to the principle of distinctness, there appears to be no deficiency in any one of the colours. In the violet rays, for instance, some of the experiments mention that I saw badly; but this is to be understood only with respect to the number of small objects that could be perceived; for though I saw fewer of the points, those which remained visible were always as distinct as, in so feeble an illumination, could be expected. It must indeed be evident, that by removing the great obstacle to distinct vision, which is, the different refrangibility of the rays of light, a microscope will be capable of a much higher degree of distinctness than it can be under the usual circumstances. A celebrated optical writer has formerly remarked, that a fly, illuminated by red rays, appeared uncommonly distinct, and that all its minute parts might be seen in great perfection; and, from the experiments which have been related, it appears that every other colour is possessed of the same advantage.

I am well aware that the results I have drawn from the foregoing experiments, both with regard to the heating and illuminating powers of differently-coloured rays, must be affected by some little inaccuracies. The prism, under the circumstances in which I have used it, could not effect a complete separation of the colours on account of the apparent diameter of the sun, and the considerable breadth of the prism itself, through which the rays were transmitted. Perhaps an arrangement like that in fig. 16, of the Newtonian experiments, might be employed; if instruments of sufficient sensibility, such as air thermometers, can be procured, that may be affected by the enfeebled illumination of rays that have undergone 4 transmissions, and 8 refractions; and especially when their incipient quantity has been so greatly reduced, in their limited passage through a small hole at the first incidence.

But it appeared most expedient for me, at present, to neglect all further refinements, which may be attempted hereafter at leisure. It may even be presumed that, had there not been some small admixture of the red rays in the other colours, the result would have been still more decisive, with regard to the power of heating vested in the red rays. And it is also evident, that at least the red light of the prismatic spectrum, was much less adulterated than any of the other colours; their refractions tending all to throw them from the red. That the same rays which occasion the greatest heat, have not the power of illumination in any strong degree, stands on as good a foundation. For since here also they have undergone the fairest trial, as being most free from other colours, it is equally proved that they illuminate objects but imperfectly. There is some probability that a ray, purified in the Newtonian manner above quoted, especially in a well darkened room, may remain bright enough to serve the purpose of microscopic illumination, in which case more precision can easily be obtained. The greatest cause for a mixture of colours however, which is, the breadth of the prism, I saw might easily be removed; therefore, on account of the coloured points, which have been mentioned in the 6th and 7th experiments, I was willing to try whether they proceeded from this mixture; and therefore covered the prism in front with a piece of pasteboard, having a slit in it of about $\frac{1}{10}$ of an inch broad.

Exper. 10. The nail, at 9 feet 2 inches from the prism. R. I fix my attention on 2 shining, red points; they are pretty bright. O. I see many more points: the object is better illuminated than in the red: the points are surrounded by black; but are orange-coloured. Y. The points now are yellow, and white surrounded by black: the object is better illuminated than in orange. The maximum of illumination is in the brightest yellow, or palest green. G. The points are green and white, as before surrounded by black: better illuminated than in orange. B. The illumination is nearly equal to red. I. Very indifferently illuminated. V. Very badly illuminated.

The phenomena of the differently-coloured points being now completely resolved, since they were plainly owing to the former admixture of colours, and the illuminating power remaining ascertained as before, I attempted also to repeat the experiments on the thermometer, with the prism covered in the same manner; but I found the effect of the coloured rays too much enfeebled to give a decisive result.

I might now proceed to my next subject; but it may be pardonable if I digress for a moment, and remark, that the foregoing researches ought to lead us on to others. May not the chemical properties of the prismatic colours be as different as those which relate to light and heat? Adequate methods for an investigation of them may easily be found; and we cannot too minutely enter into an analysis of light, which is the most subtle of all the active principles that are concerned in the mechanism of the operations of nature. A better acquaintance with it may enable us to account for various facts that fall under our daily observation, but which have hitherto remained unexplained. If the power of heating, as we now see, be chiefly lodged in the red-making rays, it accounts for the comfortable warmth that is thrown out from a fire, when it is in the state of a red glow; and for the heat which is given by charcoal, coke, and balls of small-coal mixed up with clay, used in hot-houses; all which, it is well known, throw out red light. It also explains the reason why the yellow, green, blue, and purple flames of burning spirits mixed with salt, occasion so little heat that a hand is not materially injured, when passed through their coruscations. If the chemical properties of colours also, when ascertained, should be such that an acid principle, for instance, which has been ascribed to light in general, on account of its changing the complexion of various substances exposed to it, may reside only in one of the colours, while others may prove to be differently invested, it will follow, that bodies may be variously affected by light, according as they imbibe and retain, or transmit and reflect, the different colours of which it is composed.

Radiant Heat is of Different Refrangibility.—I must now remark, that my foregoing experiments ascertain beyond a doubt, that radiant heat, as well as light, whether they be the same or different agents, is not only refrangible, but is also subject to the laws of the dispersion arising from its different refrangibility; and, as this subject is new, I may be permitted to dwell a few moments on it. The prism refracts radiant heat, so as to separate that which is less efficacious, from that which is more so. The whole quantity of radiant heat, contained in a sun-beam, if this different refrangibility did not exist, must inevitably fall uniformly on a space equal to the area of the prism; and if radiant heat were not refrangible at all, it would fall on an equal space, in the place where the shadow of the prism, when covered, may be seen. But neither of these events taking place, it is evident that radiant heat is subject to the laws of refraction, and also to those of the different refrangibility of light. May not this lead us to surmise, that radiant heat consists of particles of light of a certain range of momenta, and which range may extend a little farther, on each side of refrangibility, than that of light? We have shown, that in a gradual exposure of the thermometer to the rays of the prismatic spectrum, beginning from the violet, we come to the maximum of light, long before we come to that of heat, which lies at the other extreme. By several experiments, which time will not allow me now to report, it appears that the maximum of illumination has little more than half the heat of the full red rays; and, from other experiments, I likewise conclude, that the full red falls still short of the maximum of heat; which perhaps lies even beyond visible refraction. In this case, radiant heat will at least partly, if not chiefly, consist, if I may be permitted the expression, of invisible light; that is to say, of rays coming from the sun, that have such a momentum as to be unfit for vision. And admitting, as is highly probable, that the organs of sight are only adapted to receive impressions from particles of a certain momentum, it explains why the maximum of illumination should be in the middle of the refrangible rays; as those which have greater or less momenta, are likely to become equally unfit for impressions of sight. Whereas, in radiant heat, there may be no such limitation to the momentum of its particles. From the powerful effects of a burning lens however we gather the information, that the momentum of terrestrial radiant heat is not likely to exceed that of the sun; and that consequently the refrangibility of calorific rays cannot extend much beyond that of colourific light. Hence we may also infer, that the invisible heat of red-hot iron, gradually cooled till it ceases to shine, has the momentum of the invisible rays which, in the solar spectrum viewed by day-light, go to the confines of red; and this will afford an easy solution of the reflection of invisible heat by concave mirrors.

Application of the Result of the foregoing Observations, to the Method of viewing the Sun advantageously, with Telescopes of large Apertures and high magnifying Powers.—Some time before the late transit of Mercury over the sun's disc, I prepared my 7-feet telescope, in order to see it to the best advantage. As I wished to keep the whole aperture of the mirror open, I soon cracked every one of the darkening slips of wedged glasses, which are generally used with achromatic telescopes: none of them could withstand the accumulated heat in the focus of pencils, where these glasses are generally placed. Being thus left without resource, I made use of red glasses; but was by no means satisfied with their performance. My not being prepared, as it happened, was of no consequence; the weather proving totally unfavourable for viewing the sun at the time of the transit.

However, as I was fully aware of the necessity of providing an apparatus for this purpose, since no method that was in use could be applied to my telescopes, I took the first opportunity of beginning my trials. The instrument I wished to adapt for solar inspection, was a Newtonian reflector, with 9 inches aperture; and my aim was, to use the whole of it open.

I began with a red glass, and, not finding it to stop light enough, took 2 of them together. These intercepted full as much light as was necessary; but I soon found that the eye could not bear the irritation, from a sensation of heat, which it appeared these glasses did not stop. I now took 2 green glasses; but found that they did not intercept light enough. I therefore smoked one of them; and it appeared that, notwithstanding they now still transmitted considerably more light than the red glasses, they remedied the former inconvenience of an irritation arising from heat. Repeating these trials several times, I constantly found the same result; and, the sun in the first case being of a deep red colour, I surmised that the red-making rays, transmitted through red glasses, were more efficacious in raising a sensation of heat, than those which passed through green, and which caused the sun to look greenish. In consequence of this surmise, I undertook the investigations which have been delivered under the first 2 heads. As soon as I was convinced that the red light of the sun ought to be intercepted, on account of the heat it occasions, and that it might also be safely set aside, since it was now proved that pale green light excels in illumination, the method which ought to be pursued in the construction of a darkening apparatus was sufficiently pointed out; and nothing remained but to find such materials as would give us the colour of the sun, viewed in a telescope, of a pale green light, sufficiently tempered for the eye to bear its lustre.

To determine what glasses would most effectually stop the red rays, I procured some of all colours, and tried them in the following manner. I placed a prism in the upper part of a window, and received its coloured spectrum on a sheet of white paper. I then intercepted the colours just before they came to the paper, successively, by the glasses, and found the result as follows. A deep red glass intercepted all the rays. A paler red did the same. From this, we ought not to conclude that red glasses will stop the red rays; but rather, that none of the sun's light, after its dispersion by the prism, remains intense enough to pass through red glasses, in sufficient quantity to be perceptible, when it comes to the paper. By looking through them directly at the sun, or even at day objects, it is sufficiently evident that they transmit chiefly red rays.

An orange glass transmitted nearly all the red, the orange, and the yellow. It intercepted some of the green; much of the blue; and very little of the indigo and violet. A yellow glass intercepted hardly any light, of any one of the colours. A dark green glass intercepted nearly all the red, and partly also the orange and yellow. It transmitted the green; intercepted much of the blue; but none of the indigo and violet. A darker green glass intercepted nearly all the red; much of the orange; and a little of the yellow. It transmitted the green; stopped some of the blue; but transmitted the indigo and violet. A blue glass intercepted much of the red and orange; some of the yellow; hardly any of the green; none of the blue, indigo, or violet. A purple glass transmitted some of the red; a very little of the orange and yellow: it also transmitted a little of the green and blue; but more of the indigo and violet.

From these experiments we see, that dark green glasses are most efficacious for intercepting red light, and will therefore answer one of the intended purposes; but since, in viewing the sun, we have also its splendour to contend with, I proceeded to the following additional trials. White glass, lightly smoked, apparently intercepted an equal share of all the colours; and when the smoke was laid on thicker, it permitted none of them to pass. Hard pitch, melted between 2 white glasses, intercepted much light; and, when put on sufficiently thick, transmitted none. Many differently coloured fluids, that were also tried, I found were not sufficiently pure to be used, when dense enough to stop light. Now, red glasses, and the 2 last-mentioned resources of smoke, and pitch, any one of which, it has been seen, will stop as much light as may be required, had still a remaining trial to undergo, relating to distinctness; but this I was convinced could only be decided by actual observations of the sun.

As an easy way of smoking glasses uniformly is of some consequence to distinct vision, it may be of service here to give the proper directions, how to proceed in the operation.

With a pair of warm pliers, take hold of the glass, and place it over a candle, at a sufficient distance not to contract smoke. When it is heated, but no more than still to permit a finger to touch the edges of it, bring down the glass, at the side of the flame, as low as the wick will permit, which must not be touched. Then, with a quick vibratory motion, agitate it in the flame from side to side; at the same time advancing and retiring it gently all the while. By this method, you may proceed to lay on smoke to any required darkness. It ought to be viewed from time to time, not only to see whether it be sufficiently dark, but whether any inequality may be perceived; for if that should happen, it will not be proper to go on. The smoke of sealing-wax is bad: that of pitch is worse. A wax candle gives a good smoke; but that of a tallow candle is better. As good as any I have hitherto met with, is the smoke of spermaceti oil. In using a lamp, you may also have the advantage of an even flame extended to any length.

Telescopic experiments.—N° 1. To put my theory to the trial, I used 2 red glasses, and found that the heat which passed through them could not be suffered a moment; but I was now also convinced that distinctness of vision is capitally injured by the colouring matter of these glasses. N° 2. I smoked a white glass, till it stopped light enough to permit the eye to bear the sun. This destroyed all distinctness; and also permitted some heat to come to the eye, by transmitting chiefly red rays. N° 3. I applied 2 white glasses, with pitch between them, to the telescope; and found that it made the sun appear of a scarlet colour. They transmitted some heat; and distinctness was greatly injured. N° 4. I used a very dark green glass, to stop heat; and behind it, or towards the eye, I placed a red glass, to stop light. The first glimpse I had of the sun, was accompanied with a sensation of heat; distinctness also was materially injured. N° 5. I used a dark green and a pale red; but, the sun not being sufficiently darkened, I smoked the red glass, and, putting a small partition between the two, placed the smoke towards the green glass. This took off the exuberance of light; but did not remedy the inconvenience arising from heat.

N° 6. I used 2 pale green glasses; smoking that next to the eye, and placing it as in N° 5, so that the smoke might be inclosed between the two. This acted incomparably well; but, in a very short time, the heat which passed the first glass, (though not the second, for I felt no sensation of it in the eye) disordered the smoke, by drawing it up into little blisters or stars, which let through light; and this composition therefore soon became useless. N° 7. I used 2 dark green glasses, one of them smoked, as in N° 5. These also acted well; but became useless, for the reason assigned in N° 6, though somewhat less smoke had been required than in the former composition, I felt no heat. N° 8. I used one pale green, with a dark green smoked glass on it, as in N° 5. It bore an aperture of 4 inches very well, and the smoke was not disordered; but when all the tube was open, the pale green glass cracked in a few minutes. N° 9. Placing now a dark green before a smoked green, I saw the sun remarkably well. In this experiment, I had made a difference in the arrangement of the apparatus. The cracking of the glasses, I supposed might be owing to their receiving heat in the middle, while the outside remained cold; which would occasion a partial dilatation. I therefore cut them into pieces about a quarter of an inch square, and set 3 of them in a slider, so that I could move them behind the smoked glass, without disturbing it. After looking about 3 or 4 minutes through one of them, I moved the slider to the 2d, and then to the 3d. This kept the glasses sufficiently cool; but the disturbance of the alterations proved hurtful to vision, which requires repose; and if perchance I stopped a little longer than the proper time, the glass cracked, with a very disagreeable explosion, that endangered the eye.

N° 10. Two dark green glasses, both smoked, that a thinner coat might be on each; but the smoke still contracted blisters, though less dense than before. N° 11. To get rid of smoke entirely, I used 2 dark green glasses, 2 very dark green, 2 pale blue, and 1 pale green glass, together. Distinctness was wanting; nor was light sufficiently intercepted. N° 12. A dark green and a pale blue glass, smoked. The green glass cracked. N° 13. A pale blue and a dark green glass, smoked. The blue glass cracked. The eye felt no sensation of heat. N° 14. Two pale blue glasses, one smoked. The first glass cracked.

It was now sufficiently evident, that no glass which stops heat, and therefore receives it, could be preserved from cracking, when exposed to the focus of pencils.

This induced me to try an application of the darkening apparatus to another part of the telescope. The place where the rays are least condensed, without interfering with the reflections of the mirrors, is immediately close to the small one. I therefore screwed an apparatus to the speculum arm, into which any glass might be placed. N° 15. A dark green glass close to the small speculum, and smoked pale green in the focus of pencils, as before. I saw remarkably well. N° 16. The dark green as before; but, that more light might be admitted, a white smoked glass near the eye. Better than N° 15; but the green glass cracked. N° 17. A very dark green and white smoked glass, as before. Very distinct, but the green glass cracked in about 6 or 7 minutes. N° 18. A dark blue glass, as in N° 15, and white smoked. This was distinct; and no heat came to the eye. The sun appeared ruddy. N° 19. A dark blue and a yellow glass, close together, as in N° 15, and a white smoked one, as before. This was not distinct. N° 20. A purple glass, as in N° 15, with a white smoked one. This gave the sun of a deep orange colour, approaching to scarlet. It was not distinct. N° 21. An orange glass, as in N° 15, with a white smoked one. The colour of the sun was too red. N° 22. A white smoked glass, as in N° 15, without any other at the eye. This gave the sun of a beautiful orange colour; but distinctness was totally destroyed.

N° 23. The heat near the small speculum being still too powerful for the glasses, I had a bluish dark green glass made of a proper diameter to be inclosed between the 2 eye-glasses of a double eye-piece. All glass I knew would stop some heat; and was therefore in hopes that the interposition of this eye-glass would temper the rays, so as in some measure to protect the coloured glass. In the usual place near the eye, I put 2 white glasses, with a thin coat of pitch between them. These glasses, when looked through by the natural eye, give the sun of a red colour; I therefore entertained no great hopes of their application to the telescope. They darkened the sun not sufficiently; and, when the pitch was thickened, distinctness was wanting. N° 24. The same glass between the eye-glasses, and a dark green smoked glass at the eye. Very distinct. This arrangement is preferable to that of N° 15; after some considerable time however this glass also cracked. N° 25. I placed a very dark green glass behind the 2d eye-glass, that it might be sheltered by both glasses, which in my double eye-piece are close together, and of an equal focal length. Here, as the rays are not much concentrated, the coloured glass receives them on a large surface, and stops light and heat, in the proportion of the squares of its diameter now used, to that on which the rays would have fallen, had it been placed in the focus of pencils. And, for the same reason, I now also placed a dark green smoked glass close on the former, with the smoked side towards the eye, that the smoke might also be protected against heat, by a passage of the rays through 2 surfaces of coloured glass. This position had also the advantage of leaving the telescope, with its mirrors and glasses, completely to perform its operation, before the application of the darkening apparatus; and thus to prevent the injury which must be occasioned, by the interposition of the heterogeneous colouring matter of the glasses and of the smoke.

N° 26. I placed a deep blue glass with a bluish green smoked one on it, as in N° 25, and found the sun of a whiter colour than with the former composition. There was no disagreeable sensation of heat; though a little warmth might be felt. N° 27. I used 2 black glasses, placed as in N° 25. Here there was no occasion for smoke; but the sun appeared of a bright scarlet colour, and an intolerable sensation of heat took place immediately. I rather suspect that these are very deep red glasses, though their outward appearance is black.

In order to have a more sure criterion for heat, I applied Dr. Wilson's thermometer, N° 2, to the end of the eye-piece, where the eye is generally placed. With N° 25, it rose from 34 to 37 degrees. With N° 26, it rose from 35 to 46; and, with N° 27, it rose, very quickly, from 36 to 95 degrees. I am pretty sure it would have mounted up still higher; but, the scale extending only to 100, I was not willing to run the risk of breaking the thermometer by a longer exposure. It remains now only to be added, that with N° 25 and 26 I have seen uncommonly well; and that, in a long series of very interesting observations on the sun, which will soon be communicated, the glasses have met with no accident. However, when the sun is at a considerable altitude, it will be advisable to lessen the aperture a little, in telescopes that have so much light as my 10-feet reflector; or, which will give us more distinctness, to view the sun earlier in the morning, and later in the afternoon; for, the light intercepted by the atmosphere in lower altitudes, will reduce its brilliancy much more uniformly than we can soften it, by laying more smoke on our darkening glasses. Now as few instruments in common use are so large as that to which this method of darkening has been adapted, we may hope that it will be of general utility in solar observations.

XIV. Experiments on the Refrangibility of the Invisible Rays of the Sun. By Wm. Herschel, LL. D., F. R. S. p. 284.

In that section of my former paper which treats of radiant heat, it was hinted, though from imperfect experiments, that the range of its refrangibility is probably more extensive than that of the prismatic colours; but having lately had some favourable sun-shine, and obtained a sufficient confirmation of the same, it will be proper to add the following experiments to those which have been given. I provided a small stand, with 4 short legs, and covered it with white paper. (See fig. 14, pl. 11). On this I drew 5 lines, parallel to one end of the stand, at half an inch distance from each other, but so that the first of the lines might only be ¼ of an inch from the edge. These lines I intersected at right angles with 3 others; the 2d and 3d of which were respectively at 2½ and at 4 inches from the first. The same thermometers that have before been marked N° 1, 2, and 3, mounted on their small inclined planes, were then placed so as to have the centres of the shadow of their balls thrown on the intersection of these lines. Now, setting my little stand on a table, I caused the prismatic spectrum to fall with its extreme colour on the edge of the paper, so that none might advance beyond the first line. In this arrangement, all the spectrum, except the vanishing last quarter of an inch, which served as a direction, passed down by the edge of the stand, and could not interfere with the experiments. I had also now used the precaution of darkening the window in which the prism was placed, by fixing up a thick dark green curtain, to keep out as much light as convenient.

The thermometers being perfectly settled at the temperature of the room, I placed the stand so that part of the red colour, refracted by the prism, fell on the edge of the paper, before the thermometer N° 1, and about half way, or 1¼ inch, towards the 2d: it consequently did not come before that, or the 3d thermometer, both which were to be my standards. During the experiment, I kept the last termination of visible red carefully to the first line, as a limit assigned to it, by gently moving the stand when required; and found the thermometers, which were all placed on the 2d line, affected as annexed. Here the thermometer N° 1 rose 6½ degrees, in 10 minutes, when its centre was placed ½ inch beyond visible light.

N° 1.	N° 2.	N° 3.
45	45	44
49	45	44
51	44¾	44
50¼	43¾	43½

In order to have a confirmation of this fact, I cooled the thermometer N° 1, and placed N° 2 in the room of it: I also put N° 3 in the place of N° 2, and N° 1 in that of N° 3; and having exposed them as before arranged on the 2d line, I had the annexed result. Here the thermometer N° 2 rose to 2¾ degrees, in 12 minutes; and being much more sensible than N° 1, it came to the temperature of its situation in a short time; but I left it exposed longer, on purpose to be perfectly assured of the result. Its showing but 2¾ degrees advance, when N° 1 showed 6½, has also been accounted for before.

N° 2.	N° 3.	N° 1.
44	44	45
47	44	45
46¾	44	45
46¾	44	45

It being now evident that there was a refraction of rays coming from the sun, which, though not fit for vision, were yet highly invested with a power of occasioning heat, I proceeded to examine its extent as follows. The thermometers were arranged on the 3d line, instead of the 2d; and the stand was, as before, immersed up to the first, in the coloured margin of the vanishing red rays. The result was thus. Here the thermometer N° 1 rose 5¼ degrees, in 13 minutes, at 1 inch behind the visible light of the red rays.

N° 1.	N° 2.	N° 3.
46	46	45¾
50	46½	46
51¾	46¾	46¼
52¼	47	46¾

I now placed the thermometers on the 4th line, instead of the 3d; and, proceeding as before, I had the annexed result. Therefore the thermometer N° 1 rose 3⅛ degrees, in 10 minutes, at 1½ inch beyond the visible light of the red rays,

N° 1.	N° 2.	N° 3.
48¼	48¼	47¾
51½	48⅜	47⅞

I might now have gone on to the 5th line; but, so fine a day, with regard to clearness of sky and perfect calmness, was not to be expected often, at this time of the year; I therefore hastened to make a trial of the other extreme of the prismatic spectrum. This was attended with some difficulty, as the illumination of the violet rays is so feeble, that a precise termination of it cannot be perceived. However, as well as could be judged, I placed the thermometers 1 inch beyond the reach of the violet rays, and found the result as annexed. Here the several indications of the thermometers, 2 of which, N° 1 and 2, were used as variable, while the 3d was kept as the standard, were read off during a time that lasted 12 minutes; but they afford, as may be seen by inspection, no ground for ascribing any of their small changes to other causes than the accidental disturbance which will arise from the motion of the air, in a room where some employment is carried on.

N° 1.	N° 2.	N° 3.
48	48	47¾
48	48	47¾
48	47½	47
48½	47½	47
48	48	47¾

I now exposed the thermometer to the line of the very first perceptible violet light; but so that N° 1 and 2 might again be in the illumination, while N° 3 remained a standard. The result proved as annexed. Here the thermometer N° 1 rose 1° in 15 minutes; and N° 2 rose ½°, in the same time. From these last experiments, I was now sufficiently persuaded, that no rays which might fall beyond the violet, could have any perceptible power, either of illuminating or of heating; and that both these powers continued together throughout the prismatic spectrum, and ended where the faintest violet vanishes.

N° 1.	N° 2.	N° 3.
48	48	47¾
48½	48	47¾
48¾	48½	47¾
49	48½	47¾

A very material point remained still to be determined, which was, the situation of the maximum of the heating power. As I knew already that it did not lie on the violet side of the red, I began at the full red colour, and exposed my thermometers, arranged on a line, so as to have the ball of N° 1 in the midst of its rays, while the other 2 remained at the side, unaffected by them. Here the thermometer N° 1 rose 7° in 10 minutes, by an exposure to the full red coloured rays.

N° 1.	N° 2.	N° 3.
48½	48½	48
55½	48½	48
55½	48½	48

I drew back the stand, till the centre of the ball of N° 1 was just at the vanishing of the red colour, so that half of its ball was within, and half without, the visible rays of the sun. Here the thermometer N° 1 rose 8° in 10 minutes.

N° 1.	N° 2.	N° 3.
48½	48½	48
55½	48½	48
57	49	48½

By way of not losing time, in order to connect these last observations the better together, I did not bring back the thermometer N° 1 to the temperature of the room, being already well acquainted with its rate of showing, compared to that of N° 2, but went on to the next experiment, by withdrawing the stand, till the whole ball of N° 1 was completely out of the sun's visible rays, yet so as to bring the termination of the

N° 1.	N° 2.	N° 3.
57	49	48½
58½	49¾	49
59	50¼	49¾
59	50	49½

line of the red colour as near the outside of the ball as could be, without touching it. Here the thermometer N° 1 rose, in 10 minutes, another degree higher than in its former situation it could be brought up to; and was now 9° above the standard. The ball of this thermometer is exactly half an inch in diameter; and its centre therefore was ¼ inch beyond the visible illumination, to which no part of it was exposed.

It would not have been proper to compare these last observations with those taken at an earlier period this morning, in order to obtain a true maximum, as the sun was now more powerful than it had been at that time: for which reason, I caused the line of termination of visible light, now to fall again just ½ inch from the centre of the ball; and had the annexed result. And here the thermometer N° 1 rose, in 16 minutes, 8¾°, when its centre was ½ inch out of the visible rays of the sun. Now, as before we had a rising of 9°, and here 8¾, the difference is almost too trifling to suppose, that this latter situation of the thermometer was much beyond the maximum of the heating power; while at the same time the experiment sufficiently indicates, that the place inquired after need not be looked for at a greater distance.

N° 1.	N° 2.	N° 3.
50½	50½	50
57¾	50	49½
58½	50	49½
58¾	50	49½

It will now be easy to draw the result of these observations into a very narrow compass. The first 4 experiments prove, that there are rays coming from the sun, which are less refrangible than any of those that affect the sight. They are invested with a high power of heating bodies, but with none of illuminating objects; and this explains the reason why they have hitherto escaped unnoticed. My present intention is, not to assign the angle of the least refrangibility belonging to these rays, for which purpose more accurate, repeated, and extended experiments are required. But, at the distance of 52 inches from the prism, there was still a considerable heating power exerted by our invisible rays, 1½ inch beyond the red ones, measured on their projection on a horizontal plane. I have no doubt but that their efficacy may be traced still somewhat farther. The 5th and 6th experiments show, that the power of heating is extended to the utmost limits of the visible violet rays, but not beyond them; and that it is gradually impaired, as the rays get more refrangible. The last 4 experiments prove, that the maximum of the heating power is vested among the invisible rays; and is probably not less than half an inch beyond the last visible ones, when projected in the manner before mentioned. The same experiments also show, that the sun's invisible rays, in their less refrangible state, and considerably beyond the maximum, still exert a heating power fully equal to that of red-coloured light; and that consequently, if we may infer the quantity of the efficient from the effect produced, the invisible rays of the sun probably far exceed the visible ones in number.

To conclude, if we call light, those rays which illuminate objects, and radiant heat, those which heat bodies, it may be inquired, whether light be essentially different from radiant heat? In answer to which I would suggest, that we are not allowed, by the rules of philosophizing, to admit 2 different causes to explain certain effects, if they may be accounted for by 1. A beam of radiant heat, emanating from the sun, consists of rays that are differently refrangible. The range of their extent, when dispersed by a prism, begins at violet-coloured light, where they are most refracted, and they have the least efficacy. We have traced these calorific rays throughout the whole extent of the prismatic spectrum; and found their power increasing, while their refrangibility was lessened, as far as to the confines of red-coloured light. But their diminishing refrangibility, and increasing power, did not stop here; for we have pursued them a considerable way beyond the prismatic spectrum, into an invisible state, still exerting their increasing energy, with a decrease of refrangibility up to the maximum of their power; and have also traced them to that state where, though still less refracted, their energy, on account, we may suppose, of their now failing density, decreased pretty fast; after which, the invisible thermometrical spectrum, if I may so call it, soon vanished. If this be a true account of solar heat, for the support of which I appeal to my experiments, it remains only for us to admit, that such of the rays of the sun as have the refrangibility of those which are contained in the prismatic spectrum, by the construction of the organs of sight, are admitted, under the appearance of light and colours; and that the rest, being stopped in the coats and humours of the eye, act on them, as they are known to do on all the other parts of our body, by occasioning a sensation of heat.

In the view of the apparatus, fig. 14, pl. 11, AB is the small stand; 1, 2, 3, the thermometers on it; CD the prism at the window; E the spectrum thrown on the table, so as to bring the last quarter of an inch of the red colour on the stand.

XV. Experiments on the Solar, and on the Terrestrial Rays that occasion Heat; with a Comparative View of the Laws to which Light and Heat, or rather the Rays which occasion them, are subject, in order to determine whether they are the same, or different. By Wm. Herschel, LL.D., F. R. S. p. 293.

The word heat, in its common acceptation, denotes a certain sensation well-known to every person. The cause of this sensation, to avoid ambiguity, ought to have been distinguished by a name different from that which is used to point out its effect. Various authors indeed, who have treated on the subject of heat, have occasionally added certain terms to distinguish their conceptions, such as, latent, absolute, specific, sensible heat; while others have adopted the new expressions of caloric, and the matter of heat. None of these descriptive appellations however would have completely answered my purpose. I might, as in the preceding papers, have used the name radiant heat, which has been introduced by a celebrated author, and which certainly is not very different from the expressions I have now adopted; but, by calling the subject of my researches, the rays that occasion heat, I cannot be misunderstood as meaning that these rays themselves are heat; nor do I in any respect engage myself to show in what manner they produce heat.

From what has been said it follows, that any objections that may be alleged, from the supposed agency of heat in other circumstances than in its state of radiance, or heat-making rays, cannot be admitted against my experiments. For, notwithstanding I may be inclined to believe that all phenomena in which heat is concerned, such as the expansion of bodies, fluidity, congelation, fermentation, friction, &c. as well as heat in its various states of being latent, specific, absolute, or sensible, may be explained on the principle of heat-making rays, and vibrations occasioned by them in the parts of bodies; yet this is not intended, at present, to be any part of what I shall endeavour to establish. I must also remark, that in using the word rays, I do not mean to oppose, much less to countenance, the opinion of those philosophers who still believe that light itself comes to us from the sun, not by rays, but by the supposed vibrations of an elastic ether, every where diffused throughout space; I only claim the same privilege for the rays that occasion heat, which they are willing to allow to those that illuminate objects. For, in what manner soever this radiance may be effected, it will be fully proved hereafter that the evidence, either for rays, or for vibrations which occasion heat, stands on the same foundation on which the radiance of the illuminating principle, light, is built.

In order to enter on our subject with some regularity, it will be necessary to distinguish heat into 6 different kinds, 3 solar, and 3 terrestrial; but, as the divisions of terrestrial heat strictly resemble those of solar, it will not be necessary to treat of them separately; our subject therefore may be reduced to the 3 following general heads. We shall begin with the heat of luminous bodies in general, such as, in the first place, we have it directly from the sun; and as, in the 2d, we may obtain it from terrestrial flames, such as torches, candles, lamps, blue-lights, &c. Our next division comprehends the heat of coloured radiants. This we obtain, in the first place, from the sun, by separating its rays in a prism; and, in the 2d, by having recourse to culinary fires, openly exposed. The 3d division relates to heat obtained from radiants, where neither light nor colour in the rays can be perceived. This, as I have shown, is to be had, in the first place, directly from the sun, by means of a prism applied to its rays; and, in the 2d, we may have it from fires inclosed in stoves, and from red-hot iron cooled till it can no longer be seen in the dark.

Besides the arrangement in the order of my experiments which would arise from this division, we have another subject to consider. For, since the chief design of this paper is to give a comparative view of the operations that may be performed on the rays that occasion heat, and of those which we already know to have been effected on the rays that occasion light, it will be necessary to take a short review of the latter. I shall merely select such facts as not only are perfectly well known, but especially such as will answer the intention of my comparative view, and arrange them in the following order. 1. Light, both solar and terrestrial, is a sensation occasioned by rays emanating from luminous bodies, which have a power of illuminating objects; and, according to circumstances, of making them appear of various colours. 2. These rays are subject to the laws of reflection. 3. They are also subject to the laws of refraction. 4. They are of different refrangibility. 5. They are liable to be stopped, in certain proportions, when transmitted through diaphanous bodies. 6. They are liable to be scattered on rough surfaces. 7. They have hitherto been supposed to have a power of heating bodies; but this remains to be examined.

The similar propositions relating to heat, which are intended to be proved in this paper, will stand as follows. 1. Heat, both solar and terrestrial, is a sensation occasioned by rays emanating from candent substances, which have a power of heating bodies. 2. These rays are subject to the laws of reflection. 3. They are also subject to the laws of refraction. 4. They are of different refrangibility. 5. They are liable to be stopped, in certain proportions, when transmitted through diaphanous bodies. 6. They are liable to be scattered on rough surfaces. 7. They may be supposed, when in a certain state of energy, to have a power of illuminating objects; but this remains to be examined.

I have to mention, that the number of experiments which will be required to make good all these points, exceeds the usual length of my papers; on which account, I shall divide the present one into 2 parts. Proceeding therefore now to an investigation of the first 3 heads that have been proposed, I reserve the next 3, and a discussion which will be brought on by the 7th article, for the 2d part.

Exper. 1. *Reflection of the Heat of the Sun.*—I exposed the thermometer, which in a former paper has been denoted by N° 3, to the eye-end of a 10-feet Newtonian telescope, which carried a camera-eye-piece, but no eye-glass. When, by proper adjustment, the focus came to the ball of the thermometer, it rose from 52 to 110°; so that rays which came from the sun, underwent 3 regular reflections; one, on a concave mirror, and the other 2, on 2 plain ones. Now these rays, whether they were those of light or not, for that our experiment cannot ascertain, had a power of occasioning heat, which was manifested in raising the thermometer 58°.

Exper. 2. *Reflection of the Heat of a Candle.*—At the distance of 29 inches from a candle, I planted a small steel-mirror, of $3\frac{4}{10}$ inches diameter, and about 2¼ inches focal length. (See pl. 12, fig. 1). In the secondary focus of it, I placed the ball of the thermometer which in my paper has been marked N° 2; and very near it, but out of the reach of reflection, the thermometer N° 3. Having covered the mirror till both were come to the temperature of their stations, I began as annexed. Here, in 5 minutes, the thermometer N° 2 received 3¼ degrees of heat from the candle, by reflected rays. I now covered the mirror, but left all the rest of the apparatus untouched:—Here, in 6 minutes the thermometer lost the 3¼ degrees of heat again, which it had gained before. I uncovered the mirror once more; and, in 5 minutes, the 3¼ degrees of heat were regained. In consequence of which, we are assured that certain

Min.	N° 2. In the Focus.	N° 3. Standard.
0	54	54
1	55	54
2	56	54
3	57	54
4	57¼	54
5	57¼	54

Min.	N° 2. In the Focus.	N° 3. Standard.
0	57¼	54
1	55½	54
1½	55	54
6	54	54
0^m	54	54
1½	56	54
3½	57	54
5	57¼	54

rays came from the candle, subject to the laws of reflection, which, though they might not be the rays of light, for that our experiment does not determine, were evidently invested with a power of heating the thermometer placed in the focus of the mirror.

Exper. 3. *Reflection of the Heat that accompanies the Solar Prismatic Colours.*—In the spectrum of the sun, given by a prism, I placed my small steel mirror, with a thermometer in its focus, fig. 2, pl. 12. It was covered by a piece of pasteboard, which, through a proper opening, admitted all the visible colours to fall on its polished surface, but excluded every other ray of heat that might be, either on the violet or on the red side, beyond the spectrum. Then, placing the apparatus so as to have the thermometer in the red rays, but keeping the mirror covered up till the thermometer became settled, I found it stationary at 58°. Uncovering the mirror, I had as annexed. Here, in 2 minutes, the thermometer rose 35°, by reflected heat. I covered the mirror again, and in a few minutes the thermometer, exposed to the direct prismatic red, came down to 58° again. And thus the prismatic colours, if they are not themselves the heat-making rays, are at least accompanied by such as have a power of occasioning heat, and are liable to be regularly reflected.

Min.	N° 2.
0	58
2	93

Exper. 4. *Reflection of the Heat of a red-hot Poker.*—I placed the small steel mirror at 12 inches from a red-hot poker, set with its heated end upwards, in a perpendicular position, and so elevated as to throw its rays on the mirror, fig. 1, pl. 12. The thermometer N° 2 was placed in its secondary focus, and had a small pasteboard screen, to guard its ball from the direct heat of the poker.—Here, in 1½ minute, the thermometer rose 38½ degrees, by reflected rays; and when the mirror was covered up it fell in the next 1½ minute, 28 degrees. On which account, we cannot but allow, that certain rays, whether those that shine or not, issue from an ignited poker, which are subject to the regular laws of reflection, and have a power of heating bodies.

Min.	N° 2.
0	54½
1½	93
I covered the mirror.	
3′	65

Exper. 5. *Reflection of the Heat of a Coal Fire by a plain Mirror.*—I placed a small speculum, such as I use with my 7-feet reflectors, on a stand, and so as to make an angle of 45 degrees with the front of it, fig. 3, pl. 12. This was afterwards to face the fire in my parlour chimney, and would make the same angle with the bars of the grate. At a distance of 3½ inches from the speculum, on the reflecting side of it, was placed the thermometer N° 1; and close by it, but out of the reach of the reflected rays, the thermometer N° 4. The whole was guarded in front, against the influence of the fire, by an oaken board 1½ inch thick, which had a circular opening of 1½ inch diameter, opposite the situation of the plain mirror, to permit the fire to shine on it. The thermometers were divided from the mirror by a wooden partition, which also had an opening in it, that the reflected rays might come from the mirror to N° 1, while N° 4 remained screened from their influence. On exposing this apparatus to the fire, I had the annexed result. Here, in

Min.	N° 1.	N° 4.
0	60	60
1	62	60
2	64	60
3	66	60
4	66	60
5	67	60½

5 minutes, the heat reflected from the plain mirror raised the thermometer N° 1, 7°; while the change in the temperature of the screened place, indicated by N° 4, amounted only to half a degree: which shows, that an open fire sends out rays that are subject to the laws of reflection, and occasion heat.

Exper. 6. *Reflection of Fire-heat by a Prism.*—Every thing remaining arranged as in the 5th experiment, I removed the small plain mirror, and placed in its stead a prism, which had one of its angles of 90 degrees, and the other 2 of 45° each, fig. 3, pl. 12. It was put so as to have one of the sides facing the fire, while the other was turned towards the thermometer: the hypotenuse consequently made an angle of 45° with the bars of the grate. The apparatus, after having been cooled some time, was exposed to the fire, and the annexed result was taken. Here, in 11^m, the rays reflected by the prism raised the thermometer 4½ degrees; but, the temperature of the place having undergone an alteration of 1¾ degrees, we can only place 2¾ to the account of reflection. The apparatus becoming now very hot, it would not have been fair to have continued the experiment for a longer time; but the effect already produced was fully sufficient to show that even a prism, which stops a great many heat-making rays, still reflects enough of them to prove, that an open fire not only sends them out, but that they are subject to every law of reflection.

Min.	N° 1.	N° 4.
0	62½	62½
1	63	62¾
2	64	63
4	64½	63
5	65	63¼
8	65¾	63½
10	66½	63¾
11	67	64¼

Exper. 7. *Reflection of Invisible Solar Heat.*—On a board of about 4 feet 6 inches long, I placed at one end, a small plain mirror, and at the other, 2 thermometers, fig. 4, pl. 12. The distance of N° 1, from the face of the mirror, was 3 feet 9¾ inches; and N° 2 was put at the side of it, facing the same way, but out of the reach of the rays that were to be reflected by the mirror. The colours of the prism were thrown on a sheet of paper having parallel lines drawn on it, at half an inch from each other. The mirror was stationed on the paper; and was adjusted in such a manner as to present its polished surface, in an angle of 45 degrees, to the incident coloured rays, by which means, they would be reflected towards the ball of the thermometer N° 1. In this arrangement, the whole apparatus might be withdrawn from the colours to any required distance, by attending to the last visible red colour, as it showed itself on the lines of the paper. When the thermometers were properly settled to the temperature of their situation, during which time the mirror had been covered, the apparatus was drawn gently away from the colours, so far as to cause the mirror, which was now open, to receive only the invisible rays of heat which lie beyond the confines of red. The result was as annexed. Here, in 10^m, the thermometer N° 1 received 4° of heat, reflected to it, in the strictest optical manner, by the plain mirror of a Newtonian telescope. The great regularity with which these invisible rays obeyed the law of reflection, was such, that Dr. Wilson's sensible ther-

Min.	N° 1.	N° 2.
0	56	56
—	57	56
—	59	56
7	60	56
10	60	56

mometer N° 2, which had been chosen on purpose for a standard, and was within an inch of the other thermometer, remained all the time without the least indication of any change of temperature that might have arisen from straggling rays, had there been any such. I now took away the mirror, but left every thing else in the situation it was. The effect of this was thus. Here, in 10^{m}, the thermometer N° 1 lost again the 4° it had acquired, while N° 2 still remained unaltered; and this becomes therefore a most decisive experiment, in proof of the existence of invisible rays, of their being subject to the laws of reflection, and of their power of occasioning heat.

Min.	N° 1.	N° 2.
0	60	56
5	58	56
8	57	56
10	56	56

Exper. 8. *Reflection and Condensation of the Invisible Solar Rays.*—I made an apparatus for placing the small steel mirror at any required angle, fig. 2, pl. 12; and having exposed it to the prismatic spectrum, so as to receive it perpendicularly, I caused the colours to fall on one half of the mirror, which, being covered by a semicircular piece of pasteboard, would stop all visible rays, so that none of them could reach the polished surface. On the pasteboard were drawn several lines, parallel to the diameter, and at the distance of $\frac{1}{10}$ of an inch from each other; that, by withdrawing the apparatus, I might have it at option to remove the last visible red to any required distance from the reflecting surface. In the focus of the mirror was placed the thermometer N° 2. I covered now also the other half of the mirror, till the thermometer had assumed the temperature of its situation. Then, withdrawing the apparatus out of the visible spectrum, till the last tinge of red was $\frac{1}{10}$ of an inch removed from the edge of the pasteboard, and the whole of the coloured image thus thrown on the semicircular cover, I opened the other half of the mirror, for the admission of invisible rays. The result was as annexed. Here, in 1 minute the thermometer rose 19 degrees. I covered the mirror. Here, in 3 minutes, the thermometer fell 16°. I opened the mirror again. Here, in 2 minutes, the thermometer rose 24°. I covered the mirror once more. And, in 1 minute, the thermometer fell 19°. Now by this alternate rising and falling of the thermometer, 3 points are clearly ascertained. The first is, that there are invisible rays of the sun. The 2d, that these rays are not only reflexible, in the manner which has been proved in the foregoing experiment, but that, by the strict laws of reflection, they are capable of being condensed. And, in the 3d place, that by condensation, their heating power is proportionally increased; for, under the circumstances of the experiment, we find that it extended so far as to be able to raise the thermometer, in 2 minutes, no less than 24°.

Min.	N° 2. In the Focus of invisible Heat.
0	61
1	80
2^{m}	72
3	67
4	64

Min.	N° 2. In the Focus of invisible Heat.
5	83
6	88
7^{m}	69

Exper. 9. *Reflection of Invisible Culinary Heat.*—I planted my little steel mirror on a small board, fig. 5, pl. 12; and at a proper distance opposite to it I erected a slip of deal, ¼ inch thick, and 1 inch broad, in a horizontal direction, so as to be of an equal height, in the middle of its thickness, with the centre of the mirror. Against the side, facing the mirror, were fixed the 2 thermometers N° 2 and N° 3, with their balls within half an inch of each other, and the scales turned the opposite way. A little of the wood was cut out of the slip, to make room for the balls to be freely exposed. That of N° 2 was in the axis of the mirror; and the ball of N° 3 was screened from the reflected rays, by a small piece of pasteboard tied to the scale. The small ivory scales of the thermometers, with the slip of wood at their back, which however was feather-edged towards the stove, intercepted some heat; but it will be seen presently that there was enough to spare. When the stove was of a good heat, I brought the apparatus to a place ready prepared for it. Here we find that, in 1 minute, the invisible culinary heat raised the thermometer N° 2, 39 degrees; while N° 3, from change of temperature, rose only 1, though its exposure to the stove was in every respect equal to that of N° 3, except so far as relates to the rays returned by the mirror; and therefore the radiant nature of these invisible rays, their power of heating bodies, and their being subject to the laws of reflection, are equally established by this experiment.

Min.	N° 2. In the Focus.	N° 3. Screened.
0	52	52
1	91	53

Exper. 10. *Reflection of the Invisible Rays of Heat of a Poker, cooled from being red-hot till it could no longer be seen in a dark Place.*—The great abundance of heat in the last experiment, would not allow of its being carried on without injury to the thermometer, the scale of which is not extensive; I therefore placed a poker, when of a proper black heat, at 12 inches from the steel mirror, fig. 1, and received the effect of its condensed rays on the thermometer N° 2, placed in the focus. Then, alternately covering and uncovering the mirror, 1 minute at a time, the effect was thus. Here, in 6^{m}, we have a repeated result of alternate elevations and depressions of the thermometer, all of which confirm the reflexibility, the radiant nature, and the heating power, of the invisible rays that came from the poker.

	Min.	N° 2.
Mirror covered	0	61
Open	1	68
Covered	2	61
Open	3	64
Covered	4	59
Open	5	61½
Covered	6	58

From these experiments it is now sufficiently evident, that in every supposed case of solar and terrestrial heat, we have traced out rays that are subject to the regular laws of reflection, and are invested with a power of heating bodies; and this independent of light. For though, in 4 cases out of 6, we had illuminating as well as heating rays, it is to be noticed that our proof goes only to the power of occasioning heat, which has been strictly ascertained by the thermometer. If it should be said, that the power of illuminating objects, of these same rays, is as strictly proved by the same experiments, I must remark, that from the cases of invisible rays brought forward in the last 4 experiments, it is evident that the conclusion, that rays must have illuminating power, because they have a power of occasioning heat, is erroneous; and, as this must be admitted, we have a right to ask for some proof of the assertion, that rays which occasion heat can ever become visible. But as we shall have an opportunity to say more of this hereafter, I proceed now to investigate the refraction of heat-making rays.

Exper. 11. *Refraction of Solar Heat.*—With a new 10-feet Newtonian telescope, the mirror of which is 24 inches in diameter of polished surface, I received the rays of the sun; and, making them pass through a day-piece with 4 lenses, I caused them to fall on the ball of the thermometer N° 3, placed in their focus. Those who are acquainted with the lines in which the principal rays and pencils move through a set of glasses, will easily conceive how artfully, in our present instance, heat was sent from one place to another. Heat crossing heat, through many intersecting courses, without jostling together, and each parcel arriving at last safely to its destined place. As soon as the rays were brought to the thermometer, it rose almost instantly from 60° to 130; and, being afraid of cracking the glasses, I turned away the telescope. Here the rays, which occasioned no less than 70 degrees of heat, had undergone 8 regular successive refractions; so that their being subject to its laws cannot be doubted.

Exper. 12. *Refraction of the Heat of a Candle.*—I placed a lens of about 1.4 inch focus, and 1.1 diameter, mounted on a small support, at a distance of 2.8 inches from a candle, fig. 6, and the thermometer N° 2, behind the lens, at an equal distance of about 2.8 inches; but which ought to be very carefully adjusted to the secondary focus of the candle. Not far from the lens, towards the candle, was a pasteboard screen, with an aperture of nearly the same size as the lens. The support of the lens had an eccentric pivot, on which it might be turned away from its place, and returned to the same situation again, at pleasure. This arrangement being made, the thermometer was for a few moments exposed to the rays of the candle, till it had assumed the temperature of its situation. Then the lens was turned on its pivot, so as to intercept the direct rays, which passed through the opening in the pasteboard screen, and to refract them to the focus, in which the thermometer was situated. Here, in 3^{m}, the thermometer received 2$\frac{1}{8}$ degrees of heat, by the refraction of the lens. The lens was now turned away. Here, in 3^{m}, the thermometer lost 2$\frac{1}{8}$ degrees of heat. The lens was now returned to its situation. And, in 3^{m}, the thermometer regained the 2$\frac{1}{8}$ degrees of heat. A greater effect may be obtained by a different arrangement of the distances. Thus, if the lens be placed at 3$\frac{1}{4}$ inches from a wax-candle, and the thermometer situated, as before, in the secondary focus, we shall be able to draw from 5 to 8 degrees of heat, according to the burning of the candle, and the accuracy of the adjustment of the thermometer to the focus. The experiment we have related shows evidently, that rays invested with a power of heating bodies, issue from a candle, and are subject to laws of refraction, nearly the same with those respecting light.

Min.	N° 2.
0	53$\frac{7}{8}$
1	55$\frac{1}{2}$
2	55$\frac{3}{4}$
3	56
0^{m}	56
1	54$\frac{5}{8}$
2	54$\frac{1}{8}$
3	53$\frac{7}{8}$
0^{m}	53$\frac{7}{8}$
1	54$\frac{3}{4}$
2	55$\frac{3}{8}$
3	56

Exper. 13. *Refraction of the Heat that accompanies the Coloured part of the Prismatic Spectrum.*—I covered a burning lens of Mr. Dollond's, which is nearly 9 inches in diameter, and very highly polished, with a piece of pasteboard, in which there was an opening of a sufficient size to admit all the coloured part of the prismatic spectrum, fig. 7. In the focus of the glass was placed the thermometer N° 3; and, when every thing was arranged properly, I covered the lens for 5 minutes, that the thermometer might assume the temperature of its situation. The result was as annexed. Here, in 1 minute, the thermometer received 112 degrees of heat, which came with the coloured part of the solar spectrum, and were refracted to a focus; so that, if the coloured rays themselves are not of a heat-making nature, they are at least accompanied with rays that have a power of heating bodies, and are subject to certain laws of refraction, which cannot differ much from those affecting light.

	Min.	N° 3.
Lens covered	0	64
Open	1	176

Exper. 14. *Refraction of the Heat of a Chimney Fire.*—I placed Mr. Dollond's lens before the clear fire of a large grate, fig. 8. Its distance from the bars of the grate was 3 feet; and, in the secondary focus of it was placed the thermometer N° 1. N° 4 was stationed, by way of standard, at 2¼ inches from the former, and at an equal distance from the fire. Before the thermometers was a slip of mahogany, which had 3 holes in it, 3/10 of an inch in diameter each. Behind the centre of the 1st hole, 3/8 of an inch from the back, was placed the thermometer N° 1; and between the 2d and 3d hole, guarded from the direct rays of the fire by the partition, at the same distance from the back, was put N° 4. Things being thus arranged, the situation of the apparatus which carried the thermometers, and that where the lens was fixed, were marked. Then the thermometers, having been taken away to be cooled, were restored to their places again, and their progress marked as annexed. Here, in 9^{m}, the rays coming from the fire, through the burning glass, gave 9¾ degrees of heat more to the thermometer N° 1, than N° 4, from change of temperature, had received behind the screen. Now to determine whether this was owing merely to a transmission of heat through the glass, or to a condensation of the rays, by the refraction of the burning lens, I took away the lens, as soon as the last observation of the thermometers was written down, and continued to take down their progress as thus. Here the direct rays of the fire, we see, could not keep up the thermometer N° 1; which lost 2¼ degrees of heat, though the lens intercepted no longer any of them. I now restored the burning glass, and continued. Here again, the lens acted as a condenser of heat, and gave 1¾ degrees of it to the thermometer N° 1. I now once more took away the lens, and continued the experiment.

	N° 1.	N° 4.
Min.	Burning Lens.	Screened.
0	58	58
1½	65	60
3	68	61
5	70	61½
7	71¼	61¾
9	71½	61¾

Min.	N° 1.	N° 4.
9½	71½	61¾
11	70½	61¾
12	70¼	61¾
—	69½	61¾
14½	69¼	61¾
15^{m}	69¼	61¾
16	69½	61¾
17	70	61¾
20	70¾	61¾
25	71	61¾
25½′	71	61¾
31	68	61¾

This again confirms the same, by loss of 3° of heat. I restored the lens once more, and had as annexed.

	N° 1.	N° 4.
Min.	Burning Lens.	Screened.
31½	68	61¾
35	69½	61¾

And here the thermometer received 1½ degree of heat again; so that, in the course of 35 minutes, the thermometer N° 1 was alternately raised and depressed 5 times, by rays which came from the chimney fire, and were subject to laws of refraction, not sensibly different from those which affect light.

Exper. 15. *Refraction of the Heat of Red-hot Iron.*—I caused a lump of iron to be forged into a cylinder of 2½ inches diameter, and 2½ inches long, fig. 9. This, being made red-hot, was stuck on an iron handle fixed on a stand, so as to present one of its circular faces to a lens placed at 2.8 inches distance; its focus being 1.4 inch, and diameter 1.1. Before the lens, at some distance, was placed a screen of wood, with a hole of an inch diameter in it, by way of limiting the object, that its image in the focus might not be larger than necessary. The screen also served to keep the heat from the thermometers. N° 2 was situated in the secondary focus of the lens; and N° 3 was placed within $\frac{3}{10}$ of an inch of it, and at the same distance from the lens as N° 2. By this arrangement, both thermometers were equally within the reach of transmitted heat; or, if there was any difference, it could only be in favour of N° 3, as being behind a part of the lens which, on account of its thinness, would stop less heat than the middle. Now, as the experiment gives a result which differs from what would have arisen from the situation of the thermometers, on a supposition of transmitted heat, we can only ascribe it to a condensation of it by the refraction of the lens; and, in this case, the thermometer N° 3, by its situation, must have been partly within the reach of the heat-image formed in the focus. During the experiment, the thermometers were alternately screened 2 minutes from the effects of the lens, and exposed to it for the same length of time; and the result was as annexed. Here, in the first and 2d minutes, N° 2 gained 2° of heat more than N° 3. In the 3d and 4th, it lost 1 more than N° 3. In the 5th and 6th, it gained 1 more. In the 7th and 8th, it lost 1½ more; and in the 9th and 10th, it gained ¾ more than the other thermometer. This plainly indicates its being acted on by refracted heat. Lest there should remain a doubt on the subject, I now removed the lens, and, putting a plain glass in the room of it, I repeated the experiment, with all the rest of the apparatus in its former situation. Here we find, that both thermometers received heat and parted with it always in equal quantities, which confirms the experiment that has been given. And thus it is evident, that there are rays issuing from red-hot iron, which are subject to

		N° 2.	N° 3.
	Min.	In the Focus.	Near the Focus.
Screened	0	56	56
Open	2	62	60
Screened	4	59	58
Open	6	61	59
Screened	8	58¼	57¾
Open	10	59½	58¼

Screened	0^m	57¼	56¾
Open	2	62¼	61¾
Screened	4	60½	60
Open	6	61	60½
Screened	8	60	59½
Open	10	60¾	60¼

laws of refraction, nearly equal to those which affect light; and that these rays are invested with a power of causing heat in bodies.

Exper. 16. *Refraction of Fire-heat, by an Instrument resembling a Telescope.*—It occurred to me, that I might use a concave mirror, to condense the heat of the fire in the grate of my chimney, and, reflecting it sideways by a plain mirror, I might afterwards bring it to a secondary focus by a double convex lens; and that, by this construction, I should have an instrument much like a Newtonian telescope, fig. 10. The thermometer would figuratively become the observer of heat, by being applied to the place where, in the real telescope of the same construction, the eye is situated to receive light. Having put together the different parts, in such a way as I supposed would answer the end, I tried the effect by a candle, in order to ascertain the proper distance of the object-mirror from the bars of the chimney-grate. The front of the apparatus was guarded by an iron plate, with a thick lining of wood; and the 2 thermometers which I used, were parted from the mirrors and lens by a partition, which screened them from the heat that was to be admitted through a proper opening in the front plate, to come at the object-mirror. In the partition was likewise an opening, of a sufficient diameter to permit the rays to come from the eye-glass to their focus, on the ball of the thermometer N° 1; while N° 4 was placed by the side of it, at less than half an inch distance. In the experiment, the object-mirror was alternately covered by a piece of pasteboard, and opened again. The thermometers were read off every minute; but, to shorten my account, I only give the last minute of every change. Here, in the first 8^m, the thermometer exposed to the effects of the fire-instrument, gained 2° of heat more than the other. In the next 8 minutes, the mirror being covered, it gained 1° less than the other. The mirror being now opened again, it gained, in 5^m, 2¾ degrees more than the other. When covered 6^m, it gained 1¼ degree less than N° 4. In the next 10^m, when open, it gained ½ degree more; and, in the last 10^m, when the fire began to fail, and the mirror was again covered, it lost 1° more than the other thermometer. All which can only be accounted for by the heat which came to the thermometer through the fire-instrument; and as this experiment confirms what has been said before of the refraction of culinary heat, so it also adds to what has already been proved of its reflection. For, in this fire-instrument, the rays which occasion heat could undergo no less than 2 reflections and 2 refractions.

		N° 1.	N° 4.
	Min.	In the Focus.	Near the Focus.
Mirror covered	0	77½	77½
Mirror open	8	84	76
Covered	16	86½	79½
Open	21	89¾	81
Covered	27	89¾	82¼
Open	37	91½	83½
Covered	47	84	77

Exper. 17. *Refraction of the Invisible Rays of Solar Heat.*—I covered half of Mr. Dollond's burning lens with pasteboard, and threw the prismatic spectrum on that cover, fig. 7; then, keeping the last visible red colour ⅒ of an inch from the margin of the pasteboard, I let the invisible rays beyond the spectrum fall on the lens. In the focus of the red rays, or a very little beyond it, I had placed the ball of the thermometer N° 1; and, as near to it as convenient, the small one N° 2. Now, that the invisible solar rays which occasion heat were accurately refracted to a focus, may be seen by the annexed account of the thermometers. Here, in 1^m, these rays gave 45° of heat to the thermometer N° 1, which received them in the focus, while the other, N° 2, suffered no change. It is remarkable, that notwithstanding I kept the red colour of the spectrum $\frac{1}{10}$ of an inch on the pasteboard, a little of that colour might still be seen on the ball of the thermometer. This occasioned a surmise, that possibly the invisible rays of the sun might become visible, if they were properly condensed; I therefore put this to the trial, as follows.

Min.	N° 1. In the Focus.	N° 2. Near the Focus.
0	57	57
1	102	57

Exper. 18. *Trial to render the Invisible Rays of the Sun Visible by Condensation.*—Leaving the arrangement of the apparatus as in the last experiment, I withdrew the lens, till the last visible red colour was $\frac{2}{10}$ of an inch from the margin of the semi-circular pasteboard cover; then, taking the thermometers, I had as annexed. Here, there was no longer the least tinge of any colour, or vestige of light, to be seen on the ball of the thermometer; so that, in 1^m, it received 21° of heat, from rays that neither were visible before, nor could be rendered so by condensation.

Min.	N° 2.	N° 3.
0	57	57
1	78	57

To account for the colour which may be seen in the focus, when the last visible red colour is less than $\frac{2}{10}$ of an inch from the margin of the pasteboard which intercepts the prismatic spectrum, we may suppose, that the imperfect refraction of a burning lens, which from its great diameter cannot bring rays to a geometrical focus, will bring some scattered ones to it, which ought not to come there. We may also admit, that the termination of a prismatic spectrum cannot be accurately ascertained, by looking at it in a room not sufficiently dark to make very faint tinges of colour visible. And, to this must be added, that the incipient red rays must actually be scattered over a considerable space, near the confines of the spectrum, on account of the breadth of the prism, the whole of which cannot bring its rays of any one colour properly together; nor can it separate the invisible rays entirely from the visible ones. For, as the red rays will be but faintly scattered in the beginning of the visible spectrum, so on the other hand will the invisible rays, separated by the parts of the prism that come next in succession, be mixed with the former red ones. Sir Isaac Newton has taken notice of some imperfect tinges or haziness, on each side of the prismatic spectrum, and mentions that he did not take them into his measures. Opt. p. 23, l. 11.

Exper. 19. *Refraction of Invisible Culinary Heat.*—There are some difficulties in this experiment; but they arise not so much from the nature of this kind of heat, as from our method of obtaining it in a detached state. A red-hot lump of iron, when cooled so far as to be no longer visible, has but a feeble stock of heat remaining, and loses it very fast. A contrivance to renew and keep this heat might certainly be made, and I should indeed have attempted to carry some method or other of this kind into execution, had not the following trials appeared to me sufficiently conclusive to render it unnecessary. Admitting, as has been proved i the 15th experiment, that the alternate rising and falling of a thermometer placed in the focus of a lens, when the ball of it is successively exposed to, or screened from, its effects, is owing to the refraction of the lens, and cannot be ascribed to a mere alternate transmission and stoppage of heat, I proceeded as follows, fig. 9. My lens, 1.4 focus, and 1.1 diameter, being placed 2.8 inches from the face of the heated cylinder of iron, the thermometer N° 2, in its focus, was alternately guarded by a small pasteboard screen put before it, and exposed to the effects of condensed heat by removing it. Now, the beginning of this experiment being exactly like that of the 15th, with the thermometer N° 3 left out, the arguments that have before proved the refraction of heat in one state, will now hold good for the whole. For here we have a regular alternate rising and falling of the thermometer, from a bright red heat of the cylinder, down to its weakest state of black heat; when the effect of the rays, condensed by the lens, exceeded but half a degree the loss of those that were stopped by it.

	Min.	N° 2.	
Screened	0	55	Very red-hot.
Open	2	63½	Red-hot.
Screened	4	58	Still red-hot.
Open	6	60½	Still red.
Screened	8	57½	A little red.
Open	10	59¼	Doubtful.
Screened	12	57½	Not visible.
Open	14	58½	
Screened	16	57½	
Open	18	58¼	
Screened	20	57¼	
Open	22	58	
Screened	24	57½	
Open	26	58	
Screened	28	57½	

Exper. 20. *Confirmation of the* 19*th.*—In order to have some additional proof, besides the uniform and uninterrupted operation of the lens in the foregoing experiment, I repeated the same, with an assistant thermometer, N° 3, placed first of all at ¾ of an inch from N° 2, and more towards the lens, but so as to be out of the converging pencil of its rays, and also to allow room for the little screen between the 2 thermometers, that N° 3 might not be covered by it. Here N° 3, being out of the reach of refraction, gradually acquired its maximum of heat, in consequence of an uniform exposure to the influence of the hot cylinder; after which it began to decline. N° 2, on the contrary, came to its maximum by alternate great elevations, and small depressions; and afterwards lost its heat by great depressions, and small elevations. After the first 8^m, I changed the place of the assistant thermometer, by putting it into a still more decisive situation; for it was now placed by the side of

		N° 2.	N° 3.
			Advanced sideways.
	Min.	In the Focus.	Always open.
Screened	0	62½	63
Open	1	63¾	64
Screened	2	62⅞	64
Open	3	64	64½
Screened	4	63¾	64½
Open	5	64⅞	64½
Screened	6	64½	64½
Open	7	64¾	64
Screened	8	64½	64

that in the focus, so as to participate of the alternate screening, and also to receive a small share of one side of the invisible heat-image, which, though unseen, we know must be formed in the focus of the lens. Here, if our reasoning be right, the assistant thermometer should be affected by alternate risings and fallings; but they should not be so considerable as those of the lens. Here the changes of the thermometer N° 2 were $-\frac{3}{4}+\frac{1}{2}-1\frac{1}{4}+\frac{3}{4}-1+1$; and those of N° 3 were $-\frac{1}{4}+\frac{1}{4}-\frac{3}{4}+\frac{1}{4}-\frac{1}{2}+\frac{3}{4}$. All which so clearly confirm the effect of the refraction of the lens, that it must now be evident that there are rays issuing from hot iron, which, though in a state of total invisibility, have a power of occasioning heat, and obey certain laws of refraction, very nearly the same with those that affect light.

		N° 2.	N° 3.
	Min.	In the Focus.	In the Edge of it.
Both open	8	64½	64
Both open	9½	63¾	63¾
Open	11	64¼	64
Screened	12½	63	63¼
Open	14	63¾	63½
Screened	16	62¾	63
Open	18	63¾	63¾

As we have now traced the rays which occasion heat, both solar and terrestrial, through all the varieties that were mentioned in the beginning of this paper, and have shown that in every state they are subject to the laws of reflection and of refraction, it will be easy to perceive that I have made good a proof of the first 3 of my propositions. For, the same experiments which have convinced us that, according to our 2d and 3d articles, heat is both reflexible and refrangible, establish also its radiant nature, and thus equally prove the first of them.

Explanation of the Figures.—Plate 12, fig. 1, shews the arrangement of the apparatus used in the 2d experiment. A is the small mirror with its adjusting screws m, n. N° 2, is the thermometer in the focus of the mirror. N° 3, the assistant thermometer. B, a small screen for the thermometer N° 2. C, the candle. D, the poker which, in the 4th and 10th experiments, is to be placed in the situation of the candle; the rest of the apparatus being brought nearer to it.

Fig. 2, shews the apparatus used in the 3d and 8th experiments. A, the mirror. N° 2, the thermometer. BCD, a desk adjustable to different altitudes. E, the prism receiving the sun's rays through an opening in the window shutter F.

Fig. 3, AB is the front of the apparatus, which in the 5th experiment, is exposed to the fire of the chimney. C, is the opening in the front plate AB, for the admission of heat. D, is the small mirror which reflects the rays of heat. E, is the hole through which the heat passes to the thermometers. N° 1 and N° 4, are the thermometers. F, is a prism, which, in the 6th experiment, is to be placed in the room of the mirror D.

Fig. 4, A, is the board that holds the apparatus used in the 7th experiment. B, the prism. C, the spectrum, thrown partly on the paper with parallel lines, and partly on one of the small tables which support the board. D, the mirror which reflects the rays of heat sideways. N° 1, the thermometer which receives the reflected rays. N° 2, the standard thermometer.

Fig. 5, AB, is the front which, in the 9th experiment, is put close to the flat side of a heated iron stove. C, is the mirror. D, the feather-edged slip of deal, on two pins. N° 2, the thermometer which receives the rays condensed in the focus of the mirror. N° 3, the standard thermometer. E, a small screen tied to N° 3, to guard it from reflected heat.

Fig. 6, A, the lens in the apparatus used for the 12th experiment. N° 2, the thermometer placed in its focus. B, the screen with an aperture for admitting the rays of heat. C, the eccentric pivot for turning away the lens. D, the candle.

Fig. 7, A, the burning lens, covered; with the prismatic spectrum thrown on an opening, left for it, in the pasteboard cover of the 13th experiment. N° 3, the thermometer placed in its focus. B, the prism. C, semicircular cover, used in the 17th and 18th experiments, instead of the one with a square hole.

Fig. 8, A, the burning lens of the 14th experiment. B, the fire in the chimney. N° 1, the thermometer in the focus of the lens. N° 4, the standard thermometer. C, the hole through which the rays of heat pass to N° 1. D and E, two holes, between which the ball of the thermometer N° 4 is screened from the direct rays of the fire; while free access is given to the heat which may affect the temperature of the place.

Fig. 9, A, the iron cylinder, stuck on its handle, as it is used in the 15th and 19th experiments. B, the lens. C, the screen with an opening in it. N° 2, the thermometer in the focus of the lens. N° 3, the standard thermometer. D, the little moveable pasteboard screen.

Fig. 10, AB, the front, plated with iron, that it may bear to be exposed close to the bars of a chimney fire. C, the concave mirror. D, the plain mirror. E, the lens. N° 1, the thermometer in the focus of the lens. N° 4, the standard thermometer. F, a circular opening in the front plate AB, for admitting the rays of heat to fall on the concave mirror C. m, the first focus of the rays, from which they go on diverging, to the small mirror, and to the lens; which brings them to a 2d focus, on the ball of the thermometer N° 1.

XVI. Chemical Experiments on Zoophytes; with some Observations on the Component Parts of Membrane. By Chas. Hatchett, Esq. F. R. S. p. 327.

The experiments and observations on shell and bone, which I last year laid before the R. S., were made in consequence of my having a little before discovered that the enamel of teeth did not consist principally of carbonate of lime, but was of a nature similar to bone; with this difference, that the phosphate of lime was not deposited in and upon a cartilaginous or membranaceous substance, but was only blended with a certain portion of animal gluten. By the experiments subsequently made on various shells, crustaceous substances, and bones, it was proved, 1st. That the porcellaneous shells resemble the enamel of teeth in the mode of formation, but that the hardening substance is carbonate of lime. 2dly. That shells composed of nacre or mother of pearl, or approaching to the nature of that substance, and also pearls, resemble bone in a considerable degree, as they consist of a gelatinous, cartilaginous, or membranaceous substance, forming a series of gradations, from a tender and scarcely perceptible jelly to membranes completely organized, in and upon which carbonate of lime is secreted and deposited, after the manner that phosphate of lime is in the bones; and therefore, as the porcellaneous shells resemble the enamel of teeth, so the shells formed of mother of pearl, &c. in like manner resemble bone; the distinguishing chemical character of the shells being carbonate of lime, and that of enamel and bones being phosphate of lime. 3dly. It was proved, that the crust which covers certain marine animals, such as crabs, lobsters, crayfish, and prawns, consists of a strong cartilage, hardened by a mixture of carbonate and phosphate of lime; and that thus these crustaceous bodies occupy a middle place between shell and bone, though they incline principally to the nature of shell. And, 4thly. That a certain portion of carbonate of lime enters the composition of bones in general; the proportion of it however being, to the phosphate of lime, vice versâ to that observed in the crustaceous marine substances. On the view therefore of these facts it is evident, that there is a great similarity in the construction of shell and bone; and that there is even an approximation in the nature of their composition, by the intermediate crustaceous substances.

These remarks, with the experiments by which they are supported, form the principal features of that paper, which was honoured with a place in the last vol. of the Phil. Trans. At that time, it was not my intention immediately to pursue the subject; but I changed this resolution, after a conversation with Dr. Gray, Sec. R. S., who suggested, that many marine substances still remained to be examined in a similar manner; and that a series of experiments on zoophytes, hitherto but little known in respect to their component parts, would be very interesting, and might probably lead to some improvement in their classification. I was therefore induced to make the experiments contained in the following pages; and as the mode adopted was very similar to that which was formerly pursued, it appears superfluous here to repeat the description. It will be proper however to observe, that argill is not unfrequently lodged, as an extraneous substance, in the interstices of many of the madrepores, and such like bodies; and, as argill is precipitated by pure ammonia, it became necessary not to rely merely on the ammonia, as a test of phosphate of lime. Whenever therefore any precipitate was produced by ammonia, it was dissolved again in acetous acid, and this solution was examined by the addition of acetite of lead.

§ 1. EXPERIMENTS ON ZOOPHYTES.

*Madrepora virginea**.—This madrepore, when immersed in very dilute nitric acid, effervesced much, and was soon dissolved. The solution was perfectly transparent and colourless, with but a small appearance of gelatinous or membranaceous particles. Pure ammonia was then added, but did not cause any alteration; and the whole of what had been dissolved, was afterwards completely precipitated by carbonate of ammonia, and proved to be carbonate of lime.

Madrepora muricata.—When treated like the former, this afforded some loose particles of a gelatinous substance: these were separated by a filter, and the solution was supersaturated with pure ammonia, without effect; but on adding carbonate of ammonia, the dissolved part was precipitated, in the state of carbonate of lime.

Madrepora labyrinthica.—This, being examined in the manner above-mentioned, proved to be composed of carbonate of lime, and of a loose gelatinous substance, similar to that afforded by madrepora muricata.

Madrepora ramea.—When this madrepore was first immersed in very dilute nitric acid, a considerable effervescence was produced; and after a few hours a pale brown fibrous membrane remained, which in some measure exhibited the original

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* The different species are named according to Gmelin's edition of Linnæus's Systema Naturæ.—Orig.

figure of the madrepore. The clear solution being poured into another vessel, only afforded a large quantity of carbonate of lime.

Madrepora fascicularis.—When this was put into very dilute nitric acid, a considerable effervescence arose; and after some hours a tender membrane was left, which retained the original shape. Pure ammonia did not disturb the transparency of this solution; but a copious precipitate of carbonate of lime was obtained, by the addition of carbonate of ammonia. These experiments, on only a few of the madrepores, sufficiently prove how similar they are, in composition, to shell; for both consist of the same materials, subject to the like modifications.

Millepora cærulea.—This produced much effervescence, when immersed in very dilute nitric acid. The blue colour disappeared, as the calcareous part was dissolved, and was not afterwards restored by ammonia. Some loose detached portions of a gelatinous substance floated in the solution, which were separated by a filter. The transparency of the solution was not disturbed by pure ammonia; but a copious precipitate of carbonate of lime was produced by carbonate of pot-ash.

Millepora alcicornis.—This millepore, when treated with very dilute nitric acid, produced a great effervescence; and after a few hours a tender gelatinous substance remained, which did not retain the figure of the millepore. Pure ammonia had not any effect; but carbonate of ammonia precipitated a large quantity of carbonate of lime.

Millepora polymorpha.—This produced an effervescence when put into dilute nitric acid; and after some hours a substance remained, which completely retained the original figure of the millepore. The substance which thus remained, was composed of a strong white opaque membrane, which formed the external part; the interior of this was filled with a transparent gelatinous substance. Ammonia produced a very slight precipitate, which, being dissolved in acetous acid, was proved to be phosphate of lime, by solution of acetite of lead. Carbonate of soda afterwards precipitated a large quantity of carbonate of lime.

Millepora cellulosa.—This millepore effervesced much with dilute nitric acid; and when this had ceased, a finely perforated membrane remained, in structure and appearance like the original substance. Ammonia did not produce any effect; but a large quantity of carbonate of lime was obtained by carbonate of soda.

Millepora fascialis.—This resembled the former in every particular; and left a membrane perfectly like the millepore.

Millepora truncata.—When treated with dilute nitric acid, it effervesced much, like the former; and after a few hours a semi-transparent membranaceous substance remained, which exhibited completely the shape and structure of the original millepore. Ammonia did not disturb the transparency of the solution; but the whole of the dissolved portion was precipitated, in the state of carbonate of lime, by carbonate of ammonia.

The remark lately made on the madrepores may here also be repeated, as the composition of the millepores appears to be the same, with the single exception of millepora polymorpha, which afforded some slight traces of phosphate of lime. But time and future experiments will show whether this was an accidental circumstance, or whether the millepore polymorpha is thus distinctly characterised. It is likewise necessary to add, that when these various madrepores and millepores were exposed to red heat, in a crucible, they emitted smoke, with the smell of burnt horn or feathers, became tinged with a paler or deeper gray colour, and when dissolved in acids deposited more or less animal coal, in proportion to the quantity of the gelatinous or membranaceous substance detected by the experiments lately described*.

Tubipora musica.—Of the tubiporæ, I had only an opportunity to examine this species. Like the former substances, it was immersed in an acid, and on this occasion I employed the acetous acid. A great effervescence was produced, and the red colour was destroyed, in proportion as the calcareous part was dissolved. When the solution was completely effected, some loose particles of a tender membrane floated in the liquor, and were separated by a filter. Pure ammonia added to the solution, produced a precipitate; which proved to be argill, accidentally lodged in the interstices of the tubipore. To the filtrated liquor, carbonate of potash was added, and precipitated a large quantity of carbonate of lime.

Flustra foliacea.—When this was immersed in very dilute nitric acid, an effervescence of short duration took place; and, when this had ceased, the flustra appeared like a finely reticulated membrane, which retained the original shape. Pure ammonia being added to the filtrated solution, produced a slight precipitate; which, being dissolved in acetous acid, was proved, by acetite of lead, to be phosphate of lime. Solution of carbonate of ammonia was then added to the liquor from which the phosphate of lime had been separated, and produced a copious precipitate of carbonate of lime. When the flustra foliacea was exposed to a low red heat, in a crucible, it emitted a smell like burnt horn, but retained its shape, by reason of the carbonate of lime with which it was coated. The flustra thus burnt, when dissolved in dilute nitric acid, deposited some animal coal; but in other respects the present solution resembled the former nitric solution of this substance when in a recent state. The flustra foliacea, when long digested with boiling distilled water, communicated to it a pale brownish tinge. Infusion of oak bark being then poured into the liquor, did not produce any visible effect, even after 24 hours had elapsed; but nitro-muriate of tin formed a white cloud, in the space of a few minutes.

Corallina Opuntia.—This being put into very dilute nitric acid, produced, like the flustra foliacea, an effervescence of short duration. The coralline then remained in a membranaceous state, and retained the original figure. To the filtrated solution some pure ammonia was added, but it scarcely produced any visible

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* The order in which these experiments are placed, is not that according to which they were made; but it has been adopted, because it shows more evidently the gradations of the membranaceous substances.—Orig.

effect. Carbonate of ammonia precipitated a large quantity of carbonate of lime. Some of the corallina opuntia was then exposed to a low red heat, in a crucible; it emitted a smell of burnt horn, and in great measure retained its shape, evidently from the calcareous coating. The burnt coralline, being dissolved in dilute nitric acid, deposited some animal coal. The clear solution afforded, by pure ammonia, a very slight precipitate of phosphate of lime; after which, the carbonate of lime was precipitated as before. This coralline, when treated with boiling water, like the flustra foliacea, did not discolour it; neither was the water changed by infusion of oak bark; but nitro-muriate of tin produced a faint white cloud.

Isis ochracea.—When this isis was immersed in dilute nitric acid, a considerable effervescence was produced; and, in proportion as the calcareous substance was dissolved, the red colouring matter was deposited, in the state of a fine red powder*. When the effervescence had ceased, which was after about 3 hours, a yellowish membrane remained, which completely retained the original figure of the isis. The solution, being filtrated, was saturated with pure ammonia by which a slight precipitate of phosphate of lime was separated. A large quantity of carbonate of lime was afterwards precipitated, by solution of carbonate of potash.

Part of a branch of this isis was put into a crucible heated to a low red heat. A great quantity of smoke was emitted, which had the smell of burnt horn; and after a few minutes the branch separated at the knotty joints, into as many pieces as there were joints in the branch. These joints had all the characters of coral; but the whole of the membrane which had invested them, as well as the knotted protuberances by which they had been connected, were destroyed, by being converted into coal. From this circumstance, I was desirous to examine the internal structure of the membranaceous part, out of which these joints of coral had been dissolved by acids.

I took, therefore, the membranaceous substance which remained after the first experiment, and which retained the complete figure of the isis. This substance being opened longitudinally, exhibited a series of cavities, corresponding in form with the coralline joints, and so situated, that each of these cavities extended from one bulb or knot nearly to the next, throughout the whole of the branch. The coralline joints, when viewed separately, appeared smaller in the middle than at the ends, which were terminated by obtuse cones. In the branch these joints were so placed, that the extremities or cones were opposed point to point; but were prevented from immediate contact, by a gristly substance, which filled, and indeed principally formed in the branch, the knot or bulb of each joint, and was interposed between the cones of the coralline substance, like the common cartilages of the articulations.

From this construction it appears, that the isis is capable of great flexibility

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* This colouring substance was not dissolved, nor changed, when nitric or muriatic acid was poured on it. It appears therefore to be very different from the tinging matter of the tubipora musica, or that of the Gorgonia nobilis.—Orig.

when in a recent state; for the gristly part of the bulbs is then most probably much softer, and more elastic, than it appears to be in the dried specimens which are found in collections*. This gristly substance which forms the bulbs, and the coralline joints, are kept together, and are covered, by a thin skin or membrane, which is continued over the whole, like a tube. The joints are not therefore devoid of a coating, as seems to be implied by the definition of Linnæus.

Isis Hippuris.—Part of a branch of the isis was immersed in very dilute nitric acid, and a considerable effervescence immediately took place. When this effervescence had ceased, there appeared little or no change in the original form of the isis; but the coralline joints were now become a soft, compact, white, and opaque membranaceous substance; while the dark brown intermediate parts retained also their form, and in other characters resembled those of horn. The solution was colourless and transparent. When saturated with pure ammonia, it was not affected; but carbonate of pot-ash produced a copious precipitate of carbonate of lime. Another part of this isis was exposed to a low red heat, in a crucible. The dark brown horny parts swelled, and puffed up, with much smoke, and a smell like that of burnt horn. The coralline joints also emitted the same smoke and smell, and became dark gray. When put into dilute nitric acid, a solution was made, with effervescence, during which, there was a copious deposition of animal coal. From this solution, nothing but carbonate of lime was obtained by the usual precipitants. A tube of membrane invests the curious structure of the isis ochracea; but no such tube or outer coat exists in the isis hippuris; for the coralline joints, like those of the madrepores, millepores, &c. consists of a membranaceous substance, hardened by carbonate of lime, and the only difference appears to be, that in the madrepores and millepores, the membranaceous part is less compact and abundant; but even the striæ of the coralline joints remain visible, and unchanged, in the membrane of this isis. The brown horny part forms also a marked characteristic in this isis, and seems to approach it to certain of the gorgoniæ. This horny part does not however pervade the whole of the branch; for where the coralline joints commence, this horny substance immediately terminates, internally as well as externally, and is not to be discovered but between or in the separation of these joints.

Gorgonia nobilis.—I next proceeded to examine the gorgonia nobilis or red coral; and of this I separately subjected to experiment different pieces, some of which were polished, and deprived of their external pale red mealy coat, while others were in their original state. A piece of the unpolished red coral being put into dilute nitric acid, an effervescence immediately took place; and after some hours the whole of the calcareous substance was completely dissolved. The external coat retained the original figure, and appeared like a pale yellow tubulated membrane,

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* It must here be observed, that the articulated structure above-mentioned, is not to be found in those parts which form the main stem, with its larger branches; the joints in those parts being consolidated, so as to constitute a strong and rigid trunk, on which the whole fabric is supported.—Orig.

the interior of which was filled with a transparent gelatinous substance. From the solution I only obtained a large quantity of carbonate of lime. The next experiment was made in a manner exactly similar to the former; but a piece of the polished or uncoated red coral was now taken. The effects produced by the diluted acid were the same as before; but in the solution some loose portions of a transparent yellow gelatinous substance were now only to be seen. The filtrated solution was treated as in the former experiment, and afforded a considerable quantity of carbonate of lime. As it was possible that the action of the nitric acid, though much diluted, might be too powerful, I was induced to try the effects of acetous acid, in which I immersed a piece of the red coral in its natural state. It was gradually dissolved, with a slow effervescence, and left an external tubulated membrane, retaining the original form, and filled with a transparent gelatinous substance, as in the first experiment. The solution, when filtrated, afforded carbonate of lime.

A piece of the polished or uncoated red coral was treated with acetous acid, in a similar manner. It was slowly dissolved, and left a transparent gelatinous substance like that which has already been mentioned, excepting that it was not in detached portions. This solution, like the former, only yielded carbonate of lime. It may here be observed, that in each of the above related experiments, the red colour of the coral was gradually destroyed, as the solution of the calcareous substance advanced, and could not afterwards by any means be restored; nor could any colouring principle whatever be detected by the re-agents usually employed.

A piece of red coral, in its natural or uncoated state, was exposed to a low red heat, in a crucible, during about 10 minutes, at which time a faint smell of burnt horn was to be perceived. When the coral was taken out of the crucible, it had completely lost the red colour, and was become pale gray. It dissolved in dilute nitric acid, with effervescence, and some animal coal was separated. To the filtrated solution pure ammonia was added, and produced a very slight precipitate, which was collected, and was afterwards dissolved in acetous acid. From this solution, by the addition of acetite of lead, some phosphate of lead was obtained. The carbonate of lime was afterwards precipitated in the usual manner. As the very small portion of phosphate of lime discovered in the preceding experiment, and which had escaped the action of the acids then employed, might be only contained in the coating or epidermis, a piece of the polished or uncoated coral was treated in a similar manner; but on examining the solution it afforded a small portion of phosphate, with a large quantity of carbonate of lime; so that the result of this experiment did not differ from that of the former.

From the preceding experiments it appears, that the gorgonia nobilis or red coral, consists of 2 parts; one of which is the stem, formed of a gelatinous substance, hardened by carbonate of lime, and coloured by some unknown modification of animal matter; the other is a membranaceous tube, which, like a cuticle or cortex, coats the stem above-mentioned, and, when deprived of its hardening substance, possesses all the characters of membrane. But though carbonate of lime could only be discovered when this gorgonia was simply immersed in acids, yet it had been proved by these experiments, that a small portion of phosphate of lime is also present, but so enveloped by the membranaceous and gelatinous parts, as not to be dissolved by the acid menstrua, till these substances have been decomposed by fire. This is not an unusual circumstance, when a very small portion of a substance is enveloped by large quantities of other matter; for Bergmann, in his supplement to Scheele's Essay on the Calculus Vesicæ, observes, that the presence of calcareous earth in certain calculi, could not be discovered in the usual manner, but by operations made expressly for the purpose. I do not pretend to determine whether the very small portion of phosphate of lime in the gorgonia nobilis is an essential ingredient or not; but the mode of construction evidently proves how much this gorgonia differs from the madrepores and millepores, as well as from the gorgoniæ about to be mentioned.

Gorgonia ceratophyta.—When this was immersed in dilute nitric acid, an effervescence was produced; after which, the cortical part appeared like a thin yellowish membrane investing the stem, which was become transparent, and similar to cartilage. Ammonia precipitated from the solution a large quantity of phosphate of lime; and lixivium of pot-ash separated some carbonate of lime. A quantity of the cortex (which had been separated from the stem by beating it between folded writing paper) was steeped in the dilute acid. This solution afterwards, with ammonia, scarcely afforded a vestige of phosphate of lime; but when lixivium of carbonate of pot-ash was added, a considerable quantity of carbonate of lime was obtained. The stem, on the contrary, when thus treated, afforded much phosphate of lime, and very little of the carbonate. When burned in a crucible, it smoked, and emitted a smell like burnt horn, but the figure was not destroyed; and, when afterwards dissolved in the acid, it yielded the same products as before.

Gorgonia flabellum.—When this gorgonia was steeped in dilute nitric acid, it produced an effervescence of a short duration. The cortical part then appeared like a thin yellowish membrane, which covered the stem. The latter was transparent, and resembled softened horn of a reddish brown colour. The solution afforded a large quantity of phosphate of lime, by the addition of ammonia; after which, lixivium of potash formed a less copious precipitate of carbonate of lime. Some parts of the cortex were separated, by beating the gorgonia between folded writing paper, and were immersed in the acid. This solution was scarcely rendered turbid by ammonia; but afforded a considerable portion of carbonate of lime by potash. The stem from which the above cortical part had been separated, was next examined by the dilute acid, in which, when steeped during 3 days, it became soft, elastic, and in some measure cartilaginous. The acid was saturated with pure ammonia, and then changed to a deep yellow or orange colour: a large quantity of phosphate of lime was at the same time separated; and but very little carbonate of lime was afterwards precipitated by potash. The recent stem in great measure retained its shape, when put into a red-hot crucible; but that which had been steeped in the acid, curled up, and soon became a shapeless mass of coal, which, by a longer continuation of the red-heat, was completely dissipated. This difference appears to have been caused by the phosphate of lime, which was present in the recent stem, but was dissolved and separated in the latter case by the acid*.

The experiments prove, that the gorgonia flabellum, like the gorgonia ceratophyta, consists of a horny stem, containing a certain portion of phosphate of lime; and that this stem is invested with a membrane, hardened principally by carbonate of lime, which serves to cover and defend it, in the manner of a shell†.

Gorgonia suberosa.—The cortical part of this gorgonia was separated from the stem, and was first subjected to experiment. Some portions of this cortex were immersed in dilute nitric acid; and after an effervescence, which continued several hours, a soft yellowish membranaceous substance remained, retaining the original figure. The liquor decanted was pale yellow, which colour was much deepened by the addition of ammonia; at the same time a small quantity of phosphate of lime was deposited. A considerable portion of carbonate of lime was afterwards precipitated by carbonate of potash. Some pieces of the cortex were boiled with distilled water for about 6 hours; and to the filtrated liquor infusion of oak bark was added, by which a large quantity of gelatin was precipitated. The same pieces were afterwards boiled with lixivium of caustic potash, which effected a perfect solution, and formed the animal soap of Chaptal; at the same time, the calcareous matter subsided to the bottom of the matrass. The cortical part of this gorgonia, when put into a red-hot crucible, emitted much smoke, with a smell like horn that is burnt; after this it fell into pieces, which, being dissolved in nitric acid, afforded a small portion of phosphate of lime, and a large quantity of carbonate of lime. When the stem of this gorgonia was steeped during 14 or 15 days in dilute nitric acid, it tinged it with pale yellow. The stem after this appeared more transparent and flexible, so as to approach the characters of cartilage. The yellow liquor was changed to a deep yellow or orange colour by the addition of ammonia; but did not yield any precipitate, even when carbonate of potash was added. Part of a stem was cut into small pieces, and was boiled for several hours with distilled water. When filtrated, the water had acquired a very pale yellow tinge; and, on the addition of infusion of oak bark, yielded a slight precipitate of gelatin. Lixivium of caustic potash was then poured on the same pieces; and being boiled, a thick dark-coloured viscid substance was formed,

[*] These different effects are to be observed, when bone, and when the cartilage or membrane which remains after bone has been long steeped in acids, are subjected to a red heat.——[†] It may here be proper to observe, that the membranaceous part of all these substances, such as the madrepores, millepores, flustra, &c. &c. was dissolved, when these bodies were boiled with lixivium of caustic potash; and animal soap was formed. The same may also be said of shells; and Mr. Van Mons has noticed this effect on those of the oyster. See Annales de Chimie, tome 31, p. 123.—Orig.

which possessed all the characters of Chaptal's animal soap. When the stem of this gorgonia was exposed to a red heat in a retort, or crucible, it curled up, and smelled like burnt horn; after which, a spongy coal remained, of difficult incineration. By a long continuation of the heat, a residuum was left, so small as scarcely to be collected, which, being dissolved in dilute nitric acid, afforded, by the addition of ammonia, a slight precipitate of phosphate of lime. Another species of gorgonia, which much resembled the suberosa, excepting that the cortical part was much larger in proportion to the stem, was next subjected to examination, and proved to be of a similar composition with those already mentioned.

Gorgonia pectinata.—The cortical part of this gorgonia effervesced with dilute nitric acid, and left a soft yellowish white membrane. Ammonia precipitated a small quantity of phosphate of lime; after which a copious precipitate of carbonate of lime was obtained by potash. The stem, in its habits, resembled those which have been described.

Gorgonia setosa.—An effervescence was produced on the immersion of this gorgonia in dilute nitric acid; and after some hours the cortical part appeared like a thin yellowish membrane, which coated the horny stem. The acid solution, on the addition of ammonia, yielded a slight precipitate of phosphate of lime; and a large quantity of carbonate of lime was afterwards obtained by potash. When the cortex was separately steeped in the acid, and the solution examined in the way so often mentioned, only carbonate of lime was obtained*. On the contrary, the stem, whether recent or burnt, afforded a small portion of phosphate of lime, but scarcely any trace of carbonate. The stem which had been long steeped in the acid, became soft and transparent, like a cartilaginous or tendinous substance.

The gorgoniæ which have been enumerated, much resemble each other in the composition of their cortices, as well as in the nature of their stems. In the cortex, the predominant hardening substance is carbonate of lime; but in the stem phosphate of lime is the chief and almost the only earthy substance that is present.

The following gorgoniæ, though in like manner invested by a cortex, are different, as they do not afford any phosphate of lime. Gorgonia umbraculum, g. verrucosa, and 3 other species not described, so much resemble each other in their chemical characters, that it would be superfluous to give a separate account of them. The cortical parts of these gorgoniæ were separately immersed in dilute nitric acid. An effervescence immediately took place, and after some time they were found in the state of soft pulpy yellowish white membranaceous bodies, retaining nearly their original size and form. The acid solutions did not afford any phosphate of lime when ammonia was added; but a large portion of carbonate of lime was precipitated by solution of potash. The stems of these gorgoniæ,

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* When the cortical part had been long digested in boiling distilled water, a brownish solution was formed, which was but little affected by infusion of oak bark; but nitro-muriate of tin produced a precipitate—Orig.

when immersed 14 days or more in the dilute acid, were very little affected, excepting that they became softer and transparent, so as to approach the characters of cartilage or softened horn*. The acid in which they had separately been steeped, did not afford any precipitate by the addition of the alkalies; and the only change was in the colour, which became deep yellow when ammonia was added.

The gorgoniæ now to be mentioned differ from the former, as they are not coated with a fleshy or pulpy cortical substance. They are here placed immediately before the antipathes, on account of their great similarity in chemical properties, as well as in external appearance.

Gorgonia antipathes.—Some pieces of this gorgonia were immersed in dilute nitric acid 3 weeks, at the end of which time they were much softened, and appeared to be composed of a pale brown opaque membranaceous substance, which formed concentrical coats, of a ligneous aspect. The acid in which these pieces had been steeped, was become pale yellow, and changed to orange colour when ammonia was added; but not the smallest precipitate could be thus obtained; nor was any alteration caused by the addition of lixivium of potash. When distilled water was boiled with the gorgonia antipathes about 6 hours, it became slightly tinged with yellow; and some infusion of oak bark being added, a small quantity of gelatin was precipitated. The pieces of this substance which had been thus treated, were afterwards boiled with lixivium of caustic potash, by which the whole was dissolved, and a very dark coloured animal soap was formed. When this gorgonia was exposed to a red heat, it emitted much smoke, with a smell of burnt horn: it soon lost its shape, puffed up, and formed a spongy coal, which, by a long continued heat, left a few particles of a white substance, consisting chiefly of muriate of soda.

Another species of gorgonia was next examined, the stem of which is from $\frac{1}{4}$ to nearly $\frac{1}{2}$ an inch in diameter in the thickest parts; it is of a black colour, and a high polish, like black sealing wax: it has probably been considered as a variety of gorgonia antipathes. This by immersion for 28 days in dilute nitric acid, gradually became semi-transparent, and of a bright brownish yellow. In this softened state, it was steeped 2 days in water, and was then opened longitudinally. By this the whole structure became apparent, and consisted of thin coats or tubes of a beautiful transparent membrane, which, beginning from a central point, progressively became larger, according to the order by which they receded from the centre. These membranes were so delicate, that the fibrous texture could scarcely be discerned. The acid in which this species had been steeped was tinged with very pale yellow. Ammonia being added, changed it to a deep yellow or orange colour; but the transparency of the liquor was not disturbed by this, or any of the other precipitants which had been employed in the former experiments. When this

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* The stems of the various gorgoniæ, which had been thus softened by long immersion in dilute nitric acid, became of a deep reddish orange colour, inclining to brown, when subsequently steeped in pure ammonia; and in the course of a few hours they were completely dissolved.—Orig.

gorgonia was exposed to a red heat, it crackled, and emitted a thick smoke, with the smell of burnt horn. The shape was soon destroyed, and a compact coal remained. By continuing the red heat, a very small portion of white matter was obtained, which, as far as the quantity would allow, was proved to be muriate of soda, with some carbonate of the same.

The last species of gorgonia which I shall here mention, is one which so much resembles the gorgonia antipathes as not easily to be distinguished from it, and, like the preceding, has probably been confounded with it; but on closely comparing them, the gorgonia now treated of is found to be more flat in the stem, on the thin sides or edges of which a number of short spines or protuberances are placed very near each other. That it is very different from the gorgonia antipathes, will be proved by the subsequent experiments. Some pieces of this gorgonia were exposed to the action of dilute nitric acid for nearly 4 weeks. The structure then became very apparent, and consisted of strong fibres, which were placed nearly in a parallel direction, from one extremity of the branch to the other, and, being closely arranged side by side, formed concentric coats of a pale brown opaque substance; but these coats were by no means so distinct as those observed in the gorgoniæ formerly mentioned, though like them the fibrous substance possessed the characters of membrane. The dilute acid in which these pieces had been steeped, was become pale yellow, which changed to orange colour when ammonia was added; at the same time so large a quantity of phosphate of lime was precipitated, that the liquor became thick and viscid. The phosphate was separated by a filter; and lixivium of potash was added to the clear liquor, without producing any effect.

This gorgonia was digested in boiling distilled water during 18 hours, and tinged it with pale yellow. Infusion of oak bark was then poured into the liquor, and precipitated a small portion of gelatin. The pieces employed in the above experiment were next boiled with lixivium of caustic potash, and formed a dark-coloured animal soap; at the same time the phosphate of lime was separated, and was gradually deposited at the bottom of the matrass. Part of a large branch of this gorgonia was exposed to a low red heat. It immediately emitted a thick smoke, with the smell of burnt horn; and after a long continued heat the phosphate of lime was left, so as to retain the original figure, like bone which has been burned; but, in the present instance, the particles of the mass cohered but feebly. When this residuum was dissolved, and the phosphate separated in the usual manner, a slight cloud of carbonate of lime was produced by potash.

Antipathes Ulex.—When this had been immersed 14 days in dilute nitric acid, it became transparent, and so much softened, that from a horny substance it now nearly resembled cartilage. The acid in which it had been steeped, changed to a very deep yellow or orange colour when ammonia was added; but no precipitate could be obtained by this, or by potash. A portion of this antipathes was digested with boiling distilled water, from which some gelatin was precipitated afterwards by infusion of oak bark. The antipathes being then boiled with lixivium of caustic potash, was completely dissolved, and formed the animal soap of Mr. Chaptal.

Antipathes myriophylla.—This was subjected to experiments like those above related, and as the effects were the same, it is not necessary that particular mention should here be made of them. As the specimens of these antipathes were small, I was not able to make any additional experiments; but what has been said sufficiently proves how much they resemble the horny stems of the gorgoniæ.

Sponges.—Many species of sponge were examined, but as little or no essential difference was found in the results, I shall include them all in what is now to be related. The following species, with many others not described, were subjected to experiment. Spongia cancellata. Sp. oculata. Sp. infundibuliformis. Sp. palmata. Sp. officinalis. When the sponges had been immersed in nitric acid, diluted with 3 measures of distilled water, during 14 or 16 days, the acid became pale yellow, and was changed to an orange colour by the addition of pure ammonia. The sponges which had been thus steeped in the dilute acid, became, like the gorgoniæ, more or less transparent, and were considerably softened. In this state, if they were touched with ammonia, the part thus touched became of a deep orange colour, inclining to a brownish red; and when much softened by the acid, if afterwards immersed in ammonia, they were completely dissolved, and formed a deep orange-coloured solution*. When digested with boiling distilled water, the sponges afforded a portion of animal jelly or gelatin, which was precipitated by infusion of oak bark. The fine and more flexible sponges yielded gelatin in greater abundance, and more easily, than those which were coarse and rigid. The gelatin was gradually and progressively imparted to the water, and seems, even in the same sponge, to be a constituent principle, of different degrees of solubility; and it must be noticed, that in proportion as the sponges, particularly those which were soft and flexible, were deprived of this substance, in the like proportion they became less flexible and more rigid, so that the remaining part, when dry, crumbled between the fingers; or, when moist, was torn easily, like wetted paper. As the above properties prove that sponges only differ from the horny stems of the gorgoniæ, and from the antipathes, by being of a finer and more closely woven texture†, so this similarity will be corroborated by the following remarks. When exposed to heat, they yielded the same products, the same smell, and afforded a similar coal, which by incineration left a very small residuum, consisting chiefly of muriate of soda, occasionally mixed with some carbonate of lime, which was also often discovered when the recent sponges were

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* The same effects were observed when the horny stems of the gorgoniæ, antipathes, &c. which had been long steeped in dilute nitric acid, were immersed in pure or caustic ammonia.——† This is particularly to be observed by comparing the coarse sponges, such as spongia cancellata, with the finely reticulated parts of certain gorgoniæ, especially those of gorgonia flabellum, when divested of the external membrane.—Orig.

immersed in acids; but this, as well as the muriate of soda, is I believe merely extraneous, and arises from small shells, parts of madrepores, and such like bodies, which are often visibly lodged in the interstices of the sponges. Lastly, the sponges, when boiled with lixivium of caustic potash, were completely dissolved, and, like the horny stems of the gorgoniæ, formed animal soap, more especially when the part which is apparently insoluble in water, and which remains after the gelatin has been separated, was thus treated.

Alcyonium asbestinum.—This, after being immersed several hours in dilute nitric acid, remained unchanged in figure; a feeble effervescence was at first produced, and the reddish purple colour was destroyed. The external part became pale yellow, and was a soft opaque pulpy substance, within which was a stem, very similar in texture, but less soft, and which still appeared of a pale red colour. When pure ammonia was added to the filtrated solution, no apparent effect was produced; but carbonate of potash precipitated a large quantity of carbonate of lime. When a piece of this alcyonium was exposed to a low red heat, it soon took fire, and emitted a smell like burnt horn; after which it retained its figure, and became white. Being dissolved in dilute nitric acid, some animal coal was deposited; and on the addition of ammonia a small portion of phosphate of lime was obtained, which being separated, the carbonate was precipitated as before. Some pieces of this alcyonium were digested with boiling distilled water for 6 hours; the liquor was then decanted, and infusion of oak bark being added, a quantity of gelatin was precipitated. On the pieces of the alcyonium from which the water had been decanted, some lixivium of caustic potash was poured; and being boiled, the whole of the membranaceous or pulpy part was dissolved, and a substance exactly similar to Chaptal's animal soap was formed, while the calcareous part subsided to the bottom of the vessel.

Alcyonium ficus.—When the effervescence produced by pouring dilute nitric acid on this alcyonium had ceased, it was found unchanged in shape, and like a strong thick membranaceous substance of a fibrous texture. Pure ammonia, added to the acid liquor, precipitated a small quantity of phosphate of lime; after which a copious precipitate of carbonate of lime was obtained by potash.

Alcyonium arboreum.—This alcyonium, being steeped in the dilute nitric acid, effervesced, and was acted on like alcyonium asbestinum. The calcareous part was soon dissolved; but the form of the alcyonium remained unchanged, and still appeared like a pale yellow porous substance, enveloped by a skin or epidermis. Ammonia did not disturb the transparency of the solution; but carbonate of lime was obtained by solution of potash.—When exposed to a low red heat, it resembled alcyonium asbestinum; and a solution being subsequently made, afforded some phosphate of lime, with a large portion of carbonate. As this phosphate had not been discovered in the first experiment, and therefore appeared to have been defended from the action of the acid by the membranaceous part, that experiment was repeated, with this difference, that the acid was made to boil. A complete solution of the whole was thus made, which, like that of the burnt alcyonium, yielded phosphate of lime; and at the same time the liquor became of an orange colour, as soon as the ammonia was added. Some pieces of this alcyonium were digested with boiling distilled water, and tinged it with a pale yellow colour. Infusion of oak bark being then added, a large quantity of gelatin was precipitated. The same pieces were boiled with lixivium of caustic potash, and when dissolved formed animal soap. The calcareous part was separated during the boiling, and subsided in the form of a fine powder. From the examination of the few species of alcyonium which have been mentioned, it appears, that as the sponges resemble the horny stems of gorgoniæ, so these, in external and chemical characters, resemble the fleshy or cortical substance which invests some of those bodies; and that they chiefly differ from the gorgoniæ, by being destitute of the horny stem, which in the latter seems to supply the place of bone.

§ II. *Observations on the foregoing Experiments.*

The simplicity and uniformity of the experiments here described, will not, I flatter myself, render the facts less worthy of attention; and I must repeat, that the minutiæ of analysis did not form part of my present plan, which was only to sketch an outline, comprehending the most prominent chemical characteristics of certain bodies appertaining to the animal kingdom, which hitherto had been but little or not at all examined; so that this outline, though defective, might serve as a chain of connection, and as a basis, on which a more perfect superstructure may in future be gradually raised; and it appeared evident, that this would be most easily and speedily executed, by following a systematical and comparative plan. For this reason, a great part of my attention was directed towards ascertaining, in these animal substances, the presence and general proportions of carbonate and phosphate of lime; these being the materials essentially employed by nature to communicate rigidity and hardness to certain parts of animals, such as shell and bone; and though some other substances, as magnesia, silex, iron, with some alkaline and neutral salts, might be occasionally present in small proportions, and indeed were at times detected, yet, as these appear to have but little influence on the general characters of the bodies examined, I did not, for the present, think proper to take particular notice of them. The next object was, to examine the nature of the substance in and upon which the hardening or ossifying principles were secreted and deposited; and it seemed that the best mode of doing this, was to compare and examine this substance in the various states in which it appeared, when deprived of the hardening or ossifying matter.

From what was said in the paper on shell and bone, concerning the substance which remained after the carbonate of lime in shells, and after the phosphate of lime in bones, had been dissolved and separated by weak acids, it is evident that the substance which thus remains, is as various in relative quantity, as it is in those qualities which apparently are produced by the degrees of natural inspissation, and by the progressive effects of organization. In the porcellaneous shells, such as cypreæ, &c. this substance was proved to be much less in quantity than in those which were afterwards mentioned; and though of a quality which, like a cement or gluten, served to bind and connect the particles of carbonate of lime firmly together, so small was the degree of natural inspissation, and so little advanced was the degree of organization, that when the carbonate of lime was dissolved, even by very feeble acids, little or no vestige of jelly, membrane, or cartilage, could be perceived; nor indeed could any be detected, but by the small portion of animal coal which was formed, when these shells had been exposed for a short time to a low red heat.

But, proceeding from shells of this description to others tending to the nature of nacre or mother of pearl, such as some of the patellæ, a substance was left untouched by the acids, which had the appearance of a yellowish transparent jelly*. So that the substance which served merely as a gluten in the porcellaneous shells, was not only more abundant in these patellæ, but, being more inspissated, was become immediately visible and palpable. In the common oyster, these qualities were more strongly marked; and in the river muscle, and in the shells composed of the true nacre or mother of pearl, this substance was found not only to constitute a large part of the shell, but even to be more dense, so as no longer to appear gelatinous; and in addition to these, strong and visible marks of organization were stamped on every part, and a perfect membranaceous body remained, composed of fibres arranged parallel to each other, according to the configuration of the shells. From these facts, proved by the examination of only a very few, comparatively speaking, of the known shells, it appears that the hardening principle, or carbonate of lime, together with a substance varying from a very attenuated gluten to a tough jelly, and from this to a perfectly organized membrane, concur to form the matter of shell; and, from the result of the experiments, and from all circumstances, there is every reason to believe, that the substance with which, or on which, the carbonate of lime is mixed or deposited, is of a similar nature, and differs only in relative quantity and density, arising from progressive changes, peculiar to the various species of shells, produced by certain degrees of natural inspissation, and by an organization more or less perfect.

The experiments made on teeth, and on the bones of various animals, elucidated and confirmed the observations made on the nature of shell; for, 1st. The enamel of teeth, in relation to the other bony substances, was proved to be as the porcellaneous shells are to those formed of mother of pearl; the cementing substance of the enamel being a gluten, in the same state, and apparently of a similar nature, with that of the porcellaneous shells. And, 2dly. In certain bones, particularly those of fish, such as some of the bones of the skate, the substance which remained after the solution of the phosphate of lime, was of a gelatinous consistency, and exhibited but very imperfect traces of organization; by the others

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* The term jelly is here employed only to denote the degree of consistency of this substance, which in its nature is very different from the varieties of animal jelly called gelatin.—Orig.

however, a completely formed membrane or cartilage was left, retaining the figure of the original bone.

When therefore the component parts of shell and bone are considered, it appears that the essential characteristics are, carbonate of lime for the one, and phosphate of lime for the other; and that their bases consist of the modifications of a glutinous, gelatinous, or membranaceous substance. I experienced much gratification in tracing the progressive and connected changes in the composition of the various shells and bones; and a considerable increase of pleasure arose, in proportion as the observations made on those bodies were corroborated, and the chain of connection extended, by the developement of the facts resulting from the experiments on zoophytes, which form the principal subject of this paper.

It will now be proper to review these experiments, and to examine how far they agree with those made on shell and bone, and how far they tend to prove, that these substances are all of a nature closely connected. The experiments on the madrepores afforded the following results. Madrepora virginea, when examined by acids, left but very little of any gelatinous substance or membrane. M. muricata, and M. labyrinthica, afforded loose portions of a transparent gelatinous substance. M. ramea, and M. fascicularis, when deprived of the carbonate of lime by acids, remained in the state of completely organized membranaceous bodies, which exhibited the original figure of the respective madrepores; and the proportion of coal afforded by these last, was more abundant than what was obtained from those which were first mentioned.

To these succeeded the experiments on the millepores; from which it appeared, that millepora cærulea afforded loose detached portions of a gelatinous substance. M. alcicornis yielded the same, but in a more coherent state. M. polymorpha remained unchanged in shape, and consisted of a strong white opaque membrane, filled with a transparent jelly. Lastly, M. cellulosa, M. fascialis, and M. truncata afforded membranaceous bodies, in a complete state of organization; and all these millepores, when exposed to a low red heat, yielded various quantities of coal, according to the greater or less abundance of the gelatinous or membranaceous substance.

The universal, and only hardening principle of these madrepores and millepores, was proved to be carbonate of lime, with the single exception of millepora polymorpha, which also appears to be differently constructed from the other milleporæ. With this single exception, carbonate of lime seems to be the only hardening substance in these bodies; and when every circumstance is considered, an exact similarity is to be found between the substance forming the various shells, and that which forms the madreporæ and milleporæ; and the nature of these bodies is so completely the same, that the changes or gradations of the one are to be found in the other. For the chemical characters which distinguish the porcellaneous shells, are in a great measure approached by those of madrepora virginea; and those which were noticed in the patellæ, correspond precisely with the madrepores and millepores which afford a gelatinous substance; and lastly, the characters of the membranaceous part, exhibited by the shells formed of nacre or mother of pearl, are in like manner to be found among some of the madrepores and millepores, such as madrepora ramea, millepora fascialis, millepora truncata; for these, like the turbo olearius and haliotis iris, are composed of a fibrous membrane, hardened by carbonate of lime. It appears therefore, that the madrepores and millepores, like the various shells, are formed of a gelatinous or membranaceous substance, hardened by carbonate of lime; and the only difference is in the mode according to which these materials have been employed.

The experiments on tubipora musica proved, that in composition it resembled the foregoing substances. But a slight difference was observed, in respect to the hardening substance of flustra foliacea and corallina opuntia; for a small portion of phosphate was found mixed with the carbonate of lime; but the membranaceous part of these bodies resembled that of certain madrepores and millepores, particularly millepora fascialis. Two species of isis were next examined, namely, isis ochracea and isis hippuris: both of these were proved to be formed of regularly organised membranaceous, cartilaginous, and horny substances, hardened, in the last mentioned species, merely by carbonate of lime; but, in the isis ochracea, with the addition of a very small portion of phosphate of lime.

The subsequent experiments were made on various species of gorgonia, and first on gorgonia nobilis, which was formerly regarded as an isis. The hardening substance of this was found to be carbonate of lime, with a small portion of phosphate; but the matter forming the membranaceous part was, like that of millepora polymorpha, in 2 states; that of the interior being gelatinous; and that of the external part being a membrane completely formed, so as to cover the stem, in the manner of a tube. The results of the experiments on certain gorgoniæ, such as ceratophyta, flabellum, suberosa, pectinata, and setosa, were not a little remarkable; for, when the 2 parts which compose these gorgoniæ, namely, the horny stem, and the cortical substance by which it is coated, were separately examined, it was proved, 1st. That the stems of these gorgoniæ consist of a substance analogous to horn; and that, by long maceration in diluted nitric acid, this horny substance becomes soft and transparent, so as to resemble a cartilaginous or tendinous body; also the stems of these gorgoniæ afford a quantity of phosphate of lime, but scarcely any trace of carbonate. 2dly. That the cortical part, on the contrary, consists principally of carbonate of lime, with very little or none of the phosphate; and the carbonate of lime is deposited in and upon a soft flexible membranaceous substance, which seems much to approach the nature of cuticle.

Some other gorgoniæ, which were subsequently examined, and which much resembled the former construction, did not yield any phosphate of lime; but in every other particular they proved to be similar. The gorgonia antipathes was found to be entirely formed of a fibrous membrane; and the black shining polished gorgonia afforded, by maceration, a most beautiful specimen of membranes concentrically arranged. Lastly, the gorgonia which I have described as very much resembling the gorgonia antipathes, proved to be similar to that species, as to the membranaceous part; but so large a portion of phosphate of lime was mixed with it, as almost to approach it to the nature of stag's or buck's horn; there is therefore great reason to consider it as a different species.

The antipathes, which were next examined, were found to be little if at all different from the horny stems of the gorgoniæ. And the various sponges, which were afterwards subjected to experiment, were proved to be completely formed by the same membranaceous or horny substance, which became varied by the modifications of a more delicate construction, rather than by any essential difference in composition. This series of experiments terminated with an examination of a few species of alcyonium, namely, asbestinum, ficus, and arboreum; all of which were found to be composed of a soft, flexible, membranaceous substance, very similar to the cortical part of some of the gorgoniæ, such as gorgonia suberosa, and in like manner slightly hardened by carbonate, mixed with a small portion of phosphate of lime.

From what has been said, there is reason to conclude, that the varieties of bone, shell, coral, and the numerous tribe of zoophytes with which the last are connected, only differ in composition by the nature and quantity of the hardening or ossifying principle, and by the state of the substance with which it is mixed, or connected. For the gluten or jelly which cements the particles of carbonate or phosphate of lime, and the membrane, cartilage, or horny substance, which serves as a basis, in and on which the ossifying matter is secreted and deposited, seem to be only modifications of the same substance, which progressively graduates, from a viscid liquid or gluten, into that gelatinous substance which has so often been noticed, and which again, by increased inspissation, and by the various and more or less perfect degrees of organic arrangement, forms the varieties of membrane, cartilage, and horn. I shall now attempt to prove what I have here asserted, or at least assign the reasons which induce me to adopt this opinion; but, in so doing, I am compelled, from the close connection of the subject, to anticipate the general result of part of a series of experiments, made with a view to investigate the nature and composition of membrane.

To enter into a minute detail of these experiments, would far exceed the limits of a paper like the present; I shall therefore only mention, in a concise manner, the results of those which the subject immediately requires to be brought forward*. The method which first presents itself in such an investigation, is, the comparative analysis of the different substances, so that their relative proportions of carbon, hydrogen, and azote, should be precisely determined; but when it is recollected how long a time would be requisite for making such an immense series of analyses, and how much animal substances are subject to be modified by situation in the

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* These are the experiments to which I alluded in my former paper, and which I began at the request of my friend Mr. Home, soon after the experiments on the enamel of teeth, &c.—Orig.

body, by age, and by the degree of health of animals, and also that the nature of these, and even that of the unorganised bodies, does not always merely depend on the proportion of the constituent principles, but also on the degree and mode of combination to which these principles are subjected; I say that when all this, and the plan of the present paper, are considered, I flatter myself that I shall not be censured as hasty or negligent, if at this time I prefer a comparison of the chemical properties of the bodies in question, with those of other substances which, though not elementary, may be regarded as primary animal compounds; and when the subject is viewed in its full extent, the mode which I have adopted will perhaps be deemed that which is the most satisfactory.

§ III. *Observations on the Component Parts of Membrane.*

In relating the preceding experiments, I have had frequent occasion to remark, that a quantity of that animal jelly which is more or less soluble in water, and which is distinguished by the name of gelatin, was obtained from many of the marine bodies, such as the sponges, the gorgoniæ, and others; but in the experiments made expressly to investigate the composition of membrane, it still more frequently occurred; and though in many cases, either from the small quantity of the body under examination, or from the very small portion of gelatin thus obtained, I was obliged to content myself with ascertaining the presence of it, by the test of the tanning principle, and by nitro-muriate of tin;* yet in other experiments, when the solutions of gelatin were gradually reduced by evaporation, I had opportunities of frequently observing the various degrees of viscidity and tenacity which characterize mucilage, size†, and glue. The difference in the viscidity and tenacity of the varieties of these substances, is evidently an inherent quality, and not caused by the degree of mere inspissation: if this was the case, mucilage, size, and glue, when dry, would be of an equal quality, which is however contrary to daily experience; for the varieties of glue are not of equal tenacity. And it is well known, that glue made from certain parts of animals, such as the skin, is more tenacious, and of a better quality, than that which is made in some places from feet and sinews.

Also, when even the same part is employed, which has been taken from 2 animals of the same species, an evident difference is found, according to the comparative age of the animals; for the best and strongest glue is always obtained from the more aged animals, in whom the fibre is found to be the most coarse and strong. But a longer continued boiling appears requisite in order to extract it; and the more viscid glues are obtained, from the substances which afford

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* Nitro-muriate of tin has been proposed as a test for the tanning principle; and the experiments contained in this paper prove, that it may also be employed with much utility, to ascertain the presence of gelatin, and of certain modifications of albumen.——† The term size is employed, throughout this paper, to denote that modification of gelatin which appears to be intermediate, between mucilage and the most viscid and tenacious gelatinous substances or glues. The weaker kinds of glue may therefore come under this denomination.—Orig.

them, with greater difficulty than those of a less viscid quality, which may more properly be called size: this difference is to be observed, when muscle is boiled with repeated and frequent changes of water. Gelatin thus obtained, whether in the state of mucilage, size, or glue, when completely dried, is affected by water according to its degree of viscidity: for, when cold water is poured on dry mucilage, it dissolves it in a short time; but if cold water be poured on those varieties of gelatin which, when dissolved in a proper quantity of boiling water, would, by cooling, form jellies more or less stiff, it acts on them in different degrees, not so much by forming a complete solution, as by causing them to swell and become soft; so that, when a cake of glue has been steeped 3 or 4 days in cold water, if it swells much without being dissolved, and, when taken out, recovers its original figure and hardness, by drying, such glue is considered to be of the best quality.

I shall soon have occasion to notice, in another place, the effects of acids and of alkalies on gelatin; it will therefore here be sufficient to observe, that as it is soluble in acids, so, if dry mucilage, dry size*, and dry glue, be steeped in nitric acid diluted with 3 or 4 parts of water, they will be progressively dissolved, according to the degree of viscidity by which they are separately distinguished. When the solutions of these substances in water were examined by the tanning principle, and by nitro-muriate of tin, I have found that animal mucilage is more immediately affected by the latter than by the former; while the solutions of size and of glue are equally acted on by both. And when gold dissolved in nitro-muriatic acid was added to the solutions of mucilage, size, and glue, the gold was reduced to the metallic state in a few hours, not only on the surface, where it formed a shining metallic pellicle, but also on the sides of the glass, which were thinly coated with a deep yellow sediment, which, like leaf-gold, appeared of a fine pale green, when held between the eye and the light.

The animal mucilage which I chiefly employed in these experiments, was obtained from the corallina officinalis, as I found it to be pure, and not partly modified into gelatin or animal jelly†. But Mr. Bouvier asserts, that he obtained the latter substance‡; and this appears to me very probable; for mucilage may predominate in this coralline at one period, and gelatin or jelly at another, just as it is found to be the case with other animal substances; for it is known, that in young animals mucilage is abundant, and becomes diminished as these increase in growth and age. Hence there is every reason to conclude, that the substance which in very young animals was at first mucilaginous, becomes progressively

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* Gelatin obtained from eel-skin, evaporated to dryness.——† By this I mean, that the mucilage had not acquired the degree of viscidity requisite to form a gelatinous substance. The expression which I have employed, is not therefore to be understood as alluding to any essential difference in composition, but only to denote some variation in the degree of consistency; for the whole may be comprehended under the term gelatin, of which, mucilage may be regarded as one extreme, and the strongest and most viscid glue as the other.——‡ Annales de Chimie. tom. 8. p. 311.—Orig.

more viscid, and assumes the characters of gelatin; which, as animals increase in age, is known to become more and more viscid, as has been already mentioned in the foregoing pages. I am inclined therefore to consider mucilage as the most attenuated, and as the lowest in order, among the modifications of gelatin.

As the qualities of gelatin are so various, so the properties of the substances in which it is present as a component part, are much influenced by it; and when, for example, the skins of different animals were compared, I have always found that the most flexible skins afforded gelatin more easily, and of a less viscid quality, than those which were less flexible, and of a more horny consistency. The skin of the eel possesses great flexibility; and it affords gelatin very readily, and in a large proportion. The skin of the shark also, which is commonly used by cabinet-makers to polish their work, was in like manner, for the greater part, soon dissolved, and formed a jelly, like the former. The epidermis or cuticle of these skins, which is very thin and tender, though not soluble, was reduced into small particles by violent ebullition, and the spicula on the shark's skin were also separated. The skins of the hare, rabbit, calf, ox, and rhinoceros, were examined in a similar manner, and with the like results; but the gelatin obtained from the hide of the rhinoceros, as far as the smallness of the piece of skin would allow me to determine, appeared to be the strongest and most viscid. In every one of these experiments, the true skin or cutis was principally affected, it being completely soluble, as Messrs. Chaptal and Seguin have well observed, by long boiling; but that of the rhinoceros far exceeded the others in difficult solubility. The cutis of these skins, when first boiled, swelled and appeared horny; it was then gradually dissolved; but in the cutis of the rhinoceros a few small filaments remained, which at length contracted and adhered to the cuticle.

The cuticle of the different skins was softened, but not dissolved; and, as the cutis seems to be essentially formed of gelatin,* so the cuticle appears to contain it, though but in a small proportion: it is however necessary to its flexibility; for when, after long boiling, the cuticle of these skins was dried, it became a brittle substance, which was easily reduced to a powder. Hair was much less affected than either of the above-mentioned substances; and this, with others in some measure similar, I shall now more particularly notice. The substances to which I allude, are hair, feather, horn, horny scale, hoof, nail, and the horn-like crust which covers some insects and other animals, such as the scorpion and the tortoise. These I shall now mention, in as concise a manner as the subject will allow. When hair of various qualities, and taken from different animals, was long digested or boiled with distilled water, it imparted to the water a small portion of gelatin, which was precipitated by the tanning principle, and by nitromuriate of tin; and when the hair had been thus deprived of gelatin, and was subsequently dried in the air, the original flexibility and elasticity of it were found

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* The cartilages of the articulations are also completely soluble when long boiled with water; but this by no means happens when other cartilages are thus treated.—Orig.

to be much diminished, so that it easily gave way, and was broken. This effect, Mr. Achard has also noticed;* and I am induced to believe, from various experiments which I have made on these substances, that the hair which loses its curl in moist weather, and which is the softest and most flexible, is that which most readily yields gelatin; and, on the contrary, the hair which is very strong and elastic, is that which affords it with the greatest difficulty, and in the smallest proportion. These remarks have also been corroborated, by the assertion of a considerable hair merchant in this metropolis†, who, during a long experience of upwards of 40 years, has always found, that hair of the first named quality cannot be boiled an equal time with those last mentioned, without suffering material injury in strength and flexibility.

Feather, digested in boiling distilled water, during 10 or 12 days, did not afford any trace of gelatin by the test of the tanning principle; but nitro-muriate of tin produced a faint white cloud. The same was observed when quill was thus examined. Shavings and pieces of the horns of different animals were next subjected to experiment, and all afforded small quantities of gelatin, which was precipitated by the tanning principle, and by nitro-muriate of tin; and it was generally observed, that the more flexible horns yielded the largest quantity of gelatin, with the greatest ease; and, like the substances already mentioned, when deprived of it, and suffered to dry spontaneously in the air, they became more rigid, and were easily broken. The horns which I mean, are those of the ox, ram, goat, and chamois, which, in my former paper I considered, as I do now, to be perfectly distinct from the nature of stag's or buck's horn; for this last is as different from the former in chemical composition, as it is in construction: like bone, it affords much phosphate of lime, and, like bone, it affords a large quantity of gelatin; and it is not a little remarkable, that phosphate of lime is generally accompanied by gelatin, as in stag's horn, bone, ivory, &c. on the contrary, when carbonate of lime is the hardening substance, as in shells, madrepores, and millepores, no gelatin can be discovered; for I have frequently digested these substances many days in boiling distilled water, after having reduced them to a coarse powder that they might present a larger surface, but I never could by any test discover the slightest vestige of gelatin. The horns therefore which were first mentioned, are very different from the composition of stag's horn, and yield gradually, and with great difficulty, only a small quantity of gelatin.

Horny scale was next examined; but I shall first here make a digression in respect to the scales of fish, which I had not examined when my paper on shell and bone was read. As the scales of fish, when viewed by a microscope, and according to the observations of Mr. Leeuwenhoek, appear to be formed of dif-

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* "La perte de la partie gélatineuse ôtant aux cheveux leur souplesse, il s'ensuit que c'est aux "parties gélatineuses qui entrent dans la composition des cheveux qu'ils doivent leur pliant et leur élasticité."—Examen chimique des Cheveux, &c. Mem. de l'Acad. de Berlin. tom. 38. p. 12.——† John Collick, Esq. of St. Martin's-lane.—Orig.

ferent membranaceous laminæ, and as they exhibit the colour and lustre of mother of pearl, it might be expected, that they should prove to be of a similar nature with the substance of stratified shells, or in other terms, that they should consist of membrane and carbonate of lime. But when scales perfectly clean, and separated from the skin of different fish, such as the salmon and carp, had been immersed during 4 or 5 hours in diluted nitric acid, till they became transparent, and perfectly membranaceous; the acid liquor, being then saturated with pure ammonia, afforded a copious precipitate, which was proved to be phosphate of lime. The spiculæ of the shark's skin, formerly mentioned, were found to be of a similar composition; and we may therefore regard the spicula and scales of fish as true bony substances, in which the membranaceous part is more predominant than in common bone. I fully ascertained, that the phosphate of lime was afforded by these substances only; for when the different skins from which these scales and spicula had been taken, were separately examined in the like manner, no phosphate of lime was obtained. In addition to this I must observe, that the silver or pearly hue of pearl, mother of pearl, and of fish-scales, is only assisted and modified by the relative degrees of opacity produced, in mother of pearl and in pearl, by the interposition of the particles of carbonate of lime, and in the scales by phosphate of lime; for this peculiar lustre principally resides in the membranaceous part, and remains with it when the acetous or muriatic acids are employed as menstrua, but is completely destroyed by the nitric acid.

The horny scales of serpents, lizards, and such like animals, differ from the foregoing; as all of those which I have examined, consist merely of the membranaceous or horny substance, in a more or less indurated state, and appear to be devoid of phosphate of lime, as an ossifying matter. Horny scales in general, and the scales of the manis pentadactyla may be mentioned as an example, afford but very slight traces of gelatin after being long boiled in distilled water; and this small portion of gelatin can only be discovered by the tanning principle, and by nitro-muriate of tin, unless a very large quantity of the scales has been employed. Human nail digested in boiling distilled water during several days, was only softened; and, like quill, afforded a slight cloud, by the addition of nitro-muriate of tin. Shavings of ox's hoof, when long digested as above-mentioned, afforded a liquor which, in like manner, was only made slightly turbid by nitro-muriate of tin. Nail and hoof, when long boiled, became of a much darker colour.

The horn-like crust which covers certain insects and other animals, was subsequently examined; the experiments were principally made on the plates which covered the body of a large African scorpion, and on the common tortoise-shell of the shops. The plates taken from the scorpion were not apparently affected, though digested for a long time in boiling distilled water. The tanning principle produced no alteration, when added to the water; but a faint white cloud appeared, on the addition of nitro-muriate of tin. Tortoise-shell, in thin slips, and shavings, was digested in a similar manner during 3 weeks; but it was only slightly softened and the water, which had acquired a brownish colour, was but little affected, even by nitro-muriate of tin, which however formed a white cloud*.

From some previous circumstances, which need not here be mentioned, I was lastly induced to make some similar experiments on albumen; and as that of the blood is mixed with gelatin, and with the substance called fibrin by the chemists, which in chemical properties appears to be the same as muscular fibre, and as it is with some difficulty that the albumen can be exactly separated from these substances, I preferred the albumen of eggs, as being pure and unmixed; and, in order that it might be brought into a state in some measure similar to the bodies lately examined, by which I mean simple inspissation, I dried it, after coagulation, in a vessel which was heated to 212° of Fahrenheit, till it became perfectly hard, brittle, yellow, and semi-transparent, like horn. The albumen, in this state, was digested 8 days in boiling distilled water, which was occasionally renewed, in proportion to the evaporation. In a few hours after the commencement of the digestion, the transparent horny pieces of albumen were softened, and became white and opaque, exactly like albumen recently coagulated; but, after this, no further change was observed. At the end of 8 days, the water in which the albumen had been digested was examined, and was found exactly to resemble that afforded by quill, nail, and tortoise-shell; for the transparency of it was not disturbed by the tanning principle, though nitro-muriate of tin produced a faint white cloud†. As far therefore as could be ascertained, by long digestion in boiling distilled water, and by the effects of the re-agents, albumen was proved to be very similar to tortoise-shell, and many of the other substances previously noticed; but the close resemblance, or rather indeed identity, of albumen with those bodies, will be placed in a stronger light, by the enumeration and comparison of their other chemical properties. As I have, in the former part of this paper, had occasion to mention the gelatin obtained from the sponges and gorgoniæ, it is not necessary here to repeat those remarks, neither is it requisite that I should enter into any minute account concerning the experiments made on bladder, and some other membranes. It may therefore suffice here to observe, that all these bodies afforded more or less gelatin; that when this was separated, the remaining substance ceased to be tough, or elastic, and was easily torn, like wetted paper; and that when dry, the sponges, and such membranes as bladder and cuticle, became very brittle, and were shrivelled and curled up, like withered leaves of plants.

But, before speaking of the nature of the substance which thus remained, it will be proper, concisely to notice the effects of acids on the bodies which afford

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* The crust which covers insects like the scorpion, appears in every respect to be similar to tortoise-shell.——† When infusion of oak-bark is added to recent liquid albumen, it immediately forms a precipitate; and nitro-muriate of tin does not produce an effect till some hours have elapsed. But after coagulation the reverse takes place; for the water in which coagulated albumen has been long boiled, becomes turbid by the addition of nitro-muriate of tin; and is not in any manner affected by infusion of bark.—Orig.

gelatin; and as the most remarkable effects were produced by nitric acid, I shall to that confine the present observations. The specific gravity of the nitric acid which I employed in the whole of my experiments, was 1.38; and this acid was diluted with 2, 3, or 4 measures of distilled water, according to the quality of the substance under examination, and the intended time of immersion. But as an acid too powerful would have frustrated my intentions, I commonly added the acid, by degrees, and at long intervals, to the water in which the substance was immersed; during which time if any nitrous gas was discharged, more water was added, as this gas was a certain sign that the acid was not sufficiently diluted.

Substances like the corallina officinalis, which contain a large quantity of animal mucilage, or of the least viscid jelly, soon impart it to boiling water. In like manner, when such substances were steeped in nitric acid diluted with about 3 measures of water, the mucilage was in a few hours completely dissolved, while the membranaceous part remained untouched. Pure isinglass dissolved in the dilute nitric acid, formed a pale yellow liquor, which by evaporation became of a deeper colour, and when nearly dry was suddenly reduced to a spongy coal. This change was rapid; and was attended with a considerable effervescence, and a copious discharge of nitrous gas, not unfrequently accompanied by sparks, and sometimes flame; arising undoubtedly from nitrate of ammonia, which was formed towards the end of the evaporation. The acid solutions of mucilage, isinglass, and pure glue, were changed to a deeper yellow, when saturated by the absorbent earths, by the alkalies, and epecially by pure ammonia. In such cases, little or no precipitate was obtained from pure gelatinous substances; but some faint traces of phosphoric acid were discovered in these solutions.

The effects of the dilute nitric acid on the other various substances which have been mentioned, resembled those now described, and kept pace exactly with those of boiling water; for when they were immersed in equal quantities of the dilute acid during a given time, the solution of the gelatin took place according to the order observed in those substances, when water was employed. As an instance of this, 2 pieces of skin, recently taken from an ox, were subjected to experiment, as follows: one of the pieces was boiled in water, till the whole of the cutis was dissolved; after which, the cuticle remained, though very feeble in texture, while the hair did not seem to have suffered any material alteration. The other piece was steeped in nitric acid diluted with about 4 measures of distilled water. At the end of 5 days the cutis was dissolved, and the cuticle was become of a loose and feeble texture; but the hair had not suffered any apparent change, except that of being slightly tinged with yellow. In both cases therefore the effects of boiling water and of acid were similar, and were evidently most powerful on those parts which were the most gelatinous.

As water dissolves mucilage more speedily than size, and this last more readily than strong viscid glue, so are the effects of very dilute nitric acid on the same substances; and when equal quantities of dried mucilage, of eel-skin glue, and of the strongest glue, were dissolved in equal quantities of the dilute acid, the colour of the solutions was more intense, and the change produced by ammonia was more visible, according to the order of solubility and of tenacity. It is well known how readily gelatin is dissolved by the caustic fixed alkalies: when therefore the varieties of jelly or glue were added to boiling lixivium of caustic potash, they were soon dissolved; and when added to saturation, a brownish viscid substance was formed. I did not observe that any ammonia was produced, neither was any coal deposited, after long boiling the solution in which there was an excess of alkali. The viscid matter thus obtained, did not possess the properties of animal soap; for it neither formed a permanent lather, when mixed and shaken with water; nor, when saturated with acids, did it afford any precipitate; contrary to what happens, when animal soap is thus treated. But, when the gelatinous substance was not pure; if, for example, any parts of membrane, which are not soluble in water, were present, then, in proportion to the quantity of this substance, the alkaline solution exhibited more or less of the saponaceous characters; but these I never observed when pure gelatin was employed.

Gelatin, according to its quantity and quality, has a powerful influence on some of the physical and chemical properties of the bodies in which it is present; by these properties, I mean flexibility, elasticity, and putrescibility. So much has been said already, in various parts of this paper, tending to prove how much the degrees of flexibility and elasticity, in various animal substances, depend on their gelatinous part, that little need be added; and when it is considered that bodies, such as muscular fibre, membrane, sponge, hair, and cuticle, being deprived of gelatin, and dried in the air, become rigid and brittle, no doubt can be entertained but that this arises from the loss of the gelatinous substance; and, as an additional proof, when bodies, such as nail, feather, quill, and tortoise-shell, which contain little or no gelatin, are long boiled, and then dried in the air, like the former, they are found to have suffered scarcely any alteration in their respective degrees of flexibility and elasticity.

As to putrefaction, it is obvious to every one, that certain parts of animals are much more susceptible of it than others; and that when the carcase of an animal begins to putrefy, the most humid and flexible parts are always first affected. Thus, the viscera, muscles, and cutis, soon suffer a change; while hair, feather, scale, horn, hoof, and nail, remain unchanged, ages after the former have been decomposed; and this is evidently caused by the gelatin and moisture, which are combined in the former, and not in the latter, at least in any notable quantity. I have already mentioned the progressive and comparative effects of boiling water, and of dilute nitric acid, on the skin of the ox; and I have showed, that while the cutis was completely dissolved, the hair remained untouched. These effects are to be observed, in the same exact order, when a similar piece of skin is exposed to putrefaction; for this commences in, and chiefly affects, the cutis, while the hair is separated, unchanged in its quality. I do not therefore hesitate to assert, that the degree of putrescibility in the various parts of animals, depends principally on the presence, and on the quantity and quality, of gelatin; and the skin of the rhinoceros found on the banks of the Vilui, near Yakutsk, was preserved, in all probability, partly by the nature of the climate and soil, and partly by the superior horny quality which it possessed over other skins; for it may be much questioned, whether the hide of an ox or horse, in the same situation, would have escaped putrefaction for so long a period*.

From the preceding observations it appears, that gelatin is a component part of many animal substances. That it differs in quality, from a very attenuated jelly or mucilage, to that viscid substance called glue; the varieties of which also differ in solubility and tenacity. That it is present in various proportions; so that certain bodies, such as the cutis, and the cartilages of the joints, are formed by it; while others, like nail, quill, and tortoise-shell, can scarcely be said to contain it. And that, by its presence, in various states and proportions, it may be regarded, including inherent moisture and organic arrangement, as the principal cause of those degrees of flexibility, of elasticity, and of putrescibility, so various in the different parts of animals†. But, when gelatin has been separated from the different substances, either by repeated boiling with water, or by being steeped in dilute acids, a more insoluble substance remains, of a very different nature, which I shall now proceed to examine. When a bone or piece of ivory has, by long boiling in water, been deprived of a great part of its gelatin, and is afterwards steeped in a dilute acid, the ossifying substance is dissolved, and the cartilage remains, retaining the figure of the original bone; or, if a similar bone or piece of ivory, which has not

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* The more viscid gelatinous substances do not appear to be so immediately susceptible of putrefaction as those of the opposite quality; for, when solutions in water of animal mucilage, eel-skin glue, and strong glue, were during a certain time exposed under equal circumstances, I found the mucilage to be the first, and the glue the last, which showed symptoms of putrefaction.——† As gelatin, according to its proportion and quality, appears to produce considerable effects on the parts of animals in which it is present: and, as the gelatin in animal bodies is, in all probability, liable to be changed and modified by morbid causes, it is much to be wished, that gentlemen of the medical profession would ascertain, by experiments, how far the tonic properties of barks depend on the tanning principle. Mr. Biggin has proved, (Phil. Trans. for 1799, p. 259,) that willow bark, and especially that of the Huntingdon or Leicester willow, contains the tanning matter in a considerable quantity; and that the latter, in this respect, even equals, or rather exceeds, that of oak. My friend, the Rev. Thomas Rackett, Rector of Spetisbury and Charlton, in Dorsetshire, has employed, in these parishes, the bark of the common willow with great success, as a tonic and febrifuge. Also, Mr. Westring, of Norrköping, has observed, (Annales de Chimie, tom. 32, p. 179,) that those species of cinchona which contain the tanning principle in the greatest quantity, are the most efficacious in fevers; and that the cinchona floribunda, which contains scarcely any tanning matter, is destitute of the above-mentioned beneficial effects. Mr. Westring therefore, with great apparent reason, believes that the relative effects produced by the different species of cinchona, when employed in medicine, are in proportion to their tanning power, or the quantity of tanning principle contained in them. If any one should be induced to make experiments on the tonic effects of the tanning principle, it is to be hoped that some attention would also be paid to the medicinal properties of nitro-muriate of tin, of which, at present, I believe little or nothing is known.—Orig.

been boiled, is steeped in a dilute acid, (especially nitric acid,) the ossifying substance is dissolved, and, at the same time, but more slowly, the gelatin is separated, and causes the liquor to become yellow, when the phosphate of lime is precipitated by ammonia. The cartilaginous body which remains, after the gelatin has been thus separated, is not easily soluble in dilute acids, for, according to its texture, many weeks, and even months, may elapse, before a small part is taken up; but in concentrated nitric acid, or in boiling dilute acid, it is rapidly dissolved. This substance, when dry, is semi-transparent, like horn, and more or less brittle. It is the predominant and essential part, in the tissue or web of membrane, cartilage, sponge, the horny stems of gorgoniæ, horn, hair, feather, quill, hoof, nail, horny scale, crust, and tortoise-shell; and, though of similar chemical properties, yet in consistency it varies, from a tender jelly-like substance, to a completely formed membrane, or to an elastic, brittle, and hard body, like tortoise-shell*.

Experiments were made separately, on each of the bodies above enumerated; but as I did not find any essential difference in the results, I shall include the whole under the following observations. 1. When distilled, a small portion of water, some carbonate of ammonia, a fœtid empyreumatic oil, carbonated hydrogen gas, carbonic acid gas, and prussic acid, were obtained. 2. A spongy coal, of a gray metallic lustre, remained: this, by incineration, afforded a very small residuum, which was not always similar in quantity, even in portions of the same substance; for, 500 gr. of tortoise-shell, taken from different samples, afforded from $\frac{1}{4}$ of a gr. to 3 gr. of residuum, which consisted of phosphate of lime, and phosphate of soda; sometimes also a little carbonate of lime was present; but I do not believe these to be essential ingredients. 3. When boiled many days in distilled water, the substance was softened; and the water became slightly turbid with nitro-muriate of tin; but no effect was produced by the tanning principle. 4. Muriatic and sulphuric acids had little effect, unless heated; and the same was the case with nitric acid much diluted, or in the state proper to extract and separate gelatin; but if the immersion in the dilute acid was continued during some weeks, the acid gradually acquired a yellow tinge, and, when saturated with ammonia, became of a deeper colour, without having its transparency disturbed. 5. The substance which had thus been long steeped in the acid, was much softened, was become more transparent, and, from being horny, was now more like a cartilaginous substance: when taken out of the acid, if it was immediately steeped in pure ammonia, it changed to a deep orange colour, inclining to blood red; it was gradually and silently dissolved, without any residuum, and a deep orange or yellowish brown coloured liquor was formed.

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* These bodies, especially tortoise-shell, appear to be formed, as far as organic arrangement is concerned, in the way of stratum super stratum. This structure is peculiarly to be discovered after long maceration in diluted nitric acid; for then, tortoise-shell appears to be composed, like the black polished gorgonia, of membranaceous laminæ; and the varieties of horn differ only by a tendency to the fibrous organization.—Orig.

6. Or, when taken out of the acid, if it was first well washed in distilled water, and then boiled, it was also dissolved, and formed a pale yellowish solution: this, by evaporation and cooling, became a jelly, which was again soluble in boiling water; and was precipitated, like gelatin, by the tanning principle and more slowly by nitro-muriate of tin. 7. If the nitric acid in which the substance was immersed was not sufficiently diluted, or if heat was applied, the whole was rapidly dissolved, with a considerable effervescence, and discharge of nitrous gas. 8. This solution was yellow, like the former, the colour being intense, in proportion to the quantity dissolved; and it was also changed to a deep orange or yellowish brown by the addition of ammonia, without depositing any precipitate, unless a large quantity had been dissolved. 9. The nitric solutions of this substance, when evaporated, afforded much the same appearances as those of gelatin, but the coal which remained was less spongy. 10. This substance, whether of sponge, horn, quill, hair, nail, or tortoise-shell, &c. was strongly distinguished from gelatin, by the effects produced when boiled with caustic fixed alkali; for animal soaps were formed, exactly similar in every property excepting colour, and the whole of the original substance was completely dissolved. 11. During the process, a considerable quantity of ammonia was discharged; and, if the alkali was in excess, some coal was deposited.

12. When the animal soap was dissolved, diluted with distilled water, and filtrated, if an acid, such as the acetous or muriatic, was added, a copious precipitate was obtained, which was re-dissolved by an excess of acid. 13. This precipitate, being collected on a filter, appeared at first like a yellow or brownish viscid substance, which, when dry, was like a thick coat of varnish, or dried white of egg, and in like manner was brittle, and broke with a glossy fracture. 14. It burned like quill or tortoise-shell, leaving a spongy coal; and, when distilled, afforded products like those obtained from the bodies above-mentioned. 15. It was not readily soluble in dilute acids; and was acted on by nitric acid and ammonia, like the substances from which it had been obtained; the properties also of its solutions in nitric acid and ammonia were similar. 16. With caustic lixivium of potash it readily combined, and again formed animal soap. 17. It was not quite so insoluble in boiling water as quill or tortoise-shell; and the water in which it had been boiled was not only made turbid by nitro-muriate of tin, but yielded a precipitate when infusion of oak-bark was added, after the manner of gelatin.

These experiments proved, that this precipitate was the same as the original substance from which it had been obtained; and that the only change it had suffered, was that of being rendered rather more soluble in boiling water. The whole series of experiments on the various bodies lately enumerated, convinced me also, by the similarity of results, that they essentially consisted of one and the same substance, modified in texture by the degrees of organic arrangement and by the occasional presence, and different proportion and quality, of gelatin. But the difference in chemical properties showed, that this last mentioned substance, gelatin, was quite distinct from that which is now under examination; and, from the resemblance of certain effects observed when quill and tortoise-shell were compared with inspissated albumen, by being long digested in boiling water, I was induced to make a series of comparative experiments on albumen, similar in every respect to those which have been so lately described, of which the following are the results. 1. By distillation, the coagulated dry semi-transparent albumen, afforded products exactly similar to those obtained from tortoise-shell, and the other substances which have just been examined. 2. A spongy coal remained, of very difficult incineration; as towards the end of the process it appeared vitrified, and glazed with a melted saline coat, which was however easily dissolved by water. The residuum was again exposed to a long continued red heat, and again treated with water, till at length a few scarcely visible particles remained; which, as far as such a small quantity would permit to be ascertained, proved to be phosphate of lime. The portion dissolved by water, which was by much the most considerable, consisted principally of carbonate, mixed with a small quantity of phosphate of soda. 3. When steeped in dilute nitric acid, it was not soon affected; but, after about 4 weeks, the acid began to be tinged with yellow, which gradually became deeper in the course of 3 months; the albumen, however, though less transparent, was but little diminished. The yellow acid solution, when saturated with ammonia, changed to a deep orange colour, and remained transparent.

4. The albumen which had been thus steeped in the dilute nitric acid, was immediately immersed in ammonia; which changed it to a deep olive colour, inclining to a blood red, and gradually dissolved it, without any apparent residuum. This solution is of a deep yellowish brown.

5. If the albumen, instead of being immersed in ammonia, was washed, and then boiled with distilled water, it was dissolved, and formed a pale yellow liquor, which, by evaporation, formed a gelatinous mass; this, being dissolved again in boiling water, was, like gelatin, precipitated by the tanning principle, and more slowly by nitro-muriate of tin. 6. By concentrated nitric acid, or by the dilute acid when heated, albumen was speedily dissolved, with much effervescence, and a copious discharge of nitrous gas. 7. This solution was like that of tortoise-shell, and the other substances mentioned in the former experiments. 8. When evaporated, it afforded similar results. 9. Albumen, like tortoise-shell, quill, and nail, was dissolved by boiling lixivium of caustic potash, and formed animal soap. 10. In like manner also, a considerable portion of ammonia was discharged; and, if the alkali was in excess, some coal was deposited. 11. The animal soap obtained from albumen, when dissolved in water, yielded a precipitate, by the addition of acetous or muriatic acid; and the precipitate was re-dissolved when the acid was added to excess.

12. This precipitate, when collected on a filter, appeared more saponaceous, and less viscid, than that obtained from the substances lately examined*. When gently heated, some oil flowed from it; after which a brownish viscid substance remained, similar in its properties to that which was obtained from the animal soap made by tortoise-shell and the other bodies†. 13. It may not be improper here to repeat, that inspissated albumen, long boiled with distilled water, was not apparently diminished; but the water, like that in which tortoise-shell, quill, or nail, has been boiled, had acquired the property of becoming white and turbid, when nitro-muriate of tin was added, though it was not changed by the tanning principle. To this also may be added, that the water in which tortoise-shell, nail, and albumen, had been boiled, became in some measure putrid in a few days, and emitted an offensive smell.

I am not inclined however, to regard this as a proof that any gelatin had been separated from these bodies by means of boiling water, but rather that inspissated albumen, tortoise-shell, &c. are substances really soluble, though in so slight a degree as to approach insolubility; and that thus the prevalent opinion has arisen concerning the insolubility of coagulated albumen in boiling water. Neither is the putrefaction of the water in which the bodies abovementioned have been boiled, a proof that any other than their real substance has been dissolved; for this putrefaction appears to depend on its attenuated and diluted state, more than on any other cause. Tortoise-shell, nail, quill, and similar bodies, certainly are not liable to putrefaction; neither is albumen, when in the inspissated semi-transparent state. This last substance also, when merely coagulated, does not easily putrefy; for I kept it, when it was soft, white, and coagulated, in water, during the whole of the month of April, without finding that it became really putrid; towards the latter part of the time, it had rather a disagreeable smell; still however it was far from being absolutely putrid.

But albumen which has not been coagulated, or which has been diluted and shaken with a quantity of cold water, begins in a very few days to be putrid; liquid albumen therefore enters easily into putrefaction, though it is the reverse with that which is dense and solid: and from a comparison of the preceding experiments on tortoise-shell, quill, nail, &c. with those made on albumen, I am induced to believe that the former bodies are essentially composed of albumen, modified by the various effects of organization, and reduced to a state of density far exceeding that which is produced by simple inspissation. And though the

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[*] This precipitate, when obtained from different substances, such as hair, wool, and muscular fibre, appeared in some cases more, and in others less viscid, though similar in every other property. It will be proper also to observe, that the saponaceous solutions were always filtrated, before the addition of the acids.——[†] The yolk of eggs, when boiled with caustic lixivium of potash, forms a pale olive-coloured concrete animal soap, which, when dissolved in water, and saturated with muriatic acid, is precipitated in the state of mere fat. Yolk of egg, by incineration, affords a small portion of phosphate of soda and of lime.

bodies, which of late have been particularly mentioned, appear to consist principally of albumen, with sometimes the addition of gelatin in different proportions, yet, as in certain membranes and such like substances, portions of muscular fibre were at times found joined or interwoven; and as muscle, ligament, and tendon, seem to glide almost imperceptibly into each other, I was almost unavoidably induced to make some experiments on muscular fibre. The muscular fibre on which the greater part of these experiments was made, was that of beef; and in order to separate the liquid albuminous part or lymph as much as possible, a quantity of lean muscle of ox beef, cut into small thin pieces, was macerated 15 days in cold water, and was subjected to pressure each day, when the water was changed. The weather was very cold; and the maceration was continued to the end of the 15th day, without any sign of putrescency. The shreds of muscle, amounting to about 3 lb., were then boiled with about 6 quarts of water, during 5 hours; and, the water being changed, the same was repeated every day, for 3 weeks; at the end of which time, the water afforded only slight signs of gelatin, when infusion of oak-bark, or nitro-muriate of tin, was added. After this the fibrous part was well pressed, and was dried by the heat of a water bath.

Some of the muscular fibre thus prepared, was steeped in nitric acid diluted with 3 measures of water, for 15 days. The acid acquired a yellow tinge, and possessed all the properties of the nitric solutions of albumen. The fibre which had been thus steeped in the acid, was, when washed, dissolved by boiling water, and by evaporation became a gelatinous mass: which, being again dissolved in boiling water, was precipitated by infusion of oak-bark, and, more slowly, by nitro-muriate of tin, like the albuminous substances, when treated in a similar manner. When the fibre which had been steeped in the acid was immersed in ammonia, it was not completely dissolved, like albumen, but afforded a residuum, which will soon be noticed. The greater part was, however thus dissolved; and formed a deep orange or yellowish brown solution, similar in properties to that of albumen. When boiled with lixivium of caustic potash, this muscular fibre was completely dissolved; ammonia was discharged, and animal soap was formed; which being diluted with water, and saturated with muriatic acid yielded a precipitate, similar in every property to that which had been obtained from the animal soaps formerly mentioned, excepting that it sooner became hard and glossy, when exposed to the air*. Muscular fibre, when prepared as already mentioned, so as, by long maceration and subsequent boiling with frequent change of water, to be

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* In respect to economical purposes, it may be proper here to observe, that all animal substances whatever, exclusive of carbonate and phosphate of lime, may be converted into 2 substances of much utility, namely, glue, under which term I include all the varieties mentioned in this paper, and soap, with the additional advantage, that those parts which would be rejected in making the one, are the most proper to prepare the other. The offensive smell of Chaptal's soap is considered as an objection; but this may be removed, by exposing the soap for some time, in flat vessels, to the air; after which, it may be reduced to the proper degree of consistency, by a second boiling.—Orig.

very nearly deprived of the whole of its gelatinous part, is not easily brought into the putrid state. A small quantity was kept moistened with water, during the whole of last April; in the course of which time it acquired a musty but not a putrid smell; neither were the fibres reduced to a pulpy mass*. I am inclined therefore to suspect, that strong and completely formed muscular fibre, considered as a distinct substance, is not of easy putrescibility; and that the readiness with which muscle in general enters into putrefaction, is principally owing to the gelatin, which is combined and mixed with it, in a large proportion, as a component part, and which, with the natural quantity of moisture, is requisite to give the fibre a proper degree of toughness and flexibility. The residuum afforded by muscular fibre which had been long steeped in dilute nitric acid, and afterwards immersed in ammonia, consisted chiefly of fat, mixed with a small portion of the fibre which had not been sufficiently acted on by the acid; and little or no earthy matter was thus obtained. But when the prepared muscular fibre was dissolved in boiling nitric acid, a complete solution, resembling that of albumen, in its general properties, was formed; and some fat floated in drops at the top of the liquor. Ammonia was then added, so as to super-saturate the acid, and produced the same effects as on the nitric solutions of albumen, excepting that a copious white precipitate was obtained. This precipitate, while moist, was agitated with a quantity of acetous acid, which dissolved, and separated, a small portion of phosphate of lime; but the remainder, and by much the greatest part of this precipitate, was scarcely attacked, even when the acid was boiled. When exposed to a red heat it became dark gray, and then nearly white; after which it was in the state of carbonate of lime.

Another part was dissolved in nitric acid, and lime was precipitated by carbonate of soda. The slight excess of the latter was then saturated by acetous acid; and the whole was boiled, to expel the carbonic acid; after which the liquor, from its effects on solutions of lime, barytes, &c. evidently contained oxalic acid in solution: the precipitate was therefore oxalate of lime, mixed with a very small quantity of phosphate of lime. 200 gr. of the dry muscular fibre, dissolved and boiled with nitric acid, afforded 17 gr. of this precipitate. Though it is known that the gelatinous liquor obtained from muscle by boiling water, contains phosphate of soda, and of lime, yet I did not imagine that the greater part of the latter could be so completely separated. I therefore in some measure repeated the experiment on the muscle of veal; and found phosphate of soda, and of lime, in the liquor. But when the muscle was afterwards dissolved in boiling nitric acid, and the solution was saturated with ammonia, I was surprized to find that, though the same change in colour was produced as in all the former experiments, the liquor remained transparent; and even after several days only a few scattered particles appeared at the bottom of the vessel. Another experiment was made on the recent

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* A portion of this muscular fibre was kept under water 2 months; it did not however become putrid, nor was it converted into that fatty substance which is obtained from recent muscle under similar circumstances.—Orig.

muscle of mutton; but this was immediately dissolved in nitric acid, without being previously boiled in water. The fat being separated, the solution was, as before, saturated with ammonia; and, as usual, became of a deep orange colour, or yellowish brown: in a few hours also, a small quantity of a white precipitate subsided. This precipitate however was completely and readily soluble in acetous acid: and in every respect proved to be phosphate of lime.

Before proceeding it will be proper to observe, that the liquor from which the above precipitate was separated, as well as those afforded by the muscle of veal, by the prepared muscle of beef, by the solutions of tortoise-shell and of albumen, in boiling nitric acid, subsequently saturated by ammonia, all contained a considerable portion of uncombined oxalic acid, which was separated by acetite of lime, and of lead. But I did not find oxalic acid in the solutions formed by immersing these bodies, for a long time, in cold and dilute nitric acid; neither did I find oxalic acid in solutions made by dissolving these substances in boiling muriatic acid. It is evident therefore, that the oxalic acid observed in the above experiments, was a product of the operations, and not an educt of the substances.

We may conclude, from the experiments on the muscular substances which have been lately mentioned, that they contain lime, in various proportions, and in 2 different states, viz. carbonate and phosphate; and that the greater part of the latter is gradually separated, in conjunction with the gelatin, by means of boiling water. I would not however have it understood that phosphate of lime is an essential ingredient in gelatinous substances: for, on the contrary, isinglass, which is a perfectly gelatinous body, affords but a mere visible trace of it. The muscular fibre of beef appears to have been nearly deprived of its phosphate of lime, by the long continued and repeated boiling in water to which it had been subjected; but still so large a quantity of lime remained, that when oxalic acid was formed by the action of the boiling nitric acid, it combined with the lime, and formed an oxalate, which amounted to 17 gr., from 200 gr. of the dry muscular fibre, dissolved in nitric acid, and precipitated by ammonia.

I do not know what quantity of lime was separated with the gelatin, as I was then only intent on preparing the fibrous part of the muscle; but, from the quantity of lime which remained, and which afterwards combined with the oxalic acid, it is evident, that in the muscle of beef there is a considerable portion of earthy matter; and as, by the experiment on the muscle of veal, scarcely any precipitate was obtained after it had been boiled, and as but a small portion of phosphate of lime was present in the gelatinous liquid, it appears that in this muscle the whole of the small portion of lime which it contained was in the state of phosphate; and this being nearly separated, there did not remain any part of uncombined lime, or carbonate of lime, which, by uniting with the oxalic acid, subsequently produced, would form an oxalate; and as lime, in the states of phosphate and carbonate, is so much more abundant in the muscle of beef than in that of veal, we may infer, that the earthy matter is more abundant in the coarse and rigid fibre of adult and aged animals, than in the tender fibre of those which are young; and this seems to be corroborated by the tendency to morbid ossification, so frequently observed in aged individuals of the human species.

Gelatin, albumen, and muscular fibre, not only differ very much from each other by the relative quantity of their saline or earthy residua, but also by the proportion of one of their essential and elementary principles, namely carbon. 500 gr. of isinglass, made perfectly dry by distillation, yielded 56 gr. of coal, from which, 1.50 gr. of earthy residuum, obtained by incineration, being deducted, the proportion of coal appears to have been 54.50 gr. 500 gr. of dry albumen afforded 74.50 gr.; and as the saline residuum amounted to 11.25 gr., the quantity of mere coal was 63.25 gr. 500 gr. of tortoise-shell yielded 80 gr. of coal; from which 3 gr. of earthy matter being deducted, 77 gr. remain, for the proportion of coal. And 500 gr. of the dry prepared muscular fibre of beef, when distilled, left 108 gr. of coal, which, by incineration, afforded 25.60 gr. of earthy residuum; the coal may therefore be estimated at 82.40 gr. There appears much reason therefore to believe, that the gelatinous substances and muscular fibre, differ from simple and unorganized albumen, by a diminution of the carbonic principle in the one, and by an excess of it in the other; and as, in vegetables, the fibrous part is that which contains the largest proportion of carbon, so, in respect to the other animal substances, muscular fibre appears to contain the greatest quantity of it.

The nature of the residua obtained by the incineration of the substances lately mentioned, also deserves to be noticed. Only 1.50 gr. was obtained from 500 gr. of isinglass; and, as far as the quantity would allow, was proved to be phosphate of soda, mixed with a very minute proportion of phosphate of lime. The 3 gr. afforded by tortoise-shell, consisted of phosphate of soda and of lime, with some traces of iron: it is probable that the latter was accidentally present. The prepared muscular fibre of beef yielded 25.60 gr.; the greatest part of which was carbonate of lime, mixed with some pure lime, and a small portion of phosphate; there can be no doubt but that the latter would have been more abundant, had it not been for the repeated boilings to which the muscular fibre had been subjected. The recent muscles of veal and mutton were with great difficulty reduced to ashes; for, towards the end of the process, the ashes and remaining coal became coated and glazed with saline matter, which appeared to be soda, partly in the state of phosphate; and it is not a little remarkable that the 11.25 gr., obtained from albumen, consisted chiefly of soda, in a caustic state, by reason of the long continued heat, mixed with a small quantity of phosphate of soda, and a very minute portion of phosphate of lime. Pure albumen therefore, which has not been subjected to the effects of organization, appears to contain a considerable portion of saline matter, and very little of any earthy substance; but the contrary seems to happen in bodies which, though evidently derived from albumen, have suffered various changes by the action of the vital principle; which may be considered as the cause of organization, by which these bodies are differently modified, according to the nature of the parts of animals which, singly or conjointly, they are employed to form. In these bodies the quantity of the saline substances appears to be diminished, while that of the earthy matter is increased, especially in the coarser kinds of muscular fibre.

On a comparison of the chemical properties of the substance which remains, after the separation of gelatin from the great variety of animal substances which have been so often mentioned in the course of this paper, and which need not therefore now be repeated, there can scarcely be any doubt but that it is one and the same substance, in different states of density and texture. For the similarity of its nature was proved by, 1st. The effects of fire, and the products obtained by distillation. 2dly. Its very difficult solubility by long digestion in boiling water. 3dly. The effects produced by re-agents, on the water in which bodies like inspissated albumen or tortoise-shell had been boiled. 4thly. The effects of acids, particularly nitric acid, of ammonia, and of caustic lixivium of potash. 5thly. The animal soap which was formed; and the precipitate obtained from it, by the addition of acetous or muriatic acid*. 6thly. The difficulty attending the putrefaction of the substance in question, when pure and dense. The similarity in all these properties, appears to be a full proof, that it is the same substance which constitutes the principal part of membrane, sponge, horn, hair, &c. and even of muscular fibre.

Besides, on comparing the properties of this substance with those of pure albumen in a state of inspissation, so evident a resemblance in every respect is discovered, that few I believe will hesitate to pronounce albumen to be the original substance from which tortoise-shell, hair, horn, muscular fibre, &c. have been derived and formed. There is much reason to believe that gelatin, though it appears so different in many respects from albumen, is yet formed from it†.

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* This appears to be a strong marked character of the albuminous substances.——† In addition to the chemical properties by which gelatin and albumen are distinguished, particularly the different effects observed when these 2 substances were treated with nitric acid, I shall mention some others, not less remarkable, which are produced by the muriatic acid. When any of the varieties of gelatin, such as glue, isinglass, &c. are immersed in cold muriatic acid, they are dissolved in a few hours; and the solutions thus formed suffer no apparent change, even in the course of several months. In like manner, gelatin may be separated and dissolved from bodies which contain it, such as sponge, bladder, skin, and muscle; but the part which remains undissolved, and which, with the other substances formerly mentioned, I regard as formed of albumen more or less organized, is very differently affected; for when coagulated albumen, the undissolved part of bladder, muscular fibre, feather, quill, tortoise-shell, wool, and hair, were separately steeped in muriatic acid, they gradually became of a dark colour, and the acid was tinged with the same. The colour afforded by albumen was deep blue, inclining to purple; that of bladder was brownish purple; feather, quill, tortoise-shell, and muscular fibre, afforded a beautiful deep blue; and wool, and hair, like bladder, produced a brownish purple. The change began to take place in the coagulated albumen in about 8 or 10 days; but wool and hair were the last affected. In about 3 months, the different liquids were become very dark, though scarcely any perceptible quantity of the immersed substances appeared to be dissolved. Nitric acid, in a small proportion, changed these blue and brownish purple liquors to deep yellow; and ammonia, being then added, changed them to orange colour, and produced all those effects which were observed, when the nitric solutions of these substances were thus treated.—Orig.

It may be recollected, that in a former part of this paper mention was made, that tortoise-shell, horn, muscular fibre, and inspissated albumen, after long immersion in very dilute nitric acid, and after being well washed, were soluble in boiling water; and that a substance was formed, which, by becoming liquified when heated, by being soluble in boiling water, by being precipitated by the tanning principle and by nitro-muriate of tin, and lastly, by forming a gelatinous mass when the aqueous solution was sufficiently evaporated and cooled, approached and resembled gelatin. It would be perhaps too hasty to assert, that gelatin was thus absolutely formed; but if a substance so very similar to it could be thus produced, we may with some reason conclude that the real gelatin, with its various modifications, is formed from albumen, by the more efficacious and delicate operations of nature.

In attempting to prove that albumen or the coagulating lymph is the original animal substance, I have hitherto only stated chemical facts; but when the phenomena attending incubation are considered; when the experiments made by eminent physiologists, such as Haller, Maitre Jean, and Malpighi, are viewed; when the oviparous fœtus is seen to be progressively formed in and from the albumen of the egg, so that, on the bursting of the shell which separated it from external matter, the young animal comes forth complete in all its parts; when such strong facts as these are corroborated by those afforded by chemistry, it can scarcely be doubted that albumen is the primary animal substance, from which the others are derived; and there is much cause to believe that the formation of gelatin, and of the animal fibre especially, begins with the process of sanguification in the fœtus.

As the 3 principal and essential component parts of the blood, viz. albumen, gelatin, and fibre*, appear therefore to compose the various parts of animals, in such a manner that one, being predominant, influences the nature of that part of the animal which it is principally employed to form; and as albumen, gelatin, and fibre, by relative proportion, by the degrees of density, by the effects of organization which singly or conjointly they have experienced, by the texture of the animal substance which they, as materials and thus modified, have concurred to produce, and by the proportion of natural or inherent moisture peculiar to each part of different animals, present an immense series of complicated causes; so are the effects found to be no less numerous and diversified, by the infinite variety in texture, flexibility, elasticity, and the many other properties peculiar to the various parts which compose the bodies of animals.

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* The whole of the blood, which by anatomists is divided into serum, red globules, and coagulating lymph, when chemically examined, is found to consist of albumen, gelatin, and fibre. The serum which remains liquid after the coagulation of the blood, is composed of albumen, gelatin, some saline matter, and much water. The clot or crassamentum also affords, by repeated washing, a large proportion of albumen and gelatin; after which a substance remains, in appearance very analogous to muscular fibre, excepting that it is in a more attenuated state. This substance, called fibrin by chemists, may be regarded as that part of the blood which has undergone the most complete animalization; and from which the muscular fibre and other organs of the body are formed.—Orig.

XVII. *On the Electricity excited by the mere Contact of Conducting Substances of different kinds. By Mr. Alex. Volta, F. R. S. Prof. of Nat. Philos. in the University of Pavia. From the French.* p. 403.

The chief of these results, and which comprehends nearly all the others, is the construction of an apparatus which resembles in its effects, viz. (such as giving shocks to the arms, &c.) the Leyden phial, and still better electric batteries weakly charged; acting continually, or whose charge, after each explosion, recharges itself again; which in short becomes perpetual, from one infallible charge, from one action or impulse on the electric fluid; but which besides differs essentially from the other, by this continual action which is proper to it, and because that instead of consisting, like the ordinary phials and electric batteries, in one or more isolated plates, or thin layers of those bodies deemed the only electrics, and armed with conductors or bodies called non-electrics, this new apparatus is formed only of a number of these last bodies, chosen even among the best conductors, and so the farthest removed, according to the usual opinion, from the electric principle. This astonishing apparatus is nothing but an assemblage of a number of good conductors of a different kind, arranged in a certain manner. Thus, 30, 40, 60, or more pieces of copper, or better of silver, each applied to a piece of tin, or still much better of zinc; and an equal number of layers of water, or of some other liquid which may be a better conductor than simple water, as salt water, lye, &c. or of bits of card, or leather, &c. soaked in such liquids. Of such layers, interposed between each couple or combination of 2 different metals, one such alternate series, and always in the same order, of these 3 kinds of conductors, is all that constitutes M. Volta's new instrument; which imitates so well the effects of the Leyden phial or electric batteries; not indeed with the force and explosions of these, when highly charged; but only equalling the effects of a battery charged to a very weak degree, of a battery however having an immense capacity, but which besides infinitely surpasses the virtue and the power of these same batteries; as it has no need, like them, of being charged before hand, by means of a foreign electricity; and as it is capable of giving the usual commotion as often as ever it is properly touched. This apparatus, as it resembles more the natural electric organ of the torpedo, or of the electric eel, than the Leyden phial and the ordinary electric batteries, M. Volta calls the artificial electric organ. For the construction of this instrument, M. V. provides some dozens of small round metal plates, of copper, or tin, or best of silver, about an inch in diameter, like shillings or half-crowns, and an equal number of plates of tin, or much better of zinc, of the same shape and size: these pieces he places exactly one upon another, forming a column, pillar, or pile. He provides also as many small round pieces of card, or leather, or such like spongy matter, capable of imbibing and retaining much of the water, or other liquid, when soaked in it. These soaked roullets or circles are to be a little less in diameter than the small metal discs or plates, that they may not jut out beyond them. All these discs are then placed horizontally on a table, one over another continually alternating, in a pile as high as it will well support itself without tottering and falling down; beginning with a plate of either of the metals, as for instance the silver, then upon that one of zinc, over which is to be put the soaked card; then other 3 disks, over these in the same order, viz. a silver, next a zinc, and then another moistened card, &c.

After having raised the pile to about 20 of these stages or triads of plates, it will be already capable, not only of affecting Cavallo's electrometer, assisted by the condenser, so as to raise it 10 or 15°, charging it by a simple touching, so as to cause it to give a spark, &c. as also to strike the fingers with which we touch the top or bottom of the column, with several small snaps, the fingers being wetted with water. But if to the 20 sets of triplets of the plates be added 20 or 30 more, disposed in the same order, the actions of the extended pile will be much stronger, and be felt through the arms up to the shoulders; and by continuing the touchings, the pains in the hands become insupportable.

Mr. V. constructs and combines his apparatus in various ways and forms, more or less powerful, convenient, or amusing. One is as follows, fig. 1, pl. 13, which he calls à couronne de tasses. He disposes in a row a number of cups, of wood, or earth, or glass, or any thing but metal, half filled with pure water, or salt water, or lye; these are all made to communicate in a kind of chain, by several metallic arcs, of which one arm or link Aa, or only the extremity A, immersed in one of the cups, is of copper, or of copper silvered, and the other z, immersed in the following cup, is of tin, or rather of zinc, the 2 being soldered together near the crown of the arch. It is evident that a series of these cups, thus connected together, either in a straight or curved line, by the 2 metals and the intermediate liquid, is similar to the pillar or pile before described, and consequently will exhibit similar effects. Thus, to produce the commotion or sensation in the hands and arms, we need only dip one hand into one of the cups, and a finger of the other hand into another cup, sufficiently distant from the former; then the action will be so much the stronger as the 2 cups are farther asunder, or have the more intermediate cups; and consequently the greatest by touching the first and the last in the chain.

As to the structure in the other method, by the column or pile, Mr. V. found out various ways to prolong and extend it, in multiplying the metal plates without shaking it down; to render this instrument convenient, portable, and durable; and, among others, the 3 methods exhibited on figs. 2, 3, 4, pl. 13. In fig. 2, m m m m, are upright bars or rods, to the number of 3, or 4, or more, erected from the bottom of the pile, and extended to a convenient height, inclosing the pile like a cage, to prevent its falling. These rods may be either of glass, wood, or metal; only, in this last case, they must be hindered from touching the metal plates; which may be done, either by covering each metal rod with a glass tube, or by interposing between them and the pile some bands of cerecloth, or oiled paper, or simple paper, or any imperfect conducting substance. But the best expedient for forming the instrument of a great number of plates, as of 60, 80, 100, is to divide the pile into 2 or more, as in the figures 3 and 4; where the pieces have all their respective positions or communications, as if it was one pile only, plied and turned. In all these figures, the different metal plates are denoted by the letters A and Z, the initials of argent and zinc, and of the wet discs of card, or leather, &c. interposed at each pair of those metals, by a layer or band shaded black. The dotted lines show the contact of each couple of the metal plates A and Z, where they may be conveniently soldered together, cc, cc, cc, are metal plates forming the communication between one column, or section of a column, and another; and b, b, b, b, b, are basins of water, in communication with the bottoms or extremities of the piles.

Mr. Volta concludes with various remarks and cautions in using this instrument; showing that it is perpetual in its virtue, renewing its charge spontaneously, and serving most of the purposes of the ordinary electrical machines, and even affecting and manifesting its power by most of the human senses, viz. feeling, tasting, hearing, and seeing. *(See a Note at the end of this Volume.)*

XVIII. Some Observations on the Head of the Ornithorhynchus Paradoxus. By Everard Home, Esq. F. R. S. p. 432.

The specimens of this extraordinary animal which have been sent to Europe, have been deprived of the internal parts, and the skins are mostly dried, and but badly preserved. Such imperfect specimens have raised the curiosity of the naturalist, and excited the ardor of the anatomist, without satisfying their inquiries. It was natural, under these circumstances, to reserve any observations which had been made on this newly discovered quadruped, till the entire animal should be brought home preserved in spirit, and enable us to examine the structure of its different organs; but finding that Professor Blumenbach has been led to believe that it was an animal without teeth, an opinion which must have arisen from the imperfect state of the specimen he examined, it appeared highly proper to do away the mistake, and lay before the R. S. such observations respecting the head of this extraordinary animal, as I have been enabled to make. My opportunities of examining the Ornithorhynchus were procured through Sir Jos. Banks; who permitted me to have drawings made from the skin of one of a very large size, and which, from having been preserved in spirit, was more perfect than any of the dried specimens. Any general description of the beak of this animal, which is its most conspicuous peculiarity, becomes unnecessary, as the accompanying drawings will give a sufficiently correct idea of the outward appearances, to answer the present purpose. It was not permitted to examine the head anatomically; but a smaller dried specimen, received from Sir Jos. Banks, furnished me with the following observations.

The beak of the Ornithorhynchus, when cursorily examined, appears so strongly to resemble that of the duck, as to lead to the belief of its being calculated for exactly the same purposes; it will however be found to differ materially from it, in a variety of circumstances. The beak is found, on examination, not to be the animal's mouth, but a part added to the mouth, and projecting beyond it. The cavity of the mouth is situated as in other quadrupeds, and has 2 grinding teeth on each side, both in the upper and lower jaw; but, instead of incisor teeth, the nasal and palate bones are continued forwards, lengthening the anterior nostrils, and forming the upper part of the beak; and the 2 portions of the lower jaw, instead of terminating at the symphysis, where they join, become 2 thin plates, and are continued forwards, forming the under portion of the beak. This structure differs materially from the bill of the duck, and indeed from the bills of all birds, since in them, the cavities of the nostrils do not extend beyond the root of the bill; and, in their lower portions, which correspond to the under jaw of quadrupeds, the edges are hard, to answer the purpose of teeth, and the middle space is hollow, to receive the tongue. But in this animal the 2 thin plates of bone are in the centre; and the parts which surround them are composed of skin and membrane, in which a muscular structure probably is enclosed.

The teeth have no fangs which sink into the jaw, as in most quadrupeds, but are imbedded in the gum; and have only lateral alveolar processes, from the outer and inner edges of the jaw, to secure them in their places, but no transverse ones between the 2 teeth. The tongue is extremely short, not half an inch long; and the moveable portion not more than a quarter of an inch; the papillæ on its surface are long, and of a conical form. When the tongue is drawn in, it can be brought entirely into the mouth; and when extended can be projected about a quarter of an inch into the beak. The organ of smell in this animal, differs in some particulars from that of the quadrupeds in general, as well as of birds. The external openings of this organ are placed nearly at the end of the beak, there being only the lip beyond them, while the turbinated bones are in the same relative situation to the other parts of the skull as in quadrupeds; by which means, there are 2 cavities the whole length of the beak, superadded to the organ of smell. The turbinated bones in each nostril are 2 in number, and are distinct from each other. That next the beak is the longest, has a more variegated surface than in the duck, and has the long axis in the direction of the nostril; the posterior one is short, projects farther into the nostril, and the ridges are in a transverse direction. The posterior nostrils do not open directly under the turbinated bones, as in the duck, but about an inch farther back, and are extremely small; the cavities of the nose, in this animal, are therefore uncommonly extensive; they reach from the end of the beak nearly to the occiput.

The beak itself is formed by the projecting bones already mentioned, covered with a smooth black skin, which extends some way beyond the bones, both in front and laterally, forming a moveable lip. This lip is so strong, that when dried or hardened in spirit, it seems to be rigid; but when moistened is very pliant, and has probably a muscular structure. The under portion of the beak has a lip equally broad with the upper: this has a serrated edge; but the serræ are confined to the soft part, not extending to the membrane covering the bone, and are not met with in the upper one. The extent of the lips beyond the bones, is distinctly marked in the drawings. There is a very curious transverse fold of the external black smooth skin, by which the beak is covered, projecting all round, exactly at that part where the beak has its origin. Its apparent use seems to be to prevent the beak being pushed farther into the soft mud, in which its prey may lie concealed, than up to this part, which is so broad that it must completely stop its progress. The nerves that supply the beak, in their general course, size, and number, seem very closely to correspond with those of the bill of the duck.

The cavity of the skull bears a greater general resemblance to that of the duck than of quadrupeds: there is a very uncommon peculiarity in it, which is, that there is a bony falx of some breadth, but no bony tentorium. This is met with in no quadruped that I know of: it is found in a small degree in some birds, as the spoon-bill, and the parrot; but not at all so as to resemble the falx in this animal. The orifice of the eye lids is uncommonly small, for the size of the animal; but the eye itself was not in a state to be examined. The external opening of the ear was so small as not readily to be perceived: it is simply an orifice; but the meatus enlarges considerably beyond the size of the opening, and passes some way under the skin, before it reaches the organ, which in this specimen had been destroyed. In the duck, the orifice leading to the ear is very large, when compared with the opening in this animal. When we consider the peculiarities in the structure of the nose of this animal, which lives in water, it is natural to conclude the organ is fitted to smell in water, and the external nostrils are so placed, to enable it to discover its prey by the smell; for that purpose, the animal can apply its nose, with great ease, to the small recesses in which its prey may be concealed. The structure of the beak is not such as enables it to take a firm hold; but, when the marginal lips are brought together, the animal will have a considerable power of suction, and in that way may draw its prey into its mouth.

Explanation of the Figures.—Pl. 13, fig. 5, is a view of the beak, to show the situation of the openings of the external nostrils, marked aa. Fig. 6, another view of the beak, exposing the under portion. Fig. 7, a lateral view, to show the opening of the lips, and the situation of the eye and ear. a. The eye. b. The ear. Fig. 8, a view of the upper jaw and palate, to show the teeth in their situation. Fig. 9, a similar view of the under jaw. Fig. 10, the bones which form the beak delineated, and the soft surrounding parts only marked in outline. Fig. 11, a similar view of the bones forming the lower portion of the beak.

XIX. *Experiments on the Solar, and on the Terrestrial Rays that occasion Heat; with a Comparative View of the Laws to which Light and Heat, or rather the Rays which occasion them, are subject, in order to determine whether they are the same, or different. By William Herschel, LL. D., F. R. S. Part* 2. p. 437.

In the first part of this paper it has been shown, that heat derived immediately from the sun, or from candent terrestrial substances, is occasioned by rays emanating from them; and that such heat-making rays are subject to the laws of reflection and refraction. The similarity between light and heat in these points is so great, that it did not appear necessary to notice some small difference between them, relating to the refraction of rays to a certain focus, which will be mentioned hereafter. But the next 3 articles of this paper will require, that while we show the similarity between light and heat, we should at the same time point out some striking and substantial differences, which will occur in our experiments on the rays which occasion them, and on which hereafter we may proceed to argue, when the question reserved for the conclusion of this paper, whether light and heat be occasioned by the same or by different rays, comes to be discussed.

ART. 4. *Different Refrangibility of the Rays of Heat.*—We might have concluded this article in the first part of this paper, as a corollary of the former 3; since rays that have been separated by the prism, and have still remained subject to the laws of reflection and refraction, as has been shown, could not be otherwise than of different refrangibility; but we have something to say on this subject, which will be found much more circumstantial and conclusive than what might have been drawn as a consequence from our former experiments. However, to begin with what has already been shown, we find that 2° of heat were obtained from that part of the spectrum which contains the violet rays, while the full red colour, on the opposite side, gave no less than 7°; and these facts ascertain the different refrangibility of the rays which occasion heat, as clearly as that of light is ascertained by the dispersion and variety of the colours. For, whether the rays which occasion heat be the same with those which occasion the colours, which is a case that our foregoing experiments have not ascertained, the arguments for their different refrangibility rests on the same foundation, namely, their being dispersed by the prism; and that of the rays of light being admitted, the different refrangibility of the rays of heat follows of course. So far then a great resemblance again takes place.

I must now point out a very material difference, which is, that the rays of heat are of a much more extensive refrangibility than those of light. In order to make this appear, I shall delineate a spectrum of light, by assuming a line of a certain length; and dividing it into 7 parts, according to the dimensions assigned to the 7 colours by Sir Isaac Newton, in the 4th figure of the second part of his Optics, I shall represent the illuminating power of which each colour is possessed, by an ordinate drawn to that line. And here, as the absolute length of the ordinates is arbitrary, provided they be proportional to each other, I shall assume the length of that which is to express the maximum, equal to $\frac{27}{33}$ of the whole line. Thus, let GQ, fig. 12, pl. 13, represent the line that contains the arrangement of the colours, from the red to the violet. Then, erecting on the confines of the yellow and green the line $LR = \frac{27}{33}$ of GQ, it will represent the power of illumination of the rays in that place. For, by experiment already delivered, we have shown that the maximum of illumination is in the brightest yellow or palest green rays. From the same experiments we collect, that the illuminations of yellow and green are equal to each other, and not much inferior to the maximum; this gives us the ordinates K and M. Then, by the rest of the same experiments, we obtain also the ordinates H, I, N, O, P, with sufficient accuracy for the purpose here intended. All these being applied to the middle of the spaces which belong to their respective colours, we have the figure GRQG, representing what may be called the spectrum of illumination.

We are now, in the same manner, to find a figure to express the heating power of the refracted prismatic rays, or what may be called the spectrum of heat. In order to determine the length of our base, I examined the extent of the invisible rays, and found, that at a distance of 2 inches beyond the visible red, my thermometer in a few minutes acquired 1°¼ of heat. The extent of the coloured spectrum at that time, or the line which answers to GQ in the figure, measured 2.997 inches. If 2 inches had been the whole of the extent of the invisible part, it might be stated to be in proportion to the visible one as 2 to 3; but we are to make some allowance for a small space required beyond the last ordinate, that the curve of the heating power drawn through it may reach the base; and indeed, at 2¼ inches beyond visible red, I could still find ½ degree of heat. It appears therefore sufficiently safe, to admit the base of the spectrum of heat AQ, to be that of the spectrum of light GQ, as 5¼ to 3; or conforming to the Newtonian figure before mentioned, the base of which is 3.3 inches, as 57¾ to 33. Now if we assume for the maximum of heat, an ordinate of an equal length with that which was fixed on for the maximum of light, it will give us a method of comparing the 2 spectra together. Accordingly I have drawn the several ordinates B, C, D, E, F, G, H, I, K, L, M, N, O, P, of such lengths as, from experiments made on purpose, it appeared they should be, in order to express the heat indicated by the thermometer, when placed on the base, at the several stations pointed out by the letters. A mere inspection of the 2 figures, which have been drawn as lying on each other, will enable us now to see how very differently the prism disperses the heat-making rays, and those which occasion illumination, over the areas ASQA, and GRQG, of our 2 spectra! These rays neither agree in their mean refrangibility, nor in the situation of their maxima. At R, where we have most light, there is but little heat; and at S, where we have most heat, we find no light at all!

Exper. 21. *The Sines of Refraction of the Heat-making Rays, are in a Constant Ratio to the Sines of Incidence.* I used a prism with a refracting angle of 61°; and placing the thermometer N° 4 half an inch, and N° 1 one inch, beyond the last visible red colour, I kept N° 2 by the side of the spectrum, as a standard for temperature. Here in 8 minutes, the thermometer at half an inch from visible colour, rose 5½ degrees; and, at 1 inch from the same, the other thermometer rose 3½; while the temperature, as appears by N° 2, remained without change.

	At ½ inch.	At 1 inch.	Standard.
Min.	N° 4.	N° 1.	N° 2.
0	64	64	63½
2	67	66	63½
5	69	67	63½
8	69½	67½	63½

I now took a prism with an angle of 45°, and, placing the thermometers as before, I had as follows: Here we also had, in 10 minutes, a rise of 7° in the thermometer N° 4, and of 3¾ in N° 1; while N° 2 remained stationary.

55	55	55
59	57	54¾
61	58	55
62	58¾	55

I tried now all the 3 angles of a prism of whitish glass: they were of 63, 62, and 55 degrees; and I found invisible rays of heat to accompany all the visible spectra given by these angles. I tried a prism of crown glass, having an angle of 30°; and found invisible heat rays as before. I tried a prism of flint glass, with so small an angle as 19°, and again found invisible heat rays.

I made a hollow prism, by cementing together 3 slips of glass of an equal length, but unequal breadth, so as to give me different refracting angles: they were of 51°, 62° 30′, and 66° 30′. Then filling it with water, and receiving the spectrum, when exposed to the sun as usual, on the table, I placed the thermometer N° 1 at .45 inch behind the visible red colour, and N° 5 in the situation of the standard. The refracting angle of the prism was 62° 30′; and in 5 minutes the thermometer received 1⅝ degrees of heat from the invisible rays. On trying the other angles, I likewise found invisible heat rays, in their usual situation beyond the red colour. Now, setting aside a minute inquiry into the degrees of heat occasioned by these invisible rays, I shall here only consider them as an additional part, annexed to the different quantities of heat which are found to go along with the visible spectrum; in the same manner as if, in the spectrum of light, another colour had been added beyond the red. Then, as from the foregoing experiments it appears, that a change of the refracting medium, and of the angle by which the refractions were made, occasioned no alteration in the relative situation of the additional part AG, with respect to GQ; and as the part GQ is already known to follow the law of refraction we have mentioned, it is equally evident that the additional heat of AG must follow the same law. We do not enter into the dispersive power of different mediums with respect to heat, since that would lead us farther than the present state of our investigation could authorize us to go; the following experiment however will show that, as with light so with heat, such dispersive power must be admitted.

Exper. 22. *Correction of the Different Refrangibility of Heat, by contrary Refraction in Different Mediums.*—I took 3 prisms; one of crown glass, having an angle of 25°; another of flint glass, with an angle of 24; and a third of crown glass, with an angle of 10°. These being put together, as they are placed when experiments of achromatic refractions are to be made, I found that they gave a spectrum nearly without colour. The composition seemed to be rather a little over adjusted; there being a very faint tinge of red on the most refracted side, and of violet on the least refracted margin. I examined both extremes by 2 thermometers; keeping N° 3 as a standard, while N° 2 was applied for the discovery of invisible rays; but I found no heat on either side. After this, I placed N° 2 in the middle of the colourless illumination; and in a little time it rose 2°, while

N° 3 still remained unaltered at some small distance from the spectrum. This quantity was full as much as I could expect, considering the heat that must have been intercepted by 3 prisms. Thus then it appears, that the different refrangibility of heat, as well as that of light, admits of prismatic correction. And we may add, that this experiment also tends to the establishment of the contents of the preceding one; for the refrangibility of heat rays could not be thus corrected, were the sines of refraction not in a constant ratio to those of incidence.

Exper. 23. *In Burning-glasses, the Focus of the Rays of Heat is Different from the Focus of the Rays of Light.*—I placed the burning lens, with its aperture reduced to 3 inches, in order to lessen the aberration arising from the spherical figure, in the united rays of the sun; and being now apprised of the different refrangibility of the rays of heat, and knowing also that the least refrangible of them are the most efficacious, I examined the focus of light, by throwing hair-powder, with a puff, into the air. This pointed out the mean focus of the illuminating rays, situated in that part of the pencil which opticians have shown to be the smallest space into which they can be collected. That this may be called the focus of light, our experiments, which have proved the maximum of illumination to be situated between the yellow and green, and therefore among the mean refrangible rays of light, have fully established. The mean focus being thus pointed out by the reflection of light on the floating particles of powder, I held a stick of sealing-wax $1^s.6$ or 4 beats of my chronometer, in the contracted pencil, half an inch nearer to the lens than the focus. In this time, no impression was made on the wax. I applied it now half an inch farther from the lens than that focus; and, in $\frac{8}{10}$ of a second, or 2 beats of the same chronometer, it was considerably scorched. Exposing the sealing-wax also to the focus of light, the effect was equally strong in the same time; from which we may safely conclude, notwithstanding the little accuracy that can be expected, for want of a more proper apparatus, from so coarse an experiment, that the focus of heat, in this case, was certainly farther removed from the lens than the focus of light, and probably not less than $\frac{1}{4}$ of an inch; the heat, at half an inch beyond the focus of light being still equal to that in the focus.

ART. 5. *Transmission of Heat-making Rays.*—We enter now on the subject of the transmission of heat through diaphanous bodies. Our experiments have hitherto been conducted by the prism, the lens, and the mirror; these may indeed be considered as our principal tools, and, as such, will stand foremost in all our operations; but the scantiness of this stock cannot allow us to bring our work to perfection. Nor is it merely the want of tools, but rather the natural imperfection of those we have, that hinders our rapid progress. The prism which we use for separating the combined rays of the sun, refracts, reflects, transmits, and scatters them at the same time; and the laws by which it acts, in every one of these operations, ought to be investigated. Even the cause of the most obvious of its effects, the separation of the colours of light, is not well understood; for, in 2 prisms of different glass, when the angles are such as to give the same mean refraction, the dispersive power is known to differ. Their transmissions have been still less ascertained; and I need not add, that the internal and external reflections, and the scattering of rays on every one of the surfaces, are all of such a nature as must throw some obscurity on every result of experiments made with prisms. A lens partakes of all the inconveniencies of the prism; to which its own defects of spherical aberrations must be added. And a mirror, besides its natural incapacity of separating the rays of light from the different sorts of heat, scatters them very profusely. But if we have been scantily provided with materials to act on rays, it has partly been our own fault: every diaphanous body may become a new tool, in the hands of a diligent inquirer.

My apparatus for transmitting the rays of the sun is of the following construction: see fig. 13, pl. 13. In a box, 12 inches long, and 8 inches broad, are fixed 2 thermometers. The sides of the box are 2¼ inches deep. That part of the box where the balls of the thermometers are, is covered by a board, in which are 2 holes of ¾ inch diameter, one over each of the balls of the thermometers; and the bottom of the box, under the cover, is cut away, so as to leave these balls freely exposed. There is a partition between the 2 thermometers, in that part of the box which is covered, to prevent the communication of secondary scatterings of heat. Just under the opening of the transmitting holes, on the outside of the cover, is fixed a slip of wood, on which may rest any glass or other object, of which the transmitting capacity is to be ascertained. A thin wooden cover is provided, fig. 14, that it may be laid over the transmitting holes, occasionally, to exclude the rays of the sun; and on the middle of the slip of wood, under the holes, a pin is to be stuck perpendicularly, that its shadow may point out the situation of the box with respect to the sun. The box, thus prepared, is to be fastened on 2 short boards, joined together by a pair of hinges. A long slip of mahogany is screwed to the lowest of these boards, and lies in the hollow part of a long spring, fastened against the side of the upper one. The pressure of the spring must be sufficiently strong to keep the boards at any angle; and the slip of mahogany long enough to permit an elevation of about 85°.

In order to see whether all be properly adjusted, expose the apparatus to the sun, and lift up the board which carries the box, till the directing pin throws the shadow of its head on the place where the point is fastened. Then hold a sheet of paper under the box, and, if the thermometers have been properly placed, the shadow of their balls will be in the centre of the rays passing through the transmitting holes to the paper. A screen of a considerable size, fig. 15, with a parallelogrammic opening, should be placed at a good distance, to keep the sun's rays from every part of the apparatus, except that which is under the cover; and no more sun should be admitted into the room, than what will be completely received on the screen, interposed between the window and the apparatus.

As one of the thermometers is to indicate a certain quantity of heat coming to it by the direct ray, while the other is to show how much of it is stopped by the glass laid over the transmitting hole, it becomes of the utmost consequence to have 2 thermometers of equal sensibility*. The difficulty of getting such is much greater than can be imagined: a perfect equality in the size and thickness of the balls is however the most essential circumstance. When 2 are procured, they should be tried in quick and in slow exposures. These terms may be explained by referring to fire heat; for here the thermometers may be exposed so as to acquire, for instance, 30° of heat in a very short time; which may then be called a quick exposure: or they may be placed so as to make it require a good while to raise them to so many degrees; on which account the exposure may be called slow. It is true, that we have it not in our power to render the sun's rays more or less efficacious, and therefore cannot have a quick or slow exposure at our command; but a great difference would be found in the heat of a rising, or of a meridian sun: not to mention a variety of other causes, that influence the transmission of heat through the atmosphere. Now when thermometers are tried in various exposures, they should traverse their scales together with constant equality; otherwise no dependence can be placed on the results drawn from experiments made with them, in cases where only a few minutes can be allowed for the action of the cause whose influence we are to investigate. The balls must not be blacked: for, as we have already to encounter the transmitting capacity of the glass of which these balls are made, it will not be safe to add to this the transmitting disposition of one or more coats of blacking, which can never be brought to an equality, and are always liable to change, especially in very quick exposures.

Transmission of Solar Heat through Colourless Substances.

Exper. 24.—I laid a piece of clear transparent glass, with a bluish-white cast, on one of the holes of the transmitting machine: the faces of this glass are parallel, and highly polished. Then, putting the cover over both holes, I placed the machine in the situation where the experiment was to be made, and let it remain there a sufficient time, that the thermometer might assume a settled temperature. For

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* The theory of the sensibility of thermometers, as far as it depends on the size of the balls, may be considered thus. Let D, d, S, s, T, t, be the diameters, the points on which the sun acts, and the points on which the temperature acts, of a large and a small thermometer having spherical balls; and let x to y be the intensity of the action of the sun, to the intensity of the action of the temperature, on equal points of the surface of both thermometers. Then we have $s : S :: d^2 : D^2$, and $t : T :: 4d^2 : 4D^2$. The action of the sun therefore will be expressed by d^2x, D^2x; and that of the temperature by $4d^2y$, $4D^2y$; and the united action of both by $(x - 4y) \times d^2$, $(x - 4y) \times D^2$; which are to each other, as $d^2 : D^2$. Now the total effect being as the squares of the diameters, while $x : y$ remain in their incipient ratio, and the contents of the thermometers being as the cubes, the sensible effect produced on the particles of mercury, must be as $\frac{d^2}{d^3} : \frac{D^2}{D^3} :: \frac{1}{d} : \frac{1}{D}$; that is, inversely as the diameters. The small thermometer therefore will set off with a sensibility greater than that of the large one, in the same ratio.—Orig.

this purpose, an assistant thermometer, which should always remain in the nearest convenient place to the apparatus, will be of use, to point out the time when the experiment may be begun; for this ought not to be done till the thermometers to be used agree with the standard. In order not to lose time after an experiment, the apparatus may be taken into a cool room, or current of air, till the thermometers it contains are rather lower than the standard; after which, being brought to the required situation, they will soon be fit for action. All these precautions having been taken, I began the experiment by first writing down the degrees of the thermometers; then, opening the cover at the time that a clock or watch showing seconds came to a full minute, I continued to write down the state of the thermometers for not less than 5 minutes. The result was as annexed. Here the sun communicated, in 5 minutes, 6° of heat to the thermometer N° 5, which was openly exposed to its action; while, in the same time, N° 1 received only 4½ degrees by rays transmitted through the bluish-white glass: then, as 6 : 4½ :: 1 : .750. This shows plainly, that only ¾ of the incident heat were transmitted, and therefore that ¼ of it was intercepted by the glass.

Min.	N° 5. Sun.	N° 1. Bluish-white glass.
0	67	67
1	68¾	68⅛
2	70⅛	69⅛
3	71⅜	70
4	72⅜	70⅞
5	73	71½

I shall here, as well as in the following experiments, point out the difference between heat and light, in order, as has been mentioned before, to lead to an elucidation of our last discussion. To effect this therefore, I have ascertained, with all the accuracy the subject will admit of, the quantity of light transmitted through such glasses as I have used; but as it would here interrupt the order of our subject, I have joined, at the end of this paper, a table, with a short account of the method that has been used in making it, wherein the quantity of light transmitted is set down; and to this table I shall now refer. To render this comparative view more clear, we may suppose always 1000 rays of heat to come from the object: then 750 being transmitted, it follows that the bluish-white glass used in our experiment stops 250 of them; and, by the table at the end of this paper, it stops 86 rays of light; the number of them coming from the object also being put equal to 1000. It should be remarked, that when I compare the interception of solar heat with that of the light of a candle, it must not be understood that I take terrestrial to be the same as solar light; but not having at present an opportunity of providing a similar table for the latter, I am obliged to use the former, on a supposition that the quantity stopped by glasses may not be very different.

Exper. 25. I took a piece of flint glass, about 2½ tenths of an inch thick, and fastened it over one of the holes of the transmitting apparatus. Here the heat-making rays gave, in 5 minutes, 5½ degrees to the thermometer N° 5; and, by transmission through the flint glass, 5° to N° 1. Then, proceeding as before, we have

N° 5. Sun.	N° 1. Flint glass.
69¾	69¾
71¼	71
72⅝	72⅛
74⅛	73⅞
74⅞	74
75¼	74¾

$\frac{5}{5\frac{1}{2}} = .909$; which shows that 91 rays of heat were stopped. In the table before referred to, we find that this glass stops 34 rays of light.

Before proceeding it will be necessary to adopt a method of reducing the detail of the experiments into a narrower compass. It will be sufficient to say, that they have all been made on the same plan as the 2 which have been given. The observations were always continued for at least 5 minutes; and by examining the ratios of the numbers given by the thermometers in all that time, it may be seen that, setting aside little irregularities, there is a greater stoppage at first than towards the end; but as it would not be safe to take a shorter exposure than 5 minutes, on account of the small quantity of heat transmitted by some glasses, I have fixed on that interval as sufficiently accurate for giving a true comparative view. The experiments therefore may now stand abridged as follows.

Exper. 26. I took a piece of highly polished crown glass, of a greenish colour, and, cutting it into several parts, examined the transmitting power of one of them, reserving the other pieces for some experiments that will be mentioned hereafter.

Min.	Sun.	Greenish crown glass.
0	$66\frac{1}{4}$	$66\frac{1}{4}$
5	73	$71\frac{1}{4} \ldots 6\frac{3}{4} : 5 = .741$

This glass therefore stops 259 rays of heat, and 203 of light.

Exper. 27. I cut likewise a piece of coach glass into several parts, and tried one of them, reserving also the other pieces for future experiments.

Min.	Sun.	Coach glass.
0	$68\frac{7}{8}$	$68\frac{7}{8}$
5	$75\frac{7}{8}$	$74\frac{3}{8} \ldots 7 : 5\frac{1}{2} = .786$

It stops 214 rays of heat, and 168 of light.

Exper. 28. I examined a piece of Iceland crystal, of nearly $\frac{2}{10}$ of an inch in thickness.

Min.	Sun.	Iceland crystal.
0	67	67
5	$72\frac{5}{8}$	$71\frac{1}{4} \ldots 5\frac{5}{8} : 4\frac{1}{4} = .756$

It stops 244 rays of heat, and 150 of light.

	Min.	Sun.	Talc.
Exper. 29.	0	$67\frac{1}{2}$	$67\frac{1}{2}$
	5	72	$71\frac{3}{8} \ldots 4\frac{1}{2} : 3\frac{7}{8} = .861$

It stops 139 rays of heat, and 90 of light.

	Min.	Sun.	An easily calcinable talc.
Exper. 30.	0	50	50
	5	$54\frac{3}{4}$	$73\frac{7}{8}\ 4\frac{3}{4} : 3\frac{7}{8} = .816$

It stops 184 rays of heat, and 288 of light.

Transmission of solar Heat through Glasses of the prismatic Colours.

	Min.	Sun	Very dark red glass.
Exper. 31.	0	73	73
	5	$79\frac{1}{4}$	$74\frac{1}{4} \ldots 6\frac{1}{4} : 1\frac{1}{4} = .200.$

This glass stops 800 rays of heat, and 9999, out of 10000 rays of light; which amounts nearly to a total separation of light from heat.

	Min.	Sun.	Dark-red glass.
Exper. 32.	0	$68\frac{3}{8}$	$68\frac{3}{8}$
	5	$72\frac{1}{2}$	$70 \ldots 4\frac{1}{8} : 1\frac{5}{8} = .394.$

This red glass stops only 606 rays of heat, and above 4999, out of 5000 rays of light.

	Min.	Sun.	Orange glass.
Exper. 33.	0	$67\frac{3}{4}$	$67\frac{3}{4}$
	5	$74\frac{5}{8}$	$70\frac{3}{8} \ldots 6\frac{5}{8} : 2\frac{5}{8} = .396.$

This orange-coloured glass stops 604 rays of heat, which is nearly as much as is stopped by the last red one; but it stops only 779 rays of light.

	Min.	Sun.	Yellow glass.
Exper. 34.	0	$70\frac{1}{2}$	$70\frac{1}{2}$
	5	$74\frac{1}{4}$	$73 \ldots 3\frac{3}{4} : 2\frac{1}{2} = .667.$

It stops 333 rays of heat, and 319 of light.

	Min.	Sun.	Pale-green glass.
Exper. 35.	0	$70\frac{1}{2}$	$70\frac{1}{2}$
	5	$74\frac{1}{4}$	$71\frac{7}{8} \ldots 3\frac{3}{4} : 1\frac{3}{8} = .367.$

It stops 633 rays of heat, and only 535 of light.

	Min.	Sun.	Dark-green glass.
Exper. 36.	0	$67\frac{1}{2}$	$67\frac{1}{2}$
	5	$74\frac{1}{8}$	$68\frac{1}{2} \ldots 6\frac{5}{8} : 1 = .151.$

This glass stops 849 rays of heat, and 949 of light. This accounts for its great use as a darkening glass for telescopes.

	Min.	Sun.	Bluish-green glass.	
Exper. 37.	0	$69\frac{3}{8}$	$69\frac{3}{8}$	It stops 768 rays of heat, and 769 of light.
	5	$76\frac{3}{8}$	$71 \ldots 7 : 1\frac{5}{8} = .232.$	

	Min.	Sun.	Pale-blue glass.	
Exper. 38.	0	$70\frac{3}{4}$	$70\frac{3}{4}$	The pale blue glass stops 812 rays of heat, and only 684 of light.
	5	$76\frac{3}{4}$	$71\frac{7}{8} \ldots 6 : 1\frac{1}{8} = .188.$	

	Min.	Sun.	Dark-blue glass.	
Exper. 39.	0	71	71	The dark-blue glass stops only 362 rays of heat, and 801 of light.
	5	$76\frac{7}{8}$	$74\frac{3}{4} \ldots 5\frac{7}{8} : 3\frac{3}{4} = .638.$	

	Min.	Sun.	Indigo glass.	
Expsr. 40.	0	$61\frac{3}{4}$	$61\frac{3}{4}$	This glass stops 633 rays of heat, and 9997, out of 10000 rays of light.
	5	$67\frac{7}{8}$	$64 \ldots 6\frac{1}{8} : 2\frac{1}{4} = .367.$	

	Min.	Sun.	Pale-indigo glass.	
Exper. 41.	0	62	62	It stops 532 rays of heat, and 978 of light.
	5	$67\frac{7}{8}$	$64\frac{3}{4} \ldots 5\frac{7}{8} : 2\frac{3}{4} = .468.$	

	Min.	Sun.	Purple glass.	
Exper. 42.	0	$61\frac{3}{4}$	$61\frac{3}{4}$	It stops 583 rays of heat, and 993 of light.
	5	$67\frac{3}{4}$	$64\frac{1}{4} \ldots 6 : 2\frac{1}{2} = .417.$	

	Min.	Sun.	Violet glass.	
Exper. 43.	0	$62\frac{1}{4}$	$62\frac{1}{4}$	It stops 489 rays of heat, and 955 of light.
	5	$68\frac{1}{8}$	$65\frac{1}{4} \ldots 5\frac{7}{8} : 3 = .511.$	

Transmission of Solar Heat through Liquids.—I took a small tube, $1\frac{1}{2}$ inch in diameter, fig. 16, and fixed a stop with a hole $\frac{3}{4}$ inch wide at each end, on which a glass might be fastened, so as to confine liquids. The inner distance, or depth of the liquid, when confined, is 3 inches. Placing now the empty tube, with its 2 end glasses fixed, on the transmitting apparatus, I had as follows:

	Min.	Sun.	Empty tube, and two glasses.	
Exper. 44.	0	53	53	These glasses, with the intermediate air, stop 542 rays of heat, and 204 of light.
	5	59	$55\frac{3}{4} \ldots 6 : 2\frac{3}{4} = .458.$	

Exper. 45. I filled the tube with well-water, and placed it on the transmitting apparatus.

Min.	Sun.	Well-water.
0	$52\frac{1}{4}$	$52\frac{1}{8}$
5	$58\frac{3}{4}$	$55 \ldots 6\frac{1}{2} : 2\frac{7}{8} = .442.$

Here 2 glasses, with water between them, stopped 558 rays of heat. The same glasses, and water, stop only 211 rays of light. If we were to deduct the effect of the empty machine, there would remain, for the water to stop, only 16 rays of heat, and 7 of light; but it cannot be safe to make this conclusion, as we are not sufficiently acquainted with the action of surfaces between the different mediums on the rays of heat and light; I shall therefore only notice the effect of the compound.

Exper. 46. I filled now the tube with sea-water, taken from the head of the pier at Ramsgate, at high tide.

Min.	Sun.	Sea-water.
0	$54\frac{1}{2}$	$54\frac{1}{4}$
5	60	$56 \ldots . 5\frac{1}{2} : 1\frac{3}{4} = .318.$

The compound stops 682 rays of heat, and 288 of light.

	Min.	Sun.	Spirit of wine.	
Exper. 47.	0	$51\frac{5}{8}$	$51\frac{5}{8}$	The compound stops 612 rays of heat, and 224 of light.
	5	$57\frac{3}{4}$	$54 \ldots 6\frac{1}{8} : 2\frac{3}{8} = .388.$	

	Min.	Sun.	Gin.	
Exper. 48.	0	52	52	This compound stops 739 rays of heat, and 626 of light.
	5	$57\frac{3}{4}$	$53\frac{1}{2} \ldots 5\frac{3}{4} \cdot 1\frac{1}{2} = .261.$	

	Min.	Sun.	Brandy.	
Exper. 49.	0	56	56	This stops 794 rays of heat, and 996 rays of light.
	5	$60\frac{1}{4}$	$56\frac{7}{8} \ldots 4\frac{1}{4} : \frac{7}{8} = .206.$	

Other liquids have also been tried; but the experiments having been attended with circumstances that demand a further investigation, they cannot now be given.

Transmission of Solar Heat through Scattering Substances.

Exper. 50. I rubbed one of the pieces of crown glass, mentioned in the 26th experiment, on fine emery laid on a plain brass tool, to make the surface of it rough, which, it is well known, will occasion the transmitted light to be scattered in all directions. Supposing that it would have the same effect on heat, I tried the transmitting capacity of the glass, by exposing it with the rough side towards the sun, over one of the transmitting holes of the apparatus.

Min.	Sun.	Crown glass.
0	67	67
5	74	$70\frac{3}{4} \ldots 7 : 3\frac{3}{4} = .536.$

The glass so prepared stops 464 scattered rays of heat, and 854 of light. Now, as the same glass, in its polished state, transmitted 259 rays of heat, and 203 of light, the alteration produced in the texture of its surface acts very differently on these 2 principles; occasioning an additional stoppage of only 205 rays of heat, but of 651 rays of light.

Exper. 51. One of the pieces of coach glass, mentioned in the 27th experiment, was prepared in the same manner.

Min.	Sun.	Coach glass.
0	$66\frac{1}{2}$	$66\frac{1}{2}$
5	$73\frac{1}{2}$	$69\frac{1}{2} \ldots 7 : 3 = .429.$

It stops 571 scattered rays of heat, and 885 of light; so that the fine scratches on its surface, made by the operation of emery, have again acted very differently on the rays of heat, and of light, occasioning an additional stoppage of 375 of the former, but of no less than 717 of the latter.

Exper. 52. I took another of the pieces of crown glass, mentioned in the 26th experiment, and rubbed both sides on emery.

Min.	Sun.	Crown glass; both sides rubbed on emery.
0	$69\frac{1}{2}$	$69\frac{1}{2}$
5	$75\frac{1}{2}$	$71\frac{1}{2} \ldots 6 : 2 = .333.$

The glass thus prepared, stops 667 scattered rays of heat, and 932 of light.

Exper. 53. Another piece of coach glass, one of those that were mentioned in the 27th experiment, was prepared in the same manner.

Min.	Sun.	Coach glass; both sides rubbed on emery.
0	$69\frac{5}{8}$	$69\frac{5}{8}$
5	$75\frac{3}{4}$	$71\frac{1}{4} \ldots 6\frac{1}{8} : 1\frac{5}{8} = .265.$

It stops 735 scattered rays of heat, and 946 of light.

Exper. 54. I placed now the coach glass, one side of which had been rubbed on emery, on the transmitting hole, and over it the crown glass prepared in the same manner, both with the rough side towards the sun; but 2 slips of card were placed between the glasses, to keep them from touching each other.

Min.	Sun.	Crown glass, and coach glass; one side of each rubbed on emery.
0	67	67
5	$73\frac{5}{8}$	$69 \ldots 6\frac{5}{8} : 2 = .302.$

These glasses stop 698 scattered rays of heat, and 969 of light.

Exper. 55. I placed now the coach glass, with both sides rubbed on emery, on the transmitting hole, and over it the crown glass prepared in the same manner, with 2 slips of card between them, to prevent a contact.

Min.	Sun.	Coach glass, and crown glass; both sides of each rubbed on emery.
0	$69\frac{5}{8}$	$69\frac{5}{8}$
5	$75\frac{7}{8}$	$70\frac{7}{8} \ldots 6\frac{1}{4} : 1\frac{1}{4} = .200.$

These glasses stop 800 scattered rays of heat, and 979 of light.

Exper. 56. I used now all the 4 glasses; placing them as follows, and putting slips of card between them, to prevent a contact.

Min.	Sun.	Crown glass; the rough side to the sun. Coach glass; ditto. Crown glass; rough on both sides. Coach glass; ditto.
0	$57\frac{3}{8}$	$57\frac{1}{2}$
5	$62\frac{1}{2}$	$58\frac{1}{4} \ldots 5\frac{1}{8} : \frac{3}{4} = .146.$

These 4 glasses stop no more than 854 scattered rays of heat, and 995 of light.

Exper. 57. I used now a piece of glass of an olive colour, burnt into the glass, in the manner that glasses are prepared for church windows, which transmits only scattered light.

Min.	Sun.	Olive-coloured glass.
0	69	69
5	$76\frac{3}{4}$	$70\frac{1}{4} \ldots 7\frac{3}{4} : 1\frac{1}{4} = .161$

This glass stops 839 scattered rays of heat, and 984 of light.

	Min.	Sun.	Calcined talc.
Exper. 58.	0	$51\frac{3}{8}$	$51\frac{3}{8}$
	5	$55\frac{1}{8}$	$51\frac{7}{8} \ldots 3\frac{3}{4} : \frac{1}{2} = .133.$

This substance stops 867 scattered rays of heat, and so much light that the sun cannot be perceived through it.*

	Min.	Sun.	White paper.
Exper. 59.	0	63	63
	5	68	$63\frac{3}{4} \ldots 5 : \frac{3}{4} = .150.$

This substance stops 850 scattered rays of heat, and 994 of light.

	Min.	Sun.	Linen.
Exper. 60.	0	63	63
	5	69	$63\frac{1}{2} \ldots 6 : \frac{1}{2} = .0833.$

White linen stops 916 scattered rays of heat, and 952 of light.

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* See the 175th experiment.

	Min.	Sun.	White persian.	
Exper. 61.	0	70	70	This thin silk stops 760 scattered rays
	5	$76\frac{1}{4}$	$71\frac{1}{2} \ldots 6\frac{1}{4} : 1\frac{1}{2} = .240.$	of heat, and 916 of light.

	Min.	Sun.	Black muslin.	
Exper. 62.	0	$64\frac{3}{4}$	$64\frac{3}{4}$	This substance stops 714 scattered
	5	70	$66\frac{1}{4} \ldots 5\frac{1}{4} : 1\frac{1}{2} = .286.$	rays of heat, and 737 of light.

Transmission of Terrestrial Flame-heat through various Substances.—My apparatus for the purpose of transmitting flame-heat is as follows. Fig. 1, pl. 14, a box 22 inches long, $5\frac{1}{2}$ broad, and $1\frac{3}{4}$ deep, has a hole in the centre $1\frac{1}{10}$ inch in diameter, through which a wax candle, thick enough entirely to fill it, is to be put at the bottom; the box being properly elevated for the purpose. There must be 2 lateral holes in the bottom, 2 inches long, and $1\frac{1}{4}$ broad, one on each side of the candle, to supply it with a current of air, as otherwise it will not give a steady flame, which is absolutely necessary. At the distance of $1\frac{3}{10}$ inch from the candle, on each side, are 2 screens, 12 inches square, with a hole in each, $\frac{3}{4}$ inch in diameter, through which the heat of the candle passes to the 2 thermometers, which are to be placed in opposite directions, one on each side of the table. Care must be taken to place them exactly at the same distance from the centre of the flame, as otherwise they will not receive equal quantities of heat. The scales, and their supports also, must be so kept out of the way of heat coming from the candle, that they may not scatter it back on the balls, but suffer all that is not intercepted by them to pass freely forwards in the box, and downwards, through openings cut in the bottom. Before the transmitting holes, between the 2 wooden screens, must be 2 covers of the same material, close to the openings, fig. 2; and it will be necessary to join these covers at the side, by a common handle, that they may be removed together, without disturbing any part of the apparatus, when the experiment is to begin. The glasses are to be put before the thermometer, close to the transmitting hole, by placing them on a small support below, while the upper part is held close to the screen by a light plummet suspended by a thread which is fastened on one side, and passes over the glass, to a hook on the other side.

In making experiments, many attentions are necessary, such as, keeping the candle exactly to a certain height, that the brightest part of the flame may be just in the centre of the two transmitting holes: that the wick may be always straight, and not, by bending, approach nearer to one thermometer than to the other: that the wax-cup of the candle be kept clean, and never suffered to run over, &c. Before, and now and then between, the observations also, the thermometers must be tried a few degrees, that it may be seen whether they act equally; and the candle, during the time they cool down to the temperature, must be put out by an extinguisher, large enough to rest on the bottom of the box, without touching any part of the wax. Many other precautions I need not mention, as they will soon be discovered by any one who may repeat such experiments.

	Min.	Candle.	Bluish-white glass.	
Exper. 63.	0	$59\frac{3}{8}$	$59\frac{1}{2}$	From this experiment we find, that while the rays of the candle gave 3
	5	$62\frac{3}{8}$	$60\frac{5}{8} \ldots 3 : 1\frac{1}{8} = .375.$	degrees of heat to the thermometer

openly exposed to their action, the other thermometer, which received the same rays through the medium of the interposed glass, rose only $1\frac{1}{8}$ degrees. Hence we calculate, that this glass stops 625 rays of flame-heat, out of every thousand that fall on it. It stops only 86 rays of candle-light; but this, having been referred to before, will not in future be repeated.

	Min.	Candle.	Flint glass.	
Exper. 64.	0	$59\frac{7}{8}$	$59\frac{5}{8}$	It stops 591 rays of flame-heat, and light as before.
	5	$62\frac{5}{8}$	$60\frac{3}{4} \ldots 2\frac{3}{4} : 1\frac{1}{8} = .409.$	

	Min.	Candle.	Crown glass.	
Exper. 65.	0	$59\frac{7}{8}$	$59\frac{7}{8}$	It stops 636 rays of flame-heat.
	5	$62\frac{5}{8}$	$60\frac{7}{8} \ldots 2\frac{3}{4} : 1 = .364.$	

	Min.	Candle.	Coach glass.	
Exper. 66.	0	60	$60\frac{3}{8}$	
	5	63	$62 \ldots 3 : 1\frac{5}{8} = .542.$	It stops 458 rays of flame-heat.

	Min.	Candle.	Iceland crystal.	
Exper. 67.	0	$58\frac{3}{4}$	$58\frac{3}{4}$	
	5	$62\frac{1}{8}$	$60\frac{3}{8} \ldots 3\frac{3}{8} : 1\frac{5}{8} = .484.$	It stops 516 rays of flame-heat.

	Min.	Candle.	Calcinable talc.	
Exper. 68.	0	$58\frac{7}{8}$	$58\frac{7}{8}$	This substance stops only 375 rays of flame-heat.
	5	$61\frac{7}{8}$	$60\frac{3}{4} \ldots 3 : 1\frac{7}{8} = .625.$	

	Min.	Candle.	Very dark red glass.	
Exper. 69.	0	$60\frac{3}{8}$	$60\frac{3}{8}$	
	5	$63\frac{1}{8}$	$61\frac{3}{8} \ldots 2\frac{3}{4} : 1 = .364.$	This glass stops 636 rays of flame-heat.

	Min.	Candle.	Dark red glass.	
Exper. 70.	0	$60\frac{3}{4}$	$60\frac{3}{4}$	
	5	$63\frac{1}{8}$	$61\frac{7}{8} \ldots 2\frac{3}{8} : 1\frac{1}{8} = .474.$	It stops 526 rays of flame-heat.

	Min.	Candle.	Orange glass.	
Exper. 71.	0	$60\frac{1}{4}$	$60\frac{1}{4}$	
	5	$63\frac{3}{8}$	$61\frac{5}{8} \ldots 3\frac{1}{8} : 1\frac{3}{8} = .440.$	It stops 560 rays of flame-heat.

	Min.	Candle.	Yellow glass.	
Exper. 72.	0	$60\frac{5}{8}$	$60\frac{5}{8}$	
	5	$63\frac{5}{8}$	$61\frac{7}{8} \ldots 3 : 1\frac{1}{4} = .417.$	It stops 583 rays of flame-heat.

	Min.	Candle.	Pale-green glass.	
Exper. 73.	0	$60\frac{7}{8}$	$60\frac{7}{8}$	
	5	$63\frac{7}{8}$	$62\frac{3}{8} \ldots 3 : 1\frac{1}{2} = .500.$	It stops 500 rays of flame-heat.

	Min.	Candle.	Dark-green glass.	
Exper. 74.	0	$61\frac{1}{8}$	$61\frac{1}{8}$	
	5	64	$61\frac{7}{8} \ldots 2\frac{7}{8} : \frac{3}{4} = .261.$	It stops 739 rays of flame-heat.

	Min.	Candle.	Bluish-green glass.	
Exper. 75.	0	$61\frac{1}{8}$	$61\frac{1}{8}$	
	5	64	$62\frac{1}{8} \ldots 2\frac{7}{8} : 1 = .348.$	It stops 652 rays of flame-heat.

	Min.	Candle.	Pale-blue glass.	
Exper. 76.	0	$61\frac{1}{2}$	$61\frac{1}{2}$	
	5	$64\frac{3}{8}$	$62\frac{5}{8} \ldots 2\frac{7}{8} : 1\frac{1}{8} = .391.$	It stops 609 rays of flame-heat.

	Min.	Candle.	Dark-blue glass.	
Exper. 77.	0	$61\frac{7}{8}$	$61\frac{3}{4}$	
	5	$64\frac{1}{2}$	$62\frac{3}{4} \ldots 2\frac{5}{8} : 1 = .381.$	It stops 619 rays of flame-heat.

	Min.	Candle.	Indigo glass.	
Exper. 78.	0	$61\frac{7}{8}$	$61\frac{7}{8}$	
	5	$65\frac{3}{8}$	$63 \ldots 3\frac{1}{2} : 1\frac{1}{8} = .321.$	It stops 679 rays of flame-heat.

	Min.	Candle.	Pale indigo glass.	
Exper. 79.	0	$62\frac{1}{8}$	$62\frac{1}{8}$	
	5	$64\frac{3}{4}$	$63\frac{1}{4} \ldots 2\frac{5}{8} : 1\frac{1}{8} = .429.$	It stops 571 rays of flame-heat.

	Min.	Candle.	Purple glass.	
Exper. 80.	0	$61\frac{7}{8}$	$61\frac{7}{8}$	
	5	65	$63\frac{3}{8} \ldots 3\frac{1}{8} : 1\frac{1}{2} = .480.$	It stops 520 rays of flame-heat.

	Min.	Candle.	Violet glass.	
Exper. 81.	0	$59\frac{7}{8}$	$59\frac{7}{8}$	
	5	$63\frac{3}{8}$	$61\frac{5}{8} \ldots 3\frac{1}{2} : 1\frac{3}{4} = .500.$	It stops 500 rays of flame-heat.

	Min.	Candle.	Crown glass; one side rubbed on emery; the rough side exposed.	
Exper. 82.	0	60	60	This glass, so prepared, stops 741 scattered rays of flame-heat.
	5	$63\frac{3}{8}$	$60\frac{7}{8} \ldots 3\frac{3}{8} : \frac{7}{8} = .259.$	
	Min.	Candle.	Coach glass; one side rubbed on emery; the rough side exposed.	
Exper. 83.	0	$59\frac{5}{8}$	$59\frac{5}{8}$	
	5	$63\frac{3}{8}$	$60\frac{7}{8} \ldots 3\frac{3}{4} : 1\frac{1}{4} = .333.$	It stops 667 scattered rays of flame-heat.
	Min.	Candle.	Crown glasss; both sides rubbed on emery.	
Exper. 84.	0	$59\frac{3}{4}$	$59\frac{3}{4}$	It stops 615 scattered rays of flame-heat.
	5	63	$61 \ldots 3\frac{1}{4} : 1\frac{1}{4} = .385.$	
	Min.	Candle.	Coach glass; both sides rubbed on emery.	
Exper. 85.	0	$59\frac{7}{8}$	$59\frac{3}{4}$	It stops 680 scattered rays of flame-heat.
	5	63	$60\frac{3}{4} \ldots 3\frac{1}{8} : 1 = .320.$	
	Min.	Candle.	Crown glass. Coach glass. } One side of each rubbed on emery.	
Exper. 86.	0	$55\frac{7}{8}$	$55\frac{7}{8}$	These glasses stop 720 scattered rays of flame-heat.
	5	59	$56\frac{3}{4} \ldots 3\frac{1}{8} : \frac{7}{8} = .280.$	
	Min.	Candle.	Crown glass. Coach glass. } Both sides of each rubbed on emery.	
Exper. 87.	0	$55\frac{7}{8}$	$55\frac{7}{8}$	These glasses stop 667 rays of flame-heat.
	5	$59\frac{1}{4}$	$57 \ldots 3\frac{3}{8} : 1\frac{1}{8} = .333.$	
	Min.	Candle.	Crown glass; the rough side to the candle. Coach glass; ditto. Crown glass; rough on both sides. Coach glass; ditto.	
Exper. 88.	0	$56\frac{3}{4}$	$56\frac{7}{8}$	These four glasses stop 870 scattered rays of flame-heat.
	5	$59\frac{5}{8}$	$57\frac{1}{4} \ldots 2\frac{7}{8} : \frac{3}{8} = .130.$	
	Min.	Candle.	Olive-colour, burnt in glass.	
Exper. 89.	0	60	60	This glass stops 792 scattered rays of flame-heat.
	5	63	$60\frac{5}{8} \ldots 3 : \frac{5}{8} = .208.$	
	Min.	Candle.	White paper.	
Exper. 90.	0	$57\frac{3}{8}$	$57\frac{1}{4}$	This substance stops 792 scattered rays of flame-heat.
	5	$60\frac{3}{8}$	$57\frac{7}{8} \ldots 3 : {}^{5} = .208.$	
	Min.	Candle.	Linen.	
Exper. 91.	0	$57\frac{3}{8}$	$57\frac{3}{8}$	
	5	61	$58\frac{1}{2} \ldots 3\frac{5}{8} : 1\frac{1}{8} = .310.$	It stops 690 scattered rays of flame-heat.
	Min.	Candle.	White persian.	
Exper. 92.	0	$57\frac{1}{8}$	57	
	5	$60\frac{1}{2}$	$58\frac{3}{8} \ldots 3\frac{3}{8} : 1\frac{3}{8} = .407.$	It stops 593 scattered rays of flame-heat.
	Min.	Candle.	Black muslin.	
Exper. 93.	0	$57\frac{3}{4}$	$57\frac{3}{4}$	
	5	$60\frac{5}{8}$	$59 \ldots 2\frac{7}{8} : 1\frac{1}{4} = .435.$	It stops 565 scattered rays of flame-heat.

Transmission of the Solar Heat which is of an Equal Refrangibility with Red Prismatic Rays.—The apparatus which I have used for transmitting prismatic rays, is of the same construction as that which has already been described under the head of direct solar transmissions; but here the holes in the top of the box are only 2 inches from each other, and no more than $\frac{3}{8}$ths in diameter, fig. 17, pl. 13. On the face of the box are drawn two parallel lines, also $\frac{3}{8}$ths of an inch distant from each other, and inclosing the transmitting holes: they serve as a direction by which to keep any required colour to fall equally on both holes. The distance at which the box is to be placed from the prism, must be such as will allow the rays to diverge sufficiently for the required colour to fill the transmitting holes; and the balls of the thermometers placed under them ought to be less than these holes, that the projected rays may pass around them, and show their proper adjustment. The diameters of mine, used for this purpose, are $2\frac{1}{4}$ tenths of an inch.

Exper. 94.—I placed my apparatus at 5 feet from the prism, and so as to cause the red-making rays to fall between the parallel lines in order to find what heat-making rays would come to the thermometer along with them.

Min.	Red rays. Therm. A.	Bluish-white glass. Them. B.
0	$75\frac{5}{8}$	$75\frac{3}{8}$
5	$77\frac{5}{8}$	$76\frac{5}{8} \ldots 2 : 1\frac{1}{4} = .625.$

From this experiment it appears, that when 1000 red-making rays fall on each transmitting hole, 375 of them, if they also be the heat-making rays, are stopped by the bluish-white glass which covers one of these holes; or, what requires no other proof than the experiment itself, that 375 rays of heat, of the same refrangibility with the red rays, are intercepted by this glass.

	Min.	Red rays.	Flint glass.	
Exper. 95.	0	$75\frac{3}{4}$	$75\frac{3}{8}$	This glass stops only 143 rays of heat which are of the same refrangibility with the red rays.
	5	$77\frac{1}{2}$	$76\frac{7}{8} \ldots 1\frac{3}{4} : 1\frac{1}{2} = .857.$	

	Min.	Red rays.	Crown glass.	
Exper. 96.	0	$75\frac{7}{8}$	$75\frac{5}{8}$	This glass stops 294 rays of the same sort of heat.
	5	78	$77\frac{1}{8} \ldots 2\frac{1}{8} : 1\frac{1}{2} = .706.$	

	Min.	Red rays.	Coach glass.	
Exper. 97.	0	$54\frac{3}{8}$	$53\frac{3}{4}$	
	5	$55\frac{5}{8}$	$54\frac{3}{4} \ldots 1\frac{1}{4} : 1 = .800.$	It stops 200 rays of the same sort of heat.

	Min.	Red rays.	Iceland crystal.	
Exper. 98.	0	$76\frac{1}{8}$	$75\frac{3}{4}$	This substance stops 200 rays of the same sort of heat.
	5	78	$77\frac{1}{4} \ldots 1\frac{7}{8} : 1\frac{1}{2} = .800.$	

	Min.	Red rays.	Calcinable talc.	
Exper. 99.	0	$51\frac{3}{4}$	$51\frac{1}{4}$	
	5	$53\frac{5}{8}$	$52\frac{7}{8} \ldots 1\frac{7}{8} : 1\frac{5}{8} = .867.$	It stops 133 rays of the same sort of heat.

	Min.	Red rays.	Dark red glass.	
Exper. 100.	0	$76\frac{1}{2}$	$76\frac{5}{8}$	This glass stops 692 rays of the same sort of heat.
	5	$78\frac{1}{8}$	$77\frac{1}{8} \ldots 1\frac{5}{8} : \frac{1}{2} = .308.$	

	Min.	Red rays.	Orange glass.	
Exper. 101.	0	75	$74\frac{3}{4}$	
	5	77	$75\frac{3}{4} \ldots 2 : 1 = .500.$	It stops 500 rays of the same sort of heat.

	Min.	Red rays.	Yellowglass.	
Exper. 102.	0	$75\frac{3}{8}$	75	
	5	$76\frac{7}{8}$	$75\frac{7}{8} \ldots 1\frac{1}{2} : \frac{7}{8} = .583.$	It stops 417 rays of the same sort of heat.

	Min.	Red rays.	Pale green glass.	
Exper. 103.	0	$74\frac{1}{4}$	$74\frac{1}{8}$	
	5	$76\frac{3}{8}$	$75 \ldots 2\frac{1}{8} : \frac{7}{8} = .412.$	It stops 588 rays of the same sort of heat

	Min.	Red rays.	Dark-green glass.	
Exper. 104.	0	$68\frac{3}{4}$	$68\frac{7}{8}$	
	5	$70\frac{1}{2}$	$69\frac{1}{4} \ldots 1\frac{3}{4} : \frac{3}{8} = .214.$	It stops 786 rays of the same sort of heat.

	Min.	Red rays.	Bluish-green glass.	
Exper. 105.	0	69	$68\frac{7}{8}$	
	5	$70\frac{5}{8}$	$69\frac{3}{4} \ldots 1\frac{5}{8} : \frac{7}{8} = .538.$	It stops 462 rays of the same sort of heat.

	Min.	Red rays.	Pale-blue glass.	
Exper. 106.	0	$69\frac{5}{8}$	$69\frac{1}{2}$	
	5	$70\frac{7}{8}$	$69\frac{7}{8} \ldots 1\frac{1}{4} : \frac{3}{8} = .300.$	It stops 700 rays of the same sort of heat.

	Min.	Red rays.	Dark-blue glass.	
Exper. 107.	0	67	$67\frac{1}{4}$	This glass stops only 71 rays of the same sort of heat.
	5	$68\frac{3}{4}$	$68\frac{7}{8} \ldots 1\frac{3}{4} : 1\frac{5}{8} = .929.$	

	Min.	Red rays.	Indigo glass.	
Exper. 108.	0	$68\frac{1}{2}$	$68\frac{1}{4}$	
	5	$70\frac{1}{2}$	$69\frac{1}{2} \ldots 2 : 1\frac{1}{4} = .633.$	It stops 367 rays of the same sort of heat.

	Min.	Red rays.	Pale indigo glass.	
Exper. 109.	0	69	$68\frac{5}{8}$	
	5	71	$70 \ldots 2 : 1\frac{3}{8} = .687.$	It stops 313 rays of the same sort of heat.

	Min.	Red rays.	Purple glass.	
Exper. 110.	0	$56\frac{1}{4}$	56	
	5	$58\frac{1}{2}$	$57\frac{1}{4} \ldots 2\frac{1}{4} : 1\frac{1}{4} = .556.$	It stops 444 rays of the same sort of heat.

	Min.	Red rays.	Violet glass.	
Exper. 111.	0	$57\frac{3}{8}$	57	
	5	$59\frac{1}{4}$	$58\frac{1}{8} \ldots 1\frac{7}{8} : 1\frac{1}{8} = .600.$	It stops 400 rays of the sm sort of heat.

	Min.	Red rays.	Crown glass; one side rubbed on emery, rough side exposed.	
Exper. 112.	0	$49\frac{3}{4}$	49	This glass, so prepared, stops 389 scattered rays of the same sort of heat.
	5	52	$50\frac{3}{8} \ldots 2\frac{1}{4} : 1\frac{3}{8} = .611.$	
	Min.	Red rays.	Coach glass; one side rubbed on emery, rough side exposed.	
Exper. 113.	0	$53\frac{3}{8}$	$52\frac{7}{8}$	It stops 500 scattered rays of the same sort of heat.
	5	$55\frac{1}{8}$	$53\frac{3}{4} \ldots 1\frac{3}{4} : \frac{7}{8} = .500.$	
	Min.	Red rays.	Crown glass; both sides rubbed on emery.	
Exper. 114.	0	$50\frac{5}{8}$	$49\frac{7}{8}$	It stops 471 scattered rays of the same sort of heat.
	5	$52\frac{3}{4}$	$51 \ldots 2\frac{1}{8} : 1\frac{1}{8} = .529.$	
	Min.	Red rays.	Coach glass; both sides rubbed on emery.	
Exper. 115.	0	$54\frac{1}{8}$	$53\frac{3}{4}$	It stops 833 scattered rays of the same sort of heat.
	5	$55\frac{5}{8}$	$54 \ldots 1\frac{1}{2} : \frac{1}{4} = .167.$	
	Min.	Red rays.	Calcined talc.	
Exper. 116.	0	$51\frac{1}{8}$	$50\frac{1}{2}$	This substance stops 737 scattered rays of the same sort of heat.
	5	$53\frac{1}{2}$	$51\frac{1}{8} \ldots 2\frac{3}{8} : \frac{5}{8} = .263.$	

Transmission of Fire-Heat through various Substances.—When the same fire is to give an equal heat to two thermometers, at some short distance from each other, it becomes highly necessary that there should be a place of considerable dimensions in its centre, where it may burn with equal glow, and without flame or smoke. To obtain this, I used a grate 19 inches broad, and $8\frac{3}{4}$ high, having only 3 bars, which divide the fire into 3 large openings. In the centre of the middle one of these, when the grate is well filled with large coals or coke, we may, with proper management, keep up the required equality of radiance.

The apparatus I have used is of the following construction. A screen of wood, fig. 3. pl. 14, 3 feet 6 inches high, and 3 feet broad, lined towards the fire with plates of iron, has two holes, $\frac{3}{4}$ of an inch in diameter, and at the distance of $2\frac{1}{2}$ inches from each other, one on each side of the middle of the screen, and of a height that will answer to the centre of the fire. $2\frac{1}{4}$ inches under the centre of the holes is a shelf, about 22 inches long and 4 broad, on which are placed two thermometers, in opposite directions fixed on proper stands, to bring the balls, quite disengaged from the scales, directly 2 inches behind the transmitting holes. A small thin wooden partition is run up between the thermometers, to prevent the heat transmitted through one hole from coming to the thermometer belonging to the other. The screen is fixed on a light frame, which fits exactly into the opening of the front of the marble chimney-piece; and the ends of the frame are of a length which, when the screen is placed before the fire, will just bring the transmitting holes to be $6\frac{1}{2}$ inches from the front bars of the grate. A large wooden cover, also plated with iron, shuts up the transmitting holes on the side next to the fire; but may be drawn up by a string on the outside so as to open them when required. Two assistant thermometers are placed on proper stands to bring their balls to the same distance from the screen as those which receive the heat of the fire; but removed sideways as far as necessary, to put them out of the reach of any rays that pass obliquely through the transmitting holes. They are to indicate any change of temperature that may take place during the time of the experiment: for, notwithstanding the largeness of the screen, some heat will find its way round and over it; and this acting as a general cause, its effect must be allowed for.

Exper. 117. Having tried the apparatus sufficiently to find that the thermometers exposed to the transmitting holes would generally receive 20 or more degrees of heat, without differing more than sometimes ⅓ or at most ¼ of a degree, I now placed the bluish-white glass of the 24th experiment on a support prepared for the purpose, so as closely to cover one of the transmitting holes. A small spring, moveable on its centre, is always turned against the upper part of the transmitting glasses, to keep them in their situation.

Min.	Fire.	Bluish-white glass.
0	66	66
5	86	71 ... 20 : 5 = .250.

This glass stops 750 rays of fire-heat. By looking through it, at the same place in the fire, after the screen was removed, in order to cool the apparatus for the next experiment, I found that this glass can hardly be said to stop any of the light of the fire.

	Min.	Fire.	Flint glass.	
Exper. 118.	0	67	67	
	5	87	72 .. 20 : 5 = .250.	It stops 750 rays of fire-heat.

	Min.	Fire.	Crown glass.	
Exper. 119.	0	67	67	
	5	86¾	72½ ... 19¾ : 5½ = .278.	It stops 722 rays of fire-heat.

	Min.	Fire.	Coach glass.	
Exper. 120.	0	67½	67½	
	5	86¾	73 ... 19¼ : 5½ = .286.	It stops 714 rays of fire-heat.

	Min.	Fire.	Iceland crystal.	
Exper. 121.	0	68	68	
	5	90½	73½ ... 22½ : 5½ = .244.	This substance stops 756 rays of fire-heat.

Exper. 122. I took now the piece of talc used in the 30th experiment, and, placing it over the transmitting hole, I had the following result. But, as the unexpected event of a calcination, which took place, was attended with circumstances that ought to be noticed, I shall instead of the usual abridgement of the experiments, give this at full length.

Min.	Fire. Therm. D.	Talc. Therm. C.
0	65	65
1	72	67 ... 7 : 2 = .289:
2	77	68¾ ... 12 : 3⅜ = .281.
3	80½	69½ ... 15½ : 4½ = .290.
4	83	70 ... 18 : 5 = .278.
5	85	70¾ ... 20 : 5¾ = .287.

This substance stops 713 rays of fire-heat.

I am now to point out the singularity of this experiment; which consists, as we may see by the above register of it, in the apparently regular continuance of its power of transmitting heat, while its capacity of transmitting light was totally destroyed. For, when I placed this piece of talc over the hole in the screen, it was extremely transparent, as this substance is generally known to be; and yet, when the experiment was over, it appeared of a beautiful white colour; and its power of transmitting light was so totally destroyed, that even the sun in the meridian could not be perceived through it. Now, had the power of transmitting heat through this substance been really uniform during all the 5 minutes, it would have been quite a new phenomenon; as all my experiments are attended with a regular increase of it; but since by calcination, the talc lost much of its transmitting power, we may easily account for this unexpected regularity.

	Fire.	Very dark red glass.	
Exper. 123.	66	66	
	89¼	75 ... 23¼ : 9 = 387.	This glass stops 613 rays of fire-heat.

	Min.	Fire	Dark-red glass.
Exper. 124.	0	67	67
	5	92¾	78 ... 25¾ : 11 = .427.

This glass, which stops 999.8 rays of candle-light, stops only 573 rays of fire-heat; whereas my piece of thick flint glass, which stops no more than 91 rays of that light, stops no less than 750 of fire-heat. It does not appear, by looking through these glasses, that there is a difference in their disposition to transmit candle-light or fire-light.

	Min.	Fire.	Orange glass.	
Exper. 125.	0	66	66	
	5	80	71 . . . 14 : 5 = .357.	It stops 643 rays of fire-heat.

	Min.	Fire.	Yellow glass.	
Exper. 126.	0	$61\frac{1}{4}$	$61\frac{1}{4}$	
	5	83	$68\frac{7}{8}$ . . . 21 : $7\frac{6}{8}$. cor. – $1\frac{1}{8}$° . $20\frac{5}{8}$: $6\frac{1}{2}$ = .315	

This experiment being made early in the morning, before the temperature of the room was come to its usual height, the assistant thermometers showed a gradual rising of $1\frac{1}{8}$ degree in the 5 minutes: they are in general very steady. The glass stops 685 rays of fire-heat.

	Min.	Fire.	Pale green glass.	
Exper. 127.	0	$65\frac{3}{4}$	$65\frac{3}{4}$	
	5	85	$71\frac{3}{4}$ . . . $19\frac{1}{4}$: 6 = .312	It stops 688 rays of fire-heat.

	Min.	Fire.	Dark green glass.	
Exper. 128.	0	68	68	
	5	$88\frac{3}{4}$	$73\frac{3}{8}$ . . . $20\frac{3}{8}$: $5\frac{3}{4}$ cor. – $\frac{1}{4}$° = 255.	It stops 745 rays of fire-heat.

	Min.	Fire.	Bluish green glass.	
Exper. 129.	0	$68\frac{1}{2}$	$68\frac{1}{2}$	
	5	87	$74\frac{1}{8}$ . . . $18\frac{1}{2}$: $5\frac{5}{8}$ = .304.	It stops 696 rays of fire-heat.

	Min.	Fire.	Pale blue glass.	
Exper. 130.	0	$68\frac{1}{2}$	68	
	5	$86\frac{1}{4}$	$73\frac{3}{4}$ . . . $17\frac{3}{4}$: $5\frac{3}{4}$ = .324.	It stops 676 rays of fire-heat.

	Min.	Fire.	Dark blue glass.	
Exper. 131.	0	67	67	
	5	$84\frac{7}{8}$	73 . . . $17\frac{7}{8}$: 6 . cor. – 1° = .296.	It stops 704 rays of fire-heat.

	Min.	Fire.	Indigo glass.	
Exper. 132.	0	$69\frac{1}{4}$	$69\frac{1}{4}$	
	5	$85\frac{3}{8}$	$73\frac{3}{4}$ . . . $16\frac{1}{8}$: $4\frac{1}{2}$ = .279.	It stops 721 rays of fire-heat.

	Min.	Fire.	Pale indigo glass.	
Exper. 133.	0	$67\frac{1}{2}$	$67\frac{1}{2}$	
	5	$85\frac{7}{8}$	74 . . . $18\frac{3}{8}$: $6\frac{1}{2}$. cor. – $\frac{1}{4}$° = .345.	It stops 655 rays of fire-heat.

	Min.	Fire.	Purple glass.	
Exper. 134.	0	69	69	
	5	83	$73\frac{1}{2}$ . . . 14 : $4\frac{1}{2}$ = .321.	It stops 679 rays of fire-heat.

	Min.	Fire.	Violet glass.	
Exper. 135.	0	$66\frac{1}{2}$	$66\frac{1}{2}$	
	5	$86\frac{1}{2}$	$74\frac{1}{4}$ . . . 20 : $7\frac{3}{4}$ = .385.	It stops 615 rays of fire-heat.

	Min.	Fire.	Crown glass; one side rubbed on emery.	
Exper. 136.	0	$67\frac{5}{8}$	$67\frac{5}{8}$	This glass, so prepared, stops 723 scattered rays of fire-heat.
	5	$89\frac{3}{4}$	$73\frac{3}{4}$ . . . $22\frac{1}{8}$: $6\frac{1}{8}$ = .277.	

	Min.	Fire.	Coach glass; one side rubbed on emery.	
Exper. 137.	0	68	$67\frac{1}{2}$	
	5	$87\frac{1}{8}$	$72\frac{1}{8}$ . . . $19\frac{1}{8}$: $4\frac{5}{8}$ = .242.	It stops 758 scattered rays of fire-heat.

	Min.	Fire.	Crown glass; both sides rubbed on emery.	
Exper. 138.	0	$68\frac{1}{2}$	68	
	5	$92\frac{3}{8}$	73 . . . $23\frac{7}{8}$: 5 = .209.	It stops 791 scattered rays of fire-heat.

	Min.	Fire.	Coach glass; both sides rubbed on emery.	
Exper. 139.	0	67	67	It stops 854 scattered rays of fire-heat.
	5	88	$70\frac{1}{2}$ . . . 21 : $3\frac{1}{2}$. cor. – $\frac{1}{2}$° = .146.	

	Min.	Fire.	Crown glass. Coach glass. } One side of each rubbed on emery.	
Exper. 140.	0	66	66	These glasses stop 849 scattered rays of fire-heat.
	5	86	$69\frac{7}{8}$ . . . 20 : $3\frac{7}{8}$. cor. – 1° = .151.	

	Min.	Fire.	Crown glass. Coach glass. } Both sides of each rubbed on emery.	
Exper. 141.	0	$66\frac{3}{4}$	$66\frac{3}{4}$	These glasses stop 897 scattered rays of fire-heat.
	5	$83\frac{3}{4}$	$68\frac{1}{2}$ . . . 17 : $1\frac{3}{4}$ = .103.	

	Min.	Fire.	The 4 glasses of the 2 preceding experiments put together.	
Exper. 142.	0	66	66	These 4 glasses stop 902 scattered rays of fire-heat.
	5	80	$67\frac{3}{8}$ . . . 14 : $1\frac{3}{8}$ = .098.	

	Min.	Fire.	Olive colour, burnt into glass.	
Exper. 143.	0	$63\frac{3}{4}$	$63\frac{3}{4}$	This glass stops 849 scattered rays of fire-heat.
	5	$85\frac{3}{4}$	$67\frac{1}{2} \ldots 22 : 3\frac{3}{4}$. cor. $- \frac{1}{2}^\circ = .151$.	

	Min.	Fire.	Paper.	
Exper. 144.	0	$66\frac{1}{2}$	$66\frac{1}{2}$	This substance stops 912 scattered rays of fire-heat; it was turned a little yellow by the exposure.
	5	$83\frac{1}{2}$	$68 \ldots 17 : 1\frac{1}{2} = .0882$.	

	Min.	Fire.	Linen.	
Exper. 145.	0	$63\frac{3}{4}$	$63\frac{3}{4}$	This substance stops 910 scattered rays of fire-heat.
	5	$84\frac{3}{4}$	$67 \ldots 21 : 3\frac{1}{4}$. cor. $- 1\frac{1}{2}^\circ = .0897$.	

	Min.	Fire.	White persian.	
Exper. 146.	0	$65\frac{3}{4}$	$65\frac{3}{4}$	This substance stops 829 scattered rays of fire-heat.
	5	$81\frac{1}{8}$	$68\frac{3}{8} \ldots 15\frac{3}{8} : 2\frac{5}{8} = .171$.	

	Min.	Fire.	Black muslin.	
Exper. 147.	0	66	66	This substance stops 706 scattered rays of fire-heat.
	5	$82\frac{1}{2}$	$70\frac{1}{2} \ldots 16\frac{1}{2} : 4\frac{1}{2}$. cor. $+ \frac{1}{2}^\circ = .294$.	

Transmission of the Invisible Rays of Solar Heat.—The same apparatus which I have used for the transmission of coloured prismatic rays, fig. 17, pl. 13, will also do for the invisible part of the heat spectrum: it is only required to add 2 or 3 more parallel lines, $\frac{1}{10}$ of an inch from each other, below the two which inclose the transmitting holes, in order to use them for directing the invisible rays of heat, by the position of the visible rays of light, to fall on the place required for coming to the thermometers.

	Min.	Invis. rays.	Bluish white glass.	
Exper. 148.	0	48	47	This glass stops only 71 invisible rays of heat.
	5	$49\frac{3}{4}$	$48\frac{5}{8} \ldots 1\frac{3}{4} : 1\frac{5}{8} = .929$.	

	Min.	Invis. rays.	Flint glass.	
Exper. 149.	0	$50\frac{3}{4}$	$49\frac{7}{8}$	This glass stops no invisible rays of heat
	5	52	$51\frac{1}{8} \ldots 1\frac{1}{4} : 1\frac{1}{4} = 1.000$.	

	Min.	Invis. rays.	Crown glass.	
Exper. 150.	0	$50\frac{1}{2}$	$49\frac{3}{4}$	
	5	$51\frac{7}{8}$	$50\frac{7}{8} \ldots 1\frac{3}{8} : 1\frac{1}{8} = .818$.	It stops 182 invisible rays of heat.

	Min.	Invis. rays.	Coach glass.	
Exper. 151.	0	$54\frac{1}{2}$	$53\frac{7}{8}$	
	5	$55\frac{3}{8}$	$54\frac{5}{8} \ldots \frac{7}{8} : \frac{3}{4} = .857$.	It stops 143 invisible rays of heat.

	Min.	Invis. rays.	Calcinable talc.	
Exper. 152.	0	$51\frac{3}{8}$	$50\frac{3}{4}$	This substance stops 250 invisible rays of heat.
	5	$52\frac{7}{8}$	$51\frac{7}{8} \ldots 1\frac{1}{2} : 1\frac{1}{8} = .750$.	

	Min.	Invis. rays.	Dark red glass.	
Exper. 153.	0	$47\frac{5}{8}$	$46\frac{3}{4}$	This glass stops no invisible rays of heat. This accounts for the strong sensation of heat felt by the eye, in looking at the sun through a telescope, when red darkening glasses are used.
	5	$48\frac{5}{8}$	$47\frac{3}{4} \ldots 1 : 1 = 1.000$.	

	Min.	Invis. rays.	Orange glass.	
xper . 154.	0	$51\frac{5}{8}$	51	
	5	53	$52 \ldots 1\frac{3}{8} : 1 = .727$.	It stops 273 invisible rays of heat.

	Min.	Invis. rays.	Yellow glass.	
Exper. 155.	0	$51\frac{3}{4}$	51	
	5	53	$52 \ldots 1\frac{1}{4} : 1 = .800$.	It stops 200 invisible rays of heat.

	Min.	Invis. rays.	Pale green glass.	
Exper. 156.	0	$51\frac{7}{8}$	$51\frac{1}{4}$	
	5	$52\frac{7}{8}$	$51\frac{7}{8} \ldots 1 : \frac{5}{8} = .625$.	It stops 375 invisible rays of heat.

	Min.	Invis. rays.	Dark green glass.	
Exper. 157.	0	$51\frac{7}{8}$	$51\frac{1}{2}$	
	5	$52\frac{7}{8}$	$52 \ldots 1 : \frac{1}{2} = .500$.	It stops 500 invisible rays of heat.

	Min.	Invis. rays.	Bluish green glass.	
Exper. 158.	0	53	$52\frac{1}{4}$	
	5	$54\frac{1}{4}$	$52\frac{1}{2} \ldots 1\frac{1}{4} : \frac{1}{4} = .200$.	It stops 800 invisible rays of heat.

	Min.	Invis. rays.	Pale blue glass.	
Exper. 159.	0	$51\frac{7}{8}$	$51\frac{1}{4}$	
	5	$53\frac{3}{8}$	$51\frac{7}{8} \ldots 1\frac{1}{2} : \frac{5}{8} = .417.$	It stops 583 invisible rays of heat.
	Min.	Invis. rays.	Dark blue glass.	
Exper. 160.	0	$52\frac{1}{8}$	$51\frac{5}{8}$	
	5	$52\frac{7}{8}$	$52\frac{1}{4} \ldots \frac{3}{4} : \frac{5}{8} = .833.$	It stops 167 invisible rays of heat.
	Min.	Invis. rays.	Indigo glass.	
Exper. 161.	0	$52\frac{7}{8}$	$52\frac{1}{4}$	
	5	$54\frac{3}{8}$	$53 \ldots 1\frac{1}{2} : \frac{3}{4} = .500.$	It stops 500 invisible rays of heat.
	Min.	Invis. rays.	Pale indigo glass.	
Exper. 162.	0	$52\frac{3}{4}$	$52\frac{1}{8}$	
	5	$53\frac{3}{4}$	$52\frac{7}{8} \ldots 1 : \frac{3}{4} = .750.$	It stops 250 invisible rays of heat.
	Min.	Invis. rays.	Purple glass.	
Exper. 163.	0	$51\frac{1}{4}$	$50\frac{3}{8}$	
	5	$52\frac{5}{8}$	$51\frac{3}{8} \ldots 1\frac{3}{8} : 1 = .727.$	It stops 273 invisible rays of heat.
	Min.	Invis. rays.	Violet glass.	
Exper. 164.	0	$53\frac{1}{4}$	$52\frac{3}{8}$	
	5	$54\frac{1}{4}$	$53\frac{1}{8} \ldots 1 : \frac{3}{4} = .750.$	It stops 250 invisible rays of heat.
	Min.	Invis. rays.	Crown glass; one side rubbed on emery, rough side exposed.	
Exper. 165.	0	$49\frac{1}{2}$	$48\frac{3}{4}$	This glass, so prepared, stops 600 scattered invisible rays of heat.
	5	$50\frac{3}{4}$	$49\frac{1}{4} \ldots 1\frac{1}{4} : \frac{1}{2} = .400.$	
	Min.	Invis. rays.	Coach glass; one side rubbed on emery, rough side exposed.	
Exper. 166.	0	54	$53\frac{3}{8}$	It stops 500 scattered invisible rays of heat.
	5	$55\frac{1}{4}$	$54 \ldots 1\frac{1}{4} : \frac{5}{8} = .500.$	
	Min.	Invis. rays.	Crown glass; both sides rubbed on emery.	
Exper. 167.	0	50	$49\frac{1}{8}$	It stops 600 scattered invisible rays of heat.
	5	$51\frac{1}{4}$	$49\frac{5}{8} \ldots 1\frac{1}{4} : \frac{1}{2} = .400.$	
	Min.	Invis. rays.	Coach glass; both sides rubbed on emery.	
Exper. 168.	0	$54\frac{3}{4}$	$54\frac{1}{8}$	It stops 714 scattered invisible rays of heat.
	5	$55\frac{5}{8}$	$54\frac{3}{8} \ldots \frac{7}{8} : \frac{1}{4} = .286.$	
	Min.	Invis. rays.	Calcined talc.	
Exper. 169.	0	$51\frac{7}{8}$	$50\frac{7}{8}$	This substance stops 889 scattered invisible rays of heat.
	5	53	$51 \ldots 1\frac{1}{8} : \frac{1}{8} = .111.$	

Transmission of Invisible Terrestrial Heat.—This is perhaps the most extensive and most interesting of all the articles we have to investigate. Dark heat is with us the most common of all; and its passage from one body into another, is what it highly concerns us to trace out. The slightest change of temperature denotes the motion of invisible heat; and if we could be fully informed about the method of its transmission, much light would be thrown on what now still remains a mysterious subject. It must be remembered, that in the following experiments, I only mean to point out the transmission of such dark heat as I have before proved to consist of rays, without inquiring whether there be any other than such existing.

My apparatus for these experiments is as follows. A box, fig. 4, pl. 14, 12 inches long, $5\frac{1}{4}$ broad, and 3 deep, has a partition throughout its whole length, which divides it into 2 parts. At one end of each division is a hole $\frac{3}{4}$ inch in diameter; and each division contains a thermometer, with its ball exposed to the hole, and at 1 inch distance from the outside of the box. Four inches of the box, next to the holes are covered; the rest is open. In the front of it is a narrow slip of wood, on which may rest any glass to be tried; and it is held close to the wood at the top, by a small spring applied against it. Two screws are planted on the front, one on each side, which may be drawn out or screwed in, by way of accurately adjusting the distance of the thermometer from the line of action. In order to procure invisible terrestrial heat, I have tried many different ways, but a stove is the most commodious of them. Iron is a substance that transmits invisible heat very readily; while, at the same time, it will most effectually intercept every visible ray of the fire by which it is heated, provided that be not carried to any great excess. I therefore made use of an iron stove, fig. 5, having 4 flat sides, and being constructed so as to exclude all appearance of light. I had it placed close to a wall, that the pipe which conveys away smoke might not scatter heat into the room.

The thermometer box, when experiments are to be made, is to be put into an arrangement of 12 bricks, placed on a stand, with casters: these bricks, fig. 6, when the stand is rolled close to the stove, which must not be done till an experiment is to begin, form an inclosure, just fitting round the sides, bottom, and covered part of the top of the thermometer box, and completely guard it against the heat of the stove. The box is then shoved into the brick opening, close to the iron side of the stove, where the two front screws, coming into contact with the iron plate, give the thermometers their proper distance; which, in the following experiments, has been such as to bring the most advanced part of the balls to $1\frac{4}{10}$ inch from the hot iron. It will be necessary to remark, that on calculating the transmissions for the 5th minute, I found that it would not be doing justice to the stopping power of the glasses, to take so long a time; for, notwithstanding the use of brickwork, and the precaution I had taken, of having two sets of it, that one might be cooling while the other was employed, and though neither of them was ever very hot, yet I found that so much heat came to the box, that when it was taken out of the bricks, in order to be cooled, the thermometers continued still to rise, at an average, about 2° higher than they were. I have therefore now taken the 3d minute, as a much safer way to come at the truth.

	Min.	Stove.	Bluish white glass.	
Exper. 170.	0	56	$55\frac{3}{4}$	
	3	$59\frac{3}{4}$	$56\frac{7}{8} \ldots 3\frac{3}{4} : 1\frac{1}{8} = .300.$	This glass stops 700 invisible rays of heat.
	Min.	Stove.	Flint glass.	
Exper. 171.	0	$53\frac{3}{4}$	$53\frac{1}{2}$	
	3	$55\frac{5}{8}$	$54\frac{3}{8} \ldots 1\frac{7}{8} : \frac{7}{8} = .467.$	It stops 533 invisible rays of heat.
	Min.	Stove.	Crown glass.	
Exper. 172.	0	$50\frac{1}{2}$	$50\frac{1}{2}$	
	3	$53\frac{3}{8}$	$51\frac{1}{8} \ldots 2\frac{7}{8} : \frac{5}{8} = .217.$	It stops 783 invisible rays of heat.
	Min.	Stove.	Coach glass.	
Exper. 173.	0	$50\frac{1}{2}$	$50\frac{1}{2}$	
	3	$52\frac{1}{2}$	$51\frac{1}{4} \ldots 2 : \frac{3}{4} = .375.$	It stops 625 invisible rays of heat.
	Min.	Stove.	Iceland crystal.	
Exper. 174.	0	47	$46\frac{1}{2}$	This substance stops 726 invisible rays of heat.
	3	$54\frac{3}{4}$	$48\frac{5}{8} \ldots 7\frac{3}{4} : 2\frac{1}{8} = .274.$	
	Min.	Stove.	Calcinable talc.	
Exper. 175.	0	51	$51\frac{5}{8}$	
	3	$57\frac{1}{2}$	$54\frac{1}{4} \ldots 6\frac{1}{2} : 2\frac{5}{8} = .404.$	

At the end of 5 minutes, when the box was taken out of the bricks, the talc was perfectly turned into a scattering substance; as such, it stops 596 scattered invisible rays of heat. The sun cannot be seen through it; but this I find is chiefly owing to its scattering disposition. It stops however 997 scattered rays of light.

	Min.	Stove.	Dark red glass.	
Exper. 176.	0	58	58	
	3	64¾	60½ . . . 6¾ : 2½ = .370.	This glass stops 630 invisible rays of heat.
	Min.	Stove.	Orange glass.	
Exper. 177.	0	55½	55¼	
	3	60¾	57¾ . . . 5¼ : 2½ = .476.	It stops 524 invisible rays of heat.
	Min.	Stove.	Yellow glass.	
Exper. 178.	0	57¾	57¼	
	3	61¾	59⅜ . . . 4 : 2⅛ = .531.	It stops 469 invisible rays of heat.
	Min.	Stove.	Pale green glass.	
Exper. 179.	0	51½	51½	
	3	56¼	53¼ . . . 4¾ : 1¾ = .368.	It stops 632 invisible rays of heat.
	Min.	Stove.	Dark green glass.	
Exper. 180.	0	50	49½	
	3	53¾	50⅝ . . . 3¾ : 1⅛ = .300.	It stops 700 invisible rays of heat.
	Min.	Stove.	Bluish green glass.	
Exper. 181.	0	51	51	
	3	55½	53 . . . 4½ : 2 = .444.	It stops 556 invisible rays of heat.
	Min.	Stove.	Pale blue glass.	
Exper. 182.	0	53¾	53⅝	
	3	57⅝	55⅜ . . . 3⅞ : 1¾ = .452.	It stops 548 invisible rays of heat.
	Min.	Stove.	Dark blue glass.	
Exper. 183.	0	53½	53	
	3	55⅞	53⅞ . . . 2⅜ : ⅞ = .368.	It stops 632 invisible rays of heat.
	Min.	Stove.	Indigo glass.	
Exper. 184.	0	54⅛	54	
	3	59⅝	55⅞ . . . 5½ : 1⅞ = .341.	It stops 659 invisible rays of heat.
	Min.	Stove.	Pale indigo glass.	
Exper. 185.	0	53½	53½	
	3	59¾	55⅜ . . . 6¼ : 1⅞ = .300.	It stops 700 invisible rays of heat.
	Min.	Stove.	Purple glass.	
Exper. 186.	0	51¾	51⅛	
	3	56⅜	52⅜ . . . 4⅝ : 1¼ = .270.	It stops 730 invisible rays of heat.
	Min.	Stove.	Violet glass.	
Exper. 187.	0	51	51½	
	3	55¾	53 . . . 4¾ : 1½ = .316.	It stops 684 invisible rays of heat.
	Min.	Stove.	Crown glass; one side rubbed on emery.	
Exper. 188.	0	49¼	49¼	This glass, so prepared, stops 775 invisible rays of scattered heat.
	3	54¼	50⅜ . . . 5 : 1⅛ = .225.	
	Min.	Stove.	Coach glass; one side rubbed on emery.	
Exper. 189.	0	50	50	
	3	57¼	51⅞ . . . 7¼ : 1⅞ = .259.	It stops 741 invisible rays of scattered heat.
	Min.	Stove.	Crown glass; both sides rubbed on emery.	
Exper. 190.	0	52	52	
	3	58	53 . . . 6 : 1 = .167.	It stops 833 invisible rays of scattered heat.
	Min.	Stove.	Coach glass; both sides rubbed on emery.	
Exper. 191.	0	52	52	
	3	55¼	52¾ . . . 3¼ : ¾ = .231.	It stops 769 invisible rays of scattered heat.
	Min.	Stove.	Olive colour, burnt in glass.	
Exper. 192.	0	51½	51½	
	3	57	53½ . . . 5½ : 2 = .364.	It stops 636 invisible rays of scattered heat.
	Min.	Stove.	White paper.	
Exper. 193.	0	52	52	This substance stops only 535 invisible rays of scattered heat.
	3	57⅜	54½ . . . 5⅜ : 2½ = .465	
	Min.	Stove.	Linen.	
Exper. 194.	0	53⅜	53⅜	
	3	57¾	55¾ . . . 4⅜ : 2⅜ = .543.	It stops 457 invisible rays of scattered heat.

ARTICLE 6. *Scattering of Solar Heat.*—We are now come to a branch of our inquiry which, from its novelty, would deserve a fuller investigation than we can at present enter into. The scattering of heat, is a reflection of it on the rough surfaces of bodies: it is therefore a principle of general influence, since all bodies, even the most polished, are sufficiently rough to scatter heat in all directions. In order therefore to compare the effect of rough surfaces on heat with their effect on light, I have made a number of experiments, from which the following are selected, for the purpose of our intended comparative view.

The apparatus I have used for scattering solar heat, is like that which served for transmissions, fig. 13, pl. 13; but here the holes through which the sun's rays enter, fig. 18, are very exactly $1\frac{1}{4}$ inch in diameter each; and are chamferred away on the under side, that no re-scattering may take place in the thickness of the covering board: the distance of the centre of the holes is 4 inches. A little more than an inch below, and under the centre of the holes, are the balls of the small thermometers A and B, well shaded from the direct rays of the sun, by small slips of wood, of the shape of the ball, and of that part of the stem which is exposed. Under each thermometer is a small tablet, fig. 19, pl. 13, on which the objects intended for scattering the sun's rays are to be placed. The tablets are contrived so as to bring the objects perpendicularly under the openings, and under the centre of the balls of the thermometers, at the distance of exactly 1 inch from them. Every thing being thus alike on both sides of the box, it is evident, from the equality of the holes, that an equal number of solar rays will fall on each object, and will by them be scattered back on the thermometers, at equal angles, and equal distances.

The first 5 experiments that follow, were made with an apparatus somewhat different from the one here described; and, though the result of them may not be so accurate as if they had been made with the present one, I must give them as they are, since time will not allow of a repetition.

	Min.	Sun.	Message card scattering.
Exper. 195.	0	64	64
	5	$69\frac{3}{4}$	$66\frac{3}{8} \ldots 5\frac{3}{4} : 2\frac{3}{8} = .413.$

Here an object of a white colour, 3.6 inches long, and 2.6 broad, scattered in 5 minutes, 413 rays of heat back on one thermometer, while the other received 1000, directly from the sun. Now, in order the better to compare the proportion of light and heat scattered by different objects, we shall put these 413 rays equal to 1000; or, which is nearly the same, multiply them by 2.421. Then, since the message card also scatters 1000 rays of light, as will be found in a table at the end of the transmission table, our present object may be made a standard for a comparison with the 4 following ones.

	Min.	Sun.	Pink-coloured paper scattering.
Exper. 196.	0	64	64
	5	70	$66\frac{5}{8} \ldots 6 : 2\frac{5}{8} = .438.$

Here a piece of pink-coloured paper, of the same dimensions with the card of the last experiment, and placed in the same situation, scattered, as we find by the same mode of multiplication, 1060 rays of heat; and, by our table, it scatters 513 of light.

	Min.	Sun.	Pale-green paper scattering.
Exper. 197.	0	$64\frac{1}{8}$	$64\frac{1}{8}$
	5	$69\frac{7}{8}$	$66\frac{1}{4} \ldots 5\frac{3}{4} : 2\frac{1}{8} = .370.$

This piece of paper scatters 896 rays of heat, and 549 of light.

	Min.	Sun.	Dark-green paper scattering.
Exper. 198.	0	$64\frac{5}{8}$	$65\frac{1}{4}$
	5	$69\frac{1}{2}$	$67\frac{3}{4} \ldots 4\frac{7}{8} : 2\frac{1}{2} = .513$

This paper scatters 1242 rays of heat, and only 308 of light.

	Min.	Sun.	Black paper scattering.
Exper. 199.	0	65½	66
	5	70⅜	68 ... 4⅞ : 2 = .410.

This paper scatters 993 rays of heat, and 420 of light.

From these experiments it seems to be evident, that in scattering heat, the colour of the object is out of the question; or, at least, that it is no otherwise concerned than as far as it may influence the texture of the surface of bodies. For here we find that pale-green, which is brighter, or scatters more light, than dark-green, yet scatters less heat. Even black, so generally known to scatter but little light, scatters much heat. But, in order to put this surmise to a fairer trial, I made the following experiments with my new machine.

Exper. 200.—I covered one of the tablets with white paper, and the other with black. The quantity of sunshine admitted through the 2 openings, of 1½ inch in diameter each, being equal, I found the heat scattered on both thermometers to be as follows.

Min.	White paper.	Black paper scattering.
0	71¾	72
5	75⅝	75 ... 3⅞ : 3 = .774

I turned now the tablets, and had,

Min.	Black paper.	White paper scattering.
0	73¼	72¾
5	75⅝	75⅞ ... 2⅜ 3⅛ = .760.

These results, agreeing sufficiently well together, show that if we make white paper our standard, and suppose it to scatter 1000 rays of heat, and 1000 of light, then will black paper scatter 767 rays of heat, and 420 of light.

	Min.	White paper.	Black muslin scattering.
Exper. 201.	0	73¾	73¾
	5	77¾	77 ... 4 : 3¼ = .813.

This scatters 813 rays of heat; and, when it is suspended so that the rays which pass through it may not be reflected, it scatters only 64 rays of light.

Exper. 202.—As my intention at present was to find a black substance that should scatter more heat than a white one, I thought it would be the readiest way to examine the white and black objects separately, that of all the white ones I might afterwards take that which scattered least, and compare it with the black one which scattered most.

Min.	White paper.	White linen scattering.
0	74⅞	75
5	79	79⅛ ... 4⅛ : 4⅛ = 1000

These objects scatter heat equally, and very nearly also light; for our table gives for linen 1008.

	Min.	White paper.	White cotton scattering.
Exper. 203.	0	74½	74⅝
	5	78⅜	78½ ... 3⅞ : 3⅞ = 1.000.

These objects scatter heat equally. White cotton scatters 1054 rays of light.

	Min.	White paper.	White muslin scattering.
Exper. 204.	0	73⅜	73⅜
	5	77⅜	76⅞ ... 4 : 3½ = .875.

White muslin scatters 875 rays of heat, and 827 of light.

	Min.	White paper.	White Persian scattering.
Exper. 205.	0	74½	74⅝
	5	77⅞	78¼ ... 3⅜ : 3⅝ = .1074.

White Persian scatters 1074 rays of heat; and, when suspended like the black muslin in the 201st experiment, it scatters 671 rays of light.

	Min.	White paper.	White knit worsted; rough side outwards.
Exper. 206.	0	51	51¾
	5	52⅝	53¾ ... 1⅝ : 2 = 1.231.

White worsted scatters 1231 rays of heat, and 620 of light.

	White paper.	White chamois leather; the smooth side exposed.
Exper. 207.	74⅝	74⅝
	78⅜	79 ... 3¾ : 4⅜ = 1.167.

White chamois leather scatters 1167 rays of heat, and 1228 of light.

	Black paper.	Black velvet scattering.
Exper. 208.	75¾	75⅞
	79¼	80 ... 3½ : 4⅛ = 1.179.

Making now black paper the standard, and supposing it to scatter 1000 rays of heat, and the same of light, then black velvet scatters 1179 rays of heat, and only 17 of light. This last number we obtain, by dividing the tabular number 7, for black velvet, by .42, which is the proportion of black paper to white.

	Min.	Black paper.	Black muslin scattering.
Exper. 209.	0	75⅛	75¼
	5	78⅜	79⅛ ... 3⅞ : 3⅛ = 1.192.

Black muslin scatters 1192 rays of heat, and 43 of light.

	Min.	Black paper.	Black satin scattering.
Exper. 210.	0	$76\frac{1}{4}$	$76\frac{1}{4}$
	5	79	$80\frac{1}{8} \ldots 2\frac{3}{4} : 3\frac{7}{8} = 1.409$.

Black satin scatters 1409 rays of heat, and 243 of light.

Exper. 211.—Having now ascertained, that of all the white and black substances I had tried, white muslin scatters the least, and black satin the most heat, I placed the former on one tablet, while the latter was put on the other.

Min.	White muslin.	Black satin scattering.
0	$76\frac{3}{8}$	$76\frac{3}{8}$
5	80	$80\frac{1}{4} \ldots 3\frac{5}{8} : 3\frac{7}{8} = 1.069$.

Here the black object scattered more heat than the white one; but, in order to try again the equality of the tablets and apparatus, I placed the objects under the opposite thermometers, and had as annexed.

Min.	Black satin.	White muslin scattering.
0	78	78
5	$80\frac{5}{8}$	$80\frac{1}{2} \ldots 2\frac{5}{8} : 2\frac{1}{2} = 1.050$.

So that, notwithstanding some little difference in the apparatus, or other unavoidable circumstances, the black object gave again the greatest scattering of heat; and consequently as no colour can be more opposite than black and white, colour can have no concern in the laws that relate to the scattering of heat.

Exper. 212.—I wished now to try some experiments of the scattering power of metals, and had some plates of iron, brass, and copper, 2 inches square, set flat, and smooth-filed, by round strokes.

Min.	Iron.	Copper scattering.
0	74	$73\frac{3}{4}$
5	$78\frac{1}{2}$	$77\frac{7}{8} \ldots 4\frac{1}{2} : 4\frac{1}{8} = .917$.

	Min.	Tin foil.	Gold-leaf paper scattering.
Exper. 213.	0	74	74
	5	$77\frac{3}{4}$	$79\frac{5}{8} \ldots 3\frac{3}{4} : 5\frac{5}{8} = 1.500$.

But the tin foil was considerably tarnished.

Exper. 214.—Finding the form of the last experiments inconvenient, for want of a standard, I had recourse again to white paper.

Min.	White paper.	Tin foil scattering.
0	$50\frac{1}{8}$	52
5	$53\frac{3}{8}$	$54\frac{7}{8} \ldots 3\frac{1}{4} : 2\frac{7}{8} = .885$

This substance scatters 885 rays of heat, and 8483 of light.

	Min.	White paper.	Iron.
Exper. 215.	0	$51\frac{5}{8}$	$53\frac{1}{2}$
	5	$54\frac{5}{8}$	$55\frac{3}{4} \ldots 3 : 2\frac{1}{4} = .750$.

Some time having elapsed between the former observation and the present one, this plate of iron was not now so bright as before, and seems to have suffered more than brass or copper from having been laid by: it scatters now only 750 rays of heat, and 10014 of light.

	Min.	White paper.	Brass.
Exper. 216.	0	50	$51\frac{1}{4}$
	5	$53\frac{1}{8}$	$55\frac{3}{8} \ldots 3\frac{1}{8} : 4\frac{1}{8} = 1.320$.

It scatters 1320 rays of heat, and no less than 43858 of light.

	White paper.	Copper.
Exper. 217.	$49\frac{7}{8}$	$51\frac{7}{8}$
	53	$55\frac{7}{8} \ldots 3\frac{1}{8} : 4 = 1.280$.

It scatters 1280 rays of heat, and 13128 of light.

	White paper.	Gold-leaf paper.
Exper. 218.	$54\frac{3}{4}$	$55\frac{3}{8}$
	$56\frac{1}{2}$	$56 \ldots 1\frac{3}{4} : \frac{5}{8} = .357$.

I changed the tablets to see what difference there might be.

Gold paper.	White paper.
$55\frac{3}{8}$	$55\frac{7}{8}$
$56\frac{1}{8}$	$57\frac{3}{8} \ldots \frac{3}{4} : 1\frac{1}{2} = .500$

A mean between the two gives .429. Gold paper therefore scatters only 429 rays of heat, and no less than 124371 rays of light.

	Min.	Black velvet.	Gold paper scattering.
Exper. 219.	0	52	$51\frac{7}{8}$
	5	$53\frac{1}{8}$	$52\frac{1}{2} \ldots 1\frac{1}{8} : \frac{5}{8} = .556$.

I turned the tablets, in order to ascertain the difference.

Min.	Gold paper,	Black velvet.
0	51	$51\frac{3}{4}$
5	$51\frac{3}{4}$	$53 \ldots \frac{3}{4} : 1\frac{1}{4} = .600$.

From a mean of both it appears, that when black velvet scatters 1000 rays of heat, and only 7 rays of light, gold paper, on the contrary, scatters no more than 578 rays of heat, but 124371 of light.

ART. 7. *Whether Light and Heat be occasioned by the same, or by Different Rays.*—Before we enter into a discussion of this question, it appears to me that we are authorised, by the experiments which have been delivered in this paper, to make certain conclusions, that will entirely alter the form of our inquiry. Thus, from the 18th experiment it appears, that 21 degrees of solar heat were given in 1 minute to a thermometer, by rays which had no power of illuminating objects, and which could not be rendered visible, notwithstanding they were brought together in the focus of a burning lens. The same has also been proved of terrestrial heat, in the 9th experiment; where, in one minute, 39° of it were given to a thermometer, by rays totally invisible, even when condensed by a concave mirror. Hence it is established, by incontrovertible facts, that there are rays of heat, both solar and terrestrial, not endowed with a power of rendering objects visible.

It has also been proved, by the whole tenour of our prismatic experiments, that this invisible heat is continued, from the beginning of the least refrangible rays towards the most refrangible ones, in a series of uninterrupted gradation, from a gentle beginning to a certain maximum; and that it afterwards declines as uniformly to a vanishing state. These phenomena have been ascertained by an instrument which, figuratively speaking, we may call blind, and which therefore could give us no information about light; yet, by its faithful report, the thermometer, which is the instrument alluded to, can leave no doubt about the existence of the different degrees of heat in the prismatic spectrum. This consideration, as has been observed, must alter the form of our proposed inquiry; for the question being thus at least partly decided, since it is ascertained that we have rays of heat which give no light, it can only become a subject of inquiry, whether some of these heat-making rays may not have a power of rendering objects visible, superadded to their now already established power of heating bodies. This being the case, it is evident that the onus probandi ought to lie with those who are willing to establish such an hypothesis; for it does not appear that nature is in the habit of using one and the same mechanism with any 2 of our senses; witness the vibration of air that makes sound; the effluvia that occasion smells; the particles that produce taste; the resistance or repulsive powers that affect the touch: all these are evidently suited to their respective organs of sense. Are we then here, on the contrary, to suppose that the same mechanism should be the cause of such different sensations, as the delicate perceptions of vision, and the very grossest of all affections, which are common to the coarsest parts of our bodies, when exposed to heat?

But, let us see what light may now be obtained from the several articles that have been discussed in this paper. It has been shown, that the effect of heat and of illumination may be represented by the two united spectra, which we have given. See fig. 12, pl. 13. Now when these are compared, it appears that those who would have the rays of heat also to do the office of light, must be obliged to maintain the following arbitrary and revolting positions; namely, that a set of rays conveying heat, should all at once, in a certain part of the spectrum, begin to give a small degree of light; that this newly acquired power of illumination should increase, while the power of heating is on the decline; that when the illuminating principle is come to a maximum, it should, in its turn, also decline very rapidly, and vanish at the same time with the power of heating. How can effects that are so opposite be ascribed to the same cause? First of all, heat without light; next to this, decreasing heat, but increasing light; then again, decreasing heat and decreasing light. What modification can we suppose to be superadded to the heat-making power, that will produce such inconsistent results?

We must not omit to mention another difference between light and heat, which may be gathered from the same article of the refrangibility of heat-making rays. It is, that though light and heat are both refrangible, the ratio of the sines of incidence and refraction of the mean rays is not the same in both. Heat is evidently less refrangible than light; whether we take a mean refrangible ray of each, or, which I believe to be the better way of proceeding, whether we take the maximum of heat and light separately. This appears, not only from the view we have taken of the two spectra already mentioned, but more evidently from the 23d experiment, by which we find, that heat cannot be collected by a lens, to the same focus where light is gathered together. Our 5th article, in which an account has been given of the proportions of heat and light stopped by glasses and other substances, will afford us now an ample field for pointing out a striking difference between these two principles. From the 24th to the 30th experiments, we have the quantities intercepted by colourless substances as follows.

TABLE 1.

	Rays of heat.		Of light.		Rays of heat.		Of light.
Bluish-white glass stops	250	and	86	Iceland crystal	244	and	150
White flint glass	91		34	Talc	139		90
Greenish crown glass	259		203	Calcinable talc	184		288
Coach glass	214		168				

Now, by casting an eye on the above table, it will be seen immediately, that no kind of regularity takes place among the proportions of rays of one sort and of another, which are stopped in their passage. Heat and light seem to be entirely unconnected. The bluish-white and flint glasses, for instance, stop nearly 3 times as much heat as light, whereas the greenish crown glass stops only about $\frac{1}{4}$ more of the former than of the latter; but, as coloured glasses take in a much greater range, I will now also give a tabular result of the experiments that have been given relating to them.

TABLE 2.

	Rays of heat.		Of light.		Rays of heat.		Of light.
Very dark red glass stops	800	and	$999\frac{9}{10}$	Pale-blue	812	and	684
Dark-red	606		$999\frac{8}{10}$	Dark-blue	362		801
Orange	604		779	Indigo	633		$999\frac{7}{10}$
Yellow	333		819	Pale-indigo	532		978
Pale-green	633		535	Purple	583		933
Dark-green	849		949	Violet	489		955
Bluish-green	768		769				

From this table, I shall also point out a few of the most remarkable results. A yellow glass, for instance, stops only 333 rays of heat, but stops 819 of light: on the contrary, a pale blue stops 812 rays of heat, and but 684 of light. Again, a dark blue glass stops only 362 rays of heat, but intercepts 801 of light; and a dark red glass stops no more than 606 rays of heat, and yet intercepts nearly all the light; scarcely one ray out of 5000 being able to make its way through it.

Before proceeding to a more critical examination of these results, it will be necessary to add also a table of the same kind, collected from the experiments with liquids.

TABLE 3.

	Rays of heat.	Of light.		Rays of heat.	Of light.
Empty tube and 2 glasses stop	542	204	Spirit of wine	612	224
Spring water	558	211	Gin	739	626
Sea water	682	288	Brandy	791	996

To which may be joined, a table containing the stoppages occasioned by scattering substances.

TABLE 4.

	Rays of heat.	Of light.		Rays of heat.	Of light.
Rough crown glass stops	464	854	Olive colour, burnt in	839	984
Rough coach glass	571	879	Calcined talc	867	996
The 1st doubly rough	667	932	White paper	850	994
The 2d doubly rough	735	946	White linen	916	952
The 2 first together	698	969	White persian	760	916
The 2 next together	800	979	Black muslin	714	737
The 4 first together	854	995			

We shall now enter more particularly into the subject of these 4 tables, that we may, if possible, find a criterion by which to judge whether heat and light can be occasioned by the same rays or not. Now this I think will be obtained, if we can make it appear that stopping one sort of rays does not necessarily bring on a stoppage of the other sort; for if it can be shown that heat and light are in this respect independent of each other, it will follow that they must be occasioned by different rays; and I shall make all possible objections to the arguments I mean to draw from these tables, in order to show that no hypothesis will evade the force of our conclusions. It has been noticed, that bluish-white and flint glasses stop nearly 3 times as much heat as light; whereas crown glass stops only about ¼ more of the former than of the latter. Now in answer to this it may be alleged, "that the ingredients of which the former glasses are made, dispose them probably to stop the invisible rays of heat, and that consequently a great interception of it may take place, without bringing on a necessity of stopping much light; and that, on the other hand, the different texture of crown glass may stop one sort of heat as well as the other, so that nearly an equality in this respect may be produced."

When an hypothesis is made in order to explain any phenomenon of nature, we ought to examine how it will agree with other facts; and in this case we are already furnished with experiments, which are decidedly against the supposition that has been brought forward. For the 148th and 149th experiments show that the bluish-white and flint glasses transmit all, or nearly all, the invisible rays of solar heat; whereas crown glass, by the 150th experiment, stops a considerable number of them. But, to assist the objecting argument, let it be alleged, as has been proved by the 94th experiment, that our bluish-white glass stops a considerable portion of the heat that goes with the red rays; then, if the 86 rays of light which this glass stops, are supposed to be all of that sort, the heat which will be stopped in consequence will, according to the experiment we have mentioned, amount to 86 multiplied by .375, that is, 32 rays of heat; but since 250 have been stopped, there will remain 218 to be accounted for. In this calculation, a manifest concession has been made, which ought to be explained. When I mention 86 red-coloured or red-making rays, I mean so many of them as will make up $\frac{86}{1000}$ of the whole effect of light; for the quantity of heat and light transmitted, or stopped, in all the experiments that have been given, has been reduced to what proportion it bears to unity; and having afterwards represented the joint effect of every ray of heat and light by 1000, each mean ray of heat must be the 1000th part of that effect; but a mean ray of light, though it be likewise the 1000th part of the whole effect of light, will not be so of heat, because the whole effect of the latter is partly owing to rays that have been proved to be invisible. On this account, the 86 mean rays of red light, stopped by our bluish-white glass, cannot even amount to a stoppage of 32 rays of heat, which we have allowed.

As I have made the concession on one hand, I must explain an advantage that may be claimed on the other; which is, that mean rays and promiscuous ones have already, in a former paper, been proved to differ considerably, and that it remains therefore unknown how many red-making rays we may suppose to be stopped, in order to make up 86 mean rays of light. In answer to this however, I must observe, that the number of promiscuous rays of light and of heat must always be inversely as their power of occasioning those sensations; so that if, for instance, a red ray is supposed to be twice as heating as a green one, there will only go half the number of them to make up a certain effect of heat; and, on the other hand, if a green ray should have a double power of illuminating, there will be no more than half the number of them necessary to occasion a certain effect of light. But, by my former experiments, a red ray, though much inferior to a green one, is probably fully equal in illumination to a mean ray of all the colours united together. Now as red rays have also been proved to be accompanied by the greatest heat, and as our bluish-white glass stops hardly any invisible heat rays, we have certainly gone the full length of fair concessions, by allowing all the light stopped by this glass to be of that sort; and thus it seems to be evident, that the heat which lies under the colours, if I may use this expression, may be stopped, without stopping the colours themselves.

It will not be necessary to lay much stress on this single experiment; our 2d table affords us sufficient ground on which to rest our more forcible arguments. A dark-red glass, for instance, was found to stop 606 rays of heat, and 999.8 of light. This, even at the very first view, seems to amount to a total separation of the two principles; but let us discuss the phenomenon with precision. As only one ray in 5000 can make its way through this glass, it is evident, that if the rays of light be also those of heat, there can hardly come any heat through it but what must be occasioned by rays that are invisible. It will therefore become a question to be examined, how many of this sort we can admit, if we proceed on a supposition that heat consists of light, as far as that will go. Now this we find has already been ascertained, in a great measure, by our 13th, 17th, and 18th experiments. In the 13th, 120° of heat were given to a thermometer, in 1 minute, by the rays which accompany the coloured part of the spectrum. In the 17th experiment, on the contrary, we find only 45° of heat communicated to the same thermometer, in the same time, by the invisible rays of the same spectrum. If we would be more scrupulous, the 18th experiment limits the heat from rays totally invisible even to 21°; but in order to make every possible allowance, let the proportion be the most favourable one of 120 to 45, which, reduced to mean rays of heat, will give 727 of them visible, and 273 invisible, to make up our thousand.

To return to the experiment: if the total number of rays of heat ascribed to light should accordingly be rated at 727, it is evident, from the stoppage of light of this glass, that 726 rays of heat at least must also be intercepted; and, in consequence of the 153d experiment, which shows that our glass opposes no obstruction to any of the invisible rays, we shall require no more. But, by our present experiment, this glass stops only 606 rays of heat; so that 120 of them will remain unaccounted for. Now the moment we give up the hypothesis that heat is occasioned by the rays of light, the difficulty becomes fully resolved by our 100th experiment, which shows that full $\frac{3}{10}$ of the rays that have the refrangibility of the red are actually transmitted. In order however to make a 2d attempt to overcome this difficulty, without giving up the hypothesis, it may be supposed, "that perhaps the lens, which has been used in the 13th, 17th, and 18th experiments, might stop a greater number of invisible than visible rays, and that its report therefore ought not to be depended on." Now, though it does not appear from the 148th experiment that such a supposition can have much foundation, yet since those experiments were not made with a view to ascertain the proportion of heat contained in each part of the prismatic spectrum, we cannot lay so much stress on them as the accuracy which is required in this case renders necessary. Let it therefore, contrary to our 100th experiment, be admitted, in order to explain the phenomenon of the red glass, which stops so much light and so little heat, that all the heat which it intercepts consists entirely of the rays which are visible, and that every one of the invisible rays of heat is transmitted. Then will 999.8 intercepted rays of light be equal to 606 rays of heat; and the remaining 394 will be the number of rays we are now to place to the account of the invisible heat which is transmitted.

Having thus also got rid of this difficulty, we are next to examine how other facts, collected in the same table, will agree with our new concession. A violet-coloured glass, for instance, stops 955 rays of light; these, at the rate of 999.8, or say 1000, to 606, must occasion a deficiency of 579 rays of heat. But, by our table, this glass stops only 489 of them; and there will thus be 90 rays of heat left unaccounted for. To enhance the difficulty, this glass, by our 164th experiment, stops also $\frac{1}{4}$ of the supposed 394 invisible rays, which will amount to an additional sum of 98. And our 111th experiment shows, that actually a great number of these rays, that otherwise cannot be accounted for, come from the store of heat, the rays of which are of the refrangibility of red light.

A dark-blue glass stops 801 rays of light; these, if light and heat were occasioned by the same rays, would produce a stoppage of 485 rays of heat; but we find that our glass stops no more than 362, so that 123 rays cannot be accounted for by this hypothesis. To this we should add 66 invisible rays, that is, 394 × .167, which, according to our 160th experiment, this glass also intercepts. But the 107th experiment, if we reject the hypothesis, immediately explains the difficulty; for here we plainly see, that only 71 rays of heat of the refrangibility of red light are stopped, whatever may be the stoppage of that light itself. A yellow glass stops 819 rays of light: these will occasion a stoppage of 496 rays of heat; but this glass intercepts only 333, and therefore 163 rays of heat must also remain unaccounted for. And, turning to the 155th experiment, we find that 79 rays, or $\frac{1}{5}$ of the 394 allowed to be invisible ones, are also to be added to that number.

If in the results of our 2d table we have had an excess of heat, which the last hypothesis would not account for, we shall, on the contrary, meet with a considerable deficiency, when we come to consider those of the 3d table. For instance, our tube filled with well-water, including the glasses at the end, intercepted 211 rays of light. These, at the rate of 606 to the thousand, would produce only a stoppage of 128 rays of heat; but here we find no less than 558 of them intercepted. To evade the pressure of these consequences, it may be said, "that as before every invisible ray was supposed to have been transmitted through glasses, so they may now be all intercepted by liquids." And granting this also to be possible, though by no means probable, for the great extent of these researches has not allowed sufficient time for many experiments to be made that have been planned for execution; yet even then 128 visible and 394 invisible rays to be intercepted, will only make up 522; so that a deficiency of 36 must still remain. In sea-water, the balance will stand thus: 288 rays of light give 175 rays of heat; these and 394 invisible rays make up 569; but the rays actually intercepted were 682, which argues a deficiency of no less than 113 rays.

But if I have for a moment admitted the entire stoppage of the invisible rays of heat in liquids, the same indulgence cannot be granted for the empty tube, as we know it does neither take place in glasses, nor in air. Therefore we must calculate thus: this compound of glass and air stops 204 rays of light; these can amount only to 124 rays of heat; but it is found to stop 542 of them, so that 418 remain to be accounted for. Now, we certainly can not suppose more than 100 of them to owe their deficiency to the store of invisible heat; so that 318 will still remain unaccounted for. And thus, from the 2d table, we have given instances where the assumed hypothesis of visible and invisible heat, in certain proportions, would require a greater stoppage than our experiments will admit; and now, on the contrary, it appears, that interceptions calculated according to the same hypothesis, should be less than the results in the 3d table give them. From which we conclude, that every other proportion fixed on, would always be erroneous, either in excess or in defect.

Equal contradictions may be shown to attend all endeavours to account for the results contained in our 4th table, by admitting any visible heat at all, let the quantity be what it will. To make the proof of this general, let 1000 be the total heat, and assume x for that part of it which we would suppose to be occasioned by visible rays; then will $1000 - x$ be the remainder, which must be ascribed to rays that cannot be seen. Now by our table we find that crown glass, of which one side has been rubbed on emery, stops 854 rays of light. These alone, if not a particle of invisible heat were stopped, would be equal to $.854x$ visible rays of heat, that must be intercepted by the glass. When the other side of this glass has also been rubbed on emery, it will stop 932 rays of light, which will give $.932x$, for the quantity of heat to be intercepted, on the same supposition, that all invisible rays of heat are transmitted. But, by our 4th table, we have the actual stoppage of heat of these glasses; which will therefore give us the two following equations; $.854\,x = 464$, and $.932\,x = 667$. Then, taking the first from the last, and reducing the remaining equation, we obtain $x = \frac{203}{.078}$, for the visible part of the total heat. But $\frac{203}{.078}$, or 2602, being only a part, comes out greater than 1000, which is the whole; and this being absurd, it follows that visible rays of heat cannot be admitted, in any proportion whatever. This will equally hold good with any additional stoppage of invisible heat, provided it be equal in both glasses; and of this equality, the 165th and 167th experiments can leave us no room to doubt.

But it is high time that we should now take into consideration a more direct proof, which may be drawn from our prismatic experiments. The results of them are here brought into a table, as follows.

TABLE 5.—*Stoppage of Prismatic Heat of the Refrangibility of the Red Rays, and of the Invisible Rays.*

	Red rays.	Invisible rays.		Red rays.	Invisible rays.
Bluish-white glass stops	375	71	Pale-blue	700	750
Flint glass	143	000	Dark-blue	71	167
Crown glass	294	182	Indigo	367	222
Coach glass	200	143	Pale-indigo	313	250
Iceland crystal	200	—	Purple	444	273
Calcinable talc	133	250	Violet	400	250
Dark-red glass	692	000	Crown glass, one side rough	389	600
Orange	500	273	Coach glass, ditto	500	500
Yellow	417	200	Crown glass, both sides rough	471	600
Pale-green	588	375	Coach glass, ditto	833	714
Dark-green	786	500	Calcined talc	737	889
Bluish-green	462	800			

As a necessary introduction to the decisive experiment I am going to analyse, I must remark, that it has been shown in a former Paper, that the prism separates invisible heat from the coloured spectrum, by throwing that which is less refrangible than light to one side. But it has also been proved, that heat of the same variety in refrangibility as the different colours, is also contained in every part of the coloured spectrum. The question which we are discussing at present, may therefore at once be reduced to this single point. Is the heat which has the refrangibility of the red rays occasioned by the light of these rays? For, should that be the case, as there will then be only one set of rays, one fate only can attend them, in being either transmitted or stopped, according to the power of the glass applied to them. We are now to appeal to our prismatic experiment on the subject, which is to decide the question. First, with regard to light, I must anticipate a series of highly interesting observations I have made, but which, though they certainly claim, cannot find room in this paper. These have given me the means of acting separately on either of the extremes, or on the middle of the prismatic spectrum; and by them I am assured that red glass does not stop red rays. Indeed the appearance of objects seen through such coloured glasses, till I can give those observations, will be a sufficient proof to every one that they transmit red light in abundance. Next, with regard to the rays of heat, the case is just the reverse; for, by our preceding table, the red glass stops no less than 692, out of 1000, of such rays as are of the refrangibility of red light. The incipient stoppage also, or that in 2 minutes, of which something will be said hereafter, amounts even to 750 rays.

Now, if it should be suspected, "that on account of the great breadth of prism, some invisible heat may be thrown on the spot where the red colour falls," I do not only agree to it, but am certain it cannot be otherwise: but this again will give additional weight to our present argument; for by the 153d experiment, as our last table shows, it has already been ascertained, that all such heat will be transmitted through a red glass; so that, were it not for some of this admixture, the stoppage might be still greater. Here then we have a direct and simple proof, in the case of the red glass, that the rays of light are transmitted, while those of heat are stopped, and that thus they have nothing in common but a certain equal degree of refrangibility, which, by the power of the glass, must occasion them to be thrown together into the place which is pointed out to us by the visibility of the rays of light. The manifest use of the union of these rays, arising from their equal refrangibility, will be explained at a future opportunity, when I may perhaps throw out several hints that have already occurred to me, where the contents of this paper may be applied to the useful purposes of life.

There still remains a general argument, that heat and light are occasioned by different rays, which ought not to be omitted. This, on account of the contracted state in which the experiments have been given, cannot appear from my paper; but, by an inspection of them at full length, it is proved, that the stoppage of solar heat, setting aside little irregularities, to which all observations are liable, has constantly been greater in the 1st, 2d, or 3d minute, than in the 4th or 5th; or, more accurately, nearer the beginning of the 5 minutes, than about the end of them. Now this does not happen in the transmission of light, which, as far as we know, is instantaneous; at least a failure in the brightness of an object, when first we look at it through a glass, amounting to 1, 2, or even 3 minutes, could not possibly have escaped our observation. This seems to suggest to us, that the law by which heat is transmitted, is different from that which directs the passage of light: and in that case it must become an irrefragable argument of the difference of the rays which occasion them.

The surmise of a difference in the law of the transmission of heat and of light, is considerably supported by an argument drawn from circumstances of a very different nature. In the scattered transmissions arising from rough surfaces, we find, that when crown glass, for instance, has one of its sides rubbed on emery, it will stop 205 rays of heat more than while that surface remained polished; but the effect of the roughness produced by emery scratches, is far more considerable on the rays of light; the additional stoppage of them amounting to no less than 651.

A confirmation of the same effect we have in coach glass; which, having also one side rubbed on emery, stops only 357 rays of heat more than it did before, while there is an additional stoppage of rays of light, amounting to no less than 717. Now since the interior construction of these glasses, before and after having been rubbed on emery, remains the same, these remarkable effects can only be ascribed to the roughness of their surfaces. Hence we may conclude, that as the same cause, when it acts on the rays of heat and light, produces effects so very different, it can only be accounted for by admitting the rays themselves to be of a different nature, and therefore subject to a different law in being scattered. It has already been shown, that the rays of heat are, on an average, less refrangible than those of light; and now it appears that they are also, if I may introduce a convenient term, less scatterable.

We ought now also to take a short review of the phenomena attending the transmission of terrestrial heat. The results of the experiments which have been given, are drawn into one view in the following table.

TABLE 6.

	Rays of flame.	Fire.	Invisible heat.
Bluish-white glass stops	625	750	700
Flint glass	591	750	533
Crown glass	636	722	783
Coach glass	458	714	625
Iceland crystal	516	756	726
Talc	375	713	615
Very dark red glass	636	613	
Dark-red	526	573	630
Orange	560	643	524
Yellow	583	685	531
Pale-green	500	688	632
Dark-green	739	745	700
Bluish-green	652	696	556
Pale-blue	609	676	548
Dark-blue	619	704	632
Indigo	679	721	659
Pale-indigo	571	655	700
Purple	520	679	730
Violet	500	615	684
Crown glass, one side rough	741	723	775
Coach glass, ditto	667	758	741
Crown glass, both sides rough	615	791	833
Coach glass, both	680	854	769
The two last but two, together	720	849	
The two last together	667	897	
The four last together	870	902	
Olive-colour, burnt in glass	792	849	636
White paper	792	912	535
White linen	690	910	457
White persian	593	820	
Black muslin	565	706	

Let us now examine what information we may draw from the facts which are recorded in this table. The first that must occur is, that a candle which emits light, is also a copious source of invisible heat. If this should seem to require a proof I give it as follows. That the candle emits heat along with light, the thermometer has ascertained; and, that a considerable share of this at least must be invisible, follows from comparing together the quantity of light and heat which are stopped by different glasses. The bluish-white one, for instance, stops 86 rays of light, and 625 of heat. Hence, if only visible rays of heat came from the candle, a glass stopping more light, as for instance the dark-red glass, which stops 999.8, ought to stop all heat whatever; but the fact is, that it even stops 100 rays less than the former. This instance alone shows plainly, that the existence of invisible terrestrial heat in the flame of a candle, is proved; while, on the contrary, heat derived from rays that are visible, remains yet to be established, by those who would maintain that there are any such. But, for the sake of argument, let us endeavour to explain how visible rays of heat may be reconciled with the contents of our 6th table. "Now though we must allow," it may be said, "that there is a certain quantity of candle-heat which cannot be seen, we are however at liberty to assign any ratio that this may bear to its visible heat-rays. Let us therefore begin with the bluish-white glass, and make the most favourable supposition we can, in order to explain its phenomena. Visible or invisible, it stops 625 rays of heat, and also 86 of light. Now, as in the last column of the table we have likewise the proportional quantity of invisible heat it intercepts, which is 700 out of 1000, we may surmise that the 914 rays of light, together with the 300 of the invisible rays which are transmitted, make up the 375 rays of heat which pass through the glass. Hence, by algebra, we have the number of invisible heat-rays 878, and the number of the visible ones 122. Then, to try how this will answer, if 1000 rays of light give 122 of heat, 86 will give 10; and if out of 1000 invisible rays 700 be stopped, 878 will give 615 to be intercepted. The sum of these will be 625, which is exactly the number pointed out by our table." Now this being a fair solution of one instance, let us see how it will agree with some others.

Before proceeding however, I cannot help remarking, that the supporters of visible heat-rays must feel themselves already considerably confined, as our present argument will not allow them more than 122 of such rays out of 1000. Now, if the assumption that terrestrial heat is owing to a mixture of visible and invisible rays, in the proportion of 122 of the former to 878 of the latter, be well-founded, it ought to explain every other phenomenon collected in our table. The purple-coloured glass stops 993 rays of light, which, according to our present hypothesis, should stop 121 rays of heat: it also stops 730 invisible rays, which will give 641 rays of intercepted heat; therefore this glass should stop 762 rays of heat, out of every 1000 that come from a candle; but from our table we find that it stops no more than 520, so that 242 rays cannot be accounted for. The glass with an olive colour burnt into it, stops 984 rays of light, or 120 of heat, and 637 invisible rays, or 559 of heat. The sum is 679 which that glass should stop, but it stops actually 792; so that, as in the foregoing instance we had a deficiency of 242 rays, we now have an excess of 113; which plainly shows, that no hypothesis of any other proportion between the visible and invisible rays of heat can answer to both cases; and that consequently, not only the present, but every other assumption of this kind, must be given up as erroneous.

I shall not enlarge on these arguments, as I take them to be sufficiently clear to decide the question we have had under consideration. I also forbear going into an examination of what our 6th article, which treats of scattered heat, might afford, in addition to the former arguments. It may just be remarked, that the 211th experiment points out a black object, which scatters more heat than a white one; while the case, as to light, is well known to be the reverse. The 219th experiment also shows, that the scattering of heat of gold paper is considerably inferior to that of black velvet; whereas a contrary difference, of a very great extent, is pointed out between these two substances; for black velvet scatters only 7 rays of light, while the scattering of gold paper amounts to more than 124000. I am well aware that this difference will perhaps admit of a solution on other principles than those which relate merely to the laws of scattering, and confess that many experiments are still wanting to complete this article, which cannot now be given; but as this paper is already of an unusual length, I ought rather to apologize for having given so much, than for not giving more.

Table of the Transmission of Terrestrial Scattered Light through various Substances; with a short Account of the Method by which the Results contained in this Table have been obtained.—The transmissions here delivered are called terrestrial and scattered, to distinguish them from others, which are direct and solar; and, in the use I have made of them in the foregoing paper, it has been supposed that light-making rays, whether direct and solar, or scattered and terrestrial, are transmitted in the same manner; or that the difference, if there be any, may not be considerable enough to affect my arguments materially. In this I have only followed the example of an eminent optical writer, who does not so much as hint at a possibility that there may be a difference. Before describing my apparatus, I ought to mention that it is entirely founded on the principles of the author now alluded to,* and that no other difficulty occurs in the execution of his plan, than how to guard properly against the scatterings of the lamp: for the light which this will throw on every object, must not be permitted to come to the vanes; since these scatterings cannot remain equal on both vanes, when one of them is moveable. In the following construction, the greatest difficulties have been removed; and a desirable consistency in the results of the experiments, when often repeated, has now been obtained.

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* See Traité d'Optique, page 16, fig. 5; Ouvrage posthume de M. Bouguer.—Orig.

A board about 14 feet long, and 6 inches broad, fig. 7, pl. 14, has 2 slips of deal, an inch square, fastened on the 2 sides: these make a groove, for 2 short pieces, to slide in, backwards and forwards. The 2 sliding pieces, figs. 8 and 9, carry each a small board or vane; one towards the right, the other towards the left; but so as to meet in the middle, and apparently to make but one when placed side by side. The vanes are covered with a piece of fair white paper, which is to reflect, or rather to scatter light in every direction. To one end of the board is fixed a circular piece of wood, with an opening in it, which is afterwards to be shut up by a small moveable piece, fig. 10, intended for placing the transmitting objects on. This moveable piece contains 2 holes, at the distance of 1$\frac{1}{2}$ inch from centre to centre, and $\frac{3}{4}$ inch in diameter each. Against the circular wooden screen, and close over the opening in it, is placed a lantern containing a lamp; fig. 11. Its construction is such as to admit a current of air to feed the flame from below, by means of a false bottom, and to let it out by a covered roof; and the whole of the light, by the usual contrivance of dark lanterns, is thus kept within, so as to leave the room in perfect darkness. In the front, that is towards the vanes, the lantern has a sliding door of tin-plate, in which there is a parallelogrammic hole, covered with a spout 5 inches long, of the same shape. Two or 3 such doors, with different spouts and openings, will be required to be put in, according to the experiments to be made; but the first will do for most of them.

A narrow arm is fastened to the long board, which advances about 3 feet beyond the screen, and carries a circular piece of pasteboard, that has an adjustable hole in the centre, through which the observer is to look when the experiments are to be made. At the further end of the long board is a pulley, over which a string, fastened to the back of the slider that carries one of the vanes, is made to pass. This string returns under the bottom of the long board, towards the other end, where, close to the observer, another pulley is fixed; and, after going also over this pulley, it returns at the top of the board, to the front of the same vane, to which the other end of it is fastened at the back. By pulling the string either way, the observer may bring forward the moveable vane, or draw it back, at pleasure. At the side of the long board is a scale of tens of inches, numbered from the place of the flame of the lamp, 0, 10, 20, 30, 40, and so on to 160. A pair of compasses being applied from the last 10 towards the vane, ascertains its distance from the flame, to as great an accuracy as may be required.

When the transmitting power of a glass is to be tried, it must be placed over one of the holes of the small moveable piece, which then is fastened with a button, on the opening left for it in the circular wooden screen. Then, looking through the hole of the pasteboard at the 2 vanes, and bringing that which is seen through the glass near enough to give an image equally bright with that which is seen through the open hole, the observation will be completed. Having measured the odd inches by a pair of compasses, or immediately by a scale, we deduce, as usual, the transmitting power, by taking double the logarithm of the distance of the farthest vane from the lamp, from double the logarithm of the distance of the nearest vane. The remaining logarithm is that of the transmitting power, as compared to the light coming directly to the eye from the other vane. I have now only to remark, that the use of this instrument requires some practice, especially when coloured glasses are to be examined; it will however be found, that the difference of the colour of the 2 objects, when their light is brought to an equality, may be overcome by a little abstraction, which is required for the purpose; for, by attending only to brightness, it has often happened to me, that both objects appeared at last of the same colour; which proved to be some mean between the 2 appearances considered separately.

Some glasses stop so much light, that it will be adviseable to take them by the assistance of an intermediate one. Thus, instead of comparing the open vane directly to a red glass, I settle first the ratio of the violet one to that vane; then, taking the ratio of the red to the violet, and compounding these 2 ratios, the result will be more accurate. The reason for this will be easily comprehended, when the construction of the apparatus is considered. For a red glass, immediately compared to the open vane, would require its object to be brought extremely near the lamp, while the other must remain at a very great distance. This would occasion a considerable difference in the angles, both of incidence and of reflection, between the rays falling on one vane, and on the other. But, by dividing the observation into 2 operations, we avoid the errors that might be occasioned by the former arrangement. In the following table, the 1st column contains the names of the different substances through which light has been transmitted. The 2d column shows the transmission of light, expressed in decimal fractions; or the proportion which it bears to the whole incident light considered as unity. An arithmetical complement to this fraction, or what it wants to unity, will therefore give us the proportion of light which is stopped by each of the substances contained in the 1st column; and that quantity multiplied by 1000 is placed in the 3d column.

TABLE 7.

Substances without colour.

	Transmission.	Stoppage.		Transmission.	Stoppage.
Bluish-white glass	.914	86	Iceland crystal	.850	150
Flint glass	.966	34	Talc	.910	90
Crown glass	.797	203	Easily calcinable talc	.712	288
Coach glass	.832	168			

Glasses of the prismatic colours.

	Transmission.	Stoppage.		Transmission.	Stoppage.
Very dark red glass	.0001335	999.9	Pale-blue glass	.316	684
Dark-red glass	.000188	999.8	Dark-blue glass	.199	801
Orange glass	.221	779	Indigo glass	.000281	999.7
Yellow glass	.681	319	Pale-indigo glass	.0218	978
Pale-green glass	.465	535	Purple glass	.00675	993
Dark-green glass	.0511	949	Violet glass	.0452	955
Bluish-green glass	.231	769			

Liquids.

	Transmission.	Stoppage.		Transmission.	Stoppage.
Empty tube and two glasses	.796	204	Spirit of wine and 2 glasses	.776	224
Well-water and ditto	.789	211	Gin	.374	626
Sea-water	.712	288	Brandy	.00381	996

Scattering Transmissions.

	Transmission.	Stoppage.		Transmission.	Stoppage.
Crown glass, one side rubbed on emery.	.146	854	The four first, together	.00456	995
Coach glass, ditto	.115	885	Olive colour, burnt in glass	.0160	984
Crown glass, both sides rubbed on emery.	.0685	932	Calcined talc	.00345	997
Coach glass, ditto	.0542	946	White paper	.00556	994
The two first, together	.03158	969	Linen	.0483	952
The two next, together	.0208	979	White Persian	.0841	916
			Black muslin	.263	737

Table of the Proportional Terrestrial Light Scattered by various Substances.

The same apparatus which has been used to gain the results of the preceding table, has also been employed for the following one, with no other difference than that while the vane with the white paper remained on one side, the other vane was successively covered by the objects whose power of scattering light was to be ascertained, and both vanes were viewed directly through the 2 open holes in the screen; the eye being stationed in the same place as before. It will be found, that this table contains the scattering of more objects than have been referred to in the preceding paper; but, as I made these experiments in a certain order, I thought it would be acceptable to give the table at full length. The 1st column gives the names of the objects; and the 2d contains the number of rays of light scattered by them, when compared to a standard of white paper, which is supposed to scatter 1000.

TABLE 8.

	Rays of light.		Rays of light.
White paper scatters	1000	Pale-blue paper	665
Message card	1000	Dark-blue paper	149
White linen	1008	Indigo paper, with a strong gloss	144
White cotton	1054	Dark-violet paper scatters	75
White chamois leather, smooth side	1228	Brown paper	101
White worsted	620	Black paper, with a strong gloss	420
White Persian, suspended	671	Black satin	102
White Persian, on whitish-brown paper	719	Black muslin, suspended	64
White Persian, on white Persian	818	Black muslin, upon black muslin	18
White muslin	827	Black worsted	16
Red paper	158	Black velvet	7
Deep pink-coloured paper	513	Tin-foil	8483
Pale pink-coloured paper	621	Iron	10014
Orange paper	619	Copper	13128
Yellow paper	824	Brass	43858
Pale-green paper	549	Gold-leaf paper	124371
Dark-green paper	308		

I cannot help remarking, that in making these last experiments, I found that black paper could not be distinguished from white; and that, on bringing it a little nearer to the light than it should be to make them perfectly equal, any of my friends who happened to be present, would mistake the black for the white.

XX. *An Account of the Trigonometrical Survey, carried on in* 1797, 1798, *and* 1799, *by Order of Marquis Cornwallis, Master-General of the Ordnance. By Capt. Wm. Mudge, F. R. S. Communicated by the Duke of Richmond, F. R. S.* p. 539.

This is now one more of the reports on the national military survey of this country, which has been carried on for many years, under the immediate and successive conduct of General Roy, Colonel Williams, and Capt. (now Col.) Mudge, of the Royal Artillery, and which is still continued under the direction of the same Col. Mudge.

The contents of the work now meeting the public eye, are important and numerous: it is divided into sections. The 1st contains the calculations of the sides of the principal and secondary triangles extended over the country in 1797, 1798, and 1799; with an account of the measurement of a new base line on Sedgemoor, and a short historical narrative of each year's operation. The 2d section contains the computed latitudes and longitudes of those places, on the western coast, intersected in 1795 and 1796, and also such others, since determined, as lie conveniently situated to the newly-observed meridians. This section also contains the directions of those meridians; one on Black Down, in Dorsetshire; another on Butterton Hill, in Devonshire; and another on St. Agnes Beacon, in Cornwall. Among the contents are also to be numbered the bearings, distances, &c. of the stations and intersected objects, from the parallels and meridians. The 3d and last section contains the triangles which have been carried over Essex, the western part of Kent, and portions of the counties joining the former, Suffolk, and Hertfordshire. It is with satisfaction stated, that Mr. Gardner, the chief draftsman, with his assistants, had nearly completed the survey of this extensive tract, which, no doubt, like the map of Kent, will be given to the public: the materials for these different surveys are ample, and will be found in this section, which concludes with the altitudes of the stations and mean refractions.

Before advancing farther in the work, Col. M. entertained ideas of condensing all the data in his possession, and distributing them in it; but, when he found his paper would, in that case, be too large for the Philos. Trans. he desisted, contenting himself with presenting little more than a moiety: it is, even now, he says, of inconvenient magnitude, but he could not with propriety still further abridge it, having in several instances rejected important matter. That he will therefore take an early opportunity of compiling a 4th account, in which will be given the latitudes and longitudes of those places, in Essex, Kent, &c. found in the last section. In the former accounts of this survey, the conductors were particularly guarded in not intermixing their contents with distances determined from numerous doubtful intersections; and experience has hitherto not detected above 3 or 4 errors arising from wrong bearings or misnomers. Previously indeed to the compilation of them, a great part of the objects in Sussex, Hampshire, and the Isle of Wight, were verified by Mr. Gardner, in process of an extensive survey, carried on by the order, and performed for the service of the Board of Ordnance. This gentleman will also have it in his power to detect any errors, if such exist, in the names of places to the westward; as the Master General has been pleased to issue his directions for the survey of Devonshire, and as much of Somersetshire and Cornwall as will square the work.

The principal object proposed to be accomplished in the year 1797, was the determination of the directions of meridians at proper stations, in order to afford the necessary data for computing the latitudes and longitudes of places intersected in the surveys of 1795 and 1796. From errors which are the result of computations made on the supposition of the earth's surface being a plane, it was expedient that new directions of meridians should be observed, when the operations are extended, in eastern or western directions, over spaces of 60 miles from fixed meridians. The distance from Dover to the Land's End being upwards of 300 miles, it becomes necessary, on this principle, that 4 directions of meridians should be observed; which, with that of Greenwich, amounts to 6, dividing this space into 6 nearly equal parts. Whatever might be the stations farther to the westward, which offer as fit places for these observations, Dunnose in the Isle of Wight seemed highly eligible, not only because it is removed the necessary distance from the meridian of Greenwich, but also because it commands a most extensive view of the western coast: therefore, as the direction of the meridian was observed on this station in 1793, it became necessary to fix on 3 other places only. In the selection of these stations, it was wished to have found such as should lie nearly in the same parallel, each intermediate one being visible from those east and west of it; by which means, the differences of latitude between their respective parallels would be accurately determined. When the party was at Dunnose, in the year 1793, a hill at a very considerable distance, in a direction very nearly west, was seen just rising out of the horizon. It then occurred that this spot would, at some future period, be a very proper one for a station where a new direction of the meridian might be observed. Experience, in the survey of 1795, led to the belief that this hill was actually Black Down in Dorsetshire; therefore it was determined that the operations should commence at that station, and the event verified the truth of the suppositions.

As the high land in the vicinity of Teignmouth, in Devonshire, cuts off all view of the southern extremity of Dartmoor from Black Down, the necessary alternative was, the firing of lights on some remote station, communicating with Butterton. Rippin Tor was quickly discovered to be the most proper spot; and that eminence would, in every point of view, be a most eligible one for a new direction of the meridian, if the hills in the middle of the moor were not considerably higher. It was therefore chosen only with a view of being subservient to the purpose of finding the latitude of Butterton. From Black Down, the party removed to Butterton; at which place but few observations were made, the weather being either tempestuous or hazy, during the greatest part of the time they were at that station: these were however made under favourable circumstances, in other respects, and are therefore likely to afford accurate results. As in the case of Rippin Tor, with respect to Black Down, so Hensbarrow, in Cornwall, was selected as the spot for connecting St. Agnes Beacon with the station on Butterton; for these latter are not visible from each other, the high land about St. Austle, on the northern part of which is situated Hens or Hengist barrow, being higher and intermediate. The staff to which the lights and star were referred, was placed on a hill called Hemmerdon Ball, a secondary station in the series of 1795. On the 1st of May, the party proceeded to St. Agnes Beacon; at which place the observations were completed on the 8th. After these directions of meridians were determined, the party proceeded with the survey, and for that purpose from St. Agnes Beacon repaired to Trevose Head, a promontory on the northern coast of Cornwall. Hence they continued the survey through Cornwall, Devon, and the other counties to the eastwards in succession; the account of which, and the list of the angles taken at the several stations, concludes the account of the operations of the year 1797.

The object first attained the next year, 1798, consisted in a trigonometrical survey of the counties adjacent to the northern and southern shores of the Thames. The stations in Kent, besides that of Wrotham, where a former survey ended, were Gravesend, Gad's Hill, and the Isle of Sheppey; those in Essex were Hadleigh, South End, and Prittlewell. Observations made from these places afforded data for the proposed survey. The new base on Sedgemoor was next measured. This measurement was begun in July, and finished in August; in the course of which, very little interruption arose from any inclemency of weather. It is unnecessary to enter minutely into a description of the difficulties which arose from the frequent intervention of ditches; let it suffice to observe that, possessed of a 50-feet chain in addition, and similar to the 100 feet one, these were rendered less material than they would otherwise have been. King's Sedgemoor being sufficiently level, the base was measured horizontally; an advantageous circumstance; and the process was quite similar to that used in measuring the former bases, that have been already described. After the conclusion of this operation, the party proceeded to select such stations in the neighbourhood of the base, as might afford means of connecting it with the triangles carried on in the preceding year. The two chosen for this purpose, were Dundon Beacon, and a spot near the village of Moor Lynch. The station at Ash Beacon was visited subsequent to these just spoken of, and afterwards that on the Mendip Hills, for the purpose of taking the angle between Moor Lynch and Dundon Beacon. The operations of 1798 then terminated with a diligent search after some spot in Cornwall; for a base of only 2 or 3 miles in length: this search however was fruitless.

On commencing the account of the operations in 1799, it is observed, that were the length of a degree of the meridian, in these latitudes, accurately known, the most eligible method of carrying on the survey would be, that of working between any two determined parallels of latitude, till the space between them was completed. Yet this mode would manifestly be subject to some slight innovations, from the necessity of measuring bases in certain stages of the work; it would be right however to adopt the principle for general practice. Under this idea, it would have been proper to have commenced the operations of this year in Somersetshire, and to have carried on the triangles from the neighbourhood of the new base there, into the north of Devon. It is mentioned in one of the former accounts, that a zenith sector was formerly bespoken of Mr. Ramsden, by his Grace the Duke of Richmond, for the purpose of aiding the design of measuring the length of a degree of latitude in this country. The pressure of other business caused Mr. Ramsden to lay aside this instrument, after he had considerably advanced in its construction. The real necessity however for being supplied with an instrument of this description being made known to him, he resolved to take it in hand again, and complete it. Relying on the strength of his assurances to this effect, Col. Mudge determined to relinquish the intention of proceeding to the westward; and resolved to commence this year's operations, with running up a series of triangles along the meridian of Blenheim. But as the Master-General issued directions, at this time, to survey Essex, and parts of the adjoining counties, in the same manner, and for the same purpose, as Kent had been, Col. M. was obliged to suspend, for a short time, the intention of proceeding with the measurement of a meridional degree, and to devise the best means for carrying his lordship's instructions into execution.

For this purpose therefore, before any stations were chosen in Essex, the county was very minutely examined; when it appeared that insuperable difficulties would occur, if the survey were prosecuted with the large theodolite only. The range commencing at Havering Bower, and running to Gallywide Common, cuts off a regular communication between the stations subsequently chosen in the southern and northern parts of Essex. The difficulty resulting from this circumstance was made still greater from the want of success in the endeavours to find one spot on this range, proper for a station. The eastern part was, in some degree, found more favourable; but it was discovered that, even here, the small instrument must frequently be used as a substitute for the large one. Under these disadvantages, the survey commenced in March; the large theodolite being taken to a station on Hampstead Heath. From Hampstead, the instrument and portable scaffold were carried to Langdon Hill, and thence to Triptree Heath, near Malden; whence the party repaired to Highbeech, leaving the remainder of the county to be surveyed with the small circular instrument; which seems to have been done with considerable accuracy. After the necessary observations were made at Highbeech Col. M. proceeded to Shotover Hill, in Oxfordshire; and before May elapsed had reconnoitred the country. As the distance between Inkpin Hill and Highclere, appeared to be shorter than was necessary for a base on which the northern triangles were to rest, it became certain, that their sides would depend on the base on Hounslow Heath. The only means by which the series now proposed to be carried westwards, for the double purpose of forwarding the survey, and also of finding a portion of the meridional arc, could be properly connected with the triangles in the neighbourhood of Salisbury Plain, was the side just spoken of; for the high land in the vicinity of Calne, intercepted the view of the stations on the Marlborough range, from White Horse Hill. In order however to make a connection, though imperfect, an immediate station was chosen on this high intercepting land. At Shotover Hill the party separated, each having its instrument. This article is closed with enumerating the names of the stations visited and observed, and mentioning that Shotover Hill and Cumner Hill, in Oxfordshire, were selected principally with a view of ascertaining the situations of the observatories at Oxford and Blenheim. The names of the stations were, Nuffield, White Horse Hill, and Scutchamfly, in Berkshire. Shotover Hill, Cumner Hill, Whiteham Hill, Crouch Hill, and Epwell Hill, all in Oxfordshire. Those in Gloucestershire were, Pen, Cleave, Broadway Beacon, and the Malvern Hills. The Lecky Hills, in Worcestershire. Corley and Nuneaton, in Warwickshire. Bardon Hill, Naseby Field and Barrow Hill, in Leicestershire. Arbury Hill, and Souldrop, in Northamptonshire. Quainton, Brill, Wendover, and Bow Brickhill, in Buckinghamshire. Woburn Park, and Lidlington, in Bedfordshire. Kinsworth, Lillyhoe, Berkhamstead, Tharfield, and Bushy Heath, in Hertfordshire. From the last mentioned station, the party returned to London, in October. A list is here added of the quantities of all the angles, at those stations, taken this year 1799; which is followed by a description of the situations of all the stations, where the angles were observed; by which they may be easily discovered again, on any future occasion.

Next follows the account of the measurement of the base, before-mentioned, on Sedgemoor, including the remeasurement and adjustment of the chains. The result of the whole is as follows.

The overplus of the 273d chain was measured by Mr. Ramsden, and found to be 23.517 feet; therefore the apparent length of the base was .... Feet. 27676.4830

From the measurement in the riding-house of the Duke of Marlborough, the chain A was found to exceed 100 feet, in the temperature of 54°, 0.11425 parts of an inch; to which adding the wear by the measurement on Salisbury Plain, viz. $\frac{1}{260}$, and also half the wear by the measurement of this base, viz. $\frac{1}{100}$ part of an inch, we get $\frac{0.1191}{12}$ for the excess of the chain's length above 100 feet; therefore $\frac{0.1191}{12} \times 272.8 = 2.7075$ feet; which add .... + 2.7075

The sum of all the degrees shown by the thermometer was 98511;

Feet.

therefore $(\frac{98511}{5} - 54^\circ \times 272.8) \times \frac{0.0075}{12} = 3.1069$ feet; which also add .... $+ 3.1069$

Again, from the comparison of the 50-feet chain with the standard B, it appeared that the excess above 50 feet, in the temperature of 54°, was 0.09075 parts of an inch; therefore $\frac{0.09075}{12} \times 8 = 0.0605$ part of a foot: this also add .... $+ 0.0605$

The sum of all the degrees shown by the thermometers placed by the sides of the 50-feet chain, was 1372; therefore $(\frac{1372}{5} - 54^\circ \times 4) \times \frac{.0075}{12} = 0.0365$ parts of a foot: and this add.............. $+ 0.0365$

The sum is 27682.3944

And for the reduction of the base to the temperature of 62°, viz. for 8° on the brass scale, we have $\frac{0.01237 \times 272.8 \times 8^\circ}{12} = 2.2497$ feet; which subtract .... $- 2.2497$

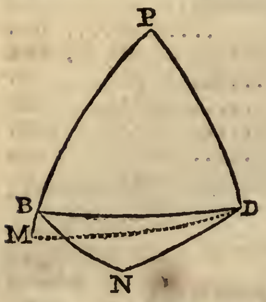
Therefore, the length of the base is 27680.1447

which, neglecting decimals, may be taken at 27680 feet. As to the probable error of the above conclusion, Col. M. knows not how to form a just opinion. On ground sufficiently hard, and otherwise favourable, he thinks a base of 5 miles might be measured so accurately, as to afford a result not differing from the truth more than 3 inches: but, on this occasion, he should not suppose the error can be less than 6, nor more than 9 inches.

Next follows a calculation of the sides of certain principal triangles in Cornwall and Devonshire, not necessary to be here detailed; and afterwards the sides of all the other angles in succession, from the west towards the eastern counties. After these particulars are stated the circumstances relating to the latitudes and longitudes of the chief stations, and the direction of the meridian at the same. After which occurs the following note:

It may probably be expected, says Col. Mudge, that I should determine the directions of the meridians at Black Down, Butterton Hill, and St. Agnes Beacon, by calculation, and afterwards compare them with the observed ones; I have desisted from the measure in the body of the work, and reserved the little I have to say for this note. If the earth were a perfect sphere, or an ellipsoid of known diameters, the direction of the meridian, at any station not very remotely situated from the parallel of another, might be determined, provided the direction of the meridian at that station were observed, and the value of the arc subtended by the space between them pretty accurately ascertained, and also the latitude of the station, at which the angle is given, nearly obtained. Thus, if it be required to find the angle at Dunnose, between Beachy Head and the meridian, from the observed angle at the latter station, and the arc between them, we shall have 39° 15′ 36″.3, the co-latitude of Beachy Head, and 55′ 28″.7 for the oblique arc. These data, two sides and an included angle, give 1° 26′ 48″.4, for the difference of longitude between Beachy Head and Dunnose, and 81° 56′ 52″.6, for the angle which the meridian at the latter makes with the former station. The difference of longitude found in a rather more correct way, has been heretofore shown to be 1° 26′ 47″.93, and the angle at Dunnose was also shown to be

81° 56′ 53″, from observation, which may be considered the same with that found by this mode of computation. In all cases in which the data were equally correct, no doubt the direction of meridians might be computed, without fear of the results deviating much from the truth; but if it be required to find the angle at Black Down, from the observed direction of the meridian at Dunnose, a different method must be used. It is however less accurate than the former one, and it has been expressly for this reason, that I have not introduced this subject into the account.



In the adjoining diagram, suppose B to be Black Down; D, Dunnose, and N, Nine Barrow Down; also let PB, the meridian of Black Down, be prolonged to M, and DM be drawn, PM being = PD. Then we shall have 3 spherical triangles, BPD, BND, BMD. Now the angle NBD was found from observations to be 4° 30′ 28″, and BND 172° 27′ 33″.5; these give the angle BDN = 3° 1′ 59″.5, nearly, because the excess of the 3 angles above 180° is 1″. The observed angle at D, Dunnose, between Nine Barrow Down and the meridian DP, or PDN, was 87° 56′ 53″; therefore 87° 56′ 53″ — 3° 1′ 59″.5 = 84° 54′ 53″.5, is the angle at D, between the meridian and the station on Black Down.

Now, the difference of longitude between B and D, or the angle at P, has been already found = 1° 20′ 46″.4; and since BP is very nearly = PD, and PD is small, we shall have rad. : tang. $\frac{1}{2}$P : cosine DP : cosine BMD = 89° 28′ 47″. But the angle PDB has been found = 84° 54′ 53″.5; therefore 89° 28′ 47″ — 84° 54′ 53″.5 = 4° 33′ 53″.5, the angle BDM; hence 180° 0′ 2″ — 94° 2′ 40″.5 = 85° 57′ 21″.5, or MBD; therefore 94° 2′ 38″.5, or DBP, is the angle at Black Down obtained in this way, which differs nearly 16″ from the observed one, viz. 94° 2′ 22″.75. It is probable, some portion of this arises from defects in the observation made at Dunnose, on the lights fired at Nine Barrow Down; only 2 lights were seen; and as the observations differed 5″ from each other, some degree of doubt exists, as to the accuracy of the angle. The angle at Nine Barrow Down, between Black Down and Dunnose, is not absolutely to be depended on for purposes of this kind, though there can be no doubt of its being sufficiently near the truth, for that to which it has been before applied. In the correction of the angles at that station, in our former accounts, we proceeded on the supposition of their being less satisfactory than the other angles of the triangles to which Nine Barrow Down is a common station. For these reasons, I am of opinion the computed angle cannot be applied as a test to the observed one; and it also appears to me, that greater objections lie against similar comparisons between the computed and observed angles at Butterton and St. Agnes; as those stations could not be seen from each other, nor the latter from Black Down. Though the computed directions of the meridians differ some seconds from the observed ones, I am by no means doubtful of the truth of the latter; as the double azimuths of the pole star, found from computation, agree very satisfactorily with those which have been used in obtaining the directions of the several meridians.—In finding the value of the oblique arc, or the line which joins Black Down and Dunnose, as used in the first method of computation, I have had recourse to the following correct expression, viz. $d = \frac{pm}{p + (m - p)\, s^2}$; where d is the length of the required degree, p that of the great circle perpendicular to the meridian, m that of a degree of the meridian itself, and s the sine of the angle constituted by the oblique arc and the meridian.

We next find a determination of the altitudes of the stations above the level of the sea; and the mean refractions deduced from observed angles of elevation and depression. After a number of individual calculations, are then inserted the following tables of altitudes and refractions at several stations.

Heights of the Stations.

Stations.	Ground above low-water mark.	Stations.	Ground above low-water mark.
Trevose Head	Feet 274	Bodmin Down	Feet 645
St. Agnes Beacon	621	Black Down	1160
Hensbarrow	1034	St. Stephen's Down	605

Stations.	Ground above low-water mark.	Stations.	Ground above low-water mark.
Bradley Knoll	Feet 973	Nuffield	Feet 757
Mendip	999	White Horse Hill	893
Westbury Down	775	Shotover Hill	599
Dundry	790	Muzzle Hill, (Brill station)	744
Lansdown	813	Whiteham Hill	576
Farley Down	700	Wendover, ground above	905
Moor Lynch	330	Bow Brickhill	683
Dundon Beacon	360	Kinsworth	904
Lugshorn Corner	49	Lillyhoe	664
Greylock's Foss-way	42	Stow on the Wold	883
Ash Beacon	655	Epwell Hill	836
Cadon Barrow	1011	Broadway Beacon	1086
Brown Willy	1368	Arbury Hill	804
Inkpin	1011		

Mean Terrestrial Refractions.

Between	Mean. Refractions.	Between	Mean Refractions.
Bodmin Down and Cadon Barrow	$\frac{1}{5}$	St. Stephen's Down and Black Down	$\frac{1}{24}$
Bradley Knoll and Westbury Down	$\frac{1}{6}$	Moor Lynch and Dundon Beacon	$\frac{1}{27}$
Maker Heights and Black Down	$\frac{1}{6}$	Dundon and Lugshorn Corner	$\frac{1}{3}$
Highclere and Inkpin	$\frac{1}{8}$	Moor Lynch and Greylock's Foss-way	$\frac{1}{6}$
St. Agnes Beacon and Trevose Head	$\frac{1}{9}$	Lugshorn Corner and Greylock's Foss-way	0
Moor Lynch and Lugshorn Corner	$\frac{1}{11}$	Cadon Barrow and horizon of the sea in the direction of Trevose Head	$\frac{1}{13}$
Hensbarrow and Trevose Head	$\frac{1}{12}$	Ditto in a northern direction	$\frac{1}{12}$
Wingreen and Bradley Knoll	$\frac{1}{13}$	Brill and Nuffield	$\frac{1}{6}$
Bodmin Down and Trevose Head	$\frac{1}{13}$	Broadway and Stow	$\frac{1}{10}$
Carraton Hill and Black Down	$\frac{1}{15}$	Epwell and Broadway	$\frac{1}{11}$
Westbury Down and Mendip	$\frac{1}{15}$	Highclere and White Horse Hill	$\frac{1}{13}$
Carraton Hill and St. Stephen's Down	$\frac{1}{15}$	Nuffield and White Horse Hill	$\frac{1}{14}$
Farley Down and Mendip	$\frac{1}{16}$	Nuffield and Bagshot	$\frac{1}{17}$
Beacon Hill and Westbury Down	$\frac{1}{16}$	Epwell and Stow	$\frac{1}{17}$
Dundry and Farley Down	$\frac{1}{16}$	Brill and Stow on the Wold	$\frac{1}{19}$
Dundon Beacon and Mendip	$\frac{1}{17}$	Wendover and Bow Brickhill	$\frac{1}{19}$
Bradley Knoll and Mendip	$\frac{1}{18}$	Kinsworth and Bow Brickhill	$\frac{1}{18}$
Lansdown and Mendip	$\frac{1}{19}$	Shotover and White Horse Hill	$\frac{1}{18}$
Moor Lynch and Dundon Beacon	$\frac{1}{20}$	Epwell and Brill	$\frac{1}{22}$
Dundry and Mendip	$\frac{1}{20}$	Bow Brickhill and Brill	$\frac{1}{32}$
Westbury Down and Farley Down	$\frac{1}{21}$		

The height of the station on Trevose Head, above the surface of the sea at low-water, was determined in 1797, by levelling. The transit instrument was used for the purpose; and there is reason to believe the result, 274$\frac{2}{10}$ feet, is within a very few inches of the truth. In the Philos. Trans. for 1797, the height of the station on Maker Heights is said to be 402 feet; this was also found by levelling. The altitude of St. Agnes Beacon, determined from that station, is 599 feet; but if the calculation be made from the base of altitude at Trevose Head, the height of that station, above the level of the sea, will be 621 feet, which gives a difference of 22 feet. It must be recollected however, that in the first result, the computation was carried through 2 intermediate stations, which gave 3 arcs, and as many mean refractions; and, considering the extreme variableness to which refractions are liable, we are assuredly not to consider 22 feet deviation from the truth as a large quantity.

Besides St. Agnes Beacon, the altitudes of Cadon Barrow, Brown Willy, Hensbarrow, and Bodmin Down, have been determined from that of Trevose Head. Of the remaining stations, some are derived from Maker Heights, others from Dunnose: most of them are mean results, that is, each station has generally been found 2 ways; and as it will serve to show what errors proceed from irregularity of refraction, and imperfection of observation, I shall exhibit a few particulars.

Height of	Deduced from	Feet.	Mean.
Black Down	Maker Heights	1169	1160
	Carraton Hill	1152	
St. Stephen's Down	Black Down	609	605
	Carraton Hill	600	
Westbury Down	Bradley Knoll	779	775
	Beacon Hill	771	
Farley Down	Mendip Hills	703	700
	Westbury Down	696	
Moor Lynch	Mendip Hills	335	330
	Ash Beacon	325	
Lugshorn Corner	Dundon Beacon	46	49
	Greylock's Foss-way	52	
Inkpin Beacon	Highclere	1014	1011
	Beacon Hill	1009	
Ash Beacon	Bull Barrow	653	655
	Bradley Knoll	657	

The above will sufficiently show what dependence is to be placed on the heights deduced from observed angles of elevation or depression; the results are indeed often less consistent, and frequently unsatisfactory; but generally they run on a parallel with these. The data from which all the heights have been computed, accompany the article. The measurement of the base on Sedgemoor, showed a fall of about 7 feet from Lugshorn Corner to Greylock's Foss-way: therefore, supposing that fall to be gradual and constant, all the way from the latter station to the surface of the sea at Bridgewater Bay, we shall get 24 feet, for the height of Lugshorn Corner from the surface of the sea. The altitude of this station, deduced from that of Trevose Head, is 49 feet; and subtracting 3 feet from it, the height of the bank on which the instrument stood above the moor, we get 46 feet for the height of the moor at Lugshorn Corner, above the level of the sea at Bridgewater Bay. But this height, supposing the fall regular, is proved to be 24 feet. There is therefore a difference of 22 feet, granting the whole of this to be an error on the side of the survey; but as the general surface of the moor at Bridgewater Bay is several feet above the surface of the sea, we may take a moiety of 24 feet, for the error of the computed height of the station at Lugshorn Corner.

The refractions contained in this account, like those in the former papers, tend to prove, that when rays of light pass horizontally, and considerably distant from the surface of the earth, they are less bent or refracted from their rectilinear courses, than theory and opinion have laid down as fact. It is very certain however, that objection lies against particular conclusions drawn from such data as we possess; because the angles of elevation and depression of corresponding stations are observed at different times, and almost always therefore under different circumstances; but with the experience and continual practice of thus obtaining means of computing these refractions, though we may not be able to determine the refracting power of the air under given circumstances, yet, as the causes which render it variable, are as likely to predominate when the angles of depression or elevation are observed from low stations as when observed from high ones, we may be enabled to make some general deductions*.

When the instrument formerly made use of by General Roy was entrusted to my care, I possessed the means of determining, in a more accurate manner than had yet been done, the refractive power of the air near the horizon. To devote much time to it, has not, as yet, been in my power; because a more rapid extension of the survey was an object of greater importance. I did not, however, lose any opportunity which the subsequent season offered; the 1st was, when the instruments were at White Horse Hill and Whiteham Hill; the 2d, when one was stationed at Brill and the other at Arbury Hill; and the 3d opportunity offered itself, when one party was stationed at the latter place and the other at Wendover. On these occasions, the instructions by which I governed myself were to observe the elevation or depression of the corresponding station at the expiration of every hour, beginning at 6 A.M. and to have the watch well regulated from observed altitudes of the sun's limb. I was also very minute in having entered in the book the state of the weather; to keep the instrument properly sheltered from the wind;

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* As many instances of strong atmospherical refraction have been related, and ingeniously accounted for, in some of the late publications of the R. S., I think it right to mention, by way of note, a very extraordinary instance of its variability. In the month of June, 1795, when the instrument and party were stationed at Pilsden Hill, in Dorsetshire, on a particular day, at about the hour of 4, I employed myself in observing the angles of depression or elevation of the surrounding hills. After I had done all that was necessary in this matter, I turned the telescope to Glastonbury Tor, and observed its depression. The air was so unusually clear, that, desirous of proving to a gentleman then with me in the observatory tent, the excellence of the telescope, I desired him to apply his eye to it: he did so, and agreeably to a desire he expressed, I again took the depression of the upper part of the old building, which I was enabled to do with great accuracy, and found it 2″ different, the first being 30′.0″ and the last 30′.2″. The unusual distinctness of this object led me to keep my eye a long time at the telescope; and while my attention was engaged, I perceived the top of the building gradually rise above the micrometer wire, and so continue to do, till it was elevated 10′45″ above its first apparent situation; it then remained stationary, and as night drew on, the object became indistinct. The following evening, I observed the depression again, and found it 29′.50″. To what cause this extraordinary change in the refraction could be owing, I am at a loss to conjecture. The former part of the day had been warm, with little wind, and cloudy. The thermometer, at the time of observation, was 65°, and continued stationary for a considerable time. The sky was cloudy, but yet, as before observed, the air was remarkably clear. The top of Glastonbury Tor, I suppose, is about 200 feet from the surface of Sedgemoor, over a considerable tract of which the line joining Pilsden with that object passes. The gentleman of whom I speak, as being with me in the tent, was Captain Darcy, of the Royal Engineers, who no doubt well remembers the circumstance.—Orig.

to be always cautious to adjust the level; and also to insert the state of the air, as to temperature and density, by noting the thermometer and barometer.

The height of the transit telescope above the ground was always $5\frac{1}{2}$ feet; therefore, an allowance must be made, at each station, for the angle which that space subtends at its corresponding one; this premised, the refraction will be found from one of the two following rules, viz. if A be the contained arc, and D, *d*, the observed depressions, the quantity answering to the refraction R, will be expressed by $\frac{A - D - d}{2}$; or, if one of the angles should be an elevation, *e*, then $R = \frac{A + e - d}{2}$: these rules give the refractions in the following table.

1. Arc. White Horse Hill and Whiteham Hill.				2. Arc. Brill and Arbury Hill.				3. Arc. Arbury Hill and Wendover.				4. Arc. Broadway Beacon and Epwell.			
Hours.	Refraction. pts. cont. arc.	Barom.	Therm.	Hours.	Refraction. pts. cont. arc.	Barom.	Therm.	Hours.	Refraction. pts. cont. arc.	Barom.	Therm.	Hours.	Refraction. pts. cont. arc.	Barom.	Therm.
		in. pts.	°			in. pts.	°			in. pts.	°			in. pts.	°
3 P.M.	$\frac{1}{10.8}$	29.5	58.0	9 A.M.	$\frac{1}{10.9}$	29.1	63.2	5	$\frac{1}{10.8}$	28.8	54.6	2	$\frac{1}{35.1}$	29.2	54.1
4	$\frac{1}{9.4}$	29.5	61.0	10	$\frac{1}{10.7}$	29.2	68.7	6	$\frac{1}{11.6}$	28.8	61.5	3	$\frac{1}{31.0}$		58.2
5	$\frac{1}{10.4}$	29.5	58.1	11	$\frac{1}{10.7}$	29.2	68.1					4	$\frac{1}{8.6}$	29.1	57.5
6	$\frac{1}{10.4}$	29.5	57.0	12	$\frac{1}{10.4}$	29.2	67.6								
7	$\frac{1}{10.0}$	29.5	57.0	3 .M.	$\frac{1}{10.4}$	29.2	72.5								
8	$\frac{1}{9.5}$	29 6	55.6	4	$\frac{1}{7.4}$	29.2	72.0								
9	$\frac{1}{12.6}$	29.6	54.5	9	$\frac{1}{9.8}$	29.2	62.3								
10	$\frac{1}{14.0}$	29.5	54.3												

On examining the refractions obtained on the first arc, we perceive them to have been tolerably regular from 3 o'clock till 8; the mean being $\frac{1}{10.1}$ part of the contained arc. The height of Whiteham Hill is 576 feet, and that of White Horse Hill 893 feet, above the level of the sea: the ray passes therefore through a tract of air considerably elevated, as the country between the stations is, for the most part, flat and low. The air is not often clear enough, or sufficiently free from tremulous motion, for these delicate observations. On the present occasion however, the state of it was highly fit for the purpose; and as care was taken, I am of opinion an error of more than 3″, taking that of the arch of altitude into the account, cannot have obtained in any of the angles. The refractions at 9 and 10 o'clock are less than at the preceding hours; but this does not appear to have been owing to any change in the refractive power of the air throughout the whole extent of the ray, because the depression of Whiteham Hill, from the other station, varied little at those hours. These changes in the observed angles of elevation at Whiteham, 44″ and 42″ being the differences, without corresponding ones at White Horse Hill, prove that some partial alteration, from floating strata, had taken place in the refraction near the former station. It will be perceived that a case may be constructed in which this will take place, causing a great variation in one of the angles, while the other apparently remains the same: and this suggested the idea, that to afford any accurate conclusions in this way, a long series of observations would be necessary. It further appears that dew could not have caused these differences at Whiteham Hill, since the same cause would equally operate to vary he observed angles at White Horse Hill; but these remained nearly the same.

The refractions on the 2d and 3d arcs, I consider as most accurate, on account of the great distance between the stations; and also as more to be depended on, from the circumstance of the ray generally passing 300 feet above the ground. The 4th arc affords another instance of the refraction varying at one station, and remaining constant at the other. This was owing to the intervention of some partial stratum of air, nearer to Epwell than Broadway Beacon. The refractions, deduced from these contemporary observations are inconclusive. The mean refractions, neglecting the 4th arc, brought under one point of view, will be as follows.

Arcs.	Mean height of ray above the sea.	Refraction. Propl. pt.	Barom.	Therm.
	Feet.		in. pts.	
1. White Horse Hill and Whiteham - -	734	$\frac{1}{10.9}$	29.5	57.8
bury Hill and Bril, 5 first refracs. - -	774	$\frac{1}{10.6}$	29.2	67.8
3. Arbury Hill and Wendover - - -	854	$\frac{1}{11.2}$	28.8	58.1

If the air had been in a quiescent state, previous to and also at the times when these observations were made, it might be expected that the differences of altitudes in the stations would be obtained, tolerably near the truth, barometrically. The remarks in the tables appertaining to the 1st and 2d arcs, shew that such opportunities offered; but those which belong to the 3d, prove the wind to have been fresh; and, as the space between the stations which constitute the extremities of that arc is 34 miles, nearly, it is not to be expected that a true result should be obtained. The differences of altitudes of the stations constituting the extremities of the first 2 arcs, obtained by means of the observed angles of elevation and depression, as well as from the heights of the mercury in the barometer, will be,

Arcs.	Obs. Ang.	Barom.	Diff.
1	317	282	35
2	60	15	45

The little that has been done on this subject, points out the necessity of doing more; it therefore remains to observe, that I shall lose no opportunity of employing the apparatus committed to my charge in the best and most diligent manner, both as relating to matters of refraction, and to all others connected with the Trigonometrical Survey.

Note referred to at page 746 of this Volume.

It was intended to subjoin to Mr. Volta's description of his electrical pile or battery, an account of Mr. Davy's discoveries, which constitute a new æra in chemistry, relative to the decomposition of the alkalies and their metallization, by means of the above-mentioned apparatus. But on further reflection it appeared, that even a summary statement of those brilliant discoveries would far exceed the limits of a note, and indeed was scarcely necessary, from the circumstance of Mr. Davy's experiments having been so lately communicated to the public, in the Philos. Trans. (see Bakerian lecture, 1808) which, from the commencement of the present century, where our labours terminate, are, or will be, it is presumed, in the possession of the Subscribers to this Abridgment.

END OF VOL. XC. OF THE ORIGINAL, AND OF THIS ABRIDGMENT.

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INDEX

TO THE

PHILOSOPHICAL TRANSACTIONS

ABRIDGED.

N. B. The Roman numerals denote the volume, and the Arabic figures the page.

ERRATA.

Vol. I. Page 233, line 3, *for* fig. 7 *read* fig. 1.
———————— 711, line 2 from bottom, *after* ABCD *add* (fig. 5, plate 15.)
Vol. II. Page 199, line 15, *for* fig. 1 *read* fig. 18.
Vol. III. Page 45, line 20, *for* plate 2 *read* plate 1.
———————— 59, last line, *for* G. *read* C.
———————— 119, line 28, *for* thee *read* thea.
———————— 452, table 2, line 2, *for* 25. 2. 4. *read* 25. 2. 3. and in line 4, *for* 1944 *read* 1644.
———————— 453, table 1, line 1, page 1, *for* 2.44 *read* 2.44½. and in line 7, *for* 2.33½ *read* 2.33.
———————— table 2, line 12, *for* 2.44. *read* 2.54.
———————— 455, table 1, line 2, *for* 6.55. *read* 6.55½, and in line 3, *for* 27. 7. 20. *read* 23. 7. 20.
Vol. VIII. In the Contents, page 4, col. 2, line 32, and page 10, col. 2, line 31, *for* Committee of the R. S. *read* Geo. Graham.
Vol. IX. Page 244, line 30, *for* tittle *read* little.
———————— 561, 562, 563, *for* D. M. *read* M. R.
Vol. X. Page 70, line 10, *read* plate I.
———————— 106, line 11, *read* fig. 16.
———————— 381, line 3, *for* A *read* Α.
———————— 676, line 22, *for* for *read* from.
Vol. XII. Page 356, line 16, *for* 9½ *read* 5½.
Vol. XIII. In the Contents, page iii. line 14, *dele* Chronology.
Vol. XVII. Page 284, note, *for* subiategra *read* subintegra.

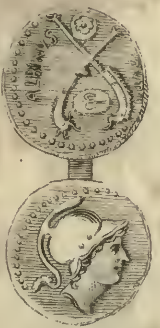
See also a few Errata at the ends, or on the pages containing references to the plates, of Vol. I. II. IV. V. VI. VII X. XIV. XV. XVI. XVII. but some of these have been corrected in a large portion of the impression while going through the press.

FINIS.

C. and R. Baldwin, Printers,
New Bridge-street, London.

Ancient Arms &c. Pa. 38, &c.

* Fig. 2.



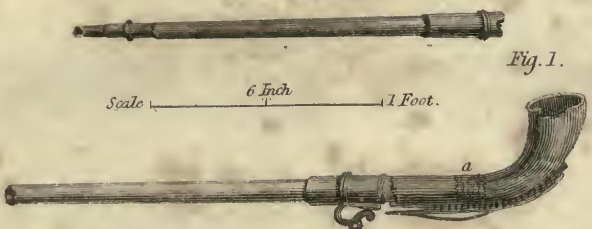
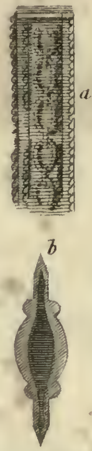


Fig. 1.



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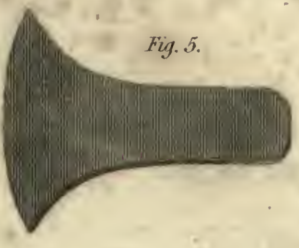

Fig. 2.

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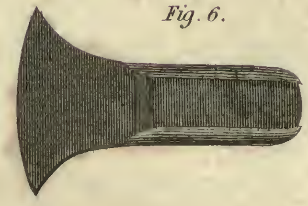

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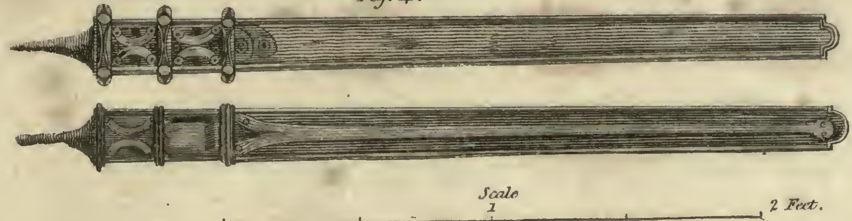

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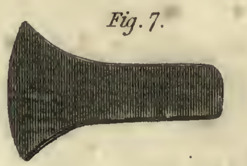

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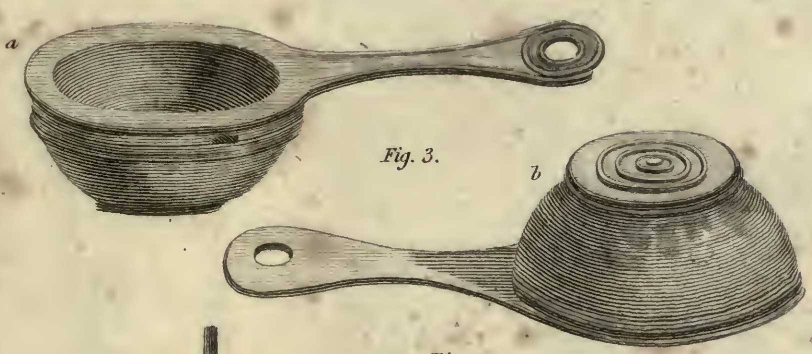
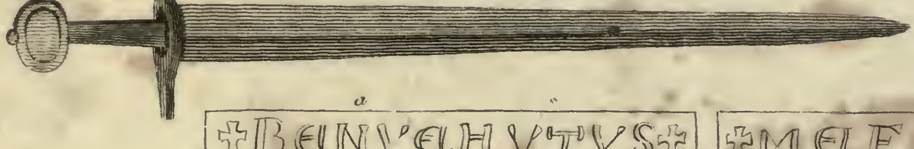


Fig. 9.

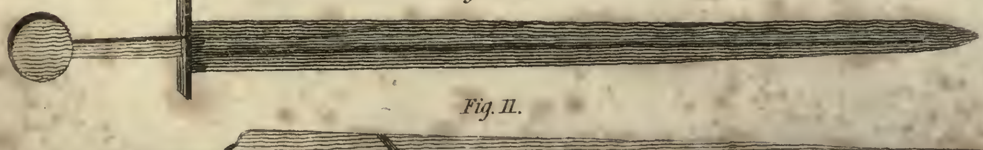
Fig. 8.



a ✠BENVENVTVS✠

b ✠ME FECIT✠

Fig. 10.



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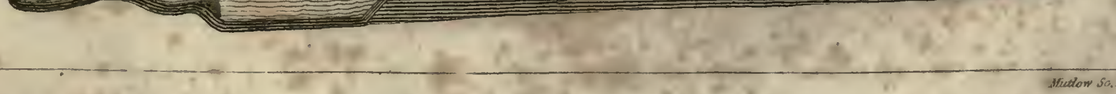

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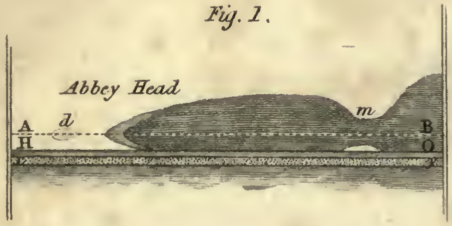
Terrestrial Refractions. Pa. 88, &c.

Fig. 1.



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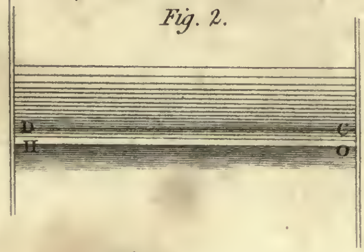

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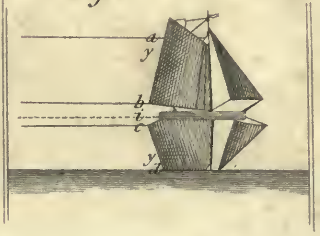

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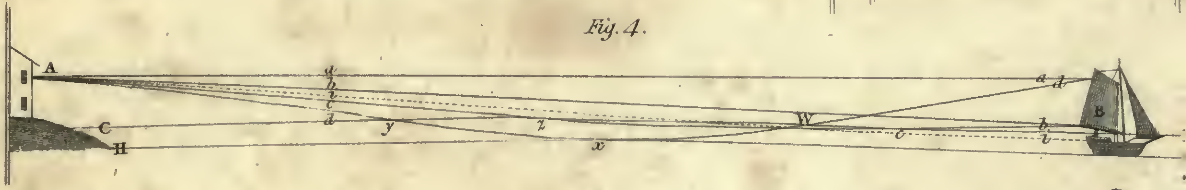

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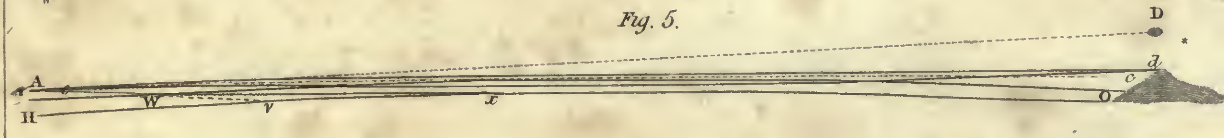

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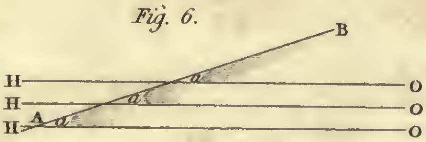

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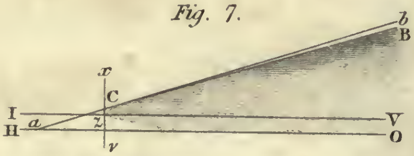

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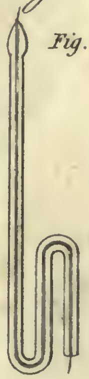

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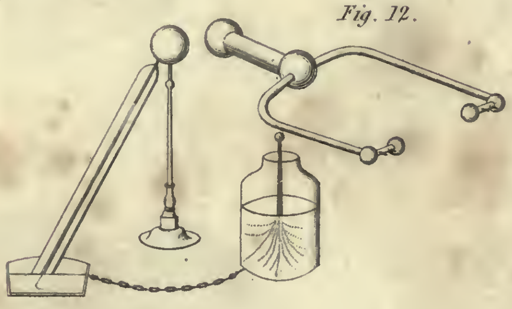


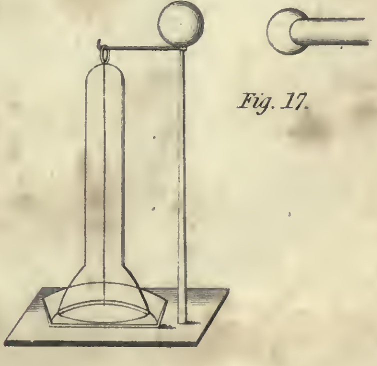
Fig. 13.

Fig. 14.

Fig. 15.

Fig. 16.

Fig. 17.



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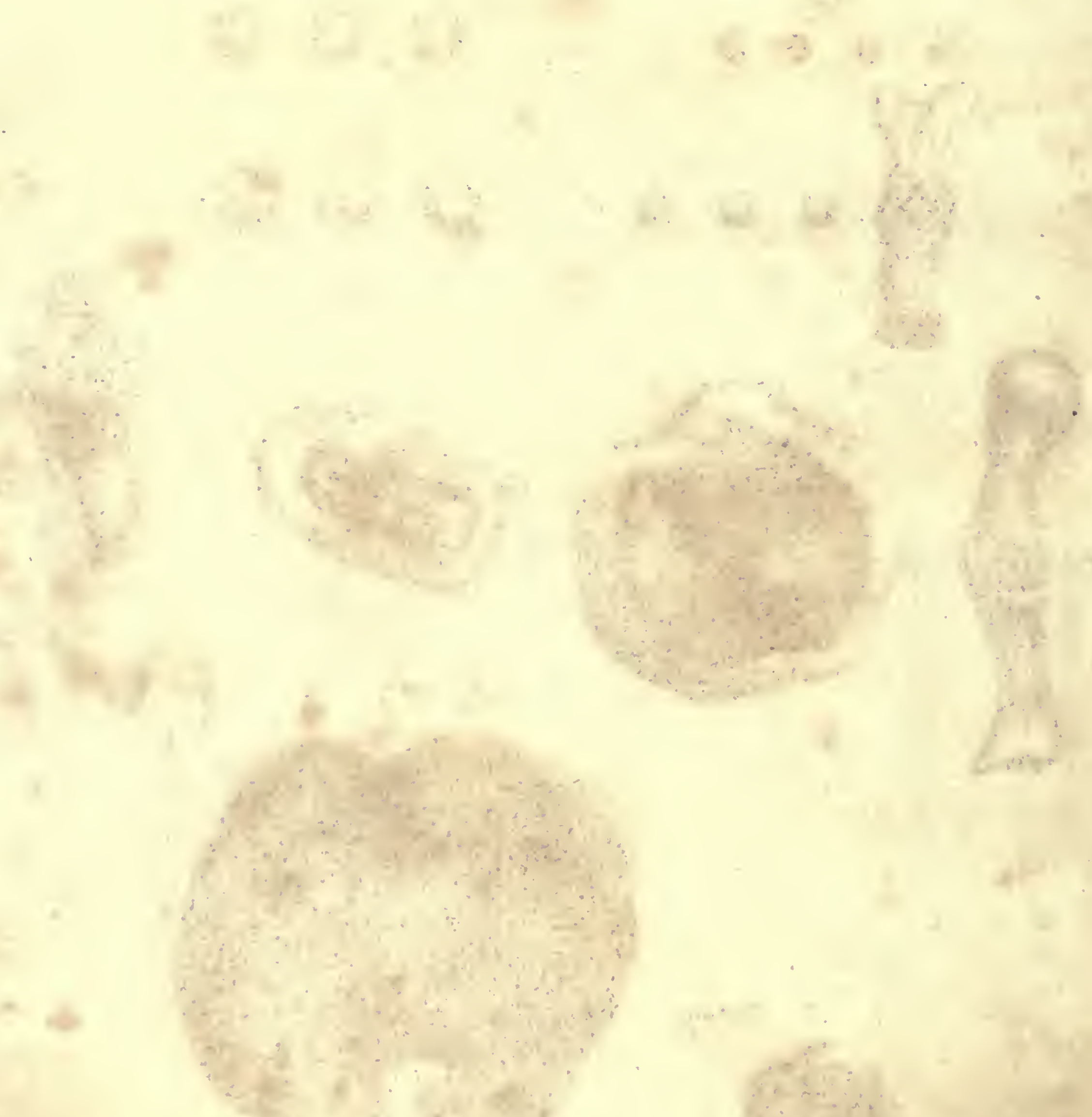

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Mutlow Sc. Ruſsell C

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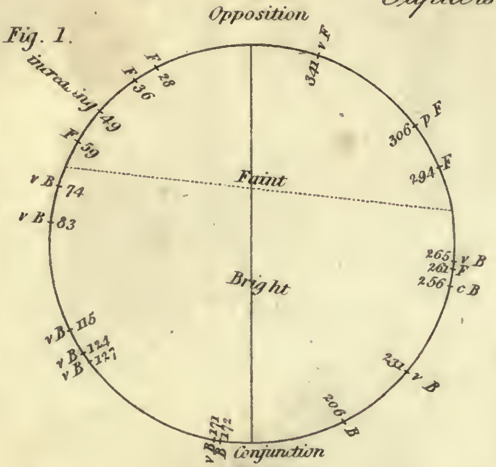
Fig. 1.



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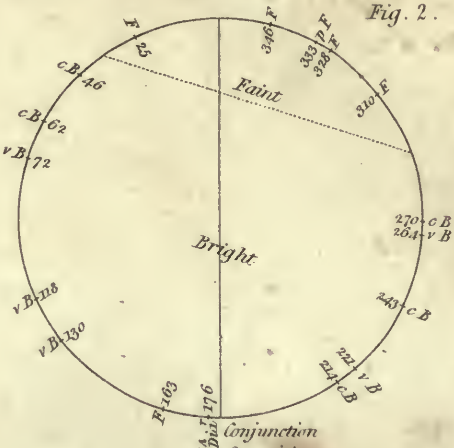

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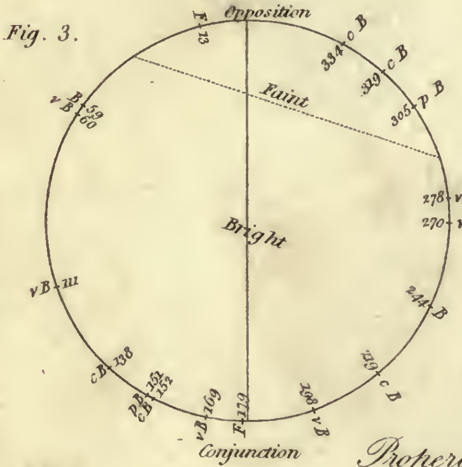

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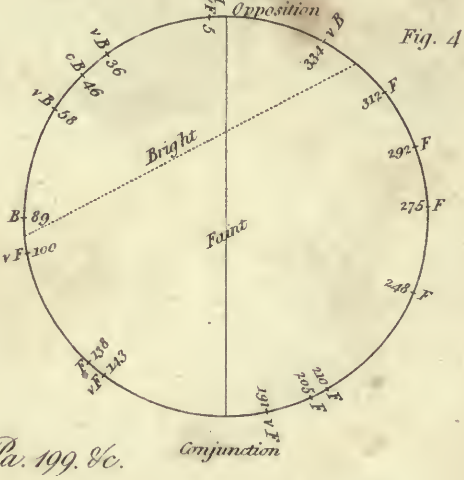

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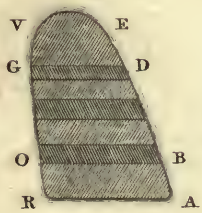

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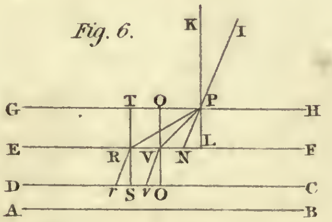

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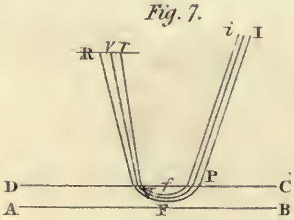

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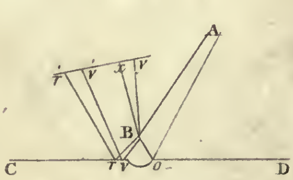

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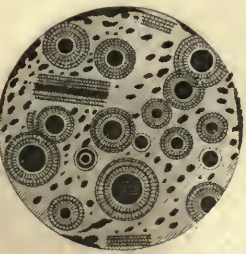

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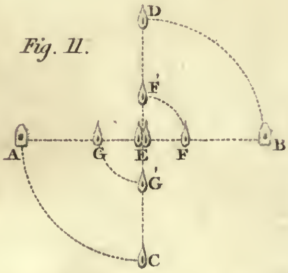

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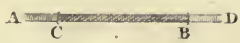

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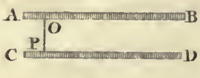

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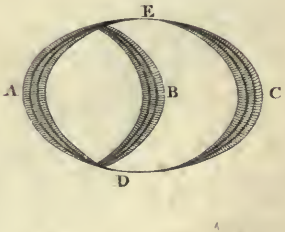

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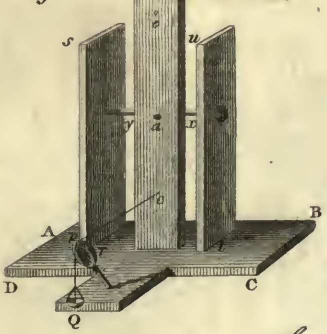
Fig. 1. Pa. 251.



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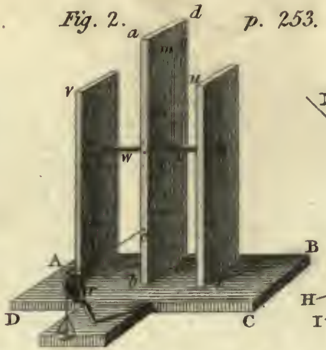
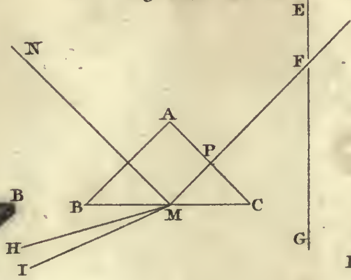


Fig 3. p. 321.



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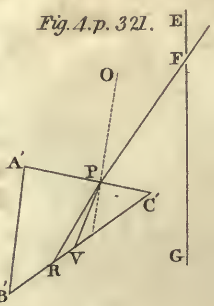

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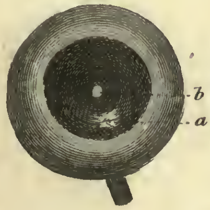

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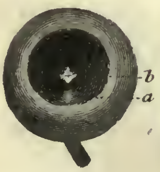

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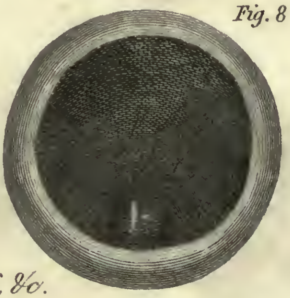

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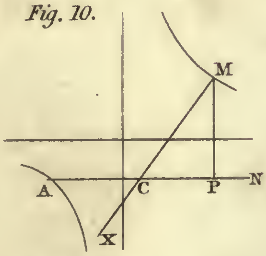

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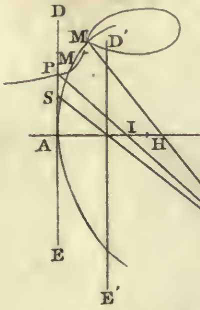

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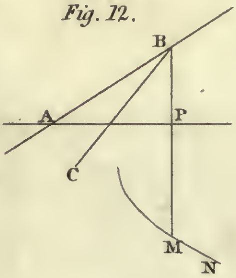

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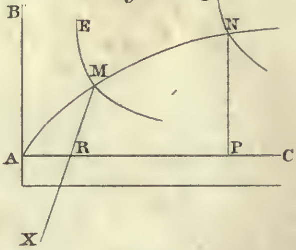

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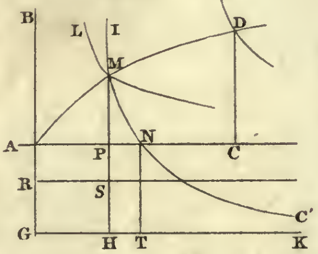

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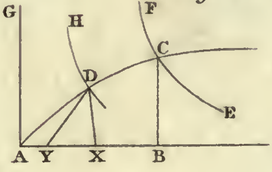

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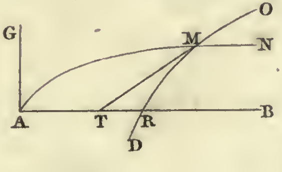

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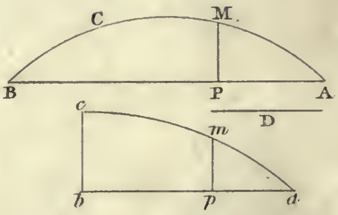

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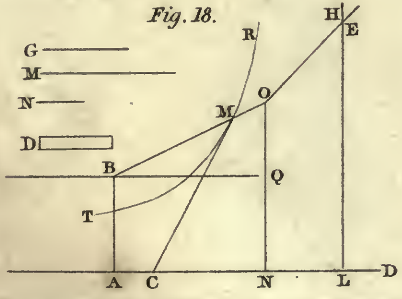

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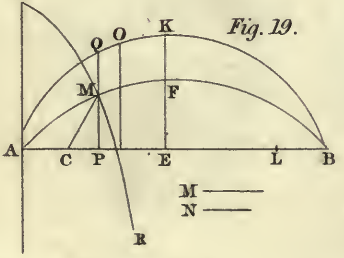

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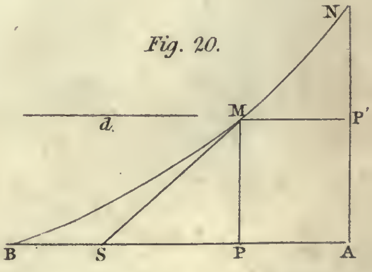

Mutlow Sc. Russell C

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Crystals of Corundam. Pa. 368. &c.

Fig. 1.

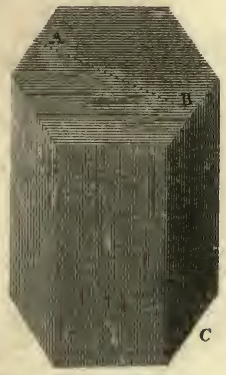
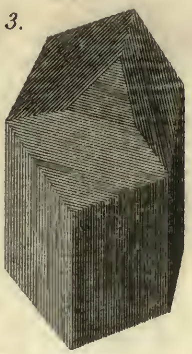


Fig. 2.

Fig. 3.



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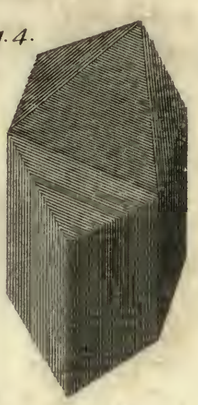

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Fig. 6.

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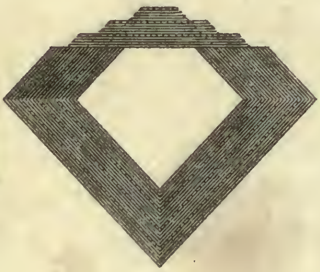

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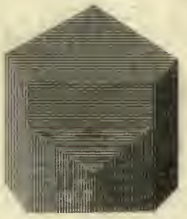
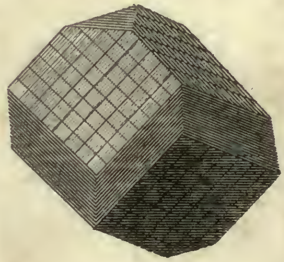


Fig. 9.



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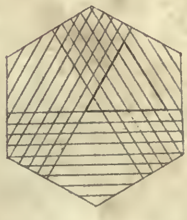

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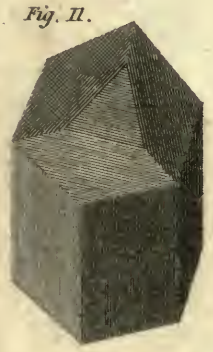

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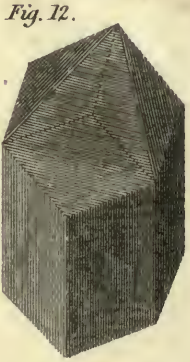

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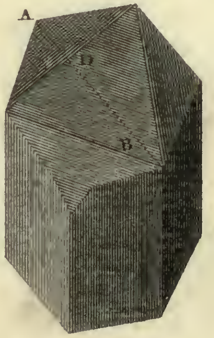

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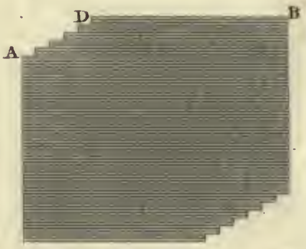

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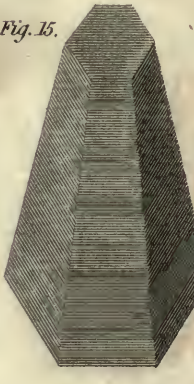

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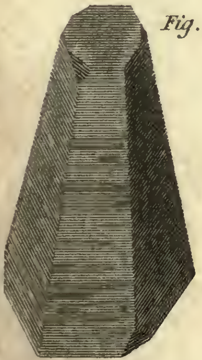

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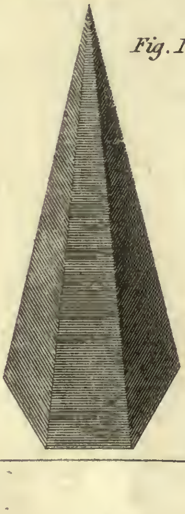

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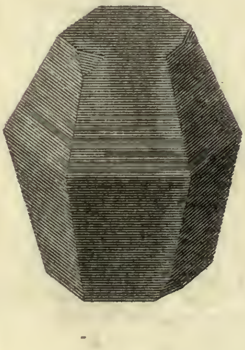

Fig. 19.

Fig. 20.

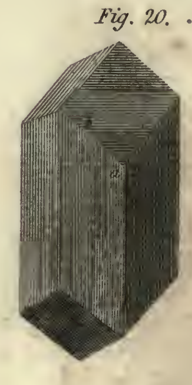

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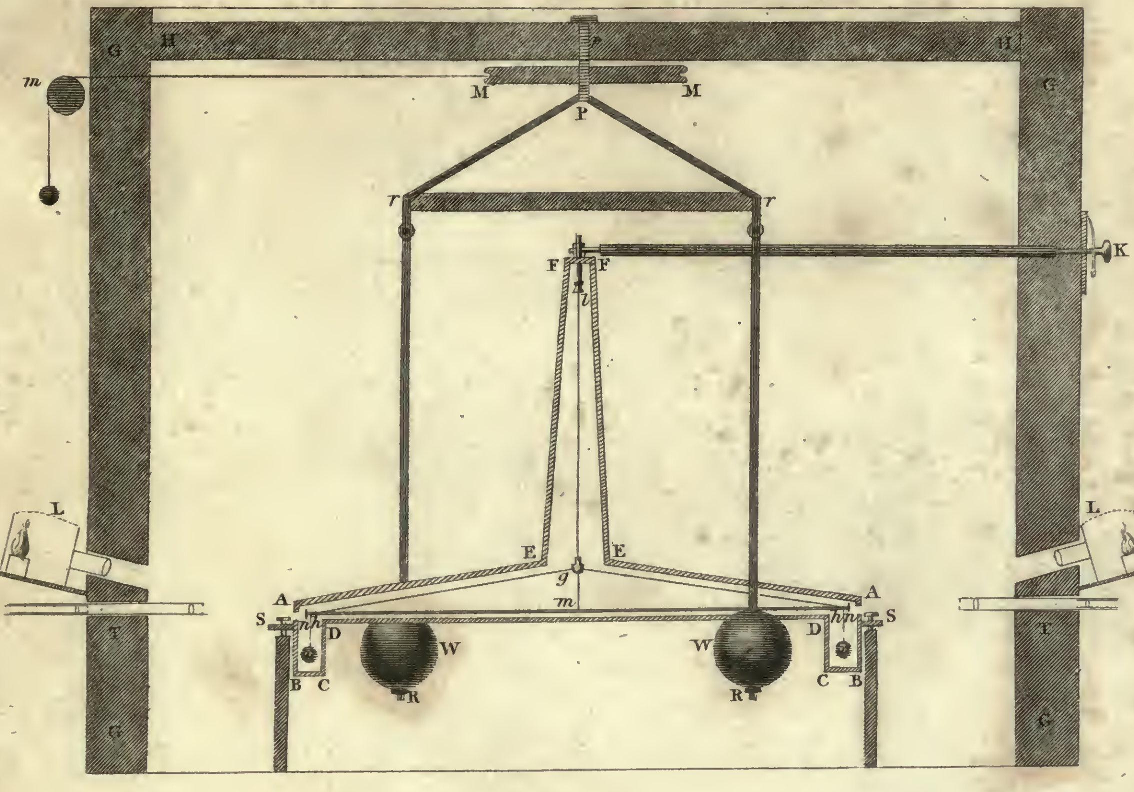
Fig. 1. Pa 389, &c.



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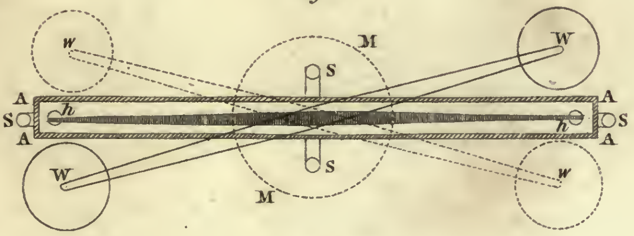

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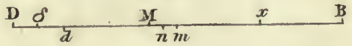

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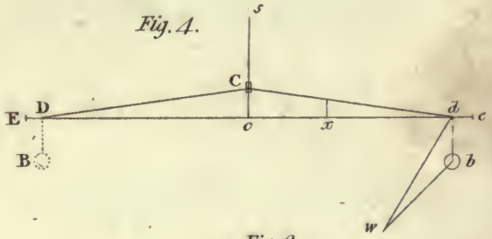

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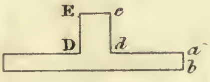

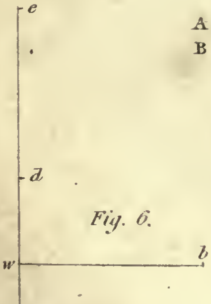

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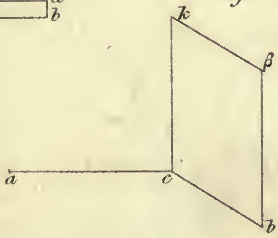

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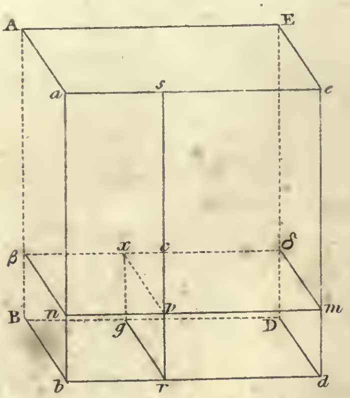

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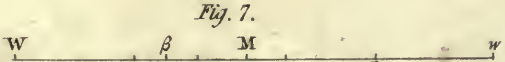

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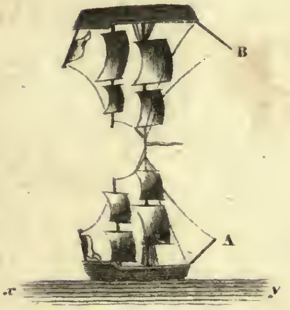
Unusual Horizontal Refractions Pa.437 &c.


Fig.1.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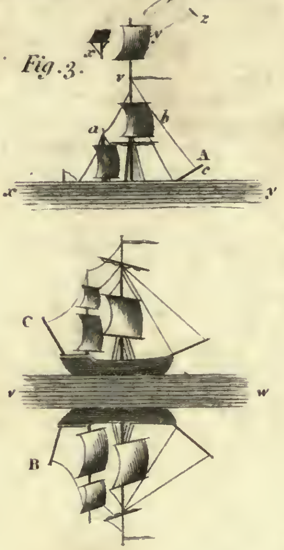
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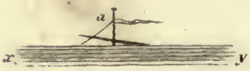
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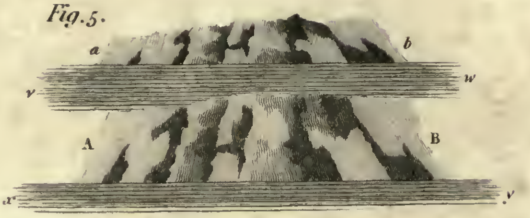
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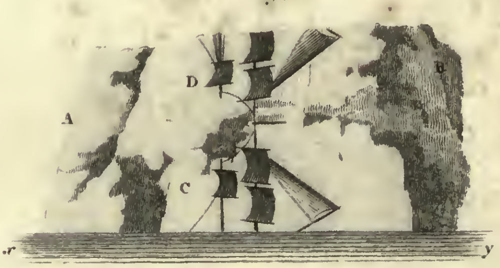
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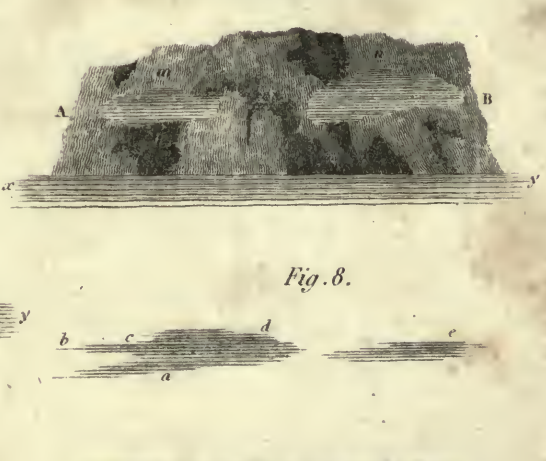
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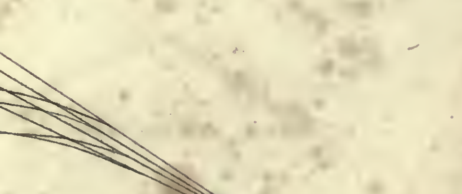
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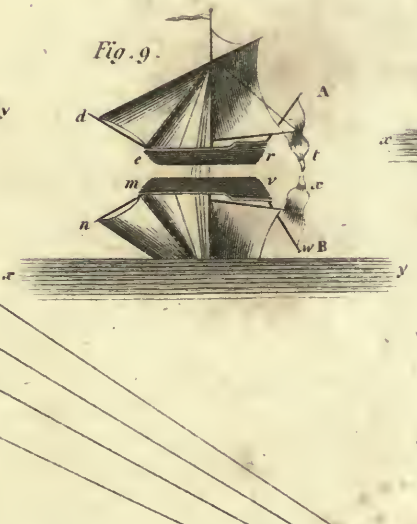
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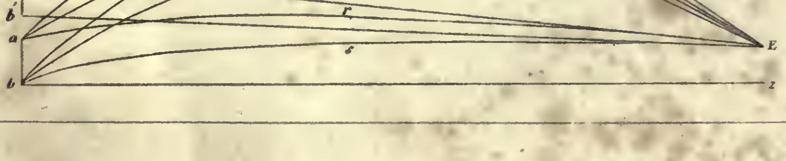
Fig.10.

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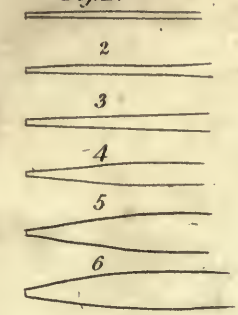
Dr. Young on Sound and Light. Pa. 625.

Fig. 1.



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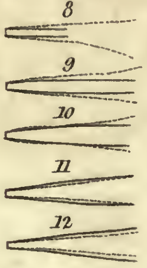

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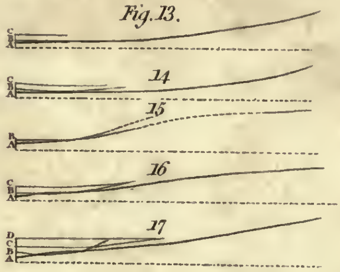

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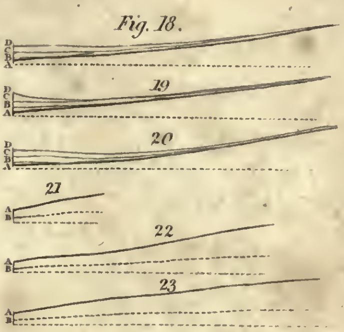

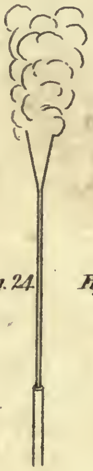

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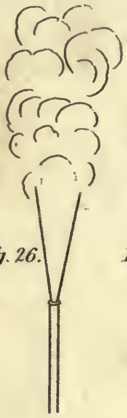
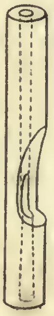


Fig. 26.

Fig. 25.

Fig. 27.



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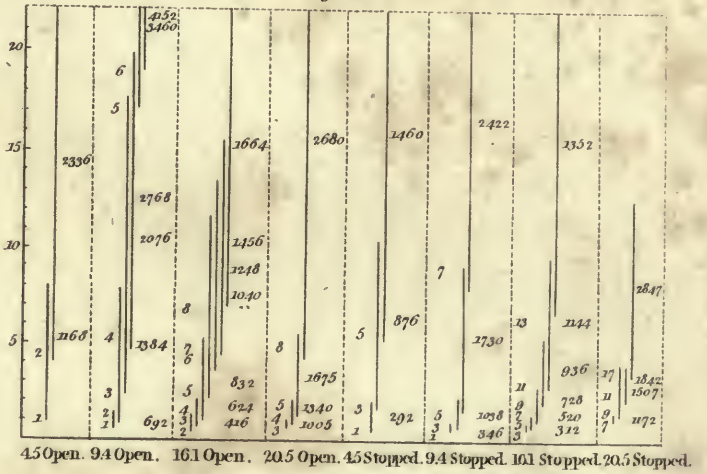

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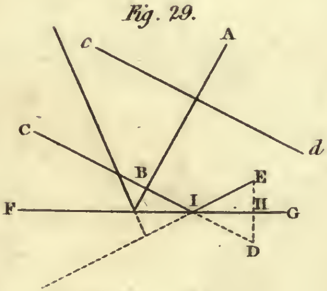

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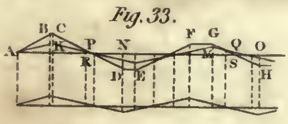

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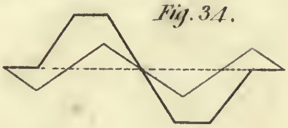


Fig. 35.

Fig. 36.

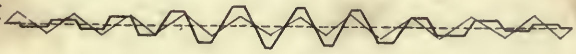


Fig. 37.

Fig. 38.

Fig. 30.

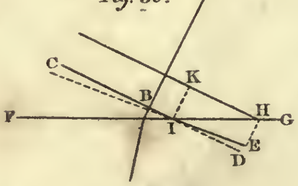
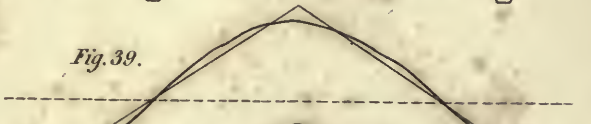


Fig. 31.

Fig. 32.

Fig. 39.



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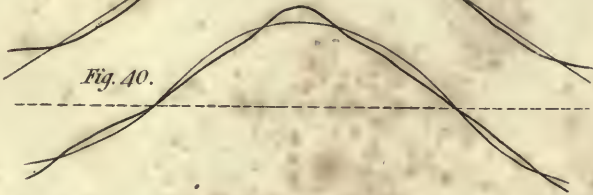

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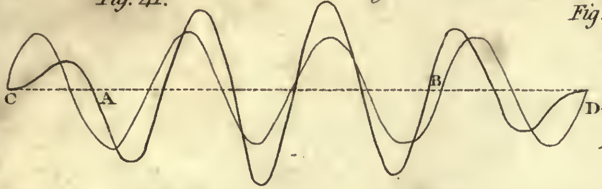
Fig. 41.



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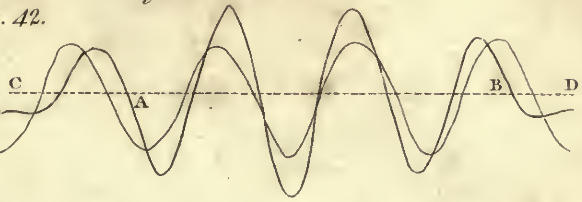

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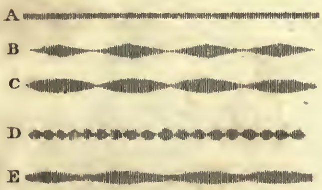

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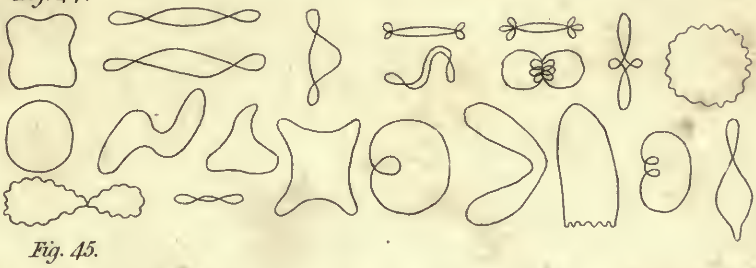

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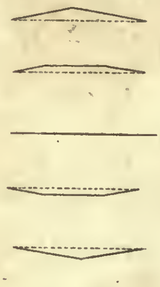

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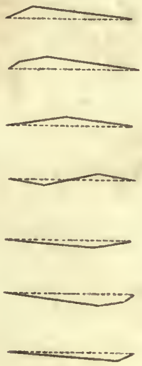

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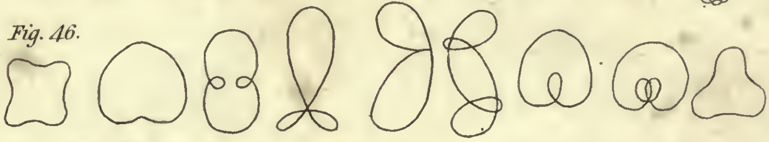

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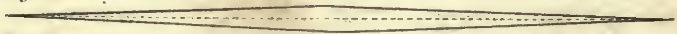


Fig. 50.

Fig. 52.

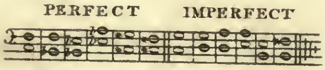
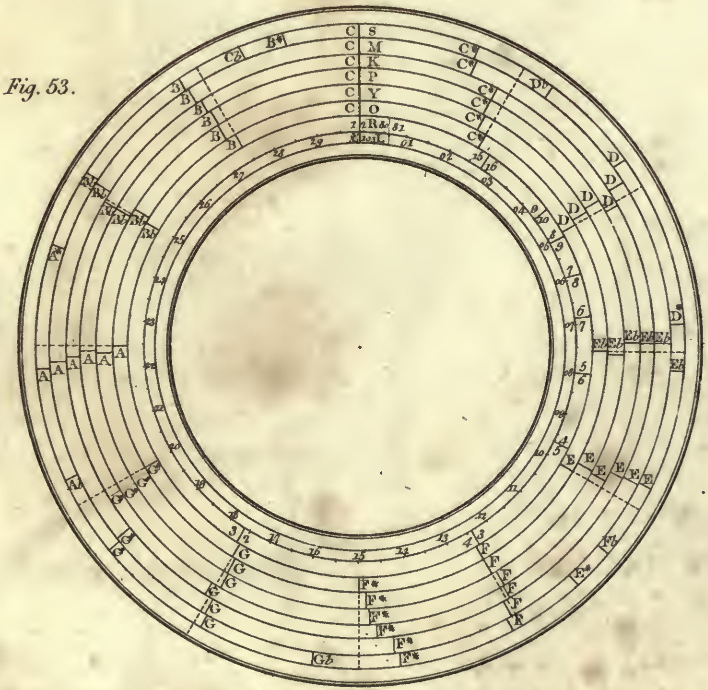


Fig. 51.

Fig. 53.



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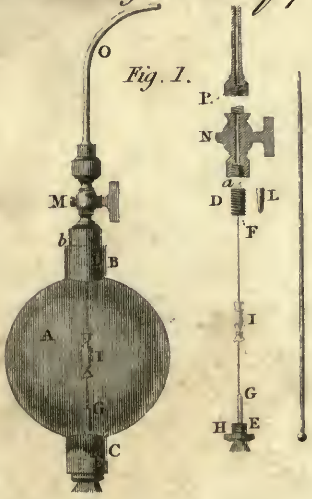
On Double Images, Fig. 2 to 12. Pa. 667, &c.

Fulminating Mercury. p. 666.

Fig. 1.



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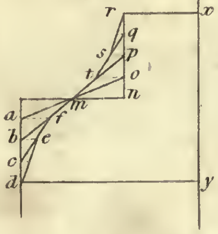

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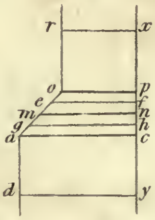

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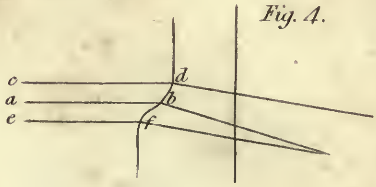

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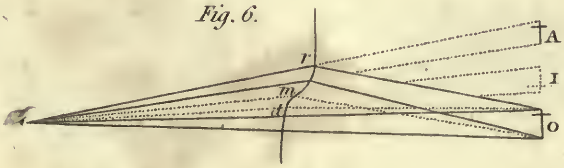

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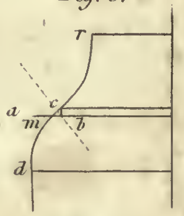

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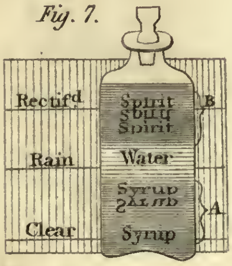

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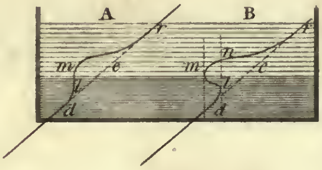

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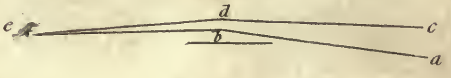

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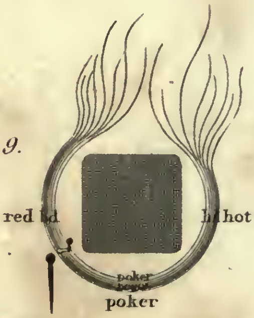

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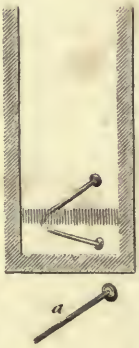
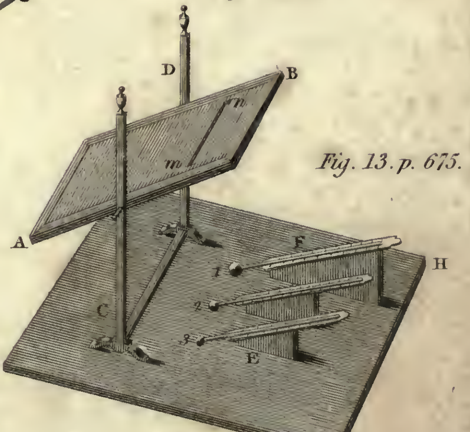


Fig. 12.

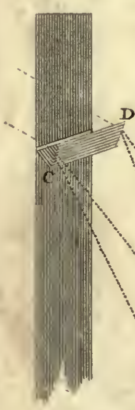
Heat & Light of Colours.

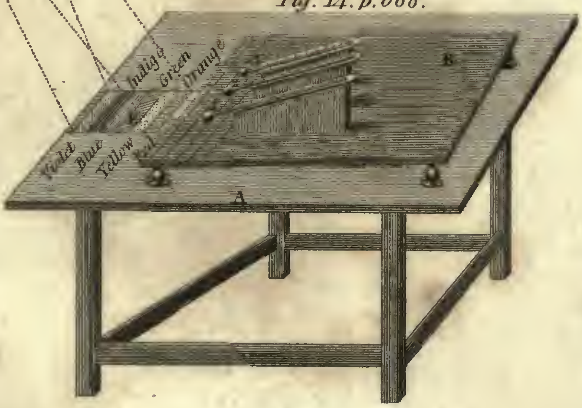
Fig. 13. p. 675.



Invisible Solar Rays.

Fig. 14. p. 688.





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Voltaic Pile. Pa. 745.

Fig. 1.

Fig. 2.

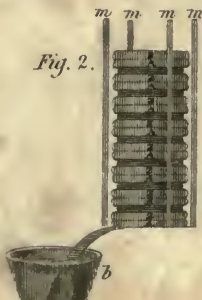

Fig. 3.

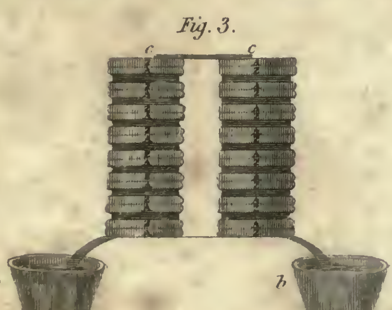

Fig. 4.

Ornithorhynchus Paradoxus. Pa. 748.

Fig. 5.

Fig. 6.

Fig. 7.

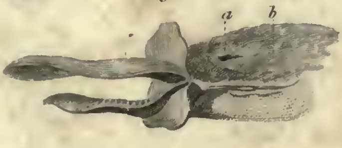

Fig. 8.

Pa. 748.

Fig. 10.

Fig. 9.

Fig. 11.

Heat making Rays.

Fig. 12 to 19. Pa. 749 &c.

Fig. 12.

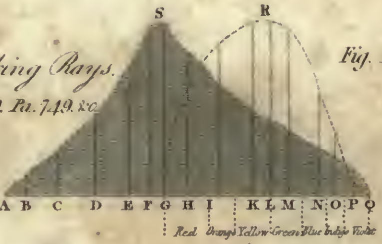

Fig. 13.

Fig. 14.

Fig. 16.

Fig. 15.

Fig. 17.

Fig. 18.

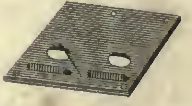

Fig. 19.

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Herschel on the Heat-making Rays. Pa. 759 &c

Fig. 1.

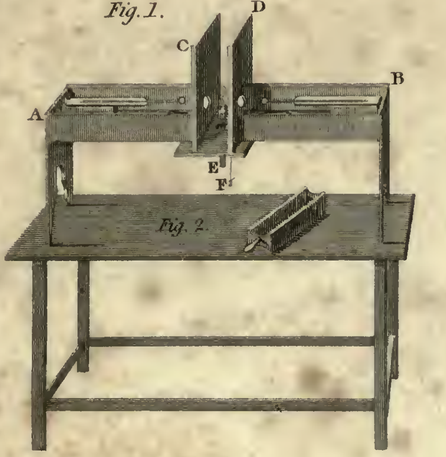


Fig. 3.

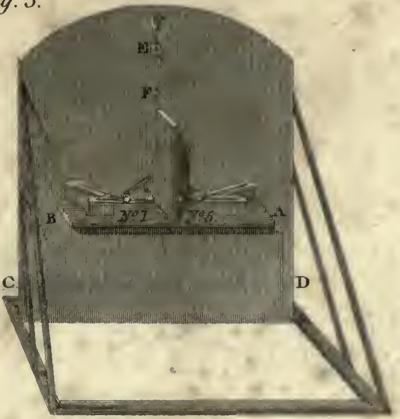


Fig. 5.

Fig. 6.

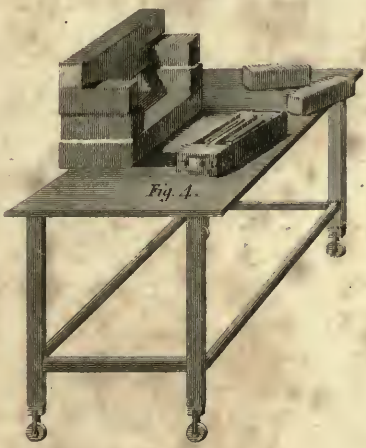


Fig. 8. Fig. 9.

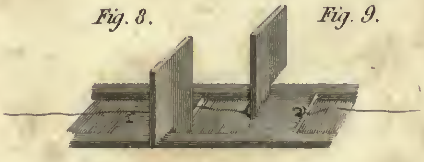


Fig. 11.

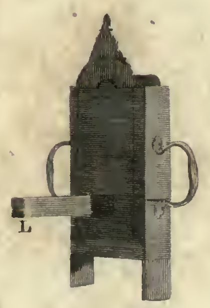
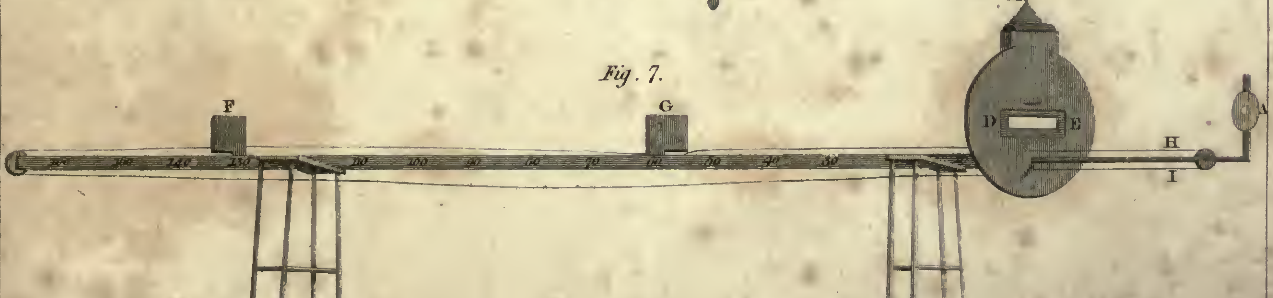


Fig. 10.

Fig. 7.



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