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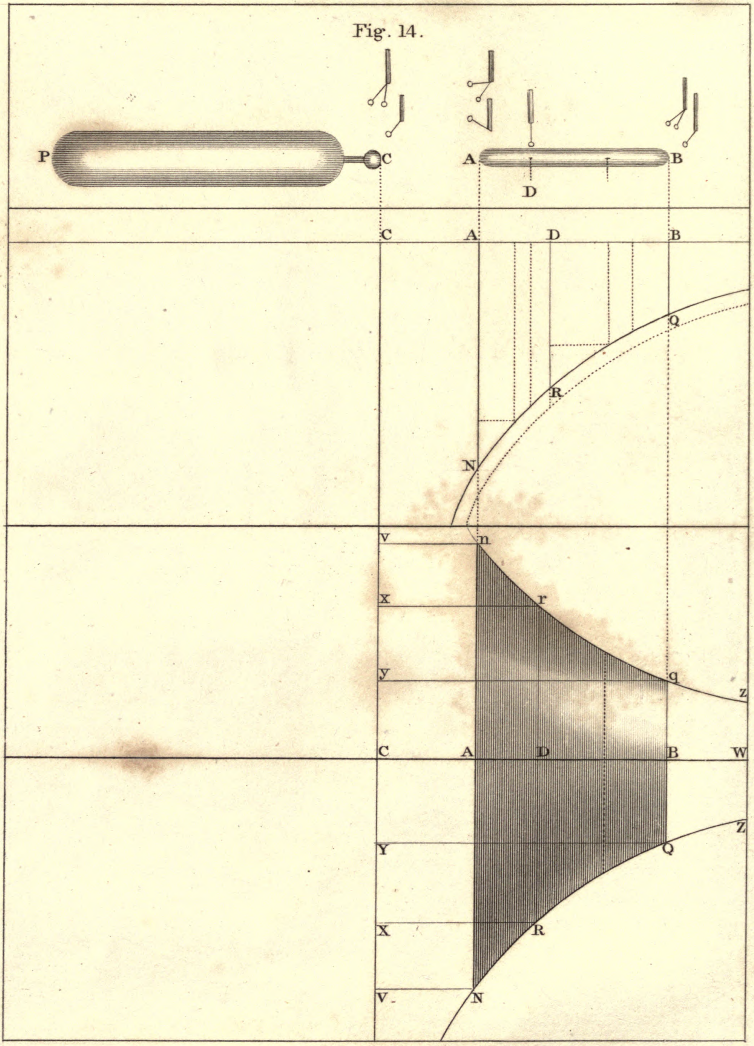

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Glasgow, 1st *May*, 1830.

PREFACE.

The work, which I here lay before the Public, constitutes an abridgement of the Lectures on *Heat* and *Electricity*, which I annually deliver in the College of Glasgow, as a necessary introduction to the elements of Chemistry; and which constitute about a third part of my usual course. Many of the students have been in the habit of applying to me to point out to them a work in the English language in which they could find the different topics which I am in the habit of discussing and illustrating. But I found myself at a loss for any work exactly suited to the objects in view. We have many excellent books on *heat*. But few or none of them embrace all the topics, which the present state of chemistry renders it necessary for me to introduce into my Lectures. I was therefore under the necessity of mentioning a variety of books, some more, and some less easily accessible, and which, therefore, it was not always in the power of the students to procure when they wanted them.

As for Electricity, we have many books which give a popular view of the science, and not a few which convey very accurate information respecting the mode of making experiments, and which, therefore, are of considerable value to the teacher; but we look in vain for any book in the English language which conveys a tolerable notion of the present state of that important science.

It was to remove this inconvenience, which has been long felt and complained of by my students, that I have been induced to draw up the present work. It will hereafter serve as a text book to the students of chemistry in Glasgow College, as it contains an epitome of all the different topics which I discuss, and all the tables to which I refer in the first part of my course; and I flatter myself that the perusal of it will render my lectures hereafter more easily understood and remembered than when the students had no text book to have recourse to. I have been at pains to treat of every thing in the plainest and simplest manner; that no difficulties in understanding it might occur even to those students whose previous education has been the most imperfect. I have hopes also that it may be found worthy of the attention of students of chemistry in general, as it contains as accurate a view of the important sciences of *heat* and *electricity* as my knowledge of these subjects enabled me to give.

The present volume may be considered as a new and enlarged edition of the first 172 pages of the first volume of my System of Chemistry. The second part, *on Electricity*, is published to fulfil a promise which I gave to the public many years ago. I have withheld it thus long from an unwillingness to encroach beyond what was absolutely necessary on the pecuniary resources of the students. This second part, instead of an abridgement, constitutes, in fact, an extension of my lectures on electricity. In the College of Glasgow the science of electricity belongs, not to the Professor of Chemistry, but to the Professor of Natural Philosophy. This induces me to confine my observations on it to those branches only that are most intimately connected with Chemistry. But I conceive that the connexion between Electricity and Chemistry is now so close that it is impossible to be master of the latter without being at least acquainted with the principles of the former.

It may be proper to mention, that I am preparing for the press as complete a view as I can draw up of the *simple substances*, and their primary and secondary compounds, to serve as a text book for the last two-thirds of my course. This will be followed with as little delay as possible by a work on Mineral Waters, Mineralogy, and Geology. Another work on *Vegetable* Chemistry, and another on *Animal*

Chemistry, will complete my plan. For in order to accommodate the students as much as possible, I mean to divide my System of Chemistry into five separate works; the first two of which, consisting of three volumes, will be sufficient for those who are beginning to study the science. Those who wish to devote themselves to the particular departments of chemistry will find the requisite information in the other three works.

CONTENTS.

INTRODUCTION.

Every one who proposes to become a student of chemistry, should endeavour in the first place to acquire a knowledge of the phenomena produced by *heat* and *electricity*. Heat, indeed, occasions such changes in bodies, and has so powerful an influence on their mode of acting on each other, that no man can become a practical chemist without studying the effects which it produces. Hence it has been customary in this country ever since the time of Dr. Cullen, to devote (in all Universities where chemistry is taught) a part of the course to explain the *effects* of heat on bodies, and the instruments by which these effects are measured.

Electricity was long considered as a branch of physics unconnected with chemistry, and depending upon quite other principles. Hence in our Universities it has been hitherto considered as belonging exclusively to the Professor of Natural Philosophy. But the discovery of the chemical decompositions produced by the Voltaic battery, and of the connection which seems to exist between the state of the electricity of different bodies and their affinity for each other, or their tendency to combine with each other or to separate from each other, has rendered it evident that electricity and chemistry are intimately connected with each other; if they do not ultimately depend upon the very same principles. Hence, in the present state of the science, every chemist finds it necessary to acquire a knowledge of the principal phenomena of *Electricity*. Indeed these phenomena are of so much importance, that it is much to be regretted that the explanation of them does not constitute a more prominent part of our University education. But unfortunately the course of study followed in these national seminaries was arranged at a time when many of the most important branches of science were only in a state of infancy: and it is not in the power of the present race of Professors to alter or improve it. All that can be done by the individual Professors, is to adapt their lectures as much as possible to the present advanced state of the sciences, which it is their province to teach. And this has been attended to by the Professors in the Scottish Universities as much as was possible.

The object of the present *work* is to lay an outline of the more important doctrines of *Heat* and *Electricity* before the Chemical Reader. It will be divided into two parts: in the first part I shall treat of *Heat;* in the second, of *Electricity*.

PART I.

OF HEAT.

Part I.

Meaning of the term heat.

THE word *heat* in the English language is used to express two different things. It sometimes signifies a *sensation* excited in our organs, and sometimes a certain *state* of the bodies around us, in consequence of which they excite in us that sensation. The word is used in the first sense when we say that we *feel heat*; and in the second, when we say that there is *heat in the fire*. In chemistry, the word *heat* is always employed in the second of these significations. *It denotes that state or condition of a body by which it excites in us the sensation of heat.*

Caloric, what.

When the French chemists contrived the new Chemical Nomenclature in 1787, they thought that it would be advantageous to possess a distinct word for each of these two meanings. Accordingly they restricted the word *heat* to the sensation, and invented the term *caloric* to denote the *cause* of this sensation, or the condition of bodies by which that sensation is produced. This innovation was unnecessary, and it had the effect of fixing down the opinions of chemists to the hypothesis respecting *heat*, adopted by the contrivers of the new nomenclature; namely, that heat is not a *property* of matter, but a *peculiar substance*; although no

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evidence sufficiently decisive to settle this disputed point has hitherto been advanced.

Heat very communicable.

Heat, whether it be a substance or a quality, is exceedingly communicable from one body to another. If we take a lump of hot iron out of the fire, we cannot prevent it from communicating its heat to all the bodies in its neighbourhood. If a hot body be placed in contact with colder bodies, it communicates heat to them, and continues to do so till all the bodies be reduced to the same temperature. If a cold body be placed in the midst of hot ones, it receives heat from them, and continues to do so till they are all reduced to the same temperature. The common opinion is, that in all such cases the hot body *loses something*, which the cold body imbibes. And this opinion has been universally adopted by all those who have examined the subject more profoundly. According to some philosophers, heat depends upon a *tremor* or *vibration* excited in bodies; according to others, upon an exceedingly *subtile* and *active matter* introduced into their pores; and according to others, upon a *vibration excited in a subtile matter*, which naturally exists in every body, though, except when thus excited, in a state of rest.

Opinions respecting heat.

Bacon was the first person who formally investigated the nature of heat. In his treatise *de forma calidi*, which he offered to the public as a model of the proper method of prosecuting investigations in natural philosophy, he enumerates all the facts respecting heat that were then known; and endeavours, from a careful examination of them, to form a well-founded opinion respecting its cause. The only conclusion which he was able to draw from his premises, was the very general one, that *heat is motion*. In this opinion Lord Bacon has had a great many followers, particularly among the philosophers of Great Britain : but it has been adopted with two different modifications.

Mr. Boyle seems to have supposed that this motion is in the small particles of heated bodies, and that it consists in a rapid vibration of these particles. This seems likewise to have been the notion entertained by Sir Isaac Newton, as may be inferred from the queries at the end of his optics. It was also the opinion of Mr. Cavendish, and seems likewise to be the view of the subject embraced by Dr. Thomas Young.

But the greater number of the French and German chemists of the last century conceived that the motion in which heat consists, is not a tremor of the particles of the hot body, but of the particles of a subtile, highly elastic, and penetrating fluid which is contained among the pores of hot bodies, or interspersed among their particles. This matter they consider as diffused through the whole universe, and as pervading with ease the densest bodies.

All the knowledge which we possess or can ever acquire respecting heat, is that of the different *effects* which it produces upon bodies. We must, therefore, before we can be prepared to form any opinion respecting the nature of heat, take an accurate survey of these effects. Now the general

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effects of heat are *expansion, fluidity, vaporization, ignition, and combustion.* These will form the subject of the five following chapters.*

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* I have adopted, in treating of heat, the arrangement of Dr. Black, from whose lectures I imbibed my first knowledge of this subject. It is very simple, and seems to me better adapted to the present state of our knowledge, than the more elaborate method which I followed in the former editions of my System of Chemistry.

CHAPTER I.

OF THE FIRST EFFECT OF HEAT, EXPANSION.

Chap. I.

IN this chapter I shall, in the first place, give an account of *expansion*, or that increase of bulk which takes place when bodies are heated. This will constitute the first section. In the remaining sections of this chapter, I shall treat of certain phenomena of heat, the knowledge of which may be considered as directly derived from the doctrine of expansion.

SECTION I. OF EXPANSION IN GENERAL.

By expansion is meant an increase in the bulk of bodies, which is observed to take place when they are heated; while there is a corresponding contraction when their temperature is diminished.

Bodies exist in nature in three states; namely, the state of *gases* or *vapours*, the state of *liquids*, and the state of *solids*. Common air furnishes a good example of a body in the gaseous state, water of a body in the liquid state, and iron of a body in the solid state. Now gases and vapours expand most when heated, liquids expand less, and solids least of all when subjected to the same increase of temperature.

Expansion of gases.

1. An accurate knowledge of the rate of expansion of gaseous bodies is essential to their investigation. Little progress could be made in deter-

Chap. I.

mining their properties or constitution till this point was settled; which it was soon after the commencement of the present century, by Mr. Dalton and M. Gay-Lussac. The experiments of Mr. Dalton were read to the Philosophical Society of Manchester, in October, 1801, and published early in 1802.* To him, therefore, the honour of discovering the law of the dilatation of gaseous bodies must be ascribed; for M. Gay-Lussac† did not publish his dissertation on the expansion of gaseous bodies till more than six months after the appearance of Mr. Dalton's paper. The great source of fallacy in experiments on the dilatation of air and gases by heat, is leaving the portion experimented on in contact with moisture; because as the temperature increases, more and more of this moisture is converted into vapour, mixes with the portion of gas under examination, and greatly increases its bulk. Mr. Dalton and M. Gay-Lussac, by keeping the gases experimented on dry, were enabled to discover that *all gases* experience the same augmentation of bulk when subjected to the same augmentation of temperature.

Equable.

According to Dalton, when 1000 volumes of air are heated from 32° to 212°, they become 1325 volumes. According to Gay-Lussac, they become 1375 volumes. While Mr. James Crichton of Glasgow, who repeated the experiment with the most minute attention to accuracy, obtained an

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* Memoirs of the Literary and Philosophical Society of Manchester, vol. v. part 2. p. 595.

† Ann. de Chim. xliii. 137.

increase from 1000 to 1374·8 volumes, which differs very little from the estimate of Gay-Lussac.

According to Dalton, the expansion for 1° is $\frac{1}{483}$, according to Gay-Lussac, it is $\frac{1}{480}$, while Mr. Crichton's result gives us the expansion for 1°, $\frac{1}{480{\cdot}23}$. Gay-Lussac's estimate is generally adopted by chemists as giving the real expansion; though I have little doubt that Mr. Crichton's constitutes a still nearer approximation to the truth.

The knowledge of this law supplies us with an easy method of determining what the bulk of a gas would be at a given temperature, provided we know its bulk at any other temperature.*

It is at present the opinion of chemists, that air

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* To perform this reduction with accuracy we must attend to the following rules:

1. If the temperature of the air be 32°, and we wish to know its bulk at a higher temperature, for example at 60°. Subtract 32° from 480. The remainder is 448. To this number 448, add the degrees above zero indicating the temperature of the air. These are 32 and 60; making 480 and 508. Then say 480 : 508 : : b (bulk of air at 32°) : b' (bulk at 60°). Let $b = 100$; then 480 : 508 : : 100 : 105·832 = volume of air at 60°.

2. If the two temperatures be higher than 32° the method is the same. Example, wanted the volume of air at 60° which measures 100 at 50°.

$448 + 50 = 498$ 498 : 508 : : 100 : 102·008 = volume
$448 + 60 = 508$ of air at 60°.

3. To reduce the volume v of a gas of the temperature t to the lower temperature t', multiply by $\frac{448 + t'}{448 + t}$. Let $t = 80°$, $t' = 60°$ then $\frac{448 + t'}{448 + t} = \frac{508}{528} = \frac{127}{132}$ and $100 \times \frac{127}{132} = 96{\cdot}21\dot{2}\dot{1}$ = volume at 60°.

and all gases experience the same increase of volume by the same increase of temperature, at whatever previous temperature this heat is applied. In other words, that the expansion of all the gases is equable. It is scarcely possible to demonstrate the truth of this opinion experimentally, because we have no means of measuring temperature, except by expansion. But the opinion is founded on very plausible reasons: and if we admit its truth, then the expansibility of all liquids and solids increases with the temperature to which they are subjected.

Gay-Lussac found the expansion of *steam* and of the vapour of *sulphuric ether* the same as that of air. It has been concluded from this, that vapours follow the same law and rate of expansion with gases; and therefore that all elastic fluids agree with each other in this important property.

Expansion of liquids.

2. No such general law exists respecting the expansibility of liquids. Every liquid has an expansibility peculiarly its own, and which therefore may be called specific. All that can be done therefore, is to determine the degree of expansion which each liquid undergoes, when subjected to a given increase of temperature. The following table exhibits the dilatation of various liquids, from 32° to 212°, supposing the bulk at 32° to be 1.

Alcohol*	0·11000	= $\frac{1}{9}$ †
Nitric acid* (Sp. gr. 1·4)	0·11000	= $\frac{1}{9}$
Whale oil (from 60° to 212°)	0·08548	= $\frac{1}{11 \cdot 7}$
Fixed oils*	0·08000	= $\frac{1}{12}$

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* Dalton's New System of Chemical Philosophy, i. 29.

† The expansion is from—8° to 172°.

Sulphuric ether*	0·07000 =	$\frac{1}{14}$
Oil of turpentine	0·07000 =	$\frac{1}{14}$
Sulphuric acid (Sp. gr. 1·85)	0·06000 =	$\frac{1}{17}$
Muriatic acid* (Sp. gr. 1·137)	0·06000 =	$\frac{1}{17}$
Brine or water saturated with salt*	0·05000 =	$\frac{1}{20}$
Water*	0·04444 =	$\frac{1}{22\cdot5}$
Water from 42°·5 to 212°†	0·04393 =	$\frac{1}{22\cdot76}$
Mercury‡	0·018018 =	$\frac{1}{55\cdot5}$
Mercury§	0·01758 =	$\frac{1}{55}$
Mercury‖	0·01680 =	$\frac{1}{59}$
Mercury¶	0·01852 =	$\frac{1}{54}$
Mercury**	0·01872 =	$\frac{1}{53}$
Mercury††	0·02000 =	$\frac{1}{50}$

Liquids differ from gaseous bodies in another circumstance. Their expansibility is not uniform; but the rate of expansion increases with the temperature, and is therefore the greater, the higher the elevation at which a given quantity of heat is added to them. Liquids differ from gaseous bodies in a very remarkable circumstance. The particles of gaseous bodies *repel* each other, but those of liquids *attract*, as is evident from their collecting together in spherical drops. It has been ascertained that the force of this attraction differs very much in different liquids. For example, it is much greater between the particles of mercury, than between the particles of water. Now these attractions diminish at a great rate, as the distance between

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* Dalton's New System of Chemical Philosophy, i. 29.

† Mr. Crichton, Annals of Philosophy, (second series) vii. 244.

‡ Dulong and Petit. § Hollstroem, Gilberts Annales, xvii. 107.

‖ General Roy. ¶ Shuckburgh, Lavoisier and Laplace.

** Lord Charles Cavendish. †† Dalton.

the particles of the liquid increase. It is obvious that this attraction between the liquid particles must act as an antagonist to the expansion produced by heat. Hence, the reason that every liquid has an expansion peculiarly its own; and hence also the reason why the rate increases with the temperature. Every increase of temperature weakens the attraction between the particles of the liquid, by increasing their distance from one another, and of course must augment the effect produced by a given increment of heat.

Accurate experiments on the rate of the expansion of few liquids have been hitherto made. The following table exhibits the rate of the dilatation of mercury for the three first *thermic units*,* as determined by the experiments of Dulong and Petit.†

	Dilatation of Mercury.
From 32° to 212° . .	$\frac{1}{55\cdot50}$
212 to 392 . .	$\frac{1}{54\cdot25}$
392 to 582 . .	$\frac{1}{53\cdot00}$

Expansion of water.

Water varies in the rate of its dilatation more than most liquids. The following table exhibits the increase of bulk of 1 of water at various temperatures, from 30° to 212°, as determined by the experiments of Gilpin and Kirwan.

Temp.	Expansion.	Temp.	Expansion.
30° Gilpin .	0·0020	39 Gilpin .	·00000
32 G . .	0·0012	44 G . .	·00006
34 G . .	·00006	48 G . .	·00018

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* By thermic unit is meant 180°, or the number of degrees between freezing and boiling water.

† Annals of Philosophy, xiii. 118.

Temp.	Expansion.	Temp.	Expansion.
49° Gilpin	·00022	100° Gilpin	·00692
54 G	·00049	102 Kirwan	·00760
59 G	·00086	122 K	·01258
64 G	·00133	142 K	·01833
69 G	·00188	162 K	·02481
74 G	·00251	182 K	·03198
79 G	·00321	202 K	·04005
90 G	·00491	212 K	·04333*

If we denote the degree of Fahrenheit's thermometer by f, the expansion of water, reckoning either way from 39°, is nearly expressed by $0·000022f^2 - 0·00000000435f^3$, as was first pointed out by Dr. Young.†

Gay-Lussac‡ has turned his attention to the phenomena of the expansion of liquids. The following table exhibits the result of his researches. He sup-

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* The following table exhibits the specific gravity of water at different temperatures, as deduced from the experiments of Captain Kater. It differs somewhat from the table given above.

Temp.	Sp. gravity.	Temp.	Sp. gravity.
50°	1·0005	61°	1·0001
51	1·0005	62	1·0000
52	1·0005	63	0·9999
53	1·0004	64	0·9999
54	1·0004	65	0·9998
55	1·0004	66	0·9997
56	1·0003	67	0·9996
57	1·0003	68	0·9996
58	1·0002	69	0·9995
59	1·0002	70	0·9994
60	1·0001		

† Young's Lectures on Natural Philosophy, ii. 392.

‡ Ann. de Chim. et Phys., ii. 130.

poses the volume of each of the liquids at its boiling temperature to be 1000. The table represents the contractions which each liquid experienced when cooled down every five degrees centigrade below its boiling point. The temperatures at which the different liquids tried boiled were as follows:

Water . .	212°
Alcohol . .	173·14
Sulphuret of carbon .	116·1
Sulphuric ether .	96·2

Temperature.	Water.	Alcohol.	Sulphuret of carbon.	Ether.
	Contractions.	Contractions.	Contractions.	Contractions.
0°	0·00	0·00	0·00	0·00
5	3·34	5·55	6·14	8·15
10	6·61	11·43	12·01	16·17
15	10·50	17·51	17·98	24·16
20	13·15	24·34	23·80	31·83
25	16·06	29·15	29·65	39·14
30	18·85	34·74	35·06	46·42
35	21·52	40·28	40·48	52·06
40	24·10	45·68	45·77	58·77
45	26·50	50·85	51·08	65·48
50	28·56	56·02	56·28	72·01
55	30·60	61·01	61·14	78·38
60	32·42	65·96	66·21	
65	34·02	70·74		
70	35·47	75·48		
75	36·70	80·11		

From the preceding table it appears that alcohol and sulphuret of carbon undergo the same dilatation. Gay-Lussac has shown that they likewise form the same volume of vapour when exposed to a boiling temperature.

Alcohol forms 488·3 its volume of vapour.
Sulphuret of carbon 491·1 its volume of vapour.

The following table exhibits the degrees marked upon thermometers filled with different liquids at the same temperature as determined by the experiments of De Luc.* The tubes containing these liquids were of glass; but as he does not mention their capacities, nor the value of a degree, the table does not enable us to determine the expansion of the liquids used.

Mercury.	Olive oil.	Essential oil of camomile.	Essential oil of thyme.	Alcohol capable of setting fire to gunpowder.	Water saturated with common salt.	Water.
80°	80°	80°	80°	80°	80°	80°
75	74·6	74·7	74·3	73·8	74·1	71·0
70	69·4	69·5	68·8	67·8	68·4	62·0
65	64·4	64·3	63·5	61·9	62·6	53·5
60	59·3	59·1	58·3	56·2	57·1	45·8
55	54·2	53·9	53·3	50·7	51·7	38·5
50	49·2	48·8	48·3	45·3	46·6	32·0
45	44·0	43·6	43·4	40·2	41·2	26·1
40	39·2	38·6	38·4	35·1	36·3	20·5
35	34·2	33·6	33·5	30·3	31·3	15·9
30	29·3	28·7	28·6	25·6	26·5	11·2
25	24·3	23·8	23·8	21·0	21·9	7·3
20	19·3	18·9	19·0	16·5	17·3	4·1
15	14·4	14·1	14·2	12·2	12·8	1·6
10	9·5	9·3	9·4	7·9	8·4	0·2
5	4·7	4·6	4·7	3·9	4·2	0·4
0	0·0	0·0	0·0	0·0	0·0	0·0
—5				—3·9	—4·1	
—10				—7·7	—8·0	

In these thermometers 0 denotes the temperature

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* Recherches sur les Modifications de l'Atmosphere, i. 271.

at which water freezes, 80° the temperature at which it boils.

Law of expansion of liquids.

Mr. Emmett has observed that if the temperatures of liquids be taken in arithmetical progression, the volumes at these temperatures are the logarithms of a certain series of numbers in arithmetical progression ; which latter are of course the reciprocals of a series of numbers in harmonical progression.* The following tables will serve to illustrate this law, which constitutes a near approximation to the truth.

MERCURY.

Temp.	Volume.	Log. of.	Diff. of numb.
50°	100183	12594·5	
80	——508	12603·98	9·48
110	—–813	12612·8	8·82
140	—1119	12621·7	8·9
170	—1424	12630·6	8·9
200	—1730	12639·5	8·9

SULPHURIC ACID.

Temp.	Volume.	Log. of.	Diff. of numb.
50°	100000	12591·6	
80	——806	12612·6	21·0
110	—1540	12633·9	21·3
140	—2320	12659·4	25·5
170	—3116	12680·0	20·6
200	—3911	12703·1	23·1

NITRIC ACID.

Temp.	Volume.	Log. of.	Diff. of numb.
50°	100000	12589·2	
80	—1530	12633·6	44·4

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* Annals of Philosophy (2d Series), viii. 254. The tables are, I believe, founded on a set of experiments which I made many years ago, and which I inserted in an early edition of my Chemistry.

Temp.	Volume.	Log. of.	Diff. of numb.
110°	103196	12682·2	48·6
140	—5132	12738·9	46·7

WATER.

Temp.	Volume.	Log. of.	Diff. of numb.
50°	100023	12590·0	
70	—197	12594·9	4·9
90	—694	12669·3	4·4

ALCOHOL.

Temp.	Volume.	Log. of.	Diff. of numb.
40°	100539	12604·8	
60	—1688	12638·2	33·4
80	—2890	12673·3	35·1
100	—4162	12710·4	37·1

Expansion of solids.

3. Solids are precisely in the same situation with liquids as far as their expansion by heat is concerned. Their particles attract each other, with more or less force according to the solid. This attraction opposes the expanding power of heat. Hence every solid must have a degree of expansion peculiar to itself; and the rate of expansion in solids as well as liquids must increase as their temperatures augment.

As glass tubes filled with mercury are employed in order to measure temperature, it becomes an object of great consequence to determine the dilatability of different kinds of glass by heat. Mr. James Crichton of Glasgow has examined this dilatability with uncommon care. The following table exhibits the results of his experiments.

Expansion of glass.

		1 in volume at 32° becomes at 212°		1 in length at 32° becomes at 212°	
	Sp. gr.	In Decimal.	In Vulg. fr.	In Decimal.	In Vulg. fr.
Colourless glass	3·176	1·002094	$\frac{1}{477\cdot4}$	1·0006963	$\frac{1}{1436}$
Brownish red	3·301	1·002147	$\frac{1}{465\cdot6}$	1·0007158	$\frac{1}{1399}$
Deep blue	3·229	1·002365	$\frac{1}{422.7}$	1·0007861	$\frac{1}{1272}$
Dusky red	3·274	1·002403	$\frac{1}{416\cdot1}$	1·0008012	$\frac{1}{1251}$
Dulong and Petit's		1·002344	$\frac{1}{426\cdot6}$	1·0007788	$\frac{1}{1284}$*

Different kinds of glass differ so much from each other that no general rule can be laid down. Lavoisier and Laplace found that it was the less dilatable by heat the more lead it contained.† Several determinations will be found in the preceding tables, and I shall add some more here. Ramsden found the expansion between 32° and 212° of a solid glass rod 0·0096944, and that of a glass tube 0·0093138. De Luc's experiments on the expansion of thermometer and barometer tubes may be seen in the following table.

Temp.	Bulk.	Temp	Bulk.	Temp.	Bulk.
32°	100000	100°	100023	167°	100056
50	100006	120	100033	190	100069
70	100014	150	100044	212	100083

I shall introduce here the table of the expansion of different solid bodies from 32° to 212°, as determined by Lavoisier and Laplace, in 1782. The experiments seem to have been made with very great care. They were supposed to have been lost; but have lately been recovered and published by Biot.‡

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* The correction pointed out by Mr. Crichton in the calculations of Dulong and Petit has been made. See Annals of Philosophy (2d series), vii. 24.

† Biot. Traité de Physique, i. 157. ‡ Traité de Physique, i. 158.

Experiments of Lavoisier and Laplace.

Substances tried.	Length of a rule at 212° which at 32° is 1,00000000.	Dilatation in vulgar fractions.
Glass of Saint Gobain .	1,00089089	$\frac{1}{1122}$
Glass tube without lead .	1,00087572	$\frac{1}{1142}$
Ditto	1,00089760	$\frac{1}{1114}$
Ditto	1,00091751	$\frac{1}{1090}$
English flint glass . .	1,00081166	$\frac{1}{1248}$
French glass with lead .	1,00087199	$\frac{1}{1147}$
Copper	1,00172244	$\frac{1}{581}$
Copper	1,00171222	$\frac{1}{584}$
Brass	1,00186671	$\frac{1}{535}$
Brass	1,00188971	$\frac{1}{529}$
Hammered iron . . .	1,00122045	$\frac{1}{819}$
Iron wire	1,00123504	$\frac{1}{812}$
Hard steel	1,00107875	$\frac{1}{927}$
Soft steel	1,00107956	$\frac{1}{926}$
Tempered steel . . .	1,00123956	$\frac{1}{807}$
Lead	1,00284836	$\frac{1}{351}$
Malacca tin . . .	1,00193765	$\frac{1}{516}$
Tin from Falmouth .	1,00217298	$\frac{1}{462}$
Cupelled silver . .	1,00190974	$\frac{1}{524}$
Silver, Paris standard .	1,00190868	$\frac{1}{524}$
Pure gold	1,00146606	$\frac{1}{682}$
Gold, Paris standard, not softened	1,00155155	$\frac{1}{645}$
Ditto, softened . . .	1,00151361	$\frac{1}{661}$

In the year 1754, Mr. Smeaton published a set of experiments on the expansion of different substances measured by means of a very ingenious instrument of his own invention, described by him in the Philosophical Transactions for that year.* The following table shows the expansions which the different substances tried, undergo from 32° to 212° supposing the original bulk to be 1.

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* Phil. Trans. 1754, p. 598.

Experiments of Smeaton.

White glass barometer tube	0·00083
Antimony	0·001083
Blistered steel	0·001125
Hard steel	0·001225
Iron	0·001258
Bismuth	0·001392
Copper hammered	0·001700
Copper, 8 parts mixed with tin 1	0·0018166
Brass, 16 parts with tin 1	0·001908
Brass wire	0·001933
Speculum metal	0·001933
Spelter solder, viz. brass 2, zinc 1	0·002058
Fine pewter	0·002283
Grain tin	0·002483
Soft solder, viz. lead 2, tin 1	0·002508
Zinc, 8 with tin 1, a little hammered	0·002692
Lead	0·002867
Zinc	0·002942
Zinc hammered ½ inch per foot	0·003011

The following table exhibits the dilatations of different substances as determined by General Roy, the accuracy of whose experiments is well known.

Of General Roy.

Glass tube	0·00077615
Glass rod	0·00080787
Cast-iron prism	0·0011094
Steel rod	0·0011447
Brass scale, supposed from Hamburg	0·0018554
English plate brass rod	0·001875
English plate brass trough	0·0018928

In the following table I shall give the result of the trials of some other artists and philosophers on the expansion of some other bodies, reckoning as usual the bulk at 32° to be 1. The expansion given is from 32° to 212°.

Steel	0·0011899.	Troughton.
Silver	0·0020826.	Troughton.
Copper	0·0019188.	Troughton.
Iron wire	0·0014401.	Troughton.
Iron	0·001446.	Hällström.
Platinum	0·0009918.	Troughton.
Platinum	0·00085655.	Borda.
Palladium	0·0010.	Wollaston.

The following table exhibits the expansion of an iron rod in length for different temperatures, as determined by the experiments of Hällström, I believe with considerable accuracy.

Expansion of iron.

Temp.	Length of an iron rod.	1st Diff.	2d Diff.	3d Diff.
— 40°	0·999632			
— 22	0·999721	89		
— 4	0·999811	90	1	
+ 14	0·999904	93	3	2
32	1·000000	96	3	0
50	1·000102	102	6	3
68	1·000211	109	7	1
86	1·000328	117	8	1
104	1·000453	125	8	0
122	1·000588	135	10	2
140	1·000734	146	11	1
158	1·000892	158	12	1
176	1·001063	171	13	1
194	1·001247	184	13	0
212	1·001446	199	15	2

We see from this table that the dilatation of iron increases with the temperature.* Few other expe-

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* If 1 be the side of the cube constituting unity of volume, d the dilatation of that side for an increase of temperature, the volume of the cube becomes $(1 + d)^3$, or $1 + 3d + 3d^2 + d^3$. But d in general being very small, $3d^2$ and d^3 may be neglected; so that

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riments have been made to determine the rate at which the expansion of solids increases with the temperature, though there is no reason to doubt that such an increase takes place in all solid bodies. The following table shows the dilatation of iron, copper, and platinum, at 212° and at some higher temperature, as determined by the experiments of Dulong and Petit.

Increased dilatation by heat.

Temp. deduced from dilatation of air.		Dilatation of iron.	Temp. indicated by a thermometer made of iron.		Dilatation of copper.	Temp. by a thermometer made of copper.		Dilatation of platinum	Temp. by a thermometer made of platinum.	
Cent.	Fah.		Cent.	Fah.		Cent.	Fah.		Cent.	Fah.
100°	212°	$\frac{1}{28200}$	100°	212°	$\frac{1}{19400}$	100°	212°	$\frac{1}{37700}$	100°	212°
300	582	$\frac{1}{22700}$	372·6	702·7	$\frac{1}{17001}$	328·8	623·8	$\frac{1}{36300}$	311·6	592·88

4. Such are the facts which have been ascertained respecting the expansion of bodies by heat. But general as the law of expansion is there are some exceptions to it, which I shall now point out. These exceptions are of two different kinds.

Maximum density of water at 39°·38.

There is a liquid, which has the greatest density a little above 39°. If we heat it above that point or cool it below it, in either case it expands. This liquid is *water*. The phenomena which led to the knowledge of this very curious fact, were first observed by the Florentine academicians. An account of their experiments was published in the

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the new cube may be expressed by 1 + 3 *d*. Hence we see that thrice the linear dilatation is very nearly the cubic dilatation.

For the same reason the dilatation of a surface will be expressed by 1 + 2 *d*.

Philosophical Transactions for 1670.* They filled with water a glass ball, terminating in a narrow graduated neck, and plunged it into a mixture of snow and salt. The water started suddenly up into the neck, in consequence of the contraction of the vessel, and slowly subsided again as the cold affected it. After a certain interval it began to rise again, and continued to ascend slowly and equably, till some portion of it shot into ice, when it sprung up at once with the greatest velocity. The attention of the Royal Society was soon afterwards called to this remarkable expansion by Dr. Croune, who, in 1683, exhibited an experiment similar to that of the Florentine philosophers, and concluded from it, that water begins to be expanded by cold at a certain temperature above the freezing point. Dr. Hooke objected to this conclusion, and ascribed the apparent expansion of the water to the contraction of the vessel in which the experiment was made. This induced them to cool the glass previously in a freezing mixture, and then to fill it with water. The effect, notwithstanding this precaution, was the same as before.† Mr. De Luc was the first who attempted to ascertain the exact temperature at which this expansion by cold begins. He placed it at 41°, and estimated the expansion as nearly equal, when water is heated or cooled the same number of degrees above or below 41°. He made his experiments in glass thermometer tubes,

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* Phil. Trans. No. 66, or vol. v. p. 2020. Abridgment, i. 540.

† Birche's Hist. of the Royal Society, iv. 253.

and neglected to make the correction necessary for the contraction of the glass; but in a set of experiments by Sir Charles Blagden and Mr. Gilpin, made about the year 1790, this correction was attended to. Water was weighed in a glass bottle at every degree of temperature from 32° to 100°, and its specific gravity ascertained. They fixed the maximum of density at 39°, and found the same expansion very nearly by the same change of temperature either above or below 39°. The following table exhibits the bulk of water at the corresponding degrees on both sides of 39°, according to their experiments.*

Specific Gravity.	Bulk of Water.	Temperature.		Bulk of Water.	Sp. Gravity of Ditto.
	1·00000	39		1·00000	
1·00000	00	38	40	00	1·00000
0·99999	01	37	41	01	0·99999
0·99998	02	36	42	02	0·99998
0·99996	04	35	43	04	0·99996
0·99994	06	34	44	06	0·99994
0·99991	08	33	45	08	0·99991
0·99988	12	32	46	12	0·99988

Mr. Dalton, in a set of experiments published in 1802, obtained nearly the same result as De Luc.

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* Phil. Trans. 1792, p. 428.

He placed the maximum density at 42·5°, not making any correction for the contraction of the glass; and observed, as Blagden had done before him, that the expansion is the same on both sides of the maximum point, when the change of temperature is the same, and continues however low down the water be cooled, provided it be not frozen.*

All these experiments had been made by cooling water in glass vessels; but when the French were forming their new weights and measures, the subject was investigated by Lefebvre-Gineau in a different manner. A determinate bulk of water at a given temperature was chosen for the foundation of their weights. To obtain it, a cylinder of copper, about nine French inches long, and as many in diameter, was made, and its bulk measured with the utmost possible exactness. This cylinder was weighed in water of various temperatures. Thus was obtained the weight of a quantity of water equal to the bulk of the cylinder; and this, corrected by the alteration of the bulk of the cylinder itself from heat or cold, gave the density of water at the temperatures tried. The result was, that the density of the water constantly increased till the temperature of 40°, below which it as constantly diminished.† These experiments seem to have been made about the year 1795. More lately a set of experiments was tried by Hällström exactly

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* Manchester Mem. v. 374.

† Jour. de Phys. xlix. 171; and Hauy's Traité de Physique, i 55 and 181.

in the same way; but he substituted a cylinder of glass for the one of metal. The result which he obtained was the same. The necessary corrections being made, he found the maximum density of water lie between 4° and 5° of Celsius, or nearly at 40° of Fahrenheit.*

Still more lately, a set of experiments have been published by Dr. Hope, which lead to the same result in a different way. He employed tall cylindrical glass jars filled with water of different temperatures, and having thermometers at their top and bottom. The result was as follows; 1. When water was at 32°, and exposed to air of 61°, the *bottom* thermometer rose fastest till the water became of 38°, then the top rose fastest. Just the reverse happened when the water was 53°, and exposed to the cold water surrounding the vessel; the *top* thermometer was *highest* till the water cooled down to 40°, then the bottom one was highest. Hence it was inferred, that water when heated towards 40° sunk down, and above 40° rose to the top, and *vice versa.* 2. When a freezing mixture was applied to the top of the glass cylinder (temp. of air 41°), and continued even for several days, the bottom thermometer never fell below 39°; but when the freezing mixture was applied to the bottom, the top thermometer fell to 34° as soon as the bottom one. Hence it was inferred, that water when cooled below 39° cannot sink, but easily ascends. 3. When the water in the cylinder was at 32°, and warm

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* Gilbert's Annalen der Physik, xvii. 207.

water applied to the middle of the vessel, the bottom thermometer rose to 39° before the top one was affected; but when the water in the cylinder was at 39°·5, and cold was applied to the middle of the vessel, the top thermometer cooled down to 33 before the bottom one was affected.*

Count Rumford likewise published a set of experiments conducted nearly on the same principles with those of Dr. Hope, and leading to the same results. They are contrived with his usual ingenuity; but as they are of posterior date, and add nothing to the facts above stated, I do not think it necessary to detail them.† Dr. Hope's experiments and those of Count Rumford coincide with those above related, in fixing the maximum density of water at between 39° and 40°.

But the most elaborate set of experiments upon this subject, are those of Professor Hällström of Abo, inserted in the Memoirs of the Stockholm Academy for 1823.‡ He has shown that the point at which the density of water is greatest, is 4°·1 of the centigrade thermometer, which corresponds with 39°·38 of Fahrenheit. The following table exhibits the specific gravity and the volume of water at different temperatures, from 32° to 86°, as determined by the experiments of Hällström. I insert it here, because it may be often useful in chemical investigations.

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* See Edin. Trans. vol. vi. The paper was published before October 1804.

† See Nicholson's Journal, xi. 228. Aug. 1805. ‡ Page 193.

Table of the expansion of water.

Temperature.			
Centigrade.	Fahren.	Sp. gr. of water.	Volume.
0°	32°	0·9998918	1·0001082
1	33·8	0·9999382	1·0000617
2	35·6	0·9999717	1·0000281
3	37·4	0·9999920	1·0000078
4	39·2	0·9999995	1·0000002
4·1	39·38	1	1
5	41·0	0·9999950	1·0000050
6	42·8	0·9999772	1·0000226
7	44·6	0·9999472	1·0000527
8	46·4	0·9999044	1·0000954
9	48·2	0·9998497	1·0001501
10	50	0·9997825	1·0002200
11	51·8	0·9997030	1·0002970
12	53·6	0·9996117	1·0003888
13	55·4	0·9995080	1·0004924
14	57·2	0·9993922	1·0006081
15	59·	0·9992647	1·0007357
16	60·8	0·9991260	1·0008747
17	62·6	0·9989752	1·0010259
18	64·4	0·9988125	1·0011888
19	66·2	0·9986387	1·0013631
20	68	0·9984534	1·0015490
21	69·8	0·9982570	1·0017560
22	71·6	0·9980489	1·0019549
23	73·4	0·9978300	1·0021746
24	75·2	0·9976000	1·0024058
25	77	0·9973587	1·0026483
26	78·8	0·9971070	1·0029016
27	80·6	0·9968439	1·0031662
28	82·4	0·9965704	1·0034414
29	84·2	0·9962864	1·0037274
30	86	0·9959917	1·0040245

The explanation of this curious anomaly that water of the temperature 39°·38 expands when we reduce it to a lower temperature, which is generally adopted, is, that 39°·38 is the lowest point at which water is perfectly fluid. When its temperature falls below this point, its particles begin to assume that new position with respect to each other, which they have when the liquid is converted into ice. Now ice has a lower specific gravity than water; of course the interval between its particles is greater: and the particles begin to assume this greater distance when cooled down below 39°·38. With respect to the equality of the volume of water at equal distances above and below 39°·38, it must be accidental and entirely owing to the diminution which glass experiences in its expansion as the temperature sinks.

Maximum density of brine.

When salt is dissolved in water, the temperature of the greatest density of the water is lowered in proportion to the quantity of salt in solution, so that it speedily sinks below the freezing point of the liquid, in consequence of which the anomaly disappears. Thus water of the specific gravity 1·0100, having about $\frac{1}{70}$th of its weight of common salt dissolved in it, has its point of greatest density at 35°·375, or 4 degrees lower than pure water. Water of the specific gravity 1·0200, or containing $\frac{1}{36}$th of its weight of salt, has its point of greatest density below the freezing point. Water of the specific gravity 1·027, or containing $\frac{1}{27}$th of its weight of salt in solution, has also its point of greatest density below its freezing point. In such liquids, accordingly, the anomaly cannot be observed.*

These facts render the common explanation of

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* See Erman's experiments, Ann. de Chim. et de Phys. xxxviii. 287.

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the cause of the maximum density of water being at 39°·38, exceedingly doubtful. The experiments of M. Erman on the expansion of Rose's fusible metal at different temperatures, is if possible still more inconsistent with it.* What is called Rose's fusible metal is an alloy of

2 parts by weight of bismuth,
1 part ——— of lead,
1 part ——— of tin.

It melts at 200°·75, which is considerably lower than the fusing point of Newton's fusible metal. Erman has found that the specific gravity of this alloy is the greatest at the temperature of 155°·75, and the least at the temperature of 110°·75. Now at both of these temperatures the metal is solid. It will be worth while to insert here the table of the change of volume which this metal undergoes at different temperatures, as determined by Erman's experiments, on account of the different rates at which these alterations take place at different parts of the series.

Expansion of fusible metal.

Temp.	Volume.	Temp.	Volume.
32°	1·00000	167°	0·99389
43·25	1·00088	178·25	
54·5	1·00192	189·5	1·00005
65·75	1·00326	200·75	1·00862
77	1·00443	212	1·01792
88·25	1·00681	223·25	1·01842
99·5	1·00803	234·5	1·01992
110·75	1·00830	245·75	1·02105
122	1·00679	257	1·02217
133·25	1·00129	268·25	1·02289
144·5	0·99480	279·5	1·02395
155·75	0·99291	290·75	1·02529

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* Ann. de Chim. et de Phys. xl. 197.

Temp.	Volume.	Temp.	Volume.
302°	1·02599	358°·25	1·03152
313·25	1·02693	369·5	1·03277
324·5	1·02784	380·75	1·03403
335·75	1·02916	392	1·03495
347	1·03072		

From 32° to 110°·75, the changes of volume are nearly proportional to the temperatures. Beyond 110°·75 (where the volume is a maximum) the metal undergoes a contraction instead of an expansion when heat is added. This contraction is at first very rapid, but it gradually diminishes to about 155°·75, which is the point of greatest density. The volume at 178°·25 is nearly the same as at 32°. The volume from 155°·75 begins to increase at first very slowly, but the increase gradually augments to 200°·75, which is the point of fusion. Between 200°·75 and 212°, the increase of volume is very considerable; but beyond that point the increase is nearly similar to what it was between 32° and 110°·75.

Erman has also made a set of experiments to determine the dilatation of phosphorus at different temperatures. It has no point of maximum density, but continues to dilate as the heat increases. The following table shows the amount of this dilatation.*

Expansion of phosphorus.

Temp.	Volume.	Temp.	Volume.
32°	1·000000	59°·9	1·005711
38·5	1·001461	62·15	1·006172
43·5	1·002571	64·175	1·006439
49·1	1·004466	66·75	1·006630
52·25	1·005212	69	1·006953
56·75	1·005366	74·75	1·008504

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* Ann. de Chim. et de Phys. xl. 209.

Temp.	Volume.	Temp.	Volume.
77°	1·008973	131°·48	1·057679
88·25	1·011889	142·25	1·061349
96·575	1·014338	152·375	1·067985
100·85	1·046847*	163·625	1·071143
110·3	1·051709	175·1	1·075070
120·425	1·054319	186·125	1·079086

We see from this table the sudden increase of volume which the phosphorus undergoes when it becomes liquid. While the phosphorus is solid, the increase of volume which it sustains is nearly proportional to the temperature. But liquefaction produces a sudden expansion independent of the temperature. After liquefaction, the dilatations from a given quantity of heat are much greater than when the phosphorus was solid, but still these dilatations are proportional to the temperature.

There are various bodies, which, when cooled down to a particular point, change their state, and from being liquids become solids. This change is frequently accompanied by a diminution of bulk. Thus when olive oil freezes, it sinks to the bottom of the unfrozen portion, indicating an increase of density or a diminution of bulk. When melted gold or silver is allowed to congeal, it also contracts in its dimensions, or diminishes in bulk. It is this diminution which prevents money from being cast of these metals, and obliges us to stamp it. But the case is very different with water and with many other liquid bodies when they congeal. They experience a considerable augmentation of bulk in

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* At this temperature the phosphorus melted.

Some bodies expand when they become solid.

the act of congealing. The consequence is, that the specific gravity of the solid body is less than that of the liquid, though its temperature be lower. These bodies, at that particular temperature at which they change their state, constitute another exception to the general law of expansion by heat. They increase in bulk when cooled down, instead of when heated. This change, however, is obviously owing to the new form which the body assumes, and indicates a new arrangement of the particles, in consequence of which they separate to a greater distance than formerly.

I found, a good many years ago, that ice, supposing it quite free from air bubbles, will remain at rest in any part of a quantity of alcohol diluted with water till its specific gravity be reduced to 0·92: hence it obviously possesses the specific gravity of 0·92. Messrs. Roget and Dumas state the specific gravity of ice to be 0·95.* When ice shoots upon the surface of water, it forms prisms, making angles of 60° and 120° with each other. Mr. Dalton has shown that if we consider the particles of water to be spherical and of the same size, and suppose a cubic vessel to be filled with water, the particles at the bottom may be conceived to be placed in regular rows. The second stratum will also constitute regular rows, but each particle will be placed in the intervals between the inferior row, so that each particle will rest upon or in the interval between 4 particles below it. Let us suppose

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* Annals of Philosophy (2d Series), iii. 392.

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the cubic box to be drawn into a rhombus, the sides of which make angles of 60° and 120° with each other, the lowest stratum will still consist of the same number of particles as before; but every particle, instead of touching four others, as in the former case, will now touch six others, so that the intervals between the particles will be less than before. But the height of the pile will be increased, every particle in the second stratum resting in the interval of three particles below it. Mr. Dalton has shown that the capacities (or the number of particles) in the two vessels will be as 0·707 to 0·750 very nearly, or as 1·00 to 0·942.* Now this is very nearly the difference between the specific gravity of water and ice. Hence it is probable that the diminution of density is owing to such a new arrangement of the particles of water.

As water.

The prodigious force with which water expands in the act of freezing has been long known to philosophers. Glass bottles filled with water are commonly broken in pieces when the water freezes. The Florentine academicians burst a brass globe, whose cavity was an inch in diameter, by filling it with water and freezing it. The force necessary for this effect was calculated by Muschenbroeck at 27,720 lbs. But the most complete set of experiments on the expansive force of freezing water are those made by Major Williams at Quebec, and published in the second volume of the Edinburgh Transactions.

* Dalton's New System of Chemical Philosophy, i. 135.

The same expansion is observed during the crystallization of most of the salts; all of them at least which shoot into prismatic forms. Hence the reason that the glass vessels in which such liquids are left usually break to pieces when the crystals are formed. A number of experiments on this subject have been published by Mr. Vauquelin.*

Cast iron, bismuth, and antimony.

Several of the metals have the property of expanding at the moment of their becoming solid. Reaumur was the first philosopher who examined this point. Of all the metallic bodies that he tried, he found only three that expanded, while all the rest contracted on becoming solid. These three were *cast iron*, *bismuth*, and *antimony*.† Hence the precision with which cast iron takes the impression of the mould.

Crystals.

When bodies crystallize, they generally increase in bulk; but when they become solid without any appearance of crystallization, diminution of bulk very frequently accompanies the change. Most of the oils when they solidify, form very regular spheres. The same thing happens to honey and to some of the metals, as mercury, which Mr. Cavendish has shown from his own experiments, and those of Mr. Macnab, to lose about $\frac{1}{23}$d of its bulk in the act of solidification.‡ When sulphuric acid congeals, it does not perceptibly expand, nor

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* Ann. de Chim. xiv. 286.

† Mem. Par. 1726. p. 273. Berthollet's Statique Chimique, ii. 348. The other metals tried by Reaumur were gold, silver, copper, tin, lead, and zinc.

‡ Phil. Trans. 1783, p. 23.

Chap. I. does it in the least alter its appearance. Sulphuric acid, of the specific gravity 1·8, may be cooled down in thermometer tubes to —36° before it freezes; and during the whole process it continually contracts. At —36°, or about that temperature, it freezes; but its appearance is so little altered, that I could not satisfy myself whether or not the liquid was frozen till I broke the tube. It was perfectly solid, and displayed no appearance of crystallization. On the other hand, cast iron expands in the act of congealing.

SECT. II. OF THE THERMOMETER.

The knowledge of expansion qualifies us for understanding the nature of the *thermometer*, an instrument contrived for measuring the alterations which may take place in the temperatures of bodies.

Discoverer of the thermometer.

"The invention of the thermometer, like that of gunpowder, is involved in considerable obscurity. Drebbel, a physician at Alkmaer in Holland, is stated by Boerhaave to have made thermometers about the beginning of the 17th century. Sanctorio, the celebrated founder of statical medicine, who was a professor at Padua at the commencement of the 17th century, lays claim to the invention of the thermometer.* And this claim is sanctioned by Borelli, who gives us an engraving, together with a description of the original thermo-

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* Com. in Galen. Art. Med. p. 736, 842; as quoted by Dr. Martine.

meter of Sanctorio.[*] Malpighi, also, who was a professor at Pisa, and the intimate friend of Borelli, ascribes in his posthumous works the original invention of the thermometer to Sanctorio.[†] These testimonies are sufficient to satisfy us that Sanctorio was the first person who thought of constructing a thermometer, at least in Italy, which was at that period the peculiar seat of the sciences.

First thermometer an air one.

Sanctorio's thermometer was merely a glass tube with a ball blown at the extremity, the open end of which, after the air had been somewhat rarefied, was plunged into a coloured liquid. When the air cooled it resumed its original bulk nearly, and a portion of the coloured liquid rose in the tube. This tube was divided into a number of equal portions, called degrees. When the temperature of this tube was raised, the air in it expanded, and the coloured liquid sank in the tube. When its temperature was lowered, the bulk of the air diminished, and the coloured liquid rose in the tube. The number of degrees which the coloured liquid rose or fell indicated the change of temperature. Thus Sanctorio's instrument was what is called an air thermometer; the changes of temperature being indicated by the alterations in the volume of the air confined in the tube. As the tube was plunged into an open dish filled with coloured liquid, it is evident that the rise and fall of that liquid would be affected not merely by alterations in the tem-

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[*] De Motu Animalium, lib. ii. prop. 175.

[†] Oper. Post. p. 30. See Martine's Essays, p. 4.

perature, but also by all changes in the density of the atmosphere. When the barometer stood high, the liquid would be more elevated in the tube than when the barometer was low, even supposing no alterations in the temperature.

Improved by the Florentine academicians,

The Florentine academicians about the middle of the 17th century made the first improvement on thermometers. They employed a long glass tube, blown at one extremity into a ball, which they filled up to a certain mark in the tube with spirit of wine. The extremity of the tube was then sealed hermetically, by melting it by a blowpipe. The tube was afterwards divided into 100 equal parts, called degrees, by means of small particles of white enamel.* Boyle claims for himself the merit of first introducing such sealed instruments into England. At first, he says, no one would believe that a liquid would expand and contract in a tube hermetically sealed. But he convinced himself of the fact by actual trial, and was still farther satisfied by the sight of a small thermometer constructed in this way from Florence.†

and by Fahrenheit.

About the beginning of the 18th century, Mr. Fahrenheit, originally a merchant in Dantzic, who, after failing in business, settled at Amsterdam as a thermometer-maker, substituted mercury for spirit of wine, and greatly diminished the size of the tube and the bulb. This rendered the instrument capable of measuring much higher degrees of

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* See Waller's Translation of the Essays, &c. of the Academy del Cimento, p. 2.

† Shaw's Boyle, i. 582.

temperature; for mercury does not boil till raised to a much higher temperature than spirit of wine.

The instrument, as originally made, laboured under a great defect, and many years elapsed before philosophers thought of the proper remedy. No two instruments agreed with each other. The scale of degrees applied to the tube was quite arbitrary. It was differently constructed and differently applied in every thermometer; and experiments made with one could not be usefully compared with those made with another.

How made to correspond with other thermometers.

The most important improvement in these instruments was the contrivance of a method of applying their scales so as to make them agree with each other when exposed to the same temperature, whatever that may be. This was attempted by different methods in different parts of Europe, till at last one was hit upon so superior to all the rest, that it was soon universally employed.

Sir Isaac Newton seems to have first proposed this method;* and Fahrenheit was probably the first thermometer-maker that put it in practice. It is founded on two discoveries made by Dr. Hooke. The first in 1664, the second in 1684. It was observed by Dr. Hooke that water is changed into ice when cooled down to a particular temperature, and that this temperature remains the same all the time that the water is changing into ice, or the ice into water. If we take a thermometer and plunge it into melting snow, taking care that

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* Phil. Trans. 1701. No. 270, p. 824.

the ball be completetly covered, the quicksilver will be contracted by the cold and descend in the tube. It will at last stop and continue at the same place so long as any considerable part of the snow remains unmelted. If we now mark the part of the tube at which the mercury stopped, and repeat the experiment with the same thermometer however often, and at places and times however distant, the result will always be the same, the mercury will always descend to the same part of the tube to which it descended the first time, and will remain stationary there so long as any considerable part of the snow remains unmelted.* This shows that melting snow is always equally cold, or has the power of reducing the thermometer to one steady density, which may be called the *melting snow expansion* of the quicksilver.

The second discovery of Dr. Hooke was of a similar nature. He found that other things being the same, water always begins to boil at the same temperature. If, therefore, we take the thermometer used in the preceding experiments, immerse it in boiling water, or surround it with steam, and

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* I have taken no notice in the text of the curious observations lately made at Geneva, that thermometers graduated in the way I have described and kept for a considerable time, at last came to stand permanently a degree or half a degree above the original freezing point, when plunged into melting snow. I do not understand the reason of this change, and am satisfied that it does not universally take place; for I possess a thermometer graduated more than 40 years ago by Mr. Crichton of Glasgow, the freezing point of which is still as accurate as when the instrument was made.

keep the liquor boiling around it for some time, the mercury will ascend to a certain point in the tube, and however long we continue to boil the water, it will ascend no higher. If we mark the part of the tube to which the mercury rose, and afterwards repeat the experiment ever so often in places of the same height above the surface of the sea, and when the height of the barometer is the same, the mercury will always rise to the same point as the first time. Thus boiling water has the power of bringing mercury to another determinate state of expansion, which may be called the *boiling water* expansion of mercury.

These two points may be marked upon the tube of any thermometer, by plunging it first into melting snow, and then into boiling water. The distance between them will be very various in different thermometers, on account of their different sizes, and the different proportions which the balls and tubes bear to each other. But being marked on each instrument, they will enable us to perceive when the instrument is reduced to the temperature of boiling or freezing water. The distance between these two points may be divided into any number of parts or degrees, taking care always to divide it into the *same* number of degrees in each thermometer, and to number them in the same way in each. The corresponding degrees in the several thermometers, will show the corresponding states of expansion in these different instruments. If we wish to measure other states of expansion *above* or *below* the primary points, we can protract the scale above or below these points, by adding to it as many degrees of the same size as the tube will hold, numbering these also in a similar manner in every thermometer. Or we may choose other fixed points which have been found out for these parts of the scale, such as the boiling point of mercury, the melting point of lead, bismuth or tin for the higher extremity of the range, and the cold produced by mixing snow or salt, or by pouring dilute nitric acid on snow, for the lower extremity.

As this method was adopted in different countries at different times, and as there never was any connexion between thermometer-makers in different countries, the consequence has been, that the space between the freezing and boiling water points, has been divided into a different number of degrees, and this has occasioned a diversity in the thermometers used in different countries, which it will be requisite to explain.

Fahrenheit's thermometer.

Fahrenheit's division is the one which is followed in this country. He did not begin his scale at the melting snow point; but at the temperature produced by mixing snow, and common salt, or snow and sal ammoniac. He marked the point at which the thermometer stood when put into such a mixture, zero (0°), and of course made it the beginning of his scale. He then plunged the thermometer into melting snow, and marked the place at which the mercury became stationary in this situation. He divided the distance between these two points into 32 equal parts or degrees. So that upon his scale, the temperature produced by mixing snow

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and common salt, is marked 0°, while the freezing point of water is marked 32°. He farther protracted the scale with degrees of the same size, to the top of the tube increasing arithmetically. When a thermometer constructed in this way is plunged into boiling water, the mercury stands at 212°. So that the interval between the freezing and boiling water points, is 180°. To measure greater colds than 0°, a series of degrees of the same size as the others was continued downwards, as far as the tube would admit. These increase arithmetically downwards, and are considered as degrees of cold. They are called degrees below zero, or by prefixing the mark (—) *minus* before them. Thus —20°, means 20° below zero.

The instrument is not now constructed in the way that Fahrenheit followed. All that is necessary, is to find the freezing and boiling water points, to mark the first 32° and the second 212°, and to divide the interval between them into 180 equal parts or degrees. The scale is then protracted upwards and downwards as far as the length of the tube will allow.

Reaumur's thermometer.

In France, numerous experiments on the construction of thermometers were made by Reaumur, during the first half of the 18th century. His thermometer was of spirit of wine. The freezing water point on it was marked 0°, the boiling water point 80°.* De Luc afterwards substituted mercury

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* Reaumur's Memoirs on this subject, will be found in the Volumes of the Memoirs of the Paris Academy of Sciences, for 1730 and 1731.

for spirit of wine, and corrected the inaccuracies under which the original thermometer of Reaumur laboured. But as he did not alter Reaumur's division, the instrument continued always to be called Reaumur's thermometer. It was employed in France before the revolution, and is still used in some parts of Europe. Every degree of Reaumur is equal to $2\frac{1}{4}$ degrees of Fahrenheit. To convert the degrees of Reaumur to the corresponding number of Fahrenheit, we must multiply by 2·25 and add 32 to the product.

Celsius's.

Celsius, who was professor of astronomy at Upsala, published an account of a new thermometer in the Memoirs of the Stockholm Academy, for 1742.* This thermometer was a mercurial one; the freezing water point was marked 0°, and the boiling water point 100°. This thermometer is used in Sweden. It was adopted by the French at the revolution, under the name of *thermometre centigrade*, and has come into general use in that country, and in several other parts of Europe. Every degree of it is equal to $1\frac{4}{5}$ degrees of Fahrenheit. To convert the degrees of Celsius to those of Fahrenheit, multiply them by 1·8 and add 32° to the product.

Thermometre centigrade.

Delisle's.

M. Delisle, a French astronomer, was invited to St. Petersburgh by the Empress Catherine I.

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* Vol. iii. p. 171. Celsius relates experiments showing that the freezing and boiling points of water are constant, other things being equal; but that the boiling water point varies with the height of the barometer. He quotes Dr. Martine, Sir Isaac Newton, Fahrenheit, &c. who had preceded him in his observations.

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During his residence in that capital from 1726 to 1748, he constructed a thermometer, which has been adopted by the Russians, and is usually known by the name of Delisle's thermometer. In it the numeration begins at the boiling water point, which is considered as the boundary between heat and cold. The numbers from that point increase downwards, and therefore express degrees of contraction. The boiling water point is marked 0°, and the freezing water point —150°. Hence, 5 degrees of Delisle are equal to 6 of Fahrenheit. To convert the degrees of Delisle to those of Fahrenheit, multiply them by 1·2. Subtract the product from 212 if the heat be below boiling water. But if the heat denoted exceed that of boiling water, we must add the product to 212°.

Whether the expansion corresponds with the heat.

After thermometers were graduated so as to give results capable of being compared with each other, it became natural to inquire whether the degrees on the scale represented equal increments of heat. The scale of a thermometer divides the increments and diminutions of bulk into a number of small equal parts, that we may see by how many of these parts the bulk of the mercury is increased at one time, or diminished at another. But it remains to be considered whether these equal alterations of bulk be produced by equal alterations of heat. Dr. Brook Taylor first thought of determining the point experimentally on a lintseed oil thermometer. The result of his experiments was published in the Philosophical Transactions for 1723.* Dr. B.

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* Vol. xxxii. No. 376, p. 291.

Taylor's mode of experimenting was again tried by Dr. Black, in 1760, without being aware that he had been already anticipated. But Dr. Black made his experiment on a mercurial thermometer. De Luc made the same experiment nearly about the same time. Dr. Crawford did the same thing several years later, and published the result in his *Treatise on Heat.**

To make the experiment successfully, it is necessary that the tube of the thermometer be perfectly cylindrical, or that the length of each degree should vary according to the size of the particular part of the tube which it represents. Dr. Taylor's experiment was made by mixing together hot and cold water, by which we can make sure of a knowledge of certain differences of temperature, independent of any thermometer. If we mix together 1 pound of water of the temperature 100°, and another pound of the temperature 200°, it is obvious that the surplus heat of the hot water will be equally divided between the two portions of that liquid. One half of it will enter the coldest portion and increase its temperature, while the hottest portion will have lost one half of its surplus heat; the whole mixture will acquire the same temperature; and it is obvious that if the thermometer be an accurate measurer of heat, this intermediate temperature will be the arithmetical mean between the temperatures of the two liquids mixed, or 150°.

The result of the experiments of Black, De Luc,

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* Experiments and Observations on Animal Heat, p. 20.

and Crawford, is that the expansion of mercury measures very nearly equal increments of heat, as high as their experiments went; that is, to 212°. The variation (if any exist) is so small, that it may be safely neglected without any sensible error. But from the experiments of Dulong and Petit, there is reason to conclude that the increasing rate of the expansion of mercury becomes sensible at higher temperatures than 212°. This increased rate becomes sensible when we expose a mercurial and an air thermometer to the same temperature, (correcting the instruments for the expansion of glass;) the following are the points on the scale at which they respectively stand.

Air therm.	Merc. therm.	Difference.
212°	212°	0
299·66	302	2·33
386·69	392	5·31
473·09	482	8·91
558·86	572	13·14
662	680	18

We see from this table (the experiments were made by Dulong and Petit), that the boiling point of mercury, measured by its own expansions, is 680°; but measured by the expansion of air, 662°. Hence the increment of expansion over that of heat at that temperature, amounts to 18°, or (nearly) $\frac{1}{37}$ of the whole.

Error corrected by the expansion of the tube.

If we plunge a common well graduated thermometer into boiling mercury, it stands, according to the observations of Mr. Crichton, at 660°: so that the expansion of the glass is equivalent to 20°.

Chap. I. It therefore almost exactly counteracts the increase of the rate of the expansion of the mercury. The consequence of this fortunate coincidence is, that an accurately graduated mercurial glass thermometer is an accurate measurer of the increase of temperature as high as the boiling point of mercury, or to 662°.

From the different methods followed by philosophical instrument makers in determining the boiling point, it was found that thermometers very seldom agreed with each other, and that they often deviated several degrees from the truth. This induced Mr. Cavendish to suggest to the Royal Society the importance of publishing rules for constructing these very useful instruments. A committee of the Society was accordingly appointed to consider the subject. This committee published a most valuable set of directions, which may be consulted in the Philosophical Transactions.* The most important of these directions is, to expose the whole of the tube as well as the ball of the thermometer to steam, when the boiling water point is to be determined. They recommend this to be done when the barometer stands at 29·8 inches. When a thermometer is to be employed as an instrument to determine the temperature of any place where it is suspended, it is obvious that it never can be accurate unless this mode of graduation be followed. But when we employ it in the laboratory to determine the temperature of

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* Phil. Trans. 1777, p. 816.

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water or any substance, it would be inconvenient and often impossible to plunge the whole instrument into the body whose temperature we wish to determine. In general, we can only apply or plunge the bulb into it. Hence it is better to graduate such instruments simply by plunging the bulb into the freezing and boiling water, in order to determine the freezing and boiling water points.

Thermometer should be small.

The thermometer merely indicates the change of temperature which it undergoes itself, when applied to a hot or cold body. It will not give us a correct idea of the temperature of another body into which we plunge it, unless it bears a very small ratio in point of size to that of the body under examination. We must wait for some time till the thermometer become stationary before we draw our conclusion. If the temperature of the body examined be undergoing alteration, (either augmenting or diminishing,) the size of the thermometer applied ought to be very small, that it may acquire the temperature of the body to which it is applied as rapidly as possible. Indeed, if the thermometer be of a considerable size, it will never indicate the maximum temperature of a body, provided that temperature be of short duration. I suspended a very large and a very small thermometer near each other in a north exposure, and shaded from the sun, to determine the summer temperature of Glasgow; and I almost constantly found the small thermometer a degree or two higher than the large one, about the time of the day when the temperature was highest, and a degree or two lower when the temperature was coldest. The mean temperature of the day indicated by each thermometer corresponded, but the extremes differed several degrees.

Its range small.

The temperatures which we can measure by a mercurial thermometer are confined within narrow limits. For mercury freezes at about 39° below zero, and boils at 660°. Hence we cannot employ it to measure greater heats than 660°, nor greater degrees of cold than —39°. Yet many temperatures connected with our most common processes are much higher than 660°. The heat of a common fire, the temperature at which silver, copper, and gold melts, and many other such points, offer familiar examples.

Wedgewood's pyrometer.

Mr. Wedgewood contrived an instrument for measuring high temperatures, which was known by the name of Wedgewood's pyrometer, and was at one time in general use. It consisted of small pieces of Cornish clay, moulded into cylinders of a determinate size, and baked in a low red heat. These pieces were of such a size as just to enter between two square brass rods, fixed on a brass plate 24 inches long, half an inch asunder at one extremity, and 0·3 inch at the other. The brass rods were divided into inches and tenths, making in all 240 divisions or degrees. When pieces of clay baked in Wedgewood's manner are exposed to heat, they shrink in their dimensions, and the shrinkage in Wedgewood's opinion was proportional to the temperature. This was the foundation of his instrument. The heat to which the clay piece was exposed was indicated when its shrinkage was measured between the brass rods. If exposed to the heat at which silver melts, it advanced between the brass rods to 22°, or 2·2 inches. If to the melting point of gold, to 32°; if to the melting point of cast iron, to 130°, and so on.

But this pyrometer of Wedgewood has been long out of use. For it was found that if a clay piece was long exposed to a low temperature, it shrunk as much in its dimensions as if it had been exposed for a short time to a much higher temperature. In short, the time of exposure has as much effect as the temperature upon the alteration of the dimensions of the clay pieces.*

Daniell's pyrometer.

In 1803, Guyton de Morveau presented to the French Institute a pyrometer of platinum, which measured high temperatures by the expansion of a plate of that very refractory metal.† Mr. Daniell, of London, constructed a pyrometer upon similar principles, and published a description of it in the 11th volume of the Journal of the Royal Institution, in 1821. His instrument consists of a bar of platinum $10\frac{1}{5}$ inches long and 0·14 inch in diameter. It is placed in a tube of black lead or earthen ware, and the difference between the expansion of the platinum bar and the earthen ware tube is indicated on a circular scale. This pyrometer indicates a change of about 7° of Fahrenheit's scale; or, in other words, 1° of Daniell is equal to 7° Fahren-

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* See for an account of Wedgewood's pyrometer Phil. Trans. 1782, p. 305; 1784, p. 358; 1786, p. 390.

† Ann. de Chimie, xlvi. 276.

Chap. I.

heit. Mr. Daniell has published the following table of results obtained by him by means of this pyrometer. He places the greatest reliance on the fusing point of silver, which was obtained by him three different times, each time within a degree of each other.*

	Daniell.	Fahrenheit.
Boiling point of mercury	92°	644°
Fusing point of tin	63	441
———— bismuth	66	462
———— lead	87	609
———— zinc	94	648
———— brass	267	1869
———— silver	319	2233
———— ditto,	——	1830†
———— copper	364	2548
———— gold	370	2590
———— cast iron	497	3479
Red heat just visible in day light	140	980
Heat of a common fire	163	1141
Full red heat	——	1200†
Orange heat	——	1650†

The thermometer has enlarged our notions of heat.

The thermometer, notwithstanding the unavoidable defects under which it still labours, has contributed very much to enlarge our notions respecting heat. *Heat* and *cold* constitute two of the most familiar words in our language. When either heat or cold is intense, it constitutes a sensation of the strongest kind. Both are capable of destroying

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* Edin. Phil. Journ. v. 397.

† As determined by Mr. Princep by an air thermometer.

life. We are accustomed to consider both heat and cold as a positive something, and it requires an attentive consideration to render it probable, that *cold* is nothing else than the absence or abstraction of heat. But the thermometer furnishes a decisive proof of the accuracy of this opinion, which is level to the meanest capacity. It *contracts* uninterruptedly by a gradual abstraction of heat, and as this contraction continues to the very lowest point which we are capable of reaching; we can form no reasonable doubt that the greatest cold is produced by the abstraction of heat, as well as the smallest. We have no doubt that the congelation of lead or tin takes place, when a certain quantity of heat is withdrawn from these bodies. The congelation of bees' wax, of tallow, and of phosphorus, is produced in the very same way, and so also is the freezing of water. Yet in this last case, *heat* gives place to another sensation, namely, of *cold*. We never in common life speak in this last case of a diminution of heat; but of an increase of cold. But the continued contraction of the thermometer, and the analogy between the freezing of lead, wax, and water, soon induce the belief that both are caused in the same way; namely, by the abstraction of heat. Thus our ideas of the operations of heat are greatly extended. We now know that no mass of matter has ever yet been found totally destitute of heat. For we have no sufficient evidence that pure alcohol has ever yet been frozen; though we are sure that it only requires a sufficient diminution of heat to produce that effect on it.

No body without heat.

Coldest climate of the globe.

Indeed, if we recollect that heat is continually emanating from surrounding bodies, and that bodies absorb it so much the more greedily the colder they are, we cannot conceive a body altogether void of it, which is exposed in the neighbourhood of others that contain it. The coldest part of the earth of which we have any accurate knowledge, is Melville island, situated in 74° 47′ of north latitude, and 110° 48′ west longitude from Greenwich, where Captain Parry wintered during the year 1819-20. During the months of November, December, January, February, and March, the thermometer was occasionally as low as —50°, and at some distance from the ship, as —55°. In October it sank as low as —28°, in May as low as —4°. During January, it was never higher than —2°, and in February than —17°. During every month of the year it froze. The hottest month was July, and the maximum temperature was 60°. There were only 5 months in the year in which the maximum temperature exceeded 32°, these were May, June, July, August, and September. The following table exhibits the maximum and minimum temperature of the months as observed by Captain Parry.

	Maximum.	Minimum.
January .	— 2° .	—47°
February .	—17 .	—50
March .	+ 6 .	—40
April .	+32 .	—32
May .	+47 .	— 4
June .	+51 .	+28
July .	+60 .	+32
August .	+45 .	+22

	Maximum.	Minimum.
September	+37°	— 1°
October	+17·5	—28
November	+ 6	—47
December	+ 6	—43

So that the highest temperature observed was 60°, and the lowest —50°, making a range of 110°. In some parts of Africa, and even of Asia, the thermometer is said to have been observed as high as 138°.

Discovery of the congelation of mercury.

Quicksilver in our climate, and indeed in every habitable climate, is usually in a liquid state; and it was long the opinion of chemists, that fluidity was a property essentially belonging to this metal. The fixing (or rendering solid) of mercury was a favourite pursuit of the alchymists, and constituted, in their opinion, one of the great steps towards the discovery of the Philosopher's stone. The discovery of its congelation constitutes a memorable era in chemistry; as it altered and materially improved the opinions of chemists respecting the effects of heat. It deserves, therefore, to be briefly noticed here.

We are indebted to M. Braun, professor of philosophy in the Imperial Academy of Petersburg, for the discovery of the congelation of quicksilver by cold. Dr. Zeiher, professor of mechanics in the same Academy, had repeated Fahrenheit's experiments with freezing mixtures, before he came to settle at St. Petersburg, and he expected to be able to prosecute them still farther in that city, where the natural cold is frequently intense. Illness preventing him from putting his project in execution, he communicated it to M. Braun, who readily undertook it. A proper opportunity occurred on the 14th December, 1759, as the thermometer on that day sank in the open air to —34°. M. Braun prepared a mixture with nitrous acid and pounded ice, into which a thermometer being put, sunk to —69°, lower by about 30° than it had fallen in any preceding experiments of this nature. Animated by the hope of producing still greater cold, he entered on his experiment anew, and all his pounded ice being exhausted, he was fortunately obliged to substitute snow in its place. With this fresh mixture, he sank his thermometer to —100°, —244°, and at last to —352°. Surprised at such results, he drew the instrument out of the mixture, and found it entire, but the quicksilver was fixed and remained immoveable for above 12 minutes. On repeating the same experiment with another thermometer, graduated no lower than —220°, all the mercury sank into the ball, and became solid as before; not beginning to re-ascend till after a great interval of time.

From these appearances, M. Braun concluded that the quicksilver in both instruments had been frozen or solidified by the cold. He announced his opinion as a probable fact to the Academy, and prepared a new set of thermometers, in order to obtain decisive evidence. On the 25th December (old style), in company with Æpinus, professor of physics, he repeated the experiments, and as soon as he found the quicksilver immoveable, he broke the bulb of his thermometer. Now all doubts were removed. He obtained a solid, shining, metallic, mass, which extended under the strokes of a pestle, in hardness rather inferior to lead, and yielding a dull dead sound like that metal. Æpinus observed, at the same time, that the frozen mercury assumed a concave surface, and that congealed pieces of it sunk in fluid mercury: all evident proofs of its great contraction.

Freezing point of mercury.

In the year 1775, Mr. Hutchins repeated these experiments at Albany Fort, Hudson's Bay, congealed the mercury by cold, but was equally at a loss with Braun, to determine the true point of congelation. Mr. Cavendish, to obviate his difficulties, sent him out a proper set of instruments, together with ample directions how to proceed. In the winter 1781-82, Mr. Hutchins made the requisite experiments, an account of which was published by Mr. Cavendish in the Philosophical Transactions for 1783. The mercury was frozen in wide glass tubes, and the thermometer to determine its freezing point was plunged into it, and kept in it during the whole process of congelation. It remained stationary during the freezing at —38°·66, showing clearly that this is the freezing point of that metal. Mr. Cavendish showed that in the act of congealing mercury contracts about $\frac{1}{23}$d of its bulk. This accounts for the very low point to which the thermometer appeared to sink in M. Braun's experiments. The freezing point was —38°·66, to which it sunk in the first place. By freezing, it diminished in bulk $\frac{1}{23}$d part, which is equivalent to 452°. So that his thermometers, supposing the true cold not to have exceeded —40°, would have indicated the low temperature of —492°.

Having thus explained at some length the nature of the thermometer, it will be proper now to point out some of the most important new facts respecting heat, which have been acquired by means of the thermometer. These facts will serve still farther to extend our knowledge, and rectify our opinions respecting heat.

SECTION III. OF SPECIFIC HEAT.

It has been already observed that heat has a tendency to diffuse itself from any hotter body to the colder bodies around it. This distribution continues till no body is disposed to take any more heat from the rest. When all mutual action has ceased, if we apply a thermometer to any one of the contiguous bodies we shall find that they all indicate the same temperatures. We must therefore adopt, as one of the most general laws of heat, that *all bodies communicating freely with each other and exposed to no inequality of external action, acquire the same temperature as indicated by the thermometer.*

Neighbouring bodies acquire the same temperature.

When bodies are brought into this state they are said in common language to be equally hot; and even chemists and philosophers were long of opinion that all bodies in such circumstances, supposing the weight the same, contained exactly the same quantity of heat. An experiment of Fahrenheit, made at the request of Boerhaave, first led to an accurate investigation of the subject.*

Experiment of Fahrenheit.

Fahrenheit took equal volumes of water and mercury at different temperatures, and agitated them together. The temperature produced was not the mean between that of the water and mercury previous to mixture, as it would have been had equal volumes of hot and cold water been thus agitated together. When the water, previous to agitation, was hot and the mercury cold, the new temperature resulting was greater than the mean, and it was less than the mean, if the mercury was hot and the water cold. To produce the mean temperature it was necessary to agitate together 2 volumes of water and 3 volumes of mercury. When such measures were taken, Boerhaave assures us that the mean temperature always resulted, whether the water or the mercury was hottest before the mixture.

From which the specific heats of bodies was deduced.

It is very surprising that Boerhaave drew as a conclusion from this experiment, that heat is distributed through bodies in proportion to their volume, not to their weight. It was Dr. Black who first drew the proper inference from this experiment about the year 1760, while a lecturer on chemistry in the University of Glasgow. Dr. Black reasoned upon the experiment in the following manner.

Let us suppose a given volume of water at 100° mixed with the same volume of quicksilver at 150°. We know that the mean temperature between 100° and 150° is 125°, which would be produced by

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* See Boerhaave's Chemistry translated by Shaw, vol. i. p. 291.

mixing water at 100° with an equal volume of water at 150. But when hot quicksilver is used instead of water, the temperature of the mixture is only 120° instead of 125°. The mercury therefore has lost 30° of heat, while the water has become warmer by 20° only. And yet the heat which the water has gained is the very same which the mercury has lost. We see from this that the same quantity of heat has a greater effect in heating mercury than in heating water. If it heat mercury 3° it will heat water only 2°, or if it heat water 1° it will heat mercury $1^\circ\frac{1}{2}$. So that if we reckon the *capacity* of mercury for heat 1, the capacity of the same volume of water will be $1\frac{1}{2}$. But mercury is $13\frac{1}{2}$ times denser than water. Consequently to find the relative capacity of the same weights of mercury and water we have only to multiply 1.5 by $13\frac{1}{2}$. The product $20\frac{1}{4}$ will give us the capacity of water for heat, if we reckon that of mercury 1; supposing Fahrenheit's experiment to have been accurately made.

Of Dr. Martine.

The experiment of Fahrenheit was observed by Dr. Black to agree very well with another experiment related by Dr. Martine in his essay on the heating and cooling of bodies. He placed before a good fire and at equal distances from it, a quantity of water and an equal volume of mercury, each of them contained in equal and similar glass vessels, and each having a delicate thermometer immersed in it. He found in repeated experiments that the mercury was warmed by the fire much faster than the water; indeed almost twice as fast. After each experiment having heated each of these two fluids to the same degree, he placed them in a cold stream of air, and found that the mercury was always cooled much faster than the water.* The reason of this result is obvious when we know that mercury has a smaller capacity for heat than water. It heats sooner than that liquid, because less heat is necessary to raise it the same number of degrees, than is required to make the same change in the temperature of water. It cools sooner than water, because it has less heat to lose than that liquid.

Of Irvine and Watt.

Dr. Black satisfied himself with establishing the fact that bodies have different capacities for heat, from these two experiments of Fahrenheit and Martine. But Dr. Irvine, who succeeded him as lecturer on chemistry in Glasgow, and Mr. Watt, who was at that time a philosophical instrument maker in that city, took up the subject and made many experiments to ascertain the relative capacities of different bodies. But they did not think it necessary to lay the result of their experiments before the public.

Of Dr. Crawford.

Dr. Crawford probably inserted a table of *specific* heats in the first edition of his Experiments on Heat, published in 1779.† He was therefore the earliest author on the subject. In the 17th volume of the Journal de Physique, published in 1780, there is a treatise on heat by

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[*] Martine's Essays, p. 74.

[†] I have never seen a copy of the first edition of this work; but have no doubt that it contained experiments on the capacity of bodies for heat, as his theory was founded on it.

Magellan, containing a pretty copious table of specific heats, furnished, as Magellan informs us, by Mr. Kirwan.

Of Wilcke.

In the Memoirs of the Swedish Academy of Sciences for 1781, there is an ingenious Essay on the specific heat of Metals, &c. by Mr. J. C. Wilcke. Whether he was acquainted with the previous labours of the British chemists on this subject does not appear, as he nowhere makes the least allusion to them: but quotes Klingenstjerna, as the author who first started the doctrine of the different capacity of bodies for heat. Klingenstjerna died in 1765, and, from the passage which Wilcke quotes, he appears to have reasoned from the experiment of Fahrenheit precisely as Dr. Black did. Thus it would seem that both Black and Klingenstjerna discovered the doctrine of the different specific heat of bodies, without any communication with each other; but from a knowledge of an experiment made long before by Fahrenheit, and erroneously reasoned from by Boerhaave. Which of the two had the precedence in point of time, I have no means of knowing.

Of Lavoisier and Laplace.

A set of experiments undertaken by Lavoisier and Laplace, to determine the specific heat of various bodies, appeared in the Memoires of the Paris Academy of Sciences for 1780; not published probably till 1784.

After the publication of the second edition of Crawford's Treatise on Heat in 1788, a considerable interval of time elapsed, before any addition was made to our knowledge of the specific heats of bodies. A set of experiments to determine the specific heats of 16 different species of wood, by Professor Meyer of Erlangen, appeared in Crell's Annals for 1798. Professor Leslie of Edinburgh, in his Treatise on Heat published in 1804, gave the specific heats of a few bodies determined in the same way as Professor Meyer had done that of the woods. Mr. Dalton adopted the same mode of experimenting, and in the first volume of his New System of Chemical Philosophy published in 1808, he gave a number of results obtained by him in that way.

Of Meyer, Leslie, and Dalton.

In 1813, a most elaborate set of experiments was published by Delaroche and Berard on the specific heats of gaseous bodies and vapours. These experiments were made in Berthollet's laboratory, and the apparatus had been constructed at the expense of that eminent chemist.* These experiments seem to have been conducted with care; but the experimenters were guilty of an oversight which prevented their results from being very near approximations to the truth. The gases on which they experimented were moist. Hence the influence of the vapour would vary, according to the specific gravity of the gas; this introduced a variable unknown quantity, which ought to have been subtracted in order to obtain the true specific heat of each gas.

Of Delaroche and Berard.

In 1823, a new set of experiments was made by Mr. Haycraft,† with an apparatus resembling that

Of Haycraft.

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* See Ann. de Chim. lxxxv. 72, or Annals of Philosophy, ii. 134.

† Edin. Trans. x. 195.

Chap. I.

of Delaroche and Berard. But he took the precaution of drying his gases with great care. This enabled him to obtain results agreeing accurately with each other, and obviously very near approximations to the truth. Indeed, his results have been since fully corroborated by the new experiments of MM. Delarive and F. Marcet, who determined the specific heat of no fewer than 14 gases.* Their method was to measure the time that a thermometer, plunged into each gas, took to be heated a certain number of degrees, the gas being exposed to a constant source of heat. To obviate the effect of mobility, the globe containing the gases was placed in the centre of a copper globe filled with rarefied air, and blackened within. The source of heat was a large vessel filled with water of 86°, into which the whole apparatus was plunged.

Of Delarive and Marcet.

Such are the principal sources from which our knowledge of the specific heat of bodies is derived. It has been ascertained that every body requires a certain quantity of heat to increase its temperature a certain number of degrees; and scarcely any two bodies agree with each other in the quantity requisite. The heat thus necessary is called the *specific heat* of the body. Dr. Black and his followers, Dr. Irvine and Mr. Watt, distinguished this property of bodies by the phrase *capacity of bodies for heat*. But the phrase *specific heat* has prevailed, being considered as a simple specification of the matter of fact.

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* Ann. de Chim. et de Phys. xxxv. 1.

Few bodies have a higher specific heat than *water*. Partly on this account, but chiefly on account of the facility with which it may be every where procured, it has been made choice of as a standard to which the specific heat of other bodies is referred. Its specific heat is considered as (1.) unity. If a substance has twice the specific heat of water, we say that its specific heat is 2; if thrice, that it is 3, and so on. If it has half or one-third the specific gravity of water, we say that its specific heat is 0·5, or 0·33, and so of the others.

Three different modes of determining the specific heats of bodies have been contrived.

Specific heat determined by mixture.

1. The first and oldest method is to mix determinate weights of the body whose specific heat we want, and of water at different temperatures together, to observe the new temperature produced by the mixture, and from this to deduce the specific heat of the body under trial. For example, if we take 1 pound of water of the temperature 100°, and 10 pounds of mercury of the temperature 200°, and agitate them together in a glass phial, after the agitation, we shall find that both liquids will have acquired a uniform temperature. The water will have become hotter, and the mercury colder. Abstracting for the present the effect of the phial, this new temperature will be nearly 125°. So that the water will have gained 25° of heat, while the mercury will have lost 75°. Thus the mercury has lost three times as many degrees of temperature as the water has gained. It is obvious, therefore, that the same quantity of heat produces three times as

much effect in raising the temperature of 10 lbs. of mercury, as in raising the temperature of 1 lb. of water. Therefore, if we take equal weights of these liquids, it is clear that the same quantity of heat will produce thirty times the effect on mercury as on water. Therefore, the specific heat of water is thirty times as great as that of mercury. If the specific heat of water be 1, that of mercury must be 0·033.

In making such experiments, it is always necessary to attend to the heat communicated or abstracted by the vessel in which the experiment is made. Suppose, for example, that the pound of water of 100° was standing in a glass phial weighing half a pound, and that we pour into it the ten pounds of mercury of the temperature 200°. It is clear that the surplus heat in the mercury would not be all expended in raising the temperature of the water. A portion of it would go to heat the phial, which would acquire as great an addition of temperature or nearly so as the water. If the specific heat of the phial were as great as that of the water, it is plain that one-third of the heat would go into the phial; so that the temperature after mixture, would only be 116°$\frac{2}{3}$, instead of 125°. But the specific heat of glass is only about $\frac{1}{6}$th of that of water. So that only $\frac{1}{6}$ of $\frac{1}{3}$ or $\frac{1}{18}$th of the surplus heat would be employed in heating the phial. The temperature after mixture instead of 125° would be only 123°$\frac{2}{3}$.

The easiest way of avoiding this error is to make two experiments. The first by pouring the hot

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mercury into the cold water, the second by pouring the cold water into the hot mercury; taking care that both trials are made in the same vessel. By taking the mean of the two experiments, we obtain very nearly the true temperature of the mixture.

It was in this way that the experiments of Irvine, Kirwan, Crawford, and Wilcke, were conducted. Dr. Irvine reduced the whole to the following mathematical formula.

Let the weight of the water be W, its temp. *w*.
———————— body be B, ——— *b*.
Let the temperature after mixture be *m*

$$\text{The specific heat of B} = \frac{W\,(m-w)}{B\,(b-m)}$$

In this formula the water is supposed to be coldest. If the water be hot and the body be cold, which in some cases may be the more convenient mode of making the experiment, the

$$\text{Specific heat of B} = \frac{W\,(w-m)}{B\,(m-b)}$$

This mode of experimenting, though theoretically easy, is in reality so difficult, that the results obtained by it are probably not very near approximations to the truth. The mode of allowing for the heat lost during the experiment, which has been uniformly employed, is certainly inaccurate.

By the calorimeter.

2. The second method of determining the specific heats of bodies was contrived by Lavoisier and Laplace, and employed by them in determining the specific heats of about eight different substances. This method appears abundantly simple; but it has

failed in the hands of every one who has attempted it since the publications of their experiments. An instrument was contrived, to which Lavoisier gave the name of *calorimeter*. It consists of three circular vessels nearly inscribed into each other, so as to form three different apartments, one within the other. These three we shall call the *interior*, *middle*, and *external cavities*. The interior cavity into which the substances submitted to experiment are put, is composed of a grating or cage of iron wire, supported by several iron bars. Its opening or mouth is covered by a lid, which is composed of the same materials. The middle cavity is filled with ice. This ice is supported by a grate, and under the grate is placed a sieve. The external cavity is also filled with ice. We have remarked already, that no caloric can pass through ice at 32°. It can enter ice, indeed, but it remains in it, and is employed in melting it. The quantity of ice melted, then, is a measure of the heat which has entered into the ice. The exterior and middle cavities being filled with ice, all the water is allowed to drain away, and the temperature of the interior cavity to come down to 32°. Then the substance, the specific heat of which is to be ascertained, is heated a certain number of degrees, suppose to 212°, and immediately put into the interior cavity enclosed in a thin vessel. As it cools, it melts the ice in the middle cavity. In proportion as it melts, the water runs through the grate and sieve, and falls through the conical funnel and the tube into a vessel placed below to receive it. The external cavity is filled with ice, in order to prevent the external air from approaching the ice in the middle cavity, and melting part of it. The water produced from it is carried off through a pipe. The external air ought never to be below 32°, nor above 41°. In the first case, the ice in the middle cavity might be cooled too low; in the last, a current of air passes through the machine, and carries off some of the caloric. By putting various substances at the same temperature into this machine, and observing how much ice each of them melted in cooling down to 32°, it was easy to ascertain the specific heat of each. Thus if water, in cooling from 212° to 32°, melted one pound of ice, and spermaceti oil 0·5 of a pound; the specific heat of water was one, and that of the oil 0·5.

By the time of cooling.

3. The third mode of determining the specific heats of bodies was suggested by Dr. Black many years ago; but seems to have been first employed by Professor Meyer of Erlangen in 1797, to determine the specific heat of woods. He took spherical pieces of wood of the same size, and dried to the same degree. Each of these was heated to the same point and suspended in a cold room. The time of cooling a certain number of degrees was noted, and the specific heats of the same bulk of woods were considered as proportional to these times. And when the times were divided by the specific gravities, the quotients represented the specific heats of equal weights. Professor Leslie extended this method to liquids as well as solids. It is not only easier than either of the other two;

but likewise susceptible of much greater exactness.

When we take the specific heat of solids in this way, nothing more is necessary than to have each body of the same size, to measure the time that each takes to lose a certain number of degrees of temperature, and to divide this time by the specific gravity of each body. The quotient will give the specific heat of equal weights of each body. But when we take the specific heat of liquids in this way, it is obvious that they must be placed in a vessel of some kind. It will be requisite, by a previous set of experiments, to ascertain the time that this vessel takes to cool the requisite number of degrees. Because this quantity must be subtracted from every result, in order to obtain the true time that the liquid took to cool. The best way is to employ the same glass vessel in every experiment. Let us suppose a glass vessel to take 5 minutes to cool from 130° to 80°. Suppose it filled with water of 130°, and when so filled to take 40 minutes to sink to 80°. When filled with sulphuric acid of 130°, let it take 25·8 minutes to sink from 130° to 80°. It is obvious that in order to get the true times which these liquids took to cool 50°, we must subtract from each result the 5 minutes occupied by the hot glass in losing 50° of heat. So that equal bulks of water and sulphuric acid lose 50°, the first in 35′, the second in 20′·8. The specific gravity of sulphuric acid being 1.847 we have $\frac{20·8}{1·847} = 11·26$. So that if the specific heat of water be 35, that of sulphuric acid will be 11.26. And 35 : 11·26 : : 1 : 0·32 = specific heat of sulphuric acid, if the specific heat of water be 1.

The following table exhibits the specific heats of all bodies hitherto determined, so far as my knowledge of the subject extends.

I. GASES.

Specific heat of gases.

From the experiments of Mr. Haycraft, and the still more numerous experiments of Messrs. Marcet and Delarive, it follows that under the *same* pressure, the *same* volumes of all gases have the *same* specific heat. Consequently the specific heats (reckoned by weight) of all the gases are inversely as their specific gravities.* Therefore, if we reckon the specific heat of oxygen gas 1, which is most convenient, the specific heats of the gases are as follows.†

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[*] Haycraft's experiments were made on *carbonic acid, oxygen, hydrogen, azote, coal gas,* and *air.* Those of Marcet and Delarive on *air, oxygen, azote, hydrogen, carbonic acid, olefiant gas, carbonic oxide, protoxide of azote, deutoxide of azote, sulphuretted hydrogen, ammonia, sulphurous acid, muriatic acid, cyanogen.*

[†] The results obtained by Delaroche and Berard, which are of too great value to be lost are as follows:

1. *Gases referred to air.*

	Specific heat. Same bulk.	Specific heat. Same weight.
Air	1·0000	1·0000
Hydrogen	0·9033	12·3401
Carbonic acid	1·2583	0·8280
Oxygen	0·9765	0·8848
Azote	1·0000	1·0318
Oxide of azote	1·3503	0·8878
Olefiant gas	1·5530	1·5768
Carbonic oxide	1·0340	1·0805

Gases.	Specific Heat.
Oxygen gas	1
Hydrogen	16
Carbon vapour	2·66
Carburetted hydrogen	2·00
Ammonia	1·8824
Steam	1·7778
Phosphorus vapour	1·3333
Phosphuretted hydrogen	1·2307
Hydrocyanic acid vapour	1·1852
Bihydroguretted phosphorus	1·1429
Azotic gas	1·1429
Olefiant gas	1·1429
Carbonic oxide	1·1429
Deutoxide of azote	1·0666
Sulphur vapour	1·
Sulphuretted hydrogen	0·94118
Muriatic acid	0·86486
Carbonic acid	0·72727
Protoxide of azote	0·72727
Cyanogen	0·61538
Tellurium vapour	0·5
Sulphurous acid	0·5
Telluretted hydrogen	0·48485

Gases referred to water.

	Sp. heat. Same weight.
Water	1·0000
Air	0·2669
Hydrogen	3.2936
Carbonic acid	0·2210
Oxygen	0·2361
Azote	0·2754
Oxide of azote	0·2369
Olefiant gas	0·4207
Carbonic oxide	0·2884
Aqueous vapour	0·8470

Gases.	Specific Heats.
Fluoboric acid . . .	0·47059
Chlorine	0·44444
Arsenic vapour . . .	0·42105
Sulphuret of carbon vapour .	0·42105
Arseniuretted hydrogen .	0·41026
Sulphuric acid vapour .	0·4
Selenium vapour . .	0·4
Selenietted hydrogen .	0·39024
Chlorocarbonic acid . .	0·32
Fluosilicic acid . . .	0·30769
Hydriodic acid . . .	0·256
Iodine vapour . . .	0·12903

The experiments of Marcet and Delarive were made under a pressure of 25·59 inches of mercury. It appears from their trials, that as the gas is dilated the specific heat undergoes a slow diminution; as will appear from the following experiments which they made on common air.*

Pressure in inches of mercury.		Centesimal.	
25·59	the volume of air heated	6°·33	in 5′
23·22	———	6·55	——
19·17	———	6·90	——
14·76	———	7·01	
10·16	———	7·30	

This is precisely what might have been expected; though the rate of diminution appears much slower than could have been anticipated. This part of the subject still requires farther elucidation.†

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* Ann. de Chim. et de Phys. xxxv. 28.

† The specific heat of the gases has occupied the attention of Dulong for a considerable time. He is of opinion that the methods

Specific heats of other bodies.

II. SIMPLE BODIES AND WATER.

	Sp. heat.
Water .	1·000
Ice . .	0·800(a)
Charcoal .	0·2631*
Sulphur .	0·183† 0·188(c) 0·19(D)
Mercury .	0·0290‡ 0·0330(c) 0·033† 0·0357* 0·0496(D)
Platinum .	0·0314(c)
Bismuth (sp. gr. 9·861)	0·0288(c) 0·04(D) 0·043§

	Sp. heat.
Lead (sp. gr. 11·456)	0·0293(c) 0·04(D) 0·042§ 0·0352* 0·050†
Gold (sp. gr. 19·040)	0·0298(c) 0·050† 0·05(D)
Antimony (sp. gr. 6·107)	0·06(D) 0·063§ 0·0645* 0·086†
Tin . .	0·0514(c) 0·060§ 0·07(D) 0·0704* 0·068†

hitherto employed are not sufficiently delicate to decide the point. He has attempted it in the following way. Laplace demonstrated that the true velocity of sound is obtained by multiplying the velocity, as deduced from Newton's formula, by the square root of the ratio of the specific heat of air under a constant pressure, to its specific heat under a constant volume. Dulong endeavoured to ascertain the real velocity of sound in each gas. This he compared conformably to the theory of Laplace, with that which the formula of Newton would indicate. The square of the quotient obtained by the true rate of sound in any gas by its velocity, calculated from Newton's formula, is equal to the ratio of its specific heat under a constant volume. Chladni had shown how by passing the different gases through a tube like a flute, the velocity of the motion of sound in each might be deduced from the nature of the tone given by each. Dulong employed a method suggested by Daniel Bernoulli; but his experiments gave a result with common air below the truth. However, the relative differences of the rate of sound in different gases, as determined by these experiments, are accurate. He found the velocity of sound, per second, in six different gases, at 32° Fahrenheit, as follows:

	Sp. heat.		Sp. heat.
Silver (sp. gr. 10·001)	0·0557(c) 0·08(D) 0·082§	Nickel .	0·10(D) 0·1035(c)
Zinc (sp. gr. 7·154)	0·0927(c) 0·0943* 0·10(D) 0·102§	Iron (sp. gr. 7·876)	0·1100(c) 0·126§ 0·125† 0·1269* 0·13(D) 0·143(a)
Tellurium .	0·0912(c)		

Air . .	1092·2 feet
Oxygen . .	1040·3
Hydrogen .	4163·9
Carbonic acid .	858·0
Carbonic oxide .	1106·7
Protoxide of azote .	859·0
Olefiant gas .	1029·9

The specific heats of these gases, deduced from these experiments (that of air being unity), are as follows:

	Supposing the volume constant.	Supposing the pressure constant.
Air . . .	1	1
Oxygen . .	1	1
Hydrogen . .	1	1
Carbonic acid .	1·249	1·175
Carbonic oxide .	1	1
Protoxide of azote	1·227	1·16
Olefiant gas .	1·754	1·531

These results agree better with the statements of Delaroche and Berard, than with those of Delarive and Marcet. But the question cannot yet be considered as answered. (See Ann. de Chim. et de Phys. xli. 113.) If any confidence can be put in the observations of Dulong and Petit, respecting the relation between the atomic weight and specific heat of bodies, noticed in page 97 of this work, then the opinion of Haycraft, and of Delarive and Marcet, must be admitted to be erroneous.

		Sp. heat.
Sheet iron .		0·1099‡
Gun metal .		0·1100‖
Copper (sp. gr. 8·784)		0·0949(c) 0·11(D) 0·114§ 0·1111*
Brass (sp. gr. 8·356)		0·11(D) 0·116§ 0·1123*
Cobalt .		0·1498(c)

III. SALINE SOLUTIONS.

		Sp. heat.
Carbonate of ammonia		0·851† 0·95(D)
Sulphuret of amm. (0·818)		0·994†
Sulphate of magnesia	1	0·844†
Water .	2	
Common salt	1	0·832†
Water .	8	
Ditto (1·197)		0·78(D)
Nitre	1	0·8167‡
Water	8	
Nitre	1	0·646†
Water	3	
Carbonate of potash	1·30	0·75(D)
Muriate of ammonia	1	0·798†
Water	1·5	
Tartar	1	0·734†
Water	237·3	
Sulphate of iron	1	0·765†
Water	2·5	
Sulphate of soda	1	0·728†
Water	2·9	
Alum	1	0·649†
Water	2·9	
Nitric acid	9⅓	0·6189‡
Lime	1	
Ditto (1·40)		0.62 (D)
Solution of br. sugar		0·086†
Ditto (1·17)		0·77 (D)

IV. ACIDS AND ALKALIES.

		Sp. heat.
Vinegar .		0.92(D)
Nitric acid.	pale	0·844†
	(1·20)	0·76(D)
	(1·2989)	0·6613‡ 0·62(L)
	1·30	0·66(D)
	(1·355)	0·576†
	(1·36)	0·63(D)
Muriatic	(1·122)	0·680†
	1·153	0·60(D)
Sulph.	1·844	0·35(D)
		0·3345‡
		0·333(a)
Do. 4. Water 5		0·6631‡
Do. 4. do. 3		0·6031‡
Do. equal bulks		0·52(D)
Acetic acid (1.056)		0·66(D)
Potash (1·346)		0·759†
Amm.	(0·997)	0·708†
	(0·948)	1·03(D)

V. INFLAMMABLE LIQUIDS.

		Sp. heat.
Alcohol	(0·817)	0.70(D)
		0·6666*
		0·64(L)‖
	(absolute)	0·62(d)
		0·602*
	(0·853)	0·58978‖
	(0·818)	0·54993‖
	(·848)	·76(D)
Sulph. ether	(0·76)	0·66(D) 0·520(d)
	(·729)	0.54329‖

	Sp. heat.
Oil of olives	0·718† 0·50(L) 0·43849‖
Linseed oil	0·528† 0·45192‖
Spermaceti oil	0·5000* 0·52(D)
Whale oil	0·5302
Oil of turpentine	0·472† 0·462(d) 0·400(a) 0·33856‖
Naphtha	0·41519‖
Spermaceti	0·399†
Ditto fluid	0·320(a)

VI. ANIMAL FLUIDS.

Arterial blood	1·0300* 0·913(b)
Venous blood	0·8928* 0·903(b)
Cow's milk	0·9999* 0·98(D)

VII. ANIMAL SOLIDS.

Ox hide with hair	0.7870*
Lungs of a sheep	0·7690*
Lean of ox-beef	0·7400*

VIII. VEGETABLE SOLIDS.

	Sp. heat.
Pinus sylvestris	0·65¶
Pinus abies	0·60¶
Tilea Europæa	0·62¶
Pinus picea	0·58¶
Pyrus malus	0·57¶
Betula alnus	0·53¶
Cotton	0·53
Quercus robur sessilis	0·51¶
Fraxinus excelsior	0·51¶
Pyrus communis	0·50¶
Rice	0·5060*
Horse beans	0·5020*
Dust of the pine tree	0·5000*
Peas	0·4920*
Fagus Sylvatica	0·49¶
Carpinus betulus	0·48¶
Betula alba	0·48¶
Wheat	0·4770*
Elm	0·47¶
Quercus robur pedunculata	0.45¶
Prunus domestica	0·44¶
Dyaspyrus ebenum	0·43¶
Barley	0·4210*
Oats	0·4160*
Pit-coal	0·28(D) 0·2777*
Charcoal	0·2631*
Cinders	0·1923*

IX. EARTHY BODIES, STONEWARE, AND GLASS.

Hydrate of lime	0·40(D)
Chalk	0·27(D) 0·2564*
Quicklime	0·30(D) 0·2229* 0·2168‡
Ashes of pit-coal	0·1855*
Ashes of elm	0·1402*
Agate (2·648)	0·195§
Stoneware	0·195†
Crown glass	0·200(a)
Crystal	0·1929‡
Swedish glass(2·386)	0·187§
Flint glass	0·19(D) 0·174†
Glass	0·1770(c)
Common salt	0·23(D)

X. OXIDES.

Oxide of iron	0·320†
Rust of iron	0·2500*

	Sp. heat.		Sp. heat.
Rust of iron nearly free from air	0.1666*	Oxide of lead and tin	0·102†
White oxide of antim. washed	0·220† 0·2272*	Oxide of zinc, do.	0·1369*
Do. nearly freed from air	0·1666*	Oxide of tin nearly freed from air	0·0990* 0·096†
Oxide of copper, do.	0·2272*	Yellow oxide of lead, do.	0·0680* 0·068†

We may now enumerate the most important general results respecting specific heats which have been hitherto obtained.

Gases, how compared with other bodies.

1. The specific heat of the same weight of water is 3·846 times greater than that of air, according to the experiments of Berard and Delaroche. Hence, to reduce the specific heat of the gases to a state in which they can be compared with that of water, we must divide the specific heat of each given in the table by 3·846.

Specific heat of water.

2. The specific heat of water has been determined in three states; that of ice, of water, and of steam, as follows:

Ice { 0·8 according to Irvine.
  0·9 ——— Crawford.
Water 1
Steam 0·847 by Delaroche and Berard.

No attempt has been made to determine the specific heat of any other body in its different states of solid, liquid, and vapour.

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* Crawford; † Kirwan; ‡ Lavoisier and Laplace; § Wilcke; ¶ Meyer; (L) Leslie; ‖ Count Rumford; (D) Dalton, New System of Chemical Philosophy, p. 62. (a) Irvine, Essays, p. 84 and 88. (b) John Davy, Phil. Trans. 1814, p. 593. (c) Dulong and Petit, Annals of Philosophy, xiii. 164, and xiv. 189. (d) Dupretz Ann. de Chim. et de Phys. xxiv. 328.

3. Of all liquids hitherto examined, the specific heat of water is greatest.

Of acids.

4. The specific heats of acid liquids are low. All the acid liquids whose specific heats have been hitherto tried, are in fact combinations of acids and water. We hardly know the specific heats of any acids except sulphuric acid, nitric acid, and muriatic acid; which are as follows:

	Sp. gravity.	Sp. heat.
Sulphuric acid .	1·844 .	0·35
Nitric acid . .	1·36 .	0·63
Muriatic acid .	1·153 .	0·60

Sulphuric acid of 1·844 is a compound of 5 real acid, and 1·125 water by weight.

Nitric acid of 1·36, contains equal weights of acid and water.

Muriatic acid of 1·153, is a compound of about 32 parts acid, and 68 parts water by weight.

The first is a compound of 1 atom acid + 1 atom water.
The second ———— 1 ———— + 6 atoms ——
The third ———— 1 ———— + 9 atoms ——

From this it would seem that muriatic acid has the smallest specific heat, and sulphuric acid the greatest. But when these acids are in the state of vapours, muriatic acid has the greatest, and sulphuric acid the highest specific heat. It is certain from experiments made on sulphuric acid, that as we dilute it with water, the specific heat of the liquid gradually increases. This will appear from the following table.

Atoms of acid.		Atoms of water.		Sp. Heat.
1	+	1	. .	0·333
1	+	4	. .	0·520
1	+	5	. .	0·603
1	+	8	. .	0·663

We see, however, that the rate diminishes rapidly.

Similar experiments have been made with nitric acid, the results of which may be seen in the following table.

Sp. gravity of acid.		Constituents by weight.		Sp. Heat.
1·36	.	1 acid + 1 water	.	0·63
1·30	.	1 + 1·5	.	0·66
1·20	.	1 + 2·5	.	0·76

5. Nothing is known respecting the specific heats of alkaline bodies, or at least nothing that can be depended on.

6. The specific heats of woods and oils approach to about half that of water. That of animal bodies, is about $\frac{3}{4}$ of that of water.

7. The specific heats of the metals are lower than of any other bodies. Platinum and mercury stand lowest, and iron is the metal which has the highest specific heat. But if we were to compare together equal volumes of bodies, the specific heat of the metals would of course be greatly increased. Iron would have nearly as high a specific heat as water; that of mercury would be less than 0·5.

Whether specific heat increases with the temperature.

8. A good many experiments have been made to determine whether the specific heat of bodies remains the same at all temperatures, or whether it increases as the temperature augments. This curious question first drew the attention of Dr. Irvine, and it was investigated with great care by

Dr. Crawford, who concluded, as Dr. Irvine had done before him, that as long as a body retains the same state, its specific heat continues unaltered. Mr. Dalton, on the contrary, was of opinion that the specific heat of every body increases with its temperature. But the first persons who established the fact by direct experiments were Dulong and Petit. Their method was to heat the body under examination to different temperatures, and then to plunge it into cold water. The change of temperature in the water enabled them to calculate the heat which the hot body had lost, and thence to determine its specific heat. They have not given us the details of their experiments; but they assure us that they were made with great care, and that the results are very near the truth. They found that the specific heat of every substance is greater at high temperatures than at low temperatures, and that it always increases with the temperatures. Thus the specific heat of iron, at different temperatures, they found as follows:

Centigrade.	Fahrenheit.	Sp. heat.
From 0° to 100°	32° to 212°	0·1098
0 to 200	32 to 392	0.1150
0 to 300	23 to 572	0·1218
0 to 350	32 to 662	0·1255

The following table exhibits the specific heats at different temperatures of the other bodies tried by them.

	Sp. heats between 0° and 100° cent.	Sp. heats between 0° and 300° cent.
Mercury	0·0330	0·0350
Zinc	0·0927	0·1015

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	Sp. heats between 0° and 100° cent.	Sp. heats between 0° and 300° cent.
Antimony	0·0507	0·0549
Silver	0·0557	0·0611
Copper	0·0949	0·1013
Platinum	0·0355	0·0355
Glass	0·1770	0·1900*

Sp. heat of compound liquids less than the mean.

9. When two liquids combine together, the specific heat of the resulting compound is *always* less than the mean of that of the two liquids which have entered into combination.

Suppose we mix together equal weights of the sulphuric acid of commerce and water. It is plain that we form a new liquid composed of the acid and water united together. If the specific heats remained unchanged, we should obtain that of the compound, by taking the mean of the specific heats of the constituents.

Sp. heat of acid 0·33
——— water 1·00
2 | 1·33
Mean . . 0·66

The mean specific heat is 0·66; but the specific heat of the compound is considerably less.

Though the weights of the two liquids united be unequal, this does not prevent us from determining the mean specific heat of the compound. In such a case, we must multiply the weight of each liquid by its specific heat, add the two products together, and divide the sum by the weight of the two liquids previously added together. The quotient will be the mean specific heat of the compound.

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* Annals of Philosophy, xiii. 167.

Thus a mixture of 1225 parts of sulphuric acid by weight, and 675 parts of water, has a specific heat of 0·52. Suppose we wish to determine its mean specific heat we proceed thus :

$$\begin{array}{lll} 1225 \times 0{\cdot}33 & = & 408 \\ \underline{675} \times 1 & = & \underline{675} \\ \overline{1900} & & 1900)\overline{1083}(\ 0{\cdot}57 = \text{mean specific} \end{array}$$

heat. But the real specific heat is only 0·52, or about $\frac{1}{10}$th less than the mean.

Why heat is evolved when liquids are mixed.

Dr. Irvine made use of this diminution of the specific heat, when two liquids combine to explain a phenomenon, which almost always occurs when different liquids are mixed together; namely, an increase in the temperature of the mixture. This increase is often small, as when water and strong spirits are mixed together. Boerhaave mixed together two ounces of water and two ounces of alcohol, each at the temperature of 44°, the temperature after mixture was 62°. But sometimes the increase of temperature is very considerable. Boyle observed, that when one part of sulphuric acid is mixed with three or four parts of water, a very great increase of temperature takes place; which, however, he did not know how to estimate. I find that when 4 parts of sulphuric acid, and 1 part of water, both at 50°, are mixed together, the temperature rises to about 300°. If we take equal volumes of sulphuric acid and water, the thermometer rises from 57° to 212°. In like manner when strong nitric acid and water are mixed together, a considerable increase of temperature takes place. Muriatic acid contains too much

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water to enable us to observe a similar phenomenon with it. But when a current of muriatic acid gas is made to pass through water, I have frequently observed the temperature rise so high, that I could not apply my hand to the glass vessel containing it, without a sensation of pain from the heat communicated.

Dr. Irvine first accounted for this evolution of heat, by ascribing it to the diminution of the specific heat. In consequence of this diminution, it is obvious that the quantity of heat contained in the liquid, will go farther in heating the mixture, than it did in heating the separate constituents. Consequently the temperature must rise, and the augmentation will be exactly proportional to the diminution of specific heat.

Irvine's mode of finding the real zero.

Dr. Irvine was not satisfied with this explanation, which if confined within proper limits, will be allowed to be satisfactory. He advanced a step farther, and endeavoured to deduce from the change in the specific heat, and the augmentation of temperature which takes place when liquids are mixed, the absolute quantity of heat which such liquids contained. Dr. Irvine's mode of reasoning will be easily understood, from the following example.

Let the specific heat of water = 1
———————— of sulphuric acid 0·333
———————— of a mixture of
equal volumes of acid and water = 0·52

When equal volumes of sulphuric acid and water are mixed together in a glass vessel, the thermometer rises from 57° to 212°, or there is an increase of temperature amounting to 155°.

According to Dr. Irvine, this evolution of heat is the consequence of the diminution of the specific heat of the bodies which have united together. It is that portion of the whole heat indicated by the diminution of the specific heat. The specific heat, had no diminution taken place, would have been 0·57. Being only 0·52, a diminution amounting to about $\frac{1}{10}$th has been experienced. Consequently 155° is $\frac{1}{10}$th of the whole heat contained in the bodies before mixture. Hence the real zero, or the place at which a thermometer would stand, could it be applied to a body destitute of all heat, is 1550° below 57, or 1493° below the commencement of Fahrenheit's scale.

Dr. Irvine, from data with which I am unacquainted, placed the real zero 900° below the commencement of Fahrenheit's scale. And his son informs us, that the same point very nearly was obtained by experiments made in a great variety of ways, and with very different liquids. Dr. Crawford, from experiments made by himself, and from a variety of well conducted experiments by Professor Gadolin, fixed the point of real zero at 1500 below 0°.

In the present state of our knowledge of heat, it is needless to discuss this subject. As the specific heats of bodies are not permanent, but increase with their temperature, it is clear that no conclusion can be drawn with respect to the real zero, from any change of specific heat produced by mixing liquids and the consequent increase of temperature; unless we know the rate at which the specific heat increases with the temperature. For any thing that we know to the contrary, the specific heat of water and sulphuric acid at the real zero may not be half what it is at 57°.

It is obvious that in all cases, when heat is evolved by mixing two liquids together there exists an affinity between them, and when mixed they enter into a chemical combination. Now it is very seldom that bodies unite chemically, without undergoing a change in their volume. Sometimes the bulk, after union, increases; but most commonly it diminishes. In the first case the atoms of which the bodies are composed, separate to a greater distance than before their union; in the second case they approach nearer each other. In the case of expansion we would expect a diminution of temperature: in the case of contraction an increase. There can scarcely be a doubt that these alterations in temperature take place in almost all chemical combinations, though other circumstances frequently accompany these combinations, which prevent us from perceiving the alteration of temperature produced by the combination. For example, when salts are dissolved in water, the density of the compound is almost always greater than the mean. Heat, therefore, should be evolved. But in most cases we are prevented from perceiving any augmentation of temperature by the liquefaction of the water of crystallization, which most salts contain. When the quantity of this water of crys-

Cold from solution of salts.

tallization is considerable, the heat necessary for its liquefaction is so great that a considerable diminution of temperature takes place. I select the following experiments which I made many years ago, to satisfy myself what change in the real density of the compound takes place even in those salts, which occasion sensible cold by their solution in water.

Salt in crystals.	Sp. gravity of salt.	Salt dissolved.	In grains of water.	Sp. gr. of solution	Mean gravity. *	Difference.
		grains.				
Carbonate of soda	1·624	300	1000	1·0880	1·0973	—0·0093
Sulphate of soda	1·3497	299	1000	1·0908	1·0634	+0·0274
Sulphate of iron	1·8	300	1000	1·1313	1·1142	+0·0171
Sulphate of magnesia	1·862	300	1000	1·1206	1·1197	+0·0009

In all these solutions, a contraction takes place, except in the case of carbonate of soda, where there is an expansion. When these salts are dissolved in water, cold is produced. But the diminution of temperature depends as much upon the rapidity of the solution as upon any thing else. But the

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* The method of calculating the mean specific gravity of two bodies, or the specific gravity of a compound, supposing that no change of volume takes place, and that we know the specific gravity of each of the constituents is as follows.

Let the two bodies uniting be A and B. Let $S =$ specific gravity of A, $W =$ its weight. $S' =$ specific gravity of B. $W' =$ its weight, then the mean specific gravity of the compound is

$$\frac{(W+W')\,S\,S'}{W\,S'+W'\,S}$$

greatest cold is produced by the solution of carbonate of soda, in which there is expansion instead of contraction. The following table shows the diminution of temperature which takes place, when 300 grains of each of the preceding salts in the state of powder, but retaining their water of crystallization or thrown into 1000 grains of water and stirred with a thermometer as long as the temperature continues to sink.

	Temperature of water before adding the salts.	Ditto after agitation.	Diminution of temperature.
Carbonate of soda	59°	43°	16°
Sulphate of soda	57·5	45·5	12
Sulphate of magnesia	56·5	51	5·5
Protosulphate of iron	58	53·5	4·5

Heat from anhydrous salts.

When these salts, in an anhydrous state, are thrown into water, the temperature rises. Thus, 300 grains of anhydrous carbonate of soda, thrown into 1000 grains of water, and stirred, elevated the temperature of the solution, from 57°·5 to 79°·5, or 22 degrees. The salt was nearly, though not completely, dissolved. The specific gravity of anhydrous carbonate of soda, is 2·640, and the specific gravity of a saturated solution at 80° of this salt, is 1·2291, and it is a compound of 1000 water + 292·3 salt, or at 80°, 100 water dissolved 29·23 of the anhydrous salt. In this case, there is a great diminution of volume for the specific gravity, supposing no alteration in volume, should be 1·1636. The contraction amounts to 0·0555. Three hundred grains of anhydrous sulphate of soda being thrown into 1000 grains of water at 61°·5, the temperature rose to 65°·5 or 4°. It continued long at that temperature. Much of the salt remained undissolved. The specific gravity of the solution was 1·1549. It consisted of 1000 grains of water holding in solution 164·84 grains of anhydrous salt. The mean specific gravity is 1·0934. The volume therefore has contracted.

M. Kupffer has recently determined experimentally the change of volume which takes place, where lead and tin, mercury and tin, and mercury and lead, are alloyed in various proportions. He found the specific gravity of the metals which he employed as follows:

Lead, ...	11·3308
Tin, ...	7·2911
Mercury, . . .	13·5886

at the temperature of 39°·38, the point of maximum density of water.

Density of alloys of tin and lead.

Alloy of		Specific gravity of the Alloy.	Mean specific gravity of the two metals.	Difference.
Tin.	Lead.			
1 atom,	1 atom.	9·4263	9·4366	— 0·0103
1	2	10·0782	10·0936	— 0·0154
1	3	10·3868	10·4122	— 0·0254
1	4	10·5551	10·6002	— 0·0431
2	1	8·7454	8·7518	— 0·0064
3	1	8·3914	8·3983	— 0·0069
4	1	8·1730	8·1826	— 0·0096
5	1	8·0279	8·0372	— 0·0093
6	1	7·9210	7·9326	— 0·0116

The specific gravity is always less than the mean. These metals, therefore, dilate when they combine.

The dilatation is least between the alloy of 2 atoms tin + 1 atom lead, and the alloy of 3 atoms tin + 1 atom lead. M. Kupffer found that an alloy of 2½ atoms tin + 1 atom lead, neither undergoes contraction nor dilatation, but is precisely the mean of that of the two metals alloyed.

The following table exhibits the specific gravity of the amalgams of tin:

Of amalgams of tin.

Amalgams of		Specific gravity of alloy.	Mean specific gr. of metals.	Difference.
Tin.	Mercury.			
1 atom,	1 atom.	11·3816	11·3480	+ 0·0336
2 atoms,	1	10·3447	10·2946	+ 0·0501
4	1	9·3185	9·2658	+ 0·0527
6	1	8·8218	8·7635	+ 0·0583

Here the specific gravity is always above the mean, so that the metals when they unite, diminish in volume, and this diminution increases as the quantity of tin increases.

The following table exhibits the specific gravity of various amalgams of lead.

Of amalgams of lead.

Volumes of			Specific gravity.	Mean gravity.	Difference.
Lead.		Mercury.			
1	+	4	13·1581	13·1116	+ 0·0465
1	+	3	13·0397	13·0003	+ 0·0394
1	+	2	12·8648	12·8147	+ 0·0501

Here also the volume diminishes when the two metals combine; and the alloy composed of 1 volume lead, and 3 volumes mercury, undergoes the least contraction.*

The alteration in volume which takes place when bodies unite together, and the quantity of heat evolved, or absorbed during the union, when studied with more accuracy than they have hitherto been,

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* Ann. de Chim. et de Phys. xl. 286.

will doubtless furnish most important data for investigating the nature of affinity, and for determining the difference in the intensity of this force in different combinations. I shall therefore give a few more examples.

(1.) The combinations of the metals with sulphur are all definite, and generally intimate. In all cases hitherto examined, these combinations are accompanied by a diminution in bulk, which in some cases is very considerable. The following table exhibits the composition and specific gravity of some of these sulphurets.

Metals.	Sp. gr.	Atomic Weights.
Iron	7·8	3·5
Zinc	6·861	4·25
Lead	11·357	13
Copper	8·9	4
Bismuth	9·833	9
Silver	10·474	13·75
Mercury	13·568	25
Sulphur	2·086	2

Of sulphurets.

Names of sulphurets.	Constituents.		Sp. gr.	Ditto by calculation.	Difference.
	Metal.	Sulphur.			
Protosulphuret of iron	1 atom	+ 1 atom	4·518	4·344	0·174
Bisulphuret of iron	1	+ 2	4·83	3·1695	1·6605
Sulphuret of zinc	1	+ 1	4·044	3·960	0·084
Sulphuret of lead	1	+ 1	7·602	7·115	0·487
Disulphuret of copper	2	+ 1	5·792	5·383	0·309
Sulphuret of bismuth	1	+ 1	7·591	6·647	0·943
Bisulphuret of mercury	1	+ 2	7·780	7·730	0·056
Sulphuret of silver	1	+ 1	7·2	6·9176	0·2824

The most remarkable of these compounds is the bisulphuret of iron, which is composed of 1 atom iron + 2 atoms sulphur. When iron combines with sulphur atom to atom, the specific gravity is considerably above the mean, being 4·518. Now when sulphur which has a specific gravity of 2·086, combines in a sulphuret having a sp. gravity more than double, one would expect that the specific gravity of the compound should diminish. It comes out by calculation only 3·4465: but is really 4·83; so that the specific gravity has increased almost $\frac{2}{5}$ths.

(2.) Oxygen being in a gaseous state and metals solid, we cannot so well determine the diminution of bulk which takes place when they combine. Yet as the oxygen is condensed into a solid in all of these compounds, we have it in our power at least to observe the comparative condensation, which may suggest some useful ideas. I shall therefore give the following examples of these combinations.

	Oxides.		Composition.
1	Protoxide of lead	13 lead	+ 1 oxygen
2	Red lead	13	+ 1·5
3	Peroxide of lead	13	+ 2
	Peroxide of tin	7·25 tin	+ 2
1	Red oxide of copper	8 copper	+ 1
2	Black oxide of copper	4	+ 1
	Oxide of bismuth	9 bismuth	+ 1
1	Black oxide of mercury	25 merc.	+ 1
2	Red oxide of mercury	25	+ 2
	Oxide of silver	13·75 silver	+ 1
	Chloride of silver	13·75	+ 4·5 chlorine
	Oxide of nickel	3·25 nickel	+ 1 oxygen.

If we reckon the specific gravity of water unity the specific gravity of oxygen gas and chlorine gas referred to water is

Oxygen	.	0·0013723
Chlorine	.	0·00308325

Of oxides.

OXIDES.	Sp. gravity.	Sp. gr. by calculation, supposing no condensation.	Density, supposing the calculated density to be 1.
Protox. of lead .	9·277	0·019136	48·469
Red lead . .	9·096	0·013068	69·605
Peroxide of lead .	8·902	0·010261	86·755
Peroxide of tin .	6·97	0·006342	109·90
Red oxide of copper	6·093	0·012308	49·619
Black oxide of do.	6·401	0·006857	93·477
Oxide of bismuth .	8·211	0·013674	60·049
Black oxide of mercury	10·69	0·035590	30·036
Red oxide of do.	11·085	0·018502	59·913
Oxide of silver .	7·143	0·020158	35·435
Chloride of do. .	5·129	0·012436	41·243
Peroxide of nickel .	4·846	0·005829	83·133

The last column of this table enables us to compare the increase of density which these combinations undergo with each other. We observe, contrary to what might have been expected, that the density increases with the quantity of oxygen added. This holds in the case of lead, copper, and mercury. It held also in the case of sulphur, in the former table. It would seem then to be a general law, that the diminution of volume increases in

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proportion to the quantity of the lightest substance added to the compound. Yet these additional doses of oxygen are more easily expelled by heat, than the portion which constitutes the protoxide.

The increase of density differs in every one of these examples. It is greatest in the peroxide of tin, and black oxide of copper, which are both difficult of decomposition by heat; though I have converted black oxide of copper into red oxide by an intense heat. The most easily decomposed oxide in the table, is the black oxide of mercury, and its increase of density is the least. Red oxide of mercury resists decomposition better, and its increase of density is greater. Oxide of silver is also easily decomposed by heat, and its increase of density is comparatively low.

We ought to be cautious in drawing conclusions from such tables, till the specific gravity of the metallic oxides be determined with more precision than has hitherto been done. I have observed that the specific gravity of black oxide of copper increases the oftener it has been exposed to a red heat. The same thing perhaps may hold with the other oxides, such of them at least as may be exposed to such a process.

Irvine's explanation of liquidity.

10. There is another general principle respecting the specific heat of bodies, which was investigated with much ingenuity by Dr. Irvine, and which deserves to be explained in consequence of the great importance attached to it, and the many ingenious theories founded upon it. According to Dr. Irvine, when a body changes its state its specific heat

changes at the same time, and it changes according to a law, which may be thus expressed—when a *solid* becomes a *liquid*, its specific heat *increases*, when a *liquid* becomes an *elastic fluid*, its specific heat increases. On the other hand, when an *elastic fluid* is converted into a *liquid*, its specific heat *diminishes*, and when a *liquid* is converted into a *solid*, its specific heat *diminishes*.

It is a curious circumstance, that though Dr. Irvine deduced from this law, an ingenious explanation of fluidity and vaporization, he never appears to have attempted to ascertain whether the law holds in other bodies besides water. The same remarks applies to his son, Dr. W. Irvine, and to Dr. Murray of Edinburgh, both of whom adopted the general law as a fundamental principle, and employed it to explain the nature of fluidity and vaporization. We have here an example of the great propensity of mankind to deduce general principles from a very limited set of facts; and of the zeal with which such general principles may be maintained, without any attempt to increase the number of facts on which these general principles are founded.

Supposing that Dr. Irvine's fact, that the specific heat of *water* is *greater* than that of *ice*, and that of *steam greater* than that of water, be correct; surely it is rather rash to infer that the same must hold good with every other body in nature; other substances of easy fusion might have been tried, such as tallow, wax, spermaceti, phosphorus, and sulphur; yet, strange as it may appear, we know nothing whatever respecting the specific heats of these bodies in a state of fusion.

According to Dr. Irvine, the specific heat of ice is 0·8; according to Dr. Crawford it is 0·9; that of water being 1·0. Dr. Crawford made the specific heat of steam 1·55.

No attempts have been made by any modern chemist to determine the specific heat of ice. But the specific heat of steam has been determined by Delaroche and Berard to be 0·847, that of water being 1. It is therefore less than that of water, instead of greater, as Dr. Irvine supposed. Thus it appears that Dr. Irvine's principle, so far from being general, does not hold even in the case of water, the only substance on which it was founded. We cannot therefore hesitate to reject the supposed general law altogether, as inconsistent with the phenomena of nature. The formulas therefore employed by Irvine and Crawford to determine the point of the real zero, by means of this general principle, require no discussion.

Sp. heat supposed to increase from oxygen.

11. Another general principle has been deduced from the experiments of Crawford, namely, that the specific heats of bodies are usually increased when they are united to oxygen. But the truth of this general principle is very doubtful.

Water, for example, is a compound of 8 parts by weight of oxygen and 1 of hydrogen. The mean specific heat of such a compound is 2·66, that of steam is only 1·78, and that of water 2·2, which is less than the mean.

In like manner the mean specific heats of carbonic oxide and carbonic acid, calculated from those of their constituents should be,

Carbonic oxide . .	1·714
Carbonic acid . .	1·454

Their true specific heats are much less, namely,

Carbonic oxide . .	1·143
Carbonic acid . .	0·727

The law, however, holds in certain metallic oxides according to the experiments of Dr. Crawford, as will appear from the following table.

	Specific heat.		
	By calculation.	By experiment.	Excess.
Peroxide of iron .	0·156 .	0.32 .	$\frac{1}{2}$
——— zinc .	0·12 .	0·137 .	$\frac{1}{8}$
——— copper .	0·17 .	0·227 .	$\frac{1}{4}$
Protoxide of lead .	0·044 .	0·068 .	$\frac{1}{3}$

But the great inequality in the excess renders the accuracy of these results more than doubtful.

The product of the atomic weight by the specific heat a constant quantity.

12. There is another very important point connected with specific heat, which was first noticed by Dulong and Petit. According to them if you multiply the atomic weight of a body by its specific heat, the product is a constant quantity. The following table will enable us to judge how far this observation is entitled to rank as a general law.

	Atomic Weight.	Specific Heat.	Product.
Mercury .	25 .	×0·03 .	= 0·75
Gold .	25 .	×0·03 .	= 0·75
Silver .	13·75 .	×0·0557 .	= 0·7658
Lead .	13 .	×0·0293 .	= 0·3809
Platinum .	12 .	×0·0314 .	= 0.3768

	Atomic Weight.	Specific Heat.	Product.
Bismuth	9	×0·0288	= 0·2593
Tin	7·25	×0·0514	= 0·3726
Antimony	5·5	×0·06	= 0·3300
Zinc	4·25	×0·0927	= 0·3939
Copper	4	×0·0949	= 0·3796
Iron	3·5	×0·11	= 0·3850
Sulphur	2	×0·19	= 0·3800

On inspecting this table it is obvious that the first three products are each twice as great as the rest. The mean product of the atomic weight into the specific heat (omitting bismuth because it deviates too far) is 0·376.

It is not improbable that I may have rated the atomic weights of mercury, gold, and silver, twice as high as they ought to be. On that supposition it would follow that the specific heat of every body is inversely as its atomic weight. This proposition is more likely to be true, because we know that it holds with respect to gaseous bodies. This law, if we suppose it to hold, would give us an easy method of determining the specific heats of all bodies. We have only to divide 0·376 by the atomic weight of the body, the quotient will be the specific heat required. The following table exhibits a few specific heats determined in this way.

Sp. heats of some simple bodies.

	Sp. Heats.
Carbon	0·5
Phosphorus	0·25
Sulphur	0·188
Selenium	0·75
Arsenic	0·079
Tellurium	0·094

	Sp. heat.
Iodine	0·0235
Potassium	0·075
Sodium	0·125
Lithium	0·5
Borium	0·043
Strontium	0·068
Calcium	0·15
Magnesium	0·25
Silicon	0·376

It would be easy to extend this table at pleasure. The probability is that it exhibits results exceedingly near the truth.

The following table, in which the specific heats of the bodies given in pages 97, 98, are rectified on the supposition that they are inversely as the atomic weights, show how very nearly they agree with the specific heats actually found by experiment.

	Specific Heats.	
	By calculation.	By experiment.
Mercury,	0·03	0·03
Gold,	0·03	0·03
Silver,	0·0557	0·0557
Lead,	0·0288	0·0293
Platinum,	0·0313	0·0314
Bismuth,	0·0417	0·0288
Tin,	0·0518	0·0514
Antimony,	0·0680	0·06
Zinc,	0·0884	0·0927
Copper,	0·0940	0·0949
Iron	0·1074	0·11
Sulphur,	0·1880	0·19

SECTION IV.

OF THE CELERITY OF THE COMMUNICATION OF HEAT.

Heat enters and leaves bodies with the same velocity.

1. It is generally admitted that heat is disposed to enter into, or to leave the different kinds of matter with the same degree of celerity. This is the foundation of the mode of determining the specific heat of bodies, by the time which they take in cooling. For it is obvious, that unless heat separate from every body, with the same celerity, we never can judge of the quantity of heat which each contains under the same temperature by this method. But if heat leave every body with the same celerity, then that body will be the longest in cooling which contains the most heat, and consequently has the greatest quantity to lose. In this way, the specific heats of various metals, woods, and liquids, have been determined, and the results agree with experiments made in a different way. So far as we know at present, there is no exception to this general law.

Conduction of heat through bodies.

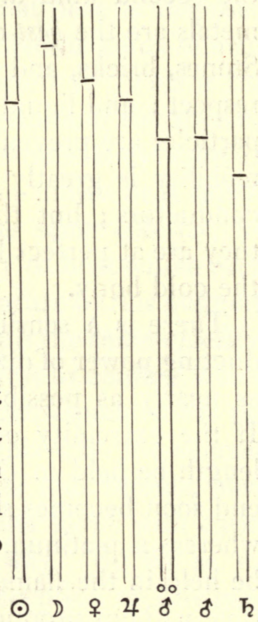
2. When a body of some length is applied to a source of heat by one extremity, while a thermometer is attached to its other extremity, we shall find that a certain portion of the heat will pass through the body to the thermometer, and cause it to rise. When heat passes in this way, it is said to be *conducted* through the body. Now, if we try various substances in this way, we shall find a great difference in the quantity of heat, which they allow to pass. Some under such circumstances speedily become hot, and transmit a great deal of heat, while

in others, the temperature rises comparatively less, and they transmit less heat to the thermometer. The first kind are said to be *good conductors* of heat; the second kind are called *bad conductors*. The metals are the *best* conductors among solid bodies. Stones, bricks, and earths, are much inferior in this respect; and liquids, and elastic fluids, when their particles are prevented from moving, or when their mobility is greatly retarded, are exceedingly *bad* conductors; but they are *good* conductors when they are at perfect liberty to move from the hot to the cold body.

There is a sensible difference between the conducting power of different metals, even when placed as nearly as possible in the same circumstances. If the extremity of a silver wire a few inches in length be held in the flame of a candle, the other end soon becomes so hot that it burns the fingers; whereas a platinum wire, of the same length, may be held in the flame of a candle for any length of time without producing that effect. A method of determining the relative conducting powers of the metals was contrived by Dr. Franklin, and executed by Dr. Ingenhousz, in the year 1780, which, though it does not give any very exact results, enables us at least to form some notions of the relative intensities of each. Wires of gold, silver, copper, tin, steel, iron, and lead, of exactly the same size and length, were fixed at equal distances in a piece of wood, through which they passed perpendicularly. The portion of the wires above the wood (which was the most considerable) was

Experiments of Ingenhousz.

covered with a coat of white wax by dipping them in melted wax. That part of the wires below the wood was now plunged into olive oil heated nearly to 212°. That wire was considered as the best conductor, which melted the wax highest up. Twelve experiments were made. The figure in the margin represents the wires above the wood in one of these experiments, and the cross lines show how high the wax was melted in each. In all the experiments the wax was melted highest up on the silver wire, next highest on the copper wire, and lowest of all on the leaden wire. In the others it varied. But gold and tin seem to stand next to copper, then comes iron, and then steel.



So that, according to these experiments, the conducting power of the seven metals tried is in the following order.*

Silver,
Copper,
Gold, }
Tin, }
Iron, }
Steel, }
Lead.

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* *Nouvelles Experiences,* par Dr. Ingenhousz, p. 380.

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Of Despretz.

But the most interesting set of experiments on this subject have been made by M. Despretz. He employed bars of the different substances of the same size, and covered with a coating of varnish. One of the ends of the bar was heated by a lamp, while a thermometer was applied to the other. The height to which the thermometer rose after the lamp had exhausted its effect, measured the conducting power of the bodies. The following table exhibits the conducting powers of the various substances tried by Despretz, according to the results of his experiments.*

	Conducting power.
Gold	100
Platinum	98·1
Silver	97·3
Copper	89·82
Iron	37·41
Zinc	36·37
Tin	30·38
Lead	17·96
Marble	2·34
Porcelain	1·22
Brick earth	1·13†

It is exceedingly probable that this difference in the rate in which heat passes through bodies is connected with the closeness of their texture, or at least with the contiguity of the particles of which they are composed. A set of experiments lately

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* Ann. de Chim. et de Phys. xix. 97.

† Traité Elementaire de Physique, par M. Despretz, p. 201.

Of Delarive and Decandolle.

made by MM. Aug. Delarive and Alph. de Candolle corroborates this view of the subject.* They took pieces of dry wood, about 5$\frac{1}{8}$ inches long, 1$\frac{1}{2}$ inch broad, and 1 inch thick. At about 1$\frac{1}{6}$ inch from the extremity of these pieces of wood, a hole was bored reaching to the centre of the piece, and rather more than $\frac{1}{4}$th inch in diameter. Five such holes were made in each piece at the distance of rather more than $\frac{3}{4}$ths of an inch from each other. Into each of these holes was put a little mercury into which the bulb of a thermometer was plunged. Over the mercury was strewed a little lycopodium powder, to prevent radiation of heat from the mercury. One of the extremities of the piece of wood was inserted in a case of tin plate, about $\frac{3}{4}$ths of an inch long, so as not to cover any of the holes. This apparatus was suspended freely in the air; and a spirit lamp was placed under the extremity covered by the tin plate. The flame was prevented from striking against any other part of the wood by the chimney of the lamp, and by pieces of glass placed vertically between it and the wood, which were carefully renewed whenever the heat began to pass through them. Care was taken to regulate the heat so as to prevent all risk of injuring the texture of the wood. Some of these pieces of wood were cut, so that the woody fibres extended longitudinally or in the direction of the wood. Others were in the contrary direction, so that the length of the piece was in the direction from the pith to the bark of the tree.

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* Ann. de Chim. et de Phys. xl. 91.

The heat was continued in each case for about a quarter of an hour after all the thermometers had ceased to ascend. The following table exhibits the rise of the several thermometers in these experiments above the temperature of the room in which the experiments were conducted.

I. The woody fibres being longitudinal, or in the direction by which the heat flowed.

Names of the woods.	Centigrade thermometer.				
	1st therm.	2d therm.	3d therm.	4th therm.	5th therm.
Cratægus aria	83°	45°	21 ·2	9°·2	4°·4
Nut wood	80·13	43	19·63	9·19	5·13
Oak	81·7	41·2	17·5	7·2	3·7
Fir	84	39·25	20·6	8·5	3·7
Poplar	79·8	34·2	14·2	6·2	2·8

II. The heat flowing in a direction opposite to that of the woody fibres or across them.

Names of the woods.	Centigrade thermometer.				
	1st therm.	2d therm.	3d therm.	4th therm.	5th therm.
Nut wood	99°·5	37°·43	13°·19	6°	3°·25
Oak	79·3	22·75	7·5	3·6	2·4
Fir	70·9	13·8	4·5	2·5	1·9
Cork	78·5	13·75	3·44	1·56	1

The following table exhibits the height to which the second thermometer would have risen, on the supposition that the first thermometer in every case rose 100° above the temperature of the atmosphere.

I. Woody fibres longitudinal.

Cratægus aria	54·28
Nut wood	53·7
Oak	50·5
Fir	47·62
Poplar	42·91

II. Heat flowing across the woody fibres.

Nut wood	37·59
Oak	28·57
Fir	19·6
Cork	17·5

This table would give the relative conducting powers of the different woods, if their specific gravity and power of radiating heat were the same in all. But this not being the case, it affords only an approximation.

In general, we see that the hardest woods conduct heat best. Though this is not accurately true, as nut wood conducts rather better than oak, though the latter is the hardest of the two.

We see how much better wood conducts heat in the direction of the woody fibres than across them; and this difference increases with the badness of the wood as a conductor. The conducting powers in the two directions may be represented very nearly by the following numbers.

	Longitudinally.	Across the fibres.
Nut wood	5	3·46
Oak	5	2·83
Fir	5	2·05

This inferiority in the conducting power of wood when the heat moves perpendicularly to the directions of the woody fibres, will enable us to explain the reason why the temperature in the interior of the trunk of a tree, is nearly that of the soil from which they draw their nourishment.

Of Dr. Trail.

The only other set of experiments on this subject with which I am acquainted, was made by Dr. Trail of Liverpool, on various liquids. The liquids under examination were put into a wooden cylinder, four inches long, and two inches in diameter. A delicate thermometer was fixed so that its bulb was in the axis of this cylinder, at some distance from its bottom. An iron cylinder was heated for 15 minutes, in boiling water, and then introduced through a hole in the lid of the wooden box, till its extremity was exactly half an inch from the bulb of the thermometer. The time which the thermometer took to rise three degrees was marked by a stop watch, when the cylinder contained different liquids. The following table exhibits the results obtained:

Liquids.	Time of heating 3°.	
Water,	7′	5″
Cow's milk,	8	25
Proof spirit,	8	
Alcohol (0·815),	10	45
Olive oil,	9	50
Mercury,	0	15
1 Sulphate of iron, 5 Water,	8	
Saturat. solution of alum	9	40
Ditto of sulphate of soda,	6	30
Solution of potash (Pharm. Lond.)	8	15*

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* Nicholson's Jour. xii. 133.

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Passage of heat through liquids depends on the expansion.

One would, at first sight, be disposed to conclude, that the conducting power of these liquids is inversely as the time necessary to cause the thermometer to rise three degrees. Were that supposition well founded, mercury would be the best, and alcohol the worst conductor of all the liquids tried. But if we consider the way in which the experiments were made, we must be sensible, that the rise of the thermometer was chiefly influenced by the change of density produced in the liquid, by augmenting its temperature. Those liquids, whose specific gravity diminishes most rapidly when heated, would allow the least heat to pass through them. Because the particles, as they receive heat, would ascend most rapidly to the surface. Accordingly, alcohol, which undergoes the greatest expansion from heat, was the liquid which allowed the heat to pass downwards most slowly. While mercury, whose density undergoes an insignificant augmentation from the addition of three degrees of heat, allowed the heat to pass through it much more rapidly than any of the other liquids. To make satisfactory experiments on the conducting powers of liquids, it would be necessary to guard against this mobility of their particles, which opposes such an obstacle to the downward motion of heat through them, that Count Rumford was of opinion, that it prevented it altogether, till the contrary was shown by the experiments of Dr. Murray and my own.

Passage of heat through gases.

Experiments have also been made by Dalton and Davy, upon the time that a thermometer takes to cool a certain number of degrees in different gases.

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The following table shows the results. The first column gives the time a thermometer took to cool 15° or 20° by Dalton's trials; the second column the time a thermometer took to sink for each gas, from 160° to 106°, in 21 cubic inches of each gas in Davy's trials.

Gases.	Time of cooling.	
Chlorine,		186″
Carbonic acid,	112″	165
Sulphuretted hydrogen,	100 +	——
Nitrous oxide,	100 +	150
Olefiant gas,	100 +	75
Common air,	100	129
Oxygen,	100	107
Azotic gas,	100	90
Nitrous gas,	90	—
Gas from pit coal,	70	55
Hydrogen gas,	40*	45†

These two columns do not agree very well with each other, owing probably to diversities in the mode of conducting the trials. But we see in general, that bodies cool slowest in the heaviest, and most rapidly in the lightest gases. It is obvious, that the rate of cooling depends chiefly upon the mobility of the gaseous particles. The process is different from the conduction of heat by solid bodies.

Rate at which bodies cool.

3. Heat is communicated from hot bodies to colder, with so much the greater celerity the greater the difference between the temperature of the two bodies is. Hence, when an iron ball is heated to redness and suspended in the air, it loses

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[*] Dalton's New System of Chem. i. 117.

[†] Phil. Trans. 1817, p. 60.

a very great deal of heat during the first few instants; but the quantity lost in an instant diminishes at a great rate as the ball approaches the temperature of the air in which it is suspended.

Hypothesis of Newton.

Sir Isaac Newton was of opinion, that the quantities of heat lost in given small times, are proportional to the excess of heat remaining in the hot body, or to the difference between its temperature and that of the air; and therefore, that the quantities of heat lost in equal divisions of time constitute a series of proportionals, or form a geometrical progression. From a set of experiments on the cooling of mercury and water, published in 1750 by Richmann,* it appears that the Newtonian law holds pretty nearly at as high temperatures as 120°. But beyond that point the verification does not seem to have been attempted.†

Experiments of Martine.

Dr. Martine, of St. Andrews, in an Essay on the Heating and Cooling of Bodies, published in 1739, showed that the Newtonian law is not strictly accurate even at pretty low temperatures; and that if it were so, bodies would take an infinite time to cool down to the temperature of the surrounding medium. He endeavoured, both from a number of experiments made by Muschenbroek, and by

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* Novi Commentarii Acad. Scien. Imper. Petropol. tom. iii. p. 308.

† Novi Comment. Acad. Scient. Imperalis Petropolitanæ, iii. 309. From Richmann's experiments in this paper on the rate of cooling of water, mercury, alcohol, naphtha, oil of turpentine, and linseed oil, he might have deduced the difference in the specific heats of bodies. But this idea does not seem to have occurred to him.

some of his own, to show that the decrements of heat were partly equable and partly in proportion to the subsisting heats. But the bodies experimented on were never raised to a higher temperature than 112°—a point too low to enable him to deduce the true law of cooling. Yet Dr. Martine had the merit of first pointing out the inaccuracy of the Newtonian law.

Of Erxleben.

Erxleben demonstrated by decisive experiments in a dissertation published in the eighth volume of the *Nova Commentaria* of the Gottingen Society, that the deviation of the Newtonian law from the truth increases rapidly as the temperature increases, and that we should fall into very great errors if we extended the law much beyond the temperatures at which it has been verified.

Of Delaroche.

M. De Laroche, of Geneva, in a paper on some properties of radiant heat, published in 1812,* pointed out the same deviation; though he does not seem to have been aware of the previous experiments of Erxleben. From his experiments it would appear, that the Newtonian law holds tolerably nearly at temperatures below 212°, but the error increases as the temperature augments, and at last becomes very great.

Of Dulong and Petit.

The prize dissertation of Dulong and Petit on the measure of temperature, and the laws of the communication of heat, published in 1818, contains an elaborate investigation of the law of cooling. From their experiments, it is sufficiently obvious, that at temperatures above 212°, the rate of cooling

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* Annals of Philosophy, ii. 100.

deviates enormously from the Newtonian law. The exhibition of a single set of their experiments will be sufficient to demonstrate this. The following table exhibits the rate of cooling of a mercurial thermometer in a vacuum. The first column represents the temperature according to the centigrade scale; the second column gives the degrees of heat lost per minute at the corresponding temperatures, supposing the rate of cooling for a minute to be equable.

240°	10.69°
220	8·81
200	7·40
180	6·10
160	4·89
140	3·88
120	3·02
100	2·30
80	1·74

Were the Newtonian law accurate, the velocity of cooling at 200° ought to be twice that of 100°. We see from the table that it is more than thrice. The velocity of cooling at 240° ought to be thrice that at 80°. We see from the table that it is more than six times as great.*

I shall now endeavour to state the principal facts respecting the cooling of bodies. For our knowledge of them we are almost entirely indebted to the dissertation of Dulong and Petit, which has been just referred to.

1. When a body cools in a vacuum, the heat

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* Annals of Philosophy, xiii. 242.

which it loses is owing entirely to radiation. When it cools in any kind of air, the process goes on more rapidly, because the quantity of heat radiated is the same as in vacuo, while an additional quantity of heat is conducted away by the air or gas.

(2.) The rate of cooling of a liquid confined in a vessel, is not altered by the size nor by the shape of that vessel.

Law of cooling.

(3.) The velocity of cooling of a thermometer in vacuo, for a constant excess of temperature, increases in a geometrical progression, when the temperature of the surrounding medium increases in an arithmetical progression. The ratio of this geometrical progression is the same, whatever be the excess of temperature considered. The truth of this law will be evident from the following table.

Excess of temp. of the therm. centigr.	Velocity of cooling Water at 0°.	Ditto Water at 20°.	Ditto Water at 40°	Ditto Water at 60°.	Ditto Water at 80°.
240°	10°·69	12·40	14·35	—	—
220	8·81	10·41	11·98	—	—
200	7.40	8·58	10·01	11·64	13·45
180	6·10	7·04	8·20	9·55	11·05
160	4·89	5·67	6·61	7·68	8·95
140	3.88	4·57	5·32	6·14	7·19
120	3·02	3·56	4·15	4·84	5·64
100	2·30	2·74	3·16	3·68	4·29
80	1·74	1·99	2·30	2·73	3·18
60		1·40	1·62	1·88	2·17

If we compare the five last columns of this table with each other, we shall find that the mean ratio is 1·161.

In general, this geometrical progression requires to be diminished by a constant quantity, in consequence of the heat radiated back to the hot body from the walls of the vessel in which it is cooling.

(4.) When a body cools in vacuo, the time of cooling is materially influenced by the nature of the surface of the hot body; those bodies cooling soonest which radiate best. But this difference does not affect the law of cooling in vacuo.

(5.) When a body cools in any gas, the same portion of its heat is carried off by radiation that would be dissipated if it were cooling in a vacuum. Another portion is conducted off by the gas. This last portion is not affected by the nature of the surface of the hot body. It depends upon the conducting power of the different gases.

Communication of heat depends on the extent of surface touching.

4. The celerity with which heat is communicated from hotter bodies to colder ones, when all other things are equal, is proportional to the extent of contact and closeness of communication between the bodies.

Every person is disposed to admit this law from general experience. It is obvious enough, that if we have two cubes and two spheres of iron, if we heat one of each sort and lay it on the other, the heat would be communicated much faster from the hot cube to the cold one, than from the hot sphere to the cold one.

When the masses which we compare are of the same shape, but differ only in size, the smaller will cool in a shorter time than the larger. Suppose two cubes of gold, the one 1 inch in diameter, and the other two. The smaller cube will cool sooner than the larger if both be placed in the same circumstances; because the surface of the larger cube is only four times greater than that of the smaller, while the quantity of matter in it is eight times greater.

This general principle enables us to see why heat passes very slowly through those bodies which have a rough and spongy texture. Wood transmits heat more slowly than metal on this account. Cork still more slowly than wood. Wool, feathers, or furs, exceed even cork in the slowness with which heat passes through them. It is indeed true that the interstices of these bodies are filled with air—but as this fluid is in a state of stagnation, and the particles at some distance from each other, it gives little assistance, or rather offers a strong resistance to the transmission of heat. This is the reason why such materials are so effectual in keeping our bodies warm in cold weather. They may be equally employed to prevent heat from entering into bodies. If we wish to prevent a lump of ice from melting in a warm place, we cannot do better than wrap it in plenty of flannel or furs, or the like materials, which produce the wished-for effect, by retarding the communication of heat to the ice from the air and the other bodies in the neighbourhood.

Fluids transmit heat more rapidly than solids.

This retardation of the communication of heat does not take place in fluids equally as solids. Fluids receive heat, and transmit it generally more rapidly than solids. Of this we have an obvious example in the air; which though very rare and light, cools bodies exposed to it very fast. When a hot body is plunged into water, it cools much

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sooner than it would do if plunged into a mass of sand or iron filings. This difference is owing to the easy mobility of the particles of fluids. When any portion of a fluid receives heat, it becomes specifically lighter, and of course ascends; another portion of cold fluid supplies its place, is heated in its turn, becomes dilated and ascends; and this renewal of cold particles will continue till the temperature of the hot body is sufficiently reduced.

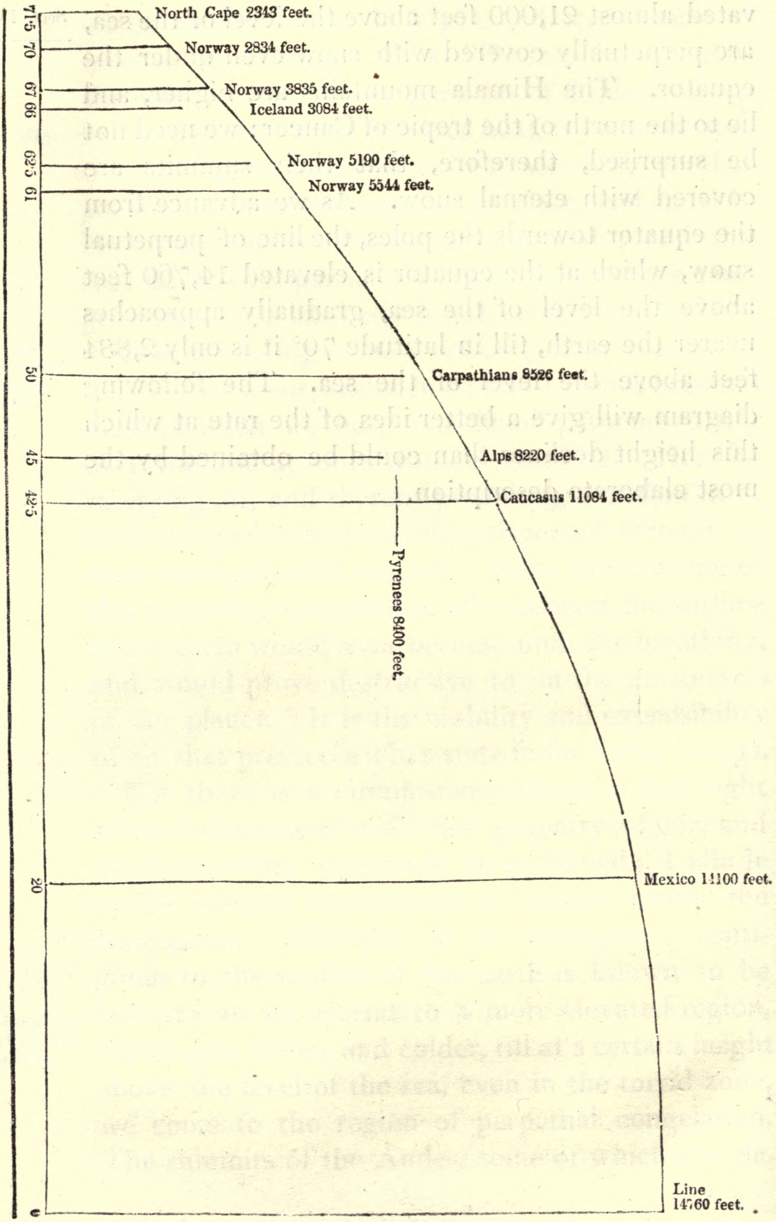
And why.

Air becomes colder as we ascend.

This ascending motion of *air*, when a lump of red hot iron is suspended in a room to cool, becomes evident when we hold our hand perpendicularly over the iron at some distance above it. Heat by rarefying air, and thereby producing a motion in it, is more quickly and equably dispersed through it, than through solid matter. Were air destitute of this property, that portion of it nearest the surface of the earth would soon become unfit for breathing, and would prove destructive to all the inhabitants of our planet. It is the mobility and expansibility of air that preserves it in a state fit for living beings.

Line of perpetual congelation.

But there is a circumstance which at first sight seems inconsistent with this property of air, and which therefore requires to be explained; I allude to the unequal distributions of temperature in the atmosphere. The part of the atmosphere contiguous to the surface of the earth is known to be hottest; as we ascend to a more elevated region, the air gets colder and colder, till at a certain height above the level of the sea, even in the torrid zone, we come to the region of perpetual congelation. The summits of the Andes, some of which are elevated almost 21,000 feet above the level of the sea, are perpetually covered with snow even under the equator. The Himala mountains are higher, and lie to the north of the tropic of Cancer: we need not be surprised, therefore, that their summits are covered with eternal snow. As we advance from the equator towards the poles, the line of perpetual snow, which at the equator is elevated 14,760 feet above the level of the sea, gradually approaches nearer the earth, till in latitude 70° it is only 2,834 feet above the level of the sea. The following diagram will give a better idea of the rate at which this height declines than could be obtained by the most elaborate description.


71.5
70
67
66
62.5
61
50
45
42.5
20
0
North Cape 2343 feet.
Norway 2834 feet.
Norway 3835 feet.
Iceland 3084 feet.
Norway 5190 feet.
Norway 5544 feet.
Carpathians 8526 feet.
Alps 8220 feet.
Caucasus 11084 feet.
Pyrenees 8400 feet.
Mexico 14100 feet.
Line
14760 feet.

The surface of the earth, instead of a quadrant of a circle, which is nearly its true form, is represented as a straight line on which the degrees of latitude are marked, beginning at the line and proceeding to N. latitude 71°·5 on the North Cape. The perpendicular height above the surface of the earth representing the limit of perpetual congelation is exaggerated about 1000 times.

From inspecting this diagram, it will be evident that the snow line though connected with the latitude, is not absolutely regulated by it. Thus in the Caucasus, in about latitude 42°½ N. the snow line is situated about 11084 feet above the level of the sea, while in the Pyrenees under the same latitude, it is only 8400 feet. In the Alps, it is 8220 feet above the level of the sea, while in the Carpathian, though situated a little farther north, it is 8526 feet or 326 feet higher. In Norway, in latitude 67°, it is 3835 feet above the level of the sea, while in Iceland, in latitude 66°, it is only 3084.

These apparent anomalies are easily accounted for. Wherever an extensive table land occurs, its effect becomes manifest upon the atmosphere, and elevates the line of congelation considerably. Thus Mexico constitutes a table land, elevated about 8000 feet above the level of the sea. The consequence is that the snow line is only 660 feet lower than under the equator. This is the reason why the snow line is so high in the Himalaya mountains.

Wherever extensive glaciers are formed, they have a tendency to sink the temperature of the

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country, and by gradually creeping lower down, they depress the line of congelation in that place. Hence the reason, why the line of congelation is lower in the Alps than the Carpathians. In the Doffrines, there are glaciers in one place only. Hence the reason why the line of congelation is higher in Norway than in Iceland. Various methods of calculating the height of the line of congelation have been proposed by ingenious philosophers of our own time: but it is obvious that no general rule can apply to all cases.

Because its specific heat increases with its volume.

Now if air, the moment it is expanded by heat, begins to ascend to a higher part of the atmosphere, and if it continue to rise till it come to air of the same density with itself, how are we to account for this regular diminution of heat, as we ascend? It would appear at first sight, that the higher parts of the atmosphere ought to be as hot, if not hotter, than those portions near the surface of the earth. The reason why this is not the case, is that the *specific heat* of air increases in proportion as its volume increases. But it is well known, that the higher from the surface of the earth any portion of the air is, the greater is its volume; for the volume of air is always inversely as the pressure upon it. At the surface of the sea, the incumbent air is pressed upon by the whole atmosphere above it. As we ascend, the air is pressed upon by all the portion of atmosphere above it, while the portion below ceases to have any action on it. The density of air continually decreases as we ascend in the atmosphere, and of course its bulk continually augments. The rate at which this augmentation of volume takes place will be obvious from the following table.*

Air.

Height in miles above the sea.	Density.	Volume.	Height of Barometer.
0	1	1	30 inches.
2·705	$\frac{1}{2}$	2	15
5·41	$\frac{1}{4}$	4	7·5
8·115	$\frac{1}{8}$	8	3·75
10·82	$\frac{1}{16}$	16	1·875
13.525	$\frac{1}{32}$	32	0·9375
16·23	$\frac{1}{64}$	64	0·46875

We see that when the height increases in an arithmetical ratio, the volume increases in a geometrical ratio.

Now it has been experimentally ascertained, that as the volume of air increases, its specific heat augments in the same proportion. If a receiver standing over the plate of an air-pump, and having a thermometer suspended in it, be exhausted, the thermometer suddenly sinks a few degress, and then gradually rises again, till it acquires the temperature of the room in which the air-pump stands. When the air is again let into the receiver, the thermometer as suddenly rises a few degrees, and then subsides again. Mr. Dalton concluded from his observations, that the true elevation of the thermometer, when air is admitted into an exhaust-

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* When the barometer (at 32°) falls 0·001 inch, it indicates an elevation of 0·954 feet; so that the elevation of 1 foot causes the barometer to fall (at 32°) 0·00105 inch, according to the experiment of General Roy.

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ed receiver, is 50°. But as he does not inform us how far the exhaustion was carried in his experiments, we can draw no conclusions from his ingenious deductions. Professor Leslie, of Edinburgh, made a very ingenious set of experiments on this subject, which it will be proper to state.

Heat evolved when air is condensed.

He employed an excellent air pump, and a glass receiver of the very largest dimensions, and approaching somewhat to the spherical form. A very delicate thermometer was suspended vertically a few inches above the plate of the air-pump. One-fifth of the air of the receiver was exhausted, and the apparatus left at liberty, till the enclosed thermometer had acquired the temperature of the room. The stop-cock was then suddenly opened, and the air allowed to return into the receiver, the thermometer rose very rapidly three centesimal degrees, from which point it afterwards slowly descended. Two-fifths of the air were then exhausted, and the experiment repeated. In like manner the rise of the thermometer was observed when three-fifths of the air, four-fifths, and $\frac{299}{300}$dths were exhausted and allowed to return suddenly into the receiver. The following table shows the results.

Air withdrawn and then let in.	Rise of the thermometer.
$\frac{1}{5}$	3° centigrade
$\frac{2}{5}$	5·3
$\frac{3}{5}$	7
$\frac{4}{5}$	8
$\frac{299}{300}$	8·3

Before any conclusion could be drawn from these experiments, it was necessary to know what part of the heat was consumed on the sides of the receiver. To be able to form some idea of this, the experiments were repeated with another receiver of the same shape as the former, but of only half the size. The following table exhibits the results.

Rarefaction.	Rise of therm.
$\frac{1}{5}$	1°·8 centigrade
$\frac{2}{5}$	3·2
$\frac{3}{5}$	4·2
$\frac{4}{5}$	4·8
$\frac{299}{300}$	5·

These new quantities obviously follow the same progression as the former. They are in fact each $\frac{3}{5}$ths of the former numbers. Now the smaller receiver having under the fourth part of the surface of the larger only the eighth part of its contents, exposes comparatively twice the extent of surface. Hence the rise of temperature which takes place in its included air must be the same as would have happened, if, while its capacity remained the same, the surface of the first receiver had been exactly doubled.

Mr. Leslie very ingeniously supposes that the air holds one part of the heat, while two parts and four parts are respectively expended on the inside of the receivers. If we admit this conjecture, the results correspond accurately with observation. For the air in the large receiver would retain one-third, and in the small one-fifth of the whole heat evolved, and the rise of temperature in the two sets of experiments would be as *5* to *3*, as was actually the case.

If we multiply the rise of the first thermometer by 3, we obtain the true quantity of heat evolved, as in the following table.

Rarefaction.	
$\frac{1}{5}$	9° centigrade
$\frac{2}{5}$	16
$\frac{3}{5}$	21
$\frac{4}{5}$	24
$\frac{299}{300}$	25

But these numbers cannot indicate the heat evolved by restoring the usual density to the whole air in the receiver. In the first experiment, only $\frac{4}{5}$ths of the air in the receiver were restored to the usual density. For the $\frac{1}{5}$th admitted had already the density of the external air. In the second experiments, $\frac{3}{5}$ths of the air were restored to their usual density; in the third $\frac{2}{5}$ths, in the fourth $\frac{1}{5}$th, and in the fifth $\frac{1}{300}$dth. To obtain the true numbers, we must add $\frac{1}{5}$, $\frac{2}{5}$, $\frac{3}{5}$, $\frac{4}{5}$, and $\frac{299}{300}$ successively to each number. We obtain the true heat evolved as follows:

$\frac{1}{5}$	11°·25 centigrade
$\frac{2}{5}$	26·6
$\frac{3}{5}$	52·5
$\frac{4}{5}$	120
$\frac{299}{300}$	7500

Thus according to the experiments and hypothesis of Leslie, when air of the following densities is restored to its original density represented by 1, the heat evolved is as follows:

Density of the air.	Heat evolved.
1·	0° Fahrenheit.
0·8	20·625
0·6	48·
0.4	94·5
0·2	216
0.0003	13500

Now, this evolution of heat is obviously owing entirely to a diminution of the specific heat of the air by condensation. It is obvious, that there must be a corresponding increase of specific heat from rarefaction.

Let θ denote the density of the air, then $45\left(\frac{1}{\theta}-\theta\right)$ will denote the heat evolved, when the density is reduced to unity;* or if the air be condensed,

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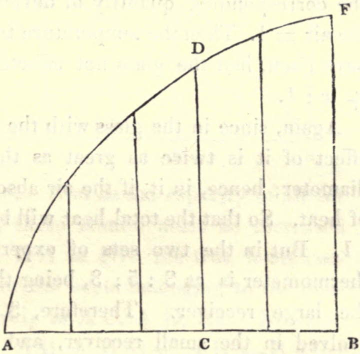
* It will be proper to explain how this formula was obtained by Mr. Leslie.



Let A F be a portion of a parabola, having B F for its axis, and A B for an ordinate; and let A B = B F. Divide the ordinate A B into five equal parts, representing the five equal divisions of the mercurial column corresponding to the different densities of the air represented in the first column of the table given in page 122. From each of these divisions, raise perpendiculars C D, parallel to B F, and meeting the curve. Then, if B C be the density of the remaining air, the rise of temperature is exhibited by C D. For, from the nature of the curve, (2 A B—A C) A C = C D × P, (the parameter, which is equal to B F). But A B was made = 5, then 45 $\left(\theta - \frac{1}{\theta}\right)$ will denote the cold evolved, when the air is rarefied to 1. Let it be required to determine by this formula, how much heat is evolved

and B F $=$ 5°, the ultimate rise of the thermometer. Whence C D $= \frac{(10 - A\,C)\,A\,C}{5}$. When the density is $\frac{2}{3}$, A C $=$ 3, and C D becomes 4·2, as in the table (page 122). The conformity of the numbers in the table, with the abscisses of the parabola, is therefore evident.

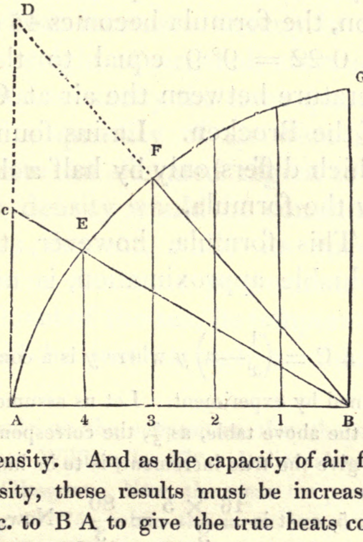
It was remarked in the text, that the two tables exhibited in pages 122 and 123 are to each other as 5 to 3. In these two sets of experiments, the different sizes of the receivers only affect the experimental results by the different ratios between their surfaces and contents; since, in other respects, as the proportion is the same, the temperature must be unaffected, the quantity of air and the heat developed, being always in the same proportion. Let then $x =$ number of additional degrees, to which the heat absorbed by the large glass would raise the temperature of the air, supposing the corresponding quantity of developed heat actually received by the air $=$ 1. Then the temperature to which the included air would have risen, had the glass not affected it, is to the actual rise as $1 + x : 1$.

Again, since in the glass with the smallest diameter, the relative effect of it is twice as great as that of the glass with the large diameter; hence, in it, if the air absorb 1, the glass will absorb $2\,x$ of heat. So that the total heat will be to the actual rise, as $1 + 2\,x : 1$. But in the two sets of experiments, the actual rise of the thermometer is as $3 : 5$; 3, being the rise in the smaller, and 5, in the large receiver. Therefore, $3\,(1 + 2\,x) =$ heat actually evolved in the small receiver, and $5\,(1 + x) =$ heat actually evolved in the large; supposing the receivers not to have absorbed any. But the circumstances of the two experiments being absolutely the same, these two quantities must be equal. We have therefore $3\,(1 + 2\,x) = 5\,(1 + x)$; or $x = 2$. Hence the whole heat evolved in the small receiver, was five times the rise of when the density of air is doubled. Here $\theta = 2$. The formula becomes $45\,(2 - \frac{1}{2})$ or $45 \times 1{\cdot}5 = 67^\circ.5$ Fahrenheit. Required the heat evolved, when air

the thermometer; and in the large, thrice that of the rise of the thermometer; or the true rise is indicated by the following table.

$\frac{4}{5}$	9° centigrade.
$\frac{3}{5}$	16
$\frac{2}{5}$	21
$\frac{1}{5}$	24
Extreme . .	25

Let A G be part of a parabola as before, G B the absciss, and A B the ordinate. Divide A B into any number of equal parts, then if A B be the density of the air, and B 4, B 3, &c. any other densities, then E 4, F 3, will represent the corresponding thermometrical rises as formerly explained. But it must be observed, that these rises are the heats actually evolved, only when the air is suffered to regain its usual density. And as the capacity of air for heat is inversely as its density, these results must be increased in the ratio of B 4, B 3, &c. to B A to give the true heats corresponding to these densities; or the heats belonging to the densities B 4, B 3, &c. are respectively as A C, A D, &c. Now, by the nature of the parabola, $A\,4\,(A\,B + B\,4)$ or $A\,B^2 - B\,4^2 = P + E\,4$: or $E\,4 \propto$ (*i. e.* varies as) $A\,B^2 - B\,4^2$. But $B\,4 : B\,A :: E\,4 : A\,C$; or $A\,C \propto \frac{E\,4}{B\,4}$; so, $A\,C \propto \frac{A\,B^2 - B\,4^2}{B4}$. Now, if $A\,B = 1$, and $B\,4 = x$, then $A\,C \propto \frac{1 - x^2}{x} \propto \left(\frac{1}{x} - x\right)$,



is condensed 30 times. Here $\theta = 30$, we have 45 $(30 - \frac{1}{30}) = 45 \times 29{\cdot}966 = 1348^\circ{\cdot}5$.

Let us apply this formula to the diminution of heat as we ascend in the atmosphere. In an observation by Lasius, the barometer at Goslar stood at 29·5 inches, and at the top of the Brocken, at 26·444 inches. Required from this the difference of temperature. If we reckon the density of air at Goslar 1, to find its density at the top of the Brocken, we say $29.5 : 26.444 :: 1 : 0{\cdot}896 =$ density of air at the top of the Brocken. By substitution, the formula becomes $45\,(1.116 - 0{\cdot}896) = 45 \times 0{\cdot}22 = 9^\circ{\cdot}9$ equal to the difference of temperature between the air at Goslar and at the top of the Brocken. Lasius found the difference 9°·4, which differs only by half a degree from that given by the formula.

This formula, however, though it furnishes a valuable approximation, is not quite correct, for if

or $A\,C = \left(\frac{1}{x} - x\right) y$ where y is a constant co-efficient to be determined by experiment. Let us assume any of the densities given in the above table, as $\frac{3}{5}$, the corresponding increment is 16, which to give the true increment, is to be increased in the ratio of $\frac{5}{3} : 1$ or $3 : 5$, or it is $\frac{16 \times 5}{3} = \frac{80}{3}$. Now, in the formula, x is $\frac{3}{5}$, whence it becomes $\left(\frac{5}{3} - \frac{3}{5}\right) y = \frac{80}{3}$ or $\left(\frac{25-9}{5}\right) y = 80$. Hence, $y = 25$. Wherefore, the decrement of temperature, at any density, x is $25^\circ \left(\frac{1}{x} - x\right)$, the density of common air being 1. In the text, I have substituted 45 for 25, in order to adapt the formula to Fahrenheit's scale.

we calculate from it what the mean temperature ought to be on the summit of Ben Nevis, we obtain about 32°.* Yet we know that the summit of that mountain is below the level of perpetual congelation, which in Scotland is elevated to the height of about 6500 feet, exceeding that of any mountain in Great Britain.

The same weight of air at all heights contains the same heat.

It is obvious from the details here entered into, that the temperature of the air must diminish in proportion as its height increases above the level of the sea; because its density diminishes, and consequently its specific heat increases exactly in that ratio. But the same weight of air, at all elevations, above any place, contains exactly the same quantity of heat. So that if a quantity of air were suddenly transported from an elevated region to the level of the sea, its density would be continually increasing during its descent, while its specific heat would diminish in the same proportion, and when it reached the level of the sea, its temperature in consequence would be just as high as that of

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* This calculation is made on the supposition that the mean annual temperature at Fort William is 47°. It lies about 53 minutes farther north than Glasgow. Now, the mean temperature at Glasgow is very nearly 47°·75; while the mean temperature of London is nearly 50°. The difference between these two temperatures 2°$\frac{1}{4}$, while the difference between the latitudes is very nearly 4 degrees. From this, it would appear, that in Great Britain, every additional degree of latitude diminishes the temperature about 0.55 of a degree of Fahrenheit. It is obvious from this, that the mean heat at Fort William is not under 47°. It should be 47°.3. Which would make the mean heat at the summit of Ben Nevis 32°·4.

other portions of air in the same latitude and elevation. Air, therefore, does not feel cold in consequence of falling from an elevated situation; though this be an opinion commonly entertained; but in consequence of its being suddenly transported from a more northerly to a more southerly situation. Thus, the unequal distribution of temperature in the atmosphere, when properly understood, constitutes no objection to the mobility and ascent of air, when its temperature is increased.

SECTION V.

OF THE RADIATION OF HEAT.

An account of the radiation of heat naturally belongs to this chapter; because we owe all that we know respecting it to the use of the thermometer. *Radiation* signifies, as is implied by the name, that heat has the property of passing off from the surface of bodies in *rays* or straight lines in the same manner as light.

Experiments of Mariotte.

The first rude attempt to examine radiation was made by Mariotte. In the year 1682, he mentioned, at a meeting of the French Academy of Sciences, that the heat of a fire reflected by a burning mirror is sensible in its focus; but when a pane of glass is interposed between the mirror and its focus, the heat is no longer sensible.* Lambert, the celebrated German mathematician, was one of the first persons who repeated and varied this experiment. He placed burning charcoal in

Lambert.

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* Mem. Paris, i. 344.

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the focus of a mirror, while another mirror was standing parallel to it, at the distance of 24 feet. A combustible body placed in the focus of this last mirror was kindled. Yet when the light of a clear burning fire was collected in the focus of a large lens, the heat evolved was scarce sensible to the hand.

Scheele.

Scheele, in his *treatise on Air and Fire*, published in 1777, made many important and new observations on the radiation of heat. He observed that the radiant heat from the fire passes through the air without heating that fluid, and that no aerial currents, however strong, have any tendency to intercept it. If we interpose a pane of glass between the fire and our face, the light passes through the glass with but little diminution; but the whole of the heat is intercepted. A glass mirror reflects the *light* of a fire, but not its *heat*, which it absorbs and retains, while a polished metallic mirror reflects both the light and the heat. The glass when placed before the fire soon becomes hot, but a polished metal may be held a long time in that position without becoming sensibly warm. But if we *blacken* its surface by holding it over a burning candle it cannot be kept four minutes in the hand opposite the fire without acquiring so much heat as to excite pain.*

Saussure, Pictet, King.

These experiments of Scheele were repeated and varied by Saussure, Pictet, and King, between the years 1785 and 1790; they were the first to intro-

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* Scheele on Air and Fire, p. 66. English Transl.

duce specula of tin, which considerably facilitated the future investigation of the subject.

Of Herschell.

In the year 1800, an elaborate paper on the heat of the solar rays, was published in the Philosophical Transactions by Sir William Herschell. He had been occupied in making telescopic observations on the sun, and employed coloured glasses to enable him to look at that luminary. But found that when they were dark enough to exclude the excess of light, they soon became so hot as to crack and break in pieces. This led him to investigate the heating powers of the different coloured rays of the solar spectrum.

It is well known that when a solar ray is made to pass through a triangular prism of glass, and received upon a white plane at some considerable distance, instead of a round image of the sun, it forms an oblong spectrum consisting of seven different coloured rays; namely, *red*, *orange*, *yellow*, *green*, *blue*, *indigo*, *and violet*, of which the *red* is the least and the *violet* the most refracted, and the refrangibility of the others is in the order of their names. He found (as had been done before him) that the violet ray possesses the least heating power, and that the heating power of the spectrum increases from the violet end to the red extremity of the spectrum. The following little table shows the rise of the thermometer in the violet ray, the green ray and the red ray.

Heating power of the rays.

Violet	.	16°
Green	.	22·4
Red	.	55

Herschell had previously examined the illuminating power of the different coloured rays of the spectrum, and he found that the greatest illuminating power resided in the middle of the spectrum, that part of it in which the yellow and the green pass into each other. The violet ray has the least illuminating power, that of the red ray is greater, though much less than the yellow-green, the illuminating powers of these three rays he found as follows.

Their illuminating power.

Violet .	1
Red .	2
Yellow-green .	4

This remarkable difference between the law which the rays of the solar spectrum follow in illuminating and heating bodies, led Herschell to suspect that in all probability the heating power does not terminate with the visible spectrum, but extends to some distance beyond it. Upon placing the thermometer below the limit of the solar spectrum, he found that it still continued to rise. The maximum point of elevation lay between ¼ and ½ inch beyond the red ray, the spectrum being received on a board 52 inches distant from the prism. The heating power still continued sensible at the distance of 1½ inch from the extremities of the red ray.

These experiments were repeated soon after by Mr. Leslie, who employed for the purpose his differential thermometer. He found the greatest heat in the red ray, and no effect whatever beyond the visible spectrum. This induced him to call in

Experiments of Leslie.

question the accuracy of Herschell's observations, and to refuse his assent to them.[*] Landriani had already announced that the greatest heat of the prismatic spectrum was in the yellow ray;[†] and Rochon had found the point of greatest heat situated between the yellow and the red ray;[‡] but the observations of these philosophers do not seem to have been known either to Herschell or Leslie.

Of Englefield.

Sir Henry Englefield repeated Herschell's experiments soon after the appearance of Leslie's paper. His results agreed with those of Herschell. He found the greatest heat beyond the limit of the red ray.[§]

Of Berard.

They were repeated again in 1813 by M. Berard in Berthollet's laboratory, by means of an apparatus furnished at the expense of that eminent chemical philosopher. He employed a heliostade by means of which the prismatic spectrum was rendered stationary. He found the heating power of the violet ray least, and it gradually augmented as the thermometer passed towards the red extremity of the spectrum. But the heating power did not terminate with the red ray; it was sensible half an inch beyond it; though at that distance it was diminished to one-fifth.[||]

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[*] Nicholson's 4to Journal, iv. 244.

[†] Volta; lettere sull'aria inflamabile nativa della paludi; 1777, p. 136.

[‡] Recueil de Memoires, sur la Mecanique et la Physique, 1785, p. 348.

[§] Royal Institution Journ. 1802, p. 202.

[||] Ann. de Chim. lxxxv. 309.

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These different experiments and observations appeared so contradictory, and so inconsistent with each other, that chemists were at a loss what conclusions to draw. The subject was partially elucidated by Wunch,* and still more completely by Seebeck,† whose experiments appear to have been continued for at least ten years. Seebeck found, as all preceding observers had done, that the thermometer was elevated when placed in any one of the coloured rays of the solar spectrum. But the position of the point of greatest intensity depends upon the *nature of the prism* by which these rays are refracted.

Experiments of Seebeck.

When a hollow glass prism filled with *water*, or *oil of turpentine*, or *alcohol*, is employed, the point where the intensity of the heat is the greatest is situated in the *yellow* ray.

When the prism is filled with a solution of corrosive sublimate, and sal ammoniac, (recommended by Blair in his paper on the achromatic telescope), or with sulphuric acid, the *greatest heat* is in the *orange* ray.

When the prism is of *crown glass* or common *white glass*, the greatest heat is in the *red* ray.

When flint glass prisms are used, the heat is greatest without the spectrum at some distance beyond the *red* ray. When Bohemian flint glass prisms were employed, the greatest heat was still

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* Magazin der Gesellsch. naturf. Freund. in Berlin, 1807.

† Alhandl. der Königk. Alad. Wissenschaften, in Berlin, 1818-19, p. 305.

beyond the *red* ray; but nearer than when English flint glass prisms were employed.

These facts being known and understood, it is easy to account for the different results obtained by different experimenters. They depended obviously upon the kind of prism employed by each.

Thus it is established that heating rays are found in every part of the coloured spectrum; but they are accumulated in greatest abundance at the least refracted end. When a flint glass prism is used they are accumulated in greatest abundance beyond the red ray, and quite beyond the visible spectrum. Since the heating and illuminating rays may be thus separated, we have no evidence that they are the same. They seem rather to consist of two distinct kinds of ray, one of which illuminates, and the other heats objects.

Chemical rays most refrangible.

Besides these two species of rays, the solar spectrum appears to contain a third species of ray, which has been distinguished by the name of *chemical ray*, because it produces chemical changes on bodies. If a piece of chalk be immersed in a solution of *nitrate of silver*, and then exposed to the direct rays of the sun, it acquires a dark colour, owing to the deposition of a thin pellicle of silver upon its surface. Chloride of silver is almost instantly blackened by exposure to the direct rays of the sun. In the same manner oxides of gold and mercury are reduced when exposed to the solar ray. Scheele, and after him Senebier, found that these changes are not produced by the red ray, nor by the rays near the least refrangible end of

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the spectrum. A feeble effect is produced by the green ray; but the greatest and most rapid alteration is produced when the oxides or chlorides are exposed to the violet or most refrangible ray.

When plants grow in the dark, they are said to be *etiolated*, and their colour is white. When such a plant is exposed to sunshine, it speedily begins to assume a green colour. Now in this case also, the least refrangible rays have but little or no effect; but the change takes place rapidly, when the plant is placed in the *violet ray*. Berard found that when the least refrangible half of the solar spectrum is collected into a focus by a convex glass, it forms an exceedingly brilliant and colourless light, into which chloride of silver may be kept without undergoing any alteration in colour. But when it is put into the focus formed by concentrating the most refrangible half of the spectrum, though this focus is much less brilliant, it blackens in a few minutes. Dr. Wollaston found that chloride of silver blackened still more speedily, than it did in the violet ray, when placed entirely above the visible spectrum at some distance from the limit of the violet ray. The same result was obtained nearly about the same time, by Messrs. Ritter and Bockmann in Germany. Thus it appears that the chemical rays are still more refrangible than the illuminating rays, and that they extend beyond them.*

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* Seebeck observed that chloride of silver when put into the red ray, acquires a pale rose colour. This is occasioned by the heat, and the effect accordingly is greatest in the hottest part of the spectrum.

Solar spectrum contains three kinds of rays.

The solar spectrum then consists of at least three distinct species of rays; namely, *illuminating rays*, constituting the visible spectrum; *heating rays*, less refrangible than the visible rays, and varying in the place of their greatest intensity according to the nature of the prism through which they are made to pass; and *chemical rays*, more refrangible than the others, and having the place of their greatest intensity beyond the uppermost limit of the visible spectrum. These three kinds of rays move in straight lines from the sun. We know not whether they all possess the same velocity; though there is no proof whatever of any difference in this respect.

Heat radiates from surfaces.

Rays of heat are capable of radiating from the surface of hot bodies, precisely as rays of light do from luminous bodies. The most important facts respecting the differences between bodies as radiators of heat, were ascertained by Mr. Leslie of Edinburgh, and made known by the publication of his *Treatise on Heat*, in 1804. It will be proper to give a view of the most important facts discovered by this very ingenious experimenter.

Effect of surface on cooling.

1. Mr. Leslie filled with hot water a thin globe of bright tin, four inches in diameter, having a narrow neck, and placed it on a slender frame in a warm room without a fire. The thermometer inserted in this globe sunk half way from the original temperature of the water to that of the room in 156 minutes. The same experiment was repeated, but the outside of the globe was now covered with a thin coat of lamp black. The time elapsed in cooling to the same temperature as in the last case was

now only 81 minutes.* Here the rate of cooling was nearly doubled; yet the only difference was the thin covering of lamp black. Nothing can afford a more striking proof than this of the effect of the surface of the hot body on the rate of its cooling.

Count Rumford took two thin cylindrical brass vessels of the same size and shape, filled them both with hot water of the same temperature, and clothed the one with a covering of Irish linen, but left the other naked. The naked vessel cooled ten degrees in 55 minutes, but the one covered with linen cooled ten degrees in 36½ minutes.† In this experiment, the linen produced a similar effect with the lamp black in the preceding. Instead of retarding the escape of heat, as might have been expected, they produced the contrary effect. The same acceleration took place when the cylinder was coated with a thin covering of glue, of black or white paint, or when it was smoked with a candle.

Greatest in still air.

2. The variation in the rate of cooling occasioned by coating the hot vessel with different substances is greatest when the air of the room in which the experiments are made is perfectly still. The difference diminishes when the atmosphere is agitated, and in very strong winds it disappears almost entirely. Thus two globes of tin, one bright, the other covered with lamp black, being filled with hot water, and exposed to winds of various degrees

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* Leslie's Inquiry into the Nature of Heat, p. 268.

† Nicholson's Jour. ix. 60.

of violence, were found by Mr. Leslie to lose half their heat in the following times:*

	Clean Globe.	Blackened Globe.
In a gentle gale .	44′	35′
In a pretty strong breeze	23′	20¼′
In a vehement wind	9·5′	9′

This is sufficient to convince us, that the effect of the lamp black in accelerating cooling cannot be owing to any power which it has of *conducting* heat, and communicating it to the air, but to the property which it has of radiating heat (to use the common expression) in a greater degree than clear metallic bodies.† That this is in reality the case is easily shown.

3. When a canister of tin, of a cubic shape and considerable size, is placed at the distance of a foot or two from a concave mirror of bright polished brass, having a delicate thermometer in the focus, the thermometer experiences a certain elevation. If the canister be coated with lamp black, the thermometer rises much higher than when the metal is left bright. Here we perceive that more heat radiates from the lamp black than the clear metal; since the elevation of the thermometer is in some degree the measure of the radiation. A common thermometer does not answer well in similar experiments, because it is affected by every change of

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* Inquiry into the Nature of Heat, p. 271.

† Petit and Dulong have shown by direct experiments that the quantity of heat taken away from hot bodies by gas by *conduction* is independent of the nature of the surface of the hot body. See Annals of Philosophy, xiii. 322.

temperature in the room in which the experiments are made. But Mr. Leslie has invented another, to which we are indebted for all the precision that has been introduced into the subject. He has distinguished it by the name of the *differential thermometer*. It was employed also by Count Rumford in his researches.

Differential thermometer.

This thermometer consists of a small glass tube bent into the shape of the letter U, and terminating at each extremity in a small hollow ball, nearly of the same size; the tube contains a little sulphuric acid tinged red with carmine, and sufficient to fill the greatest part of it. The glass balls are full of air, and both communicate with the intermediate tube. To one of the legs of the tube is affixed a small ivory scale divided into 100 degrees; and the sulphuric acid is so disposed, that in the graduated leg its upper surface stands opposite to the part of the scale marked 0. The glass ball attached to the leg of the instrument to which the scale is attached, is, by way of distinction, called the *focal ball.* Suppose this thermometer brought into a warm room, the heat will act equally upon both balls, and expanding the included air equally in each, the liquor in the tube will remain stationary. But suppose the focal ball exposed to heat while the other ball is not; in that case the air included in the focal ball will expand, while that in the other is not affected. It will therefore press more upon the liquid in the tube, which will of course advance towards the cold ball, and therefore the liquid will rise in the tube above 0, and the rise will be proportional to the degree of heat applied to the focal ball. This thermometer, therefore, is peculiarly adapted for ascertaining the degree of heat accumulated in a particular point, while the surrounding atmosphere is but little affected, as happens in the focus of a reflecting mirror. No change in the temperature of the room in which the instrument is kept is indicated by it, while the slightest alteration in the spot where the focal ball is placed is immediately announced by it.

In making experiments on the radiation of heat, Mr. Leslie employed hollow tin cubes, varying in size from three inches to ten, filled with hot water, and placed before a tin reflector, having the differential thermometer in the focus. The reflector employed was of the parabolic figure, and about 14 inches in diameter. This apparatus afforded the means of ascertaining the effect of different surfaces in radiating heat. It was only necessary to coat the surface of the canister with the various substances whose radiating properties were to be tried, and expose it, thus coated and filled with hot water, before the reflector. The heat radiated in each case would be collected into the focus where the focal ball of the differential thermometer was placed, and the rise of this instrument would indicate the proportional radiation of each surface. These experiments were conducted with much address. The following are the principal results obtained.

Radiation as the excess of heat above the air.

4. When the nature and position of the canister is the same, the rise of the differential thermometer is always proportional to the difference between the

temperature of the hot canister and that of the air in the room in which the experiment is made.*

Rise of thermometer inversely as the distance.

5. When the temperature of the canister is the same, the effect upon the differential thermometer diminishes as the distance of the canister increases from the reflector, the focal ball being always understood to be placed in the focus of the mirror. Thus if the rise of the thermometer, when the canister was three feet from the mirror, be denoted by 100, it will amount only to 57 when the canister is removed to six feet. On substituting a glass mirror for the reflector, and a charcoal fire for the canister, when the fire was at the distance of 10 feet the thermometer rose 37°, and at the distance of 30 feet it rose 21°.† From Mr. Leslie's experiments it follows, that the effect on the thermometer is very nearly inversely proportional to the distance of the canister from the reflector. He found likewise that when canisters of different sizes were used, heated to the same point, and placed at such distances that they all subtended the same angle at the reflector; in that case the effect of each upon the differential thermometer was nearly the same. Thus a canister of

3 inches at	3 feet	distance raised the thermometer	50°
4 inches –	4 feet	. . . . . .	54
6 inches –	6 feet	. . . . . .	57
10 inches –	10 feet	. . . . . .	59

From these experiments we learn, that the effect

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* Leslie, p. 14.

† Ibid. p. 51.

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of the canister upon the thermometer is nearly proportional to the angle which it subtends, and likewise that the heat radiated from the canister suffers no sensible diminution during its passage through the air.

Heat radiates in all directions.

6. Heat radiates from the surface of hot bodies in all directions; but from Mr. Leslie's experiments we learn, that the radiation is most copious in the direction perpendicular to the surface of the hot body. When the canister is placed in an oblique position to the reflector, the effect diminishes, and the diminution increases with the obliquity of the canister. Mr. Leslie has shown, that the effect in all positions is proportional to the visual magnitude of the canister as seen from the reflector, or to its orthographic projection. Hence the action of the heated surface is proportional to the sine of its inclination to the reflector.

Such are the effects of the temperature, the distance, and position of the canister with respect to the reflector. None of these, except the first, occasion any variation in the quantity of heat radiated, but merely in that portion of it which is collected by the mirror and sent to the focal ball; but the case is different when the surface of the canister itself is altered.

7. Mr. Leslie ascertained the power of different substances to radiate, by applying them in succession to a side of the canister, and observing what effect was produced upon the differential thermometer. The following table exhibits the relative power of the different substances tried by

that philosopher, expressed by the elevation of the differential thermometer produced.

Radiating power of different bodies.

Lamp black	100
Water by estimate	100+
Writing paper	98
Rosin	96
Sealing wax	95
Crown glass	90
China ink	88
Ice	85
Minium	80
Isinglass	80
Plumbago	75
Tarnished lead	45
Mercury	20+
Clean lead	19
Iron polished	15
Tin plate	12
Gold, silver, copper	12

From this table it appears, that the metals radiate much worse than other substances, and that tin plate is one of the feeblest of the metallic bodies tried. Lamp black radiates more than eight times as much as this last metal, and crown glass 7·5 times as much.

8. Such are the radiating powers of different substances. But even when the substance continues the same, the radiation is very considerably modified by apparently trifling alterations on its surface. Thus metals radiate more imperfectly than other bodies; but this imperfection depends upon the brightness and smoothness of their surface. When, by exposure to the air, the metal acquires that tarnish which is usually ascribed at present to oxidizement, the power of radiating heat is greatly increased. Thus it appears from the preceding table, that the radiating power of lead while bright is only 19; but when its surface becomes tarnished, its radiating power becomes no less than 45. The same change happens to tin, and to all the metals tried.

Increased by tarnishing.

And scratching the surface.

When the smoothness of the surface is destroyed by scratching the metal, its radiating power is increased. Thus if the effect of a bright side of the canister be 12, it will be raised to 22 by rubbing the side in one direction with a bit of fine sand paper.* But when the surface is rubbed across with sand paper, so as to form a new set of furrows intersecting the former ones, the radiating power is again somewhat diminished.

Varies with the thickness of the coating.

9. The radiating power of the different substances examined was ascertained by applying a thin covering of each to one of the sides of the canister. Now this coat may vary in thickness in any given degree. It becomes a question of some importance to ascertain, whether the radiating power is influenced by the thickness to a given extent, or whether it continues the same whatever be the thickness of the covering coat. This question Mr. Leslie has likewise resolved. On a bright side of a canister he spread a thin coat of liquified jelly, and four times the quantity upon another side; both dried into very thin films. The effect of the thinnest film was 38, that of the other 54. In this case the effect increased with the thickness of the coat. The augmentation goes on till the thickness of the coat of jelly amounts to about $\frac{1}{1000}$dth of an inch; after which it remains stationary. When a surface of the canister was rubbed with olive oil, the effect was 51: a thicker coat of oil produced an effect of 59. Thus it appears that when a metallic surface is covered with a coat of

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* Leslie, p. 81.

jelly or oil, the effect is proportional to the thickness of the coat, till this thickness amounts to a certain quantity; but when a vitreous surface is covered by very thin coats of metal, no such change is perceived. A canister was employed, one of the sides of which was a glass plate. Upon this plate were applied, in succession, very fine coats of gold, silver, and copper leaf. But notwithstanding their thinness, the effect was only 12, or the same that would have been produced by a thick coat of these very metals. But when glass enamelled with gold is used, the effect is somewhat increased; a proof that varying the thickness of the metallic coats would have the same effect as varying the thickness of jelly, provided they could be procured of sufficient tenuity.* As long as an increase of thickness alters the radiating power of the coat, it is obvious that the surface of the canister below exerts a certain degree of energy. And the action exerted by metallic bodies appears to be greater than that exerted by vitreous bodies.

10. Such are all the circumstances connected with the radiating surface hitherto observed, which influence its power. For hitherto it has been impossible to ascertain the efficacy of *hardness* and *softness*, or of *colour*, upon radiation; though it appears, from Mr. Leslie's experiments, not unlikely that softness has a tendency to promote radiation.† But as the effect, as far at least as measured by the differential thermometer, depends not only upon

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* Leslie, p. 110.

† Ibid. p. 90.

the radiating surface, but likewise upon the surface of the focal ball, and likewise of the reflector; it will be necessary also to consider the modifications produced by alterations in the surface of these bodies. This inquiry, for which, like the preceding, we are indebted to Mr. Leslie, will throw considerable light on the nature of radiation.

Surfaces radiate and absorb heat equally.

11. When the focal ball is in its natural state, that is to say, when its surface is vitreous, it has been already observed, that the side of the hot canister coated with lamp black raises the thermometer 100°. If the experiment be repeated, covering the focal ball with a smooth surface of tinfoil, instead of rising to 100°, the thermometer will only indicate 20°. A bright side of the canister will raise the thermometer, when the focal ball is naked, 12°; but when the ball is covered with tinfoil, the elevation will not exceed 2^1 degrees.* From these experiments it is obvious, that metal not only radiates heat worse than glass, but likewise that it is not nearly so capable of imbibing it when the rays strike against its surface. If the surface of the tinfoil be furrowed by rubbing it with sand paper, the effect produced when the focal ball is exposed in the focus will be considerably increased.† It has been already observed that the radiating power of tin is likewise increased by scratching it. These facts entitle us to conclude, that those surfaces which radiate heat most powerfully, likewise absorb it most abundantly when it impinges against them.

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* Leslie, p. 19.

† Ibid. p. 81.

Reflection inversely as radiation.

12. The very contrary holds with respect to the reflectors, as might indeed have been expected. Those surfaces which *radiate* heat best, *reflect* it worst; while the weakest *radiating* surfaces are the most *powerful* reflectors. Metals of course are much better reflectors than glass. When a glass mirror was used instead of the tin reflector, the differential thermometer rose only one degree; upon coating the surface of the mirror with lamp black, all effect was destroyed; when covered with a sheet of tinfoil the effect was 10°.*

Reflecting power of various bodies.

To compare the relative intensity of different substances as reflectors, Mr. Leslie placed thin smooth plates of the substances to be tried before the principal reflector, and nearer than the proper focus. A new reflection was produced, and the rays were collected in a focus as much nearer the reflector than the plate as the old focus was farther distant. The comparative power of the different substances tried was as follows:†

Brass . . .	100	Lead	60
Silver . . .	90	Tinfoil softened by mercury	10
Tinfoil . . .	85	Glass	10
Block tin . . .	80	Ditto coated with wax or oil	5
Steel . . .	70		

When the polish of the reflector is destroyed by rubbing it with sand paper, the effect is very much diminished. When the reflector is coated over with a solution of jelly, the effect is diminished in proportion as the thickness of the coat increases, till

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* Leslie, p. 20.

† Ibid. p. 98.

its diameter amounts to $\frac{1}{1000}$th part of an inch. The following table exhibits the intensity of the reflector coated with jelly of various degrees of thickness.*

Thickness of coat.	Effect.
Naked Reflector	127
$\frac{1}{400000}$	98
$\frac{1}{100000}$	93
$\frac{1}{50000}$	87
$\frac{1}{20000}$	61
$\frac{1}{10000}$	39
$\frac{1}{5000}$	29
$\frac{1}{2000}$	21
$\frac{1}{1000}$	15

All these phenomena are precisely what might have been expected, on the supposition that the intensity of reflection is inversely that of radiation. Mr. Leslie has shown that it is the anterior surface of reflectors only that acts. For when a glass mirror is employed, its power is not altered by scraping off the tin from its back, nor by grinding the posterior surface with sand or emery.†

13. Such are the phenomena of the radiation of heat as far as the radiating surface, the reflector, and the focal ball are concerned. It cannot be doubted from them, that heat is actually radiated from different surfaces, and that bodies vary considerably in their radiating power. We have seen also that substances differ no less from each other in their power of reflecting heat, and that the in-

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* Leslie, p. 106.

† Ibid. p. 21.

tensity of the latter power is always the inverse of the intensity of the former. Before we can be able to form a judgment of the way in which the heat is conveyed in these cases, it will be necessary to examine the effect of the different mediums in which the radiation may take place, and the obstructions occasioned by putting different substances between the radiating surface and the reflector. Both of these points have been examined by Mr. Leslie with his usual acuteness.

14. Radiation is confined to elastic fluids, and does not take place to a sensible degree when the medium in which the hot body is placed is either a solid or a liquid body. From the experiments of Leslie, compared with those of Dulong and Petit, we are entitled to conclude that the radiation is the same whatever the elastic fluid be that surrounds the hot body. The rapidity of cooling varies with the different gases, and with the density of the gases that surround the hot body; but this variation is owing not to any alteration in the radiation; but to differences in the conducting powers of the gaseous bodies themselves. Dulong and Petit have shown that hot bodies are capable of cooling in an absolute vacuum, and the loss of each in such a case can be ascribed only to radiation. Hence it is obvious that Professor Leslie's theory that the radiation of heat is owing to the abstraction of it by pulses of air cannot be correct.

Effect of screens.

15. When a substance is interposed by way of screen between the hot canister and the reflector, the effect is either diminished or destroyed altoge-

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ther, according to circumstances. These circumstances have been examined by Mr. Leslie with great sagacity. Indeed, the development of the effect of screens constitutes perhaps the most curious and important part of his whole work. A screen may affect the radiation of heat three ways; 1. By its distance from the hot canister; 2. By its thickness; and, 3. By the nature of the substance of which it is composed. Let us take a view of each of these in succession.

Increases with the distance.

First, From all Mr. Leslie's trials, it appears that a screen diminishes the effect of radiation upon the differential thermometer situated in the focus of the reflector, in proportion to its distance from the canister. When placed very near the canister, the effect is comparatively small; but it increases rapidly as the screen is drawn away from the canister; so that the elevation of the differential thermometer is soon prevented altogether. When the canister is at the distance of three feet from the reflector, if the side painted with lamp black produce an effect equivalent to 100, this effect upon interposing a pane of glass at the distance of two inches from the canister will be diminished to 20. When the pane is advanced slowly forward toward the reflector, the effect of the radiation gradually diminishes; and when it has got to the distance of one foot from the screen, the radiation is completely intercepted.*

Second, When a screen of thin deal board is used

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* Leslie, p. 28.

And the thickness.

instead of the pane of glass, and placed at the distance of two inches from the canister, the radiation is diminished, and the diminution is proportional to the thickness of the board.

With a board $\frac{1}{8}$ inch thick the effect is 20
———————— $\frac{3}{8}$ inch do. 15
———————— 1 inch do. 9

Thus the radiation diminishes very slowly as the thickness increases.*

Radiated heat intercepted by solids.

Third, When a sheet of tinfoil is substituted for the glass pane, and put into the same position, the effect, instead of 20, is reduced to 0; and this happens however thin the tinfoil is; even gold leaf of the thickness of $\frac{1}{300000}$dth part of an inch, though pervious to light, completely stops the progress of radiating heat. When a sheet of writing paper is substituted for tinfoil, the effect is 23.† Thus it appears, that substances vary considerably from each other in their property of intercepting radiating heat; and likewise that the power of intercepting heat is inversely as the power of radiating it. Those substances which radiate most heat, intercept the least of it when in the situation of screens; and those which radiate the least heat, on the contrary, intercept the most. But it was formerly observed, that the power of absorbing heat was the same with that of radiating it. Hence those substances which absorb least heat are the most powerful interceptors of it, and the contrary.

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* Leslie, p. 38.

† Ibid.

These facts lead naturally to the opinion, that the property of absorbing heat depends upon the *surface* of the substance which is interposed as a screen; an opinion which Mr. Leslie has established by the following experiments. He took two panes of glass, and coated one side of each with tinfoil, leaving the other side bare. These two panes were pressed together: the tinned side of each being outmost, and applied as a screen at two inches distance from the canister. The whole of the rays of heat appeared to be intercepted, for the thermometer was not acted upon at all. But when the glass side of the screen was outmost, the effect of radiation was equivalent to 18. Here we find the very same screen, in the very same position, intercepting very different proportions of the radiated heat, according to the nature of its external surface. When the tin was outmost, the whole heat was stopped; but when the glass was outmost, about $\frac{1}{5}$th passed on to the reflector. The effect was analogous when two sheets of tin, each painted on one side with a thin coat of lamp black, were employed as a screen, and placed two inches from the canister. Pressed together, and having their metal sides outmost, the radiation produced no effect upon the thermometer; but when the blackened sides were outmost, the effect was equivalent to 23. When only one of the plates is used, and its blackened side turned to the canister, the effect is equal to 4. If the two plates be used with their blackened sides outmost, and at the distance of two inches from each other, all effect is destroyed.[*]

Heat permeates glass more as the temperature increases.

16. But the subsequent experiments of M. De la Roche have somewhat modified the consequences which appeared to follow from the very ingenious experiments of Leslie, and show a much greater analogy between the radiation of light and heat than that philosopher had supposed. De la Roche found that radiant heat in some cases passes directly through glass: that the quantity of radiant heat which passes directly through glass is so much greater relative to the whole heat emitted in the same direction, as the temperature of the source of heat is more elevated; that calorific rays, which have already passed through a screen of glass, experience in passing through a second glass screen of a similar nature a much smaller diminution of their intensity than they did in passing through the first screen: that rays emitted by a hot body differ from each other in their faculty of passing through glass: that a thick glass, though as much or more permeable to light than a thin glass of a worse quality, allows a much smaller quantity of radiant heat to pass; but the difference is so much the less as the temperature of the radiating source is more elevated: and that the quantity of heat which a hot body yields in a given time by radiation to a cold body situated at a distance, increases, *cæteris paribus*, in a greater ratio than the excess of temperature of the first body above the second.[†]

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[*] Leslie, p. 35.

[†] Annals of Philosophy, ii. 100.

The conclusions from the observations of De la Roche have been called in question by Mr. Powell. He admits that when a hot body becomes luminous it gives out *heat* capable of passing directly through transparent screens. But this new heat acts more on a smooth black surface than on an absorptive white one. From this he concludes that it is different from common radiant heat. We have no evidence that it is the same as light. It is great from red hot metals, though the light be feeble. It exists in the solar ray and is what produces the photometrical effect in Leslie's Photometer.* But this ingenious explanation of Mr. Powell has, I think, been obviated by a very happy and instructive experiment of Mr. Ritchie. Between the focal balls of a differential thermometer and equally distant from both he placed a hot body. The liquid remained stationary, because both balls were equally acted upon. But if one half of one of the balls farthest remote from the hot body be coated black or rendered opaque, the liquid immediately sinks in the tube under that bulb. The reason, says Mr. Ritchie, is that the radiant heat which formerly passed through the glass is intercepted by the opaque coating, and of course dilates the air within the ball.† Mr. Powell ascribes the apparent dilatation of the air to the more rapid cooling of the coated glass from radiation.‡ But Mr. Ritchie has obviated this objection to his conclusions in a very ingenious manner. "Let," says

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* Phil. Trans. 1825, p. 187. † Edin. Jour. xi. 281.

‡ Annals of Philosophy, 2d Series, xii. 13.

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he, "two air thermometers be procured, having their bulbs large and blown very thin. Place them at a convenient distance from each other, and then put a heated iron ball between them in such a position that the fluids in the two stems will sink exactly the same number of degrees. Let one of the hemispheres in the ball A, formed by a plane passing through the centres of the two balls, be coated with China ink. Let two of the alternate quarters of the ball B, formed by a plane cutting the former at right angles, be also coated with China ink. Place the thermometers in their original positions, raise the iron ball to an elevated temperature (though still invisible in the dark), place it in its former position, and carefully observe the number of degrees the fluid descends in each stem. A striking difference will now be observed. The fluid will be found to have sunk several degrees lower in the thermometer B, than in A. The same experiment may also be performed with a differential thermometer having the bulbs coated as formerly described."*

It is obvious that Mr. Powell's explanation will not apply to the experiment just described. We must allow, I think, that heat radiated from hot bodies not luminous, and, therefore, radiant heat, passes through thin glass, since it is intercepted by the opaque coating. It would not be surprising if the power of producing *heat* and *light* were properties of the same substance. It may produce light

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* Annals of Philosophy, 2d Series, xii. 123.

when acting with a certain intensity, or when the particles follow each other at certain limited intervals. When these intervals are changed heat may be produced. It is even conceivable that those rays which are invisible to our eyes, and which therefore we are accustomed to consider as pure heat, may produce an illuminating effect upon the eyes of some other animals.

Mr. Leslie's explanation of the radiation of heat agrees so ill with the phenomena, that I am not aware of its having been embraced by any other person. The explanation which has been generally adopted, was originally started by M. Prevost, of Geneva, as early as 1791, and was afterwards expounded in a work entitled *Du Calorique Rayonnant*, published in the year 1809.

Prevost's theory of radiation.

Prevost supposes that heat is a discrete fluid, every particle of which moves in a straight line. These particles go one in one direction and another in another, so that every sensible point of the hot space is a centre, from which depart, and to which arrive, rows of particles or calorific rays. The consequence of this must be, that if we suppose two neighbouring spaces, in which heat abounds, there must be a constant interchange of heat between them. If it abounds equally in each, the interchanges will balance each other, and the temperature will continue the same. If one contains more than the other, the interchanges will be unequal, and by a continued repetition of these unequal changes, the temperatures of both will be in time reduced to equality. If we suppose a body placed in a medium hotter than itself, and the temperature of that medium constant, we may consider the heat of the medium as consisting of two parts, one equal to that of the body, the other equal to the difference between the temperature of the two. The first of these two parts may be left out of view, being counterbalanced by the radiation of the body; the excess alone requires consideration, and relatively to that excess, the body is absolutely cold.

According to this theory, all bodies are constantly emitting rays of heat in all directions, and constantly receiving rays of heat from all directions. When the rays emitted and the rays received are equal in number, the temperature remains constant. When a body sends out more rays than it receives, its temperature sinks; when it receives more than it sends out, its temperature rises. If a body be placed in the neighbourhood of another, it will intercept a great many rays that were moving towards the second body, in the direction now blocked up. But if the temperature of the intercepting body be the same as that of those whose rays it intercepts, it will send out as many rays as it stops, and thus the temperature of the second body will not be affected. If the intercepting body be *colder* than those whose rays it intercepts, it will receive more than it can give out; the consequence of which will be, that the temperature of the second body will be reduced. The very contrary must happen if the intercepting body be hotter than those whose rays it intercepts.

This theory will enable us to explain a set of

Apparent radiation of cold explained.

phenomena, which have occasioned much discussion, and been considered by some as incompatible with the received notions concerning heat; I mean the apparent radiation of cold.

The first attempt to determine the effect of cold radiated from a mirror, was made by the Florentine academicians in 1667, and is thus related in the Essays of that Academy translated by Mr. Waller. "We were willing to try if a concave glass set before a mass of 500lb. of ice, made any sensible repercussion upon a very nice thermometer of 400 deg. placed in its focus. The truth is, it immediately began to subside, but by reason of its nearness to the ice, 'twas doubtful whether the direct or reflected rays of cold were more efficacious: upon this account we thought of covering the glass, and (whatever may be the cause) the spirit of wine did indeed presently begin to rise. For all this we dare not be positive, but there might be some other cause thereof, besides the want of reflection from the glass; since we were deficient in making all the trials necessary to clear the experiment."*

Gaertner, modeller to the king of Poland, repeated this experiment in a more satisfactory way in the year 1781. He contrived a set of wooden parabolic mirrors which he gilded internally, with which he made various experiments, among others was the following. "If instead of a hot body, cold water be put into the focus of the mirror, it

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* Waller's translation, p. 103.

gives out even in summer an agreeable coolness. But if instead of cold water I made use of ice, there was a considerable production of cold at the distance of ten or twenty paces.

This experiment was repeated by M. Pictet in somewhat a different manner, and the account of it published by him in 1791 in his *Essai sur le feu.* He took two concave mirrors indifferently polished, and having a focus at nine inches distance, and placed them opposite each other and 10½ feet asunder. A delicate air thermometer was put into one of the foci, and a matrass full of snow into the other, the thermometer sunk several degrees, and rose again when the matrass was removed. When nitric acid was poured on the snow the thermometer sunk several degrees lower.

This experiment has been considered by some as inexplicable, unless we admit the radiation of cold. The snow, it has been said, is not absolutely destitute of heat. It must therefore radiate some heat. Consequently its presence ought rather to raise than sink the thermometer. But this objection can be insurmountable to those only who have not taken the trouble to make themselves acquainted with the principles of Prevost's theory.

To explain what happens, we have only to recollect that heat is constantly radiating from all bodies and entering into all bodies, and that while the temperature continues unaltered, the rays of heat lost by radiation must be just compensated by the rays received. From the nature of radiation it is obvious that the intensity of the ray must be inversely as the square of the distance from the radiating object. A thermometer in a room may be considered as in the centre of a sphere of bodies constantly radiating heat upon it. The rays which it throws out just compensate those which it receives, and consequently its temperature remains unchanged. Suppose a mirror placed in its neighbourhood. It will intercept a considerable number of rays sent to the thermometer by the surrounding sphere. Indeed, if it be near enough and large enough, it will intercept a whole hemisphere of these rays. But this can produce no change upon the temperature of the thermometer, because the difference between the size of the mirror and of the hemisphere intercepted, will be exactly compensated by the smaller distance of the mirror. If we put the thermometer into the focus of the mirror the effect will continue the same. The mirror will indeed collect the rays from one of the hemispheres, and reflect them upon the thermometer; but it will intercept the rays from the other hemisphere. The temperature of the thermometer will not be affected, because the number of rays sent to the thermometer being the same, it can make no difference whether these rays be supplied by reflection or radiation.

Suppose a second mirror to face the first. The cylinder of rays from the second hemisphere is now intercepted. But then all the rays which pass through its focus to its face are reflected in the direction of the intercepted cylinder. Thus a cylinder of reflected rays is formed equal to and instead of the intercepted one. The temperature of the thermometer must still continue unaltered, because it continues to receive as many rays as it emits.

Let us now place a hot body in the focus of the second reflector. The cylinder from this reflector will now consist of an extraordinary number of rays, which will be collected by the other mirror and sent to the thermometer. The thermometer will now receive more rays than it emits, and consequently will rise. Substitute a matrass full of ice instead of the hot body in the focus of the second reflector, the consequence will be that the rays which would have otherwise passed through the focus of this reflector will be absorbed by the ice. Fewer rays will make their way to the other mirror and be collected into its focus. The thermometer will now radiate more heat than it receives, and of course will fall. When nitric acid is poured upon snow the temperature sinks about 40°. It will therefore radiate still less heat than the ice. The thermometer therefore must sink still lower.*

There is an experiment by Count Rumford, which he considered as incompatible with the common opinions respecting radiation; but which admits of as easy an explanation as the cooling occasioned by the snow in Pictet's experiment. He took a conical metalic tube about 18 inches long, whose re-

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* On this subject the reader may consult Mr. Davenport's paper, Annals of Philosophy (1st series), p. 338, and Mr. Crampton Holland's paper on the same subject in the Edin. Transactions, 179.

spective apertures were 1 inch, and 5 inches in diameter. The inside of the tube was polished so as to reflect heat powerfully and to radiate little. This tube being supported in a horizontal position, the focal ball of a differential thermometer was placed at the larger opening, while a matrass full of ice was put before the smaller opening. The thermometer did not sink, or sunk only a very little. But when the position of the ice and thermometer was reversed, by placing the former before the wide opening, and the latter in the narrow opening of the tube, the thermometer now sunk a considerable space, indicating a much more rapid decrease of temperature. This experiment is easily explained by the application of Prevost's theory. It is evident from the well known laws of optics, that the incident rays of heat which are parallel to the axis of the tube are reflected in the form of cones, and cross in the axis. However, the largest hollow cylinder of incident rays will form the focus of greatest intensity; and a multitude of other rays will be brought by complex reflections to the same spot. The tube is, in fact, a truncated cone; for the smaller end has a diameter. The principal focus is not at the apex of the cone; but short of it, and we may suppose the thermometer placed in this point. Now it is easy to see that the rays from a considerable portion of the surrounding sphere are intercepted by the tube; but this is compensated by the rays reflected by the inside of the tube, and falling upon the thermometer placed in the focus. Hence a thermometer placed at the narrow end of the tube

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undergoes no alteration whatever. But when the matrass with ice is placed at the wide end of the tube, it intercepts the incident rays which would have been reflected from the inside of the tube upon the thermometer. The thermometer now radiates more heat than it receives, and must therefore fall.

The radiation of heat being understood, we are enabled to explain, by means of it, a variety of natural phenomena of a very interesting nature. I may give an example or two to enable the reader to perceive the importance of this part of the doctrine of heat.

Cause of dew.

1. It is to the radiation of heat that we are indebted for the condensation of dew, to which plants are so much indebted for their nourishment, as was first satisfactorily explained by Dr. Wells in his *Essay on Dew*, which constitutes one of the most beautiful examples of inductive reasoning in the English language. To understand the way in which dew is formed it is necessary to know that water is capable of being converted into vapour at all temperatures from 32° to 212°. Hence the atmosphere is seldom or never destitute of aqueous vapour. But the absolute quantity that can exist depends upon the temperature. At 32° it can contain only $\frac{1}{150}$th of its volume of vapour, while at 52° it can contain $\frac{1}{86}$th of its volume. When air containing vapour diminishes in its temperature, a portion of its vapour is usually condensed into water. The amount of the diminution of temperature necessary to cause air to deposit moisture depends upon the quantity of vapour which it contains.

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If the quantity be as great as can exist at the given temperature, then the smallest diminution of temperature will occasion the deposition of humidity. But if air at 72° contain only as much vapour as it can retain at the temperature of 52°, it is obvious that it must be cooled below 52° before it begin to deposit moisture.

During the day a good deal of water is converted into vapour from the surface of lakes, seas, and rivers, and from the earth itself, and mixes with the atmosphere. The temperature of the atmosphere usually sinks considerably after sunset, and is often 20° or 30° colder than at the hottest part of the day. Hence it must approach much nearer the point of depositing moisture than during the day. The greatest difference between the temperature of day and night takes place in this country in spring and autumn, and these are the seasons in which the most abundant dews are usually deposited. Dewy nights are usually clear. On cloudy nights dew seldom falls.

Many years ago, a curious set of experiments on dew was made by M. Dufay. He placed a glass cup in the middle of a silver basin, and left both in the open air during a dewy night. Next morning the silver basin was found dry; but the glass cup was wet with dew. When the experiment was reversed by placing a silver cup in the middle of a glass basin, the glass was still moist and the silver dry.* These and many other similar experiments, remained unexplained till Dr. Wells turned his

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* Mem. Par. 1736, p. 352.

attention to the subject. It is only necessary to say that the metals are bad radiators of heat, while glass is a good radiator. Hence in a cloudless night the temperature of the glass exposed to the aspect of the sky will sink much lower than that of metals. It will cool the air in its neighbourhood more, and of course dew will be deposited on it in preference. Dr. Wells found, as Mr. Six had done before him, that a thermometer laid on a grass plot in a clear night sunk 6°, 8°, 13°, or even 20° lower than a thermometer hung at some height from the ground. Because grass radiates heat well. In short, dew is deposited on those substances which radiate heat well, while it avoids, for an obvious reason, all bad radiators. These depositions do not take place on cloudy nights, because clouds radiate the heat back again, and thus prevent the temperature of good radiators from sinking much below that of the atmosphere.

Deposition of moisture on windows.

2. In frosty weather moisture is almost always condensed upon the inside of the windows of our apartments (during the night when the room is without fire), in the form of dew or hoar frost. The glass being a good radiator, is speedily cooled below the temperature of the room. Vapour from the air in the apartment is consequently condensed upon it, and it assumes the form of dew or hoar frost according to the temperature of the glass. This condensation is much more abundant when the window shutters are closed than when they are left open. Because in the latter case the radiation from the different parts of the room upon the

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window, supplies a considerable portion of the heat radiated by the glass, and prevents the temperature from sinking so low.

Formation of ice.

3. Dr. Wells has shown, by a very happy induction, that the formation of artificial ice in India depends upon the radiation of heat from the surface of the water. The nights which answer best for making this ice are clear and calm. The water is put in shallow pans, and it is so placed that the heat from the ground cannot easily penetrate to it. The congelation is usually greatest before sun-rise, and as dew is copiously deposited it is obvious that no evaporation can take place from its surface. So that the explanation of this process formerly given by Dr. Black, namely, the cold produced by evaporation, is fallacious.

Use of mats to preserve plants.

4. In this country it is not uncommon for cold nights to occur in the month of May, when the fruit trees are in blossom. In such cases careful gardeners are in the habit of covering up those trees and plants that would be injured by the cold with mats, and the precaution is generally successful. These mats act simply by preventing the heat from escaping from the plant by radiation; as they radiate as much or almost as much back, as they receive from the plant. The cold weather is usually over before the mat is cooled down so low by the action of the air as to cease to compensate to the plant for the loss of its heat by radiation.

CHAPTER II.

OF FLUIDITY.

All the substances in nature exist either in the state of solids, liquids, or gases, and the state depends upon the temperature to which they are exposed. There are many bodies, which in the common temperature of our atmosphere are *solid;* but which become *liquid* when exposed to a temperature sufficiently high. Tin and lead are solid metals; but they become liquid when exposed to a temperature below ignition. Sulphur melts at a still lower temperature; and water becomes liquid at so low a temperature that in this country it is very seldom in a solid state.

All solids may be fused, except the diamond.

All the solid bodies with which we are acquainted are capable of being converted into liquids by temperatures which we have it in our power to produce; with one remarkable exception, namely, the *diamond;* which it has been hitherto impossible to melt by the application of heat. But this does not constitute a true exception to the general law, that all solids may be rendered liquid by heat. Because the diamond in consequence of some of its properties cannot be exposed to a temperature so high as some other bodies can. Lime and magnesia were long considered as infusible by heat; and it was not till they were exposed to the very high temperature produced by burning a mixed stream of oxygen and hydrogen gases that their refractory nature was overcome. But the diamond cannot be exposed to such a test, because it is a combustible body, and catches fire, and is consumed at comparatively low temperatures. Nor can we expose it to a sufficiently high temperature to fuse it in close vessels; because with many substances, as iron, it combines when strongly heated with them, and because it is probably as refractory or more so than lime or magnesia, in vessels of which it cannot therefore be heated with any chance of fusion. Charcoal is in the same predicament with the diamond, because its nature is very nearly the same with that of the precious stone.*

There is a vast difference in the temperatures requisite to fuse different solid bodies. Lime, magnesia, alumina, and the other earthy bodies, together with silica, are so refractory that they have been fused only in minute particles by an ignited current of oxygen and hydrogen gases. Platinum cannot be fused in our furnaces, but it yields before the blowpipe, urged by a stream of oxygen gas. Dr. Clarke, by his oxygen and hydrogen blowpipe, melted 100 grains of platinum, and kept it in fusion for some minutes. Iron may be melted in a good draught furnace. Mr. Mushet in this way

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* Dr. Silliman is of opinion that he melted charcoal by exposing it to the intense heat of Dr. Hare's deflagrator. I repeated the experiment and obtained a microscopic bead; but it consisted of the earthy matters of which charcoal is never destitute.

melted pieces of iron about half an ounce in weight; and Sir George Mackenzie fixed its fusing point at 158° Wedgewood. Gold and copper melt easily in a wind furnace; and silver may be fused in a common fire urged by a pair of bellows. Zinc and antimony melt at about a red heat. Lead, bismuth, and tin at temperatures considerably lower. And the fusing point of mercury is so low that in this country we never see it in a solid state, except when we expose it to the action of a freezing mixture.

Liquids may be congealed,

It is no less true that all liquid bodies become solid when we expose them to a temperature sufficiently low. Thus *water* in frosty weather is changed into *ice*. Olive oil becomes solid, like tallow, when cooled down sufficiently low. Oil of turpentine requires a much more intense cold to freeze it; but the weather is occasionally cold enough even in this country to cause it to congeal. Neither mercury nor sulphuric ether ever freeze in Great Britain. But they would have congealed in Melville island, where Captain Parry wintered. For the thermometer at some little distance from the ship stood at —55°, which is about 10° lower than the freezing point of sulphuric ether.

Except alcohol.

There is, however, one liquid which has never yet been frozen; at least the evidence that it has been is somewhat unsatisfactory. This liquid is *alcohol*. Common spirits freeze in severe colds. But absolute alcohol, or alcohol of the specific gravity 0·798, did not congeal when it was exposed by Mr. Walker to a cold of —90°. It was indeed

Chap. II. announced in 1813, in the Edinburgh newspapers, and repeated in the scientific Journals, that Mr. Hutton had succeeded by a peculiar process, which he kept secret, in congealing it by reducing it to the temperature of —110°. But as Mr. Hutton has never made his process known to the public, though it is not easy to conceive any good reason for concealing it, we may be allowed to entertain some doubts respecting the accuracy of the newspaper statement. Professor Leslie, if I understand him rightly, exposed absolute alcohol to a cold of —120° without observing any congelation.*

Alcohol excepted (if it really constitute an exception), all liquids are converted into solids when exposed to a sufficiently low temperature; and, the diamond excepted, all solids are converted into liquids when exposed to a sufficiently high temperature. Fluidity then is a very general effect of heat, and heat is obviously the efficient cause of fluidity. The addition of heat converts a solid into a fluid, while the abstraction of heat converts a fluid into a solid.

When we compare *fluidity* and *expansion* we find this remarkable difference between them. Expansion takes place at every temperature, and is the constant concomitant of the addition of heat. But

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* M. Bussy moistened a quantity of cotton with liquid sulphurous acid (this acid liquefies at about —10°) wrapped it round the bulb of a thermometer in the receiver of an air-pump, and exhausted the receiver. The thermometer sank to —90°.5. Alcohol of 0·85, exposed to the same action, became solid.

fluidity never takes place till bodies are heated up to a particular degree. This point is constant in every particular body, so as to characterize and distinguish it from every other. If the body be usually solid we call the point at which it changes to a liquid the *melting point.* But when the body in the usual temperature of the atmosphere is liquid the point at which it changes its state and becomes solid is called its *freezing point.* Thus we talk of the *melting* point of lead, sulphur, and tallow, and of the *freezing* point of water, oil, mercury.

As this point constitutes a remarkable fact in the history of bodies, it will be worth while to exhibit a tabular view of the melting points of those bodies which have been determined with tolerable accuracy.

Melting points.

	Fahrenheit.	Observer.
Cast iron melts at	3479°	Daniel
Gold	2590	Daniel
Silver with $\frac{1}{4}$ gold	2050	Princep*
Copper	2548	Daniel
Silver with $\frac{1}{10}$ gold	1920	Princep*
Silver	2233	Daniel
Silver	1830	Princep*
Brass	1869	Daniel
Antimony	810	Mortimer
Zinc	648	Daniel
Lead	606	Crichton, jun.
Bismuth	497	Crichton, jun.
Tin	442	Crichton, sen.
Sulphur	218	Irvine

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* Phil. Trans. 1828, p. 79. These melting points were measured by the dilatation of air.

	Fahrenheit.	Observer.
Bees' wax (bleached)	142°	Nicholson
Spermaceti	112	Nicholson
Phosphorus	108	Thomson
Tallow	92	Nicholson
Olive oil	36	
Ice	32	
Milk	30	
Vinegar	28	
Sea water	27·5	Nairne
Blood	25	
Wines	20	
Oil of turpentine	14	Morgueron
Sulphuric acid	1	Cavendish
Mercury	—39	Cavendish
Nitric acid	—45·5	Fourcroy and Vauquelin.*
Sulphuric ether	—46	

In certain circumstances liquids may be cooled down several degrees below their freezing point before they begin to congeal. The late Mr. Crichton of Glasgow observed that tin melted in a crucible may be cooled down to 438° or 4° below

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* The following melting points of alloys of tin and lead determined by M. Kupffer deserve to be recorded.

Tin.	Alloy of	Lead.	Point of fusion.
1 atom	+	1 atom	466°
2	+	1	385
3	+	1	367
4	+	1	372
5	+	1	381

The compound of easiest fusion is that of 3 atoms tin and 1 atom lead. This compound changes its volume very little when the two metals are fused together. The melting point of lead according to Kupffer is 633°, that of tin 446°. See Ann. de Chim. et de Phys. xl. 302.

its freezing point; but the instant it begins to congeal a thermometer plunged in it rises up to 442°, and continues at that point till the whole tin has become solid. I have cooled water in thermometer tubes to 8°, and once to 5°, before it began to freeze. In all such cases whenever the liquid begins to freeze a thermometer plunged in it immediately starts up to the freezing point of the liquid, and continues at that point till the whole liquid has congealed. The instant that any solid body is dropt into water cooled down below the freezing point, it begins to congeal. The nature of the solid seems to be indifferent. A pin, a grain of salt, a piece of vegetable, animal or mineral substance. Agitation or even wind, provided all solid matter be excluded, does not seem to occasion the commencement of congelation. These phenomena show an analogy between the separation of heat and elasticity. For the congelation is occasioned by the separation of a certain quantity of heat from the liquid.

Effect of salts in altering the freezing of water.

2. When salts are dissolved in water, it is well known that its freezing point is in most cases lowered. Thus sea-water does not freeze so readily as pure water. The experiments of Sir Charles Blagden have given us the point at which a considerable number of these solutions congeal. The result of his trials may be seen in the following table. The first column contains the names of the salts; the second the quantity of salt, by weight, dissolved in 100 parts of water; and the third, the freezing point of the solution.*

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* See Phil. Trans. 1788, p. 277.

Names of Salts.	Proportion.	Freezing point.
Common salt . .	25 . .	4
Sal ammoniac . .	20 . .	8
Rochelle salt . .	50 . .	21
Sulphate of magnesia	41·6 . .	25·5
Nitre . . .	12·5 . .	26
Sulphate of iron .	41·6 . .	28
Sulphate of zinc .	53.3 . .	28·6

From this table it appears that common salt is by far the most efficacious in lowering the freezing point of water. A solution of 25 parts of salt in 100 of water freezes at 4°. These solutions, like pure water, may be cooled down considerably below their freezing point without congealing; and in that case the congelation is produced by means of ice just as in common water, though more slowly.

When the proportion of the same salt held in solution by water is varied, it follows from Sir Charles Blagden's experiments, that the freezing point is always proportional to the quantity of the salt. For instance, if the addition of $\frac{1}{10}$th of salt to water lowers its freezing point 10 degrees, the addition of $\frac{2}{10}$ths will lower it 20°. Hence, knowing from the preceding table the effect produced by a given proportion of salt, it is easy to calculate what the effect of any other proportion will be. The following table exhibits the freezing points of solutions of different quantities of common salt in 100 parts of water, as ascertained by Blagden's trials, and the same points calculated on the supposition that the effect is as the proportion of salt.

Quantity of salt to 100 of water.	Freezing point by experiments.	Do. by calculation.
3·12	28+	28·5
4·16	27·5	27·3
6·25	25·5	25
10·00	21.5	20·75
12·80	18·5	17·6
16·1	13.5	14
20	9·5	9·8
22·2	7·2	7
25	4	4

Of strong acids.

3. The strong acids, namely, sulphuric and nitric, which are in reality compounds, containing various proportions of water according to their strength, have been shown by Mr. Cavendish, from the experiments of Mr. Macnab, to vary in a remarkable manner in their point of congelation according to circumstances. The following are the most important points respecting the freezing of these bodies that have been ascertained.

When these acids diluted with water are exposed to cold, the weakest part freezes, while a stronger portion remains liquid; so that by the action of cold they are separated into two portions differing very much in strength. This has been termed by Mr. Cavendish the *aqueous* congelation of these bodies.

When they are very much diluted, the whole mixture, when exposed to cold, undergoes the *aqueous* congelation; and in that case, it appears from Blagden's experiments, that the freezing point of water is lowered by mixing it with acid rather in a greater ratio than the increase of the acid.

The following table exhibits the freezing point of mixtures of various weights of sulphuric acid, of the density 1·837 (temperature 62°), and of nitric acid of the density 1.454, with 100 parts of water.

Sulphuric Acid.		Nitric Acid.	
Proportion of acid.	Freezing point.	Proportion of acid.	Freezing point.
10	24·5	10	22
20	12·5	20	10·5
25	7·5	23·4	7*

The concentrated acids themselves undergo congelation when exposed to a sufficient degree of cold; but each of them has a particular strength at which it congeals most readily. When either stronger or weaker, the cold must be increased. The following table, calculated by Mr. Cavendish from Mr. Macnab's experiments, exhibits the freezing points of nitric acid of various degrees of strength.†

Nitric acid.

Specific gravity.	Strength.	Freezing point.	Difference.
1·42	568	—45·5	+15·4
1·41	538	—30·1	+12
1·39	508	—18·1	+ 8·7
1·37	478	— 9·4	+ 5·3
1·35	448	— 4·1	+ 1·7
1·33	418	— 2·4	— 1·8
1·31	388	— 4·2	— 5·5
1.28	358	— 9·7	— 8
1·26	328	—17·7	—10
1·23	298	—27·7	

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* Phil. Trans. 1788, p. 303.

† The strength is indicated by the quantity of marble necessary to saturate 1000 parts of the acid. Phil. Trans. 1788, p. 174.

The following table exhibits the freezing points of sulphuric acid of various strengths.*

Sulphuric acid.

Specific gravity.	Strength.	Freezing point.
1·837	977	+ 1
1·827	918	—26
1·770	846	+42
1·670	758	—45

Mr. Keir had previously ascertained that sulphuric acid of the specific gravity 1·780 (at 60°) freezes most easily, requiring only the temperature of 46°. This agrees nearly with the preceding experiments, as Mr. Cavendish informs us that sulphuric acid of that specific gravity is of the strength 848. From the preceding table we see, that besides this strength of easiest freezing, sulphuric acid has another point of contrary flexure at a superior strength; beyond this, if the strength be increased, the cold necessary to produce congelation begins again to diminish.

Dr. Black's opinion.

4. Before Dr. Black began to deliver his chemical lectures in Glasgow in 1757, it was universally supposed that solids were converted into liquids by a small addition of heat after they have been once raised to the melting point, and that they returned again to the solid state on a very small diminution of the quantity of heat necessary to keep them at that temperature. An attentive view of the phenomena of liquefaction and solidification gradually led this sagacious philosopher to observe their inconsistence with the then received opinions, and to form another, which he verified by direct experi-

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* Phil. Trans. p. 181.

ments. He drew up an account of his theory, and the proofs of it, which was read to a literary society in Glasgow on April 23, 1762;* and every year after he gave a detailed account of the whole doctrine in his lectures.

That fluidity is occasioned by latent heat.

The opinion which he formed was, that when a solid body is converted into a liquid, a much greater quantity of heat enters into it than is perceptible immediately after by the thermometer. This great quantity of heat does not make the body apparently warmer, but it must be thrown into it in order to convert it into a liquid; and this great addition of heat is the principal and most immediate cause of the fluidity induced. On the other hand, when a liquid body assumes the form of a solid, a very great quantity of heat leaves it without sensibly diminishing its temperature; and the state of solidity cannot be induced without the abstraction of this great quantity of heat. Or, in other words, whenever a solid is converted into a fluid, it combines with a certain dose of heat without any augmentation of its temperature; and it is this dose of heat which occasions the change of the solid into a fluid. When the fluid is converted again into a solid, the dose of heat leaves it without any diminution of its temperature; and it is this abstraction which occasions the change. Thus the combination of a certain dose of heat with ice causes it to become water, and the abstraction of a certain dose of caloric from water causes

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* Black's Lectures, preface, p. 38.

it to become ice. Water, then, is a compound of ice and heat; and in general, all fluids are combinations of the solid to which they may be converted by cold and a certain dose of heat.

Such is the opinion concerning the cause of fluidity, taught by Dr. Black as early as 1762. Its truth was established by the following experiments:

Proved by experiment.

First, If a lump of ice, at the temperature of 22°, be brought into a warm room, in a very short time it is heated to 32°, the freezing point. It then begins to melt; but the process goes on very slowly, and several hours elapse before the whole ice is melted. During the whole of that time its temperature continues at 32°; yet as it is constantly surrounded by warm air, we have reason to believe that heat is constantly entering into it. Now as none of this heat is indicated by the thermometer, what becomes of it, unless it has combined with that portion of the ice which is converted into water, and unless it is the cause of the melting of the ice?

Dr. Black took two thin globular glasses four inches in diameter, and very nearly of the same weight. Both were filled with water; the contents of the one were frozen into a solid mass of ice, the contents of the other were cooled down to 33°; the two glasses were then suspended in a large room at a distance from all other bodies, the temperature of the air being 47°. In half an hour the thermometer placed in the water glass rose from 33° to 40°, or seven degrees: the ice was at first four or five degrees colder than melting snow; but in a few minutes the thermometer applied to it stood at 32°. The instant of time when it reached that temperature was noted, and the whole left undisturbed for ten hours and a half. At the end of that time the whole ice was melted, except a very small spongy mass, which floated at the top and disappeared in a few minutes. The temperature of the ice-water was 40°.

Thus $10\frac{1}{2}$ hours were necessary to melt the ice and raise the product to the temperature of 40°. During all this time it must have been receiving heat with the same celerity as the water glass received it during the first half-hour. The whole quantity received then was 21 times 7,=147° ; but its temperature was only 40°: therefore 139 or 140 degrees had been absorbed by the melting ice, and remained concealed in the water into which it had been converted, its presence not being indicated by the thermometer.*

That heat is actually entering into the ice, is easily ascertained by placing the hand or a thermometer under the vessel containing it. A current of cold air may be perceived descending from it during the whole time of the process.

But it will be said, perhaps, that the heat which enters into the ice does not remain there, but is altogether destroyed. This opinion is refuted by the following experiment.

Second, If, when the thermometer is at 22°, we expose a vessel full of water at 52° to the open air,

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* Black's Lectures, i. 120.

and beside it another vessel full of brine at the same temperature, with thermometers in each ; we shall find that both of them gradually lose heat, and are cooled down to 32°. After this the brine (which does not freeze till cooled down to 4°) continues to cool without interruption, and gradually reaches 22°, the temperature of the air ; but the pure water remains stationary at 32°. It freezes, indeed, but very slowly ; and during the whole process its temperature is 32°. Now, why should the one liquid refuse all of a sudden to give out heat, and not the other ? Is it not much more probable that the water, as it freezes, gradually gives out the heat which it had absorbed during its liquefaction ; and that this evolution maintains the temperature of the water at 32°, notwithstanding what it parts with to the air during the whole process ? We may easily satisfy ourselves that the water while congealing is constantly imparting heat to the surrounding air ; for a delicate thermometer suspended above it is constantly affected by an ascending stream of air less cold than the air around.* The following experiment, first made by Fahrenheit, and afterwards often repeated by Dr. Black and others, affords a palpable evidence, that such an evolution of heat actually takes place during congelation.

Third, If, when the air is at 22°, we expose to it a quantity of water in a tall beer glass, with a thermometer in it and covered, the water gradually

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* Black's Lectures, i. 127.

Chap. II. cools down to 22° without freezing. It is therefore 10° below the freezing point. Things being in this situation, if the water be shaken, part of it instantly freezes into a spongy mass, and the temperature of the whole instantly rises to the freezing point; so that the water has acquired ten degrees of heat in an instant. Now, whence came these ten degrees? Is it not evident that they must have come from that part of the water which was frozen, and consequently that water in the act of freezing gives out heat?

From a good many experiments which I have made on water in these circumstances, I have found reason to conclude, that the quantity of ice which forms suddenly on the agitation of water, cooled down below the freezing point, bears always a constant ratio to the coldness of the liquid before agitation. Thus I find that when water is cooled down to 22°, very nearly $\frac{1}{14}$ of the whole freezes;* when the previous temperature is 27°, about $\frac{1}{28}$ of the whole freezes. I have not been able to make satisfactory experiments in temperatures lower than 22°; but from analogy I conclude, that for every five degrees of diminution of temperature below the freezing point, without congelation, $\frac{1}{28}$ of the liquid freezes suddenly on agitation. Therefore, if water could be cooled down 28 times five degrees below 32° without congelation, the whole would congeal instantaneously on agitation, and the temperature of the ice would be 32°. Now it deserves attention

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* A medium of several experiments.

that $5 \times 28 = 140$, gives us precisely the quantity of heat which, according to Dr. Black's experiments, enters into ice in order to convert it into water. Hence it follows, that in all cases when water is cooled down below 32°, it loses a portion of the heat which is necessary to constitute its liquidity. The instant that such water is agitated, one portion of the liquid seizes upon the quantity of caloric in which it is deficient at the expense of another portion, which, of course, becomes ice. Thus when water is cooled down to 22°, every particle of it wants 10° of the heat necessary to keep it in a state of liquidity. Thirteen parts of it seize ten degrees each from the fourteenth part. These thirteen of course acquire the temperature of 32°; and the other part being deprived of $10 \times 13 = 130$, which, with the ten degrees that it had lost before constitute 140°, or the whole of the heat necessary to keep it fluid, assumes of consequence the form of ice.

Fourth, If these experiments should not be considered as sufficient to warrant Dr. Black's conclusion, the following, for which we are indebted to the same philosopher, puts the truth of his opinion beyond the reach of dispute. He mixed together given weights of ice at 32° and water at 190° of temperature. The ice was melted in a few seconds, and the temperature produced was 53°. The weight of the ice was 119 half-drams;

That of the hot water . .	13
of the mixture . . .	254
of the glass vessel . .	16

Sixteen parts of glass have the same effect in heating cold bodies as eight parts of equally hot water. Therefore, instead of the 16 half-drams of glass, eight of water may be substituted, which makes the hot water amount to 143 half-drams.

In this experiment there were 158 degrees of heat contained in the hot water to be divided between the ice and water. Had they been divided equally, and had the whole been afterwards sensible to the thermometer, the water would have retained $\frac{143}{262}$ parts of this heat, and the ice would have received $\frac{119}{262}$ parts. That is to say, the water would have retained 86°, and the ice would have received 72°: and the temperature after mixture would have been 104°. But the temperature by experiment is found to be only 53°; the hot water lost 137°, and the ice only received an addition of temperature equal to 21°. But the loss of 18° of temperature in the water is equivalent to the gain of 21° in the ice. Therefore $158° - 18° = 140°$ of heat have disappeared altogether from the hot water. These 140° must have entered into the ice, and converted it into water without raising its temperature.[*]

In the same manner, if we take any quantity of ice, or (which is the same thing) snow at 32°, and mix it with an equal weight of water at 172°, the snow instantly melts, and the temperature of the mixture is only 32°. Here the water is cooled 140°, while the temperature of the snow is not increased at all; so that 140° of heat have disappeared.

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[*] Black's Lectures, i. 123.

They must have combined with the snow; but they have only melted it without increasing its temperature. Hence it follows irresistibly, that ice, when it is converted into water, absorbs and combines with heat.

It is rather difficult to ascertain the precise number of degrees of heat that disappear during the melting of ice. Hence different statements have been given. Mr. Cavendish, who informs us that he discovered the fact before he was aware that it was taught by Dr. Black, states them at 150°*; Wilke at 130°; Black at 140°; and Lavoisier and Laplace, at 135°. The mean of the whole is very nearly 140°.

Latent heat of water.

Water, then, after being cooled down to 32°, cannot freeze till it has parted with 140° of heat; and ice, after being heated to 32°, cannot melt till it has absorbed 140° of heat. This is the cause of the extreme slowness of these operations. With regard to water, then, there can be no doubt that it owes its fluidity to the caloric which it contains, and that the caloric necessary to give fluidity to ice is equal to 140°.

To the quantity of heat which thus occasions the fluidity of solid bodies by combining with them, Dr. Black gave the name of *latent heat*, because its presence is not indicated by the thermometer: a term sufficiently expressive, but other philosophers have rather chosen to call it *caloric of fluidity*.

Dr. Black and his friends ascertained also, by

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* Phil. Trans. 1733, p. 313.

experiment, that the fluidity of melted wax, tallow, spermaceti, and metals, is owing to the same cause. Landriani proved that this is the case with sulphur, alum, nitre, and several of the metals;* and it has been found to be the case with every substance hitherto examined. We may consider it therefore as a general law, that whenever a solid is converted into a fluid, it combines with heat, and that this is the cause of its fluidity.

Latent heat of other bodies.

5. The only experiments to determine the latent heat of other bodies besides water, that have been hitherto published, are those of Dr. Irvine† and his son Mr. William Irvine.‡ The following table exhibits the result of their trials.

Bodies.	Latent heat.	Do. reduced to the specific heat of water.
Sulphur . .	143·68 . .	27·14
Spermaceti . .	145	
Lead . . .	162 . .	5·6
Bees wax . .	175	
Zinc . . .	493 . .	48·3
Tin . . .	500 . .	33
Bismuth . .	550 . .	23·25

The latent heat of spermaceti, wax, and tin, were determined by Dr. Irvine; that of the rest by his son. The latent heat in the second column expresses the degrees by which it would have increased the temperature of each of the bodies respectively when solid, except in the case of spermaceti and wax; in them it expresses the

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* Jour. de Phys. xxv.

† Black's Lectures, i. 187.

‡ Nicholson's Jour. ix. 45.

increase of temperature which would have been produced upon them while fluid.

6. Dr. Black has rendered it exceedingly probable also, or rather he has proved by his experiments and observations, that the softness of such bodies as are rendered plastic by heat depends upon a quantity of latent heat which combines with them. Metals also owe their malleability and ductility to the same cause. Hence the reason that they become hot and brittle when hammered.

It is greatly to be regretted that this interesting subject has not been farther prosecuted. Meanwhile the knowledge we have already gained will enable us to account for many important natural phenomena. It will be worth while to give a few examples.

Freezing mixtures explained.

1. It is well known that there are certain substances which when mixed together induce a considerable cold. These substances are used for the purpose under the name of *freezing mixtures*. *Snow* and *salt* are two such substances. If we mix them together in about equal quantities in a glass or stone ware vessel, a thermometer plunged into the mixture sinks down to zero and continues at this low temperature for a considerable time. There is reason to believe that this important experiment, which has added so much to the enjoyment of the rich in hot climates, was first made by Sanctorio about the close of the 16th century. Nitre and snow had been already used as a freezing mixture by the Italian pastry-cooks. But Sanctorio informs us in his commentary on Avicenna that he pro-

duced the same effect by substituting common salt for nitre, in the proportion of three parts snow to one part of salt; and that he had repeatedly performed the experiment before numerous audiences.

A still greater diminution of temperature is produced by pouring nitric acid previously diluted with water upon snow. And when dry snow and dry chloride of calcium (retaining its water of crystallization) in the proportion of one part of the former to two parts of the latter are mixed, so great a cold is produced that mercury may easily be frozen by it even in a warm room.

The first person who made experiments on freezing mixtures was Fahrenheit. But the subject was much more completely investigated by Mr. Walker in various papers published in the Philosophical Transactions from 1787 to 1801. Several curious additions have been made by Professor Lowitz. The experiments of Lowitz have been repeated and extended by Mr. Walker.* The result of all these experiments may be seen in the following tables, which I transcribe from a paper with which I have been favoured by Mr. Walker.

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* Phil. Trans. 1801, p. 120.

TABLE I.—*Frigorific Mixtures without Ice.*

Mixtures.	Parts.	Thermometer sinks.	Degree of cold produced.
Muriate of ammonia Nitrate of potash . . Water	5 5 16	From + 50° to + 10°.	40
Muriate of ammonia Nitrate of potash . . Sulphate of soda . . Water	5 5 8 16	From + 50° to + 4°.	46
Nitrate of ammonia Water	1 1	From + 50° to + 4°.	46
Nitrate of ammonia Carbonate of soda . Water	1 1 1	From + 50° to — 7°.	57
Sulphate of soda . Diluted nitric acid .	3 2	From + 50° to — 3°.	53
Sulphate of soda . . Muriate of ammonia Nitrate of potash . Diluted nitric acid .	6 4 2 4	From + 50° to — 10°.	60
Sulphate of soda . Nitrate of ammonia Diluted nitric acid .	6 5 4	From + 50° to — 14°.	64
Phosphate of soda . Diluted nitric acid .	9 4	From + 50° to — 12°.	62
Phosphate of soda . Nitrate of ammonia Diluted nitric acid .	9 6 4	From + 50° to — 21°.	71
Sulphate of soda . Muriatic acid . .	8 5	From + 50° to 0°.	50
Sulphate of soda . Diluted sulphuric acid	5 4	From + 50° to + 3°.	47

N. B. If the materials are mixed at a *warmer* temperature than that expressed in the table, the effect will be proportionally *greater*; thus if the most powerful of these mixtures be made, when the air is + 85°, it will sink the thermometer to + 2°.

TABLE II.—*Frigorific Mixtures with Ice.*

Mixtures.	Parts.	Thermometer sinks.	Degree of cold produced.
Snow, or pounded ice Muriate of soda . .	2 1	*From any Temperature.* to — 5°.	*
Snow, or pounded ice Muriate of soda . . Muriate of ammonia	5 2 1	to — 12°.	*
Snow, or pounded ice Muriate of soda . . Muriate of ammonia Nitrate of potash .	24 10 5 5	to — 18°.	*
Snow, or pounded ice Muriate of soda . . Nitrate of ammonia	12 5 5	to — 25°.	*
Snow . . . . Diluted sulphuric acid	3 2	From + 32° to — 23°.	55
Snow . . . . Muriatic acid . .	8 5	From + 32° to — 27°.	59
Snow . . . . Diluted nitric acid .	7 4	From + 32° to — 30°.	62
Snow . . . . Muriate of lime . .	4 5	From + 32° to — 40°.	72
Snow . . . . Cryst. muriate of lime	2 3	From + 32° to — 50°.	82
Snow . . . . . . Potash	3 4	From + 32° to — 51°.	83

TABLE III.—*Combinations of Frigorific Mixtures.*

Mixtures.		Thermometer sinks.	Degree of cold produced.
	Parts.		
Phosphate of soda Nitrate of ammonia Diluted nitric acid	5 3 4	From 0° to — 34°.	34
Phosphate of soda Nitrate of ammonia Diluted mixed acids	3 2 4	From — 34° to — 50°.	16
Snow Diluted nitric acid	3 2	From 0° to — 46°.	46
Snow Diluted sulph. acid Diluted nitric acid	8 3 3	From — 10° to — 56°.	46
Snow Diluted sulphuric acid	1 1	From — 20° to — 60°.	40
Snow Muriate of lime	3 4	From + 20° to — 48°.	68
Snow Muriate of lime	3 4	From + 10° to — 54°.	64
Snow Muriate of lime	2 3	From — 15° to — 68°.	53
Snow Cryst. muriate of lime	1 2	From 0° to — 66°.	66
Snow Cryst. muriate of lime	1 3	From — 40° to — 73°.	33
Snow Diluted sulphuric acid	8 10	From — 68° to — 91°.	23

Chap. II.

In order to produce these effects, the salts employed must be fresh crystallized, and newly reduced to a very fine powder. The vessels in which the freezing mixture is made should be very thin, and just large enough to hold it, and the materials should be mixed together as quickly as possible. The materials to be employed in order to produce great cold ought to be first reduced to the temperature marked in the table, by placing them in some of the other freezing mixtures; and then they are to be mixed together in a similar freezing mixture. If, for instance, we wish to produce a cold = — 46°, the snow and diluted nitric acid ought to be cooled down to 0°, by putting the vessel which contains each of them into the first freezing mixture in the second table before they are mixed together. If a still greater cold is required, the materials to produce it are to be brought to the proper temperature by being previously placed in the second freezing mixture. This process is to be continued till the required degree of cold has been procured.*

It is sometimes a matter of convenience to produce cold when we have no snow or ice. This may be done by dissolving rapidly any salt containing much water of crystallization. Glauber salt is one of the most convenient, and dilute muriatic acid or sulphuric acid the most suitable liquid to dissolve it in. The following experiments by Professor Bischof of Bonn, will enable the reader

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* Walker, Phil. Trans. 1795.

to judge of the degree of cold which may be produced in this way.

Cold produced by dissolving sulphate of soda in dilute sulphuric acid.

	Mixture.		Sinks the thermometer.		Cold produced.
			From	To	
(1)	500 gr.	Sulphuric acid			
	500 gr.	Water			
	1250 gr.	Glauber salt	54°·5	16°·25	38·°25
(2)	500 gr.	Sulphuric acid			
	750	Water			
	1560	Glauber salt	54·5	22·44	32·06
(3)	500 gr.	Sulphuric acid			
	635	Water			
	1400	Glauber salt	54·5	20·19	34·31
(4)	500 gr.	Sulphuric acid			
	208	Water			
	885	Glauber salt	54·5	14	40·5
(5)	500 gr.	Sulphuric acid			
	500	Water			
	1250	Glauber salt	54·5	10⅝	44—
(6)	500 gr.	Sulphuric acid			
	300	Water			
	990	Glauber salt	54·5	7¼	47·25
(7)	500 gr.	Sulphuric acid			
	250	Water			
	937	Glauber salt	54·5	7¼	47·25
(8)	500 gr.	Sulphuric acid			
	500	Water			
	1000	Glauber salt	54·5	7¼	47·25
(9)	500 gr.	Sulphuric acid			
	416	Water			
	1150	Glauber salt	54·5	6⅛	48·4
(10)	500 gr.	Sulphuric acid			
	333	Water			
	1040	Glauber salt	54·5	5	49·5

The acid and water must be previously mixed and allowed to cool, before the glauber salt be added. It should be in powder, but retaining all its water of crystallization.*

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* Schweigger's Iahrbuch, xxii. 370.

Theory of freezing mixtures.

All substances which produce cold on mixture, act chemically on each other. They are either both solid, or at least one of them is solid, and they begin to liquefy as soon as they are mixed. Indeed every mixtures that generates cold, contains a considerable quantity of water in a solid state, and consequently destitute of its latent heat. This is the case with snow, which is almost always a constituent of every freezing mixture. Chloride of calcium in crystals contains more than half its weight of solid water. Now whenever the two substances so mixed begin to act on each other, the tendency to liquefaction is greatly increased. The degree of cold is always proportional to the rapidity of the liquefaction. Hence snow and chloride of calcium or potash, which liquefy immediately on mixture, produce a much greater cold than snow and common salt, which liquefy very slowly. But the cold produced by the former mixtures continues a much shorter time than that produced by the latter.

In proportion as the solid bodies become liquid they must absorb latent heat. This they can only obtain from the bodies in the neighbourhood. Hence they absorb heat with avidity from all bodies in contact with them. But when a body absorbs heat from those in contact with it, we say that it is cold; and the more rapidly it absorbs heat the colder it is.

2. There are many salts which dissolve in hot water in considerably greater quantity than in cold water. If such hot solutions be set aside till they

Heat produced by sudden crystallization.

cool, the excess of salt which they contain will be deposited in crystals, and no more will remain in solution than cold water is capable of taking up. But in some rare instances it happens that if the hot solution be put into a phial and corked up and allowed to cool without agitation, no crystals will be deposited, so that we obtain a cold solution holding more salt than water of the same temperature is capable of dissolving. But the moment we draw out the cork from the phials containing them, a quantity of the salt suddenly crystallizes, and the temperature of the liquid at the same time rises. Carbonate of soda and sulphate of soda constitute two such salts.

To form a solution of carbonate of soda which may be cooled down to 50° without crystallizing in a close phial, but which deposits crystals when the cork is drawn, we have only to dissolve one ounce of anhydrous carbonate in 4·22 ounces of hot water. When such a solution is agitated by drawing the cork at the temperature of 50°, a copious precipitate of small crystals in the form of stars takes place; and the temperature of the solution is elevated 14°. The crystals deposited in this case, supposing them anhydrous, weigh 123·15 grains, the water of crystallization belonging to which is 223.6 grains. The whole weight of the liquid and phial (substituting for the glass the requisite weight of water) was 2149·5 grains. Of which 223·6 grains make $\frac{1}{9.61}$ part. Now if this water of crystallization had given out the whole of its latent heat, this evolution would have raised the temperature to 14°·96. Thus we see that the elevation of temperature in this case is owing to the latent heat given out by the water when it becomes solid by uniting to the precipitated salt.

To form a solution of sulphate of soda possessing the requisite properties, we must dissolve 51 parts of the crystals in 49 parts of hot water. When such a solution is allowed to cool in a corked phial, if we draw the cork it begins immediately to crystallize on the surface, and the crystals shoot slowly till they reach the bottom of the phial. The whole liquid appears converted into fibrous crystals. But, in reality, two-thirds of the salt are deposited in crystals, and one-third remains in solution. When the weight of the solution of sulphate of soda was 2118 grains, and that of the phial 1032 grains, the increase of temperature was 24°. The crystals of salt deposited weighed 713 grains; the water of crystallization of which amounts to 399 grains; the weight of the solution and phial are equivalent to 1752 grains of water. Now, 399 is about $\frac{1}{4\cdot39}$ of 1752. And $24° \times 4\cdot39 = 105°\cdot36$. This is $34°\frac{2}{3}$ less than the latent heat of the water of crystallization. The reason of this is, that the crystals deposited at first do not amount to 713 grains, but only to 530 grains. The evolution of heat preventing the 183 residual grains from being deposited, till the liquid cools. Now, the water of crystallization of 530 grains of the salt is only 300 grains, constituting $\frac{1}{5\cdot83}$ of the whole. Now, $24° \times 5\cdot83 = 139°\cdot92$ = very nearly to the latent heat of the water of crystallization. Thus we see that the heat evolved in these cases of crystallization is owing chiefly, if not entirely, to the water of crystallization giving out its latent heat at the instant it becomes solid.*

The reason why these two salts remain in solution, must be the strong affinity which the water has for heat. In like manner, both sulphur and phosphorus often remain liquid at the common temperature of the atmosphere; but when touched with a rod, they give out heat and become solid.

3. Iron, copper, gold, silver, and one or two other metals, are both malleable and ductile. They may be flattened under the hammer and drawn out into wire, by being squeezed through holes drilled in a steel plate. Now, when any of these metals has been hammered for some time so as to increase its surface, to a certain amount it becomes brittle. If we still continue to hammer it, instead of extending farther, it cracks and splits and breaks in pieces. To restore the malleability, we must heat the piece of metal in the fire and allow it to cool slowly. This process is called *annealing*.

Annealing explained.

Now, there is a circumstance which always accompanies the hammering of metals, or the drawing them out into wires or plates. They become hot; sometimes even red hot, and usually so hot that they burn the fingers. Thus it appears that the forcing out of heat occasions brittleness in these metals. And to restore the malleability and duc-

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* The specific heat of a saturated solution of carbonate of soda is about 0·75, that of a saturated solution of sulphate of soda about 0·73 The specific heat of the phial was reckoned 0·2.

tility, we must heat them artificially, and allow them to cool slowly; that is to say, we must again restore the heat which had been forced out. Thus malleability and ductility would seem to depend upon a certain quantity of latent heat in the metals which possess these qualities; and the process of annealing is merely the restoration of this latent heat after it has been separated. If annealing consisted in merely heating metals without adding any foreign heat, there is no visible reason why an iron rod which has been heated to redness by hammering should require annealing. For it is already as hot as is required by the annealing process. Yet, it is well known, that iron which has been made red hot, by hammering has lost its malleability, and that it cannot be hammered again till it has been annealed.

CHAPTER III.

OF VAPORIZATION.

Chap. III.

By vaporization is meant the property which heat has of changing liquid bodies into *vapours.* I shall divide this chapter into three sections. In the first we shall treat of *vapours,* in the second of *gases,* and in the third of *spontaneous evaporation.*

SECTION I. OF VAPOURS.

Nature of vapour.

Vapour is an extremely rare, light, and expansible body, like *air;* and capable, like it, of being easily reduced into less space by external pressure, and resisting, like it, the force which thus compresses it. Suppose a single teaspoonful of water to be put into a glass globe capable of holding several gallons, and exhausted of air. If heat enough be applied to the globe, the water gradually diminishes in bulk, and at last disappears, so that the vessel will appear empty. Yet it is completely filled with the water now existing in the state of transparent and invisible vapour. By increasing the heat, we increase the expansive force of this vapour; and we may increase it till it burst the globe and shatter it in pieces.

1. A glass jar capable of holding 100 cubic inches being filled with mercury, and placed inverted

into a basin of mercury, if we let up into it about 19 grains of water, and raise the temperature to 212° the water will disappear, being converted into vapour, and the mercury will at the same time sink to the bottom of the jar, the 19 grains of water converted into vapour occupying the volume of 100 cubic inches. The bulk into which water expands by the ordinary boiling heat, producing a steam equally elastic with common air, is very great. But it was very much overrated at first. Dr. Desaguliers reckoned the expansion 14,000 times the original bulk. Mr. Watt was the first person who determined it with an approach to accuracy. From his experiments he drew, as a consequence, that water when converted into steam increases in bulk 1800 times. From the experiments of Guy-Lussac it appears that the specific gravity of steam is 0·625, that of air being 1. If, therefore, 100 cubic inches of air weigh 31·1446 grains, as follows from the experiments of Biot and Arago, it is obvious that the same volume of steam will weigh 19·465 grains. Now a cubic inch of water at 60° weighs 252·52 grains. It is obvious that 252·52 grains of water, when converted into steam, would occupy 1297·3 cubic inches, which, therefore, should be the true increase of volume when water is converted into steam. But if 0.625 be the specific gravity of steam at 212° compared with that of air, reckoned 1 at the same temperature, then the expansion will be greater, and will approach nearly to the determination of Mr. Watt. For on that supposition the specific gravity of steam at 212° compared with air, reck-

Dilatation of water when converted into steam.

oned 1 at 60° would be only 0·481. So that 100 cubic inches of it would weigh 14·98 grains. And a cubic inch of water, when converted into steam, would expand into 1689 cubic inches.

This change, like fluidity, is produced by the increase of the temperature of the body so changed. Now the temperature at which the change takes place is found to differ in almost every kind of matter. The temperature must be kept up, in order that the vaporous form may remain. If the heat be reduced the vapour loses its elasticity, and collapses into that form from which it was produced. Thus steam whenever it impinges against a cold body, is immediately condensed again into water.

Volatile and fixed bodies.

2. The *vaporific* or *boiling* point of most bodies is higher than that which is necessary to give them liquidity; but to this there are some exceptions. Thus arsenic and benzoic acid are converted into vapours at a lower temperature than is required to melt them. Bodies easily converted into vapour, are called *volatile;* while those are called *fixed,* which require a high temperature to induce this change, or which cannot be converted into vapour, by the greatest heat which we have it in our power to raise.

Boiling point of liquids.

3. The temperature at which bodies are converted into vapour is usually called their *boiling point.* This point is very different in different liquids; but, other things remaining the same, it is constant for every particular liquid. The following table exhibits the boiling points of a few liquids, in which that point has been determined with precision.

	Boiling point.
Muriatic ether	52°
Sulphuric ether	96
Bisulphuret of carbon	116
Ammonia (sp. gr. 0·945)	140
Oil of laurel*	150
Acetic ether	160
Alcohol (sp. gr. 0·798)	173
Water	212
Nitric acid (sp. gr. 1·42)	248
Crystallized muriate of lime	302
Oil of turpentine	314
Naphtha	320
Phosphorus	554
Sulphuric acid (sp. gr. 1·843)	620
Whale oil	630
Mercury	668

The boiling points of the following saline solutions have been determined by Mr. Griffith.†

Of saline solutions.

	Salt in 100 of the solution.	Boiling point.
Sulphate of soda	31·5	213°
Corrosive sublimate	——	214
Cyanuret of mercury	3·5	214
Acetate of copper	16·5	214
Bitartrate of potash	9·5	214
Nitrate of barytes	26·5	214
Sulphate of potash	17·5	215

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[*] This is a volatile oil which comes from Demerara. It is obtained by tapping some unknown tree in the interior of South America. It is a volatile oil holding a resin in solution, and begins to boil, I find, at 150°. But the temperature rapidly rises as the boiling proceeds. Showing the presence of a number of oils differing in their volatility.

[†] Journal of Science, xviii. 90.

	Salt in 100 of the solution.	Boiling point.
Acetate of lead	41·5	215°
Nitrate of lead	52·5	216
Protosulphate of iron	64	216
Sulphate of copper	45	216
Potash-sulphate of copper	40	217
Boracic acid	—	218
Chlorate of potash	40	218
Prussiate of potash	55	218
Oxalate of ammonia	29	218
Oxalate of potash	40	220
Alum	52	220
Sulphate of zinc	45	220
Muriate of barytes	45	220
Carbonate of soda	—	220
Phosphate of soda	—	222
Borax	52·5	222
Bisulphate of potash	——	220
Sulphate of magnesia	57·5	222
Nitrate of strontian	53	224
Common salt	30	224
Tartrate of potash	68	234
Sulphate of nickel	65	235
Sal ammoniac	50	236
Nitrate of potash	74	238
Potash-tartrate of soda	90	240
Nitrate of soda	60	246
Acetate of soda	60	256
Muriate of lime (saturated)	—	264*

Boiling point affected by the height of the barometer.

4. The boiling point of liquids is not quite so steady as the melting or freezing point; for it is affected by the degree of pressure to which the liquid is exposed. This was first observed by

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* By my determination.

Chap. III. Boyle while engaged in his experiments with the air-pump. Fahrenheit afterwards noticed that the boiling point of water, examined by the same thermometer, differs at different times. And he found that this depended upon the height of the barometer. When the barometer stands high the boiling point is a little higher than usual, while it is a little lower than usual when the barometer is low. The knowledge of this fact induced him to attend to the height of the barometer when he graduated his thermometers. The determination of General Roy has been generally adopted in this country, as upon the whole the most correct. The following table shows the temperature at which water boils (according to him) at the different heights of the barometer.

Barometer.	Boiling point.
26 inches	204°.91
26.5	205.79
27	206.67
27.5	207.55
28	208.43
28.5	209.31
29	210.19
29.5	211.07
30	212
30.5	212.88
31	213.76

From this table it appears that for every half inch of variation in the barometer, the boiling point of water varies 0.88 of a degree. Consequently every tenth of an inch that the barometer rises or falls either raises or lowers the boiling point of water by 0.176 of a degree.

The mean height of the barometer at the sea shore is 29.82 inches. In this country it seldom rises much higher than 31 inches, or sinks much lower than $28\frac{1}{2}$ inches. Since the year 1817 to 1829 (both inclusive) it has never been higher in Glasgow (at about 50 feet above the level of the sea) than 30.8 inches, nor lower than 28.417 inches. So that in Glasgow the boiling point of water varies from $213^{\circ}\frac{3}{4}$ to $209^{\circ}\frac{1}{3}$, or almost $4\frac{1}{2}$ degrees of Fahrenheit.

Elasticity of steam increased by heat in close vessels.

5. When water is heated in close vessels and cannot make its escape till it overcome a considerable pressure, its boiling point may be made to rise to a much higher temperature than 212°. Papin, an ingenious French physician, who resided in London, contrived a vessel for this purpose, to which he gave the name of *digester*. It was a cylindrical copper vessel, having a lid nicely fitted to it and kept fast by screws. If this vessel be half filled with water, and the lid screwed down tight, and if it be then set upon burning coals, a portion of the water is soon converted into *steam*. This conversion begins at the boiling point of water. But the elastic vapour being confined, presses upon the surface of the water, and thus prevents the conversion of any more of it into steam, till the temperature of the water rises above the boiling point. This heat being conveyed to the steam, it now becomes capable of bearing the addition of another portion of vapour without being condensed into water. Thus the quantity, and consequently the elasticity of the steam is continually increasing with

the temperature of the water. By this contrivance the temperature of water has been raised in close vessels as high as 419°. Muschenbroek even assures us that he raised the temperature of water in digesters high enough to melt tin. Now tin fuses at the temperature of 442°.

The elasticity of steam at 419°, is 1050 times greater than that of atmospheric air; so that it exerts a force equivalent to 14,700 lbs. upon every square inch of the inside of the vessel in which it is confined. This pressure is so enormous, that few vessels can be made strong enough to withstand it. Dreadful accidents have taken place in consequence of the bursting of such vessels. It was to guard against the possibility of these, that what is known by the name of the *safety valve* was added to the digester. This consists of an opening made in the digester to let out the steam when it becomes too elastic to be confined, without hazarding the bursting of the vessel. This opening is covered with a metal plate, ground flat so as to fit it exactly. This mouth piece is kept down by a lever, to the extremity of which is fixed a weight. This weight is so contrived as to exert a force capable of being overcome before the elasticity of the steam be great enough to burst the vessel. Suppose the surface of the safety valve to be a square inch. If the weight exert an action equivalent to 15 lbs. the elasticity of the steam must be double that of the atmosphere, or its temperature must be 250° before it forces open the safety valve.

Safety valve.

6. When the steam of boiling water comes in con-

Sect. I.

Why high pressure steam does not scald.

tact with any part of the living body, it occasions instantly a most severe scald; but when steam from water of a higher temperature than boiling water or *high pressure steam*, as it is called, issues into the atmosphere, the finger or any part of the body may be passed through it with impunity. It has not the property of scalding. And if a thermometer be put into it, we find the temperature greatly below that of boiling water. So that high pressure steam has a much *lower* temperature than *low* pressure steam, or steam issuing freely from boiling water.

Whoever has an opportunity of seeing these two different species of steam, will find no difficulty in understanding the reason of this difference. When steam issues from the spout of a boiling tea-kettle, it is at first invisible, and it is not till it has advanced some distance in the air, that it begins to assume the appearance of a visible cloud. But condensed steam is visible the instant that it issues from the mouth of the pipe. The high pressure steam (supposing its elasticity double) occupies only half the bulk of common steam. The moment it comes into the atmosphere its volume is doubled. This occasions a prodigious increase in its capacity for heat, and at the same time mixes it with the cold atmospheric air. These two circumstances sink its temperature so low, that it is no longer capable of scalding.*

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* It is obvious that when high pressure steam is conveyed in pipes, it will undergo an expansion or condensation in proportion as the diameter of the pipe increases or diminishes. Accordingly

Boiling point in vacuo.

When the pressure of the atmosphere is withdrawn, the temperature at which liquids boil becomes much lower. From the experiments of the late Dr. John Robison of Edinburgh, it appears that by removing the pressure of the atmosphere, the boiling point of liquids is lowered about 145°. Hence, in a good vacuum sulphuric ether will boil at the temperature of —49°, alcohol will boil at 28°, water at 67°, and sulphuric acid at 460°. Mr. Barry of London, has applied his knowledge of this fact to the preparation of volatile oils, and the various inspissated juices of plants used in medicine. The still is connected with a globular copper receiver of large size, which is filled with steam to the exclusion of the atmospheric air. This steam being condensed and the communication between the still and receiver being opened, the air in the still divides itself equally over the still and receiver. The receiver being 5 times the size of the still, it is obvious that by this first condensation of steam, the density of the air within the apparatus is reduced to $\frac{1}{5}$. Another repetition of the same process will reduce the density of the air within the apparatus to $\frac{1}{25}$, and a third to $\frac{1}{125}$. And it is obvious, that by continuing the repetitions long enough, any degree of exhaustion whatever might be obtained. I have seen water boiling briskly in Mr. Barry's apparatus at the temperature of 98°.

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its temperature will be found to vary according to the diameter of the pipe. Upon this subject, the reader may consult a very curious set of experiments by Peter Ewart, Esq. in the Philosophical Magazine (new series), vol. v. p. 247.

Refining sugar.

This method of boiling in vacuo was applied very ingeniously by Mr. Edward Howard, to the refining of sugar. The syrup is thus concentrated to the granulating point without any risk of burning or decomposing any of the sugar by too much heat. This process is now very generally followed by the sugar refiners of Great Britain. The sugar refined in this way is beautifully white, and the loss sustained during the process is greatly diminished.

Steam engine.

As steam possesses the elasticity of air, and as it may be immediately condensed by the application of cold, it is obvious that it may be immediately applied as a moving force, and that it possesses in this way almost unlimited power. The medium in which it is so applied, is familiarly known by the name of *steam engine*, and constitutes the finest present ever made by science to the arts. It is admitted to have been invented by the Marquis of Worcester; though it does not appear that he was ever able to interest the public in his invention, or that he attempted to apply it to any useful purpose.

It was reinvented by Captain Savery, who took out a patent towards the end of the 17th century, and published an account of it in the year 1696, in a book entitled the *Miner's Friend.* In Savery's machine, the elasticity of steam was applied directly to force water up a pipe. The waste of steam was so enormous, and the quantity of fuel necessary so great, that it does not appear ever to have been attempted to apply it directly for the purpose of draining water out of mines, which was the object that Savery had in view when he took out his patent.

In 1705, a new patent was taken out jointly by Captain Savery, Mr. Newcomen, a blacksmith at Dartmouth in Devonshire, and Mr. Crawley, a glazier, in the same place. The merit of the machine has been universally ascribed to Newcomen, under whose name it always went. It consisted essentially of a metallic cylinder, in which a piston, made air tight, was capable of moving from the top to the bottom. The top of the cylinder was open, the bottom close. The piston was attached to the piston rod or chain, which connected it with the end of the working beam. The working beam was supported on a gudgeon, and the end opposite to that to which the piston rod was attached was loaded, and of course the pump rod attached to it, was at the bottom of the well from which the water was to be pumped. The cylinder is filled with steam, till all the air is driven out. The piston was at the top of the cylinder. The steam in the cylinder is condensed by means of a jet of cold water. A vacuum is produced in the cylinder. The atmosphere presses upon the top of the piston, and forces it to the bottom of the cylinder. The pump rod at the other end of the working beam is drawn up. It makes a stroke, and a quantity of water is pumped out of the well or mine. Steam is again introduced below the piston in the cylinder, the vacuum is removed and the piston rises to the top in consequence of the load at the other extremity of the working beam. The cylinder is filled with steam as before; this steam is condensed, the piston is forced down, more water is pumped up, and thus the machine continues to act as long as it is supplied with steam.

The great improvement in the steam engine was made by Mr. Watt. He contrived to condense the steam in a separate pipe, and by this means prevented the cylinder from being cooled by the injection of cold water. This produced an enormous saving of fuel. He substituted steam for the pressure of the atmosphere in Newcomen's engine, and made the machine to act both when the piston descended and ascended in the cylinder. This alteration, together with the mode of producing a circular motion, which he contrived, enabled him to apply the steam engine as a power for moving machinery. It is now used in Great Britain to a prodigious extent, and has carried the manufactures of the country to a pitch which it was impossible to have anticipated. It has of late been applied to move vessels in the water, independent of wind or tide, and even against those powers on which formerly the motions of ships entirely depended. Thus it is likely to introduce as great an alteration by sea as it has done by land. But it is not our province to enter upon any description of this wonderful machine. Abundance of descriptions of it have been laid before the public, to which we beg leave to refer the reader.

Elasticity of vapour below 212.

7. Water is converted into vapour at all temperatures, even at 32° or lower. But the elasticity at low temperatures is low, and it increases as the temperature increases, till at 212° it is equal to that

of the atmosphere or capable of supporting a column of mercury 30 inches in height.

As steam is employed in the steam engine as a moving force, it becomes a question of considerable importance to determine its elasticity at different temperatures, because the knowledge of this will put it in the power of engineers to determine the temperature at which it may be most economically employed, and the strength of the vessels which will be necessary when steam is used at elevated temperatures. The first set of experiments on this subject, approaching to accuracy, was made by Professor John Robison, of Edinburgh, and inserted by him in the article *Steam*, in the third edition of the Encyclopædia Britannica, published about the year 1797. Mr. Dalton published a still more accurate set of experiments in 1802.*

He determined the elasticity of steam from 32° to 212° by experiment; but the elasticities below 32° and above 212° were inferred from observing the rate at which the elasticities determined by experiment increased. Unfortunately the application of empirical formulæ never gives results which can be depended on, unless they be confined within the limits of the experiments. Dr. Ure, of Glasgow, made an elaborate set of experiments on the same subject in 1816, the result of which is published in the Philosophical Transactions for 1818.† Mr. Southern had made a set of experiments on the

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* Manchester Memoirs, v. 559.

† Page 338.

same subject in 1803, and carried them to considerably higher temperatures than Dr. Ure had it in his power to do. These experiments have been given to the public by Mr. Watt.* They seem to have been made with great care, and are therefore of considerable importance.

The results of all these experimenters may be seen in the following table.

Table of the elasticity of steam at different temperatures.

I.—*Elasticity of Vapour below* 32° *according to Dalton.*

Temperature.	Force of vap. in inches of mercury.
-40°	0·013
-30	0·020
-20	0·030
-10	0·043
0	0·064
1	0·066
2	0·068
3	0·071
4	0·074
5°	0·076
6	0·079
7	0·082
8	0·085
9	0·087
10	0·090
11	0·093
12	0·096
13	0·100
14°	0·104
15	0·108
16	0·112
17	0·116
18	0·120
19	0·124
20	0.129
21	0·134
22	0·139
23°	0·144
24	0·150
25	0·156
26	0·162
27	0·168
28	0·174
29	0·180
30	0·186
31	0·193

Temp.	Force of vapour in inches of mercury.			
	Robison.	Dalton.	Ure.	Southern.
32°	0·0	0·200	0·200	0·16
33		0·207		
34		0·214		
35		0·221		
36		0·229		
37		0·237		
38		0·245		
39		0·254		
40	0·1	0·263	0·250	
41		0·273		
42		0·283		0·23
43		0·294		
44		0·305		
45		0·316		
46°		0·328		
47		0·339		
48		0·351		
49		0·363		
50	0·2	0·375	0·360	
51		0·388		
52		0·401		0·35
53		0·415		
54		0·429		
55		0·443	0·416	
56		0·458		
57		0·474		
58		0·490		
59		0·507		

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* They constitute an Appendix to Mr Watt's edition of Professor Robison's articles *Steam* and *Steam Engines*.

TABLE *continued.*

	Force of vap. in inches of mercury.					Force of vap. in inches of mercury.			
Temp.	Robi-son.	Dalton.	Ure.	South-ern.	Temp.	Robi-son.	Dalton.	Ure.	South-ern.
60°	0·35	0·524	0·516		115°		2·92	2·810	
61		0 542			116		3·00		
62		0·560		0·52	117		3·08		
63		0·578			118		3·16		
64		0·597			119		3‘25		
65		0·616	0·630		120	3·0	3·33	3·300	
66		0·635			121		3·42		
67		0·655			122		3·50		3·58
68		0·676			123		3·59		
69		0·698			124		3·69		
70	0·55	0·721	0·726		125		3·79	3·830	
71		0·745			126		3·89		
72		0·770		0·73	127		4·00		
73		0·796			128		4·11		
74		0·823			129		4·22		
75		0.851	0·860		130	3·95	4·34	4·366	
76		0·880			131		4·47		
77		0·910			132		4·60		4·71
78		0·940			133		4·73		
79		0·971			134		4·86		
80	0·82	1·00	1·010		135		5·00	5·070	
81		1·04			136		5·14		
82		1·07		1·02	137		5·29		
83		1·10			138		5·44		
84		1·14			139		5·59		
85		1·17	1·170		140	5·15	5·74	5·770	
86		1·21			141		5·90		
87		1·24			142		6·05		6·10
88		1·28			143		6·21		
89		1·32			144		6·37		
90	1·18	1·36	1·360		145		6·53	6·600	
91		1·40			146		6·70		
92		1·44		1·42	147		6·87		
93		1·48			148		7·05		
94		1·53			149		7·23		
95		1·58	1·640		150	6·72	7·42	7·530	
96		1·63			151		7·61		
97		1·68			152		7·81		7·90
98		1·74			153		8·01		
99		1·80			154		8·20		
100	1·6	1·86	1·860		155		8·40	8·500	
101		1·92			156		8·60		
102		1·98		1·96	157		8·81		
103		2·04			158		9·02		
104		2·11			159		9·24		
105		2·18	2·100		160	8·65	9·46	9·600	
106		2·25			161		9·68		
107		2·32			162		9·91		10·05
108		2·39			163		10·15		
109		2·46			164		10·41		
110	2·25	2·53	2·456		165		10·68	10·800	
111		2·60			166		10·96		
112		2·68		2·66	167		11·25		
113		2·76			168		11·54		
114		2·84			169		11·83		

TABLE *continued.*

Temp.	Force of vap. in inches of mercury.				Temp.	Force of vap. in inches of mercury.				
	Robi-son.	Dalton.	Ure.	South-ern.		Robi-son.	Dalton.	Ure.	South-ern.	Prony &c.*
170°	11·05	12·13	12·050		224°		37.53			
171		12·43			225		38 20	39.110		
172		12·73		12·72	226		38.89			
173		13·02			226.3			40·100		
174		13·32			227		39.59			
175		13·62	13·550		228		40.30			
176		13·92			229		41.02			
177		14·22			230	44.5	41.75	43·100		
178		14·52			230.5			43·500		
179		14·83			231		42.49			
180	14·05	15·15	15·160		232		43.24			
181		15·50			233		44.00			45
182		15·86		16·01	234		44.78			
183		16.23			234.5			46.800		
184		16.61			235		45·58	47.220		
185		17.00	16·900		236		46 39			
186		17.40			237		47·20			
187		17.80			238		48·02			
188		18.20			238.5			50·30		
189		18.60			239		48·84			
190	17.85	19.00	19·000		240	54.9	49·67	51.70		
191		19.42			242			53·60		
192		19.86		20·04	245		53·88	56·34		
193		20.32			248.5			60·40		
194		20.77			250	66.8	58·21	61·90	60·00	
195		21.22	21·100		251.6					60
196		21.68			255		62·85	67·25		
197		22.13			260	80.3	67·73	72·30		
198		22.69			264.2					75
199		23.16			265		72·76	78·04		
200	22.62	23.64	23·600		270	94.1	77·85	86·30		
201		24.12			275		83·13	93·48		90
202		24.61		24.61	280	105.9	88·75	101·90		
203		25.10			285		94·35			
204		25.61			285.2			112·20		105
205		26.13	25·900		290		100.12	120 15		
206		26.66			293.4				120·00	120
207		27.20			295		105·97	129·00		
208		27.74			300		111·81	139·70		
209		28.29			302					135
210	28.65	28.84	28·880		305		117·68	150·56		
211		29.41			309.2					150
212		30.00	30·000	30.00	310		123·53	161·30		
213		30.60			312			166.25		
214		31.21			316.4					165
215		31.83			320		135·00			
216		32.46			322.7					180
216.6			33·40		328.46					195
217		33.09			330					
218		33.72			334.4					210
219		34.35			339.26					225
220	35.8	34.99	35·540		340					
221		35.63			343.4					240
221.6			36·700		343.6				240·00	
222		36.25			419					1050†
223		36.88								

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* Ann des Mines, x. 73. The numbers in this column seem to have been obtained by calculation.

† Perkins.

We see that the elasticity increases at a very rapid rate with the temperature. Attempts have been made to represent this increase by empirical formulas, so as to enable us to calculate the elasticity of steam for any given temperature. But such formulas, from the imperfection of the data on which they are founded, cannot be accurate, and indeed do not apply to higher temperatures than those deduced from experiment. They are therefore of little use; and indeed are very apt to mislead. I think it better therefore to omit them.

Vapours from other liquids.

8. Water is not the only liquid whose vapour has been examined with care. There are seven others whose boiling points, and the specific gravity of whose vapours, have been determined almost entirely by the sagacity and industry of M. Gay-Lussac. The following table exhibits the boiling points and specific gravities of the vapours of these various liquids.

	Sp. gravity, air being 1.	Boiling point.
Water . .	0·6235* . . 0·6250†	212°
Hydrocyanic vapour	0·9476* . .	79·7
Alcohol . .	1·6030* . .	173
Muriatic ether .	2·219‡ . .	52
Sulphuric ether .	2·586* . . 2·5808†	96
Bisulphuret of carbon	2·6447* . . 2·6386†	116
Oil of turpentine .	5·013* . .	314
Hydriodic ether .	5·4749* . .	148

* Gay-Lussac, Ann. de Chim. xci. 95, 150.—Ann. de Chim. et de Phys. i. 218.

† Despretz, Ann. de Chim. et de Phys. xxi. 143.

‡ Thenard, Mem. D'Arcueil, i. 121.

9. These specific gravities are compared to that of air, which is considered as 1, when heated to the boiling point of each liquid. They do not therefore enable us to form a correct idea of the increase of volume, which takes place when each liquid is converted into vapour. I have therefore calculated the following table, exhibiting the specific gravity of the vapour of each at the boiling point, if we reckon the specific gravity of air at 60° to be unity.

	Sp. gravity at boiling point, air at 60° being 1.
Steam	0·481
Hydrocyanic vapour	0·912
Alcohol	1·311
Muriatic ether	2·255
Sulphuric ether	2·415
Bisulphuret of carbon	2·376
Oil of turpentine	3·342
Hydriodic ether	4·666

The specific gravity of these liquids, from which these vapours are produced, is at the temperature of 60° as follows.

True specific gravity of.

	Sp. gravity.
Water	1·000
Hydrocianic acid	0·7039
Alcohol	0·798
Muriatic ether	0·874*
Sulphuric ether	0·632
Bisulphuret of carbon	1·272
Oil of turpentine	0·792
Hydriodic ether	1·9206†

By a very careful set of experiments made by MM. Biot and Arago, it has been ascertained that

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* At 41°.

† At 72°.

at the temperature of 32°, and under a pressure of 0·76 metre, 100 cubic inches of atmospherical air weigh 32·8755 grains. Hence at the temperature of 60°, and when the barometer stands at 30 inches, 100 cubic inches of air will weigh 31·1446 grains. From these data it is easy to calculate the increase of bulk which each of these liquids undergoes when converted into vapour, at the boiling point of each liquid.

Expansion of liquids when converted into vapour.

	Increase of volume when converted into vapour.	Increase of volume that of turpentine vapour being 1.
Water . . .	1689 .	8·79
Hydrocyanic acid .	625·7 .	3·25
Alcohol . . .	493·5 .	2·57
Muriatic ether .	314·15 .	1·6
Sulphuric ether .	212·18 .	1·1
Bisulphuret of carbon	434·06 .	2·26
Oil of turpentine .	192·15 .	1
Hydriodic ether .	333·74 .	1·7*

It is obvious from this table that when water is converted into steam, it undergoes a much greater

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* Cagnard de la Tour, made a curious set of experiments which deserve to be noticed. He enclosed ether, alcohol, and water in small glass tubes, which were never more than half filled. He sealed the tubes hermetically without expelling the air from the empty portion. He then exposed the tubes to heat till the liquids assumed the gaseous form.

Ether became gaseous in a space scarcely double its volume at the temperature of 320°. Pressure 38 atmospheres. Alcohol became gaseous in a space about thrice its volume at the temperature of 404°½. Pressure about 129 atmospheres. Water broke the glass tube, because it began to dissolve it. When this was prevented by adding a little carbonate of soda, the water became gaseous in a space four times its volume at the temperature at which zinc melts, or about 648°.

expansion than any other liquid hitherto examined. It expands 8 times as much as sulphuric ether, and almost 3½ times as much as alcohol. This explains a phenomenon, which may at first sight appear paradoxical, why the vapours of alcohol and ether are heavier than those of water, though the liquids themselves which produce these vapours are lighter than water.

10. It is obvious that the specific gravity of the vapour of water is proportional to its elasticity. Consequently, if we know this specific gravity at any one temperature, we may from our knowledge of the elasticity determine the specific gravity at any other. The following table exhibits the specific gravity of vapour at various temperatures. And it will be easy for any person by having recourse to the table of elasticities given in page 215 of this volume, to determine the specific gravity of vapour at any temperature that he pleases.

Specific gravity of vapour of water.

Temperature.	Specific gravity.	Weight of 100 cubic inches in grains.
32°	0·004166	0·12974825
36	0·0048828125	0·15206406
56	0·009765628	0·304128125
78	0·01953125	0·60825625
100	0·0390625	1·2165125
125	0·078125	2·433125
150	0·15625	4·86625
180	0·3125	9·7325
212	0·625	19.465
250	1·25	39·93
293·4	2·5	79·86
343·4	5·	159·72
419·	21·875	681·3

From this table we see that at the temperature of 419°, water when converted into steam expands only 37 times. When such steam comes into the atmosphere it would expand 35 times. This would prodigiously increase its specific heat, and of course diminish its temperature. It is probable that at a temperature not much higher than 500°, the steam of water would not much exceed double the bulk of the water from which it was generated. The expansive force of such steam would be truly formidable. It would when it issued into the atmosphere suddenly expand almost 650 times. We do not know at what temperature water would become vapour without any increase of volume. It would then support a column of mercury 3243 feet in height, and exert a force of 19,459 lbs. upon every square inch of the vessel containing it.

No doubt the vapours of other liquids increase in their elasticity with the temperature as well as the vapour of water, though we are not yet in possession of sufficiently accurate experiments on the subject. The conjecture of Dalton that all vapours increase in elasticity equally by equal increments of heat, reckoning from the boiling points of each, has been found by subsequent investigation not to hold good. It has accordingly been abandoned by Mr. Dalton himself.

Theory of steam.

11. Such are the phenomena of the conversion of liquids into elastic fluids. Dr. Black applied his theory of latent heat to this conversion with great sagacity, and demonstrated that it is owing to the very same cause as the conversion of solids into

liquids; namely, to the combination of a certain dose of caloric with the liquid without any increase of temperature. The truth of this very important point was established by the following experiments.

First, When a vessel of water is put upon the fire, the water gradually becomes hotter till it reaches 212°; afterwards its temperature is not increased. Now heat must be constantly entering from the fire and combining with the water. But as the water does not become hotter, the heat must combine with that part of it which flies off in the form of steam: but the temperature of the steam is only 212°: therefore this additional heat does not increase its temperature. We must conclude, then, that the change of water to steam is owing to the combination of this heat; for it produces no other change.

Dr. Black put some water in a tin-plate vessel upon a red hot iron. The water was of the temperature 50°: in four minutes it began to boil, and in 20 minutes it was all boiled off. During the first four minutes it had received 162°, or $40\frac{1}{2}$° per minute. If we suppose that it received as much per minute during the whole process of boiling, the caloric which entered into the water and converted it into steam would amount to $40\frac{1}{2} \times 20 = 810°$.* This heat is not indicated by the thermometer, for the temperature of steam is only 212°; therefore Dr. Black called it *latent heat.*

Second, Water may be heated in a Papin's diges-

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* Black's Lectures, i. 157.

Chap. III. ter to 400° without boiling: because the steam is forcibly compressed, and prevented from making its escape. If the mouth of the vessel be suddenly opened while things are in this state, part of the water rushes out in the form of steam, but the greater part still remains in the form of water, and its temperature instantly sinks to 212°; consequently 188° of heat have suddenly disappeared. This heat must have been carried off by the steam. Now as only about $\frac{1}{5}$th of the water is converted into steam, that steam must contain not only its own 188°, but also the 188° lost by each of the other four parts; that is to say, it must contain 188° × 5, or about 940°. Steam therefore, is water combined with at least 940° of heat, the presence of which is not indicated by the thermometer. This experiment was first made by Dr. Black, and afterwards, with more precision, by Mr. Watt.

Third, When hot liquids are put under the receiver of an air-pump, and the air is suddenly drawn off, the liquids boil, and their temperature sinks with great rapidity a considerable number of degrees. Thus water, however hot at first, is very soon reduced to the temperature of 70°; and ether becomes suddenly so cold that it freezes water placed round the vessel which contains it. In these cases the vapour undoubtedly carries off the heat of the liquid; but the temperature of the vapour is never greater than that of the liquid itself: the heat therefore must combine with the vapour, and become latent.

Fourth, If one part of steam at 212° be mixed with nine parts by weight of water at 62°, the steam instantly assumes the form of water, and the temperature after mixture is 178·6°; consequently each of the nine parts of water has received 116·6° of caloric; and the steam has lost 9 × 116·6°=1049·4° of caloric. But as the temperature of the steam is diminished by 33·3°, we must subtract this sum. There will remain rather more than 1000°, which is the quantity of heat which existed in the steam without increasing its temperature. This experiment cannot be made directly, but it may be made by passing a given weight of steam through a metallic worm, surrounded by a given weight of water. The heat acquired by the water indicates the heat which the steam gives out during its condensation. Mr. Watt was the first person who attempted to determine accurately the latent heat of steams with precision. His experiments were made in 1781; but the account of them has been only published very recently in Mr. Watt's edition of the articles *Steam* and *Steam Engine*, by Professor Robison.* The result of the experiments is, that the latent heat of steam is 950°. This agrees nearly with the determination of Mr. Southern in 1803. He found it in three experiments made at very high temperatures 942°, 942°, and 950°.† The experiments of Lavoisier make it rather more than 1000°. According to Rumford it amounts to 1040·8°. Thus we have the latent heats of steam as follows:

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* Page 6.

† Ibid. p. 165.

Watt	950°
Southern . . .	945
Lavoisier . . .	1000
Rumford . . .	1040·8
Despretz . . .	955·8
Mean	978·3

This number 978°·3 cannot deviate very far from the truth. We cannot err much then, if we adopt for the latent heat of steam, 1000°, which was the number established by the experiments of Lavoisier. I am satisfied from some trials which I some time ago made, that the true number is not under 1000° if it does not exceed it.

Latent heat increases as the sensible diminishes.

12. It has been already observed, that in a vacuum, the boiling point of liquids is lowered by about 145°. It occurred to Dr. Black many years ago, that by distilling ardent spirits in vacuo, a great saving of fuel would probably result. The project was first attempted to be realized by Mr. Watt. He half filled a small still with water, and then closely united it with the receiver. The receiver had a small hole in its bottom, which had a plug fitted to it. The water in the still was made to boil violently till all the air within the apparatus was forced by the steam out at this hole. While the steam was blowing violently out at the hole, it was suddenly stopped by the plug, and the bottom of the still was set on ice. This soon cooled the contents, and the steam within the apparatus was condensed into water. A lamp was now set under the still, and in a few minutes, the whole apparatus grew warm—

a proof that steam was produced from the water, and that it was passing into the receiver. Cold water was put into the refrigeratory and the distillation went on—slowly indeed—but very well—and the ebullition was distinctly heard in the still, although the head of it was scarcely sensibly warm to the hand. But the result of the process was, that the latent heat of the steam was greatly increased by the diminution of its sensible heat. The temperature of the steam in the experiment was found to be 100°. The water in the refrigeratory was raised by the condensation of this steam from 57° to 77°, and the vessel had acquired as much heat as would have raised the water 1°. Consequently, 21° of heat had been acquired from the steam. The quantity of water distilled was $\frac{1}{51}$ of that in the refrigeratory. Therefore, 21° × 51 will give the heat extricated from the steam. This amounts to 1071. From this we must subtract the sensible heat lost by the steam; for it was cooled down from 100° to 77°, so that it lost 23°. The remainder 1048°, is the latent heat of the steam. The latent heat of steam at 212°, according to Mr. Watt's experiments, made with a similar apparatus, is 940. Hence it follows, that the latent heat of steam of 100° exceeds that of 212° by 108°, which is very nearly the difference between the sensible heats of the two steams.

Mr. Watt made other experiments with much more care. He found that water distils perfectly well at 70°, and that in this state the latent heat approaches 1300°, and certainly exceeds 1200°.

The unexpected result of these experiments is that no advantage is to be expected in the manufacture of ardent spirits by distilling in vacuo. For the latent heat of the steam is as much increased as the sensible heat is diminished.

A little consideration will satisfy us that this fact might have been anticipated, had Mr. Watt, when he made his experiments, been aware that the volume of vapour in a given weight increases in proportion as its sensible heat diminishes. For its specific heat must increase with this augmentation of volume, and occasion an increase of heat necessary to maintain its temperature just proportional to the diminution of the sensible heat.

And vice versa.

In the year 1813, a paper was published by Mr. Sharpe, in the 2d volume of the 2d series of the Manchester Memoirs. He shows in this paper; 1. That if the source of heat be the same or invariable water heats equably or in the same time, from 120°, up to the highest temperature which it can reach without boiling. Suppose for example, that it is heated from 120° to 130°, or 10° in three minutes; then it will be heated from 270° to 280° in exactly the same time. The reason of this equality probably is, that the difference between the temperature of the fire (the source of heat in these experiments) and the water was so great, that the 150° or 200° which had been added to the water, had no sensible effect in diminishing that difference. 2. That whatever be the temperature of steam from 212° upwards, if we take the same weight of it and condense it by water, the temperature of the water will always be raised the same number of degrees. Or in other words, the absolute quantity of heat is always the *same* in the *same weight of* steam, whatever the temperature of that steam is. M. Clement informed me several years ago, that he had established the same fact by a set of experiments of his own. And I have seen frequent allusions to these experiments of Clement in the French Journals, though I am not aware that the experiments themselves have ever been published.

From these experiments of Mr. Sharpe, confirmed as they have been by those of Clement; it follows that the latent and sensible heats of steam added together, make a constant quantity. Or in other words, that the latent heat diminishes as the sensible heat increases, and *vice versa*. Thus Mr. Sharp has proved that the truth which Mr. Watt established under the boiling point, holds also above the boiling point—and that the proposition is universally true.

Latent and sensible heats a constant quantity.

If we neglect all that portion of sensible heat which lies below 32°, we may say that at 212°, the sensible heat of steam is 180°, while its latent heat is 1000°, making together the sum of 1180°. And this sum being constant, we can easily determine what the latent heat of steam is at any temperature. We have only to subtract the sensible heat from the constant quantity 1180°. The following little table exhibits the latent heat of steam at different temperatures.

Chap. III.

Temp.	Latent heat.	Temp.	Latent heat.
32°	1180	180°	1032
36	1176	212	1000
56	1156	250	962
78	1134	293·4	918·6
100	1112	343·4	868·6
125	1087	419	793
150	1062		

It is obvious from this table, that while the specific gravity or elasticity of steam increases in a geometrical progression with a ratio of 2, the latent heats diminish in a geometrical progression whose ratio does not very much surpass unity, being probably 1·0306. We might therefore calculate the temperature at which the elasticity of steam increases in a geometrical progression whose ratio is 2, or carry the table given in page 215 to any length. The following will serve as a specimen of the way in which this might be done.

Temperature.	Elasticity in inches of mercury.
293·4°	120
343·4	240
370·2	480
395·2	960
419·5	1920

It is obvious, however, that the elasticity by no means increases at so great a rate as we have supposed from the table in page 215. I think there can be no doubt that somewhere about 350°, the increase of elasticity begins to follow a much smaller rate. For the elasticity at 419°, as we learn from the experiment of Perkins, instead of 1920, is only equal to 1050 inches of mercury.

If we could apply such a pressure to water that we could heat it till its sensible heat rose to 1212°, it is obvious that it would be converted into steam having the specific gravity, and consequently the volume of the original water. The latent heat of such steam would be 0°; but its elasticity would be prodigious. The instant that the pressure upon it was removed it would expand, and its latent heat would increase at the expense of its sensible heat. It is obvious from this that the existence of latent heat in steam is owing to its expansion and the consequence of its expansion, and that the moment we reduce it to the bulk of the water from which it was generated all the latent heat becomes sensible. We may conceive that water has a strong affinity for heat, that an atmosphere of it accumulates round every integrant particle of the water, and by its repellent property forces the aqueous particles nearly 12 times farther from each other than when they constituted water.

Latent heat of other liquids.

13. Few experiments have hitherto been made to determine the latent heats of other liquids. A few have been determined by M. Dupretz and by Dr. Ure by the method employed by Mr. Watt. Count Rumford's modification of the process constitutes a great improvement. He sunk the water in the refrigeratory 4° lower than the temperature of the room, and continued the distillation of the liquids under examination till the temperature of the water had risen 4° higher than that of the room. During the first part of the process the water of the refrigeratory was imbibing heat from the room.

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During the last part of the process it was giving out heat to the room, and these two opposite currents must have just balanced each other. He then determined the weight of the liquid which had distilled over, and thus was able with ease to determine its latent heat. The following little table exhibits the latent heats of such vapours as have been examined.

	Latent heats.	Ditto referred to water.
Steam	1000°	
Alcohol vapour (sp. gr. 0·793)*	597	375°·86
Sulphuric ether (sp. gr. 0·715)*	314	163·44
Oil of turpentine*	299·16	138·24
Ammonia (sp. gr. ·0978)†	837·28	862
Nitric acid (sp. gr. 1·494)†	531·99	335
Naphtha†	177·87	73·77

If we calculate the latent heats of these different vapours from their expansions we obtain the following results.

	Latent heats.	Ditto referred to water.
Alcohol	578°	360°
Ether	236·7	123·1
Oil of turpentine	246·7	114

As these are considerably below the experimental results it remains to be seen whether there be an error in the expansions, or whether the conclusion

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[*] Dupretz; Ann. de Chim. et de Phys., xxiv. 323. It is probable that the numbers given are rather under the truth.

[†] Ure; Phil. Trans. 1818, p. 388.

that the latent heat should be proportional to the expansion be erroneous.

SECTION II. OF GASES.

Meaning of the term *gas*.

The word *gas* appears to have been introduced into chemistry by Van Helmont. He seems to have applied the term to every thing which is driven off from bodies in the state of vapour by heat.* It was introduced into modern chemistry by Macquer, who applied it to all those substances which Black, Priestley, and Cavendish, and the British chemists in general, had called *airs*. Macquer was of opinion that atmospherical air is a simple or elementary body, while those bodies to which the term *gas* was applied by him, were in his opinion compounds either of air with other bodies, or of certain bodies differing from air with each other.†

The term is at present applied to all substances which possess the mechanical properties of atmospherical air; that is to say, which are invisible and

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* He divides gases into five classes. "Nescivit, inquam, Schola Galenica hactenus differentiam inter Gas ventosum (quod mere aer est, id est ventus per syderum blas commotus), Gas pingue; Gas siccum, quod sublimatum dicitur, Gas fuliginosum, sive endemicum, et Gas sylvestre, sive incoercibile, quod in corpus cogi non potest visibile." Van Helmont de flatibus, § 4. Opera Van Helmont, p. 399, Francofurti, 1682.

† See his Dictionaire de Chimie; 2d edition, article Gaz. Macquer always spells the word *gaz*, I know not for what reason, and in this he was followed for a time by the French writers in general. Van Helmont always spells the word *gas*, and this mode of spelling is now generally employed.

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elastic, and capable of indefinite expansion and rarefaction without losing their aerial form. About 24 such bodies are at present known. The following table exhibits the names and specific gravities of these bodies which I have determined by very careful and accurate experiments.

Sp. gr. of gases.

Gases.	Sp. grav. at 60°.	Weight of 100 cubit inches in grains.
Air	1·	31·1446
Hydrogen gas	0·069$\dot{4}$	2·1614
Carburetted hydrogen	0·555$\dot{5}$	17·3025
Ammonia	0·5902$\dot{7}$	18·3837
Subphosphuretted hydrogen	0 694$\dot{4}$	21·6280
Phosphuretted hydrogen	0·902$\dot{7}$	28·1164
Bihydroguret of phosphorus	0·972$\dot{2}$	30·2794
Carbonic oxide	0·972$\dot{2}$	30·2794
Azotic gas	0·972$\dot{2}$	30·2794
Olefiant gas	0·972$\dot{2}$	30·2794
Deutoxide of azote	1·041$\dot{6}$	32·4402
Oxygen	1·111$\dot{1}$	34·6048
Sulphuretted hydrogen	1·180$\dot{5}$	36·6816
Muriatic acid	1·2847$\dot{2}$	40·0121
Carbonic acid	1·527$\dot{7}$	47·4691
Protoxide of azote	1·527$\dot{7}$	47·4691
Cyanogen	1·805$\dot{5}$	56·2316
Sulphurous acid	2·222$\dot{2}$	69·2064
Fluoboric acid	2·361$\dot{1}$	73·5355
Protoxide of chlorine	2·444$\dot{4}$	76·1300
Chlorine	2·5	77·8615
Chlorocarbonic acid	3·472$\dot{2}$	108·1400
Fluosilicic acid	3·611$\dot{1}$	112·4700
Hydriodic acid	4·3402$\dot{7}$	135·1760

These gases are all invisible except chlorine and

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protoxide of chlorine; both of which have a strong green or yellowish green colour.

Theory of gases.

Gases differ from vapours in this remarkable particular that they do not lose their elastic state by the application of ordinary cold or ordinary pressure. Many philosophers have adopted the opinion that they owe this elastic state to the latent heat which they contain, as is the case with vapours. M. Amontons, an ingenious member of the French Academy of Sciences about the beginning of the 18th century, was the first who proposed this idea with regard to the atmosphere. He supposed that it might be deprived of the whole of its elasticity, and condensed, and even frozen into a solid body, were it in our power to apply to it a sufficient cold —that it is a substance which differs from others in being incomparably more volatile, and which is therefore converted into vapour and preserved in that form by a weaker heat than any that ever occurs or can take place in our globe—and which, therefore, cannot appear under any other form than the one it now wears so long as the constitution of the world remains the same as at present.

Though this opinion may appear at first sight an extravagant flight of the imagination, it is notwithstanding supported very strongly by analogy. We know that water is easily converted by heat into a vapour, which, so long as it is kept sufficiently hot, possesses all the mechanical or sensible qualities of air. Alcohol yields a vapour which retains its elasticity at a still lower temperature than steam. The vapour of sulphuric ether is as elastic as common air at the temperature of 96°. Muriatic ether has the form of a vapour, and is as elastic as common air at the temperature of 52°, so that we cannot preserve it in a liquid form in summer. Ammonia retains its elastic form, and possesses all the properties of a gas till it be cooled down to 46° below zero. In this climate, therefore, it always retains its elastic form, and is considered as a gas. But in Melville island, where Captain Parry wintered, it would assume the liquid form during a part of the year, as the thermometer in February stood as low as 55° below zero.

Gases have been condensed into liquids.

This opinion of the similar constitution of vapours and gases, has been still farther confirmed by Mr. Faraday, who by the united action of pressure and cold, has succeeded in condensing eight different gases into liquids.* His method was, to shut up the materials for generating the gas in a strong glass tube hermetically sealed. Heat was applied (if necessary) to generate the gas at one end of the tube, while the other extremity was kept cool. The condensation occasioned by the accumulation of the gas in the tube, together with the cold, at last reduced the gas to the liquid state, and it was collected in the cold part of the tube. Thus mercury and concentrated sulphuric acid were sealed up in a bent tube, and being brought to one end, heat was carefully applied, while the other end was preserved cool by wet bibulous paper. Sulphurous acid gas was produced where the heat acted,

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* Phil. Trans. 1823, p. 189.

and was condensed by the sulphuric acid present. But when that acid had become saturated, the sulphurous acid passed to the cold end of the tube, and was condensed into a liquid. The other liquid gases were generated, and condensed by similar contrivances.

The following table exhibits the names of the gases, which Mr. Faraday succeeded in condensing into liquids, with the pressure necessary to produce the liquefaction. This pressure is denoted by the number of atmospheres necessary to produce it, each atmosphere being equivalent to a column of mercury, 30 inches in height.

	Atmospheres' pressure.	Sp. gravity.
Sulphurous acid	2 at 45°	1·42
Cyanogen	3·7 at 45°	0·9
Chlorine	4 at 60°	——
Ammonia	6½ at 50°	0·76
Sulphuretted hydrogen	17 at 50°	0·9
Carbonic acid	36 at 32°	——
Muriatic acid	40 at 50°	——
Protoxide of azote	50 at 45°	——
Protoxide of chlorine	Not determined.	——
Carburetted hydrogen	1200*	

Sir Humphry Davy has shown that the elasticity of the vapours, from these liquid gases, increases at a great rate with their temperature. Thus sulphuretted hydrogen, condensed in a tube at 3°, had a vapour floating over it, which exerted a pressure of

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* Perkins; Phil. Trans., 1826, p. 544.

14 atmospheres; when heated to 47° it exerted a pressure of 17 atmospheres. Liquid muriatic acid at 3°, exerted a force equal to 20 atmospheres. When heated 22° its force was equal to 25 atmospheres, and by a farther addition of 25° its elastic force became equal to 40 atmospheres. Carbonic acid at 12° exerted a force of 20 atmospheres, and at 32° its elasticity was equal to 36 atmospheres. Liquid ammonia at 32° exerts a force of 5 atmospheres, and at 50° a force of $6\frac{1}{2}$ atmospheres. Liquid protoxide of azote at 32° exerts a force of 44 atmospheres, and at 45° a force of 51·3 atmospheres nearly.*

Mr. Faraday's attempts to condense hydrogen, oxygen, fluoboric, fluosilicic, and phosphuretted hydrogen gases, were unsuccessful. Indeed oxygen, azotic gas, and hydrogen, have been subjected to a pressure of 800 atmospheres, without any tendency to assume the liquid form. All attempts to condense them, therefore, by mere pressure, are likely to end in disappointment.

Absorption of gases by water.

But there are other phenomena connected with the gases, which may throw some light on their constitution. It is well known that water has the property of absorbing them, and that it absorbs a definite volume which characterizes each particular gas. Dr. Henry has ascertained, that the volume of each gas absorbed by water, is the same whatever be the pressure to which the gas is previously subjected. If we double the weight of carbonic acid, by subjecting it to the pressure of two atmo-

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* Phil. Trans. 1823, p. 200.

spheres, water will still absorb its own volume of it. The following table exhibits the volumes of each gas absorbed by 100 volumes of water, supposing the temperature and pressure the same in all cases.

	Absorption in volumes.	Authority.
Cyanogen	450 .	Gay-Lussac
Sulphuretted hydrogen .	366·6 .	Thomson
Chlorine	200 .	Berthollet
Carbonic acid . . .	106 .	Cavendish
Protoxide of azote . .	76 .	Saussure
Olefiant gas . . .	15·3 .	Saussure
Phosphuretted hydrogen .	5 .	Thomson
Deutoxide of azote .	3·7 .	Dalton
Oxygen	3·7 .	Henry
Carburetted hydrogen .	3·7 .	Dalton
Azote	2·5 .	Dalton
Carbonic oxide . .	2·01 .	Henry
Hydrogen	2 .	Dalton

Depends upon the tendency to assume the liquid state.

Mr. Thomas Graham has suggested that these gases before they can be absorbed by or combine with water, in all probability assume the liquid form. If this conjecture be admitted, it is clear that the quantity of each absorbed must bear some relation to its elasticity. It may not be in the inverse ratio exactly, because the amount of the affinity between the gases and water may and probably does differ considerably. This is doubtless the reason why sulphuretted hydrogen is more absorbable than chlorine, although chlorine is condensable by a pressure of 4 atmospheres, while sulphuretted hydrogen requires 17 atmospheres to condense it. But we may conclude in general, that those gases which are but little absorbed by water, are much more difficult to condense into the liquid state by pressure than those which are absorbed in great quantity, by that liquid. Accordingly we find that all the gases liquefied by Mr. Faraday, are absorbed in considerable quantity by water. He succeeded with sulphurous acid, muriatic acid, and ammonia, which are absorbed by water in great quantity, and likewise with the first five gases in the preceding table; but he failed with all the rest. It is not unlikely that olefiant gas might also be condensed into a liquid, though the pressure requisite would be much greater than what was required to condense any of those that precede it in the table. But the very small absorbability of all the rest, renders it unlikely that any of them could be condensed into a liquid by simple pressure. We see indeed from Mr. Perkins' experiment that carburetted hydrogen is condensed by a pressure of 1200 atmospheres. The same pressure would probably condense all the gases above it in the table.

Upon the whole, the notion that gases and vapours have the same constitution, and differ only in the obstinacy with which the former retain their latent heat, is supported by such strong analogies, and so many gases have actually been reduced to liquids by the united action of cold and pressure, that it has been generally adopted by modern chemists.

SECTION III.

OF SPONTANEOUS EVAPORATION.

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Water evaporates at all temperatures.

Every body knows that water evaporates at all temperatures, however low. After a heavy fall of rain the roads become deep, and the country becomes studded with little ponds of water. But after a few days or weeks of fair weather, the roads get dry and dusty, and the little ponds of water disappear. And this takes place not only in summer but even in winter, when the weather happens to continue dry for some time. The Mediterranean sea receives many very large rivers. The Nile, the Po, the Rhone, the Ebro, the Danube, the Nieper, the Don, and many other rivers of smaller extent empty themselves either directly into the Mediterranean or into the seas connected with it, and constituting as it were a part of this great inland ocean. Yet notwithstanding this great and regular influx of water, this sea not only does not increase in size; but a constant current sets in from the Atlantic through the Straits of Gibraltar. An evident proof that the natural evaporation from the surface of the Mediterranean, is more than sufficient to dissipate all the water thrown into it from a vast tract of Europe and Africa.

And some other liquids.

Water is not the only liquid that evaporates at temperatures below the boiling point. Alcohol, ethers, bisulphuret of carbon, volatile oils, nitric acid, and some other liquids (even mercury), are in similar circumstances. Sulphuric acid, indeed, in the state in which it is prepared by our manufacturers, not only does not evaporate, but even absorbs moisture from the atmosphere and becomes much heavier. But when we expose sulphuric acid totally destitute of water (in which state it is a white solid) to the atmosphere, it evaporates at the common temperature with great rapidity, and speedily fills the apartment in which it is exposed with a white smoke, so thick and so offensive, that I have been frequently obliged to quit the apartment till the offensive vapour was dissipated by throwing open the windows. The fixed oils do not evaporate sensibly till their temperature is raised to a considerable height. Indeed, the term *evaporation* as applied to them is not quite correct, for they cannot be converted into vapour or distilled over without acquiring new properties.

Evaporation confined to the surface.

Let us consider the evaporation of water at low temperatures with attention.

1. This evaporation is entirely confined to the surface, and is therefore proportional to the surface. Hence, if the quantity of water to evaporate be constant, it will disappear the sooner the shallower it is, because its surface will be the greater. If we cover the surface of the water with oil, the evaporation is stopped altogether.

On the western coast of France, and in some places on the south coast of England, sea salt is obtained by allowing sea water to evaporate spontaneously, till the salt separates in crystals. The sea water is allowed to flow into a number of shallow, oblong divisions dug on the sea shore, and lined internally with clay. The sea water in these pits presents a great surface, and in the hot weather of summer, it gradually evaporates away (provided the weather be dry), leaving the salt in crystals. Salt obtained in this way, is called *bay salt*. The crystals are large and hard, and not very white. This kind of salt is preferred to every other by the curers of fish. At one time, the superiority of Dutch cured herrings was ascribed to the bay salt with which they were salted.

Increased by wind.

2. Water does not evaporate so rapidly when the air is still, as when it is agitated by a brisk wind. And the more rapid the current of air the more rapidly, *ceteris paribus*, does the water disappear. Hence, in manufactories where the evaporation of water constitutes a part of the process, the vessels containing the water to be evaporated, ought to be placed so that a brisk current of air shall pass over their surface. In Glasgow and the neighbourhood, soda and alum leys are brought to the requisite degree of concentration, by passing over their surface a brisk current of air which has previously passed through a fire; so that the flame of the fire plays over the surface of the evaporating liquor. And this process has been found more convenient and economical, than the common method of evaporating liquids, by applying heat to the bottom of the vessels containing them.

When air is perfectly still the vapour as it forms accumulates over the surface of the water, and in proportion as this accumulation increases, does the

Chap. III. process of evaporation diminish. So that at last it is reduced to nothing.

And by heat.

3. The rate of evaporation increases with the temperature. Evaporation goes on at 32°, or even at lower temperatures. For ice and snow are constantly emitting vapour, and diminishing in weight when surrounded by a dry atmosphere. If two or three ounces of ice be confined in the exhausted receiver of an air-pump over concentrated sulphuric acid, it evaporates all away in little more than 24 hours; and yet during the whole time the temperature of the ice is considerably lower than 32°. In the open air, and consequently under the pressure of the atmosphere, the evaporation of the ice, though quite sensible, is a great deal slower.

Mr. Dalton made a set of experiments to determine the rate at which water evaporates from a vessel of a given diameter at different temperatures. By observing the rate at which the evaporation increased or diminished according to the temperature, he was enabled to construct a table exhibiting the quantity evaporated in a given time at every temperature, from zero up to 212°—supposing the atmosphere in which the evaporation is going on to be quite free from vapour. He found that the rate of evaporation was always proportional to the elasticity of the vapour generated, measured by the length of a column of mercury which it is capable of supporting. By referring to the table of the elasticities of vapour at different temperatures given in page 215, it will be seen that the quantity evaporated from a given surface at the following temperatures is nearly as follows:

Temperature.	Rate of evaporation.
212°	512
180	256
150	128
125	64
100	32
79·5	16
58	8
38	4
18·5	2

The following table showing the force of vapour at all temperatures, from 32° to 80°, was constructed by interpolation from a set of experiments made with great care, by Mr. Crichton of Glasgow, to determine the force of vapour at 32°, 40°, 50°, and 60°. The three last columns of the table show the quantity of vapour in grains which would be driven off in a minute from a circular vessel of water six inches in diameter, as determined by Mr. Dalton, according as it is placed in circumstances less or more favourable for evaporation.*

Table of the force of vapour, and rate of evaporation.

Temperature.	Force of vapour in inches of mercury.	Quantity evaporated per minute from a circular vessel 6 inches diameter in grains.		
		1	2	3
32°	0·2000	0·80	1·03	1·26
33	0·2066	0·83	1·07	1·30
34	0·2134	0·86	1·11	1·35
35	0.2204	0·89	1·14	1·39
36	0·2277	0·92	1·18	1·45
37	0·2352	0·95	1·22	1·49
38	0·2429	0·98	1·26	1·54
39	0·2509	1·02	1·31	1·60
40	0·2600	1·05	1·35	1·65
41	0·2686	1·09	1·40	1·71

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* Manchester Memoirs, v. 585, 1st series.

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Temperature.	Force of vapour in inches of mercury.	Quantity evaporated per minute from a circular vessel 6 inches diameter in grains.		
		1	2	3
42°	0·2775	1·13	1·45	1·78
43	0·2866	1·18	1·51	1·85
44	0·2961	1·22	1·57	1·92
45	0·3059	1·26	1·62	1·99
46	0·3160	1·31	1·68	2·06
47	0·3264	1·36	1·75	2·13
48	0·3372	1·40	1·80	2·20
49	0·3483	1·45	1·86	2·28
50	0·3600	1·50	1·92	2.36
51	0.3735	1·55	1·99	2·44
52	0·3875	1·60	2·06	2·51
53	0·4020	1·66	2·13	2·61
54	0·4171	1·71	2·20	2·69
55	0·4327	1·77	2·28	2·78
56	0·4489	1·86	2·35	2·88
57	0·4657	1·90	2·43	2·98
58	0·4832	1·96	2·52	3·08
59	0·5012	2·06	2·61	3·19
60	0·5200	2·10	2·70	3·30
61	0·5377	2·17	2·79	3·41
62	0·5560	2·24	2·88	3·52
63	0·5749	2·31	2·97	3·63
64	0·5944	2·39	3·07	3·76
65	0·6146	2·46	3·16	3·87
66	0·6355	2·54	3·27	3·99
67	0·6571	2·62	3·37	4·12
68	0·6794	2·70	3·47	4·24
69	0·7025	2·79	3·59	4·38
70	0·7260	2·88	3·70	4·53
71	0·7507	2·98	3·83	4·68
72	0·7762	3·08	3·96	4·84
73	0·8026	3·18	4·09	5·00
74	0·8299	3·29	4·23	5·17
75	0·8581	3·40	4·37	5·34
76	0·8873	3·52	4·52	5·53
77	0·9175	3·65	4·68	5·72
78	0·9487	3·76	4·83	5·91
79	0·9809	3·88	4·99	6·10
80	1·0120	4·00	5·14	6·29

Were we to suppose the atmosphere perfectly dry in both cases, the rate of evaporation ought to be four times greater in summer than in winter from the surface of lakes and the earth in this country. In reality the difference is much greater, because the atmosphere is usually loaded with moisture in winter, and comparatively dry in summer. And to know the true rate of evaporation we must always subtract from the quantity that would be evaporated what exists already in the atmosphere.

Evaporation most rapid in a vacuum.

4. Water evaporates much more rapidly in vacuo, provided the vapours be withdrawn in proportion as they are formed, than it does under the pressure of the atmosphere, supposing the temperature and all other things the same. This is beautifully exemplified in Professor Leslie's mode of producing ice, by placing a vessel containing water a little above a flat dish filled with sulphuric acid in the exhausted receiver of an air-pump. The sulphuric acid absorbs the vapours as fast as they are produced; the rate of evaporation is kept up, and so great a degree of cold produced that the water soon freezes even in summer. The cold produced is here the criterion of the rapidity of evaporation. And in favourable circumstances Mr. Leslie not only froze mercury by this process; but, if I understand him rightly, he sunk the thermometer to —120°.

5. Such are the constant phenomena of spontaneous evaporation. Let us now attend to the explanations of these phenomena which have been successively proposed by men of science.

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It was supposed at first that the vapour which rises from water below the boiling point is quite different from the vapour of water produced at or above the boiling point. In particular it was supposed that this low temperature vapour is devoid of elasticity. This led to an explanation of spontaneous evaporation first suggested by Dr. Halley, and afterwards explained in detail by Leroi of Montpellier, Dr. Franklin, Dr. Hamilton of Dublin, and several other men of science. According to Dr. Halley water is *dissolved* by air in the same way as salt or sugar is *dissolved* by water. This hypothesis accounts for many of the phenomena of spontaneous evaporation in a simple and satisfactory manner. It gives us a reason why evaporation is so much promoted by wind—because the solvent is rapidly renewed. Heat increases the solvent power of air just as it does that of water.

Evaporation ascribed to solution.

But erroneously.

But when Deluc and Saussure demonstrated that vapour of all temperatures is possessed of elasticity, and that it augments the volume of air when mixed with it exactly in proportion to its elasticity, the theory of Halley lost much of its plausibility. And after Saussure and Pictet had ascertained that evaporation goes on better in vacuo than under the pressure of the atmosphere, and that the vapour thus formed in vacuo possessed the same elasticity, and occupied the same volume, as when formed under the pressure of the atmosphere, the theory was no longer tenable. For how can spontaneous evaporation be considered as a solution of water in air when it goes on better without air? Or how could it be considered as a chemical union between air and water when it was known that the volume of vapour depended in all cases upon the temperature, and that the kind of air, or even the absence of all air, was a matter of indifference?

After these facts had become known towards the end of the last century, there was a general disposition among well informed chemists to abandon the theory of Halley. But it was the Essays and Experiments of Mr. Dalton, published in 1802,* which completely established the true theory of spontaneous evaporation.

Method of determining the volume of vapour in the atmosphere.

Water and many other liquids have a tendency to assume the elastic form of vapours at all temperatures however low. At low temperatures the elasticity of these vapours is small. Hence unless the particles of vapour be at a great distance from each other, they unite together again, and assume the form of a liquid. Thus the quantity of vapour which can exist in the atmosphere is regulated by the elasticity of that vapour. And this elasticity is measured by the height at which it is capable of supporting a column of mercury. We usually reckon the mean height of the barometer at the seashore in this country 30 inches.† At 32° vapour of water is capable of supporting a column of mercury 0·2 inch in height. It is clear, therefore, that at that temperature the utmost quantity of the

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* Manchester Memoirs, 1st series, v. 535.

† I have already stated that the true height is a little lower than this, or 29.82 inches.

vapour of water which can exist in the atmosphere is about $\frac{1}{150}$th of its volume.

Mr. Dalton has shown that if p = pressure of the atmosphere in inches of mercury, f = elasticity of vapour contained in the atmosphere, x = volumes of dry air in 100 volumes of the given atmospherical air. Then

$$\frac{p\,x}{p-f} = 100 \text{; consequently } x = \frac{100}{\dfrac{p}{p-f}}$$

Let air be saturated with moisture at 32°. In that case we have

$$p = 30 \qquad \frac{p}{p-f} = \frac{30}{29{\cdot}8} = 1{\cdot}00671$$
$$f = 0.2$$
$$x = \frac{100}{1{\cdot}00671} = 99{\cdot}333.$$

So that the vapour in 100 volumes of such air has a volume amounting to 0·666, which is just $\frac{1}{150}$th of the whole.

At the temperature of 60°, $f = 0{\cdot}52$, we have therefore

$$\frac{p\,x}{p-f} = \frac{30\,x}{29{\cdot}48} = 100 \text{; and } x = \frac{100}{\dfrac{30}{29.48}} = 98{\cdot}267$$

So that the whole vapour capable of existing in the atmosphere at the temperature of 60° is $\frac{1733}{100000}$.

The highest temperature that I have ever seen in Great Britain was 93°. At that temperature $f = 1{\cdot}5$

$$\frac{p\,x}{p-f} = \frac{30\,x}{28{\cdot}5} = 100 \text{; and } x = 95.$$

So that the vapour capable of existing in the atmosphere at that temperature is $\frac{5}{100}$ or $\frac{1}{20}$.

Thus we can easily determine the volume of vapour, which is capable of existing in the atmosphere at any temperature.

A still easier method of obtaining the same volume, is to ascertain by the preceding table, the force of vapour at any given temperature. Let this number be made numerator of a fraction whose denominator is the constant number 30. This fraction will denote the volume of vapour, capable of existing in the atmosphere, at the given temperature, supposing it reduced to the density of steam at 212°, or supposing its specific gravity 0·625. Thus at 60° the force of vapour is 0·52, its volume capable of existing in the atmosphere, at that temperature, is therefore $\frac{52}{3000}$, or rather more than $\frac{1}{58}$th part.

Mr. Dalton's method of determining the elasticity of vapour, at various temperatures, was this. He had a barometer tube shut at one end, and open at the other, and quite dry. It was filled with newly boiled mercury, inverted in a basin of mercury; the height of the mercury was accurately marked, and the tube was graduated into inches, and tenths, by means of a file. A little water was then poured into the tube in place of the mercury, so as to moisten the whole inside. Mercury was poured into the tube again, and it was inverted as before, into a basin of mercury. By degrees a thin film of water collected on the surface of the mercury. The mercury stood a little lower in the tube than before, and the difference of altitude indicated the elasticity of the vapour of water, at the temperature at which the observation was made; or the height of the column of mercury which it would be capable of supporting.

Mr. Dalton then took a cylindrical glass tube, open at both ends, 2 inches in diameter, and 14 inches long. To each end of it a cork was fitted, perforated so as to admit the barometer tube to be passed through, and to be held fast. Into this cylinder he put water of different temperatures, and observed how far the top of the mercury was depressed by each. This depression gave the elasticity of the vapour at the given temperatures. Proceeding in this way, he constructed the table inserted in page 215 of this volume. And by observing the rate at which the elasticity varied, he was enabled to extend his table down to a temperature as low as —40°.

Rate of evaporation depends on the quantity of vapour in air.

6. But the rate of evaporation, which has been just explained, holds good only when the atmosphere is perfectly dry, or when it contains no vapour whatever. If vapour be already present in the atmosphere, as is always the case in this country, then the quantity capable of evaporating at a given temperature, will be the quantity in the table diminished by the vapour already existing in the atmosphere. If we suppose the atmosphere to contain as much vapour, as is capable of existing in it at the given temperature, then it is obvious that no evaporation whatever would take place. If the quantity of vapour contained in the atmo-

sphere be less than it can support, we must find how much less, and this quantity subtracted from the rate of evaporation at the given temperature, when the atmosphere is dry, will give us the true quantity, which will evaporate under the existing circumstances. Suppose the temperature to be 60°. At that temperature, the force of vapour is 0·52 inch, so that at 60° the atmosphere is capable of containing about $\frac{1}{57}$th of its volume of vapour. But let us suppose that the elasticity of the vapour already in the atmosphere, is 0·26. We must subtract 0·26 from 0·52; the remainder 0·26 gives the elasticity of the steam that really can enter the atmosphere. Now 0·26 represents the elasticity of steam at 40°. So that though the temperature be 60, yet in consequence of the vapour already existing in the atmosphere, no more water will evaporate from a given surface than would evaporate at 40°, if the atmosphere at that temperature were perfectly dry.

Hygrometers.

Various instruments have been contrived, to enable us to ascertain the quantity of vapour existing in the atmosphere by inspection. These instruments are distinguished by the name of *hygrometers.* The three most celebrated of these instruments are the hygrometer of Deluc, of Saussure, and of Leslie.

Deluc's hygrometer consists of an extremely thin piece of whalebone attached to an index, which by the whalebone swelling when it imbibes moisture, and contracting as it dries, is turned

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round, and thus points out the degree of moisture or dryness of the air.

Saussure's hygrometer consists of a human hair boiled in a caustic ley, and moving an index precisely in the same way by its expansions and contractions.

Leslie's hygrometer is merely his differential thermometer with one of the balls covered with silk and bibulous paper and kept moist. This moisture evaporating produces cold and contracts the volume of air within the ball. Hence the coloured liquor rises in the tube under this ball. The instrument therefore measures the rapidity of evaporation. But this rapidity is always (other things being equal) directly as the temperature, and inversely as the quantity of moisture already in the atmosphere. This instrument, however, cannot point out the true quantity of vapour already in the atmosphere, because the rate of evaporation is not merely affected by the quantity of vapour already in the atmosphere, but likewise by every alteration in the temperature of the air.

Method of finding the quantity of vapour in air.

To determine the elasticity of the vapour in the atmosphere, the simplest method is one long ago employed by Leroi, and afterwards by Mr. Dalton. It is founded upon a well-known property of glass. If a piece of glass one degree colder than the atmosphere, be surrounded with air holding as much vapour as it can hold at the given temperature, moisture will be deposited upon it. The method is this. Suppose we wish to determine the quantity of vapour actually in the atmosphere. We must repair either to the fields or at least to an open window; for were we to determine within doors our results would seldom be accurate. Provide a very thin glass tumbler, a thermometer and a quantity of water ten or fifteen degrees colder than the atmosphere. In summer the water from our springs, which in the neighbourhood of Glasgow have a temperature of about 47°.75, will answer very well. But in winter the water must be cooled with ice, or we must sometimes use a mixture of snow and salt, or when ice or snow cannot be had, we may easily reduce the temperature of the water low enough for the purpose by dissolving in it a quantity of pounded crystals of glauber salt or of carbonate of soda. Pour the water into the tumbler. Moisture is immediately deposited upon the outside of the glass (if the water be cold enough). Pour back the cold water into the vessel which originally contained it, and wipe the outside of the tumbler dry. When the water has become a little warmer, pour it again into the tumbler, and observe whether moisture be deposited on the outside. If it be, the water must be allowed to get a little warmer, and the trial again repeated. Proceed in this way till the water is just not cold enough to cause the condensation of vapour on the outside of the glass. Over against the temperature of the water thus determined in the preceding table, will be found the elasticity of the vapour in the atmosphere at the time of our observation. If the temperature of the air be 60°, and water of the temperature 50° is just incapable of condensing vapour on the outside of the glass; then 50° represents the temperature which measures the elasticity of the vapour in the atmosphere. Over against 50° in the table we find 0.36 inch, which represents the elasticity of the vapour in the atmosphere at the time of the observation.

Mr. Daniel's hygrometer is an instrument contrived to make the above observation still more conveniently. The glass is cooled by means of ether, and the temperature of the glass when moisture is deposited on it is indicated by a thermometer, which constitutes a part of the apparatus.

Mr. Thomas Jones has contrived a still simpler instrument. It is merely a mercurial thermometer with a large bulb which is turned up by bending the glass tube into the shape of a 6. A little below the bulb is tied a piece of silk. When the instrument is to be used, a few drops of ether are let fall upon the silk cloth. Cold is produced. The moment is watched when moisture condenses on the top of the bulb. The height of the thermometer at that instant is noted. It gives the temperature which we look for in the table, and over against it is placed the elasticity of the vapour in the atmosphere at the time of the observation.

After trying both Daniel's hygrometer, and Mr. Jones' thermometer, I am satisfied that the results obtained by them are not so accurate as those obtained by the simple tumbler and cold water, provided the tumbler be made of sufficiently thin glass.

7. The quantity of vapour in the atmosphere at Glasgow, and doubtless in every part of Great

Britain, is greater in summer than in winter. Yet the rate of evaporation is greater in summer than in winter; because during winter the atmosphere is much nearer the point of saturation than in summer. The following table exhibits the mean elasticity of vapour in the atmosphere in Glasgow during the year 1823, and likewise the evaporation, determined by the elasticity of the vapour evaporated, for every fortnight. The observations were made daily, with scrupulous attention to accuracy, by Dr. Hugh Colquhoun of Glasgow, who was at that time my assistant.

Rate of evaporation in Glasgow.

		Elast. of vapour in atmosphere inches mer.	Evaporation inches mer.
May	1 fortnight	0·2707	0·1482
	2 ——	0·3494	0·1429
June	1 ——	0·3052	0·2259
	2 ——	0·2822	0·3232
July	1 ——	0·3526	0·2475
	2 ——	0·3819	0·2037
August	1 ——	0·4000	0·1854
	2 ——	0·3546	0·2415
September	1 ——	0·3790	0·1945
	2 ——	0·3404	0·1253
October	1 ——	0·3233	0·0766
	2 ——	0·3026	0·0798
November	1 ——	0·2808	0·0747
	2 ——	0·3232	0·0450
December	1 ——	0·2449	0·0295
	2 ——	0·2453	0·0212
1824. January	1 ——	0·2481	0·0604
	2 ——	0·2578	0·0535
February	1 ——	0·2468	0·0650
	2 ——	0·2170	0·0784

The following table exhibits the average elasticity of vapour in the atmosphere at Glasgow, and the rate of evaporation monthly during the time in which the observations were made.

		Elasticity of vapour in atmosphere.	Rate of evaporation.
1823.	May	0·3113	0·1454
	June	0·2937	0·2746
	July	0·3077	0·2249
	August	0·3766	0·2143
	September	0·3596	0·1666
	October	0·3126	0·0782
	November	0·3020	0·0598
	December	0·2431	0·0253
1824.	January	0·2531	0·0568
	February	0·2314	0·0720

At Funchal.

Few observations on the dew point have been kept in other parts of the world. Mr. Daniel has given us a table for London for three years. Dr. Heineken has marked the maximum and minimum dew point at Funchal in Madeira (about north latitude 32°½) monthly for the year 1828, as follows.*

	Maximum.	Minim.	Rain in inches.
January	65°	50°	4·08
February	56	50	1·64
March	65	48	1·68
April	63	45	3·35
May	69	51	2·14
June	70	54	0·21
July	72	61	0·10
August	73	63	0
September	75	69	1·39
October	74	56	0
November	72·5	54	2·56
December	67	50	0·52
			17·67

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* Brewster's Journal (new series), i. 34.

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We see that the quantity of rain has no relation to the height of the dew point. The year was uncommonly dry. The rain in 1825 was 20·43 inches; in 1826, 43·35 inches; in 1827, 18·15 inches. The mean temperature of Madeira is, according to Dr. Heineken, 66·°3.*

Importance of spontaneous evaporation.

8. The property which water has of evaporating spontaneously at all temperatures, is one of the most important in the whole economy of nature. For upon it the growth of plants, and the existence of living creatures upon the earth, depends. The vapours thus continually rising, not merely from the surface of the sea, lakes, and rivers, but also from the dry land, are again condensed and fall in the state of rain and dew. The rain penetrates into the earth, and makes its way out again in springs. These collecting together constitute rivers, which making their way to the sea, afford the means of living and enjoyment to numerous tribes and languages which occupy their banks. Let us suppose for a moment that this spontaneous evaporation were to cease, and let us contemplate the consequences. No more rain or dew could fall, the springs would cease to flow, the rivers would be dried up; the whole water in the globe would be accumulated in the ocean; the earth would become dry and parched; vegetables being deprived of moisture, could no longer continue to grow; the cattle and beasts of every kind would lack their usual food; man himself would perish; the earth

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* Brewster's Journal (new series), i. 41.

would become a dull, inanimate, steril mass, without any vegetables to embellish its surface, or any living creature to wander through its frightful deserts.

If the atmosphere contained no vapour whatever, the annual evaporation from the surface of water could easily be determined from the data already stated in this section, provided we were acquainted with the mean temperature of the place. But as the atmosphere is never free from vapour, we must either determine the mean quantity present by trial, or determine the actual evaporation by experiment. Now as far as evaporation is concerned, the surface of the globe presents three principal varieties; namely, water, ground covered with grass or other vegetables, and bare soil.

Evaporation from water.

Dr. Dobson made a set of experiments during the years 1772, 1773, 1774, and 1775, to determine the evaporation from the surface of water at Liverpool during these years. He took a cylindrical vessel of 12 inches diameter, and having nearly filled it with water, exposed it beside a rain gauge of the same aperture, and by adding water, or removing it occasionally, he kept the surface at nearly the same height. By carefully registering the quantities added or taken away, and comparing them with the rain that fell, the amount of evaporation was ascertained. The mean annual evaporation from the surface of water at Liverpool amounted to 36·78 inches. The mean annual fall of rain at Liverpool, as ascertained by Dr. Dobson, is (without reckoning the dew) 37·48 inches. We see at once from this that more rain falls at Liverpool than can be accounted for by the evaporation. Consequently there must be a supply of vapour from the sea, and probably from the warmer regions of the globe.*

From soil.

A set of experiments upon the evaporation from ground covered with vegetables and from bare soil was made by Mr. Thomas Hoyle and Mr. Dalton, at Manchester, during the years 1796, 1797, and 1798. They got a cylindrical vessel of tinned iron ten inches in diameter, and three feet deep. There were inserted into it two pipes turned downwards for the water to run off from it into bottles. One of these pipes was near the bottom of the vessel, the other was an inch from the top. This vessel was filled up for a few inches with gravel and sand, and all the rest of it with good fresh soil. It was then put into a hole in the ground, and the space around filled up with earth, except on one side for the convenience of putting bottles to the two pipes. Water was poured on to sadden the earth, and as much as would was suffered to run through without notice, by which the earth might be considered as saturated with water. For some weeks the soil was constantly above the level of the upper pipe, but latterly it was always a little below it; which made it impossible for any water to run through the upper pipe. For the first year the soil at top was bare, but during the last two years it was covered

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* Phil. Trans. 1777, p. 244.

with grass the same as a green field. Things being thus circumstanced, a regular register was kept of the quantity of rain water that ran off from the surface of the earth by the upper pipe (while that took place), and also of the quantity which sunk down through the three feet of earth, and ran out through the lower pipe. A rain gauge of the same diameter was kept close by to find the quantity of rain for any corresponding time. By this apparatus the quantity evaporated from the earth in the vessel during three years was ascertained. The annual evaporation was 25·158 inches. Now if to the rain we add 5 inches for dew (not reckoned in Mr. Dalton's observations), it follows that the mean annual evaporation from earth at Manchester amounts to 30 inches. It follows, likewise, from these observations of Dalton and Hoyle, that there is but little difference between the evaporation from *green soil* and bare soil. For the evaporation during the first year, when the soil in the vessel was bare, differed but little from that of the two following years when it was covered with grass.*

The mean annual evaporation at Glasgow may be estimated from the table which has been inserted in page 257 of this work. The mean elasticity of the vapour evaporated is 0·1283 inch. And it is probable that the elasticity for March and April, the two months not given, will not deviate much from the annual mean. Now 0·1283 is the elasti-

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* Manchester Memoirs, 1st series, v. p. 360.

city of the vapour of water at the temperature of 20°. From Mr. Dalton's table inserted in page 245 it may be estimated that at the temperature of 20° a circular vessel of water, 6 inches in diameter, if exposed to a perfectly dry atmosphere, would lose per minute 0·44 grain of its weight by evaporation. This in a year would amount to 916½ cubic inches. Hence it follows by a very easy calculation that the annual evaporation at Glasgow amounts to 32·413 inches.

I am disposed to take 32 inches as the mean annual evaporation all over Great Britain. It is certainly less than this in a great part of Scotland, and a portion of England. But I am persuaded that it exceeds it considerably in the south-east of England, and probably also in East-Lothian and Berwickshire. Let us now see how the rain and dew correspond with this evaporation.

Annual fall of rain.

9. The quantity of rain which falls annually in any place is determined by means of a rain gauge. This is usually a circular or square vessel having a surface of a determinate number of square inches. It is funnel shaped below, and terminates in a pipe which conducts the rain water to a bottle or some other vessel, where its quantity may be accurately measured. This rain gauge is set up in a field at a distance from every thing that may screen it or have a tendency to keep off the rain. When rain falls it enters into the vessel in proportion to the size of the surface, and being measured, and the quantity divided by the number of inches in the

area of the rain gauge, the quotient gives the depth of rain which has fallen in the place where the gauge is fixed.

Rain gauges.

There have been many disputes about the accuracy of rain gauges. It has been affirmed that the quantity of rain which falls into them depends entirely upon the wind, and that, therefore, we cannot know by means of them how much rain falls in any place unless they be so constructed that their surface shall always be perpendicular to the wind that blows. But it seems to me quite evident, that if a rain gauge be placed horizontally, and in a situation completely exposed on every side, and neither screened nor overlooked by any neighbouring building, exactly the same proportion of rain will fall into it as on the same area of the surrounding ground. Consequently it must (if the rain which falls into it all remain) give us a correct notion of the rain falling in the place where it is situated.

Less rain falls in high than low places.

There is indeed a curious fact, first pointed out by Dr. William Heberden, which has not yet been explained in a satisfactory manner. He placed a rain gauge on the square part of the roof of Westminster Abbey, at such a distance from the western towers as probably to be little affected by them. Another rain gauge was placed on the top of a house, but considerably lower than the first; and a third rain gauge on the ground in a garden belonging to the house. The observations were continued from July 1766 to July 1767, or a year. The rain water collected in each rain gauge was as follows:

Top of Westminster Abbey .	12·099 inches
Top of a house . .	18·139
On the ground . .	22·608*

This observation has been frequently repeated, and it has been found to hold constantly. More rain is always collected in the lower than in the upper gauge. This difference may in some situations be partly owing to the lower gauge being screened from the action of the wind. Suppose a rain gauge placed at the bottom of a high wall. Should the wind blow from the side of the wall opposite to that at the foot of which the gauge is placed while it rains, it is obvious that when the rain by sinking below the top of the wall is screened from the wind, it will fall perpendicularly downwards, whereas while above the wall it was driven obliquely by the wind. In consequence of this change of direction more rain would fall into the gauge situated at the bottom of the wall, than into that placed upon the top. But this explanation will not apply to the great multitude of cases where all the rain gauges are equally exposed to the action of the wind, and not screened by any higher building. It would seem, therefore, that the quantity of rain increases at a great rate as we descend in the atmosphere.

The annual quantity of rain is greatest at the equator, and gradually diminishes as we approach the pole. This will be evident from the following little table, showing the annual depth of rain in different latitudes.

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* Phil. Trans. 1769, p. 359.

	Lat. N.	Fall of rain.
Granada	12	126 inches.
Cape François	19° 46′	120
Calcutta	22 23	81
Rome	41 54	39
England	50 to 55	31
Petersburg	59 16	16
Uleaborg	65 1	13½

Number of rainy days.

On the contrary, the number of rainy days is smallest at the equator, and increases in proportion to the distance from it. From north latitude 12° to 43° the mean number of rainy days is 78.

From 43° to 46° the mean number is 103
—— 46° 50° do. do. 134
—— 50° 60° do. do. 161

It is obvious from this that the rain falls in very heavy showers in warm climates, and that it becomes more and more gentle as we advance towards the pole. Consequently in hot climates the air must be much less loaded with moisture, and the rate of evaporation much greater than in cold climates. For when rain falls very heavy a great portion of it runs off by the surface, and flows into the sea. But this happens to a much less extent when the fall of rain is more gentle.

Rain in Great Britain.

But instead of attempting to compare the quantity of rain and evaporation over the whole surface of the globe, for which we have not sufficient data, let us confine ourselves to Great Britain. The annual evaporation for Great Britain is probably about 32 inches. The following table exhibits the mean annual fall of rain in different places in Great Britain.

Manchester	36·140 inches
Liverpool	34·119
Chatsworth	27·664
Lancaster	39·714
Kendal	53·944
Dumfries	36·919
Glasgow	21·331
Edinburgh	24·5 nearly
Kinfauns Castle, near Perth,	
(1) On a hill, 600 feet above the sea	39·61 feet
(2) In the garden, 20 feet do. .	25·19
(3) On Kinfauns castle, 129 feet do.	20·76
London	23·10
Mean	31·882

But I have not inserted the mean rain at many places where it is very considerable. At Plymouth, for instance, it is not less than 44 inches. The annual fall in the Stockey muir, about 12 miles from Glasgow, is about 42·6 inches, at Largs about 43½ inches, and at Gordon Castle about 29·3 inches. I think, therefore, that (including the dew) the mean fall of rain over all Great Britain cannot be estimated at less than 36 inches.

Excess above evaporation.

If this estimate be a near approximation to the truth, the evaporation is less than the rain by about 4 inches. This excess must be supplied from the neighbouring seas. The 4 inches of rain not again elevated in the state of vapour must be annually carried into the sea by means of the different rivers.

Now a quantity of water which would cover the whole surface of Great Britain to the depth of 4 inches (supposing that surface level) would amount to 1,238,784,152,000,000 cubic inches, which is equal to 4,467,725,610,767 imperial gallons, or 17,729,069,884 tons, supposing the ton to contain 252 imperial gallons.*

Let us compare this with the quantity of water actually conveyed to the sea by some of the principal rivers in Great Britain.

Quantity of water in the Clyde.

The breadth of the Clyde at the New Bridge, Glasgow, is 410 feet, and its mean depth $3\frac{1}{2}$ feet. The velocity of the water at the surface is 1·23 inches, and the mean velocity of the whole water is 0·558,132 inch per second. From these data it may be inferred that the quantity of water discharged per second is $76\frac{2}{3}$ cubic feet. This amounts to 2,417,760,000 cubic feet, or 473,017,448 imperial gallons, or 1,877,053 tons. The river Clyde drains about $\frac{1}{30}$th of Scotland, or about $\frac{1}{83}$d part of Great Britain. Hence if the water discharged into the sea by the Clyde afforded a fair average of the whole island, the total amount of the water discharged annually by all the rivers in Great Britain would be only 155,795,399 tons, which does not amount to one hundredth part of the excess of the rain above the evaporation.

But there are two circumstances which serve in some measure to account for the enormous difference between the two quantities. The water dis-

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* In making this calculation I have supposed the area of Great Britain to be 77,244 square miles.

charged by the Clyde was determined in summer, when the river is always much smaller than during winter. It is therefore considerably under the real quantity, though I have not data to determine how much. The district drained by the Clyde is not nearly so rainy as many other tracts both in England and Scotland. Hence the water which it discharges should be less than the average for Great Britain. To this may be added that the size of the tract of country drained by the Clyde is somewhat arbitrarily estimated at $\frac{1}{30}$th of Scotland. The want of good maps makes it very difficult to determine how much it is in reality.

If our estimate of the difference between rain and evaporation be an approximation to the truth, it follows that $\frac{1}{9}$th of all the rain that falls is raised by evaporation from the sea. This is one of the reasons why the most rain falls on the west coast of Great Britain when the wind blows from the south-west and west, and most upon the east coast when it blows from the east. As the west wind blows for a much longer period of the year than the east wind, more rain falls on the west than the east coast of Great Britain. Glasgow, which is an inland city and at some distance from the mountains, is drier than Edinburgh, which is more contiguous to the eastern ocean, and much drier than Greenock or Largs, which are contiguous to the western ocean.

10. While the moisture present in the atmosphere continues in the state of vapour it is perfectly transparent and colourless; or at least it only communicates to the sky that lively blue colour which

Blue colour of the sky.

we observe it to possess when the atmosphere is loaded with vapour. This colour seems owing to the property which vapour has of absorbing the least refrangible rays of light, and transmitting only the most refrangible. For it is well known that the aspect of the sky becomes darker and darker in proportion as we ascend in it; and when we get to such a height that all the aqueous vapour is left below us the sky appears to the eye perfectly black. This is the aspect which it has from the top of Mont Blanc, as described by Saussure, and likewise from the summits of the Andes.

Clouds.

But though the presence of vapours cannot be detected in the atmosphere by their colour, yet we can easily judge when the air is loaded with them; because we can see to a much greater distance, and the outline of the distant mountains is much more distinct than when the air is dry. In this country it is a comparatively rare thing to see the sky perfectly transparent. Much more frequently the vapour assumes a visible form, or becomes that opaque fleecy looking matter which we denominate a *cloud*. Sometimes these clouds are very high, very thin, and very small in size. At other times they sink lower down and envelope the whole face of the sky. Sometimes two or even three layers of clouds can be seen at different heights above each other. Whenever this happens, one of the layers is moving with more velocity than the others, or we observe one layer moving in one direction, and another in a different one. Sometimes these clouds sink down to the very surface of the earth and envelope it, in which case they are known by the name of *mists* or *fogs*.

How formed.

There seems little reason to doubt that the cause to which Dr. Hutton ascribed the formation of rain, is in reality the cause of the formation of clouds. Dr. Hutton showed, by experiment, that when air of different temperatures, each containing as much vapour as is compatible with the temperature, are mixed together, a precipitation of moisture always takes place. A simple inspection of the table of the force of vapour given in page 245 will be sufficient to satisfy us that this must always be the case.

The force of vapour at	40° is 0·26 inch.
do. do.	60° is 0·52
The mean of which is	0·39
But the force of vapour at	50° is 0·36

Now since 0·36 is less than 0·39, it is clear that whenever two such strata are mixed, a precipitation of moisture must always take place. Again

The force of vapour at	60° is 0·52 inch.
do. do.	80° is 1·01
The mean of which is	0·76
But the force of vapour at	70° is only 0·726

showing clearly that when air at 60° and 80°, each saturated with moisture, are mixed, moisture must be precipitated.

Now whenever moisture is precipitated from air, in consequence of the mixture of air of different temperatures, this precipitated moisture always assumes the appearance of a *cloud* or *mist*. We have a good example in the cloud formed when steam issues from the spout of a boiling tea-kettle. The steam has the temperature of 212°, and we shall suppose the air at 60°, and saturated with moisture.

Force of vapour at 60° is 0·52 inch
Do. do. 212° is 30
The mean of which is 15·26

But the force of vapour at 136°, the intermediate temperature, is only 5·14 inches. Hence a great proportion of the vapour, must be precipitated. But this precipitated vapour, instead of assuming the form of drops of water, which one would have expected, is converted into a *cloud* or *mist*.

Nature of clouds.

Now *mist* or a *cloud* consists not of solid drops; but of a multitude of very thin vesicles of water, enclosing some aerial substance within them, similar to the vesicles usually blown from soap suds. That this is the structure of clouds and mists was affirmed by Derham and others, his contemporaries. Derham informs us, that he examined them by means of a microscope, and found them to be vesicles.* Indeed the vesicular structure is obvious from the circumstance of clouds continuing elevated at a considerable height in the atmosphere, and that fogs may be often seen elevating themselves up the sides of mountains. If clouds or fogs consisted of round *drops*, their precipitation would be rapid. For a drop whose diameter amounted to $\frac{1}{1000}$th of an inch would acquire a velocity of nine or ten feet

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* Derham's Physico-Theology, p. 48.

per second. Whereas we see clouds hover at a small elevation for hours, and they can in consequence be transported from the sea, lake, river, or marsh, from which they are raised, far into the country or to the tops of mountains, where the requisite supply of moisture cannot be had any other way.

Clouds composed of vehicles.

M. de Saussure, senior, while travelling in the Alps happened to be enveloped in a mist which was almost stagnant. He was astonished at the size of the drops, as he thought them, and at seeing them floating slowly past him without falling to the ground. Some of them were larger than the largest peas. Catching them in his hand he found them to be bladders, inconceivably thin, and he found upon examination that they all had this structure. Indeed the optical phenomena exhibited by clouds and mists, show clearly that they are of a vesicular structure. If clouds were composed of drops they would exhibit a rainbow every time the sun shone upon them, supposing the spectator placed between the sun and the cloud. No such appearance, however, ever takes place. But every person must have observed the *halos* which are formed when a portion of fog is interposed between our eye and the sun or moon. The same halo may be perceived when the cloud formed by the mixture of steam and air is interposed between our eye and a candle or the sun. According to the measurements of De Saussure, the diameter of the smallest of the vesicles, of which clouds are usually composed, is

Chap. III. about $\frac{1}{4222}$, and of the greatest about $\frac{1}{2620}$ of an English inch.

But though there is no doubt that clouds consist of a congeries of vesicles, we have no conception of the way in which these vesicles are formed. Nor is it easy to conceive why these vesicles are sometimes lighter than air, sometimes a little heavier, and sometimes exactly of the same specific gravity with the air in which they float. Indeed if the aerial matter with which these vesicles are filled were saturated with moisture, while the air in which they float is dry, we would see a reason why they should be lighter than air. On such a supposition the clouds should rapidly disappear. Accordingly we generally find that when clouds rise in the atmosphere they speedily diminish in size and at last vanish away; being gradually converted again into vapour. If the air within the vesicles were in the same state with respect to moisture as the air in which the cloud floats, the vesicles should be heavier than air, and constitute what we distinguish by the name of *fogs*.

The most probable cause of the difference of gravity between clouds and the air in which they float is a difference in their temperature from that of the surrounding medium. The formation of clouds seems to be connected with electricity, though in what way the vesicular form is induced by electricity, we have no conception. The vesicles seem to be all charged with the same kind of electricity. This causes them to repel each other, and of course prevents them from uniting into drops of rain.

Clouds then are formed whenever two strata of air of different temperatures, and each saturated with moisture, are mixed together. The sky in this country is usually much freer from clouds when the wind blows from the east than when it blows from the west. Because the temperature of the east wind being low, there is less chance during its continuance of strata of different temperatures mixing together. The comparatively high temperature of the west wind renders the chance of an intermixture of air of different temperatures during its continuance much greater. Should a west wind blow in the upper regions of the atmosphere, while an east wind blows near the earth, the whole sky would become clouded, because the contiguous portions of the two strata mixing together would occasion a deposition of moisture.

Formation of rain.

11. The formation of rain is still involved in impenetrable obscurity. Rain never falls in this country unless the sky be cloudy, and unless that peculiar kind of dense black cloud appear well known by the name of *rain cloud.* Whenever the particles constituting clouds lose their vesicular form and unite together in drops, rain falls. This change is probably connected with some electrical phenomena which are not yet understood. Clouds are attracted by mountains, and more rain falls in mountainous districts than in any other. We can conceive the mountain in the opposite electrical

state from the cloud. This would account for the attraction. When the cloud came close to the mountain its electricity would be abstracted, and the vesicles in consequence might collapse into drops.

No rain in the Valles.

12. In that part of Peru called Valles, which lies on the north and south side of Lima, in south latitude 12°, bounded on the east by the Andes, and on the west by the Pacific Ocean, it never rains at all. But during winter the earth is covered with so thick a fog as to intercept the rays of the sun. This fog appears almost every day during winter with a density that obscures objects at any distance. About 10 or 11 o'clock it begins to rise, but without being totally dispersed; though it is then no impediment to the sight, intercepting only the direct rays of the sun by day, and that of the stars by night. Sometimes it is so far dispersed that the disc of the sun becomes visible, but the heat from his rays is still precluded. In the winter season these vapours dissolve into a very small mist or dew, which they call *garua,* and thus every where moisten the earth. These garuas never fall in quantities sufficient to damage the roads or incommode the traveller; but they render the most arid and barren parts fertile. They convert the disagreeable dust in the streets of Lima into mud.

Now in that country the wind always blows from the south; that is from a colder to a warmer region. Sometimes it veers a point or two to the east. But it always blows between the south and south-east. When the fogs come on the south wind is barely felt, and a scarcely perceptible air seems to come from the north, which forms the fog.*

The obvious reason why it never rains in that country is that the wind constantly blows from a colder to a hotter part of the world. We see also the cause of the fogs. They are occasioned by the mixture of the hot air from the north with the colder air from the south.

Rain is produced by irregular winds. If the winds were always to blow steadily in the same direction no rain whatever would fall.

Rarely rains in Egypt.

When a country is quite flat, as is the case with Egypt, it seldom rains, although the winds are not quite steady. In Egypt it very seldom rains. During June, July, August, and part of September, the wind blows from the north. During the latter part of September it blows from the east. The winds are most variable about the winter solstice. From that to March they are mostly southerly.†

The heavy rains that fall in India always take place during the shifting of the monsoons, and while they last the winds are always variable. Even in this country steady dry weather is always accompanied by a steady direction of the wind, whereas in rainy weather the winds are unsteady and variable.

Rain owing to variable winds.

These facts are sufficient to show the connexion of rain with the variable nature of the winds.

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* See Ulloa's Voyage, vol. ii.

† See Volney's Travels.

CHAPTER IV.

OF IGNITION.

Chap. IV.

By *ignition* (perhaps the word *incandescence* would answer better) is meant a property which bodies possess of giving out *light* whenever their temperature is raised to a certain point. When a body is heated up to the temperature of incandescence we say in common language that it is *red hot.*

1. There is reason to believe that all bodies (susceptible of the property) become red hot at exactly the same temperature. But the exact point of incandescence has not been determined. There are two reasons for this. 1. It is higher than the range of a mercurial thermometer. Consequently we are not in possession of an instrument capable of measuring it with precision. 2. But even if we had such an instrument, it would not be easy to determine the point of incandescence, because that point is not well defined. We depend upon the acuteness of our sight to inform us of the beginning of ignition. Now we are not sure that we can perceive the very weakest light. The presumption is rather the contrary, as we know that other animals see objects in such weak light, as appears to us to be darkness. And even our eyes will in one con-

dition be affected by a weak light to which they are insensible at another time.

Bodies become red hot at 800°.

Sir Isaac Newton calculated the temperature at which a piece of iron begins to shine in the dark to be 635°. He supposed that the heat lost by such an iron, when suspended in a cold room in a given time, was always proportional to the excess of its heat above that of the surrounding medium. He measured the time which elapsed while the iron was cooling from a red heat till it became cold enough to bear the application of a thermometer, and then observed the rate of its cooling till it reached the temperature of the room. This enabled him to calculate what the original temperature of the iron had been. But this law of Newton, though very plausible and seducing, is very different from the real law of cooling. We are sure that the lowest point of ignition is higher than 635°. For the boiling point of mercury is 662°, and boiling mercury is quite invisible in the dark. Mr. Daniel measured, by means of his metallic pyrometer, the lowest point at which a heated iron appears red hot in day light, and found it 980° Fahrenheit. But this must be a good deal higher than the point of ignition of the same iron in the dark. Antimony, when heated to the melting point, appears visibly red hot in the dark. Now Dr. Cromwell Mortimer found the melting point of this metal to be 810°. From an experiment by Sir H. Davy it appears that fusible metal becomes luminous when heated to the temperature of 812°.* The point of ignition then is

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* Phil. Trans. 1817, p. 53.

even lower than 810°. We cannot err very far if we place the commencement of ignition at 800°, or between 750° and 800°. Mr. Princep has determined, by means of an air thermometer, that a full red heat indicates a temperature of 1200°. And what he calls an orange heat, a temperature of 1650°.

From the experiments of Mr. Thomas Wedgewood we have reason to conclude that all bodies susceptible of the requisite temperature become red hot at exactly the same point. Wood and most liquids cannot be made luminous by heat, because they are dissipated long before they reach the requisite temperature. But metals, stones, and earthy bodies, can all be heated to redness.

Gases do not become red hot.

There is, however, one remarkable exception to this. It does not appear that the gases become luminous even at a much higher temperature. The following ingenious experiment of Mr. T. Wedgewood seems to set the truth of this exception in a very clear point of view. He took an earthen ware tube, bent so in the middle that it could be sunk, and make several turns in a large crucible, which was filled with sand. To one end of this tube was fixed a pair of bellows; at the other end was a globular vessel, in which was a passage, furnished with a valve to allow air to pass out, but none to enter. There was another opening in this globular vessel filled with glass, that one might see what was going on within. The crucible was put into a fire; and after the sand had become red hot, air was blown through the earthen tube by means of the bellows. This air, after passing through the red hot sand, came into the globular vessel. It did not shine; but when a piece of gold wire was hung at that part of the vessel where the earthen ware tube entered, it became faintly luminous: a proof that though the air was not luminous, it had been hot enough to raise other bodies to the shining temperature.

Light heats bodies.

2. Thus it appears that whenever a body is heated to a certain point (about 800°) it immediately begins to emit light. On the other hand, whenever light is concentrated upon the surface of opaque bodies they always become hot. Indeed all opaque bodies become hotter when they are exposed to the direct rays of the sun. The degree of heat induced is influenced by the colour of the body; for *black* bodies, *ceteris paribus*, acquire the *most*, and *white* bodies the *least* heat. When the colour of a body is neither white nor black, the degree of heat induced is proportional to the depth of the shade. Supposing the shades equal, *blue* is the colour that becomes hottest next to black; *green* and *red* follow next after blue; and *yellow* is next to white. The reason seems to be that the intensity of these colours, measured by depth of shade, is in the order in which they have been named. *Blue* may be so intense as to differ but little from *black*. Gadolinite has so deep a shade of green that to the eye it appears almost black. The same remark applies to some varieties of hornblende. Red and yellow are such lively colours that they seldom are so deep as to be mistaken for black.

Heat from the solar rays.

The degree of heat producible by the direct rays of the sun is not easily ascertained; because it is apt to be dissipated as fast as it accumulates. M. de Saussure made a little box lined with fine dry cork, the surface of which was charred to make it black and spongy, in order that it might absorb the greatest possible quantity of the sun's rays, and be as bad a conductor of caloric as possible. It was covered with a thin glass plate. When this box was set in the sun's rays, a thermometer laid in the bottom of it rose in a few minutes to 221°; while the temperature of the atmosphere was only 75°.* Professor Robison constructed an apparatus of the same kind, employing three very thin vessels of flint glass, which transmit more heat than any of the other species of glass. They were of the same shape, arched above, with an interval of $\frac{1}{3}$ inch between them. They were set on a cork base prepared like Saussure's, and placed on down contained in a pasteboard cylinder. With this apparatus the thermometer rose often in a clear summer day to 230°, and once to 237°. Even when set before a bright fire, the thermometer rose to 212°.† But when its rays are concentrated by a burning-glass, they are capable of setting fire to combustibles with

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* Voyages sur les Alpes, ii. 932.

† Black's Lectures, i. 547. When the apparatus was carried to a damp cellar before the glasses were put in their places, so that the air within was moist, the thermometer never rose above 208°. Hence Dr. Robison concluded, that moist air conducts better than dry; a conclusion fully confirmed by the subsequent experiments of Count Rumford.

ease, and even of producing a temperature at least as great, if not greater, than what can be procured by the most violent and best conducted fires. In order to produce this effect, however, they must be directed upon some body capable of absorbing and retaining them; for when they are concentrated upon transparent bodies, or upon fluids, mere air, for instance, they produce little or no effect whatever.

Count Rumford has shown by direct experiment, that the heating power of the solar rays is not increased by concentrating them into a focus, but that the intensity of their action is occasioned by a greater number of them being brought to bear upon the same point at once.*

Lavoisier, by means of a powerful burning-glass, not only melted gold, but actually caused it to evaporate. For a piece of silver placed at some height above the gold was sensibly gilded. The utmost heat of a glass-house furnace acting on gold for a month produced no sensible evaporation of that metal whatever.

Rays of the moon not hot.

Thus it appears that when heat is accumulated in bodies to a certain amount it causes them to emit light,—and that, on the other hand, when light is accumulated in them, it causes them to become hot. There is indeed one apparent exception to this general law. The light of the moon, though concentrated by the most powerful burning glasses, is not capable of raising the temperature

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* Jour. de Phys. lxi. 32.

of the most delicate thermometer. M. de la Hire collected the rays of the full moon when in the meridian by means of a burning-glass 35 inches in diameter, and made them fall upon the bulb of a delicate air thermometer. No effect was produced though the lunar rays by this glass were concentrated 306 times.* But this is not surprising when we consider the prodigious difference between the intensity of the solar and lunar rays. M. Bouguer, by a very simple and ingenious set of experiments, has shown that the illuminating power of a solar ray is 300,000 times greater than that of a lunar ray of the same size.† Indeed, we may satisfy ourselves by a very simple calculation that this estimate of Bouguer is not below the truth. During a part of every lunation we have an opportunity of seeing the moon in the sky at the same time with the sun. If we compare the moon under such circumstances with a white cloud, when any such happen to be in its neighbourhood, the brightness of the moon and the cloud appear to the eye very nearly identical. So that the moon emits no more light than is emitted during the day by a white cloud of the same visual size. When the whole sky is covered with white clouds the direct rays of the sun are intercepted; but the light of the day is very nearly the same as if the sky were unclouded and the sun shining. It follows from this that the light emitted by the moon is to that emitted by the sun as the apparent size of the moon to that of the whole sky.

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* Mem. Paris, 1705, p. 346.

† Ibid. 1726, p. 12.

Now the moon when its visual diameter is a maximum does not cover one millionth part of the hemisphere of the sky. But let us take Bouguer's measurement, and let us admit that the direct rays of the sun are capable of elevating the thermometer 237°. The rays of the moon would only possess $\frac{1}{300000}$th part of this effect, or $\frac{1}{1250}$th part of a degree. So that even when concentrated 306 times the effect would scarcely amount to $\frac{1}{5}$ of a degree. But I am satisfied that the light of the moon is at least $\frac{1}{1000000}$ of times less intense than that of the sun, so that their heating power, even when concentrated 306 times, could not amount to so much as $\frac{1}{20}$th of a degree.

Why do heat and light mutually evolve each other.

3. How comes it that heat and light are mutually disposed to produce each other? Various solutions of this question have been offered at different times by those philosophers who are fond of speculating on such subjects. It was believed that there exists a mutual repulsion between the particles of light and heat, and hence it was inferred that the accumulation of either would occasion the expulsion of the other. And this was offered as a sufficient explanation of the phenomena. But when we examine appearances somewhat closely they afford but little to induce us to adopt such an opinion; if they be not (as I think they are) directly inconsistent with it.

The quantity of light given out by bodies always increases with the temperature. At first it is very feeble, then it becomes a dingy red, then a bright red, then yellow or orange, and at last so intense a white that the eye cannot behold it. Now, if we adopt the notion that the light thus emitted existed previously in the luminous body, we must admit that every body in nature possesses the very same quantity of light, and this quantity must be allowed to be inexhaustible. For the light continues as long as we keep up the temperature. An ounce of gold might be kept red hot for a month or a century, and that by communication of heat from a body not itself luminous, as heated air, without any diminution of the intensity of the light which it is constantly emitting. The same remarks must apply to the quantity of heat which all bodies contain. For the heat emitted by them is always proportional to the quantity of light concentrated upon them. So that by powerful burning-glasses as high a temperature is produced in bodies as can by any other method. Thus the hypothesis that we are considering supposes that all bodies contain an infinite quantity both of light and heat—a supposition quite inconsistent with the very hypothesis from which it flows.

A second hypothesis supposes that light and heat are absolutely the same thing—the only difference between them being that light is radiated in straight lines through space, while heat exists fixed in bodies. It produces the phenomena of light while it is moving in rays, and the phenomena of heat while it is fixed in bodies. This hypothesis accounts tolerably well for the mutual evolution of light by heat, and heat by light; but it is inconsistent with a number of well established facts, which have been already stated while treating of the *radiation of heat.* For nothing is better ascertained than that heat is emitted in straight lines from surfaces.

They seem mutually convertible.

In all probability light and heat, though not absolutely the same, are yet mutually convertible into each other. Upon what the difference between them depends cannot be explained. Whether they move with different degrees of velocity, or whether it be that the interval between the particles of heat when in motion is too great to produce in us the sensation of vision; it may be that to produce this sensation a succession of particles following each other within a determinate interval of time may be necessary. This difference in the interval may perhaps account for the different kinds of rays, which exist in the solar beam. But upon this difficult subject nothing better than conjecture can be offered.

Phosphorescent bodies.

4. There are several bodies which seem to contain light as a constituent; since it can be driven off from them by the application of a moderate heat. The quantity of light which they thus contain is not very great. Yet while it is making its escape, the bodies appear luminous, as if they were surrounded by a low lambent flame, varying in colour according to that of the body from which the light is issuing. The minerals which possess the property of emitting light when heated, are called *phosphorescent.* Fluor spar, and phosphate of lime, are the two minerals which possess this property in the greatest perfection. Almost every variety of fluor spar is more or less phosphorescent;

but some kinds are much more so than others. The same remark applies to phosphate of lime; the kind which is most phosphorescent of all is the compact variety from Estremadura in Spain. There are some kinds of calcareous spar, which likewise phosphoresce; and the *tremolite* seems to owe its phosphorescing quality to the carbonate of lime with which it is usually contaminated.

There are some bodies which seem to contain light as a constituent, and which they appear to part with first, when they begin to undergo spontaneous decomposition. That this is the case, has been rendered very evident by a set of experiments made long ago by Mr. Canton,* and repeated and carried a great deal farther by Dr. Hulme.† It has been long known that different kinds of meat and fish, just when they are beginning to putrefy, become luminous in the dark, and of course give out light. This is the case in particular with the whiting, the herring, and the mackerel. When four drams of either of these are put into a phial containing two ounces of sea water, or of pure water holding in solution ½ a dram of common salt, or two drams of sulphate of magnesia, if the phial be put into a dark place, a luminous ring appears on the surface of the liquid within three days, and the whole liquid, when agitated, becomes luminous, and continues in that state for some time. When these liquids are frozen, the light disappears, but is again emitted as soon as they are thawed. A

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* Phil. Trans. lix. 446.

† Ibid. 1800, p. 161.

moderate heat increases the luminousness, but a boiling heat extinguishes it altogether. The light is extinguished also by water, lime water, water impregnated with carbonic acid gas, or sulphuretted hydrogen gas, fermented liquors, spirituous liquors, acids, alkalies, and water saturated with a variety of salts, as sal-ammoniac, common salt, sulphate of magnesia; but the light appears again when these solutions are diluted with water. This light produces no sensible effect on the thermometer.* After these experiments, it can scarcely be denied that light constitutes a component part of these substances, and that it is the first of the constituent parts which makes its escape when the substance containing it is beginning to be decomposed.

Most bodies absorb light.

Father Beccaria, and several other philosophers, have shown us, by their experiments, that there are a great many substances which become luminous after being exposed to the light.† This property was discovered by carrying them instantly from the light into a dark place, or by darkening the chamber in which they are exposed. Most of these substances, indeed, lose their property in a very short time, but they recover it again on being exposed to the light; and this may be repeated as often as we please. We are indebted to Mr. Canton for some very interesting experiments on this subject, and for discovering a composition which possesses this

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* The same experiments succeed with Canton's pyrophorus, as Dr. Hulme has shown.

† Phil. Trans. lxi. 212.

property in a remarkable degree.* He calcined some common oyster shells in a good coal fire for half an hour, and then pounded and sifted the purest part of them. Three parts of this powder were mixed with one part of the flowers of sulphur, and rammed into a crucible which was kept red hot for an hour. The brightest parts of the mixture were then scraped off, and kept for use in a dry phial well stopped.† When this composition is exposed for a few seconds to the light, it becomes sufficiently luminous to enable a person to distinguish the hour on a watch by it. After some time it ceases to shine, but recovers this property on being again exposed to the light.

The only effect, which heat had on this pyrophorus was to increase the separation of light from the pyrophorus, and of course to shorten the duration of its luminousness. Two glass globes hermetically sealed, containing each some of this pyrophorus, were exposed to the light and carried into a dark room. One of them, on being immersed in a basin of boiling water, became much brighter than the other, but in ten minutes it ceased to give out light: the other remained visible for more than two hours. After having been kept in the dark for two days, they were both plunged into a basin of hot

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* Phil. Trans. lviii. 327.

† Dr. Higgins has added considerable improvements to the method of preparing Canton's pyrophorus. He stratifies the oyster shells and sulphur in a crucible without pounding them; and after exposing them to the proper heat, they are put into phials furnished with ground stoppers.

water; the pyrophorus which had been in the water formerly did not shine, but the other became luminous, and continued to give out light for a considerable time. Neither of them afterwards shone by the application of hot water; but when brought near to an iron heated so as scarcely to be visible in the dark, they suddenly gave out their remaining light, and never shone more by the same treatment: but when exposed a second time to the light, they exhibited over again precisely the same phenomena; even a lighted candle and electricity communicated some light to them.

Light from friction.

Many bodies emit light from friction. The way in which the effect is produced, in this case is not well understood. It seems to be connected with electricity. The evolution of sparks when flint and steel are struck against each other, is a case of combustion, and does not come under the class of bodies that become luminous by friction.

CHAPTER V.

OF COMBUSTION.

Chap. V.

THIS effect of heat differs from those already treated of in one remarkable and important particular; it is not universal. There are many substances which are not susceptible of undergoing combustion, though subjected to ever so high a temperature,—and there are some bodies which are capable of undergoing combustion without the application of any artificial heat whatever. It is, however, a very general effect of heat, and it deserves to be treated somewhat in detail, because the earliest attempts at generalizing in chemistry were founded upon a theory of combustion; and because the different theories of combustion have had a most striking effect upon the progress and aspect of the science.

Meaning of the term.

1. By *combustion* is meant, in common language, a remarkable evolution of *heat* and *light* or of *fire* when certain bodies combine together. This is well exemplified in the burning of wood, or pit coal, or oils, or spirits in the open air. These bodies give out an enormous quantity of heat and light during their combustion. The substance with which they combine is the oxygen of the atmosphere. When phosphorus is raised to the temperature of

148°, it catches fire, and burns with great splendour, combining with the oxygen of the atmosphere, and giving out a vast quantity of heat and light. Hydrogen gas requires a red heat to cause it to begin to burn. It then unites rapidly with the oxygen of the atmosphere, and gives out little light, but a great quantity of heat.

In all these cases of combustion we only see one of the substances which combine. The oxygen of the atmosphere, though just as essential an ingredient, is not visible, and its agency was long unknown. This is the reason why the wood, the pit coal, the oil, the phosphorus, the hydrogen, has received, in common language, the name of *combustible*. As both the bodies commonly called combustibles, and the oxygen of the atmosphere, are essentially necessary for combustion,—as during the combustion they unite together, and as the evolution of light and heat, which constitutes the most striking part of the phenomenon, are the immediate consequence of this union, it is evident that in strict propriety of language the oxygen is as well entitled to the name of combustible as the other body with which it unites. But as common language was fixed before the nature of the process of combustion was understood, the term *combustible* came to be applied to one of the two substances which unite together, and not to the other; to that, namely, which is the most conspicuous. It would be in vain for us to attempt to alter the common language, and to affirm that the oxygen is just as much a combustible as the wood or the pit

Combustible, what.

coal which we burn on our fires. We would never be able to prevail upon mankind to adopt our nomenclature. It will be better for us then to apply the term *combustible*, as it is done in common life, and we may distinguish the other constituent by the name *supporter of combustion*.

Supporter, what.

Combustion then is a union of a *combustible* with a *supporter*, attended with the evolution of *heat* and *light* or of *fire*.

Combination occasions a change of temperature.

2. No chemical combination of two bodies with each other ever takes place without a change of temperature. Sometimes the temperature sinks, as when sulphate of soda is dissolved in a dilute mineral acid, or when snow and common salt are mixed together. Cold is produced only when solid bodies become liquid by uniting together. And the more rapidly they liquefy, the greater is the intensity of the cold produced. In the greater number of chemical combinations heat is evolved, and the more rapidly the combination takes place, and the more intimate it is, the greater is the evolution of heat. Those combinations, which are accompanied by the evolution of heat, always undergo an increase of density, or the particles constituting them approach nearer each other than they were before. An example will make this approach of the particles obvious.

Usually an increase.

Water is a compound of

2 volumes hydrogen gas,
1 volume oxygen gas,

united together and condensed into a liquid.

Sp. gravity of hydrogen gas $0{\cdot}069\dot{4}$
oxygen gas $1{\cdot}111\dot{1}$

that of common air being unity.

	Grains.
2 cubic inches of hydrogen gas weigh	0·043228
1 cubic inch of oxygen gas weghs	0·346048
	0·389276

Weight of 3 cubic inches of the constituents of water before combination	0·389276 gr.
Weight of 3 cubic inches of water	757·38 gr.

And why.

Now, the volumes before and after combination are inversely as these weights. From this it follows that 1700 cubic inches of the constituents of water, when they combine together and assume the form of water, occupy little more than 1 cubic inch. So that the particles approach very nearly 12 times nearer each other than when they were in the gaseous state. This is an enormous condensation. Now, the heat evolved during the rapid union of oxygen and hydrogen gases is the greatest that we have it in our power to produce.

Combustion only follows rapid combination.

3. It is only when bodies combine with each other rapidly that their combination is accompanied by combustion. When the combination takes place slowly, the total quantity of heat evolved may be the same as in the rapid combination; but as it is evolved slowly, it is dissipated as it appears, and never accumulates in sufficient quantity to constitute *fire*, which always makes its appearance when heat is accumulated in a body to a certain amount. This is probably the reason why combustion is confined to cases of rapid combination.

Number of supporters.

4. In all cases of combustion which occur in common life, the supporter of combustion, which combines with the burning body, is the oxygen of the atmosphere. But there are three other bodies which possess analogous properties to oxygen. They accumulate, like it, round the positive pole of the galvanic battery. Like it, they combine with almost all the simple bodies, and form, with them, compounds of an analogous nature. These three bodies are *chlorine, bromine,* and *iodine*: the first of which is a gas like oxygen, the second a liquid, and the third a solid. These four bodies, from the analogy of their properties, may be conveniently classed together, and they may be distinguished by the name of *supporters of combustion.* Of these *chlorine* is the best, and *iodine* the worst supporter; probably owing to the state of their aggregation. The similarity of chlorine, bromine, and iodine, to each other, is much greater than their analogy with oxygen, which they resemble chiefly in their property, like it, of combining with all simple bodies, and of acting occasionally as supporters of combustion.

Sulphur and phosphorus occasionally act the parts of supporters. When sulphur, in a liquid state, is made to combine rapidly with copper or zinc or iron, and perhaps also with some other bodies, it becomes solid in the instant of union, and the new compound becomes red hot, and exhibits all the phenomena of a short combustion. When liquid phosphorus is brought in contact with hot lime, barytes, or strontian, a rapid combination takes place, and all the phenomena of a brilliant combustion present themselves. In this last case the oxygen previously united with the calcium, barium, and strontium, suddenly enters into combination with the phosphorus, and may contribute to the combustion; but probably the greatest share of the effect is due to the sudden change of the phosphorus from a liquid to a solid state.

Potassium also frequently produces the phenomena of combustion, and may therefore be considered as an occasional supporter. And doubtless sodium and the metallic bases of the alkaline earths would show similar properties if we had it in our power to procure them. In all these cases, the potassium is liquid, and suddenly changes by combining with oxygen or with another metal into a solid body.

Number of combustibles.

The combustible bodies consist of all the simple substances, with the exception of the four supporters and *azote*. There exist likewise some striking anomalies among the simple bodies, in the phenomena of their union with the supporters. But it will be better to leave the consideration of these phenomena, till we come to treat of those bodies themselves in which they occur.

Glowing explained.

5. There is another set of phenomena which have been considered as connected with combustion; but which I rather think should be classed as instances of phosphorescence. When the green oxide of chromium is heated nearly to redness, it becomes of itself red hot, and glows for some time like a live coal. By this glowing it undergoes no

alteration in its weight; but becomes much more difficultly soluble in acids than formerly, and its specific gravity is probably increased. This glowing does not always take place; though it is a very common property of the oxide. No doubt the presence or the absence of the property depends upon the mode of preparing the oxide. I consider the light that escapes in this example as having been chemically combined, and adhering so strongly, that a pretty high temperature is requisite to drive it off. In like manner when the mineral called *gadolinite* is exposed to a heat very nearly amounting to a red heat, it becomes red hot and glows for a little time with considerable brilliancy. This also I consider as a case of phosphorescence.

What shows the agreement of these phenomena with the phosphorescence of fluor spar and phosphate of lime, is that though no sensible alteration is made in the weight or chemical constitution of the substances; they cannot be made to glow a second time by exposure to heat. The green oxide of chromium may indeed have its property of glowing restored by heating it with carbonate of potash or soda, which converts it into chromic acid, digesting the chromate thus formed with oxalic acid, which reduces the acid to green oxide, and then throwing it down by ammonia. But fluor spar and phosphate of lime recover their phosphorescing quality when decomposed and again united, as was ascertained by Scheele; so that those bodies which phosphoresce, and those which glow, resemble each other in every part of the phenomenon.

Quantity of heat evolved during combustion.

6. To estimate the quantity of heat evolved during the burning of different combustibles is not only important in a philosophical point of view, but of considerable consequence also, as an object of economy. A set of experiments on this subject was made by Lavoisier and Laplace. They burnt various bodies in the calorimeter, and estimated the heat evolved by the quantity of ice melted in each experiment. Dr. Crawford made a similar set of experiments. He estimated the heat evolved by the increase of temperature which the water experienced with which he contrived to surround the burning bodies.* A still more numerous set of experiments has been made by Mr. Dalton, chiefly on the heat evolved during the combustion of gaseous bodies. He filled a bladder capable of holding 30,000 grains of water with the gas; this bladder was fitted with a stop-cock and a pipe. A tinned vessel was procured capable of holding 30,000 grains of water; the specific heat of which being ascertained, and as much water added as made the specific heat of both equivalent to that of 30,000 grains of water, the gas was squeezed out of the bladder, lighted, and the extremity of the flame made to play upon the bottom of the tinned vessel. The quantity of heat evolved was estimated by the increase of temperature produced upon the water in the vessel.†

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* See his experiments on animal heat, pp. 254, 320, 333.

† Dalton's New System of Chemical Philosophy, p. 76. When the substances burnt were not gaseous, they were converted into a lamp and burnt with or without a wick as the case required.

Chap. V.

A very numerous set of experiments was likewise made by Count Rumford on the heat evolved during the combustion of oils, spirits, and various woods; and from the length of time which he devoted to the subject, and the numerous precautions to which he had recourse, it is probable that his results are near approximations to the truth.*

These different experiments cannot well be compared together, in consequence of the great difference in the mode of conducting them. In Dalton's experiments a good deal of the heat would of necessity be lost, yet as all the gases were placed as nearly as possible in the same circumstances, they ought to give us, though not the absolute heat evolved, yet the relative quantity given out during the combustion of each of the gases which he employed. I shall therefore give them in the following table.

Experiments of Dalton.

Substance burnt 1 lb.	Oxygen consumed in lbs	Ice melted in lbs.	Ratios of heat.
Hydrogen gas	8	320	16
Carburetted hydrogen	6	85	4·25
Olefiant gas	4·375	88	4·4
Carbonic oxide	0·4375	25	1·25
Olive oil	3·033	104	5·2
Wax	3·14	104	5·2
Tallow	—	104	5·2
Oil of turpentine	—	60	3
Alcohol	3·477	58	2·9
Sulphuric ether	4·296	62	3·1
Phosphorus	1·333	60	3
Charcoal	2·666	40	2

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* Nicholson's Journal, xxxii. 105; xxxiv. 319, and xxxv. 95.—Gilbert's Annalen, xlv. 1, 306.

Substances burnt 1 lb.	Oxygen consumed in lbs.	Ice melted in lbs.	Ratios of heat.
Sulphur	1·5	20	1
Camphor	3·021	70	3·5
Caoutchouc	5·265	42	2·1

In these experiments, the greatest loss of heat would be sustained in the burning of the charcoal because it does not give out flame. Camphor and oil of turpentine would also be underrated, because much of their carbon flies off unburnt in the form of lamp black.

Of Davy.

Sir Humphry Davy, in his very curious and interesting researches on flame, has given us the results of a set of experiments on the heat produced by the combustion of five different gases. The gases were made to issue from the mouth of a platinum tube, and the jet being inflamed was made to play upon the bottom of a copper vessel containing oil, raised previously to the temperature of 212°. The bulk of each gas consumed, and the time of burning, were in each case as nearly as possible the same. The following are the results.*

6	Olefiant gas heated the oil to	270° or	58°
1	Hydrogen	238	26
4	Coal gas	236	24
3	Sulphuretted hydrogen	232	20
1	Carbonic oxide	218	6

The numbers in the first column give the relative quantities of oxygen consumed during these combustions. These results cannot well be compared with those in the preceding table. It is obvious

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* Phil. Trans. 1817, p. 52.

that much heat would be dissipated during the continuance of the experiment, in consequence of the previously high temperature of the oil.

It is not easy to see why the oil was previously heated to 212°. It could have no other effect than that of rendering the loss of heat greater than it otherwise would be.

Of Lavoisier, Crawford, Dalton, and Rumford.

The following table exhibits the result of the experiments of Lavoisier, Crawford, Dalton, and Rumford.

Rumford. Substances burnt 1 lb.	Oxygen consumed in lbs.	Ice melted in lbs.	Experimenter.
Hydrogen	8	295	Lavoisier
		480	Crawford
		320	Dalton
Charcoal	2·66	96·5	Lavoisier
		69	Crawford
		40	Dalton
Phosphorus	1·33	100	Lavoisier
Olive oil	3·033	148	Lavoisier
		89	Crawford
		104	Dalton
		93·073	Rumford
Rape oil	3·033	124·097	Rumford
Wax	3·14	133	Lavoisier
		97	Crawford
		104	Dalton
		126·242	Rumford
Tallow	—	104	Dalton
		111·582	Rumford
Alcohol	3·477	67·47	Rumford
Sulphuric ether	4·296	107·027	Rumford
Naphtha	—	97·834	Rumford

The result of Count Rumford's experiments on the combustion of woods will be seen in the following table.

Species of wood.	Quality.	Ice in lbs. melted by the heat developed during the burning of a lb. of the combustible.
Lime . .	Joiner's dry wood, 4 years old	46·145
Ditto . .	Ditto	46·406
Ditto . .	Ditto, highly dried over a chafing dish . . .	52·806
Ditto . .	Ditto	54·210
Ditto . .	Ditto, rather less dried	51·777
Beech . .	Joiner's dry wood, four or five years old . .	45·089
Ditto . .	Ditto	45·002
Ditto . .	Ditto, highly dried over a chafing dish . . .	48·445
Ditto . .	Ditto	48·245
Elm . .	Joiner's wood, rather moist	36·196
Ditto . .	Joiner's dry wood, four or five years old . .	40·478
Ditto . .	Ditto	40·068
Ditto . .	Ditto, highly dried over a chafing dish . . .	46·020
Ditto . .	Ditto	44·868
Ditto . .	Ditto, dried and scorched in the stove	41·200
Oak . .	Common fire wood in moderate shavings . .	34·120
Ditto . .	Ditto, in thicker shavings, leaving a residuum of charcoal	32·997
Ditto . .	Ditto, in thin shavings	35·062
Ditto . .	Ditto, in thin shavings, well dried in the air . .	38·946
Ditto . .	Joiner's wood, very dry, in thin shavings . .	39·840
Ditto . .	Ditto	39·728
Ditto . .	Thick shavings, leaving 0·92 grains of charcoal .	34·969
Ash . .	Joiner's common dry wood	40·888
Ditto . .	Same kind, shavings dried in the air . . .	44·960
Ditto . .	Ditto, highly dried over a chafing dish . . .	47·265
Maple .	Seasoned wood, highly dried over a chafing dish .	48·156
Service . .	Ditto, ditto	48·173
Ditto . .	Same kind, scorched in a stove	43·116
Cherry . .	Joiner's dry wood	44·452
Ditto . .	Same kind, highly dried over a chafing dish .	49·205
Ditto . .	Same kind, scorched in a stove	46·350
Fir . . .	Joiner's common dry wood	40·429
Ditto . .	Shavings well dried in air	45·333
Ditto . .	Highly dried over a chafing dish . . .	49·838
Ditto . .	Dried and scorched in a stove	44·477
Ditto . .	Thick shavings, leaving much charcoal . .	38·260
Poplar . .	Joiner's common dry wood	46·134
Ditto . .	Same kind, highly dried over a chafing dish .	49·548
Hornbeam	Joiner's dry wood	42·400
Ditto . .	Ditto	42·145
Oak . .	With 19·6 per cent. of water imperfectly burnt, leaving in the combustion a charcoal residuum amounting to . . . . . } 0·81 gramme 0·73 0·94	35·228 34·121 34·556

Conclusions.

It is not very easy to draw any conclusion from these experiments. The enormous differences between the results obtained by Lavoisier, Crawford, and Dalton, when they operated upon the same substance, destroys all reliance upon the accuracy of the experiments. The most important of all the substances tried was hydrogen and charcoal, because they constitute almost the only combustible constituents of the substances commonly employed for the purpose of fuel.

Now from Mr. Dalton's mode of experimenting it is clear that his result must have been below the truth. It is evident therefore that Lavoisier's statement of the quantity of heat evolved during the combustion of hydrogen is greatly below the truth. There are two inaccuracies in the calculations employed by Dr. Crawford. He overrated the specific gravity of hydrogen gas, and likewise its specific heat. These occasioned an error in his estimate in excess. I think it probable, that we will not be far from the truth if we say, that 1 lb. of hydrogen gas, while burning, gives out as much heat as would melt 400 lbs. of ice, which is equivalent to 56,000 degrees. Or it would raise the temperature of 1 lb. of water 56,000 degrees.

All the experimenters would be likely to underrate the heat evolved during the combustion of charcoal. I think therefore that we may estimate the heat evolved during the combustion of 1 lb. of charcoal, as sufficient to melt 100 lbs. of ice. This is equivalent to 14,000 degrees of heat, or it would raise the temperature of 1 lb. of water 14,000 degrees.

But the quantity of oxygen consumed during the combustion of a lb. of hydrogen and a lb. of charcoal is very different. The hydrogen requires 8 times its weight of oxygen, while the charcoal requires only 2·66 times its weight of that principle. Hence it is obvious, provided our estimates be tolerable approximations to the truth, that the heat evolved during the union of 1 lb. of oxygen with hydrogen, would melt 50 lbs. of ice, while the heat evolved, during the union of 1 lb. of oxygen with carbon would melt only 37·6 lbs. of ice. Thus it appears that the heat evolved during the burning of hydrogen and charcoal, is not equal whether we compare the action during the union of equal weights of oxygen, or of the combustibles themselves.

The results would approach somewhat nearer equality, if we were to compare the atomic weights of each, which seems to be the most reasonable way of considering the subject. The atom of carbon is 6 times as heavy as that of hydrogen. But during its combustion it combines with two atoms of oxygen, while hydrogen unites only with one atom. To compare the effect of the union of the same number of atoms of oxygen with hydrogen and carbon, we should multiply the heat from the combustion of the same weight of carbon as hydrogen, by 3. It is obvious that when the same number of atoms of oxygen unite with hydrogen and with carbon, the heats evolved in both cases are to each other, as 4 to 3. If the heat be reckoned 4 when the hydrogen combines with oxygen, it will be only 3 when the carbon unites with oxygen. If Lavoisier's experiments on the combustion of phosphorus be correct, the heat which it gives out is double that given out by carbon. So that when the same number of atoms of hydrogen, carbon, and phosphorus, unite each with one atom of oxygen, the heats given out are as follows:

Ratios of the heats evolved.

Phosphorus . .	6
Hydrogen . .	4
Carbon . . .	3

An accurate set of experiments on this subject, could not fail to give us many interesting and highly useful results.

Indeed a set of experiments has been lately made by M. Dupretz. But he has merely published the results without entering into any details. It is therefore impossible to know how far his results can be depended on. According to him when one gramme of oxygen combines by combustion with the following bodies, the heat evolved is

For Hydrogen	2578° centigrades
Carbon	2967
Iron .	5325

Phosphorus, zinc, and tin, disengage nearly the same quantity of heat that iron does.*

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* Ann. de Chim. et de Phys. xxxvii. 180. The ratios of the heat given out when one atom of these bodies unites with one atom of oxygen are as follows:

Hydrogen . .	5
Carbon . . .	5·75
Iron . . .	10·25

Order in which bodies begin to burn.

7. Every substance seems to have a temperature of its own, at which it is capable of burning; and it never burns till it is raised to the requisite temperature. The following is the order of combustibility of different bodies according to the observations of Davy, beginning with the body which burns at the lowest temperature.*

Phosphorus,
Phosphuretted hydrogen gas,
Hydrogen and chlorine,
Sulphur,
Hydrogen and oxygen,
Olefiant gas,
Sulphuretted hydrogen,
Alcohol,
Wax,
Carbonic oxide,
Carburetted hydrogen.

Thus it appears that carburetted hydrogen is the gas which requires the highest temperature, and phosphuretted hydrogen the lowest temperature to maintain its combustion.

When these bodies are burnt in rarefied air, the heat evolved is less, because a smaller quantity burns at once. And whenever the temperature produced is not sufficient to maintain the combustion, the flame is extinguished.† Hence, phosphorus will burn in air rarefied 60 times, while carburetted hydrogen gas will not burn in air rarefied four times.

When gases are rarefied by heat, their combusti-

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* Phil. Trans. 1817, p. 48.

† Davy, ibid.

bility, as appears from Davy's experiments, is increased instead of being diminished, as Grotthus supposed.

Effect of gases in preventing explosion.

When a mixture of two volumes hydrogen gas and one volume oxygen gas is diluted with determinate quantities of other gases, it loses its property of exploding when an electric spark is passed through it. The following table exhibits the volumes of the different gases which destroyed the explosive property of one volume of such a mixture according to the experiments of Davy* and Henry.†

	Volumes.
Hydrogen	8
*Azote	6
Oxygen	9
Protoxide of azote	11
*Cyanogen	1·5
Carburetted hydrogen	1
*Carbonic oxide	4
Sulphuretted hydrogen	2
Olefiant gas	0·5
Muriatic acid gas	2
Fluosilicic acid gas	0·83
*Ammonia	2
*Carbonic acid	3

Inflammation took place, when the volumes of these gases present were as follows:

Hydrogen	6
Oxygen	7

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[*] Phil. Trans. 1817, p. 59.

[†] Ibid. 1824, p. 282. Those marked thus (*) by Henry, the others by Davy.

Protoxide of azote . . .	10
Carburetted hydrogen . . .	0·75
Sulphuretted hydrogen . . .	1·5
Olefiant gas	0·33
Muriatic acid gas	1·5
Fluosilicic acid gas . . .	0·75

Davy is of opinion that the reason why such mixtures prevent the inflammation of the hydrogen, is, that they serve to carry off the heat, or to prevent it from reaching the requisite point.

Nature of flame.

8. Flame is the rapid combustion of volatilized matter. The tallow or the wax is melted and drawn up to the top of the wick of a candle. Here it is boiled and converted into vapour, which ascends in the form of a column. This vapour is raised to such a temperature, that it combines rapidly with the oxygen of the surrounding atmosphere, and the heat evolved is such as to heat the vapour to whiteness. Flame then is merely volatile combustible matter heated white hot. The combustion can only take place in that part of the column of hot vapour that is in contact with the atmosphere, namely, the exterior surface. The flame of a candle then is merely a thin film of white hot vapour, enclosing within a quantity of hot vapour which for want of oxygen, is incapable of burning. But as it advances upward in consequence of the outer film being already consumed, it gradually constitutes the outer surface of the column, and assumes the form of flame. And as the supply of hot vapour diminishes as it ascends, and at last fails altogether, the flame of a candle gradually tapers to a point.

That this is the nature of flame has been beautifully shown by my late friend Mr. Oswald Sym, in a paper which has been greatly admired, but which has not perhaps attracted all the attention which it deserves.* Mr. Davies, in a very interesting paper, has fully confirmed the accuracy of Mr. Sym's observations.†

Platinum coil maintains the combustion of alcohol vapour.

9. There are several vapours which burn at a lower temperature than that which produces flame. During the combustion heat is evolved; but the ultimate products are quite different from those formed, when the same vapours burn with flame. Sir H. Davy discovered that if a coil of platinum wire of rather a small size, be placed round the wick of a spirit of wine lamp, while the alcohol is burning, and allowed to remain till it gets red hot, we may blow out the flame without putting an end to the combustion. The wire continues red hot, the vapour continues to rise and undergoes a slow combustion round it, during which it gives out as much heat as keeps the wire always at a red heat. This combustion without flame goes on as long as any spirit remains to evaporate, and it may be kept up for weeks without intermission. The alcohol is converted into acetic acid, and a substance having a peculiar smell. No doubt water is also formed. The atomic constituents of alcohol are,

3 atoms hydrogen
2 atoms carbon
1 atom oxygen.

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* Annals of Philosophy, (1st series,) viii. 321.

† Ibid. (2d series,) x. 447.

While the atomic constituents of acetic acid are

2 atoms hydrogen
4 atoms carbon
3 atoms oxygen.

Were we to suppose two atoms of alcohol during this slow combustion to unite with 5 atoms of oxygen; they might be converted into 1 atom acetic acid and 4 atoms water. For two atoms alcohol contain,

	Hydrogen.	Carbon.	Oxygen.
	6 atoms	+ 4 atoms	+ 2 atoms
Adding of oxygen	.	. .	. 5
Total	6	+ 4	+ 7
1 atom acetic acid contains	2	+ 4	+ 3
4 atoms water contain	4	+ 0	+ 4
	6	+ 4	+ 7

But it is obvious from the peculiar smell of the acetic acid formed, that the substances generated are more numerous than acetic acid and water.

The vapour of ether, camphor, and several of the volatile oils, may be made to undergo this combustion without flame as well as alcohol, and the phenomena are nearly the same.

Palladium wire has been substituted for platinum; but when wires of other metals are employed the process does not succeed.

If a coil of small platinum wire, previously heated to redness, be let down into a glass containing a mixture of coal gas and air, or vapour of ether and air, it will continue red hot till the whole gas or vapour be consumed.

Cause of this.

This curious process seems to depend upon two properties which platinum possesses. Its specific heat is very small, and it is one of the worst conductors of heat of all the metals. Hence a comparatively small quantity of heat is sufficient to make it red hot, and being a bad conductor, the waste of heat is small compared to what it would be if silver or gold (which are very good conductors) were employed. Red hot platinum is sufficiently hot to cause the rapid union of alcohol vapour with oxygen, but not sufficiently so to cause it to burn with flame.

Spongy platinum, &c. becomes red hot when in contact with alcohol vapour.

10. In the year 1822, M. Dobereiner, Professor at Jena, announced that if spongy platinum, obtained by exposing ammonia-muriate of platinum to a low red heat, be heated and put into the vapour of alcohol, it becomes red hot and remains so as long as any of the vapour is unconsumed.* He found that powdered black oxide of manganese, oxide of nickel, oxide of cobalt, oxide of uranium, oxide of tin, &c. when in the loose and porous state in which they are procured by decomposing the oxalates of the respective metals by heat in the open air, may be substituted for spongy platinum. So that the phenomenon is much more general than was at first supposed. In the year 1823, Dobereiner discovered that if a jet of hydrogen gas be made to strike against a small collection of this spongy platinum at the distance of an inch or two from the mouth of the jet, the platinum becomes

Or by a jet of hydrogen.

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* Schweigger's Jahrbuch, iv. 91.

red hot and continues so as long as the hydrogen jet plays upon it. Or if a mixture of oxygen and hydrogen gases be put into a glass jar, and a little spongy platinum be let up into it; the platinum speedily becomes red hot and causes the mixture of gases to explode.* This curious experiment was almost immediately repeated and verified in every part of Europe.

Mr. Garden found that the black matter which remains behind when native platina is dissolved in aqua regia, and which contains a good deal of osmium and iridium, may be substituted for the spongy platinum.† It is only necessary to heat it to redness and allow it to cool before using it. Thenard and Dulong found that palladium, rhodium, and iridium, might be substituted for the spongy platinum. Osmium requires to be heated to 104° or 122°. Nickel in a spongy state acts also, but very slowly.‡ Platinum foil in its natural state would not do, but when crumpled like paper it became red hot like spongy platinum.||

When the temperature is elevated, but not so high as that of boiling mercury, all the metals have a greater or smaller effect when treated with a jet of hydrogen gas. Gold in plates requires to be heated to 436°, when in foil to 496°, but when in powder it causes the combination of oxygen and hydrogen gas, if it be heated to 248°.

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* Schweigger's Jahrbuch, viii. 321.

† Annals of Philosophy, (2d series) vi. 466.

‡ Ann. de Chim. et de Phys. xxiv. 380.

|| Ibid. xxiii. 440.

Also other bodies.

Even charcoal, pumice, porcelain, glass, and rock crystal, determine the combination of oxygen and hydrogen gases, at a temperature below 662°.* When the fragments of glass employed were angular they produced twice the effect of the same quantity of glass, consisting of rounded fragments.

Thenard and Dulong found that platinum, in all states, and likewise all the other bodies, gradually lose this property of becoming incandescent, by exposure to the air. But the property may be restored again at pleasure, simply by heating the bodies red hot, and allowing them to cool; or by plunging them into nitric acid, either cold or hot, allowing them to remain for some time in it, then drying them in a heat of about 390°. The acid may now be washed off with water, or even with an alkaline ley, without depriving the spongy mass of its property. Concentrated sulphuric acid, or muriatic acid, may be substituted for nitric acid, but they do not answer so well.†

Theory of this.

The theory of this curious process seems connected with the capillary attraction for oxygen gas of the small pores in the spongy platinum. It has been proved by the experiments of Count Morozzo, and M. de Saussure, junior, that the pores of charcoal, and other porous bodies, have the property of absorbing certain gaseous substances so copiously, that they exist within the pores in a condensed state. Thus charcoal absorbs 9¼ times its own bulk of oxygen gas. We may admit that the oxy-

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* Ann. de Chim. et de Phys. xxiv. 381. † Ibid. p. 383.

gen of the atmosphere is condensed to this amount in spongy platinum. Count Morozzo found that when charcoal thus impregnated with oxygen gas is placed in contact with hydrogen gas, the two gases combine, and water is formed. We may admit that when the hydrogen gas strikes the surface of the spongy platinum, combination takes place, and some water is formed. This occasions the evolution of heat, which elevates the temperature of the spongy platinum in consequence of its small capacity for heat, and its imperfect conducting power. This elevation occasions the formation of water in that part of the jet which strikes the platinum, as it consists of a mixture of hydrogen and common air. Thus more heat is evolved, the platinum becomes red hot, and the combustion goes on precisely, as when the red hot platinum coil is left in contact with the vapour of alcohol. When the spongy platinum loses its property of becoming red hot, the reason probably is that it has lost the property of condensing oxygen gas in its pores. The ignition, or the action of nitric acid, would seem to restore this property.

Platinum with clay occasions the combination of oxygen and hydrogen.

11. Dobereiner found that when a little spongy platinum was made up into a ball with pipe-clay of about the size of a pea, and baked in an incipient red heat, to make the whole adhere together, these balls possessed a similar action upon a mixture of oxygen and hydrogen gases, as the spongy platinum itself. When let up into a jar containing a mixture of oxygen and hydrogen gases in the requisite proportions, the two gases combine with

great rapidity, and in a few minutes, and are converted into water. A highly valuable and instructive set of experiments on the use of these platinum balls in the analysis of combustible gases, has been made by Dr. Henry.*

He confirmed the results obtained by Dobereiner, by letting up a platinum ball (previously heated to redness by the blow-pipe, and just allowed to cool) into an explosive mixture of 2 volumes of hydrogen gas, and one volume of oxygen.

Its action on carbonic oxide.

When carbonic oxide gas is mixed with this explosive mixture, and a platinum ball let up, the carbonic oxide is first acted on, and converted into carbonic acid. If the quantity of oxygen be sufficient, the hydrogen also is converted into water, and an explosion almost always takes place.

On olefiant gas.

When olefiant gas is mixed with the explosive mixture, and in equal volumes with it, and a platinum ball is let up, the explosive mixture is rapidly converted into water, while not above $\frac{1}{10}$th or $\frac{1}{8}$th of the olefiant gas is consumed. The action on the olefiant gas is greater when the explosive mixture exceeds the volume of the olefiant gas. Thus when $2\frac{1}{2}$ volumes of the explosive mixture is mixed with 1 volume of olefiant gas, about $\frac{1}{4}$th of the olefiant gas is consumed. When the oxygen gas present is sufficient to saturate both the hydrogen and olefiant gas, then in general the action is much more rapid, and both gases are consumed.

On carburetted hydrogen.

When carburetted hydrogen gas is added to the

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* Phil. Trans. 1824, p. 266.

explosive mixture, and a platinum ball is let up, the hydrogen and oxygen combine rapidly into water, but the carburetted hydrogen is not acted on at all, unless the quantity of oxygen gas present be very considerable, and even then the action is very imperfect.

When we make a mixture of carbonic oxide, carburetted hydrogen, and oxygen gases, and let up a platinum ball, the carbonic oxide is converted into carbonic acid, but the carburetted hydrogen remains unacted on.

When the mixture consists of

Hydrogen,
Carbonic oxide,
Carburetted hydrogen,
Oxygen,

the two first gases combine with oxygen, and undergo combustion, but the carburetted hydrogen remains unaltered. If olefiant gas be present, it is also partly consumed.

Method of analyzing mixtures of these gases.

When these four inflammable gases are mixed together with oxygen, and exposed to the action of the platinum ball, the carbonic oxide is first converted into carbonic acid, then the hydrogen is converted into water. The olefiant gas is next acted on, but only imperfectly, and the carburetted hydrogen is not acted on at all.

This order obviously depends upon the temperature necessary to occasion the combustion of each of these gases. Davy ascertained that hydrogen gas and oxygen combine silently, and form water at a temperature between 662° and 800°—that carbonic oxide is as easily consumed as hydrogen—that olefiant gas requires a red heat—and carburetted hydrogen a white heat, to cause them to burn.

Dr. Henry ascertained that when a platinum ball is let up into a mixture of carbonic oxide gas and oxygen, the two gases begin to unite slowly when heated to the temperature of 300°, and they unite rapidly at 340°.

Olefiant gas and oxygen begin to combine at 480°, and are slowly, but completely, consumed at 520°. But a mixture of carburetted hydrogen and oxygen is not in the least acted on when heated to 555°. Nor is a mixture of cyanogen and oxygen acted on though heated to the same temperature.

A mixture of muriatic acid and oxygen gas, begins to be acted on by the platinum ball, and water to be formed, when the temperature is raised to 250°, and a mixture of ammonia and oxygen at the temperature of 380°.

The following table shows the effect of various gases, in preventing the action of the platinum ball on an explosive mixture of oxygen and hydrogen gases, constituting 1 volume according to the experiments of Dr. Henry.

Not prevented by many volumes of	Hydrogen
——————	Azote
———— by 10 volumes of	Oxygen
——————	Protoxide of Azote
Prevented by 1 volume of	Cyanogen
Not prevented by 10 volumes of	Carburetted hydrogen
Prevented by ½ volume of	Carbonic oxide

Prevented by 1·5 volume of	Olefiant gas
Not prevented by 6 volumes of	Muriatic acid
———————— 10 volumes of	Ammonia
————————————————	Carbonic acid.

Dr. Henry has ingeniously applied these facts to the analysis of mixtures of

Olefiant gas,
Hydrogen,
Carbonic oxide,
Carburetted hydrogen.

The olefiant gas is removed by chlorine, and its volume determined. Then oxygen gas is mixed with the mixture of the three remaining gases, and a platinum ball let up. The carbonic oxide is converted into carbonic acid, and the hydrogen into water. Caustic potash by absorbing the carbonic acid determines the volume of carbonic oxide. Two-thirds of the diminution of volume (subtracting the carbonic oxide) is the volume of hydrogen. Nothing remains but the carburetted hydrogen, which being mixed with oxygen gas in the requisite proportion, is fired by electricity, and its quantity determined in the usual way.

First explanation of combustion.

12. The first attempt to explain combustion was crude and unsatisfactory. A certain elementary body, called *fire*, was supposed to exist, possessed of the property of devouring certain other bodies, and converting them into itself. When we set fire to a grate full of charcoal, we bring, according to this hypothesis, a small portion of the element of fire, which immediately begins to devour the charcoal, and to convert it into fire. Whatever part of

the charcoal is not fit for being the food of fire is left behind in the form of ashes.

Hypothesis of Hooke.

A much more ingenious and satisfactory hypothesis was proposed in 1665 by Dr. Hooke. According to this extraordinary man, there exists in common air a certain substance which is like, if not the very same with, that which is fixed in saltpetre. This substance has the property of dissolving all combustibles; but only when their temperature is considerably raised. The solution takes place with such rapidity, that it occasions both heat and light; which in his opinion are mere motions. The dissolved substance is partly in the state of air, partly coagulated in a liquid or solid form. The quantity of this solvent present in a given bulk of air is incomparably less than in the same bulk of saltpetre. Hence the reason that a combustible continues burning but for a short time in a given bulk of air: the solvent is soon saturated, and then of course the combustion is at an end. Hence also the reason that combustion succeeds best when there is a constant supply of fresh air, and that it may be greatly accelerated by forcing in air with bellows.*

And Mayow.

About ten years after the publication of Hooke's *Micrographia*, his theory was adopted by Mayow, without acknowledgment, in a tract which he published at Oxford on saltpetre.† We are indebted to him for a number of very ingenious and important experiments, in which he anticipated

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* Hooke's Micrographia, p. 103. See also his Lampas.

† De Sal-nitro et Spiritu Nitro-aereo.

several modern chemical philosophers; but his reasoning is for the most part absurd, and the additions which he made to the theory of Hooke are exceedingly extravagant. To the solvent of Hooke he gives the name of *spiritus nitro-aereus.* It consists, he supposes, of very minute particles, which are constantly at variance with the particles of combustibles, and from their quarrels all the changes of things proceed. Fire consists in the rapid motion of these particles, heat in their less rapid motion. The sun is merely nitro-aerial particles moving with great rapidity. They fill all space. Their motion becomes more languid according to their distance from the sun; and when they approach near the earth, they become pointed, and constitute *cold.**

Hypothesis of Beecher and Stahl.

The attention of chemical philosophers was soon drawn away from the theory of Hooke and Mayow to one of a very different kind, first proposed by Beecher, but new-modelled by his disciple Stahl with so much skill, arranged in such an elegant systematic form, and furnished with such numerous,

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* Though Mayow's theory was not original, and though his additions to it be absurd, his tract itself displays great genius, and contains a vast number of new views, which have been fully confirmed by the recent discoveries in chemistry. He pointed out the cause of the increase of weight in metals when calcined; he ascertained the changes produced upon air by respiration and combustion; and employed in his researches an apparatus similar to the present pneumatic apparatus of chemists. Perhaps the most curious part of the whole treatise is his fourteenth chapter, in which he displays a much more accurate knowledge of *affinities,* than any of his contemporaries, or even successors for many years.

appropiate, and convincing illustrations, that it almost instantly caught the fancy, raised Stahl to the highest rank among philosophers, and constituted him the founder of the Stahlian theory of combustion.

Phlogiston.

According to Stahl, all combustible substances contain in them a certain body, known by the name of PHLOGISTON, to which they owe their combustibility. This substance is precisely the same in all combustibles. These bodies of course owe their diversity to other ingredients which they contain, and with which the phlogiston is combined. Combustion, and all its attendant phenomena, depend upon the separation and dissipation of this principle: and when it is once separated, the remainder of the body is incombustible. Phlogiston, according to Stahl, is peculiarly disposed to be affected by a violent whirling motion. The heat and the light, which make their appearance during combustion, are merely two properties of phlogiston when in this state of violent agitation.

The celebrated Macquer was one of the first persons who perceived a striking defect in this theory of Stahl. Sir Isaac Newton had proved that light is a body; it was absurd, therefore, to make it a mere property of phlogiston or the element of fire. Macquer accordingly considered phlogiston as nothing else but light fixed in bodies. This opinion was embraced by a great number of the most distinguished chemists; and many ingenious arguments were brought forward to prove its truth. But if phlogiston be only light fixed in bodies, whence comes the heat that manifests itself during combustion? Is this heat merely a property of light? Dr. Black proved that heat is capable of combining with, or becoming fixed in bodies which are not combustible, as in ice or water; and concluded of course, that it is not a property but a body. This obliged philosophers to take another view of the nature of phlogiston.

Considered as a subtile matter without gravity.

According to them, there exists a peculiar matter, extremely subtile, capable of penetrating the densest bodies, astonishingly elastic, and the cause of heat, light, magnetism, electricity, and even of gravitation. This matter, the *ether* of Hooke and Newton, is also the substance called phlogiston, which exists in a fixed state in combustible bodies. When set at liberty, it gives to the substances called caloric and light those peculiar motions which produce in us the sensations of heat and light. Hence the appearance of caloric and light in every case of combustion; hence, too, the reason that a body after combustion is heavier than it was before; for as phlogiston is itself the cause of gravitation, it would be absurd to suppose that it possesses gravitation. It is more reasonable to consider it as endowed with a principle of levity.

Some time after this last modification of the phlogistic theory, Dr. Priestley, who was rapidly extending the boundaries of pneumatic chemistry, repeated many experiments formerly made on combustion by Hooke, Mayow, Boyle, and Hales, besides adding many of his own. He soon found, as they had done before him, that the air in which combustibles had been suffered to burn till they were extinguished, had undergone a very remarkable change; for no combustible would afterwards burn it, and no animal could breathe it without suffocation. He concluded that this change was owing to phlogiston; that the air had combined with that substance; and that air is necessary to combustion, by attracting the phlogiston, for which it has a strong affinity. If so, the origin of the heat and light which appear during combustion remains to be accounted for; since phlogiston, if it separates from the combustible merely by combining with air, cannot surely act upon those bodies in whatsoever state we may suppose them.

Heat derived from the air.

The celebrated Dr. Crawford was the first person who attempted to solve this difficulty, by applying to the theory of combustion Dr. Black's doctrine of latent heat. According to him, the phlogiston of the combustible combines during combustion with the air, and at the same time separates the heat and light with which that fluid had been previously united. The heat and the light, then, which appear during combustion, exist previously in the air. This theory was very different from Stahl's, and certainly a great deal more satisfactory. But still the question, What is phlogiston? remained to to be answered.

Phlogiston considered as hydrogen.

Mr. Kirwan, who had already raised himself to a high rank among chemical philosophers, by many ingenious investigations of some of the most difficult parts of the science, attempted to answer this question, and to prove that phlogiston is the same with hydrogen. This opinion, which Mr. Kirwan informs us was first suggested by the discoveries of Dr. Priestley, met with a very favourable reception from the chemical world, and was adopted either in its full extent, or with certain modifications, by Bergman, Morveau, Crell, Wiegleb, Westrumb, Hermbstadt, Karsten, Bewley, Priestley, and Delametherie. The object of Mr. Kirwan was to prove, that hydrogen exists as a component part of every combustible body; that during combustion it separates from the combustible body, and combines with the oxygen of the air. This he attempted in a treatise published on purpose, intitled, *An Essay on Phlogiston and the Constitution of Acids.**

During these different modifications of the Stah-

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* I have omitted, in the historical view given in the text, the hypothesis published in 1777 by Mr. Scheele, one of the most extraordinary men that ever existed. When very young, he was bound apprentice to an apothecary at Gottenburgh, where he first felt the impulse of that genius which afterwards made him so conspicuous. He durst not indeed devote himself openly to chemical experiments; but he contrived to make himself master of that science by devoting those hours to study which were assigned him for sleep. He afterwards went to Sweden, and settled as an apothecary at Köping. Here Bergman first found him, saw his merit, and encouraged it, adopted his opinions, defended him with zeal, and took upon himself the charge of publishing his treatises. Encouraged and excited by this magnanimous conduct, the genius of Scheele, though unassisted by education or wealth, burst forth with astonishing lustre; and at an age when most philosophers are only rising into notice, he had finished a career of discoveries which have no parallel in the annals of chemistry. Whoever wishes to behold ingenuity combined with simplicity, whoever wishes to see the inexhaustible resources of chemical analysis;

Combustion explained by Lavoisier.

lian theory, the illustrious Lavoisier was assiduously occupied in studying the phenomena of combustion. He seems to have attached himself to this subject, and to have seen the defects of the prevailing theory as early as 1770. The first precise notions, however, of what might be the real nature of combustion, were suggested to him by Bayen's paper on the oxides of mercury, which he heard read before the Academy of Sciences in 1774. These first notions, or rather conjectures, he pursued with unwearied industry, assisted by the numerous discoveries which were pouring in from all quarters; and by a long series of the most laborious and

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whoever wishes for a model in chemical researches—has only to peruse and to study the works of Scheele.

In 1777, Scheele published a treatise, entitled Chemical Experiments on Air and Fire, which perhaps exhibits a more striking display of the extent of his genius than all his other publications put together. After a vast number of experiments, conducted with astonishing ingenuity, he concluded, that caloric is composed of a certain quantity of oxygen combined with phlogiston; that radiant heat, a substance which he supposed capable of being propagated in straight lines like light, and not capable of combining with air, is composed of oxygen united with a greater quantity of phlogiston, and light of oxygen united with a still greater quantity. He supposed, too, that the difference between the rays depends upon the quantity of phlogiston: the red, according to him, contains the least; the violet the most phlogiston. By *phlogiston*, Mr. Scheele seems to have meant *hydrogen*. It is needless, therefore, to examine his theory, as it is now known that the combination of hydrogen and oxygen forms not caloric but water. The whole fabric, therefore, has tumbled to the ground; but the importance of the materials will always be admired, and the ruins of the structure must remain eternal monuments of the genius of the builder.

accurate experiments and disquisitions ever exhibited in chemistry, he fully established the existence of this general law—"In every case of combustion, oxygen combines with the burning body." This noble discovery, the fruit of genius, industry, and penetration, has reflected new light on every branch of chemistry, has connected and explained a vast number of facts formerly insulated and inexplicable, and has new-modelled the whole, and moulded it into the form of a science.

After Mr. Lavoisier had convinced himself of the existence of this general law, and had published his proofs to the world, it was some time before he was able to gain a single convert, notwithstanding his unwearied assiduity, and the great weight which his talents, his reputation, his fortune, and his situation, naturally gave him. At last Mr. Berthollet, at a meeting of the Academy of Sciences in 1785, solemnly renounced his old opinions, and declared himself a convert. Mr. Fourcroy, professor of chemistry in Paris, followed his example. And in 1787, Morveau, during a visit to Paris, was prevailed upon to relinquish his former opinions, and embrace those of Lavoisier and his friends. The example of these celebrated men was soon followed by all the young chemists of France.

Mr. Lavoisier's explanation of combustion depends upon the two laws discovered by himself and Dr. Black. When a combustible body is raised to a certain temperature, it begins to combine with the oxygen of the atmosphere, and this oxygen during its combination lets go the caloric and light with which it was combined while in the gaseous state. Hence their appearance during every combustion. Hence also the change which the combustible undergoes in consequence of combustion.

Thus Lavoisier explained combustion without having recourse to phlogiston; a principle merely supposed to exist, because combustion could not be explained without it. No chemist had been able to exhibit phlogiston in a separate state, or to give any proof of its existence, excepting only its conveniency in explaining combustion. The proof of its existence consisted entirely in the impossibility of explaining combustion without it. Mr. Lavoisier, therefore, by giving a satisfactory explanation of combustion without having recourse to phlogiston, proved that there was no reason for supposing any such principle at all to exist.

But the hypothesis of Mr. Kirwan, who made phlogiston the same with hydrogen, was not overturned by this explanation, because there could be no doubt that such a substance as hydrogen actually exists. But hydrogen, if it be phlogiston, must constitute a component part of every combustible, and it must separate from the combustible in every case of combustion. These were points, accordingly, which Mr. Kirwan undertook to prove. If he failed, or if the very contrary of his supposition holds in fact, his hypothesis of course fell to the ground.

He refutes the opinion of Kirwan.

Lavoisier and his associates saw at once the important uses which might be made of Mr. Kirwan's essay. By refuting an hypothesis which had been embraced by the most respectable chemists in Europe, their cause would receive an eclat which would make it irresistible. Accordingly the essay was translated into French, and each of the sections into which it was divided was accompanied by a refutation. Four of the sections were refuted by Lavoisier, three by Berthollet, three by Fourcroy, two by Morveau, and one by Monge. And to do the French chemists justice, never was there a refutation more complete. Mr. Kirwan himself, with that candour which distinguishes superior minds, gave up his opinion as untenable, and declared himself a convert to the opinion of Lavoisier.

Thus Mr. Lavoisier destroyed the existence of phlogiston altogether, and established a theory of combustion almost precisely similar to that which had been proposed long ago by Dr. Hooke. The theory of Hooke is only expressed in general terms; that of Lavoisier is much more particular. The first was a hypothesis or fortunate conjecture which the infant state of the science did not enable him to verify; whereas Lavoisier was led to his conclusions by accurate experiments and a train of ingenious and masterly deductions.

According to the theory of Lavoisier, combustion consists in two things: first, a decomposition; second, a combination. The oxygen of the atmosphere being in the state of gas, is combined with heat and light. During combustion this gas is *decomposed*, its heat and light escape, while its base *combines* with the combustible and forms the product. This product is incombustible; because its base, being already saturated with oxygen, cannot combine with any more.

Lavoisier's theory only applies to common combustion.

13. But the theory of Lavoisier was intended only to explain the combustion of bodies surrounded with atmospherical air or oxygen gas. It does not apply in so satisfactory a manner, when both the supporter and the combustible are in a solid state, and when the product is gaseous. This is the case with gunpowder which is an intimate mixture of nitre, charcoal, aud sulphur. The saltpetre contains abundance of oxygen in a solid state. It is the rapid union of this oxygen with the charcoal and sulphur, and the conversion of them into carbonic acid and sulphurous acid, which occasions the violent combustion and explosive property of gunpowder. Nor does it explain the evolution of fire, when sulphur combines rapidly with copper, or phosphorus with barytes or strontian or lime. These and many other similar phenomena, have led modern chemists to the opinion, that combustion is a consequence of the rapid or sudden union of bodies with each other.

Combustion ascribed to electricity.

Sir H. Davy (and his opinion has been embraced by Professor Berzelius) has gone a step farther, and endeavoured to explain the way in which chemical affinity acts with such energy. According to him, all bodies having an affinity for each other are in different states of electricity, the one *plus* and the other *minus*. The more intensely any body is plus and another minus, with the more energy will they unite, and the more violent will be the phenomena of combustion which they will exhibit during their union. Thus oxygen is highly negative, and hydrogen highly positive. Hence the energy with which they unite, and the great heat evolved.

Winterl and Oerstedt went a step farther, and endeavoured to account for the heat and the light which appear when bodies unite with great energy. Plus or vitreous electricity, and minus or resinous electricity, in the opinion of the majority of electricians, consist each of a particular *fluid*. These two fluids have a strong affinity for each other; and when they unite, they constitute (in the opinion of Winterl and Oerstedt) *fire*. Hence when a vitreously electric body unites with a resinously electric body, the two fluids combine as well as the bodies in which they were inherent, and make their escape under the form of *fire*.

We shall not inquire how far the two parts of this hypothesis are consistent with each other, or how far the doctrine of permanent electric states is consistent with the phenomena of electricity. It will be better to reserve all discussions respecting the electric theory of chemical combination and combustion, till we have taken a view of the principal electric facts which have been ascertained; which will constitute the subject of the second part of this volume.

CHAPTER VI.

OF THE NATURE OF HEAT.

Chap. VI.

Having made ourselves acquainted with the effects which heat produces on bodies, it may be worth while to make a few observations upon the opinion which this knowledge is calculated to induce us to form respecting the nature of heat. Philosophers are at present divided in their sentiments on this subject. Some consider heat as a peculiar substance, which produces all the different phenomena which we have described, by entering into bodies combining with them or leaving them. While others think that it is a property of matter, a motion of a particular kind, the nature of which has never been explained in a satisfactory manner.

Heat considered as a property and as a peculiar substance.

During the 17th century, from the time of Bacon to that of Newton, the latter of these opinions prevailed, at least in this country, almost universally. During the 18th century the former opinion was equally prevalent; probably from the popularity of Cullen and Black, both of whom taught it in their lectures. Since the beginning of the present century some of the most popular writers on Chemistry in this country have reverted to the old opinion. This was first done by Count Rumford, who published some curious experiments, incompatible, in his opinion, with the opinion that heat is a body. His example was followed by Sir Humphry Davy, and Dr. Thomas Young.

Heat does not alter the weight.

1. From the most careful experiments that it has been possible to make, it may be concluded that no accumulation of heat in bodies produces any alteration whatever in their weight. Dr. Fordyce, indeed, drew, as a conclusion from an experiment of his, that when water is frozen into ice it becomes sensibly heavier.* But Count Rumford, on repeating this experiment, found no sensible difference in the weight of the ice and water. We may therefore conclude, that the apparent increase of weight observed by Dr. Fordyce was owing to the condensation of aqueous vapours upon the surface of the glass vessel containing the frozen water.† Lavoisier had already come to the same conclusion from his experiments, which seem to have been made with the most scrupulous attention to accuracy.—It cannot be shown then that heat, supposing it a peculiar substance, possesses *gravitation.*

First three effects best explained on the supposition that it is a body.

2. The first three of the effects of heat are best explained by considering heat as a *substance.* Indeed it is difficult to form any clear conceptions of them upon any other supposition. If heat be a peculiar substance there is no difficulty in conceiving how, by its entering into bodies, it increases their bulk, and how the bulk diminishes when it is withdrawn. There is no difficulty in conceiving

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* Phil. Trans. 1785, p. 361.

† Ibid. 1799, p. 179.

how more heat may be requisite to produce a given effect upon one body than upon another, or why the specific heat of different bodies is different, on the supposition that heat is a body. Though if heat were mere motion, neither the property which it has to expand bodies, nor the different capacity of bodies for heat, could be explained in a satisfactory manner. The radiation of heat admits of an equally simple explanation, if heat be a body, and so do fluidity and evaporation.

Thus these three effects of heat would lead us to consider heat as a peculiar kind of body. Accordingly this is the opinion which all those persons have adopted who have turned their chief attention to expansion, specific heat, radiation, fluidity, and evaporation.

The last two if it be a property.

3. But ignition and combustion do not admit of so easy an explanation, if we admit heat to be a body. We cannot give an intelligible explanation of ignition without admitting that heat and light are mutually convertible into each other. But if this mutual convertibility were admitted, there is nothing in the phenomena of ignition incompatible with the notion that heat is a substance.

But no satisfactory explanation of the evolution of heat and light, during every case of combustion, has been yet given, flowing from the opinion that heat is a peculiar substance. It would be much easier to explain it, if we were permitted to consider heat rather as a property of matter than as a substance *sui generis*. We might then assign a reason why it should make its appearance in all cases of rapid combination, and never in any other case. The explanation that the fire evolved during combustion, is merely a union of the plus and minus electricity, with which the bodies combining together were charged, appears at first sight plausible; but it will not bear a rigid examination. For it is incompatible with the very hypothesis from which it professes to flow. For if chemical affinity be merely the consequence of different states of electricity, and if bodies unite merely because they are in different electrical states, it is clear that they could not continue united unless these different electrical states were permanent. But if the plus and minus electricity were to combine and fly off under the form of fire, there would be an end of the different electrical states which caused the bodies to unite, and, of course, they would cease to continue united, which is contrary to matter of fact.

The phenomena of combustion then accord better with the notion that heat is a mere property of matter, while the other effects of heat agree better with the opinion that it is a body.—Neither supposition will enable us to explain all the phenomena. I think, therefore, that it will be safest for us, in the present state of our knowledge, to acknowledge our inability to solve this difficult problem, and to confess that we are incompetent to decide whether it be a substance or a quality.

Whether the phenomena of heat and combustion be incompatible with heat being a body.

4. But Sir Humphry Davy and Count Rumford have made experiments, which they think incompatible with the opinion that heat is a body.

Davy found that a thin metallic plate was heated by friction in the exhausted receiver of an air-pump, even when the apparatus was insulated from bodies capable of supplying heat by being placed on ice. He found also that by rubbing two pieces of ice together so much heat is evolved that the ice melts.

Count Rumford took a cannon cast solid and rough as it came from the foundry; he caused its extremity to be cut off, and formed, in that part, a solid cylinder attached to the cannon $7\frac{3}{4}$ inches in diameter, and $9\frac{8}{10}$ inches long. It remained joined to the rest of the metal by a small cylindrical neck. In this cylinder, a hole was bored 3·7 inches in diameter, and 7·2 inches in length. Into this hole was put a blunt steel borer, which by means of horses, was made to rub against its bottom; at the same time a small hole was made in the cylinder perpendicular to the bore, and ending in the solid part a little beyond the end of the bore. This was for introducing a thermometer to measure the heat of the cylinder. The cylinder was wrapt round with flannel to keep in the heat. The borer pressed against the bottom of the hole with a force equal to about 10,000 lbs. avoirdupois, and the cylinder was turned round at the rate of 32 times in a minute. At the beginning of the experiment, the temperature of the cylinder was 60°; at the end of 30 minutes, when it had made 960 revolutions, its temperature was 130°. The quantity of metallic dust or scales produced by this friction, amounted to 837 grains.

To make this experiment more striking, Count

Rumford contrived, to inclose the cylinder above described in a wooden box filled with water, which effectually excluded all air, as the cylinder itself and the borer were surrounded with water, and at the same time did not impede the motion of the instrument. The quantity of water amounted 18·77lbs. avoirdupois, and at the beginning of the experiment was at the temperature of 60°. After the cylinder had revolved for an hour at the rate of 32 times in a minute, the temperature of the water was 107°; in 30 minutes more it was 178°; and in two hours and 30 minutes after the experiment began, the water actually boiled. According to the computation of Count Rumford, the heat produced would have been sufficient to heat 26·58lbs. avoirdupois, of ice-cold water boiling hot; and it would have required nine wax candles of a moderate size, burning with a clear flame all the time the experiment lasted, to have produced as much heat. In this experiment all access of water into the hole of the cylinder where the friction took place was prevented. But in another experiment, the result of which was precisely the same, the water was allowed free access.*

The experiments of Rumford were repeated and diversified by M. Haldot. He contrived an apparatus by which two bodies could be pressed against each other by means of a spring, while one of them turned round with the velocity of 32·8 inches per second. The friction took place in a strong box

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* Nicholson's Journal, ii. 106.

containing 216 cubic inches of water. The results obtained so nearly resemble those of Count Rumford, that it is unnecessary to enter into particular details. The rubber was brass. When the metal rubbed was zinc the heat evolved was greatest; brass and lead afforded equal heat, but less than zinc; tin produced only $\frac{7}{9}$ths of the heat evolved during the friction of lead. When the pressure was quadrupled, the heat evolved became seven times greater than before. When the rubber was rough, it produced but half as much heat as when smooth. When the apparatus was surrounded by bad conductors of heat, or by non-conductors of electricity, the quantity of heat evolved was diminished.*

In these experiments, it is contended by Davy and Rumford, that no source of this enormous quantity of heat can be pointed out. It cannot be supposed to be absorbed from the neighbouring bodies. Nor can it be derived from the bodies themselves, without supposing the quantity of heat which they contain to be infinitely great.

I am disposed to think that a part at least, if not the whole, of the heat evolved in Count Rumford's experiments proceeded from an augmentation of density, and a consequent diminution of specific heat in the metallic cylinder subjected to friction. There can be little doubt at least, that the quantity of heat produced depends in some measure upon the size of the cylinder. For surely it will not be

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* Nicholson's Journal, xxvi. 30.

contended, that it would be equally great when a very small cylinder is used as when a great one is employed.

Heat from percussion.

Berthollet, Pictet, and Biot, have made a set of experiments, to ascertain the quantity of heat evolved when ductile metals are suddenly struck forcibly, as when they are stamped in the process of coining. The experiments were made upon pieces of gold, silver, and copper, of the same size and shape, and care was taken that all the parts of the apparatus had acquired the same temperature before the experiments began. Copper evolved most heat, silver was next in order, and gold evolved the least. The first blow evolved the most heat, and it diminished gradually, and after the third blow was hardly perceptible. The heat acquired was estimated by throwing the piece of metal struck into a quantity of water, and ascertaining the change of temperature which the water underwent. The following table exhibits the increase of temperature, experienced by two pieces of copper by three successive blows;

1st Blow	1st Piece . . .	17·44°
	2d Piece . .	20·80
2d Blow	1st Piece . .	7·30
	2d Piece . .	3·69
3d Blow	1st Piece . .	1·90
	2d Piece . .	1·46

The whole quantity of heat evolved by each of these pieces of copper is nearly the same; that from the first piece being 26·64°, and that from the second 25·95°.

Chap. VI.

The following table exhibits the heat evolved from two pieces of silver treated in the same way.

1st Blow	1st Piece	6·19°
	2d Piece	7·30
2d Blow	1st Piece	5·85
	2d Piece	2·14
3d Blow	1st Piece	2·76
	2d Piece	2·02
Total evolved from the 1st piece		14.74
Ditto from the 2d		11·46

The change in specific gravity which the metals underwent, was found to be proportional to the heat thus evolved, as appears from the following table, deduced from their experiments. The specific gravities were taken at the temperature of 46·5°.

Specific gravity of gold	19·2357
Ditto annealed	19·2240
Ditto struck	19·2487
Specific gravity of silver	10·4667
Ditto annealed	10·4465
Ditto struck	10·4838
Specific gravity of copper	8·8529
Ditto struck	8·8898
Ditto struck a second time	8·9081

From these experiments it is obvious, that the heat evolved when metals are struck is owing to the condensation, and proportional to the condensation. Hence, when they can no longer be condensed, they cease to evolve heat. These philosophers observed, during their experiments, that heat or cold is propagated much more rapidly, from one piece of metal to another, when they are struck, than when they are simply placed in contact.*

There can be little doubt that friction occasions an increase of density as well as hammering. But this increase must be confined to the solid portion, and not exist in the scales or dust rubbed off, which alone Count Rumford examined. Thus the experiments of Rumford and Davy, though they admit of an easier explanation if we were to consider heat as a mere property of matter, do not furnish the demonstration of the immateriality of heat which these philosophers thought they did.

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* Mem. d'Arcueil, ii. p. 441.

PART II.

OF ELECTRICITY.

Part II.

Electricity, which occupies so conspicuous a place in modern science, was nearly unknown to the sages of antiquity, and can scarcely be said to have commenced as a science till after the beginning of the 18th century.

The ancients were aware that *amber* when rubbed acquires the property of attracting light bodies to it.[*] Theophrastus, speaking of the *lyncurium stone*, observes, that it has an attractive power like that of amber, and that it is said not only to attract straws and small pieces of sticks, but even copper and iron, if they be beaten into thin pieces.[†] These

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[*] Pliny says, "Cæterum attritu digitorum accepta vi caloris (*succina*) attrahunt in se paleas et folia arida, ut magnes lapis ferrum." Lib. xxxvii. c. 3. But amber seems not to have been distinguished by the ancients from some other transparent resins. Pliny informs us that it grows in India, and that Archelaus, king of Cappadocia, had specimens of it adhering to the bark of trees.

[†] Το λυγκυριον ἕλκει γαρ ὡσπερ το ελεκτρον· ὁι δε φασιν ὀυ μονον καρφη κὰι ξυλον, ἀλλα και χαλκὸν κὰι σίδηρον, ἐαν ᾗ λεπτος. ὡσπερ και Διοκλης ελεγεν. Theophrast. περι των λιθων, sect. 1. There are very strong reasons for believing that the lyncurium of the ancients is the same mineral with the *tourmalin* of modern mineralogists. See Watson, Phil. Trans. 1759, p. 394.

two facts constitute almost every thing respecting electricity known to the ancients.

Dr. Gilbert greatly multiplied the number of substances which are capable of acquiring electrical properties. Mr. Boyle made some attempts to explain the phenomena of electricity known in his time, upon mechanical principles.* Haukesbee first attended to the light emitted by excited bodies.† And his apparatus for exciting electricity probably led the way to the electrical machine.‡

Discoveries of Gray.

But it was Mr. Stephen Gray who laid the true foundation of electricity, as a science, in the important papers inserted by him in the Philosophical Transactions between the years 1720 and 1736. He found that certain bodies can be excited by friction, and others not. This led him to divide bodies into two sets, viz. *electrics* and *non-electrics.* He ascertained that non-electrics may be excited by being brought into the neighbourhood of excited electrics. Finally, he discovered that electricity passes with ease through any length of non-electrical bodies; but not through electrics. This induced him to call the former *conductors*, and the latter *non-conductors* of electricity.

Of Du Fay.

In the year 1733 M. du Fay, of the French

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* See Shaw's Boyle, i. 506.

† Dr. Wall first concluded from experiment that all electric bodies became luminous when rubbed. Phil. Trans. 1708, vol. xxvi. p. 69.

‡ *Physico-Mechanical experiments on various subjects. Containing an account of several surprizing phenomena touching light and electricity, producible on the attrition of bodies,* &c. 1709.

Academy of Sciences, began his electrical career. He published in succession eight Memoirs on Electricity, equally remarkable for candour and scientific acumen. To him we are indebted for three important discoveries. 1. He found that conductors may be excited by friction, as well as non-conductors, provided we take the precaution to insulate* them before we begin to rub. It was from not attending to this necessary precaution that Mr. Gray had failed in exciting these bodies. 2. Electrical bodies, when excited, attract those that are not excited, communicate electricity to them, and repel them. 3. There are two kinds of electricity, the *vitreous* and the *resinous*. The first is produced when we rub smooth glass with a woollen cloth, or with hair; the second when we rub *amber*, *sealing-wax*, or *gum lac*. Bodies having the *same* kind of electricity *repel*, those having *different* electricities *attract* each other. The reason why excited bodies attract light substances to themselves, is that they produce in them the contrary electricity from that which they themselves possess. Hence they are attracted in consequence of the general law that bodies having the opposite electricities *attract* each other. The last of M. du Fay's papers on electricity appeared in the Memoires de l'Academie for 1737. He died of the small-pox in 1739, at the early age of 41.

In the year 1746 a discovery was made by Mr.

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* By *insulation* is meant surrounding a body with non-conductors, so that no conductor shall be in contact with it.

Of Cuneus, Muschenbroeck, and Allamand.

Cuneus in company with Messrs. Muschenbroeck and Allamand, which attracted the attention of the whole civilized world, and gave such an eclat to electricity, that it for many years drew to itself almost the exclusive attention of men of science. Observing that electrified bodies, when exposed to the common atmosphere, soon lost their electricity, and were capable of retaining but a small quantity, these gentlemen imagined that if an excited body were terminated on all sides by electrics, it might be capable of receiving a stronger power, and retaining it a longer time. A gun barrel was suspended on silken strings, having one of its ends very near a glass globe, which was turned rapidly, while electricity was excited in it by friction with the hands. A wire was hung from the other extremity of the gun barrel, which dipped into a beer glass full of water, and held in the hands of one of the gentlemen. After the globe had been driven for some time, the gentleman who held the beer glass approached his finger to the gun barrel to draw a spark from it. The consequence was a *shock*, differing in violence according to the length of time that the globe had been turned. Sometimes it affected only the arms as far as the elbow, sometimes to the shoulders; and sometimes the whole body was affected.* Such was the origin of the famous Leyden phial experiment, which within a few months was publicly exhibited for money in London and Paris, and all the principal cities of

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* Hist. de l'Academie, 1746, p. 1.

Europe. This famous experiment was immediately repeated in Paris by the Abbé Nollet, and by M. Lemonnier, and in London by Dr. Watson. But it was Dr. Franklin who first explained it in a satisfactory manner.

Of Dr Franklin.

Dr. Franklin first presented himself to the scientific world, as an experimenter on electricity, in 1747. But his *Experiments and Observations on Electricity, made at Philadelphia in America*, were not published till the year 1754. This publication forms a remarkable era in the history of Electricity. It was translated into almost every European language, and the opinions which it contained, were almost universally adopted. Dr. Franklin conceived that only one electric fluid existed,—and that all the phenomena of electricity were occasioned either by its accumulating in bodies, in more than its natural quantity, or by its being withdrawn from bodies so as to leave in them less than the usual quantity. When a body contains its natural quantity of electricity, it exhibits no electrical phenomena whatever. When electricity accumulates in it, the phenomena of the *vitreous* electricity of Du Fay are exhibited. When electricity is deficient, we perceive in it the phenomena of the *resinous* electricity of Du Fay. Hence Dr. Franklin substituted for *vitreous* and *resinous*, the terms *positive* and *negative*, or *plus* and *minus* electricity.*

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* I shall employ in the ensuing treatise the terms *positive* and *negative* electricity to denote the two electricities discovered by Du Fay. But I do not adopt the hypothesis of Franklin, that only one electric fluid exists. The phenomena do not seem expli-

Besides this theory, which was probably the cause of the immediate celebrity of Dr. Franklin, electricity is indebted to him for three capital discoveries, upon which his reputation as an electrician will finally rest. 1. Electricity is dissipated at a great rate by points, so that it is impossible to accumulate it in pointed bodies. This led to the *thunder rod* as a security for buildings. 2. The second and great discovery of Dr. Franklin was that lightning and thunder are occasioned by the accumulation of electricity in the atmosphere; or, in other words, that *lightning is the same thing with electricity*. This he proved by drawing lightning from the heavens, and showing that it possessed all the characters of common electricity. 3. His third discovery was the analysis of the Leyden phial, which contributed more than any thing else, to establish his peculiar theory of electricity.

Dr. Franklin's theory was exhibited in a Mathematical dress by Æpinus in 1759,[*] and by Mr. Cavendish in 1771.[†] They laid down the hypothesis, and deduced the mathematical consequences from it. These consequences, provided we admit the truth of certain suppositions which Æpinus was obliged to make, flow directly from the theory.

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cable on any other supposition than the existence of two electric fluids. But the terms *positive* and *negative* applied to these two merely as proper names are more convenient and less objectionable than the terms *vitreous* and *resinous* applied to them by the French Electricians.

[*] *Tentamen theoriæ electricitatis et magnetismi.*

[†] Phil. Trans. 1771, p. 584.

Of Lord Mahon.

In the year 1779 a highly important work on electricity was published by Lord Mahon, afterwards Earl of Stanhope.* In this work the author enters at great length into the nature of what are called *Electric Atmospheres*. The term, I believe, was introduced by Dr. Franklin, though his notions on the subject want that precision, which usually characterises his ideas. Mr. Canton demonstrated that the air in the neighbourhood of a charged conductor, or an excited body, gradually acquires the same kind of electricity, with which the body itself is charged: so that the air round an excited body, to some unknown distance, is in the same state of electricity with the body itself. The greater the size of the excited body, and the more electricity which it contains, the greater will be the distance to which the surrounding air will be possessed of the same kind of electricity. Now it is this portion of surrounding air, which has been called an *electric atmosphere*.

Lord Stanhope demonstrated by very beautiful and decisive experiments, that the quantity of electricity in this electric atmosphere diminishes inversely as the square of the distance from the electrified body. Hence the reason why the electrical atmosphere cannot possess any sensible electric properties, at any considerable distance from the electric body. It was this investigation that led

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* *Principles of Electricity, containing divers new theorems and experiments, together with an analysis of the superior advantages of high and pointed conductors.*

Lord Stanhope to the important discovery of the *returning stroke.*

Of Coulomb.

The next series of discoveries on electricity was made by M. Coulomb, and published by him in four Memoirs inserted in the Memoires de l'Academie de Paris for the years 1785 and 1786. M. Coulomb had constructed a very delicate electrical balance, which measured the attractive and repulsive forces by the tortion of a very fine wire. By means of this balance he made three capital discoveries, which constitute in fact the first principles of all electrical theory. 1. The attractions and repulsions of electrical bodies vary inversely as the square of their distances. So that these forces vary precisely in the same ratio as gravitation does. 2. When insulated bodies are charged with electricity, they do not retain their electricity but gradually lose it. This is partly owing to the surrounding atmosphere, which never being absolutely free from conducting particles, these particles gradually get possession of the electricity and carry it off. It is partly owing also to the electric bodies, which serve as insulators. There is probably no substance absolutely impervious to electricity; though some are more and some less so. Hence the electricity gradually passes off along the insulating body. Coulomb determined the effect of both of these causes, from which the electricity is carried off from an excited body, and thus put it in the power of electricians to calculate how much electricity an insulated body loses in a given time. Such a calculation becomes necessary when we wish to compare the

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forces of electrical bodies at different distances. For these comparisons being made in succession, could not lead to accurate results unless we ascertain and allow for the electricity lost during the course of the experiment. 3. Coulomb showed that when electricity was accumulated in any body, the whole of it is deposited on the surface and none of it penetrates into the interior. So that a hollow sphere, however thin its walls be, may be charged with just as much electricity as a solid metallic sphere of the same diameter. It was obvious from this that electricity was not accumulated in bodies in consequence of any affinity which it has for them; but solely in consequence of its repulsive action.

Of Poisson.

The latest improvements in the theory of electricity have been made by M. Poisson, who taking the two electricities of Du Fay, and the laws discovered by Coulomb for a basis, has applied mathematical calculation to the science, and has shown the way in which it is distributed on the surfaces of bodies of various figures, and the thickness of the stratum of electricity in different parts of these bodies. A good many of his theoretical deductions had been already obtained experimentally by Coulomb. But several of them have not yet been verified by experiment; nor would it be easy to devise experiments capable of determining the accuracy of the consequences which he has deduced from mathematical reasoning.

Such is a short and imperfect view of the history of electricity. Let us now proceed to the general principles of the science.

CHAPTER I.

GENERAL PRINCIPLES.

THERE are some properties of bodies, so essential (so to speak) to their existence, that we can scarcely conceive them to be wanting without destroying the body altogether. Thus gold is *extended* and possessed of *weight*. Nor can we conceive this metal to exist at all if we were to suppose it deprived of *extension*. There are other properties not so essential to the existence of bodies. They may be wanting, or they may be present, without altering the nature of the body in which they occasionally reside. Lead is not always fluid; but it may be rendered fluid by heating it sufficiently. Iron does not always *point to the north*, but it may be made to do so by communicating magnetism to it, and then suspending it, so that it is at liberty to turn freely in every direction. Glass has not always the property of drawing light bodies to it, and then repelling them again. But if we rub it with the dry and warm hand, or still better if we rub it with a piece of leather covered over with an amalgam of zinc and tin, it acquires this property.

Electrical properties what.

1. Thus glass and many other bodies acquire by friction a property, which they did not possess before; the property of alternately *attracting* and *repelling* light bodies. Now this is the property

which is distinguished by the name of *electricity*. It was first observed in *amber*, to which the Greeks gave the name of ἤλεκτρος. Hence the term *electricity*.

2. If we make the excited tube approach the cheek, we have a sensation resembling the application of a cobweb to the cheek.

The spark.

3. If we make our finger approach within a small distance of the rubbed tube, we hear a snap, and a spark passes between the tube and the finger. In the dark, the glass tube appears covered with a bluish light.

When glass or any other substance has thus acquired the property of attracting light bodies, it is said to be *excited*.

4. What is the cause of the new properties induced in these bodies by friction? We are altogether ignorant of it; though a variety of hypothetical explanations have been advanced. Some have ascribed the new properties to the agency of a peculiar fluid, existing in all bodies, and called the *electrical fluid;* but not sensible till it either accumulates beyond the usual quantity, or till it becomes actually deficient. Others suppose the existence of two distinct fluids, which they endow with certain properties to enable them to explain the electrical phenomena. While a third set deny the existence of an electrical fluid altogether, and consider the attractions and repulsions as mere properties induced into matter. Whatever the cause of these new properties may be, we may distinguish it, although unknown, by applying to it the term *electricity*.

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5. Glass, sulphur, sealing wax, resins, gums, silk, wool, hair, and even paper and wood, if well dried, may be excited by friction, But when we attempt to excite a piece of metal by the same process, we find it impossible to succeed. It was supposed at first, that metals were incapable of being excited by friction, and on this account all substances were divided into two classes, namely, *electrics* and *non-electrics*—the first consisting of those bodies which may be excited by friction, the second of those that cannot. But it was afterwards ascertained, that metals do not want the power of being excited by friction; but they are unable to retain the electricity after they have obtained it. If we place a plate of metal upon a cake of sealing wax or gum lac, and rub it with the fur of a cat, we shall find upon examining it in the usual way, that it has become excited.

Electrics and non-electrics.

Excitation.

Insulation.

Bodies thus supported on *electrics*, are said to be *insulated*. Insulation must always be attended to when we wish to induce any electricity in metallic bodies. Coulomb has shown that the best insulator is *gum lac*. Glass in order to insulate should be varnished. If not, it does not insulate except in dry weather. A silk thread in order to insulate should be of some length.

Even the friction of fluids upon solid bodies is sufficient to excite them. Mercury shaken in the inside of a glass tube electrifies the tube. If a wooden vessel containing mercury be cemented on the top of a glass receiver, and the receiver be exhausted by means of an air-pump, the mercury is driven by the atmosphere through the pores of the wood, and falls in a shower into the receiver, striking against its sides. The jar by this shower of mercury is excited. In like manner, we can excite glass by blowing on it with a pair of bellows. Whether two gases produce electricity when they rub against each other, as is the case when two opposite currents of air cross each other, has not been determined. But analogy renders the thing very probable. In this way probably is induced part of the electricity which almost always exists in the atmosphere, and which occasions so many important meteorological phenomena.

6. An excited body attracts and repels light bodies at a distance—nor is the effect prevented by interposing a solid body. Thus, if we suspend within a glass jar, a small ball composed of the pith of elder, about the tenth part of an inch in diameter, by a fine silk thread, it will be attracted to an excited glass tube placed on the outside of the jar.

Conductors and non-conductors.

7. The reason why some bodies are easily excited by friction, while others cannot, depends upon the facility with which electricity is capable of moving through them. Through the former it moves very slowly or not at all—but through the latter it moves with great facility. The former are *non-conductors* or *bad conductors* of electricity; the latter are *good conductors*.

Metals and charcoal, water and all liquids (oils excepted), are good conductors. Melted wax and tallow are good conductors; but these bodies while

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solid, conduct very badly. The flame of alcohol and ice are good conductors.

Glass, resins, gums, sealing wax, silk, sulphur, precious stones, oxides, air, and all gases, are non-conductors, or at least very bad conductors.

If a conductor, how long soever it may be, be brought in contact (by one extremity) with an excited non-conductor, it instantly takes a portion of the electricity from the non-conductor. Indeed, if the conductor be in communication with the earth, the whole accumulated electricity will soon pass along the conductor, and be dissipated in the earth; so that we shall not be able to find any traces of electricity either in the non-conductor or conductor. But if the conductor be insulated, by suspending it from silk strings, for example, or supporting it by cylinders of gum lac, it will become excited as well as the non-conductor. And the electricity will pass along the whole of it, so that the extremity farthest removed from the excited electric will exhibit the phenomena of electricity to as great an extent as the extremity nearest the excited electric. Suppose an iron or copper rod twenty or thirty feet in length to be suspended on silk strings, and that an excited glass tube be placed in contact with one extremity of it, the other extremity will immediately exhibit all the usual phenomena of an excited body. But if we substitute a rod of glass or sealing wax in place of the metallic rod, it will not exhibit any sensible electricity at the remote end, even although its length should not exceed a few inches.

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If we suspend a small pith ball by a slender metallic wire, and touch it with an excited glass tube, we shall find that it will not retain the communicated electricity an instant; but if the pith ball be suspended by a silk thread, it will retain the electricity communicated to it by an excited glass tube for a considerable time, if the day be dry, and the air clear, when the trial is made.

Dry air is a non-conductor, but moist air is a conductor. Hence the reason that electrical experiments do not succeed in rainy or foggy weather. We may, however, in general succeed in producing electrical phenomena whatever be the state of the atmosphere, if we take care to dry all the substances which we employ, and keep them dry by bringing them into the neighbourhood of a good fire. Or by covering them with a thin coating of tallow, which keeps the moisture at such a distance that we may charge a Leyden jar, as usual, even in a very moist atmosphere. If we use the tallow, we must beware of such a heat as would melt it, for becoming a conductor by fusion, all electric action would be destroyed. This fire has the effect also, by raising the temperature of the air, of increasing its dryness, and thus preventing it from carrying off the electricity so rapidly as it would otherwise do.

Electricity does not alter the bulk of bodies.

8. Electricity does not alter the bulk of bodies. The bulk of a piece of metal is not increased by charging it with electricity, nor does the bulk of a glass tube, or stick of sealing-wax, alter when it is excited, except so far as the friction may increase the temperature of the body.

Neither is the nature of the electricity altered by the temperature of the body which contains it. An electric spark drawn from *ice* does not feel *cold*, nor does a spark feel hot when drawn from red hot iron. Dufay was of opinion, that a spark drawn from a living body was more painful, and communicated a much more burning sensation, than when drawn from dead matter.

Its motion very rapid.

9. Electricity moves through conducting bodies with such rapidity, that no interval of time whatever can be perceived during the transit, how long soever it may be. Dr. Watson, and several other members of the Royal Society, made an electric charge pass through a wire, extending more than two miles, or 12,276 feet. One gentleman held a charged Leyden jar in one hand, and the extremity of the wire in the other. Another gentleman held the other extremity of the wire in one hand, and with the other hand brought a short iron rod in contact with the knob of the Leyden phial. The consequence was a discharge of electricity, and both the gentlemen received the shock at the same instant of time. So that electricity passes over a circuit of two miles instantaneously, or at least in an interval of time too short to be noticed.* Indeed in one of Dr. Watson's experiments the circuit through which the electricity passed was nearly six miles in length, yet no sensible interval of time was taken up by it in this long extent through which it had to pass.

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* Phil. Trans. 1747, p. 49; 1748, p. 491.

Bodies do not retain electricity in a vacuum.

10. It has been found that bodies cannot be made to retain electricity in the vacuum of an air-pump. The electricity flies off as fast as it is evolved, so that no substance can become charged. Hence it is obvious that the presence of dry air is necessary to prevent the electricity of excited bodies from being dissipated. It is commonly supposed that air produces this effect by the pressure which it exerts upon the surface of the excited body. But it is much more natural to ascribe it to the state of electricity which the surrounding atmosphere acquires. It has been demonstrated by the experiments of Mr. Canton, and Lord Stanhope, that the air surrounding an excited body acquires the same kind of electricity with the body itself. The intensity of this electricity of the air diminishes inversely, as the square of the distance. Hence at a small distance from the excited body it becomes insensible. Now, as electricities of the same kind *repel* each other, it is obvious that the electricity of the air surrounding an excited body, will repel the electricity of the excited body; and this repulsion being the same on all sides, it must have the effect of preventing the electricity of the excited body from being dissipated. When this repulsive force is withdrawn there is nothing to oppose the repulsive force which the particles of electricity exert against each other. Hence they must be immediately dissipated.

Two kinds of electricity.

11. If we suspend two small pith balls, by means of very slender wires, from a stick of sealing wax, and rub the wax with a dry woollen cloth, the two

balls will *repel* each other. If we suspend another pith ball, by a slender wire from a stick of sealing wax, and rub it also with a dry woollen cloth, this pith ball, if brought near the two former, will also be *repelled* by them. If we suspend the pith balls by slender wires from dry glass tubes, and rub the tubes with dry woollen cloth, the result will be the same,—all the pith balls will repel each other. But if we suspend a small pith ball by a slender wire from a glass tube, and another in a similar way from a stick of sealing wax, and make the two balls approach, after having rubbed the glass tube and the stick of sealing wax, the pith balls, instead of repelling, will *attract* each other.

Hence it is obvious that the electricity excited in glass is different from that excited in sealing wax: since bodies having the electricity of glass, *attract* those having the electricity of sealing wax; while bodies having each, either the electricity of glass or of sealing wax, *repel* each other. M. Dufay, the discoverer of this most important fact, distinguished these two electricities by the names of *vitreous* and *resinous*, from the two substances in which he first perceived them. Dr. Franklin afterwards termed them *positive* and *negative* electricities, and these terms have come into general use.

Positive and negative.

Bodies charged with the same kind of electricity, whether positive or negative, *repel* each other; but bodies charged with different electricities *attract* each other. Bodies charged with positive electricity, *repel* bodies charged with positive electricity and *attract* bodies charged with negative electricity. Bodies charged with negative electricity, *repel* bodies charged with negative electricity, and *attract* bodies charged with positive electricity.

Friction evolves both electricities.

12. When two substances capable of being excited, are rubbed against each other both acquire electricity, but each is charged with a different kind. One becomes positively electrified, and the other negatively. This law holds universally; but what kind of electricity each substance acquires depends upon the substance against which it is rubbed. For example, if we rub dry woollen cloth against *smooth glass*, it acquires *negative* electricity, while the glass becomes *positively* electrified. But if we rub woollen cloth against *rough glass*, it acquires *positive* electricity while the glass is charged with *negative* electricity. The following table contains a number of electric substances arranged in such a way, that when they are rubbed against each other, the substance first in the list becomes positively, and the substance below it negatively electrified.

1 Fur of a cat,
2 Smooth glass,
3 Woollen cloth,
4 Feathers,
5 Wood,
6 Paper,
7 Silk,
8 Lac,
9 Rough glass,
10 Sulphur.

The fur of a cat acquires positive electricity against what substance soever it be rubbed. Sulphur acquires negative electricity against whatever substance it be rubbed. Feathers acquire negative electricity when rubbed against the fur of a cat, smooth glass or woollen cloth; but they acquire positive electricity when rubbed against wood, paper, silk, lac, rough glass and sulphur. The same observation applies to every substance in the table. It assumes *negative* electricity when rubbed against any substance *above* it in the table, and *positive* electricity when rubbed against any substance *below* it.

Electricity by induction.

13. When an insulated conductor is brought into the neighbourhood of an insulated charged conductor, its electricity undergoes a new arrangement. The end of it next the excited conductor assumes a state of electricity opposite to that of the excited conductor; while the farther extremity assumes the same kind of electricity. Suppose a conductor charged with positive electricity. The end of the insulated conductor next it becomes negative, and the end farthest off positive. And intermediate between these two points, there occurs a place where neither positive nor negative electricity can be perceived. This place is called the *neutral point*.

This is the reason why light bodies are attracted by excited electrics. They acquire the opposite electricity, and of course are attracted. When they touch the excited electric they acquire the same kind of electricity which it has, and consequently are repelled. This electricity they speedily lose by coming in contact with conductors connected with the earth. They are again attracted, again repelled; and these alternate attractions and repulsions continue till the electricity of the excited body is dissipated.

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Electricity evolved by fusion, heat, pressure, &c.

14. Though friction is the usual way of producing electricity, yet it is evolved in many other ways. It has been ascertained that when bodies are *melted* and allowed to congeal again, they frequently become electric. It has been found that some bodies become electric by *pressure*. There are several mineral bodies, the most remarkable among which is the *tourmalin*, that acquire marked electric properties when their *temperature is elevated* a certain number of degrees; which properties they lose again when they are allowed to cool. Few chemical *combinations* or *decompositions* take place without evolving electricity. Finally Volta has shown that when two metals (supposing both insulated) are placed in contact and then separated, both give signs of electricity; the one being always positively, and the other negatively electrified.

CHAPTER II.

OF ELECTRICAL APPARATUS.

Chap. II. ELECTRICAL investigations being chiefly conducted by means of the electrical machine, it will be requisite before proceeding any farther with an account of the phenomena or their investigation, to give a short description of the electrical machines at present used by electricians.

There are two distinct kinds of electrical machines at present employed; namely, the *cylindrical*, and the *plate* machine.

The cylindrical machine undoubtedly originated from the glass globes of Mr. Hauksbee. He fitted them upon an axis, which he turned round rapidly by means of a winch, and subjected them to the friction of the hand, or of a woollen cloth. After the discovery of the Leyden phial, improvements in this simple apparatus were successively introduced. Oiled silk was found to answer better as a rubber than the hand. Cushions of leather stuffed with hair or wool, and made as smooth and equal as possible, were found still preferable. And Mr. Canton discovered in 1762, that if a quantity of amalgam of tin mixed with some whiting, be spread upon the cushion, it would excite the globe to a great degree with very little friction.*

It was gradually found that glass cylinders might be substituted for the globes with advantage. And these cylinders at last came to be generally substituted for the globes. The cylindrical machine at present used in this country, was the contrivance of Mr. Edward Nairne, a very ingenious philosophical instrument maker in London. His first account of it appeared in the Philosophical Transactions for 1774. But he afterwards added considerably to its simplicity and its elegance.

Cylindrical machine.

Nairne's cylindrical machine consists essentially of a glass cylinder of any dimensions, supported on two glass pillars upon a mahogany board. This glass cylinder has at one extremity a winch, with a handle by means of which it may be turned briskly round. On each side of the glass cylinder and parallel to it, are two cylinders of tin plate japanned, nearly as long as the glass cylinder, but not so thick, and terminated at each end by a hemispherical surface, so as to avoid all angles or points, and to make the whole cylinders as smooth and equable as possible. These tin plate cylinders are supported each by a glass pillar of the same size and height as the pillars which support the glass cylinder. These two tinplate cylinders are called the two prime conductors of the machine. To one of them is fixed the cushion, which is to act as a rubber. This cushion consists of leather stuffed with hair, and

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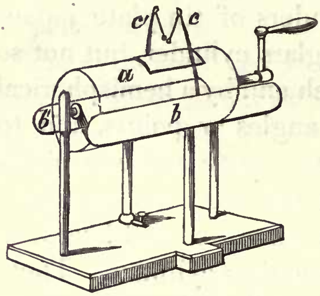
* Phil. Trans. vol. lii. p. 461.

made by means of a spring to press equably against the glass cylinder. To the cushion is attached likewise a flap of black silk, which covers the upper part of the glass cylinder, and prevents the electricity from being dissipated till it reach the conductor on the opposite side.* This second conductor is precisely similar to the first; only instead of a cushion it has a number of pointed brass wires attached to its inside, pointing directly to the glass cylinder, and approaching as near it as possible, so as not to touch it. The use of these pointed wires is to draw the electricity from the glass cylinder, in proportion as it is evolved, and to conduct it to the conductor to which they are attached. Each of these conductors has a certain number of holes in its surface, to which wires, electrometers, &c. may be attached, and into which brass knobs may be inserted to regulate the discharge of the electricity.



The figure in the margin represents a view of Nairne's cylindrical machine.

In it *a* is the glass cylinder, and *b b′* the two prime conductors. *c*, *c′* are two electrometers attached to the two conductors, to shew that they are in different electrical states, the one to which the rubber is attached being negative,

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* This flap is greatly improved when it becomes stained with grease. Hence an old flap will be always found to act better than a new one. The new one becomes immediately as good as the old, if we impregnate them with tallow.

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and the other positive. Hence the two electrometers are attracted when the machine is worked.

The prime conductor to which the cushion is attached becomes charged with negative electricity when the machine is put in motion; while the opposite prime conductor becomes charged with positive electricity. That the machine may be capable of acting, it is requisite that the prime conductor to which the cushion is attached, should be connected with the ground by means of a metallic chain suspended from it.

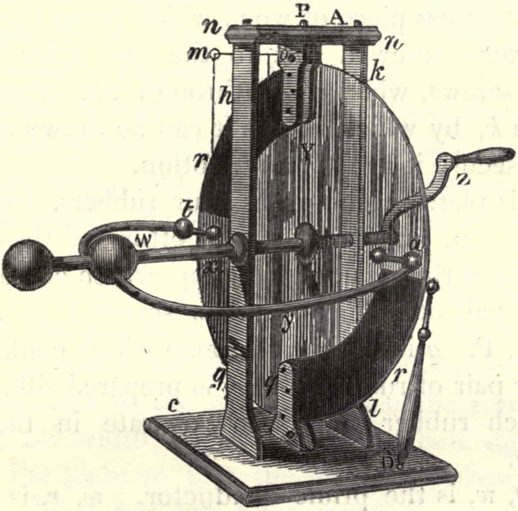


Plate machine.

Plate machines were first brought to a state of perfection by Mr. Cuthbertson, when he settled in Amsterdam about the year 1769. The above figure represents his plate machine, as it is now in common use. A C D is the mahogany stand to which all the different parts of the machine are fixed. C D is a square piece of mahogany which forms the basis of the machine, about two feet long, one broad, and an inch and a half thick. To this bottom two upright beams or stiles are fixed, which constitute the wooden frame of the machine. *g h*, one of these beams, is a straight wooden stile, glued fast to C. *k l* is the other beam or stile, not glued as the former, but fixed by three screws—one at the front to draw it close to C D, and two under the bottom, to draw the stile downwards upon the bottom, that it may be secure against any motion. *n n* is a cross piece of wood which forms the top of the frame, screwed fast to the two stiles by two brass screws, which pass through the cross piece into *h k*, by which means it can be drawn so tight as to secure it against any motion.

This plate machine has four rubbers, two fixed at the top, and two at the bottom of the frame. *o* is the spring frame at the top, which contains the upper pair of rubbers, and is screwed to A by a screw, P. *q* is the spring frame which contains the under pair of rubbers. *r*, *r*, is prepared silk, sewed to each rubber, and seen separate in the next figure.

a, *t*, *w*, is the prime conductor. *w*, *x*, is a solid stick of glass, which serves to support, and also to insulate the conductor. The end *w* is mounted with brass, which screws into the centre of the large ball of the conductor. *Y*, *y*, is a round plate of mirror glass, fixed upon an axis, one end of which turns in a hole in the stile *g*, *h*, and the other runs through a hole in the middle of the stile *k*, *l*, and is turned by means of a winch, *z*, which causes the plate to revolve and pass between the rubbers fixed at the top and bottom of the frame.

Fig. 3, represents the spring frame of the upper cushions on one side, and the silk, for the sake of distinction, is taken off.

Fig. 3.

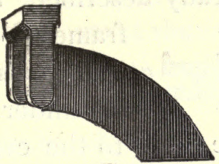
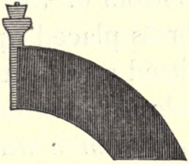


Fig. 4, represents a single rubber with the silk flap. It is covered with leather, and stuffed with slips of woollen cloth. Then a piece of silk properly prepared, and cut to the shape, is sewed fast to the middle of the rubber. By turning the screw one way it will draw the rubbers close, and by turning it the other it will loosen them. As it is difficult to get at the head of this screw to turn it with the fingers, there are holes made at the side of the head of the screw in which the end of a wire may be put, by which it may be easily turned. To fix the screw frame and rubbers to the machine, open the rubbers to about the width of half an inch, then slip them upon the plate so that the top comes close under *n*, *n*. Then put the long screw, P, through the hole in the top of the machine, and screw it into the top of the frame of the rubbers, very tight, taking particular care that the plane of the rubbers applies to the plane of the plate. This will hold the rubbers in their proper place. *m*, is a wire from which proceeds two or three silk cords fastened to

Fig. 4.



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the edge of the silk flaps, which serve to prevent them adhering to the edge of the plate while turning.

The rubbers, *q*, are constructed exactly as those already described, but are fixed differently to the frame of the machine. The piece, *a*, *b*, which forms the bottom of the frame for the under rubber, is cut open from one side quite to the centre of the piece, and at the bottom of the frame of the machine is a piece of brass placed upright with a male screw, to which is fitted a nut with a female screw, having holes at the edge for the convenience of screwing it tight by means of a wire. The two sides with the rubbers, are placed upon the bottom of the machine, so that the upright brass pin goes into the groove, and is there screwed tight by the female nut.

a, *t*, *w*, is the prime conductor of the machine. *w* is a large brass ball with a shank about an inch long fitted into the cylindric part, so that it may be turned for the purpose of placing the hole, which is seen at one side of the ball, in any position that the experiment may require. This hole is useful for a variety of purposes. The conductor is supported and insulated by the solid glass cylinder, W, *x*. If required at any time to be taken to pieces, the cylinder part of the conductor must be unscrewed from the large ball, where there is a square nut, which being unscrewed, W, *x*, may be taken out. One end of the two receiving arms or tubes, *t*, *a*, is screwed into the large ball by W, one having a left handed screw so that the arm may be supported by the shoulder to prevent it falling by its weight. The two cross pieces fronting the plate, with two or three points to receive the excited fluid, are each screwed on to their respective arms, one with a right handed screw, the other with a left handed one, for the same purpose as the screws at the other end of the arms. The end, *x*, of the glass cylinder is mounted with a joint, which fits into the frame of the machine opposite the hole in which the axis moves.

Amalgam.

To give this machine all its energy, the rubbers require to be covered with amalgam. This is made by taking equal weights of zinc and tin, and melting them in a crucible. The melted mixture is poured into twice its weight of mercury in a wooden box made for the purpose. The box is to be well shaken till the metals are cold. The amalgam is then to be reduced in a metal mortar to a very fine powder, and afterwards mixed with a sufficient quantity of hog's lard to make it into a paste. I believe the best proportions for making amalgam would be

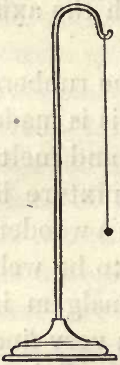
Zinc .	8·5 parts
Tin .	7·25
Mercury	37·5

Such an amalgam, when kept, is apt to crystallize. But it is rendered fit for use by pounding it in a mortar before using it.

When the machine is to be used, the rubbers should be taken off and cleaned by scraping off the old amalgam with a knife and rubbing the silk with a cloth. The rubbers should then be placed in a warm place till they are well dried. New amalgam must now be spread on with a knife. It should be laid on flat and smooth, and to such a thickness that the surface of the amalgam is level with the surface of the silk, and there should be no openings or separations between the amalgam and the silk.

Electrometers.

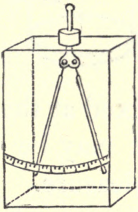
A variety of instruments have been invented for detecting electricity when it exists. These instruments have received the name of *electrometers*. The simplest of these is the pendulum electrometer represented in the margin. It consists of a glass rod fixed in a sole, and bent at the top so as to form a hook. From this hook hangs a thread of raw silk, to the bottom of which is fixed a very small ball made of the pith of the elder, quite round and weighing only a small fraction of a grain. When an excited glass rod or any similar body is brought near this pith ball it is immediately attracted, and the stronger the electricity the farther will the ball be drawn from the perpendicular.



Another common and very useful electrometer is represented in the margin. It consists of two pieces of straw, *a*, *a'*, both smooth and of the same size and length, suspended parallel to each other, and at a very small distance from each other by two very fine metallic wires bent at the top into hooks, which are attached to two small holes, *b*, *b'*, in a small piece of metal terminating above in a knob, *c*. When any excited body is made to touch the

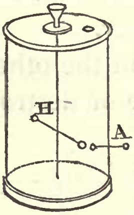
knob, *c*, the straws, which are very light and hang freely suspended, acquiring the electricity of the touching body immediately separate from each other, and indicate the existence of electricity. And the farther they separate it is obvious that the quantity of electricity must be the greater.

To prevent the agitation of the air and the other accidents which would speedily derange or destroy the straws, the electrometer is usually enclosed in a square glass box as here represented, upon one of the faces of which is pasted a circular division to mark the degree to which the straws separate. The neck and upper part of this glass box is varnished with gum lac, in order that it may insulate the better.



Instead of straws, slips of gold leaf are employed with advantage. This constitutes what is called Bennet's gold leaf electrometer, which is a very delicate instrument, and capable of indicating very minute quantities of electricity. See a description, Phil. Trans. 1787, p. 26.

In these and many other common electrometers which I think it needless to describe, the instrument cannot be considered as a true measurer of the quantity of electricity, because as the two straws or the two slips of gold leaf separate farther and farther from each other, it is evident that gravitation will act more and more powerfully to bring them back again to their naturally vertical position. Hence the repulsive force of the straws or leaf is not proportional to the distance to which they separate from each other. These instruments cannot of course be employed to measure the energy of electricity.



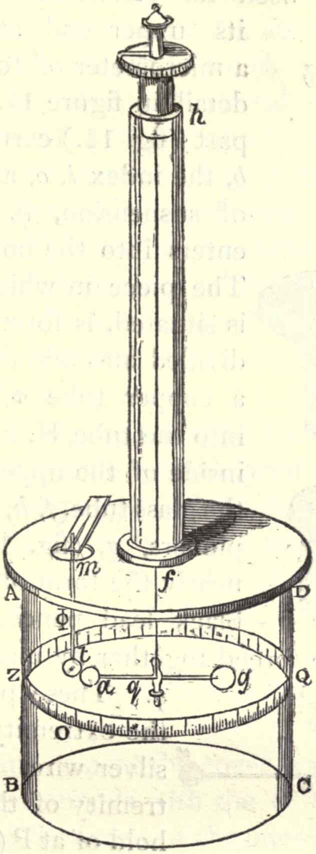
But the electrometer of Coulomb is free from this defect. It is represented in the margin. It consists of a glass vessel having a lid also of glass, in the centre of which a small hole is drilled. Through this hole passes an untwisted raw silk thread four inches long, and fixed at the top to a micrometer, by means of which it may be turned round any number of degrees at pleasure. To the silk thread is attached a very fine gum lac thread, H, having at each extremity a small knob. This lac needle with its knobs weighs only $\frac{1}{4}$ grain. A small hole is drilled in the side of the vessel, at A, through which passes a fine wire terminated at both extremities by a knob. When an excited body is placed in contact with the knob at A, the knob at the other extremity will acquire the same electricity as the excited body. This electricity it will communicate to the knob of the lac needle suspended by the silk thread which was previously almost in contact, and the two knobs will repel each other. The moveable knob attached by the silk thread will separate from the other, and the quantity of electricity will be proportional to the distance to which it is driven off.

Coulomb's balance.

Coulomb's electrical balance is an instrument intended to measure the quantity of electricity in bodies, and indispensable in accurate experiments.

Fig. 10, represents this instrument as originally constructed by Coulomb.

Fig. 10.



A, B, C, D, is a glass cylinder 12 inches in diameter, and 12 inches in height. On the top of it is placed a glass plate 13 inches in diameter, which entirely covers the glass cylinder. This plate is pierced with two holes about 20 lines in diameter. One of these is in the middle at *f*, on which is

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elevated a glass tube 24 inches in height. This tube is cemented over the hole, *f*, by the common cement used for electrical machines. At its upper end at *h* is placed a micrometer of tortion seen in detail in figure 11. The upper part (fig. 11.) carries the button *b*, the index *i*, *o*, and the pincers of suspension, *q*. This piece enters into the hole G (fig. 11). The piece in which the hole G is situated, is formed of a circle, divided into 360 degrees, and of a copper tube Φ, which enters into the tube H, soldered to the inside of the upper extremity of the glass tube, *f*, *h*, (fig. 10). The pincers, *q*, (fig. 11), have very nearly the form of the point of a black lead pencil, and the two sides may be forced together by means of the ring *q*. These pincers support the extremity of a very fine silver wire. The other extremity of this wire is laid hold of at P (fig. 12), by the pincers of the cylinder, P, O, of copper or iron; the diameter of which does not exceed a line, and the extremity of which, P, is slit, forming a pair of pincers which are shut by the ring, Φ. The little cylinder swells out, and is pierced at C to pass through it the needle, *a*, *g*,

Fig. 11.

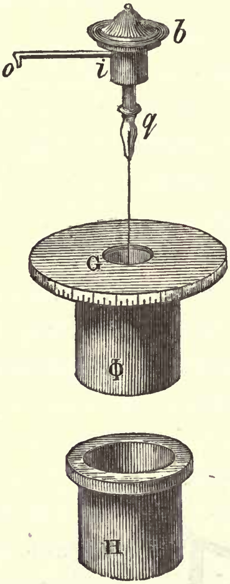
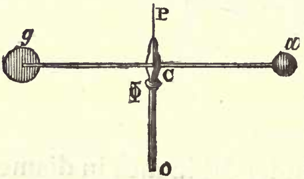


Fig. 12.



(figures 10 and 12.) The weight of this little cylinder must be sufficient to stretch the silver wire without breaking it. The needle, *a*, *g*, seen suspended horizontally in figure 10, about the middle of the large glass cylinder in which it is contained, is formed either of a silk thread covered with sealing wax, or of a straw equally covered with sealing wax, and terminated from *q* to *a*, a space of 18 lines, by a fine cylinder of gum lac. At the extremity, *a*, of this needle, is a little ball of elder pith from two to three lines in diameter. At *g* is a vertical plane of paper imbibed with turpentine, which serves to counterbalance the ball, *a*, and to retard its oscillations.

The cover, A, D, is pierced with another hole at *m*. By this second hole is introduced a small cylinder, *m*, Φ, *t*, the lower part of which, Φ, *t*, is of gum lac. At *t* is a ball equally of the pith of the elder. Round the glass vessel, at the height of the needle the circle, Z, Q, is described, and divided into 360 degrees. Or a slip of paper divided into 360 degrees may be pasted round the glass at the height of the needle.

To use this instrument the cover is so placed that the hole, *m*, corresponds with the 0° of the circle, Z, O, Q. Then the index of the micrometer, *o*, *i*, is fixed at the degree, 0°, or the first division of the micrometer. The whole micrometer is then turned in the tube, *f*, *h*, till, looking by the vertical wire which suspends the needle and the centre of the ball, the needle, *a*, *g*, is found to correspond with the first division of the circle, Z, O, Q. Then by the hole *m*, the other ball, *t*, suspended by the thread, *m*, Φ, *t*, is introduced, so that it touches the ball, *a*, and that looking by the centre of the thread of suspension, and the ball, *t*, we observe the first division, o, of the circle, Z, O, Q. The balance is now fit for all the different operations which it is calculated to perform.

CHAPTER III.

OF THE RATE AT WHICH CHARGED BODIES LOSE THEIR ELECTRICITY.

Chap. III.

For our knowledge of this subject, which constitutes an essential preliminary to all other electrical investigations, we are indebted to the sagacity of Coulomb.

Bodies lose electricity.

It is well known that an electrified conducting body, though insulated, soon loses its electricity. This is occasioned by two causes. 1. The air always contains conducting bodies, (the particles of vapour for example,) which gradually carry off the electricity by coming in contact with the electrified body imbibing its electricity, and then flying off. The rapidity of the action of these conducting bodies will vary with their number, which is exceedingly different according to the different states of the atmosphere. 2. No substance seems to be absolutely impervious to electricity, all seem to allow it to pass though with greater or less rapidity. Hence the electricity passes slowly off along the substance employed as an insulator. The rapidity with which it escapes in this direction, will depend upon the nature of the insulating body. Glass has such a tendency to condense vapour that it is a very bad insulator, except in a very dry atmosphere. It answers better if it be covered over with a varnish, which puts an end to the tendency of vapour to condense on its surface. Resinous bodies answer better as insulators than any other, because moisture has but little tendency to be deposited on them from the atmosphere. Hence this great source of the loss of electricity does not exist in them. Coulomb found that, when the charge was not great, a small cylinder of *sealing wax* or of *lac* about $\frac{1}{24}$ inch in diameter, and from $\frac{3}{4}$ to $\frac{5}{6}$ths of an inch in length, insulates completely a sphere of elder pith about half an inch in diameter. He found also that when the air was very dry, a very fine silk thread passed through boiling sealing wax insulates perfectly, if it be 5 or 6 inches long, and the pith ball be not highly charged with electricity.*

Effect of the air, in carrying off electricity.

1. Coulomb's first set of experiments were made to determine the rate at which a body loses its electricity by the contact of atmospherical air. He suspended to a very fine silk thread covered with sealing wax, and terminated by a small cylinder of gum lac 18 or 20 lines long, a small sphericle of elder pith similar to that, on the needle of his electric balance. He introduced it by the hole in the lid of the balance, and brought it in contact with the pith ball on the needle. An insulated pin with a large head, was charged with electricity and brought in contact with one of the balls, by which an equal quantity of electricity was imparted to

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* Mem. Paris, 1785, p. 615.

both. The two balls mutually repelled each other and separated to such a distance, that the force of torsion of the wire of the balance was equal to the repulsive force of the balls. Let us suppose the ball of the needle driven to the distance of 40°. By twisting the suspending wire, he brought it to a smaller distance, 20° for example. Let us suppose that this was produced by twisting the thread of suspension 140°. The moment of time when the ball corresponds exactly to 20° is marked. As the electricity is dissipated, the balls in a few minutes will begin to approach each other. To preserve them always at the same distance of 20°, the suspending thread is untwisted by means of the index 30 degrees, and in consequence of this untwisting, the balls separate from each other a little more than 20 degrees. The instant that the balls approach each other within 20° is marked, and the interval of time which has elapsed between the two observations, is accurately noted down. Suppose this interval to be 3 minutes. It is clear that the repulsive force at the time of the first observation was measured by $140 + 20 = 160$ degrees. Three minutes after it was measured by $110 + 20 = 130$ degrees. That is to say, it had diminished 30°, or at the rate of 10° per minute. As the mean force between the two observations is measured by 145 degrees, and as it diminishes 30° in 3′, or 10° per minute; the electric force of the two balls diminishes $\frac{10}{145}$ per minute. It was in this way that Coulomb formed the following table, representing observations made on the 28th of May, the 29th of

Chap. III. May, the 22d June, and 2d July. These four days were chosen because on them the heat was nearly the same, but there was a considerable difference in the degree of moisture existing in the atmosphere on each, as indicated by Saussure's hygrometer.

	Time of experiment.	Distance of the balls.	Torsion of the micrometer.	Time elapsed between two consecutive observations.	Electric force lost between two observations.	Mean force between two observations.	Ratio of electric force lost per min. to the mean force of the body.
1st Experiment, 28th May, Saussure's Hygrometer 75°. *Thermometer* 66°·875. *Barometer* 29.979 *inches.*							
1st Exper.	6*h* 32′ 30″	30°	120°	$5\frac{3}{4}$	20	140	$\frac{1}{40}$
2d . .	6 38 15	30	100	$6\frac{1}{4}$	20	120	$\frac{1}{38}$
3d . .	6 44 30	30	80	$8\frac{1}{2}$	20	100	$\frac{1}{42}$
4th . .	6 53 0	30	60	10	20	80	$\frac{1}{40}$
5th . .	7 3 0	30	40	14	20	60	$\frac{1}{42}$
6th . .	7 17 0	30	20				
2d Experiment, 29th May, Hygrometer 69°. *Thermometer* 66°.875. *Barometer* 30.008 *inches.*							
1st Exper.	5*h* 45′ 30″	30	130	$7\frac{1}{2}$	20	150	$\frac{1}{56}$
2d . .	5 53 0	30	110	$9\frac{1}{2}$	20	130	$\frac{1}{61}$
3d . .	6 2 30	30	90	$9\frac{3}{4}$	20	110	$\frac{1}{54}$
4th . .	6 12 15	30	70	$20\frac{3}{4}$	30	75	$\frac{1}{58}$
5th . .	6 33 30	30	40	18	20	60	$\frac{1}{54}$
6th . .	6 51 0	30	20				
3d Experiment, 22d June, Hygrometer 87°. *Thermometer* 67°.437. *Barometer* 29.75 *inches.*							
1st Exper.	11*h* 53′ 45″	20	80	3	20	90	$\frac{1}{13\frac{1}{2}}$
2d . .	11 56 45	20	60	3	20	70	$\frac{1}{11}$
3d . .	11 59 45	20	40	$5\frac{1}{4}$	20	50	$\frac{1}{13\frac{1}{2}}$
4th . .	12 5 0	20	20	$11\frac{1}{4}$	25	28	$\frac{1}{13\frac{1}{2}}$
5th . .	12 16 15	20	5				
4th Experiment, 2d July, Hygrometer 80°. *Thermometer* 67° 437. *Barometer* 29.929 *inches.*							
1st Exper.	7*h* 43′ 40″	20	80	$5\frac{1}{3}$	20	90	$\frac{1}{14}$
2d . .	7 49 0	20	60	$8\frac{1}{3}$	20	70	$\frac{1}{19}$
3d . .	7 57 20	20	40	12	20	50	$\frac{1}{30}$
4th . .	8 9 15	20	20	$8\frac{1}{4}$	10	35	$\frac{1}{19}$
5th . .	8 17 30	20	10				

From the last column of the preceding table, it is obvious, that when the experiments are made on the same day and in the same circumstances, the rate of electricity lost to the total force is a constant quantity; and that this rate varies in proportion as the hygrometer indicates a variation in the humidity of the air. Hence it follows, that for the same state of the air the loss of electricity is always proportional to the electric density.

It is obvious, that the ratios in the seventh column of the preceding table, refer to the loss of electricity sustained per minute by the two balls. And as the two balls were equal, the loss of electricity sustained per minute by one of the balls, will be only half the quantity given in this column. The mean loss of electricity sustained by the two balls on the 28th May, being $\frac{1}{40 \cdot 5}$, it is clear that the loss sustained by one of the balls will only be $\frac{1}{1}$.

Depends upon moisture.

We see that the loss of electricity from the contact of air, depends upon the moisture contained in it. For it is greatest when the hygrometer is highest, and least when the hygrometer is lowest. A more extensive set of experiments is still wanting to enable us to determine whether the loss be all owing to the moisture present; and if it be, at what rate it increases with the increase of moisture.

Coulomb determined experimentally, that the loss of electricity per minute is the same, whatever the nature of the electrified body may be, and whatever its shape, if it be not highly electrified. For in that case, bodies which have sharp edges or angles lose their electricity much faster than bodies which have no such angles or edges.

Loss of electricity from imperfect insulation.

2. To determine the loss of electricity from imperfect insulation, the natural method would appear to be to employ insulating substances, which possess the property of insulating very imperfectly. But when this is done, the electricity goes off so fast, that there is not time for measuring by means of the balance the rate at which the charge diminishes. This induced Coulomb to employ a pretty good insulator. The pith ball attached to the needle was perfectly insulated as in the first set of experiments; but the ball introduced through the hole in the balance, instead of being insulated as in the former experiments by a small cylinder of gum lac 15 or 18 lines in length, was supported by a thread of raw silk, such as it comes from the cocoon, and fifteen inches long.

The following table exhibits the results of two sets of experiments made precisely as in the former table, and on the same days, as the dates will show, namely, the 28th and 29th of May. The former table shows the loss of electricity on these days from the contact of air. Hence, by comparing the first table with the second, we can easily determine the quantity of electricity lost in a given time along the insulating substances employed as a support.

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	Time of experiment.			Distance of balls.	Torsion of Micrometer.	Time between two successive observations.	Electric force lost between two observations.	Mean force between two observations.	Rate of electric force lost per minute to that which remains in the body.
First Experiment, 28th May.									
1st Exper.	10°	0′	0″	30°	150	2½′	30	165	$\frac{1}{14}$
2d . .	10	2	30	30	120	5½	40	130	$\frac{1}{18}$
3d . .	10	8	0	30	80	5	20	100	$\frac{1}{25}$
4th . .	10	13	0	30	60	16½	40	70	$\frac{1}{29}$
5th . .	10	29	30	30	20	21	20	40	$\frac{1}{42}$
6th . .	10	50	30	30	0	16½	10	25	$\frac{1}{41}$
7th . .	11	7	0	30	—10				
Second Experiment, 29th May.									
1st Exper.	7°	34′	0″	30°	150	2′ 40″	20	170	$\frac{1}{23}$
2d . .	7	36	40	30	130	4 50	20	150	$\frac{1}{29}$
3d . .	7	41	30	30	110	6 50	20	130	$\frac{1}{44}$
4th . .	7	48	20	30	90	7 25	20	110	$\frac{1}{43}$
5th . .	7	55	45	30	70	11 45	20	90	$\frac{1}{53}$
6th . .	8	7	30	30	50	17 30	20	70	$\frac{1}{61}$
7th . .	8	25	0	30	30	17 30	15	50	$\frac{1}{58}$
8th . .	8	42	30	30	15	22 30	14	38	$\frac{1}{56}$
9th . .	9	5	0	30	3				

From the inspection of this table, it is obvious that the diminution of the electricity which is at first much more rapid than it would have been, had it been produced solely by the contact of the air, becomes at last, when the density is diminished to a certain point, precisely the same. It is certain from this that a silk thread 15 inches long, becomes a perfect insulator, when the reciprocal action of the two balls is measured, in the first experiment of the second table by a torsion of 40°, and below that. For then the loss of electricity is only $\frac{1}{41}$ per minute, the same as it had been observed on the same day, from the contact of air alone. In the second experiment of the 2d table, the silk thread became a perfect insulator when the repulsive action of the two balls was 70°, and below; for then the loss of electricity was only $\frac{1}{60}$ per minute, as it had been found the same day from the contact of the air alone.

CHAPTER IV.

OF THE LAW ACCORDING TO WHICH ELECTRICAL ATTRACTIONS, AND REPULSIONS VARY.

Chap. IV.

THIS law was first investigated by Coulomb in 1785. By a set of experiments with his electrical balance he showed, that two small balls charged with the same kind of electricity repel each other with a force varying inversely as the square of their distances from each other. He demonstrated also by experiments conducted with the same apparatus, that two balls charged with different kinds of electricity, attract each other with forces varying inversely as the squares of their distances from each other.*

Experiments on electrical repulsions.

1. The following are the experiments by which Coulomb demonstrated the law of electrical repulsion.

(1.) Having electrified the two balls of the balance by means of the head of a large pin, the index of the micrometer corresponding with 0, the ball *a* of the needle separated from the ball *t*, 36 degrees. (See fig. 10) p. 375.

(2.) Having twisted the suspending wire 126

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* Mem. Paris 1788, p. 572.

degrees, the two balls approached each other, and remained at 18 degrees distance.

(3.) The suspending wire being twisted 5·67 degrees, the two balls approached within $8\frac{1}{2}$ degrees of each other.

The two balls before being electrified touched each other, and the centre of the ball *a* attached to the needle, is not farther distant from the point where the torsion of the suspending wire is 0, than half the diameter of the two balls. The silver wire which suspended the needle was 28 inches long, and it was so fine that its weight was only $\frac{1}{16}$th of a grain. And Coulomb demonstrated that, to twist this wire 360 degrees the force requisite at the point *a*, with a lever *a n* of four inches long is only $\frac{1}{340}$th of a grain. Hence, as the forces of torsion are as the angles of torsion, the least repulsive force between the two balls, separates them sensibly from each other.

In the first experiment the index of the micrometer being at 0, the balls separated 36°. This occasioned a torsion of $36° = \frac{1}{3400}$th of a grain. In the second experiment the distance of the balls was 18°, but as the micrometer was turned 126°, it follows that at the distance of 18° the repulsive force was 144°. Thus when the distance is reduced to one half, the repulsive force is quadrupled.—In the third experiment, the suspending wire was twisted 567°, and the two balls were reduced to the distance of 8°·5 from each other. Here the total torsion was 576°, or four times as much as in the second experiment. And there is only half a degree

Varies as $\frac{1}{d^2}$

wanting to render the distance of the balls in the third experiment, one half of what it was in the second. Thus it follows from these three experiments, that the repulsive forces exercised upon each other, by two balls charged with the same kind of electricity, are inversely as the squares of the distances.

The electricity of the two balls diminishes a little during the continuance of the experiments. Coulomb ascertained that on the day on which the experiments were made, the electrified balls being in consequence of their repulsion 30° distant from each other, under an angle of torsion of 50°, approached each other 1° in three minutes. As he took only two minutes to make the three preceding experiments, the error resulting from the loss of electricity may be neglected. But when a still greater degree of precision is required, or when the day is moist, the best way of proceeding is, by a preliminary experiment to determine the loss of electricity per minute, and then to employ this observation to correct the result of the experiments made on that day.

The distance of the two balls when they are separated from each other by their mutual repulsion, is not accurately measured by the angle which they form, but by the chord of the arc which joins their centres. The lever at the extremity of which the action is exercised, is not measured by half the length of the needle, or by the radius; but by the cosine of half the angle formed by the distance of the two balls. These two quantities, one of which is smaller than the arc, and diminishes of course the distance measured by that arc, while the other diminishes the lever, compensate each other in some respects. When the arcs are small as in the preceding experiments, we may take them as giving the distance without sensible error. In other cases a rigorous calculation must be made.

Mode of determining the law of electrical attractions.

2. The balance did not answer so well for determining the law of the attractions of bodies charged with different kinds of electricity. Because the balls, when brought within a certain distance of each other, approach and come into contact, which prevented the attracting force at different distances from being observed. Coulomb therefore determined the attractive force by bringing a small ball charged with electricity, within a certain distance of a sphere excited by the opposite electricity. The attractive force was measured by the number of vibrations in a given time, in the same way as the force of gravitation in different parts of the earth's surface, is measured by the number of vibrations of a pendulum of a given length. The apparatus employed in these experiments was the following.

Fig. 13.

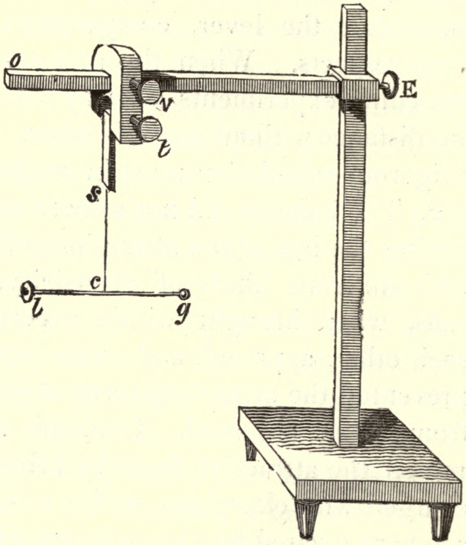
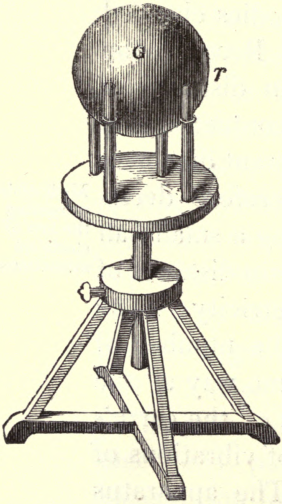


Fig. 14.



The needle, *l*, *g*, (fig. 13), of gum lac was suspended by the raw silk thread, *s*, *c*, seven or eight inches long. At the extremity, *l*, was fixed perpendicularly to the needle, a small circle 8 or 10 lines in diameter, but very light and formed from a sheet of gilt paper. The silk thread is fixed at *s*, to the lower extremity of a slip of wood dried in the oven, and covered with a coating of lac varnish. This piece of wood is fixed by screws at *t*, to a frame which moves along the rod, *o*, E, and which may be stopped at pleasure by means of the screw, *v*.

G, is a globe of copper or pasteboard, covered by tin-foil supported by four pillars of glass coated over with lac varnish, and having the upper extremities of each, (that the insulation may be more complete) terminating in a stick of sealing wax three or four inches long, these four pillars are fixed below in a piece of wood which is placed on a small table, which may be elevated and lowered at pleasure and fixed at the requisite height for the experiment. The rod *o E* may also, by means of the screw *E,* be fixed at any height required.

Things being thus prepared, the globe *G,* was so placed that its horizontal diameter *G, r,* corresponded with the centre of the disc *l,* distant from it some inches. A spark was given to the globe by means of a Leyden bottle, while at the same time a conducting body was presented to the disc *l.* The consequence of this was evidently that the disc became charged with an electricity different from that of the globe G. Hence when the conductor was withdrawn, the globe and the disc mutually attracted each other.

The globe G, was a foot in diameter, while that of the disc *l,* was seven lines; the needle *l g,* was 15 lines long. The silk thread *s c,* was a single fibre, such as is spun by the silk worm 8 lines long. When the frame (which slid along *o* E,) was at *o,* the disc *l,* touched the globe at *r,* and in proportion as the frame was drawn towards E, the disc was withdrawn from the centre by quantities given by the divisions 0, 3, 6, 9, 12 inches, (marked upon the leg *o,* E, but not given in the figure,) the globe was electrified positively, and of consequence the disc negatively. The result of the experiments was as follows.

(1) The disc l, being 3 inches from the surface of the globe or 9 inches from its centre gave 15 oscillations in $20''$.

(2) The disc being 18 inches from the centre of the globe, gave 15 oscillations in $40''$.

(3) The disc being 24 inches from the centre of the globe gave 15 oscillations in $60''$.

Let us see what consequences may be deduced from these three experiments.

It is well known that when all the points of a spherical surface act by an attracting or repelling force inversely as the square of the distance, on a point placed at some distance from that surface, the action is the same as if the whole spherical surface were concentrated in the centre of the sphere. As in the preceding experiments, the disc l, was only 7 lines in diameter, and was never less than 9 inches from the centre of the sphere, we may without sensible error suppose all the lines which go from the centre of the sphere to a point of the disc, parallel and equal. Consequently the whole action of the disc, may like that of the globe, be considered as united in its centre. So that in the small oscillations of the needle, the action which makes the needle oscillate will be a constant quantity for a given distance, and it will act in the direction of the line which joins the two centres. If we call φ the force, and T, the time of a certain number of oscillations, we will have T, proportional to $\frac{1}{\sqrt{\varphi}}$. But if *d*, is the distance G, *l*, from the centre of the globe to the centre of the disc, and if the attracting forces be inversely as the square of the distance or as $\frac{1}{d^2}$, it will follow that *T*, will be proportional to *d*, or to the distance. Hence in the preceding experiments, if the attractive force varies inversely as the square of the distance, the time of the same number of oscillations should be as the distance from the centre of the disc to the centre of the globe. Let us compare the experiments with the theory.

	Distances.	
1st Experiment .	9 inches .	20″
2d do. .	18 do. .	40″
3d do. .	24 do. .	60″

The distances are as . .	3,	6,	8
The times as . . .	20,	40,	60
By theory they should be .	20,	40,	54

The numbers in the first and second experiments correspond with the theory. When we compare the third with the first, the discordancy amounts to $\frac{1}{10}$th. But it is necessary to remark that these three experiments occupied about 4 minutes. Upon the day of the experiments, the loss of electricity per minute was $\frac{1}{40}$th. Now Coulomb demonstrated that when the density of the electricity is not very great, the electric action of two electrified bodies diminishes in a given time exactly as the electric density. Hence as the experiments lasted 4′, and the loss was $\frac{1}{40}$ per minute, the in-

Varies as $\frac{1}{d^2}$

tensity must have been diminished (independent of distance) about $\frac{1}{20}$th. Hence to have the corrected time of the 15 oscillations in the last experiments, we must say $\sqrt{10} : \sqrt{9} :: 60'' :$ quantity sought $= 57''$. This differs by only $\frac{1}{20}$th from the number 60″ found by experiment.

Thus it appears that the electrical attractions as well as repulsions vary inversely as the square of the distance.

Lord Stanhope's experiments on electric atmospheres.

The late Earl of Stanhope showed by a very ingenious set of experiments, which he published in the year 1779, that the density of electricity in the electrical atmosphere surrounding an excited body, diminishes inversely as the square of the distance from the charged body. As this in fact comes to the same thing as the demonstration of the rate at which electrical attractions and repulsions vary, made by Coulomb seven years later, or in the year 1785, electricity is really indebted to this nobleman for this important discovery. On this account it will be proper here to point out the steps by which his Lordship was led to the conclusion just stated.

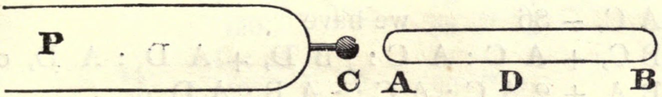
It had been already ascertained by Canton, that an electrified body communicates its own electricity to all the particles of air which come in contact with it. These particles are immediately repelled and their place supplied by a new set of aerial particles. The consequence of this must be, that the air immediately surrounding an excited body must be also excited, and must possess the same kind of electricity with the excited body.

It is obvious that the electrical density of this air must diminish according to its distance from the surface of the excited body. Hence according to Lord Stanhope, the reason why bodies charged with the same kind of electricity *repel* each other. They must move to those parts of their atmospheres where the electricity is *least.* Bodies excited with different kinds of electricity on the contrary approach each other, because each moves towards the extremity of its electrical atmosphere.

The reason why points part with electricity so easily, is that they penetrate into a much less dense electrical atmosphere. Hence the resistance to the escape of electricity is small.

When a body is placed within the electrical atmosphere of another, but beyond the striking distance, the end next the charged body, assumes an opposite electricity, while the end farthest remote assumes the same kind of electricity with the excited body, and there will be a neutral point somewhere between these two extremities.

Fig. 15.



Let P C, (fig. 15) be a charged body, and let A B, be a brass cylinder terminating in hemispheres, placed within the electrical atmosphere of P C, but so far from it that a spark cannot pass from C to A; and let A B, be insulated. Let P C, be charged with positive electricity. The extremity A, will be charged with negative electricity, while the extremity B, will be charged with positive electricity. Between A, and B, there will be a point, D, which is neutral, or which exhibits no phenomena either of vitreous or resinous electricity. Now, Lord Stanhope showed by numerous experiments, that the place of the point, D, varies with the distance of A from C. And that it is always a fourth point of an harmonical division of the line, B A C, the other three points C, A, B, being given.

It is well known to mathematicians, that three quantities are said to be in harmonical ratio, when the first is to the third, as the first minus the second is to the second minus the third. Suppose the line, C B, to be divided in the points, A, and D, in harmonical proportion, the quantities are as follows:

1st, C B,
2d, C D,
3d, C A,

Then we have $CB : CA :: CB - CD, (= BD) : CD - CA, (= AD)$

Let $AB, = 40$ inches,
$AC, = 36$ we have
$BC, + AC : AC :: BD, + AD : AD,$ or
$BA, + 2AC : AC :: AB : AD,$ or
$40 + 72 : 36 :: 40 : AD.$ Hence,
$AD = 12\frac{6}{7}$.

Thus it appears, that when the distance between C, and A, is 36 inches, and the cylinder, A B, 40 inches, the neutral point, D, is situated $12\frac{6}{7}$ inches from the extremity A, and $26\frac{1}{7}$ inches from the extremity, B.

The following table exhibits the result of the experiments made by Lord Stanhope, A B, being placed at different distances from the prime conductor, P C. The length of A B, was always 40 inches.

Length of AC, in inches.	Length of A D, in inches.	Proportion of A D, to A B.
4	$\frac{4 \times 40}{48} = 3\frac{1}{3}$	$\frac{1}{12}$ or $\frac{1}{12}$
8	$\frac{8 \times 40}{56} = 5\frac{5}{7}$	$\frac{2}{14}$ or $\frac{1}{7}$
12	$\frac{12 \times 40}{64} = 7\frac{1}{2}$	$\frac{3}{16}$ or $\frac{1}{5\frac{1}{2}}$
16	$\frac{16 \times 40}{72} = 8\frac{8}{9}$	$\frac{4}{18}$ or $\frac{1}{4}$
20	$\frac{20 \times 40}{80} = 10$	$\frac{5}{20}$ or $\frac{1}{4}$
24	$\frac{24 \times 40}{88} = 10\frac{10}{11}$	$\frac{6}{22}$ or $\frac{1}{3\frac{2}{3}}$
28	$\frac{28 \times 40}{96} = 11\frac{2}{3}$	$\frac{7}{24}$ or $\frac{1}{3\frac{3}{7}}$
32	$\frac{32 \times 40}{104} = 12\frac{4}{13}$	$\frac{8}{26}$ or $\frac{1}{3\frac{1}{4}}$
36	$\frac{36 \times 40}{112} = 12\frac{6}{7}$	$\frac{9}{28}$ or $\frac{1}{3\frac{1}{9}}$
40	$\frac{40 \times 40}{120} = 13\frac{1}{3}$	$\frac{10}{30}$ or $\frac{1}{3}$
44	$\frac{44 \times 40}{128} = 13\frac{3}{4}$	$\frac{11}{32}$ or $\frac{1}{2\frac{10}{11}}$
48	$\frac{48 \times 40}{136} = 14\frac{2}{17}$	$\frac{12}{34}$ or $\frac{1}{2\frac{5}{6}}$

These results show that the point D, is always such that the lines C B, C D, C A, are in harmonic proportion, the points C, A, and B, being given. To obtain them it is necessary that the air should be dry. Should there be any moisture in the

Chap. IV. atmosphere to carry off the electricity from A B, the situation of D, will no longer be such as to divide the line C B, in the harmonic proportion.

It is obvious enough that the two limits of the point D, are the *middle* of the line A B, and the *nearest* extremity A. It will be in the middle of the line A B, when the body A B, is at an infinite distance from P C. It will be in A, when the body A B, touches P C.

It is clear that the position of D, must be always such, that the plane passing through it, will divide the electricity of the body A B, into two equal parts if the density of the electricity of the electrical atmosphere diminished inversely as the simple distance from the charged body P C. Then the whole of the electricity of this atmosphere round A B, would be represented by the hyperbolical area (see Frontispiece) A N Q B, contained between the equilateral hyperbola N R Q, the asymptote A B, from the centre C, and the ordinates A N, and B Q. And if the electrical atmosphere had its electricity diminished according to that law, the point D, would be found by taking the ordinate D R, a mean proportional between A N, and B Q. But if the electricity of the electric atmosphere diminishes inversely as the squares of the distance from P C, then this electricity round A B, will be represented by the hyperbolical solid *n* N Q *q*, formed by the revolutions of the hyperbolical space A N Z W, round the asymptote C W. This space is bounded by the portion of the equilateral hyperbola intercepted between the ordinates A N, B Q.

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It is a known property of this hyperbolical solid *n* N, Z *z*, that the area of any perpendicular section represented by *n* A N, will be to the area of any other perpendicular section, *r* D R, *q* B Q, inversely as the squares of their respective distances, (A C, C D, C B,) from the centre C of the hyperbola. Hence the quantity of electricity superinduced upon the curved surface of the insulated cylindrical body A B, at the distances C A, C D, C B, from the charged body P C, will be exactly proportional to the areas of the circular sections represented by *n* A N, *r* D R, *q* B Q, &c. Now to find the position of the point D, we have to find the point in the asymtote C W, through which a plane *r* D R, being passed, will divide the hyperbolic solid *n* N Q *q*, into two equal parts. That plane will obviously determine the position of D.

Now to divide the hyperbolical solid into two equal halves, we have only to take D R, an arithmetical mean between the two ordinates, A N, B Q; or the point, D, is such that the ordinate, D R, is equal to half the sum of the two ordinates, A N, B Q. Or, which is the same thing, (from the nature of the hyperbola), we must take the hyperbolical abscisse, C D, a mean harmonical proportional between the two abscisses, C B, and C A. That is, we must take the point, D, so that the whole line, C B, is to its extreme part, C A, as the other extreme part, B D, is to the middle part, D A. For it is a well known proposition, that the reciprocals of quantities which are in arithmetical progression, will be in harmonical progression.

That the plane, $R\,D\,r$, will divide the solid, $n\,N\,Q\,q$, into two equal parts may be demonstrated in the following manner.

Let p denote the ratio of the circumference of a circle to its diameter, or (which is the same thing) the ratio of the area of a circle, to the square of its radius.

Then the cylinder, $v\,V\,N\,n$, (whose height is $A\,C$, and the radius of whose base is $N\,A$,) will evidently be equal to $p \times N\,A \times N\,A \times A\,C$.

The cylinder $x\,X\,R\,r$ will be equal to $p \times R\,D \times R\,D \times C\,D$.

And the cylinder $y\,Y\,Q\,q = p \times Q\,B \times Q\,B \times C\,B$.

Now from the nature of the hyperbola, every rectangle under any abscisse, and its correspondent ordinate will be a given quantity, which we shall call $\underline{c\,c}$. Therefore,

$$N\,A \times A\,C = R\,D \times D\,C = Q\,B \times B\,C = \underline{c\,c}.$$

And the cylinder $v\,V\,N\,n = p \times c\,c \times N\,A$.

$$x\,X\,R\,r = p \times c\,c \times R\,D.$$

$$y\,Y\,Q\,q = p \times c\,c \times Q\,B.$$

Therefore, (dividing by the common quantity, $p \times c\,c$) the cylinders $v\,V\,N\,n$, $x\,X\,R\,r$, $y\,Y\,Q\,q$, are exactly in the same ratio to each other, as the ordinates $N\,A$, $R\,D$, and $Q\,B$, respectively.

Now $R\,D$ is an arithmetical mean between $A\,N$ and $B\,Q$, therefore the cylinder $x\,X\,R\,r$, is an arithmetical mean between the cylinders $v\,V\,N\,n$ and $y\,Y\,Q\,q$. That is to say, $v\,V\,N\,n - x\,X\,R\,r = x\,X\,R\,r - y\,Y\,Q\,q$.

But it has been demonstrated that every infinite acute hyperbolic solid, is equal to the cylinder whose base is the same with that of the solid, and whose height is equal to the distance between that base and the centre of the hyperbola. Therefore,
$v\,V\,N\,n =$ infinite acute hyperbolic solid $n\,N\,Z\,z$.
$x\,X\,R\,r = r\,R\,Z\,z$.
$y\,Y\,Q\,q = q\,Q\,Z\,z$.

Consequently,
$v\,V\,N\,n - x\,X\,R\,r = n\,N\,Z\,z - r\,R\,Z\,z = n\,N\,R\,r$ and $x\,X\,R\,r - y\,Y\,Q\,q = r\,R\,Z\,z - q\,Q\,Z\,z = r\,R\,Q\,q$.

Hence it follows that $n\,N\,R\,r = r\,R\,Q\,q$.

Thus it follows evidently from the experiments of Lord Stanhope, that the density of the electricity of electrical atmospheres diminishes inversely as the square of the distance from the excited body.

CHAPTER V.

OF THE DISTRIBUTION OF ELECTRICITY ON BODIES.

Chap. V.

Coulomb demonstrated that it is a consequence of the law, that the particles of electricity repel each other inversely as the square of their distance, that the electricity when accumulated in a conducting body is distributed totally on the surface of the body, and that none of it exists in the interior of the body.* He showed likewise the truth of this law experimentally.

Coulomb's electrometer.

The electrometer which he employed for that purpose was exceedingly delicate. It was made in the following manner. A thread of gum lac was drawn at the flame of a candle about the thickness of a stout hair, and from ten to twelve lines in length. One of its extremities was attached to the thick end of a small pin deprived of the head. This pin was suspended by a thread of raw silk, such as it is spun by the silk worm. To the other extremity of the gum lac thread was fixed a circle of tinsel, about two lines in diameter. The whole of this electrometer wa suspended in a glass cylinder to prevent it from being affected by currents of air. Its sensibility was such that a force amounting only

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* Memoires Paris 1786, p. 75.

to the 60 thousandth part of a grain, made it move more than 90 degrees.

To this electrometer a small degree of the same electricity was given as that of the body to be tried. The substance subjected to experiment was a cylinder of solid wood, four inches in diameter, pierced by several holes of four lines in diameter, and four lines in depth. This cylinder was placed upon a nonconductor, and several electric sparks were given to it either by means of a Leyden phial, or the plate of an electrophorus. At the extremity of a small cylinder of gum lac of a line in diameter, was insulated a small circle of gilt paper of a line and a half in diameter.

Electricity confined to the surface of bodies.

This little circle of gilt paper was made to touch the surface of the excited wooden cylinder. It was then presented to the electrometer, previously excited slightly with the same kind of electricity as the wooden cylinder. The electrometer was forcibly repelled. It was then introduced into one of the holes in the wooden cylinder and brought in contact with the bottom, taking care not to touch the sides of the hole, nor the surface of the cylinder of wood. When presented to the electrometer, it either gave no signs of possessing any electricity, or gave feeble symptoms of containing an electricity different from that of the cylinder of wood. The reason of this opposite electricity, which was only occasionally observed, was that when the little cylinder of gum lac is introduced into the hole, the electrical action of the surface of the excited body communicates to the gum lac, a

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small quantity of an electricity different from its own, because this small thread of gum lac is insulated within the sphere of its activity. The proof that this explanation is satisfactory, is, that if we touch the gilt paper circle we do not destroy the small quantity of electricity which it possesses, showing that this electricity must be lodged in the lac, and not in the gilt paper.*

We may take a hollow sphere of metal, having in it a hole of a few lines in diameter, place it upon a nonconductor, and communicate electricity to it by means of an electrical machine. When the sphere is thus excited, we may introduce into its interior a small metallic globule attached to the extremity of a gum lac thread. Upon withdrawing this metallic globule, and presenting it to an electrometer we shall find that it has no electricity whatever, or that it has (occasionally for the reason already explained) a feeble electricity of the opposite nature from that of the metallic sphere.

Thus it appears that the quantity of electricity accumulated in a body, depends not upon the quantity of matter in the body, but upon the extent of its surface; and that hollow bodies, however thin, may be charged with just as much electricity as bodies of the same surface, and quite filled with matter. Advantage is taken of this important fact in constructing the prime conductors of electrical machines, which are always made hollow and thin for the sake of lightness.

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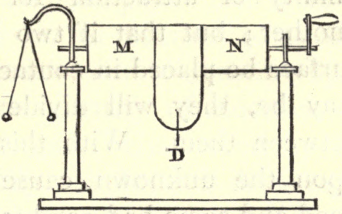
* Mem. Paris 1786, p. 72.

Experimental proof of this.

There is a curious enough experiment, which serves to show that electricity is confined to the surface of bodies, and which therefore may be mentioned here. Let M N, be an insulated conductor, movable on an axis. Let a metallic ribbon be rolled on this cylinder to the extremity of which is attached a silk thread D. To this conductor let a sensible electrometer be attached, composed of two metal or linen threads, to the extremities of which two pith ball sphericles are attached. When electricity is communicated to this conductor, the pith balls immediately repel each other. Unroll the ribbon by pulling the silk thread D; in proportion as the ribbon is unrolled, the pith balls approach, and if the weather be dry, all signs of electricity disappear when the ribbon is unrolled; and the balls begin anew to separate, when the ribbon is again rolled on, and diverge as far as ever when matters are restored to their original state.



Electricity then is all deposited on the surface of bodies, and the only obstacle to its leaving that surface and being instantly dissipated, is the pressure of the atmosphere.*

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* Strictly speaking there is another obstacle. It is the difficulty with which electricity passes along certain bodies. This seems to be owing to something like an adhesion between the surface of the body and the fluid. This view of the subject is strengthened by the well known fact, that the conducting power diminishes with

Electricity proportional to the surfaces.

Coulomb proved by very simple but convincing experiments, that electricity deposites itself on bodies according to their surface, that it has no affinity or attraction for one body more than another; but that if two bodies having the same surface be placed in contact, whatever their nature may be, they will divide the electricity equally between them. With this distinction, depending upon the unknown cause why some bodies are good and some bad conductors, that if a good conductor touch an excited body of the same surface, it will instantly take possession of one half of its surplus electricity, whereas a bad conductor requires to be left a little while in contact with the excited body, to be able to assume its proper quantity.*

When the excited body is a sphere, the thickness of the electric coating ought from reasons of symetry to be every where equal. And this conclusion is conformable to experiment.

When additional quantities of electricity are accumulated upon the surface of a sphere, it comes to the same thing as far as the experimental investigation is concerned, whether the thickness of the coating of electricity be increased, or whether, the thickness remaining the same, the density of the electricity augments on each point of the surface. The first supposition is more conformable to our notions of a fluid than the second.

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the intensity of the electricity. This adhesion must to a certain extent be similar to attraction, and it is probably the great cause of the attractions and repulsions between excited bodies.

* Mem. Paris 1786, p. 69.

Chap. V.

Coulomb made a set of experiments, to determine the way in which electricity is distributed on the surface of bodies of various shapes.* His method was to apply a small gilt paper disc insulated on a thread of gum lac, to different parts of the excited body in succession, and to measure by means of his balance the quantity of electricity with which it became charged. He proved by a set of experiments equally simple and decisive, that the quantity of electricity with which the disc became charged, was sensibly the same with that of the point of the surface of the excited body with which it had been placed in contact, and that when the electricity of an excited body, is either augmented or diminished the variations on every part of the surface is proportional to the quantity of electricity already present. Thus if on one part of a surface the electricity be twice as great as upon another part, if we double the charge or reduce the charge to one half, the first part of the surface will receive twice as much, or part with twice as much as the other part.

Coulomb showed also, that the quantity of electricity with which the gilt paper disc becomes charged, is sensibly the same with that of the body which it is made to touch, and that when two bodies are brought into contact, the electricity contained

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* The best account of these experiments which I have seen, is given by Biot in his Traité de Physique, ii. 263. Biot seems to have derived his information from Coulomb's manuscripts. For I am not aware that they were ever published by their author, owing doubtless to the state of France at the beginning of the Revolution.

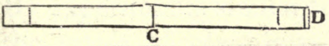
in them is sensibly divided in proportion to the surface of each. He insulated a globe of 8 inches diameter, and charged it with positive electricity, as he did also the movable ball of the balance. He then touched this globe with another whose diameter was only 1 inch; and applied this little globe to his balance. A torsion of 144° was necessary to balance the repulsive force at a fixed distance. He next touched the surface of the large globe in a tangent direction with a circular plane, isolated, and having a diameter of 16 inches. Having removed this plane, he repeated the experiment with the small globe. A torsion of 47° was now sufficient to balance the repulsive force.

To understand the consequences to be drawn from this experiment, we must recollect that the surface of the great globe was equal to that of 4 great circles, each 8 inches in diameter: each face of the circular plane of 16 inches diameter, was equal to 4 circles of 8 inches in diameter, and as the plane has two faces, its surface was exactly double that of the sphere. The electricity in the great globe before it was touched by the plane may be represented by 144, and after it was touched by it by 47. Hence the electricity carried off by the plane, was obviously 144—47=97. So that the plane carried off 97, while the globe retained 47. But 97 is very nearly double 47. Thus we see that the electricity was divided between these two bodies in proportion to their surfaces.

In determining the quantity of electricity on different parts of the surface of a body, we must take care that the gum lac to which the gilt paper disc is attached, be not a conductor. Because in that case, electricity would accumulate in it, and vitiate all the results. In general the lac, which has the darkest colour is the best for the purpose. But the threads ought to be examined by bringing them near a charged conductor, and then presenting them to an electrometer. All those which exhibit any symptoms of electricity, ought to be rejected.

Distribution on a steel plate.

Coulomb determined the distribution of electricity upon a steel plate, 11 inches long, 1 inch broad, and half a line thick. It was insulated and charged with electricity. To be able to touch it over all its breadth, the gilt paper disc was made an inch long, and three lines broad. He first applied the disc to the centre of the plate, at C, then at 1 inch from its extremity, and he obtained the following results.



	Torsions observed.	Mean torsions in the middle.	Ditto 1 inch from end.	Ratio.
Touched in the middle,	370°			
At 1 inch from end,	440	360°	440°	1·22
In the middle,	350	350	417·5	1·20
At 1 inch from end,	395	335	395	1·18
In the middle,	320		Mean	1·10

That is to say, if the electricity at the centre of the plate be represented by 1, that at an inch from the end will be represented by 1·2. So that they are nearly equal.

The experiment was repeated with this difference, that the gilt paper disc was applied to the very

Chap. V. end of the plate, but still entirely on the surface of it. The results were as follows.

	Torsions observed	Mean torsions in the middle.	Ditto at the end.	Ratio.
Touched at the end,	400°			
In the middle,	195	195°	395°	2·02
At the end,	390	190	390	2·05
In the middle,	185	185	370	2·00
At the end,	350		Mean	2·02

That is to say, if the quantity of electricity in the middle of the plate be 1, that at the extremity will be 2·02 or very nearly double.

The experiment was repeated a third time, but the disc instead of being applied at the extremity of the plate, was applied to the edge D, so as to be equally in contact with both faces. The result was as follows.

	Torsions observed.	Mean torsions in middle.	Mean torsions on the edge.	Ratio.
Touched in the middle,	305			
On the edge,	1175	295°	1175°	3·98
In the middle,	285	285	1156	4·05
On the edge,	1137		Mean	4·01

That is to say, if the electricity in the middle be 1, that at the edge is 4·01, or four times as great. So that it contains the accumulated electricity of the extremities of each side of the plate.

These experiments were repeated with a plate of steel, twice as long as the preceding, but agreeing with it in all its other dimensions. The result was exactly the same.

It is obvious from these experiments, that when the gilt paper disc is applied to one of the sides of the steel plate, it only imbibes the electricity of that side, and not of the other. We see too that the electricity is almost uniformly diffused upon the surface of the plate. Since the increase does not become sensible till we approach within an inch of the end; and that the length or shortness of the plate has no effect upon this distribution.

Distribution on a cylinder.

The rapid augmentation of electricity towards the extremities of plates, is not peculiar to them. It takes place equally in all long prismatic or cylindrical bodies. And the smaller the diameter of these bodies is in proportion to their length, the more rapid is their augmentation. Coulomb insulated a cylinder of 2 inches in diameter, and 30 inches in length, terminated by two hemispheres. By experiments similar to those just stated, he found the ratios of electricity on different parts of this cylinder as follows.

At the centre . .	1
At 2 inches from end .	1·25
At 1 inch from end .	1·80
At the centre of the end	2·30

When the cylinder becomes smaller as we approach the extremity the increase of electricity becomes still more rapid. And when the extremity becomes elongated like the point of a cone, the accumulation at that extremity becomes so great, that the resistance of the air is not sufficient to retain it, and it escapes producing sparks of light which are visible in the dark.

In these bodies the uniform distribution of elec-

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tricity extends to within a very small distance of the extremity. It is clear that such bodies ought to lose their charge of electricity with great rapidity, as is known from experiment to be the case.

Distribution on circular plates.

Coulomb made analogous experiments on circular plates. The following table shows the result of one of these trials.

Distance from the edge.	Electricity observed.
5 inches (centre of plate)	1
4	1·001
3	1·005
2	1·17
1	1.52
0·5	2.07
0	2·90

If we represent the distances from the edge by x, and the electrical intensities by y, Biot has shown that the relations of these to each other may be represented by the following empirical formula.

$$y = 1 + A\,(\mu^{x} - \mu^{2r-x})$$

r being the radius of the plate, and A and μ two constants to be determined from observation. We obtain A by making $x = o$, and substituting for y, the value found, as in the table. We have

$$2{\cdot}9 = 1 + A\,(1 - \mu^{2r})$$

If we take 1 inch for the unity of length, $2\,r$ will be equal to 10. Now as μ is very nearly 0·3, its tenth power is so small a quantity that it may be neglected. We have $2{\cdot}9 = 1 + A$, and $A = 1{\cdot}9$.

To determine μ let us make $x = 1$, and substitute for y its value 1·52. In this case $\mu^{2r-x} = \mu^{y}$ a quantity too small to be sensible. We have, therefore,

$$1{\cdot}52 = 1 + 1{\cdot}9\,\mu \text{ or}$$

$$\mu = \frac{0{\cdot}52}{1{\cdot}9} = 0{\cdot}27.$$

But the calculation agrees better with experiment, if we make $\mu = 0{\cdot}3$.

Why electricity is dissipated by points.

This subject attracted the attention of M. Poisson, who applied to it all the resources of the most refined calculus, and determined the thickness of the coating of electricity upon bodies of different forms from the hypothesis that positive and negative electricity are two fluids, the particles of each of which repel each other with forces varying inversely as the square of the distance; while the vitreous electricity attracts the resinous with forces varying according to the same law.* He showed that the exterior surface of the electrical coating coincides with that of the body, and that as the coating is very thin, the interior surface is but little distant from it. In a sphere both the exterior and interior surfaces are spherical, and the centre of these surfaces is the same with that of the centre of the body.

In an ellipsoid the thickness of the coating at the extremities of the two axes is proportional to the relative lengths of the axes.

The electric coating acts by attraction and repulsion on the particles of electricity, situated beyond

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* See Mem. de l'Institut, 1811.

the surface of the body, or on the surface of the body itself. It attracts them if of a different nature, and repels them if of the same nature. This repulsion is exercised against the particles of the air, which repels the electricity in its turn, and keeps it in its place. The repulsion exercised by the electrical coating is as the square of its thickness. It is therefore variable at different points of the body, unless that body be spherical. And as it increases as the square of the thickness, and as this thickness is proportional to the excess of the diameter of the body in one direction above that in another, it may easily overcome the pressure of the atmosphere, and thus enable the electricity to make its escape. Hence the reason of the effect of points in discharging bodies charged with electricity. M. Poisson has shown that the repulsive force of the electricity at the summit of a cone, if it could accumulate on it, would be infinite.

Mutual action of excited bodies on each other.

When several excited bodies are placed in the neighbourhood of each other they produce certain effects upon the state of the electricity in each. These effects have been also subjected to calculation by M. Poisson. His analytical investigations were founded upon the following general principle which he first announced.

"When several excited bodies are brought into the neighbourhood of each other, and arrive at a permanent electrical state, it is necessary that the resultants of the actions of the electrical coatings which cover them upon any point in the interior of any one of these bodies, should be nothing. For if this was not the case the combined electricity in the point under consideration would be decomposed, and the electrical state would change, contrary to the hypothesis of its permanence."

This principle furnishes at once as many equations as there are bodies under consideration, and as the problem presents of unknown quantities. But the solution of these equations frequently surpasses the present state of analytical investigation. But M. Poisson has succeeded in overcoming all the analytical difficulties in the case of two spheres, placed in contact, or in the neighbourhood of each other, and charged each with any quantity of electricity.

When the two spheres are equal, and in contact, and charged with electricity, whether positive or negative, calculation shows that there is no free electricity at the point of contact of the two spheres. From that point the coating of electricity augments in thickness as its distance increases from the point of contact, and it always attains its maximum at 180° of distance, or at the other end of the diameter that passes through the point of contact of the two spheres. The rate at which the thickness of the coating increases depends upon the relative lengths of the diameters of the two spheres.

Experiments on this subject were made by Coulomb in the way already described in the former part of this chapter. These experiments were compared by Poisson with the results of his calculations, and found to accord with them very closely. The following table shows the relative thickness of the coating of electricity upon two equal globes at different distances from the point of contact as determined experimentally by Coulomb, and by calculation by Poisson.

Distribution of electricity on two equal globes.

	Thickness of coating of electricity as determined by Coulomb.	 Poisson.
Point of contact	0	0
20°	Insensible	Insensible
30	0·2083	0·1707
60	0·7994	0·7452
90	1·0000	1·0000
180	1·0576	1·1400

The following table exhibits the results of experiment and calculation with two unequal spheres, the diameters of which were to each other as 1 to 2. The observations apply to the smaller sphere.

On two unequal globes.

Position of the points compared.	Proportional thickness of coating. Coulomb.	 Poisson.
30°	Insensible	Insensible
60	0·5882	0·5563
90	1·0000	1·0000
180	1·3333	1·3535

We see that in this case the thickness of the coating in the smaller sphere (though insensible to 30°) varies more than when the two spheres are equal. For at 60° and 180° the thicknesses are to each other as 0·5882 to 1·3333, or as 1 to $2\frac{1}{4}$; while in the other case they are to each other as 0·7994 to 1·0576, or as 1 to $1\frac{1}{3}$. By a sort of compensation the greater of the two spheres experiences less variation than in the case of equality. For the thickness of the coating insensible at 6° or 7° increases so rapidly that at 30° it is $\frac{5}{6}$ths of what it is at 90°, and from 90° to 180° the variation is insensible.

Coulomb compared the absolute thickness of the electrical coating on the two globes at 90° from the point of contact. In the smaller globe he found it 1·25, in the larger 1. Poisson obtained by calculation 1·238 and 1.

When two globes of different sizes (one charged with electricity) are brought in contact and then separated, the smaller one of the globes is, compared to the other, the smaller is the quantity of electricity which it carries off. But this proportion is very far from being that of the surfaces. For then the coatings of electricity would be of equal thickness in both, whereas it is always thickest in the smaller globe, and the thickness increases as the size of the globe diminishes. But from the slowness of the increase in Coulomb's experiments, there is reason to believe that it does not go on indefinitely.

When the globes after contact are separated.

When the two unequal spheres after being separated from each other, are placed at a certain distance from each other, a very curious phenomenon is observable in the smaller sphere. To explain it intelligibly, let us suppose the electricity of the two spheres to be positive. We have seen that this electricity is null at the point of contact. But the instant we separate the two spheres, the elec-

Chap. V. tricity at the point of contact of the smaller sphere becomes negative. This state diminishes in proportion as the smaller globe is carried to a greater distance from the large globe, and at a certain distance depending on the ratio between the diameters of the two globes, it totally disappears. Then the point of contact of the smaller globe becomes in the same state as when actually in contact. From this position, if we increase the distance, the electricity remains of the same nature over the whole extent of the globe, and of the same kind as during the contact. In the larger globe, the electricity continues always the same as during the contact. In an experiment made by Coulomb, the diameter of the larger globe was 11 inches, and that of the smaller eight inches. While the distance between them was less than 1 inch, the point of contact of the smaller globe gave signs of an electricity opposite to that of the large globe. When the distance became 1 inch, the electricity of the point of contact of the smaller globe became null, as when in contact, and at all greater distances, it remained of the same nature as that of the other sphere. When the diameter of the smaller globe was only 4 inches, (that of the greater remaining the same) the opposite electricities were sensible to the distance of two inches. When the diameter of the smaller globe was only 2 inches or less, the opposite electricities continued to the distance of 2 inches 5 lines, but not farther.

On the contrary when the difference between the size of the two globes diminishes, the distance at which the two electricities manifest themselves also diminishes, and it becomes null when the two globes become equal.

These experiments succeed only when the air is very dry. If it be moist, the electricity of the larger globe escaping neutralizes that in the smaller globe, and renders the phenomena indistinct or quite insensible.

Effect when two excited globes approach.

When two excited spheres are made gradually to approach without having ever been in contact, the thickness of the electrical coat at the points of the two globes immediately opposite to each other, becomes greater and greater, and increases indefinitely as the distance between them diminishes. The pressure on the atmosphere increasing as the square of this thickness, must at last overcome the resistance of the air, and the fluid passing from the one globe to the other, occasions a spark which appears before the two globes come into contact. The electricity is different, though of equal thickness in each globe. If the one is excited with positive, and the other with negative electricity, it is positive in the former and negative in the latter. When both contain the same electricity, (positive for example) there takes place a decomposition of the combined electricity in the globe which contains less of the positive electricity than it would have done in case of contact. The negative electricity resulting from this decomposition accumulates at the point opposite the other globe, while this latter globe remains positive throughout its whole extent.

M. Poisson has also subjected to calculation the state of the most remote points of two excited globes, when placed at a certain distance from each other. As the two globes approach, the thickness of the coating of electricity in these points, approaches more and more to what it would be at the instant of contact. But as it arrives at this limit very slowly, the consequence is, that at very small distances the state of these points differs considerably from what it would be, if actual contact or the emission of a spark were to take place.

CHAPTER VI.

THEORY OF THE MOTIONS PRODUCED BY ELECTRICAL ATTRACTIONS AND REPULSIONS.

Chap. VI.

Franklin admitted only one fluid.

THE first theory of electricity which connected the phenomena in a satisfactory manner and enabled electricians to foretell the results, was that of Dr. Franklin. According to him, electricity is produced by a *fluid*, the particles of which *repel* each other with a force inversely as the square of the distance; while they are *attracted* by matter with an equal, and equally varying force. Æpinus was the first who applied mathematical calculation to the development of this theory, and he showed that it was incapable of accounting for the electrical attractions and repulsions, unless we admit also that matter repels matter, with a force varying inversely as the square of the distance. These three data being admitted, namely,

Data of Æpinus.

f repels *f*, with a force varying as $\frac{1}{d^2}$

f attracts *m*, . . . $\frac{1}{d^2}$

m repels *m*, . . . $\frac{1}{d^2}$ *

all the phenomena of electricity, admit of a precise and satisfactory explanation.

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* Here *f* signifies the electric fluid, *m* signifies matter.

Not admissible.

But the third of these data, namely, that matter *repels* matter, is not only unsupported by observation, but quite inconsistent with it. For in the motions of the heavenly bodies no such repulsions have ever been observed, though if they had existed, they could not have escaped notice during the rigid mathematical investigations to which these motions have been subjected. I do not see therefore how it can be admitted. And yet without it, the Franklinian hypothesis is quite incapable of explaining the phenomena of electricity.

The second datum, that the electric fluid is attracted by matter with a force inversely as the square of the distance, is also inconsistent with the electrical phenomena. For the quantity of electricity accumulated in bodies is always proportional to the extent of their surface, and not to the quantity of matter in them, as would be the case if any attraction or affinity existed between them. All substances, whatever their nature may be, are capable of receiving the same quantity of electricity, provided the extent of their surfaces be equal. And, finally, it has been shown that electricity accumulates only on the surfaces of bodies, and that nothing but the pressure of the ambient atmosphere prevents it from making its escape.

It is indeed true, that there are certain bodies, called *non-conductors* or *electrics*, through which electricity is incapable of passing, or of passing only with more or less difficulty. What the force is which attaches it to these bodies, and prevents it from leaving them or passing through them, we do not know. But that it is not an affinity or attraction to the atoms of these bodies is evident from this, that it accumulates only on their surface, and that the quantity of electricity does not depend upon the quantity of matter, or the kind of matter, but entirely upon the quantity of surface.

Two fluids must be admitted.

But if matter does not repel matter, and if there does not exist any attraction or affinity between electricity and matter, then the Franklinian theory of negative and positive electricity cannot be correct. The original theory of Du Fay, modified a little chiefly in consequence of the facts ascertained by Coulomb, involves no assumptions inconsistent with the phenomena of nature, and has been employed by Poisson so happily, and has been found to apply so accurately to all the different cases, that the probability of its accordance with truth is very great. At any rate it enables us to reduce all the electrical facts into a simple and luminous system, and to foretell the result of any combination of electrical actions. Even, therefore, if we suppose it a mere mathematical hypothesis, its importance and utility to all who wish to understand the principles of this most important science, must be admitted to be very great.

According to this theory there exist two distinct electrical fluids, namely, the *vitreous* and the *resinous*, or *positive* and *negative*.* The particles of posi-

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* For the reasons already specified in pp. 347, 348, I employ the terms positive and negative, in preference to vitreous and resinous. But I would be understood to use them merely as the name of each fluid, and not in the sense of Franklin.

Theory of two fluids.

tive electricity repel each other with a force varying inversely as the square of the distance. This is the case also with the particles of negative electricity. The particles of positive electricity attract those of negative electricity, with a force varying inversely as the square of the distance. In consequence of this attraction, the two electricities are capable of uniting, and when thus combined, the bodies in which they exist exhibit no symptoms of electricity. It is only when decomposed that the phenomena of electricity become evident, and they are produced by the mutual attractions and repulsions of the two fluids. Friction has the property of decomposing the combined electrical fluid, the positive electricity accumulating in one of the bodies rubbed while the negative electricity accumulates in the other. Hence the reason why when two bodies are rubbed against each other and separated, they are always in two opposite states of electricity. If the bodies rubbed against each other be non-conductors, this difference of state remains, in consequence of the difficulty which the fluids experience in moving through them; but if they be conductors, the two fluids again combine the instant the friction is at an end; so that unless they are insulated and instantly separated from each other, as soon as we cease rubbing them, the difference of their states cannot be perceived.

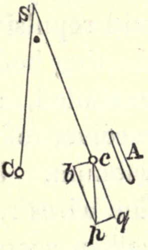
Finally, no attraction or affinity exists between the electric fluids and matter.

Such is a short outline of the theory of electricity, which, chiefly in consequence of the galvanic discoveries, is gradually superseding the Franklinian hypothesis, and will probably very speedily be universally adopted by electricians. But if there be no affinity or attraction between electricity and matter, it may appear, at first sight, difficult to account for the fact that when bodies are excited, that is, contain a superabundance of electricity, they *attract* or *repel* each other with forces varying inversely as the square of the distance; bodies having the same kind of electricity repelling, and those having different kinds attracting each other. But this apparent difficulty admits of a very simple explanation.

Why excited bodies attract and repel.

If we suppose two excited and insulated spheres placed at a small distance from each other, it is obvious that the only forces which can occasion the motion of the bodies, are the mutual attraction or repulsion of the fluid in the one, to the fluid in the other. For the repulsions exercised by the particles of fluid in each body on one another, can have no effect in producing a motion in the centre of gravity of either body. If the two spheres consist of non-conducting matter, the unknown power which gives them the non-conducting property, will prevent the escape of the electricity from each. Therefore the mutual attractions and repulsions of the fluids, as they cannot escape from the matter, may be supposed to carry the matter along with them, and thus to cause the globes to approach or recede, according as they are charged with different kinds of electricity, or with the same kind.

When an excited conducting body is insulated the superinduced electricity forms a coating on its surface, and (if we suppose the body spherical) the thickness of this coating will be every where the same. This electricity presses upon the ambient air, which prevents it from making its escape. The excited sphere, in consequence of this action of the electricity, which is proportional to the square of its thickness, will be less pressed upon by the surrounding atmosphere than if it were not excited. But as the pressure, though diminished, is every where equal, there will be no tendency in the sphere to move from its place. Let us suppose the conducting sphere to be charged with positive electricity, and let us conceive a mass of sealing wax or gum lac, charged negatively, to approach it. A portion of the combined electricity, natural to the sphere, will be decomposed. The positive portion will accumulate on the surface of the sphere next the mass of sealing wax, being attracted by its negative electricity. The superabundant positive electricity already in the sphere will accumulate at the same surface for the same reason. While the decomposed negative electricity will accumulate at the opposite surface of the sphere, being repelled by the negative electricity of the sealing wax. Thus the coating of electricity next the sealing wax will become thicker than before, while the coating at the greatest distance will become thinner. Hence the electricity in the part of the sphere next the sealing wax will press more upon the air than before, while the air will press more than before upon that surface of the sphere which is farthest from the sealing wax. Both of these pressures have a tendency to cause the sphere to move towards the sealing wax, and if the weight of the sphere be sufficiently small it will move accordingly.



If the sphere, for example, were a small globule of elder pith, suspended by a silk thread, and previously hanging perpendicularly. Let C be a small excited sphericle of pith of elder suspended by the silk thread S C, and let the excited stick of sealing wax A, be made to approach it. C will deviate from the perpendicular, and assume the position of S C′. But it is obvious that this deviation from the perpendicular will have a limit. When the pith ball hung in the position S C the whole of its weight was supported by the thread S C; but in the position S C′ this is no longer the case. If we represent the weight by the line C′ p, it is evident that we may resolve it into the two forces C′ q, and C b. C′ q being in the direction of the thread is destroyed by its resistance. The other C′ b being perpendicular to that direction, has a tendency to bring C′ back to C, and is only prevented by the action of the sealing wax. If we call the angle C S C′ a, this second force will amount to p. sin. a p being the weight of the pith ball C. Thus it will increase with the angle a, and must soon become strong enough to counteract the tendency of C to move towards the sealing wax.

These examples might be easily carried much farther. But enough has been said to enable the reader to conceive how conducting bodies impregnated with different kinds of electricity approach each other. It is owing, partly at least, to the unequal pressure of the air on the body, and of the electricity on the air. Accordingly these attractions and repulsions are not so powerful, nor can a conducting body be excited to such a degree, if air be excluded, as by putting it into the exhausted receiver of an air pump, or into a torricellian vacuum. But that metallic knobs attached to very fine wires repel each other when charged in a torricellian vacuum has been shown by Sir H. Davy.* It is obvious from this that attractions and repulsions of excited bodies may be exerted, though not to so great a degree, even when air is entirely excluded. When the two bodies are charged with the same kind of electricity, the repulsions which take place are occasioned by the same pressures. Suppose both bodies charged with positive electricity, the positive electricity in the pith ball will accumulate on the surface farthest distant from the other body, while the portion nearest will become negatively electrified. Hence the coating of electricity farthest off will be thickest. It will therefore press most upon the ambient air, while the air will press most upon the surface of the pith ball nearest the positively electrified body. The consequence of these two pressures will be a tendency on the part of the pith ball to recede. It will accordingly do so till the force C′ *b* become strong

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* Phil. Trans. 1822, p. 69.

enough to counteract these two pressures. When the repulsions take place in the torricellian vacuum we must ascribe them to the action of the electricity of the same kind existing in each body. The unknown cause which attaches the electricity to the surface of the body must prevent it from leaving the body.

Thus all the electrical attractions and repulsions may be explained in a satisfactory manner without having recourse to either of the suppositions, without which Æpinus found that the Franklinian hypothesis could not be reconciled to the phenomena; namely, that there exists an attraction between electricity and matter, and that matter repels matter with a force varying inversely as the square of the distance.

CHAPTER VII.

OF THE ELECTRICITY OF THE ATMOSPHERE.

Chap. VII.

Lightning and electricity the same.

After discovering the Leyden phial, and witnessing the effects of electrical batteries, electricians could not avoid perceiving the analogy which existed between lightning and electricity. This analogy was accordingly pointed out by various individuals, especially by the Abbé Nollet. But it was Dr. Franklin, who, having discovered the power of pointed bodies to discharge electricity at a distance, first thought of employing that method to render atmospherical electricity sensible. Not having in America the requisite means for such an experiment, he requested the electricians in Europe to turn their attention to it. The first person who attended to his suggestion was M. D'Abilard, who constructed an apparatus for the purpose at Marly la Ville, about 6 leagues from Paris. It consisted of an iron rod 40 feet long, the lower extremity of which was brought into a centry box where rain could not enter; while on the outside it was fastened to three wooden posts by silken strings defended from the rain. M. D'Abilard was obliged to leave home, but in his absence he intrusted his

Verified by D'Abilard.

machine to one Coisier, a joiner, who had served 14 years among the dragoons, and on whose courage and understanding he could depend. This artisan had all the requisite instructions given him, and he was desired to call some of his neighbours, particularly the curate of the parish, whenever there should be any appearance of a thunder storm. On Wednesday the 10th of May, 1752, between 2 and 3 in the afternoon, Coisier heard a pretty loud clap of thunder. Immediately he ran to the machine, taking with him a phial furnished with a brass wire; and presenting the wire to the end of the rod, a small spark issued from it with a snap, like that of a spark from an electrified conductor. Stronger sparks were afterwards drawn in the presence of the curate and a number of other people. The curate's account of them was that they were of a blue colour, an inch and a half in length, and smelled strongly of sulphur. In taking them Coisier received a stroke on his arm, a little below the elbow.

Eight days after this, M. Delor witnessed the same appearances at his house situated in one of the highest spots in Paris. His apparatus was similar to that of D'Abilard, excepting that his iron rod was 99 feet in length. There was no thunder nor lightning, only a cloud passed over the house.

Franklin's electric kite.

Dr. Franklin, after having published his views on this subject, was waiting for the erection of a spire in Philadelphia to execute his experiment. It occurred to him that by means of a common kite, he could have a readier and better access to the

Chap. VII. regions of thunder, than by any spire whatever. Preparing therefore a large silk handkerchief, and two cross sticks of sufficient length on which to extend it, he took the opportunity of the first approaching thunder storm, to take a walk into a field in which there was a shade convenient for his purpose. But dreading the ridicule which too commonly attends unsuccessful attempts in science, he communicated his intended experiment to nobody but his son, who assisted him in raising the kite.

The kite being raised, a considerable time elapsed before there was any appearance of its being electrified. One very promising cloud had passed over it without effect; when at length, just as he was beginning to despair of his contrivance, the rain having wetted the string which was of hemp, and rendered it a better conductor, he observed some loose threads on it to stand erect, and to avoid one another, just as if they had been suspended on a common conductor. Struck with this promising appearance, he immediately presented his knuckle to the key which was tied to the end of the string, and the discovery was complete. He perceived a very evident electric spark. Others succeeded even before the string was fully wet, so as to put the matter past all dispute; and when the rain had wetted the string, he collected sparks very copiously. This happened in June 1752; about a month after the French electricians had verified the theory, but before any information on the subject had reached Dr. Franklin.

This experiment of Franklin was attended with extreme danger. Had his cord been more moistened, or a better conductor than it was, it is probable that this celebrated man would have fallen a sacrifice to his temerity, and we would have been deprived of all the benefits which he afterwards conferred upon science, upon his own country, and upon mankind in general. This actually happened soon after to Professor Richmann of Petersburg, who was struck dead by a flash of lightning on the 6th of August 1753, drawn by his apparatus into his own room, as he was attending to an experiment which he was making with it.

Hazard of the experiment.

Death of Professor Richmann.

He had provided himself with an instrument, which he called an electrical gnomon, the use of which was to measure the strength of electricity. It consisted of a rod of metal terminating in a small glass vessel, into which he had (for what reason does not appear,) put some brass filings. At the end of this rod a thread was fastened, which hung down by the side of the rod when it was not electrified, but when it was it avoided the rod, and stood at a distance from it, making an angle at the place where it was fastened. To measure this angle he had the arch of a quadrant fastened to the bottom of the iron rod.

He was observing the effect of the electricity of the clouds, at the approach of a thunder storm upon his gnomon, and of course standing with his head inclined towards it, accompanied by M. Solokow (an engraver, whom he frequently took with him to be a joint observer of his electrical experiments, in order to represent them the better in his

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figures), when this gentleman, who was standing close to his elbow, observed a globe of blue fire, as he called it, as big as his fist, jump from the rod of the gnomon, towards the head of the Professor, which, at that instant, was about a foot distant from the rod. This flash killed Mr. Richmann; but Mr. Solokow could give no account of the particular manner in which he was immediately affected by it. For at the same time that the Professor was struck, there arose a sort of steam or vapour, which entirely benumbed him, and made him sink down upon the ground; so that he could not remember even to have heard the clap of thunder, which was very loud.

The globe of fire was attended with a report as loud as that of a pistol. A wire which brought the electricity to the metal rod was broken to pieces, and its fragments thrown upon Mr. Solokow's clothes. Half of the glass vessel in which the rod of the gnomon stood, was broken off, and the filings of metal that were in it, were thrown about the room. The door case of the room was half split through, the door torn off and thrown into the room. The shoe on the Professor's left foot was burst open, and there was a blue mark on the foot at that place. From which it was concluded, that the electricity had entered by the head, where there were evident marks of injury, and made its way out again by the left foot.*

Improvements in the kite.

M. de Romas made the experiment with the kite

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* See Phil. Trans. 1754, vol. xlviii. p. 765. Vol. xlix. p. 61. Professor Richmann was born in Livonia, on the 11th July, 1711,

in a more perfect manner than the first attempt of Dr. Franklin. He twisted a fine iron wire into the cord of the kite.* To prevent the observer from being exposed to danger, the lower extremity of the string terminated in a silk cord eight or ten feet in length, by means of which the kite with its string was insulated. Instead of drawing sparks with the finger, which makes the observer himself receive the charge, he received them by means of a metallic conductor, connected with the ground by a chain, which he held in his hand by means of an insulating glass handle, so that it resembled our common discharger. Romas describes the sparks given out from the string to this discharger during a thunder storm in a letter to the Abbé Nollet, in very glowing language. "Conceive," says he, "plates of flame nine or ten feet long, and an inch thick, which made as much noise as a pistol. In less than an hour I had certainly thirty plates of this size, without reckoning a thousand others of seven feet, and below that. But what gave me the

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his father, William Richmann, having died of the plague before the birth of his son. He was educated at Revel, Halle, and Jena, and devoted himself to mathematics and physics. He went to Petersburg as tutor to the children of Count Ostermann. In 1735, he became an adjunct of the Academy. In 1741, he was made extraordinary Professor of Physics, and in 1745, on the death of Krafft, he became ordinary Professor of Physics. Though his papers are valuable, his death has given him a greater degree of celebrity than he would otherwise probably have attained. See Novi Comment. Acad. Petropol. tom. iv. for 1752—3, p. 36.

* Mr. Cuthbertson employed a brass wire which is better, and M. Charles used a cord all of metal threads.

greatest satisfaction in this new spectacle, was, that the greatest of these plates were spontaneous, and that notwithstanding the abundance of the fire which they contained, they fell always on the nearest conductor. This constancy gave me so much security, that I was not afraid to draw sparks by means of my conductor, even when the thunder storm was at its height, although the glass handle of that instrument was only two feet in length. I conducted where I pleased without feeling in my hand the smallest commotion, sparks of fire six or seven feet long, with the same facility as those whose length did not exceed seven or eight inches."

I refer those readers, who may wish to employ an electrical kite for similar purposes to the late Mr. Cuthbertson's *Practical Electricity*, p. 237, where they will find the contrivances which he employed to secure the observer against all risk of danger. It may, however, be proper to observe, that the experimenter should never hold the string of the kite in his hand, even though the lower part of it be of silk, in case rain or any accidental cause should make it a conductor. The tremendous size of the sparks described above by Romas, shows that in such a case they might produce fatal effects.

Intensity of electricity at the top of a rod.

Indeed, it is easy to see how intense the electricity may become at the end of the cord, even when the thunder clouds are not very formidable in appearance. Poisson has demonstrated that in the case of an insulated cylinder of small dimensions in the neighbourhood of a charged conductor, the thickness of the coating of electricity, on the cylinder is inversely, as its diameter compared to the osculating sphere of the charged conductor. Let $r =$ diameter of the cord of the kite, $R =$ diameter of the osculating sphere of the thunder cloud, $E =$ thickness of the coating of electricity of the thunder cloud, e that of the string, then it has been demonstrated that $e = m\dfrac{E\,R}{r}$, m being a constant coefficient, which experiment has shown to be very nearly $\dfrac{9}{48}$. But the thickness of the coating at the extremity of the cylinder, being to that at any other part, as 2·3 to 1, the expression for it will be $\dfrac{20{\cdot}7}{48} \cdot \dfrac{E\,R}{r}$.

To apply this expression to the thickness of the coating of electricity at the end of the kite string, let us suppose the diameter of the string to be one line, while the radius of the osculating circle of the cloud is 1,000 feet. Then $R = 144{,}000$ lines, and $r = 1$. The expression becomes

$$\frac{20{\cdot}7}{48} \cdot \frac{144{,}000\ E}{1} = 62{,}100\ E.$$

Thus it appears that the thickness of the coating of electricity at the extremity of the cord of the kite, is no less than 62,100 times greater than upon the charged cloud. Now, as the pressure on the air is equal to the square of the thickness, it will be almost 400 million times greater at the end of the string, than on the surface of the cloud. We need not be surprised then that such terrible sparks issue from the end of the string, even when the thunder cloud is not very considerable.

Thus, it has been demonstrated by the sagacity of Dr. Franklin, that *thunder and lightning* is merely a case of electrical discharges from one portion of the atmosphere to another, or from one cloud to another. Air, and all gases, are non-conductors; but vapour and clouds, which are composed of it, are conductors. Clouds consist of small hollow bladders of vapour, charged each with the same kind of electricity. It is this electric charge which prevents the vesicles from uniting together, and falling down in the form of rain. Even the vesicular form which the vapour assumes, is probably owing to the particles being charged with electricity. The mutual repulsion of the electric particles may be considered as sufficient (since they are prevented from leaving the vesicle by the action of the surrounding air, and of the surrounding vesicles,) to give the vapour the vesicular form.

Formation of clouds.

In what way these clouds come to be charged with electricity, it is not easy to say. But as electricity is evolved during the act of evaporation,*

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* M. Pouillet has lately published a set of experiments, which seem to overturn Volta's theory of the evolution of electricity by evaporation. He has shown that no electricity is evolved by evaporation, unless some chemical combination takes place at the same time. But it follows from his experiments that electricity is evolved abundantly during combustion. The burning body giving out resinous, and the oxygen vitreous electricity. In like manner, the carbonic acid emitted by vegetables is charged with resinous elec-

the probability is, that clouds are always charged with electricity, and that they owe their existence, or at least their form, to that fluid. It is very probable that when two currents of dry air are moving different ways, the friction of the two surfaces may evolve electricity. Should these currents be of different temperatures, a portion of the vapour which they always contain, will be deposited; the electricity evolved will be taken up by that vapour, and will cause it to assume the vesicular state constituting a cloud. Thus we can see in general how clouds come to be formed, and how they contain electricity. This electricity may be either vitreous or resinous according to circumstances. And it is conceivable, that by long continued opposite currents of air, the charge accumulated in a cloud may be considerable. Now, when two clouds charged, the one with positive, and the other with negative, electricity, happen to approach within a certain distance, the thickness of the coating of electricity increases on the two sides of the clouds which are nearest each other. This accumulation of thickness soon becomes so great as to overcome the pressure of the atmosphere, and a discharge takes place, which occasions the flash of lightning. The noise accompanying the discharge constitutes the thunder clap, the long continuance of which partly depends upon the reverberations

tricity, and the oxygen (probably) charged with vitreous electricity. These two sources are sufficiently abundant to account for the vast quantity of electricity so often accumulated in the clouds. See Ann. de Chim. et de Phys. xxxv. 401.

from neighbouring objects. It is therefore loudest and largest, and most tremendous, in hilly countries.

Description of a thunder storm.

A thunder storm in this country commonly commences in the following manner. A low dense cloud begins to form in a part of the atmosphere that was previously clear. This cloud increases fast, but only from its upper part, and spreads into an arched form, appearing like a large heap of cotton wool. Its under surface is level, as if it rested on a smooth plane. The wind is hushed, and every thing appears preternaturally calm and still.

Numberless small ragged clouds, like teazled flakes of cotton, soon begin to make their appearance, moving about in various directions and perpetually changing their irregular surface, appearing to increase by gradual accumulation. As they move about they approach each other, and appear to stretch out their ragged arms towards each other. They do not often come in contact; but after approaching very near each other, they evidently recede either in whole, or by bending away their ragged arms.

During this confused motion, the whole mass of small clouds approaches the great one above it; and when near it, the clouds of the lower mass frequently coalesce with each other, before they coalesce with the upper cloud. But as frequently the upper cloud coalesces without them. Its lower surface, from being level and smooth, now becomes ragged, and its tatters stretch down towards the others, and long arms are extended towards the ground. The heavens now darken apace, the whole mass sinks down; wind rises and frequently shifts in squalls; small clouds move swiftly in various directions; lightning darts from cloud to cloud. A spark is sometimes seen coexistent through a vast horizontal extent, of a zigzag shape, and of different brilliancy in different parts. Lightning strikes between the clouds and the earth—frequently in two places at once. A very heavy rain falls—the cloud is dissipated, or it rises high and becomes light and thin.*

These electrical discharges obviously dissipate the electricity, the cloud condenses into water, and occasions the sudden and heavy rain which always terminates a thunder storm. The previous motions of the clouds, which act like electrometers, indicate the electrical state of different parts of the atmosphere.

Explanation of the phenomena.

Thunder then only takes place when the different strata of air are in different electrical states. The clouds interposed between these strata, are also electrical, and owe their vesicular nature to that electricity. They are also conductors. Hence they interpose themselves between strata in different states, and arrange themselves in such a manner as to occasion the mutual discharge of the strata in opposite states. The equilibrium is restored, the clouds deprived of their electricity collapse into rain, and the thunder terminates.

In thunder storms the discharges usually take place

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* Robison; Supplement to third Edition of the Encyclopœdia Britannica, Vol. ii. p. 681.

between two strata of air, very seldom between the air and the earth. But that they are sometimes also between clouds and the earth cannot be doubted. I once witnessed a thunder storm from the summit of Lochnagar, a very high granite mountain in Aberdeenshire. The thunder was at a great distance east. The first clap was nearly due east, distant not less than ten or twelve miles. The second was north-east, and equally far off. Thick black clouds intervened between me and the thunder, so that I saw no lightning. After watching the progress of the storm for about half an hour, suddenly a white cloud of a very peculiar appearance stretched itself between the part of the heavens where the thunder was and the earth. This cloud was composed of distinct parallel fibres bent as in the margin. This cloud continued about half an hour, during which it conveyed away all the surplus electricity from the clouds to the earth. For no more thunder was heard, and the clouds discharged themselves in a heavy shower of rain which terminated the thunder storm.

Electrical discharges without noise.

These electrical discharges sometimes take place without any noise. In that case the flashes are very bright, but they are single flashes passing visibly from one cloud to another, and confined usually to a single quarter of the heavens. When they are accompanied by the noise, which we call *thunder*, a number of simultaneous flashes, of different colours, and constituting an interrupted zigzag line, may generally be observed stretching to an extent of several miles. These seem to be occasioned by a number of successive or almost simultaneous discharges from one cloud to another; these intermediate clouds serving as intermediate conductors, or stepping stones for the electrical fluid. It is these simultaneous discharges, which occasions the rattling noise, which we call thunder. Though they are all made at the same time, yet as their distances are different, they only reach our ear in succession, and thus occasion the lengthened rumbling noise so different from the snap, which accompanies the discharge of a Leyden jar.

Nature of the thunder clap.

If the electricity were confined to the clouds, a single discharge (or a single flash of lightning) would restore the equilibrium. The cloud would collapse and discharge itself in rain, and the serenity of the heavens would be restored. But this is seldom the case. I have witnessed the most vivid discharges of lightning from one cloud to another, which enlightened the whole horizon, continue for several hours, and amounting to a very considerable number, not fewer certainly than 50, and terminating at last in a violent thunder storm. We see that these discharges, though the quantity of electricity must have been immense, did not restore the equilibrium. It is obvious from this, that not only the clouds but the strata of air themselves, must have been strongly charged with electricity. The clouds being conductors served the purpose of discharging the electricity with which they were loaded, when they came within the striking distance.

But the electric stratum of air with which the cloud was in contact, being a non-conductor, would not lose its electricity by the discharge of the cloud. It would immediately supply the cloud with which it was in contact with a new charge. And this repeated charging and discharging process would continue to go on till the different strata of excited air were brought to their natural state.

Electrical state of the atmosphere.

From the atmospherical electric journal kept by Mr. Read, at Knightsbridge, during two whole years from the 9th of May, 1789, to the 9th of May, 1791, it appears that clouds, and rain, and hail, and snow, are always charged with electricity; sometimes with negative, but more frequently with positive, electricity. When the sky is serene and cloudless, the strata of air are generally charged with positive electricity. In such cases the thunder rod is charged by induction. The highest end acquiring the opposite state of electricity from the air, and the lowest end the same kind of electricity, while a portion of the rod towards the middle is neutral. During the first year, there occurred only 7 days in which no electricity could be perceived. But during the second year, when the apparatus was much more complete, not a single day occurred which did not give indications of electricity in the atmosphere.

During the first year the electricity was positive 241 times, and during the second year, 423 times. This difference was chiefly owing to the apparatus. During the first year, there occurred 73 days in which the signs of electricity were so weak, that the kind could not be determined, and there were 7 days in which no signs of electricity at all were perceptible. But during the second year, the apparatus was so much more perfect, that no day occurred without signs of electricity, and it was ascertained, that on those days in which the electricity is weak, it is always positive. During the first year, the electricity was observed negative 156 times, and during the second year, 157 times. During the first year, sparks could be drawn from the apparatus during 98 days, and in the second year during 106 days. From these facts, the probability is, that the electrical state of the atmosphere did not differ much during each of the two years, during which the observations were kept.

It would tend greatly to promote the progress of meteorology, which is obviously very much connected with electricity, if a register were kept in the torrid zone of the state of the electricity of the atmosphere during a whole year. The weather in these countries is so regular, and the transition from dry weather to rain so marked, that we have reason to expect corresponding changes in the state of the electricity of the atmosphere. The heaviness of the rain and the large size of the drops in these countries, indicate that the clouds from which the rain comes are situated at a great height above the surface of the earth. If the accumulation of electricity should be at a corresponding height, this would render a greater height necessary for the rod by means of which the electrical indications are determined.

CHAPTER VIII.

OF THE THEORY OF THE LEYDEN JAR, AND OTHER SIMILAR APPARATUS.

Ch. VIII. It has been already stated in a preceding chapter, that the Leyden jar was accidentally discovered at Leyden, by Cunæus, Muschenbroeck, and Allemand, at a very early period of electrical science. It constituted a memorable era in the history of electricity, because it furnished electricians with a method of accumulating electricity at pleasure, and thus of producing many curious phenomena, and investigating the laws of electricity with greater certainty and success. The theory of the Leyden jar was first investigated by Dr. Franklin, and has been long familiar to electricians. The *condenser* and *electrophorus*, instruments, so useful in many investigations, are nothing else than modifications of it. The Leyden jar, as at present employed by electricians, consists of a thin cylindrical glass of any size, from that of an eight ounce phial, to that of 400 cubic inches or even more, usually terminated at the upper extremity by a mouth as wide as will admit the insertion of the hand to

coat the inside of the jar with tinfoil. The jar is covered both inside and out with tinfoil pasted on from the bottom to within a third of the top. The upper portion of the jar is coated over with lac varnish, or if not varnished it should be rubbed over with a thin coating of tallow to prevent moisture from adhering to the glass, which in damp weather or in a moist atmosphere, would prevent the possibility of charging the jar. A cork covered over with lac varnish, or with sealing wax, is fitted into the mouth of the jar, through which passes a brass wire, terminating at its upper extremity in a knob or small sphere of brass, and dividing within the jar into three wires, which spread wide, and in consequence press lightly against the inside coating of the jar.

Mode of charging the Leyden phial.

Let us suppose that the knob *b* is placed within the striking distance of the prime conductor of an electrical machine, while the outside coating in consequence of the jar standing on a common table communicates with the earth. When the machine is moved, a spark of positive electricity strikes the knob *b*, and makes its way along the wire to the inside of the jar. This spark, from the nature of glass, cannot pass through the jar; but is deposited upon the inside surface of the jar. It will repel the positive electricity on the outside surface of the ar, which will be partially driven out of it, and will pass into the earth; while at the same time it will attract the negative electricity, which will therefore accumulate on the outside surface of the glass. Suppose another spark to pass from the prime conductor, to the knob *b*. This will occasion an additional quantity of positive electricity to accumulate on the inside surface of the jar. More positive electricity will be driven out of the outside surface, and more negative electricity will be attracted into it. Thus every spark which passes from the conductor to the knob, occasions an increase of positive electricity on the inside surface, and an increase of negative electricity upon the outside surface; till at last there is a very considerable accumulation of the two electricities on the two surfaces. The mutual attraction which takes place between these two electricities, will have a tendency to prevent them from escaping spontaneously, and will also occasion a much greater accumulation of each on the respective surfaces. Suppose now that a good conductor, a metallic wire for example, is made to touch at once the knob *b*, and the outside coating of the jar, the two electricities will rush into combination along this wire, a snap or an explosion will take place accompanied by a brilliant light, the two electricities will combine, and all symptoms both of positive and negative electricity will disappear, or almost disappear. If instead of a metallic wire the medium of communication between the inside and outside coating, be the human body; as for example, if we grasp the jar by the bottom with one hand, and touch the knob with the finger of the other, the two electricities unite in our body, and the act of union is attended with a shock more or less violent and painful, according to the size of the jar, and the greatness of the charge. When a number of jars are united together, constituting what is called an electrical battery, the shock is so great as to prove fatal, when passed through the body of an animal of considerable size, as a sheep, or a large dog.

Charge on the surface of the glass.

In the Leyden phial the electricities are not lodged upon the coatings of the jar, but upon the two surfaces of the jar itself. This was demonstrated by Dr. Franklin. Instead of coating the inside of the jar with tinfoil, we may fill it with water up to the usual height of the coating. This indeed was the coating originally used by the discoverers of the Leyden jar. Instead of pasting the outside coating to the jar, we may make it merely to fit the jar, and so that the jar may be easily slipt out of it without using force. Dr. Franklin charged a jar containing water, and having the outside coating merely in contact. He then poured out the water, and drew the jar out of its outside coating, taking care never to establish any communication between the inside and outside. New water was now poured into the jar, and a new coating applied on the outside. The jar was still found charged, and when a communication was established between the knob and the outside coating, an explosion took place just as would have happened had the original coatings not been displaced. Whenever the quantity of positive electricity accumulated on the inside surface of the jar is so great, that by its repulsive force it prevents the

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accumulation of any more, then the jar has received all the charge which it can take.

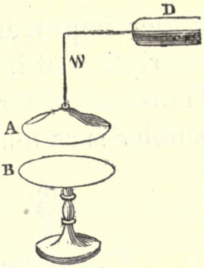
If we insulate a Leyden jar, and place the outside coating in contact with an insulated electrometer consisting of two pith balls suspended by linen threads from a hooked wire; as soon as a spark of positive electricity passes into the knob, the balls separate, indicating a charge of positive electricity. If we touch the wire to which the pith balls are attached, they immediately close, because by this contact, we carry off the positive electricity which had been driven out of the outside surface by the spark of positive electricity, which was deposited on the inside surface. If we now touch the knob of the jar, so as to draw off the uncombined positive electricity from the inside surface, the pith balls again separate. But they are now charged with negative electricity. Thus we see, that when positive electricity accumulates on the inside surface, positive electricity is driven off from the outside surface, and that negative electricity accumulates on that surface. When the jar is charged, if we put it under the receiver of an air-pump, and exhaust the air, the two electricities being no longer retained in their place, rush towards each other, forming a path which appears luminous in the dark.

Leyden jar furnishes either electricity.

By means of the charged Leyden jar, we can procure at pleasure either positive or negative electricity, which is very convenient for many purposes. From the mode of charging the phial, the inside has generally a charge of positive electricity, while negative electricity is accumulated on the outside. Suppose it thus charged, if we take it up in one hand, and make the knob rub against the surface of a cake of wax or a thin plate of glass covered with lac varnish; the parts of the plate touched by the knob will have acquired positive electricity, and the parts opposite on the under surface of the glass will have acquired negative electricity. If we now suspend the glass plate insulated in the air, and blow upon it a mixture of sulphur and minium powder previously triturated together, the minium alone will adhere to the parts of the plate that have been electrified positively. If we blow the same mixture of powder upon the other side of the plate, the sulphur alone will adhere to the parts which are electrified negatively. Thus we will have the same lines traced on the two sides of the glass plate; but with different coloured powders. The positive electricity will be covered with the red powder of minium, the negative electricity with the yellow powder of sulphur. This curious experiment was first made by Lichtenberg, and these lines are called the lines of Lichtenberg. The reason of the phenomenon is, that when sulphur and minium are triturated together, sulphur acquires positive electricity, while minium acquires negative. Hence they naturally attach themselves to those surfaces which have an electricity opposite to their own.

Condenser explained.

The instrument called the *condenser*, so useful in electrical investigations, is very nearly of the same nature with the Leyden jar, or at least depends upon the same electrical laws. This instrument was first contrived by Æpinus, but it was brought into the convenient form for use, which it has at present, by Volta. Suppose a body A, to be placed in contact with the charged prime conductor of an electrical machine. It will acquire a certain electrical charge. If while thus charged, we bring the body B, (supposed in a natural state and communicating freely with the ground,) into its neighbourhood, the presence of this new body will enable A to take a much stronger charge than before. For the electricity with which it is charged acts upon the electricity of B. It drives the electricity of the same kind with itself into the ground, and attracts the opposite electricity, and fixes it upon that surface of B which is nearest A. By this attraction, the elasticity of the electricity in A is diminished. The consequence is, that an additional quantity of electricity passes from the prime conductor into A. This new quantity acts in its turn upon the electricity in B, drives an additional portion of the electricity of the same name into the earth, and attracts an additional quantity of the opposite electricity. This new charge of electricity in B, by its attraction, serves still farther to diminish the elasticity of the electricity in A, and thus an additional quantity will flow into it from the prime conductor. Thus the two bodies act reciprocally upon each other, and mutually increase the charge in each.



Let D be the prime conductor of a machine, and let A be a metallic plate suspended from the conductor by means of the wire *w*. If D be weakly charged, it will communicate a weak charge also to A. This may be ascertained by bringing an electrometer into its neighbourhood. Now let the metallic plate B, communicating with the ground, be made gradually to approach A, till it is as near as possible, without however being sufficiently so to receive a spark from A. The presence of the plate B will enable A to take a much greater charge, as may be easily ascertained by means of an electrometer. If we now remove A by means of a glass rod, taking care that B shall retain its position till A is insulated, then A will be much more strongly charged than it would have been had not B been near it.

The nearer we can bring B to A, the greater is the effect which it will produce in increasing the charge. But this diminution of distance is limited by the tendency which the electricity of A has to pass off to B. The best way then is to interpose between them some non-conductor, through which of course the electricity cannot pass; such for example as a thin plate of glass, or a thin coating of lac varnish. By this contrivance, the distance between the plates may be rendered very small. Thus arranged, the two plates constitute what is called the *condenser*.

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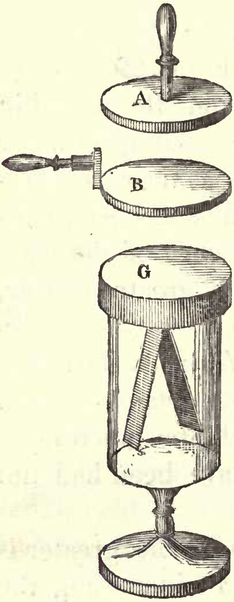
Bennet's doubler.

Bennet's *doubler* may be considered as a still farther improvement of the condenser. The first description of it was published in the Philosophical Transactions for 1787. The method of using this doubler is as follows.



It consists of three parts. 1. A polished brass plate A, with an insulating handle fixed in its centre. 2. A similar plate B, with an insulating handle fixed in its periphery. 3. The cap of Bennet's gold leaf electrometer, G, which serves for the third plate. The two plates A and B, are varnished on their under side. The handles are made of mahogany, fixed to the plates by means of glass nuts covered with sealing wax.

Suppose we have to examine the electricity of the plate G.

1. Place B upon G, and communicate some electricity to the latter, while the plate B is touched with the finger. It is evident that G will receive a greater charge than if B were not near it. In fact these two plates constitute the common *condenser*.

2. Remove the source of electricity from the contact of G, and take the finger from off B. Raise B, by its insulating handle, and B and G will exhibit the opposite states of electricity more strongly than when they were in contact.

3. Place A on B and touch A with the finger. It is obvious that a quantity of the opposite electricity from that in B will accumulate in A, while the same kind of electricity will be driven off. In short A will be charged with the same kind of electricity as G.

4. Place B upon G, and touch B with the finger as before, and at the same time apply A edgeways to G. In this situation A will communicate the greatest part of its electricity to G.

5. Remove A, take the finger from B, and raise B from G. The opposite electricities in B and G will now be stronger than before, on account of the additional electricity afforded by A.

6. Place A upon B again, and proceed as in No. 3. An additional dose of electricity may now be communicated from A to G. By proceeding in this manner for a certain time, the electricity originally communicated to G, though at first too small to affect the strips of gold leaf, will at last become sufficiently sensible to produce a considerable divergence in them.

This doubler was still farther improved by Mr. Nicholson, who constructed what he called his *revolving doubler*, an account of which may be seen in the Philosophical Transactions for 1788, and in Nicholson's Journal for May, 1800.

The best condenser consists of two metallic plates, having their contiguous surfaces covered with a coat of varnish. Care must be taken to place them upon each other, without rubbing them against each other. For friction would develope

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electricity in the coating of resin, which would adhere to it, and might occasion errors in delicate experiments. To render the instrument convenient, the plate B is fixed upon a solid metallic foot, and an insulating handle of varnished glass is fixed in the centre of the upper surface of the plate A. A wire terminating in a knob is usually fixed in the plate A, by means of which, the electricity to be examined is conveyed to it, while the finger is in contact with plate B, or its metal stand in order to secure its communication with the ground. When A is to be removed from B, the condenser should be placed upon a table, and A should be lifted up perpendicularly from B, without any friction, otherwise A would be apt to discharge itself upon B.

Theory of the condenser.

The condenser evidently depends upon the same principles as the Leyden phial, and indeed its theory is so obvious, that a very few words will be sufficient to explain it. Suppose plate A to be charged with positive electricity, a certain quantity of negative electricity will be attracted and fixed in plate B. This will allow A to take a farther charge, which will accumulate a still additional quantity of negative electricity in B; and so on. If the distance between A and B were nothing, the quantity of positive electricity in the former would be exactly balanced by the quantity of negative electricity in the latter. But there is always a certain distance between A and B. Hence, the positive electricity in A will exceed the negative electricity in B, and it will exceed it the more the greater the distance between the two plates is. But in all cases there must be an excess of vitreous electricity in A; so that the two electricities accumulated in the two plates, will not exactly neutralize each other. The very same thing happens in the Leyden jar, and hence it will be found, that when the communication is made between the two sides of the jar, all electrical symptoms do not vanish. A small charge will still be found in the jar, which will occasion a slight shock, when, holding the discharged jar in one hand, we touch the knob with the other.

In the condenser, the electricity does not reside in the metallic plates, but on the faces of the thin coat of varnish which separates them. This is easily demonstrated by employing a condenser whose plates are separated by a thin disc of glass. After plate A is charged with electricity, place the condenser upon an insulating stool. Then remove plate A. On applying the finger to it, only a very small spark will be given, amounting to no more than a very small portion of the electricity. Now, remove the plate of glass by taking it up by one of the corners, and examine plate B. It also will communicate only a very slight spark. We see then that the two electricities were in fact upon the two surfaces of the glass. Thus the condenser differs from the Leyden phial in nothing but the shape.

Electrophorus.

The *electrophorus*, an instrument for which we are indebted to Volta, is quite similar in its nature to the condenser, and depends upon the same principles as the Leyden phial. The electrophorus usually consists of three parts. 1. A metallic plate, round and about six inches in diameter. It should be thin and provided with an insulating stand. 2. A cake of resin of the same dimensions as the metallic plate. This resin is melted, and then cast upon the metallic plate. It should also be thin. We shall call this metallic plate the *sole* of the electrophorus, and the resin we shall call the *cake*. 3. A metallic plate, or a wooden plate quite smooth, all round, and without any projecting points covered with a coating of tinfoil. It should be of the same dimensions as the other two parts. We shall call it the *cover*. It must be provided with an insulated glass handle like the upper plate of the *condenser*.

Pour the melted cake upon the insulated sole and let it remain till it has become firm and hard. If we now examine the apparatus, we shall find it negatively electrified, and when the finger is applied to any part of it, especially the sole, a spark is produced. If the apparatus be now suffered to remain at rest, the electricity is gradually dissipated and disappears, but it may be again restored by rubbing the resin with a piece of catskin with the fur on. By this friction resinous electricity is developed on the cake. If the insulated sole be placed in contact with an electrometer, we shall find the pith balls diverge, and indicate negative electricity. If we touch the sole with our finger, the pith balls fall down, and all signs of electricity disappear. The cake obviously repels the negative electricity from the sole, and attracts the positive electricity. Hence there will be an accumulation of negative electricity in the cake, and of positive electricity in the sole.

If we apply the cover to the cake, lifting it by the insulating handle, and after allowing it to remain a little, lift it off again, and apply it to an electrometer, we shall find no signs of electricity. But if we replace it on the cake, and place an insulated electrometer in communication with it, the pith balls will diverge with negative electricity. If we touch the cover with the finger, and then withdraw it, we shall find it charged with positive electricity. It is obvious that the action of the cake upon the cover is the very same as upon the sole. The negative electricity has been repelled, and the positive electricity attracted. So that both the sole and the cover, if they be made to communicate with the ground, will be found charged with positive electricity.

As the electricity does not soon leave the cake after friction, if it be kept in a dry place the cover may be charged with positive electricity, simply by placing it on the cake, and touching it with the finger, and then removing it. And the charge will be sufficient to give sparks, and thus to enable us to apply the electrophorus to a variety of useful purposes. It may continue to act for a month or even longer. But the attraction between the two opposite electricities will gradually overcome the resistance which the resin manifests to the disengagement of its electricity. The positive electricity will gradually make its way, unite with the negative electricity on the surface of the resin, and all the electricity will of course be discharged. When this happens the electrophorus may be again brought into action by rubbing the cake with a piece of catskin as before.

The theory and mode of action of the electrophorus is so obvious after the preceding explanation of the Leyden phial and the condenser, that it is quite unnecessary to offer any farther elucidation.

CHAPTER IX.

OF THE ELECTRIC SPARK.

Chap. IX.

Spark discovered by Dr. Hall.

THE first person who seems to have perceived the electric light was Otto Guericke. But he has left nothing satisfactory on the subject. Dr. Hall, however, in the year 1708 observed that when pieces of amber are rubbed with flannel they give out a great deal of light, with a crackling noise. This light he compared to lightning. He observed the same thing with the diamond, and lac, and sealing wax, when rubbed. He found that all bodies which are capable of being excited by friction or *electrics* have the property of becoming luminous when rubbed. This friction excites electricity and gives them the property of attracting light bodies. Thus Dr. Hall recognised that when electrical bodies are excited by friction they emit light.[*] Mr. Hauksbee had indeed in 1705 observed the light emitted when mercury is agitated in a glass tube in vacuo, and several other similar phenomena. But he was not aware that electricity had any share in producing this light.[†] Very soon

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[*] Phil. Trans. xxvi. 69.

[†] Ibid. xxiv. 2129.

after the discovery of Dr. Hall we find Hauksbee describing the light evolved by the friction of glass and various other bodies, and it is obvious from his account of his experiments that he was aware that this light was connected with electricity.*

Positive spark a brush.

When a spark is drawn from a body electrified positively it has the form of a pencil of rays, or *brush,* or it forms a cone, the apex of which is in the body from which the spark proceeds. This brush was first described by Mr. Gray, and when originally observed it excited great surprise in him and his friends. When the spark is drawn from a body negatively electrified it has the appearance of a *star.* These two forms of the light serve very well to distinguish the two electricities. The *pencil* or *brush* characterizing positive, and the *star* negative electricity. No satisfactory explanation of the cause of this diversity can be offered. We know too little respecting the nature of the electric fluid and of light to be able even to offer a conjecture upon the subject.

Negative a star.

Electric the same as solar light.

When the electric light is examined by means of a prism, it is found composed of the same seven coloured rays as the solar light. But the proportion of the different rays, and of course the colour of the electric spark, varies according to circumstances.

When a given quantity of electricity occasions a spark by passing from one body to another, its

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* Phil. Trans. xxvi. 87.

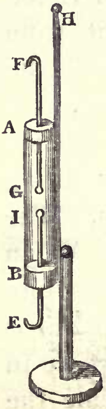
brilliancy is always the greater the smaller the size of the body from which it is drawn. Hence it happens that more brilliant sparks may be drawn from a small brass knob, fixed to the prime conductor of an electrical machine, than from the prime conductor itself. A short spark is always white; but a very long spark is usually reddish or rather purplish. When we draw a spark from the prime conductor of an electrical machine, by means of a metallic knob, the spark is white; but when we draw it by the hand it is purple. If we draw it by means of a wet plant, or water, or ice, the colour is red. The same spark will vary in colour according to its length. When short it is always white, when very long it is purple or violet. A spark which in the open air does not exceed a quarter of an inch in length will appear to fill the whole of an exhausted receiver, four inches wide, and eight inches long. In the former case it is white, in the latter the light is very feeble, and the colour violet.

Resembles the light of burning bodies.

Thus the electric spark, like the light from burning bodies, does not always contain all the rays which exist in solar light. When the spark is feeble, only the most refrangible rays appear; when it is strong all the rays appear together, rendering the light of the most dazzling whiteness. The electrical brush is always violet coloured. When a spark is passed through a torricellian vacuum, made without boiling the mercury, the colour is indigo. Now this is precisely what takes place in combustion. When the combustion is feeble the colour of the light is blue; but when it is strong the colour is white.

Probably owing to the condensation of air.

It is well known that electricity occasions a current in the air. This current, indeed, may be felt with the finger, and it has been frequently employed to produce very rapid motions in light bodies. A great variety of amusing exhibitions of this sort are usually shown by lecturers on electricity; most of which may be found described in the popular treatises on electricity, which are in every body's hands. It is evident from this and from the prodigious rapidity with which electricity moves, that when an electric spark passes through air, that fluid must be suddenly and enormously compressed. Now it has been shown in the first part of this work that when air is suddenly compressed sufficiently it gives out light, and that the colour of the light depends upon the rapidity and extent of the compression. This circumstance has led electricians to suspect that the light of the electric spark is not inherent in electricity; but merely the result of the compression of the air.



That air is violently and suddenly compressed when an electric spark is passed through it may be shown by the following contrivance, for which we are indebted to Mr. Kinnersley of Philadelphia. A B, is a glass tube about ten inches long, and nearly two inches in diameter. It is closed air tight at both ends by two brass caps. Through a hole in the upper cap passes the small glass tube H, open at both ends, and the bottom of it is plunged into a little water in the lower end of the tube A B. Through the middle of each of the brass caps a wire, F G and E I, is introduced, terminating each in a brass knob within the tube, and capable of sliding through the caps, so as to be placed at any distance from each other at pleasure. If the two knobs be brought into contact, and a Leyden jar be discharged through the wires the air within the tubes undergoes no change in its volume; but if the knobs G and I be placed at some distance from each other when the Leyden jar is discharged through the wires a spark passes from the one knob to the other. The consequence is a sudden rarefaction of the air in the tube shown by the water instantaneously rising to the top of the small tube, and then as suddenly subsiding to H. After which it gradually sinks again to the bottom of the tube as the air slowly recovers its original volume.

Only appears when electricity passes through elastic media.

The most valuable set of experiments which we possess on this interesting branch of electricity are those of Sir Humphrey Davy, published in the Philosophical Transactions for 1822. By boiling mercury for some time in a bent barometrical tube shut at one end through which a platinum wire passed, he contrived to form a torricellian vacuum quite free from air. But the vapour of mercury was quite sensible in it. In all cases when the mercurial vacuum was perfect, it was permeable to electricity, and was rendered luminous by either

Chap. IX. the common spark, or the shock from a Leyden jar, and the coated glass surrounding it became charged. But the degree of the intensity of this phenomenon depended upon the temperature. When the tube was very hot, the electric light appeared in the vapour of a bright green colour, and of great density. As the temperature diminished, it lost its vividness, and when it was artificially cooled to 20° below zero, it was so faint as to require considerable darkness to be perceptible. The charge likewise communicated to the tin or platinum foil was higher, the higher the temperature. This, like the other phenomena, must depend upon the different density of the mercurial vapour.

In all cases, when the minutest quantity of rare air was introduced into the mercurial vacuum, the electric light changed from green to sea green, and by increasing the quantity of air to blue and to purple—and when the temperature was low, the vacuum became a much better conductor.

A vacuum being made by means of fused tin, the electric light at temperatures below 0° was yellow, and of the palest phosphorescent kind, requiring almost absolute darkness to be perceived, and it was not perceptibly increased by heat.

Davy tried to form a vacuum above boiling olive oil, and above chloride of antimony, which boils at about 388°. The light produced by the electricity passing through the vapour of the chloride was much more brilliant than that produced by its passing through the vapour of oil—and in vapour of oil it was more brilliant than in vapour of mercury.

The light was pure white in the vapour of the chloride, and of a red inclining to purple in the vapour of the oil, and, in both cases, permanent elastic fluid was produced by its transmission.

When the temperature was diminished, the electrical light (transmitted through vapour of mercury) diminished also till the temperature was reduced to 20°. But between 20° and —20° it seemed stationary.

Unless the electrical machine was very active, no light was visible during the transmission of the electricity; but that the electricity passed was evident, from the luminous appearance of the rarefied air in other parts of the syphon. When the machine was in great activity there was a pale phosphorescent light above, and a spark on the mercury below, and brilliant light in the common vacuum. A Leyden jar weakly charged, could not be made to transmit its electricity by explosion through the cooled torricellian vacuum; but this electricity was slowly dissipated through it—and when strongly charged, the spark passed through nearly as much space as in common air, and with a light visible in the shade. At all temperatures below 200°, the mercurial vacuum was a much worse conductor than highly rarefied air—and when the tube containing it was included in the exhausted receiver, its temperature being about 50°, the spark passed through a distance six times greater in the Boylean than in the mercurial vacuum.

From these, and various similar experiments related by Davy, it seems demonstrated that electricity is capable of passing through a perfect vacuum, but that the light emitted, depends upon the vapour or air through which it passes, and that if the vacuum were perfect no light whatever would appear.

The appearance of the electric spark when it passes through water, seems to show that the light is visible only, when the electricity passes from one body to another, not by conduction, but through elastic media. For the electric spark may be easily made visible in water. But if we add a quantity of acid to the water, it is scarcely possible to make the spark visible in such a mixture. Now, the addition of an acid increases very materially the conducting power of water.

Supposed to be owing to the union of the two electricities.

It was an opinion maintained about twenty years ago, by many eminent experimenters in Germany, that the electric light is of the same nature with *fire*, and that it is formed by the union of the two electricities. This opinion appears to have been first stated by Winterl; and, unless I misunderstand Ritter, he seems to have entertained the same sentiments. But this opinion, though it appears at first sight plausible; and though it would be very convenient to be able to account so well for the analogy which obviously exists between fire and electricity, will not bear a rigid examination. Every person who has seen an electric spark, must be aware that the passage is so instantaneous that it is impossible to say from which point it proceeds, or to which it goes. If the spark be long, that is to say if the distance between the two knobs between which it passes

But erroneously.

be considerable, the presence of the two kinds of electricity may be at once observed. Suppose one of the knobs attached to the prime conductor of an electrical machine, and the other attached to a conducting body connected with the earth—the portion of the spark nearest the prime conductor of the machine exhibits all the characters which distinguish positive electricity—while the portion of the spark nearest the other knob, exhibits the characters of negative electricity. There can be no doubt, therefore, that every spark is composed of the two electricities. When two charged bodies are placed within the striking distance, no spark will pass unless the one body be charged with positive, and the other with negative electricity. The two electricities are attracted towards each other, advance at the same instant from each of the charged bodies, and uniting together somewhere between the two knobs, all symptoms of electricity are at an end. When a spark is short, the whole distance between the two knobs through which it passes, is equally illuminated. But when the spark is long, those portions of it which are next the knobs, are much brighter than towards the centre of the spark. Near the knobs the colour is white, but towards the centre of the spark purplish. Indeed, if the spark be very long, the middle part of it is not illuminated at all, or only very slightly. Now, this imperfectly illuminated part is obviously the spot where the two electricities unite, and it is in consequence of this union, that the light is so imperfect.

From all this, it is sufficiently obvious that both the electricities, when in an insulated state, are capable of giving out a brilliant light; consequently the electric spark cannot be owing to the two electricities combining together. Who ever has observed the passage of long sparks from one body to another, cannot but have remarked, that the place where the light is the least, or where there is an interval of no light, lies much nearer the negative than the positive knob. In general, about two-thirds of the spark consist of positive electricity, and about one-third of negative electricity. The cause of this difference between the length of the two electricities in such cases, we have no means of explaining.

CHAPTER X.

OF ELECTRICITY INDUCED BY HEAT.

Chap. X.

Heat renders bodies conductors.

It has been long known that heat produces considerable changes upon the conducting power of various bodies. Some of the best electrics become conductors, when heated sufficiently. Thus *glass* when heated to redness, becomes a conductor.[*] In like manner *resins* when melted become conductors. Baked wood at the common temperature, is a non-conductor, but when heated very hot, as when just taken out of the oven, it is a conductor. *Hot air* is also capable of conducting electricity. Dr. Priestley observed, that a charge could not be passed through melted tallow.[†] But there can be no doubt that it conducts much better than cold tallow. In this moist climate it becomes somewhat difficult to keep electrical machines in good order. Indeed in crowded rooms it is often impossible to obtain sparks from a cylindrical machine, or to charge a Leyden jar, without certain attentions, which never fail to obviate the difficulties. The cylinder of the electrical machine, and the uncoated part of the Leyden jar, should be rubbed over with

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[*] Priestley; Hist. of Electricity, p. 610.

[†] Ibid. p. 615.

tallow. This by preventing the glass from becoming damp or moist, allows us to collect electricity, and to charge a jar almost as well in a moist atmosphere, as in a dry. Suppose an electrical machine thus covered with a thin coating of tallow, and in good working order. Bring it near a fire and allow it to remain till the heat has melted the tallow. It will now be impossible to draw a single spark from it. Withdraw it from the fire, and allow it to remain till the tallow gets solid again. The machine will now be found in excellent working order. These facts seem to show that melted tallow acts upon the glass cylinder, precisely as moisture does, and of course that melted tallow is a much better conductor, than solid tallow.

Water is a conductor, and so is ice.* But, if we can put any confidence in the experiments of Achard, ice when cooled down to —13° becomes a non-conductor. Dr. Priestley found that snow is a much worse conductor than water.† But that may be owing to the air interposed among the particles of snow.

From these facts, and there are perhaps others that might be collected, it would appear that all bodies become conductors, when sufficiently heated. And perhaps all bodies would become non-conductors, if we could lower their temperature sufficiently.

But heat is not merely capable of altering the conducting power of bodies; there are some bodies, which, when heated, become charged with electricity,

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* Priestley; Hist. of Electricity, p. 609.

† Ibid. p. 610.

both positive and negative, and which when thus excited, retain their charge for a considerable time. The most remarkable of these bodies is the *tourmalin*.

The tourmalin.

The tourmalin is a hard crystallized mineral, which occurs in granite, and mica slate. It has been observed also imbedded in dolomite, at St. Gothard. The primary form of the crystal is an obtuse rhomboid, but it occurs most commonly crystallized in three, six, or nine sided prisms, terminated sometimes by three sided, sometimes by six sided pyramids, and sometimes by other terminations, which are minutely described by crystallographers.

This mineral seems to have been known to the ancients, and is mentioned by Theophrastus, under the name of lyncurium (λυγκυριον).* The first modern writer who notices it is Lemery, who had obtained a specimen from Ceylon, which he exhibited to the members of the French Academy under the name of a Magnet.† He notices, though rather inaccurately, the property which it has of attracting light substances to it. It was employed by jewellers for ornamental purposes, and they observed that when heated, it acquired the property of attracting light bodies as ashes to itself; on that account it was called by the Dutch *aschentrecher*, and by the Germans *aschenzieher*, (*attractor of ashes.*) Æpinus having been informed of this property by Lehman, naturally referred it to electricity. He was fortunate enough to procure a tourmalin of considerable size, which enabled him to investigate

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* Περι λιθων, c. 50.

† Hist. de l'Academie Royal, 1717, p. 7.

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the electrical phenomena exhibited by this curious mineral. The result of his observations was published in the Memoirs of the Berlin Academy for 1756.* Though Æpinus discovered the electrical laws which characterize the tourmalin, yet as the stones which he employed were not in their natural state, but had been cut by the jeweller, it will be better to state what takes place when we make use of a tourmalin in its natural state.†

Becomes charged by heating it.

Let us suppose a tourmalin in the form of a nine sided prism, terminated at one extremity by a three sided, and at the other by a six sided summit. This tourmalin is to be grasped about the middle by a pair of fine pincers, and then plunged into boiling water or hot mercury, and kept in it for some minutes, till it has acquired the temperature of the liquid. When taken out of the water, present one of its ends to a small electrometer already charged with positive or negative electricity, one of the ends of the prism will attract the electrometer, while the other will repel it; showing that its two extremities are in different states of electricity, the summit terminating in six facets is charged with positive electricity, while the summit with three facets is charged with negative electricity. If the electrometer be sufficiently sensible, we shall find that both the electricities diminish rapidly as we pass from the extremity of the prism

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* Collection Academique, xiii. p. 247.

† The electrical properties of the tourmalin have been lately investigated with much address by M. Bequerel. See An. de Chim. et de Phys. xxxvi. 1.

towards its centre, and that the central portion of the prism is neutral. Mr. Canton ascertained that these electric properties only developed themselves in the tourmalin, when the temperature was changing. This has been confirmed by Becquerel. When the tourmalin was at 212°, the electricity was appreciable. It increased rapidly while the stone cooled down to 156°, when it was at its maximum. From 156° to 104° it remained nearly stationary, and sank rapidly from 104° to 70°, when it was nearly the same as at 212°. If we break the tourmalin in two while in this state of excitement, each of the fragments will possess precisely the electrical properties of the whole prism. This fact was discovered by Canton. It is said also that if the tourmalin be very long, it does not acquire electrical properties when heated; but this is an experiment which I have never had an opportunity of trying.

Electricity only appears when the temperature is changing.

Other phenomena.

From the experiments of Æpinus, it appears that the stone in order to exhibit the electrical phenomena just described, must be equally heated in every part. When unequally heated, the electricity appears, but the poles are reversed. However, if we deposite the tourmalin upon an electric, it soon assumes its natural state, the six sided summit becoming positive, and the three sided summit negative. Becquerel has ascertained that if the positive extremity of a tourmalin be heated, while the negative extremity is kept cold, negative electricity is developed in the positive end, while the negative end remains at zero; and *vice versa.*

When the tourmalin is once excited by heat, it retains its electricity for a long time, if care be taken to place it upon non-conductors. Æpinus has found it electric after an interval of six hours.*

Thus the tourmalin possesses the characters of a charged Leyden phial, one of its poles being positive, the other negative. Æpinus showed that if these poles be coated with tinfoil, they may be discharged precisely in the same way as a Leyden phial. The great difference between the charged tourmalin and the Leyden phial, is the method of charging. In the tourmalin, in order to produce a charge, we have nothing more to do than to apply equable heat. Can any explanation of this curious mode of charging be offered?

Explanation of these phenomena.

If we melt sulphur in an iron cup, and allow it to cool after insulating the cup, we shall find that the sulphur is charged with negative electricity, and the iron with positive. Something similar to this must take place in the tourmalin. It must be composed of a number of elements similar, as far as their electrical properties are concerned, to the sulphur and iron: and it can be shown that if we suppose the tourmalin so constituted, (since the stone is a non-conductor,) heat would produce the very electrical phenomena which distinguish and have given celebrity to this mineral.

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* These facts, as stated by Æpinus, if accurate, seem inconsistent with the statement of Canton and Becquerel, that the electricity is only developed while the stone is changing its temperature.

Suppose we have a number of glass plates coated at the centre on each side with a piece of tinfoil, while the rest of the disc is covered with varnish to prevent the bad effects of moisture upon the glass. Let these plates be placed parallel to each other upon a non-conducting body about an inch asunder, and let the contiguous plates communicate with each other by means of a slip of tinfoil pasted on. From the last glass plate a chain passes communicating with the ground, while the first plate communicates with the prime conductor of an electrical machine. By means of this machine let the plates be charged with electricity. Let the apparatus be now insulated and examined. It will be found similar to the tourmalin. The end next the electrical machine will be charged with positive electricity, the end farthest off will be charged with negative electricity, while the central portion will be neutral. Indeed, neither the positive nor negative electricity extends sensibly beyond the 4th plate from either extremity. The 24 middle plates are sensibly neutral. If this apparatus, while thus charged, were broken in two, we should find that both the halves possessed exactly the characters of the whole pile. The two extremities of each half would be charged with positive and negative electricity, while the central portion would be neutral. Such a pile, therefore, represents precisely the state of the excited tourmalin; and therefore serves to throw light upon the nature of that stone.

The tourmalin is not the only mineral which acquires electricity by heat. There are several others, some of the most remarkable of which it will be proper to enumerate.

Boracite similar to the tourmalin in its electric properties.

There is a hill of sulphate of lime, called Kalkberg, situated near Lunebourg, in the dutchy of Brunswick, in which small cubic crystals are found. These cubes are white, have a specific gravity of 2·566, and are composed of two atoms of boracic acid combined with one atom of magnesia. They are distinguished among mineralogists by the name of *boracite*. If we examine the cubic crystals of boracite we shall find that only four of the solid angles are complete, constituting alternate angles placed at the extremity of two opposite diagonals at the upper and lower surface of the cube. The other four solid angles are replaced by small equilateral triangles. When the boracite is heated all the perfect solid angles become charged with *negative* electricity, while all the angles replaced by equilateral triangles become charged with *positive* electricity. So that the boracite has eight poles; four positive and four negative. These are obviously the extremities of four diagonals connecting the solid angles with each other. One extremity of each of these diagonals is charged with positive, and the other extremity with negative electricity. In general the electricity of boracite is not so strong as that of the tourmalin. This curious law of the excitability of the boracite and of its eight poles was discovered by Hauy in 1791.*

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* Hauy's Mineralogie, 260, 2d Edition.

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Axinite, mesotype, and the *silicate of zinc,* are also minerals, which become electric when heated, and which, like the tourmalin, exhibit two opposite poles, the one positive, the other negative. It is not every crystal of axinite and mesotype which possesses this property; but those only which are destitute of symmetry; that is to say, one of whose extremities is shaped differently from the other. No doubt this remark applies also to the silicate of zinc; though the crystals of that mineral being usually asicular it is not so easy to determine by observation the degree of symmetry which they may possess.

The *topaz, prehnite,* and the titaniferous mineral called *sphene,* are also capable of being excited by heat, and have two opposite poles like those already mentioned.

CHAPTER XI.

OF ELECTRICITY BY PRESSURE.

Chap. XI. It is natural to expect that the dilatation and compression of the particles constituting the surfaces of bodies will have an effect upon the development of their electricity. This opinion was confirmed by an observation of Hauy, that Iceland spar and several other crystallized minerals, become electric by simple pressure between the fingers. He found the same property in several other minerals, though not so much marked as in Iceland spar. The topaz, euclase, arragonite, fluor spar, carbonate of lead, and rock crystal, give signs of electricity when pressed.* M. Libes fixed an insulating handle to a metal disc, and pressed it (holding it by the handle) against a piece of gummed taffeta. The taffeta acquired positive electricity, while the metallic disc acquired negative electricity.† The effect increases with the pressure; but it ceases altogether as soon as the taffeta loses its glutinosity, which renders it easily compressible.

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* Hauy; Mineralogie; i. 188, 2d edition.

† Ann. de Chim. et de Phys. xxii. 5.

When two elastic bodies are pressed they become differently excited.

This subject has lately engaged the attention of M. Becquerel, who has made a vast number of experiments on different bodies, and has drawn this general conclusion from them all.—" When two substances, of what nature soever, provided one of them be elastic, are insulated, and pressed against each other—one of them becomes charged with positive, and the other with negative electricity. But when the compression is removed, these two different states cannot be observed, unless one of the bodies, at least, be a bad conductor of electricity."

The mode chosen by Becquerel to obtain these results, was to form the substances to be tried, into small discs, about one-tenth of an inch thick, to fix each to a varnished glass rod by way of handle—to take one of these handles in each hand, and squeeze the two discs together. After separating them, each disc is to be presented to a delicate electrometer. A single pressure is sufficient to repel the small disc of Coulomb's electrometer. But by repeating the contacts any electrometer whatever may be charged. Suppose, for example, two insulated discs, the one of *cork*, the other of *caoutchouc*. After the pressure, the cork has become positive, and the caoutchouc negative. When cork is pressed against *the skin of an orange*, it becomes positive, and the orange skin negative. When orange skin is pressed against caoutchouc it becomes positive, while the caoutchouc becomes negative.

Iceland spar, sulphate of lime, fluor spar, sulphate of barytes, when pressed against cork, become

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positively electrified, while the cork becomes negatively. But cyanite and retinasphaltum, when pressed against cork, acquire negative electricity, the cork becoming positive. In like manner, pitcoal, amber, copper, zinc, silver, &c. when pressed against an insulated disc of cork, become negative.

But they must be insulated.

When one only of the discs is insulated while the other communicates with the ground, the insulated body will have acquired after pressure, the electricity belonging to it; but the non-insulated body will exhibit no signs of electricity whatever. Thus an insulated disc of cork pressed against Iceland spar, selenite, fluor spar, &c. acquires negative electricity; but when pressed against copper, zinc, and other similar substances, it acquires positive electricity. Even fruits, the orange for example, when gently pressed against an insulated disc of cork, communicate an excess of positive electricity. In proportion as the fruit dries, the electric properties of the cork diminish. When ripeness has given the orange all the elasticity of which it is susceptible, and before the surface becomes moist from decomposition, then the electricity from pressure is the greatest possible.

Insulated cork pressed against any part of the animal body, provided it be not moist, receives an excess of negative electricity. The hair and down of animals produce nearly as much electricity as Iceland crystal would do; but of the contrary kind.

The imperfect liquids, when compressible, give analogous results. Cork pressed lightly against inspissated oil of turpentine, acquires negative electricity.

It is not necessary that the substances pressed against each other be of different natures; when two discs composed of the same materials, as skin, amadou, &c. are pressed against each other, the one acquires negative, and the other positive electricity.

Electricity thus evolved continues a long time.

When electricity is thus evolved in bodies by pressure, it is preserved for a considerable time, provided the bodies be non-conductors. Hauy found that Iceland spar still gave signs of electricity after an interval of eleven days. Sulphate of barytes of Royat, unless it be well insulated, parts with the electricity evolved in it almost immediately. But an insulated crystal of it exhibited signs of electricity after an interval of half an hour. The time during which the electrical properties are preserved, is probably inversely as the conducting power of the electrified body. This preservation of electricity continues even though the body be surrounded by moist air, or even though it be in contact with moist substances. It would appear from this, that bodies rendered electric by pressure, are in the same circumstances with a charged Leyden phial. The internal surface of the body is probably charged with the opposite electricity of the external surface; and the action of these opposite electricities on each other, prevents that on the external surface from being dissipated.

Electricity appears only when the bodies are separated.

As long as the two bodies pressed against each other are in contact, neither of them gives any symptoms of electricity. The better conductors they are, the more rapidly they must be separated from each other, after pressure, in order that each may retain the peculiar kind of electricity which has been accumulated in it by the pressure. When a disc of cork is pressed against an orange, if we separate the two substances rapidly, the cork will be found to possess a considerable excess of positive electricity. This excess diminishes the more slowly the two substances are separated from each other, and when we separate them very slowly, all electrical symptoms disappear.

It would appear from all this, that when two surfaces are pressed against each other, one of them always acquires an excess of positive electricity, while the other acquires an excess of negative electricity. These two opposite electricities just neutralize each other. Now, it is a curious and inexplicable circumstance, that as long as the pressure is kept up, even though the two bodies pressed against each other be good conductors, the two electricities, though attracted towards each other, never actually combine. But whenever the pressure is withdrawn, unless the bodies be instantly separated from each other, the two electricities combine and neutralize each other completely, unless in the case when at least one of the pressed bodies is a non-conductor.

Heat plays a considerable part in these developments.

There can be no doubt that heat plays a considerable part in the development of electricity by pressure. It has been long known, that the higher the temperature of any body is raised, the greater tendency has it to acquire negative electricity when rubbed against any other body. In the same manner, when we continue to elevate the temperature of Iceland spar, we at last bring it into such a state, that it acquires negative electricity when pressed against a disc of cork. If we take a piece of well dried cork, and cut it in two by means of a very sharp knife, and then press the two cut surfaces against each other, it frequently happens that however hard the pressure be, and however rapidly we separate the two surfaces, neither exhibits any signs of electricity after the separation. But if we slightly heat one of the pieces of cork, by holding it near the flame of a candle, and renew the pressure, we shall find each surface possessed of a different kind of electricity. Two pieces of Iceland spar, of the same temperature, do not become electric by pressure, but if one of them be made hotter than the other, the electricities are immediately evolved. It is probable from this, that when two discs of the same substance are pressed against each other, it is necessary that their temperatures should differ, otherways no electricity will be evolved.

The intensity of the electricity proportional to the pressure.

It would appear from the experiments of M. Becquerel that the intensity of electricity, measured by Coulomb's balance, is proportional to the pressure. That is to say, that a double pressure will produce a double intensity, a triple pressure a triple intensity, and so on. The following tables exhibit the intensities of electricity evolved by the pressure of various bodies against each other. The intensity of pressure was measured by the weight attached to one end of a balance forcing up the other end which produced the pressure.*

Cork pressed against Iceland spar.

Pressures.	Intensity of electricity.
1	1·5
2	3·4
3	5·6
4	6

Cork pressed against polished sulphate of barytes crystals.

Pressures.	Intensity of electricity.
1	1·05
2	2·1
3	3·1
4	4·2
6	6·3

Cork against polished rock crystal.

Pressure.	Intensity of electricity.
4	3·9

Cork against sulphate of lime.

Pressure.	Intensity of electricity.
4	1.9

These tables show also the difference in the absolute quantity of electricity evolved by the same extent of pressure applied to different bodies.†

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* A delailed account of the apparatus illustrated by figures, may be seen in the Ann. de Chim. et de Phys. xxii. 21.

† Ann. de Chim. et de Phys. xxii. 5.

CHAPTER XII.

OF ELECTRICITY BY CONTACT.

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This branch of electricity originated from a dispute between Galvani and Volta, about the year 1790. Galvani accidentally discovered, that when the hind legs of frogs, deprived of their skin; but having the lumbar nerve attached to them and laid bare, are placed upon pieces of glass, the lumbar nerve being previously enveloped in a piece of copper foil; if we take a piece of zinc, and placing one end of it on the muscles of the leg, we bring the other in contact with the copper foil, the muscles of the limb are thrown into violent convulsions. These convulsions are renewed whenever the metals are brought into contact for a certain time after death. They cease when the irritability of the muscles is exhausted, and this takes place the sooner the oftener the convulsions are produced by bringing the two metals in contact. Galvani ascribed these convulsions to an *animal electricity*, which in his opinion exists in all animals, and is the cause of muscular motion. When Volta repeated the experiment, he was struck with the necessity of two metals for the success of the experiment. He was

Convulsions in the muscles of a frog.

Ascribed to animal electricity.

To common electricity.

induced, in consequence of this, to ascribe the phenomenon to common electricity, and to affirm that when two metals are placed in contact with each other, one acquires vitreous, and the other resinous electricity. To verify this opinion, he prepared two circular polished plates, the one of copper, and the other of zinc. To the centre of each of these, was cemented a varnished glass handle. The two plates were brought into contact by means of these handles, and separated so as to preserve a parallel position with respect to each other. These were made to touch the plates of a condenser, the contact being renewed several times, after touching each plate to restore it to a neutral state. It was found that the zinc possessed an excess of positive electricity, while the copper possessed an excess of negative. Galvani did not accede to this opinion of Volta. He repeated his experiments, and succeeded in producing the convulsions by means of a single metal; but only when the energy of the limb was very great. Nay, in this last case, he succeeded in producing the convulsions, simply by bringing other animal matters in contact with the lumbar nerve, and the crural muscles of the frog. But Volta answered these objections, by showing that the smallest quantity of any foreign metal sufficed to enable a single metallic plate to produce the convulsions.

Two insulated metals brought into contact and separated are in different electric states.

When zinc is brought into contact with copper or silver, and again separated by means of an insulating handle, the zinc is found positive, and the copper or silver negative. It is with these metals that the different states of electricity after contact may be most easily perceived. If *iron*, or *manganese*, or even *plumbago*, be substituted for the zinc plate, the result is the same. I presume that if *cadmium* were substituted for zinc it would also be found positive after the separation of the metallic plates. Gold, platinum, palladium, or even mercury, may be substituted for the copper or silver. When tin or lead is substituted for copper, the different states of the metals is not easily recognised, but in this case also the zinc is positive, and the tin or the lead negative. Indeed there is no reason to doubt that no two substances exist in nature which would not acquire the opposite electrical state if placed in contact and then separated again. Though with respect to many of them the differences probably would be too small to be recognised even by means of a condenser.

A considerable number of experiments on the electricity by contact was made by Sir H. Davy. When oxalic, succinic, benzoic, or boracic acid, perfectly dry, either in powder or in crystals, are touched upon an extended surface with a plate of copper insulated by a glass handle, the *copper* becomes *positive*, and the *acids negative*. When zinc or tin is substituted for copper the effect is the same. Phosphoric acid perfectly dry, when applied to copper becomes negative, and the copper positive. When metallic plates are made to touch dry lime, strontian, barytes, or magnesia, these alkaline bodies become *positive*, the metal negative. With soda the effect is the same. Potash attracts moisture so rapidly that the experiment cannot be

Chap. XII.

tried with it. When sulphur is applied to polished lead, or to any other metal, it becomes positive.*

Elements of bodies decomposed by electricity either positive or negative.

It has been ascertained that electricity, when employed in the way to be described hereafter, and known by the name of the Voltaic battery, is capable of decomposing all compound bodies, and the decomposition takes place according to a particular law. When two platinum wires attached to the two poles of the battery is plunged into a vessel of water, the water is reduced into its elements, and the *oxygen* is always extricated from the wire attached to the *positive* pole, while the hydrogen rises from the wire attached to the *negative* pole. When the wires are plunged into a strong solution of muriatic acid, the chlorine is accumulated round the positive wire, and the hydrogen round the negative. The law according to which *hydrobromic* and *hydriodic* acids are decomposed is the same. The bromine and iodine are attracted to the positive pole, while the hydrogen is attracted to the negative pole. When chloride of sodium or potassium is decomposed, the chlorine passes to the positive pole, while the sodium and potassium pass to the negative pole. Were a compound of sulphur with oxygen to be decomposed, the oxygen would attach itself to the positive pole, while the sulphur would attach itself to the negative pole. But with sulphuret of zinc or of iron the case would be different. The sulphur would collect round the positive pole, and the metal round the negative

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* Phil. Trans. 1807, p. 34.

pole. When salts are decomposed, the acid is attracted to the positive pole, and the base to the negative.

Now as bodies are attracted by those in a different state of excitement from themselves, it follows that oxygen, chlorine, bromine, and iodine, and acids, would not be attracted to the positive pole, unless they themselves were in a negative state; nor would hydrogen and bases be attracted to the negative pole unless they were in a positive state. From this it has been concluded that bodies which have an attraction for each other are in opposite states of electricity, and that it is to these opposite states that their attraction for each other, and their union with each other, is owing. The current of electricity destroys their union by bringing them into the same electrical state. In consequence of this view, which is at least exceedingly ingenious and plausible, bodies have been divided into two sets, those which are negative, and those which are positive. The following table exhibits a list of the negative bodies beginning with those which possess the negative property in the highest degree, and terminating with those in which it is lowest.

Table of negative bodies.

Oxygen,
Chlorine,
Bromine,
Iodine,
Sulphur,
Phosphorus,
Selenium,
Arsenic,
Molybdenum,
Chromium,
Tungsten,
Boron,
Carbon,
Antimony,
Tellurium,
Columbium,

Titanium,
Silicon.

The following table exhibits a list of the positive bodies beginning with the one in which the property is weakest, and ending with the one in which it is strongest.

Table of positive bodies.

Gold,
Platinum,
Palladium,
Osmium ?
Iridium,
Rhodium,
Mercury,
Silver,
Copper,
Nickel,
Cobalt,
Bismuth,
Tin,
Zirconium,
Lead,
Cerium,
Uranium ?
Iron,
Cadmium,
Zinc,
Manganese,
Aluminum,
Thorinum,
Yttrium,
Glucinum,
Magnesium,
Calcium,
Strontium,
Barium,
Lithium,
Sodium,
Potassium.

It is not easy to decide where hydrogen should be placed. Compared with oxygen it is strongly positive. But it combines with the potassium, and must with respect to it be negative. The bodies nearest the head of the first list being most powerfully negative, and those nearest the bottom of the second list being most powerfully negative, have the greatest chemical affinity for each other. Bodies in the same list have but little affinity for each other, those towards the bottom of the first list have but little affinity with those towards the top of the second list. However the bodies in the same list are not destitute of affinity for each. Thus sulphur combines readily with arsenic. Because these two bodies assume different states with respect to each other. When we decompose sulphuret of arsenic, the sulphur is attracted to the positive pole, and the arsenic to the negative; showing that the former is in a negative state, and the latter in a positive. It is for this reason that almost all the substances constituting the first list, are capable of uniting with each other as well as with those of the second. Now it deserves attention, that when the bodies constituting the first list unite with each other, they constitute *acids* or substances which act the part of acids; when they combine with the substances constituting the second list, they constitute *bases* or substances which act the part of bases. All the acids are combinations of the negative bodies with each other, all the bases are compounds of the negative bodies with the positive. I have left out azote because it is not easy to say where it ought to stand; but it belongs undoubtedly to the class of negative bodies, and should stand probably before *sulphur*.

Hydrogen I have purposely omitted. It constitutes *acids* by combining with the greater number of the negative bodies; but we are ignorant at present of any compound which it forms with any of the positive bodies, excepting with potassium, which according to the experiments of Gay-Lussac and Thenard, absorbs hydrogen gas and forms a compound. But this combination has not succeeded in the hands of other experimenters.

CHAPTER XIII.

OF ELECTRICITY BY CHEMICAL COMBINATIONS.

Ch. XIII. Ever since the discovery of the identity of lightning and electricity, the attention of electricians has been turned to the accumulation of electricity in the atmosphere. And various causes for such an accumulation have been assigned. The opinion of Volta has been most commonly adopted. According to him whenever a body changes its state, it becomes electric. Now water is continually ascending into the atmosphere in the state of vapour, or falling from it in the state of rain. By these continual changes of state, which this fluid undergoes, Volta supposed that the accumulation of electricity in the atmosphere was chiefly produced. This opinion was verified by Lavoisier and Laplace. But when Saussure repeated the experiments, he was unable to obtain any satisfactory results. M. Pouillet has recently examined the subject with much care, and has found that no sensible quantity of electricity is evolved when water changes its state, unless at the same time some chemical action more or less vigorous accompanies the change. But whenever gaseous two bodies unite with each other, or a gaseous body with a solid body, one of

Evaporation.

Does not evolve electricity.

the uniting bodies always gives out positive electricity, and the other negative electricity.* These experiments being of great importance, both for understanding the sources of atmospherical electricity, and for determining the kind of electricity possessed by those bodies which have a chemical affinity for each other, it will be worth while to state them somewhat particularly. When charcoal is burnt it sometimes gives out positive, and sometimes negative electricity, and sometimes no electricity at all. This depends upon the way in which the combustion is conducted. To obtain constant results, M. Pouillet proceeded in the following manner. He took a piece of charcoal of such a diameter that it could receive the form of a cylinder whose bases were nearly plain. This piece of charcoal was placed vertically, two inches and a half, or three inches below a plate of brass which rests upon one of the discs of the condenser. The charcoal communicated with the ground, and was lighted at its superior base, taking care that the fire did not reach the lateral surface. A current of carbonic acid rises and strikes against the plate, and in a few minutes the condenser is charged. The electricity which it receives from the carbonic acid gas is always *positive*. If the plane be allowed to communicate to the sides of the charcoal, or if it be inclined so that the carbonic acid formed must slide up along the base of the charcoal, no sensible effect is obtained.

Electricity evolved by combustion of charcoal.

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* Ann. de Chim. et de Phys. xxxv. 401.

To obtain the electricity which the charcoal itself takes by combustion, M. Pouillet placed its inferior end directly upon the disc of the condenser, and then lighted its superior base. In a few minutes the condenser was charged with negative electricity. From these experiments, we learn that when charcoal is burnt, it becomes charged with negative electricity, while the carbonic acid evolved is charged with positive electricity. Now the combustion of charcoal is the combination of it with oxygen, so as to constitute carbonic acid. According to Pouillet, during this combination, the oxygen gives out positive electricity, which is found in the carbonic acid gas, while the charcoal gives out negative electricity, which is found in the portion of the charcoal not yet burned. Now when the carbonic acid gas is again decomposed into its elements, the oxygen takes back positive electricity, and the carbon negative electricity. Is not this the reason, why the oxygen gas is attracted towards the positive pole of the Voltaic battery, while the carbon is attracted to the negative pole?

And hydrogen.

The flame of hydrogen gave contradictory results with respect to its electricity, as had been the case also at first with charcoal. In the course of a few minutes it gave indications both of positive and negative electricity; very intense and very weak indications, and often it was impossible to obtain any indication at all. But these difficulties were at length overcome by M. Pouillet in the following manner.

The hydrogen gas was made to flow out of a glass tube. The flame was vertical, having a breadth of 4 or 5 lines, and a height of about three inches. A coil of platinum wire was employed to conduct the electricity from the flame to the condenser. When this coil was so much larger than the flame as to enclose it, and to be distant from its external surface about 4 inches, signs of positive electricity make their appearance. These signs become more and more intense as the distance diminishes. But when the coil becomes so small as to touch the flame, the electrical signs become weak and uncertain. Thus it appears that round the flame of hydrogen, there is a sort of atmosphere at least 4 inches in thickness, which is always charged with positive electricity.

If a very small coil of platinum wire be placed in the centre of the flame, in such a manner, that it is enveloped on all sides; and made to communicate with the condenser, that instrument becomes immediately charged with negative electricity. Thus it appears that the outside of the flame of hydrogen gas is always charged with positive electricity, and the inside with negative electricity. It follows from this, that there is a layer of the flame where the electricity is insensible. Accordingly if we regulate the coil in such a manner that it penetrates nearly one-half into the bright part of the flame, all electrical indications disappear.

From these experiments, it appears that the electricity evolved during the combustion of hydrogen, is quite similar to what appears during the combustion of charcoal. The oxygen before it enters into combination is charged with positive, and the hydrogen with negative electricity. Oxygen then must part with positive electricity when it combines with hydrogen, and hydrogen must part with negative electricity when it combines with oxygen.

If instead of making the hydrogen gas flow out of a glass tube, we make it flow out by a tube of metal, which does not communicate with the ground, but only with the condenser, this metal tube which touches the hydrogen, but not the flame, always becomes charged with negative electricity. But if it communicate with the ground it loses the negative electricity, which it had lately carried to the condenser, and the product of the combustion preserves an excess of positive electricity.

If we examine at a height of a few inches above the vertical flame, we find both the electricities in the same quantity and not recomposed. For if we present a soldered plate of zinc and copper, the zinc plate attracts the negative, and the copper plate the positive electricity. When we go to a distance sufficiently great above the vertical flame, the electrical fluids can no longer be recognised, because they have combined and neutralized each other.

And of other bodies.

Pouillet examined the flames of alcohol, ether, wax, oils, fat, and many vegetable bodies. The flames of all these bodies presented the same phenomena as that of hydrogen. That is to say, that a zone of air surrounding the flame was electrified plus, while the interior of the flame was electrified minus. All these combustions exhibit examples of oxygen uniting with hydrogen and carbon. The oxygen gives out positive electricity, while the combustible body, whether hydrogen or carbon, or a compound of the two, gives out negative electricity.

It has been ascertained by the experiments of Priestley, Ingenhousz, Sennebier, Saussure, &c. that plants while vegetating act upon atmospherical air; sometimes forming a great quantity of carbonic acid which disengages itself insensibly, and sometimes on the other hand giving out oxygen gas in a state of greater or less purity. Now it appears from the preceding experiments, that when carbonic acid is formed by combustion, it is electrified plus. This led M. Pouillet to suspect that the carbonic acid given out during the processes of vegetation, would be in the same predicament. To determine this point, he made the following experiment.

And by vegetation.

Twelve glass capsules, about 8 inches in diameter, were coated externally for two inches round the lips with a film of lac varnish. They were arranged in two rows beside each other, either by placing them simply on a table of very dry wood, or by putting them on a table previously varnished by gum lac. They were filled with vegetable mould, and were made to communicate with each other by metallic wires which passed from the inside of the one to the inside of the other, going over the edges of the capsules. Thus the insides of the 12 capsules, and the soil which they contained, formed only a single conducting body. One of these capsules was placed in communication with the upper plate of a condenser by means of a brass wire; while at the same time the under plate was in communication with the ground. Things being in this situation, and the weather very dry, a quantity of corn was sown in the soil contained in the capsules, and the effects were watched. The laboratory was carefully shut, and neither fire, nor light, nor any electrified body, was introduced into it.

During the two first days the grains swelled, and the plumula issued out about the length of a line, but did not yet make its appearance above the surface of the earth. But on the third day the blade appeared above the surface, and began to incline to the window which was not provided with shutters. The condenser was now charged with negative electricity. Consequently the carbonic acid gas, which disengages itself during the germination of seeds, is charged with positive electricity, and is therefore precisely in the same state as the carbonic acid gas formed by combustion. This experiment was several times repeated with success. But the electricity cannot be recognised unless the weather be exceedingly dry, or unless we dry the apartment artificially by introducing substances which have the property of absorbing moisture.

These capsules being insulated, and the air being very dry, and the soil so dry that it is an imperfect conductor, it is evident that the electricity would be retained. Accordingly when the condenser was brought into a natural state after one observation, and if it was then replaced for experiment only during one second, it was found to be charged with electricity.

It is obvious enough that the gaseous fluids given out by plants during the processes of vegetation, being charged with electricity, must contribute to furnish no inconsiderable portion of the electricity with which the atmosphere becomes loaded. No doubt the carbonic acid gas evolved from animals by respiration is also charged with positive electricity; though it would be somewhat difficult to determine the point by actually charging a condenser in consequence of the moisture with which the expired air is always loaded.*

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* The reader may consult with advantage an elaborate and most ingenious set of experiments by Sir H. Davy, to establish his own views, which are rather inconsistent with those of Pouillet, in Phil. Trans. 1826, p. 398.

CHAPTER XIV.

OF GALVANISM.

Ch. XIV.

Electricity by contact of two metals.

If we take a number of thin plates of glass, and cover the under surface of each with a thin coating of zinc, and the upper surface with an equally thin coating of silver or copper, and if we place these discs above each other in a determinate order; namely, the zinc side of each always undermost, and the silver or copper side always uppermost, slight electrical effects will be produced, which increase in intensity as we augment the number of discs of glass. When two of these discs are laid upon each other the silver coating will be electrified minus, and the zinc coating plus. The undermost side of the second glass disc will, therefore, be in a state of positive excitement. It will therefore expel positive electricity from, and attract negative electricity to, the upper side of the disc which is coated with silver. Suppose, now, another glass disc similarly coated to be applied. The silver side of the second disc is minus before the application of the new disc. The zinc coating, in consequence, will become more positive than the zinc coating of the second disc. Hence the upper side of this third disc will be more negative than was that of the upper side of the second disc. It is easy to see how the charge increases in intensity with the number of discs. The apparatus is analogous to the electrophorus or the condenser. The quantity of electricity thus disengaged is extremely small, and it does not become sensible till the discs of glass amount to thirty or forty. The opposite sides of every glass are in different electrical states, and the quantity of disengaged electricity in each increases with their number. If we bring the two sides of each disc in contact by means of a wire, the equilibrium is instantly restored, and some time elapses before the discs become again charged.*

Pile by two metals and paper.

If instead of glass, which is a non-conductor of electricity, we employ an imperfect conductor, as a leaf of paper, similar electrical phenomena make their appearance. Suppose we take round pieces of paper of the size of half a crown, silvered on the one side by the application of very thin zinc leaf and gilt on the other by means of Dutch leaf, and pile them above each other so that the silvered side of each shall be always undermost, we obtain an apparatus which will exhibit electrical phenomena. The method is to put the paper discs within a glass cylinder, and, at least, a couple of thousand paper discs must be employed, so as to fill the glass cylinder completely from top to bottom. The extremities of the glass cylinder are shut by plates

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* See Jaeger's experiments with this pile, Gilbert's Annalen, xlix. 64.

of brass directly in contact with the paper discs, and provided with metallic buttons. The button at the zinc end of the cylinder is always in the state of positive electricity, while the button at the copper end of the cylinder is in the state of negative electricity. This kind of apparatus was first constructed by De Luc, and afterwards by Zamboni, and they have been constructed in different ways; though, as the theory is the same in all, it seems unnecessary here to enter into minute details.

The paper in these piles takes the place of the discs of glass in the former pile. Now as paper is an imperfect conductor, if we discharge the electricity, by bringing the top and bottom of the pile in contact by means of a wire, the charge is very speedily renewed when the wire is withdrawn. Two of these piles are usually placed at a small distance from each other, and so that the positive pole of the one corresponds with the negative pole of the other. A very light and insulated needle suspended in equilibrium upon a very moveable axis between these two poles, oscillates continually between them. The piles discharge their electricity by means of this needle, which is in consequence attracted alternately by the one and the other. De Luc placed a small bell between the two piles, which was struck by small metal balls suspended between the pole of the piles and the bell. These small balls, being alternately attracted and repelled, kept striking the bell, which thus continued for a long time continually ringing. Sometimes it stopped suddenly; but after a certain interval it began again, and the bell kept ringing as before.

These piles continue to act for several months; but they finally lose their activity; owing probably to a change induced into the state of the metallic coatings of the paper. Even before they finally become inactive, their intensity varies with the temperature, and the moisture or dryness of the air.

When the paper of which these piles are constructed is made perfectly dry, their intensity is very much diminished, obviously because dry paper is a non-conductor of electricity. The consequence is, that when the electricity is discharged, a considerable time elapses before it accumulates again. But Dr. Jaeger found that when the temperature was raised to 104°, or as high as 140°, the pile begins again to act as well as ever.* We must conclude from this, that dry paper while cold, is a non-conductor of electricity, but that it becomes again a conductor, when heated up to 104° or 140°.†

Volta's pile.

If, instead of solid non-conductors, or demi-conductors, we employ liquid bodies which conduct electricity, the energy of the pile is prodigiously increased. A pile constructed in this way, constitutes the pile of Volta—and it must be ranked among the greatest discoveries of modern times. It has not only added greatly to our knowledge of

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* Gilbert's Annalen der Physik, xlix. 47.

† The reader who is interested in the investigation of these *dry piles*, as they have been called, may consult Gilbert's Annalen der Physik, vol. xlix. in which both the papers of De Luc and Zambon, and the investigations of Jaeger, are to be found.

electricity, but has contributed in a surprising degree to the extension and perfection of chemical science. If we compare the state of chemistry before the discovery of the Voltaic pile with its present aspect, we cannot but be astonished at the difference—and this difference is in a great measure owing to the discoveries made by means of this new instrument of investigation.

I have already in a preceding chapter, given an account of the controversy between Galvani and Volta, which led to the discovery of the *pile*. As it was originally constructed by Volta, it consisted of a number of round pieces of zinc, and silver, or copper, each about the size of half a crown. There were as many pieces of pasteboard, as there were of zinc, of the same round shape, but a little smaller size than the metal discs. These pieces of pasteboard were soaked in an aqueous solution of common salt, which is a better conductor of electricity than pure water. These pieces of metal and moist card were piled upon each other in the following order. Zinc, silver, moist card ; zinc, silver, moist card, &c. always observing the same order, till all the pieces be piled upon each other. The card should not be so much soaked with liquid, that the weight of the plates laid above it will be sufficient to squeeze it out, and cause it to run down the pile. Because in that case, it would insinuate itself between the zinc and silver discs, which has a surprising effect in diminishing the efficacy of the pile. In such a pile, the uppermost plate is silver, and the undermost is zinc. The intensity of the electrical phenomena increases with the number of pairs of plates; but they become sensible when the pairs of plates amount to about a dozen. The zinc extremity of such a pile is charged positively, and the silver or copper extremity negatively. If these two extremities be brought into contact by means of a conducting substance, a metallic wire for example, a current of electricity sets through the wire, and continues to pass through it as long as the pile retains its activity. Electric sparks may be taken, and a Leyden jar may be charged by such a pile, precisely as by an electrical machine; though only to a low degree of intensity. If we moisten the finger of each hand, and apply them thus wet to the two extremities of the pile, at the instant of contact we feel a shock; the intensity of which increases with the number of pairs of plates of which the pile is composed. As long as the fingers are kept in contact with the poles of the pile, we feel no sensation of pain, unless a portion of the cuticle be rubbed off, or any part of the finger wounded, in which case we become sensible of a burning sensation in the wounded part, which becomes more and more painful according to the number of plates, and the degree of activity of the pile. If we touch the negative end of the pile with a wet finger, and bring a platinum or gold wire from the positive end in contact with the tongue, we become sensible of a strong acid taste. If the wire from the negative end be placed in contact with the tongue, while we touch the positive pole with a wet finger, we become sensible of a strong burning or alkaline taste. In the same manner, we become sensible of light, when the electric current is made to pass through the eye, or to approach the nerves distributed to the eye.

Such were the phenomena observed by Volta, the inventor of the pile, and described by him in his first paper on the subject.* The chemical effects were first observed by Messrs. Nicholson and Carlisle. Before giving an account of them, it may be proper to explain the various modifications which Volta's pile underwent in the hands of chemical experimenters.

Improved by Cruikshanks.

The first improvement was made by Mr. Cruikshanks of Woolwich. The object of it was to facilitate the construction of the pile. He employed square plates of zinc and copper, each about four inches long, and as much in breadth. These plates were soldered together two and two; and cemented in a trough of wood, by means of pitch, or any non-conducting substance with which the inner side of the trough was covered. These soldered plates of zinc and copper were placed all in the same way in the trough; that is to say, the zinc sides were all turned towards one end of the trough, and the copper sides towards the other end. These plates were cemented into the trough, at the distance of about a quarter of an inch from each other, thus dividing the trough into as many separate cells as there were pairs of plates. To make the trough fit for action, nothing more was necessary than to fill the cells

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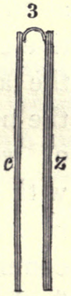
* Phil. Trans. 1800. p. 403.

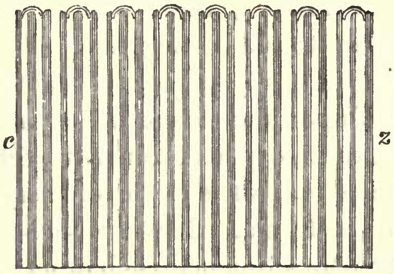
with the liquid destined to act as a conductor. It is evident, that such a trough would act the part of Volta's pile; and that the end of it terminated by the zinc plate would be positive, while that terminated by the copper plate would be negative.

Farther improved.

It was soon observed, that the energy of such a trough did not depend upon the extent of surface in which the copper and zinc plates touched each other; but upon the extent of surface of these plates in contact with the liquid. It was sufficient if the zinc and copper plates touched each other in a single point, provided these plates were themselves plunged into the liquid, so that a copper plate should always be exactly opposite to a zinc plate in the same cell, without the two plates touching each other in any part. Hence, instead of soldering the zinc and copper plates together, it was found sufficient to allow a small ribbon of copper to issue from the summit of each copper plate. This ribbon was soldered to the top of the zinc plate. A section of two such plates, is here represented, in which *z* is the zinc plate, and *c* the copper, while 3 represents the ribbon of copper by which the two plates are soldered together. The wooden trough was divided into cells, by cementing into it, at regular distances, as many plates of glass, or varnished wood, as there were plates of zinc and copper cemented together. The trough thus prepared, was filled with the liquid intended to act as a conductor. The metallic plates were then slipped in, taking care that all the zinc plates



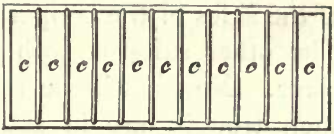
were turned the same way, and that each pair was separated from each other by the glass or wooden diaphragm which divided the trough into its various compartments. The figure in the margin represents a section of a small portion of such a trough, in which the middle line in each trio represents the glass plates which divide the trough into different cells, while *cz*, &c. represent eight pairs of the cemented metallic plates slipped over the glass diaphragms, so that there is a zinc plate in every cell, exactly opposite to the corresponding copper plate; each a certain distance from the other. The zinc plates are all turned the same way. At the extremities of the trough, there was a cell filled with the conducting liquid, and containing only one metallic plate. The extremity at which this plate was zinc constituted the positive end of the trough. The other extremity into the last trough of which a copper plate plunged was the negative extremity. There was usually a piece of wood fixed at each extremity of the trough, with a small hole in it for the introduction of a wire, in order to make the two extremities of the trough communicate with each other, which is necessary for bringing the trough into action.

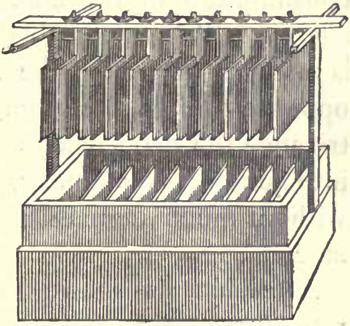


When the size of the metal plates is considerable, four inches square, for example, if the trough contains a considerable number of pairs, 50 or 100 for

Porcelain troughs.

example, it becomes so heavy as to be almost unmanageable. To obviate this inconvenience the troughs are now made of porcelain, and of such a size that each trough is divided into ten separate cells, by means of porcelain divisions in the trough. The figure in the margin represents the surface of such a trough, viewed from above. *c c c*, &c. represents the different cells into which the trough is divided by the porcelain diaphragms introduced into trough. The ten pairs of plates belonging to each trough are properly screwed to a small rectangular piece of wood sufficiently strong to support them, as represented in the margin. The piece of wood to which the plates are screwed is well dried and then varnished, in order to be rendered a non-conductor of electricity. The plates are so screwed into the wood that a plate of zinc and a plate of copper can be introduced into the same cell at the requisite distance, and exactly opposite to each other. These two plates communicate only by means of the conducting liquid into which they are plunged.





Zinc plates surrounded with copper.

A little reflection is sufficient to show that the energy of each pair of plates depends upon the extent of surface of the two metals exactly opposite to each other, and separated by the conducting surface. When a single plate of zinc is introduced into a trough, and a single plate of copper is placed opposite to it of exactly the same size, it is obvious that only one of the sides of the zinc plate, and one of the sides of the copper plate, exert their energy. The other sides of both plates are totally inefficacious. Now it will be seen afterwards that when such troughs are in activity it is the zinc plate which is wasted and not the copper. But both sides of the zinc plate dissolve. Thus when the trough is constructed in the way now described the consumption of the zinc is twice as great as its energy. To remedy this defect the copper plate is made to go round the zinc plate, so as to be opposite to it at a certain distance. By this contrivance the two surfaces of the zinc are brought into action. This doubles the energy of the trough without much increasing the consumption of the zinc.

Morichini has shown that the energy of the apparatus increases in proportion as the surface of the electro-negative metal is extended. This metal may, he says, be sextupled with always an increasing energy; beyond that point the rate at which the energy increases is slower.*

Such is the present state of the galvanic apparatus. The porcelain troughs are exceedingly convenient, and would be preferable to every other form of the apparatus were it not for a circumstance

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* Berzelius, Traité de Chimie, i. 138.

of very considerable importance to the experimenter. The cells are so large that the quantity of liquid necessary to fill them is very considerable, and the expense of this liquid, when the number of troughs is considerable, becomes so great as greatly to limit the power of experimenting with such an apparatus. I have a galvanic battery consisting of four hundred pairs of zinc and copper plates, each 4 inches square. These are placed in 40 porcelain troughs, each of which holds 200 cubic inches, so that the 40 troughs require 8000 cubic inches which amounts to nearly 29 imperial gallons of 277·274 cubic inches each. So that somewhat more than a gallon of concentrated nitric acid is required to charge the battery sufficiently high. I generally employ about a gallon of nitric acid of the specific gravity 1·3, and half a gallon of strong sulphuric acid diluted with the requisite quantity of water to fill the troughs.

Hare's rolled plates.

Dr. Hare, professor of chemistry at Philadelphia, has contrived a new modification of the galvanic apparatus. He takes two plates, one of copper and the other of zinc, and placing a disc of leather between the two, he rolls them up in the form of a spiral. The leather disc is now removed, and the two metals are prevented from touching each other by interposing slips of wood. Each of these plates is made to communicate with a plate of a different kind of metal, the zinc with copper, and the copper with zinc, precisely in the way already described when giving an account of the troughs. A number of these double spirals are fixed upon a piece of wood in the same way as has been just explained. These cylinders are now introduced, each into a cylindrical glass vessel. This method of construction is attended with several important advantages. By this contrivance plates of a very large size may be introduced into a very small vessel; so that the expenditure of liquid is the least possible. Besides the greatest part of the two sides of each plate is active, being placed opposite to a face of the other metal.

Deflagrator.

Dr. Hare has constructed another apparatus which he calls a *deflagrator*, and which appears to possess very great power. He takes a plate of zinc three or four inches square and encloses it in a case of copper, distant from it about a line, and touching it nowhere. Any number of these plates thus enclosed in copper is attached to a horizontal piece of wood, and fixed immoveably that there may be no risk of the plate of zinc touching the copper case in which it is enclosed. The zinc plate at the first of these is united at the top to the copper case of the next zinc plate. And this is continued through the whole. These copper cases are placed at a very small distance from each other, and between each pair is introduced a piece of card dipped in linseed oil varnish, and half dry. They are then compressed so as to adhere so closely to each other, that no water can insinuate itself between them. Things being thus disposed, the apparatus is plunged into a trough containing the liquid, and not divided into cells; the varnished card answering all the purposes of the diaphragms in the porcelain troughs. Four such pieces of apparatus, containing each fifty plates of zinc, surrounded each by its copper case, when plunged into their proper troughs, produce very powerful effects.

The apparatus employed by Orsted, and of the efficacy of which he speaks in high terms, approaches very nearly to this last one of Hare. Indeed the theoretical construction of both is the same.

Nature of the imperfect conductors used.

The energy of the galvanic battery depends very much upon the nature of the liquid employed as a conductor between the pairs of plates. The better a conductor of electricity it is, the more powerful, *ceteris paribus*, is the energy of the battery. When we employ pure water the energy is a minimum. Indeed it seems doubtful whether the pile possesses any activity when the water used is perfectly pure, and perfectly freed from atmospherical air. Water containing a salt in solution answers much better, because the addition of a salt increases very much the conducting power of water. Common salt, sulphate of soda, alum, nitre, or sal ammoniac, are often employed. The more easily the salt is decomposed the better does it answer. Acids on that account answer better than salts. The three acids usually employed are the sulphuric, muriatic, and nitric. Of these the muriatic acid has the least energy; the sulphuric comes next in order; but the nitric acid is the most efficacious of all.

The stronger the saline or acid solution is, the more powerful is the action of the battery. The action of nitric acid is the most energetic; but it ceases most speedily. The experiments of Gay Lussac and Thenard have shown that the addition of 10, 20, 30, &c. parts of concentrated nitric acid to a given weight of water increases the intensity of the battery 10, 20, 30, &c. times, or that the energy is directly proportional to the strength of the acid employed. The charge usually employed in this country is a mixture of 1 part of concentrated acid with about 20 or 25 parts of water.

The action of the sulphuric acid lasts longer than that of the nitric, and that of the muriatic is more lasting than of the sulphuric. The addition of some sulphuric acid to the nitric acid solution (as was the practice of Sir H. Davy) renders the action much longer; because the sulphuric acid prevents the nitric acid from being saturated by the oxide of zinc formed. The energy, of course, continues till the whole nitric acid is decomposed.

Of the salts, sal ammoniac is the most powerful. A solution of common salt in vinegar acts well, and is most commonly used by experimenters, as the cheapest ingredient likely to answer the purpose.

Having given a sufficient account of the galvanic apparatus, it is time to turn our attention to the effects which it produces. These are of two kinds, *electrical* and *chemical*.

Zinc plates plus, copper plates minus.

1. *Electrical effects.* A battery constructed of zinc and copper plates, with a liquid conductor between every pair, evolves the two opposite electricities. The positive electricity goes on increasing in every zinc plate from the copper end

of the battery to the zinc end. So that the zinc end is charged positively, and the intensity of the charge depends upon the number of pairs of plates; though the absolute quantity of electricity evolved depends upon the quantity of surface of the two metals opposed to each other. A single pair of plates, supposing their surface very large, will evolve as much electricity as a great number of pairs of plates of a small size. But the effect produced by the great number of plates in giving shocks, or charging a Leyden jar, will be much greater than is produced by a single pair of plates of very large dimensions. The Leyden jar is charged in an instant. But the intensity of the charge depends upon the number of pairs of plates. Even a large electrical battery of Leyden jars will be charged in an instant to the same degree of intensity as the single Leyden jar. We see from this that the quantity of electricity evolved by the Voltaic apparatus is large; but that its intensity is *low*.

What is meant by *intensity*.

It is not easy to convey an accurate idea of what is meant by the *intensity* of electricity. If you charge a single electrical jar as high as it will bear, its intensity will be greater than can be induced into a battery of jars, and the larger the battery the smaller is the *intensity*. This intensity is measured by the effect produced upon a pendulum electrometer, in causing it to diverge from the perpendicular. The attractions and repulsions produced by the small jar, will be greater than by the battery. Yet the quantity of electricity in the battery, is much greater than in the single jar. If the electricity be considered as a fluid, we may consider the degree of intensity, as depending upon the smallness of the distance at which the particles of electricity are placed from each other. The nearer these particles are to each other, the greater will be the repulsion which they will exercise upon each other, and the greater will be their attraction for the opposite electricity. Electricity of high intensity, has the particles nearer each other; electricity of low intensity, has the particles at a greater distance.

The copper plates of the Voltaic battery are all charged negatively, and the intensity of the charge increases from the zinc end, to the copper end; that copper plate which constitutes the pole of the battery, being most intensely negative. The intensity of the charge of negative electricity, depends upon the number of pairs of plates, precisely as that of the positive electricity. Every thing that has been said of the latter, applies equally to the former.

Intensity of the battery low; absolute quantity very great.

The absolute quantity of electricity evolved by the action of the Voltaic battery, seems to be enormous; but its intensity when compared with the electricity from an electrical machine, is always low. This is the reason why many substances which conduct common electricity very well, conduct Voltaic electricity rather imperfectly. Pure water is in this predicament, and so is the cuticle of the human body. This is the reason, why, in order to ensure the perception of the shock, it is necessary to moisten the fingers, before we apply them to the two poles of the battery. The shock is only felt at the instant of touching or withdrawing the fingers from the poles of the battery. As long as the contact is continued, we are sensible of nothing, unless the cuticle be abraded, or any part of the hand wounded or sore. The violence of the spark depends upon the number of pairs of plates. When we take shocks from a battery of two hundred pairs of plates, each four inches square, by grasping a platinum wire in each hand, and bringing these wires into contact with the two poles of the battery, the shock is attended with considerable pain, if the battery be in full action. It occasions an involuntary convulsion in the arms, which produces a momentary removal of the wire from the poles. But immediately after, the contact being renewed, a new shock is felt. This goes on with inconceivable rapidity, and occasions at last so much pain, that it is impossible to continue to bear it.

It heats and fuses wire.

When electricity passes through conducting bodies, it produces no sensible effect upon them. But if we diminish the size of the conducting body, and augment the quantity of electricity so much, that difficulty begins to be encountered, when so large a quantity of electricity is obliged to pass through so small a body, effects begin at last to be perceptible. Suppose the conducting body to be a metallic wire. The wire becomes hot. If the quantity of electricity be increased, or the diameter of the wire be diminished, it becomes red hot. If the electricity be still farther increased, the wire melts and is dissipated in smoke.

The very same effects are produced upon wires, by Voltaic electricity, and common electricity. And in producing these effects, the power of the Voltaic battery depends, not upon the number of pairs of plates, but upon the extent of surface. So that a single pair, supposing the surface equally great, will be capable of igniting as much wire, as any number of pairs of plates, constituting altogether no greater extent of surface, than the single pair. The reason is evident. The effect upon the wire, must depend upon the absolute quantity of electricity that passes through it in a given time. The intensity of the electricity accumulated in a single pair of plates, of however large dimensions, is so small, that it will not give sensible shocks, and will scarcely pass through liquids. Yet its absolute quantity may be as great, as that which is accumulated in 200 pairs of plates. Hence, when it passes through a wire, it will exhibit as great an effect upon that wire, as would be produced by a battery composed of 200 pairs of plates.

Calorimotor.

Dr. Hare, of Philadelphia, has constructed an apparatus, which he calls *calorimotor*. It consists of a number of plates of zinc, and as many of copper, about 18 inches square each. The copper plates are all placed in contact with each other, by means of ribbons of metal soldered to them. The same is the case with the zinc plates. These plates are placed alternately at a small distance from each other in a frame. When plunged into a conducting liquid, they constitute, in fact, only a single pair of zinc and copper plates, of enormous dimensions.

When the zinc is brought into contact with the copper, by means of a platinum wire, of very considerable size, it produces the ignition of that wire, for a length of about two inches, and keeps it ignited as long as the apparatus retains its activity. Yet this apparatus, though so powerful in igniting wire, gives no shocks, and produces no sensible chemical effect whatever. These facts have led Dr. Hare to suppose, that the Voltaic battery produces two distinct and unconnected effects; namely, the *evolution of electricity*, which, in his opinion, occasions the shocks and the chemical decompositions, and which depends on the number of pairs of plates, and the *evolution of heat*, which occasions the ignition of metallic wires, and is proportional to the extent of surface in action. But the considerations stated above, afford a satisfactory explanation of the effects of this apparatus, without any necessity to have recourse to the notion, that a current of heat passes through the Voltaic battery, as well as a current of electricity. The chemical effects produced by electricity must, of necessity, depend upon its intensity; but the property which it has of igniting metals must depend upon its absolute quantity. In the calorimotor the intensity is a minimum; being only that produced by a single pair of plates; but the absolute quantity may be as great as we please, as it depends upon the extent of the surface of the two plates, opposite to each other.

When different wires are exposed to the action of the Voltaic battery, the effect is found not to be the same upon all. Platinum wire is more easily ignited than any other, and zinc wire is the most difficult of ignition of all hitherto tried. Now as ignition depends upon a greater quantity of electricity passing through the wire, than it is capable of transmitting readily, it is obvious that the worst conductors will be most easily ignited, and the best conductors with most difficulty. According to this view, platinum is the worst conductor of electricity of all metals hitherto tried, while zinc is the best conductor.

Children's experiments on the conducting power of different metals.

The most unexceptionable set of experiments, on the conducting power of the different metals, was made by Mr. Children, in the year 1814. These experiments I was fortunate enough to witness.* Mr. Children's battery consisted of 20 pairs of zinc and copper plates, each six feet long, and 2 feet six inches broad, joined together by straps of lead, and plunged into a mixture of nitric and sulphuric acids, diluted with from 20 to 40 times their weight of water. This battery when in full action, rendered a platinum wire 5 feet 6 inches long, and $\frac{11}{100}$ths of an inch in diameter red hot throughout, so that the ignition was visible in full daylight. 8 feet 6 inches of platinum wire $\frac{44}{100}$ of an inch in diameter, were heated red. A bar of platinum $\frac{1}{6}$th inch square, and $2\frac{1}{4}$ inches long, was heated red hot and fused at the end. A round bar of the same metal $\frac{276}{1000}$ths of an inch in diameter, and $2\frac{1}{2}$ inches long, was heated bright red throughout.

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* An account of them will be found in Phil. Trans. 1815, p. 363.

To determine the relative conducting powers of different metals, two wires of the metals to be compared were taken of equal diameter and length, one end of each was in contact with one of the basins of mercury, communicating with the poles of the battery, and the other end being bent to an angle, the wires were connected by hooking them to each other. The length of each wire was 8 inches, and its diameter $\frac{1}{30}$th of an inch. The results were as follows.

1. The wires being platinum and gold, the platinum was instantly ignited, while the gold remained unaffected.

2. Gold and silver. The gold was ignited, the silver not.

3. Gold and copper. Both metals were equally ignited.

4. Gold and iron. The iron was ignited, the gold unchanged.

5. Platinum and iron. The iron ignited instantly next the pole of the battery. Then the platinum became ignited through its whole extent. After this, the iron became more intensely ignited than the platinum, and the ignition of the latter decreased.

6. Platinum and zinc. The platinum was ignited, the zinc was not; but melted at the point of contact.

7. Zinc and iron. The iron was ignited; the zinc bore the heat without fusing.

8. Lead and platinum. The lead fused at the point of contact.

9. Tin and platinum. The tin fused at the point of contact.

10. Zinc and silver. The zinc was ignited before it melted; the silver was not ignited.

From these experiments it has been deduced, that the order of these metals as conductors of electricity is as follows:

Silver,
Zinc,
{ Gold,
{ Copper,
Iron,
Platinum.

Tin and lead fuse so immediately at the point of contact, that no conclusion respecting them could be drawn.

Experiments of Mr. Harris.

But the most complete set of experiments on the conducting powers of the different metals are those of Mr. Harris.* He determined the degree of heat induced into wires of the same size by passing through them the contents of an electric battery charged exactly to the same intensity. This heat was measured by the expansion induced in a given volume of air by the wires. The conducting powers were considered as inversely as these expansions. The following table exhibits the relative heats measured by the rise of a column of spirits (in a tube) pressed upon by the expanded air.

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* Phil. Trans. 1827, p. 18.

	Effect.
Copper,	6
Silver,	6
Gold,	9
Zinc,	18
Platinum,	30
Iron,	30
Tin,	36
Lead,	72
Brass,	18
Gold 1 part, Copper, 1,	20
Gold 3, Copper 1,	25
Gold 1, Copper 3,	15
Copper 1, Silver 1,	6
Copper 1, Silver 3,	6
Copper 3, Silver 1,	6
Gold 1, Silver 1,	20
Gold 1, Silver 3,	15
Gold 3, Silver 1,	25
Tin 1, Lead 1,	54
Tin 3, Lead 1,	45
Tin 1, Lead 3,	63
Tin 1, Zinc 1,	27
Tin 3, Zinc 1,	32
Copper 8, Tin 1,	18

If we consider the conducting power of these metals as the inverse of these numbers, it will be as in the following table.

Table of conducting powers of metal.

	Conducting power.
Copper,	12
Silver,	12
Copper 1 part, silver 1 part,	12
Copper 1, silver 3,	12
Copper 3, silver 1,	12
Gold,	8
Gold 1, copper 3,	4·8
Gold 1, silver 2,	4·8
Zinc,	4
Brass,	4

	Conducting power.
Copper 8, tin 1,	4
Gold 1, copper 1,	3·6
Gold 1, silver 1,	3·6
Gold 3, copper 1,	2·88
Gold 3, silver 1,	2·88
Tin 1, zinc 1,	2·$\frac{2}{3}$
Platinum,	2·4
Iron,	2·4
Tin 3, zinc 1,	2·25
Tin,	2
Tin 3, lead 1,	1·6
Tin 1, lead 1,	1$\frac{1}{3}$
Tin 1, lead 3,	1·1428
Lead,	1

Probably in order to render these numbers correct representatives of the conducting power of electricity, they ought to be divided by the conducting power of the metals for heat.

The heat produced by a powerful Voltaic battery, when in full action, is very great. Pieces of charcoal are raised to a white heat, even when surrounded with hydrogen or azotic gas, and the light emitted is so intense, that the eye cannot bear the glare. The galvanic light is precisely similar to that of the sun. Objects viewed with it, have precisely the same colours, as when viewed by the light of day.

Mr. Children's battery fused and reduced oxides of tungsten and molybdenum. Columbic acid, oxide of uranium, oxide of titanium, and oxide of cerium, were fused, but not reduced. Iridium, weighing 7·1 grains, was fused into a porous globule whose specific gravity was 18·68.* Spinel, gadolinite, and zircon were fused, magnesia was agglutinated; but ruby, sapphire, and quartz were not affected. Diamond powder having been put into the middle of some soft iron wire, it was put into the current, and kept of a dull red heat for six minutes. The diamond powder had disappeared; the interior of the iron was fused into numerous cavities, and all that part of it which had been in contact with the diamond was converted into blistered steel.†

Chemical effects.

II. The chemical effects of the Voltaic battery have been investigated with much sagacity, and have thrown a flood of light upon the nature of chemical combination. Every substance constituting the liquid conductor interposed between the pairs of plates undergoes decomposition, one of its constituents being attracted to the positive plate, and the other to the negative. Suppose the liquid conductor to be *water*. The *oxygen* is attracted to the *positive* or zinc plate, which it converts into an oxide, while the other constituent, the *hydrogen*, is attracted to the *negative* or copper plate. Hydrogen does not combine with copper. It therefore makes its escape from the face of the copper plate in the

Constituents go to different poles.

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* Large plates have a great advantage over a battery composed of numerous small plates when the object is the production of heat. Because the intensity of the electricity being much smaller, the loss is much less. When the 2000 pair of plates battery belonging to the Royal Institution was in action, it was impossible to walk across the room without being sensible of pretty strong shocks.

† Phil. Trans. 1815, p. 370.

form of hydrogen gas. When pure water is used the decomposition is exceedingly slow, because pure water conducts electricity of low intensity very badly. If we add nitric acid to the water the energy of the battery is very much increased, because the conducting power of the liquid is greatly augmented, and because the nitric acid undergoes decomposition much more easily than water. In this case both of the constituents of the liquid conductor undergo decomposition; the water is reduced into its elements as before, and the oxygen, as before, unites to the zinc plate, while the hydrogen is disengaged from the copper plate. The nitric acid is decomposed into oxygen and deutoxide of azote. The oxygen unites to the zinc, while the deutoxide of azote is disengaged in abundance from the copper plates. If the nitric acid solution used possess considerable strength, a good deal of heat is evolved, and the quantity of deutoxide of azote and hydrogen given out is very annoying to the experimenter. The zinc is oxydized and dissolved very fast, and the energy of the battery is soon destroyed. Because in a very short time the whole nitric acid is either decomposed, or combined with the oxide of zinc. The liquid becomes a solution of nitrate of zinc in water, which is comparatively speaking a bad conductor of electricity.

It would appear, that while the energy of the Voltaic battery continues, two opposite currents of electricity pass through every stratum of liquid, interposed between every pair of plates. The nega-

Decomposition of water explained.

tive electricity passes towards the copper plate, and the positive electricity towards the zinc plate. To form a conception of the way in which these currents pass, let us consider the conducting liquid in its simplest state, or consisting of pure water. Water is a compound of oxygen and hydrogen. Let us consider only a single row of the particles of it, lying between the zinc and the copper plate. Let *o* represent oxygen, and + hydrogen. A particle of water may be represented by the symbol ♀, and a row of particles of water may be represented thus,

♀ ♀ ♀ ♀ ♀ ♀ ♀ ♀ ♀ ♀ ♀ ♀

The current of negative electricity seems to pass along the particles of oxygen, and the current of positive electricity along the particles of hydrogen. From M. Pouillet's experiments related in the last section, it would follow that when oxygen and hydrogen combine, the former gives out positive, and the latter negative electricity. We may conclude from this, that when they become charged again with these respective electricities, they will separate from each other. The particle of oxygen next the zinc plate, being thus disengaged from the particle of hydrogen with which it was united, will be attracted to the zinc plate, give out its electricity to it, and combine with it. The same thing will happen to the particle of hydrogen nearest the copper plate. The electricity constantly passing along the row of particles, will occasion a succession of decompositions of the particles of water. But the hydrogen in each, will instantly combine again with the next particle of oxygen in succession. It is not then the hydrogen which passes through the liquid from one plate to the other, but a rapid succession of decompositions; and as the first particle of water has parted with its oxygen, it is obvious that the last particle must part with its hydrogen.

The same explanation applies to all the decompositions that take place, in the liquid interposed between the respective pairs of zinc and copper. But it is usual to interpose between the two poles of the Voltaic battery, a vessel containing the water or other substance to be decomposed. A platinum wire is made to pass into this liquid from each pole, and the series of decompositions may thus be observed.

Law of decomposition.

The nature of these decompositions was first investigated by Berzelius and Hisinger, in a paper published by them, in Gehlen's Journal for 1803.* They showed that a number of salts which they dissolved in water, and placed in contact with the two poles of a galvanic pile, by means of iron or silver wires, were decomposed, the acid being deposited round the positive wire, and the base round the negative pole.† Ammonia was also decomposed; azotic gas separating from the positive pole

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* Vol. i. p. 115.

† The salts tried, were—
Sulphate of ammonia,
Nitrate of ammonia,
Muriate of ammonia,
Phosphate of ammonia,
Borate of ammonia,
Prussiate of ammonia,
Common salt,
Nitrate of potash,
Bisulphate of potash,
Sulphate of potash,
Muriate of lime.

of the battery, and hydrogen gas from the negative pole. When lime water was tried, no decomposition of the lime took place.

Discoveries of Davy.

In the Philosophical Transactions for 1807, an admirable paper was published by Sir Humphry Davy, entitled, *On some Chemical Agencies of Electricity*, containing a very minute and complete investigation of the chemical decompositions produced by Voltaic electricity. To perceive the full value of this paper, it would be requisite to have an idea of the previous state of our knowledge of this intricate subject. It had been already observed, that when two platinum wires, from the two poles of a galvanic pile, were plunged each into a vessel of water, and the two vessels united by means of wet asbestus or any other conducting substance, an acid appeared round the positive wire, and an alkali round the negative wire. This alkali was said by some to be ammonia, by others to be soda. The acid was variously stated as muriatic acid, nitric acid, or even *chlorine*. And it was generally admitted, that these acids and alkalies were generated by the galvanic action. Davy demonstrated, by decisive experiments, that in these cases the acid and alkali were derived from the decomposition of some salt, contained either in the water, or in the vessel in which the water was placed. Most commonly the salt decomposed was common salt, and he showed that agate, basalt, and various other stony bodies, which he used as vessels, contained quantities of common salt appreciable by the galvanic action. When the same agate cup was used

Ch. XIV. in successive experiments, the quantity of acid and alkali evolved diminished each time, and at last no appreciable quantity could be perceived. When glass vessels were used, soda was disengaged at the expense of the glass, which was sensibly corroded. When the water into which the wires were dipt was perfectly pure, and when the vessel containing it was free from every trace of saline matter, no acid or alkali made its appearance, and nothing was evolved except the two constituents of water, namely oxygen and hydrogen; the oxygen appearing round the positive wire, and the hydrogen round the negative wire.

When a salt was put into the vessel into which the positive wire dipt, the vessel into which the negative wire dipt being filled with pure water, and the two vessels being united by a slip of asbestus moistened with water, the acid of the salt made its appearance round the positive wire, and the alkali round the negative wire, before it could be detected in the intermediate space. But if an intermediate vessel containing a substance for which the alkali has a strong affinity, be placed between these two vessels, the whole being united by slips of asbestus, then great part of, or even the whole of the alkali, was stopt in this intermediate vessel. Thus if the salt was nitrate of barytes, and sulphuric acid was placed in the intermediate vessel; much sulphate of barytes was deposited in the intermediate vessel, and very little or even no barytes made its appearance round the negative wire. Upon this subject a most minute, extensive, and satisfactory series of experiments was made by Davy, leaving no doubt whatever respecting the accuracy of the general fact. Indeed this paper of Sir H. Davy constitutes one of the most important contributions, ever made to scientific chemistry, and threw a ray of light upon chemical affinity, which may ultimately produce the most important consequences.

Electro-chemical theory.

The conclusions drawn by Davy from his experiments, are that all substances which have a chemical affinity for each other, are in opposite states of electricity, and that the degree of affinity is proportional to the intensity of these opposite states. When such a compound body is placed in contact with the two poles of a galvanic battery, the positive pole attracts that constituent which is negative, and repels the positive. The negative pole acts in the opposite way, attracting the positive constituent, and repelling the negative. The more powerful the battery, the greater is the force of these attractions and repulsions. We may, therefore, merely by increasing the energy of a battery, sufficiently, enable it to decompose any compound whatever. Oxygen, chlorine, bromine, iodine, and acids, being negative bodies, are attracted to the positive pole; while hydrogen, alkalies, earths, oxides, metals, and bases of all kinds, being positive, are attracted to the negative pole.*

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* If this view of the subject, which is probably more conformable to truth, than the view of Pouillet given in page 497 of this work, be admitted, it will be necessary to modify the explanation of the way in which the electrical currents pass, given in page 531. If oxygen be negative, and hydrogen positive, it is obvious that the

Ch. XIV.

Such are the electrical and such the chemical phenomena produced by the Voltaic battery. We shall see hereafter the splendid series of chemical discoveries which flowed from these investigations of Davy.

Theory of the pile.

With respect to the theory of the pile it is not necessary to say much. The original opinion of Volta, that all the phenomena are produced by common electricity, seems to me to be fully established. But that the whole current of electricity evolved is merely the consequence of the new arrangement of the fluids in the metallic plates, in consequence of their juxta position, is not so evident. The energy of the pile is proportional to the extent of surface of the two metals opposite to each other, and intercepted by the fluid conductor placed between them. The metals themselves require to be in contact only in a single point; but a broad surface of each requires to be in contact with the fluid. The energy increases with the conducting power of the fluid. But this is not all. The fluid in all cases undergoes decomposition, and when the decomposition ceases the active agency of the pile is at an end. That fluid answers best which is most easily decomposed, and the energy of the pile is proportional to the rapidity with which the decomposition takes place. If the only

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positive current will be attracted to the oxygen and must pass through it, while the negative current will pass through the hydrogen. These currents by neutralizing the electricity in both bodies, will occasion the separation of the oxygen and hydrogen, and produce the decompositions referred to in page 531.

use of the liquid were to transmit the electricity from one pair of plates to the next in succession, no reason could be assigned why the decomposition of the liquid should be necessary for the activity of the pile. It may be said, indeed, that this decomposition is the necessary consequence of the passage of a current of electricity through the liquid; at least if we take the same view of chemical affinity which Sir H. Davy endeavoured to establish. The constituents of the liquid remain in combination, because they possess different electrical states. The current of electricity, by rendering the electricity of both the constituents the same, destroys their affinity, and occasions the decomposition of the liquid.

Electricity derived from the decomposition of the liquid conductor.

According to this, the decomposition of the liquid conductor is merely the consequence of the current of the electricity which passes through the pile. But if this explanation were the true one, it is evident that all liquids would act equally well in the galvanic pile, provided they were capable of conducting electricity equally well. It may be said, perhaps, that nitric acid is a better conductor than any other liquid, and that this is the reason why it answers better for charging the pile. But even were we to grant this it would not remove nor even diminish the difficulty. When a Voltaic battery composed of porcelain troughs with zinc and copper plates, as formerly described, is put into action by filling the cells with dilute nitric acid, it possesses at first great energy. The zinc plates dissolve with rapidity in this acid, and considerable heat and abundance of nitrous acid fumes are evolved. By degrees the violence of the action diminishes; the zinc dissolves more slowly, and at last all chemical action is at an end. When this has taken place we shall find, if we try, that the battery has become totally inert. When this has happened, if we examine the liquid in the troughs, we shall find that it contains no free nitric acid; but that there is dissolved in it a considerable quantity of nitrate of zinc. The whole nitric acid has been either decomposed or united with oxide of zinc. But a solution of oxide of zinc is a good conductor of electricity. If, therefore, the liquid in the cells answered no other purpose than to conduct the electricity from one pair of plates to another, we can see no reason why the battery should lose its energy, while so good a conductor as solution of nitrate of zinc is interposed between the plates.

But a solution of nitrate of zinc, though it conducts electricity, is incapable of acting either upon zinc or copper. Its tendency to undergo decomposition is at an end. If any decomposition take place it is merely the particles of the water that undergo it, and so slowly that it is scarcely perceptible. From all this the conclusion seems unavoidable, that much of the electricity, which acts with so much energy in the galvanic battery, is evolved by, and in consequence of, the decomposition of the liquid which constitutes an essential part of every galvanic pile. On this supposition we see a reason why those liquids should answer best which are most easily decomposed, and why after the decomposition is at an end the energy of the pile ceases likewise. In short, the chemical decompositions seem to be at once a consequence and a cause of the energy of the galvanic pile.

It must not be disguised that this view of the subject seems inconsistent with the experiments of Pouillet related in a former chapter. He found that whenever two bodies united by combustion, the supporter gave out positive, and the combustible negative electricity, and he inferred in consequence that when such a compound underwent decomposition the supporter would absorb positive, and the combustible negative electricity. The two constituents of the liquid decomposed in the galvanic pile should therefore, instead of giving out electricity, absorb it. The electrical theory of chemical affinity is also hostile to the notion that bodies give out electricity when they undergo decomposition. Bodies, according to that theory, remain united as long as they continue in opposite states of electricity; but when they assume the same state, their union is at an end. Therefore, when bodies undergo decomposition they ought to absorb electricity, and not to part with it.

The phenomena of the Voltaic battery, however, seems to me to show clearly that the electricity evolved is connected with the decomposition of the conducting fluid, since its energy is always proportional to it, and since both always cease together. It would be very desirable that Pouillet's experiments should be repeated and confirmed; and that the conclusions which he has drawn from them should undergo a rigid examination. Because air surrounding burning charcoal is positive, and the carbonic acid formed negative, does it follow that oxygen, when it enters into combination with carbon, gives out positive electricity, while the carbon gives out negative electricity? I do not see the legitimacy of the conclusion very clearly.

CHAPTER XV.

OF ELECTRO-MAGNETISM.

Chap. XV.

In this chapter I shall give as concise an account as possible of the recently discovered facts which have shown the dependency of magnetism on electricity, because they are necessary to complete the sketch which I proposed to give of the present state of the science of electricity.

SECTION I.

SKETCH OF THE PHENOMENA OF MAGNETISM.

Magnet what.

The term *magnet* or *loadstone* is usually applied to an ore of iron composed of two atoms of peroxide, and one atom of protoxide of iron. This ore has the colour of iron, the metallic lustre, a specific gravity of about 5, and it crystallizes in octahedrons. It is usually called *magnetic iron ore* by mineralogists. This ore has usually the property of attracting iron filings to itself. Pieces of it that have lain in the earth in a particular direction, are found endowed with *two* or *more poles*, which alternately attract and repel the poles of a magnetic needle. It is to such pieces that the name of *magnet* is applied.*

Artificial magnet.

If a loadstone, possessed of two poles, be drawn along a bar of steel two or three times, always in the same direction, the steel bar will become a magnet. Or if we place a bar of steel in a direction parallel to the axis of the earth, or rather parallel to the dip of the needle, and strike it smartly while in that position, for some time; it will acquire magnetic properties. These properties may be communicated to other bars, and the magnetic virtue in each may be increased by methods described by all writers on magnetism, and therefore generally known.

Poles.

1. If a steel bar, or a steel wire thus converted into a magnet, be suspended by the middle by a slender string, or supported on a pivot on which it can turn easily, one end of it will always point towards the *north*, and the other end towards the

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* It was called by the Greeks μαγνης, μαγνητος, and μαγνητις, according to some from the name of a shepherd, who first discovered it on mount Ida. Theophrastus does not notice it in his treatise πεϱι λιθων; but it is mentioned by Aristotle, and probably other Greek writers. Pliny treats of it under the name of *magnes*, and gives an account of its property of attracting iron. Its polarity was unknown to the ancients, nor is it known by whom it was discovered. It was certainly known in the 12th, or beginning of the 13th century. The mariner's compass was introduced by Flavio Gioia, or Giri, of Melphi, in the year 1302.

south. If we make it deviate from this position, it will oscillate for some time, and at last settle in the same position as at first. That extremity of the bar or wire which points to the *north* is called the *north pole* of the magnet, and the other extremity which points to the south is called the *south pole.*

Of the same name repel, of different attract.

2. If we present the north pole of one magnet to the north pole of another magnet, both capable of moving freely upon their axes, they *repel* each other. In like manner the two south poles repel each other. But the north pole of one magnet *attracts* the south pole of another, and *vice versa.* Coulomb demonstrated that these attractions and repulsions vary inversely as the square of the distance.*

3. If we determine the direction of a magnetic needle properly suspended and at liberty to move, we shall find in general that it does not point due north, but either to the west or east of north. The exact point of the compass to which it points varies in different parts of the earth. For instance, if we sail from the Straits of Gibraltar to the West Indies, in proportion as we recede from Europe and approach America, the compass will point nearer and nearer due north; and when we come to a certain part of the Gulf of Mexico, it will point exactly north. But if we sail from Great Britain to the southern coast of Greenland, we shall find the needle deviate farther and farther from the north

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* Mem. Paris, 1785, p. 587.

as we approach Greenland, where the deviation will not be less than 45° or 50° west from north.

Declination of the needle.

Even if we examine a compass stationed in the same place it will not always continue to point exactly in the same direction. Suppose when we first begin to make our observations it were to point due north, after a certain number of years, if we again observe the direction, we shall find it to point several degrees *west* or *east* from north. This change in the direction of the compass in the same place is called the *variation* or *declination*. According to Dr. Halley, it was discovered by Gellibrand in 1645; and according to Bond, it was first noticed by Mr. John Mair. The following little table will show the variation of the compass at London from the time that it was first observed, or at least recorded:

Years.	Variation.				Observers.
1580	11°	15′	0″	East	Barrows.
1622	6	0	0	——	Gunter.
1634	4	5	0	——	Gellibrand.
1657	0	0	0	.	Bond.
1672	2	30	0	West	Halley.
1682	4	30	0	——	——
1692	6	0	0	——	Halley.
1722	14	20	0	——	Graham.
1747	17	40	0	——	Graham.
1774	21	16	0	——	Cavendish.
1786	23	17	0	——	Gilpin.
1790	23	39	0	——	Gilpin.
1796	24	0	0	——	Gilpin.
1800	24	3·6	0	——	Gilpin.
1809	24	11	0	——	Gilpin.

Years.		Variations.		Observers.
1814	.	24° 21′ 10″ West	.	Lee.
1815	.	24 17 5 ——	.	Lee.
1816	.	24 17 54 ——	.	Lee.
1817	.	24 17 0 ——	.	Lee.
1818	.	24 15 43 ——	.	Lee.
1819	.	24 14 47 ——	.	Lee.
1820	.	24 11 44 ——	.	Lee.
1821	.	24 11 18 ——	.	Lee.
1822	.	24 9 55 ——	.	Lee.
1823	.	24 9 48 ——	.	Lee.

We see from this table that it reached its greatest western variation in 1814, or 157 years after the needle was observed by Bond to point due north. Since 1814, it has been moving slowly eastward. And if it take as many years to return as it did to proceed westward, it will reach the point of no deviation in the year 1971. Should it go as far to the eastward as it did westwards, and take as long a time, it will reach the easternmost declination in the year 2128. The total arc of declination will be 48° 35′ 48″, and the period occupied in passing over the whole of it will be 314 years. This would be an alteration in the variation amounting at an average to 9′ 17″ annually. But it is much smaller than this towards its western and eastern limits, while it is much more rapid when it deviates but little from due north. Thus during the nine years that elapsed between 1814 and 1823, the progress eastwards is only 11′ 22″, or only 1′ 1″·6 annually. While from 1657 to 1672, the declination west amounted to 2° 30′, or 10′ annually. Between 1672 and 1682, the increase amounted to 2°, or 12′ annually. It was the same nearly between 1682 and 1692. Between 1692 and 1722, the average annual increase of declination was 16′ 40″. This was the maximum. After the year 1722, the rate diminished very rapidly. It seems to have reached half way, or about 12° of western declination about the year 1714, or in 57 years. To complete the other half 100 years were required. These circumstances render it impracticable to calculate the length of the period of the variation from any data in our possession.

Earth has four poles.

The first person who attempted to account for the declination of the needle, was Dr. Halley. He considered the globe of the earth as constituting one great magnet, having four poles, two to the north and two to the south, at considerable and unequal distances from the poles of the earth. Two of these poles (one to the north, the other to the south,) are fixed. The other two are moveable. And to account for the motion of these last, he considers them as the poles of an internal globe concentric with that of the earth, and not moving with the same velocity round its axis; so that its poles gradually fall behind those of the external globe.* This subject has been investigated with much ingenuity by Professor Hansteen, of Christiania, who has had the advantage of another century, and a prodigious number of new observations. He has adopted the Halleyan hypothesis, that the earth is a magnet with four poles, two north and two

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* Halley; Miscelanea Cureosa, vol. i. pp. 27 and 43.

south. One of the north poles and one of the south are much weaker than the other two. The action of these poles on each other occasions a slow change in their position, which is followed of necessity by a change in the declination of the needle. Hansteen calculates that these poles revolve round the pole of the earth, the one in 1740 and the other in 860 years.

Their situation.

The strongest north pole is at present situated in north latitude 73° 53′, and west longitude 87°. This is in Barrow's Straits, a little to the south-east of Melville Island. The magnetical observations made during the late voyages of Captains Ross and Parry, furnished the data from which Hansteen calculated the situation of this pole.

The weakest north pole Hansteen considers as situated in north latitude 85° 12′, and east longitude 140° 6′. This is in the arctic sea, to the east of Nova Zembla, and not quite 5 degrees from the terrestrial pole. But Hansteen not considering the position of this pole as determined with sufficient accuracy, is at present on a journey to Siberia in order to settle it from a sufficient number of actual observations on the declination and dip of the needle, in different stations on the northern shores of Europe and Asia.

The strongest south pole, according to Hansteen, is situated in south latitude 68° 52′, and east longitude 132° 35′, not far from the meridian of Van Dieman's Land, though greatly to the south.

The weakest south pole is in south latitude 78° 16′, and west longitude 135° 59′. It lies in the great South Sea, almost as far to the west of Cape Horn as that promontory is west from London, and greatly within the polar circle, being less than 12 degrees from the south pole. These positions of the magnetic poles of the earth differ a good deal from those assigned by Halley, and are certainly much nearer the truth. Hansteen was enabled to attain this approximation to accuracy by the vast number of magnetical observations that have been made in all parts of the world during the latter part of the last century, and the portion of the present which has elapsed, chiefly by Captain Cooke and the other commanders of the numerous voyages of discovery, which have been sent out at different times by the British government.

Daily variation.

4. In the year 1722 Mr. Graham discovered that the magnetic needle has a daily variation. Mr. Canton made a few observations on this daily variation, and gave the following as the mean monthly daily variations at London for the year 1759.

January	7′	8″	July	13′	14″
February	8	58	August	12	19
March	11	17	September	11	43
April	12	26	October	10	36
May	13	0	November	8	9
June	13	21	December	6	58

But the most complete set of observations on the daily variation of the needle were made by Colonel Beaufoy. He observed three times a day, about half past eight in the morning, at noon, and about seven in the evening, and he continued his observations for several years without interruption.* The declination was always least in the morning, and greatest at noon. The mean of observations kept by him for 2 years and 6 months give the declinations as follows.

Morning	.	24°	14′	39″
Noon	.	24	21	54
Evening	.	24	16	4·5

From Canton's observations, it would appear that the declination increases with the heat of the weather; but Colonel Beaufoy did not find this rule to hold in his observations.

More lately the subject has been taken up by Mr. Barlow and Mr. Christie, who have published important observations on the diurnal variation of the needle in the Philosophical Transactions. They observe that the needle begins to decline to the east early in the morning, and obtains its greatest deviation eastwards about seven in the morning. The needle then begins to move westwards to about half past ten o'clock, when the diurnal variation is zero. The needle now begins to decline westwards, and continues to do so till about half

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* These valuable observations will be found in the Annals of Philosophy (1st series), beginning at vol. ii. and continuing till the end of vol. vi. They were again resumed in vol. ix. p. 390, and continued till vol. xvi. including a series of three years and nine months. In the Annals of Philosophy (new series) i. 94, will be found a summary of all his observations, drawn up by Colonel Beaufoy himself.

past one or two o'clock, when it has reached its maximum. It then begins to diminish, the needle again moving eastward. At about 5 o'clock the deviation again vanishes, and the needle points again to the magnetic north. It continues to move east till late in the evening.[*]

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[*] The following table exhibits the mean monthly variation of the compass, from April 1817, to March 1819, as determined by Colonel Beaufoy.

From April, 1817, to March, 1819.		Years 1817, and 1818, westerly variation.	Years 1818, and 1819, westerly variation.	Differ. in morning, noon and evening.	Difference in 1817, and 1818.	Difference in 1818, and 1819.	Mean of the two.
April	Morning	24° 31′ 52″	24° 34′ 06″	*n* — *m*	12′ 51″	10′ 44″	11′ 48″
	Noon	— 44 43	— 44 50	*n* — *e*	8 45	8 14	8 30
	Evening	— 35 58	— 36 36	*e* — *m*	4 6	2 30	3 18
May	Morning	— 32 20	— 36 18	*n* — *m*	10 15	9 31	9 53
	Noon	— 42 35	— 45 49	*n* — *e*	7 50	7 14	7 32
	Evening	— 34 45	— 38 35	*e* — *m*	2 25	2 17	2 21
June	Morning	— 31 09	— 33 47	*n* — *m*	11 5	11 24	11 15
	Noon	— 42 14	— 45 11	*n* — *e*	8 9	7 31	7 50
	Evening	— 34 05	— 37 40	*e* — *m*	2 56	3 53	3 25
July	Morning	— 31 14	— 34 24	*n* — *m*	10 52	10 35	10 43
	Noon	— 42 06	— 44 59	*n* — *e*	6 23	6 45	6 34
	Evening	— 35 43	— 38 14	*e* — *m*	4 29	3 50	4 9
Aug.	Morning	— 31 16	— 34 40	*n* — *m*	11 35	11 18	11 26
	Noon	— 42 45	— 45 58	*n* — *e*	9 6	8 8	8 34
	Evening	— 33 45	— 37 50	*e* — *m*	2 29	3 10	2 52
Sept.	Morning	— 33 02	— 34 24	*n* — *m*	8 34	10 53	9 44
	Noon	— 41 36	— 45 22	*n* — *e*	6 58	7 54	7 26
	Evening	— 34 38	— 37 28	*e* — *m*	1 36	2 59	2 18
Oct.	Morning	— 31 06	— 35 36	*n* — *m*	9 40	7 52	8 46
	Noon	— 40 46	— 45 28				
Nov.	Morning	— 31 49	— 33 24	*n* — *m*	6 6	8 17	7 10
	Noon	— 37 55	— 41 41				
Dec.	Morning	— 34 03	— 37 04	*n* — *m*	3 59	4 16	4 07
	Noon	— 38 02	— 41 20				
Jan.	Morning	— 34 02	— 35 42	*n* — *m*	5 55	4 12	5 03
	Noon	— 39 57	— 39 54				
Feb.	Morning	— 34 22	— 34 17	*n* — *m*	6 29	5 38	6 03
	Noon	— 40 51	— 39 55				
March	Morning	— 33 18	— 33 18	*n* — *m*	8 19	8 24	8 22
	Noon	— 41 37	— 41 42	*n* — *e*	7 50	6 25	7 07
	Evening	— 33 47	— 35 17	*e* — *m*	0 29	1 59	1 15

Mr. Barlow has given a very ingenious and plausible explanation of the daily variation, by supposing the sun to possess a certain magnetic action on the needle. See his *Essay on Magnetic Attractions*, p. 96.

Sect. I.

Dip or inclination.

5. If a steel wire, supported horizontally upon its centre of gravity, be converted into a magnet, it will no longer remain horizontal. The north pole dips down towards the earth, while the south pole is elevated. This is called the *dip* or *inclination* of the magnetic needle. It was first observed by Robert Norman in 1576. He constructed an instrument to measure the angle which such a needle made with a supposed horizontal needle, and he states it for London at 71° 50′. If we carry such a needle south towards the equator, we observe that the dip diminishes in proportion with the diminution of latitude; till at last at a certain point of the earth's surface constituting the magnetic equator, the needle has no dip at all, but stands perfectly horizontal. When we pass this equator, the dip begins again to make its appearance; but it is the south pole which is now sunk towards the earth, while the north pole is elevated, and the dip of the south pole increases as the latitude increases.

To understand the reason of this dip, we have only to recollect that the earth is a magnet, and that its poles are situated below its surface. The directive property of the needle, is owing to the attraction of these poles. When the needle is on the north side of the magnetic equator, the earth's north poles will have the greatest effect, and the needle will point directly to that north pole, which is so situated as to have the greatest intensity relative to the needle. If the needle was directly over that pole, its north pole pointing to it would cause the needle to assume a position perpendicular to the horizon. The farther south it is, the greater will be the action of the earth's south poles. This will occasion a diminution of the dip. At the magnetic equator both the north and south poles will act with equal energy on the needle. The consequence must be that it will assume a direction perfectly perpendicular. To the south of the magnetic equator, the south poles of the earth will act more powerfully upon the needle than the north poles. Hence the south pole of the needle will begin to dip, and this dip will increase as we advance southwards.

Magnetic equator.

As the magnetic poles do not coincide with the terrestrial poles, it is obvious that the magnetic equator cannot coincide with the terrestrial equator. From the numerous observations made in different parts of the torrid zone, there is reason to conclude that the magnetic equator is distant at a maximum about 12° from the earth's equator. It would appear to be a great circle of the sphere cutting the earth's equator, in west longitude about 113°, or a little to the west of Gallego in the Pacific Ocean, and in east longitude 67° in the Indian Ocean. But in all parts of the Pacific Ocean, to the west of the Gallipago Node, there must exist another magnetic equator, as is obvious from the observations of Cooke, Bailey, and various other observers. It must cut the equator once, if not twice, between Gallipago and the Chinese Sea. Indeed, the existence of four terrestrial poles ought to give origin to different magnetic equators, in different parts of the globe. For a magnetic equator is merely a place where the north and south poles of a magnet are equally acted upon by the terrestrial poles. The nodes of the magnetic equator must of course shift their positions, as the magnetic poles of the earth by acting upon each other alter their direction. Probably they will be found to revolve slowly round the earth's equator.

The dip of the needle or the *inclination* in the same place, undergoes a slow change as well as the declination, and doubtless depending upon the same cause, the gradual alteration in the position of the earth's magnetic poles. The following table exhibits the amount of the dip in London, during an interval of 245 years, according to the best observations which have been made.

Years.	Dip.	Observers.
1576	71° 30′	Norman,*
1600	72 00	Gilbert,
1676	73 47	Bond,*
1720	75 10	Whiston,*
1723	75 00	Graham,*
1772	72 19	Nairne,†
1776	72 30	Cavendish,‡
1805	70 21	Gilpin,‖
1821	70 3	Sabine.§

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* As given by Cavendish, in his paper on the Meteorological Instruments of the Royal Society. Phil. Trans. 1776, p. 375. Some of Mr. Graham's observations only gave a dip of 73° 30′.

† Phil. Trans. 1772, p. 476. ‡ Ibid. 1776, p. 375.

‖ Ibid. 1806. Meteorological Table.

§ Ibid. 1822, p. 1.

From this table it appears, that the dip reached its maximum in London, about the year 1720, and that it has been diminishing ever since. But the difficulty of constructing an accurate dipping needle is so great, that all the observations hitherto made, can be considered only as approximations.

The following table exhibits the amount of the dip at Paris, in N. latitude 48° 50′ 14″, and East longitude from Greenwich 9′ 21″·6* during a series of 147 years.

1671	75° 0′	Picard.
1754	72 15	La Caille.
1776	72 25	Le Monnier.
1780	71 48	Cassini.
1798	69 51	Humboldt.
1810	68 50	†
1818	68 35	†

If we except the observations of Lemonnier, the dip has been constantly diminishing at Paris since the year 1671, when it probably exceeded the dip in London.

The annual diminution of the dip in London at present, amounts nearly to 3′.

6. Mr. Barlow of Woolwich, in his experimental investigations, in order to discover a method of correcting the deviation of the needle on shipboard occasioned by the iron on board the vessel, discovered that there is a plane passing through every iron sphere in which, if the centre of a needle be

Equator of a sphere.

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* See Phil. Trans. 1826, p. 77.

† These two copied from Dupretz Traité de Physique, p. 446.

situated, the iron sphere has no effect in altering the declination of the needle. This plane at Woolwich was inclined about 20° to the horizon. It was therefore *perpendicular* to the dip of the needle.* The reason of the non effect of iron upon a needle whose centre is in this plane is sufficiently obvious. The iron must act equally upon both poles of the needle. It cannot therefore have any tendency to alter its declination. When the needle is any where out of that plane, one of its poles being more attracted than the other, it deviates from its true position, and Mr. Barlow succeeded in determining the law of this deviation.†

Magnetism proportional to the surface.

7. Another very important general fact, for which we are indebted to Mr. Barlow, is that the intensity of the action of iron on a magnetic needle, is proportional to its surface, not to its solid contents. So that a hollow sphere of iron, however thin, has as great an effect upon a needle as a solid iron sphere of the same size.

Other magnetic bodies.

8. It was supposed at first that iron or substances containing ron, were alone susceptible of the magnetic influence. But it was afterwards observed, that cobalt and nickel are also magnetic metals, and that like steel they may be converted into permanent magnets. Coulomb shewed, that all bodies were to a certain extent susceptible of magnetic influence.

White hot iron not magnetic.

9. White hot iron has no effect upon the magne-

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* Essay on Magnetic Attractions, p. 6.

† See his Essay on Magnetic Attractions.

tic needle; but as the iron cools it begins to act, and it acts most powerfully at what is called a blood red heat. Mr. Barlow and Mr. Charles Bonnycastle observed, that between a bright red and a blood red heat, it acts in the opposite way from what it does when cold. That is to say, if it attracts the north pole of the needle when cold, it will attract the south pole at the temperature above specified.*

Needle deflected by motion.

10. Mr. Barlow ascertained, that when a magnetic needle is placed at a certain distance from a mass of iron, the needle is deflected when the iron is put into rapid motion. To observe the effects properly, the needle should be neutralized from the action of terrestrial magnetism, by magnets properly placed. When this is attended to, and the iron put in motion; if the direction of the motion be towards the needle, the north pole is deflected about 30° towards the iron; when the motion is from the needle, the north pole is deflected about 30° from the mass of iron. When the needle is in the line of the axis of motion, it suffers no deflection whatever.†

Magnet puts metallic discs in motion.

11. About the year 1824, M. Arago discovered, that when plates of copper and other substances are set in rapid rotation beneath a magnetized needle, they cause it to deviate from its direction, and finally drag it along with them. This curious fact was repeated and verified by Messrs. Herschel and Babbage in the year 1825. These gentlemen

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* Phil. Trans. 1822, p. 117.

† Ibid. 1825, p. 117.

then mounted a powerful compound horse shoe magnet capable of lifting 20 pounds, in such a manner, as to receive a rapid rotation about its axis of symmetry placed vertically, the line joining the poles being horizontal and the poles upwards. A circular disc of copper, 6 inches in diameter, and 0·85 inch thick, was suspended centrally over it by a silk thread without torsion, just capable of supporting it. A sheet of paper properly stretched was interposed, and no sooner was the magnet set in rotation, than the copper commenced revolving in the same direction, at first slowly, but with a velocity gradually and steadily accelerating. The motion of the magnet being reversed, the velocity of the copper was gradually destroyed; it rested for an instant, and then immediately commenced revolving in the opposite direction. This alteration of direction might be repeated at pleasure.

The rate of rotation was not diminished by interposing between the copper and the revolving magnet plates of paper, glass, wood, copper, tin, zinc, lead, bismuth, or antimony. But it was greatly diminished when the substance interposed was iron. One sheet of tin plate reduced the rate of rotation to about $\frac{1}{5}$th part, and two sheets almost destroyed it altogether.

When discs of other metals were substituted for copper, they were found also capable of being made to rotate, though with different degrees of rapidity. The order of the different metals, and the intensity of the force of each, that of copper being reckoned 1, was as follows:

	Force.
Copper . . .	1
Zinc	0·90
Tin	0·47
Lead	0·25
Antimony . . .	0·11
Bismuth . . .	0·01
Wood . . .	0·00

When these discs were cut with a pair of scissors in various radii, stretching from the circumference nearly to the centre, the force was greatly weakened ; but it was nearly restored again by soldering these intervals with any metal whatever.

Silver was found to hold a high rank among the metals, and gold a very low one. Mercury lies between antimony and bismuth. The only other substance in which any traces of magnetism has been perceived besides the metals, is charcoal in that curious state in which it is deposited at a red heat from coal gas, lining the retorts and rendering them gradually useless.

Copper revolving produced no effect upon copper. It was always necessary in order that motion should be produced, that one of the bodies should be a magnet. It is obvious from this that magnetism is introduced into copper, and the other metals simply by induction. Messrs. Babbage and Herschel have explained, in a very ingenious manner, how in consequence of this magnetism by induction, the rotations produced in these experiments may be explained.*

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* Phil. Trans. 1825, p. 467. See also valuable experiments on the same subject by Mr. Christie, ibid. p. 497.

Mode of measuring the intensity of the directing force.

12. The intensity of the directive force of a magnetic needle, may be measured by causing the needle to vibrate in small arches, and counting the number of vibrations performed in a given time: the intensity of the directing force being inversely as the square of the number of vibrations. Precisely as the variation of gravitation is determined by transporting the same pendulum from one part of the earth to another, and reckoning the number of vibrations made in a given time, the force of gravitation, as is well known, is inversely as the square of the number of vibrations. It is easy to show that the same law must hold with respect to the directive force of magnetism.

I have endeavoured, in the preceding observations, to lay a short view of the principal facts respecting magnetism before the reader, without entering into minute details or attempts to connect them by any theory; though the connexion between electricity and magnetism is so close, that the same theory slightly modified must apply to both. There may be conceived to exist two magnetic fluids, the *boreal* and *austral.* The particles of the same fluid repel each other, while boreal magnetism attracts austral with a force inversely as the square of the distance. This hypothesis, with certain suppositions respecting conducting and non-conducting bodies, would enable us to explain all the phenomena of magnetism with considerable success. But the object of the preceding sketch was merely to enable the reader to understand the facts respecting electro-magnetism which I am now going to state, and which, for some years past, have drawn much of the attention of the scientific world.

SECTION II.

OF ELECTRO-MAGNETISM.

It had been observed as long ago as the time of Dr. Franklin, that electricity has a sensible action on the magnetic needle. After the discovery of galvanism, many curious observations on the subject were made by Ritter; but they were too vague to lead to any important conclusions. The great discovery was made by Professor Œrsted of Copenhagen in the year 1819.

Action of the wire of a galvanic pile on the needle.

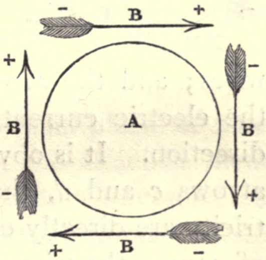
1. He ascertained that if a magnetic needle be presented to the wire which unites the two poles of a galvanic pile in full action, it is attracted by it, and the needle has a tendency to arrange itself at right angles to the conducting wire. If the needle be *above* the conducting wire, (supposed in the magnetic meridian), the *north* pole turns to the *left* of the current of positive electricity. If it be below the conducting wire, the same pole turns to the *right.* When the needle is placed on the *west* side of the conducting wire, (supposed to be nearly in the magnetic meridian,) and parallel to it, the north pole will be elevated. But when placed on the east side of the conducting wire, the north pole is depressed.

2. The phenomena of electro-magnetism are best observed when only a single pair of plates is used, and the larger the size of the plates, the better can the phenomena be observed. A good

apparatus for the purpose consists in a box of copper composed of two copper plates each about a foot square, and about $\frac{3}{4}$ths of an inch distant. A bottom and ends of copper are soldered to their sides so as to complete the box. In the centre between the two sides and not touching the copper anywhere, a zinc plate of nearly the same size as the copper, is fixed either by means of baked wood or sealing wax. When this vessel is filled with dilute nitric acid, and the copper and zinc plates are connected by a wire, most of the phenomena of electro-magnetism may be exhibited by means of this wire.

Action on the needle explained.

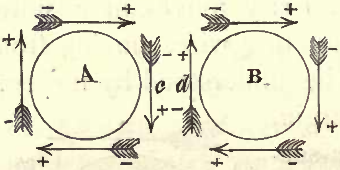
3. To form a conception of the cause of this difference in the declination and inclination of the needle according to its position, we may suppose that the current of electricity moves in a spiral direction round the conducting wire, moving from left to right. This will be understood by inspecting the figure in the margin, in which A represents a section of the conducting wire, and the arrows B, B, B, B represent the direction in which the current of electricity moves round A. It is obvious from the position of these arrows that the action on the magnetic needle ought to be opposite above and below, and likewise upon the left and right of the conducting wire.



4. M. Ampere, to whom we are indebted for an investigation of the phenomena of electro-magnet-

Electric currents moving the same way attract.

ism conducted with much sagacity, discovered that when two electric currents are moving in the same direction in conducting wires parallel to each other they *attract*, but when moving in opposite directions they repel. He contrived to place parallel to each other two conducting wires, uniting the negative and positive poles of a galvanic battery. One of these conducting wires was moveable, and the consequence was, that it was attracted by and made to approach the immoveable conducting wire. When the currents of electricity were moving contrary ways, the moveable conducting wire was repelled and receded from the immoveable. As similar electricities repel and dissimilar attract, this phenomenon may appear at first sight inconsistent with the laws of electricity. But the simple inspection of the figure in the margin will serve to render it intelligible. A and B are sections of the two conducting wires; and the arrows indicate the way in which the electric currents move along them in a spiral direction. It is obvious from the directions of the arrows *c* and *d*, that negative and positive electricity are directly opposite to each other, and that of course the wires ought to attract each other as they are found to do. This way of accounting for the phenomena of electro-magnetism was first employed by Œrsted. It was afterwards used by others; particularly by Dr. Wollaston and M. Ampere, with much felicity.



5. M. Arago and Sir Humphrey Davy observed about the same time that the conducting wire while the pile is in activity attracts iron filings; but the moment the contact is interrupted, the filings drop off. But bodies not magnetic are not attracted by it; showing that the phenomenon cannot be explained upon the common principles of electrical attraction.

A spiral wire communicates magnetism.

6. Arrago observed that if a copper wire be wound spirally round a glass tube, and then made to unite the two poles of a galvanic pile, if a steel needle be introduced into the inside of the tube, it is instantly converted into a magnet. If the needle be left too long in the tube, the magnetic properties are again weakened or even destroyed. The best way is to hold the needle in the hand, and introduce it about half way into the tube and then withdraw it, the magnetic virtue will be acquired as completely as if it had been wholly within the tube. If common electricity be made to pass along the spiral conducting wire, the needle is equally converted into a magnet.

7. M. De la Rive fixed two small plates of zinc and copper beside each other, and attached them to a piece of cork. The two plates were connected together by a brass wire wound into a spiral. When this little apparatus was placed upon the surface of an acid liquid, the plate being undermost and allowed to float at pleasure, it arranged itself in the magnetic meridian in consequence of the polarity of the spiral wire.

8. Advantage was taken of this property which

a spiral wire has of increasing the intensity of the action of the electric current on the magnet, by Schweigger and Poggendorf, to construct a very useful little instrument called the *electric multiplier*. By its means the presence of a much smaller current of electricity can be detected, than by any other known means. A common brass wire is taken and coiled round with silk thread, in order to insulate the wire. This wire is then lapped round the hand or a piece of wood 50, 100, or 200 times. The greater the number of repetitions, the more powerfully does it act. This hank of wire is kept in its position by tying it with silk threads, and the two extremities of the wire are left loose and of some length. When these two extremities are brought in contact with a galvanic pile in action, the current of electricity passes through all the windings of the wire. If we place a magnetic needle very moveable upon a proper support in the middle of this hank of wire, it is immediately affected and tends to turn itself so as to be at right angles to the hank. The power of the multiplier increases with every turn of the wire. It may therefore be made so powerful as to detect exceedingly minute electrical currents. Various modifications of this multiplier have been made. A ribbon of copper covered with silk and wound a great many times round and fixed in a frame answers very well.*

Electric multiplier.

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* Those who are curious on this subject, will find the different modifications of the doubler described in Gilbert's Annalen der Physik. Tom. lxvii. lxx. lxxi. lxxii. lxxiii.

Discoveries of Faraday.

9. Next to the discoveries of Professor Œrsted, those of Mr. Faraday on electro-magnetism* are by far the most important. He first observed that the position of the magnetic needle with respect to the conducting wire, greatly modified the effects produced. He ascertained that the apparent attraction of the needle on one side, and its consequent repulsion on the other, did not occur under all circumstances; but that according as the wire was placed nearer to or farther from the pivot of the needle, attractions or repulsions were produced on the same side of the wire. When the wire is made to approach perpendicularly towards one pole of the needle, the pole will pass off on one side in that direction which the attraction and repulsion at the extreme point of the pole would give. But if the wire be continually made to approach the centre of motion by either the one or the other side of the needle, the tendency to move in the other direction diminishes: it then becomes null, and the needle is quite indifferent to the wire. And ultimately the motion is reversed, and the needle powerfully endeavours to pass the opposite way.

From these facts, Mr. Faraday concluded that the centre of magnetic action or the true pole of the needle is not placed at its extremity, but in its axis at a little distance from the extremity and towards the middle,—that this point has a tendency to revolve round the wire, and necessarily therefore

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* They will be found in the 12th volume of the Journal of the Royal Institution.

the wire round the point. And as the same effects in the opposite direction take place with the other pole, Mr. Faraday concluded that each pole had the power of acting on the wire by itself, and not as any part of the needle or as connected with the opposite pole. The attractions and repulsions, he viewed merely as exhibitions of the revolving motion in different parts of the circle.

Rotation of the wire round the magnet.

After many unsuccessful attempts, Mr. Faraday at last succeeded in verifying the existence of this revolving motion by the following ingenious apparatus. A small quantity of mercury was put into the bottom of the tube A, (see fig. in the margin,) closed below with a cork, and a small magnet, *b*, was passed through the cork, and made to project above the surface of the mercury. A piece of clean copper wire, *c*, was taken about two inches in length, and amalgamated at both ends. A small loop was formed at one end, by which it was suspended from a wire, passing through a cork in the upper extremity of the tube, and terminating in another loop. This gave the copper wire free motion. It was of such a length, that the moveable end just dipped into the mercury. The mercury was now connected with one of the poles of a galvanic battery by means of the magnet, while the wire at the top of the tube was connected with the other pole. The moveable copper wire immediately began to revolve round the magnetic pole, and continued to do so as long as the contact continued. On bringing the magnetic pole from the centre of motion to the side of the wire, there was neither attraction nor repulsion; but the wire endeavoured to pass off in a circle, still leaving the pole for its centre, and that either on one side or the other according to circumstances.

All the directions of the motion are reducible to two. When a current of electricity passes through the wire, the north pole rotates in one direction, and the south pole in the other. Suppose the extremity of the moveable wire dipping into the mercury to be negative, then the motion is in the same direction as that of the hands of a watch, if we suppose the watch lying with its face corresponding with the surface of the mercury in the tube. If the lower point of the moveable wire be positive, the revolution takes place in the contrary direction. If the wire be made fast, and the magnetic pole be made to revolve round, the motion is similar and in the same direction.

This first simple arrangement was varied exceedingly by Mr. Faraday and other persons, and a great variety of pieces of apparatus contrived to exhibit the revolutions of magnetic poles round conducting wires, or rather indeed of conducting wires round magnetic poles.* Many curious and amusing exhibitions have been the result, though very

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* These, it would not suit the limited views of the author of this very short sketch, to describe here. A considerable number of them will be seen figured and described in Mr. Watkins' Popular Sketch of Electro-magnetism and Electrodynomics, published in 1828. Taylor, London.

Chap. XV. little has been added to Mr. Faraday's original and important discovery.

These curious phenomena seem to me to throw some doubt on the theory, originally proposed by Professor Œrsted, to explain the facts which he had discovered in electro-magnetism. He supposes that two opposite currents of electricity are moving in spirals from the two different poles of the galvanic battery. From the positive pole there passes a current of positive electricity to the negative pole, and from the negative pole, a current of negative electricity to the positive pole. These two currents move in opposite spirals, yet though they must be continually crossing each other, they cannot combine, otherwise there would be an end of the electricity. For when the two electricities unite, all excitement disappears.

According to Œrsted the *negative* electricity propels the *north* pole of a magnet, but has no effect upon the south; while *positive* electricity propels the *south* pole, but has no power over the north pole.

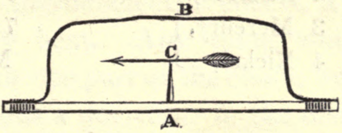
The most elaborate theory of electro-magnetism, is that of Ampere, to whom we are indebted for some very ingenious modifications of the apparatus, and some discoveries of considerable importance. He refers magnetism entirely to electricity, and explains all the phenomena of magnetism by the action of electrical currents, moving at right angles to the magnetic meridian. It would be impossible to discuss this theory, or even to render it intelligible to the reader, without entering into details which would swell this sketch to a length, greatly beyond what would be proper. I must therefore refer the readers who wish to investigate the nature of Ampere's theory, to his own papers on the subject of Electro-magnetism, in the 15th and some subsequent volumes of the Annales de Chimie and de Physique. They will amply repay the trouble of a careful perusal. For, even if we should feel disposed to reject the theory, as too bold for the present state of our knowledge, the multiplicity of new and important facts which Ampere's papers contain, and the ingenuity and plausibility with which he supports his views, must always give them a distinguished place in the Annals of Electro-Magnetism.

SECTION III.

OF THERMO-ELECTRICITY.

Electricity evolved by heating two metals.

The magnetic needle is so delicate a test of the presence of an electric current, however small, that it has enabled men of science to discover its existence, where it was not before suspected to exist. Sebeck discovered that if a bar of antimony, A, (see fig. in the margin,) eight inches long, and about an inch wide, have a slip of copper B, or a copper wire soldered, or firmly tied to its two extremities, and bent into a rectangular shape, if we apply the heat of a lamp to one of the extremities of the antimony bar, a needle placed



within the current as at C, will be deflected from the magnetic meridian, and will tend to place itself at right angles to the antimony bar; showing that a current of electricity is passing through the circuit composed of the antimony and copper.* Dobereiner repeating this experiment, the bar A being of antimony, and B of copper, found that the heat of the hand was sufficient when applied to one of the extremities of A, to cause the needle to deflect ten or twelve degrees from the magnetic meridian.†

Of the metals, bismuth and antimony are the two which produce the greatest effect, when they are used together; bismuth being the most negative, and antimony the most positive of all the metals hitherto tried, as thermo-electrics.

Order of the metals.

An important series of experiments was made upon the metals, as termo-electrics, by Dr. Trail of Liverpool, and Mr. Scoresby,‡ and likewise by Professor Cumming of Cambridge.‖ He found the order of the metals, as thermo-electrics, beginning with the most negative, and terminating with the most positive, as follows:

1 Galena,
2 Bismuth,
3 Mercury, }
4 Nickel, }
5 Platinum,
6 Palladium,
7 Cobalt, }
Manganese, }

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* Annals of Philosophy, (2d Series,) iv. 318. And Gilbert's Annalen der Physik, lxxiii. 430.

† Ibid. p. 115.

‡ Edinr. Phil. Trans. ix. 465, and Annals of Philosophy, vi. 449.

‖ Annals of Philosophy, (2d Series,) v. 427; vi. 177, 288, 322.

8 Tin,
Lead,
Brass,
Rhodium,
Gold,
Copper,
Ore of iridium and osmium,
Silver,
Zinc,
Charcoal,
Plumbago,
Iron,
Arsenic,
Antimony.

The place of *tellurium* in the series is immediately above *antimony*.

Every one of the metals in this series gives a *western* declination to the north pole of a magnet placed immediately below it, when it is united with any of those that precede it in the list; and an eastern declination when it is united with any of those that follow it in the series; it being understood that a portion of the circuit is heated by the application of a spirit lamp.

It is obvious that the above order of the metals is very different from that which they follow when placed in the galvanic pile along with liquid conductors. No satisfactory explanation of this difference has yet been given. It is probably connected with the goodness of the metals, as conductors of heat.

Heat not necessary for these effects.

In order to produce thermo-electric effects, it is not necessary to apply heat. Any thing which alters the temperature in one part of the chain, from that of the rest, occasions a deviation in the declination of the magnet: for example, if we produce cold in any part of the antimony bar, by applying ether to it, and allowing it to evaporate; or if we cool it by the application of ice. The greatest effect of all is produced on the magnet, when one part of the bar is heated, and the other cooled. It is evident from this, that the evolution of electricity depends upon the difference in the temperature of different parts of the metallic chain.

But a difference of temperature.

Seebeck has even ascertained, that in order to produce thermo-electric effects, two different metals are not necessary.* A single metal will answer, provided there be differences in its texture and cohesion. Seebeck cast rings of antimony, bismuth and zinc, and took care that certain parts of them were cooled much more rapidly than others. The parts rapidly cooled assumed a fine grained texture, and a greater degree of density, while the other portions of the ring that were slowly cooled became crystalline in their texture. When these rings were heated at the point of contact of the different textures, they acted upon the magnet. There is a curious experiment of Becquerel, which proves the same thing. He heated to redness, one of the ends of the wire which constitutes the multiplier of Schweigger, and brought it, while in this state, in contact with the other end of the wire which was cold. The needle immediately suffered a declination, shewing the evolution of a current of electricity. The thicker the wire of the multiplier is, the more easily does this experiment succeed ; probably because the difference of temperature immediately disappears in very small wires. When the two extremities of the

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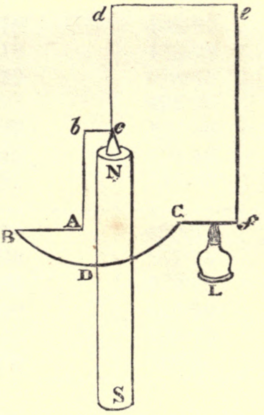
* Gilbert's Annalen der Physik, lxxiii. 431.

multiplier are soldered together, or when heat is applied at a distance from the point of contact, no sensible action is produced on the magnet. But if we touch the wire in the neighbourhood of the heated place, with a thick cold wire of the same metal, the magnet is immediately affected. It would seem from this, that a mere difference of temperature is not sufficient to evolve electricity, but that the heat must also be abstracted more rapidly from one of the sides of the heated point, than from the other side.

Even liquids may be made to act as thermo-electrics. Seebeck found that concentrated acids, for example, the sulphuric, nitric, and muriatic, occupy a place above bismuth, while the concentrated fixed alkalies stand at the other extremity of the series below antimony. When the acids are diluted with water, they approach the middle of the series. But water produces no effect upon the alkalies. Water and liquid ammonia occupy very nearly the middle of the series.

Thermo-electric rotation.

Professor Cumming discovered, that rotatory motions may be induced by thermo-electrics, as well as by common electro-magnetism. The figure in the margin represents the contrivance which he employed. Platinum and silver wires were soldered together, poised upon a magnet and heated by a spirit lamp. A B D C represents the platinum wire,



A *b c d e f* C the silver wire. N S is the magnet, *c* N the support of the wires, and L a spirit lamp. The platinum wire is so much thicker than the silver wire, that the part A B balances the projecting part of the silver wire *d e f* C. A wire is attached to *d e* at right angles, with a small weight to counterbalance B D C.* He afterwards found it more convenient to bend the parallelogram into the form of a semicircle. When the lamp and magnet are placed opposite to each other, the rotation is produced. But the effect is improved by placing another magnet 90° from the first, having its poles in the contrary direction, and being connected with it by a bar of soft iron placed beneath them. With this arrangement, the rotation will be from right to left, or from left to right, according to the position of the lamp.†

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* Annals of Philosophy (2d Series), vi. 179.

† Ibid. p. 436.

APPENDIX.

ELASTICITY OF STEAM.

Since the table given in page 215 of this volume was printed, a most elaborate set of experiments has appeared, by a Committee of the Royal Academy of Sciences of Paris, consisting of Prony, Arago, Gerard, and Dulong. These experiments were the result of an application of the French government to the Academy, to point out the best means of preventing accidents from the bursting of the boilers of steam engines. The following table exhibits the elasticity of steam at various temperatures, till it amounts to 24 atmospheres. An atmosphere is measured by a column of mercury of 29·922 inches (0·76 metre), which has been adopted in France as the mean height of the barometer at the surface of the sea.

Elasticity of steam, the pressure of the atmosphere being 1.	Corresponding temp. in deg. of Fahrenheit.	Elasticity of steam, the pressure of the atmosphere being 1.	Corresponding temp. in deg. of Fahrenheit.
1	212°	13	380·66
1½	234	14	386·94
2	250·5	15	392·86
2½	263·8	16	398·48
3	275·2	17	403·83
3½	285	18	408·92
4	293·7	19	413·78
4½	300·3	20	418·46
5	307·5	21	422·96
5½	314·24	22	427·28
6	320·36	23	431·42
6½	326·26	24	435·56
7	331·7		
7½	336·86	25	439·34
8	341·78	30	457·16
9	350·78	35	472·73
10	358·88	40	486·59
11	366·85	45	499·14
12	374	50	510·6

The last six temperatures in the table are deduced by calculation. The Commissioners of the Academy employed the formula $e=(1+0{\cdot}7153\,t)^5$. In this formula e expresses the elasticity in atmospheres of 0·76 metre (29·922 inches) pressure, and t the temperatures setting out at 100° centigrade (212° Fahrenheit). For the methods employed to determine the temperature at the various elasticities in the table, and the precautions taken to avoid errors, I refer the reader to the report inserted in the Annales de Chimie et de Physique, xliii. 74. The last column in page 217 of this volume, taken from the Annales de Mines, was from an interim report of the Commissioners to the French government, and was deduced by calculation from the best previous experiments. By comparing the table here given with the determination of Perkins, given in page 217, on the authority of Clement, we see how erroneous Perkins' experiment must have been. As he never himself published the result, it is probable that he was himself aware of its inaccuracy.

This table of the French Academicians constitutes a precious addition to our knowledge of the elasticity of steam. The experiments, which were conducted by Dulong and Arago, constitute some of the most difficult and even dangerous for which science is indebted to the zeal and intrepidity of philosophers. Mercury, by the elasticity of the steam, was forced up a tube of glass to the height of 42·6517 feet. The diameter of the tube was 0·1968 inches. They took the opportunity of subjecting common air to the pressure of from 1 to 24 atmospheres, and found that the Boylean law, namely, that the bulk diminishes as the pressure increases, holds as far as twenty-four atmospheres.

INDEX

THE END.

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