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* The three last articles are, in the text, erroneously numbered XIX. XX. and XXI.

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II. Foreign.

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Number 2.

INTELLIGENCE AND MISCELLANIES.

I. Domestic.

II. Foreign.

ERRATA.—VOL. XII.

Page 18, line 5 from top, and passim, for *Bayshot* read *Bagshot*.
" 21, line 13 from top, for *Pupivere* read *Pupivore*.
" 21, line 19 from top, for *Dadder* read *Dodder*.
" 22, line 17 from top, for *Ricard* read *Ricord*.
" 22, line 29 from top, for *Cain* read *Coin*.
" 23, line 32 from top, for *devour* read *devours*.
" 23, line 33 (also p. 25, l. 19 and 28, and p. 26, l. 16 and 38) for *larva* read *larvæ*.
" 23, line 34 from top, after *penetrate* read *the*.
" 25, top line, for *Tillandria asneoides* read *Tillansia usneoides*.
" 25, line 7 from top, for *lepedopterous* read *lepidopterous*.
" 25, line 30 from top, for *cicada* read *cicadæ*.
" 27, line 8 from top, after *substance* read *as*.
" 27, line 9 from top, before *remain* dele *but*.
" 28, line 2 from top, for *his analysis* read *its analysis*.
" 29, line 22 from top, for *bodies* read *body*.
" 31, line 6 from top, for *Alexandrine* read *Alexandrian*.
" 31, line 11 from top, before *even* dele *not*.
" 34, line 2 from bottom, for *equally* read *equably*.
" 48, title, for *Corbonicometer* read *Carbonicometer*.
" 52, line 3 from bottom, for *learning* read *reasoning*.
" 124, line 5 from bottom, for *circle vcl G* read *circle vl Gw*.
" 126, line 4 from top, for = read ×.
" 126, line 13 from top, for *But* CeD read *But* Ce.
" 126, line 14 from top, for *am* : *a*S : : Cc : CS, and *dn* : *dt* (or *a*S) Ce : Ct read *am* : Ce : : *a*S : CS, and *dn* : Cc : : *dt* (or *a*S) : C*t*.
" 126, line 11 from bottom, for *refutation* read *repetition*.
" 130, for P read D.
" 131, line 12 from top, for *thereby* read *and then by*.
" 321, line 6 from bottom, for *discharging* read *charging*.

STEAM BOAT BABCOCK

THE

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JOURNAL OF SCIENCE, &c.

Article I.—*On the present state of Chemical Science.*—By Denison Olmsted, Professor of Mathematics and Natural Philosophy in Yale College.

[Continued from Vol. XI. p. 358.]

Under the head of Attraction, by far the most interesting investigations, that have been made within a few years past, are those which relate to the subject of Definite Proportions, and the Atomic Theory. By the subject of Definite Proportions, is not to be understood any thing of the nature of visionary hypothesis or abstract speculation, but a class of well established facts, confirmed by the most rigorous experiments. A difficulty is sometimes experienced by the chemical student, in comprehending this doctrine; but it is believed to be owing more to the vague and immethodical manner, in which the doctrine is treated of in some of the elementary works, than to any thing intrinsically obscure in the doctrine itself. The principal facts respecting Definite Proportions, may be comprehended in four short propositions.

Proposition 1. *The elements of any compound always maintain the same ratio to each other.* Thus, in sulphuric acid, two parts of sulphur are always combined with three of oxygen; and if, during the formation of the acid, either ingredient be present in excess, that excess will remain uncombined. Water consists of two measures of hydrogen and one of oxygen. These elements, when fired together, immediately combine and form the liquid; and if there be an excess of either of them, so much of that ingredient will remain after the water is formed. The same holds true with every chemical compound whatsoever.

Proposition 2. *The respective quantities of any number of alkaline, earthy, or metallic bases, required to saturate a given quantity of any acid, are always in the same ratio to each other, to what acid soever they be applied.* For example, let us take potash and soda for the bases, and sulphuric acid for the acid. It is found by experiment, that two parts of soda will saturate as much of the acid as three parts of potash. The meaning of the proposition is, that the same rule will hold good with respect to all the other acids: in all cases, two parts of soda will saturate as much of an acid as three of potash. Having, therefore, ascertained the ratio between these two bases in respect to their power of saturating any one acid, as the sulphuric, we know of course the respective quantities of each required for any other acid, as the nitric, the muriatic, and fifty others. If I ascertain by experiment that it takes two ounces of soda to neutralize a certain portion of nitric acid, I know without an experiment that it will take three ounces of potash to do it; because a previous application of these two bases to sulphuric acid, showed that the ratio of their saturating power was as two to three.—Moreover, the same rule holds true with regard to all the bases. If I apply them all to a certain portion of one acid, I ascertain, by experiment, the respective quantities of each required to neutralize it. I then proceed to another acid, suppose the nitric. Here I have only one experiment to perform, namely, to ascertain how much of one of the bases it takes to neutralize a certain portion of the acid; I then know, without experiment, how much it will take of each of the other bases to do it, because the previous application of them all to the sulphuric acid, had taught me the ratio of their saturating powers; and the proposition asserts that this ratio is the same for one acid as for another. We must then ascertain, by experiment, the ratio of all the bases to one of the acids, and of one of the bases to all the acids, and our work is done. Suppose that we have 50 acids and 100 bases, consisting of alkalies, earths and metals. We must apply, first, each of the bases to one of the acids, which would imply 100 experiments; and, secondly, one of the bases to the remaining 49 acids, making, together, 149 experiments. Without the knowledge of the law under consideration, to ascertain the same facts it would be necessary to find, by experiment, how much of each of the 100 bases it required to saturate each of the 50 acids, implying 5000 experiments. Now, 149 : 5000 : 1 : $33\frac{83}{149}$; that is, the labour is reduced more than 33 times, and we have the great advantage of the accuracy of numerical ratios, instead of experiments, which, when they become so numerous, are apt to be more or less imperfect. In illustrating this subject to the learner, I have found an advantage in placing before him a row of spheres, like marbles, to represent acids, and another row of cubes, like dice-blocks, to represent bases. It will then be obvious how much less labour is implied in applying first, all the cubes to one of the spheres, and, secondly, one of the cubes to all the spheres, than in going through the entire process of applying each cube, successively, to all the spheres.

Proposition 3. *The respective quantities of any number of acids, required to saturate a given quantity of any base, are always in the same ratio to each other, to what base soever they be applied.* In this proposition, the same relation is declared to exist between the acids, with respect to their respective powers of saturating any base, as was declared, in Proposition 2d, to exist between the bases, with regard to their respective powers of saturating any acid : and the illustrations in this case, are similar to those of the other.

The respective quantities of several bodies which produce the same effects in combination, are called *chemical equivalents.* Thus, in the example given under Proposition 2, two parts of soda and three of potash are equivalents, because one saturates just as much acid as the other. This fact may be generalized; and it is a most interesting and curious fact, that the ratios between the weights of all bodies, that are capable of entering into chemical combination, whether simple or compound, are constant, and may be accurately expressed by numbers, being all referred to a common standard of unity. Thus, by inspecting a table of chemical equivalents, we may perceive that the numbers 1, 2, 3 and 4, are attached to the substances A, B, C and D, respectively; signifying that when A and B are found in combination with each other, the quantity of A is one half that of B. In like manner, it is one third that of C, and one fourth that of D. So the number 2, which is the representative number of B, imports that the body B, when it enters into combination with the others, has to each of them a ratio which is expressed by the number 2, and the several numbers attached to those bodies respectively. Thus the quantity of B is to that of A, as 2 to 1; to that of C, as 2 to 3; and to that of D, as 2 to 4. We might extend the number of elements to one hundred, or one thousand, and we should still find the same curious law obtaining; namely, that when any two, or three, or even the whole number of the series, entered into combination, their respective quantities would be in the same ratio to each other, as the numbers attached to them as equivalent quantities.*

The utmost facility of calculation is imparted to the subject of chemical combinations and decompositions, by Dr. Wollaston's Scale of Chemical Equivalents. If a series of numbers, beginning with 10 and increasing by 1, be written under each other at such distances that the intervening spaces shall be the measures of the ratios between any two numbers, such a scale will be a line of numbers;—equal distances will denote equal ratios. The distances between 50 and 100, will be the same as that between 1 and 2, because 50 : 100 : : 1 : 2. Now, if we write opposite to the numbers on this scale, the bodies of which the numbers themselves are the equivalents, then the distances between these bodies will, in like manner, be the measures of the ratios of their combining quantities, and will be the same with the distances between the numbers. So far the scale amounts to little else than a synoptic table of chemical equivalents; but the excellence of this arrangement is, that by means of the slide, we can instantaneously solve a great number of cases which arise out of combinations and decompositions, the solution of which, in the ordinary way, would require a tedious number of computations. Without moving the slide, the scale tells us that the equivalent of common salt (Muriate of Soda) omitting fractions, is 74—that it consists of 34 parts of muriatic acid, and 40 of soda; and that if we would decompose 74 grains of salt by Nitrate of Barytes, we must employ 166 grains of this agent, and we should obtain, by double decomposition, 132 grains of Muriate of Barytes, and 108 grains of Nitrate of Soda, all of which numbers will be found on the scale opposite to their respective bodies, because all these are

*The smallest quantities capable of entering into combination are here understood.

equivalent quantities. But instead of 74 grains of salt, which is the number on the scale belonging to muriate of soda, we happen, in a certain case, to have 100 grains; and, in order to obtain the particulars above mentioned, we must institute a proportion for each one, in order that each may have the same ratio to 100 grains, as the numbers on the scale have to 74. But, by setting the slide so as to have the number 100 stand against muriate of soda, the required quantities of all the other bodies will be indicated by the numbers standing opposite to them on the slide. This result depends on the principle that, if we move the slide either way, and ever so much, the numbers that stand opposite to the various bodies, will remain constantly in the same ratio to one another. Thus, before moving the slide, 20 stood opposite to sulphur, and 10 opposite to oxygen; now 100 is placed against sulphur, and we find that 50 stands against oxygen; and 100 is to 50 as 20 to 10. This constancy in the ratios between the numbers that are found opposite to the various bodies in any position of the slide, results from the peculiar property of the line of numbers, where equal ratios are measured by equal distances, as will be easily comprehended by those who are acquainted with Gunter's sliding rule.

After the constancy of the proportions in which bodies enter into chemical combinations was discovered, chemists began to contemplate, with unusual interest, certain other phenomena which attend those cases, where several different bodies are formed by the union of the same elements. Hence they were led to remark the following law.

Proposition 4. *When two substances, A and B, unite so as to form several different compounds, let the quantity of A remain the same in them all, then the respective quantities of B will be such, that all the higher proportions will be in a ratio to the lowest which may be expressed by whole numbers.** Thus, a given quantity of sulphur forms four different compounds, by combining with different quantities of oxygen; and these quantities are to each other as the number 2, 4, 5, and 6. The sub-carbonates contain just half as much carbonic acid as the neutral carbonates. Carbonic oxide has just half as much oxygen as carbonic acid. A similar defi-

* It is commonly said that all the higher proportions of B are *simple multiples* of the lowest. But this is not always true: thus in the example which follows, the third proportion (5) is obviously not a multiple of 2.

niteness of proportion has been observed also among gaseous bodies, whether the combining quantities are estimated by weight or by volume. Water, for example, is constituted of just two measures of hydrogen and one of oxygen; and a remarkable instance of the same kind occurs in the combinations of nitrogen with different quantities of oxygen, which are to each other, respectively, as the numbers 1, 2, 3, 4, and 5. Nor in all the foregoing cases, is it known that any intermediate compound exists to destroy the harmony of these proportions. The only question which can arise here, is whether the same *definiteness* governs *all* chemical combinations, or whether some constituents of a compound do not unite in every proportion, without observing such distinct gradations as are apparent in other cases? If we hold melted lead on the fire in a ladle, a yellow oxide will form on its surface. By augmenting the heat, this yellow oxide will pass through a great number of shades of colour, by imperceptible gradations, until it becomes a bright red. All these shades of colour arise, from the different quantities of oxygen which successively combine with the lead, and the question occurs, do not these gradual changes of colour oppose the idea of distinct stages or gradations in the process? According to the doctrine of definite proportions, ought not the lead to combine with one dose of oxygen, and then to refuse any more until it can receive as much more *all at once*? All these insinuations against the doctrine, may be set aside, by observing (what undoubtedly happens) that only a *part* of the lead is reduced to the next stage of oxidation at each successive moment; and the red and yellow oxides being blended together, the mixture assumes different shades of colour according as one or the other predominates. A number of cases of this kind occur in chemical combinations, where it is difficult, at the first view, to see the operation of the law of definite proportions. But more attention will frequently lead to the detection of some circumstance, which shows that the case is not an exception to that law—its operation was merely concealed. Still, the foregoing law of definite proportions is most apparent in cases where the strongest affinity prevails, and is hardly discernible in combinations of a feebler kind.

Definite Proportions and Atomic Theory, are phrases used by some writers with little discrimination, as though both implied the same thing. But they differ widely from each other. The laws of definite proportions are a class of facts established by rigorous experiments: the object of the Atomic Theory is to account for those facts. It is a very ingenious structure, and derives a high probability from its affording so complete an explanation of the foregoing laws of attraction; but were the whole doctrine of atoms discarded, the truth of the propositions respecting definite proportions would remain unshaken.

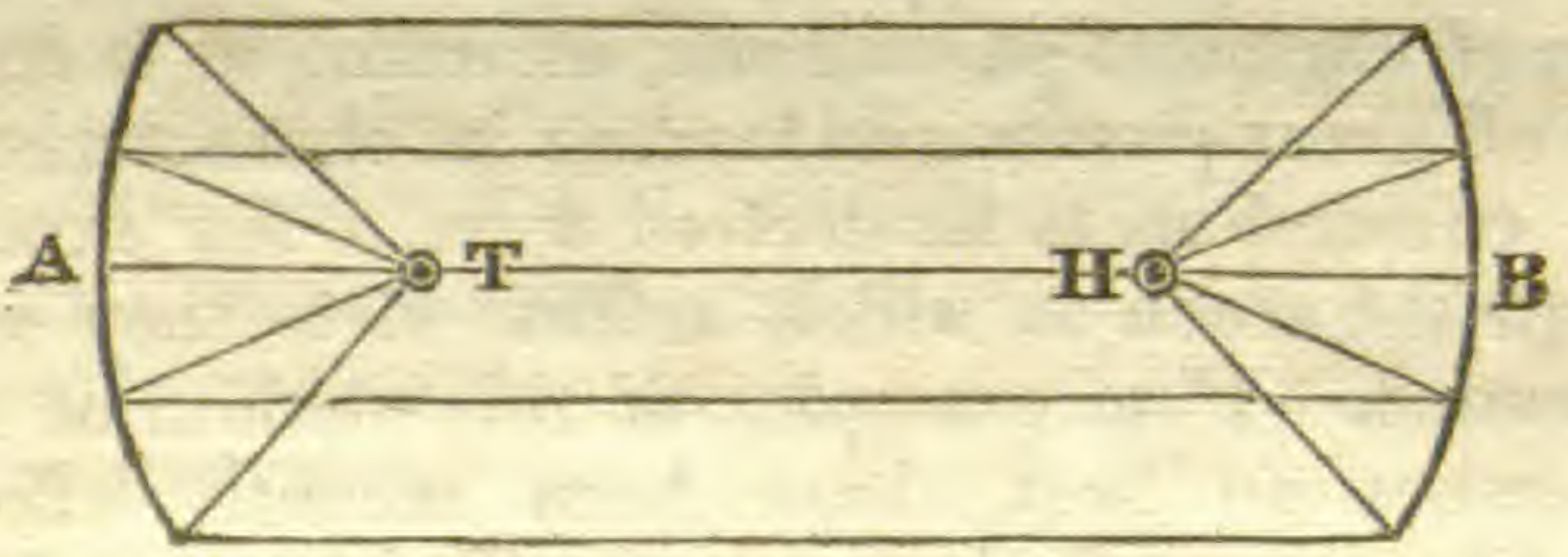
The Atomic Theory proceeds on the supposition that every body is an assemblage of minute solid particles, which, although they may be *divisible*, at least mathematically, are still no longer *divided*; and that when different elements unite, forming chemical compounds, these particles, and not the masses, combine with one another. It is not, therefore, opposed to the mathematical doctrine of the infinite divisibility of matter; it only assumes that matter is not, in fact, infinitely divided. These undivided particles are the atoms in question. In how many respects these ultimate parts of different bodies may differ from each other, we do not know; but, for explaining the phenomena of definite proportions, it is only necessary to assume that they differ from each other *in weight*. Grant then that compound bodies are formed by a union of atoms of the foregoing description, and let us see how this fact may be applied to account for the phenomena of definite proportions. To recur to an example already cited, nitrogen combines with five different proportions of oxygen, forming five compounds which are exceedingly diverse from each other. They are, Nitrous Oxide, Nitric Oxide, Nitrous Acid Gas, Nitrous Acid, and Nitric Acid. If we take a given quantity of Nitrogen, suppose ten grains, and combine it with five grains of Oxygen, we form Nitrous Oxide; with five more, and we have Nitric Oxide; with five more, Nitrous Acid Gas; with five more, Nitrous Acid; and with five more, Nitric Acid. Nor is it known that any other combinations of these two elements exist. Now suppose that these several compounds are formed by the union of a certain number of atoms of nitrogen with a certain number of atoms of oxygen, the latter number varying in the several compounds, how is it probable that the atoms unite to form the first compound? The most simple combination possible would be the union of an equal number of atoms of each element. It is plain that one atom of nitrogen can combine with no less than one atom of oxygen, because the atoms are, by the hypothesis, indivisible, or at least, undivided; nor is it probable that, in forming the first compound, one atom of nitrogen combines with any more than one of oxygen; for this union being the most simple of all, we may conclude that Nature that loves simplicity, would not neglect it. The first compound, then, being formed of one atom of nitrogen and one of oxygen, it is obvious that no new compound can be formed until we add at least one more atom of oxygen; and hence the reason is plain, why in all the higher combinations the quantity of oxygen is just twice, or thrice, or four times that in the lowest, there being respectively just twice, or thrice, or four times as many atoms of oxygen.

As Kepler and Newton have taught us how to weigh the sun and the planets, so, on the other hand, Dalton and his associates, who invented the theory before us, have taught us how to weigh the ultimate particles of matter. In the case of atoms, however, it is not absolute, but relative weights that we obtain—it is the ratios of these weights. Magnitudes are to one another as their equimultiples, one to five as ten to fifty, or as ten millions to fifty millions; consequently if we could ascertain that there were just as many particles in a given mass of copper as in another given mass of tin, the weights of the two masses would be in the same ratio to each other as the weights of their particles to each other. We have only then to weigh the masses to learn the ratio of their atoms. But the difficulty lies in proving that the two masses consist of precisely the same number of atoms. When two elements form only one compound, it is assumed that an equal number of atoms are united each to each. When two elements form several different compounds, as oxygen and nitrogen, it is assumed that the lowest proportion consists of an equal number of the atoms of each element. This appears like a gratuitous assumption; and resting as it does at the basis of the hypothesis, it may be inferred that the whole hypothesis has nothing to support it but the complete explanation it affords of the laws of definite proportions. This circumstance, indeed, must be allowed to form a strong presumption of the truth of the hypothesis; and, the presumption is further strengthened by two other considerations. One is, that such a union of the atoms would be the most simple, and therefore accord best with the known operations of nature; and the other is, that the weight of an atom comes out the same when deduced from different premises. Thus the weight of an atom of hydrogen as deduced from its relations to oxygen in water, is 125, oxygen being taken for unity; and the same number is obtained by deducing its weight from its relations to nitrogen in ammonia. A theory which brings us to the same conclusion by different routes, and whose calculations often coincide with the results of chemical analysis to the place of thousandths in decimals, must be founded in truth.

The laws of HEAT were so thoroughly investigated, and so faithfully expounded, by Black, Scheele, Crawford, Rumford, Lavoisier, by Leslie, Dalton and Prevost, that in this department little has been done, within a few years past, but to establish the same laws by more accurate and rigorous experiments. In this way several of the French chemists have labored very successfully. They have availed themselves of the improvements which have been gradually making in the construction of apparatus; and by this means they have been enabled to operate with a degree of precision, that was unattainable by their predecessors. They have also, in many instances, shown how advantageously that mathematical knowledge, for which the men of science of that nation are so distinguished, may be applied in the investigation of the laws even of chemical phenomena, affording as it does an instrument of great power, and lending its characteristic aid to carry the conclusions to be derived from a few experiments, far beyond the reach to which at first view they seemed to have extended. Fine examples of this method of conducting researches on heat, have been recently afforded by Biot, by Clement and Desormes, by Dulong and Petit, and by Berard and Delaroche.

Upon the supposition that heat is the only positive principle, and that cold is merely the necessary result of its absence, as darkness is the absence of light, it has been found difficult to account for the apparent *radiation of cold* between two parallel concave reflectors. The case is this: if two concave metallic mirrors (suppose of burnished brass, or tin, or silvered copper,) be placed parallel to each other, and in the focus of one of the mirrors there be placed a thermometer, and in the focus of the other a pan of coals or a red-hot cannon ball, the rays of heat proceeding from the focus to the adjacent mirror, will go out parallel from that to the other mirror, and be reflected by it to the thermometer and cause it to rise, shewing that there are rays of heat, and that they are subject to the same law of reflexion as those of light. Now if we remove the hot body, and place in its stead a mass of ice, (the thermometer being supposed to have regained the temperature of the room,) *rays of cold* will apparently proceed from it, reach the thermometer by the same route as before, and cause it to descend. Why should we infer such a principle as heat in the one case, more than such a principle as cold in the other? Have we not here the same evidence of the existence of rays of cold, as we had before of rays of heat?

Of the various solutions which have been given of this phenomenon, in accordance with the supposition that heat is the only positive principle, the greater part appear to be altogether unsatisfactory. The only explanation which appears to me to throw any light on the subject, is that which ascribes the depression of temperature to the circumstance, that *a quantity of heat is intercepted by the cold body which would otherwise be conveyed to the thermometer.* If we narrowly consider the circumstances, we shall perceive that all the heat which is conveyed to the thermometer by the mirrors, comes through that point which is now occupied by the cold body, and consequently is intercepted by that. Thus,



Let A B be two parallel concave reflectors. Let a thermometer be placed in the focus T, and first, let the focus H remain unoccupied. Now of all the rays of heat, proceeding from every direction and falling on the mirror A, none will be conveyed to the thermometer but such as come to A parallel to each other. But if we follow back these rays, we shall find that they are the same that had previously passed through the focus H. Now let the cold body be placed in this focus, and it is evident that the whole body of parallel rays of heat, which passed through this point, will be cut off, while the cold body itself does not radiate an equal amount of heat, and therefore occasions a loss of heat to the thermometer. That a depression of the thermometer is occasioned in this way, is plainly a matter of fact; the only question is, whether the removal of this portion of heat, is adequate to account for the entire reduction of temperature,—a point which a few experiments would decide.

In the department of GALVANISM, for the finest contributions that have been made to the science, within the last few years, we are indebted to Dr. Hare. His Calorimotor and Deflagrator are instruments distinguished alike for the ingenuity of their construction, and for the splendor of the phenomena which they produce.

The peculiarity of the Calorimotor, in point of principle, is, that its plates of copper are so connected with each other, and its plates of zinc with each other, as to make the whole equivalent to only one or two huge pairs of plates. It had been observed before, that the heating powers of the Voltaic apparatus, with a given extent of surface, depended on the *size* of the plates, while its electrical powers, such as communicating the shock to the animal system, and effecting the decomposition of bodies, depended on their *number*. The thought occurred to Dr. Hare, that as the ordinary arrangements of the Voltaic apparatus combine the circumstances of size and number, so the substance afforded by such batteries, is a compound of heat and electricity; and that if he could make the whole series equivalent to a single pair of plates, the effects would be almost exclusively those of heat. The result corresponded entirely with his expectations. This instrument exhibits the most intense light and heat, but scarcely any electrical effects. The experiments of Dr. Hare appear to me to render his theoretical views on this subject extremely probable, namely, that the product of the common Voltaic battery is a compound of heat and electricity.

The Deflagrator consists of a series of coils or plates of copper and zinc, so arranged, that,

1. The zinc is surrounded by the copper.

2. The metals can be instantaneously covered with the fluid.

3. The fluid may be contained in a single trough or other vessel, without the insulation, between the members of the series usually attempted by rosin, glass or porcelain.

4. The series consists of a good many members, and not of a few huge ones, as in the Calorimotor.

The chief peculiarity of the Deflagrator, is, that its whole metallic surface may be *simultaneously* immersed in the acidulous fluid; by which means the loss of power that usually occurs during the filling of the troughs, is prevented, and the whole power of the apparatus is greatly augmented by this concentration.

These instruments display, in no ordinary degree, a union of the philosopher and the mechanist; for while they are admirably adapted for research, and have already considerably extended the boundaries of science, they are unrivalled for the perfection of their structure, and the consequent facility with which they are brought into operation.

The most interesting discoveries which have recently been made in the department of Galvanism, are those which relate to its connexion with magnetism. That strong electrical discharges are capable of affecting the magnetic needle, has long been known; but that a magnetic current accompanies the electrical (or the calorific) in its passage between the poles of the galvanic battery, is a fact first announced to the world by Professor Oersted, of Copenhagen, in the year 1819. To display the curious results arising from the union of these two mysterious agents, no form of the Voltaic apparatus has been found more suitable than Hare's Calorimotor. If a wire be placed between the poles of this instrument, at the moment of its immersion in the acidulous fluid, the wire indicates strong magnetic properties, attracting iron filings so greedily, as to become tufted all over with them in an instant. And, what is particularly to be remarked, is, that the effect is not confined to iron and those few metals which were supposed to be the exclusive residence of the magnetic influence, but extends also to wires of every sort of metal.* "It is remarkable, also, contrary to what is observed in any other effect of electricity or galvanism, that the influence of the uniting wire passes to the needle, through plates of *glass*, metal, or wood, the disk of an electrophorus, or a stone-ware vessel of water; nor does the sudden interposition of any of these bodies destroy or sensibly diminish the effect."† (Henry.)

* And probably to all conducting substances; but among the substances hitherto tried, none but iron or steel retains the magnetic *virtue*, after the connexion with the instrument is broken—nor does iron become permanently magnetic when connected directly with the magnetic poles; it appears to require the intervention of glass, air or other bad conductor, so that it may be magnetised *by influence*.—Editor.

† Does not this fact favor the idea that the magnetic influence is a mere associate of the other powers, existing entirely independent of either caloric or electricity?

On contemplating these phenomena, the conclusion almost irresistibly forces itself on the mind, that the agent producing magnetic phenomena is a specific fluid, which passes over these wires from one pole to the other, as on a bridge; that the wire merely forms a substratum for it,—a line of passage; and that it acts not by any virtue which it imparts to the wire, but in its own appropriate character. That hence we may infer, that the magnetism of the loadstone is nothing inherent in the iron, but due to its *affinity* for the magnetic fluid; that where such an affinity exists, the body in which it dwells may become permanently magnetic, in a greater or less degree, while substances which are destitute of such an affinity, merely give a passport to the fluid without retaining it a moment, when separated from the poles of the battery.

When the deflagrator is immersed, an overwhelming and astonishing flood of light instantaneously bursts forth, accompanied by a degree of heat not surpassed by any arrangements hitherto adopted. The ingenious inventor performed with this instrument a series of some of the most brilliant and striking experiments that were ever exhibited; reaching, if not transcending, in effect the utmost powers of the Compound Blow Pipe, and of Children's celebrated Voltaic battery.

It was not until two years afterwards that the deflagrator came into the hands of Professor Silliman. This gentleman repeated the experiments of Dr. Hare, and discovered some peculiar properties of the instrument; for example, that when the common Voltaic battery was made to form a part of the series in common with the deflagrator, both instruments were paralyzed, their powers being almost entirely lost. In prosecuting these experiments still farther, Professor Silliman announced successively the fusion of several bodies hitherto regarded as infusible, as charcoal, plumbago, and anthracite, verifying the doctrine long ago promulgated by Dr. Black, that all solids become fluid by the addition of heat. The fusion of charcoal was attended with a remarkable circumstance, namely, an actual transfer of the solid substance from the positive to the negative pole. Was this borne along by the *mechanical* action of the electric or magnetic current, or was it *attracted* from one pole to the other in consequence of the opposite states of excitement, which the two poles were in? Since the existence of an electric current, possessing sufficient mechanical power to bear along with it such portions of matter as were here removed, is still hypothetical, while strong electric and magnetic powers are known to be concomitant products of this apparatus, is it not the most reasonable supposition, that this transfer of matter from the positive to the negative pole was the effect of one or both of these agents exerting their appropriate power of attraction? It is well known, from the experiments of Sir H. Davy, that the two poles are opposite in a very high degree, that is, one is strongly positive and the other strongly negative, and both therefore are in a condition to exert the strongest electrical attractions. Or if it were ascertained that the poles were in opposite magnetic states, and strongly excited, this fact would be sufficient to account for the transfer of matter which took place. In the present state of our knowledge, it is more reasonable to ascribe the effect to the attraction of one or the other of these agents, or to both of them acting conjunctively, than to a current which transports the particles of matter by its mechanical action. In the one case, we employ in the explanation, causes which are known to exist and to be adequate; in the other case, we adduce a cause which is purely hypothetical.

(To be continued.)

ART. II.—*Improvement in the manufacture of Magnetic Needles.* By Prof. AMOS EATON.

TO PROF. SILLIMAN.

SEVERAL years of the early part of my life were devoted to an extensive land agency, among the western and northern spurs of the Catskill mountains. During this period, I ran most of the outlines of two hundred thousand acres, besides four turnpike roads across this Alpine district. The difficulties to which I was almost daily subjected, by the irregularity of the magnetic needle, were often very embarrassing. The old surveyors of that time assured me, that these fits, as they denominated those irregularities, were produced by the action of magnetic ores, which they believed abounded in this mountainous district. At one time I entertained the opinion, that I had collected facts sufficient to demonstrate, that while snow was melting away, these fits were the most frequent.

But on comparing different compasses, I found that they frequently varied, not only from the common direction of the magnetic needle, but from each other. For example, when set in some directions, one compass would vary, while other compasses would vary when set in different directions, and would not vary when set in the same directions. On extensive alluvial plains, where we could not suspect the presence of extensive ore-beds, all these difficulties occurred with equal force. And what appeared to be a still greater mystery, on changing needles, the variation seemed to be governed by the compass, not by the needle.

After considering every proposed hypothesis, and trying every proposed remedy, I abandoned the subject, as totally inexplicable; and contented myself with correcting these aberrations by ranging back-flags and using two compasses.

While exercising the students of Rensselaer school in land-surveying, at the last summer term, the same difficulties revived the same enquiries. In a conversation with an ingenious artist, Mr Julius Hanks, of Troy, I learned that his best compasses had in some instances, been subject to those fits of aberration. He showed me a compass of most elegant and accurate workmanship, with a nonius and double levels, which had been returned by the purchaser on account of the frequency of those fits. I carried this compass to the school with a determination to search out if possible, the cause of its frequent fits. By applying delicately suspended needles, which might be called a suit of magnetrometers, I found a point in the limb, which attracted a fine needle at the distance of six tenths of an inch. This point caused the needle belonging to the compass, to deviate at the distance of half an inch on each side; beyond that limit it was not affected. Consequently, when the course to be taken brought the needle within that limit it would deviate, and point accurately in all other directions. Any practising surveyor will readily perceive, that in tracing the lines around a field, the needle might come within this limit several times, or it might not fall within it in running a dozen fields. Hence the supposed irregularity of the fits.

My conclusions from these experiments were, that a scale from a screw-cutter or a punch, or a tooth from a file, &c. too minute for the eye, might have been lodged in that particular point. On consulting Mr. Hanks, he said this might frequently happen, and it was not improbable that all those fits complained of by surveyors, might be traced to the same cause ; inasmuch as all compass cards and graduated circles were wrought with very fine steel instruments. To illustrate the subject, I took out the screws from the under side of the card and inserted the point of the finest sewing needle, less than the twentieth of an inch in length ; whereby I actually produced four additional points of disturbance.

To obviate the difficulty, Mr. Hanks cut off seven tenths of an inch from each pole of the needle, ground the poles to very sharp points, and tipped them with brass caps, extending to the original length of the needle. This measure, by withdrawing the magnetic poles from the sphere of attraction, proved a perfect remedy. Mr. Hanks presented the same compass to the school, where it has been used almost daily for two months ; and it is one of the most perfect instruments that I have ever used. It has no more fits, and is totally undisturbed by magnetic ores, real or imaginary. Mr. Hanks has since corrected a theodolite in the same way, which had been thrown aside as useless for several years. If the disturbing steel scale is in the card near the graduated circle, Mr. H. proposes lengthening the pivot and raising the circle by introducing an additional circle beneath the graduated one. But he has not made this experiment ; and it is probable no such case will ever occur. For if it were near the pivot, it would not disturb the needle ; and so little work is required in the card with slender instruments, that scales will not often be left in that part.

Another important advantage which will attend tipping needles with silver, brass, &c. is that of preserving the points from rust. It has been demonstrated by conclusive experiments, that magnetism resembles electricity in acting most powerfully from the sharpest points. Hence the absurdity of needles made of square bars. Hence, also, the utility of preserving the finely sharpened points by tips. I will add, that, of all forms of needles which I have used, the flat kinds are best, which are wide in the middle, and of a true taper to the points.

Yours respectfully,

AMOS EATON.

Rensselaer School, Troy, N. Y. Nov. 1826.

ART. III.—*Notices respecting Diluvial Deposits in the State of New-York and elsewhere;* in a letter to the Editor from Prof. AMOS EATON.

TROY, (N. Y.) Nov. 23, 1826.

DEAR SIR,

I DULY received your package and the letter from Prof. Buckland to you of March 1, and yours of Sept. 27, and of the 18th inst. to myself. I have concluded that I cannot have the second part of the Hon. Stephen Van Rensselaer's Canal Survey in readiness sooner than March or April. It is a positive order of Mr. V. R. that the second part shall be condensed like the first, embracing a mere statement of facts, of but few pages. My manuscripts would fill three or four common octavo volumes. These I must cut down to half of one volume. As you are desirous to furnish Prof. Buckland with facts connected with the subject of his Reliquiæ Diluvianæ, before he publishes the second volume, I will transcribe and condense some of the most important facts of that kind. I must be so very brief as to be scarcely intelligible. In the printed report I shall enumerate localities and give a connected train to the whole.

1. *The district examined for the purpose of opposing or confirming the opinions of Buckland and Conybeare in regard to the alluvial formations* contains an east and west parallelogram, four hundred and eighty miles long, and about twenty wide. This commences about twenty miles east of Connecticut river, and extends a considerable distance along the south shore of Lake Erie. There is also a north and south parallelogram, two hundred and eighty miles long, and about ten wide throughout the whole extent, and about forty miles wide in the northern half. This commences above Crown Point, on Lake Champlain, and extends down the Hudson river to its mouth. In addition to this, I have examined most of the country among the western spurs of the Catskill mountains.

2. *Plastic clay.* I have found numerous small beds, embraced in the *marly clay,* (London clay,) but have not been able to discover it as a stratum. I do not believe any thing analagous to that stratum in Europe exists in this district.

3. *Marly clay.* [This is the London clay of Conybeare. But Mr. Pierce first published it under this name.] I found

this stratum to be universal throughout the district. It is always present, excepting those localities where its absence can be explained on satisfactory grounds, which are consistent with Conybeare's hypothesis.

4. *Bayshot sand and crag.* I find these deposits very extensively spread over the marly clay; and they are co-extensive with it; but, being uppermost, are more frequently removed by explainable causes. I cannot view them as distinct strata; for they pass into each other laterally in all parts of the district. The bayshot sand is almost unbroken from near the head of Lake Champlain to Coxackie, a distance of about eighty miles It runs down the west side of the Hudson, generally about six or seven miles in breadth. It rests immediately upon the marly clay, and contains large quantities of iron bog-ore.

5. *Diluvion.* I find a diluvial trough, extending from Little Falls, along the Erie Canal, one hundred and sixty miles. After numerous examinations, I feel a confidence in the following description. It is, as it would have been, the whole having been filled to its present level with marly clay, covered with bayshot sand and crag, generally overspread with a layer of shell-marl, had it then been cut up, by a strong current running from Little Falls westerly, into islands, ridges, embankments, &c.; and after these channels were thus made, had they been filled with a confused mass of gravel, sand, clay, trees, leaves, fresh-water shells, &c. Whether the appearances originated in this manner, or in any other way, such is the present aspect. At the direction of Mr. Van Rensselaer, I caused diggings to be made, to the depth of forty and fifty feet; and in one case a well was dug one hundred and eighteen feet. The American hemlock (pinus canadensis) appeared every where to the greatest depth of this deposit; also, immense quantities of fresh-water shells.—They were chiefly of the genus Mya, (Unio of Bruguires,) and Helix, (Lymnæa of some authors.) The insulated remains of the stratified (antediluvial) deposites, present the marly clay, bayshot sand and crag, beautifully crowned with almost snow-white shell-marble, a fine yellowish soil, and vegetable mould, or peat. I may add, that nothing is more manifest, than that these deposites could not have been made by any existing cause. Seventy miles of this region is occupied by the summit level of the canal. The surrounding country is but a few feet higher, and all the water flows naturally into

Lake Ontario, or through the stupendous chasm at the Little Falls. I shall give many more localities in confirmation of the hypothesis of diluvial deposites, in the Canal Report.

6. *Ultimate diluvion.* You may be surprised at this new name. I do not like the name ; but I take it for the present. You have more than once cautioned me against new names. I have censured others for it, and often retracted my own. But I have either discovered a new stratum, or I have misinterpreted the descriptions given by others. Although I conversed with a number of scientific friends several years ago on this subject, and although I have examined two or three thousand square miles with a view to settle the question ; this is the first time I have ventured to make it public. From some expressions used by Buckland, and also from others employed, some by Conybeare, and even by Cuvier, I was inclined to believe, that they had observed similar facts without drawing any extensive conclusions.

Hypothesis, engrafted upon those of Buckland. During the last days of the deluge, when the strength and violence of the waters had abated, and they were subsiding by the common laws of equilibrium ; the last and, consequently, the finest sediment was deposited upon every formation which was then uppermost.

Facts. All elevated plains, from which the original forests have not been removed and whose surfaces have not been disturbed, are now covered, immediately beneath the vegetable mould, with a mantle of fine earth, finest at the surface, and this is every where nearly similar, and unlike the stratum upon which it rests. It is most perfect, as far as I have examined, upon that variety of crag, which American Agriculturists call hardpan. Almost the whole of the vast tract of land called Hardenburg patent, west and southwest of Catskill mountains, containing several million acres, and most of the high ranges in New-England, and the lands west of Lake Champlain, present a most perfect example of the hardpan crag covered with this ultimate diluvion.

7. *Hypothesis.* Antediluvial animals were few on this continent ; and these were chiefly large species of the order Pachyderma of Cuvier.

Facts. The new cavern in Root's Nose, on the Erie Canal, I carefully investigated, aided by three accurate assistants. This is four hundred feet in extent. I caused two important caverns to be minutely examined in the Helderberg, by three good assistants, of which Mr. Finch, the geologist, was one. In addition to these, I have caused the important points considered by Buckland, to be searched out, in several of the Kentucky and Illinois caverns. Nothing resembling the bones so abundant in European caverns has hitherto been discovered. Whoever will take the trouble to make personal inquiry, or to look over the journals of the last half century, will learn that all the bones disinterred in this country, which may be called antediluvian, belong to the Pachyderma (thick skin) order. I cannot learn that a fragment of hyena bone has ever been found in this hemisphere. I have taken measures to secure every important discovery made by the workmen on the canal for the four last years. I distributed one thousand copies of a pamphlet giving plain instructions for making collections, along the canal line, while the labor of excavating three hundred and sixty miles was going on. But not a fragment of a dry-land animal was discovered.

I may add, that we found stalagmites in all the caverns as described by Buckland. And on shelves and other situations which had protected it from the touch, and in almost every part of the new cavern in Root's Nose, we found a deposite resembling what I have, in this letter, denominated ultimate diluvion.

When my report is completed, although it is restricted to extreme brevity, I hope to present a connected view of facts, which may claim a share of your confidence.

Most respectfully, yours,

AMOS EATON.

Prof. B. SILLIMAN.

ART. IV.—*Views of the Process in Nature, by which, under particular circumstances, vegetables grow on the bodies of Living Animals.* In a letter from Dr. SAMUEL L. MITCHILL, of New-York, to A. P. DE CANDOLLE, Magistrate of the city of Geneva, Professor of Natural History, and Director of the Botanic Garden there, &c. &c. dated November 1, 1826.

MY DEAR SIR,

IN the memoir which I wrote upon parasitical animals, and which was published in Francis and Beck's New-York Medical and Physical Journal, I noticed, among others, those that tormented insects, such as the Acarus, (or Mite,) and the Ichneumon, (or Pupivere.) I also mentioned the *Œstrus* and *Hippobosca ;* and the Zoophytes, called *Entozoa*, infesting the internal parts of other creatures.

My present object is to consider a portion of the history of certain vegetables that may be deemed parasitical. But it is not my intention to treat of those which support themselves on living plants, like the Cuscuta, or Dadder ; the Viscum, or Misseltœ ; and the Epidendrum, or Air-plant : nor of the fungous tribes and lichens, in their great number and variety. Nor is it now my purpose to offer a sentiment on the mucor, or mould, often overspreading the surface of organized substances, no longer endowed with animation.—The observations I have to make are limited to a vegetable process going on occasionally, in the bodies of animals, more especially of insects ; and long ago known under the name of the *Vegetating Wasp*, or *Fly.* The prevailing opinion is, that these vegetables are funguses sprouting from the bodies of dead insects, as may be seen in Hutton, Shaw and Pearsons' Abridgement, &c. Vol. XII. pp. 15—16.

My attention was called to these curious appearances, in the year 1808, when my friend, William A. Burwell, Esq. brought me, from his own plantation in Virginia, the larva of an insect, upon which a vegetable had fixed itself, and grown to a considerable size. He had found several others of the same kind, and in a similar condition. From the long and semi-cylindric figure, the wrinkled and whitish surface, marked by rings, the scaly head and strong jaws, the numerous feet, and the arched or curved attitude, I was induced to consider it as belonging to the species of *Melo-*

lontha, or May-bug, whose grub is destructive, at times, to the roots of grass in meadows and pastures. The vegetable was single, and had been somewhat injured by handling and transportation; yet the lower part of the stem and the point of attachment, were very distinct. My informant assured me, that, when picked up, the vegetables were complete in this, and various other specimens. But there was no more than one on each.

Some years afterwards, another vegetating insect was presented to me by the late William M. Ross, M. D., who obtained it in the Island of Jamaica, during his residence there. It was a full grown individual of a *Sphynx* or Hawk-moth, whose whole body had been covered with a vegetable crop, issuing thick from the thorax and abdomen.

Another Sphynx, with its body covered with a harvest of parasitical vegetables, has since been exhibited to me, by J. B. Ricard Maddiana, M. D. who brought it from the Island of Guadaloupe.

The same gentleman, distinguished for his researches in different departments of natural science, gave me several vegetating wasps (vespæ) procured by himself in the same place, where he resided several years. A fortunate incident brought very interesting facts to his knowledge, at Bay-Mahant, near the small river *du Cain*. On the 16th June, 1823, as he was on a botanizing excursion, he saw, lying on the ground, a wasp's nest, which had, by means unknown to him, been separated from a branch of the *Laurus persea*, (avocatier,) near which it had fallen. The creatures were in a strange condition after this disaster to their dwelling. Some were flitting about over the cells, and by the softness of their wings, and the faintness of their colours, were easily known to have been hatched but a short time. Many others were lying dead on the ground. On examining these he instantly perceived vegetables proceeding from their bodies, and this uniformly from the anterior part of the sternum, or thorax. He collected about fifty of these vegetating wasps. On inspecting the nest, he found a considerable proportion of the cells empty. This, however, was not the case with them all; for there were still some that contained young wasps in the state of larva, and which had not reached the last stage of their metamorphosis He drew them from their cells, and satisfied himself that there was an incipient vegetation; and, moreover, that its progress had kept pace with the growth of the crysalis.

After these observations, he satisfied himself in a very rational way, wherefore the vegetable parasite was situated on the fore part of the body. It was remarked, that rarely or never, was there more than one vegetable on a single wasp.

Botanists have pronounced this parasitical production, to be a species of *Sphæria*, belonging to the natural order of the Fungi. Upon the supposition, that it is propagated by seeds in the ordinary mode, it plainly appears that these seeds would, on being wafted through the air, alight upon the most exposed part of the unhatched insect, that was accommodated for its reception. This would, of course, be near the head. Being fixed there, it would increase with the enlargement of the animal; and drawing nourishment from its body, would continue to grow, even after it had attained its last and perfect state, until the Sphæria destroyed the life of the wasp.

If the declaration that a vegetable of any sort could take a root, or sustain itself upon a living animal, rested upon a solitary occurrence, it might be suspected there was a mistake in the matter. But in the present instance, there is no room left for such an objection, inasmuch as the vegetating wasps collected on the spot, and carried away in complete preservation, put the fact beyond all doubt, that under particular circumstances, the body of an insect, while yet alive, becomes the soil or base upon which vegetables fasten themselves, and from which they derive support.

The mind becomes reconciled to such a procedure, by considering the history of the *Ichneumon*, an insect of the hymenopterous order. It is called *Pupivorous*, by reason of the voracity with which its larva devour the larva, crysalides, and even eggs of other insects; more especially those of the lepidopterous order. Some of them penetrate bodies of their prey, with their numberless brood, slowly corroding and consuming, but killing at last. While others (the ophions) are attached to the skin of the larva, by the foot stalk of a Cocoon, through which their heads pierce the internal parts, while their tails remain in their own inclosures. This cruel operation frequently continues until the large and invaded larva completes its Cocoon, in the form of a general cover or envelope, when it dies consumed and exhausted. After this, the family of Ichneumons come forth, first bursting through their own Cocoons, and afterwards that of their deceased prey. In this warfare of insects, it is stated as a fact, that one species of Ichneumon sometimes destroys the larva of another species of Ichneumon. These occurrences furnish strong and instructive analogies.

Here we find that the living bodies of caterpillars and their crysalides, are the habitations and nurseries of other insects. The Creator has ordered one tribe to be arrayed against another, apparently, among other purposes, for that of putting a limit to their own excessive multiplication.

Upon investigating their history, there seems to be another check upon their inordinate increase. The fungous tribes of cryptogamic vegetables, seem, in various instances, the destroyers of the insect race. Their germs or seeds, conveyed by the winds, or otherwise, to the surface of these creatures, find them to be situations or places, fit for their adhesion; and their thrift and bulk overpower the being upon which they fasten.

If it now may be considered as certain, that a vegetable may grow upon the larva or crysalis of a wasp, and continue to increase until the change into the complete or imago-state, and after, why may not the like happen to the larva and crysalis of the Sphynx and Melolontha? The proof, in the actual condition of my information, is not so direct and conclusive, that the fungous adherents took possession of the latter, while they were yet alive. Nevertheless, the crop is much more abundant on the body of the Sphynx. Hence arises a strong presumption that the seeds were scattered on the back and sides of the larva, which was exposed every where, to their influence, and not incased and protected, as the young wasps are in their cells. Whereupon it might be inferred, they would germinate and enlarge until after the beginning of the fourth metamorphosis, when they would probably overcome their supporter.

Dr. Maddiana, however, thinks that in some instances, the vegetation commences only after life has ceased. In confirmation of this opinion, he relates an occurrence in the Island of Trinidad, during 1811. He found a wasp (Vespa Mexicana) in an apparently perfect condition, glued somehow, by one wing only, to the leaf of a tree. From all the parts of its body, issued filaments from one to three inches long. They were wholly different from the Sphæria, being black, shining, and resembling the plant called *Spanish Beard*, or

"Tillandria asneoides." To my own mind it appears quite as likely that the seeds of the vegetable were planted on the larva or crysalis. It is not necessary to suppose that death must have preceded their insertion.

In contemplating this subject, an idea has presented itself, that vegetables may be considered as in certain respects gaining an ascendancy over animals. In regard to the lepedopterous insects, in particular, which commit such extensive ravages upon plants, it would almost seem that the vegetable tribes retaliated, or made reprisals.

I have thought it expedient to offer this remark, notwithstanding the special consideration bestowed upon the *veg tating fly* of the Caribbee Islands, by Dr. Watson, in the English Philosophical Transactions for 1763; the profound reflections on the facts thereunto appertaining, by M. Fougeroux, in the Memoirs of the French Academy of Sciences for 1769; and the ingenious speculations of Dr. Hill.

A main result from their researches has been, that the larva of individuals belonging to the family of *Cicada*, conceal themselves among the dead leaves, to undergo their change, where many of them die. After life has departed, the species of fungus, termed *Clavaria*, sprouts from the body, which serves as a soil, exactly adapted to its support. And thus is produced the peculiar compound of one or more vegetables springing from the body of an animal.

In Edwards's Gleanings, Vol. VII. pl. 335, p. 263, there are figures of vegetating larva from the Island of Dominica. The vegetables were of the fungus order, and arose from the heads of the insects. He thinks the latter are a *Cicada*. There were many of them found together, buried in the earth. In the next plate, No. 335, he has copied vegetating wasps found near Havana, by Father Torrubia, and first published by him in Madrid. The vegetables seem to be different from each other, and from all that I possess. Yet they appear to be the same that Watson was acquainted with.

Three occurrences in this country deserve to be mentioned. Stephen W. Williams, in a letter to me, dated Deerfield, Mass. March 29, 1824, describes a remarkable production of the kind. He states, on the authority of several most respectable citizens, that they have repeatedly seen a vegetable growing from the body of the common grub, (melolontha?) They have observed them so many times, and in so many places.

rising to the height of several inches, that some of the witnesses were inclined to believe the product was the tall blackberry, (rubus villosus.) The grub he means is found in wood-yards, around the stumps of dead trees, and often in sward-ground; in which latter it has been known to do extensive damage, by devouring the roots of grass, and, sometimes, every plant in its way. In 1822, these devastators not only killed the herbage of large tracts, but also preyed upon the maize and potatoes.

The like appearances have been noticed in Pennsylvania by Jacob Cist. His history of the insect, called likewise the *May-bug*, illustrated by good figures from nature, may be seen in Silliman's American Journal of Science, &c. for August, 1824, (Vol. VIII. No. 2, p. 269 and seq.) In meadows, where they are abundant, it is not unusual to find a number of the larvæ bearing vegetable sprouts, in some instances three inches long. These excrescences generally proceed from the space between the head and under part of the thorax, and, in a few instances, from the mouth. Mr. C. thinks, correctly, these are a species of fungus; though he observes there is a vulgar but prevailing notion, that *such grubs are changed to briars!* Usually there is but a single vegetable on an individual grub, though two now and then occur. He says, in every case where he observed the vegetation, the grub was not only dead, but decayed. The sprout, rising above the surface of the ground, is the indication where the animal lies. Mr. C. supposes the seed, swallowed by the grub, causes the death, and, after that event, germinates in the decaying remains.*

On a survey of the facts, the following inferences seem to be warranted:

First, that this kind of vegetation is not confined to a single species of insect; but obtains in several, to wit, the Wasp, Sphynx and Melolontha. There is strong reason to suppose it extends to others.

Secondly, that these soils, the bodies of insects, nourish more than one species of vegetable, as the sphœria, clavaria,

* Addison Phillco, M. D. has sent me several specimens of larva or grubs bearing plants, though there was no more than a single vegetable on one animal. In his letter, dated at Sangamon, Illinois, May 4, 1826, he writes that his neighbor, Capt. Hathaway, ploughed up a number of them in some old ground where turnips had been raised the preceding fall. The excrescences were invariably near the head of the creature, and in some instances sprouted into three divisions, like leaves.

and probably others not yet investigated. Further research may be expected to discover more.

Thirdly, that a part, at least, of this order of parasitical vegetables, begin their work of annoyance, like the larvæ of the Ichneumon, in the body of the living insect, and continue it until the creature is killed by its destructive inroads.

Fourthly, that such of these mixed associations of vegetable with animal substance, are not prone to rapid putrefaction, but remain entire long enough to be collected by naturalists, and become the objects of scientific inquiry.

The chief or leading fact, intended herein to be established, is the derivation of nourishment by the vegetable, from *the living* animal. There is nothing more common than the conversion of animal matter by putrefaction and mixture, into manure, or a material for fertilizing land and supporting vegetables. Gardeners and farmers are practical commentators on the efficacy of composts abounding in excreted and decomposed animal products, to promote the growth of the plants they cultivate. If, therefore, the bodies of *dead* insects, should sustain vegetables, the fact would only be conformable to numberless other occurrences which happen almost every day.

The mind will perhaps be more easily reconciled to my conclusion, by considering other examples of vegetables growing upon living animals. Are not some of the Crustacea, as the *Portunus*, and more especially the *Maja*, of the New-York shores, the calcareous soil or base, on which a crop of vegetables grow? Does not the like growth obtain on various Molluscas, such as the *scallops* and *oysters* of our coast, in the form of a dense vegetable covering? And, do not certain reptiles, as *tortoises* of the mud, when they occasionally emerge from their sunken habitations, exhibit bucklers beset with similar productions? In these instances, the epidermis or skinny covering, is not intended; but a growth or crop of foreign or adventitious substances.

Herein, there is need of additional inquiry. The Botanist must determine the several species of vegetables, thus taking possession of living animals. The Zoologist must decide upon the various kinds of animals, that bear the vegetable forest or harvest, and the Anatomist ought to trace the connection between the two classes of beings.

I thank you sincerely, for the parcel of excellent pamphlets, written by yourself, which I lately received. I admire you for writing well, on such a variety of subjects. Their perusal has afforded me much instruction. The accompanying letter was peculiarly agreeable, both for its matter and manner. I hope you have received the articles I directed to you in return. May you long live, an ornament to the age, and a contributor to science.

SAMUEL L. MITCHILL.

ART. V.—*A Review of the Principia of Newton.*

[Continued from Vol. XI. p. 246.]

IT was tauntingly objected to the Newtonian Philosophy, by the Cartesians and others, that his analysis of powers or forces, as the causes of the great phenomena of nature, went not to explain the agency employed, or its *modus Operandi*, whether it be mechanical or spiritual. They, therefore, denominated those powers occult qualities, or perpetual miracles. If we cannot ascend continually in the grand scale of causes and effects, and resolve those already discovered into others still higher, or consider them as the last links of a chain dependent only and immediately on the Author of the Universe, those philosophers agreeably to their schemes of working out *a priori*, and perfect systems, would suppose nothing to have been done. If such cavils could be an objection to our author's philosophy, they might be made, with equal justice, to all philosophy, and indeed to all scientific knowledge ; for the nature and essence of things generally, are unknown to us, and from the total inadequacy of our faculties to comprehend them, none except the Cartesian hypothesisers have ever made any attempt, or pretensions towards a knowledge of that kind. True, or potential philosophy looks only to the proximate cause, and that not with a hope of discovering its nature, or essence, but its existence and operative effects. Thus the *fact* of the existence of some power which we call gravity, which causes bodies to descend to the earth, and which is proved also by Newton, to extend to the heavens, and more particularly, the laws and modifications of this force, are what constitute a prominent part of his philosophy. It was well observed by Mr. Cotes, in his introduction to the Principia, that those indeed are occult causes, whose existence is occult, and imagined not proved, but not those, whose real existence is clearly demonstrated by observations. The same objections might be made to the affinities, and fundamental principles of Chemistry, as the operative agents are equally occult qualities, their nature and essence being wholly unknown.

Newton repeatedly discarded any attempts to investigate the physical properties or nature of those forces, whose laws he has so successfully investigated; "I do not," says he, "enquire into the physical causes and seats of those forces; I indifferently, and promiscuously use for each other, the words attraction, impulse, or any kind of propension towards the centre, by considering these forces not physically, but mathematically," &c. Not to detain the reader longer on this apparently obsolete point, I would refer the curious, who wish for more information, to the Principia itself, and the very excellent introduction to it, by that great Philosopher, and Analyst, Mr. Roger Cotes.

The principal subject of the 2d and 3d sections of the Principia, was the investigation of the ratio of the central force, which would be necessary for bodies to move in certain curvilinear orbits, the bodies being supposed before the action of the centripetal force to be in motion by virtue of its inertia only, which by the first law of motion would be uniform, and rectilinear. This, it is well known, must be the effect of some force, which has ceased to act. Hence curvilinear orbits are the effects of a motion continued by inertia, and such variations of centripetal force, as may be necessary for movements in any particular curve. These are the physical principles on which the mathematical investigations of those sections depend. But another class of problems has arisen out of these, by considering the action of a constant force only, or this regulated, or modified by the action of some contiguous bodies, whether by virtue of a constant force, or one perpetually varied according to any law of distance. These subjects are with great sagacity treated of generally, by our author, in the subsequent parts of this work, as far as they related to his grand philosophical object. But he had not leisure to pursue them to the extent, to which his successors, from curiosity, or a wish to display their powers of analysis, have carried them. In the general enthusiasm, which prevailed among mathematicians near the close of the 17th century, for propounding to one another the most difficult questions, those which arose out of our author's inventions, and which he had neglected to pursue, were the most prominent. Among these were the problems of finding the curve along which a body would make equal descents in equal times in a vertical line; another to find the curve along which, the body descending would describe equal distances from the point of its departure in the same time. But that, which attracted the greatest attention, and was supposed to be of the greatest importance, was the one proposed by John Bernouilli, the famous mathematician, critic and Cartesian philosopher. This was to find a curve along which a body would descend by the force of gravity from one given point to another not in the same vertical line, in the shortest time possible. On account of the supposed difficulty of the problem, Bernouilli allowed to mathematicians one whole year for its solution. In a short time after its promulgation through Europe, he was presented with an anonymous solution of this great problem, on which he observed, " that though the author had not given his name, he saw clearly on his work the stamp of the lion." This first solution was produced by Newton; afterwards others were given by Taylor, De la Hospital, Leibnitz, &c. The part of our author's solution which shows the relation between the time of descending down a plane or right line compared with that of the *Brachystocronon*, or curve of quickest descent is, in our opinion, one of the most beautiful synthetic demonstrations in the whole compass of the mathematics. These remarks may appear digressive, but they are not irrelevant to our object, inasmuch as all these and other analogous problems, have emanated from the general principles of motion under the influence of certain forces developed in the 2d and 3d sections of the Principia, and may be properly considered as supplementary to them.

It was not the object of Newton to pursue a subject, which he had originated, even though most fertile in consequences, through all its ramifications, and which, when three or more forces are acting on a body, and in different directions, would require volumes and all the refinements of his own analysis, to develope, to the full extent of which it might be susceptible. The principal if not the only end in view in the Principia appears to be, the substantiation of the author's system of philosophy by mathematical demonstrations. In this sublime and arduous undertaking, he has been completely successful, but has left to his successors the details of his work, and all the minor advantages of his immense fabric. But even that grand object could not be accomplished without great skill, and address in the mathematics. This science, at his time embraced little more than what had been handed down from the Grecian and Alexandrine schools, and the improvements of Des-Cartes, Vieta, Wallis, and a few moderns. Those regarded principally the elements of the science. Geometry in the time of our author, was insufficient for the determination of the Trajectories, in which the celestial bodies move, not even by observations. Their methods of obtaining this object were tentative, indirect, and hypothetical. The application, therefore, of the laws of force, as investigated in the 2d and 3d sections, though constituting a new *era* in philosophy, could only apply to some potential movements, which were not known to exist, before the *a posteriori* process from observation had proved their reality. This problem of finding the trajectories by observation, and mathematics only, is indeterminate; still, however, it was necessary for that approximation, which has been resorted to for that purpose, to make further advances in that science This was the object of our author's 4th and 5th sections, which are purely mathematical. The principal physical application of them will be found in determining the orbits of the comets, or any new planet which may appear. The difficulty in this grand problem consists principally in finding the the radius vector at any time, which can only be done correctly, by an assumption of those very principles, which it was the object of Newton to verify. This verification, however, is satisfactorily obtained by the numerous successive observations, which have been made on the celestial bodies by astronomers in past ages, whereby their periodical times have been ascertained with great accuracy. Their radii vectores in respect to the sun, may also be inferred, from those observations made from the earth, and the laws of Kepler deduced as he actually has deduced them wholly from phenomena. The accordance of Newton's physical principles with those long and numerous observations, would alone have been sufficient for the verification of his system, independently of a trial of them by new bodies such as comets, or new planets. But his views were more comprehensive, and went to the extension and improvement of astronomy, and physics generally, by the aid of his discoveries. This work too, has been accomplished by him and his successors, so as to present an entire development by deductions directly from his assigned causes, of all the motions and phenomena of the great bodies of the universe. Astronomy is now made perfect in theory, and nearly so in every thing which relates to practice. It will be our business to point out the steps which have been taken in the Principia towards the accomplishment of that great object.

By the combination of mathematical with the physical principles of Newton, the invention of the orbit, or trajectory of a new planet, or comet, to all that degree of accuracy which is attainable by approximating methods, may be considered as complete, so as to ascertain their motions from a few observations during the whole period of the former, or visible appearance of the latter. This is by far the greatest advance ever made in Astronomy. The 4th and 5th Sections of the Principia now under consideration, appear to have been the first attempts of our author towards this great improvement. These we shall pass over at present, as they have been superseded by others, which have been exhibited in his other works, and by the methods of Boscovitch, Templehoff, Le Place, &c. We would only remark, that the 20th and 21st Lemmas relate to a method of describing the conic sections by the revolution of angles about given points, and that this principle has been seized on by succeeding mathematicians as a prolific source of improvements in Geometry, particularly by Maclaurin, in his Geometria Organica, who has extended it to numerous curves of the higher orders.

The next, or the 6th Section, is short, but not unimportant. Assuming the Laws of Kepler, as demonstrated experimentally and mathematically, they constitute a certain basis for an analysis of that which has been one of the greatest problems among astronomers of the two last centuries, viz. to determine the angular motion of a body moving in any of the conic sections about the focus. Though motion in a Parabola, or Hyperbola, can scarcely be found in any of the great bodies of nature, not even in projectiles near the surface of the earth, and the comets must move in Ellipses, unless urged by a force such as would cause their velocities to be equal to, or greater than that due to an infinite height, it is nevertheless of great use in Astronomy to be able to calculate the angular motion of a body moving in a parabola, as this figure is the limit of all excentric ellipses, and approximates so nearly in curvature to them, that it may safely be assumed for an orbit of that kind, in the part of it near the vertex. On account of the greater facility of calculating the angular motion in a parabola, than in a very eccentric ellipse, Astronomers have generally considered comets as moving in that curve. Our author, for that reason, has, with his usual sagacity, given a geometrical solution of the angular motion corresponding to any given area of a body moving in a parabola. Dr. Halley, and others since his time, have produced analytical solutions better adapted to practice. They had their prototype in our author, and have only rendered the solution easier, but not more elegant. The assumption of a periodical time infinite, since it has been found to agree with the motions of comets in the lower parts of their orbits, shows how very eccentric they must be, and that their periodical times cannot be determined from observations of their motions within the regions of the planets.

The next in order of the grand problems of our author, is that celebrated one of Kepler, which, since his time, has been considered as the foundation of all true Astronomy. Assuming the principles established by Newton, it is reduced to one nearly mathematical, viz. to cut an ellipsis by a line drawn from its focus to its perimeter, so that the area included between that line and any other line drawn to the perimeter from the focus, may include a given area: or, which will amount to the same thing, the equable description of areas, which corresponds with the equable angular motion of a body in a circle, being given, to determine from thence the angular motion of a body about the focus of an ellipse, while describing the same areas. Many solutions of this great problem have been attempted, first by Kepler himself by an indirect method, next by Bishop Ward hypothetically, by Bullialdus, who corrected Ward's hypothesis, by Cassini and others. But a direct geometrical or analytical solution was never, I believe, given before those of our author, as exhibited in the 6th section of the work before us. His analytical solution has been much improved, and accommodated to practice, by Dr. Keil, but as a speculative problem, in which the powers of genius are displayed, nothing can exceed the solution given by Newton, if we except some of his own in his 2d Book.

In a preliminary article, the author proves, that in no oval figure, can an area cut off at pleasure, from a given point within the figure, be universally found by an equation of any finite terms and dimensions. To show this, he supposes the radius vector prolonged, to revolve about the given point as its pole, and that spaces proportioned to the area generated within the oval, are constantly generated in that right line. This motion will, by perpetual gyrations, to which there is no limit, produce a spiral of which every intersection by a right line will, when algebraically expressed, be a root of the equation, consequently the number of the roots will be infinite, and an equation only of infinite dimensions will express that number. The area, therefore, in relation to the radius vector, or any rectilinear co-ordinates, can only be expressed by an equation whose geometrical construction requires a transcendental curve. This, perhaps, may be better understood from the consideration of the radius vector being a function of the elliptic area, or the elliptic arc, which cannot be expressed but by an infinite equation. The transcendental curve, assumed by our author, is the cycloid; but geometrical solutions, though constituting the acme of perfection with the celebrated ancients, and on which account *only* the *oracular* problems could be of any account, though luminous in theory, are not very well calculated for practice, especially since all our operations are performed numerically, by decimal estimates in numbers or logarithms. Our author, apparently sensible of this, has afforded another partially analytical solution of this famous problem, with a reference to the improvement of that sublime science, which appeared most to attract him. This has been made more practical by Dr. Keil, by reducing our author's principles to a form more susceptible of logarithmic calculation. But even this solution, if facility of theory and practice both be regarded, may be still further simplified; for, suppose x to be the eccentric anomaly to the radius a, and b the eccentricity, or the distance of the focus from the centre of the ellipse, we shall then have $\text{Sin.}\ b\,x + a\,x = \text{A}$, the arc of the circle representing the mean anomaly, which is given. By reducing Sin. $b\,x$ to terms of the arc x, by the known series for that purpose, and by reverting the series the value of x, the eccentric anomaly will be known, from which the true or coequate anomaly is easily found. The hypothesis of Bishop Ward supposes the angles to be equally described about the upper focus of the ellipse. This has been shown, by Bullialdus and others, to be erroneous, and not applicable to Astronomy, except in orbits of little excentricity.

Our author's fertile genius has produced a third solution of this great astronomical problem, which, though by the commentators of his work esteemed as most ingenious, is not so direct as the preceding, and, in our opinion, not better calculated for practice.

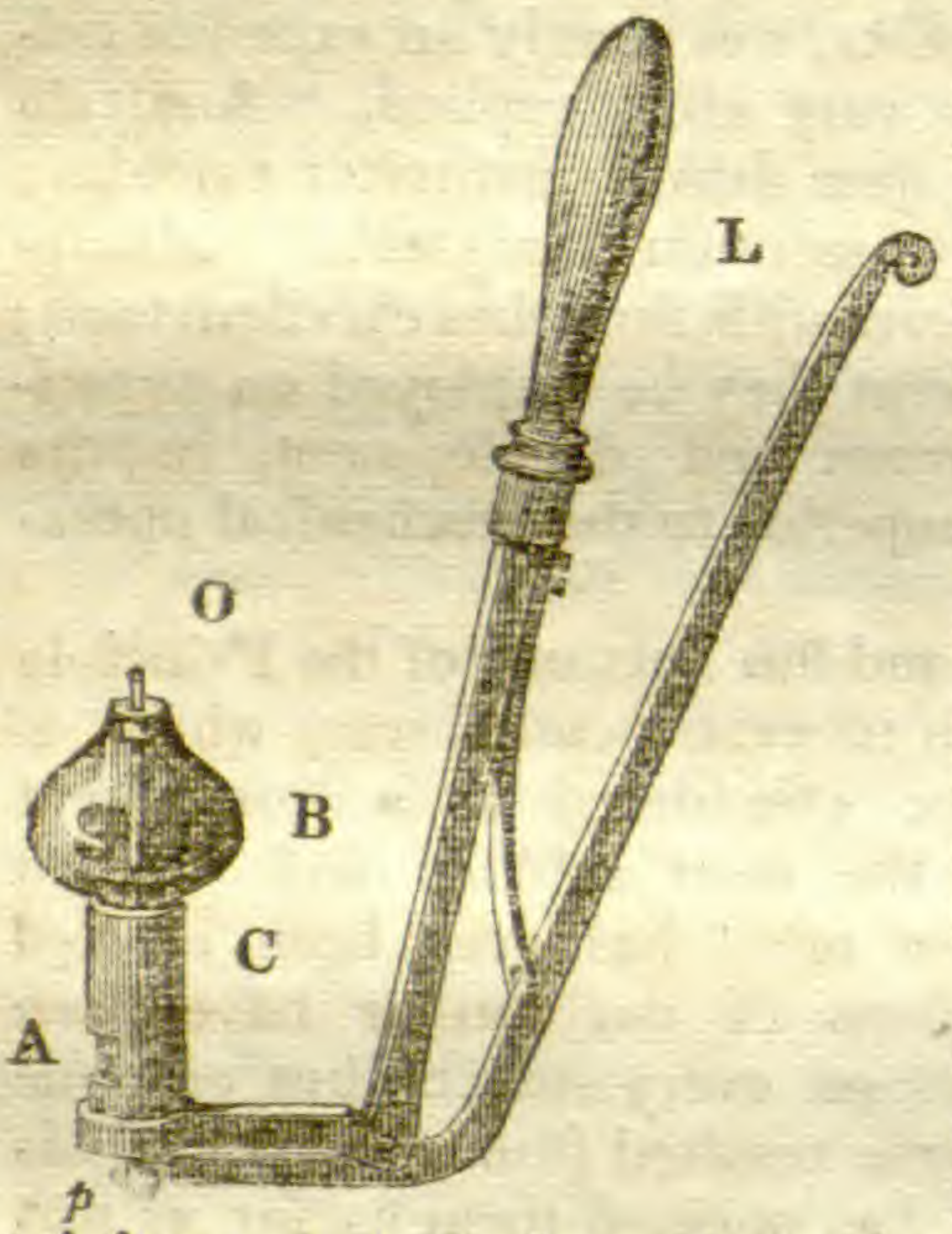
These, and many other varied solutions of the most difficult problems in the Principia, are generally delivered without any *analysis*, by which term I would be understood to mean, the *principles* by which the author arrived at the solution, whether geometrical or algebraical, and not that dexterity of symbolical operations, or refined artifices of managing them, to which the use of the word has been almost exclusively applied by John Bernouilli, and others since his time. To this mathematician, who was perpetually boasting of his superior skill in *analysis*, which, in effect, was merely an expedite calculus, Dr. Brooke Taylor very aptly replied, "Analysin constituunt precepta, juxta quæ deinde instituitur calculus; qui non est Analysis, sed instrumentum Analyseos." *Analysis indeed*, as it is called by Bernouilli, furnishes excellent tools; but these are of no use, except they be employed on *principles*, which, for their invention and development, require faculties of the mind much superior to the mechanical operations of symbols.

The succeeding 7th, 8th and 9th Sections of the Principia relate to the inverse problem of centripetal forces, which, if that of the three bodies be considered as a component part, may be pronounced the most sublime and difficult of any on which the human mind has ever been exerted with success. The inventions of our author have been most fertile in consequences on every subject, but on none more than in those which have resulted from this part of his work. The author himself has pursued them as far as was necessary for his object, and has laid down the leading principles of what has been done since, by Maclaurin, Simpson, Clairaut, Euler, Le Place, &c. The review of this important part of our author's work will be given in some future number.

ART. VI.—*Improved Eudiometrical Apparatus;* by ROBERT HARE, M. D. Professor of Chemistry in the University of Pennsylvania.

PISTON VALVE VOLUMETER.

I HAVE contrived some instruments for taking volumes of gas, at one time, precisely equal to those taken at another time. I call them volumeters, to avoid circumlocution. They are of two kinds; one calculated to be introduced into a bell glass, over water or mercury; the other may be filled through an orifice, as is usual in the case of filling a common bottle over the pneumatic cistern. The following figure will convey a due conception of one of them, which, having a piston, I call the piston valve volumeter.



The lever L, is attached by a hinge to a piston *p*, which works inside of a chamber, C. The rod of this piston extends beyond the packing through the axis of the bulb, B, to the orifice, O, in its apex, where it sustains a valve, by which this orifice is kept close, so long as the pressure of the spring, acting on the lever, at L, is not counteracted by the hand of the operator.

Suppose that, while the bulb of this instrument, filled with water or mercury, is within a bell glass, containing a gas, the lever be pressed towards the handle, the valve is drawn back so as to open the orifice of the apex of the bulb, and at the same time the piston descends below an aperture, A, in the chamber. The liquid in the bulb will now of course run out, and be replaced by gas, which is securely included, as soon as the pressure of the spring is allowed to push the piston beyond the lateral aperture in the chamber, and the valve into the orifice, O, in the apex of the bulb.

The gas thus included may be transferred to any vessel, inverted over mercury or water, by depressing the orifice of the bulb below that of the vessel, and moving the lever, L, so as to open the aperture, A, in the chamber, and the orifice of the bulb, simultaneously.

The bulk of gas, included by the volumeter, will always be the same; but the quantity will be as the density of the gas into which it may be introduced. Hence, in order to measure a gas fairly, the liquid, whether water or mercury, over which it may be confined, should be of the same height within as without. This is especially important, in the case of mercury, which, being nearly fourteen times heavier than water, affects the density of a gas materially, even when its surface within the containing vessel does not deviate sensibly from the level of its surface without.

To remove this source of inaccuracy, I employ a small gage, which communicates through a cock in the neck of the bell, with the gas within. In this gage any light liquid will answer, which is not absorbent of the gas. In the case of ammonia, liquid ammonia may be used; in the case of muriatic gas, the liquid acid. In article twenty-three,* a bell glass is represented, furnished with a gage of the kind which I have used, and which was described in the account of the mercurial sliding rod eudiometer, in this Journal for October, 1825.†

The density of the gas will be in equilibrio with that of the air, when the bell is supported at such a height, as to cause the liquid in each tube of the gage to be in the same level.

* Of the engravings and descriptions of apparatus used in the chemical course of the University of Pennsylvania.

† See also an account of an improved mercurial sliding rod eudiometer in this number.

SIMPLE VALVE VOLUMETER.

Besides the lower orifice, O, by which it is filled with gas, the volumeter, which this figure represents, has an orifice at its apex, A, closed by a valve attached to a lever. This lever is subjected to a spring, so as to receive the pressure requisite to keep the upper orifice shut, when no effort is made to open it.

When this volumeter is plunged below the surface of the water of a pneumatic cistern, the air being allowed to escape, and the valve then to shut itself under the water, on lifting the vessel, it comes up full of the liquid, and will remain so, if the lower orifice be ever so little below the surface of the water in the cistern. Thus situated, it may be filled with hydrogen, proceeding, by a tube, from a self-regulating reservoir. If the apex, A, be then placed under any vessel, inverted duly in the usual way, the gas will pass into it, as soon as the valve is lifted.

Volumes of atmospheric air are taken, by the same instrument, simply by lowering it into the liquid of the cistern, placing the apex under the vessel into which it is to be transferred, and lifting the valve ; or preferably by filling it with water, and emptying it in some place, out of doors, where the atmosphere may be supposed sufficiently pure, and afterwards transferring the air, thus obtained, as above described, by opening the valve while the apex is within the vessel in which the mixture is to be made. In this case, while carrying the volumeter forth and back, the orifice must be closed. This object is best effected by a piece of sheet metal, or pane of glass.

It is necessary that the water, the atmosphere, and the gases, should be at the same temperature during this process.

SLIDING ROD GAS MEASURE.

The construction of this instrument differs from that of my sliding rod eudiometers, in having a valve which is opened and shut by a spring and lever, acting upon a rod passing through a collar of leathers. By means of this valve, any gas, drawn into the receiver, is included so as to be free from the possibility of loss, during its transfer from one vessel to another. This instrument is much larger than the eudiometers for explosion, being intended to make mixtures of gas, in those cases where one is to be to the other, in a proportion which cannot be conveniently obtained by taking more or less volumes of the one, than the other, by means of the volumeters; as, for instance, suppose it were an object to analyze the air, according to Dr. Thomson's plan of taking 42 per cent. of hydrogen. The only way of mixing the gases by a volumeter in such a ratio would be to take the full of the volumeter 21 times of hydrogen, and 50 times of atmospheric air. By the large sliding rod instrument, this object is effected at once by taking 42 measures of the one, and 100 measures of the other.

BAROMETER GAGE EUDIOMETER.

The following is an engraving of the barometer gage eudiometer for explosions. R, is a glass receiver. Within the receiver, near W, is an arc of platina, by the ignition of which the gas is inflamed. C, is a cock with three orifices, either of which may be made to communicate with the receiver, according to the position of the lever, L. More than one of the orifices cannot be open at once, but all may at the same time be closed. The barometer gage, G G, is seen beside the receiver, with which it communicates through the pipe P, and the valve cock V, by means of which the communication, between the gage and the receiver, may be suspended at pleasure. The pipe A, conveys to the receiver, the gaseous mixture from the bell glass B. By one of the pipes D, a communication with the air pump may be established. The other pipe is used when different kinds of gas are to be successively introduced; or when a portion of residual gas is to be drawn out for examination. T T, are rods for conveying the ignition to the platina wire. *m*, is a wooden dish, holding mercury for the gage tube.


R
G
P
A
W
B
V
G
T
C
T
L
D
m

It is well known, to those who are familiar with pneumatics, that if a receiver communicate simultaneously with an air pump, and a barometer gage, the extent of the exhaustion will be indicated by the height of the mercury in the gage tube; so that if there be a scale of equal parts associated with the tube, the quantity of air taken from the receiver at any stage of the exhaustion, will be to the quantity held by it when full, as the number opposite the mercurial column, when the observation is made, to that to which it would rise, if the receiver were thoroughly exhausted.

Hence having exhausted the vessel, thoroughly, if the mercury stand at 450 degrees, by the gage, on allowing any gaseous fluid to enter till it sinks to 150°, the quantity in the receiver will be 300 parts; and if of this, by explosion, or any other means, any number of parts be condensed, the mercury in the gage must rise that number of degrees.*

In order to have a receiver strong enough to resist explosion, and at the same time sufficiently capacious to hold quantities of gas many times larger than have heretofore been exploded at once, I have provided a stout tube, six feet in length, tapering from two inches to one inch in diameter internally; open at the larger, and closed at the smaller end.

This tube is cemented, at the larger end, into a brass ferrule, which is screwed into a casting of the same metal, and fastened to a tripod of iron. Into the same casting a brass plug screws, through which are inserted stout wires, one of them insulated, for producing galvanic ignition in an arc of platina wire, as already described in the case of my other eudiometers.†

* That portion of the bore of the tube which is not occupied by mercury, adds to the capacity which influences the gage, and the portion of the gage which is emptied of mercury, varies in extent; but as the air, which remains in the gage, is not subjected to the explosion, the extent of the condensation is uninfluenced by it. A slight error may arise from the sinking of the mercury, in the dish, as the quantity in this receptacle, lessens, by its rise in the tube; and, vice versa, when subsidence ensues. This movement will be to the movement of the mercurial column, in the tube, as the square of its diameter, to the square of the diameter of the mercurial stratum in the dish, and the diameters of these being respectively as 20 to 1, it would be 1·400 of the whole height of the scale; this difference may be allowed for, or may be remedied by raising or lowering the dish, by an appropriate screw, or employing a dish of a superficies so large, and a gage tube with a bore so small, as to render the effect of the rise, or subsidence of the mercury in the gage, insignificant.

† One of the greatest difficulties which I encountered, was in the imperfection of stop-cocks, in the common form. This I obviated by two contrivances of my own; one invented about sixteen years ago, the other in the summer of 1825. Of these I shall publish a description, with engravings, as soon as I can conveniently.

With the gage tube is associated a scale, divided into 450 equal parts. Instead of inhaling, successively, due portions of hydrogen and atmospheric air, as heretofore described, I have found it better to mix them previously in known volumes, by means of the volumeters described in articles 19 and 20.* Having, by the aid of one of those instruments, made a mixture of one part of hydrogen with two of atmospheric air, it follows, that if 300 measures be taken by a sliding rod eudiometer, or other adequate means, there will be a mixture, in the quantity so taken, of 200 parts of atmospheric air, and 100 of hydrogen. In case equal volumes of these aëriform fluids be mixed in one bell glass, 200 measures would contain 100 of each. This mode of procuring such mixtures, is preferable, from its saving trouble, and lessening the chances of error in the measurement; and because the gaseous fluids become more thoroughly blended; a result which does not follow their admixture, as immediately as might be expected.

Having prepared a mixture of two volumes of atmospheric air, with one of hydrogen, and the receiver being exhausted as far as practicable, if any small quantity of the mixture be exploded in it, by exciting ignition in the platina wire, all the oxygen will be condensed. The residuum, consisting of hydrogen and nitrogen, will not interfere with the result of any subsequent experiment, although the receiver should not be thoroughly exhausted. Under these circumstances, let the exhaustion be carried to 400°, and let 300 measures of the mixture enter, so as to depress the mercury in the gage to 100 on the scale. An explosion being effected, the mercury in the gage will rise at first to about 215°, and after the gas shall have regained its previous temperature, will be found somewhat above 220°.

Of course there will be a deficit produced of more than 120 parts, of which one-third, or a little more than 40 parts, will be the quantity of oxygen in 200 parts of the air, subjected to analysis.

In order to ascertain the influence of temperature, a thermometer is placed in the receiver, the state of which is noted before and after explosion; and the deficit is estimated, either by allowing for the difference produced by the temperature, or awaiting the refrigeration, until the mercury in

* Of the engravings and descriptions of apparatus used in the chemical course of the University of Pennsylvania.

the thermometer be at the same height as before the explosion.

From this account of the barometer gage eudiometer, and those previously given of the sliding rod instruments, it must be evident that I have contrived three methods of analyzing the atmosphere, or other mixtures containing oxygen, or hydrogen gas.

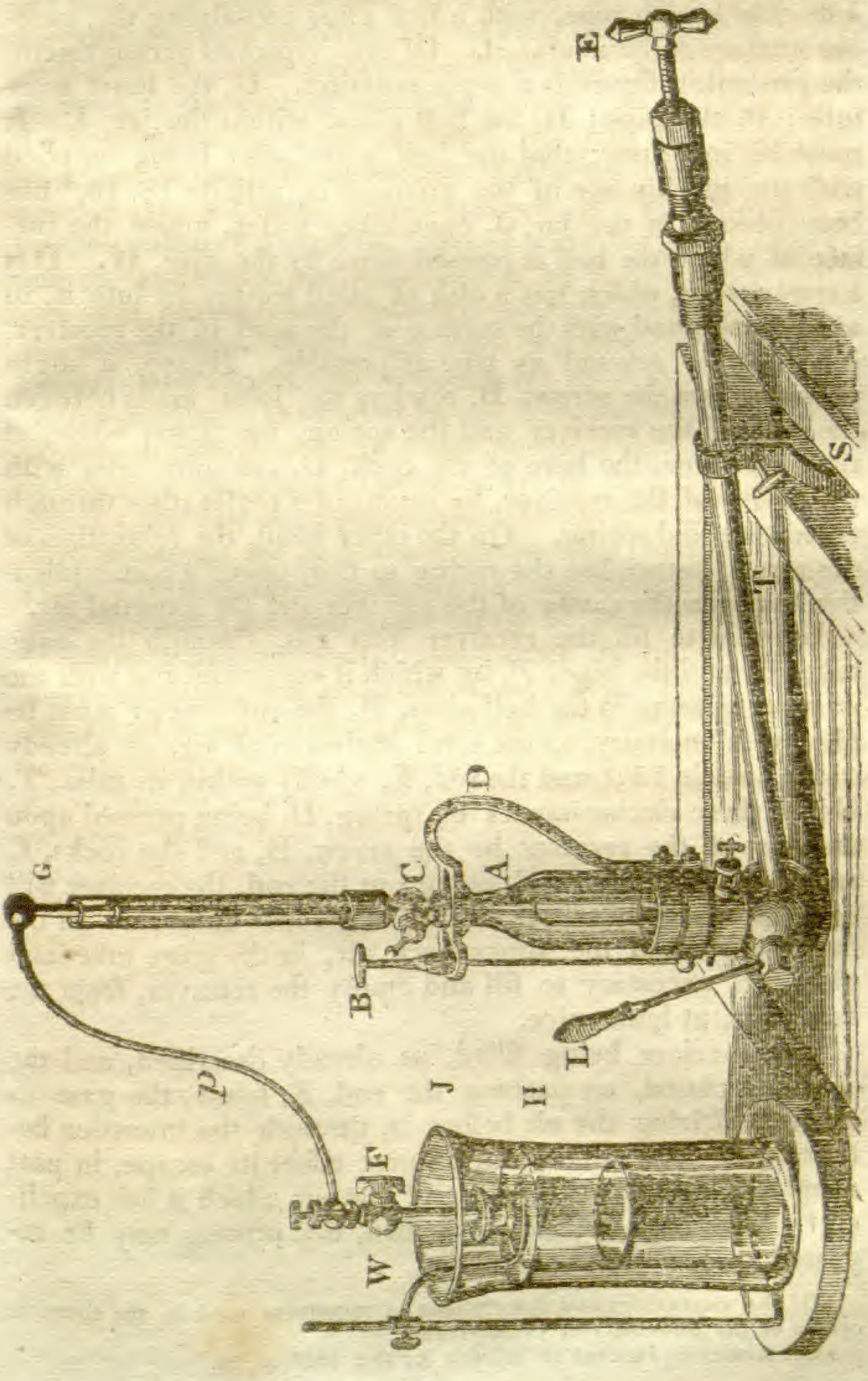
In the barometer gage instrument, the deficit is known by its effect upon the mercury in the gage tube; in one of the sliding rod instruments, the deficit is compensated by water, and the quantity of this liquid, which enters for this purpose, is known by the portion of the sliding rod which remains without, after excluding the residual gas. In the instrument with the sliding rod and gage, the deficit is compensated by introducing the rod, the gage enabling us to know when it has been introduced sufficiently; while the graduation shows the ratio of the gaseous matter condensed, to the quantity confined.

When the diversity of these methods is considered, it is pleasing to observe but little difference in the results obtained by them.

A great number of experiments performed by means of the barometer gage eudiometer, or those of the sliding rod construction, over water, and over mercury, gave $20\frac{66}{100}$ as the quantity of oxygen in 100 parts of the air. In twenty experiments the greatest discordancy did not amount to $\frac{1}{1000}$ part in 100 measures of air.

I am now constructing a barometer gage eudiometer, in which I mean to use phosphorus to abstract the oxygen. I have already performed some experiments with one of this kind, but owing to defects in the process, which I have no doubt of obviating, the results were not satisfactory.

IMPROVED MERCURIAL SLIDING ROD HYDRO-OXYGEN EUDIOMETER.



The method of operating with the steel eudiometer and water gage, over mercury, has been facilitated, by allowing the upper end of the inner gage tube to communicate through a flexible leaden pipe, with a bell glass containing the gaseous mixture to be analyzed. Of this improved arrangement, the preceding figure is a representation. G, the inner gage tube; P, the pipe; H, the bell glass, within the jar, J. It must be imagined, that the bell glass, after being supplied with the gas by one of the volumeters, articles 18, 19,* has been placed in the jar, J, containing water, under the surface of which the bell is pressed down by the wire, W. D is a steel spring, which has a disk of oiled leather let into it, so as to correspond with the surface of the apex of the receiver, A, which is ground as true as possible. Hence, a slight pressure from the screw, B, renders the joint, made between the apex of the receiver and the spring, air tight; while, at the same time, the bore of the cock, C, communicates with the cavity of the receiver, by means of a perforation through the leather and spring. On the other hand, the relaxation of the screw, permitting the spring to rise, opens a communication between the cavity of the receiver and the external air.

In order to fill the receiver with gas, through the gage tube, G, and the pipe, *P*, by which it communicates with the gaseous mixture in the bell glass, H, the eudiometer must be filled with mercury, to the total exclusion of air, as already stated, article 15;* and the rod, E, wholly within its tube, T. Under these circumstances the spring, D, being pressed upon the apex of the receiver by the screw, B, and the cocks, C and F, both open; on drawing out the rod, the receiver will be proportionably supplied with the gaseous mixture. In order to get rid of the atmospheric air, in the gage tube and pipe, it is necessary to fill and empty the receiver, from the bell glass, at least twice.

The receiver being filled, as already described, and the cock, F, closed, on pushing the rod, E, home, the gaseous mixture, driving the air before it, through the interstice between the gage tubes, will in part effect its escape, in part supply, in the tubes, the place of the air which it has expelled.† The cock, F, being opened, this process may be repeated.

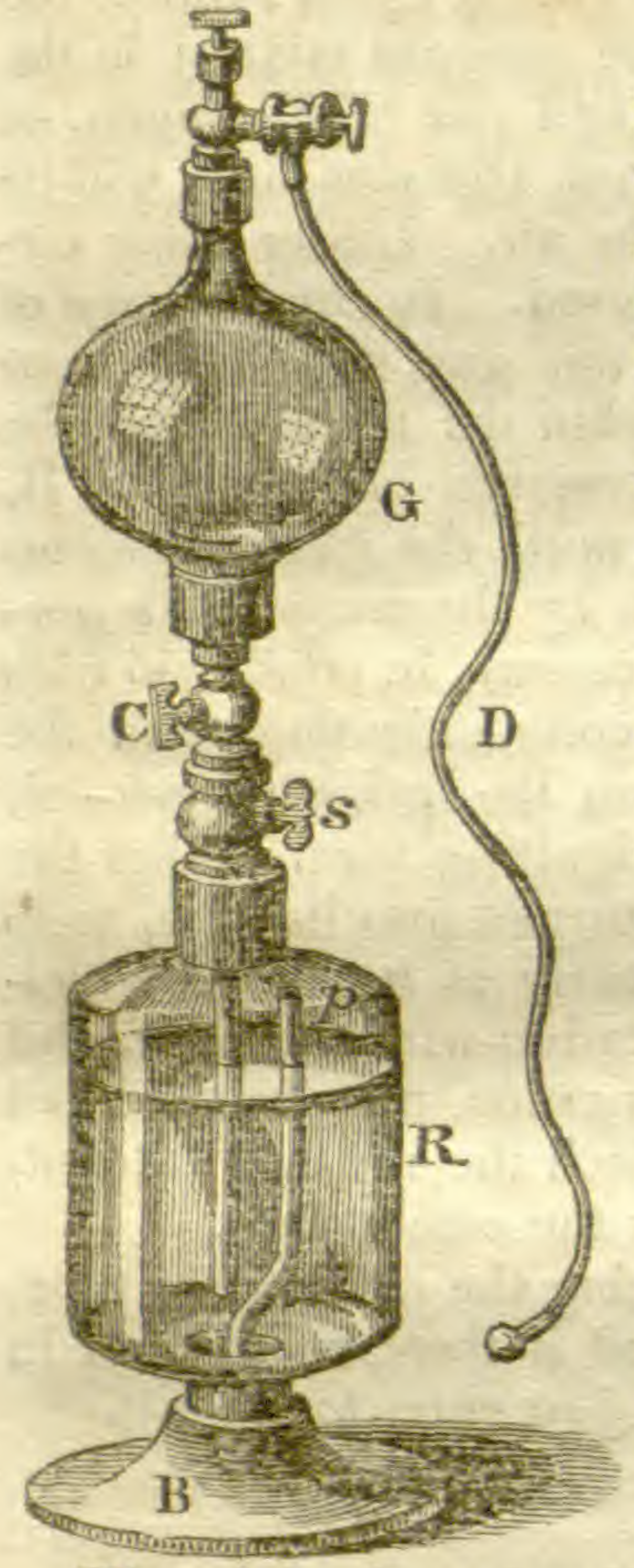
* Of the engravings and descriptions of apparatus used in the chemical course of the University of Pennsylvania.

† See American Journal of Science for Oct. 1825, p. 75.

After the apparatus has, by these means, been purged of atmospheric air, the cocks, C and F, being open, suppose the rod drawn out 300 degrees. If the gaseous mixture in the bell consist of two volumes of air and one of hydrogen, of the 300 measures drawn in by the rod, 100 measures will be hydrogen, and 200 measures will be air. Under these circumstances the cock, F, must be closed. In consequence of the hydrostatic pressure to which the gas will have been subjected in the bell, its density within the receiver will be greater than without. Hence the pressure of the screw, B, on the spring, L, must be relaxed until the gage indicates that the gas within the receiver has, by the escape of a portion of it, become, with respect to pressure, in equilibrio with the atmosphere. The cock, B, communicating with the gage, is then closed, the pressure on the spring is restored, and an explosion is effected by the igniting wire. After the explosion, the rod is to be so far returned into its tube, as to compensate the condensation as nearly as it can be anticipated. By restoring the communication with the gage, and duly removing the rod, the compensation may be rendered exact. The number of degrees which the rod has entered, must represent the deficit caused by the combustion.

By loosening the spring, and forcing the rod into the tube, the residual gas will be expelled, and if there be no error in effecting the expulsion, the rod will just enter to the hilt.

CORBONICOMETER.



The apparatus here represented, is one which I have contrived, for withdrawing a known portion of residual air from the barometer gage eudiometer, in order to wash it with lime water.

P, is a pipe, which causes a communication between the upper part of the receiver, R, and the cavity under the hollow pedestal, B. The lower orifice of this pipe, where it enters the cavity of the pedestal, is covered by a valve opening downwards. The receiver is surmounted by a brass cap, into which, as well as the socket in the pedestal, it is cemented air tight. In the axis of this receiver, and descending nearly to the bottom, may be seen a tube, which is soldered into a perforation communicating with the bore of the cock, C, so as to establish a communication between the inside of the receiver and that of the globe, G.

The globe is surmounted by a valve cock, furnished with a gallows and screw, so that a leaden pipe, D, terminated by a brass knob, duly perforated, may be joined to it, air tight, without difficulty. Hence, if the pipe be annexed, at the other end, to the cock of the barometer gage eudiometer, a communication between the inside of the receiver of this instrument, and the globe, G, may be easily opened or suspended at pleasure.

Suppose the receiver, R, to be occupied by lime water, as represented in the figure. Place the pedestal, B, over the hole in the air pump plate, which the rim of the pedestal is ground to fit. On working the pump, the air of the receiver, above the lime water, is drawn out through the valve at the bottom of the pipe, P. Of course, the air in the globe follows it through the pipe, which leads from it into the receiver. Having exhausted the globe and receiver, if the screw, S, be so loosened as to allow the atmosphere to enter the receiver, and press upon the surface of the lime water, while the globe remains exhausted, the lime water will of course rise into and fill the globe. Should the receiver, under these circumstances, be again exhausted, while, by means of the flexible pipe, a communication with the barometer gage eudiometer is effected, the pressure of the gas in the eudiometer being greater than that of the rare medium of the exhausted receiver, it follows that this gas will press into the globe, and cause a portion of the lime water to descend into the receiver. In this way, suppose 100 measures, by the barometer gage, taken from the eudiometer. The valve cock may then be closed, the screw, S, in the neck of the receiver, relaxed so as to admit the atmosphere. The lime water will rise into the globe, until the pressure of the gas therein be nearly equal to that of the atmosphere. By agitating the globe, the carbonic acid will combine with the lime in the water. When this object is effected, the residual gas may be allowed to re-enter the eudiometer, where the quantity of it may be measured, and consequently the extent of the absorption known. It is not necessary that the apparatus should remain upon the air pump plate during the whole process. By means of the valve, which covers the perforation in the pedestal, in which the pipe, P, is inserted, the exhaustion may be sustained, during the removal of the receiver from the air pump, to any part of the laboratory, where it may be convenient to connect it with the eudiometer.

ART VII.—*Reply to a criticism of* PROF. OLMSTED, *upon the arguments respecting the materiality of heat, adduced by* DR. HARE.

IN the number of the American Journal of Science, for October last, I observe some strictures by Prof. Olmsted, on an essay of mine, on the question *whether heat can be motion*, published in 1822, in Vol. 4 of the same work.

The following passage is partially quoted, from Sir Humphrey Davy's Elements, by the learned professor, as introductory to his strictures.—I beg leave to quote it in full. "It seems possible," says Sir Humphrey Davy, "to account for all the phenomena of heat, if it be supposed that in solids the particles are in a constant state of vibratory motion, the particles of the hottest bodies moving with the greatest velocity, and through the greatest space—that in fluids, and elastic fluids, besides the vibratory motion, which must be conceived greatest in the last, the particles have a motion round their own axes, with different velocities, the particles of elastic fluids moving with the greatest quickness—and that in ethereal substances, the particles move round their own axes and separate from each other, penetrating in right lines through space. Temperature may be conceived to depend upon the velocity of the vibrations; increase of capacity on the motion, being performed in greater space; and the diminution of temperature, during the conversion of solids or fluids into gases, may be explained on the idea of the loss of vibratory motion, in consequence of the revolution of particles round their axes at the moment when the body becomes liquid or aeriform—or from the loss of rapidity of vibration, in consequence of the motion of the particles through greater space."

After his partial quotation of this passage, Prof. Olmsted proceeds as follows:

"He (Dr. Hare) has attempted to show, that the supposition that temperature results from the velocity of the particles of heated bodies, subjected to a vibratory motion, is inconsistent with the laws of mechanics. 'It is fully established in mechanics, (says Dr. Hare,) that when a body in motion is blended with, and thus made to communicate motion to another body, previously at rest, or moving slower, the velocity of the compound mass, after the impact, will be found by multiplying the weight of each body by its respective velocity, and dividing the sum of the products by the aggregate weight of both bodies.' He then proceeds to show that the phenomena of temperature do not coincide with this law. Thus, when water or mercury of different temperatures is added together, the resulting temperature is not a mean, as it would be, were temperature merely the operation of a law of motion; but the water is affected too little, and the mercury too much, to admit of our referring the change to such a law. Little as I am disposed to adopt the views of Sir Humphrey Davy, I cannot but think that Dr. Hare has here suggested an answer which is not altogether unobjectionable. The application of his rule or test, makes it necessary to suppose, that the particles subjected to impact, are all moving in the same direction—that they all actually come into collision, each upon each, and that they are non elastic; none of which conditions are capable of being proved actually to exist, although it is only when they are all present, that the law of motion which he adduces holds true—however, if Dr. Hare be allowed to have fully and clearly refuted the hypothesis of Sir Humphrey, his argument is still imperfect, for it by no means establishes the doctrine of the materiality of heat, to prove that Davy has failed of showing that it is a product of motion. Both parties, in my view, evince how idle it is to reason respecting chemical phenomena upon mechanical principles."

However "*idle*" it may be to advance mechanical principles as the means of explaining the phenomena of Chemistry, I assert that, *when mechanical principles have been brought forward as the means of explanation*, it is not *idle* to show the explanation thus founded inconsistent with its own premises.

Though I might have hesitated in applying to the reasonings of so great a man as Sir H. Davy, the epithet employed by Prof. Olmsted, I challenge him to point out in my essay any word which tends to show, that I do not think it idle to employ mechanical principles in reasonings on Chemistry.

We concur in disapproving of the hypothesis of Sir Humphrey Davy, but because I have met it with arguments upon its own basis, instead of briefly denouncing it, Prof. Olmsted accuses me, no less than the illustrious author, of polluting chemical science, with mechanical reasonings.

If these reasonings be idle, let the great English Chemist, who introduced them, bear the weight of Prof. Olmsted's animadversion. Besides erroneously holding me up as the friend of a method of reasoning, of which I am really the antagonist, the criticisms of Prof. Olmsted would convey to any person, who had not read my essay, an impression that I had been so dull as to consider a disproval of the hypothesis of Sir H. Davy as establishing that which I have myself espoused; and that I had advanced no direct arguments in favour of the materiality of heat, although to such arguments, the latter part of the essay is devoted. I beg leave here to quote the reasoning, as I am still of opinion that it is unanswerable, notwithstanding the unaccountable neglect with which it has been treated by the professor.

"We see the same matter, at different times, rendered self-attractive or self-repellent; now cohering in the solid form with great tenacity—and now flying apart with explosive violence, in the state of vapour. Hence the existence, in nature, of two opposite kinds of reaction, between particles, is self-evident. There can be no property without matter, in which it may be inherent. Nothing, can have no property. The question then is, whether these opposite properties can belong to the same particles. Is it not evident that the same particles cannot, at the same time, be self-repellent and self-attractive? Suppose them to be so—one of the two properties must predominate; and in that case, we should not perceive the existence of the other. It would be useless, and the particles would, in effect, possess the predominant property alone, whether attraction or repulsion. If the properties were equal in power, they would annihilate each other, and the matter would be, as if void of either property. There must, therefore, be a matter in which the self-repellent power resides, as well as matter in which attraction resides."

In support of my opinion, I also cited the radiation of heat in vacuo, agreeably to an experiment of Sir H. Davy himself, in which a thermometer in the focus of one mirror, is influenced by a hot body in the focus of another mirror, the whole being within an exhausted receiver. I will thank Prof. Olmsted to explain how heat can be transmitted under such circumstances, even with more ease than in pleno, if the cause of it be not material.

I did not dwell on this fact, because I supposed its importance generally known and admitted, and conceived that it would produce the most forcible impression, when viewed in its greatest simplicity.

In opposition to Davy's hypothesis, I had advanced several arguments, of which Prof. Olmsted notices but one. With respect to that, it does not appear to me that he has adduced any fact, or any learning, which can invalidate the application, which I have made of a rule admitted by him to be true to a limited extent. It is enough for me, if the case in point fall within the limits of that rule. What is the case in point? The particles of two masses, mercury and water, while undergoing a vibratory rotatory motion, with unequal velocities, have their movements equalized by contact. Will Prof. Olmsted deny that the resulting motion ought to be nearest to that, to which the heavier particles were previously subjected?

The reasoning in my essay which Prof. Olmsted has overlooked, is as follows:—As, in order for one body, or set of bodies, in motion, to resist another body, or set of bodies, in the same state, the velocity must be as much greater as the weight may be less, it is inconceivable that the particles of steam should, by any force arising from their motion, impart to the piston of a steam engine the wonted power: or that the particles of air should prevent a column of mercury, almost infinitely heavier, from entering any space in which they may be included by beating it out of the theatre of their vibratory and rotatory movements.

Again, admitting it to be conceivable that the momentum of particles so light may be competent to such effects, it is utterly impossible that these could be permanently sustained; since in all cases where motion is communicated, what is gained by one body is lost by another: so that the motion of the body communicating the motion, is lessened at every impact, and finally ceases.—Further, since it is self-evident that a body, acting directly upon another, cannot produce a motion greater than its own, it is incredible that heated solids should, by any possible movements of their particles, produce the prodigious velocities, which, according to the disputed doctrine, must be attributed to aeriform matter, when its levity and its power of resistance, as above exemplified, are taken into view.

I must leave it to the reader to judge how far these arguments merit the oblivion, to which Prof. Olmsted would consign them.

ART. VIII.—*Observations relative to some of the Mountain Districts of Pennsylvania, and the Mineral resources of that State, in its Anthracite, Bituminous Coal, Salt and Iron, with miscellaneous remarks;* by JAMES PIERCE.

A CONSIDERABLE proportion of the State of Pennsylvania is occupied by mountains, generally uninviting to the settler, and mostly remains in a state of nature. It is crossed by the Blue Ridge, Alleghany, numerous minor ridges, and extensive tracts of elevated table land, that spread over an average width of 150 miles. Though undesirable for agriculture, much of this mountain region contains, in its anthracite, bituminous coal, salt and iron, mineral treasures that will be a source of inexhaustible wealth to the State.

In no part of the world is anthracite, so valuable in the arts and for economical purposes, found as abundantly as in Pennsylvania. Its cheap diffusion will be of incalculable advantage to the Atlantic States, where the increased expense of fuel begins to be felt; and its further enhancement would limit the population, and materially interfere with the progress of manufactures. To agriculture it will be a material auxiliary: fuel being supplied from the interior of the earth, the surface can be more extensively cleared and cultivated, and wherever our numerous calcareous valleys are intersected by canals or navigable streams, lime, so valuable as a manure, and in the arts, can be calcined at a low rate, by the aid of anthracite. Experience has demonstrated that for the manufacture of iron this fuel is peculiarly advantageous, as it embraces little sulphur, or other injurious ingredients; produces an intense, steady heat; and, for most operations, it is equal, if not superior, to coke. Bar iron, anchors, chains, steamboat machinery, and wrought iron of every description, has more tenacity and malleability, with less waste of metal, when fabricated by anthracite, than by the aid of bituminous coal, or charcoal, with the important additional advantage of a diminution of expense, at least fifty per cent. in labour and fuel; and iron castings are stronger when the melting has been effected by the aid of anthracite. Possibly, in the quick process of heating and fusing, it communicates less carbon and oxygen. For breweries, distilleries, and the raising of steam, anthracite coal is decidedly preferable to bituminous coal, or other fuel, the heat being more steady and manageable, and the boilers less corroded by sulphurous acid, while no bad effects are produced by smoke and bitumen.

The anthracite of Pennsylvania is located between the Blue Ridge and the Susquehanna, and has not hitherto been found in other parts of the State, except in the valley of Wyoming.

The anthracite district is principally occupied by mountains running parallel to the Blue Ridge, often broad with table land summits, and rising generally about 1500 feet above the ocean. With the exception of a few narrow valleys, this region has little surface inviting cultivation. The summits, by repeated fires, have been divested of much timber, leaving, thinly scattered, pitch and yellow pine and white oak, and are generally too stony for tillage, but they may, at some future period, afford good ranges for cattle and sheep. In an extensive elevated valley, bordering upon the head waters of the Lehigh, there is considerable land clothed with a dense forest of beech, hemlock, maple, birch, &c. with a good soil for grazing. The anthracite mountains, and ranges connected with them, are mostly in a state of nature, and afford retreats for panthers, wolves, bears, deer, and other animals resident in the unsettled parts of our country. In passing from the Berwick turnpike to Wilkesbarre, in a distance of thirty-five miles, I noticed but three dwellings, and two of these were log taverns lately erected. Between twenty and thirty panthers have been killed, within three years, by the hunters of Lowrytown, a settlement recently formed on the Lehigh.

The rocks of the above described region are of the transition class, and present little diversity, being principally gray wacke slate, which occurs in abundance, loose on the surface and in ledges. It is sometimes based on old red sand stone, and surmounted by an unstratified rock, an aggregate of quartz pebbles of various dimensions, with a cement principally silicious. In the Blue Ridge, in addition to the above described rock, a silicious gray wacke, resembling fine grained granular quartz, is common. It appears in some places massive, but is often slaty. Its cement is mostly silicious; some alumine, however, is indicated in its composition.

The beds and veins of anthracite range from north-east to south-west, and may often be traced for a considerable distance by the compass. The veins have the inclination of the adjacent strata of gray wacke, with which they often alternate, usually between 20° and 45°. In a few places they are horizontal and vertical. The beds and veins of anthracite have narrow strata of dark coloured, fine grained, argillaceous schist, for the roof and floor. This slate generally contains sulphuret of iron, and disintegrates on exposure to the air. The sulphates of iron and alumine are often observed in the schist, and it frequently presents impressions of plants, and sometimes of marine shells. Impure pulverulent coal is usually connected with this slate, and is said to be a good material for printer's ink.

Anthracite has been found in greatest quantity, in sections of the coal region most accessible by water. Extensive veins and beds range from the Lehigh to the Susquehanna, crossing the head waters of the Schuylkill and Swatara about ten miles north-west of the Blue Ridge, and it abounds contiguous to the Susquehanna and Lackawanna. But in no part of the district does anthracite occur in such apparently inexhaustible beds, or is so abundantly raised, as in the vicinity of Mauch Chunk, a village situated on the Lehigh, thirty-five miles from Easton, and one hundred and eight, by water, from Philadelphia.

The coal is there excavated on the flat summit of a mountain that rises near 1500 feet above the ocean. It is of good quality, and presents beds of unparalleled extent; is disclosed for several miles on the summit, wherever excavations have been made, and is indicated in many places by coal slate, in a pulverulent state, on the surface. The mountain rises with steep acclivity, particularly on the north-west side, and when penetrated at various altitudes, discloses coal at about the same distance from the surface. Strata of gray wacke slate, containing mica, sometimes rest on the coal, parallel with the mountain side. In the deep excavations made on the summit, no termination of the coal bed has been found, and it is not improbable that anthracite forms the nucleus of the mountain for a considerable distance.

The coal is rendered accessible by removing from the flat summit, gravelly loam, which is from a few inches to four feet in depth, and disintegrated slate with impure coal, from two to four feet. The coal rests in a horizontal position, narrow parallel seams of argillaceous schist intervening. This schist exhibits saline efflorescences. Strong chalybeate springs, holding in solution sulphate of iron, issue from the mountain's side. The coal excavation on the surface is extensive, and from thirty to forty feet in depth, forming a hollow square, bounded by lofty mural precipices of coal. Waggons are admitted by avenues that serve to discharge water from the mine. The coal is easily detached by picks and bars. From this bed, in 1825, about 750,000 bushels of coal were sent to Philadelphia, and it is expected that a million of bushels will be forwarded the current year. The expense of raising coal is 40 cents the ton.

This coal mountain range is reported as extending in a south-west direction to the Susquehanna. To the north-east, beyond the Lehigh, it is connected with Broad Mountain, the first considerable elevation west of the Blue Ridge, or Kittetany Mountain, as it is sometimes called.

Rocks in place rarely occur on the table land summit, adjacent to the coal bed. Old red sand stone exists in places near the mountain's base, with superincumbent strata of gray wacke slate, and silicious aggregate of quartz pebbles, resembling the mill stone quarries of the Shawangunk mountain, which probably may be put to the same use.

About ten miles of the coal mountain, the village of Mauch Chunk, and an extensive tract adjacent to the Lehigh, are the property of a company having a capital of a million of dollars, incorporated by the Legislature of Pennsylvania. They have constructed a good turnpike, that rises gradually 936 feet in nine miles, the distance between Mauch Chunk and the coal bed. The cost of transportation by this road is 60 cents the ton; about seven tons are conveyed with ease on two waggons, drawn by four horses.

A rail-way has been surveyed on the mountain's side to the coal bed. It will be single, with places for turning out, and terminates near the Lehigh, at an elevation of about 200 feet above its level. The coal, in its descent from thence to the yard, or boats, can be riddled of earth and fine particles. Iron waggons, carrying 3 tons of coal, will descend on the rail-way, by their own gravity, regulated by a piston connected with the wheels, and working in a horizontal cylinder with vases and stop cocks. The resistance of air or water in the cylinder, will be sufficient to retard or arrest the waggons in their course.

It is calculated that the expense of transportation on the rail-way will not exceed 25 cents the ton.

The Lehigh Company are endeavouring to procure coal 2½ miles from Mauch Chunk, by tunnelling 200 feet below the precipitous ridge, that occupies the eastern brow of the coal mountain. The excavation of a sufficient magnitude for the passage of teams, has been extended more than 600 feet in a hard rock of quartz pebbles, without finding coal. Twelve workmen are constantly employed, and have advanced about a foot each day. Shafts have been sunk 60 feet in the table land, at the base of the narrow rocky ridge: good coal was found after penetrating seven feet of earth and slate. The bottom of the shaft is supposed to be about 80 feet above the tunnel. If no coal is struck in proceeding horizontally, the tunnel will still be serviceable for discharging water from the great coal bed above, and to receive coal from that bed which may extend to within a short distance of the excavation, and can be worked in horizontal galleries. The sides of the ridge, above the tunnel, are too precipitous for the passage of waggons. The elevated summit of the ridge commands a view of many wild, rocky, wood clad ranges, and deep ravines.

The improvement of the navigation of the Lehigh, is one of the conditions annexed to the charter of the company: this has already been effected in respect to a descending navigation, from Mauch Chunk to the Delaware, and further improvements for an ascending navigation, and to extend the water communication up the river to Stoddartsville, are in progress. The Lehigh is a copious rapid stream, that has it origin among wood clad rocky mountains, forty miles north-west of Mauch Chunk. Its waters are pure, and well stored with trout, pike, sunfish, catfish, eels, perch and other fish. There is considerable good pine and other timber, adjacent to the Lehigh, 18 miles above Mauch Chunk, much of which is the property of the company, who have thus formed a settlement for cutting and rafting timber, at which they employ 150 men. The descent from Stoddartsville to Mauch Chunk is 925 feet, and for the effectual improvement of the navigation, 38 large dams will be required. They will afford valuable sites for mills and manufactures. When the contemplated improvements are effected, the Lehigh will be navigable within 12 miles of the rich valley of Wyoming, and much of the wheat of that section of country, which is now transported 60 miles to Easton, may find a readier market at Merchant Mills on the Lehigh. The descent from Mauch Chunk to Easton is 364 feet, to overcome which, it is calculated that 21 dams and 52 locks will be necessary. Many dams have been already constructed of pine logs, at an expense of about three thousand dollars each. They are located at the head of rapids, enabling the navigator to command an artificial freshet, when the stream from its dispersion, would not otherwise admit of the passage of boats. Water from the dam is copiously admitted into a rail-way that extends to the foot of the rapid. The gates, in the lock at the head of the rail-way, of peculiar construction, were invented by one of the managers, Mr. White, to whom the company are indebted for many ingenious improvements. The gates are attached by hinges to the bottom of the lock, and rise by the force of water admitted from a floom, constructed parallel with the lock, and remain suspended, forming a section of the dam. If the gate of the floom is closed, the water between the gates passes off, and they fall by their own weight and the pressure of the water from the dam. To facilitate an ascending navigation, short canals to the termination of the rapids will be required. A canal of a mile and a quarter, commencing at Mauch Chunk, has recently been excavated: the locks are of the new construction above mentioned.

The Lehigh from Mauch Chunk to the water gap, 11 miles, winds between rocky mountains, with a brisk current, but presents no falls. The road usually runs near the stream, and sometimes at a considerable elevation above, on the steep mountain's side. In its passage through the Kittetany, or Blue Ridge, the river has a pretty tranquil and but slightly inclined course. On the adjacent elevation, yellow pine, hemlock and spruce, are interspersed with trees of annual verdure. From the water gap to the Delaware, the river pursues its course in a deep ravine, with rarely alluvial borders of much extent, and is seldom seen from the road. The soil in this district of country, generally rests on limestone sinks, indicating caves, and fissures in the rocks are often observed, and will render canalling in places difficult. From the confluence of the Lehigh with the Delaware to tide water, the descent is 150 feet. The rapids of the Delaware in dry seasons, present great impediments to forwarding coal. A canal will probably be found indispensable to secure a permanent ascending and descending navigation of this stream. Boats are rarely lost in the descent.

Coal is conveyed to market from Mauch Chunk, landing in flat bottomed shallow boats, 12 feet wide by 16 in length, connected by hinges, and denominated boats in sections. Six or seven of these boats, each laden with ten tons, are usually united, and are navigated to the Delaware by four hands; from thence to Trenton, five are required. Six days are commonly occupied in the descent to tide water, and in the return. The boats are disposed of for lumber. The construction of the boats, independent of materials, costs 60 cents each. If formed of pine plank, they are probably disposed of without loss, when the navigation is completed. The boats can be conducted by small steam vessels, and returned, producing an important saving. At present, the expense of raising and transmitting coal from the Lehigh beds to market, is less than from any part of the coal regions. It does not exceed two dollars the ton, of 28 bushels, and will be materially lessened by the construction of canals and rail-ways. If, as is probable, the price in Philadelphia should under competition be reduced to four or five dollars, this coal trade will still afford a large profit on the capital employed. When the numerous canals connecting the extensive coal region of Pennsylvania, with the navigable waters of the sea-board shall be accomplished, coal will be far the most economical fuel for our commercial towns, and parts accessible by water. In large cities, it will be peculiarly valuable from its safety, and may save considerable expense in the construction of dwellings. By adopting stove furnaces and pipes, they can dispense with chimnies and fire-places, and the removal of soot, and obstructions by sweeping will not be required.

In the valley of the Delaware, which in the latitude of Easton, has a width of thirty miles, presenting a rich soil, with a calcareous basis, anthracite will be of great utility from the low rate at which it can be afforded, superceding the necessity of retaining groves for fuel, now frequent on superior arable land, and will enable the manfacturer of lime to furnish that valuable manure at a very low rate. It is now calcined in kilns, which may be continued in blast without intermission, at the cost of two cents the bushel, by the aid of anthracite. From twenty to thirty bushels of lime can in ordinary kilns be daily subtracted from below.

The village of Mauch Chunk is situated on the western bank of the Lehigh, in a deep romantic ravine, between rocky mountains, that rise in some parts precipitously to 800 or 1000 feet above the stream. Space was procured for dwellings, by breaking down the adjacent rocks and by filling a part of the ravine of the Mauch Chunk creek. A portion of this stream has been transferred to an elevated rail-way, and is used to propel a grist mill. Within six years the Lehigh Company have erected, and are the proprietors of, about 120 dwellings and buildings of every description, including a large hotel, a store, two furnaces, a grist mill, and several saw mills: about 800 men are employed by the company. Stricter moral obligations are here subscribed to and observed, than could be enforced by a state, or the general government, as the penalty of violation is dismissal, without reprieve, from a very desirable service, and the ejectment of tenants at will from their dwellings.

Tippling houses, and the retail of ardent spirits, are not tolerated. There is but one tavern and store in the village, and they are owned by, and under the control of, the company. Drunkards are not suffered to remain. Abuse or neglect of their families, and cruelty to cattle, are grounds of dismissal. There is no regular place of worship, but clergymen of every denomination are invited to preach, and dissipation is prohibited on the sabbath. By a small annual contribution from each workman, and heads of families in the village, an able physician is procured, who attends the sick without further compensation.

Labourers, in the employment of the company, are furnished with daily rations of whiskey—a practice to be deprecated, as inducing habits of intemperance. Beer should be substituted, as was once contemplated. More than an equivalent in money is now offered to those who abstain from ardent spirits, so unnecessary for the performance of labour, which considerably enhances the receipts of those who accept of the terms.

The company have a small furnace in operation, which produces daily about 3500 pounds of castings. The ore used is of a good quality, procured twenty miles below, near the Lehigh. A ton of coal is exchanged at the furnace for the same weight of ore. Limestone, necessary for a flux, is furnished at the same rate. A third part of bog ore, found near the village, is mixed with the purchased ore. The head workman informed me that by blending a tenth part of pounded anthracite with charcoal, in smelting, a third more work is done in a given time, than would be produced by charcoal alone. Pigs are melted for castings entirely by pounded anthracite, producing better castings and great diminution of labour. A stronger blast, however, is necessary for anthracite, than is required for charcoal. Sledge hammers, cast from meltings by anthracite, have been found sufficiently strong to be useful. The coal is pounded in a water mill. A large furnace has recently been erected by the company, where castings for the rail-way will shortly be produced.

Several bodies of anthracite occur north-west from the coal beds of the Lehigh company, and about eleven miles from Mauch Chunk. The most extensive is located in the Beaver meadow, south of the Berwick turnpike. It has been ascertained to be more than fifty feet in thickness, and of a quality equal, if not superior, to the best procured by the Lehigh Company. A considerable quantity has been raised this season, and conveyed to the Lehigh for shipment. By a rail road, this coal may be brought at a moderate expense to navigable waters, and be productive.

In travelling north from the Berwick turnpike, I ascertained, that veins of coal range in a northern direction from the Beaver meadow for many miles. One was mentioned as existing three miles from Lowry town.

Coal has not yet been discovered in Pennsylvania to the north-west of the Lehigh, but as it is an unexplored region of the same geological character, rocks, &c. with the anthracite district, it is not improbable that it may be discovered in that direction, and it is rendered more probable by the existence of narrow veins of anthracite in the gray wacke eminences of Sullivan and Ulster in the State of New-York, connecting the Catskill mountains with the anthracite ranges of Pennsylvania, with the exception of a vein of anthracite, said to have been recently discovered in the Delaware water-gap ; that mineral does not occur within ten miles of the Blue or Kittetany mountains, or of the Shawangunk, a part of the same chain, and which presents similar rocks.

From the principal coal bed of the Lehigh Company, to a considerable branch of the Schuylkill, the distance is but three miles. The navigation of this stream is susceptible of improvement, and may, at a future period, be the medium of conveyance for coal found on its borders.

Between Mauch Chunk coal bed and Mount Carbon, a distance of eighteen miles, veins of coal running in a south-west direction occur. It is mountainous, unsettled, and little known.

Next to Mauch Chunk, Mount Carbon, or Pottsville, as it is now called, situated at the head of the Schuylkill canal, has been the principal source of the supply of anthracite. Many large veins are worked within three miles of the landing; and some have been opened seven miles to the north-east, in the direction of the Lehigh beds. The chief veins wrought, are, one situated on an eminence adjacent to the village; Bailey's mine, about two miles from Pottsville, and near the turnpike to Lunbury, and on the territory of the New-York Schuylkill coal company, about three miles from the village. On almost every eminence adjacent to Pottsville, indications of coal are disclosed. The veins generally run in a north-east direction, with an inclination of about 45 degrees, and are from three to nine feet in thickness. Commencing at or near the surface, they penetrate to an unknown depth, and can often be traced on hills for a considerable distance, by sounding, in a north-east or south-west direction. Some veins have been wrought to the depth of two hundred feet without a necessity of draining; the inlined slate roof shielding them from water. Where the ground admits, it is considered the best mode of working veins to commence at the back of a coal eminence, or as low as possible, and work up, filling the excavation with slate and fine coal, leaving a horizontal passage for the coal barrows. A section of a wide vein near Pottsville has been wrought by this mode several hundred feet into the hill. The same vein is explored from parts of the summit by vertical and inclined shafts. The coal and slate handled, are raised by horse power, in waggons by a railway that has the inclination of the vein. Veins of coal alternate with gray wacke slate in the hill. Vegetable impressions sometimes occur in the argillaceous schist that forms the roof of the Pottsville coal veins.

On the extensive tract occupied by the New-York company, coal is reported as inexhaustible. I was informed by the company's superintendant at Pottsville, (Mr. Baker,) that coal beds, from forty to one hundred and fifty yards in width, are there indicated by coal slate; good coal is found in sounding between the layers of slate; but they have not been much explored; in one or two places veins in vertical and horizontal position occur; but they have generally, on the lands of the company, the usual inclination and direction. About three hundred men are employed by this company.

It is contemplated to render a western branch of the Schuylkill navigable, which will give easy access to a large body of coal, the property of the New-York company, situated five miles from Pottsville.

Coal at the head of the canal is sold for ten cents the bushel. The raising costs about three, and cartage a cent a mile ; the canal tolls amount to about $5\frac{1}{2}$ cents, and freight near four, making the whole expense from the mine to Philadelphia from 4 dollars to 4 $\frac{50}{100}$ the ton, according to the location of the coal bed, or economy used. A considerable saving in the item of cartage would be effected by the formation of rail-ways to the principal mines. They are generally situated considerably above the landing, and present a descent most of the way to the coal yards. It is probable that there will be a further extension of the canal into the coal region, which, with the formation of rail-ways, will give access to coal beds otherwise too remote for profitable working.

Should it be found necessary, to enable the proprietors of coal beds at Pottsville to come in competition in the market with coal from other localities delivered at a cheaper rate, the canal company will find it for their interest to lower the rate of toll, and would be compensated by an increased quantity shipped.

From Pottsville to Philadelphia, a distance of 132 miles, the descent is 588 feet. The navigation of the Schuylkill is improved by alternate dams and canals. Between the coal mountains and the Blue Ridge, the canal passes through a wide, extensive, elevated, and very broken valley, of a generally poor soil, partially cultivated, and thinly occupied by inhabitants of German descent, who speak the language of their ancestors. It was represented that not one in ten could converse in English. There is a narrow range of limestone in parts of this valley, adjacent to the Blue Ridge.

The passage of the Schuylkill and canal through the Blue Ridge is interesting. The mountains bordering the ravine are lofty and precipitous, presenting ledges of old red sand stone, with coarse and fine silicious gray wacke. The turnpike winds on the mountain's side at a considerable elevation above the stream. The navigation through the pass is effected by stone dams of magnitude, and permanent construction ; one of them is of thirty feet altitude. Groups of locks, water falls, and broad sheets of water, were frequent. The expense of lockage would here have been considerably lessened by adopting the inclined plane. From the Blue Ridge the Schuylkill, for forty or fifty miles, winds through a valley in which there is considerable limestone, the fissures and cavities of which in some places rendered the formation of a retentive canal difficult. I noticed four miles from Reading, and not far from the river, extensive beds of rock, closely resembling in composition and colours, the calcareous breccia of which the columns in the capitol at Washington are formed.

The navigation of the Schuylkill has been much interrupted by droughts the past season, and frequent repairs required. Boats have often grounded in the shallow water of dams. It will probably be found advisable to concentrate the water in a canal for most of the distance. Fevers, so generally prevalent within a few years in the valley of the Schuylkill, have been attributed by some to water stagnating in the dams.

The village of Pottsville, of recent origin, contains several good stone dwellings and stores. A weekly journal, edited with ability, is here published. A reading room containing many books and periodical publications, does much credit to the village.

Beds and veins of coal occur in numerous places between Pottsville and the Susquehanna, and are found on the side of Broad mountain, a central elevation. Mines are opened and worked at the head waters of the Swatara and Stony creek, at Peters' mountain, and a few miles east of Danville.

South-west of Pottsville the coal becomes more easily ignited, and that at Peters' mountain is reported to contain bitumen. It is probable that the coal of that vicinity embraces, like the Wilkesbarre, much more inflammable gas than the Lehigh, which may have led to the supposition that it was bituminous.

The coal from this part of the State can be forwarded to Philadelphia by the aid of rail-ways, and lateral canals communicating with the Union Canal, which runs through a fertile limestone valley, in a parallel course, and at no great distance from the coal hills.

Anthracite is found on several of the streams that discharge into the Susquehanna, on its eastern side. A large bed, not yet opened, exists a few miles eastwardly from Berwick, and numerous veins occur from an elevated part of the Wilkesbarre mountain, to the Kingston and Shawnese mountains, that form the western border of the basin of Wyoming. No anthracite has been discovered to the west of these mountains, or north of the Lackawanna range, with which they are connected.

Veins of coal in the vale of Wyoming, are not only very numerous, occurring on almost every farm, but many are of uncommon thickness, in some instances from 18 to 35 feet; and the vegetable impressions are far more abundant and diversified than were elsewhere observed. At Bowman's, one of the most considerable mines now worked, they are particularly numerous, pervading the superincumbent strata. The vein is about eighteen feet thick, and wrought in galleries. The coal, which is very compact, is detached by blasts. The inclination of the vein is about one foot in four.

There is less uniformity in the angle of inclination and direction of the coal strata in this neighborhood, than was observed near the Schuylkill. They approach nearer to the horizontal.

From the abundance of sulphuret of iron in the slate contiguous to, and dividing coal veins, the springs proceeding from the coal beds and the Susquehanna are strongly impregnated with salts. These mineral waters often occur, in both mountain and valley, and indicate beds of coal.

A canal route has recently been surveyed by State commissioners, and located through the valley of Wyoming, on rising ground west of the Susquehanna. When the work is accomplished it will lessen the charge of forwarding coal to market from this vicinity, and the valley of the Lackawanna. From Wilkesbarre to the Chesapeake, the descent is about 500 feet. By the hazardous and precarious medium of the Susquehanna, coal cannot now be conveyed in arks for less than \$3 $\frac{50}{100}$ the ton, which, with other charges, makes the expense to the mouth of the river, five dollars the ton. The Philadelphia market may be resorted to through the medium of the Union Canal. A canal to connect the Susquehanna with the Lehigh is practicable. The distance to Philadelphia, by this route, from Wilkesbarre, is 162 miles; and the lockage required, 1279 feet.

The coal of the Susquehanna is readily kindled in grates of ordinary construction; and by the experiments recorded in No. 2, Vol. X. of the Journal of Science, it has been ascertained, by the Editor, to contain double the quantity of hydrogen gas embraced in the anthracites of the Lehigh and Schuylkill, and in bituminous coal, an important characteristic not before suspected. The valley of Wyoming, and its valuable beds and veins of coal, have been correctly described in No. 1, Vol. IV. of the Journal of Science, by Mr. Z. Cist, an able naturalist, whose recent death is lamented by all acquainted with his merit.

I visited several large coal beds and veins in the valley of the Lackawanna; they run in a north-east course; some were wide, and the coal is of a good quality. Coal veins are of frequent occurrence from the confluence of the Lackawanna with the Susquehanna to near the head waters of the former river; they are variously inclined, from nearly horizontal to an angle of forty-five degrees. Vegetable impressions are rarely, if ever, contained in the coal slate of these beds and veins.

The most considerable body of coal in this region is situated between twenty and thirty miles from the Susquehanna, at the ragged islands, in a narrow valley, adjacent to the Lackawanna, and in the bed of that stream, which washes the southern base of the Lackawanna mountain, a lofty, rocky chain, that bounds the partially cleared valley of the Lackawanna to the north-west. This mountain is well clothed with trees of diversified verdure. Considerable good pine, and much heavy timber, principally hemlock, maple, beech, and birch, is found near its base, and adjacent to the upper part of the river.

This coal bed, supposed to be very extensive, is the property of the Hudson and Delaware canal and coal company, and has been penetrated thirty feet without finding the termination of the coal. From this bed, which rests in nearly a horizontal position, a considerable quantity of excellent coal has been raised, but from its low situation the excavation was soon filled with water. It has been occasionally cleared by pumps propelled by water. It is supposed that there is sufficient descent of ground, to free it by draining. The quarrying may perhaps be interrupted by freshets. I observed on the south bank of the river a wide vein of coal, which rises above the stream with sufficient inclination to run galleries clear of the water. A large tract on the side of the mountain that ascends gradually to the east from the coal bed, has been cleared at the expense of the company, and a village will shortly arise. Coal beds extend several miles higher up the stream. In value, and good quality, the Lackawanna anthracite compares advantageously with that of the Lehigh. The coal of this region will be conveyed to New-York, a distance of 217 miles, through the medium of a canal now constructing under the able superintendence of Judge Wright, by the proprietors of the coal bed. This canal commences at the Hudson, near Kingston, and passes to the Delaware, 67 miles, through a valley located between the Shawangunk mountain, and the gray wacke ranges, spurs from the Catskills. Except near the Delaware, and on approaching the Hudson, where considerable excavations in limestone and other rocks became necessary, there was little difficulty in constructing the work, as for much of the distance it passes through sandy and gravelly loam. For twenty miles it runs on the side of a mountain north of the valley, and at a a considerable elevation. At the summit level the canal extends eighteen miles without a lock. It will probably be completed from the Hudson to the Delaware the present season.

Beds of dark argillaceous schist, of small extent, are in a few places cut through. Limestone, of a good quality for calcining, occurs at the base and in places on the side of the Shawangunk ridge, adjacent to the canal in approaching the Hudson and Delaware, and will be useful to the part of Pennsylvania situated between the Delaware and Susquehanna, in which there is no limestone. The construction of the canal up the Delaware, on its eastern bank, to the Lackawaxen, a distance of twenty miles, will be arduous and expensive. For several miles it is located on the steep rocky side of a mountain. The passage up the valley of the Lackawaxen will be comparatively easy. Parts of this valley are settled, and contain considerable alluvial land. About one thousand feet of lockage are required from the Hudson to the western termination of the canal. The coal bed is to be connected with the canal by a railway of a few miles, passing over a considerable eminence. Lumber and coal will for many years be the principal articles transported down the canal. An extension of canal navigation up the Delaware into the State of New-York, which is practicable, would enhance the value of the stock. The rates of toll demanded for coal, will, if maintained, exclude individuals from participating in the coal trade through the medium of this canal. The canal will communicate with a large tract of good grazing land in Wayne county, a part of the district called the beech woods, that extends in Pennsylvania and New-York about one hundred miles from north to south, and from ten to fifty miles in breadth. It is heavily timbered, principally with beech, maple, hemlock, and birch, with occasional groves of good pine. The soil, often based on hard pan, is tenacious of manure and moisture, and good for grazing and tillage. Its surface is undulating, but rarely mountainous; and a considerable proportion is sufficiently free from stone for the purposes of agriculture. Viewed from the eastern brow of an elevated range, situated between the Lackawaxen and Lackawanna, this tract of country had the aspect of an immense plain; its dense forest was dressed in the gay hues of autumn, blended with the perennial verdure of pine and hemlock. The blue peaks of the Catskill mountains to the north-east, towering far above the general elevation, presented an irregular profile on the verge of the horizon. To the east, beyond the fire-seared, barren, rocky ranges of Pike county, the Shawangunk and Highland ranges were distinguished. Emigrants from New-England are busily occupied in cutting out farms in the beech woods. The first clearing is a work of toil; but, as there are no sprouts from roots, less labour is eventually required than to subdue some descriptions of oak land. From the little durability of hemlock, beech and maple, in fencing, it may in time be necessary to substitute the loose gray wacke slate of the surface. Old red sand stone occurs in the gray wacke region, and often supports a good soil. Flocks of sheep may here be advantageously introduced. Through the medium of the canal the farmers of Wayne and other counties can be amply furnished with lime and gypsum, so useful in agriculture.

The western part of Pennsylvania is abundantly supplied with bituminous coal, as the eastern is with anthracite. It is found on the rivers Conemaugh, Alleghany and Monongahela, and in numerous places to the west of the Alleghany ridge, which is in general its eastern boundary. It occurs on this mountain at a considerable elevation, and elsewhere, in nearly a horizontal position, alternating with gray sand-stone, that is often micaceous and bordered by argillaceous schist. The veins are generally narrow, rarely over six feet in width. This mineral is abundant, and of very good quality near Pittsburg, where it is valuable for their extensive manufactures. Beds of bituminous coal are reported as occurring in Bedford county, in the north-west part of Luzerne, and in Bradford county. In this last county, nine miles from the

Susquehanna, there is an extensive bed of coal, regarded as bituminous. It has been penetrated thirty feet without fathoming the depth of the strata.

Bituminous coal is abundant in Tioga county, state of New-York, adjacent to the route of a feeder required for a canal contemplated, to connect the Susquehanna with the Seneca lake. The summit level is forty-four feet above the river, and upwards of four hundred above the lake. It occurs on the Tioga, and on the Chemung, a branch of that river. When the canal communication is effected, the interchange of anthracite and bituminous coal for salt and gypsum, will be highly valuable for Pennsylvania and New-York.

Bituminous coal exists on the Loyal Sock and other streams that descend the western side of the extensive peninsula, situated between the north and west branches of the Susquehanna.

The centre and northern part of this section of the State, is elevated and mostly in a state of nature. It is crossed by barren ranges, interspersed with valleys and well timbered table land, which will in time be occupied for grazing and tillage. The rocks of the eastern part that fell under my observation, are transition, mostly gray wacke slate. In the western part, adjacent to the west branch of the Susquehanna, limestone is the predominant rock.

This stream, for near fifty miles, winds with a moderate current through a rich valley, with wide alluvial borders often occurring. The valley is bounded to the south by the Bald Eagle mountain, an extensive elevated rocky range.

Springs, holding in solution muriate of soda, are common in various parts of the bituminous coal region; they are generally weak near the surface, but deep springs disclosed by boring, are often strong. One, containing as much salt as the ordinary waters of Salina, has recently been discovered, by boring, about twenty miles from Montrose, bordering on the state of New-York. They occur in some of the southern counties of that State, adjacent to Pennsylvania, and on the Loyal Sock and other streams, auxiliary to the west branch of the Susquehanna.

But the most productive saline springs of Pennsylvania, are situated on the banks of the rivers Conemaugh and Kiskaminitas, about thirty miles east of Pittsburgh. These rivers for many miles wind rapidly through rocky romantic ravines, bordered by hills of from three to four hundred feet elevation, that rise with steep acclivities, presenting mural and projecting precipices of gray sand-stone, in places jutting over the road and torrent. The sand-stone is ordinarily fine, but is sometimes a coarse aggregate, principally quartz. Its thin laminæ are generally in a horizontal position. The lower strata, often in a decomposing state, contain vegetable impressions. This rock usually rests on dark and very fissile argillaceous schist, that contains much sulphuret of iron, and forms the roof and floor of numerous beds of bituminous coal, adjacent to these streams. These beds are from a few inches to five feet in thickness, and occur at various altitudes, from 200 feet above the river, to a great depth below. The salt works on the Conemaugh and Kiskaminitas, situated 4 miles apart, are supplied with water by boring. The richest water is procured by penetrating from four to five hundred feet. Copper tubes, 1½ inches in diameter, are inserted in the perforation, in which the salt water rises to a level with the river, accompanied by sulphuretted hydrogen gas, often in considerable quantity. This gas diminished after many outlets had been made, and the water did not rise so high. In boring, fresh water is seldom found below one hundred feet from the surface. Veins of coal and slate were penetrated at various depths, and narrow beds of limestone, lying deep, were passed through. Some of the lower strata were represented as very hard, and others soft; this last is supposed to be gypsum. Salt springs are generally struck by boring, in the ravine at Kiskaminitas; but in two instances the ground was penetrated 450 and 650 feet, without meeting salt water.

In the process of manufacturing, salt water is pumped, by horse power, into large troughs, where the earthy particles, not held in solution, mostly subside. It is soon passed into the boiling pan, which is of cast iron, and shallow. After boiling a considerable time, it is drawn off into vats, where the oxide of iron, which is abundant, and earthy salts, subside, together with a portion of muriate of soda. The clear brine is passed off to a boiler, in which the salt, in fine crystals, is precipitated, and then removed to drain. No use is made of the sulphate of soda, of which there is considerable in the water. It would, perhaps, be an improvement of the process, if the precipitation of the iron and earthy salts was effected with less boiling, and the salt crystallized in shallow pans, by a heat short of ebullition, ; the crystals would be larger, and the salt better and less of it lost. Fine salt, made here, does not answer for the packing of provisions for exportation.

The salt manufactured at Kiskaminitas and Conemaugh, has some years amounted to 300,000 bushels; it is sold from 20 to 25 cents at the works. The expense of manufacturing does not exceed ten cents the bushel. A large portion of the numerous salt works are established near the river, in the ravines of the Kiskaminitas, and coal for fuel is procured from veins situated above the works, in the side of the hill, and costs but a cent a bushel.

Less salt is now made on the Conemaugh than in former years, as the springs are weak and the price of the article too low to render it profitable. Seven years since, there was not a building in the ravine of the Kiskaminitas: it now contains a considerable population, and presents, at the base of a precipitous eminence, many dwellings and salt works, from which black bituminous smoke rises in clouds over the hills, or draws through the dusky valley. A clear stream, of considerable breadth, is seen rapidly winding among the mountains.

The western canal of Pennsylvania is located in the valley of the Conemaugh, and will add much to the productiveness of these works, and afford great facilities for the conveyance of salt to the Atlantic and Western market. At present, it is transported on waggons to the east, and in boats, by a precarious navigation, down the Conemaugh and Alleghany rivers, to Pittsburgh.

Considerable salt is made near Pittsbugh, from a fountain procured by boring 270 feet. The water is strong, and is raised by a small steam engine. There is little sulphate or carbonate of lime in the water. The salt is white, and of a good quality. This fountain is sufficient for the annual manufacture of 25,000 bushels of salt. Salt is manufactured in Pennsylvania at weaker saline waters in the vicinity of the Ohio.

There are salt springs on the Chenango, in Mercer county. Near the Mahony, in Beaver county, a fountain of salt water was procured by boring to the depth of 200 feet. It is probable that strong saline water, in much of the western secondary country, may be obtained by boring, as it often occurs contiguous to bituminous coal, and is indicated by salt licks, and by slate containing sulphur.

A canal route has been surveyed through the most fertile part of the counties of Pennsylvania bordering on Ohio, to connect the waters of the river Ohio with Lake Erie, which will give additional value to the products of agriculture, and of the salt springs, of that part of the state. In the summer of 1825, the price of wheat at Pittsburgh was but 25 cents the bushel; at the same time, adjacent to Lake Erie, from whence there was an uninterrupted navigation to the Atlantic market, it commanded 75 cents the bushel.

The soil, in a considerable proportion of the counties of Pennsylvania, bordering on the State of Ohio, is fertile. The northern division of the counties contiguous to Lake Erie and the State of New-York, has a good soil for grazing, and is in general heavily timbered with beech, hard maple, and birch. But adjacent to, and between the head waters of the rivers Alleghany and Susquehanna, embracing a portion of eight counties, there is an elevated, mountainous, rocky and extensive district of country, clothed mostly with hemlock, pitch-pine and maple, with frequent entangled thickets of laurel, almost exclusively tenanted by numerous panthers, wolves, and other wild animals found in the unsettled parts of the State, with the addition of elk and beaver.

The soil and aspect of this region is so forbidding, that it will long remain unoccupied, and much of it be ever useless for agriculture. Its mineral resources are little known, but it is reported to contain much coal, bog, and other ores of iron. In the county of Clearfield, a considerable part of which is in this mountain district, a large amount of iron is manufactured by the aid of bituminous coal and charcoal. Iron ore occurs in various parts of Pennsylvania, but is most abundant, and of the best quality, in the extensive calcareous valleys situated between ridges of the Apalachian mountains, particularly in the counties of Center and Huntington. It is mostly raised from beds of argillaceous earth, resting on limestone. The best ores of iron in this country exist in or adjacent to calcareous districts. The iron manufactured in Center and Huntington is called the Junietta, and is distinguished for tenacity, malleability, and other valuable qualities. The furnaces and forges, situated on never failing streams, are numerous. Bituminous coal, from the Alleghany mountain, is often used for making pig iron, &c. for which anthracite will probably be substituted, when the canal through the valley of Junietta is completed. About 50 per cent. of iron in pigs is extracted from the Junietta ore, and it loses one-third in passing from the bloom to bar iron.

At Belfonte, a pleasant village in Center county, in the process of making bar iron, powerful rollers are substituted for the trip-hammer. The half bloom, heated by bituminous coal, is quickly passed between successive rollers, until highly compressed. A smooth bar, of the usual weight and shape, is thus produced in a minute's time. I was informed by an experienced and disinterested manufacturer, that bar iron, formed by this process, is softer than the produce of the trip-hammer, and not as desirable for plough-shares, and work subject to much friction, but for all other purposes equally good. Soft bar iron cannot be made from ores located west of the Alleghany mountain.

There is more land capable of cultivation in Center and Huntington counties, than is common in the mountain districts of Pennsylvania. The calcareous valleys are wide and extensive, and the ranges narrow, and of little elevation. In Huntington county, a quarter of the surface is first rate land, and more than two-thirds is under partial improvement. In Center county there is a large body of table land, called the barrens, from which the timber has been cut for the use of furnaces. It is uncultivated, and held in little estimation from its total destitution of springs, and the impossibility of procuring water by sinking wells. The soil is of an excellent texture for wheat or grazing, and stone rarely occurs on the surface; but the earth rests on calcareous rocks, replete with fissures, into which the rain-water sinks to a great depth. This uninhabited tract has in some places a width of five miles, and extends thirty: it would afford good ranges for sheep, if cleared of underwood.

Springs are numerous and large in these calcareous valleys. A clear, cold, and never failing mill-stream, issues from limestone caves, near Belfonte, from which the name of the village is derived.

ART. IX.—*Remarks on the Anthracites of Europe and America;* by WILLIAM MEADE, M. D. &c.

TO THE EDITOR.

SIR,—I have perused, with much pleasure, your remarks, in the last number of the Journal, on the properties and economical uses of the Rhode-Island coal; they are such as must convince the most prejudiced readers, that the anthracites of this country have more valuable properties than many are willing to allow them. I confess, it was with some degree of surprise that I heard of the quantity of inflammable gas which you obtained from this coal; and though your explanation of the fact is perfectly satisfactory, as accounting for its production by the decomposition of the water with which it is mechanically or chemically combined; yet may it not partly arise from a small quantity of bitumen, with which the coal is impregnated, and which would also account for the lambent flame which is perceptible when it is burning. From some late experiments made by the Hon. Mr. Knox, he has ascertained that bitumen exists in many fossils where it never was before suspected; and he has proved this by extracting it, by distillation, from the Newry pitch stone, and even from minerals which are component parts of primitive rocks.

Your remarks on the proper method of burning this coal, are so judicious, that I have little to add on this subject; but as you allude to an essay of mine, published in Boston many years ago, on the Rhode-Island coal, and express a wish for any additional information I possess on the same subject, I take the liberty of making a few more observations, the result of further inquiry and experience. These are perfectly at your service, if you think them of sufficient interest for insertion in your valuable and useful Journal.

All mineralogical writers, that I have ever consulted, give but a very vague and imperfect description of the real anthracite coal. They frequently confound it with a very inferior species of coal, found in some parts of England and Wales, to which it has no resemblance. It appears to me that the geological character of the anthracite is alone sufficient to distinguish it from any other, and this is particularly illustrated in America, where, though it is found so extensively, it never has been traced as associated with any other class of rocks but those of the transition, bordering on the primitive ; nor has a particle of bituminous coal ever been found in the same formation, or connected in any manner with the anthracite.*

It may be considered as a pretty general fact, that no other coal has been discovered, or is likely to be discovered, in the whole tract of country from the Susquehanna to the Penobscot, except anthracite ; and also, that in those places where it has been found, its geological associations are decidedly transition ; at least, I know of but one solitary exception, and this is in the vicinity of Hadley, on the Connecticut river, where small veins of bituminous coal have been discovered. But this only confirms my remark ; as the rocks in which it has been traced are purely secondary, or what Jamieson characterizes as of the flœtz trap formation. To illustrate these positions, let us commence with the Pennsylvania anthracite, on the Susquehanna, the Schuylkill, and the Lehigh : here its geological situation and localities are decidedly transition. The whole of the extensive coal formation, commencing at Rhode-Island and extending to Providence, Worcester, and Lancaster, in Massachusetts, and even to Keene in New-Hampshire, is also unequivocally transition, nearly connected with the great primitive ranges. Many other localities may be pointed out in tracing it to the eastward, but I know of none where the indications of this coal are so strong as at Thomastown, in Maine, where the anthracite is seen cropping out on the surface, with all its associating minerals, such as gray wacke, gray wacke slate, and transition limestone ; yet, with such indications of coal, and under such favourable circumstances, as vicinity to water carriage, I do not find that any attempt to explore it has been as yet made.

As there is no situation where it would be more desirable to find coal than the banks of the North River, many attempts have been made to discover it, but without success ; nor could any other coal but anthracite be expected by those who have any geological knowledge. In illustration of this opinion, I may mention that small veins of coal have been

* Since the observations of Dr. Meade were made, bituminous coal has been found in a number of places, in the valley of the Connecticut, in the *secondary coal formation*, which exists in this valley, in connexion with an older formation—probably of the transition class, to which the localities of anthracite, mentioned by Dr. Meade, belong. See Prof. Hitchcock's Geology of the Connecticut, in this Journal, Vol. VI. EDITOR.

discovered near Fishkill, on the banks of the Hudson, associated with gray wacke slate, which I found, upon examination, as I suspected, to be a perfect anthracite, similar to that of Rhode-Island; but the indications at present are not sufficiently encouraging to prosecute the discovery.

Though I have visited many of the English and Irish coal mines, I have never seen any coal, in either country, except that from Kilkenny, which could be called a pure anthracite, or had any resemblance, either in external character or chemical properties, to the Rhode-Island and Pennsylvania coal; and I am certain, if the English possessed any of so pure a quality, they could not have been so long ignorant of its use in the arts, in many of which it has great advantages over bituminous coal. In describing the different species of coal, they constantly refer to culm, as particularly applicable to the burning of lime and bricks, as well as the manufacture of salt; but English culm has no resemblance whatever to the anthracites of this country; it is a small coal, never found massive, and difficult to ignite, and burns with very little flame; in short, it is a very impure species of coal, and scarcely ever applied to any other useful purpose except what has been mentioned. The Kilkenny coal burns like the American coal, with little flame, and without smell or smoke. The culm gives out a very disagreeable smell, and much smoke; in consequence of which, it is never used for the drying of malt. The brewers and malsters in England are well acquainted with this, and therefore import the Kilkenny coal from Ireland for their purposes, which I am confident they would not do if they possessed coal in England of the same quality.*

When the anthracite of Rhode-Island is in a state of strong ignition, a very remarkable circumstance may be observed, which is strongly characteristic of it, and which distinguishes

* Having, since the above was written, seen a late number of Brande's Journal, which contains extracts from his Lectures on Geology, I find the following account of the great coal districts in England, so fully corroborating my own statements, that I shall here give it in his own words. After describing the bituminous coal of England, with the accompanying strata, without alluding to anthracite, or any such species of coal, which he would naturally have done if he was acquainted with it, he adds, "A third kind of coal, called culm, or stone coal, contains scarcely any bitumen, and abounds with earthy matter; it is very difficult of inflammation. Besides this, there are some others, such as cannel coal and splent coal."—See Brande's Journal, No. 39, page 36.

Surely this description, as given by Mr. Brande, has little resemblance to that of the anthracite of America, or to its properties.

it from bituminous coal. The anthracite burns away slowly; no part of it is either consumed or altered, except the surface, which is exposed to the flame; and if a large piece, which has been in the fire for some hours, is taken out, and either plunged into cold water, or let extinguish itself, it will be found to have lost considerably in size and weight, and its surface will be charred; but if this piece is broken with a hammer, the interior appearance of the coal is not in the smallest degree altered from its original state, but preserves the same steel gray, or metallic lustre, thus consuming away gradually in the flame, like the diamond when ignited in oxygen gas.

Though it is many years since I first paid attention to the subject of the Rhode-Island coal, and was the first person who called the attention of the public to the great advantages they would derive from an extensive use of it in the arts, yet I have a perfect recollection of the difficulties we laboured under in order to persuade many, whose interest it was to give it a fair trial. The first impression which it was difficult to get over was, that it never could be ignited; and when they were at last convinced that this was erroneous, the next objection they started, was, that the heat was so intense that is was impossible either to regulate or check it. The blacksmith complained that he no sooner put an iron rod into the fire, for the purpose of making nails, than the rod was completely melted; as soon, however, as a little experience taught them the management of the fire, they preferred this coal to any other.

As soon as I ascertained, by experiment, that the Rhode Island coal contained at least 90 per cent. of pure carbon, it naturally occurred to me, that it might be made use of with advantage in furnaces for the smelting of iron ore, in the same manner that coke is in England, it being perfectly obvious that this coal, in its natural state, had all the essential qualities of coke, without either the trouble, waste and expense, which is necessary for converting bituminous coal into coke, before any use can be made of it by the iron manufacturers. It is scarcely necessary to state here, that the universal application of pig iron in almost any shape to the purposes of manufactures, and even the necessaries of life, may be traced entirely to the present improved state of the fabrication of pig iron with coke, or the char of pit coal. In order to determine this question, I accompanied one of the proprietors of the coal mine to Kingston, in Massachusetts, where several furnaces were at work in smelting bog iron ore. After some persuasion, and not without a *douceur* to the workmen, in order to reconcile them to the experiment, we proceeded to charge the furnace with alternate layers of ore, charcoal, and Rhode Island coal, broken into small pieces; and in order to see that the experiment was fairly tried, I never left the furnace for 24 hours. The process went on as usual, the common blast being sufficient to keep up the requisite heat, and on tapping the furnace, it was found that a much greater quantity of metal flowed from it, than was usually produced by the use of charcoal alone, and that it was also of a better quality: it was also ascertained that the furnace required to be tapped much oftener than before. This satisfied every person concerned; but as the place was at a very inconvenient distance from the coal mine, it did not turn out so advantageous to them as it otherwise would.

I am not aware that any fair experiments have been yet made by the iron manufacturers, to introduce the use of this or the Pennsylvania coal into their blast furnaces. I have often heard it asserted that it did not or would not succeed, but no sufficient explanation of its failure has ever been given. Of all the uses to which coal of this kind can be applied, that of manufacturing iron from the ore is the most important. A person who is in any degree acquainted with the true principle of reducing ores to the metallic state, will require something more than bare assertion to convince him, that coal which has been proved to contain 94 per cent. of pure carbon, is not adapted to the smelting of iron ore. It has been asserted that it would not bear the blast of the bellows; but so far from this being the fact, I am willing to assert, from experience, that it bears the blast better than charcoal, and that this is one of its essential qualities; as, by this means, while the lighter kinds of charcoal are dissipated by the blast, without being allowed to come into contact with the ore, this coal, not being driven off by the wind of the bellows, remains longer in a state of ignition in contact with the ore; thus producing much more metal from a given quantity of it, than can be obtained from charcoal alone. It would be uncandid in me not to state that, in the experiment which I witnessed at the Kingston furnace, it was observed that small pieces of coal ran out at the opening from whence the metal was drawn, flowing on the surface of the slag or scoria; but this is easily explained, by referring to the statement which I have given of the chemical properties of the coal, which only consumes on the surface exposed to ignition in oxygen. The largest pieces, therefore, remaining in contact with the metal, were only partly consumed, and falling down with it to the bottom of the furnace, flowed out with the slag, without being entirely consumed in the process. This, however, only showed that the coal was not broke small enough, and admits of an easy remedy. The same circumstance occurs sometimes when charcoal is used, and those who examine the scoria after a blast, will frequently perceive in it particles of charcoal unconsumed, which have intermixed with it, and flowed out of the furnace in that state. Let me, therefore, urge on the iron manufacturers to abandon their prejudices, and to turn their attention to the expediency of the use of this fossil coal, so abundant in their country, and so favourably situated for their purpose. Without doing so, iron mines may be discovered without advantage, and canals constructed in vain.

While the coal fields at Rhode-Island were extensively worked, an immense quantity of small coal had accumulated in the neighborhood of this mine, which was unfit for the market, and was therefore considered as useless; it ought not, however, to have escaped notice, that this small coal was adapted to many useful purposes, being very similar to what is called culm in England and Wales, which is generally and extensively used in the burning of lime and brick. One of the most favourable situations that could be wished, for the extensive use of this small coal in the burning of lime, occurs within a short distance from the mines, and with the advantage of water carriage; I refer to the valuable quarries of the finest lime-stone which is made use of principally for the manufacture of lime. Here, and at Smithfield, there are so many kilns in operation, and for so many years the consumption of fuel has been so great, that there is scarcely a tree left in the neighborhood, and fuel has risen to such a price as will soon render this once lucrative business, an unproductive one, though with so many advantages of an inexhaustible supply of the purest limestone, and convenient water carriage. A proposal was therefore made to the proprietors of those works, to substitute the small coal instead of wood, for the use of the kilns, offering to supply them at a very low rate with any quantity of it: only one, however, could be prevailed on to make a trial of it, which he did in his own way, and when the operation, as he thought, was finished, he found that the greater part of the contents was vitrified, or converted into a slag, which adhered so firmly to the sides of the kiln, that it could be removed only by crow bars. At first he could not be persuaded that it would produce sufficient heat, and therefore was careless about the quantity. The consequence was, as may naturally have been expected, that the heat produced was so intense as to vitrify the greater part of the limestone, which adhered to the sides of the kiln. Such an obvious effect, however, which would never have occurred had the coal been applied in proper proportions, so far from satisfying the owner with the certainty of success, only prevented him from making a second attempt.

Much such another circumstance as the above, occurred in the neighbourhood of Boston, where there are extensive brick kilns, but where, also, there exists a great scarcity of wood for fuel. It was not without difficulty, however, that one of the proprietors consented to make trial of the Rhode-Island coal. The process of burning went on as usual; but when the operation was supposed to be completed, and they came to examine the result, it was found that the surface of the bricks was completely vitrified, and that they adhered to each other in one solid mass, to the great disappointment of the owner. It was impossible, however, not to see and be convinced, from this experiment, that the failure arose, not from any defect in the coal, but from the excessive and intense heat, produced either by too great a quantity being used, or by keeping up the fire longer than was necessary; and thus an experiment which completely proved the applicability of this coal to so valuable a purpose, tended only to convince the ignorant workmen that the failure arose from the unfitness of the coal for any such purpose. Let me now be permitted to ask such persons, how it happens that English brick is considered so superior to American brick, that large quantities are still imported, and command a higher price in the market? To this I will answer, Because they are better burnt than the American brick; and this arises from the use of *culm*, a very inferior kind of coal to that which is rejected in this country.

In the essay, which I published several years ago, on the application of Rhode-Island coal to the arts and manufactures of this country, I pointed it out as peculiarly calculated for producing that equable and steady heat which is required for the rolling and slitting of sheet iron. This has now become an article of prime necessity in this country, and I am prepared to say that no fuel whatever is so well adapted for the purpose. The sheet iron is heated to the necessary degree for the roller, in half the time that it can be done by any other fuel; and a manufacturer assured me that the surface of the iron, heated in this manner, was not only much cleaner, but that the iron lost much less of its weight by this process, than by any other.

It is, as you observe, extremely singular, that the public should have been so long prejudiced against the use of the Rhode-Island coal, as well as of every species of anthracite, in any way, or for any purpose whatever. I early perceived this prejudice, but foresaw that a period would arrive when its real merits would be appreciated. Indeed, I never had any doubt but its true value would be discovered by the artists; but I did not expect that the time would so soon arrive when it would be so generally introduced into domestic use; nor am I now prepared to say, that I prefer it to bituminous coal, when used in common grates; not that it will not burn in grates, as is fully experienced in Philadelphia, but because it differs so essentially from bituminous coal, in the liveliness of the flame, and the quickness with which it can be ignited. Where a uniform temperature is required to be kept up in all parts of a house, there is no mode whatever of accomplishing it equal to the use of the anthracite, in those stoves or furnaces which are now so ingeniously and judiciously constructed in Philadelphia. I think that it is quite a mistake to imagine that a great draft of air is required to keep up the ignition of the fire with this coal; and I am the more convinced of this, from having seen the manner in which the same kind of coal is burnt in the county of Kilkenny, where no other fuel is used. There the grates are precisely the same as those used in England; all that is required is *patience*, and that the fire should have sufficient time to kindle. No coal whatever answers the purpose of cooking better; but I always observed, in Kilkenny, that the kitchen fire was made the night before, in order to have it in a state of perfect ignition the next morning. Indeed, I was perfectly satisfied of this, from experience at Rhode-Island, where I have frequently seen the Irish workmen, in cold weather, sitting round large fires, placed on iron bars, in the open air;

and, in one instance, I observed a large fire, made in the open air, in an old iron pot which had a hole in the bottom—one of those contrivances the result of accident and necessity.

I cannot take leave of this subject, without expressing some surprise that so little pains has been taken to ascertain whether the anthracites of this country may not be applied to the use of the steam engine. Let us consider how little benefit the English would have derived from the discovery of the steam engine, if it were not for the inexhaustible supply of coals which are afforded from their mines. That the woods of their country are not inexhaustible, may be easily evinced by the great scarcity of timber in the neighborhood of populous cities, and the consequent difficulty of obtaining wood at at a reasonable price, for the use of the numerous steamboats now navigating their rivers. Those difficulties will increase so as to be a material obstacle to their use. It is a common opinion, that there can be no other mode of heating the boilers, but by the reverberatory furnace, now in use. I am well aware that the anthracite or non-bituminous coal of this country, is not well, if at all calculated for a reverberatory furnace ; but I am also convinced that a boiler may be so constructed as to be heated with equal facility, if not greater, by the use of this coal, than by the use of wood, which has so many inconveniencies attending it, particularly in steamboats ; and I am sure no person will presume to contest this, when informed that a large steam engine was kept in constant operation, at the mines in Rhode-Island, with the use of no other fuel than the coal on the spot. If this fact is not convincing, I know not to what other to appeal. It is not difficult to foresee that the period will soon arrive when the obstacles which impede the use of this coal in the steam engine will be removed. That a boiler may be so constructed as will answer the purpose of generating a sufficient quantity of steam, with a very limited proportion of fuel, has been already exemplified by the ingenuity of Perkins, who, as if with an eye to this subject, has invented a generator, peculiarly calculated for the use of this coal, and which, if successful, will totally explode the use of wood, and remove many of the objections to the use of steam engines for the purposes of navigation. Greater prejudices existed here, at one time, against the use of this coal in any form, and for the most obvious purposes ; these are now happily dissipated by the light of reason and experience.

W. M.

ART. X.—*Proofs, drawn from Geology, of the abstraction of Nitrogen from the atmosphere, by organization;* by Prof. LARDNER VANUXEM, of South-Carolina College.

WE know of but two sources of nitrogen; the atmosphere, and organized bodies.

It is admitted as true, that there exists a class of rocks whose creation, deposition, or fixation, was anterior to the existence of organized bodies. In this class of rocks, chemists have not been able to detect nitrogen in any of its constituents; or show its presence in any manner whatever. This negative fact has a positive value, and allows us to infer, that as no nitrogen has been found in this class of rocks, consequently we must conclude, that no nitrogen exists in it. With the subsequent classes of rocks it is otherwise, all of which, with the exception of the volcanic class, contain organized bodies. Now as the greater part of these organized bodies are those of marine animals, containing nitrogen as an essential constituent of their composition; and as we know of no source whence these bodies could have obtained their nitrogen, but from the atmosphere; we have a right to conclude that it was obtainedfrom that source.

As it plainly appears, that those organized bodies which contain nitrogen, drew this principle from the atmosphere; (that being the only source anterior to their existence,) it is evident that the atmosphere no longer contains the same quantity of nitrogen that it originally possessed, by the quantity held by the organized bodies now entombed in the bowels of the earth, and by those now living, whether on the surface of the earth, in the air, or in the waters of the earth.

It must be very evident to those conversant with geological facts, and with facts of natural history, that the quantity of organized bodies is very considerable, and calculated in the aforementioned manner, to affect materially the constitution of the atmosphere.

The consequences which follow from this abstraction of nitrogen from the atmosphere, are of great importance to geology, throwing a flood of light upon certain parts of it, and enabling us to account satisfactorily for many facts, which in the present condition of nature, are involved in the greatest obscurity.

The application of chemistry to geological phenomena, also shows an abstraction of oxygen from the atmosphere; but as an exception may be made to the application of the abstraction of oxygen, which is not the case with nitrogen, in the present state of our knowledge, no argument will be drawn from it. The facts for and against the abstraction of oxygen, will be spoken of in the subsequent part of this paper.

The abstraction of nitrogen from the atmosphere being shewn by the negative fact of its mere existence in the class of rocks which pre-existed to organization; it being a constituent of animal bodies, and existing also in certain plants;* it follows, that the density or mass of the atmosphere, must have been diminished by the quantity of nitrogen existing in the organized products entombed in rocks, and which constitutes living matter.

The density of the atmosphere is as the pressure; and the pressure is as the quantity of matter; (all things else being the same) all our observations show, that the temperature of the atmosphere is, as the density, or quantity of matter contained in it. The greater the temperature, the greater the quantity of water held in solution. The greater the quantity of water in the atmosphere, the greater the heat, and the greater the moisture at certain times; and the greater the rains, torrents, winds, storms, inundations, and other abrading powers of the earth. The powers or increase of life, are also in the ratio of heat and moisture.

What are the geological facts which require at one period of the earth—a greater heat—a greater degree of moisture—greater alternations of land and water—and greater powers of adhesion, than exist now, or than existed anterior to some of the last geological revolutions of our globe. The facts are exhibited by coal, salt, mechanical products and marine shells, and in this order, I shall cursorily consider them.

In the primitive class of rocks, we have no coal, no salt, nor even gypsum. The appearance of the substances commenced with the mechanical rocks, or where known geological causes began their operations. From the period of the operations of known causes, theory ought to be admitted; for fact and theory can mutually subserve each other; whereas, either alone, from the imperfect manner in which facts are collect-

* Such as yield prussic acid, ferment of the French chemists, gluten, &c.

ed, is liable to lead to great errors.* It is to be lamented, as the progress of knowledge is retarded, that the notion of a particular period, for particular products and rocks, should exist so strongly in the minds of most geologists, when it is unsupported, to the extent given to it, by the facts themselves. In proof of the truth of this assertion, I refer to their writings, and particularly to the articles Bituminous Coal, and Salt, where little or no attempt has been made to apply the known causes of their production to the facts observed ; but all their efforts seem to be exerted by a determination to reduce all the coal to a precise geological period ; and all the salt to another.† These remarks are made with the view of throwing doubt on the notion of a precise period for each of the depositions to be spoken of, and substituting a limited series of periods, during any part of which, any one, or all of them, according to circumstances, might be produced ; for they are less dependent upon time, than upon locality. But most of them are incompatible depositions for the same locality, and time, unless under peculiar circumstances. A change of circumstances might reverse the order of the depositions, as regards their locality. In other words, two or more depositions of limestone, as regards time, may each have their parallels of coal, salt, and the mechanical products ; the same locality, exhibiting as many different products, as there were successive depositions.

OF COAL.

All the coal which is used in commerce, is taken from the last rocks of the transition, and the first rocks of the secondary class : for few or no masses of coal, either anterior or

* The production of mechanical rocks requires that a part of the earth's surface should be exposed to the action of abrading powers. This effect would give rise to currents, charged with the products of these powers. From these currents, gravel, sand, and mud, would be deposited according to their known laws. Whilst this action was going on, the sea would be depositing its productions ; and the land itself might be producing the elements of coal. Thus, on the same plain, or at the same time, we might have coal, shells, salt, gravel, sand, and mud, or clay, or marl, (according to its composition ;) all which products, by most geologists, are considered to be depositions, differing as to time.

† One of the causes which seems to me to have mainly contributed to mislead geologists, is the kind, and colour, of the rocks, which accompany these substances. Being uniform for each substance, they have been conjectured to be universal, and to have been deposited under the same circumstances, and at the same period of time. In a memoir, which I am preparing, on the colour of rocks, I hope to place this subject in its proper point of view.

posterior to these periods, are found in any country, in sufficient abundance to merit exploration.

That coal is a product of vegetation, or organized life, is conceded by almost every geologist. I know of no good argument to show the contrary ; and the arguments in favor of its vegetable origin are many. From the abundance of coal, and its being limited to a geological period, we must infer a greater degree of heat, and moisture, than existed subsequently.

All the beds of coal are accompanied by impressions of plants. Be the place where found, where it may, these plants are not very dissimilar from each other ; and they are analogous to tropical plants.* This fact shows that the degree of heat and moisture was greater, or the plants would not resemble those of the tropics.

There are three kinds of coal in nature ; two of which only are used in commerce, with a few local exceptions. These coals, are lignite or fossil wood, which is the most recent kind. The next in age is the bituminous. The oldest, and last, is the anthracite. The composition of these coals is as follows : Lignite consists of proximate principles ; carbon, bitumen, acetic acid, and water. Its ultimate principles are, carbon and water. Bituminous coal consists of bitumen, carbon, and water. Its ultimate principles, are carbon and water. Anthracite has for proximate and ultimate principles, carbon and water. Lignite and bituminous coal are resolvable into anthracite by the application of heat under pressure. In the lignite, the acid is first decomposed, and finally the bitumen ; leaving carbon and water as the products.

The explanation of these various kinds of coal, as they exist in the earth, requires that a greater degree of heat should have existed, when the oldest of these coals was formed, than was requisite for the most modern, or the one intermediate in age and in composition.

Coal seems to have been deposited in two different modes ; first, in estuaries, or hollows, which appear to have received the vegetable or coaly matter of the surrounding country.—

*Those not well acquainted with modern geological observations, may be disposed to regard the plants as vegetable matter, which constitutes and occurs in coal depositions, as the production of tropical regions, transported to the places where found. To such I refer the memoir of Mr. A. Brongniart, "Sur des vegetaux fossilles traversant les couches du terrain houiller," annales des mines, troisieme livraison, annee, 1821. In this memoir, a mass of facts is presented, which precludes every opinion of the kind.

These are those masses, which are sometimes fifty feet or more in thickness, but are of limited extent, and whose upper and lower surfaces are not parallel to each other, but are more or less plano-convex. The other, and more common mode, is in beds, or layers, where surfaces are parallel, and usually of great extent, but of no great thickness. This kind rarely or never presents single beds; they are, on the contrary, often very numerous. In some places more than fifty of them have been counted. Generally they alternate with slate, clay and sandstone and sometimes with shell limestone. Coal beds of this kind, furnish the facts which afford manifest proofs that the plants which produced the coal grew and died where the coal is found. How can these alternations of masses of vegetable matter, indurated mud, sand and limestone, be accounted for, if we admit not the presence of land-floods of fresh water, and salt water, as often as the products of these phenomena are exhibited?

OF SALT.

Neither salt, nor the elements of salt, with the exception of soda, are to be found in the primitive class of rocks. We are consequently obliged, in the present state of our knowledge, to suppose that it remained in solution until the geological period at which we find it. From the writings of European geologists, it is impossible to refer salt to any particular period. Thus the salines of Bex, of Colancolan, in the Andes of Peru, of Cordona, and of Moutier, would place it in the transition class; whilst those of the Jura, of Cheshire in England, of Poland, and of Saltzbourg, are considered to be coeval with the middle secondary. This discordance of position is what we ought to suppose from theory, for, like coal, its deposition depended upon local circumstances, and not upon universal tendency.* One obvious cause of the deposition of salt, is the alternation of land and sea-water, combined with great atmospherical evaporating powers, which is the common opinion of its mode of formation, and which the marly products, frequently accompanying it, plainly show. The salt springs of the western country, manifestly point to an origin different from the one just mentioned. All our information of them shows that they occur in limestone, and not in salt marl. Is it unreasonable to suppose, that seas have

* As in granite, gneis, &c.

existed, so circumstanced, that more water, at certain times, was lost by evaporation, than was received by their ordinary sources? Such seas, on becoming saturated, would deposit their salt, along with its peculiar stony or shell productions. Or the mass of insoluble matter, at their bottom, might become charged with a strong saline solution. Either of these suppositions will explain the phenomenon of the salt of the western country.

The position of salt, on taking the facts of the European geologists, accords, almost precisely, with the limits which we have fixed for the anthracite and bituminous coal, namely, commencing with the last of the transition (the gray wacke formation) and the first formations of the secondary class; for, neither anterior nor posterior to these periods, is salt known to exist, in notable quantity. Whence this accordance, if it depended not upon the active powers inferred for this particular period? The conditions required for the ordinary mode in which we find salt, are, first, strong winds, to cause a great mass of salt water to be thrown upon the land; secondly, strong hot winds, to favour the evaporation of the water; and, finally, abrading powers, to furnish the earthy materials requisite to shield the salt deposited from the action of its solvent.

OF MECHANICAL PRODUCTS.

The geology of the United States presents, in the clearest point of view possible, a greater accumulation of rolled stones, (conglomerate) of sand, (sandstone) of mud, (slateclay, &c.) and other products of the destructive agents of nature, towards the close of the transition, and the commencement of the secondary class than at any other period prior or subsequent. Those who are familiar with the geology of our range of mountains, (the Apalachian chain,) and with the range of the old red sandstone, cannot but have had the same conclusion forced upon them. The period of the deposition of the rocks of these ranges, is the period to which our reasoning applies, and consequently strengthens the correctness of the application made, of geological phenomena, to show an abstraction of nitrogen from the atmosphere.

OF MARINE SHELLS, &c.

The pelagian shells and their coexistent marine remains, are among the most wonderful of the phenomena of nature.

not only for the immense quantity of them compacted together, but for the thickness of the masses of rock in which they are found, for their great superficial extent, and for their geographical distribution, being met with over almost the whole of the known surface of the earth.* By all philosophers who have treated of the origin of these organic remains, they are considered to afford manifest proofs of a greater degree of heat for the period of their creation, or life at the places where they are met with, than is presented at this time. These remains, even as regards their genera, have few or no analogues beyond the tropics. Those remains that are analogous, are products of the intertropical seas.

The oldest deposition of pelagian shells and of their co-existents, is about coeval with the anthracite coal;† they are most abundant near the age of the bituminous coal, less numerous at the period of the most modern deposition of salt, and they are finally lost to the series of Neptunian rocks which succeed, or rest upon the secondary ones, for not one of them has been found in the tertiary class.

Most of the facts which have been brought in view, have long been known, and a greater degree of heat has likewise been inferred from them for the period to which they relate, than subsequently existed. To account for this greater degree of heat, various conjectures have also been formed, some of which are the key stones of the "Theories of the Earth" of the earlier geologists. Of late, this subject has been taken up by Sir Alexander Crichton, who has published a very interesting memoir in the Annals of Philosophy for February and March, 1825. Its title is, "On the Climate of the Antediluvian World, and its independence of Solar influence; and on the formation of Granite." This memoir is worthy of the perusal of those who take pleasure in the long past events of organic nature. To the Huttonians it must be acceptable, as it furnishes an argument in favour of their side of the question; the conclusion arrived at by the author, being the igneous fluidity of the primitive class of rocks; the facts relative to coal, marine organic remains, &c. being considered as the result of the change of temperature, or refrigeration of

* Captain Parry, in his third voyage, found them as high as between the 73d and 74th degrees north latitude.

† Of Pennsylvania.

the fiery globe, in its passage to the state in which we now find it.

As yet, I am not prepared to say that the primitive rocks are of the origin imputed to them by the Plutonians, nor am I disposed to assent to the opinions of the Neptunians; for too few facts are known to enable me to coincide with either of those schools in their sweeping conclusions. If the igneous origin of the primitive class of rocks were fully adequate to explain the facts in question, still it is subject to the objection of being founded too much upon analogy, and ought not to be put in competition with the veiw or theory which explains the facts, by causes deduced from positive knowledge.

With respect to the position of the abstraction of nitrogen from the atmosphere, it is, as before mentioned, the deduction from the result of our experimental knowledge; for all the known minerals which preceded organization, have been analysed, with the exception of one of them, (macle or chiastolite) and nothing is so rare as the discovery of a new mineral, which at the same time is an abundant one. The importance of the position will be better appreciated, when geology shall have made such advances, as that each mass of rock containing animal remains, shall have been subjected to admeasurement—when a computation, even though coarsely approximative, shall have been made of the various animals known to exist—when chemists shall have given us their relative quantities of nitrogen—for then the result, in a measure, becomes the subject of calculation.

Facts which show an abstraction of oxygen from the atmosphere, &c.

With a few exceptions, which, at most, for quantity, are merely fractional, all the mineral iron which is found contemporaneously with the rocks of the primitive class, is in the state of the black oxide: the ferroso-ferricum of Berzelius. The iron existing in combinations, or with carbonic, or any other acid, is in the state of the protoxide. Iron has not the property of decomposing water, unless at a red heat. It can acquire oxygen only by means of moisture and atmospheric air.

Pyrites, or sulphuret of iron, is extremely abundant in the primitive class, the debris of whose rocks have given rise to the whole of the materials of the mechanical rocks. Pyrites cannot decompose water; like iron, it requires the agency of moisture and atmospheric air. The other metals, for the greater part, are similarly circumstanced.

The iron which is found in the noncontemporaneous veins of the primitive, and of the transition crystalline rocks, and all the iron of the mechanical rocks, is in the state of the red oxide, and of the hydrate of this oxide. Our common ores of iron, are the hydrates, all of which are of modern origin, when compared with the iron of the primitive class. Whence the oxygen of these ores of iron, if not from the atmosphere?

The objection to the abstraction of oxygen by means of iron, &c. is, that plants, when exposed to light, have the property of decomposing carbonic acid, retaining the carbon, and setting the oxygen free. It is very true, however, that this process goes on only whilst they are acted upon by the direct ray of the sun; for, in the night, it is equally certain, that carbonic acid is extricated, and not oxygen. As we know not the relationship between the quantity of carbonic acid given out in the night, with the oxygen liberated in the day, uncertain as we are whether the oxygen required to produce the carbonic acid of the night is greater or less than was furnished by the day, I have thought it better, in the present state of our knowledge, merely to state the facts, leaving the application till experiment shall have given us more certain data.

OF GYPSUM.

No argument can be drawn from gypsum, which has a direct relationship with the subject matter in question; but indirectly it exhibits important features, which seem to me to be worthy of consideration.

To the accurate observations of Mr. Brochant we are indebted for the important fact, that as yet no gypsum has been found in the primitive class of rocks. All the localities cited as appertaining to the primordial period, were proved, by this geologist, to belong to the transition series. Gypsum, therefore, either existed in solution during the primitive period, or was subsequently formed from its elements. Of these, lime and water are alone found in the primitive class; for no sulphuric acid, combined or uncombined, is known to exist contemporaneously with the minerals of that age.

Gypsum, as a rock, is found in the transition class, probably anterior to the first deposition of salt, as in the Alps;

also with the salt of that class; likewise with the salt of the secondary class, where it is most abundant; and, finally, at one locality in the tertiary class, in the environs of Paris, whence, in part, was derived its commercial name.

From the experiments of Mr. Beudant, it is certain, that no marine animal can live in water saturated with gypsum. Does not this fact militate against the idea of gypsum being an original production, existing in solution, since marine animals were abundant, and must have lived in the gypseous solution? Does it not, on the contrary, seem highly probable, that gypsum was formed from its elements, at various periods, and in various localities?

The admission of the subsequent formation of gypsum, explains many facts belonging to the period of its deposition, and without it we should be almost constrained to believe in the annihilation of pyrites, so abundant in the primitive rocks, if we did not suppose that this mineral, when disengaged from the materials which gave birth to the succeeding classes, was operative in the production of gypsum.

Gypsum is a product daily forming in all places where carbonate of lime is present, and pyrites is undergoing its conversion to sulphate of iron. The result of the combined action of these substances upon each other, is sulphate of lime, (gypsum,) oxide of iron, and carbonic acid, uncombined, when oxygen has access to the iron.

Whence did the plants, whose remains have produced our coal beds, obtain their carbon? Either we must suppose that the atmosphere contained more carbonic acid than it does now, or we must derive it from the carbonic acid set free from limestone, by sulphate of iron. For, in the present state of our knowledge, this is the only source, and the only way, in which we can rationally account for the carbonaceous food of plants. As to the supposition of a greater quantity of carbonic acid, at that time, in the atmosphere, it is objectionable, on the ground that it has no support, but from explaining a known fact.

The red colour of the marl of gypsum, and the red rocks, as of sandstone, &c. common to the period of gypsum, tend to confirm the view that gypsum is not an original product, but the result of the combined action of its elements, existing in the primitive class of rocks, and in the atmosphere, for the oxide of iron, their calcining matter, is a resultant, as before mentioned.

Art. XI.—*Vindication of the Memorial on the upward force of Fluids ;* by E. C. Genet.

Prospect Hill, Town of Greenbush, November 12, 1826.

TO THE EDITOR.

Sir,—I have received with infinite gratitude the numbers of your valuable Journal, containing two extracts of my Memorial on the upward forces of fluids, written by Dr. Pascalis. I ought to have much sooner acknowledged your kindness in forwarding these numbers, and your extreme indulgence in allowing that my feeble essays should be noticed in such a scientific record. But, sir, as farming is my usual avocation, I will not disguise, that having wasted too much time for an agriculturist, (who is also the head of a large family,) in experimenting, and in writing and publishing the book above mentioned, as well as in promoting other plans for the improvement of the navigation of the majestic Hudson, which flows along my meadows, I have since, almost exclusively, exerted myself to replace the time which an invincible inclination for philosophical and economical subjects, had appropriated to more pleasing though less profitable pursuits. My laborious duties are now fulfilled, all my crops are gathered, a bountiful Providence has filled my barns and cellars, and, sitting near a comfortable fire, I drew from my desks your numbers and other magazines, in which also my late essays have been noticed, and I find that several of those periodical works, including yours, have treated me with that benevolence and hospitality which denotes the friends of mankind and of the useful arts, while others have passed upon my production a sentence perhaps more merited. I ought prudently to let the matter rest there, in a state of quiescence and equipoise, until more extensive experiments than those which I have been able to make, should prove who is in the right, the author or the critics. But as we are always a little blind to the faults of our children, I cannot resist the natural impulse which excites me to repel, if practicable, the incorrect statements and unfair reports of my theories and experiments, pre-eminently made by the editors of the Boston Journal of Philosophy and the Arts. You must not imagine, however, that I am displeased to find that those gentlemen have endeavoured to recreate their readers at my cost; very far from it; *J' aime la plaisanterie*, as much as any of my old fellow citizens, and having been used in my youth to the great world, I have never been affronted at a criticism, a censure, or a joke, aimed at me, if it were grounded and deserved; but when innocent plans practically offered to my adopted country in atonement of the errors which I may formerly have committed under a foreign revolutionary impulse, are indirectly converted into an undisciplined ambition to rank with the philosophers and sages of the earth, I feel myself bound in duty to those who have, with less prejudice, investigated and countenanced my contemplated improvements, to shew that my censors are themselves incorrect, and have adduced neither facts nor experiments to reverse what I have advanced. I beg you then, Sir, to grant me some room in your Journal, to hold my ground against my assailants, and to suffer that your press should become my shield, as the strong shield of the powerful Ajax was the rampart of the lightly armed Teucer.

The Boston reviewers, Sir, after a short exordium on the intrinsic value of all discoveries in the arts and sciences, and other very judicious remarks on the too frequent mistakes of those who flatter themselves that they have made discoveries of greater value than future ages will assign to them, express a desire to act towards me with humanity, and to analyse my pamphlet with a respectful attention. But unfortunately, on their first step, they are stopped short in their charitable intention, by the title of the book, which they pretend not to understand. "*A Memorial on the upward Forces of Fluids!! What does that mean?*" They are at a loss to explain it. I thought it was very plain, and sufficiently explained, had they read the Memorial with the least degree of attention; but if I must be more explicit, I will observe to those gentlemen, that according to my theory there are two different kinds of upward forces in fluids, the one due to the principle of gravity, the other to what I call the principle of levity. *The first* is the mechanical result of the pressure of the heaviest particles against the lightest, which makes them push the latter upwards, by virtue of a centripetal force, which draws the heaviest towards the centre of the earth in proportion to their specific degree of affinity with the unknown cause of gravity, or, as I have suggested it, in proportion to their privation of the unknown cause of levity. *The second* seems, in my opinion, to be due to the action of another fluid, which draws upward towards the etherial regions, certain particles of matter and aerial fluids, in proportion to their degree of affinity with the unknown cause of that ascensive and centrifugal force. I apprehend, however, that the feigned *embarrass* of my Boston reviewers on the interpretation of that mystical title was simply a pretext to introduce a paragraph of my Memorial, containing a series of facts, on the existence of the new force which I propose to put in requisition for the use of man, and which they humorously depict " as a grotesque assemblage, containing the intellectual germ of the whole volume, *leaving out the theory on the pendulum as an extraneous bee in the author's bonnet.*" If that bee is under my bonnet, I can assure them that it has not stung me, and that, until they refute, in a more philosophical manner, my conjecture on the concurrent influence of caloric on the variations of the pendulum, such an insect, extracted from their hive, will prove to be nothing more than a buzzing and unproductive humblebee, that I will dismiss without any further chastisement.

I agree, notwithstanding, Sir, with Dr. Pascalis, that it was a bold undertaking not to coincide implicitly in the Newtonian solution of the retardation of the pendulum under the equator, by the compression of the poles, and by the centrifugal force augmented by the diurnal rotation of our planet, though the doctor himself admits, that " should there be one single element in the universe which cannot be controled by gravity—(and certainly caloric is one)—gravity cannot be said to be an universal law."

I have, Sir, more attentively reflected on that interesting subject, since the publication of my Memorial, and I am confirmed in the opinion that there is indeed a fluid which exerts its action on the pendulum, varying according to the latitudes, and increasing from the equator to the poles, very much like the magnetic fluid, the minimum of which is under the magnetic equator, and the maximum towards the poles, as it has been observed by Humboldt, who has found that the vibrations of a good and well suspended magnetic needle were, in the space of ten minutes, in Peru, 211, and in Paris, 245. But what is that fluid? Is it, as I have supposed it, the caloric, which exerts its influence on the rarefaction and condensation of the air, as well as on the contraction or expansion, even in the vacuum of any of the substances used to construct the pendulum? Or is it the galvanic fluid of our straified globe, more excited by the radiant matter of the sun at the equator than at the poles, and retarding the pendulum by its increased attractive force? Or is it a remnant of the nebulous fluid of Herschel, of which our planet seems to have been originally formed, which occupies, as a nucleus, its center, and which, by its attractive and repulsive affinities, produces the phenomenon of the centripetal and centrifugal forces, and perhaps also the magnetic currents; in as much as the experiments of Morichini, and other scientific men, in various countries, have proved that the violet rays of the sun, separated by the prism, contain the pure magnetic principle, from which fact it might be inferred, that these violet rays being refracted or repelled, when they strike the earth, take their course and flow as a stream through the upper strata of its geological crust and the medium of its atmosphere, towards the northern and the southern poles.

It is not yet in the power of philosophy to give a conclusive answer to those questions; but as human knowledge is advancing, with a wonderful rapidity, towards extraordinary results, we must be prepared to expect great changes in the most accredited systems, and if the wise and modest Newton were living, and enlightened by the splendid body of science acquired in the last and present century, we may venture to affirm, that he would be less positive and assuming than many of his too ardent disciples, if we may judge of what he would do, by what he has said in his 31st question on optics. "*Quam ego attractionem appello fieri potest ut ea efficiatur impulsu, vel aliqua causa nobis ignota.*" (What I call attraction may be the effect of impulse, or some other cause to us unknown.)

Many observations are still wanting, to fix definitely the figure of the earth and the variations of the needle and pendulum; and I sincerely hope that Parry and Franklin will succeed in their contemplated attempt to penetrate as far as the poles, to settle those points. But, Sir, if unfortunately they are arrested in their glorious career, by insurmountable barriers of ice, is it not to be regretted that instead of promoting the improvement of aerial navigation, which would supply the most convenient, the safest and the cheapest means, to cross the frigid zone which encircles the polar sea, or extends to the pole, gentlemen who hold the trumpet of fame, in a city celebrated for the inventive genius and enterprising spirit of its inhabitants, should pervert their literary talents to discredit *æronautism*, and to deprive their country of the additional honor of having subdued the air by the improvement of that science, after having subdued the waters by the power of steam?

But I forget, Sir, that I am not writing now in support of ærostation, and only answering the Boston reviewers. Returning, then, to those gentlemen, I find that, having condemned me, they triumphantly undertake to show that their mathematical science is in no way inferior to their cosmological knowledge. They take hold, for that purpose, of the description of my ærostatic elevator, and magisterially observe, that I give to the large wheel of that machine a diameter of 30 feet and a circumference of 90, "while, in the time of Metius, *every thing*, of 30 feet in diameter, had a circumference of 94 feet."—I could reply to this charge, that Metius has never said that *every thing*, let the figure be what it would, had those proportions, and that he has restricted them to the circles only, which being similar figures, have a circumference proportionate to their radii, or to their diameters. I could also represent that, in common mechanical practice, when a rigid mathematical calculation is not requisite, it is customary to estimate the circumference to be to the diameter in the proportion of about 3 to 1, and that the word *about*, which is indeed in my manuscript, had been omitted by the printers. But, Sir, unwilling to avail myself of such a vindication, on a question of mathematics, I contend that the proportions of the circumference of the circle to its diameter are not yet precisely known. Archimedes thought that a circle, having 7 feet diameter, would have 22 feet circumference. Adrien Metius, on the contrary, who professed mathematics at Calmar, in the sixteenth century, was of opinion that the proportion of the diameter of a circle to its circumference, was that of 113 to 355. While Bezout, professor of mathematics in France, and member of the Academy of Sciences of Paris, has proved, in his *Cours de Mathematiques*, published in 1770, that Metius was incorrect, and that, by following his proportions, if the diameter of a circle were 3,000,000 feet, there would be an error of one foot on the circumference, and that the surest proportion was 1 to 3,141,592,653,5897932. But now all those rules of Metius, Archimedes, Bezout and others, are revered by a citizen of Boston, who, as the public journals report, has discovered the true quadrature of the circle, by means of equative comparasive quantities, formed by a complication of arcs and circles, and who proves, by calculations which he is willing to lay before the most scientific men, that the quantity of a circle, whose diameter is 1, and which had hitherto been considered to contain 78,5898, &c. contains more than 79, &c. These facts are sufficient to prove that the authority of Metius is not as conclusive as the editors of the Boston Journal make it; and that if so many errors, notwithstanding the extensive use of the circle, have been committed by the most skilful men, an approximate proportion of 1 to 3 was not absolutely improper, in the rough description of a wheel, and did not deserve the unmerited sarcasm of my having changed the rules of Metius. *Ce n'est point moi Messieurs qui ai change tout cela, c'est, entre autres, un scavant de l'Athenes d'Amerique.*

Next to this side blow, given to the wheel of my elevator, with the heavy volume of Metius, the Boston reviewers are not better pleased with the use I make of the same wheel to recal down the balloon after its ascension, without considering that a loss of gas, for which a valve or stop cock has been provided, is to aid the descent of the balloon after it has performed its ascension in the cupola, or tower, and that a new supply of gas, trifling as to its cost, will soon regenerate the force of ascension. They then inquire " why not carry up a boat on an inclined plane, by the exertion of a horse or a steam engine, or by a falling weight, as one Robert Fulton has proposed it a great while ago ?" to which question I answer, that it would require the power of seven horses and one half power, to draw up a boat of 60 tons on a rise of 4½ inches per chain, and twenty horses on a rise of one perpendicular foot to 15 horizontal, and that the primary cost and keeping of such a number of horses, or of a standing steam engine equal in power, would much exceed the small expense attending the construction and keeping of an ærostat, as it may be seen by the calculations on that subject in my memorial, and as I could more particularly show if that information were required for actual experiment. With respect to the machine very concisely described by the reviewers as the falling weights of Fulton, it is well known that his views on that subject, though applicable, and previously to Fulton effectively applied, to the extraction of coal and other minerals in England, Scotland, Germany, and other countries, have met with difficulties and objections as to their applicability to canals and rail-ways, which induced Fulton himself to abandon them.

From this last specification of my patent, the reviewers pass to what they call the conversion of the ærostat into an hydrostat, and to the use I propose to make, for the navigation of lakes, rivers, and seas of that new power, increased by the superior upward forces of water against atmospheric air, and of atmospheric air against hydrogen gas. But here permit me, Sir, to enter a protest against their veracity as reporters. When their duty was to give an exact description of the experiment upon which this new application is founded, they present an erroneous one, evidently intended to make their readers believe that the said experiment is contradicted by the very principles of hydrostatics, that I have myself offered in the introduction of my Memorial as a test, in order to enable those who should not be acquainted with those principles to judge if I had made a discreet and correct use of them. The principle to which I allude is, that a body immerged in water loses a portion of its weight, eqnal to the volume of water it has displaced, and as the weight of a cubic foot of common fresh water is, in English weights, 62½ pounds, it follows of course that a body, although devoid of weight, plunged under water, could not rise more than the weight of an equal volume of water. That principle is not denied, but I do not plunge my hydrostat into water and separate it from the stratum of its homogeneous fluid. On the contrary, the hydrostat used for the experiment I have reported, was an oblong spheroid, rather approaching the cylindrical form, the capacity of which, measured with a gasometer, was eqnal to a cubic foot, and it is a fact, that the said hydrostat, so regulated, has invariably supported when kept afloat on fresh water, a weight of about 100 pounds, and on salt water, of 108; and when immersed or plunged into those two kinds of water, its upward or uplifting force has diminished in proportion to the depth of water under which it was plunged, losing, under the pressure of an incumbent foot of water, about one third of its force. The opposition of the water to the intruding air, and the disinclination of the air to be forced below its stratum, would seem, accordingly, if my experiment has been correct, to be proportionate to the base or inferior area of the hydrostat, and to the principle of levity of the air subject to the various degrees of intensity of the fluids in contact. We differ on the words *sinking* and *floating*, which every swim-mer knows are very far from being synonymous, which I had cautiously distinguished in my specifications, and which I could not imagine that criticism could have perverted. The only application, Sir, of the hydrostat, which the Boston reviewers have suffered to pass unmolested, is the *hydronaut*, (for I cannot imagine that the simple combinations of the laws of statics united in that self-moving boat, are the objects of their stale comparison to the perpetual motion.) They let it go without scrutiny, although I do not conceal that it is the only one of my applications which has raised doubts, in the minds of scientific mechanicians, friendly to the other plans. It is questioned whether the force created by the hydrostats will be sufficient to operate at once on the paddles and on the pumps, and whether a stagnation of the machinery will not follow the first stroke of the hydrostat. I am not entirely of that opinion, and I think that in case of stagnation in the pumps, as I have anticipated in my Memorial, additional hand-pumps could easily remove the difficulty. At all events, I think proper to record here Sir, that I have, since the grant of my patent, discovered natural means, entirely independent of the power created by the ascension of the hydrostats, that would carry off all the water discharged from the cylinders into the recipients, and insure the success of the machine, if the steam-boat interest, every day increasing in extent and influence, did not unite its efforts to cry it down.

But, Sir, the most unfair part of the review, is the absurd description of the æronaut, denominated by the reviewers, *the flying apparatus*, when the whole burthen of my observations on that subject, is intended to prove that an attempt to fly in the air would be vain, and that the best method to propel ourselves in that fluid is to imitate the natation of the fishes. The flight of the birds is the result of a force purely mechanical, while the swimming or natation of the fishes is the result of a static power, combined with a mechanical force. Deprived of the balloons or ærostats, men would never have succeeded in supporting themselves in the air; but by the help of the ærostatic power, and of the means of propulsion and direction which I have offered, they will be able, whenever they will take proper measures, to navigate the air with as much facility as the fishes swim in the water; because to succeed in that proud undertaking, three requisites only were wanted, which are all three, now, by the progress of science and our natural intellect, under our control; I mean the power of ascension obtained by ærostation; the power of propulsion easily supplied by mechanics; and, finally, the power of guidance or rationality, which mankind (with a few exceptions) possess in a superior degree to the porpoises or sturgeons.

Another unfair statement of the Boston reviewers, is to represent my æronaut as being surmounted with a wind-mill. A wind-mill is a passive and standing machine, moved by a current of air, and entirely subject to its action; which machine would produce no kind of effect in the air, being deprived of a point of rest, inasmuch as it would possess in itself no self-created power to react against the atmosphere in which it would be immersed. Quite on the contrary, the aerial fins of my æronaut (the figure of which is assimilated to several known fishes, not *with a vengeance*, as the editors say, but with the most scrupulous imitation,) instead of being moved by the air, would act directly upon that fluid, and compel it to favour the progress, of the machine, as is fully demonstrated in the Memorial.

I now quit, Sir, the Boston reviewers, to mention another review printed at Philadelphia, and entitled the Franklin Journal and American Mechanics Magazine, published by Dr. Thomas P. Jones, in which I was informed my Memorial had been critically noticed. But I find, that, excepting a few additional acerbities and ungenteel allusions to my advancing years, the editor has copied verbatim, and taken upon trust, what the Boston Review has said on that subject. His notice of my work is little more than an echo.*

If, however, Sir, some reviewers in Boston and Philadelphia have tried to put down my upward forces, I have the consolation to know that other American Journals have been more indulgent; that a favourable report has been made to the Legislature of this State, on the means which I have proposed to insure the safety of passengers on vessels of all descriptions, including *steam-boats*, and to prevent their sinking or foundering; that the Philosophical Society of New-York has warmly recommended to the patriotism of the citizens of the United States, to raise by subscription the very small sum necessary to construct an aerial vessel on my plan; and that if I may credit the newspapers, an English gentleman, a Mr.

* Dr. Jones has published an original article on this subject, in the last number of his Journal. EDITOR.
February 7, 1827.

Green, near London, is said to have constructed, to navigate the air, a machine similar in principles to mine, though of smaller dimensions, and that from the result of several experiments already made, he prefers ascending in a still moonlight night, the air being then less troubled by currents. But nothing, Sir, flatters me more than the impartial analysis made of my Memorial, by the scientific Doctor Pascalis, in your excellent repertory of the arts and sciences, a work so generally esteemed in Europe and in America.

Yours, &c. E. C. GENET.

ART. XII.—*Newellian Sphere.*

THE machine to which the above appellation has been given, is designed to show, by mechanical representation, the motions of the planetary bodies. It was originally the contrivance of Mr. Theodore Newell, of Vermont, who being in limited circumstances, enjoyed, in consequence, but few opportunities for scientific acquirements. An accident, which occurred to him several years since, deprived him, in a great degree, of the use of his limbs. Being thus rendered incapable of labouring in his occupation, which was that of a farmer, he commenced, at an advanced age, the construction of his first machine, which was merely a simple contrivance, showing the motion of the earth around the sun, without giving, however, its axis any inclination to the plane of the path which it describes. By the assistance which he received, at subsequent periods, from the liberality of different individuals, as well as from information communicated, and new ideas occasionally suggested by men of science, who had an opportunity of examining his invention, as well as from his own determined perseverance in accomplishing his object, the machine was brought, by a series of improvements, to that degree of perfection which it possessod when the inventor arrived with it in Middletown, Connecticut, in the fall of 1825. At that place, and at Hartford, he succeeded, by his exertions, in interesting several gentlemen in the success of his invention. An association was soon formed, with the requisite funds for the construction of machines. This association engaged a mechanic, of competent abilities, to execute the work, under the immediate superintendence of two gentlemen attached to the Military Academy.

On a more particular examination of the machine, it was ascertained, that the motions produced were not given with the degree of accuracy required to place it on an equal footing with others, heretofore designed for the same purpose.—The estimations of the work, likewise, were, in certain cases, made upon erroneous principles. As an instance of the first, only 365 days were designed to be given to the year, which is not so near an approximation to the true number as is given by most other machines: and, as an instance of the second, in describing the rotary motion of the earth, the effect of a compound motion, arising from its revolution around the sun, was not considered in the calculation. From these reasons it was found necessary to make an entire change in the calculation of the wheel work. In the communications of motions, likewise, a sufficiently strict adherence to mechanical principles was not maintained. The application of the driving to the impelled parts, being made in a manner inconsistent with the production of a steady and equable motion, not affording that uniform resistance so essential to a contrivance designed to be impelled by a time moving power.

In order to do away these imperfections, it was necessary to new model the whole, and a complete change in the mechanical arrangement of the different parts has been the consequence.

With the alterations above mentioned, important additions have been made, and the design so extended as to embrace many of the most interesting phenomena of the solar system, which are not represented in the original contrivance. Among the principal of these phenomena, are those which would result from the addition of the superior planets; the inclinations of their orbits to the ecliptic, particularly that of the moon, with the change of its nodes; the correct relative distances, from the sun, of the earth and inferior planets; and the firmament, with the principal stars in its several constellations. The whole, it is believed, having been accomplished in such a manner as to enhance greatly the value of the machine, rendering it, as an instrument of instruction in the science of astronomy, more perfect, and consequently more valuable, than any that has heretofore been devised for the same purpose. A description of the machine, in its present improved state, is given as follows:

Description of the Newellian Sphere.—The frame work consists of three large circles of mahogany, of about 6 feet in diameter, intersecting each other at right angles, in the form of an armillary, and supported upon a standard in such a manner that two of them are vertical and one horizontal. In the common center of these circles the sun is placed. The plane of one of these vertical circles represents the ecliptic, or the plane of the orbit in which the earth moves in its revolution around the sun. The circle itself is faced with brass, on which the graduations of longitude, of the zodiacal signs, and of the months and days of the year, are accurately engraved. The other two circles, cutting the ecliptic at points 90° distant, may be considered as colures, the vertical passing through the solstitial, and the horizontal through the equinoctial points of the ecliptic. Joining the intersections of the two latter circles, is a horizontal metallic rod, supporting the sun in its center, itself forming the axis, and its extremities the poles of the ecliptic. On this axis, and supported by it, is a concentric circle of brass, about three feet in diameter, which, for the sake of distinction, will here be called the annual circle. In this circle are two metallic braces, like two equal and parallel cords, at right angles to the axis. On one side of the axis, and supported between two circles, or wheels, placed opposite, in the braces, is a terrestrial three inch globe, representing the earth, with its axis inclined at the requisite angle to the ecliptic plane. The circumference of one of these wheels is geared, through the intervention of a small pinion, to the circumference of another equal wheel, firmly fixed and stationary upon the axis.

By this contrivance, the earth, when carried around the sun by the revolution of the annual circle, has the parallelism of its axis, or its uniform inclination to the ecliptic, preserved in all points of its orbit. Without, and near to the annual circle on the north side, is a wheel of 14 inches diameter, stationary upon the main axis. This we shall call the ecliptic wheel. A small pinion attached to the adjacent limb of the annual circle, runs on the circumference of this wheel, and describes the circumference in a tropical year, carrying with it the annual circle, with the earth which it contains. Motion is communicated to the pinion last mentioned, by a train of wheels reaching to the main axis, when a crank or time-piece operates at its arctic extremity. The rotation of the earth or its revolution about its axis, is produced by a motion drawn from the train of wheels just mentioned. A spindle, with its extremities resting in the braces of the annual circle, regulates the equable motion of the two wheels between which the earth is supported, and has attached to it two pinions, one of which gives motion, through the intervention of several small pinions, to a wheel with a level or inclined face; moving upon which, and carried by the other pinion, is the moon's wheel, the moon itself being borne upon an arm or vector, extending from the wheel over the earth. By this contrivance, the revolution of the moon is performed in the requisite time, the mean inclination of its orbit to the ecliptic, likewise the mean motion of its nodes in antecedentia is shown. On the main axis, and without the annual circle on the south, is a combination of 5 wheels and 10 pinions, constituting the train belonging to the superior planets. To each wheel of this train there is attached a quadrantal arm, or vector, extending over to the ecliptic plane, near the extremities of which the planets are placed. The first wheel, or wheel of Mars, in the combination, is impelled by a driver attached to the annual circle, that in its turn operates as a driver to the wheel of the asteroid Ceres, and that again gives motion to Jupiter, and from thence motion is communicated to Saturn, and through Saturn to Herschel. The inferior planets are moved by a combination of 3 wheels, and pinions similar to those just described, placed upon the main axis within the annual circle, and near to the arctic brace. These planets are borne upon arms extending from their respective wheels, in the manner represented for the superior planets. These arms or vectors are formed of brazen tubes. A longitudinal slit, or opening, of sufficient length, is made at the extremities of each. A slide, to which the planet is attached, moves in this opening by the action of a wire passing from it through the whole length of the tube to the main axis, when it conforms to a groove in the circumference of an eccentric wheel. By this contrivance a reciprocating or vibratory motion is given to the planet on its arm, or vector, producing a change in latitude, and giving to the plane of the path which it describes its proper inclination to the ecliptic. The earth and inferior planets are placed at their exact mean relative distances from the emblematic sun. This relation is not maintained in the superior planets, owing to the too great dimensions it would give to the machine. The relative magnitudes of all, however, are shown, save the earth and its satellite, which are necessarily enlarged. Enclosing the whole machine is a spherical envelope of blue silk, stretched upon wires, in such a manner, that different sections may be displayed while others are folded up for viewing the machinery within. On this envelope the several constellations of the heavens, with their imaginary figures, are represented. All the stars of each, which are included in the 1st, 2d, 3d, and 4th magnitudes, have their proper relative positions and their magnitudes distinctly shown. A brazen graduated semi-circle, moveable on the poles of the ecliptic, serves to point out the latitude and longitude of any of the heavenly bodies. To return now to the earth, there is, in addition to what is usually represented on a three inch terrestial globe, a moveable brass meridian, or semi-circle, a moveable equator and horizon circle. These circles are graduated, the equatorial into hours, and the horizontal into degrees, from its cardinal points. The latter is moveable upon two pivots, placed opposite in the equatorial circle. By the assistance of these, if the 12 o'clock point of the moveable equator be brought to the meridian of any place, the horizon may be easily adjusted to that particular place by inclining it a number of degrees equal to the latitude of the place, as shown by the graduated meridian. Enclosing the earth (the earth revolving freely within) are three circles, forming a brass armillary, every way corresponding to the great armillary of the heavens, before described. One of these forms the ecliptic, the other two the colures, serving to point out the geocentric positions of these planets, particularly those of the inferior planets and the moon. A brass pointer made fast to one of the braces with a folding joint, when extended, points out the vertical position of the sun upon the earth's surface for any instant. This is called the solar index. By bringing the small graduated meridian under this index, the declination of the sun is pointed out for any given time. The same is likewise done of the Moon. The moon's wheel is graduated each way from where the moon's vector is inserted, to 180°, by means of which, the angular ecliptic distance of the moon from the sun is designated by the solar index, and, consequently, the times of quadratures, conjunctions, and oppositions. By the assistance of this graduation, and of the ecliptic limits placed at their proper distances from the nodes of the inclined lunar wheel, the approximate times of solar and lunar eclipses and the quantities of those eclipses nearly, are determined. Near the antarctic extremity of the earth's axis, and attached to the projecting centre of one of the wheels which support the earth in the annual circle, is a stationary plate, or dial, having its 12 o'clock point constantly towards the sun. By referring the points on this dial to the earth, the places where the sun is rising or setting, and the beginning and end of twilight, is shown for the given time. Near the north extremity of the ecliptic, or main axis, is a weekly wheel, with an index, showing the several days of the week.

The wheel work of the machine is made of brass, with the exception of some of the pivots and small pinions, which, on account of their greater action, are made of steel. The power for communicating motion to the whole, is applied to a diurnal wheel, at the arctic extremity, either by hand, through the intervention of a crank, or by a time-piece, geared to the diurnal wheel. If it be necessary at any time to hasten the movements of the machinery, for illustration, the time-piece may be easily disengaged, by withdrawing the diurnal wheel from its action, the motion of the time-piece not being checked. By observing the time when thus disengaged, and setting the machine forward a space equal to the elapsed time, it may be again thrown into gear, and the whole move on as though there had been no interruption.

It may seem a matter of impossibility, that a time piece, of ordinary dimensions, burthened with the resistance which such a mass of machinery would be supposed to present, should yet be enabled to move the whole, and itself preserve an uniformity in its motion. This doubt, however, will in a great measure be removed, when we consider the immense mechanical advantage under which it operates. The diurnal wheel, with which it immediately communicates, has a velocity, in comparison with the principal annual circle, of 365¼ to 1, and consequently possesses a mechanical advantage in that ratio. This advantage is still greater on the combination of wheels carrying the superior planets, in as much as their motion is slower; and, in fact, the comparative moderate motion of the different parts, gives a great ratio in favour of the diurnal driver. It must be understood, likewise, that the parts of machinery are so nicely adjusted and supported by their centers of gravity, that they repose in equilibrium in every position which they can possibly be made to assume by the motion of the machine. From this adjustment, an uniform resistance is offered, at all times, to the action of the moving power. The immense mechanical advantage, then, which the power has been shown to possess, is only exerted to destroy what little resistance may be supposed to arise from the friction and rigidity of the materials of which the moving parts are constructed. In the machine described, a weight of eight ounces, suspended at the end of a crank, with a three inch arm, is sufficient to put the whole in motion.

In the general arrangement of the several component parts of the machine, all those precautions were used which would insure strength and stability to the whole, and ease and regularity in the individual and combined movements of the parts, consistent with the disadvantages which unavoidably arose from the restrictions of the arrangement, making it necessary to order the agents in such manner as to produce a limited ratio and preciseness in the several motions. These difficulties have been overcome, it is believed, in such a manner as to make the least sacrifice of mechanical principles; the motions being shown, as nearly as could possibly be done, and a simplicity of character and a directness of application maintained in the agents, so as to divest it of that complexness which would necessarily enhance the expense, and consequently diminish its usefulness.

The variable motion of the earth in its elliptic orbit, causing it to describe more days from the vernal to the autumnal equinox, than through the remaining portion of the year, is produced by an unequal distribution of the cogs, or teeth, in the ecliptic wheel. The same variations are extended to the lunar train, and to the division of days on the great ecliptic circle. These variations in the relative distances of the teeth in the same wheel, are invisible to very close observation, and no essential interference of the working parts is occasioned in consequence. The calculations are so made, likewise, that but two sizes of cogs are used in the wheel work of the whole. The train of the superior planets comprehends one division, and the remaining parts of the second division. The advantage of this is found in the mechanical construction.

Extreme nicety is necessary in cutting, forming and facing the teeth, and the artist is very evidently able to perform this to greater perfection, where they are all of the same pattern and dimensions. 365¼ solar, or 366¼ siderial rotations, are given to the earth in one revolution around the sun

—this varies a few minutes from the requisite number. The aggregate amount for a given number of years can easily be compensated for, and the machine set forward or backward, to its proper station. 365¼ divisions are likewise made on the ecliptic circle, the ¼ being appended to the 28th of February. These divisions consequently correspond to the first year after a bissextile. A deduction of ¼, ½, or ¾, of a day, must therefore be made from the time pointed out by those divisions, on the 2d, 3d, or 4th year subsequent to a bissextile.

The equation of time, or the sun fast or slow clock, is likewise shown on the ecliptic circle for each ten days through the circuit of the year. The relative motions of the superior planets are such, that taking the ratio of any two that are adjacent, and comparing it with that as produced by the machine, they will be found to agree to four places of decimals, being as near an approach to coincidence as could be attained without increasing vastly the size and number of teeth in the wheels and the complexness of the machine. This small amount of deficiency or increase being known, can easily be compensated for, for any given time. The same degree of accuracy is preserved throughout the other parts. Neither bands nor cords are used in the communication of motion to any of the parts, so that no irregularities are produced by that stretching and slipping which is generally an unavoidable consequence of that mode of gearing. Motion is transmitted in every case, save that of the eccentric wheels abovementioned, by the intervention of toothed wheels and pinions. When a train of these are combined, a certain degree of play in each is unavoidable, for the purpose of an easy motion, so that a slight degree of irregularity is perceived in the extreme parts. This irregularity will consequently produce a corresponding disagreement in the mean motion of the heavenly bodies which the machine is intended to represent. These deviations, however, being comparatively trifling, will not, it is believed, detract any thing from the merit of the machine as an assistant in instruction, in showing the general motions of the bodies in the planetary system, and in illustrating the various phenomena which those bodies at certain times exhibit. For nice and accurate calculations, reference must be had, as in all similar cases, to figures. This is more peculiarly necessary in determining the motions of the heavenly bodies, which, from their mutual and constant attractions, are ever deviating from regular paths, embracing such a combination of changes, as to exhaust even the almost boundless powers of modern analysis in their representation. Those general phenomena which are represented by the machine are enumerated as follows:

First. The phenomena of the primary planets, showing their mean motions in their revolution about the sun, in their proper directions from west to east in paths, the planes of which are inclined, their mean requisite angles to the ecliptic —illustrating the changes of latitude and longitude—the phenomena of conjunctions, oppositions, quadratures, stationary points, &c. and pointing out the positions of their ascending and descending nodes—their apparent paths in the heavens, and their relative positions, for any given time, in relation to the fixed stars.

Secondly. The phenomena of the inferior planets, particularly their motions, direct and retrograde—their inferior and superior conjunctions—their elongations, transits, and the phenomena of the morning and evening star.

Thirdly. The phenomena of the change of seasons, of day and night, the varying declination of the sun, its rising and setting for different latitudes, its longitude, its amplitude, and azimuth, and its appearance, as well as that of the other heavenly bodies, in a right parallel or oblique sphere.

Fourthly. The phenomena of the diurnal motions of the heavenly bodies—the circles of perpetual apparition and occultation—the rising, setting, altitude, latitude, longitude, and declination of the principal fixed stars—the constellations in which they are located, and the phenomena of the appearance of different constellations at different seasons of the year.

Fifthly. The phenomena of the moon's phases, in its varying position with regard to the sun and earth—its periodic and synodic revolutions—the inclination of its orbit to the great standard, the ecliptic plane—the retrograde motions of its nodes—its latitude and longitude, and the approximate time of its rising, southing, and setting, for any given day and place.

Sixthly. The phenomena of solar and lunar eclipses, and occultations—the time and quantities of those eclipses nearly, and their repetition in each revolution of a lunar cycle.

Seventhly. The phenomena of the tides, shewing nearly the time of flood and ebb, spring and neap, for any given place, by reference to the position of the sun and moon.

Eighthly. The propriety of the general steps in the operation for calculating the latitude and longitude of places on the earth's surface, from observations of the heavenly bodies.

Ninthly. The phenomena of solar and siderial time, and the propriety of the intercalary day for each fourth year, with its suspension, each fourth century, according to the Gregorian or new style.

The foregoing are some of the principal astronomical phenomena, mechanically illustrated by the Newellian Sphere, a contrivance, considering the object for which it was designed, more perfect, it is believed, than any that has heretofore been devised On the value of machines of this description for the purpose of instruction, it is unnecessary long to dwell. Their utility is sufficiently acknowledged in the many attempts that have heretofore been made to construct them by persons well skilled in the science of astronomy. The whole system of planetary bodies is condensed, as it were, into a compass so small, that the mind easily embraces every part, and sees at a single glance the principal relative motions and changes which the mutual actions of those bodies have been found to produce. It will not, indeed, be pretended, that the machine will yield, at one view, a clear conception of the celestial motions, or explain the operations of those laws which the doctrine of matter unfolds, and upon which human reason has reared by far the most stupendous fabric of its power. A perfect knowledge of so extensive and intricate a science as that of astronomy, is by no means so easily to be attained. There is indeed no more a "royal road" to astronomy at the present time, than there was to geometry, in the days of Pythagoras. An unwearied zeal—long and constant application—are the only means which can insure to the mind a clear and adequate conception of the truth and application of its sublime theories. To the professed scholar, therefore, one who has time to devote to scientific researches, a machine of this description will not be so essentially beneficial: his investigations of mechanics and physics, will easily prepare him for a knowledge of the celestial motions, all of which he will readily comprehend as he gradually advances in his labors. But to those whose different pursuits will not permit them to enter into a detail of studies connected with this important science, or who have not time to wade through a long course of book demonstrations, or patience to undergo the drudgery of unravelling a chain of geometrical diagrams, such a machine will be found of essential importance, by addressing to the eye, during a comparatively small portion of time devoted to examination, a mass of information, which, by no other method, could so easily be obtained. To the juvenile student in astronomy, it may render an essential service, pointing out to him, in a comprehensible manner, the grand outlines of that science, which, in the prosecution of its study, he will pursue understandingly, with a better capability, from the general views which he thus acquires, of filling up, to the best advantage, the minuter parts. These general ideas will, from association, become so firmly impressed upon the mind, as not to be easily effaced, and thus be rendered more permanent, and the motions of the heavenly bodies be made more evident and satisfactory, than could possibly be done by the best diagrams, with the most familiar explanations, in the same space of time.

In introducing an invention to the notice of the public, and requesting their patronage, it is necessary that they be assured there is merit in the contrivance, and that it possesses advantages over others that have been designed for the same object. With regard, then, to the Newellian Sphere, we conceive that it has merits which none other, at present in use, is found to possess. The one selected for a comparison, is that recently constructed by Dr. Pearson, of Edinburgh, it being generally acknowledged to be, both in the simplicity of its contrivance and the accuracy of its motions, much superior to those of Martin, Ferguson, &c. which have been generally used in our seminaries of learning. The Newellian Sphere, it is believed, is superior to Pearson's machine, in the following respects:

First. In simplicity of contrivance and mechanical arrangement of parts.

Secondly. The plane of the ecliptic being vertical to the horizon, the motions of the planets correspond more exactly with their real motions, and consequently reference is made from one to the other with greater ease.

Thirdly. The inclinations of their orbits to the great standard, the ecliptic plane, is produced; the planets, in Pearson's machine, all moving horizontally in the same plane.

Fourthly. The fixed stars, constituting an important portion of the visible universe, are represented. The paths of the planets in the firmament, are consequently pointed out and easily traced, and their positions readily determined, by reference from the machine to the heavens, which no other contrivance of the kind has ever been known to represent.

Fifthly. The adaptation of time, as an impelling power, in such manner that similar artificial and natural phenomena are produced at coincident times.

Sixthly. The machine, notwithstanding its bulk, is very portable ; the silk envelope being readily folded up, the armillary circles easily revolved into the same plane, and the projecting arms of the planets disengaged without inconvenience ; the whole secured in an appropriate case, without occasioning any derangement to the parts, or requiring any separation of the wheel work of the machine.

Such are the principal points of superiority which it is believed this machine possesses over that constructed by Dr. Pearson. The mean periodic times of the planets are, it is allowed, pointed out to a closer degree of exactness by the Doctor's contrivance ; but the difference is so trifling, in comparison, as to be more than counterbalanced by the superior advantages which the sphere has been shown to possess in other respects. Of the actual value of the machine, however, an enlightened public are yet to judge. From the general approbation which has been bestowed on the original contrivance, by men of acknowledged talents and scientific acquirements, it is confidently believed, that in its present much improved state, it will be found still more deserving of their attention, and will receive a proportionably greater share of patronage and support.

To the inventor, Mr. Newell, much praise is certainly due for the zeal he has displayed in the cause of science. In deciding upon the merits of the invention, these considerations should enhance its value, and serve as powerful inducements to remunerate him, promptly, for some portion of the time and expense devoted to this object.

However valuable and meritorious such inventions may be, still, the demand for them being comparatively limited, the inventor will probably receive a much smaller recompense, than is usually obtained for those contrivances, which are brought into use in the daily concerns of life.

Mr. Newell has devoted many years to the accomplishment of his object, and is now verging towards that period of life when nature, by her ordinary laws, withdraws her children from the world. For the last twenty years, he has had to contend with poverty, pains and infirmities, to an extent almost indescribable. Could the first of these ills be alleviated, the burden of those afflictions, which have weighed so heavily for the past, would be comparatively much reduced, and himself be enabled to taste some of those enjoyments of which he has so long been deprived, and which would serve to smooth the short path-way he is yet to tread in life. We conclude these remarks, in the confident belief, that an appeal, in a case so deserving, will not be regarded with indifference by the American public.

Art. XIII.—*Notice of the Steam-Boat Babcock.*

Newport, R. I. Nov. 25, 1826.

TO THE EDITOR.

Sir,—I have delayed answering your inquiry relative to the steam generators invented by Mr. Babcock, until the question of their practical utility should be completely set at rest, by their successful operation. It is so often that accounts are given in our papers and journals, of inventions and discoveries in mechanics, that have eventually proved erroneous in principle and useless in practice, that the public have become cautious in listening to mere descriptions, until they have passed the ordeal of a successful experiment. Improvements in the steam engine, have, for more than fifty years, exercised the ingenuity, not only of the practical but of the scientific mechanic, and though amid the hosts of patents yearly taken from the American and European offices, we find more for this object than for any other, yet, at this day, the engine moves with the same simple, graceful grandeur, with which it was constructed by the illustrious Watt, without any material alteration, and seems to have sprung from his genius, like Minerva from the brain of Jupiter, perfect at its birth.

Though the low pressure engine has never been essentially improved, yet, it appears to me, that the time is fast arriving when this method of applying steam will be considered complex and cumbersome, and until some superior method is invented, it will be entirely superseded by those upon the high pressure plan; but this can never be, until some substitute is discovered for its justly dreaded boiler; this is the desideratum that will make a revolution in the annals of "the greatest present ever made by philosophy to the arts." The invention of Mr. Babcock is intended to effect this object, and consists in converting water into steam by injecting it into a series of cast iron tubes, previously heated to a requisite degree of temperature. The tubes are arranged transversely in a furnace, in two rows, one tube in the upper and one in the lower row, being alternately connected with each other by elbows. Connected in this way, they make two distinct generators; the end of one leads into the top of a cylinder of a high pressure engine, and one end of the other into the bottom of the cylinder. Each of these generators has a small forcing pump attached to it, for the purpose of injecting the water to be converted into steam, and which is supplied from a small reservoir placed on the top of the furnace, (or in any other convenient situation.) The pumps are made to work alternately, by a proper connexion with the machinery of the engine. To set the engine in motion, the reservoir is filled with water, a fire is kindled in the furnace, and when the tubes are heated, an injection is made by hand from one of the pumps into one of the generators, (suppose the one leading into the top of the cylinder.) The heat instantly converts the water into steam, and by opening a valve in the top of the cylinder, the down stroke of the engine is made. While descending, the machinery causes the other pump to inject a quantity of water into the other generator connected with the bottom of the cylinder. A valve admitting the steam into it, is then opened, and the up stroke of the engine is made. Its motion is then continued without any further assistance; the valves that lead off the steam are of course opened and closed alternately—the engine itself, differing in no respect from the common high pressure ones, excepting, that when used in a boat upon the salt water, it is necessary to have a condenser attached, as only water that is fresh can be used, as the salt would, by filling up the tubes, render them useless. The improvement, therefore, consists in merely attaching these generators to a high pressure engine, as a substitute for a boiler. The advantages gained by it are—in the saving of fuel, the saving of room, and a saving of weight. It is impossible, at present, to make an accurate estimate of these advantages, as the generator and engine, upon which they would be predicated, were most wretchedly constructed, and cannot be considered as a fair test of their power. Poor, however, as they were, the gain will be evidently seen in the following statement. They were placed in a boat of eighty tons measurement, and that drew four feet and a half of water. The diameter of the cylinder of the engine was ten inches, the length of the stroke of the piston was three and a half feet, the generators were cast in lengths of three and a half feet, were five inches internal diameter, and an inch and a half thick. These lengths were arranged horizontally in the furnace, in two rows, seven in each, and connected alternately by elbows. The forcing pumps were two inches internal diameter, and the quantity of water injected, varied from three to five cubic inches. The space occupied by the furnace was about seven feet in length, and four in width and height.

The whole of the machinery was very poorly constructed, and the boat was much too heavily timbered for her size, yet she performed an average passage, between this place and Providence, in three hours and a half. The distance is called thirty miles; the quantity of wood burnt varied from two to three feet, and the whole quantity of water on board never exceeded a barrel, nor was even the whole of that necessary, as it was saved by a condenser.

During the last summer, she made a trip to New-York in twenty-five hours, a distance of one hundred and seventy miles; the quantity of wood then consumed, was, by actual measurement, one cord and three quarters. Now had a high pressure boiler been attached to the engine, instead of the generators, it would have occupied at least nineteen feet in length; the weight of the mass of brick work enclosing it, together with the weight of the boiler, with the water contained in it, would make the space and weight at least four times that of the generators, and the quantity of wood used in a trip to Providence, of three hours and a half, instead of three feet, would have been at least sixteen feet; the saving of fuel and the advantages in space and weight, are therefore apparent.

The subjoined sketch is taken from one that was draughted for a boat now building to ply upon the Hudson, and will be much more perfect in its construction, than the one now described. It was not thought necessary to show any thing more than the arrangement of the tubes, and their connection with the cylinder.

A B, fig. 3, shows a portion of two of the tubes, connected by the double elbows, and in fig. 1, the whole of the elbows are shown by the letter A, as they are arranged in the furnace, the tubes themselves being concealed within. C and D are the tubes leading the steam into the top and bottom of the cylinder. Fig. 2, is an inverted side view of the elbows on the opposite side, but without the forcing pump, which is represented in fig. 1, by E. The injecting tube F, leads from it into the generator, that causes the down stroke of the engine. G is the axis of the wheel to which the shackle bars are attached; upon it is the eccentric H, which moves the forcing pump E; there is a similar forcing pump upon the other side of the furnace, for injecting the water into the other generator, moved by another eccentric, upon the same axis G, but geared so as to work alternately with the other pump.

The condenser, the reservoir for the water, and a front view of the furnace, it is not thought necessary to show, as the plan can be understood without it. The method of converting water into steam, by injecting it into a generator, previously heated, has been, I believe, many times attempted, but invariably failed. Mr. Babcock's claims to originality, consist, therefore, merely in the method of obviating the cause of failure, and that is done by using two generators instead of one, and by having the water injected into them alternately, so that while one is in operation, the other has time to regain its exhausted caloric; and he thinks that if the two generators were connected with each other, so as to make but one, and of course the injection of water constant, that they would cool faster than they could be heated.

The public, however, will rest satisfied with their success, without troubling themselves about the cause of it. There are a number of boats building for the Hudson, the Connecticut, and this bay, which will be in operation in the spring.

Very respectfully, your obedient servant.

JOSEPH H. PATTEN.

ART. XIV.—*Meteorological Journal, kept at Westfield, Massachusetts, from November* 1, 1824, *to November* 1, 1826; by E. DAVIS.

	A.	B. 1825.	C. 1824 & 5	D. 1825 & 6.	E. 1824 & 5.	F. 1825 & 6.
1824.						
Nov.	38,2	39,7	54,6	44,6	27,8	23,0
Dec.	33,5	30,6	44,6	46,0	20,8	12,6
1825.		(1826.)				
Jan.	26,6	26,7	40,0	48,3	13,3	–5,6
Feb.	29,8	29,0	43,0	48,0	7,3	–2,0
March.	39,2	35,9	47,1	56,0	31,3	21,0
April.	48,5	47,3	60,7	53,6	32,6	31,0
May	56,0	66,4	66,0	74,3	44,0	49,6
June.	68,9	67,2	78,5	77,0	59,0	57,0
July.	76,0	72,8	84,3	77,3	65,3	64,0
Aug.	68,9	69,8	77,0	79,6	60,3	60,0
Sept.	57,3	63.5	69,1	77,3	48,3	52,6
Oct.	51,9	50,6	70,0	66,0	37,6	39,0

A and B, average monthly temperature. C and D, average temperature of the warmest day in each month. E and F, average temperature of the coldest day in each month.

Remarks.

1. Records of temperature are made in my Journal at sunrise, at sun-setting, and at 2 and 9, P. M. The thermometer is Farenheit's, and hangs in the shade, north of the house.

2. July is the warmest month, and January the coldest.

3. The annual temperature averages about 50°, as appears from my Journal, and from a Journal kept the latter part of the last century by the late Rev. Noah Atwater.

4. The mercury rises highest in June, and sinks lowest in February.

5. The extreme ranges of the thermometer in 1825, were $-4°$ and $95°+$, in 1826, -17 and $91°+$.

6. Unusually warm days at all seasons of the year, and rainy days, especially during the winter months, are followed by strong westerly winds. This town is bounded west by mountains.

7. The quantity of rain that falls annually at the present time, I do not know. In a journal kept by Mr. Atwater, I find the following facts. In nine years, from Jan. 1786 to Jan. 1795, the average number of inches that fell, in rain, was 37, the average number of inches of snow, 53½. 1775 was remarkable for the great quantity of rain that fell, it being 51¾ inches—of snow 52 inches. In 1796 only 21 inches of rain fell.

Smoky Atmosphere.—From the 7th to the 10th of October, 1825, the atmosphere was so smoky as almost to render the sun invisible at mid-day. It extended over all the New-England States, and south to Virginia. Many people, who had left their windows open on the night of the 7th, were awakened by the strong smell of smoke, and got up, with the impression that their houses were on fire. It is generally supposed to have been caused by the fires that raged about that time in Maine.

Similar smoky days in the autumn of 1820, were attributed to extensive fires near the Mississippi. The state of the atmosphere was such, and also the temperature, as proves it to have been so light, that the smoke would fall to the earth, though I had no barometer to ascertain its exact weight. The only objection against its being smoke from these fires, is the improbability that it would extend over so great an extent of territory. But this is no insuperable objection. From an observation of these facts, it has occurred to me, that what is usually denominated the "Indian summer," may be attributed to the same cause, viz. *smoke diffused through the lower regions of the atmosphere.*

If the atmosphere is *dry* and *light*, smoke will be accumulated, and fall near the earth's surface. If the atmosphere holds in solution a great quantity of water, the carbonic acid and some other ingredients entering into the composition of smoke, will be absorbed. If the quantity of water be small, a complete absorption will not take place, and consequently the smoke will continually accumulate. If, now, after the atmosphere has been dry a day or two, the specific gravity should suddenly be diminished, the smoke will fall and produce an Indian summer. This smoky weather usually occurs after the autumnal rains, when the temperature is mild and air serene; when it would seem that the atmosphere is both dry and light. A series of hygrometrical and barometrical observations are necessary to determine the truth of this hypothesis.

I do not pretend to give the true solution of the phenomenon under consideration—I give that which seems to me at present most plausible. I wish if any of your correspondents are possessed of facts, which refute the solution given, that they will give them publicity, since there seems to be a want of information on this subject.

Calculations of weather. The almanac maker is not the only man, who predicts what the weather will be at some future period. Almost every person has signs, which, in his view, are indicative of rain, or sunshine, heat or cold. This prophetic spirit is not possessed of the present generation alone, but is discovered to have existed in all nations, and in all preceding ages. M. du Hamel, Kirwan, Bacon, and others, have laid down maxims for prognosticating the weather. Their rules were the result of much labor, but have been little regarded. It seems, therefore, useless to attempt to give conclusions, which indicate a particular state of the weather in future, since the commonalty will abide by their ill-conceived maxims, until meteorological phenomena can be reduced to a system, and find a place among the elementary books of science, that correct information may be diffused among all classes. That such an event will ever occur, will perhaps, be considered beyond the reach of probability. The attention that has been, and still is given to this subject, must in time result in some system of general truth. We, probably may not be able for any considerable time previous, to tell what particular day will be fair or stormy, but we may be able to discover certain precursors of a particular kind of weather. Such an event is not improbable. There is much truth in many *signs*, that the common class of people regard as indicating what the weather will be in future; the philosopher should therefore select the true from the false, and not treat the whole as nugatory—a relic of superstition.

E. DAVIS.

Westfield, Dec. 1826.

ART. XV.—*Notice of Floating Islands.*

NEWBURYPORT, Oct. 31, 1826.

TO THE EDITOR.

DEAR SIR,—

IF the following remarks will answer any valuable purpose, they are at your service for insertion in your Journal of Science. AMOS PETTINGALL, Jun.

That a few floating reeds, upon a pond, should collect together, and adhere with sufficient compactness to sustain small pieces of earth and decayed shrubs and plants, and thereby exhibit small clumps of vegetables moving on the water, is not surprising; but that islands of any magnitude should be found in this vagrant state, has ever been considered a subject of considerable curiosity. Passing over the mythological fiction of the floating *Delos*, as founded upon questionable evidence, and the island of Chemmis, with those called the Cyanean, reported as floating, by the less doubtful testimony of Herodotus, the first of which history gives a minute and circumstantial account, are those in Lake Vadimon, near Rome, (now called *Lago de Bassanello*,) described by Pliny major and Seneca. Pliny the younger, in the 20th Letter of his 8th Book, gives a very interesting description of the same, in which he mentions the circumstance of sheep, which, while grazing, imperceptibly fell upon some of these islands, lying on the borders of the lake, and were carried off by the wind, and borne to the opposite shore. It is also asserted by Bœthius, that in *Loch Lomond* there are floating islands upon which cattle graze.* A few small ones, of the same description, are said to exist in a lake in the province of Honduras in America. These, the only instances which I can readily collect, serve to show that it is a subject of rare curiosity.

The island, which I am about to describe, is situated nearly one mile south of the market-house in Newburyport, about two stones cast from what is called Old-Town meeting-house, in a

* Upon turning to the Modern Geographical Treatises, I do not find this alluded to; and if the account be fabulous, it will prove these objects of curiosity more rare, and thereby add to their interest.

pond in the rear of the adjoining burying-ground. Its length averages about 140 feet, and its breadth 120, containing nearly half an acre. Its surface is thickly studded with dog-wood, although not a bush of it is found beyond the limits of the island, as though it were an enemy to the water that surrounds it. There are upon it six large trees, two of which measure in girth three feet and upwards, besides several clusters of willow trees of small growth. These rise and fall with the island. The pond is usually dry during the summer months, and at these seasons the island has been found so low, that you would descend, *perceptibly*, in passing to it from the dry bed of the pond: I visited it yesterday, and found it elevated about 18 inches above the level of the pond's bottom, owing to the rains that have recently fallen.

The customary rise of the pond in the fall and spring, is about 8 feet, although it has been known to rise 12: the island preserves the same elevation above the surface of the water in the different periods of its rise. I have been told, to-day, by a man of unequivocal veracity, that he has forced a pole, ten feet in length, down through the centre of the island, and with this, as far as he could extend it with his arm, has been unable to meet with a solid and permanent bottom. He also informed me, that when the pond was very high, these large trees standing upon the margin of the island, overhang the water with considerable obliquity, owing, probably, to the roots being brought to a great degree of tension, and preventing the exterior part of the island from rising with the centre. It is not entirely detached from the bed of the pond, but seems to be a kind of a stratum peeled off from the solid parts below. In passing across its surface, the whole island is considerably agitated, and presents a waving appearance, like the sea; you are toiling continually to ascend, as though it were a surface of flexible ice. It seems once to have been a subject of much notoriety, but appears to have escaped the notice and knowledge of many of our modern townsmen. I was unacquainted with it myself, until yesterday, though I have skated frequently round it. This may lead some to think that this statement is an exaggeration, but it is not so. The real fact is not to be discovered by one observation; they should be repeated at different seasons of the year, when the pond is dry and when it is full, or it may be visited by a thousand different persons at as many different times, and no remarkable phenomena appear. I have mentioned to a number, considerably older than myself, that I visited yesterday one of the greatest curiosities in New-England, and when told what it was, they have replied with a smile, " I have always known the floating island in the meeting-house pond."

ART. XVI.—*Examination of Mr. Quinby's Principle of Crank Motion.*

THERE is contained in the 7th Vol. of the " Journal of Science and Arts," a paper by Mr. Quinby, of N. York, on the subject of crank motion. He has undertaken to show that there is no loss of power in communicating rotatory motion by means of the crank ; and in the endeavor to prove this assertion, he has so blended correct mechanical principles with incorrect, that at first sight, his demonstration appears plausible : a careful examination cannot fail to convince any one, even slightly acquainted with mechanics, that however ingenious the solution may be, it is in point of fact incorrect.

The proof is based upon the well known proposition, that when a weight in descending causes an equal weight to ascend, through a space equal to that gone over by the descending weight, there is no loss of power. After announcing this fact, Mr. Quinby sets out to show that if the shackle bar acted always in a direction parallel to that of the piston rod, there would be no loss of power. Let us refer to his demonstration : the circle vlGw (fig. 1) is constructed so that its radius CG shall be a third proportional to the quadrant AD and radius CD, of the circle ADBE, representing the circle in which the lower end of the shackle bar moves : he then shows that the mean tendency to rotation in the wheel, caused by equal powers, acting at the different points of the wheel, in directions parallel to that of the piston rod, is equivalent to a constant force, (equal to each of these powers acting on the crank,) acting at the point G of the circle vclG, in the direction PG ; then concludes,—since P in descending through the space Py raises W equal to P, through a space Wx equal to Py, there is no loss of power.

This is certainly a very ingenious argument, but it can not stand the test of examination. The weight P, at the distance GC, from the centre C, holds in equilibrio an equal weight at the distance Cv, equal to CG,—the same weight applied at the distance CD would keep in equilibrio an equal weight W at the distance CE, equal to CD, or a greater weight (W′) at the distance Cv: the same will be the case when P descends raising W, P acting at D will raise W′ (greater than W) at the distance C*v*, through a space equal to that gone over by P in its descent, then the effective power of P applied at D is greater than that of an equal power acting at G;—it is shown by Mr. Quinby, that the mean power of the crank is equal to the constant force P acting at G,—therefore less than P acting constantly at D,—therefore there would be a loss of power if the shackle bar were supposed to remain vertical.

Having, it is hoped, shown the fallacy of the attempt to prove that no power is lost in crank motion, it will be proper to say a few words upon the *actual loss* of power; but first let me remark upon the manner in which Mr. Ward's proposition, relative to the crank, is treated by Mr. Quinby.

Mr. Ward's idea is, that "the effects produced at the several points of division of the quadrant, are as the perpendiculars respectively from these points to the lines of force." Mr. Quinby denies this, and undertakes to prove the proposition to be incorrect; the error in his proof can easily be made evident, by following the course of his demonstration.

The circle ADBE (fig. 2) represents that in which the end of the shackle bar moves; *a*S and *dt* are two positions of the shackle bar, corresponding respectively to the points *a* and *d*; *am* and *dn* are the ordinates of the circle, ADBE, drawn from the points *a* and *d*; C*c* and C*e* are perpendiculars drawn from the centre, to the lines S*a* and *td*. P represents the constant force acting in the direction SA, upon the upper end of the shackle bar. "Now by referring to what is demonstrated in Vol. 1, Chap. 6, Art. 195, of Gregory's Mechanics, it is obvious that the value of P, estimated in the direction S*a*, or which is the same thing, the tension of the shackle bar when in the position S*a*, is equal to $\frac{P \times rad.}{\text{Cos} < ASa}$;

and the value of P in the direction *td*, or the tension of the shackle bar, when in the position *td*, is equal to $\frac{P \times rad.}{\text{Cos} < Atd}$;

and (by mechanics) the tendency which P has to produce rotation when the crank is at a, is equal to the tension of the shackle bar, at that time, multiplied by the distance Ce; *i. e.* $=\left(\frac{P\times rad.}{\text{Cos}<\text{AS}a}\right)\times \text{C}e$: and the effect produced at the point d, is equal to $\left(\frac{P\times rad.}{\text{Cos A}td}\right)=\text{C}c$" : these expressions are not *equal* to the tendency to rotation, but they are *proportional* to it. To be *equal*, each should be divided by the radius of the crank: that is of no consequence just now, since they are to be put in the form of a proportion: "and now, if the inference drawn by Mr. Ward were true, then would $\frac{P\times rad}{\text{Cos}<\text{AS}a}\times \text{C}e : \frac{P\times rad}{\text{Cos}<\text{A}td}\times \text{C}c :: am : dn$; or (by dividing the first and second terms by P. *rad*, and substituting in place of am and dn their proportionals Ce and Cc) $\frac{\text{C}e}{\text{Cos}<\text{AS}a} : \frac{\text{C}c}{\text{Cos}<\text{A}td} :: \text{C}e : \text{C}c$." But C$e$D and C$c$ are *not* proportionals of am and dn, for $am : a$S : Cc : CS, and $dn : dt$ (or aS) : : Ce : Ct; in these two proportions, the third terms are the same, and in order that the terms of the first couplet in one proportion, should be proportionals to the terms of the first couplet of the other, the fourth terms must be equal; but Ct is evidently less than CS, hence Cc and Ce are *not* proportionals to am and dn, and the remaining part of Mr. Quinby's demonstration founded upon this assumption, can be of no avail.

In showing the actual loss of power in the application of the crank, we will consider as proved, the fact shown by Mr. Quinby, that in the actual case in practice, the tendency to rotation is the same as it would be if the shackle bar remained constantly vertical; a refutation of his demonstration is unnecessary.

Let Sa (fig. 3) represent the position of the shackle bar, at the point a of the circle AEBD, in which the lower extremity of the bar moves, and Sa the value of the constant power P, applied to the upper extremity of the bar; by resolving the force Sa into the two forces ab and Sb, the first in the direction of the radius, the second parallel to the tangent at the point a; Sb is the component tending to produce rotation; the Sb multiplied by the arm of its lever, Ca, must be equal to P, multiplied by the lever am, or (calling Ca, R;

the variable *am*, x; the effective force $\Phi)\ \varphi = \frac{P.\ x}{R}$: the mean value of this expression, or the mean tendency to rotation, will be found by considering x as the ordinate of the centre of gravity of the semicircle AEB, in which case x is a mean of all the ordinates of the semi-circle : in this case $x = \frac{R^2}{Arc\ AE} = \frac{R^2}{R.\ \frac{3.1416}{2}}$, then $\Phi = \frac{P.\ x}{R} = \frac{P \times R}{R\ 1.5708} = P. \times$ ·6366 ; this being the value of the effective force, the power lost, or the difference between the power applied and the effective force, will be P—P×·6366, or P×·3634, showing a loss of more than one third of the power applied.

Fig. 1

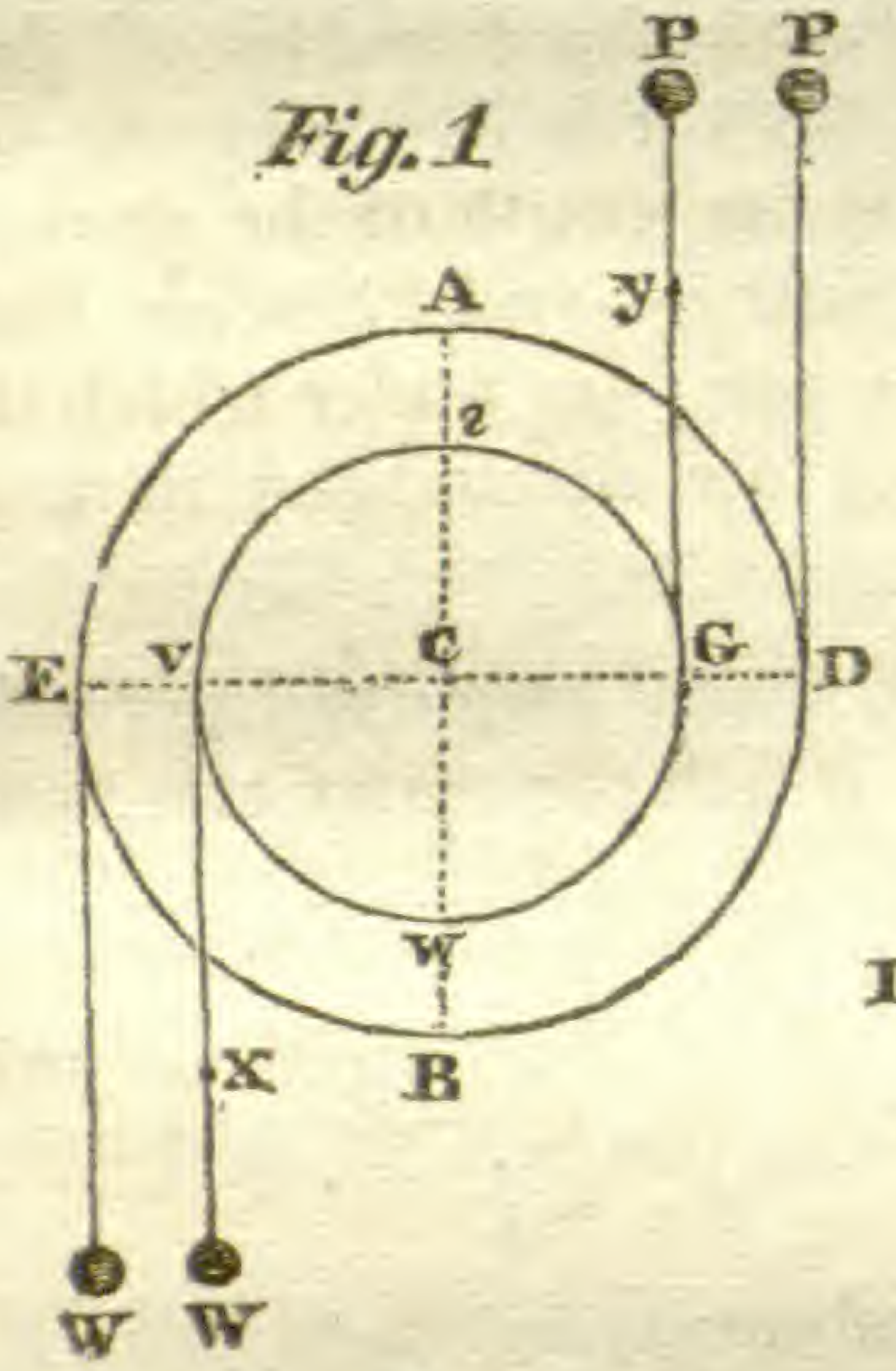


Fig. 2

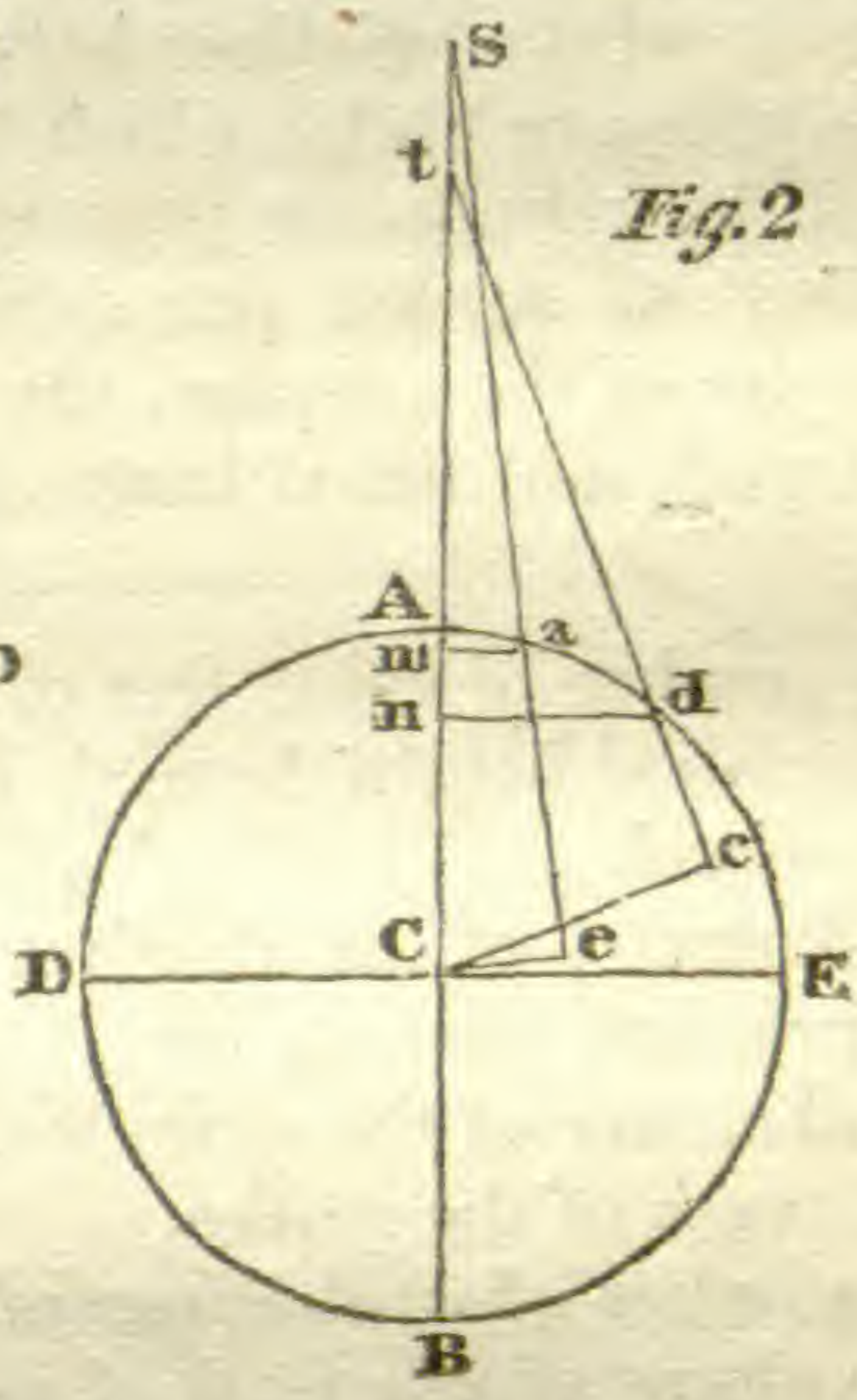
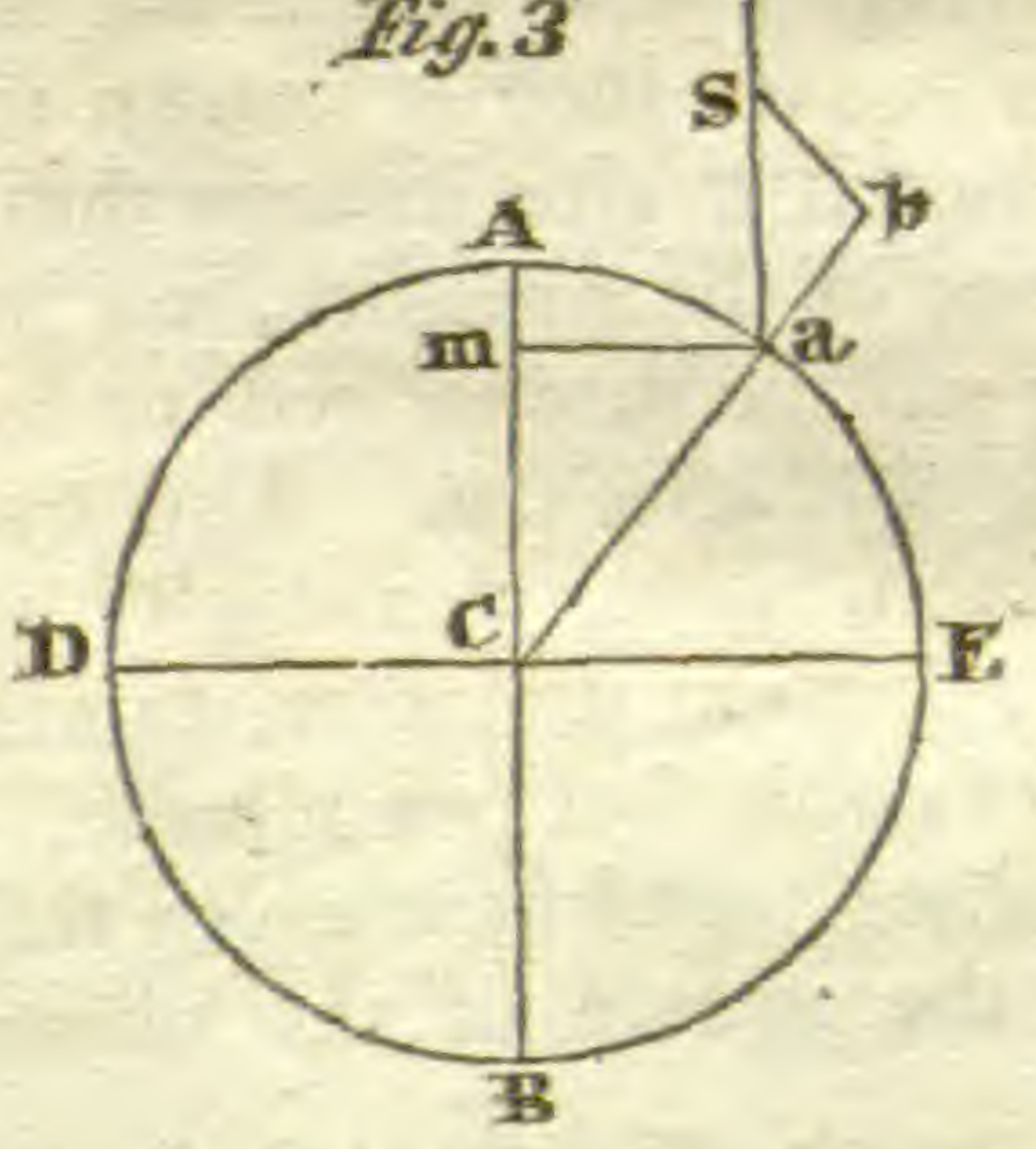


Fig. 3



ART. XVII.—*Investigation relative to the Blowing Machinery of a Blast Furnace;* by Mr. A. B. QUINBY.

THE proper construction of the blowing machinery of a blast furnace, is a subject that deeply interests many individuals. I am not aware that any formulæ have ever been given to assist in planning such machinery. From the nature of the problem, it is easy to perceive, that there must be a relation between the quantity of water employed, the vertical height through which the water acts upon the wheel, the capacity of the air-pipe,* and the pressure under which the air is discharged.

There is also a relation between the diameter of the air-pipe, the pressure under which the air is discharged, the diameter of the cylinder or piston, the length of the stroke, and the number of strokes per minute :—and likewise between the diameter of the air-pipe, the pressure under which the air is discharged, and the volume of air discharged in a given time.

Put P = number of cubic feet of water employed per minute.
h = vertical height through which the water acts upon the wheel.
d = diameter of the air-pipe.
p = pressure (per sq. inch,) of air discharged per minute.
D = diameter of the cylinder or piston.
L = length of the stroke.
N = number of strokes per minute.
M = volume (cubic feet) of air discharged per minute.

Then, since the velocity with which the air issues under a pressure of 1 lb. per square inch is known to be 20,726 feet per minute ;† and since it is also known that the velocity varies as the square root of the pressure, we have for the velocity with which air will be discharged under the pressure p lbs. per square inch, $20726 \times \sqrt{p}$. And since the pressure of 1 lb. per square inch, gives a velocity of 20726 feet per minute, an air-pipe 1 inch in diameter, (under this pressure,) will discharge 113.04 cubic feet per minute.

Now the volume discharged in a given time will evidently be in proportion to the square of the diameter of the air-

* The air-pipe is the pipe from which the air is discharged into the tuyere of the furnace.

† See Farrar's Hydrodynamics, p. 382.

pipe. Hence for the volume discharged under the pressure p, and through an air-pipe whose diameter is d, we have $113.04 \times \sqrt{p} \times d^2$ cubic feet per minute. But M is also equal to the volume discharged per minute.

Therefore $M = 113.04 \sqrt{p} \times d^2$ (I)

Whence $p = \dfrac{M^2}{(113.04 \times d^2)^2}$ (II)

$d = \dfrac{\sqrt{M}}{\sqrt{(113.04 \times \sqrt{p})}}$ (III)

Next, to get the velocity with which the piston will move, we have $D^2 : d^2 :: 20726 \times \sqrt{p} : \dfrac{20726 \times \sqrt{p} \times d^2}{D^2}$. And this quantity divided by twice the length of the stroke, will give the number of strokes per minute.

Hence $N = \dfrac{20726 \times \sqrt{p} \times d^2}{D^2 \times 2L}$ (IV)

Whence $d = \dfrac{\sqrt{D^2 \times L \times N}}{(\sqrt{10363 \times \sqrt{p}})}$ (V)

$p = \dfrac{(D^2 \times L \times N)^2}{(10363 \times d^2)^2}$ (VI)

$D = \dfrac{\sqrt{(10363 \times \sqrt{p} \times d^2)}}{\sqrt{(L \times N)}}$ (VII)

$L = \dfrac{10363 \times \sqrt{p} \times d^2}{D^2 \times N}$ (VIII)

And, now, to obtain the momentum of the piston,* we have $\dfrac{20726 \times \sqrt{p} \times d^2}{D^2} \times D^2 \times .7854 \times p = 16278.2 \times \sqrt{p} \times d^2 \times p$; lbs. raised one foot high per minute.

We must now change the denomination of this momentum into that of *cubic feet of water*, raised one foot high per minute. To do this, we have only to divide by $62\frac{1}{2}$; the number of lbs. that are equal to one cubic foot of water.

Hence $\dfrac{16278.2 \times \sqrt{p} \times d^2 \times p}{62\frac{1}{2}} = 260.45 \times \sqrt{p} \times d^2 \times p =$ the momentum of the piston in cubic feet of water, raised one foot high per minute.

* By momentum of the piston, is meant the product of the whole pressure and the space, in feet, through which the piston moves in one minute.

But from the nature of the problem $P \times h =$ the momentum of the piston in cubic feet of water raised one foot high per minute.*

Therefore $P \times h = 260.45 \sqrt{p} \times d^2 p$ (IX)

Whence $$P = \frac{260.45 \times \sqrt{p} \times d^2 \times p}{h} \quad \text{(X)}$$

$$h = \frac{260.45 \times \sqrt{h} \times d^2 \times p}{P} \quad \text{(XI)}$$

$$d = \frac{\sqrt{P \times h}}{\sqrt{260.45 \times \sqrt{p} \times p}} \quad \text{(XII)}$$

$$p = \frac{(P \times h)^{\frac{2}{3}}}{(260.45 \times d^2)^{\frac{2}{3}}} \quad \text{(XIII)}$$

It is now easy to perceive how these formulæ are to be applied—it may, however, not be improper to remind the reader, that the value obtained for h, in equation (XI), by assuming values for d, and p, and P, is not the whole height of the fall, but merely the vertical height through which the water must act upon the wheel. To get the whole height of the fall, it will be necessary to add about *one-fifth* to the value obtained from the formulæ. This, however, can always be decided by experienced and intelligent mill-wrights.

Before concluding this investigation, it is proper to note, that in the formulæ for the momentum of the piston, the quantity P vanishes. This shows that when the friction and the inertia of the piston are not regarded, the result is independent of the diameter of the cylinder. When, however, the friction and the inertia of the piston are regarded, the result is not independent of the diameter of the cylinder. The difficulty of ascertaining the amount of the friction and and of the inertia in a given case, is the cause that induced me to neglect them in the formulæ I have given.

In planning the machinery, however, no essential error will arise from neglecting to consider the friction and the inertia of the piston. The only quantities that require to be corrected on account of friction, &c. are P and h. If P be given, we must add as much to the value of h, obtained from the formulæ, as will be sufficient to overcome all the friction, &c. of the machinery. And if h be given, we must add as much to the value of P, obtained from the formulæ, as will

* It is scarcely necessary to remark, that I have here reference to my theory of water-wheels.

be sufficient to overcome all the friction, &c. of the machinery.

On the subject of the proper diameter for the cylinder, it depends essentially on the quantity of blast which we wish to employ. If we wish to employ only 900 cubic feet of air per minute, the proper diameter of the cylinder will be about 33 inches.*

To shew this, we will suppose that the pressure under which the air is discharged is 4 pounds per square inch; then by applying formula (III,) the diameter of the air-pipe is found $=2$ inches, (very nearly.) We must now assume a value for N.—Suppose $N=20$: thereby taking $D=33$, and applying formula (VIII,) we find $L=3$ feet 9 inches; which is about the proper length in proportion to the diameter 33.

As, however, there will always remain some compressed air in the cylinder, (at the end of each stroke,) it will be necessary to increase the diameter, or the length of the cylinder, a little beyond what is given by the formulæ. This is a matter that can only be estimated by scientific and practical men. Next, let us suppose that 4000 cubic feet of air per minute, is the quantity of blast required; and let this be discharged under a pressure of 4 pounds per square inch: then by applying the formula, it is found that the diameter of the air-pipe must be 4½ inches. But this is essentially too great.†

We will now assume a diameter for the air-pipe. Suppose that the diameter of the air-pipe is 4 inches: then by applying formula (II,) the pressure per square inch is found to be 15½ pounds.

And by taking $L=5$, and $N=20$, and applying the formulæ we find the diameter of the cylinder 60½ inches. If now we take 20 feet for the vertical space through which the water acts upon the wheel, and apply formula (X,) we shall obtain 7139 for the number of cubic feet of water required per minute.

From these results it is plain that when the quantity of the blast required per minute is great, it is better to employ two

* The blast furnaces of New-Jersey and Pennsylvania, where charcoal only is used, employ from 700 to 900 cubic feet of blast per minute. The largest furnaces in Europe, where coke is used, employ a much greater blast

† The air-pipes at some of the largest furnaces in Europe are from three and a half to four inches in diameter. In this country, where charcoal only is used, they are from two to two and a half inches in diameter.

or more cylinders; and to construct the furnace with two tuyeres; and, *of course*, to have two air-pipes.

There are reasons, however, which render it better to employ two or more cylinders even when the furnace has but one tuyere; and when the quantity of blast required per minute, is not great. These are—1*st*, The blast will be more uniform; and, 2*nd*, A smaller receiver will be sufficient.

A. B. QUINBY.

August 11, 1826.

ART. XVIII.—*New Demonstration of the Binomial Theorem*; by Prof. THEODORE STRONG, of Hamilton College.

CLINTON, Jan. 30, 1827.

TO THE EDITOR.

DEAR SIR,—

SHOULD you think the following demonstration of the Binomial theorem worthy of a place in your valuable Journal, you will oblige me by inserting it.

Assume the identical equation $(a\pm x)^1=a\pm x=a^1\pm\frac{1}{1}\times a^{1-1}x^1$ (For the first power of a quantity is the same as the quantity itself; therefore $(a\pm x)^1=a\pm x$ and $a^1=a$, also $a^{1-1}=a^0=\frac{a}{a}=1$) to this add the expression

$$\frac{1\times(1-1)}{1\times2}a^{1-2}x^2\pm\frac{1\times(1-1)\times(1-2)}{1\times2\times3}a^{1-3}x^3+\frac{1\times(1-1)\times(1-2)\times(1-3)}{1\times2\times3\times4}a^{1-4}x^4\pm\text{ \&c. ad infinitum.}$$

Every term of this expression equals nothing, for it has the factor $1-1=0$, therefore $a^1\pm\frac{1}{1}\times a^{1-1}x^1$ will not be increased or decreased by the addition of this expression. The addition gives $(a\pm x)^1=a^1\pm\frac{1}{1}\times a^{1-1}x^1=a^1\pm\frac{1}{1}\times a^{1-1}x^1+\frac{1\times(1-1)}{1\times2}a^{1-2}x^2\pm\frac{1\times(1-1)\times(1-2)}{1\times2\times3}a^{1-3}x^3+$ &c. ad infin.

$$=a^{m}\pm\frac{m}{1}a^{m-1}x+\frac{m\times(m-1)}{1\times2}a^{m-2}x^{2}\pm\frac{m\times(m-1)\times(m-2)}{1\times2\times3}a^{m-3}x^{3}+$$

&c. ad infin. In which $m=1$, and I have put m instead of 1, in order to avoid confusion in the process.

Now multiply both sides of the equation $(a\pm x)^{m}=a^{m}\pm\frac{m}{1}a^{m-1}x$ $+\frac{m\times(m-1)}{1\times2}a^{m-2}x^{2}\pm\frac{m\times(m-1)\times(m-2)}{1\times2\times3}a^{m-3}x^{3}+$ &c. ad inf. by $a\pm x$ and I have $(a\pm x)^{m}\times(a\pm x)=(a\pm x)^{m+1}=a^{m+1}\pm\frac{m}{1}a^{m}x+$ $\frac{m\times(m-1)}{1\times2}a^{m-1}x^{2}\pm\frac{m\times(m-1)\ (m-2)}{1\times2\times3}\times a^{m-2}x^{3}+$ &c. ad inf. $\pm\frac{1}{1}\times a^{m}x+\frac{m\times2}{1\times2}a^{m-1}x^{2}\pm\frac{m\times(m-1)\times3}{1\times2\times3}\times a^{m-2}x^{3}+$ &c. ad infinitum.

Hence by adding $(a\pm x)^{m+1}=a^{m+1}\pm\frac{m+1}{1}a^{m}x+\frac{(m+1)}{1\times2}\times$ $(m)a^{m-1}x^{2}\pm\frac{(m+1)\times(m)\times m-1}{1\times2\times3}a^{m-2}x^{3}+$&c. ad infin. which is of the same form as the expression for $(a\pm x)^{m}$ only $m+1$ takes the place of m, that is, m is every where increased by 1. In like manner, if both sides of this equation are multiplied by $a\pm x$, $m+1$ will be increased by 1, or will be changed into $m+2$, and so on to $m+3$, $m+4$, $m+n$, n, denoting any positive integer which gives $(a\pm x)^{m+n}=a^{m+n}\pm\frac{m+n}{1}a^{m+n-1}x$

$$+\frac{(m+n)\times(m+n-1)}{1\times2}a^{m+n-2}x^{2}+\frac{(m+n)\times(m+n-1)\times(m+n-2)}{1\times2\times3}a^{m+n-3}x^{3}+$$

&c. ad infin. or $(a\pm x)^{p}=a^{p}+$ $\frac{p}{1}a^{p-1}x+\frac{p\times(p-1)}{1\times2}a^{p-2}x^{2}\pm\frac{p\times(p-1)\times(p-2)}{1\times2\times3}a^{p-3}x^{3}+$ &c.

ad infin. in which $p=m+n$ which is the known formula for the expansion of a Binomial. Again, by dividing both sides of the equation $(a\pm x)^m = a^m \pm \frac{m}{1}\times a^{m-1}x + \frac{m\times(m-1)}{1\times 2}a^{m-2}x^2 \pm$ &c. ad infin. by $a\pm x$ after the manner of common division, I have $(a\pm x)^{m-1} = a^{m-1} \pm \frac{m-1}{1}a^{m-2}x + \frac{(m-1)\ (m-2)}{1\times 2}a^{m-3}x^2 \pm$ &c. ad infin. in which m, is changed into $m-1$, divide by $a\pm x$ again, and $m-1$ will be changed into $m-2$, and so on to $m-n$, if n denotes the number of divisions by $a\pm x$. hence I have $(a\pm x)^{m-n} = a^{m-n} \pm \frac{m-n}{1}a^{m-n-1}x + \frac{(m-n)\times(m-n-1)}{1\times 2}a^{m-n-2}x^2 \pm$ &c. ad infin. or $(a\pm x)^{-p} = a^{-p} \pm \frac{(-p)}{1}a^{-p-1}x + \frac{(-p)\times(-p-1)}{1\times 2}a^{-p-2}x^2 \pm \frac{(-p)\times(-p-1)\times(-p-2)}{1\times 2\times 3}a^{-p-3}x^3 +$ &c. ad infin. If $p = n-m$, which is the expansion of a Binomial in the case of the exponent negative, but integral, and it is of the same form, as in the case of the exponent positive, only p instead of being positive, is here every where negative. I will now proceed to powers, which are denoted by fractional exponents. To this end, I take $(a\pm x)^{\pm pq} = a^{\pm pq} \pm \frac{(\pm pq)}{1}a^{\pm pq-1}x + \frac{(\pm pq)\times(\pm pq-1)}{1\times 2}a^{\pm pq-2}x^2 \pm$ &c. ad infin. and $(a\pm x)^{\pm p} = a^{\pm p} \pm \frac{(\pm p)}{1}a^{\pm p-1}x + \frac{(p\pm)\times(\pm p-1)}{1\times 2}a^{\pm p-2}x^2 \pm$ &c. ad infin. $\pm p$ and q being both integers. These formulæ are manifestly true from what I have previously shown. Now $(a\pm x)^{\pm p}$ is the qth root of $(a\pm x)^{\pm pq}$ as is well known. Hence to derive

$(a\pm x)^{\pm p} = a^{\pm p} \pm \frac{(\pm p)}{1} a^{\pm p-1} x + \frac{(\pm p)\times(\pm p-1)}{1\times 2} a^{\pm p-2} x^2 \pm$ &c. ad infin. from $(a\pm x)^{\pm pq} = a^{\pm pq} \pm \frac{(\pm pq)}{1} a^{\pm pq-1} x +$ &c. ad infin. we must evidently divide $\pm pq$ wherever it occurs by q. Hence it appears, that if I have the equation $(a\pm x)^{\pm r} = a^{\pm r} \pm \frac{(\pm r)}{1} a^{\pm r-1} x + \frac{(\pm r)\times(\pm r-1)}{1\times 2} a^{\pm r-2} x^2 \pm$ &c. ad infinitum, and wish to obtain the s root of it, I must divide $\pm r$ wherever it occurs, by s, and that whether $\pm r$ be exactly divisible by s, or not. For as the same rule is used in the extraction of the s root of $(a\pm x)^{\pm r} = a^{\pm r} \pm \frac{(\pm r)}{1} a^{\pm r-1} x +$ &c. ad infin. as in the extraction of the q root of $(a\pm x)^{\pm pq} = a^{\pm pq} \pm \frac{(\pm pq)}{1} a^{\pm pq-1} x +$ &c. ad infin. and as the q root is had by dividing $\pm pq$ wherever it occurs, by q: therefore by analogy of process, the s root of $(a\pm x)^{\pm r} = a^{\pm r} \pm \frac{(\pm r)}{1} a^{\pm r-1} x +$ &c. ad infin. is had by dividing $\pm r$ by s, which gives $(a\pm x)^{\frac{\pm r}{s}} = a^{\frac{\pm r}{s}} \pm \frac{\left(\frac{\pm r}{s}\right)}{1} a^{\frac{\pm r}{s}-1} x + \frac{\left(\frac{\pm r}{s}\right)\times\left(\frac{\pm r}{s}-1\right)}{1\times 2} a^{\frac{\pm r}{s}-2} x^2 \pm$ &c. ad inf. which is yet of the same form as when the exponent is integral. Hence, universally, I have $(a\pm x)^u = a^u \pm \frac{u}{1} a^{u-1} x + \frac{u\times(u-1)}{1\times 2} a^{u-2} x^2 \pm$ &c. ad infin. in which u denotes any number whatever, whether integral or fractional, positive or negative, and the law of the formation of the successive terms is evident. In the case of u, positive and integral, all the terms after $+\frac{u\times(u-1)\times(u-2)\times \&c. \text{ to } (u-(u-1) \text{ inclusive}}{1\times2\times3\times \&c. \text{ to } (u-1) \text{ inclusive}}\times a^{o} x^{u}$, will equal nothing, for they all have the factor, $u-u=o$. The demonstration which I have here given, I believe is new and satisfactory.

Art. XIX.—*Notice of some recent experiments in boring for fresh Water, and of a pamphlet on that subject.*

The newspapers have, for some time, contained notices of the operations and success of Mr. Disbrow, in boring for fresh water, in New-Jersey, and elsewhere, and often in situations the most unpromising. The statements appeared, from the first, to be well authenticated, and the extraordinary success which has continued to attend Mr. Disbrow's efforts, has now drawn the public attention, so powerfully, to the subject, that they will probably receive with no little satisfaction, a connected account of the several attempts to obtain fresh water by boring. This account is contained in "An essay on the art of boring the earth for the obtainment of a spontaneous flow of water," &c. recently published in New-Brunswick. The author states that the practice of *boring* for coal and other minerals, has been known in Europe for the last fifty or sixty years, and that it has been applied, also, to obtain "a greater quantity of water in wells, that did not, at all times, afford a sufficient supply. It appears that a person, whose name is now forgotten, applied the art of boring to obtain salt water in our western States, the salt having been before obtained only in the salt licks. His first trial was made in boring from the bottom of a deep well. Salt water was obtained; and it being found, that it rose in a tube, nearly or quite to the surface, an attempt was made to bore without the aid of a well. The effort was successful, and water was obtained at the depth of 70 or 80 feet, which rose and overflowed at the surface.

The boring was, afterwards, in the progress of these enterprises, prosecuted in situations, where the indications of salt were less distinct, and the auger was sunk to the depth of 400, 500, 700, and even 900 feet. By shutting out the fresh water springs, salt water was often obtained from great depths, where otherwise it would not have been procured,—and thus, by degrees, it became as common to bring salt water above the surface, as it was formerly to dig wells. "In this state the art rested, in America, until the year 1823, when Mr. Levi Disbrow, after seeing the operation of boring in the western States, formed the project of bringing *fresh water* above the surface, in New-Jersey."

We will now cite some of the principal facts relating to Mr. Disbrow's operations.

1. At the distillery of Mr. Bostwick, at Brunswick, New-Jersey, at the depth of 175 feet, and 40 above the Raritan river, good water was obtained, which for $2\frac{1}{2}$ years has discharged $1\frac{1}{2}$ gallons a minute, at three feet above the ground. At the depth of 137 feet, the water overflowed the surface, but the boring was continued to the depth of 175 feet, to increase the quantity. The strata perforated, were chiefly old red sandstone, with occasional thin strata of slate or gray wacke, and at long intervals blue clay occurred. By tapping the tube four feet from the top, and inserting a tube half as large as the main one, a copious supply of water, at the rate of $4\frac{1}{16}$ gallons the minute was obtained, for a distillery situated on lower ground, while, from the top of the tube, the water flows at the rate of $1\frac{1}{2}$ gallons the minute, to supply a house, stables, a milk-house,* &c. The temperature is 52° Fahr. and the cost was $425. The water in Brunswick is stated to be generally brackish, and disagreeable to the taste. This boring was commenced in May, 1824.

2. This was commenced August 7, 1824, about one mile from the preceding, and 47 feet above the level of the Raritan—passed 60 feet through "soft red shell," (red sandstone) when they struck the first vein of water. At 110 feet they encountered a rock "harder than granite." It was about four feet in thickness, and in the centre of it the poles of the borer became so magnetized, that a heavy jack-knife could be suspended from them. The magnetizing stratum seemed to be about four inches in thickness. Water was generally found either in the stratum immediately above, or immediately below the red shell. Clay was not abundant until towards the end of the boring, when they perforated four or five feet of blue clay, and at the depth of 394 feet struck a vein

* Franklin Magazine, Vol. II. p. 35.

of water, which immediately overflowed at the top, and on the 14th of Nov. 1826, was discharging two gallons a minute, at the height of two feet eight inches from the ground. This well is tubed to the depth of 194 feet, with a copper tube of 1½ inches in diameter. There was an ebb and flow of water in this well until the winter of 1824, when it was thought necessary to bore it deeper, and this appearance ceased, the copper tube that was introduced having probably shut out the particular vein of water that caused it. The temperature of this spring at the depth of 250 feet was 52° Fahr.; at 394 ft. it is 54°; which is supposed to favour the idea "that the temperature increases as we get more into the interior of the earth."

3. Simpson's distillery—perforation 176 feet—cost 440 dollars—tubed 18 feet—discharges two gallons a minute—temperature 52°—begun Nov. 1824, finished Feb. 1825—strata, 7 feet clay, 60 red shell, then water—at 140 feet struck a thin stratum which magnetized the poles—After this they perforated gray granite* for a foot or two, and then the red shell.

4. New Brunswick, 10 feet above the Raritan, through made ground 12 feet—then red shell 208--work left unfinished by Mr. Disbrow, it being his own property, and he being too much engaged to prosecute it—but the water, in small quantity, rises above the surface.

5. Jersey City, opposite to New-York, surrounded by salt water, began April, 1825, in a well of very brackish water, 24 feet deep. Perforation 208 feet in rock, (*granite* it is named;) after descending 146 feet, struck several small veins of water. In November, 1826, they were still boring; water ran over the tube 21 inches, without machinery, and it was said, that, by a pump, three gallons a minute of excellent water, could be raised.

6. Alexandria. Bored about 250 feet, when water rose within 36 feet of the surface: the boring was continued to

* As this country belongs to the old red sand stone formation, we would inquire of those who have opportunities to observe, whether the granite mentioned so often in these accounts, was really a true granite, or only a sand stone composed of the materials of granite, which might well produce a deception. It does not appear probable that granite should be found, except as the foundation rock of the whole series, supposing that the perforation penetrated quite to the bottom of the secondary rocks. Much less does it appear probable that granite and red sand stone should alternate in the manner here described, and still less that clay should be found below granite, as it is described to be in the second account in the pamphlet.—Ed.

the depth of 450 feet, when the work was suspended, to be again resumed.

7. Hudson, 70 feet above the river. Began to bore May, 1825; went down 144 feet through clay, 40 feet sand, 30 hard pan, and gravel 4; then struck a vein of water, which rose 17 feet—then through 70 feet of slate, and the work still going on in Nov. 1826.

"8. North side of the Raritan, half a mile from New-Brunswick; 80 feet above the level of the Raritan. Began in 1825, in the bottom of a well that was 48 feet deep, and quite dry; 60 feet of red shell; struck a vein of water which gave eight feet water to the well; indications of coal; red shell, and now and then thin strata of gray wacke, to the depth of 250 feet, when water came above the surface, discharging one and a half gallons at the height of seven feet. Cost about 400 dollars, and is tubed 100 feet, with one and a half inch copper tube, connected at intervals of twelve feet, with brass screw joints. These copper tubes cost 50 cents a pound; and the screws are 75 cts. a piece.

"9. Albany. Began May, 1825. Boyd & M'Culloch, Brewers. Level 20 feet above the Hudson. Began in a 30 feet well; 11 feet of coarse gravel and hardpan; 41 feet black slate rock; very little water until after passing a depth of 200 feet of same rock; struck several small veins. At 250 feet encountered inflammable gas. Now 280 feet deep, and water within four feet of the surface.

"10. Albany. Water Works Company. Began in August, 1825. Twenty feet of clay, sand, and gravel; 17 feet hardpan; struck a vein of water which instantly overflowed at the surface, at the rate of five gallons a minute. The diameter of the bored hole of this spring was eight inches, whereas all the previous ones were from two and a half to two and three-quarter inches in diameter.

"11. The same Company are now boring back of the Capitol, in Albany, on very high ground, and are now at the depth of 120 feet in hardpan.

"12. New-York. Manhattan Company. Began Sept. 1825. Level 40 feet above the Hudson. Bored 40 feet in sand and gravel; 60 feet in hard granite; from that depth to 240 feet, occasional veins of water; increasing in quantity as the depth increases; water, when a tube is introduced, now within 30 feet of the surface; discharges 12 gallons a minute, by means of a pump. When down to the depth of

260 feet, the chisel passed a vacuum of an inch; the water immediately sunk one foot; but in the course of a few hours, when the cavity was filled, the water rose again. This has been the case in several wells; boring still continuing.

"13. Harper's Ferry. Began June, 1825. Level 12 feet. Very high mountains in the rear. Fifty feet of soft gray slate; 50 feet of granite, but not very hard; occasionally quartz and granite. At 110 feet, touched a fine vein of water: some quartz and granite at the depth of 264 feet. Discontinued for the present, but is to be resumed.

"14. Baltimore. Mr. Bosley, two miles west of the city. Seven feet common soil: 33 feet rotten gray rock; 140 feet of the same rock, only harder; struck a vein of water, which arose to within 22 feet of the surface. Some mischievous person having at this time thrown in a piece of steel, it cost a great deal of time, labour, and money to cut it up, it being of 30 inches in length. The boring is going on, and much praise is due to Mr. Bosley for the liberal spirit he has shown in the prosecution of this work, under so many discouragements.

"15. Horsimus. Eighty feet above the level of the Hudson. Mr. Haight's Carpet Manufactory. Began June, 1825, in a well 30 feet deep, which did not give sufficient quantity of water, as the machinery was idle one day in six. Bored 30 feet in sand, gravel, and hardpan. Got plenty of water.

"16. New-Hope. June, 1825. Sixty feet level. Eighty feet red shell, 110 limestone. Still going deeper.

"17. Philadelphia. Matthew Carey, Esq. Began in a well 48 feet deep, quite dry. Thirty-eight feet of hard granite, and brought 4 feet of water in the well. Object obtained. Granite, therefore, in this case, as well as in the spring No. 5, in Jersey City, is intersected by veins of water.

"18. Northern Liberties, Philadelphia. Level 40 feet. Forty-five feet sand and gravel; 145 feet gray granite rock, only varying from soft to hard. Struck several veins of water in this granite. Water at present within twenty-five feet of the surface.

"New-York. Corner of Rivington and Columbia streets. Commenced August, 1825. By degrees sunk a cast-iron tube, and began in a well 20 feet deep. Ten feet quicksand; 20 feet marsh, mud, and clay; ten feet gray clay; stopped at granite. All the upper brackish water is excluded by the tube, and have now a fine flow of water from the junction of clay and granite. This water is excellent, and is pumped up by a small pump.

"20. New-York. Broad-street. Mr. Tunis Quick. Four feet common made ground, or street filling, of the consistence of mud; 6 feet of yellow clay; 19 feet gravel and quicksand; 10 feet of gray clay. Came to granite, fastened the cast-iron tube, and water rose to within 7 feet of the surface. Put in a pump; object obtained.

"21. New-York, corner of Avenue D, and Fifth-street Bored through 6 feet of common street filling; 10 feet marsh mud; 65 feet of quicksand and gravel; 15 feet of gray clay. Came to granite, and obtained fine water. Tubed 96 feet. Bore 8 inches diameter. Water within 4 feet of the top.

"22. Dry Dock Company, corner of Avenue D, and Tenth-street. Six feet common filling; 10 feet marsh mud; 12 feet quicksand; 53 feet of common shore sand and gravel; 6 feet of hardpan; 3 feet of coarse gravel. Stopped at the granite, and got plenty of water within 4 feet of the surface.

"23. Newark Meadows. Grazing Company, at the junction of Belleville and Newark Turnpike, on the *salt marsh*. Ten feet of marsh, mud, and roots; 12 feet of fine quicksand; 36 feet blueish gray clay; 6 feet sand; 20 feet of ash coloured clay. Met with water: bored 20 feet farther in very stiff variegated clay, on the reddish cast. Touched the red free stone, and stopped. Water excellent and soft, and within three feet of the surface. The reclaiming of salt meadows has been a great object obtained. The reclaiming of the fresh water springs is of still higher importance.

"24. Allen-street, near Hester-street, New-York. Began in a well 40 feet deep, that had about two feet of brackish water in it. Commenced in quicksand: bored 20 feet: now and then a stratum of gravel; 2 feet clay, 5 feet of coarse gravel and sand. Obtained water, as is always the case, before coming to the solid rock. All the waters of these last mentioned wells are like the water of the old *Tea Water Pump*."

Whatever may be thought of the peculiar hypothesis exhibited in the pamphlet, from which we have partly abstracted and partly copied the above details, all will admit, that the facts have a very important practical bearing. If we need more facts, before we can fully admit the position taken by the author of the pamphlet, that fresh water may be obtained by boring *in every place*, the facts already ascertained do certainly countenance this opinion—especially if we are to suppose, what candour must presume, that *all* the results have been published, and that unfavorable trials are narrated, as well as those of an opposite character.

There can be no doubt that the research ought to be prosecuted with vigor and perseverance, and especially in the most unfavorable circumstances. If, for instance, as in some cases cited in this notice, good water, in abundance, can be brought near to the surface, from beneath the filth and putrescence of a great city; its corporation and its citizens ought, if necessary, to deny themselves their luxuries, and even a part of their daily bread, to procure what is next in importance to bread itself. New-York would then have no longer to regret, that a Schuylkill does not flow at her doors, and that the magnificent waters which wash her quays are sacred to the finny tribes and to a boundless commerce.

A remarkable example, in confirmation of the views of Mr. Disbrow, and of his historian, occurred in a port on the British side of the English Channel a few years since.*

They were boring the rocks for the purpose of deepening the channel, when suddenly a jet d'eau darted through the sea, and, on fixing a tube in the hole, the water rose quite above the waves. It ebbed and flowed with the tide, but was quite fresh and pure, and answered, afterwards, for watering the shipping of the port, it being necessary only to bring the boats, with their water casks, along side.

The circumstances leave no doubt that aerial agents, acting under pressure, in cavities, were in this case the moving power; and the geysers of Iceland and the spouting springs of the Azores, fully prove, that the flow of water in jets, is not always owing to gravitation, and that aerial agency may be sufficent for any effect of this kind. The ground taken up by the writer of the pamphlet is therefore not altogether visionary. Still, with this admission, we should not be prepared to say, that aerial agency, capillary attraction, centrifugal force†, or gravitation itself, is the uniform agent in raising water to,

* We quote from recollection, but are confident that we are substantially correct. The facts are related, if we mistake not, in Tillock's Philosophical Magazine.

† As suggested by a writer in Vol X. p. 395, of this Journal.

and above the surface. If the general agent be gravitation, other causes may also operate. We cannot doubt that aerial or gaseous agency is much concerned in the phenomena at least of brine springs, and this agent often converts these springs into boiling fountains, where inflammable gas is constantly evolved, and being kindled, burns with brilliancy, and often for a long time.

There can be as little doubt that gaseous and aerial agency is the great power, that in volcanoes, raises the masses of semi-fluid lava, through thousands of feet in altitude,—that eventually ruptures the mountain—that projects showers of ignited stones and rocks—that tears asunder the solid strata, shakes the mountains from their foundations, and causes the globe itself to vibrate and tremble.

POSTSCRIPT.

Since making the above abstract, the following extract of a letter from Mr. SAMUEL HEPBURN to Mr. D. D. CHESNUT, dated *Milton, (Penn.) Jan.* 31, 1827, has been communicated to the editor.

"The Messrs. Pollocks have been entirely successful in their search after water, and at a very moderate expense,—some $60. The time consumed in boring was about thirty days. The well, or hole, is within the walls of their distillery, at the foot of the "Red Hill." They commenced boring in the bottom of a well they had formerly dug, to a depth of ten feet from the surface, and procured a supply sufficient for their purposes at a depth of 110 feet—that is, at 120 feet from the surface. The 10 feet well was sunk at a spot where a weak spring issued from the hill. The diameter of the hole is three inches: It now yields one and a half gallons of water per minute, and rises eight feet in the ten feet well, the diameter of which is from 8 to 10 feet. The water is very soft, and free from any impregnation of lime, which is so much the case with the well water generally in this neighborhood. The hole is sunk at least 80 feet below the bottom of the river, from which it is distant some 400 yards. The boring, as Mr. Pollock informs me, was entirely through the red slate rock, that mainly composes the hill—precisely the "saliferous rock" of Professor Eaton, which is stated to constitute "the floor of all the salt springs in the western country." (*Query.*—Might salt water be had by boring something deeper ?) The "red hill" rises 80 to 100 feet above the foundation of the distillery, and exhibits the rock on its summit, and on its southern side, especially, for a distance of two or three miles. Its course is east and west. On the northern side of the hill the red rock is found to rest on a hard, compact limestone, and water is here found, at a depth of from 20 to 30 feet—springs "never-failing," but weak, are found among the ridges. The more slaty part of the rock disintegrates rapidly on exposure, but much of it is found to resist the action of the atmosphere, and to constitute a good building stone.

New Mineral Springs.—The apparatus of Mr. Disbrow, which has been so usefully employed here and elsewhere in boring for fresh and pure water, has been recently used by some gentlemen of Ballston Spa, in several parts of that town. The Ballston Gazette informs, that, "after penetrating the earth to the depth of 80 feet, they found mineral water, which was surcharged with the carbonic acid gas, and contained all the mineral properties of our other fountains, and rose freely from four to five feet above the surface, discharging at the rate of about a gallon per minute. They have penetrated the earth to the depth of 170 feet, and are now in a magnesian calcareous slate rock ; and although they have discontinued operations for the present, or during the cold weather, they intend to commence again on the opening of the spring. There is every prospect of their finding a *saline water*, as at the depth of 80 feet it contained 240 grains of salt to the gallon. [*New-York Times.*

The expense of boring is, on an average, not more than 2½ dollars the foot, except when granite is encountered, when, perhaps, the chisel cannot penetrate more than three or four inches in a day.

Art. XIX.—*Notice of various facts relating to Palestine;* in a letter from the Rev. Isaac Bird to Prof. Hall.

TO PROFESSOR SILLIMAN.

The following is an extract of a letter, written in reply to a communication, addressed by me to the late Reverend Pliny Fisk, Missionary to the Holy Land, in which were a number of inquiries, relating to the Geology and Mineralogy of that most interesting country. If you deem it worthy of a place in the American Journal of Science and Arts, it is at your service. Yours truly. F. Hall.

"As to the garden seeds, we are sorry to say, that since our departure from America we have never, until a few weeks since, had a foot of land at our disposal, and that which is now secured to us is not convenient in itself, nor in its vicinity to our present dwellings. The late Greek invasion has interrupted our plans with regard to it. The suburbs in which we live are, for a mile or two in extent around the city, one continued orchard of mulberry trees, for the production of silk. We sometimes call these suburbs gardens; but, although a few garden vegetables are raised in them, yet scarcely any thing can be found exhibiting the aspect of a real American garden.

"The few questions you suggested to Mr F., had helived, would no doubt have been decisively and correctly answered. In regard to the sepulchre of our Saviour, I am sure Mr. F. had strong doubts whether it had ever been identified, the place now shewn as such, being from top to bottom, entirely an artificial structure, exhibiting not the least vestige of the original rock in which the sepulchre was hewn. But I do not think he had the smallest leaning to the opinion of Dr. Clarke. You wish a specimen of the rock in which are the sepulchres of the Hill of Offence. I have none by me, but have no doubt it is a soft limestone, as are the "Sepulchres of the Patriarchs," as they are called by Dr. C. Respecting these sepulchres, you inquire whence came the marble of which they are composed? I answer, the marble, if marble it can be called, was furnished on the ground itself, and remains just where nature placed it. All that art has done is, to cut away the surrounding rock. Accordingly, instead of the fine surface which we might have expected, had they been of Grecian marble, we find them of a dull, dirty color, and rough from decomposition.

"The rocks of Carmel and Tabor are doubtless calcareous, as you suggest: I have visited only the latter. Those of the banks of the Dead Sea are deeply tinged with iron, which gives the appearance of having been burned. But it is really singular, that any traveller should ever have suggested that there is any appearance of a volcano in that region. Certainly there is none on the *western* shore. The mountain to which Dr. C. has alluded must have been *Frank Mountain*, or Mount Ferdees, as the neighbouring Arabs call it. From Bethlehem it has precisely the appearance which the Dr. ascribes to it. The Dead Sea seems "*very near*," when viewed from Bethlehem, as the same author observes; therefore, this mountain, situated in the same direction, must have appeared to him as near the western shore of that sea. But instead of being *actually* near it, the distance between it and the sea must be at least 12 or 15 miles. Mr. Fisk and I visited it together, and found not the most distant indication of its ever having been a volcano. You may find a description of it in Dr. Pococke, under the name perhaps of the "Mount of Bethulia." It is supposed also to be the "*Herodium*" of Josephus. (See B. 14, c. 13, sec. 9. and B. 15, c. 9. sec. 4.) It is about 4 or 5 miles from Bethlehem.

"The tombs of the last Jewish Sanhedrim are not the same as those of the Kings. The former are a mile and a half from Jerusalem. Mr. F. visited them in the spring of 1823; went into six different apartments, and counted 63 coffins or places of deposit for dead bodies, when, his light failing, he returned. The Jews say there are just 72 coffins in these tombs. The cave of David is natural. Mr. F. and myself also visited this place together. The only apartment we entered was 80 or 90 feet long, 40 broad, and perhaps 20 high. We discovered passages leading off in various directions, and were told they terminated in other apartments, but they were too narrow and dusty to be comfortably explored. The dust spread over the floor of the cave some inches in depth, and so filled the air from our walking in it, that we were much annoyed by it. You remark in the pamphlet, that Burckhardt saw on Mount Lebanon "*one fossil shell*." You may be interested to know that the rocks of some of the high peaks E. of Beyroot, the highest that I have visited, are apparently made up of little else than vegetable and shelly remains. Of these shells, I some time since forwarded a few specimens, in a box with other minerals, to Prof. Silliman.*

If, my dear sir, these remarks are sufficiently definite to be useful to you, I shall rejoice in having had it in my power to oblige a friend and correspondent of our dear departed brother, and in whatever way we can serve you in any future inquiries that may suggest themselves, we beg you will make use of us without apology.

Yours, truly. ISAAC BIRD.

ART. XX.—*Bitumen, and other volatile ingredients, in Stones.*

IN the analysis of stones, a considerable per centage is often set down as loss. The progress of chemistry has constantly tended to diminish the amount of loss, by showing that certain principles, usually either soluble or volatile, had been overlooked. It was long, before fluoric acid and the fixed alkalies were discovered in minerals, and lithia has recently been added to the catalogue of their constituent principles.—The existence of bitumen in stones has been a good while known, but it seems to have been reserved for a late writer† to prove, that this substance exists in many stones, in most of which its presence had not been even suspected. Mr. Knox ascertained the loss of weight by igniting the powdered substances in a platina crucible, and discovered the nature of the volatile parts, by distillation in an iron retort.

* For a notice of them, see Vol. X. p. 21, of this Journal.—ED.

†The Right Hon. George Knox, in the Philosophical Transactions for 1823. The paper on this subject was forwarded by Dr. Wm. Meade to the Editor.

SUBSTANCES.	LOCALITY.	*Loss by ignit.*	PRODUCTS ON DISTILLATION.	CHANGE OF APPEARANCE AND RESIDUUM IN THE RETORT, WITH REMARKS.	
1. Pitchstone	Arran	5 per cent.	4.5 parts water and bitumen	like soft pumice—articulated	colour passed from ash-gray to reddish-white
2. Pearlstone		3.25	2.5 do. do. floating	like basalt—white	
3. Pumice				do. do.	melted without loss
4. Amygdaloid	Disco Island	3.25	3.1 bituminous water		
5. Basaltic Greenstone	Near Newry	6.25	1.5 pure bitumen		the water having been expell'd by a heat below redness
6. Transition do.	Carlingford	2.	1.5 bitumen nearly pure		
7. Bole		24.5	— bitumen saline acid and ammoniacal		bitumen lost by accident
8. Basalt	Giant's Causeway	6.051	6. bitumen and water	pumice—like No. 1	
9. do.	Disco Island		2.312 do. do.	do. do.	[tort's neck
10. Obsidian		1.75	0.2 bituminous ammoniacal water	like glassy pumice	a piece sublimed in the re-
11. Greenstone	Clack Hill	2.	2. bituminous water, chiefly bitumen	* velvet black—powder not changed at melting—heat of cast iron	partly volatile at 98, blood heat, through glass
12. Wacke	Disco Island	19.4	11.42 do.—4 sub. in. carb. acid & do. carb'ed [hydrogen		
13. Iron clay	do.	21.	18.25 bituminous water		ignited under charcoal, gave a black chalk
14. do.	Howth	5.	4. bitumen and a little water	mass like pumice	
15. Bole	Disco Island			results like No. 7, but no ammonia	distilled in a coated green glass retort
16. Hornblende	Upper Saxony	3.	.75 bituminous water, some of which was lost		
17. Tourmaline	Karolulike, Green		0.7 do. do.	like pumice—white	the stone velvet black
18 Augite	Arendahl	0.5	0.35 do. do. chiefly bitumen	a powder	heat not great
19. Common serpentine	Zoplitz	12.5	10.5 do. do.		
20. Clay slate	Bangor	3.25	3. do. do.	coherent, not hard	broke like No. 1
21. White felspar	Killiney	0.40	0.35 bituminous ammoniacal water		
22. do.	do.			distilled in a coated green glass retort	same result, but no ammonia
23. do. flesh red	Aberdeen		do. without ammonia		[came black
24. Menilite	Menil Montant	1.25	3.75 bituminous water with a little ammonia	do. do.	ignited under charcoal, be-
25. Adhesive slate	do.		18. water and bitumen		bitum. yellow & supernatant
26. Mica slate		2.	volatile bituminous water—no ammonia	a heavy pumice	broke like No. 1
27. Mica silver white			1.33 bituminous water—traces of ammonia	slightly coherent	
28. Fetid quartz		0937	do. do.—like naptha	no ammonia nor sulphuretted hydrogen	lost its odour
29. Fat quartz		1.	do. do.—very fetid		
30. Rock crystal		0.		powder unaltered in colour	unchanged
31. Adularia		0.			
32. Pearl blue	Greenland	0.4		powder snow white	
33. Marble	Carrara	0.15	water		
34. Lucullite	Galway	0.188	alkaline—acid and oily exhalations	* This mark refers to No. 12.	

All these substances, except rock crystal and pearl white adularia, scintillated more or less when thrown into boiling hot nitre.

From these curious and interesting experiments, Sir George Knox infers, that the bitumen, being similar in smell, colour, and volatility, existed previously in the stones, and was not formed by the fire, although he admits this to be possible; he thinks the ammonia* a product, and not an educt. He inquires whether the conversion of obsidian into pumice does not justify the arrangement of those two substances together; whether volcanic fires may not have their origin in rocks of the flœtz trap formation, since bitumen, or a volatile inflammable oil, is combined with them all. He thinks the appearance of inflammable matter in mica and slate remarkable, and that it is equally so that rock crystal and adularia are destitute of this principle. He infers from the scintillation of the powder of stones with nitre, that carbon may be much more extensively diffused through the mineral kingdom than has been imagined, and suggests that distillation should be a regular part of analysis—that "*loss*" ought not, of course, to be charged to water, as bitumen and carbon are now proved to be often present, and that the residuum in the retort ought to be examined for carbon, either by burning it off, or in some other way.

Art. XXI.—*Notice of a Meteorological Register for the years* 1822, 1823, 1824, *and* 1825; from observations made by the Surgeons of the army, at the Military Posts of the United States.

We have been favoured by Dr. Lovell, surgeon general of the United States army, with this able and important document. It is the result of very numerous observations, made under favourable circumstances, at fixed stations, over an extent of country embracing nearly 20 degrees of latitude, and almost 30 of longitude.

The scientific world will feel much indebted to the enlightened policy which dictated this course of observations, constituting the best basis for general conclusions re-

* Are we to infer then that nitrogen exists in these stones, or must we impute this element of the ammonia to atmospheric air in the pores or vessels? Editor.

specting the climate of the United States hitherto published.*

It is to be hoped, that, agreeably to Dr. Lovell's suggestion, the observations will be continued, until a fair average shall be obtained, of all those variations to which our climate is liable.

It is justly observed, in the remarks prefatory to the Register, that the question so often agitated, whether the climate of a country is permanently affected by the progress of cultivation and population, can be more satisfactorily settled in the United States than perhaps in any other country. "For here, within the memory of many now living, the face of whole districts of country has been entirely changed. And in several of the States two centuries have effected as much as two thousand years in many parts of Europe. In this respect, the "landing of the pilgrims" in 1620 is as remote a period as that of the invasion of Gaul, or of Britain by Julius Cesar."

In this register, "the first twelve tables for each year give the mean of the observations at the several posts for each month, and the thirteenth the mean for the whole year. The last or general table gives the average of all the observations at the several stations, and also the average for the several years.

This last or general table we shall copy, as it is the most important, and presents results exceedingly interesting and instructive.

Climate is influenced as much by elevation above the level of the sea as by latitude. It is remarked that the elevation of the north-western or interior stations over those on the Atlantic coast, has not been accurately ascertained, but the following are given as approximations :—

Name.	*Situation.*	*Feet above tide water.*
Fort Brady,	Outlet of Lake Superior,	595
Fort Howard,	South end of Green Bay,	600
Fort Crawford,	Prairie du Chien,	580
Fort Snelling,	Junction of St. Peters and Mississippi,	780
Council Bluffs,	near junction of Platte and Missouri,	800

* Dr. Bigelow's interesting memoir on the time of the flowering of our principal fruit trees afforded very important and accurate means of judging of the actual temperature. Such observations should be multiplied. See the 1st vol. of this Journal.

It is remarked that although these observations may not be, in every instance, accurate, they are probably sufficiently so for the purpose of general abstracts—"for, the mean of each month being deduced from 90, and of each year from 1095 observations, occasional errors would not materially affect the result."

"In order to ascertain the means for the several years, as given in the last table, the extreme stations are taken, and as many intermediate ones at the north and south respectively, as are found to be equi-distant from them, or nearly so. The aggregate of these should give the mean of the centre of the district of country in which the observations were made, and the result appears to be near the truth. For the latitude of the city of Washington is 38° 53′, and the average of mean temperature, is 56° 56′; the centre of the several stations at which these observations were made, is in latitude 38° 13′, and the average mean temperature is 56° 62′.

Besides the general table, with which we shall conclude this article, Dr. Lovell has given another, shewing the comparative temperatures of places in Europe and America, in nearly the same latitude, by which it appears that the temperature is higher in Europe, especially in the higher latitudes.

PLACES.	North lat.	Longitude.	Mean Temp.
Petersburg	59 56	30 24 E.	38 80
Stockholm	59 20	18 00 E.	42 39
Edinburg	55 57	3 00 W.	47 70
Berlin	52 32	13 31 E.	49 00
Leyden	52 10	4 32 E.	52 25
London	51 31	- - -	51 90
Rouen	49 26	1 00 W.	51 00
Paris	48 50	2 25 E.	52 00
Vienna	48 12	16 22 E.	51 53
Nantes	47 13	1 28 E.	55 53
Poitiers	46 39	0 30 E.	53 80
Fort Brady	46 39	84 43 W.	41 37
Padua	45 23	12 00 E.	52 20
Fort Snelling	44 53	93 08 W.	45 00
Bourdeaux	44 50	0 26 W.	57 60
Fort Sullivan	44 44	67 04 W.	42 44
Fort Howard	44 40	87 00 W.	44 50
Marseilles	43 19	5 27 E.	61 80
Fort Crawford	43 03	90 53 W.	45 52
Fort Wolcott	41 30	71 18 W.	51 02
Council Bluffs	41 25	95 43 W.	50 82
Pekin	39 54	116 29 W.	55 50
Washington	38 53	76 55 W.	56 56
Algiers	36 49	2 17 E.	72 00
Fort Johnston	34 00	78 05 W.	66 68
Cantonment Clinch	30 24	87 14 W.	68 77
Grand Cairo	30 00	31 23 E.	73 00
St. Augustine	29 50	81 27 W.	72 23

From this, it appears that in the higher latitudes, the average difference for the same degree of mean temperature, is 14° 30′, and in the lower ones 7° 30′, the mean of which is 11°. Thus the mean temperature at Stockholm, in latitude 59° 20′, is about the same as at Fort Sullivan, in latitude 44° 44′; while that at Rouen, in latitude 49° 26′, is about the same as at Fort Wolcott, in latitude 41° 20′; and at St. Augustine, in latitude 29° 50′ it is but 0.77 lower than at Grand Cairo, in latitude 30°.

AVERAGE OF THE OBSERVATIONS AT THE SEVERAL POSTS.

PLACES OF OBSERVATION.	THERMOMETER. Mean Temperatue. VII.	II.	IX.	Aggregate of Mean temp.	Highest deg.	Lowest deg.	Range.	WINDS. N. Days.	NW. Days.	N. E. Days.	E. Days.	S.E. Days.	S. Days.	S. W. Days.	W. Days	Prevailing.	WEATHER. Fair Days.	Clou Days.	Rain Days.	Snow Days.	Prevailing.
Fort Brady	36.69	49.06	38.38	41.37	90	–33	123	1.74	4.77	1.05	2.24	7.24	2.60	2.27	8.24	W.	13.30	3.27	7.83	6.02	Fair.
Fort Snelling	39.96	52.34	42.70	45.00	96	–29	125	2.88	7.13	2.33	1.16	4.02	3.52	6.05	3.24	NW.	16.94	5.50	5.77	2.22	Fair.
Fort Sullivan	38.26	49.61	39.66	42.44	94	–19	113	3.26	6.89	2.04	2.08	0.79	7.02	3.68	4.77	S.	17.91	9.39	2.31	0.81	Fair.
Fort Howard	37.81	52.98	42.71	44.50	100	–38	138	0.70	0.70	11.52	0.19	0.08	0.39	16.04	0.78	SW.	15.47	7.98	4.56	2.42	Fair.
Fort Crawford	39.06	53.92	43.58	45.52	96	–28	124	5.58	7.12	1.04	0.29	4.12	5.70	3.04	1.58	NW.	16.80	6.29	3.87	1.33	Fair.
Fort Wolcott	48.54	56.39	48.14	51.02	88	–1	89	3.04	6.54	3.37	0.66	2.68	2.00	10.06	1.83	SW.	15.31	8.16	5.94	1.02	Fair.
Council Bluffs	44.22	60.14	48.11	50.82	108	–21	129	5.89	4.52	2.12	1.83	4.02	7.39	3.06	1.60	S.	19.68	6.54	2.95	1.25	Fair.
Fort Columbus	48.37	59.77	50.34	52.82	104	–3	107	0.72	9.02	3.49	0.87	4.04	3.91	6.29	2.06	NW.	20.41	3.56	5.47	0.98	Fair.
Fort Mifflin	51.68	63.31	50.85	55.28	96	6	90	0.50	6.37	4.54	0.74	6.20	1.24	8.20	2.62	SW.	21.20	5.12	5.20	0.41	Fair.
Fort Severn	53.40	62.11	56.70	57.40	92	8	84	3.08	6.00	4.00	2.00	3.33	6.91	2.16	2.33	S.	19.67	4.50	5.08	1.17	Fair.
Washington	52.23	62.18	55.65	56.56	95	10	85	2.62	7.47	4.97	1.05	3.19	2.66	7.63	0.74	SW.	17.30	6.05	6.44	0.63	Fair.
Fort Johnston	63.98	69.38	66.68	66.68	92	26	66	8.79	3.29	1.31	1.60	0.64	8.97	1.56	4.24	S.	16.87	7.60	5.85	0.12	Fair.
Fort Moultrie	61.93	67.81	63.75	64.49	92	19	73	1.78	1.15	6.85	3.80	5.59	5.07	4.41	1.73	NE.	22.89	2.48	5.00	0.02	Fair.
Canton. Jesup	61.83	74.89	68.22	68.31	97	7	90	2.38	2.99	4.38	3.80	7.05	3.28	4.55	1.97	S E.	18.63	4.49	7.25	0.05	Fair.
Baton Rouge	62.87	74.54	66.82	68.07	99	18	81	4.58	3.00	2.50	2.67	5.00	4.84	4.75	3.08	SE.	20.16	4.08	6.16	-	Fair.
Canton. Clinch	64.43	74.12	67.77	68.77	95	11	84	2.05	4.10	4.13	1.47	7.11	2.05	8.67	0.80	SW.	18.69	2.27	9.46	-	Fair.
St. Augustine	70.94	74.46	71.29	72.23	94	42	52	1.08	2.91	12.50	1.75	7.50	0.75	2.50	1.41	NE.	20.66	3.91	5.83	-	Fair.
Canton. Brooke	68.64	79.05	69.42	72.37	92	40	52	0.16	4.00	7.08	3.00	4.58	2.83	6.25	2.50	NE.	18.16	3.91	8.33	-	Fair.
Average of the several years 1822	52.25	63.47	55.48	57.06	108	–29	137	5.07	4.93	2.67	1.71	3.39	4.60	4.95	3.10	N.	18.90	5.03	5.63	0.85	Fair.
1823	51.26	60.68	53.72	55.22	100	–38	138	3.15	3.85	4.84	1.65	4.10	3.19	7.22	2.29	SW.	16.48	6.16	5.98	1.77	Fair.
1824	51.27	61.22	53.90	55.56	96	–33	129	3.85	4.65	2.84	1.79	3.83	5.33	4.73	3.40	S.	17.55	5.03	6.29	1.49	Fair.
1825	54.48	63.56	56.78	58.27	102	–25	127	3.23	4.81	4.72	1.45	5.52	2.79	3.09	3.24	SE.	16.91	5.67	6.49	1.32	Fair.
AVERAGE	52.31	62.24	54.97	56.52	108	–38	146	3.82	4.56	3.77	1.65	4.21	3.98	5.00	3.01	SW.	17.46	5.47	6.10	1.36	Fair.

It would be desirable that the different stations should be furnished with good barometers and rain and snow gages, that observations made with these instruments may be added to the others.

INTELLIGENCE AND MISCELLANIES.

I. DOMESTIC.

I. *Notice of native Iron from Canaan, Conn*—We are informed by Mr. Wm. Burrall, in a letter, dated August 16th, 1826, that his father was surveying a piece of land on the mountain, about three years since, and by accident noticed a black vein in a quartz rock; he pounded upon it some time with a stone, and with considerable difficulty got out two small pieces, the largest of which is in our possession. He has never been at the place since; and probably no other person has ever discovered it, or knows where it is. It is surrounded by woods one or two miles on every side; and is on the top of a mountain 700 or 800 feet above the common average of the land in the town. Mr. Burrall says there is evidence in that quarter of masses of iron, or its ores, of considerable extent, as his compass was materially affected; but the particular vein from which he obtained the pieces, appeared to be of no great extent; and the width of it is the same as that of the piece in our possession, which measures two inches wide, and two thick. It weighs eight ounces.

The following notice of the same facts has been received from Mr. C. A. Lee.

Native Iron, on Canaan mountain, a mile and a half from the south meeting-house. This is particularly interesting, as it is the first instance in which native Iron, not meteoric, has been found in America. It was discovered by Maj. Burrall of Canaan, while employed in surveying, several years ago. Ii formed a thin stratum or *plate*, in a mass of mica slate, which seemed to have been broken from an adjoining ledge. It presents the usual characters of native Iron, and is easily malleable. For some distance around the place where it was found the needle will not traverse, and, a great proportion of the tallest trees have been struck with lightning. Whether these phenomena are connected with the existence of a large mass of native Iron, as yet undiscovered, I leave for others to determine ; the facts, however, may be relied on.

Physical and Chemical properties of the native Iron of Canaan, ascertained in the Laboratory of Yale College, by Mr. C. U. SHEPARD, *at the request of the Editor.*—In its first appearance to the eye, the native Iron of Canaan resembles highly crystalline plumbago ; being every where invested with a thin coating of this mineral, which completely defends it from oxidation. Its structure is visibly crystalline : separating with considerable readiness into pyramidal masses, and more usually into oblique tetrahedra. This cleavage, however, never takes place without the intervention of thin scales of plumbago. It falls considerably short of meteoric Iron in malleability, toughness, and flexibility ; as well as in the silvery whiteness of its lustre, which, in part, is no doubt due to the plumbago diffused through it. In hardness and magnetic properties, it does not differ perceptibly from pure Iron. Its specific gravity varies from 5.95, to 6.72.

Intermingled with it, occasionally, is *native Steel.* One angular fragment, weighing about eight grains, was perfectly brittle, sufficiently hard to scratch glass, and possessed of the characteristic granular structure, and silvery white color of steel. With the microscope no scales of plumbago were noticable in it. Dissolved in dilute nitric acid, it afforded an evident quantity of black, carbonaceous matter, upon the surface of the solution.

A fragment of the native Iron, weighing 100 grains, was dissolved in dilute nitro muriatic acid. The plumbago attached to it being left behind, was separated, and found to weigh 6 grains. To the solution was added, in excess, perfectly caustic liquid ammonia, by means of which the Iron was thrown down. The ammoniacal solution was then examined for lead, copper, or any other metal which might be present, by adding to it hydro-sulphuret of Ammonia. No precipitate, nor change of color, was produced, though suffered to remain for several days, which leads to the conclusion, that our mineral is unalloyed by any metal. In this respect, therefore, it differs from the native Iron of Saxony, in which Klaproth found, lead 6.0, and copper 1.50. The Iron being washed and heated, weighed 127 grains ; which being in the state of a peroxide, according to Mr. Children indicated 88.90 metallic iron, or according to Klaproth, 92.21 metallic iron.

To secure greater accuracy, the process was repeated with 50 grains of the mineral, from which were separated 3.50 grains plumbago. The Iron was precipitated as before; and after being heated, weighed 63 grains, which, according to Children, indicated 44.10 metallic Iron, or by Klaproth's rule, 45.90.

Remarks by the Editor.

There can be no question that the native iron, above described, is a genuine production of the earth; and that it holds no connexion with meteoric iron. The mass bears the marks of a true metallic vein—it has smooth sides (saalbandes) and small specks of blue and white quartz are sticking in it; nickel, constantly found in the meteoric irons, is absent from this specimen, and if it were a question whether native iron be a true production of mines, this discovery decides it.

Y. C. Feb. 15, 1827.

II. *Notice of Sulphuret of Antimony, Automalite and Pleonaste, at Haddam, Connecticut; with various other localities of minerals*: by CHAS. U. SHEPARD.—More than a year since, I had occasion to examine a large quantity of minerals obtained at the celebrated locality of Chryso-beryl, in Haddam; among them, I had the satisfaction to discover *Sulphuret of Antimony*, *Automalite*, and *Pleonaste*.

The *Sulphuret of Antimony* was very sparingly disseminated through feldspar, in the form of compressed rhombic prisms; and also in small masses, whose structure was perfectly lamellar. It is of a dull lead gray colour, soft and flexible. Immediately on being placed before the blowpipe it melts, and is almost totally converted into the form of a white vapor, with a strong odour of sulphur.

The *Automalite* presents rather an ambiguous appearance, differing widely from the fine transparent crystals of this mineral, found in New-Jersey. It is rarely in distinct crystals, but more frequently in moderate sized masses, whose structure, however, is distinctly crystalline, and is associated with massive manganesian garnet. The crystals are about a quarter of an inch in diameter, and unusually modified for this mineral, having each of their edges replaced by two planes, which are deeply striated lengthwise. They are easily susceptible of cleavage parallel to their primary planes, but in no other directions. It is of a dark green color, and nearly opaque, but by transmitted light, when viewed thro' a small fragment, presents a fine blueish green color. Its hardness is rather above that of quartz, and its specific gravity 4.38. Before the blow-pipe it is infusible.

The *Pleonaste* occurs in small octohedral crystals, and is, I believe, pretty generally distributed through the Pinite of this locality, as I noticed but few specimens that did not contain more or less of it. Its color is black; and lustre splendent.

Some time ago, Dr. Wells of Windsor, Mass. gave me a mineral for examination, which he found in his neighbourhood. I find it to be *Laumonite.* The following are its characters. It exists in slender prisms, apparently rhombic, traversing in various directions a yellowish talc; the Laumonite constituting the greater part of the mass. It possesses a pearly lustre; is white, opaque, and of sufficient hardness to scratch glass. Before the blow-pipe, it intumesces, and fuses into a colorless glass.

The same gentleman has subsequently presented me specimens of a beautiful *columnar Bitterspar*, from Florida, (Ms.) It is intermingled with asbestos, and closely resembles this mineral from Miaska, in Siberia.

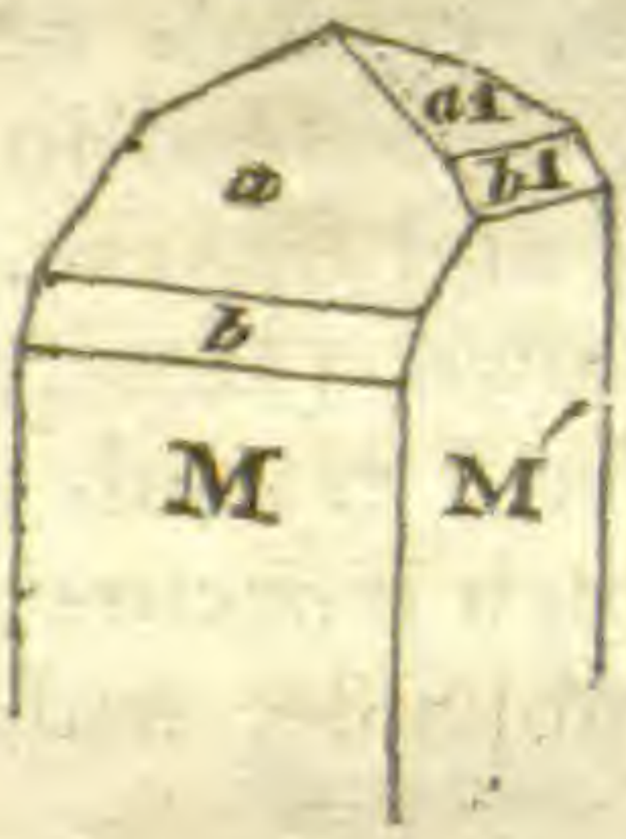
In Maine, at Norway, upon the road from Paris to Waterford, are found, in Gneiisoid Hornblende rock, crystals of *Phosphate of Lime*, of a greenish white color, imbedded in calcareous spar, and accompanied by *Pargasite* and *Sphene.*

Pyroxene occurs very abundantly at Belchertown, (Mass.) It exists in laminated masses, rarely in well defined crystals; and composes the greater part of the rock in which it occurs; the remainder consisting of Feldspar and Hornblende, with occasional crystals of Sphene.

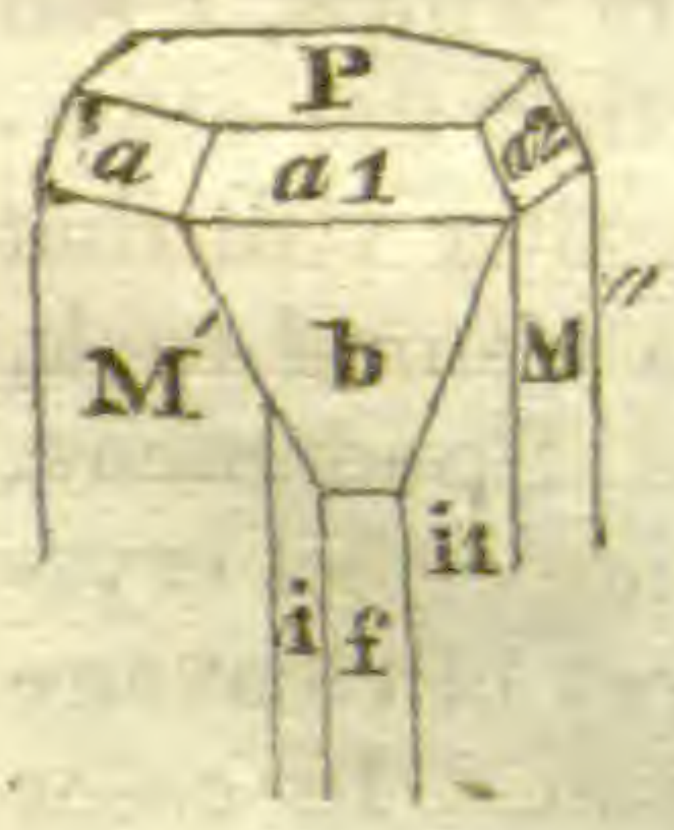
The following *uncertain substance*, from Goshen, (Ms.) has long been in my possession; an account of which, I am now induced to give, for the purpose of directing the attention of those Mineralogists to it, who may hereafter visit that interesting spot. I met with it in breaking up a small rock, containing blue Tourmalines, Rubellite and Spodumene. These are its characters. It possesses a very imperfect crystalline structure, having occasional appearances of cleavage parallel to the sides of an oblique rhombic prism. Its color is uniformly a delicate pink, closely resembling the color of the Chesterfield Rubellite. It is translucent on the edges, and exhibits a very feeble lustre. It is so soft as to be scratched by a quill, is fragile, and emits a strong argillaceous odour when moistened, but does not adhere to the tongue. Its specific gravity is between 2.2. & 2.5. Before the blow-pipe, it immediately whitens, and melts into a pearly superficial enamel.

III. *Measurements of Crystals of Topaz from Huntington, Conn.;* by CHAS. U. SHEPARD.—From a fine collection of Huntington topazes, obligingly put into my hand by Prof. Silliman, I have measured several crystals with the *reflective goniometer*. Annexed are figures of three of the most perfect I examined, with the measurements obtained. These crystals were well defined, and perfectly transparent; and, from the pains I took to be exact, I trust that my measurements of their angles, are very close approximations to their true value.

M on M′ - - - - - -	124° 20′
—— g - - - - - -	160° 30′



M on M′ - - - - - -	124° 20′
—— b - - - - - -	135°
—— a - - - - - -	124° 30′



M′ on M″ - - - - - -	55°	40′
—— i - - - - - -	161	
—— f - - - - - -	116	10
—— i1 - - - - - -	72	
P on a1 - - - - - -	135	30
—— b - - - - - -	116	15
—— f - - - - - -	90	
a1 on b - - - - - -	160	36
b on f - - - - - -	150	30
i on f - - - - - -	116	

The crystals afforded by this locality, which as yet has been imperfectly explored, resemble, very strikingly, in their modifications and colour, those from Saxony, but are generally of much larger dimensions, and appear to constitute a greater proportion of the rock in which they occur.

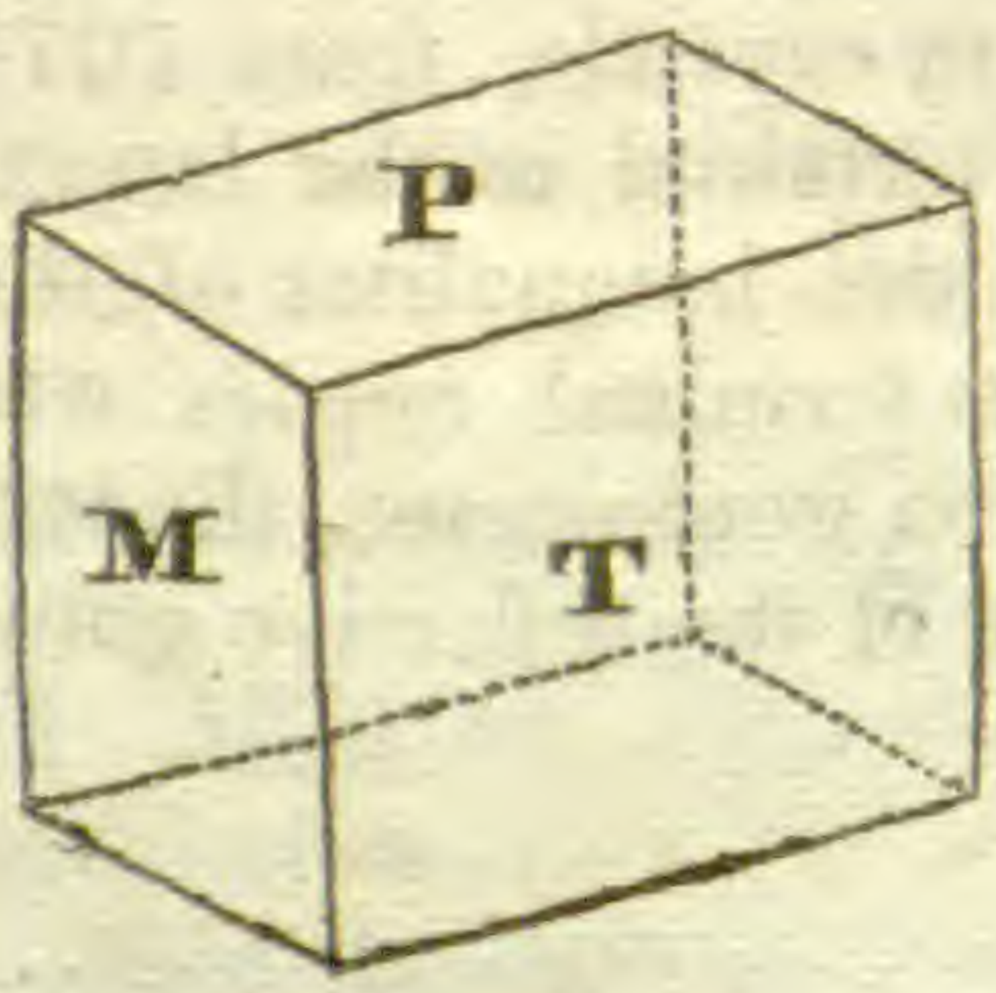
New-Haven, Dec. 22, 1826.

IV. *A comparison of the crystallographical characters of the Cyanite and Sillimanite;* by CHAS. U. SHEPARD.—Having noticed, in Haidinger's edition of Moh's Mineralogy, the following remarks concerning the Sillimanite, I was led for some time to consider the distinct nature of this substance as highly questionable.

"Its analysis agrees exactly with that of prismatic disthene-spar, by Klaproth. No exact crystallographical description is given, but the angle of 106° 30′ is very near the incidence of P on M, 106° 15′, in that species; also the specific gravity is not much different, and the great hardness may, perhaps, be accounted for by the want of a more general diffusion of correct methods for ascertaining this property. Sillimanite is, therefore, probably, a variety of the prismatic disthene spar."

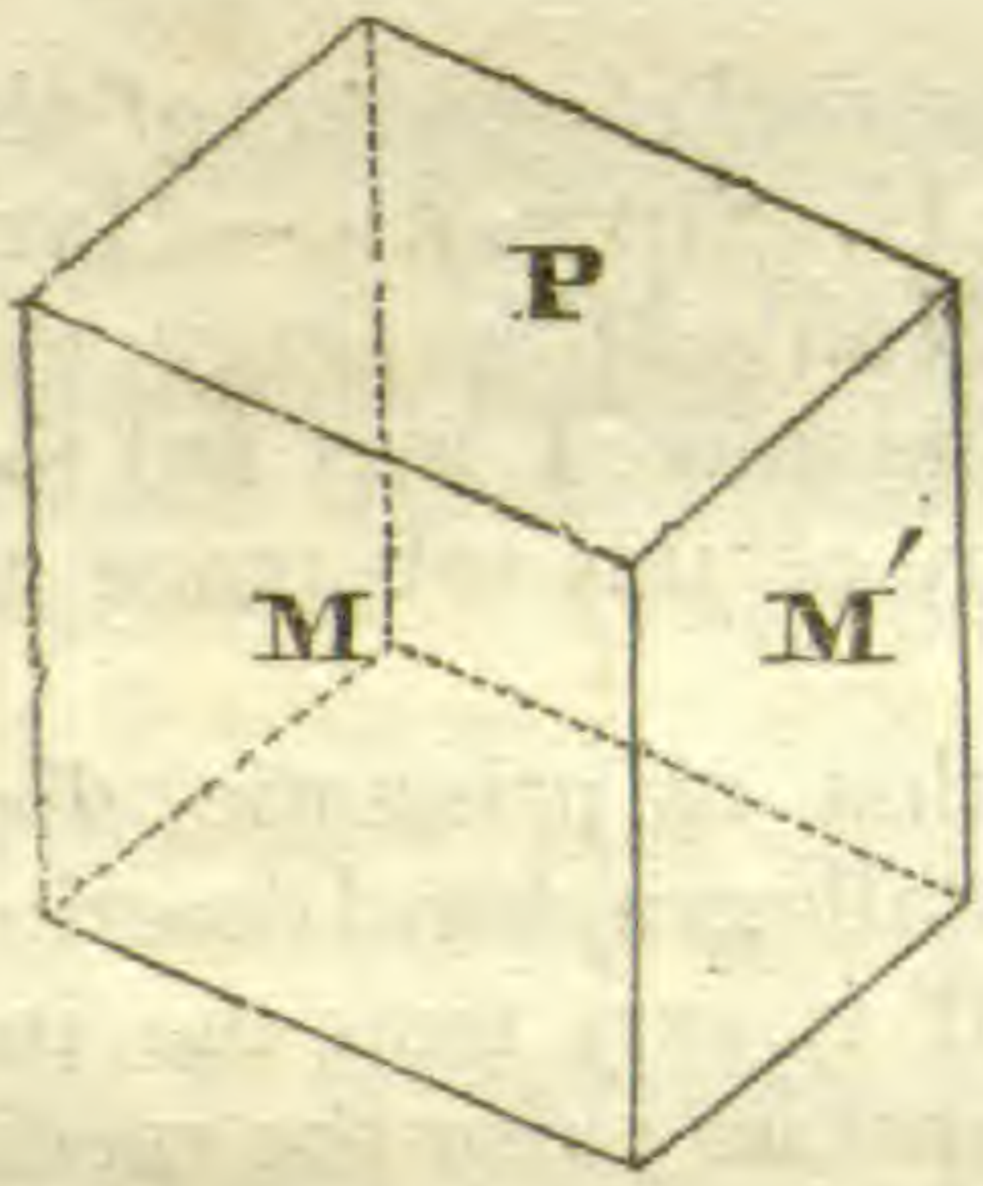
I am happy to say, however, that a recent opportunity of examining some excellent crystals of this mineral has fully satisfied me, that it cannot be identified with the prismatic disthene-spar, or cyanite: and that it will, most probably, with the greatest propriety, continue to bear its present name.

Cyanite, as is well known, is crystallised in irregularly terminated rhombic prisms, which commonly have their edges replaced by planes. It possesses three cleavages: two of which are parallel to the sides of the prism, and the third is in the direction of what are regarded its terminal planes, thus giving rise to a *doubly oblique prism*—its primary form, whose angles are,



M on T	- -	106° 15′
P on M	- -	100 50
—— T	- -	93 15

The *Sillimanite* occurs in lengthened rhombic prisms, never terminated with regularity, or modified upon their edges. It does not admit of cleavage parallel with the sides of this prism, but has a terminal cleavage, thus presenting us with an *oblique rhombic prism*, for its primary form, whose angles are,



M on M′	-	- 108°
M or M′ on P		- 114

The Sillimanite possesses a *supernumerary* cleavage in the direction of the longer diagonal of the prism, which is remarkably distinct, and may be effected with great ease. The planes produced by this cleavage have a brilliant pseudo-metallic lustre, every way equal to that upon the terminal planes of crystallised mica.

From a comparison of these statements, it is almost unnecessary to remark that the crystalline structure of the Sillimanite is wholly incompatible with that of the Cyanite.

The measurements which I have given above of the Sillimanite, it will be observed, differ slightly from those obtained by Mr. Bowen. He found the inclination of the lateral planes to be about 106° 30′ and 73° 70′, and that of the base to the axis of the prism 113°. My results, however, I flatter myself, will be found to be not far from the truth, as they are the *means* of numerous trials. For, it may be remarked, that the incidence of the lateral planes is not constant in this mineral, varying in different crystals, from 107° to 109° ; nor is the inclination of the terminal to the lateral planes perfectly uniform, arising from the inequalites of the terminal cleavage. The value of the terminal angles was obtained solely by means of the common goniometer, the terminal planes being too dull for the use of the reflective goniometer.

New-Haven, January 2, 1827.

V. *Notice of Minerals from Plymouth, Conn.*—Professor SILLIMAN,—Sir,—The parcel of minerals from Plymouth, Con. discovered by Messrs. Erastus Smith and Silas B. Terry, and forwarded to you for examination, in compliance with your request I have examined :—the following are among the more interesting of them.

1. An ore of *titanium*, which at first view appeared to be specular iron. Its colour is iron black, passing into steel gray, and its structure is lamellar. One specimen, however, presented me with a fragment of a large crystal, exhibiting sections of a rhombic prism, of 110° and 70°, having its obtuse and lateral edges replaced by planes, which formed, with the primary lateral planes, angles of 125°. The cross-fracture of this mineral varies, from uneven to flat conchoidal: its lustre is glistening, and sometimes brilliant. It is opaque, brittle, and hard enough to scratch glass ; is not magnetic, either before or after having been submitted to the action of the blowpipe, and is infusible without addition, but with borax fuses into a reddish transparent glass. Its specific gravity is 4.5. It occurs imbedded in granite, in various sized masses, the largest of which weigh more than a quarter of a pound. Thus it appears to approach in its characters very near to the *Nigrine*, which mineral has hitherto been found more commonly in alluvium, and only in small angular or rounded masses.

2. *Phosphate of Lime* in foliated masses, of a pale greenish color, and imbedded in massive *Cyanite.*

3. *Stilbite* in laminæ, which are disposed in a radiating manner. It is of a grayish white color, translucent, and has a shining pearly lustre. It yields easily to the knife, and before the blowpipe melts with intumescence into a white blebby glass. While undergoing fusion, it emits a phosphoric light, as is the case with all Stilbites, which I find to possess this character in a more eminent degree than the Heulandite.

4. *Zoisite*, both gray and blue, and closely resembling specimens of this mineral found at Williamsburgh and Chesterfield, Mass. Yours, very respectfully,

CHAS. U. SHEPARD.

New-Haven, Dec. 13th, 1826.

VI. *Minerals from New South Shetland.*—A small box of specimens, recently brought from New South Shetland, contained the following substances, which (from the situation in which these minerals usually occur,) indicate the existence of *trap rocks* in those islands ;—*Laumonite*, *Calcedony*, *Prehnite* and Stilbite, in transparent flesh colored crystals, and in compact brick red masses.

Crystals of quartz, in portions of large geodes, and most of the specimens indicating that they formed either imbedded masses or veins. C. U. S.

VII. *Pyrites investing Quartz, Vegetable Stalks, &c.;* in a letter from Mr. Lucius Lyon, dated Detroit, Mich. Ter. Sept. 7, 1826.—We have received specimens of a mineral which, "by rubbing against any hard substance, or even woollen, or cloth of any description, acquires a strong yellowish resinous lustre, which led the Indians, who first observed it, to suppose it was *gold*, and they were accordingly very cautious about discovering the place where it was found. It was sent to Mr. Lyon for examination, by Col. Boyd, U. S. agent for Indian affairs, at Mackinac, and is found on the river Marquette, in the north-western part of the peninsula of Michigan. It is said to be abundant."

"Before the blow-pipe it burns for a short time with a bluish flame, and yields a slight odour of sulphur; the smaller particles decrepitate, and it is difficultly fusible by itself, but with borax, melts easily into a bluish glass."

In addition to the above observations, contained in Mr. Lyon's letter, we will mention, that this mineral is not magnetic, but becomes decidedly so after being heated red hot on charcoal. Its colour, before heating, is a delicate and beautiful bronze; it becomes black by heat, and then ceases to emit the sulphurous odour.

This mineral occurs in the specimens sent, for the most part, in the form of minute rounded ovoidal (not angular) masses, of the size and shape of a common small written o, and from that up to the dimensions of a capital O. On being broken, they are found to be composed of quartz, with a very thin coating of iron pyrites—in general not thicker than foolscap paper, but still the coating is perfect, and leaves no part of the stone uncovered. Among these minute pebbles, are found small vegetable stalks, not larger than a common pin, and they also are completely invested by the pyrites, so that their broken ends, and the almost imperceptible roughness of their surfaces, are exactly copied by this delicate mineral drapery. When these small sticks are broken, the pyrites appear as a very thin film, perfectly covering the woody fibre, which is not in the least mineralized or penetrated. It is exactly in the condition of seasoned wood, and burns readily, with the usual odour of that substance when burning. The surface of both the invested wood and stones, which is of the colour of the bronze in statues standing in the open air, assumes, by being rubbed with the finger or broadcloth, a very brilliant metallic polish.

If we mistake not, these minute bodies, which we are informed are so abundant as to be easily obtained by the 100lbs. are unquestionably of aqueous origin, as far as regards the investing coat of pyrites, and thus this fact, along with some similar ones, which have been observed elsewhere, may be of some use in illustrating the origin of pyrites in certain cases.

Sept. 29, 1826.

VIII. *Mr. Webster's notice of the seasoning of Timber, and of the acceleration of Water Wheels during the night.*—To the Editor.—In Nov. 1825, I weighed a cleft of green oak wood, and laid it in my garret. At the end of a year, I weighed it again. The weight was as follows:

When green,	6lb.	10 oz.	=oz.	106
Seasoned,	4	12	=do.	76
Difference,	1	14	do.	30

Then to ascertain what a ton would lose of weight in the same time and under like circumstances:

106 oz. : 30 oz. : : 32,000 oz. : 3056 oz. = 566 lbs.—Loss of a ton.

2000 lb.—566 lb = 1434 lb.—the weight of a ton of green wood after a year's seasoning.

I need not observe that wood will not season well until it is split. It is almost in vain to attempt to season round wood covered with bark.

In the year 1799, I spent a night in making observations to ascertain whether the popular opinion, that mill-wheels driven by water have an accelerated velocity, with the same head of water, during the night, is well founded. By an article in a late number of the American Journal, I observe that Prof. Cleaveland has made observations with a similar view, which seem to disprove the results of my observations. But I am not satisfied with his experiment and observations. I question whether the experiment can be fairly made, except on a small stream, in a calm night, when no wind or moving object disturbs the water; at the same time great care must be taken to keep the water at the same altitude, and the wheel with uniform friction. But there is an important circumstance in his case, which must have rendered his experiment incomplete. This is, that he discontinued his observations at 12 o'clock. But the greatest acceleration of the wheel is not till the break of day. My observations, made in 1799, were conducted with great care, from sun-set to sun-rise, and these gave an acceleration of one-ninth—the wheel making 16 revolutions at sun-set, and 18 at day-break. See my History of Pestilential Diseases, vol. II. p. 298, Am. edit.

N. WEBSTER.

New-Haven, Nov. 1826.

IX. *Curious effect of Solar Light;* communicated in a letter dated Berlin, Conn. Feb. 22, 1826, to Dr. CHAS. HOOKER.—Last week (Feb. 13, 1826) I observed a rather singular meteorological phenomenon, which never had attracted my notice before. About half an hour before sun-set, I was walking on elevated ground,—the sky very clear, except a bank of light clouds to the N. of E. extending a few degrees above the horizon. I observed, directly opposite the sun, a perpendicular beam of light, shoot up on the clouds, and further S., near the horizon, a cluster of such beams, very oblique. Very soon, several beams, that were perpendicular, or nearly so, shot up around the first I saw, and some pale and scattered ones in the interval of the two clusters, and to the N. The whole was very exactly like the diverging beams that shoot out from the horizon just before sun-rise or after sun-set; its point of divergence, or centre, was below the horizon, apparently as many degrees below as the sun was above.—The brightness, particularly of the two clusters, was very considerable—light, soft and white. It undoubtedly was a reflection of the sun from the bank of clouds. It may be common, but I have never had my attention excited by it before. The weather showed rather singular changes after it—by 7 o'clock, the sky was overcast by a thin layer of *cirro stratus*, not so as to darken the light of the new moon very greatly—by 10 o'clock it had condensed into a uniform *stratus*, and by 8 o'clock next morning a storm came on from the N. E., the mountains covered with fog—by noon the wind veered to N. W. and at the same hour when I saw the appearance in the N. E. the sky was perfectly clear.

J. G. PERCIVAL.

P. S. Aug. 1, 1826, near sun-set, I observed on the Delaware, below Philadelphia, a similar appearance to the above. The eastern half of the hemisphere was covered with light clouds *(cirro stratus).* A faint radiation, much less distinct than in the former instance, extended nearly to the zenith, and laterally nearly to the horizon. The semicircle was much larger and more complete than the former, but not so distinct as to strike a careless observer. The center of radiation, as in the former instance, appeared to be opposite the sun. The clouds soon passed off with a westerly wind, and the following day was clear.

X. *Agriculture—Wheat.*—Dr. JOSEPH E. MUSE, President of the Dorchester Agricultural Society, Md. in an address delivered before that body, Nov. 9, 1826, states the opinion, that animal manures are peculiarly necessary for wheat crops, because gluten, the characteristic proximate principle of wheat, contains nitrogen, which assimilates it to animal bodies. If, therefore, manures of animal origin are withheld, the soil becomes less adapted to the production of wheat.

It is proposed by Dr. Muse, as an interesting inquiry, "What proportion of gluten does wheat afford in different parts of the United States?" He supposes that this principle prevails most in the wheat of the south. It would be interesting to ascertain this fact by experiment.

Dr. Muse, speaking of the analysis of Amerîean wheat, says, "From 100 parts of white flint wheat, grown in Dorchester, Md. on a clayey soil, I obtained 30 parts of gluten. Wheat grown in northern Europe, is stated to yield only about 20 parts of gluten in 100."

Dr. Muse concludes, from his own experiments, that cotton may be profitably raised in Maryland. He recommends the introduction of the madder, or *rubia tinctorum*, and a greater attention to the farinaceous and saccharine roots—potatoes, beets, carrots, &c. From 100 parts of the beet he obtained 12 of saccharine matter; and from the carrot 10 of saccharine and 4 of mucilaginous matter. His address contains many judicious and valuable remarks relative to American agriculture.

XI. *Ærostation.*—Mr. EUGENE ROBERTSON, well known for his adventurous æronautic excursions, which have given him a celebrity, like that which his father so long sustained, ascended from the Castle Garden, in New-York, on the evening of Oct. 16, 1826, in company with a young lady. His balloon, of the capacity of 16,000 cubic feet, was filled with hydrogen gas, obtained in the usual mode, and four attendant balloons, each of ten feet in diameter, were attached to the main ærostat. They ascended at sunsetting, at 40 minutes past 5 o'clock.

The circumstances chiefly interesting, other than the events commonly attendant on such occasions, were the following:

1. The great balloon was slowly charged, and in the mean time small balloons, of different sizes, were launched, to ascertain the direction of the wind. One of these, of seven feet in diameter, was supplied with a parachute, supporting a kitten; and a match, lighted before the ascent of the balloon, was intended in due time, to burn in two, the rope by which the parachute and kitten hung to the balloon. Every thing succeeded, to the amusement of the spectators.

2. As they ascended, the little flotilla of balloons was extremely agitated. They appear to have been more inflated than the great balloon, and consequently their ascending power was greater; in fact, one of them, owing to the expansion of the gas, soon burst, with a shock and a report; but the equilibrium of the principal balloon, having been adjusted independently, was not disturbed by this event.

3. At a certain elevation, the flashes of artillery appeared like sparkles of fire, and the sound did not arrive till six or seven seconds had elapsed, denoting an elevation of between seven and eight thousand feet, or about $1\frac{1}{3}$ mile.

4. A bottle of water, emptied into the air, (in imitation of the experiment of Dr. Jeffries, in passing the English channel, during his famous aerial voyage,) did not produce either report, shock, or sound, but the circumstances were not supposed to be exactly similar.

5. Champaign wine, when the bottle was uncorked, instantly evaporated like smoke, and the few drops that could be swallowed while they were exhaling, were peculiarly lively and stimulating.

6. The moon having risen, a kind of white shadow overspread several places, which was attributed to the refraction of the rays by fogs, emanating from ponds or swamps.

7. Being near the ground, and over a populous region in Jersey, the speaking trumpet produced echoes that might easily be mistaken for answers to the calls made in this manner for help in landing.

8. Mr. Robertson having landed the lady, again ascended, at a quarter past 7 o'clock, P. M. He carried one of Davy's lamps, but the brilliancy of the stars rendered it unnecessary to use it, as he could by the eye distinguish the smallest marks on his instruments.

9. Notwithstanding the loss of gas, which had been let off to facilitate the descent, the larger balloon, now eased of the weight of one person, had more ascensive power than the small ones, which therefore hung behind, and sometimes took a position exactly under the larger one: when the rapidity of ascent was diminished, the small balloons were much agitated—striking sometimes against the boat and sometimes against the balloon. Watching his opportunity, when they approached, the æronaut therefore ripped up two of them with the stabs of a knife; one had burst before, and one only remained, which gave him no particular trouble.

10. At an elevation of more than 4000 feet, Fahr. thermometer was at 39°, the air was still and calm, and there were neither currents nor whirls, a fact easily ascertained by the use of a long pendant of thin animal membrane, fastened to a silk six feet long, fixed to the boat, and which thus answered as for an aerial log or floater. This aerial log shows also, much more sensibly than the barometer, when the balloon is ascending or descending.

11. At 40 minutes past 7 o'clock, the barometer had fallen to 16 inches and a few lines, and the thermometer of Farenheit was 4° below freezing.

12. Then concentrated muriatic acid produced very little cloudiness in the air, thus indicating the absence, in a great measure, of water. The lips were very dry, and the hygrometer indicated absolute dryness.

13. At the altitude of 21000 feet, the thermometer of Fahrenheit stood at 21°, and caustic potash, which had been kept close in a glass bottle, with a ground glass stopper—when exposed to the air, remained perfectly dry, and was pulverized, without the slightest deliquescence.

14. The Madgeburgh hemispheres, with a good vacuum, when the communication with the air was opened, were fill-

ed in one second, whereas five would have been required at the earth's surface.

15. Respiration was laborious and painful—the faculties were blunted—the cold was insufferable, especially in the hands, and more particularly in the one employed for holding on by a pole, which felt like iron, producing torpidity and cramp ; and it was necessary to interchange the hands, alternately protecting one in the clothes.

16. Ether, in a drop on the objective glass of the telescope, evaporated in 4½ seconds : porous substances, sponges, cloth and cork, lost half their weight—but wood, paper, pasteboard and metals, lost less weight.*

17. An electrical machine did not give any marks of excitement, but many circumstances influence this excitement, and Gay Lussac, in his truly philosophical ascent, found electricity and magnetism in undiminished energy.

18. The æronaut being unable any longer to endure the painful circumstances in which he was placed, opened the valve for the escape of gas, and his floater evinced that he was descending—while the mercury in the barometer at the same time ascended. Luminous points soon began to appear on the earth, which he reached in safety, and having secured his balloon and instruments, returned with his companion to New-York.—*Abstracted from the New-York Literary Gazette, of December* 23, 1826.

XII. *Cadmia.*—Extract of a letter to the Editor, from Mr. Isaac Doolittle, dated at Bennington Iron Works, Vt. Nov. 29, 1826.—The cadmia which collected in our furnace, during a blast of little more than four months, weighed 680 pounds, not including many small fragments, and several specimens of considerable size, which would, I have no doubt, have made a total weight of 700 pounds, or upwards. It was deposited near the top of the furnace, just at the bottom of the *charge*, where the temperature is lower than in any other place in the furnace, and where it is constantly varying, in consequence of the introduction of fresh

* Excepting the diminished force of gravity, no reason is apparent why they should lose any weight, except so far as they contained vapour or volatile matter. Metals, it would seem, ought rather to weigh more, for the same reason that they weigh more in air than they do in water ; perhaps the benumbing effects of cold and a distressed respiration, might have produced some error in the experiments.—Ed.

materials, which takes place from twice to three times, or more, every hour.

The form of the deposit was an elipsoidal zone, embracing the interior circumference of the furnace, though higher on one side than on the other; and varying in thickness from four to nine inches in its thickest, or upper edge; its height was from fifteen to nineteen inches. The upper surface appeared rough and *unfinished*; the side next the fire, was a curvilinear surface, remarkably smooth and even.

The structure of the mass is slaty, and answers perfectly to the description given by Dr. Torrey and Prof. Keating, of a similar substance found at Ancram, and published in Vol. V. p. 235, and Vol. VI. p. 180, of the American Journal. It was not without considerable difficulty that we broke through the arch formed by that substance, but this once effected, the remainder of the mass was easily detached from the wall.

Remarks by the Editor.

The Cadmia of Bennington is readily volatilized on charcoal, by the compound or oxy-hydrogen blowpipe, and burns with the usual beautiful combustion of zinc. Pulverized, mixed with charcoal powder, and wrapped in sheet copper, it readily forms brass, when heated by the compound blowpipe.

As this cadmia appears to be almost a pure oxyd of zinc, we would recommend that it be saved, and that, if practicable, measures be taken to collect it more effectually, that it may be employed in the manufacture of brass, to which object it appears to be eminently adapted. Metallic zinc might, without doubt, be obtained from it, but probably that manufacture would be less profitable than that of brass.

Feb. 21, 1827.

XIII. *Localities of Minerals;* communicated by C. A. Lee. *White Augite*—at Canaan, Conn. in dolomite. It occurs in six-sided prisms, of a tabular form, often with perfect terminations. Some of the crystals are apparently rectangular prisms, with a slight replacement of two opposite angles. Many specimens present the appearance of having been broken and cemented again—the two different pieces being frequently inclined, at a considerable angle. In other specimens in my possession, the crystals cross each other, at different angles, or are hemitropic. The dolomite, in which they are found, is of a soft friable texture, and is constantly disintegrating, leaving the augite disengaged on the surface.

Epidote—in amorphous masses, of a beautiful green colour in gneiss, Canaan.

Scapolite—at Canaan, very abundant, associated with

Tremolite—both bladed and fibrous.

Ferruginous oxide of titanium—same locality.

Black mica—very fine specimens can be obtained from the hill of gneiss above mentioned.

Opal—Sheffield, Mass.

Carb. of iron—Cornwall, Conn.

Schorl—Cornwall—in crystals two or three inches in diameter.

Coccolite—Cornwall—of different colours.

In the west part of the town, there is a locality of *sahlite*, both crystalized and in granular concretions, yielding, by mechanical division, an oblique four-sided prism, with rhombic bases.

There is also another variety of *augite* near the above locality, occurring in amorphous masses, and deeply impregnated with iron. It has frequently a tinge of green, and is considered by many to be copper ore.

New-York, Oct. 18, 1826.

XIV. *Preservation of grass in gravel—Production of the potatoe on a mutilated vine.*—Extract from the common place book of the Rev. R. EMERSON, forwarded to the Editor.—Oct. 1824. On opening a drain near my house, where gravel had been carted in to fill a cavity, five years ago, we found, at the depths of two and three feet, turf with the grass perfectly green and fresh, as when first deposited. This circumstance may possibly suggest a good mode of preserving delicate plants through the winter, which are liable to mould in a damp cellar, and require watering in a dry one. It may be well to put no straw about them, nor to place them in large quantities, without gravel interspersed, lest they should ferment.

Sept. 1825. While walking through a potatoe field, I observed a number of potatoes, one nearly the size of a hen's egg, formed on one of the *vines*. By inspecting the vine, I found it nearly eaten off and quite excoriated near the root by an insect. A few more instances of the same kind were afterwards discovered, though the potatoes on the vines were not so large. Does not this go to prove, concerning the potatoe, what Sir H. Davie has shown of the apple—that the substance which constitutes the fruit, if not derived wholly from the atmosphere through the leaves, is at least modified by it, and then descends, through the pores of the plant, to its proper location, and in its proper state to constitute fruit? In this case, as the natural communication was interrupted, the substance descended but a little way from the leaves, and there assumed the natural form. If this be the true theory, it is obvious that greater care should be taken than is commonly the fact, not to injure the vines of this important plant, till the crop is mature. I have seen them occasionally mown for forage. It is likewise obvious to remark that this hypothesis affords a ready solution to the fact, so well known to practical farmers, that a crop of potatoes exhausts the soil but very little, if at all, as the substance is supposed to be derived principally from the atmosphere.

XV. *Taxadermia.*—" A very interesting work has lately been received in this country, from Vienna, by Baron Lederer. It is styled *Taxadermia*, or the art of preparing and preserving, in a simple and effectual manner, specimens of the animal kingdom for a cabinet of natural history, by J. F. HANMAN. With a laudable zeal for diffusing in this country the valuable information which this book contains, a translation of it from the German has been undertaken by John Knevills, Esq. of Newburgh, from which we are promised such extracts as will greatly contribute to assist those who are engaged in the study of natural history, as well as in the curious and useful art of stuffing and preserving animals in general. We shall, in our next number, commence with the mammalia and amphibia, and shall continue, as we find room, to proceed with instruction, for the preparation of birds, fishes, insects and vermin."

XVI. *Herbarium.*—A gentleman in New-England has an herbarium containing above three thousand species of plants, well preserved and carefully arranged, of which he would be willing to dispose for a reasonable compensation. It contains nearly all the indigenous plants of New-England, with many species collected in every Atlantic State south of Connecticut, and in several of the western States; also foreign specimens, from Europe, Asia and Africa. It contains a arge proportion of cryptogamic plants. The specimens are arranged in folios, according to the Linnæan classification, with labels containing the name of the plant, synonyms, habitat, time of flowering, &c. The Editor of this Journal can direct to sources of further information, if desired.

XVII. *Notice of an ascent up Mount Lafayette, and of irised shadows;* in a letter to the Editor, dated New-Haven, November 30, 1826.—On the 7th of August, accompanied by my friend, Mr. Sparhawk, of Dartmouth College, and a guide, I set out for the summit of Lafayette. The peak thus designated, is about 15 miles distant from Mount Washington, and falls very little below it in altitude, or in the magnificence of effect—emerging as it does with a bold outline from the plains of Franconia. The ascent was rugged, and occupied us several hours. At 11 o'clock A. M. we gained the summit. On our arrival, our view was for some time interrupted by passing clouds, most of which swept along below our feet,—lingering for a moment as they touched the mountains, and then passing on to mingle with the immense sea of vapour, which extended in every direction to the horizon. This wide abyss of clouds occasionally opened for a moment, and gave us a view of the plains below, or sometimes fell beneath a neighboring peak, which emerged from the expanse, like an island in the ocean. These sublime appearances continued to interest us until 4 or 5 o'clock, when these light clouds entirely disappeared, and were succeeded by heavy and black thunder clouds, whose approach from the S. W. was announced by peals of thunder, which were reiterated with torrents of rain falling below us, until the world beneath was veiled from our view, and nothing was visible save the dark expanse of the heavens.

A slight mist now began to fall around us, and suddenly the sun burst through the clouds; when, what was our astonishment and delight, on beholding our shadows reposing upon the bosom of the cloud, having around each of their heads an *entire* rainbow!

The circles formed by the rainbows appeared to be eight or ten feet in diameter—were perfectly defined, and glowed with the most splendid hues. This unexpected phenomenon continued for the space of twelve or fifteen minutes, when the iris gradually faded away upon the evanescent vapour.

I should be very happy to see in your Journal an explanation of the cause of this peculiar variety of rainbows.

Very respectfully yours, FORREST SHEPARD.

XVIII. *New works on Mineralogy and Geology.*—Dr. Emmons, of the Rensselaer School, has published "A Manual of Mineralogy and Geology; designed for the use of schools, and for persons attending lectures on these subjects, as also a convenient pocket companion for travellers in the United States of America." It is a duodecimo volume of 230 pages.

Dr. Comstock of Hartford, has still more recently published "Elements of Mineralogy, adapted to the use of seminaries and private students." This is an octavo of 338 pages.

XIX. *Philosophical Institute of Nantucket.*—We are gratified to observe, that in this insular community—devoted necessarily, in a great degree, to commerce, there is a society for the cultivation of liberal knowledge. At a late meeting, an interesting memoir was read by the president, WALTER FOLGER, Esq. on aerolites. It gives a succinct account of some of the principal events of this nature, and of some of the leading theories which have been published to account for them. The subject is highly interesting, although veiled in mystery. All that we can do at present, is to register the facts with great precision, and to analyse every new stone that falls. Thus our successors will be furnished with materials from which they may, in due time, be enabled to form a theory capable of proof.

XX. *Carpenter's Memoir on the division or extinction of mercury by trituration.*—We have not room for the insertion of this important paper, the principal object of which is to prove, that in the blue pill, the mercury is not in the state of oxid, but merely of minute division. The facts adduced by Mr. Carpenter, appear to establish his proposition, and contradict the common opinion, both with respect to the nature of the blue pill, and to the supposed inactivity of metallic mercury. Mr. Carpenter also recommends a new formula for the preparation of the blue pill.

The memoir is worthy of the attentive perusal of the chemist and physician.

XXI. *Geological survey of Pennsylvania.*—Proposals have been issued for making a geological and mineralogical survey of Pennsylvania, for publishing a series of geological maps, and forming state and county geological and mineralogical collections. The maps will be projected, drawn, and engraved, on a uniform scale of two miles and a half to an inch, by Henry S. Tanner, author of the New American Atlas, &c. &c. from original documents and surveys, to be furnished by Lardner Vanuxem, professor of geology and mineralogy, and Major John Wilson, late civil engineer of South Carolina, geographical surveyor. The whole to be under the superintendence of Peter A. Browne, the original projector.

On the 30th of Sept. 1826, the plan of the above survey and publication was submitted to a public meeting assembled in Philadelphia for that purpose, who resolved that the proposition should be adopted, and passed a vote of thanks to the proposer. They also appointed a highly respectable committee to take the subject into consideration, and a report was made, at a meeting held 6th December, 1826, which was unanimously adopted. From that report the following statements are cited.

" Pennsylvania is undoubtedly the richest state of the Union, in mineral productions: her soil presents every variety of formation, except perhaps the volcanic, and her geology would therefore possess a general interest. Of the minerals furnished to us in such great abundance, nature seems to have particularly selected those which are the most useful to man. Among these may be mentioned, iron of the finest quality, anthracite and bituminous coal in inexhaustible quantities, salt, excellent lime, lead, copper, and zinc.

" We know that Pennsylvania possesses these mineral riches; but of the manner in which they are distributed over its surface, their position relatively to each other, and, in a word, of the *geology* of the state, we are greatly ignorant. To remove this ignorance, and to convey a general knowledge of our mineral productions, by printed descriptions, public collections, and geological maps, is a project which cannot fail to advance the cause of science and of public improvement, and which is worthy of the patronage of every man who is anxious for the honor or the interests of the state.

" Of the means which can be adopted for attaining this important object, the committee are convinced that the plan proposed by Mr. Browne is the most eligible, and ought to receive the public approbation and support. By dividing the whole state into sections, and publishing a large number of separate maps, the expense of the work will be undoubtedly much increased; but this disadvantage will be more than counterbalanced, by the greater minuteness of the survey, by the greater local interest which it is likely to excite, and by the division of the cost into small sums, payable at convenient intervals.

"In recommending this scheme with earnestness, not only to *individual* but *Legislative* patronage, as meriting in an eminent degree the encouragement of a Republic, *whose immense resources* depend for their development upon a minute and thorough exploration of her soil and natural productions, the committee feel that they are performing a duty, imposed upon them by every consideration involving the wealth and prosperity of the state.

"They are not disposed to indulge in extravagant anticipations; but have no hesitation in declaring their unanimous opinion, that, in case proper encouragement be afforded to this laudable and magnificent design, the cause of natural science will be greatly advanced; the value of lands, at present apparently useless, will be fully developed and incalculably increased; important aid will be afforded to the grand system of internal improvement; the manufacturing and agricultural interests of the country will be essentially promoted; and the whole commonwealth enriched, and most materially and beneficially affected, in her various and most interesting relations.

"The Committee therefore recommend for adoption, the plan proposed by Mr. Browne, and that a Committee be appointed to assist him in carrying it into execution."

A Committee of twenty-five persons, distinguished for character and influence, and residing in both town and country, was accordingly appointed, and we cannot doubt that every effort will be made to carry into effect a plan which eminently deserves the patronage of the State of Pennsylvania any of the nation. We heartily wish the projector and his friends every desired degree of success, both in the effort to raise the necessary funds, and in the subsequent execution of an enterprise distinguished alike for its useful, honourable and arduous character. Mr. Maclure set the example of a gigantic national survey, which drew, with a masterly hand, the great outlines; and during the almost twenty years that have since elapsed, a multitude of local observations and surveys, more or less extended, have been made.

In no instance, however, has an *entire State* been surveyed, *mineralogically and geologically*; and we think it fortunate that the first great effort of this kind should be made in a state distinguished for its extent, and for the variety and richness of its mineral productions, and situated so near the geographical centre of the United States, that, both the knowledge acquired, and the example exhibited, will prove the more eminently useful.

Fine models of geological surveys and maps are exhibited in the Transactions of the English Geological Society, and in many continental memoirs. We doubt not that the gentlemen who are to be charged with this responsible duty, will avail themselves of every aid ; and it would be happy if their attention were at the same time directed to every circumstance connected with the geography, topography, climate, diseases, and general statistical interests of this important State. It would be happy, also, if either their own observations, or those of their scientific friends, were directed to *every* department of the natural history of the territories which will be so minutely surveyed.

XXII. *Mineralogy of Nova Scotia.*—In the course of the past year, much interesting information has been obtained concerning the mineral productions of Nova Scotia, by Messrs. J. B. Quinby and Francis Alger, of Boston. These gentlemen have been occupied in that quarter, with the search for iron ores. Among the minerals they met with, (in addition to several rich deposites of magnetic, specular, and argillaceous irons,) were the following :—Smoky quartz in crystals of gigantic size ; amethyst, accompanied by magnetic iron, and sometimes by calcedony and jasper ; laumonite in abundance, " associated with exceedingly beautiful crystals of rhomb spar, chabasie, mesotype, heulandite and ichthyophthalmite ;" crystallized oxide of manganese ; sulphuret of antimony, and native copper.—*Boston Journal of Philosophy and Arts, vol.* 3, *no.* 4.

XXIII. *Garnet, (Cinnamon Stone ?) &c.*—Dr. Webster has recently found at Carlisle, Ms. uncommonly fine crystals, of dodecaedral garnet, idocrase (Egeran,) sahlite, pargasite, and scapolite. The crystals are imbedded in the limestone, and when this is removed by the cautious use of acids, the garnets exhibit a most brilliant lustre, and perfect dodecaedral form ; the angles and edges being often replaced. These crystals vary in size, some are quite small, while others are an *inch or more* in diameter. They are perfect gems, and will probably prove to be cinnamon stone, or essonite. *Idem.*

XXIV. *Native Gold.*—A mass weighing 8½ ounces, (of a conical shape, and having adhering to it a number of small crystals of quartz,) has been found in New-Fane, Vt. It occurred in an alluvial situation, consisting "of thin strata of clay, sand, and water-worn stones. The rocks *in situ* are all of the primitive class, consisting of hornblende, hornblende slate, and green stone porphyry, which are often found alternating with mica slate."—*Idem.*

XXV. "*Flora Cestrica;*—An essay towards a *Catalogue* of the phænogamous *plants*, native and naturalized, growing in the vicinity of the borough of West-Chester, in Chester county, Pennsylvania: with brief notices of their properties and uses, in medicine, rural economy, and the arts: to which is subjoined an Appendix of the useful cultivated plants of the same district. By WILLIAM DARLINGTON, M. D. *West-Chester, Penn.* 1826." pp. 152, 8vo.

Much has been done within a few years to facilitate the investigation of the botany of the United States. The author of this work began his investigations at a time when, to use his own expression, "the works which professed to treat of the plants of this country were few in number, and those few far from being complete." Experiencing much difficulty in examining our native plants, from the want of satisfactory aids, he early determined to publish a catalogue of the plants of his district, thinking that, should the lovers of botany throughout the country follow his example, materials would thus be gradually collected for a complete *American Flora.* Since that time, a new aspect has been given to this science in our country, by the labours of Pursh, Nuttall, Elliott, Torrey and others, whose publications are highly creditable to the authors, and valuable auxiliaries to the botanical student.

The work before us, though limited in its scope to the plants of a small district, is by no means the least creditable of our botanical publications, and is one which no lover of the science can willingly dispense with. In a country so extensive as ours, and so varied by climate, soil, and agricultural improvements, the same species of plants must assume various characters in different places, and it is only by accurate observations, made in many districts of the country, that American Botany can acquire the same precision as that of some foreign countries. The author of this work is evidently a man of accurate observation, a zealous lover and cultivator of botanical science, and eminently a *practical naturalist*—having constantly in view the cui bono while prosecuting his researches. The work is much more than would be implied by " an essay towards a Catalogue," as it is modestly termed by the author. To the name of each genus and species is annexed a neat concise description, with the synonyms, various common names, time of flowering, and habitat; and a brief notice is made of such plants as are known or reputed to possess medicinal or other remarkable properties. The plan of the volume, the large full page, and the execution generally, we think very commendable. We subjoin the author's remarks on a change he has suggested in the name and place of the Linnæan Class *Icosandria*.

" The Class Icosandria is unquestionably a highly natural one—of which the name given by Linnæus conveys no accurate idea: and yet his attention to the name, evidently led him to exclude from it some plants which, in my opinion, ought to belong to it. It is called Icosandria, because the greater number of the genera belonging to it have *about twenty stamina*. But this is by no means the essential character of the class; for Linnæus himself says ' Pro charactere tamen non assumendus est numerus, cum omnes polyandri staminibus parieti interno calycis insertis (non vero receptaculo) huc amandandi sint.' It is the insertion of the stamina upon the calyx which marks the true character of the class: and I humbly conceive that all hermaphrodite plants thus characterized ought to be referred to the same class, without regard to the number of the stamina. Hence I can perceive no good reason why the genus Ribes, which has but five stamina, may not be introduced into this natural assemblage, as well as Eugenia, Rosa, and some others, in which the stamina are very numerous. Neither of those genera have any pretension to the name Icosandria, strictly speaking; yet they certainly all agree in the essential characteristic of the class. The same remark may be made in relation to some other genera, which the later botanists have already transferred thither,—although they have fewer than twenty stamina, such as Agrimonia, Cuphea, &c. and it might probably be extended with propriety still further; so as to comprehend Melastoma, and indeed every other genus in which the stamina (and the petals, when present) are inserted regularly upon the inner edge, or rim, of a concave monophyllous perianth. It was from this view of the subject, that I was induced to propose the name of CALYCANDRIA, as being more appropriate, and correct. This term is expressive of the true character of the class, and is sustained by analogy in the Linnæan name Gynandria."

"Every botanist will at once perceive the nature of the suggestion, and will form his own opinion whether or not it is worthy to be entertained, or considered. Very probably the old name, having been imposed by the great founder of the sexual system, and consecrated by long usage, will continue to be preferred—although, like the name October, for the *tenth* month, it does not express what it means. Certain it is, however, that a veneration for high authority has not deterred the moderns from abolishing whole Linnæan classes, whatever it may have in preventing the modification of Linnæan names! But let the decision, in this instance, be what it may, it is deemed unnecessary to enlarge upon a proposition so obvious in its character. I shall content myself with having respectfully submitted the idea; and will dismiss the subject without further remark,—except merely to observe, that I have placed this class after Polyandria, for the sake of keeping in an uninterrupted series all the classes which are founded upon the number of the stamina."

XXVI. *Compendium of Torrey's Flora.*—"A Compendium of the Flora of the northern and middle states: containing generic and specific descriptions of all the plants, exclusive of the Cryptogamia, hitherto found in the United States, north of the Potomac. By JOHN TORREY, M. D. Prof. of Chemistry in the West Point Military Academy," &c. New-York, 1826. pp. 403. 12mo.

It would be superfluous to speak of the talents and learning of this author; and after the favourable reception which the first volume of his Flora has met with, this Compendium needs no recommendation. We are pleased to learn, by the advertisement accompanying this little work, that the author will soon publish the other volume of his Flora, which has been unavoidably delayed by his call to the chair of Chemistry in the Military Academy at West Point. That work, when completed, will be the standard book of reference for the botanists of the northern and middle states. This compendium, which is after the model of Smith's *Compendium Floræ Britannicæ*, containing the essential generic and specific characters of all the plants described in the larger Flora, with the habitat, time of flowering, &c. of each plant, will be a convenient manual for the travelling botanist, and will be almost indispensable to the student of botany who is not in possession of the author's larger Flora.

XXVII. *Remarks on a " Curious Effect of Solar Light,"* p. 164—in a note to the Editor.—Since the communication on page 164 was struck off, I have read an account of a similar appearance in the Isle of Wight, in the Journal of the Royal Institution. The writer of that article supposes it to have been an effect of perspective, and that it was caused by clouds below the western horizon, and therefore not visible to the spectator—(it occurred after sun-set). In both cases observed by me, it occurred before sun-set.—In the first instance, the sky was perfectly clear, except the bank of clouds in the N E.—I do not recollect a single cloud in the rest of the hemisphere, and my attention was attracted by the extraordinary clearness of the sky. The air had that dense transparency, (if I may so express myself,) which sometimes precedes rain, and which the succeeding changes in the weather explained. In the second instance, the western half of the hemisphere, though not entirely free from clouds, was comparatively clear; and the sun, at the time I observed the appearance, was shining bright and unclouded. The air, too, had a singular clearness, which I had noticed particularly the evening before, on the Chesapeake, by the great brilliancy of Jupiter and Venus, then near their conjunction. This appearance was followed, too, after a few days, by a long interval of almost continued rains, which lasted through the greater part of August. In the first instance, there could have been no cloud between me and the sun, or even between the sun and the place of radiation; at least none was visible. I think that appearance must have been the combined effect of reflection and perspective. The light of the sun might have been reflected from a denser and more distant stratum of cloud, and then, in passing to the eye, have penetrated the intervals of lighter and more broken strata, intervening between the eye and the point of reflection,—this being below the horizon, as in the case observed in the Isle of Wight it was above it.—The reflected light would then come to the eye in the same manner as the direct light of the sun, in passing through the fissures of clouds covering the sun; and the same effect of perspective would follow.

In the second instance, the effect might have been produced as explained by the writer in the Journal of the Royal Institution; since there were clouds intervening between the sun and the place of radiation, although there were none between the sun and the spectator; still the explanation I have given of the first case, would be equally applicable here.—Indeed, I think the appearance in the Isle of Wight might be explained in the same way; since the writer acknowledges there was quite a dense haze in the eastern horizon;—and it does not appear to me necessary that vapour should assume the definite form of cloud, to produce such effects of reflection and perspective. J. G. PERCIVAL.

II. FOREIGN.

Action of Platinum on combustible Gases mixed with Oxigen—Dr. WM. HENRY, to whom chemistry is indebted for a most excellent elementary work, and for many valuable researches, communicated to the Royal Society, in June, 1824, an important paper, upon the action of finely divided platinum on gaseous mixtures. He availed himself of the singular observation of Sir Humphrey Davy, stated in the Philosophical Transactions in 1817:—"that fine wires of platinum, being heated much below the point of visible redness, and immersed in a mixture of coal gas and oxigen gas, in due proportions, immediately became white hot, and continued to glow until all that was inflammable in the mixture was consumed. The wire, repeatedly taken out of the mixture, and suffered to cool below the points of redness, instantly recovered its temperature on being again plunged into the mixed gases. The same phenomena were produced in mixtures of oxigen with olefiant gas, with carbonic oxide, with cyanogen, and with hydrogen; and in the last case there was an evident production of water. When the wire was very fine, and the gases had been mixed in explosive proportions, the heat of the wires became sufficiently intense to cause them to detonate. In mixtures which were non-explosive, from the redundancy of one or other gas, the combination of their bases went on silently, and the same chemical compounds were formed as by their rapid combustion."

In 1823, Professor DOBEREINER, of Jena, ascertained "that when platinum, in the form of sponge, is introduced into an explosive mixture of oxigen and hydrogen, the metal, even though its temperature had not been previously raised, immediately glows, and causes the union of the two gases to take place, sometimes silently, at others with detonation. It is remarkable, however, that platinum, in this form, though so active on mixtures of oxigen and hydrogen, produces no effect, at common temperatures, on mixtures of oxigen with those compound gases, which were found by Sir Humphrey Davy to be so readily acted upon by the heated wire." Dulong and Thenard found that at the common temperature of the atmosphere, carbonic acid slowly unites with oxigen by the agency of platinum sponge—but it does not produce any action upon the mixtures of oxigen with olefiant gas, carburetted hydrogen, or cyanogen, at any temperature, or in any length of time. It was this inefficiency of platinum sponge upon the compounds of charcoal and hydrogen, in mixture with oxigen, while it acts so remarkably on common hydrogen, and also, although slowly, on carbonic oxide, that led Dr. Henry to suppose that some interesting phenomena in gaseous analysis might thus be solved; for instance, it might be expected that platinum sponge would separate hydrogen from carburetted hydrogen, leaving the latter unaltered.

This expectation, and others of a similar character, were fully verified. To ascertain these and similar facts, Dr. Henry made artificial mixtures of the combustible gases, in known volumes, and having mixed them with oxigen, exposed them, sometimes to contact with the sponge, and sometimes with the balls made of clay and platinum, described by Prof. Dobereiner.*

* The proportions used by Dr. Henry, were 2 parts of fine china clay and 3 of spongy platinum, mixed with water into a paste, which was moulded into small spherules, about the size of peas. The sponge best adapted to the purpose of acting upon mixed gases, is obtained by using a little pressure to the ammonia muriate, after putting it into the crucible. If too light and porous, the sponge is apt to absorb mercury by being repeatedly passed through it, and to become amalgamated. In order that the balls or sponge might be removed after their full action, they were fastened to pieces of platinum wire.

Dr. Henry's results are necessarily stated so much in detail, that it is difficult to give an intelligible abstract, and we must therefore refer our readers to the original memoir. It appears that by the discovery of this singular property of platinum, a new instrument of analysis is placed in our hands, and it is obvious that Dr. Henry has used it with his accustomed skill and accuracy.

Collection of Aerolites and meteoric Iron.—Mr. Heuland, of London, is forming a collection of the different aerolites and native or meteoric irons, and has already obtained between forty and fifty. He is desirous of obtaining those of North America for a full equivalent. There have already been sent him from this country, the aerolite of Maryland, and that of Maine, besides that of Weston, which he possessed before; the meteoric iron of Louisiana has also been sent, and the native iron of Canaan, Conn. (not meteoric.)

This collection of aerolites and native iron will form a most interesting addition to Mr. Heuland's splendid cabinet, and will afford means more extensive than have hitherto been possessed of drawing, with correctness, general conclusions as to the origin of these mysterious bodies.

Correction.—The aerolite said in Chaldni's new catalogue of meteorites, to have fallen at Menabilly, in Cornwall, England, it is well ascertained was erroneously reported.

In Chaldnis catalogue there is also an omission of Mr. Heuland's native iron, from Omoa, in the province of Honduras.

Embalming.—Dr. Granville has lately read before the Royal Society, "a monograph on Egyptian mummies, with observations on the art of embalming among the ancient Egyptians," in which he draws the conclusions, that the abdominal viscera were more or less perfectly abstracted, either through an incision on one side of the abdomen, or through the anus. The thoracic cavity was not disturbed. That the contents of the cranium were removed, sometimes through the nostrils, and in others through one of the orbits. The body was then probably covered with quick lime, to facilitate the removal of the cuticle, the scalp and the nails being, however, left untouched: after which it was immersed in a melted mixture of bees-wax, resin, and bitumen, until thoroughly penetrated; and, ultimately, subjected to a tanning liquor, probably made with the saline water of the neighbouring natron lakes. The bandages were then applied, with the occasional interposition of melted resin, or wax and resin, the lumps of resin, myrrh, &c. having been previously placed in the abdomen.

In order to fully establish these conclusions respecting the mummifying process, Dr. Granville had prepared several imitative mummies by its means; some of which bore the closest resemblance to the Egyptian, and had withstood putrefaction for upwards of three years, though exposed to the vicissitudes of a variable climate, without any covering, or other precautionary measure. None of the substances used appear to be sufficient, either singly or conjointly, without the wax, to preserve the body, or convert it into a perfect mummy.—*Lond. Phil. Mag. & Journ. July,* 1825.

Foreign Literature and Science—extracted and translated by J. Griscom.

1. *Ice House of Saint-Ouen.*—The new ice house established at St. Ouen, differs from all others, by its extent, (one hundred feet in diameter,) by its interior construction, in which every precaution has been taken to prevent as far as possible, the melting of the ice; and by the means adopted to produce ice for filling it. These methods, which were secured by a patent, were not put into activity until after the hard frosts of January. It was on the Seine and on the canal of St. Denis, that the 5 millions of pounds, which scarcely filled the half of the ice house were collected; but experiments made upon a very great scale, at temperatures very little inferior to zero, and which were prolonged to the early part of April, have proved, that in the mildest winters, when no ice is formed upon the river, nor even on stagnant waters, the establishment of Saint-Ouen may collect on its own domain, the quantity necessary for an extensive consumption.

The directors have interested themselves in rendering the use of ice more convenient and economical. Adjoining the ice house is a fountain which preserves the water needful for the day's consumption, at the temperature of zero; and portable ice-holders have been contrived, holding from 100 lbs. to 500 lbs. in which the ice may be preserved for daily use for ten or fifteen days, which gives each family the facility of using it at discretion. Means are on hand for constructing refrigerators for the purpose of lowering the temperature of the chambers of the sick, and other apartments.—*Rev. Enc. Av.* 1826.

2. *Mineralogy of Vesuvius.*—M. MONTICELLI, Secretary of the Royal Academy of Sciences at Naples, who has observed and described various eruptions of Vesuvius, and found an immense collection of its products, has undertaken, with the aid of M. Covelli, an associate of the Academy, a complete description of those interesting substances, among which are many entirely new. They find that the greater number of the crystals of Vesuvius present frequent anomalies in their structure and composition. Often, in the interior of their mass, are crystals or crystalline grains belonging to different species, without lessening, in any degree, the perfection of the exterior form.

The number of species described, amounts to 82. In the first class, the order of metalloides is first presented: Sulphur, sulphurous, and sulphuric acids, muriatic acid, azote, boracic acid, carbonic acid, water and sulphuretted hydrogen. The second order, that of electro-negative metals, comprehends 13 families. The Arsenic family is composed of two sulphurets, realgar, and orpiment. The first is found in small crystals formed by sublimation, and which are referable to the octo-decimal and bi-decimal varieties of Haüy. The Silicium family furnishes quartz in small crystals, prismatic and fusiform, in needles, grains, &c. It is quite rare in the productions of Vesuvius. The Lead family includes two species, one of which is new. The first is sulphuret of lead, in small laminæ, disseminated in various aggregates, composed of fragments of calcareous and other rocks. The second is muriate of lead, to which the authors give the name of *Cotunnia* in honor of the Nestor of the Neapolitan physicians. They indicate two varieties, or sub-species; the crystalline and the corneous. The latter is of a pearl white, and an aspect like gum arabic. The muriate of lead is commonly white and colourless; it sublimes at a red heat, leaving no residue, and producing white thick fumes. It is wholly soluble in water, and reducible to metallic lead by the interior flame of the blow pipe. It is found in the cavities of the sandy crust which covers the middle and eastern part of the cone, near the opening formed by the eruption of 1822.

The family of Copper includes three species,—pyrites, sulphate and muriate. These are found in the cavities of a lava formed of pyroxene and amphigene, small laminæ of a beautiful green, which the authors suspect to be analagous to the uranite of Cornwall.

The Iron family is composed of 8 species, viz: sulphuret, carburet, oligist, oxidule, green sulphate, red sulphate, muriate, and permuriate.

The next family produces sulphate and persulphate of manganese, chloruret and perchloruret of the same metal: the two last exist only in a state of mixture with other salts. The families which follow, present us with Zircon in crystals of 4 or 5 millimetres diameter; a sulphate of alumine; nepheline; topaz in crystals which are referable to the sexbisoctonal, septemduodecimal, and tredecioctonal varieties; sulphate of magnesia, muriate of magnesia, condrodite in rectangular prisms, terminated by two quadrangular pyramids, or in octagonal prisms, terminated by two pyramids of the same number of faces, with truncated summits. The same substance (Humite of Bournon) is met with in small globuliform masses, disseminated in a granular aggregate of limestone and greenish mica. In the same family are found, also, common serpentine, peridote in crystals, talc, and spinelle.

The family of calcium has 20 species, viz: sulphate, fluate, carbonate and phosphate of lime, arragonite, sphene, wollastonite, amphibole, pyroxene, epidote, prehnite, thompsonite of Brooke, stilbite, garnet, idocrase, gismondine, pseudo-nepheline, tourmaline, gehlenite, and melilite.

The soda and potash families contain muriate and sulphate of soda, sodalite, analcime, sulphate of potash, alum, amphigene, meionite, feldspath, called *eisspath*, hauyne, and mica.

The 2d class includes but two species,—muriate of ammonia, and petrolium.

The 3d class contains the species not yet arranged, and the new substances: such are *brieslakite*, which lines the cavities of the lavas of La Scala and of the current of Obianos, near Pouzzoles; *humboldtilite*; the *zurlite* of Ramondini; *davyine*; *cavolinite*, from the celebrated naturalist Philip Cavoli-

ni; *christianite*, dedicated to Prince Christian, of Denmark, and *biotine*, from the distinguished French philosopher.
Bul. Univ. Av. 1826.

3. *Gay-Lussite.*—A mineral thus named, in honour of the distinguished co-editor of the *Annales de Chemie et de physique*, was found by the enlightened French traveller, J. B. Boussingault, at Laguilla, a small indian village on the S. W. of the city of Merida, in Colombia, in crystals, disseminated in clay. Its analysis proves it to be a double carbonate, with bases of lime and soda, constituting a new species, analagous to Dolomite. It consists in the 100 parts, of

Carbonate of lime	32.95
Carbonate of soda	34.76
Water	32.29
	100.00

An. de Chem. Mars, 1826.

4. *Composition of Feldspar and Serpentine.*—In a memoir communicated to the Society of Physics and Natural History of Geneva, on the 15th of November, 1825, by *M. Peschier*, the author draws the conclusion that titanium is a constituent part of feldspar and serpentine, and he points out the sources of error in the analysis of Vauquelin, Klaproth, and Brande, which prevented these celebrated chemists from recognizing the presence of this substance in their examination of these primitive earths. Of the five kinds of feldspar analysed by Peschier, he finds the proportions of Titanium as follows:

Adularia	10 per cent.
Green Feldspar of Siberia.	12
Vitreous F. of Drachenfels	10
White F. of Auvergne	3.25
Andalusite of Tyrol	15.50

The third variety, according to Peschier, contains both potash and soda; the fourth 14.18, and the fifth 4.30 of soda, but no potash. The first contains 14, and the second 10.40 of Potash, but no soda.

The analysis of Serpentine, agreeably to the same chemist, is as follows:

The variety examined was from the Palatinate.

Silex	22
Magnesia	29
Alumine	17
Lime	2
Oxide of Iron	12
Manganese	2
Titanium	6
Soda	6
Water and Carbonic Acid	5.50

Two other varieties yielded 5.25 and 8 of titanium.

It results from the researches of M. Peschier,

1. That titanium forms one of the principal constituents of feldspath and serpentine.

2. That the analysis of serpentine can be accurate only by employing the process used for rocks, so modified as to separate the titanium.

3. That an alkaline principle exists in serpentines, as well as in stones to which they are analogous.

In a word, (says the author,) my researches demonstrate that the greater portion of primitive mountain rocks include titanium in the number of their constituent principles, and that this substance is more generally diffused throughout nature than has been supposed.—*Idem.*

5. *Jeweller's Powder.*—The powder commonly employed by jewellers, under the name of *colour*, for giving to their gold the fine yellow and the beautiful *mat* which fine gold presents when not polished, is composed of

Saltpetre	40
Alum	25
Marine salt	35
	100

But an examination of the *colour* now sold in Paris to jewellers, was found by M. Casaseca to consist of

White oxide of arsenic	2.135 gr.
Alum, with base of potash	4.190
Marine salt	13.560
Oxide of iron and clay	0.115
	20.000

This powder is said to answer the purpose intended by the jewellers, but it is justly observed, by M. D'Arcet, in a note to Casaseca's paper, that the proper authorities will doubtless adopt some administrative measures to oblige those who prepare, sell, or employ the new composition, to use every precaution in preventing a mixture which contains so much oxide of arsenic, from being dangerously extended as an article of commerce.—*Idem.*

6. *Strength of leaden pipes.*—Experiments made by M. Jardine, of Edinburgh, to determine the strength of leaden pipes, proved that a tube $1\frac{1}{2}$ inch in diameter and $\frac{1}{5}$ in. thick, would sustain a pressure of 30 atmospheres, or 420 pounds to the square inch. Under this pressure the tube began to dilate, and at 600 pounds to the square inch, or 43 atmospheres, it was ruptured. Another tube, 2 inches diameter, same thickness, sustained 25 atmospheres, and burst with a pressure of 30. The method employed, was to close the tube at one end, and apply a forcing pump at the other. *Idem.*

7. *Titanium.*—Cubic crystals of this metal, precisely similar to those Dr. Wollaston has described, have been observed in the high furnaces of Baden, in the scoria of a high furnace at Madgesprung, and in the scoria of many forges in Germany. Until recently, these cubes have been taken for iron pyrites. *Idem.*

8. *Progress of mutual instruction in Denmark.*—Second Report made to the King, the 28th of January, 1825, by J. Abrahamson, aid-de-camp of his Majesty, &c.

Mr. Abrahamson pursues, with indefatigable zeal, the enterprise which he began more than six years ago—that of introducing mutual and elementary instruction throughout the States of the King of Denmark. He has the satisfaction to see his efforts crowned with a far greater success than he had dared to promise himself. In his first report addressed to the King, in the month of January, 1824, he announced, that at the end of the year 1823, there were in Denmark 244 schools, in which this mode of instruction was in full operation, and 263 others which had begun to be organized on this plan; so that the total number of schools which had adopted the method was 607. This number has since increased to such an extent, that at the end of the year 1824, there were 605 schools completely organized, and 412 others whose organization had commenced. There are, at present, therefore, in Denmark, 1017 communes which have frankly declared themselves in favour of mutual instruction ; and it is more than probable that, in a few years, this method will be introduced into all the schools of the kingdom. Nevertheless, the introduction of the system is not decreed, but simply permitted and encouraged by the government. Among the causes of this success, Mr. Abrahamson cites, in the first place, the powerful protection of the King, who, not content with contributing to the progress of this instruction by his royal munificence, condescends also to visit in person, the establishments in those towns and villages which he passes through. Mr. Abrahamson has the satisfaction to find the number of adversaries to this method considerably diminished. As a proof of the truth of this assertion, the following anecdote is given in his first reports. In a rural district, in the Island of Zealand, many persons complained to the pastor of the new schools, alleging that their children learned rather to play and amuse themselves than to read and write. The pastor having asked for an explanation, the country people answered, that formerly they were obliged to *drive* their children from home to compel them to go to school; whereas now, on the contrary, they were pressing to have their breakfast more early, that they may get off. The pastor invited the parents to attend the school sometimes and hear the recital of the lessons. This they did, and became from that time the most ardent partizans of the schools.

We congratulate Mr. Abrahamson on his remarkable success, which ought to encourage him to proceed in his honorable career, without being diverted by the trifling disgusts which ignorance, malevolence, or jealousy, will never fail to throw in the way of good and useful things.—*Rev. Enc. Juil.* 1825.

9. *Georama.*—Among the new inventions in Paris, destined to render the study of geography more easy and intelligible to young people, must be distinguished this beautiful machine.

The *Georama*, or *View of the Earth*, is a hollow sphere of 40 feet diameter, formed by an assemblage of 36 bars of iron, which represents the parallels and meridians, and which are covered by a bluish cloth, destined to admit the lights, and to represent seas and lakes. The land, mountains and rivers, are painted with much care on paper, pasted on this covering. The two poles are situated, as in maps of the world, at the extremities of the vertical diameter of the sphere. Around this diameter are two spiral staircases, which land on three little circular galleries, placed one above another, so that the spectator, at his pleasure, can approach any point of the sphere that he wishes to examine. This disposition, as convenient as it is ingenious, at first astonishes him. The imposing grandeur of the blue vault which represents seas, the irregularity of the masses of land which interrupt their monotony, the novelty of his situation, all concur to produce a sort of stupor and hesitation, from which he is soon relieved as he discovers, though in a reversed situation, the parts of the world which he has been accustomed to behold.

The relief of mountains is expressed by shades more or less prolonged; rivers, by lines of a paler colour; volcanos by a fiery colour. All analogous divisions (and one may judge how numerous they are, since France has the names of all its departments and chief places) are designed by similar letters. All confusion is avoided, by the manner in which the delineations are made.

We can recommend the Georama, with confidence, to the friends of science. It will produce both pleasure and instruction.—*Idem.*

10. SWITZERLAND. *Extension of Education.*—The *Nouvelliste Vaudois*, (one of the best daily papers published in Switzerland,) of the 7th of October, states that there has been organized in the Canton of Zurich, a numerous association for the amelioration of the condition of primary schools, and the improvement of teachers. The number of primary schools in the Canton of Zurich, exceeds 400 ; during the last twenty years, the government has devoted 17,000 francs to the instruction of teachers; 30,000 in the construction of new school-houses; 27,000 in aid of the education of the poor. Independently of the moderate salaries allowed the primary teachers, there exists a fund of 49,500 francs, destined for the relief of those who have need of charitable aid.

In the prefecture of Andelfurgen, in the same Canton, a society of teachers has existed for six years, who assemble periodically, with the view of communicating the experience mutually acquired in the practice of the honourable functions of instruction.

In the Canton of Basle, the teachers have formed a society of *mutual assurance*. By the payment of an admission fee, and an annual assessment, every teacher may insure to his wife assistance after his death, and to his children a suitable education.—*Rev. Ency. Mars*, 1826.

11. *Necrology*.—Italy has sustained a heavy loss in the person of SCIPIO BREISLAK, who died on the 15th of February, 1826, at the age of 78. Born at Rome, of a father originally from Swabia, he devoted himself, at an early age, to the study of the exact and natural sciences. While still young, he was appointed, at the request of the celebrated Stay, professor of physics and mathematics at Ragusa, a city remarkable for the number of its literary men. He there became acquainted with the learned family of Count de Sorgo, and particularly the Abbe Fortis, who inspired him with the love of natural history. On his return to Rome, he taught in the College Nazareno, the physical and mathematical sciences, and contributed to the improvement of the mineralogical cabinet of that college. He had always felt the necessity of studying nature in nature herself; he made many journeys into the mountains, to visit the places where she more conspicuously reveals her mysteries, and he was thus led to undertake those geological researches which engaged his attention during his life.

From Rome he repaired to Naples, to examine the principal phenomena which that country presents to the curiosity of observers. There he met a second time the Abbe Fortis, and also the celebrated Delfico, and other learned men, who encouraged him by their example to devote himself to the study of natural history. He made the most dangerous experiments in the Solfatara of Puzzola, where he erected a large chemical apparatus for deriving the greatest possible advantage from its mineral ingredients. Obliged, from considerations of health, to abandon this enterprise, he devoted his attention to the instruction of the pupils of the Royal Artillery of Naples, and published, besides various other works, his "Travels in Campania," of which a French translation has been printed at Paris.

Political vicissitudes took him to Rome, and from thence to the capital of France, where he associated with the most distinguished of its savans in the sciences he professed, such as Fourcroy, Chaptal, Haüy, Brongniart, Cuvier, Vaquelin, &c. On returning to Italy in 1802, he was appointed inspector of the nitre and powder manufactory of the kingdom, member of the institute, and successively of the societies of London, Edinburgh, Berlin, Petersburg, Munich, Turin, &c. Continuing his researches for the benefit of natural science, he published several works :—" On the refinements of salt petre," and an " *Introduction to Geology*," reprinted with additions and improvements, under the title of " *Geological Institutions* ;" in which are collected and arranged, with great method, the principles of geognosy and geology. This work, whose merit is generally acknowledged, has been translated into many languages. Notwithstanding his great age, M. Breislak never ceased to communicate to the institute of Milan his various memoirs. By direction of the governments, he published in 1822 his beautiful *Geological description of the Province of Milan ;* and he was engaged in a similar work upon the country between the Verbano and the Lario, when death interrupted his researches. A mineralogical cabinet, formed by this philosopher, has excited the admiration of all amateurs who have seen it, and of the Emperor himself. It was granted by M. Breislak to the illustrious house of Borromeus.—*Idem.*

12. *Preservation of Walls from Dampness.*—In a recent memoir by D'Arcet and Thenard, it is satisfactorily shown that a composition of one part of wax and three parts of oil boiled with a tenth of its weight of litharge, spread over the wall in a melted state, is a durable and effectual preservative from injury by dampness. When this coating is to be spread upon stone, or other porous matter, it should be heated once or twice previously, which may be accomplished by the partial application of a portable furnace. The composition is then more effectually absorbed. Surfaces of plaster, or gypsum, such as walls, busts, reliefs, &c., may in the same manner be preserved from injury.

If the cost of wax is an object of importance, resin may be used as a substitute. One part of drying oil, and two or three parts of rosin, form a suitable composition. They may be melted together in an iron or earthern vessel, taking care to manage the heat so as to prevent boiling over.

Statues of plaster may be safely exposed to the weather, if well covered with this cement, and if the latter be mixed or compounded with metallic soap, various colours may be given to the statue, so as to make it resemble marble and other durable materials.—*Annales de Chimie, Mar.* 1826.

13. *Scientific Reward.*—The astronomical prize founded by the late M. DELALANDE, to be annually given to the person who, in France or elsewhere, shall have made the most interesting observation, or produced the memoir most useful to the progress of astronomy, has been awarded to Captain SABINE, for his "Account of experiments to determine the figure of the earth, by means of the pendulum vibrating seconds in different latitudes," London, 1825, 4to. and which includes the result of numerous observations of the pendulum, which he has made in the northern hemisphere, from Spitzbergen to the Portuguese Island of St. Thomas.

Idem.

14. *Volcanic Ashes.*—A small quantity of the ashes of Mount Etna, sent from Prof. Ferrari, at Palermo, to Mr. Vauquelin, was found by the latter to consist of

Silicia	28.10
Sulphate of lime	18.00
Sulphate of iron	20.88
Alumine	8.00
Lime	2.60
Carbon	1.
	78.58

Water, sulphate of copper, sulphate of alumine, traces of a muriate and of free sulphur, appear to make up the difficient 21.42 parts.—*Idem.*

15. *Limitation of the Labour of Children in Manufactories.*—By an act of Parliament, passed June 22, 1825, children under 16 years of age are not allowed to be employed in spinning factories, or others of that nature, more than 12 hours a day, not including the time for meals. Work is to commence at 5 A. M. and terminate at 8 P. M. On Saturdays they are to work but 9 hours, leaving off at half past 4. In case of an interruption from accidents in the machinery, &c. the employer shall be at liberty to prolong the subsequent labour to the amount of one hour per

day, during a specified time. No children are to be employed in manufactories, under nine years of age. The walls and ceilings are to be white-washed once a year. The penalty for an infraction of the law, is a fine, in each case, not less than £10, nor exceeding £20 sterling.

16. *Refrigerating Compound.*—I have analyzed, (says M. Vauquelin,) an English refrigerating salt, of which the following is the composition:

Muriate of potash	57
Muriate of ammonia	32
Nitrate of potash	10

This salt, put into 4 parts of water, and agitated promptly, reduced the thermometer from 20+ to −5° Reaumur's scale, (=77°+ to 21°+Fah.) Synthesis furnished a salt having the same properties.—*Bul. Univ. June*, 1826.

17. *Dry Voltaic Batteries. Extract from a Report made to the French Academy of Sciences, by* M. AMP` RE, *on the dry piles of* M. ZAMBONI. We find in this memoir, besides the description of some apparatus which the piles keep in continual motion, the following results.

The energy of the dry pile ceases at the end of two years. M. Zamboni has ascertained this by an experience of twelve years. The diminution in the two first years, varies according to the manner in which the pile has been constructed.

The pile is more energetic in summer than in winter, in regard both to the tension which is produced, and the promptitude with which it is manifested.

The tinned paper, commonly called silvered paper, developes with the black oxide of manganese an electric force very superior to that which is obtained when the paper is covered with copper: this last is known by the name of gilt paper.

A pile formed with disks of paper, tinned only on one side, without any substance interposed, gives electrical effects which must arise from the mere circumstance of the metallic plate, which is pasted to the upper surface of the paper, touching it more closely and intimately than it is touched in its turn by the inferior paper of the stratum placed above it.

Zamboni has examined, in these piles, which he calls binary, whether the action of the elements takes place as in those which are composed of leaves of tin, covered with oxide of manganese, or the contrary. He has found that either of these results may be obtained at will, by imbuing with various substances, the paper pasted to the tin. If oil is used, the action is opposed to that produced by manganese; but if honey, an alkaline solution of any kind, sulphate of zinc, or milk half curdled, the binary pile acts like that in which the elements are sprinkled with oxide of manganese.

In using a dry pile, of a thousand pair, of which the plates were not more than 5 or 6 centimetres in diameter, M. Zamboni obtained from the condenser sparks an inch long, so that, with this pile, an electric battery could be kept constantly charged, at a tension which might be rendered as great as desirable, by muliplying to a sufficient extent the number of plates.

Zamboni thinks that a pile of fifty thousand pairs of plates of the size of ordinary sheets of tinned paper, would furnish a *constant* source of electricity, of a tension equal to that of a strong electrical machine. He expresses the wish that such an instrument may be constructed, and states many interesting experiments to which it might be applied.—*Idem.*

18. *Action of Poisons on the Vegetable Kingdom.*—In an interesting memoir read before the Society of Physics and Natural History of Geneva, Dec. 16, 1824, by F. MARCET, it is proved that poisons, both mineral and vegetable, act upon plants in a manner as certainly destructive as upon animals. The metallic poisons appear to be absorbed and drawn into different parts of the plant, and alter and destroy the tissue by their corrosive power.

Vegetable poisons, particularly those which can be proved to destroy animals only by their action on the nervous system, also occasion the death of plants; and it is inferred from the similarity of their effects, that there must exist, in the vegetable structure, a system of organs, which is affected by certain vegetable poisons, nearly in the same manner as the nervous system of animals.

The metallic poisons employed by F. Marcet, were the oxide of arsenic and salts of mercury, tin, copper and lead; and the vegetable opium, nux vomica, seeds of coculus menispermis, prussic acid, water of the cherry laurel, belladona, alcohol, oxalic acid, hemlock, and digitalis. The mode of experimenting was various,—watering the plants by solutions of the poisonous material, inserting the roots or stems of

fresh plants in a vessel containing the solution, and also by inserting the poison under the bark, or in the pith of the plant. The effects were always decisive, in some instances manifesting themselves in a few moments, (particularly in the case of vegetable poisons,) by the binding of the petiole crispation of the leaves, sickliness and final death of the plant.
Idem.

19. *Russian Mines.*—One of the periodical journals of St. Petersburg furnishes an interesting statement of the produce of the gold and silver mines of Russia. From 1818 to 1823, the mines belonging to the crown had yielded 4145 pounds (livres) of pure gold, and 340 pounds of pure silver; and the mines appertaining to private owners, 10385 pounds of pure gold, and 821 pounds of pure silver. During the second half of the year 1824, the crown mines afforded 970 pounds of gold, and 86 of silver, and the private mines 3067 pounds of gold, and 245 pounds of silver.
Rev. Ency. Av. 1825.

20. *Technological Institutes* have been founded at Moscow and Stockholm for the cheap instruction of young people in those sciences which have a practical application to the useful arts.—*Idem.*

21. *Deaf and Dumb.*—MR. ABRAHAMSON of *Copenhagen*, of whom we have often had occasion to speak in this Journal, does not limit the circle of his activity to the schools of mutual instruction alone. He is one of the most active members, placed by government at the head of the Deaf and Dumb Institution. Formerly, this institution was able to receive only 50 pupils; but for some time past, it has contained 70, and preparations are making to extend the number to 90, which, agreeably to the last census, is the number of deaf and dumb in the states of Denmark, whose parents are not able to afford them the necessary instruction under the paternal roof. An establishment of the same kind at Sleswig, provides for all the deaf and dumb of the German provinces of Denmark.
Idem.

22. An *Iron Bridge*, constructed near Potsdam, (Prussia) was opened to the public on the 1st of August last. It is composed of nine arches of iron, cast in Silesia. Its length

is 600 feet, the width of the carriage way 20 feet, and each of the side walks 5 feet.—*Idem.*

23. *Bhagavad—Gitâh.*—WM. DE HUMBOLDT, brother of the celebrated traveller of the same name, read at the last sitting of the Royal Academy of Berlin, a metrical translation of several parts of a great philosophical and religious poem, called Bhagavad—Gitâh; to which he added illustrations of the metaphysics of the Hindoos, compared with the systems of the Greeks. One is agreeably surprised to find in M. W. de Humboldt, the learned translator and commentator of Pindar and Sophocles, a person who is initiated in the secrets of the Sanscirt grammar, as well as those of the Basque tongue, and of the primitive idioms of the new continent. We cannot but expect, from this various knowledge, labours which will add to the amount of our literary acquisitions, and by which the rest of Europe will be solicitous to profit.—*Idem.*

24. *Geneva.*—The Reading Society (Société de Lecture) of this city, founded in 1818, is now in possession of a Library, obtained by donation and purchase, which amounts to 12,600 volumes. In 1824 the society received 94 different journals, viz: 54 French, 22 German, 16 English, and 2 Italian. The circulation of books in private families, amounted in the same year to 11,150 volumes. The terms per day for those who take out books, is $\frac{13}{100}$ of a franc=2½ cents nearly.—*Idem.*

25. *A new substance* has been discovered by M. BALARD, an ingenious chemist of Montpelier, in the mother water of salt works, and in the lixivia of the ashes of sea-plants, by treating these solutions with chlorine. It is a liquid of a deep red-brown colour, very volatile, and its vapour resembling in appearance that of nitrous acid. It has about three times the density of water, although it boils at 47° cent.—The name of *Brome* has been adopted for this substance, from βρωμος, (fœtor,) in consequence of its strong and unpleasant odour, which resembles considerably that of chlorine. It has extensive chemical affinities, forming acids and salts, as well as direct combinations with the metals. The memoir of the discoverer has been fully sanctioned by a committee of

the Royal Academy, consisting of Vauquelin, Thenard and Gay-Lussac.—*Annales de Chimie, Aug.* 1826.

26. *The power of conducting Electricity*, is possessed by the following metals, in the order and ratio of the numbers annexed to each.

Copper	100
Gold	93.6
Silver	73.60
Zinc	28.50
Tin	15.60
Platina	16.40
Iron	15.80
Lead	8.30
Mercury	3.45
Potassium	1.33

Memoir of Becquerel.—*Idem.*

27. *Solar Spots.*—It is admitted by the careful and scientific observers of meteorological phenomena, who register their observations in the journals of the royal observatory of Paris, that the comparison of the spots of 1825 with the temperature, seems to confirm the opinion of some distinguished philosophers and astronomers, that the appearance of solar spots is an indication of an abundant emission of light and heat. The spots, during the past year, have been very numerous, and it is well known that the temperature was uncommonly high. They justly remark, however, that the multiplicity of causes which modify the temperature of the earth, is so great and so various, that isolated results can never lead to certain general conclusions. It is only by combining, in a suitable manner, long series of observations, that the immediate influence of the spots can be duly appreciated.—*Annales de Chimie, Decem.* 1825.

28. *Electro-magnetism.*—In a memoir by D. Colladon, read at the Academy of Sciences, (Paris) on the 21st of August last, it is fully shewn that deviations of the magnetic needle, precisely similar to those that are produced by the Voltaic current, may be obtained by common electricity. He made use of a galvanometer, formed by 100 turns with two needles, agreeably to the plan of M. Nobili. The wire was doubly covered with silk, to insure its perfect insulation.

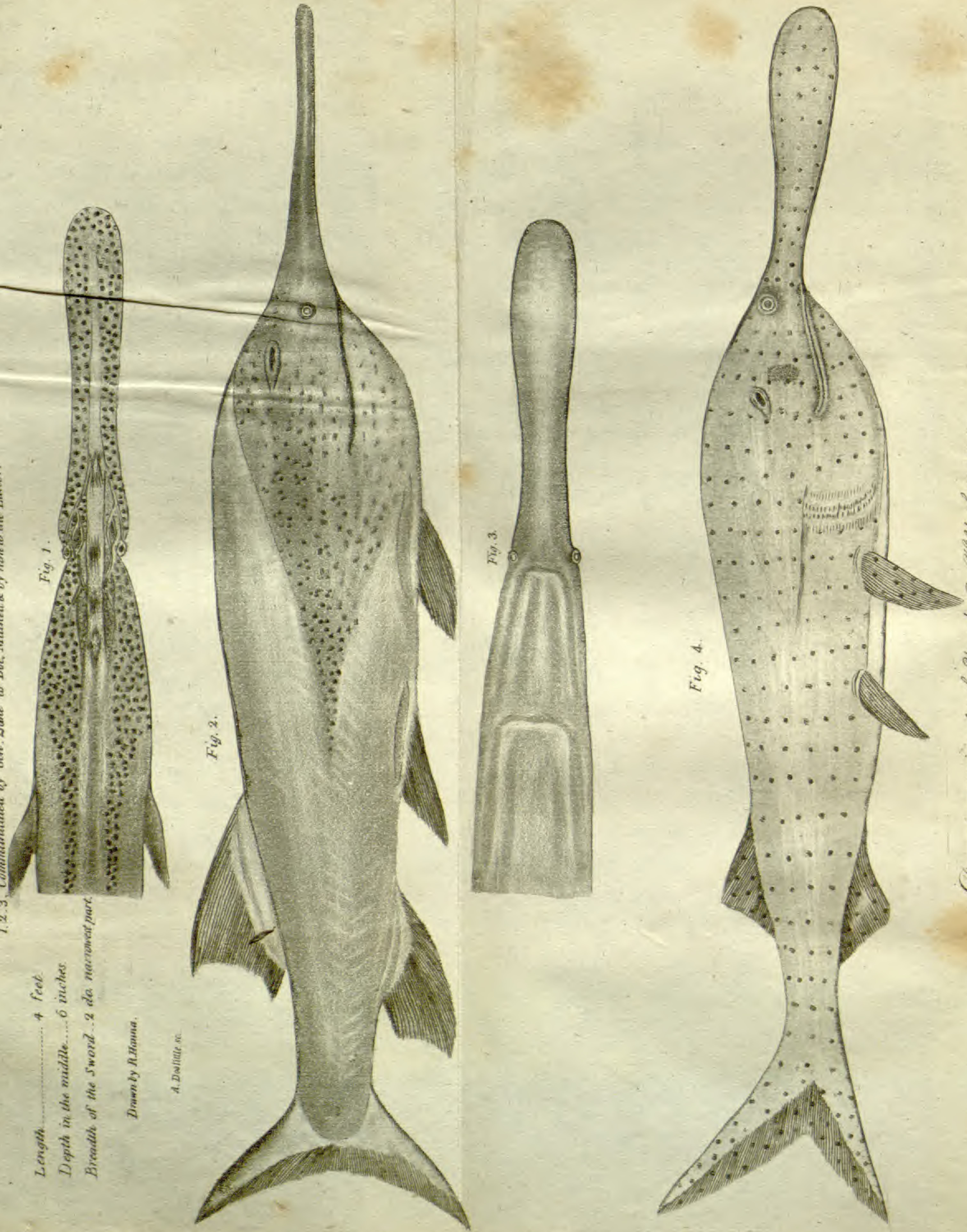
With a battery of 30 jars, containing 4000 square inches of surface, the needle deviated to the amount of 30°. This was effected by placing the galvanometer in a separate chamber, and connecting it with the battery by two copper wires, covered with silk, and suspended by insulating cords. At the end of each of these wires was soldered a very fine point, destined to draw off the electricity. One of these points was applied to the outside of the battery, and the other, held by a glass support, was brought to the knob of one of the jars. When within the distance of 4 or 5 centimetres, the deviation commenced, and at one or two centimetres it advanced to 23°, was then weakened, and ceased entirely after a continuance of 5 seconds. The experiments were repeated in presence of Arago, Ampere and Savary. The want of success with previous experiments by common electricity, is attributed to a defective insulation, and to the want of sufficient care in obtaining a continued current of electricity. This appears to be effected by drawing the current from a battery by means of fine points.—*Ann. de Chem. Sept.* 1826.

24. *New formation of Anhydrous Sulphuric Acid.*—When sulphuric acid is distilled, says M. Gmelin, let the receiver be changed at the moment when it is filled with opaque vapours, and let it be covered afresh with ice. Anhydrous sulphuric acid will be deposited in crystals upon its surface, and liquid acid, less dense than that which remains in the retort, will collect in the bottom. It appears that during the distillation, the acid is divided into two portions, one of which yields its water to the other.—*Ann. de Chem. Juin*, 1826.

Paddle Fish or Spoonbill Sturgeon of the Mississippi.

(Polyodon of La Cepede — Spatularia of Shreber & Shaw.)

1.2.3. Communicated by Genl. Zane to Doc. Mitchell & by him to the Editor.



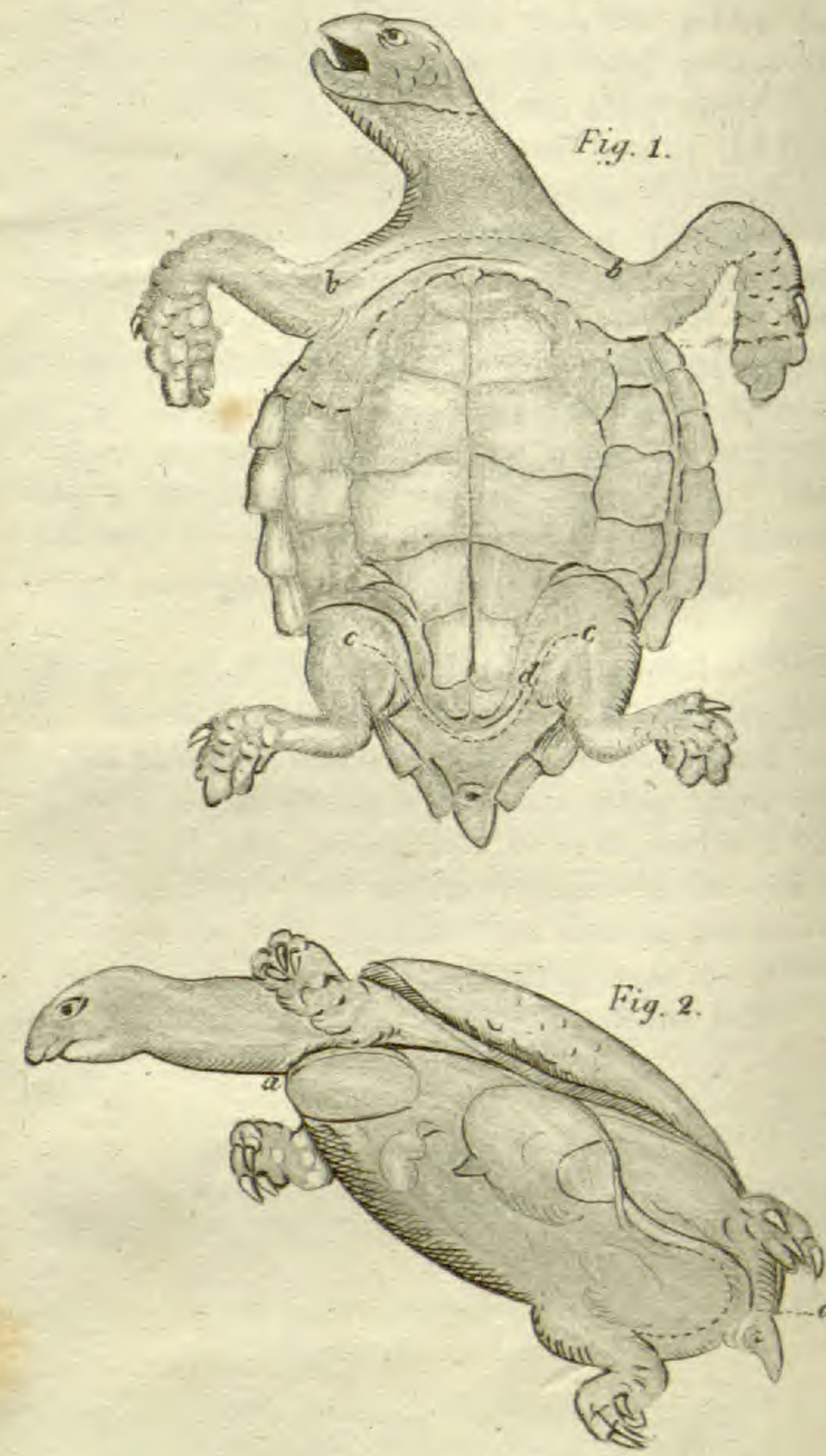
Length .... 4 feet.
Depth in the middle.... 6 inches.
Breadth of the Sword...2 do. narrowest part.

Drawn by R. Hanna.

A. Doolittle sc.

Drawn & communicated to the Editor by Doc. Hildreth. 2/3 full size

Pa. 286.



THE

AMERICAN

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ART. I.—*Notice of the Spoonbill Sturgeon, or Paddle Fish, of the Ohio, (Polyodon feuille of Lacepede.)*

THE fish, whose figures are annexed, appearing to be peculiar to the western waters of this country, and being very rare, even in those regions, it was thought that the notice communicated for this Journal, by Dr. S. P. Hildreth—that of Mr. Clemens to Dr. Mitchill, and the note of the latter to the editor, would prove acceptable, although the fish is not unknown to learned ichthyologists.

The drawings 1, 2, 3, were communicated by Gen. Zane to Dr. Mitchill, and by him to the editor; 4 is by Dr. S. P. Hildreth.

1. *Dr. Mitchill's Notice.*

TO PROF. SILLIMAN.

NEW-YORK, May 21, 1826.

DEAR SIR,

The fish figured by Dr. Hildreth, and mentioned in yours of the 16th inst. to me, is considered by naturalists as peculiar to the waters of the Mississippi.

The popular name is the *Paddle Fish.* It is known by some ichthyologists, by the name of *Spatularia.* But the dominant and modern term is *Polyodon*, from the existence of many small teeth in the throat. The snout has been compared to a broad vegetable leaf, whence the appellation of *P. feuille*, given by Lacepede.

This animal was first brought to my notice, by the late Prof. B. S. Barton, as long ago as the time when we were students together in the University of Edinburgh. Afterwards, Prof. Douglas of the Military Academy, at West Point, brought me the head and some other parts of the fish, from his official journey, with Gov. Cass and others, towards the north-west; since which communications, Gen. Zane sent me a correct drawing, of a large individual of the species, taken in the Ohio, almost as high as Wheeling.

It is more nearly allied to the *Sturgeons* than to any other family; though some have traced an analogy between it and the *Sharks*. I consider the former opinion the more just. If I recollect right, however, Mr. Maudit distinguished it as the *Squalus Spatula*.

There is but a single species to the genus; and it is worthy, perhaps, of the particular attention of Fredonian citizens, by reason of its absence from all the waters of the globe, except the great father of North American streams and his tributaries.

With high and respectful consideration, I remain yours,

SAMUEL L. MITCHILL.

2. *Communication from Dr. S. P. Hildreth to the Editor.*

The specimen in natural history, whose figure is above delineated, is a variety of the finny tribe, peculiar to the waters of the Mississippi and some of its tributary streams. With us it is called the "Spoonbill Sturgeon." It is very rarely seen in this part of the river Ohio; and the subject of this memoir, with two others, is all that I have heard of being taken, since the first settlement of the country; at which time, one or two were caught in a net, or seine, the only way in which any have been taken in this vicinity. From their exceeding rarity, I consider those caught here, as wanderers or travellers who had lost their way, and not as regular inhabitants, as most of the fish in this part of the river may be said to be. The length of this specimen was five feet, and his weight about forty pounds. His particular dimensions are as follows:—Length of spatula or nose, thirteen inches—width at the broadest part, three and a half inches—from the eyes to the back part of the gills, ten and a half inches—from the gills to the pectoral fins, three inches—from the gills to the tail, three feet—caudal fin, very forked ; upper fork much the longest, and twelve inches across—dorsal and anal fin, opposite—from the top of the head to the lower jaw, eight inches—length of the gill flap, seven inches—back and sides a light slate colour, spotted with black—belly, white—skin, glabrous or smooth, like that of an eel, and but lightly covered with mucus—the flesh is very compact and firm, and hard when boiled ; affording no very enticing dish for the epicure.

From its solid muscular structure, it is probably a fish of great activity in its native element. The head is large in proportion to the body, and mouth very capacious, being in amplitude of jaws fully equal to the pike. The jaws are without teeth ; but the fauces are lined with several tissues of the most beautiful net work, evidently for the purpose of collecting its food from the water, by straining, or passing it through these ciliary membranes, in the same manner as practised by the spermaceti whale. Near the top of the head are two small holes ; from their open appearance and apparent communication with the fauces, or back of the mouth, it is possible he may discharge the water through them, in the manner practised by cetaceous animals. At the back part of the head, and attached by its lower edge to the upper part of the gills, is a loose, ensiform membrane, seven inches in length, and three inches in width at the base, and terminating in a point on the sides of the fish. The eyes are placed at the base of the snout or spatula, and further forward, as relates to the head, than is common in most fishes—spine, cartilaginous. Of what use this long nose can be, is not so easy to determine—it is perfectly smooth and delicate ; composed of cartilaginous substance, and two inches thick. It is possible he may use it for moving and digging up the soft mud in the bottom of the river, and when the water is fully saturated, draw it through the filamentous strainers in search of food.

This specimen was caught in a net, near Marietta, in June, 1821. A drawing and minutes of his dimensions were taken at that time, but the anatomical examination, and critical description of all his parts, was not so accurate and particular as it ought to have been, nor, as I shall endeavour to make, should an opportunity offer again—such as it is, however, it may be thought to be worth preserving.

3. *Notice communicated to Dr. Mitchill, by I. W. Clemens, and received June* 30, 1822.

The fish represented by the enclosed drawing, was taken in the Ohio river, a few miles below this place. It was four feet eight inches long, of a lead color on the back—the belly somewhat of a yellow cast—from occiput to tail it very much resembled what is here called a pike. Its snout, eye, and organ of hearing, is much better delineated by the drawing than description.

It had seven fins, viz: one caudal, which was forked and stood perpendicular; one anal, one dorsal, two ventral, and two pectoral—these are the appearances externally. It had five pair of gills, which were double. Each of those duplicatures were thickly set with teeth, of about the diameter and consistence of best Russian bristles, and one and a fourth inches long—the throat rough, and large enough to admit a common sized wrist.

Its heart was single—one auricle, and one ventricle—a very capacious liver and gall bladder: the liver, gall bladder, and ligaments, weighed 5½ ounces avoirdupois.

Its intestines, one continued tube with but one reflection, which was soon after entering the abdomen: this reflection was 2½ inches, and in shape a double curve.

About 3 inches from the anus is an excrescence about 2 inches in diameter, and ½ an inch thick, of a pale vermillion color, and attached to the intestines—in shape like a star-fish.

This excrescence was hollow, but of a thick and firm consistence. The rectum also thick and strong, and divided into 5 cells, in each of which were found a number of worms with flattened heads. It had no food in its intestines—all that was observable, was a small quantity of a substance resembling chyle, but of the consistence of honey. It had no solid bone except in its fins, and even they became cartilaginous as soon as they entered the skin: it had no scales. At the opening of the gills there was a reflection 8 inches in in length, which gradually decussated until it came to a point. This reflection was a very pliable skin, and of a calico appearance.

From the occiput to the tail was found a cartilaginous substance, resembling that found in a sturgeon. There were reflections from the skin, which connected themselves to this cartilage—they were not regular, some perpendicular and others diagonal. The flesh of a beautiful white color, somewhat coarse in its texture, but palatable to the taste. It was a male—the two milts lay longitudinally, and 2¾ inches long—the ductus deferens issuing from the milts, united and opened by a small hole behind the anus.

JAMES W. CLEMENS.

ART. II.—*Notice of Fossil Trees, near Gallipolis, Ohio;* by DR. S. P. HILDRETH.

ABOUT two miles above Gallipolis, and half a mile from the Ohio river, is the location of several petrified trees. They are found near the base of a mural precipice of sandstone rock, 50 feet in height, and crowned with earth and trees to an elevation of 70 feet. From the foot of the rock, the ground gradually descends 30 or 40 feet to the Ohio bottom, which is low and swampy near the hill. This descent is probably made by the debris and earth rolling down from the hill, and gradually accumulating for ages, so as to cover a larger portion of the sandstone rock below the surface, than now appears above. The Ohio river no doubt once washed the base of the rock, but has gradually changed its channel to its present bed.

The rock in which the trees are imbedded, is a coarse sandstone, and they appear in the face of the rock at different elevations, some near the present surface of the ground, and others 4 or 5 feet above. They are 7 in number, and scattered through a space 80 rods in length—some appear to have fallen, or been deposited with their tops, or branches, towards the river, and others in the opposite direction—some came out of the rock obliquely, and others at right angles; they vary in diameter from 8 to 18 inches. I am not satisfied as to what family of trees they belong, but some of them look like elm. They are readily distinguished from the rock in which they are imbedded, by their different color and composition; their color being much darker, and texture much harder; having a reddish brown cast, like iron ore, and so hard as to scintillate briskly, when struck with a hammer or head of an axe, affording evidence of their silicious composition. The interstices of the laminæ, are in some places filled with small crystals of quartz; and in others with thin layers of stone coal. There is evidently a considerable quantity of iron in its composition, as the surface becomes quite red, after being heated in the fire. The cortical part seems to have been more difficult to petrify than the ligneous portions, as it is in most of the trees readily separated from the wood and from the surrounding rock; being also easily broken, and resembling iron rust in color and appearance on some of the trees; and on others like black sand or emery. The trees do not project much beyond the face of the rock, but appear to have been broken off at the same time when the rock was rent in which they are imbedded. Sandstone is the principal rock formation throughout this part of the state of Ohio, forming mural precipices from 50 to 100 feet high, and in some places for half a mile, or a mile in extent, on the margins of the Ohio bottoms on both sides of the river, and ununderlying the river hills and country adjacent for a great distance; appearing near the beds of creeks and ravines, where the superincumbent earth has been washed away by the streams; but is seen no where to better advantage, than near the Ohio river. It is of various qualities; micaceous, argillaceous and quartzose or silicious; some so hard and compact as to make good mill-stones, and nearly resembling granite in color and texture; and some so fine and close grained as to bear the chisel of the sculptor nearly as well as marble. From the position of these fossil remains, I am led to conclude that the trees were brought to this spot by the water, at that remote period when the valley of the Ohio was an ocean, and covered in a vast bed of sand by some great convulsion of nature. The sand in time became cemented into rock, and the spaces occupied by the ligneous parts of the trees were, by infiltration, filled up with silicious particles and iron, with some partial attempts at carbonization. Had there been a large pile of trees in a body, they would probably have formed stone coal, as is the case in the sand rock, a few miles above; but this is only conjecture. There is a bed of stone coal in the same hill, not far from the trees. Native alum and copperas are also found in this vicinity.

Art. III.—*Observations on the Climate and Productions of Washington County, Ohio;* by Dr. S. P. Hildreth.

Judging from the parallel of latitude, one would be led to suppose that the climate of Washington County, was similar to that of the eastern states, embraced in the same latitude;

but observation and experience have produced a different conclusion. Owing to the nature of the soil, or more probably to the influence of the southern breezes, which find their way up the valley of the Ohio river, the springs are at least two weeks earlier, and the autumns two weeks later, than they are east of the Alleghany Mountains. These observations more particularly apply to the country bordering on the Ohio river, as there is a marked difference in the time of the blossoming of fruit trees at the distance of twenty miles back from the river, they being at this distance several days later, nearly a week in most seasons. Peach trees are usually in blossom by the 20th of March; and at Bellepre, twelve miles below, and a little to the south of Marietta, I have seen them in blossom the last of February. Apple trees generally blossom the 1st of April, and by the fore part of May have fruit of the size of a musket ball. But the most blooming prospects are sometimes turned into sadness and disappointment, by an unexpected frost about the middle of April; usually taking effect after a spell of warm growing weather; and may, perhaps, be in part occasioned by the great absorption of caloric, from the rapid growth of vegetation through our forests and fields.

Fruit trees, of all kinds, suited to the climate, grow with wonderful rapidity; peaches being often produced the third year after the stones are planted, and apples in four or five years from the seed. Engrafted scions have been known to bear fruit the same season in which they were set. So rapid is the growth of apple trees that there are several in this vicinity whose trunks are six feet in circumference; the ground which they now occupy being covered with the primeval forest thirty years ago. Cherries, plums, quinces, &c. flourish with great luxuriance, with their more humble neighbors, raspberries, gooseberries, currants, &c. Pear trees have been subject to the same disease, so destructive to this beautiful and useful tree, as that which has prevailed in the eastern states. The disease appears to be occasioned by the ravages of an insect of the beetle family, (scolytus pyri) very small, but sufficiently large to kill the largest trees in a few seasons, by destroying the laburnum under the bark of the branches. The remedy used by myself for several years, has been to cut off the decaying branches, below, in the sound wood, as fast as they appear to be diseased. Under this course, some of the largest and oldest trees have regained a healthy appearance. Nearly all the first planted pear trees in this county, are entirely dead from this disease. We have a large winter pear, whose fruit sometimes weighs thirty-six ounces; and apples often weigh from twenty to twenty-six ounces on the young and healthy trees. They are also of the most choice and rare kinds; having been selected in the first settlement of the country from the best orchards in Massachusetts, Rhode-Island, Connecticut, and New-Jersey. The climate suiting that variety of fruit, apples have greatly improved in size, flavour, and beauty, since their introduction into this county. It is probable, however, that as their growth is so much more rapid, their duration will be less than at the eastward; but that is of small consequence, as any person who will take the trouble to plant, may have a bearing orchard in six years.

"Indian corn planting" commences the 1st of April, and continues till the middle of June, thus affording to the husbandman ten weeks, in which he may plant and be certain of a crop—a very great convenience to those who are opening a new farm, and who are unable to clear their lands early in the spring. Indian corn, in this climate, if planted in May, on rich land, and well cultivated, never fails of producing a good crop, let the season be as it may, either very wet or very dry; for twenty years, the period of time the writer has been in this county, there has not been a single failure of the crop of corn, in the rich bottom lands. Crops of potatoes, oats, flax, &c. sometimes fail, but corn seems so well suited to the soil and climate, that it may be considered a certain crop.

In autumn, the farmer has from September to the middle of December to sow his wheat; the ground being sufficiently open till that time, in most seasons, for ploughing. If not sown by the middle of November, they prefer waiting until February, as the frost is injurious to the tender roots of the late sown grain. Wheat is ready for reaping the last of June, and rye by the twentieth of the same month. The wheat in the two last years has been much injured, by an insect of the moth, or miller tribe, the egg of which is deposited in the grain while in the milk, and hatched into a winged insect, after the wheat is in the barn, or stack, making its appearance in August or September; being active earlier or later, according to the temperature of the place where the wheat is deposited, warmth accelerating, and cold retarding its growth. Before its exit from the *kernel*, the grain is completely excavated, leaving only the cuticle, or bran, still retaining its shape. The insects were much more plenty in

1825, than in the last year; the mild winter of 1824, preserving the miller unharmed, while the cold of 1825, nearly exterminated the race. They are natives of a more southern climate, being found on the Mississippi every year; and in proof of their migrating character, it is in evidence, that they are confined as yet to the neighborhood of the Ohio river and tributary streams, and have extended their travels but a few miles into the country on either side of the river. It has been observed that reaping the wheat early, while yet in the milk, effectually prevents their ravages; the shrinkage of the grain compresses so closely the tender larva as to destroy its life. Previously to the year 1825, this insect had not been known to injure the wheat in this county.

The productions of the garden are abundant, and in fine perfection. In my garden we have peas fit for the table by the middle, or twentieth of May—cucumbers in the beginning of June, and early York cabbage, with well formed, hard heads, by the middle of the same month; early corn, fit for boiling, is common on the fourth of July; and other articles of the vegetable family are equally early.

This climate seems to be well adapted to the cultivation of the vine. Six or eight kinds of foreign grapes are cultivated in gardens with success. The vines flourish with great luxuriance, and produce abundantly, the finest grapes. Instead of plucking the leaves to admit the rays of the sun, as directed by European cultivators, the clusters here need sheltering from the fervid heat of August, those grapes being much more sweet and well flavored that grow in the shade. Amongst the kinds cultivated are the white and purple, schaslar, or sweet water, Madeira, muscadine, and Cape of Good Hope; these all stand our winters well, unless more than usually severe. Next season I shall try the raisin grape from cuttings now growing in Clarksburgh, Va. about eighty miles south-east from Marietta, raised from the seed. I propose engrafting them into stocks of our native hill grapes, which are of the raisin kind, the roots of which are already planted in my garden.

Wine, to a large amount, might be made from our native grapes, if persons acquainted with the process would turn their attention to it. Many barrels are annually made, half grapes and half cider, with the addition of some spirit, affording a very palatable liquor. The uplands are in many places literally loaded with grapes; the vines spread themselves on low trees and shrubs, while the fruit remains hanging in crowded clusters, long after the leaves are all fallen, affording the most delicious repasts to the bears, raccoons, and other wild animals of the forest.

Another source of comfort to these remains of the aboriginals, is found in the great quantities of nuts that abound in the remote parts of the county, as yet uncultivated. The nuts of the beech, chesnut, black walnut, butternut, various kinds of hickory nuts, besides the acorns and more humble chinquapin and hazel nut, literally cover the ground in many places, and large droves of hogs are fattened without any expense to the owners.

The woods abound with the native or purple mulberry, whose leaves are said to afford food for silk worms, fully equal to the white mulberry; and that the worms will flourish here, was proved, more than twenty-five years since, by the family of the late Gen. Rufus Putnam, whose females, in the early settlement of the county, used to supply their own sewing silk from the cocoons of worms of their own raising; and in the year 1806, at Bellepre, a sister of my wife reared more than six thousand silk worms; these were fed from the leaves of the white mulberry, as it was then supposed no other would answer. They were very healthy, and furnished the raw material for many yards of silk, had any one known how to manufacture it. Since then, the raising of silk worms has not been attempted, but many of the inhabitants have turned their attention to the raising of an article next to silk in fineness, beauty, and value, and that is merino wool.

Large flocks of merino sheep are owned in this county. Mr. Seth Adams was the first person who introduced them into this state, and I think he imported them himself from Spain, as early as the year 1804 or 1805. The blood of this valuable animal has been kept with great purity by several highly enlightened cultivators; and from the mildness of the climate and the well directed efforts of the owners, the merino sheep has much improved in size, beauty of form, and fineness of wool, and will ultimately become a prolific source of wealth and independence to the state. They are far more healthy than the common sheep, and require no more food or extra attention. Their increase is rapid, as it is not uncommon for a well fed, healthy ewe, that has yeaned in the winter, to bring forth again in the autumn of the same year; and if she has twins at each birth, as they are often known to do, a flock may be quadrupled in a short time. One cause of the health and rapid increase of sheep here, is the exemption from long, cold storms of rain and snow.

The climate for placidity, may be compared to that of the western Pacific Ocean, subject to few excesses of airy eccentricity, high winds or storms of any kind being very uncommon. If we have a snow storm, the snow falls evenly and quietly, never drifting or thrown up into heaps by the wind, so that we may in fact say we never have what is properly a "*snow storm.*" The snow is usually attended with a gentle breeze from the north-west. Storms, when we have any, are in the summer, accompanied with thunder and lightning, and sometimes, though rarely, with hail, and a high wind for a few minutes only. Storms of rain from the north-east are unknown here. Autumn is very pleasant and delightful; so mild, so quiet; the orchards bending under their loads of yellow fruit, and the woodlands putting on a thousand varying hues, as the trees gradually prepare to shed their "leafy honours," not precipitated by untimely frosts, but like a green and vigorous old age, giving up their strength and beauty to the gradual decay of nature. Winter makes his approaches with caution, as if loth to disturb the serenity of autumn, and is seldom here until the last of December.

In the early settlement of the county, when the woods were full of wild plants, neat cattle could live very comfortably the whole winter, without any assistance from man; and at this time large numbers of hogs pass the winter, as independently as the deer and the bears, subsisting on nuts and acorns. Single individuals are sometimes destroyed by the bears or wolves, but a gang of ten or twenty hogs are more than a match for a wolf or a panther. An old hunter informed me that he once saw a large panther spring from a tree into a drove of woods hogs, who were aware of his approach, and prepared for defence; the moment he touched the ground, the large hogs fell upon him with their tusks, and the weight of their bodies, and killed him, and tore him in pieces in a few minutes. A few years ago, wolves were so abundant, that it required great care to raise a flock of sheep; but the bounty bid on their scalps by the state, and by spirited individuals, has so diminished their numbers, that danger from them is but comparatively small.

The first settlers of the county landed at the mouth of the

Muskingum river, on the 7th day of April, 1788 ; at that time the trees were putting forth their leaves, and the bottoms were covered with a growth of herbs and grass nearly knee high ; seeming almost like the work of enchantment to these weary travellers, who had descended the Ohio in boats, and left but a few days before the country at the " head waters," near the mountains, as destitute of vegetation as in the midst of winter. This day was for many years celebrated as " an anniversary" by the inhabitants, on which they feasted and made merry, as they recounted the dangers and privations of the " first settlers." Their numbers then amounted only to a few individuals, now to at least eight hundred thousand, enjoying civil and religious privileges, equal, if not superior, to any other state in the Union.

In proof of the mildness of the climate, beyond what would be inferred from latitudinal position, there is now growing on the waters of Fish Creek, in Virginia, ten or twelve miles from the Ohio river, and fifty-two east of Marietta, a grove of the towering magnolia, of several acres in extent. Some of the trees are 2 or 3 feet in diameter, and lofty in proportion. They are situated on the upland, a little distance from the creek, and in the season of flowering, fill the wilderness with delicious fragrance for several miles round ; though probably not quite so far as the flowers of the magnolia were smelt at sea by the first discoverers of East Florida, which I think was stated to be at the distance of 60 miles, as they approached the land, and long before the land was in sight. I have heard of no other grove or detached trees, within 500 miles of this place. I have not seen the blossoms, that the variety may be described ; but from the magnitude of the leaves, more than 3 feet in length, and of proportionate width, I should suppose them to be the same that grow in Alabama and Florida. A gentleman living near them has engaged to furnish us some of the young trees next spring, when an attempt will be made to domesticate them as ornamental trees. The melia azedarach, or Pride of China, stands the winter when not very severe ; several trees, the produce of seeds from the Mississippi, are now growing in this town.

As we proceed westward, the climate is still more mild ; and in the southernmost bend of the Ohio river, near the mouth of Big Sandy, I should judge the temperature to be about equal to that of Italy, in the days of Pliny and Tacitus.

Marietta, Ohio, Jan, 12th, 1827.

ART. IV.—*Abstract of Meteorological Observations, made at Marietta, Ohio, in the year* 1826—*Thermometer placed in the shade, in a N. E. exposure, and Observations taken at* 7 *A. M. in winter, and at* 6 *in summer, and at* 2 *and* 9 *P. M. in lat.* 39° 25′ *N. and lon.* 81° 30′ *W. of London.* By Dr. S. P. HILDRETH.

Months	THERMOMETER.			WEATHER.			RAIN.		Course of the Winds.
	Day the highest.	*Day the lowest.*	*Mean for month.*	*Fair*	*Cloudy.*	*Variable.*	*Inches*	*100th.*	
January	68—19th	—1 16th	30 22-100	9	14	7	2	40	N. 2d day—N W. 10—W. 6—S. W 11—S. 2
February	67—25th	14—15th	37 20-100	11	15	2	3	65	N. 4—N. W 12—W. 4—S W. 8—S. 3—N. E. 1
March	80—23d	20—27th	47	8	17	6	6	50	N. 7—N. W. 13—S. W. 8—E. 2—N. E. 1
April	82—17th, 24th	22—11th	51 17-100	10	13	7	4	60	N. 4—N. W. 9—S. W. 7—W. 7—S. E. 2—N. E. 1
May	92—2d	38—6th	67 80 100	20	4	7	2	00	N. 9—N. W. 5—W. 4—S. 6—S W. 6—S. E. 1
June	90—2d, 7th	53—13th	73 80-100	12	7	11	6	00	N. 4—N. W. 4—W. 10—S. W. 10—S. 1—S. E. 1
July	94—9th	50—25th	72 36-100	18	4	9	5	70	N. 14—N W. 1—S. W. 12—S E. 1—N. E. 1—W. 2
August	95—15th	52—22d	71 35-100	23	2	6	1	55	N. 5—N. W. 3—W. 3—S. W. 7—S. E. 6—N. E 3—E. 3
September	92—20th	45—30th	66 36-100	14	11	5	1	35	N 7—N. W. 2—W. 4—S W 8—S. 5—N. E. 2—S. E. 2
October	86—6th	24—25th	55	15	12	4	3	00	N. 7—N. W. 6—W. 4—S W. 5—S 4—E. 4—S. E. 1
November	74—6th	20—29th	43	13	13	4	2	15	N. 4—N. W. 8—W. 5—S. W. 6—S. 3—S. E. 2—N. E. 2
December	68—6th	4—28th	32	14	10	6	2	70	N. 4—N. W. 13—W. 5—S. W. 6—S. 3—S. E. 3—E. 4—N. E 2
Mean temperature for the year, 54 degrees.							41	60	

N. B There has been, this year, an unusual number of winds from the N. and N. W. Nearly every rain this past summer, has been followed with wind from the northward, when in many previous summers the wind continued to the southward after rain.

I am indebted to JOSEPH WOOD, Esq. Register of the U. S. Land Office in this place, for the following abstract of rain, which fell in the years stated, viz.—In 1818, 4 feet 3 inches—1819, 3 feet ½ inch—1820, 3 feet 4 inches—1821, 3 feet 7 inches—1822, 3 feet 7½ inches—1823, 3 feet 4 inches, to October, after which no Journal was kept.

Art. V.—*Fluids in the Cavities of Minerals.*

We have long owed to Dr. Brewster, of Edinburgh, a correction of a hasty statement respecting the new fluids which he discovered in the cavities of quartz crystals, topaz, and other minerals. These fluids were incidentally mentioned, in an account which we drew up in the summer of 1824, and published in the 8th volume of this Journal, p. 282, entitled "Facts tending to illustrate the formation of crystals in geodes." The fluids discovered by Dr. Brewster, were mentioned, as objects only of microscopic observation. Microscopes were used in examining the phenomena, but it is also true, as Dr. Brewster remarks, (Edinburgh Journal of Science, Vol. II. p. 141,) "that some of the cavities are nearly the *fifth* of an inch long, and that the fluids have been taken out of the cavities, looked at with the naked eye, and touched, tasted and subjected to chemical experiments."

The discovery of Dr. Brewster appeared to us very interesting, and we cited the earliest notice of it from the Edinburgh Journal. (See Vol. VII. p. 186, of this Journal.)—We have been ever since intending to give an abstract of Dr. Brewster's more detailed account of his discoveries. On reviewing this account, it seems, however, to be scarcely capable of much condensation, without material injury, and therefore we now publish it entire, with the annexed plate. It is an abstract by the author, of a paper communicated by him to the Royal Society of Edinburgh, in March, 1823, and which was ordered to be printed in the 10th volume of their Transactions.

On the existence of two new Fluids in the Cavities of Minerals, which are immiscible, and possess remarkable physical properties. By David Brewster, LL. D. F. R. S. Lond. and Sec. R. S. Edin.

The unpublished memoir, of which we now propose to give an abstract, is divided into eight sections, namely,

Sect. 1. On the existence of a new fluid in the cavities of minerals.

Sect. 2. On the co-existence of two immiscible fluids, of different physical properties, in the cavities of minerals, and accompanied with a vacuity.

Sect. 3. On the phenomena of two immiscible Fluids, *without a vacuity*, in the cavities of minerals.

Sect. 4. On the changes which these fluids have undergone in particular crystals.

Sect. 5. On the vaporisation and decomposition of the new fluid at low temperatures, when enclosed in the cavities of minerals.

Sect. 6. On the phenomena of the two new fluids when taken out of the cavities.

Sect. 7. On the existence of moveable crystals in a fluid cavity of quartz.

Sect. 8. On the phenomena of a single fluid in the cavities of minerals and artificial crystals.

SECT. I.—*On the existence of a new fluid in the cavities of Minerals.*

While examining the cavities of crystallised bodies, our author observed such remarkable differences in the phenomena of the fluids which they enclosed, that he found it impossible to explain them upon the supposition of their being fluids possessing the ordinary properties of that class of bodies. Hence he was led, by the following train of reasoning, to ascribe these phenomena to new fluids, possessing new physical properties.

In examining the topazes from New-Holland, Scotland and Brazil, he observed the cavities arranged in strata. These cavities are sometimes beautifully crystallised, and sometimes amorphous, sometimes extremely shallow, and at other times deep.

They are filled with a colourless and transparent fluid, as shown at ABCD, fig. 1, plate 2, and have almost always a vacuity V, of a circular form, which moves by an inclination of the plate to different parts of the cavity. The depth of the cavity may be easily estimated by the breadth of its bounding line ABCD, which, in the flat cavities, is generally the same as that of the circle V. In very shallow cavities, this boundary is a narrow line, scarcely visible, and in deep ones it is broad, with a penumbral termination inwards, arising from the deviation of the light at the separating surfaces of the fluid, and the topaz, and at that of the fluid and the vacuity.

When the hand is applied to the crystal, the heat of it gradually expands the fluid. The vacuity V consequently diminishes, and being in a short time reduced to a physical point, it entirely disappears. When the fluid again cools, by withdrawing the hand, it of course contracts and quits the sides of the cavity. The vacuity V re-appears, increasing till it resumes its former magnitude; and it deserves particular notice, that the evanescence and re-appearance of the vacuity takes place simultaneously in many hundred cavities, of the same general form, which may be seen in the field of view.

In order to obtain an accurate measure of the temperature at which the vacuity re-appears, which is almost the same as that at which it vanishes, our author plunged the topaz in heated water, and, by means of an accurate thermometer, obtained the following results:

Nature of the cavities.	*Temperature at which the vacuity re-appeared.*
1. Topaz from New-Holland, with shallow cavities,	$74\frac{1}{2}°$
2. Blue Topaz from Aberdeenshire, with cavities of different forms,	74°-82°
3. Colourless topaz from Brazil,	$79\frac{1}{2}°$
4. Topaz from New-Holland, with large and rugged cavities,	$79\frac{3}{4}°$
5. Topaz from New-Holland, with a very flat cavity,	$81\frac{1}{4}°$
6. Another colourless topaz from Brazil, with a deep cavity,	$83\frac{3}{4}°$

When the cavities are very small and narrow, only one vacuity re-appears; but when they are large, several small circular vacuities make their appearance, and gradually unite into one, though sometimes they remain permanently separate. When the cavities are deep, a very remarkable phenomenon accompanies the re-appearance of the vacuity. At the instant that the fluid has acquired the temperature at which it quits the sides of the cavity, a rapid ebullition takes place, and the transparent cavity is for a moment opaque, with an infinite number of minute vacuities, which instantly unite into one vacuity, that gradually enlarges as the temperature diminishes.

In order to determine the expansion which takes place by a given increment of temperature, our author measured the relative size of the vacuity, and the cavity at the temperatures of 50° and 80°, the temperature at which the fluid had expanded, so as wholly to fill the cavity. In many cases this could be estimated with tolerable accuracy, and it may be stated in general, from the estimates and measures taken by different persons, to whom the cavities were shown, that the fluid expands fully *one-fourth* of its size, by an increment of 30° of heat; and that it is *nearly* 32 *times more expansible than water, by an increment of* 30° *of heat at the temperature of* 50°.

This extraordinary result proved beyond a doubt, that the substance contained in the cavity was a new fluid, differing from all known fluids in its high expansibility, and resembling in this respect a gaseous more than a fluid body.

In order to confirm this result, our author was desirous of examining the other physical properties of this remarkable substance. He noticed, in the deep cavities especially, the singular volubility of the fluid, and its slight adherence to the sides of the cavity, as indicated by the motion of the vacuity V. In small cavities containing water, the adhesion of the fluid to the stone is so strong, that the air-bubble moves with extreme difficulty, and even when very large, it often changes its place by starts, and remains stationary at the bottom, or in the middle of the cavity. In the present case, however, the vacuity moved about with great facility, and in the cavity $\frac{1}{20}$ of an inch long, by $\frac{1}{58}$ and $\frac{1}{62}$ of an inch wide and deep, the slightest tap of the finger on the microscope, caused the air-bubble to tremble and oscillate in this microscopic level. Hence the new fluid is distinguished by a second physical property, no less remarkable than the first.

Although no doubt was now entertained of the accuracy of the conclusion, that the fluid was a new one, yet it was conceived possible to obtain an approximate measure of its refractive power, and thus to put its novelty beyond the reach of a doubt. In order to do this, it became necessary to observe the manner in which the total reflexion of the upper surface of the cavity was modified by the contact of the fluid, and to measure the angle at which total reflexion was effected, by the separating surface of the fluid and the solid. For this purpose, our author took a plate of topaz AB, fig. 2, with a stratum of cavities *mn*, perfectly parallel to the natural surface of the plate. He then placed upon each surface the rectangular prisms ABC, ABD, and introduced between them a thin film of oil of cassia. Rays of light RS, RS were then allowed to fall upon the stratum of the cavities *mn*, so that the rays reflected from the upper surface of the cavity could be examined by a microscope, whose object lens is LL. Upon making this arrangement, the stratum of cavities was seen in the most beautiful manner. The vacuity V, fig. 3, of a cavity seen in this way, shone with all the brilliancy of total reflexion, the separating surface of the new fluid ABCD, and the cavity, exhibited a faint gray tint, while the surrounding portions of the solid topaz were comparatively black. The variations which the vacuity V undergoes by heat, are now finely seen, and, at a temperature of 80°, it vanishes in a brilliant speck, leaving the whole of the cavity ABCD of the same uniform tint as in fig. 4.

The phenomena now described are not so distinctly seen when the stratum *mn* is deeply seated beneath the surface of the topaz, in consequence of the duplication and overlapping of the images formed by double refraction.

This inconvenience, however, may be nearly removed by making the plate of topaz very thin; or it may be entirely remedied, in plates of any size, by making the incident rays RS pass along one of the resultant axes of the topaz, while the reflected rays SL pass along the other resultant axis.

In order to compare the angle at which total reflexion took place at the upper surfaces of the fluid and cavity, with that which would have taken place had the fluid been water, a drop of water was placed on part of the lower surface of the plate AB, fig. 2, and it was found that the light reflected at the same angle of incidence, was much more brilliant from the separating surface of the new fluid and the cavity, than from the separating surface of the topaz and the water, a result which indicated in the most unequivocal manner, that *the new fluid had a refractive power inferior to water, and that it differed in this respect from every other known fluid.*

With a specimen of *amethyst*, our author was enabled to determine that the refractive power of the expansible fluid was about 1.211.

In the remainder of this section, the author describes analogous phenomena in *cymophane*, *quartz*-crystals from Quebec, and *amethyst* from Siberia, the last of which is a specimen of very great interest, from the cabinet of Mr. Allan. In these crystals the vacuities re-appear as follows:

Cymophane,	$83\frac{1}{4}$° Fahr.
Quartz from Quebec, different cavities in the same specimen,	76° 80° 125°
Amethyst from Siberia,	$83\frac{1}{3}$°

SECT. II.—*On the co-existence of two immiscible fluids, of different physical properties, in the cavities of minerals, and accompanied with a vacuity.*

The phenomenon of two immiscible fluids, as exhibited in topaz, is represented in fig 5, where V is the vacuity, NNN the new fluid, and WWW another fluid, which we shall distinguish by the name of the *second fluid*. This second fluid WW commonly occupies the angles of triangular cavities, as in Fig. 5, or the terminations of longitudinal ones. It is always separated from the new fluid by a curved surface *m n*, *m n*, &c. It never expands perceptibly with heat, and never mixes with the new fluid NN. By a little management, the vacuity V may be made to come in contact with the bounding lines *m n*, *m n*, &c.; but it never affects its curvature, and seldom enters the fluid W. When the vacuity V has been made to vanish by heat, these bounding lines remain exactly the same.

Having at first observed this second fluid only in the angles of cavities, as in Fig. 5, considerable difficulty was experienced in proving that it was a fluid. The difficulty of conceiving two fluids existing in a transparent state, in absolute contact, without mixing in the slightest degree, induced several persons to refer it to an optical illusion, and to consider the line which separated it from the new fluid as a septum or partition in the cavity. The beautiful curvature of the bounding line, however, and its perfect similarity to that of two contiguous fluids, rendered this supposition untenable.

Having found specimens in which the second fluid occupied a large part of the cavity, most of the difficulties which had formerly presented themselves were removed; but something was still wanting to prove its fluidity. This desideratum was fortunately obtained in a specimen of topaz belonging to Mr. Sivright. In examining this specimen, I observed a very remarkable cavity, of the form shewn in fig. 6, where A, B and C are three separate portions of the new fluid (shaded lightly,) insulated by the interposition of the second fluid DEF (shaded darkly.) The first portion A of the new fluid had four vacuities V, X, Y, Z, while the other two portions B, C, had no vacuity. Having often succeeded in making the vacuities pass from one branch of a cavity to another branch, our author did not doubt that the vacuities of the portions B and C had passed over the second fluid into the portion A. In order to determine this, an accurate drawing of all the phenomena was taken at a temperature of 50°, as represented in fig. 6, and the changes carefully watched which took place, by raising the temperature to 83°. The new fluid at A gradually expanded itself, till it filled all the four cavities V, X, Y, Z; but as the portions B, C, had no cavities for this purpose, they could only expand themselves, by pushing back the supposed second fluid DEF. This actually happened. The second fluid quitted entirely the edge of the cavity at F. The two portions of new fluid B, C, were immediately united into one; and the second fluid having retreated to its new limit *m n n′ o*, and being itself but slightly expansible, like common fluids, its other limit necessarily advanced to *p q r*. This experiment, which has often been repeated and shewn to others, involves one of those rare combinations of circumstances, which Nature sometimes presents to us, in order to lay open some of the most mysterious of her operations. Had the portions B, C, of the *new fluid* been accompanied, as is usual, with their vacuities, the interposed *second fluid* would have remained immoveable between the two equal and opposite expansions: but, from the accidental circumstance of these vacuities having passed over into the other branch A of the cavity, the *second fluid* is placed in a sort of unstable equilibrium, and, like the arms of a lever, it yields to every variation of the power and of the resistance.

If any additional evidence were wanted on this subject, we have only to examine the mode in which the two portions of the new fluid B, C, are united into one, by a disunion of the second fluid at *g h*, and again separated by its reunion. Upon the application of heat, the summits *g h* become more acute, and gradually approach to each other, till they suddenly unite, and force back the surface of the second fluid into the line *m n n′ o*. A portion of the second fluid, however, is retained by capillary attraction, in the angular meeting of the planes, between *c* and F, and between *d* and F, and also a small portion at *f*, a phenomenon which affords an ocular explanation of the immobility of the second fluid in the terminations and angles of cavities. When the fluids again cool, the surface *n n′* approaches to *c d*, and when *n* is near *c*, the two surfaces *n n′*, and those of the same fluid in *c* F and *d* F, suddenly start into union, in virtue of their mutual attraction, and the portions B and C are again separated.

In order to examine the refractive power of the second fluid, our author made the arrangement represented in fig. 2, and found that the second fluid W always reflected less light than the new fluid, and consequently that its refractive power approached nearer to topaz than the new fluid. By the same means, he determined that the angle at which total reflexion took place at the separating surface from the topaz, was very nearly the same as if it were water.

Two immiscible fluids, possessing the properties now described, exist also in *Quartz*, *Amethyst*, and *Cymophane*, and there is reason to conclude that the one never occurs without the other, as the second fluid has, in almost every case, been discovered in cavities where the difficulties of observation had at first prevented it from being detected.

Passing over the *third* section, in which our author explains the phenomena of two immiscible fluids coexisting without a vacuity; and also the *fourth* section, in which he shews that the fluids are sometimes indurated like a resinous substance within the cavities, we come to

SECT. V.—*On the Vaporisation and Decomposition of the New Fluid at low Temperatures, when enclosed in the Cavities of Minerals.*

Let ABCD, fig. 7, be the summit of a crystallised cavity in topaz, and let the length of the cavity be in a vertical direction, so that SS is the second fluid, NN the expansible fluid, bounded by a circular line *a b c d*, and V the vacuity in the new fluid, bounded by the circle *e f g h*. Let the face ABCD be placed under a compound microscope, so that the rays of a luminous body incident upon it, may be reflected at an angle less than that of total reflexion. When the observer now looks through the microscope, the temperature of the room being 50°, he will see the second fluid SS shining with a very feeble reflected light, the new fluid NN with a light perceptibly brighter, and the vacuity VV with a considerable brilliancy. The boundaries *a b c d*, *e f g h*, are marked by a well-defined outline, and also by concentric coloured rings of thin plates, produced by the extreme thinness of each of the fluids at the edges.

If we now raise the temperature of the room gradually to 58°, we shall observe a brown spot appear in the centre of the vacuity V *e f g h*. This spot marks the visible commencement of evaporation from the new fluid below, and arises from the attenuated vapour which attaches itself to the roof of the cavity. As the heat increases, the brown spot enlarges, and becomes very dark. It is then succeeded by white, and one or more rings rise in the centre of the vacuity. The vapour then seems to form a drop, and all the rings disappear, by retiring to the centre, but only to reappear with new lustre. During the application of heat, the circle *e f g h* is in a state of constant contraction and dilatation, like the pupil of the eye when exposed to light, being always greatest when the rings disappear, and contracting its dimensions when they are again formed.

When the vaporisation is so feeble as to show itself only by a single ring of one or two tints of the second order, these tints may be made to disappear instantly by the slight degree of heat arising from a single breath upon the crystal; and the same effect is produced by the approximation of a heated body. When the heat reaches the fluid, however, it makes it throw off fresh vapour, and the rings again appear.

If we put a drop of Ether upon the crystal when the rings are in a state of rapid play, the cold occasioned by its evaporation immediately causes them to disappear, till the temperature again rises.

When the temperature is perfectly uniform, the rings remain stationary, and it is interesting to observe the first ring produced by the vapour swelling out to meet the first ring at the margin of the fluid, and sometimes coming so near it, that the darkest parts of both form a broad black band.

As the heat increases, the vacuity V advances to the summit AB, and disappears at 79½°, exhibiting several curious phenomena which we have not room to describe. One of these, however, is so singular that it deserves to be particularly noticed. After the vacuity V *e f g h* has disappeared entirely, a brown spot comes from the summit AB, and takes its station in the centre of the ring of new fluid *a b c d*. This brown tint sometimes rises to higher orders of colours; but disappears by the application of heat. That the coloured rings formed within VV are vapour, and not a film of the fluid itself, may be inferred from its never mixing with the fluid with which it is in immediate contact. It might, however, be a fluid substance, arising either from the decomposition of the fluid itself, or from the condensation of gaseous matter within the vacuity; though this is not very probable, from its constant disappearance when it has accumulated to a certain degree, and its constant reproduction while the temperature remains the same.

These views respecting the vaporisation of the expansible fluid, have been fully confirmed by the discovery of cavities, in which the expansible fluid occupies only *one-third* or *one-fourth* of the cavity. These cavities are represented in fig. 8, where AB is the cavity, V the vacuity in the expansible fluid *m n o p*, and A *m n*, B *p o* the second fluid. When heat is applied to this cavity, the vacuity V does not contract, as in ordinary cases, but *expands*, till its circumference coincides with the boundary *m n o p*. This unexpected effect might have arisen from the expansible fluid occupying the lower part of the cavity below V, as in the section, fig. 9. In this case *c e f d* might have been the vacuity, and the surface of the fluid *e f* might have risen by heat, and gradually filled the vacuity V, while its boundary at *c* and *d* retired to *m* and *n* as *e f* ascended. In order to determine if this supposition was true, I placed AB vertically between two rectangular prisms of glass; and having examined in succession the light reflected from the surfaces *m p* and *n o*, I found that it had suffered total reflexion, both from the side *c d* and the side *g h* of the vacuity, and consequently that the vacuity occupied the whole thickness of the cavity. After the heat was applied, the sides *c d* and *g h* continued equally luminous, and when *c g* and *d h* had retreated to *m n* and *p o*, as shewn in fig. 10, it became quite manifest that the space *m n o p* was not filled with the *expanded fluid*, but with the fluid *in the state of vapour*. The coloured rings at first appeared both on the faces *c d* and *g h*, and when the whole was converted into vapour they disappeared, and the light reflected from both the surfaces *m p*, *n o*, which was now uniform, was not that of total reflexion, nor yet that of the expanded fluid, but of an intermediate intensity, corresponding to that of a dense vapour, with a refractive power much lower than 1.211.

There is another set of phenomena of exquisite beauty to an optical observer, which seem to arise either from the decomposition of the fluid, or the condensation of gaseous matter in the vacuity.

When heat is applied to the cavity, the new fluid has its surface in a state of constant agitation, resembling, in the closest manner, a surface into which a fluid is discharging itself by drops. When the vacuity is just filled up, one or more drops quit the point where the vacuity disappeared, and pass along the surface of the cavity, like a drop of oil adhering to it in close contact, and never mixing with the fluid. Each of these drops begins in a short time to spread circularly, and to exhibit within its disc an immense number of close coloured rings. By slow cooling, the drops become thinner, and the rings less numerous, and more completely displayed, till they entirely disappear at a particular temperature. When the cooling is effected quickly, the matter which composes the thin plate that exhibits the rings, discharges itself rapidly in gaseous bubbles.

SECT. VI.—*On the Phenomena of the two new fluids when taken out of the cavities.*

From the extreme minutenes of the cavities in topaz, our author's first attempts to extract the fluid were not attended with much success; but he at last fell upon a method by which he has opened more than a hundred cavities.

When the most expansible of the new fluids first runs from the cavity upon the surface of the topaz, it neither remains still, like the fixed oils, nor disappears, like evaporable fluids. Under the influence, no doubt, of heat and moisture, it is in a state of constant motion, now spreading itself in a thin plate over a large surface, and now contracting itself into a deeper and much less extended drop.* These contractions and extensions are marked by a very beautiful optical phenomenon. When the fluid has extended itself into a thin plate, it ceases to reflect light, like the most attenuated part of the soap-bubble, and when it is again accumulated into a thicker drop, it is covered with all the coloured rings of thin plates. When one of the drops of fluid is very minute and perfectly circular, it resembles, in the most accurate manner, the small drops which pass from the vacuity, and which have been described in the preceding section.

After performing these motions, which sometimes last for ten or twelve minutes, the fluid suddenly disappears, and leaves behind it a residue of minute and separate particles, which are opaque by reflected, but transparent by transmitted light. Upon examining this residue with a single microscope held in the hand, it again started into a fluid state, and

* A round hemispherical drop often stretches itself into a plane of more than twelve times its original area.

extended and contracted itself as before. This was owing to the moisture of the hand; and our author could at any time revive the indurated substance, by the approach of a moist body. A portion of the fluid, which was taken out of a cavity twenty days ago, is still capable of being restored to a fluid state by moisture. This portion was shown to an eminent naturalist, the Rev. Dr. Fleming, of Flisk, who remarked, that had he observed it accidentally, he would have ascribed its apparent vitality to the movements of some of the animals of the genus Planaria.

After the cavity has remained open for one or two days, the second fluid comes out of it, and hardens very speedily into a yellowish resinous-looking substance, which is perfectly transparent. This substance absorbs moisture, but with less avidity than the other. It is not volatilized by heat. It is not soluble in water or alcohol; but it is rapidly dissolved with effervescence by the sulphuric acid. The nitric and muriatic acids also dissolve it.

The residue of the first fluid is volatilized by heat; and it is also dissolved, but without effervescence, by the sulphuric, the nitric, and the muriatic acids. After standing some time, both these substances acquire a brilliant lustre, as if some metallic body entered into their composition.

It would be improper to conclude this paper, says our author, without noticing the relations which are supposed to subsist between this class of phenomena and the two contending Geological Theories. The existence of highly rarified gas in the cavities of crystals, has been regarded by the distinguished President of the Royal Society of London, as "seeming to afford a decisive argument in favour of the igneous origin of crystalline rocks;" and the "fact of almost a perfect vacuum existing in a cavity containing an expansible but difficultly volatile substance," (as naphtha,) he likewise considers as highly favourable to the same theory. The discovery of compressed gas in similar cavities might have been regarded as neutralising in some degree the first of these arguments: but Sir Humphrey Davy remarks, that it may be explained, by supposing the crystal to have been formed under a compression much more than adequate to compensate for the expansive effects of heat.

Without presuming to combat these deductions, or to suggest any of the numerous explanations by which the Neptunist might reconcile with his own system the compressed and dilated condition of the included air, I shall content myself with stating, that the facts described in the preceding paper appear to me decidedly hostile to the igneous origin of crystals, and in some points of view, favourable to their aqueous formation. The existence of a fluid which entirely fills the cavities of crystals, at a temperature varying from 74° to 84° may, upon the principles assumed in the opposite argument, be held as a proof that these crystals are formed at the ordinary temperature of the atmosphere, while the fact of a perfect vacuity existing in *sulphate of barytes*, and capable of being filled up by the expansion of the aqueous fluid, at a temperature not exceeding 150°, authorises the analogous conclusion, that the crystal could not have been formed at a higher temperature. On the other hand, the filling up of the vacuities in *sulphate of iron*, and *sulphate of nickel*, at a temperature much above that at which they were formed, may lead geologists to renounce a species of argument which appeals only to our ignorance, and to withdraw from the defence, even of their outworks, those faithless auxiliaries which are so ready to enlist themselves in the service of either power.

There is one geological relation, however, of the preceding facts, which may deserve some attention. Hitherto the contending theorists have limited their idolatry to two of the elements; but the existence of two new substances in minerals, one of which combines a great degree of fluidity with the high expansive power of the gases, renders it probable, either that these substances existed at the formation of the globe, or that they are the result of laws of crystallographic combination which have escaped the notice of the philosophical geologist. Were such fluids the product of the ordinary processes of crystallization, they would occur in artificial as well as in natural crystals: and consequently, while they remain undiscovered in the cavities of the first of these classes of bodies, we are entitled to attach a new difficulty to the aqueous hypothesis.

Had the two new fluids occurred only in one mineral, or in minerals of a particular composition, they might have been supposed to have some relation to the elementary principles of the body, and to have arisen either from some accidental irregularity, which prevented them from crystallizing, or from the decomposition of the matter subsequently to its crystallization. The perfect identity, however, of the two fluids, as found in pure Quartz, in Amethyst, in Topaz, and in Cymophane,—minerals brought from the most opposite parts of the globe,—from Scotland, Siberia, New Holland, Canada and Brazil,—establishes the universality of their existence, and adds to the probability of the supposition that they have performed some important function in the organization of the mineral world.

Art. VI.—*Notes on the Mineralogy of Nova Scotia;* by Francis Alger, of Boston.

It is certainly remarkable, that, notwithstanding the taste which has of late years existed for the study of mineralogy, and the number of mineralogical excursions which have been made to many other quarters, yet this country, although so near to our own territory, and abounding with minerals, has hitherto been, in this respect, wholly neglected.

Having, about two years since, received specimens of iron ore from Nova Scotia, and shortly afterwards, information from a person well acquainted with the country, in regard to water power, iron ore, and the necessary facilities for the erection of iron works, we were induced to visit there in the month of May, 1826. Measures were soon taken to form a company, and a smelting furnace has since been established in the county of Annapolis, on Moose river, under very favourable prospects. During the months of July and August, being occasionally in search of iron ores to supply the furnace, I also examined partially for other minerals. My observations were confined principally to the county of Annapolis, but I have received specimens from other sections.

With a view of directing the attention of geologists to this country, I shall proceed to describe, so far as I am able, some of its geological features, the minerals which I met with, and their localities. Greenstone, red and white sandstone, and clay-slate, are the principal and most extensive rock formations which are here observed. They either form high, abrupt precipices, extending along the sea-shores in stratified beds, or else occur in detached rocks. The highest precipices consist of sandstone, and the highest and most important one which I saw, was at the head of St. Mary's Bay, which measures in some places 100 feet in perpendicular height. It is composed of red and white sandstone, alternating with each other, in parallel strata, at an inclination of very nearly 5°. It appears to have once extended further into the bay, as the shore is uniformly composed of similar sandstone, beyond low water mark. The tide rises here ordinarily about 25 feet; but sometimes, when the wind blows powerfully in the right direction, it rises to a much greater height, even to 30 and 35 feet, as I was informed by a person residing near. When this is the case, in consequence of its dashing with such great force, the sea separates large masses of the stone, which are afterwards cut into convenient shapes for the sides and hearths of fire places. This sandstone is sometimes slaty in its structure, but close in its texture, hard enough to strike fire with steel, and the grains appear to be almost uniformly of one size, occasionally intermixed with minute scales of mica. The cement is quite ferruginous and sometimes passes into red chalk, in the form of narrow veins or seams.

About eight miles nearer the mouth of the bay, is a precipice of greenstone, varying in height from 60 to 70 feet, extending along the sea-shore and forming a barrier to the sea for a long distance. In some places it is almost perpendicular, but its general form is very irregular, as it corresponds in its course with the windings of the shore. In this precipice, I found specimens of laumonite, which were taken from veins traversing it nearly in a vertical direction, many of which were 12 inches in thickness, formed chiefly of carbonate of lime, occasionally in beautiful separate crystals, interspersed with small and brilliant plates of specular iron ore, which has frequently deceived the inhabitants, who have believed it a more precious metal. Quartz is also near the laumonite, in masses of imperfect prisms, radiating from the center, and terminating in pyramids of amethyst. In many places, both in vallies and on mountains, granite and cyanite (sienite? Ed.) are scattered over the surface, in large masses, or apparently boulders, and often piled together in the greatest confusion; but I have never observed either of them in distinct beds, although they may eventually be found to exist. These aggregated rocks are not therefore metalliferous, but I have occasionally noticed garnet, chlorite and schorl, disseminated through them, though not in abundance.

This country is extremely rich in iron ores, and other metals have also been found. The most extensive deposits of iron ores are at Nictaure and Clements, in Annapolis county. At Nictaure the ore was first discovered on a hill, elevated perhaps 600 feet above the level of the sea, and about eight miles from the Bay of Fundy. It constitutes a regular vein, traversing clay slate, running nearly in a N. E. and S. W. direction. On removing the stratum of earth which covers the vein, and consists of a clayey ferruginous soil, about two feet thick, the naked ore presents itself to view, transversely intersected by numerous seams, some of which are open fissures, and others filled up with a soft, friable substance, like red ochre. The average width of the vein is 6 feet at the top; but as the walls incline from a perpendicular outwards, the width increases as the mining operations advance; so that I found, at the depth of 4 feet, a difference in width of 4½ inches. In some places the substance of the vein is to be observed mixed with, and passing into, that of the walls or matrix.

Being present when the ore was raised, I observed a remarkable tendency which it has of breaking into rhomboidal fragments—a circumstance which much facilitates the mining labour. When broken, it is found completely filled with, or, as it were, made up of, marine shells, exhibiting impressions of terebratulae. In one specimen I found half of an original shell, perfectly white and unbroken. The slate also exhibits these impressions, especially when in immediate contact with the ore, so as to receive one half of each shell. This ore is hard and slightly magnetic, although some specimens give a red streak and powder; it contains considerable calcareous earth, derived probably from the shells, and yields about 55 per cent. of iron.

The ore at Clements is situated on land supposed to be 400 feet above the level of the sea. It forms a vein or bed, and, like the other, runs in a N. E. and S. W. direction, traversing a hard clay slate; but as there are no distinct seams of separation between the ore and walls, its exact width cannot be easily ascertained: it is, apparently, however, as much as 10 feet. Large masses of ore, of many tons weight, are scattered over the surface, in the neighbourhood of the vein, and do not appear to differ essentially from it. This ore is fine granular, extremely hard, magnetic and compact, without any seams. The impressions of shells are rarely observed in it or its matrix; but they both contain small quantities of iron pyrites. It yields, by fusion, from 60 to 70 per cent. Although these localities are distant from each other at least 40 miles, yet, as there is a great similarity between the ore from each, and they exist in the same range of mountains, running in a N. E. and S. W. direction, there is not the least doubt that they ultimately unite and form one continuous vein.

At Digby Neck, about a mile east from the greenstone precipice, magnetic iron ore occurs in detached masses, which are often hollow, containing octahedral crystals, variously modified by secondary planes. At this locality, no regular vein has been met with, although many attempts have been made to discover one by digging. The masses are evidently out of place. Specular iron ore also occurs with it in masses of a laminated stratum, mixed occasionally with quartz and feldspar. They are both rich ores.

In the North mountains, about 8 miles from the town of Digby, is a deposit of the magnetic iron ore, in large masses, associated with greenstone. When I first found this locality, I considered it to be part of a vein; but as the ore was shortly afterwards removed and conveyed to the smelting furnace, it proved to be merely an assemblage of weighty masses, thrown as it were into a hollow of the mountain. Subsequently, on examining the spot, and the land adjoining, not the least signs of more ore could be discovered, the compass not being in the slighest degree attracted. Amethyst, in beautiful crystals, varying in colour from deep violet to nearly white, occurs with this ore, generally in the form of an incrustation over the masses; but sometimes in their interior constituting druses. Also, ribbon agate and detached crystals of transparent quartz.

At Clements, in the vicinity of the furnace, are found boulders of greenstone, of a soft, loose texture, containing narrow fissures and cavities, in which I discovered beautiful crystals of chabasie, mesotype and heulandite, associated with analcime, and small globular masses of stilbite, which, when broken, present a foliated or radiated structure, and usually a white colour, with a high pearly lustre. Also, red and green jasper, with carnelian in narrow veins. Also, green diallage, through which is disseminated sulphuret of iron, without any tendency to crystalize, or in the form of grains; the whole forming apparently an immense rock, the summit of which only is visible at the surface, the rest being under ground. Also, near the mouth of Bear river, sulphuret of iron, forming a wide bed in clay slate, extending back into the forest to the distance of three miles; its real value, however, is not at present known to the inhabitants, though it undoubtedly will, before many years, afford an abundant supply of copperas. It is principally the amorphous variety, and by the action of air and water, effloresces, (even in its native bed,) and is converted into sulphate of iron.

Between Bear river and Digby, fragments of brown and red hematite have been found, but as yet only in small quantities; it is highly probable, however, to judge from the appearance of the neighbouring land, that an extensive deposit of these ores does somewhere exist.

At Bridgetown, on the Annapolis river, gigantic quartz crystals have been found in alluvial soil. Within two years past, a number of broken fragments have been picked up by a farmer, during his agricultural labour, and he informed me of a large lump, as he called it, weighing more than an hundred pounds, and as clear as glass, having been found. He said, however, that his boys, wishing to see the inside of it, broke it into pieces and gave it away to travellers, one of whom took a specimen to England, where it was afterwards cut by the lapidaries into articles for jewellery. I found, imbedded in the soil, a crystal, which measures from its base to its terminal point 19 inches, across the base 13 inches, across each lateral plane 9 inches, and the length of one of the terminal planes, unduly extended, is 10 inches. It weighs precisely 90 pounds. Its colour varies from light to dark smoky, sometimes passes into straw yellow, and resembles the cairngorm stone, brought from Scotland. It receives much additional beauty by the long and slender prisms of black schorl which traverse its surface in every direction, and even penetrate its substance to the depth of several inches. Some of these prisms are 3 inches in length, and vary in thickness from $\frac{1}{8}$ inch to microscopic. The interior of this crystal shows to great advantage, by placing it in a situation where the rays of the sun can impinge upon one of its lateral planes whose surface is smoother than the rest.—Attached to the base are small pieces of feldspar, which seem to imply that the crystal was derived from the neighbouring granite rocks.

In the same town, about 8 miles in a N. E. direction, is found a substance which some of the inhabitants declared to be copper ore, to which they attached a high value; but it is now pretty well ascertained to be chlorophæite. It occurs in nodules about the size of a pea, sometimes larger, imbedded in amygdaloid. When a mass is first broken, the small nodules are of a greenish colour; but by exposure to the air it changes and finally becomes black. It is very brittle, possesses a remarkable greasy feel, and may be easily scratched by the finger nail. Before the blowpipe it remains without any alteration.

Up the Bay of Fundy, on Cape Dory, native copper is said to have been found; but the specimens which I have seen, bear evident marks of artificial fusion. Also, on Cape Spail, magnetic iron ore, inclosing compact red oxide of iron, accompanied by geodes of amethyst. Also, at Cumberland, black oxide of manganese, frequently crystalized; and coal forming small veins in red sandstone. Bog iron ore, in extensive beds, is found in various towns, and occasionally mixed with it is the earthy phosphate of iron.

ART. VII.—*Remarks on Prof. Eaton's proposed improvement in the manufacture of Compass Needles;* by a SURVEYOR.

TO THE EDITOR.

THE last number of your valuable Journal contains an article entitled "Improvement* in the manufacture of Compass Needless, by Prof. Eaton," which, from the importance of the subject, I was induced to read with much attention. After I had done so, however, I was somewhat surprised that the author should have denominated that an improvement, which at best could be nothing more than a method of repairing a defective or damaged instrument.

It appears from Mr. Eaton's statement, that he was led to this discovery in examining a compass of fine workmanship, which had been returned to the maker as an imperfect one. A series of experiments proved that the cause of the defect was the lodgement of a small scale of steel in the limb of

* This title was written by the Editor.

he instrument. This of course would have an effect upon the needle and thus destroy its accuracy.

To remedy this defect, Mr. Eaton caused the needle to be cut off $\frac{7}{10}$ of an inch from each pole, and to be armed at each extremity with brass tips; after which, we are told, the instrument was perfect. This method seems then to be recommended to general use, as it is said that "another important advantage which will attend tipping needles with silver, brass, &c. is that of preserving the points from rust. It has been demonstrated by conclusive experiments, that magnetism resembles electricity in acting most powerfully from the sharpest points."

I may remark, that, although it has been rendered probable that most metallic bodies possess magnetism, the only substance in which it is strikingly and permanently developed, is steel; and hence this is altogether employed for the construction of magnetic needles. This being the case, my objections to Mr. Eaton's proposed improvement are,

1st. That the needle possesses the weight and consequently the friction of a long one, with only the magnetic power of a short one; it having been sufficiently proved, that the friction is nearly proportional to the weight of the needle.*

2d. That in the construction of magnetic needles, those are the best in which the opposite magnetisms exist in each half, gradually increasing in intensity, from the center to the extremities. But this is not the case when the ends are tipped with brass, in which, if any magnetism is developed, it may be of another kind from that which exists in the steel to which it is attached. Such a needle, therefore, is liable to the objections which would apply to one having consecutive points, or which assumes polarity, in parts other than the extremities.†

For these reasons, I cannot but think that the method suggested by Mr. Eaton must greatly impair the directive force of the needle, and that its adoption would be injurious rather than beneficial; we are therefore at no loss to account for the circumstance stated by him, that the needle, when thus tipped, was not affected by magnetic ores, *real* or imaginary.

* See Biot, and other authors on magnetism.

† A very convenient method of ascertaining this fact, is to detach the needle, cover it with a clean piece of paper, and then sprinkle on the paper some fine iron filings. We can then detect the number and exact position of poles of the needle.

A remark or two as to the particular case which led to the use of these brass tips. If a scale of steel or iron was lodged in the plate, it is difficult to perceive how the simple shortening of the needle would completely obviate the disturbance which it might occasion. It might render the error less manifest, but could not wholly correct it.

As this subject is one of much consequence, particularly in this country, where the compass is so much employed, I will venture to add a few suggestions which, in my opinion, will be more likely to improve the manufacture of compass needles, than the method proposed by Prof. Eaton.

A person, upon purchasing a compass, if he wishes to obtain an accurate instrument, should test every part of the plate, limb, &c. with a fine magnetized sewing needle, suspended by a fibre of silk. If this is not at all affected by passing under it the different parts of the plate, it may be inferred that there is no point of attraction. But if it is affected, we can easily detect the exact spot in which the iron or steel is lodged. In the latter case, the better way is to select another instrument and treat it in the same manner; and so on until one shall be found which is entirely free from objection. This rigid scrutiny might indeed increase the cost of perfect instruments, but this is of little consequence in a matter where accuracy is all important. Above all, no principle of justice would warrant the punishment or injury of the needle, for a fault of the plate.

As to the best form of compass needles, there is still some dispute; Capt. Kater preferring the pierced rhombus, five inches in length and two in breadth,—Coulomb, and others, the arrow-shaped, &c. In one thing, however, most experimenters agree, viz. that they should be pointed; as by this means the highest degree of magnetism is developed and maintained in the extremities, which is the grand desideratum in the construction of magnetic needles. How can this object be effected if these extremities consist of brass or silver?

Finally, I imagine that it will be much easier to keep a common needle properly pointed and free from rust, than it will be to obtain one of equal directive power, upon the plan proposed by Prof. Eaton. A SURVEYOR.

Art. VIII.—*Notice of Fries' Systema Mycologicum;* by E. Davis, Principal of Westfield Academy.

The fungi have probably received less attention than any other part of botany. The reason is obvious, it is a difficult undertaking to explore such a boundless field of evanescent substances. Some of them are daily changing their appearance, but must be examined at a particular stage of their existence, or they will not exhibit the characters ascribed to them in the books. Furthermore, the preservation of them is difficult, so that different collections cannot be compared; and very few persons find leisure from their various occupations, to devote themselves to a subject requiring such accurate observation, and such patient investigation. A want of books, also, prevents not a few from entering this field of research.

My object in this communication, however, is to give a compendious view of a natural system of the fungi, as exhibited in the introduction of the work named at the head of this article. The first volume was published at Lund, Germany, A. D. 1821, and the second in 1822; the remaining part of the work I have not seen, and I know not whether its publication has been completed.

The natural systems most approved, have divided plants into natural classes, according to the number of cotyledons and petals, or position of the stamens. Our author says those are in error who derive the chief distinction of classes from the external form, and that all the diversities of genera, order, &c. arise from the mutual relation of elements in their numberless modifications. Modern chemistry has discovered that the elements unite in definite proportions, and it seems that a constancy of species is to be derived from these alone. All those plants whose elements are combined in the same proportion constitute a class, which is characterized by some essential organ. The presence of the organ is an index of the elementary proportion.

The whole evolution of a fungus is determined by what the author calls *cosmica momenta*, of which there are four—1, Nisus reproductivus, or earth and water. 2, Air. 3, Caloric. 4, Light. The first is the principal agent in producing *sporidia*, or fruit, the first and second in producing *floccos*, or elongated fibres, on which the fruit appears; the first and third produces the *uterus*, or a closed fungus, and the first and fourth the *hymenium*, or an open fungus. These are the four leading characters, and the system is divided into four classes, a single class being composed of those plants that exhibit one of these characters more prominent than the others. I supposed from his use of the word *elements*, that he would classify them in such a manner, that all those that contained a greater proportion of carbon, or oxygen, &c. should constitute a class; but he seems to use the terms element and cosmica momenta as nearly synonimous, which savors too much of the doctrine of the alchemists. M. M. Gay Lussac and Thenard have given laws for the arrangement of vegetable substances in chemistry, according as one or another of the elementary parts predominates, but whether the adoption of such principles of classification in botany will be in any respect superior to the old system, it may be doubted. I do not object to the system of fungi before us, since by elements he evidently means the action only of certain forces conspiring in the evolution of certain characters. *How* these forces tend to the production of these characters, it is not our business to inqnire. If it be a fact that the agaraci polypori, &c. never grow in the dark—that a lycoperdon may grow in the absence of light, but not in the absence of caloric, and that rhizomorpha may grow in the absence of light and caloric, but not in the absence of air, then it becomes a plain matter of fact, that these forces tend to produce certain organs. I know of no facts that oppose the system under consideration. I do not think, however, that a system can be formed, taking the definite proportion of chemical elements for the foundation of the system. That each of the *cosmica momenta* evolve uniformly certain characters in phenogamous plants, remains to be discovered. Our author suggests such a fact.

But in the fungi, although all the plants of the same class have the same prominent organ, yet it may appear under various modifications, which lays the foundation for a division into orders. That order that possesses the character of the class in the greatest perfection is denominated the central order, the others radii. In the division of orders into genera, there is also a central genus. An order is sometimes divided into two series, each approximating to the contiguous orders. The system is constructed with such nicety, that the place of a plant in the system may be designated by a formula of letters, which also serve for a description of its natural class, order or genera.

There being four leading characters, of course the system has four classes; the names of which are, Coniomycetes, Hyphomycetes, Gasteromycetes, and Hymenomycetes, signified by the letters C, M, U, and H. The class C has sporidice, naked, M, thallus flocose; U, a closed fungus; and H, an open fungus. Each class is divided into four orders, and each order into four genera, arising like the classes from the actions of the natural causes. The orders are designated by the letters E, M, U and H, and are the same in every class. CE denotes first class, first order, and UU third class, third order. If an order is divided into two suborders, as the fourth order of the fourth class, it is expressed thus, HH^1 for the first suborder, and HH^2 for the second. The genera are represented by either of these letters, E, M, G, X, or U, according to its *habitat*. E denotes that it grows on decaying plants, or on those recently dead, M that it grows on plants in the process of fermentation, G that it grows on the ground. The second suborder of the fourth class, fourth order, stands as follows:

Genera.	Formulæ.
1. Thelephora	HH^2 E
2. Hydnum	„ M
3. Polyporus	„ X
4. Agaricus	„ G

In the artificial system the orders and genera are not limited to four—they are regarded as natural families, having many allied genera. Agaricus has three allied genera, Conthurellus, Merulius, and Schizophyllum.

Our author's artificial system is only an extension of the natural system. He would do the same with phenogamous plants, demolishing the present natural system, and substitute in its place a natural system which would supersede the necessity of the present artificial system. On this point, however, he has only thrown out hints, and I shall say very little respecting it. The work is written in unclassical Latin, and abounds in new coined words which has rendered the decyphering difficult.

Westfield, March 30, 1827.

ART. IX.—*Notices of the Lead Mines and Veins of Hampshire County, Mass. and of the Geology and Mineralogy of that region ;* by ALANSON NASH.

Extract of a letter from the Author to the Editor.

WILLIAMSBURGH, March 8, 1827.

SIR,—The object of this communication is to acquaint the public with the lead mines in Hampshire county, Massachusetts. These mines have often been said to be connected with the celebrated one at Southampton, and the Southampton mine to extend to Hatfield and Leverett, 20 or 30 miles.

In presenting a view of these mines, some difficulties occurred on account of their connexion with the rocks in which they are located ; I have therefore sent along the whole of my observations, both mineralogical and geological, submitting them entirely to your disposal.

I make no pretensions to great acquirements either in literature or science, but should this communication contain any thing that comports with the design of the Journal of Arts and Sciences, you are at liberty to publish so much, and what you may think proper. I know not what has been published by others.

I am, Sir, yours, &c. A. NASH.

Granite, coloured indigo blue.

This is a rock that has received and very justly deserves considerable attention. It is the rock that furnishes us the beryls, spodumene, and curious tourmalines of this neighborhood, as well as the most of our metallic veins.

In Williamsburgh, Conway, Goshen, Chesterfield, Westhampton, Southampton, Northampton, Whately, and Leveret, it seems to be the foundation rock upon which the others rest. Thus, as we ascend a hill or mountain, we commonly find mica slate at the foot and sides, resting upon the granite ; but as we rise, the mica slate gradually disappears, leaving the granite to form the highest eminences. As we pass north from Williamsburgh into Conway, the granite evidently takes shelter under the mica slate, where, in turn, the mica slate alone, or with very narrow prominences of granite, composes the most precipitous elevations. Here the strata of mica slate and the superincumbent rocks seem to be thicker and less liable to decomposition; the same remarks will apply as we pass west from Williamsburgh into Chesterfield; though north, in Goshen, the granite is in great quantities. Enormous rounded masses of granite may be seen in this section, wherever granite predominates, lying on the surface in vallies, as well as on the highest mountains. Often these masses, as in the north-east part of Williamsburgh, are seen eighteen or twenty feet in diameter, resting upon a mere patch of mica slate. Another, not far distant, reposes upon a granite ridge, quite on its summit, so that the rain that falls on one side of the boulder runs to the east, while on the other it makes off towards the west. Perhaps I ought to mention that it is only when the granite forms beds and veins in mica slate, or when it lies contiguous to mica slate, that any of the minerals are found; and upon the highest elevation no rock in this vicinity is so completely destitute of minerals, except the metallic veins, for these are often found in the Alpine districts.

Mica Slate—green pale.

The mica slate of this region consists of several varieties. In Williamsburgh, Conway and Whately, it is not much disposed to stratification, but in Cummington, Goshen and Chesterfield, it becomes distinctly stratified, and abounds in staurotide. This staurotide is crystalized in all its usual forms, and seems to lie between the strata, so that when any large quantity of mica slate is disturbed, it usually splits into slabs several inches thick, and the staurotide appears on the faces. These slabs are much used for hearth and step stones, and are frequently dug and wrought out into sinks, cisterns, &c. Owing to the softness of the rock the staurotide is easily detached, in the direction of the strata, in plates, some of which I have obtained 18 inches in length by 6 or 8 in breadth, containing hundreds of crystals of the staurotide.

Where this rock approaches the talcose slate, at or near their junction, it affords excellent grit stones, which have been extensively manufactured in Cummington and Chesterfield. The variety of mica that contains the staurotide, has but very little if any quartz associated with it, but the other varieties usually have nests of it in profusion. In Chesterfield it is generally of that kind which is called milky quartz; where the mica is disintegrated, these nests fall out and lie scattered upon the ground. In one instance, in Chesterfield, I noticed an immense boulder of it, full 10 or 12 feet in diameter.

There is a variety of mica slate, seen only in Chesterfield, containing cyanite and garnets of an enormous size. It seems to be a stratum that has been broken up, and is now scattered, here and there, in specimens of all sizes, up to 30 feet in diameter. In going from Chesterfield to Williamsburgh, this variety is first noticed, one mile east of Chesterfield meeting house, in a large block, lying by the road side. Beginning at this block, and running one mile north, we find the boulders, here and there, in a state of decomposition;—some of them have wasted almost entirely away, leaving the nests of quartz and scales of mica in their stead. The mica scales, of which this rock is chiefly composed, are of all colours, some being a beautiful straw colour, a dove brown, or an orange yellow, while others are a grass green, or a jet black, and are often several inches in surface, and so loosely attached to each other, that a child will easily separate the hardest specimens; indeed, the rock seems to be only an aggregate of mica scales, with very little if any cement. Notwithstanding the frail texture of the rock, garnets are scattered through it in immense quantities, from the size of a pea to three inches in diameter. These garnets, when perfect, are always dodecaedra, and, as usual, so hard as to resist the best file. The garnets are easily detached from the rock, with a hammer and chisel, although, owing to their extreme brittleness, great care is requisite in the operation.

Not one in ten of the garnets has all the sides perfect;—some want one or two sides, and some more. The imperfect sides adhere firmly to the mica, while the perfect ones are easily separated from it. Not unfrequently these garnets are crystalized in groups of three, four, five or six in a group, exhibiting seven or eight faces, or more, just as their situation exposes them. Some of the garnets in this rock seem to be 5 or 6 inches in diameter, without any sides at all.—These are the massive garnets, and are frequently composed of grains, or small garnets in immense numbers.

Along with garnets, there is cyanite and quartz in this rock. The quartz is of a milk white, often transparent, or limpid, and in nests of all sizes. The cyanite usually lies next to the quartz, and frequently passes into it, and when the quartz is transparent, forms unusually fine specimens. This cyanite is of an azure blue, highly translucent, often transparent, and in crystals sometimes eighteen inches in length, by two or three in breadth.

I traced this mica slate north into Goshen. Two or three miles from Mr. Searl's, towards Goshen, the mica slate grows harder and forms a stratum in another kind of mica slate, the whole reposing upon granite. After passing into Goshen, it again becomes softer, lying in boulders on the surface, which wholly disappear at a place called the Lily Pond. Here this mica alternates with hornblende. Often the red oxide of titanium is found in the mica slate in Conway, Whately, and Williamsburgh. In general, the crystals are imbedded in the mica, although, many times, they are wholly in the nests of quartz, with which the mica slate abounds. I have often found dark brownish garnets in this mica slate, and small garnets, of the usual colour, in great quantities.

Veins and beds of granite, and veins and beds of hornblende traverse this rock, but as Professor Hitchcock is about to present the public with a particular account of them, I omit to describe them.

Gneiss—green and red, mixed.

I know nothing of this rock, I have copied it from Prof. Hitchcock's description of the Connecticut Valley.

Micaceous Limestone—deep blue.

The micaceous limestone seems to occupy the place that granular limestone usually does. It is called micaceous limestone from the circumstance that it contains mica disseminated through it. Along with the mica is generally a portion of silex.

When tested by the acids, a rapid effervescence takes place, until the lime is taken up, leaving from fifty to sixty per cent. of silex and mica. Sometimes the silex is wanting, then mica only remains, which when washed appears of a clove colour. In other instances, the mica will also be absent, and the residuum will be silex, as coarse, very often, as common scouring sand.

This limestone is usually found in company with mica slate, and in this vicinity is seen in boulders, patches and broken strata. In Williamsburg, the stratum is entirely broken up, with only now and then a boulder to be seen, but north in Conway, and east in Whately, the stratum is less disturbed, and may be seen in patches, and sometimes the stratum may be traced many rods in length. It occurs in the two latter towns in such abundance, that were it not for the silex and mica in it, lime might be made in sufficient quantities to supply all this region. I have often seen veins of granite, in this rock, of all sizes, up to three feet in width. In Goshen and Chesterfield this limestone is found in large quantities, and in addition to the silex and mica, it contains a portion of hornblende, which renders it still less valuable. The hornblende is scattered through the limestone in a semi-crystalized form, and is so firmly incorporated, that it will undergo *fracture* in common with the rest of the rock.

I have often seen boulders of this rock that had nests of mica slate and granite scattered through it, as in the profile following.

This limestone often alternates with mica slate, and frequently passes into it by insensible gradations. I have often seen a ledge that was limestone on one side, but on the other mica slate. It also frequently alternates with the hornblende stratum, or greenstone, or sienitic granite, and where it could be measured, it is generally about six feet thick. At a locality in the south-west part of Whately, the strata of rocks are completely turned upon their edges. Here mica slate, micaceous limestone and hornblende alternate. These strata appear to have once covered granite, and to have been thrown back into their present position, by some convulsion of nature, as in the following profile.

Here, 1 represents a stratum of mica slate, resting upon a protrusion of granite. Between this and the strata, in a vertical position, flows a small stream. 2 Represents a vertical stratum of mica slate, four inches in thickness; 3, a stratum of micaceous limestone, seven feet thick; 4, mica slate, four feet; 5, sienitic granite, thirteen feet; 6, mica slate six feet;

7, sienitic granite, six feet; 8, micaceous limestone, seven feet; 9, mica slate, eight feet. I have stated that this rock generally contains much mica and silex; such is the fact, although it sometimes has but little of these ingredients in it, and has been burned into excellent quick lime.

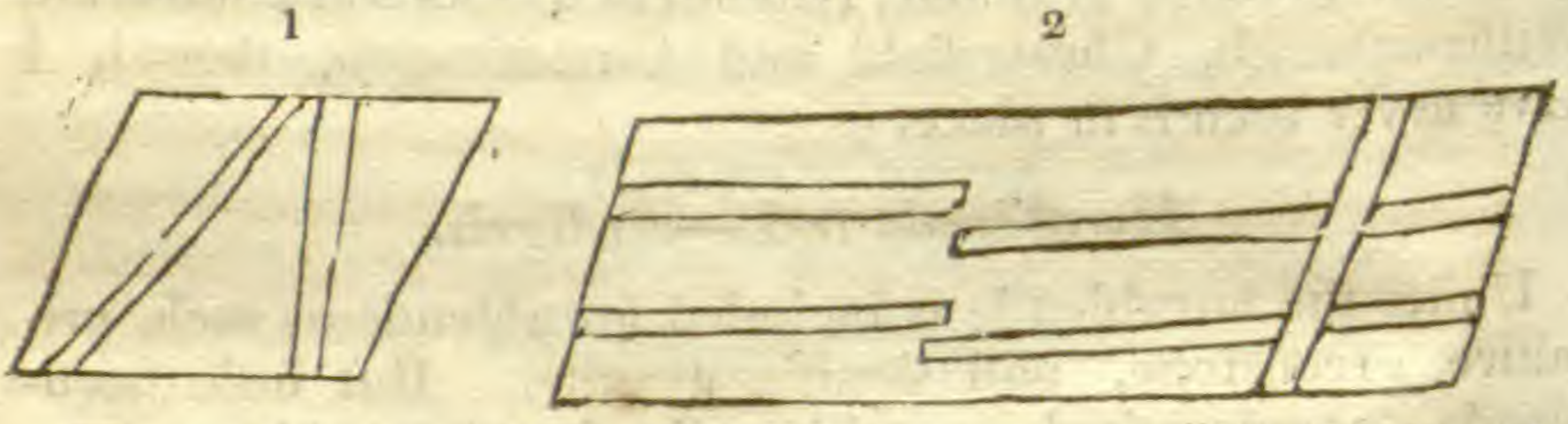
Sometimes also, it appears to be the real granular limestone, or an approximation to it. I have seen some boulders that were, on one side, of the micaceous character, while on the other they were, decidedly, granular limestone. I have found several specimens of granular limestone in Williamsburgh, that had mica scales disseminated throughout them, and strongly partook of the micaceous character. This granular limestone, approaching the micaceous, or the micaceous approximating to the granular, is found in blocks and boulders in Williamsburgh, Chesterfield and Cummington, though I have never seen it in place.

Hornblende rock—verdigris.

Under the hornblende is included hornblende as such, primitive greenstone, and sienitic granite. But little hornblende, comparatively speaking, is found in this region; and whatever there is appears in blocks, or globular masses, of all sizes. This hornblende is generally amorphous, but sometimes with a strong tendency to crystalization. It frequently forms beds and veins in mica slate, and sometimes alternates with it; and here I remark, that if granite veins deserve attention, the hornblende veins do not merit less.

The sienitic granite is first noticed in going a mile or two west from Hatfield. Here it breaks up through the tertiary formation, 50 or 60 feet high, looking towards Connecticut river. This *abruption* continues quite across Hatfield, two or three miles into Northampton, and is known by the inhabitants in the vicinity, by the name of the *Rocks*. South in Northampton, as also west in Hatfield, it appears on any little elevation, and is covered by a few feet of tertiary.

The southern limits of this rock are at or near Shepard's manufactory in Northampton, and its western near the bounds between Hatfield and Williamsburgh. It passes north into Whateley; here it is primitive greenstone, and this rock is composed of feldspar, hornblende and mica. The mica scales are small and but few in proportion, and in fact the rock seems to be a real sienite, with only an intrusion of a little mica; the hornblende is of its usual appearance, but the feldspar is often of a beautiful flesh colour. Some ledges and even hand specimens are, on one side, of this flesh colour, while on the other, the feldspar is of its common whitish aspect, and this too without any approximation of the colours one to the other; the flesh colour will retain its character perfectly until it reaches its limit, and then commences the white, equally perfect, without the least seam or fissure between them. Sometimes a vein of white feldspar will interpose itself between the colours; the feldspar veins are of all lengths and breadths; often they are pushed several inches out of their course, and form shoulders; sometimes they diverge one from the other, and then again they intersect one another, as in the profile.



Here, 1 represents the diverging veins, and 2 the disturbed and intersecting ones. This rock is disposed to stratification, although the strata are of very unequal thickness. The strata appear to be seamed or fractured in two directions, at right angles, so that where they are disturbed or broken up, they present a vast quantity of parallelograms. These parallelograms are of all sizes, from a few cubic inches up to 15 or 20 yards, and they have generally extricated themselves from their original bed, where there is a sudden disruption of the rock. Primitive greenstone composes a large share of this rock, and is frequently isolated in the sienitic granite; sometimes only an inch or two is thus imprisoned, at others several cubic yards. The greenstone breaks along with the sienite, and is firmly attached to it. I have also seen real hornblende thus associated with sienite, sometimes inclined to crystalization, and often composing one half of a block, while the other half was indubitable sienite.

Nearly all the greenstone is in Whately, where it is only a continuation of the same rock, which constitutes the sienitic granite at Hatfield. The granite runs about to the line between Whately and Hatfield; here commences the greenstone, and runs northerly almost across Whately; the junction of the greenstone and granite may be seen, and the greenstone is the granite, wanting the feldspar and mica, (and substituting hornblende ?—Ed.)

The best place for viewing this greenstone, is found by starting west from the congregational meeting-house in Whately. The first rock we come to is greenstone in a range; then we strike a range of mica slate, then again, the greenstone, and so on, alternately for a mile. Towards the north part of the town, the greenstone has considerable of the sienitic granite associated with it, but soon disappears, leaving mica slate in its stead.

Perhaps I ought to mention, that sienite, sienitic granite, and primitive greenstone, are occasionally seen in blocks and boulders in Williamsburgh. The sienitic granite contains one metallic vein at Hatfield, and in its vicinity I have often noticed druses studded with quartz crystals; sometimes also I have seen druses in this rock, though not in the vicinity of the mine.

Talcose Slate—pale red.

I know of no patch or stratum of this rock nearer Connecticut river than Cummington, although in excavating the earth and on its surface, and in the bottom of brooks, we meet with good specimens in Williamsburgh, indicating that here was once a stratum, which has been destroyed or broken up, by some agent, adequate to the work. The specimens found with us are all full of small garnets, which are often beautiful. Fasciculite is seen in the west part of Cummington. This is the predominating rock, and some localities afford good talc; the best locality is found one half mile north of Hubbard's leather manufactory, in a ledge of imperfect steatite; the talc traverses the steatite in veins, and may be easily cut out with a chisel. Along with this talc are found small rhombs of a pearly, yellowish, shining colour which I took to be rhombic spar.

On the west, this rock is bounded by chlorite slate, and on the east by the mica slate. The iron mine of Hawley and the silicious carbonate of manganese of Plainfield and Cummington are at the junction of the mica and talcose slate. The talcose slate at Cummington has also an abundance of garnets as well as fasciculite, and has much the same appearance as that seen at Williamsburgh; indeed, I believe it to be the very same rock, only that the stratum was vastly thicker at the former, than at the latter place.

Old red Sandstone and Conglomerate—deep green.

The old red sandstone and conglomerate I have coupled together. These rocks, in this region, lie above all others, and seem to have taken their present position long since the formation of the primitive rocks, but anterior to the production of hills and mountains, and were I to hazard a conjecture upon the subject, I would say that the earth was first formed without any mountains or vallies, or nearly so, and covered with water. The water by currents and its movements abraded away the rocks, even down to granite in many instances, and the fragments of the primitive rocks thus torn to pieces and afterwards cemented, form the sandstone and conglomerate. Accordingly, we find conglomerate composed of quartz, granite and mica slate, and all the primitive rocks cemented together. The sandstone I conceive to be the finer part of this debris; perhaps not all the debris was thus cemented, and then it would be driven about hither and thither in the form of sand. After the deposit of the conglomerate and sandstone, I also suppose that the mountains and hills emerged from the circumambient water by the operation of some cause adequate to the work. These suppositions may perhaps account for the vertical position of many of the strata of rocks, for the globular or rounded masses of rocks every where found, for the shell limestone met with in various parts of the world, even in Alpine districts, for the want of the strata of primitive rocks, in many places, and for the fact, that conglomerate or sandstone reposes or lies near granite, as at Southampton and Leveret, which I shall soon notice. These suppositions will also account for the fact that conglomerate lies often up the sides of mountains, which is the case upon Mount Holyoke, south of Amherst. Here the conglomerate is seen to cover the foot of the mountain, and gradually disappears as we advance to the top. At the adit to the mine at Southampton, the first rock which we see on entering the passage is conglomerate reposing against other (primitive Ed.) rocks. Blocks and boulders of old red sandstone, likewise, and of conglomerate, are often found in this region, scattered here and there, and appear to have been once a stratum, but are now broken up and greatly decomposed. In elevated districts of this vicinity, and at Southampton, it is seen east of the granite, in patches and broken strata and in boulders, scattered upon the surface of the ground. In the west part of the town, where the granite elevations begin, the conglomerate approaches near to it, and is seen lying here and there, amongst granite, although I know not that a junction of the different strata has ever been found. At Leverett the sandstone and conglomerate are separated from the granite by a narrow plain, and here compose Mount Toby, and at Deerfield the Sugar Loaf. In the Connecticut valley this rock seems to be covered with alluvion, and the tertiary formation is in some instances many feet deep, but sometimes approaches and even rises above the surface. At Northampton, above the south bridge, the stream has worn down a channel through the tertiary to the sandstone.

A continuation of this sandstone may be seen at Hatfield, and also at Whately; in both places it rises above the surface, and at the latter place is hewn into hearth stones. It dips under the earth at the congregational meeting-house, and fifty rods north is again exposed to view, by a small stream that has carried away the earth down to it.

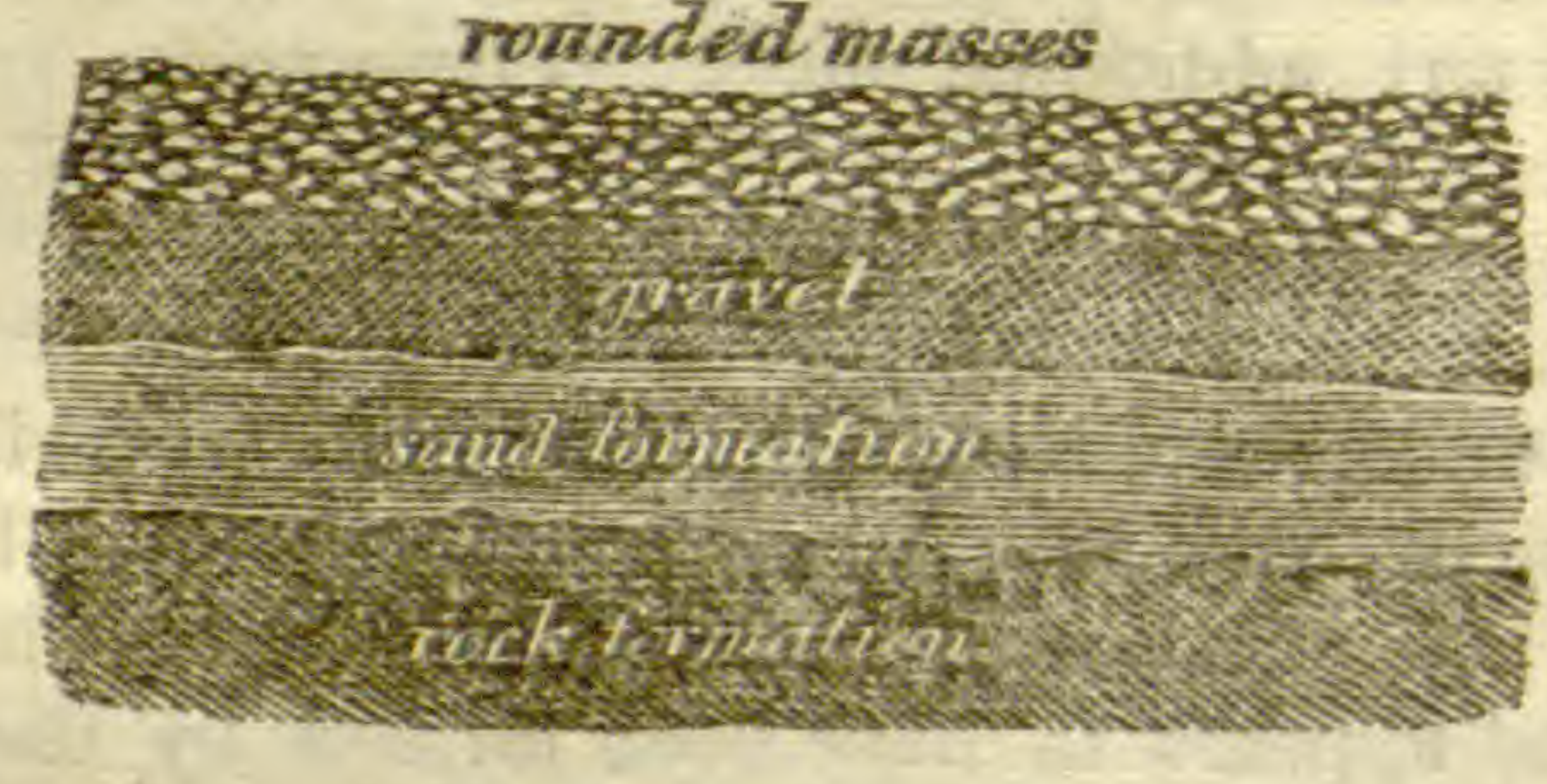
Here I will observe, that I have omitted any description of the coal formation and secondary greenstone, which are embraced within the limits of the accompanying map. These rocks have been amply described by Prof. Hitchcock, nor do I know of any additional facts. These rocks are coloured vermilion.

Geest—orange.

What I understand by geest, is the sand, gravel, earth and soil, which cover the rock formation, or lie upon it; as such I consider it in this communication. It covers almost all the surface, and appears to have been formed by the abrasion, disintegration and decomposition of rocks, both primitive and secondary. It is found in vast abundance in vallies and between hills and mountains, and covers the rock formation, from one inch to fifty or an hundred feet or more deep. As we advance up a hill or mountain the soil or earth becomes thinner, until we reach the summit; here it is often wanting, and the rocks are left bare to the skies. On plains it is often found to be sand or gravel, without stones of any kind; but as we advance towards the hills or mountains, rounded masses of rocks, of all kinds and sizes, present themselves, and increase in size and number, as we draw near to the protrusion of rocks. Brooks and streams have frequently cut through the tertiary, down to the rock formation, carrying the sand and earth down the stream, but leaving the rounded masses behind.

Upon excavating the earth, these rounded masses are often found in great abundance, quite down to the rock formation, though they are sometimes confined at and near the surface. When the rounded masses are found as in the first description, the earth is generally that kind which is called "hard-pan," it being indeed extremely hard. When the masses are only near the surface, sand usually lies next the rock formation. This sand, under a magnifier, appears to be composed principally of minute particles of quartz, and small scales of mica; the sand is frequently in layers, and sometimes contains scales of mica an inch or two in diameter.

Next above the sand is a layer of gravel, which appears full of small garnets, precisely the same in appearance as those seen in the mica and talcose slate of this region. Many of them have all their sides perfect, although generally the angles seem to be rounded off by attrition; the gravel often grows coarser as it approaches the stratum which lies upon it. This stratum consists of rounded masses of rock, of all sizes, mixed with gravel and earth, as in the profile below.



Among the rounded masses may be found fragments of all the rocks of this region, viz. of granite, talcose and mica slates; sienite, greenstone, hornblende, and micaceous limestone. Rounded masses of quartz are also seen, sometimes extremely smooth. The coarser particles of gravel also seem to consist chiefly of the above named rocks, and appear to have been reduced to their present size by attrition. The best place for viewing this latter description of geest, is three-fourths of a mile south of the meeting-house in Williamsburgh. I have seen the same appearances at Leveret, on the plain between the sandstone and granite. The question now occurs, how have these masses, and especially the quartz become thus rounded and smoothed? Whence, also, have come the garnets that are found here, and whence the mica scales seen in the sand formation? Not from any locomotion of the masses themselves, for they have none: not from the attrition of the earth in which they are imbedded, for the earth is not moved, below two or three feet, at all, and this only by the frost, whereas, these masses in the hard pan, are found at all depths, quite down to the rock formation: not by the decomposition of the rocky strata, for the masses are as sound as those just broken from a ledge. The foregoing inquiries, I am of an opinion, can be satisfactorily answered in no other way, than by supposing that the earth was once covered with water, and that these masses, after being detached from their respective rock strata, were rounded, and the quartz polished by currents. The garnets, we must believe, were originally on mica and talcose slate, and detached by the destruction of their native rocks. The hard pan, I conceive to be the debris of primitive rocks; indeed, the stony aspect of this earth seems to betray its origin. I ought to have mentioned that the hard pan earth forms by far the greatest share in this region. See note A, at the end of this communication.

Alluvion.

I have not much examined this earth. It, however, forms only a narrow strip along the banks of Connecticut river, generally on both sides, and also it is often seen beside the streams in this region. It has been supposed that Connecticut river was once dammed up by the range of Mount Holyoke and Mount Tom, and that the lake thus formed deposited the earth at its bottom; but that the river was ever dammed up, is much doubted by many, at least, much of the earth that has been supposed to have been deposited at the bottom of the lake, is decidedly of the tertiary formation.

Metallic Veins—marked 12 on the map.

The first metallic vein that I shall notice, is the vein of siliceous carbonate of manganese, at Cummington. This mineral first makes it appearance west of the congregational meeting-house, in the stone walls by the way side. It has been called the siliceous oxide of manganese, but has been ascertained by Dr. Emmons of Chester, to be the carbonated oxid of manganese, containing a portion of silex. I have been told that it has been traced south almost across the town; in going north, through the lots, it is seen scattered, for a few rods in width, at random, on the surface of the ground, and in stone walls. Hundreds of tons may be had by taking the trouble to collect it. The mineral is in rounded masses of various sizes; being broken they present a rose red, which, upon being exposed to the light for a few days, fades into a pale red, and finally the mineral becomes black, and is covered with a pellicle of the black oxide of manganese. I broke open one of these blocks, and by exposure to the weather, for a few months, it become black on the outside, as is universally the case with this mineral. Upon going north two miles, we find this mineral in place, at the junction of the talcose and mica slates: here the rocks rise above the geest, and the vein is seen several feet wide, and fifteen or twenty rods long, until the rocks disappear again under the earth. At the vein the black and gray oxide are found in abundance, and along with them there is a small vein of the siliceous carbonate seen also, imbedded in the black oxid: the manganese at this locality contains iron, and, several years since, the inhabitants of the vicinity, supposing the mine to be one of iron, erected a forge to smelt the ore, but it yielded so little iron that the project was soon abandoned. The rounded masses are found lying upon the ground in the immediate vicinity of the vein, and also north, in Plainfield, in considerable quantities. This vein may be nothing more than the continuation of the Hawley iron mine; at least, the Hawley mine and the manganese being both found at the junction of the talcose and mica slates, afford strong reasons for this conjecture. I have said that this manganese is seen at the junction of the talcose and mica slates; such is indeed the fact, two miles north of Cummington meeting-house, but at the meeting-house the mica slate is altogether the predominating rock; yet upon careful examination, it is sometimes found to possess a portion of talc with it, and I am inclined to believe that the rocks, at the junction in this place, pass into one another by insensible gradations.

Leveret Veins—marked 1 *and* 2 *on the map.*

There are two metallic veins in Leveret, both in granite. The first is one mile from the congregational meeting-house in a northwest direction, on the land of a Mr. Field. The vein runs nearly north and south, and was once considerably wrought, but abandoned on account of its unpromising appearances, and the shaft is now almost filled with leaves, earth and stones. The gangue is quartz and sulphate of barytes, and contains galena, pyritous copper, and blende, disseminated through the matrix. The vein is several feet wide, and may be traced some rods in length by the quartz crystalized and scattered along the surface of the ground. The second is one mile and a half from the first, directly south, on an eminence of granite eight or ten rods long, protruding itself above the geest. The direction of the vein is north and south, and it is seen along the whole length of the granite, until it sinks under the geest. The vein is but two or three inches wide on the surface, but widens as it descends into the rock; the gangue is sulphate of barytes, amorphous, lamellar, and of a pearly lustre. Galena, along with pyritous copper, is disseminated throughout the barytes, and is very abundant on the surface, but grows scarce, as the vein is followed down into the rock; which, however, has been done but a few feet. There the vein is nearly one foot wide, and is almost wholly barytes, with only very little galena, or any other mineral. Whether the vein is rich in ores deep down in the rock can be determined only by actual examination.

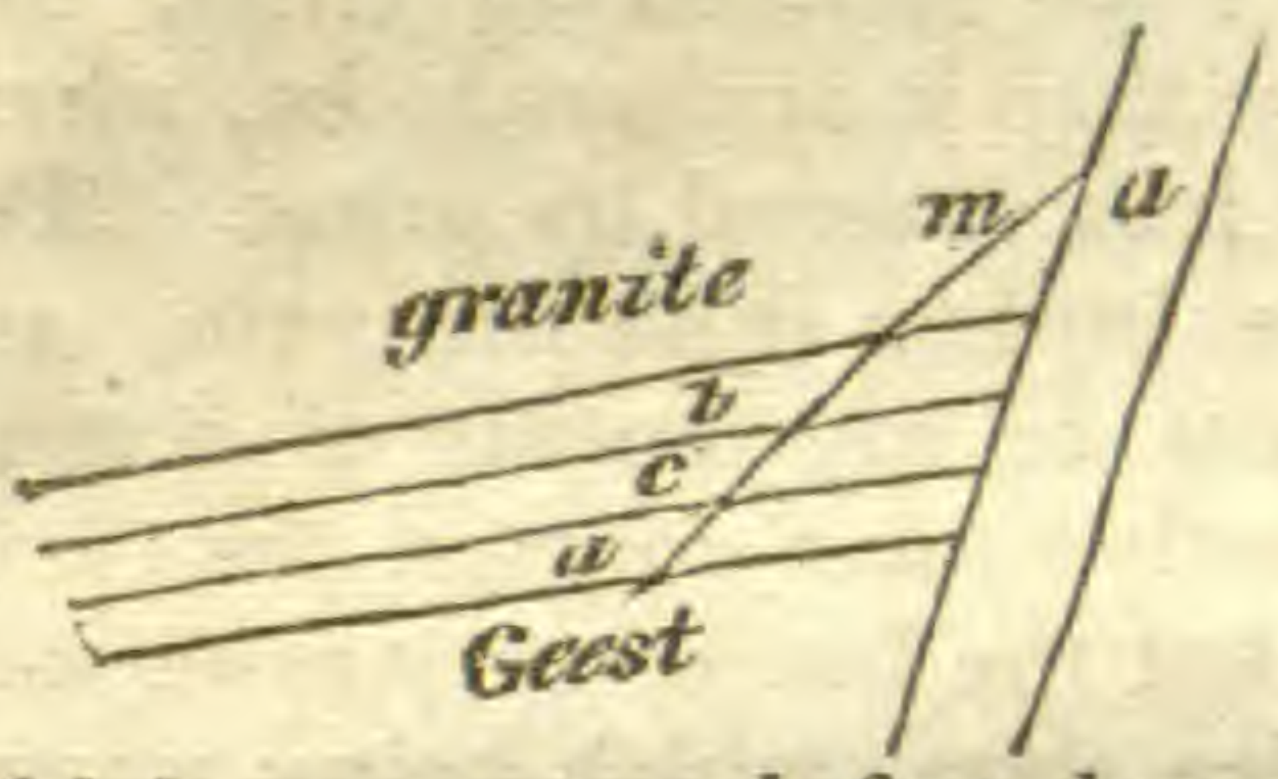
Hatfield Vein—marked 3 *on the map.*

This vein is in Hatfield, a mile or two west of the town, sixty rods north of the road leading from Hatfield to Williamsburgh, where it is intersected by the road from Whately to Northampton. There the sienitic granite breaks up through the tertiary, forty or fifty feet. As soon as the granite makes its appearance, a vein of sulphate of barytes is seen along with it, and may be traced on the surface, thirty rods or more, until it is concealed by the earth. The direction of the vein is north-west and south-east, and has had two shafts sunk into it during its course; the first was made near where the vein is first seen in the rock; it is fifteen feet deep. There the vein was one foot wide on the surface and three at the bottom. The second shaft was sunk at the place where the vein and rock are concealed by the earth. At this place, the vein is four feet wide on the surface, and seven at the bottom, twenty feet deep. The sulphate of barytes is found amorphous, which is its general character, although it is often laminated and sometimes cellular.

Enough choice specimens may be here obtained for all the cabinets in America. In the barytes are often seen druses, lined with quartz crystals; galena and blende are disseminated in the barytes, sometimes in great abundance, but generally in small quantities. The blende is of its usual resinous aspect, and considerable in proportion. The galena is in cubic masses, from the tenth of an inch to two or three in diameter; sometimes it is foliated, and again it is diffused throughout the barytes.

The next vein marked on the map, is situated three or four miles west of the Hatfield locality. Near the east part of Williamsburgh it discovers itself, breaking up in granite through mica slate. Greenstone and micaceous limestone are three or four feet wide; the gangue is quartz, in which is disseminated more or less of the black oxide manganese. Upon the ground near by these, are blocks of the gangue containing galena. As usual, the gangue is full of druses, studded with quartz crystals, some of which are imperfect amethyst. The direction of the vein where it first makes its appearance, is north-west, but it soon turns north, running into Whately a mile or more. It then turns north-east, under mica slate, and is finally concealed by a hill of greenstone. Along the whole distance, the vein may be generally seen, although it is often concealed for a few rods, by geest, but may be easily traced by detached blocks lying upon the ground. As the vein runs into Whately, it contains more galena but less manganese. Where the vein runs under the mica slate, it grows narrower as the stratum of mica slate becomes thicker, and the mica soon presents only a vein of crystalized quartz, two or three inches wide. I picked up a hand specimen of barytes near this vein in mica slate, but could find no more.

This vein, as before observed, is in granite,] protruding above mica slate, greenstone and micaceous limestone; and one remarkable circumstance is noticed where it first shows itself. This is on a hill of moderate elevation, which has apparently been lifted up by the granite beneath. The strata of mica slate, greenstone and micaceous limestone, resting upon the granite, have been thrown back almost into a vertical position. The vein, instead of following the hill in a longitudinal direction, cuts almost directly across it, and what is remarkable, sends off a vein of quartz two or three inches wide, across the strata, in a vertical position, as follows: there *a* represents the metallic vein cutting across the hill; *b, c, d,* the strata of mica slate, greenstone and micaceous limestone, in a vertical position, reposing again granite: *m*, the vein of quartz branching off from the metallic vein across the strata. The quartz has druses in it, where it intersects the greenstone *c*, which are several feet deep. The druses are studded with crystals, beautifully approaching to a green, owing apparently to the colouring matter of the greenstone having penetrated the quartz.



By going west one half mile from the last mentioned vein, another is seen, marked 5 on the map. This is also in granite, on a moderate elevation, and seems to rise from under the mica slate, but in the course of five or six rods, it sinks again under the mica slate and is soon lost, although, as with the last mentioned vein, a vein of quartz is traced in the mica slate, but grows narrower as the mica becomes thicker. But little galena or any other mineral is seen at this locality, although in the gangue of the vein, which is quartz scattered upon the ground in the vicinity, galena is seen in abundance. In several specimens in the neighbourhood, I found the earthy oxide of lead. (?) This lead is of a rather dull red colour, friable, or easily rubbed to pieces between the fingers; it is in cavities in the gangue, some of which are two or three inches in diameter, and completely filled with the oxide. Several blocks of the gangue contained black manganese, somewhat granular. Upon breaking the manganese, it had a metallic lustre, or at least it contained minute particles of a mineral scattered through it, of a steel gray, which I took to be iron; but it was insensible to the magnet. Sometimes the manganese was attached to the outside of the blocks only, then again it filled cavities; and in one instance, it appeared to be a kind of cement to hold two or three different blocks together. The width of this vein, where it is seen, is two or three feet.

Geest seems to cover all the rocks in the vicinity of this mine, to a considerable depth, and it is only where the vein is seen that the granite and mica slate appear to rise above it, although blocks and masses of quartz, strongly disposed to crystallization, and often containing galena, manganese and pyritous copper, are seen scattered at random upon the ground, for one-fourth or one-third of a mile, in the region around this mine. Sometimes I am inclined to think that this mine is much more extensive than it appears to be, where the vein is discovered, and that it is covered by geest. The blocks are probably the gangue broken from the vein, and scattered about by some currents or movements of waters, that might at one period or another have covered the earth. It may indeed be supposed, that the blocks or masses may have been nests of quartz in mica slate, which being wasted away, would leave the quartz as we find it; but the fact, that the masses are all rounded and that they appear much abraded, would seem to militate against the last supposition. I have often seen these rounded masses in the neighbourhood of other veins.

No. 6 is situated at the south-west part of Whately, on a high mountainous range of granite. The vein is three or four feet wide, and contains galena in considerable quantities, with a gangue of quartz. The situation of the vein is near the summit, and it makes its appearance where the granite emerges from geest, and runs along the range three-fourths of a mile, until it is again concealed by geest. The direction of the range is north-easterly, and the vein seems inclined to follow it. Sometimes the vein is covered, for a few rods, by geest, but may be easily traced by blocks on the surface, which blocks are much disposed to crystalization, and contain galena.

No. 7 is the Whately vein, already described by Prof. Hitchcock. One circumstance respecting this mine, which he omitted, I will notice; that is, the vein often sends off small veins of quartz into the granite. Some of them are several inches wide. These veins run off in all possible directions; sometimes they stray a few feet from the parent vein, and then return to it again; at other times they run parallel to it; then others branch off at right angles, and are lost in the granite. In all cases they are firmly attached to the granite, and they seem to be an ingredient of the rock, as much even as the mica or feldspar.

No. 9 is a vein of manganese, situated in the south-east part of Conway, two or three miles from the meeting-house. It is in mica slate or granite, and which it is not easy to determine, as geest conceals almost the whole of the mine. The manganese is of an indigo colour, its gangue quartz, which is often yellow, although much of it is milk white. Particles of a steel gray metallic lustre, insensible to the magnet, are seen disseminated in the manganese.

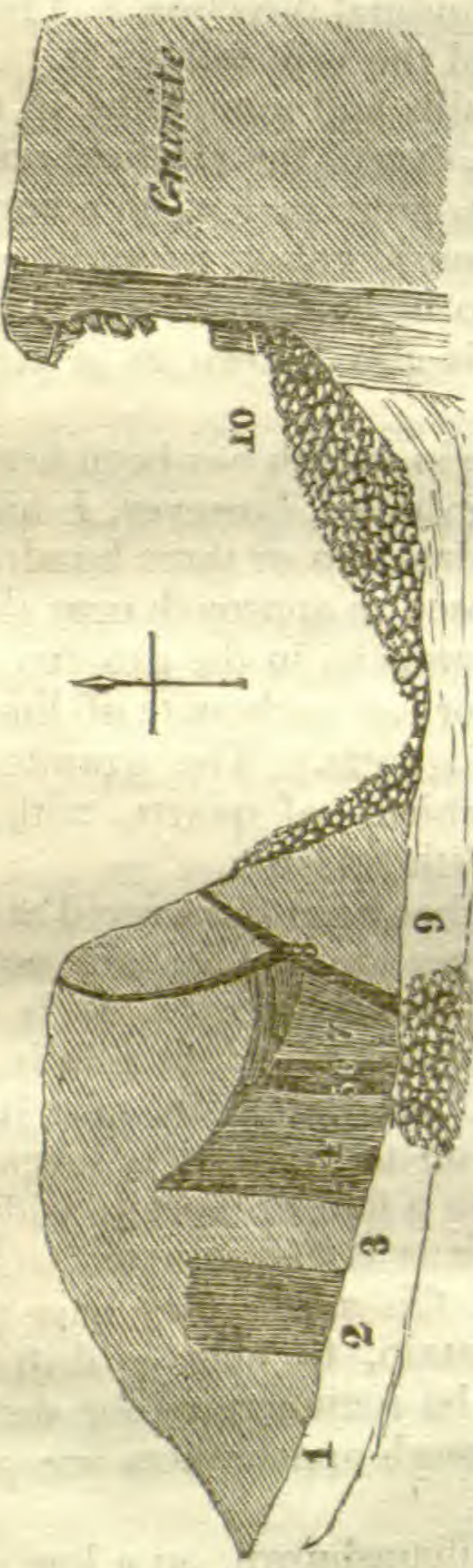
No 8 is in the east part of Conway. I know but little about this mine, and have put it down only to excite attention. I was at this place several years since, and saw a vein of quartz in granite, much crystalized, and some rounded masses containing galena.

No. 10 is in the north-east part of Williamsburgh. This vein has not been actually seen, but large blocks of quartz, much crystalized, are seen in a range, several rods wide, and one-fourth of a mile long, scattered in great profusion. The vein is evidently covered with geest. The blocks, upon breaking them, show the radiated quartz in abundance, and are rich in galena, with considerable pyritous copper. The galena is frequently in various stages of oxidation, and often the red oxide of lead (?) is found. Upon breaking open a block containing galena, and exposing it to the weather for a month or two, the galena grows considerably oxidated, and in the course of a year I have seen specimens completely forming the real red oxide of lead.

No. 11, also, has never been actually found, but there are pretty strong indications of a mine in Goshen, sixty rods west of the congregational meeting-house, and in a direction running parallel with the street. The rock, in this vicinity, is mica slate resting upon granite, which now and then shows itself above the mica slate. The whole appearance is much the same as in other places where I have seen a vein running under a patch of mica slate. Galena is found in crystalized masses of quartz, upon the ground: one specimen has been picked up that had a face twenty by twelve inches in diameter, filled with crystals. I have never had an opportunity to examine the region where these blocks are found, and have only put it down to excite attention.

No. 13 is situated quite at the south part of Williamsburgh, and runs into Northampton. It is remarkable for its connexion with the argentine, and the pseudomorphous crystalizations of quartz which it affords. Radiated quartz is also found at this locality. In order to have a correct idea of this mine, the reader must imagine a mountainous range running north and south, and gradually sloping on its eastern side until it reaches the foot. At the foot, a small brook runs parallel with the range, and on its eastern bank, three or four rods distant, a ledge rises thirty feet, looking directly up the mountain. The vein is the ledge, and extends along the bank of the brook a third of a mile, or more, but sinks under the earth at each end. Apparently, the granite in which this ledge is situated has been lifted up the thirty feet, on the eastern side of the vein, bringing up with it a part of the vein lying next to the elevated granite. How wide the vein is cannot be told, as the western part or side of it next the brook is concealed by geest. The vein or ledge contains more or less galena through its whole distance, and the rock or gangue is exceedingly tough and hard, so that it is almost impossible to break it with a sledge. The ledge is full of seams or fissures, which, admitting the water to freeze in them in the winter, thus project large blocks and boulders out of their beds, which blocks, falling away from the ledge, roll down to the bank of the brook. On the faces or sides of these detached blocks and boulders, and in druses, both in detached boulders and in the ledge itself, are found the pseudomorphous crystalizations of quartz. These crystals are in the form of hog-tooth spar, and in cubic projections, and were undoubtedly moulded by the carbonate and fluate of lime, which, in some unaccountable manner, have been displaced. The moulds themselves are quartz, and their surface is covered with the most minute crystals, pointing every way. These crystals on the outside of the mould are in six-sided prisms, with pyramidal terminations. Upon breaking open the moulds they are either hollow, or filled with small transparent crystals, in six sided prisms, with pyramidal terminations. These crystals often shoot out from the inner surface of the mould and completely fill it. Sometimes they rise from the face of the quartz, over which the spar and cubic crystals are moulded.

Some of the cavities or druses have their faces completely lined with the hog-tooth and cubic projections confusedly aggregated. The argentine is on the west side of the brook, opposite to the pseudomorphous crystalization, five or six rods distant. On this side of the brook too, the rocks seem to have been lifted up twenty or thirty feet, looking down the stream, and presenting a mural front towards the south, but gradually sloping northerly until they sink under geest. In this mural front is seen the argentine, forming two veins, one four, the other eighteen inches wide. The argentine is of a milk-white color, its texture is firm and is in undulating layers, which, upon a cross fracture, present a very remarkable pearly lustre. The argentine may be had by tons if any one will trouble himself to collect it. As this is the only known locality in America, excepting the one at Southampton, it is very interesting to geologists and mineralogists. The rocks forming the argentine cliff appear to be much displaced and confused, and are made up of granite, mica slate, micaceous limestone, and argentine, as may be seen in a profile accompanying this communication.



Argentine cliff, and a section of galena vein, containing the pseudomorphous crystalization.

The profile shows the mural front containing the argentine veins and a section of the ledge a few rods distant on the east side of the brook containing the pseudomorphous crystalization. First, beginning on the western side of the cliff, (1) is twenty feet of granite; next mica slate, (2) six feet, passing into granite above; (3) granite, five feet; (4) mica slate, seven feet, also passing into granite above; (5) micaceous limestone, one foot, running into mica slate above; (6) a vein of argentine four inches wide, and rising six feet high, cut off by mica slate; (7) mica slate, four feet, with granite over it; (8) a vein of argentine eighteen inches wide at its base, forking into the granite as it rises above the mica—this argentine vein rises fifteen feet, almost to the top of the cliff; (9) granite, eighteen feet—(it will be seen that the granite in the cliff caps the whole of the other rocks;) (10) the section of the metallic vein, containing the pseudomorphous crystalizations. The veins of argentine would be called by some dykes; probably this appellation, as they now appear, is correct, but I think that any one who views this cliff must say that the whole has been in some way forced up above the surface, or thrown back from the metallic vein, and that the argentine once formed veins in the rocks.

No. 14. At this locality are seen blende, pyritous copper, and galena. It is in the south-west corner of Northampton, on the side of a steep hill. The vein, three or four feet wide, is in granite, which rises above geest, and runs along in the granite, several rods, in a longitudinal direction. The range of the hills is north-easterly, and the vein follows it. Indeed the general course of all the hills and mountains in this region is the same, and the veins, except the Hatfield one, conform to it; that is, they all run, more or less, in a north-east direction. This vein was considerably wrought, several years since, and is supposed to be connected with the celebrated Southampton mine; it is also known as a place denominated "the minerals."

No. 15 is the Southampton vein, which has been heretofore amply described. One circumstance, however, I have not seen noticed; it is this, for the last two or three hundred feet, in the drift which is now supposed to approach near the vein, druses are met with, in great numbers, in the granite. These druses generally contain more or less carbonate of lime crystalized amongst the crystals of quartz. The granite is extremely hard, and is formed chiefly of quartz, with a few small scales of mica, and but little feldspar.

No. 16 is the new mine that has been discovered at Southampton, three or four miles from the first, or old one, in a south-west direction, towards Montgomery. It is on the same mountain range with the old one. This vein is also in granite, its locality being on a high mountain, near its summit. It makes its appearance on the surface, is several rods in length, and from six inches to a foot or more in width; the gangue is quartz, in which galena is disseminated. A company has been formed to work this mine; they have begun, some distance down the mountain, to blast a drift to the vein. Whether they ever will be remunerated for their toil, time alone can determine, although appearances are promising.

Iron Bed. This is in Williamsburgh, on a low or wet piece of ground, 30 rods south of the road leading to Northampton, and half a mile below the village. Several specimens of what is called bog iron ore, have been obtained in digging a drain. The ore settles at the bottom of the drain in the form of rust. Sometimes particles of iron, with a metallic lustre, are conglomerated. The earth of this bog, when dried, seems to be composed principally of red ochre; but sometimes it contains yellow ochre. This bog covers an acre or two, and the earth yields from 20 to 50 per cent. of iron.

List of simple minerals.

Schorl. In Williamsburgh, in veins of granite, in mica slate, and in granite lying contiguous to mica slate, in six, nine and twelve-sided prisms, with triedral summits. Also, in Chesterfield, frequently stellated. Indeed, crystals of schorl are found in this region, of all sizes, where granite is near mica slate. Sometimes the schorl is amorphous, and in fragments.

Beryl. In Williamsburgh, in granite veins; some of the crystals are two or three inches in diameter, and five, six, or more, in length. Also, at Goshen and Chesterfield, and wherever there are granite veins, or where granite lies next to mica slate.

Red oxide of Titanium. In Williamsburgh, in quartz in mica slate; some of the crystals are as large as a man's thumb, and handsomely geniculated. Also, at Chesterfield, Goshen, Conway, and Whateley, in mica slate, and in nests of quartz in mica slate.

Precious Garnet. In Williamsburgh, in granite; at one locality a face of a boulder of granite was filled with them. The garnets are, many times, transparent, or nearly so; and in all cases highly translucent.

Common Garnet. In Williamsburgh, in mica slate, talcose and hornblende slates, in vast quantities. Also, at Chesterfield, of an enormous size, in the cyanite rock; when perfect, all are dodecaedra. Sometimes the garnet is massive, and sometimes crystalized in groups.

Epidote. In Williamsburgh, in quartz; this epidote is in layers, and contains small garnets, which are a focus around which the epidote is radiated.

Zoisite. In Williamsburgh; also, at Conway and Chesterfield.

Plumose mica. In Williamsburgh, in granite. This mica is often in the form of plumes, some of which are six inches in length.

Graphic granite. At Williamsburgh. Among the curious resemblances to written characters, I observed one like a large capital J. This curious assemblage I believe is found only in granite veins, or where granite lies near mica slate.

Asparagus stone. In Williamsburgh, in granite in prisms or cylinders, several inches in length, and another variety of

Phosphate of lime. In Williamsburgh, in mica slate, not having any describable form.

White marble. In Goshen, in a boulder dug up in excavating the earth for a mill race. Also, at Williamsburgh, in a rounded mass. Also, at Cummington.

Cummingtonite. At Chesterfield and Goshen, in mica slate.

Talc. At Cummington, in steatite, associated with bitter spar.

Actynolite. At Williamsburgh, in talcose slate.

Fasciculite, (radiated hornblende.) At Williamsburgh. Also, in talcose slate.

Staurotide. At Goshen, Cummington and Chesterfield, in mica slate stratified; it lies between the strata, so that plates may be obtained full of crystals.

Serpentine.—In Williamsburgh, lying in masses here and there; this is the common serpentine.

Precious Serpentine. At Williamsburgh, in a bank formed by a stream cutting down into the tertiary formation.—This serpentine is fully equal to that of New-Haven; it exists only in small blocks.

Green jasper, was found in the same bank in small blocks.

Red jasper, was also found at the same locality.

Milky quartz. At Chesterfield, in nests in mica slate, and in blocks and boulders, scattered upon the ground.

Amethystine quartz. At Chesterfield, in granite. This quartz is nearly transparent or limpid, and is highly colored.

Ferruginous quartz. At Williamsburgh, in blocks. It strongly attracts the needle.

Crystalized quartz is found wherever there is a vein of galena. It is generally in six-sided prisms, with pyramidal terminations; sometimes it is mammellated, at others cellular. Some crystals are transparent, others not at all so. I have seen crystals several inches long, and one or two in diameter. Often it is radiated.

Galena. This mineral is found in most of the metallic veins of this region. It is often met with in quartz scattered upon the ground, in the vicinity of metallic veins.

Pyritous Copper. This mineral is generally associated with galena.

Blende. This is another mineral found also in metallic veins.

Earthy oxid of lead. (? Ed.) In cavities in quartz, near a galena vein in Williamsburgh.It is friable, and often has carbonate of lime along with it.

Green carbonate of copper, in Williamsburgh, in cavities in quartz, near a lead mine.

Cyanite, at Williamsburgh, in mica slate, in small quantities.

Argentine. In Williamsburgh.

Sulphate of barytes is often seen forming the gangue in galena veins.

Manganese is frequent in galena veins in quartz. It also forms a vein at Conway, and another at Cummington.

Compact oxid of manganese. In Williamsburgh, in quartz. This manganese has a metallic lustre when broken only at certain spots. This lustre is a steel gray.

Iron Pyrites. In Williamsburgh, in quartz.

Kaolin. In Williamsburgh, of a superior quality; also at Conway.

White augite, (spodumene? Ed.) In Williamsburgh. It is associated with granite in boulders.

Brown haematite. In Savoy, found in a stone wall by the way side, as we rise the hill from Adams. This haematite is stalactical, and the fracture is radiated from the centre of the stalactite.

Granite veins in granite.

Granite veins in granite, as well as in mica slate, are frequently met with in this region. They are in fact granite veins as much as any granite, or hornblende veins, seen in mica slate, are true veins. These granite veins in granite are, I believe, found only in granite contiguous to mica slate, or very probably the granite in which they are situated, may be itself only immensely large granite veins in mica slate;

thus forming granite veins in granite veins. What leads to this conclusion is the fact, that small granite veins in mica slate, often contain in themselves still smaller granite veins.

These granite veins in granite are of all sizes, from a mere line in breadth, up to three or four feet. They are as firmly attached to the granite in which they are situated, as the granite is to itself, and upon a fracture, one side of the vein will cleave to the granite on *its* side, and the other to the other side; but they are in reality granite veins. They are of a coarser texture than the granite in which they are situated, and of so different a complexion, that they may be distinguished at the distance of a furlong, and not unfrequently they dash off into mica slate, lying next to granite. See figures 1, 2 and 6, at the end of this communication.

These granite veins run in all possible directions; sometimes they run parallel one with another, and continue their breadths for rods, with mathematical exactness, and then grow narrow or run into one another. Sometimes they converge gradually together, and then diverge again into their former distances from one another; at other times they meet at a focus from all directions, as in figures 3 and 4; then again parallel veins will be cut off by one running at right angles with them, and these parallel veins branch out or run into one another; and some of them are jogged out of their course a foot, or two or three feet, to the right or left, forming shoulders as in figures 6 and 7, while other parallel veins with them continue their accustomed course. I have often seen veins in mica slate send off branches across the mica slate, into the neighbouring granite, as in figure 2, and often a vein of granite in mica slate will run parallel with the strata, and another vein then turns directly across the strata, and across the parallel vein to the other side, and then turns again about at the same angle, into the strata, and runs parallel again, as in fig. 1. Sometimes they gradually narrow and come to a point as may be seen in several figures in this communication. The granite veins in granite and in mica slate, as well as hornblende veins in mica slate, all seem to be perfectly analogous to the galena veins in this region, and so far as the granite and hornblende veins are concerned, I must say that they seem to be cotemporaneous, or nearly so, with the rock in which they are situated. They all adhere firmly to the rocks in which they are found, without the least fracture or fissure between them. I ought to have mentioned that the granite veins as well as the hornblende veins, frequently connect beds of their respective rocks together, and sometimes seem to connect together different veins.

The best place for viewing granite veins in granite, is two miles south of the meeting-house in Williamsburgh, on the road to Shepard's manufactory, on the land of Mr. William Pomeroy. This locality is in full view as we pass along the road, and can scarcely fail to attract the attention of the geologist.

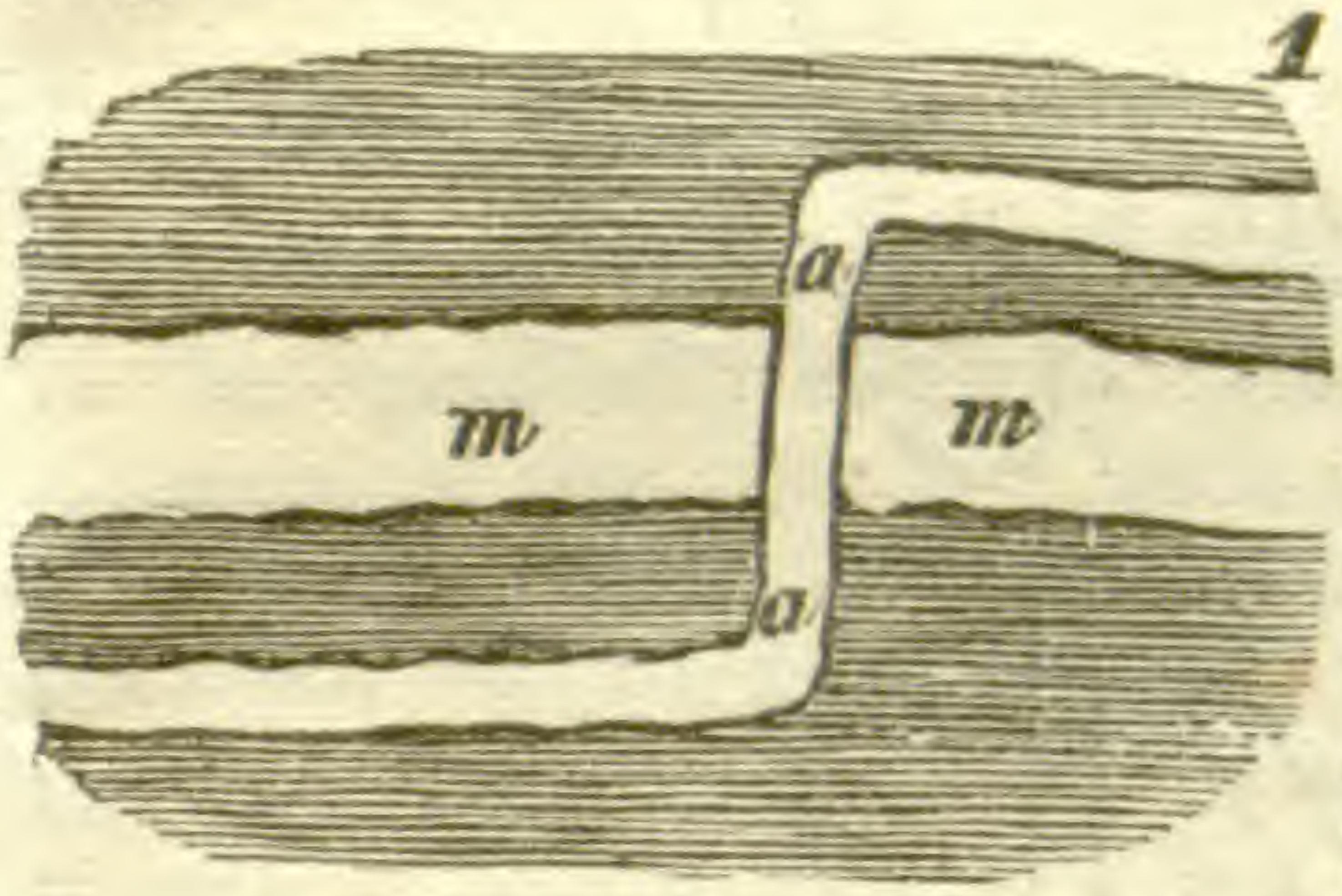


Fig. 1. Here *m m* is a granite vein, three feet wide, in mica slate, running parallel with the strata: *a a* is another granite vein, six inches wide, running parallel with the strata, then cutting directly across the first granite vein, *m m*, to the other side, and then running parallel again.

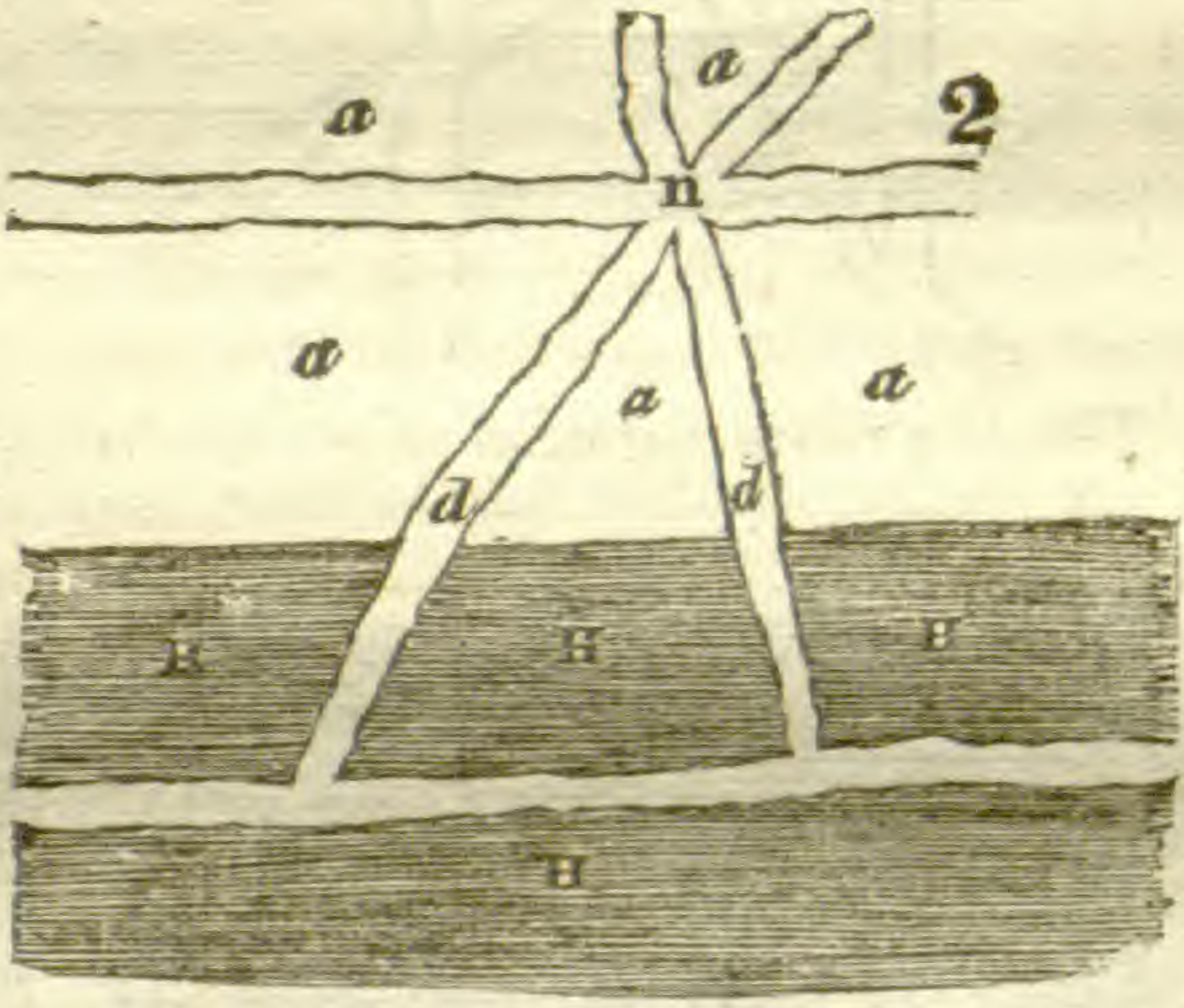


Fig. 2. Here H H H H represent mica slate lying next to granite, *a a a a a*, with a granite vein, eight inches wide, in it, running parallel with the strata. This vein sends off two branches *d d*, across the strata of mica slate, which lie in a vertical position, into granite lying next the mica slate: these branches have a focus at *n*, with another granite vein ten inches wide, but in granite.

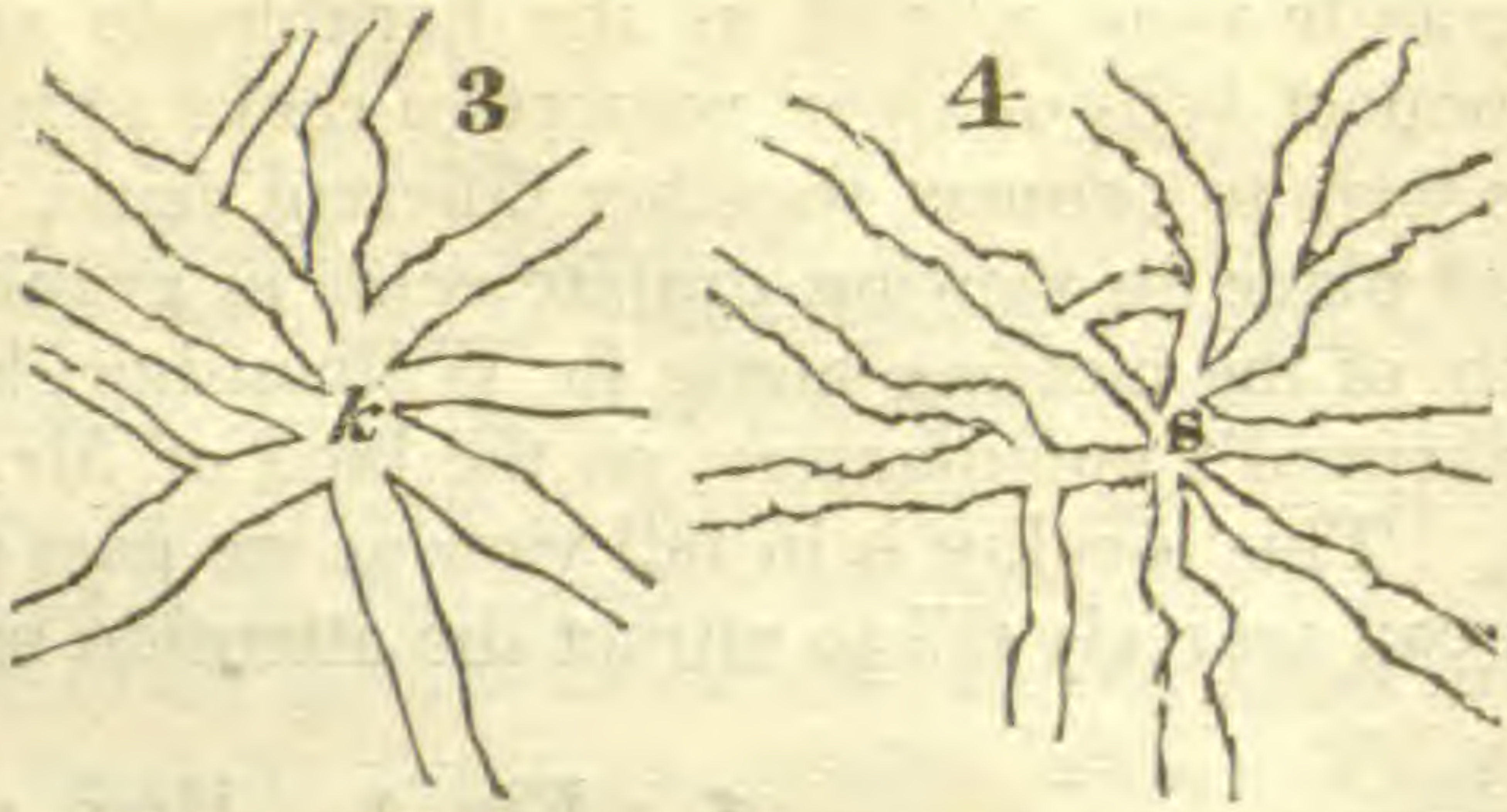


Fig. 3 and 4. These two figures represent granite veins in granite, forming a focus each, as at S and *k*.

Fig. 5. This is a representation of granite veins in granite.

Fig. 6 represents mica slate lying next to granite; *g* is mica slate; *U* is granite with several granite veins it, running in a longitudinal direction. These granite veins are generally from six to ten inches wide, and are cut off by another granite vein B, seven inches wide, running into the the mica slate, then turning and running parallel with the strata, which are in a vertical position.

Fig. 7. This figure is also a representation of granite veins in granite. Indeed they show themselves in all possible forms.

Remarks upon galena veins.

As we recede from the Connecticut valley, on either hand, the primitive rocks emerge from the secondary formation, until they gradually rise into mountains. As we approach the mountains, one primitive rock disappears after another, until we see the granite composing the mountains themselves, with the other rocks leaning against it. Sometimes the other primitive rocks seem to be wanting, and then the granite makes its appearance at a low level, soon after we leave the secondary formation, as at Southampton and Leveret.

Where the granite first makes its appearance, or soon after, is the place for galena veins; that is, they are found in the mountainous ranges of granite, which stretch along north-east and south-west, on the borders of the Connecticut valley, running parallel with it. My observations have been principally confined to the range along the western border, where it will be seen, by the map accompanying this communication, that most of the veins described are situated. I have never had time to examine, with much attention, the range along the eastern border. Two veins only have been discovered on this side, and these are both at Leveret; very probably others might be found, by thorough examination.

These veins, except that at Southampton, never have been extensively wrought, on account of the very ready demand for capital for other purposes; but when the population of this country shall have become dense, and capital abundant, we may expect that these mines will be thoroughly explored; and every thing promises that they will be found as rich in treasures as similar veins in other countries.

The galena veins, all except that at Hatfield, are situated in granite of the oldest formation; that is, in the granite which emerges into mountains, and seems to be the foundation rock upon which the others rest. A question naturally occurs; whence is the origin of these veins? The Neptunians would say, that they were once fissures, and were filled from above with a mineral solution, that once covered the globe; and if the question were asked, how these fissures were made, they would say that the mountains were unequally supported, and one part subsided, thereby forming a fissure; and also that fissures were sometimes made by desiccation of the rocks. Against these suppositions the following considerations may be stated.

If desiccation, or subsidence of the mountain, produced these fissures, they would be widest on the surface, and narrower as they descend into the rock; but the reverse is the fact. These veins have been invariably found, when wrought, to be narrowest on the surface, but to widen as the shaft went down into them. And again, if a mineral solution did once cover the globe, we ought, by the laws of gravitation, to find beds of metallic matter in vallies and plains; but no such beds are found in this region, and no one will suppose that there was just enough of this mineral solution to fill the fissures, and no more. But, granting that this was actually the case, I cannot conceive by what law this mineral solution would direct itself to the fissures only, when their surfaces were so insignificant, and when the fissures were much higher than vallies or plains. It is only in elevated regions, that the galena veins of this neighborhood are seen, and the supposition that they were filled from above, is at war with undoubted philosophical principles. Nor do I believe with the Plutonians, that these veins were filled by an injection from a fiery furnace below, but that they are cotemporaneous with the rocks in which they are found; and I think that I am warranted to make this conclusion from the following facts.

1st. They are perfectly analogous to granite veins found in this region. It will be said, that the walls of metallic veins correspond to one another. It is true; and such is the fact with the granite veins found in this region; and in one instance I saw a granite vein traversing mica slate, and passing directly through a nest of quartz, leaving one half of it on one side, and the corresponding half on the other; but the vein of granite was firmly adhering to the quartz and mica, without the least fissure between them. That a metallic vein does not adhere to the walls so firmly, is no argument against its cotemporaneous origin with the rock in which it is situated. In one case the vein is lapideous, in the other it is metallic; they are different.

2d. In the drift to the Southampton mine, crystals of calcareous spar are seen in druses, in great abundance, in the solid granite, as the galena vein is approached. I am further led to believe that the galena veins were created along with the rocks in which they were situated, from the fact that the mica slate, in the immediate vicinity of the galena veins, often has veins of quartz in it, much disposed to crystalization; and when this is the case, it is a pretty sure indication that a galena vein is near by. Sometimes the nests of quartz in mica slate, near galena veins, have this tendency to crystalization, and may occasionally include a little galena or copper pyrites. The galena veins of this region sometimes cut through mica slate, but their real place seems to be in granite, below the mica slate, and they only now and then run into the mica slate above. Whenever they do run thus into the mica slate, they invariably grow narrower as the stratum of mica slate grows thicker, and are soon lost to our view. These galena veins seem all to have been exposed by the wearing away of the superincumbent rocks, and we may with propriety suppose that many more are concealed by the rocks above them, and also by the tertiary formation.

It is worthy of remark, that the surfaces of the galena veins of this region, especially when the gangue is quartz, are frequently smooth and somewhat polished, and have the same external appearances as the blocks of quartz found in brooks and exposed to a torrent. The same remarks will apply to nests of quartz seen in mica slate; they frequently have this polished surface, which seems to have been done by the agency of water. In the vicinity of galena veins are found blocks of their gangue, containing their minerals. These blocks are loose, detached masses, generally rounded and polished, and were undoubtedly broken from the veins and scattered about by the agency of water, whence, by attrition, they have acquired their rounded and smooth surface.

The galena veins of this region seem to have been exposed to view by the agency of the ocean, which has, to appearance, swept away vast quantities of rocks down to granite. All these veins, except that at Hatfield, seem disposed to follow the granite ranges which stretch along the borders of the Connecticut valley, north-east and south-west. They follow these ranges in a longitudinal direction, although when there is a sudden swell, they frequently shun it, by turning a little to the right or left, and then turn again into their wonted course, and pursue it until their concealment or termination.

It has often been supposed, that the galena veins of this region were all connected with that of Southampton, which has been said to extend to Hatfield and Leveret. For a long time I supposed this to be the fact, and that all these veins were only ramifications of the Southampton vein; but, after after the most careful examination, I am constrained to say, that they are all separate and distinct, and have no connexion with one another. Each has something peculiar to itself, and in no instance have they been known to run into one another. Would a vein, ten or fifteen miles distant from another, with different minerals and a different gangue, and running in a different direction, have, naturally, any connexion with that vein? Such is the fact as regards the Southampton and Hatfield veins. The truth seems to be, that the granite-ranges, along the borders of the Connecticut valley, are filled with different veins of galena, that have no connexion with one another. Fourteen have been already discovered, and probably many more will be. I have said that the true place for the galena veins is in granite of the oldest formation, but that they sometimes extend into the mica slate above. Such is the fact; and I am of the opinion, that the sienitic granite of Hatfield, containing the vein at that place, occupies both the place of mica slate and of the hornblende rock, and rests immediately upon granite; at least the sienite and granite run into one another at Northampton, without any mica slate between them.

I am aware that it will be objected to the cotemporaneous origin of the galena veins with the rocks in which they are situated, that the walls of the veins do not adhere to the rocks, but that they are very determinate, being marked by a delicate seam, or by a layer of some indurated argillaceous substance between them. This is generally the case, but not always, and in reply I can state, that this is sometimes the fact with nests of quartz in granite, especially when amethystine. I have several specimens now before me, that were blasted out of granite, of the oldest formation, having this indurated substance adhering to them. Now, how much more should we expect to find this substance in galena veins, when the quartz not only contains a variety of minerals ; but when the gangue itself is often *sulphate* of barytes?

Note A. The geest of this region may be properly divided into three varieties. First, that which is found in the neighborhood of granite, and may be properly called the hard-pan formation, and seems to owe its origin to the abrasion, disintegration and decomposition of granite, and of some other primitive rocks. This variety is usually found on granite hills and mountains, affording a shallow covering to the rocks beneath. As we descend a hill or mountain, this covering grows thicker and thicker, until we reach the plain or valley below ; here it is frequently of great thickness, even of 50 or 100 feet or more. Torrents, coming from elevated regions, carry more or less of this covering down the stream, and deposit it on the banks of the stream in low lands. This, together with leaves, sticks, rotten wood and decayed vegetables, forms alluvion. This variety of geest has imbedded in it vast quantities of blocks and boulders of granite, and some other primitive rocks. These blocks and boulders are often seen lying about upon the surface of the ground, and are exposed also to view, by excavating the earth and when torrents carry away the earth in which they are imbedded. This earth is frequently cut through and carried away down to the rock formation, leaving these blocks and boulders behind, in the bed of the stream. The soil which this variety constitutes, is covered with a few inches of vegetable mould, and is fertile, but not adapted to tillage. It is stiff and hard, retaining much moisture ; and affords excellent pasture. The timber growing upon this soil is maple, mountain ash, beech, birch, red ash, hemlock, spruce, rarely chesnut or butternut, sometimes walnut, but these three last are usually found upon what is called the second variety.

The second variety also contributes its share to the formation of alluvion, whenever circumstances are favourable, but it is seen at a lower level than the first, and amongst mica slate, micaceous limestone, greenstone, &c. This variety has no granite boulders in it, though it contains pebbles, blocks and boulders, of the rocks with which it is associated, some of which are of an enormous size. It affords a mellow soil, well adapted for tillage, and the growth of grain. Grass grows well from this soil, whenever there is moisture sufficient, but it is much exposed to suffer by drought. The timber is chesnut, butternut, walnut, elm, white and yellow oak, soft maple, sometimes hard maple and hemlock. The third variety is called plain land, and lies directly upon, or covers the secondary rocks. It is usually about as extensive as conglomerate and old red sandstone, although it sometimes covers more or less of the primitive formation. This is the fact at Hatfield and Northampton, where the sienitic granite is often covered several feet in depth with this variety. It sometimes even approaches into the regions of mica slate and granite. When in the regions of granite, it generally lies in small hills, but in some instances it is directly upon the rock formation, with the first variety over it, as is seen in the profile under the tertiary formation. This variety is a sand; it contains more or less scales of mica, some of which are an inch or two in diameter, and small angular fragments of quartz. It has frequently a mixture of clay with it, and it sometimes runs into clay. Generally speaking, this variety has no blocks, boulders or pebbles in it, but is a dry sand, and were it not for the rains that constantly water the earth, we should have the deserts of Arabia wherever it exists. This soil is naturally so dry, that it is good neither for grazing nor tillage; but by the application of gypsum or ashes, it amply rewards the toil of the husbandman. The timber of this soil is shrub oak, white and yellow pine, some white oak, &c. It has been sometimes said that this variety is an original formation; be this as it may, it has been carried evidently over the rocks since the formation of conglomerate and old red sandstone; i. e. these rocks are covered with it along the Connecticut valley in some instances to a great depth. At other times we see but a few feet or inches of it over them. Often, they emerge above it, even sometimes into mountains, as is the case with Mount Toby, in Sunderland. When this variety is found in the neighborhood of primitive rocks, I have noticed that it is often seen deposited in layers with gravel between them. From the large scales of mica seen in this sand, one would be led to suppose that at some period it had come in contact with granite or mica slate, or that it was formed by abrasion.

P. S. I have entirely omitted the coal formation, not being able to furnish any additional facts; and on the map, the coal formation, as well as the geest, though but a small part of it, as it is really seen on the surface, is represented as geest.

ART. X.—*Taxidermia.*

IN our last number, we mentioned our intention of giving some extracts from a late German work on the art of preparing and preserving specimens of natural history, by J. F. HANMAN. Having been favoured with a manuscript translation of the most interesting part of this memoir, (for the communication of which we are indebted to Dr. WM. MEADE,) we now commence the subject, with instructions for preparing the class mammalia.

On the art of preparing and preserving specimens of the Animal Kingdom, for Cabinets of Natural History, in a simple and effectual manner. Translated from the German of J. F. HANMAN.

CHAPTER I.—GENERAL INSTRUCTIONS.

Section 1.—*Of the different methods of stuffing animals.*

When the science of natural history first began to excite attention, it was soon perceived that good collections of specimens were indispensable to a proper prosecution of the study. Great exertions were accordingly made to discover the best methods of preparing and exhibiting them; many observations and experiments being instituted, the results were communicated, sometimes in fugitive pamphlets, and sometimes in essays dispersed, through larger publications. The greatest difficulties were encountered in the preparation of animals; this branch of the art engaged many ingenious minds and industrious hands. But the methods and means made use of, varied in proportion to the number of collectors and preparers. Every individual had his peculiar manner, which he had either invented himself, or which he had extracted, and perhaps improved from the directions then published. Almost every collection will show this, if the specimens are closely scrutinized. But there were few among the earlier artists who succeeded well, and it is but of late years that the art of preparing and preserving animals, so as to retain their natural attitude and appearance, has arrived at any degree of perfection. Of these methods, one of the simplest and most ancient was drying or baking the specimen; but it is at the same time the most imperfect, and it can be recommended only in a very few cases, such as the preservation of insects. To prepare the smaller species of birds and beasts in this way, (for none but small ones could be attempted,) the entrails being removed, and the brains drawn out through an aperture in the roof of the bill, the vacancies were then filled with some antiseptic drug. Wires being then introduced into the limbs, to give the degree of stiffness necessary to fix them in the intended attitude, they were lastly exposed to a gentle but gradually increasing heat, until they became thoroughly dried. But such specimens could not last long, for they were unluckily, a complete harbor for every species of insect detructive to collections of this kind. Notwithstanding, very powerful agents were made use of, which under different circumstances, might have proved successful, yet, the great mass of animal matter remaining in them, which could not be thoroughly saturated with the preservative appplication, afforded an unstinted feast to these depredators. An improvement upon this mode, was the removing, in addition to the entrails and brains as above stated, of all the larger muscles, such as the fleshy parts of the breast, wings and thighs, leaving the bones, and filling the vacancy with tow, dipped in some powerful solution.

The next project was stuffing; that is, the skin was taken off and the flesh being cut away, and the bones scraped clean, it was filled with various suitable materials, impregnated with antiseptic and corrosive agents. Thus prepared, the skins certainly kept better, but were nevertheless deficient in other qualities equally essential to every perfect specimen, such as a just proportion of the limbs and other parts of the body, a natural attitude, and the like. These defects arose partly from an improper mode of skinning, partly from a wrong way of stuffing, so that to avoid these defects, they even hit upon the notion of cutting a model of the body of the animal out of wood, and stretching the skin over it. The bodies of smaller beasts and birds were formed out of gypsum, and then the skin drawn over it, or the feathers glued on, one by one. But these schemes were too tedious to find many followers, especially as it required great expertness to succeed tolerably. Besides, as the models were often imperfect, and the skin could not therefore be made to fit on neatly, the old defects were not thereby obviated.

Specimens of the smaller birds and quadrupeds were also preserved in spirits, but these also are now skinned and dried, thus retaining their shape, and especially their colours, which last are often changed and entirely discharged by alcohol. This last mode of preservation should therefore be applied only to subjects which will not bear stuffing, on account of their soft, juicy or slimy texture, such as many amphibia, some of the smaller fishes, and all the vermes.

All mammalia and birds, as well as most amphibia and fishes, in order that they may show to advantage, and to afford instruction as well as entertainment, should be skinned and stuffed. Our closest attention must be directed to imitate nature as nearly as possible in the contour of their bodies, in position and attitude. In stuffing, we must aim at giving the prepared skin the same appearance as if the body of the animal was yet inclosed therein.

Our author then proceeds to mention several names of persons who have been distinguished for their skill in the art, and discusses the relative merits of their methods. He gives us the names of Naterer, Schaumburg and Hoffman. The last is his favourite, and was also his instructor. A close application to the study, for more than twenty years, as we are informed, entitles him to great weight in the precepts he delivers; but the superior excellence of his mode, and the fulness and minuteness of his instructions, will of themselves be a sufficient recommendation, especially when we consider the total destitution of books (at least in this country) written professedly on this subject.

In his second section, he describes the requisite instruments and apparatus; but, as there is nothing peculiar in these, and they are all mentioned over again in the body of the work, it has not been thought necessary to present his formal list of these articles. It has also been concluded, for a similar reason, to omit his observations and minute directions, delivered in the third, fifth and sixth sections, respecting antiseptic drugs and solutions, the painting of the eyes, bills, legs, &c. of birds, and the manner of putting the specimens up in cabinets or glass cases.

His fourth section contains a very ingenious recipe for the formation of artificial eyes, which will be given under the head of birds.

We now proceed to the preparation of quadrupeds.

II.—Of Stuffing Quadrupeds, (Mammalia.)

Section 1.—*Flaying, or stripping off the skin.*

The first thing to be observed is to close the mouth and nostrils, by stuffing in *tow*, so that during the operation no blood or other filth may exude from them, and thus give the operator unnecessary labour in washing off the stains. For although this is not attended with the difficulties we meet with in removing spots and filth from the plumage of birds, still we must endeavor to guard against it as far as possible, because the washing out of such spots consumes time, and is a very disagreeable job.

In order to take the skin off, the animal is to be laid before us in such a manner that the head may be towards the right, and the tail to the left hand. But, as the covering of animals varies very much, a corresponding variation takes place in this operation. In general they are covered with hair, and there is but a very slight difference between the management of such as have horns, and such as have no horns. The incision in all these is made on the back, but on the contrary, such as are covered with prickles, or have armour, or scales, as also the *whales* (cetaceous tribe) are cut open along the belly.

Before we commence stripping the skin, take some soft blotting paper, tear it into pieces, and moisten it with water. These pieces of damp paper we lay near at hand, and make use of them during the operation, for preventing any filth from attaching to the hair, by sticking them along the edge of the skin as it is taken off, so that they project a little beyond the edge and keep the hair from touching the flesh, and thus getting soiled.

The animal accordingly being laid before us (if of the hair clad species) upon its belly, the head, as before mentioned, towards the right hand ; now place the point of the knife between the shoulders, exactly upon the back bone, pierce the skin, and draw it along slowly till you come to the crupper, or near the insertion of the tail. The skin being thus slit open, we attempt to detach it from the body with the knife, until we can seize hold of it with the fingers, then partly thus, and with the flat handle of the knife, we loosen it from the flesh down to the belly, then turning over the subject, proceed in the same way with the other side. In the mean while, we are not to neglect using the above mentioned moist paper, for the skin soon becomes dry along the edge, and curls so that the hair cannot possibly be secured from soiling without this precaution. We then are to attempt stripping the skin from the tail, by pushing it down on all sides with our nails to the very extremity. This business is attended with much difficulty, especially in such animals as have very delicate *tails*, but we may render it less laborious, by twisting the tail round, as you would a withe, until a sligh crashing is heard. But great care is necessary with the mures (glires.) especially the naked tailed, for the skin, as well as every other part of these animals, is of so slight a texture, that they tear with the slightest force.

The tail being stripped, we separate the skin at the anus, with the shears, from the body, and then go on with the parts of generation. We then are to strip the legs one after the other down to the nails, claws or hoofs, and then proceed to cut loose from the muscles the bones, so that, entirely freed from the flesh, they still retain their connecting ligaments in the joints; but we separate in the knee joint the upper thigh bone from the lower, leaving it in the body, as useless for our purpose ; but all the other bones are to remain in their connexion in the skin. We may indeed leave a part of the thigh bone, and this may be some assistance in forming the artificial leg (thigh ;) but we must not leave the whole ; perhaps the half is sufficient.

The hind legs being done, the skinning is continued towards the breast, until we reach the shoulder blades. Here we go on, exactly as we did at the hind legs, and divide at the joints, where the shoulder blade is articulated to the arm bone, the bones from one another. The bones being carefully freed from the muscles, we proceed with the skinning. About the neck this is soon performed. We then come to the head, a part which should be managed with much care, if we wish to succeed.

The stripping of the skin from the head of quadrupeds varies, for the skull of some of them are provided with horns, in others they are wanting. As the hornless are the most numerous division, I will describe the treatment of these first. But there are also two ways of preparing these heads for stuffing. The most certain, particularly for beginners, is this : Strip the skin loose as far as the ears, and then endeavour to raise the ears with all their skin out of their cavities with the knife ;

then go on with the dissecting to the eyes; raise these too out of their cavities, being careful not to injure the eyelids.

Continue the dissecting down to the nose, as low down as you can without injuring the nostrils. Then we cut through the skull and the underjaw, (in small animals with the shears, in the larger with a knife, but the saw must be used for the largest,) in the same direction, which will be mentioned in the chapter on birds, and as it is represented in figure 1, in the plate more clearly by the line *a b*. Thus the whole upper part of the skull and the lower jaw, to the back toothless part thereof, remains in the skin.

The carcase thus flayed, is laid aside for the present, the skull and jaw bone carefully freed from flesh, and the brains removed according to the other rather more difficult method; the bones of the skull are cut through at the cavities of the eyes, and nothing remains attached to the skin but that part of the skull bones from the eyes to the nose, and the jaws.

As to horned beasts, strip the skin off to the horns, and then, by sharp instruments, break or saw out the horns, taking care to leave some part of the skull bone attached to them. The skin of the other parts of the head is removed in the above described manner, and the skull either cut through at the eye sockets, or the upper part left therein. In the latter case, in replacing the skin, and stuffing, the small pieces of skull bone left sticking to them in removing the horns, are again pressed into the vacancies occasioned by their being cut out.

One other circumstance should not be omitted, which is, that some animals have heads so large that the integument of the neck cannot be stripped over them. There is no other means to obviate this difficulty, but to extend the incision along the back, up to the head. Every thing being finished and properly sewed, the seam in the neck will be as slightly perceptible as that in the back. As to those animals the covering of whose backs does not admit of being cut through, let the incision be made on the abdomen, beginning between the fore legs, and carrying it along to and between the hind legs. The rest of the labour is similar, and differs in no wise, both in flaying and stuffing, from the general mode just described.

Section 8.—*Stuffing.*

After having effectually rubbed or spread over the inside of the skin, the bone joint and ligament, with some antiseptic menstruum, we next place the naked carcase of the animal before us, and model (shape) out of tow in one piece, an artificial head and neck, copying the form, length and thickness, of the original, as nearly as possible, by winding it round with thread or twine, and thrust it into the cavity of the skull bone remaining attached to the skin, where for convenience it may be secured, by passing a bent wire through the skull and the artificial head. The eye sockets then being filled up with fine cut tow, and the muscles that have been removed from the bone of the head replaced by tow, draw back and fill and smooth the skin over the head and neck, as it was before.

Wind tow around the bones of the leg, to give it the shape and size of the muscles that covered them before, and towards the end let the flax be left long: wind it rather loose, so that by the pressure of the fingers, it may afterwards receive the flat shape of the shoulder blade. Both legs being similarly shaped, draw the skin over them, and by smoothing and pressing, make it fit on.

We proceed in the same way with the hind legs, only, as must be evident, the thighs should be more prominent than the shoulder blades of the fore parts. If part of the bone has been retained, the artificial thigh will be more easily completed. But that we may not exceed or fall short in the dimensions, we are to attend closely to the true body, and direct ourselves thereby.

The bones of the tail are supplied by the insertion of a wire, which must be pointed at the end, and stuck into the artificial body. The size of the wire must depend upon the bigness of the animal, and being wound round, of a proper thickness, with tow, is thrust into the skin of the tail, and the skin completely drawn back over it. In smaller animals, for instance the glires, this is a troublesome and hazardous business, which may fail by very slight negligence.*

Next comes the formation of the body itself, which is formed of tow, wound very tight with pack-thread or twine, so that it may resemble, in size and shape, the real body lying before us. This is then laid into the skin, forward, between the artificial shoulder blades, and behind, between the thighs. These are properly pressed and filled, in their natural position; the end of the wire inserted in the tail, is run into the body, and then the whole neatly stitched up.

* In mures, the bone of the tail may be suffered to remain.

In stuffing the larger animals, we may make use of hay or moss with advantage, especially of the kind of moss growing in morasses, and which are known to botanists by the general terms of sphagnum and fontinalis. Moreover, we must remark that the artificial body. and every other part, be not made too large, for should the skin be too much stretched, it might be attended with unpleasant consequences. At the same time, it should not be made too small, a medium is entirely the better way; but it is only by great practice and a good eye, that we can hit the mark. In fact, the stuffing of quadrupeds, is attended with more difficulties than that of other animals, and not evey one will succeed immediately at the first attempt.

Section 9.—*On setting up.*

A specimen thus prepared, lying before one, choose a wire of a suitable size, for instance, if it is a polecat, (iltis,) of the bigness of a strong knitting needle. Five pieces of wire are necessary, which are to be cut off in length, according to the size of the parts into which they are to be thrust. The wire for the throat should reach through the head and neck, into the mid thickness of the body. The leg wire must also reach pretty far into it, and besides, must project from under the soles of the feet, so far as to enable us to secure the animal, by their means, to a board, or the pedestal. Let these wires be sharpened well at one point, then push the neck wire from without through the skull and neck, deep into the body, so far that the upper end may not appear from without. Next let the legs be stretchted out (straightened) beginning first with the hind legs. Pierce through the soles of the feet with the sharpened end of the wire, thrusting it up along the bones of the leg, pretty far into the body. Part of the wire as aforesaid, projects below the soles, for fastening the animal. Proceed with the fore legs in the same way; here also the points of the wire should enter almost into the centre of the body. The legs are then bent into a natural posture, as also the head and neck, and finally the body and tail, according to the attitude in which you wish to place the animal. Holes having been bored into the board, branch of a tree, or other pediment, on which the animal is to be fixed, at proper distances, the projecting wires at the soles are inserted into them firmly, and then by bending, pressing and patting, you give it finally the attitude you think most expedient or becoming. It were certainly desirable, could we always have living creatures to copy after in this particular, but in the absence of these, a lively imagination, connected with a close acquaintance with the habits of animals, must be the guide of the artist.

Good drawings and plates, are also of great service to the beginner; for it is really almost impossible, without such knowledge and other means, to give by guess, or at a venture, to these specimens, a proper and natural position. And upon what else does the beauty of these preparations depend? Let it be ever so carefully and successfully executed, let the operator spare no pains in the performance, still, unless the attitude in which it is exhibited be proper, and true to nature, it can appear to no advantage. Our whole attention is to be applied to this point, and we must grudge no industry or care, not to to fall into this common fault of many, who in other respects are able operators. Having thus given the animal its proper position, the feet and toes, and every thing else, properly placed, we then examine the head once more, and if any deficiency be discovered, remedy it by stuffing up tow wherever wanting, through the openings of the eyes and mouth. Let paper or tow be stuffed into the nostrils, to prevent shrinking in the baking, and this is afterwards to be removed. The mouth and the lips, unless they are intended to appear open, are to be closed up with pins or wire; but all these, together with the wires that are to support the ears, and the pasteboard or cards on which the ears being stretched out with pins, are secured from curling while drying. As to this expedient for keeping the ears in their natural posture, we must be cautious; if it is well applied it is effectual, otherwise of great harm. But, to esteem it superfluous, and not to apply it, would inevitably occasion the ears to shrivel up, so that their shape could not be recognized at all.

Every thing being inspected, and here and there retouched and finished off, let the subject be placed near to a warm stove, in order to dry gradually. When this is effected, and the wires, cards, &c. at the ears, mouth and nose, removed, our labor is over, and the specimen finished.

Although the preparation of small quadrupeds, is an easy branch of the art, yet to prepare the larger and largest, is attended with so many difficulties, that even a practised artist may be at a loss, (versucht) and one of less experience will not easily succeed.

In cabinets, (museums) we generally find the larger specimens but badly, and frequently even miserably executed. This is especially the case with foreign specimens, whose skins are sent us carelessly stripped off, and shrunk together. For stuffing the freshly taken off skins, of larger animals, we proceed to be sure pretty much in the manner just described ; but for them it is necessary, as is self-evident, not only to have larger tools and coarser substances, but also more bodily power, and more time. As on these we cannot form with equal exactness the artificial body in all its parts, as in smaller subjects without much subsequent refilling and retouching, through the various openings in the skin, as well as through the jaws, we cannot expect in them to hit so justly the natural shape. To imitate, in the artificial figure, the strong muscles, tendons, and veins, that show through the skin, for instance in a live horse, requires inexpressible trouble and patience: it may at last be brought about by rags, cords, &c. with great labour ; but for this, there must be a living model or good drawing, or copper-plate to copy after.

To describe every thing appertaining to this subject, is impossible; in the first place, because it would extend this work too far, and then again while all these niceties, or slights of art, depend upon the expertness and ingenuity of the artist himself, and upon place and circumstances. I am convinced that any one who shall carefully follow and practice the above rules, in the smaller subjects, will at length be able to finish large ones. Respecting the management of of the dried skins of foreign animals, it is the same as the treatment of the dried skins of birds, which will be described at large hereafter. Still, we may, as there is no delicate plumage to prevent it, pursue a shorter method for softening them, viz. by steeping the hides immediately in water, and let them soak sometime. Even the hair will not suffer by this, for when it is perfectly dry it may be put to rights by a comb, and light brushing. The longer the hide has been steeped, the easier and better will the stuffing succeed. If, as is generally the case, the skin of the legs has been slit open, let it be neatly sewed in the first place ; as to the rest, proceed as above described, only they are to be stuffed much fuller, (harder than fresh skins,) for this reason, because however perfectly they may have been soaked, these hides never regain their former extension. These are perhaps the most important rules to be observed.

IV.—Preparation of Amphibia.

Section 21.—Tetrapodal amphibia.

The stuffing and preserving of these animals is attended with the fewest difficulties. They are quite as easily skinned and stuffed as any, and are more easily kept than all others. We shall commence with the ranae, frogs and toads.

In preparing toads, we should be very cautious to guard against the corrosive secretion, which may well be termed poison, that exudes from the skin, as it often produces blisters and sores on the skin of some persons; especially we should take care that none of it enters the eyes, for it will, in the first place, occasion a violent and burning pain, and after wards perhaps even inflammation.

The secretion, even of the common green frog rana, (Esculenta) may bring on similar consequences if it gets into the eye.

In stuffing toads, first let them be strewed with salt; then draw out from the warts on their backs this milky juice, which may then be easily wiped off. Moreover, we may, in holding them by the back, make use of an old glove or napkin.

In order to skin them, we must first attempt to *stun* them by repeated blows on the head, for so strong is the vital principle in amphibious animals, in which particular they exceed all other animals, that they are not very readily killed. Now open the mouth, and cut out the tongue with a small pair of scissors, then squeeze the body till you are able with tweezers or pincers to seize hold of the stomach, and thus draw out all the entrails through the mouth. The body being thus completely emptied, divide with a pair of scissors (which, in order that it may not pierce the skin must have blunt points) the back bone, at the first vertebra of the neck, push the stump towards the orifice of the mouth, and seize fast hold of it with the pincers. Holding it thus tight, you turn the jaws inside out, and begin to strip off the skin. By drawing the back bone out gradually, and helping with the other hand, the fore legs to the last joint of the toes will soon be stripped; this joint, to which are attached the nails or tubercles, remains fixed to the skin and separate from the other toe joints. We then go on with stripping down to the anus, which is divided with the scissors, taking care not to cut too near the orifice, which might occasion a hole that would give trouble in filling.

The hind legs are then to be stripped down to the toes, the last joint of these being also left on the skin. The stripping is performed without exertion, and it is not easily possible to damage or tear so tough a skin. To convince yourself now, that life is not even yet extinct in the carcase thus beheaded, flayed, exenterated, and shockingly mangled, it is only requisite to strew over it a little salt, and you will be astonished at the leaps it is still capable of making.

With but little trouble, the eyes and the brains are taken out of the head from without, the skull and under jaw remain in the skin which is turned in, and this may be rendered easier by blowing air into it through the mouth repeatedly. Then comes the stuffing, or more properly the *filling*. This is done with fine dry sand, termed writing or silver sand, which is poured through the mouth, and assisted by pressing and turning externally, and by repeated blowing through the mouth, until it enters down to the ends of the toes, so that the skin becomes quite plump (straff.) As it sometime happens that some moisture still remaining in the skin wets the sand, and prevents it from running into and filling properly the extremities, you may give it air, or push it down with a blunt wire or knitting needle through the mouth, until every limb, as well as the body, is filled out as fully as it was before the flaying. But that the sand may not run out at the mouth, let the sand here be moistened a little, and the mouth either neatly sewed up or glued; finally, you wash the outside of the skin clean from all filth and sand, with pure water. If this be neglected, the sand that sticks upon the surface could not, after drying, be removed without injuring the whole, for a viscous fluid, which spreads over the whole skin of the frog, resembles glue and dries very hard. Then, in order to give the stuffed frog its intended natural attitude, take a little block or board, fix the hind legs first in a natural position, then give the body under the breast just behind the fore legs, a support by a lump of rags or soft paper, squeezed together, and then fix the fore legs properly. The head will stand erect without support, but the flanks (lower belly abdomen) should be pressed by the fingers until they receive the proper shape of the frog's body.

We should endeavour, with a blunt wire, to pack the sand very close, especially round the place where the skull, left in the skin, terminates, as it is very apt to contract a hollowin this part, which looks very unnatural. In the cavities of the eyes we are to place little paper pellets; and the toes must be stretched out, and held so by pins stuck into the board. Every thing being thus settled in a natural posture, it is to be placed in the sun, or in a warm stove, to be properly dried. If we intend that the attempt should succeed fully, it is necessary to have a living animal of the same species, to copy the attitude and to produce the same hollows, projections, swells, &c. in the filled skin, by pressing, squeezing and pinching, as they appear in the living specimen.

It being perfectly dried, the creature is loosened from the board, the mouth opened, and the sand all emptied out. The paper balls are taken out of the eye sockets, and the artificial eyes, (sec. 4) fastened in with glue, gum arabic, or thick lac varnish. The skin, in order to give it the natural lustre, is also to be covered with a light varnish. But, as in many of those animals that show brilliant colours, these are apt to fade, or grow dull and change, these are to be restored by painting with water colours, and then the whole covered over with a light *Bernstein* varnish, or the above described *spirit lac.*

Frog-skins, thus prepared, need no other stuffing; they may then be placed in glass closets, where, secure from clumsy handling and dust, they will not readily be attacked by hostile insects.

If you fix them with gum arabic, on little stands decorated with moss, they will show very handsomely, and will keep unchanged for many years.

The larvæ of the frogs (tadpoles) may also be stuffed in this manner, only the greatest precaution is necessary, that the tender skin of these perishable creatures be not torn. Having once succeeded in stripping it off, there are no peculiar difficulties in the filling; and in this way we may have the frogs in our cabinets in all their various metamorphoses. However, as easy as it is to prepare the frog in its perfect state, by this process, just as great, on the contrary, is the difficulty in managing them in their imperfect state. By this the patience of the operator may be put to the test, and he who expects to succeed in all his attempts, must be an adept in his art.

All the species of lizard, except the very large ones, are treated in the same manner as frogs and toads, only the freeing of the tail is more difficult, it being very tender and apt to tear. In those that have a fleshy crest, it requires some support, until thoroughly dried, otherwise it would lose its shape and shrink together. This support is given by a slip of stiff paper, upon which the crest is stretched out, and glued with weak gum-water; or the gum may be dispensed with, as the skin is for the most part provided with a glutinous matter, by which the slip of paper will be held fast, merely by first wetting the skin. When the animal is dried sufficiently, take off the papers and throw them away. The crest of the little water salamander (lacerta taeniata) is so tender, that, in order to stretch it out fully, it must be moistened and softened with water.

Lizards (lacertæ) of the largest size, such as the crocodile, alligator and cayman, could (I should think) hardly be prepared in this way. They must be managed pretty much as the quadrupeds, (sec. 7 and 8,) be cut open along the belly; and, in fixing them up, in order to give the limbs sufficient support, make use of strong iron wire, or sticks of wood, &c.

As I have never been so fortunate as to have a chance of stuffing one of these gigantic amphibia, I cannot, from experience, recommend any particular method; nevertheless, if one should luckily fall into my hands, I should treat it just as I have now advised, and a dexterous operator, who has practised the stuffing of indigenous animals, can never be at a loss in managing even these huge monsters.

The treatment of tortoises, on account of their natural coat of mail, is more difficult than that of any other amphibious animal. That such is the fact, the great number of such specimens in most cabinets, unfortunately proves. They are generally miserably stuffed, or even merely dried—that is, baked with the flesh—a much worse mode than the most imperfect stuffing. But, notwithstanding the preparation of them is attended with many and great difficulties, yet an experienced stuffer will still overcome them. As the chief difficulty lies in the opening and dissecting, but not in filling, we must attend first to the examination of the subject, to determine to which of the three known families it belongs, as in respect to bodily conformation, especially in the junction of the two shields, they differ widely, and must therefore be managed very differently.

The sea turtle, which are readily distinguishable from the others by their broad fin-like feet, are easiest managed; for this reason, not only because their limbs are larger, and on this account, together with their head and neck, cannot be entirely drawn into the shell, but because the shields (the upper and lower) are united by a membrane, easily separated. You make an incision in the middle of the soft skin of the belly, then where the shield ends, carry it along the same to one of the sides, separate the connecting membrane, then continue the cut at some little distance from the edge of the shield, to the bottom of the neck; endeavour to separate the bones that are attached within, to the breast plate, and draw it open as far as it will stretch, in order to take out the entrails. In the annexed plate, fig. 1, this incision is designated by the line *a a*; now separate from within, but without injuring the skin, the neck from the body; strip the skin off from it up to the head, separate it here too; then draw out the brains, by making an opening at the point where the vertebrae of the neck were interlocked.

We next are to separate from within, the bones of the fore fins or legs from the other bones that are fixed to the breast plate, so that the skin of the limbs down to the toes may be stripped. As at the neck, so down to the very extremities; nothing must be left in the shell; even the muscular parts must be entirely removed. The hind legs and the tail being loosened from the upper shell, to which they are grown, without injuring the skin, are also to be stripped to their extremities.

But he who has never seen a tortoise dissected, in doing the above, will hit upon many peculiarities in the structure of the internal parts, that may easily make a novice in such labor start. On this account, I advise none but the more experienced to engage in this branch of stuffing. For, if I would make myself as intelligible to the former, as I could wish, I should be obliged to preface the mode of preparation, with a full anatomical description, and this would be too prolix an undertaking for this little treatise. If any wish to obtain information on these points, I would direct them to Schneider's Natural History of Tortoises, in which every necessary branch is treated at full length. Further, he who has had considerable practice in preparing other kinds of animals, will readily discover a way to overcome any difficulties he may encounter in this department.

All flesh and fat being entirely removed from within, the skin is to be rubbed on the inner side, with some dry preservative, whether lime or ashes, and then the skin of the limbs is to be turned right side out. Now commences the stuffing; first the head and neck, then the fore legs or fins, and the hind legs and tail. For this take tow, cut up fine, which is shoved by a stick little by little, into the part, and by pressing and turning from without, as well as by assistance of the stick within, try to give each its natural shape again. Then let the body be filled up with tow properly, and sew up the opening through which the stuffing has been performed, neatly. If it is intended to give the animal a standing or creeping attitude, wires must be introduced into the legs, as taught in the stuffing of quadrupeds; also, to prevent shrinking, let the fin-like feet be stretched out properly. The animal being then sufficiently dried in the oven, let it be daubed over with lac or varnish of colophony once or twice, and then it may be placed or hung up free, or in a glass closet, so that the shells are either in a horizontal or vertical position, in which case the wire may be but weak. I have seen tortoises with two incisions through which the stripping and removing of the internal parts had been performed, viz: one began at the toes of one of the four feet, ran along its upper side, up the leg, over the neck to the toes of the other leg. A second cut ran across the hind legs and tail close to the back shell, and parallel with its hinder edge, and by this opening, the remaining entrails and the tail were drawn out. The shells in this way retained their connexion, and the whole animal was drawn by two openings. But it is certainly more difficult to fix the limbs of animals, thus treated, in their natural posture, and to conceal by dexterous sewing, this long incision, than in the manner first mentioned, which I prefer by far.

The second division of turtles, the river tortoises, are characterized chiefly by having feet adapted for swimming, that is, their feet have real toes armed with nails, claws, and joined by webs. In them the two shells are joined by a strong membrane, and besides, are strengthened by two hinges, (angular,) which may, however, be pretty readily divided. They are, therefore, treated like the sea turtles. In the plate, fig. 2 represents one of them, the place for the incision being designated by the line *a a*.

The third division, land tortoises, are managed with the greatest difficulty. They differ from the others in having thick club-shaped feet, armed with long nails, and also in having the upper hollow shell connected with the lower by complete ossified seams. This close junction can be divided by a saw alone, and could the cut afterwards be completely concealed, which, I doubt not, may be effected, for instance, by strong cement, this species might be prepared in the same manner as the former, and I should, without doubt, have made the attempt, had I not, unfortunately, failed in procuring a sufficient number of specimens. Since, however, this cannot be remedied, I must be satisfied with describing the methods as related to me by others, and comparing them with the observations I have made in such specimens as I have examined in cabinets. Accordingly, they are to be divested of their covering by two openings, as mentioned above in speaking of the sea turtles. Yet I would not recommend the anterior cut to be made above, but upon the lower side, as is shown by the dotted line *b b* in the plate, fig. 1, and of a length but just sufficient to admit of the neck and fore legs being separated and drawn out from within. The posterior incision I should also make not back of the hind feet, but directly back of the lower shield, across the belly, (see the dotted line *c c* in the plate, fig. 1) and but barely large enough to permit of such of the entrails being removed as could not be got at through the first cut, and of the freeing and stripping out of the hind legs and tail. To draw out the entrails and the muscular parts adhering to the shields, we may make use of small sharp hooks of wire. There is nothing difficult as to the stuffing, which must be done in the manner before directed, in treating of the first species of testudo. The smaller kinds might also be filled with sand, in doing which, however, to prevent its running out, the mouth is to be previously glued together; for the same reason the sand should be made damp at the seam. Perhaps a better way would be to sew up the openings, and then to pour in the sand through the mouth. I am fully convinced that this is the better way, for in stuffing with tow or cotton, one must be greatly on his guard not to stuff either too loosely or too firmly, and that the material used is not forced into clumps, which can only be prevented by frequently, during the operation, loosening it with a wire, and by not stuffing in too large a parcel at once.

It is unfortunate that, through the very great tenacity of the skin in amphibia, every little defect is so very evident, defects that cannot always be avoided with all possible expertness; whereas, in animals covered with hair or feathers, greater faults are so completely concealed by these coverings that they are not even suspected. This fact greatly enhances the difficulty of preparing amphibia, and on this account is sand so much to be preferred as the agent in filling, for it runs equally into every crook and cranny, without leaving void spaces or distending the skin too much. If you wish to be convinced of this by ocular demonstration, try the experiment, and stuff two frogs, one with sand and the other with tow or cotton. All seams are to be avoided as much as possible; they disfigure the specimen very much, and when they cannot be avoided, as in tortoises, let the incision be made no larger than is actually necessary, and then let it be sewed up with all possible care and neatness.

The eggs, also, of the larger amphibia, may be easily preserved, such as the eggs of tortoises. These, it is well known, are not covered like the eggs of birds with a hard calcareous shell, but with an elastic parchment-like skin, which would shrivel up if dried empty.

If you wish to preserve such eggs in your cabinet, pierce a small hole at one end, press out all the contents through this opening quite clean, insert the barrel of a quill and blow it out round, then, with the assistance of a small funnel, pour in fine sand, and by filling and blowing alternately you will succeed in filling them completely. Then, after being quite dried by the heat of a stove, let the sand run out and the work is done.

Section 22.—Of Snakes.

The reptile amphibia, or snakes, are more easily prepared than any other. In general they are treated in the same manner as frogs and lizards. But since much caution is necessary in meddling with the poisonous kinds, as the teeth may occasion serious injury, even after the animal has been long dead, and since there are some species whose bodies are too thick towards the middle to be drawn out through the jaws, we are either to enlarge the opening of the jaws towards the ears, and afterwards to sew it up neatly, or we may pursue the following plan.

The animal being dead, make a very small incision on the belly, in the middle, where the body is thickest, about a finger's length, and then endeavor, by the assistance of a flat handled knife, to peal off the skin from both sides, and entirely round the body, for the whole length of the cut. Next divide the carcase with shears, or in the larger kinds with a knife, being careful not to injure the skin, then tie a string around the upper portion of the carcase, and strip the skin down, drawing the body through the opening down to the skull, from which separate and throw it aside. Then we are to take out the tongue, brains, and eyes.

We proceed in a similar manner with the other part of the body, only care is to be taken when you come to the anus, lest, by cutting too close, a hole be occasioned through which the same might afterwards escape. Should this unluckily happen, let it be carefully stitched up before filling. Towards the tail, also, much caution is necessary in stripping, as it tears very easily at that part, (the skin.)

The disengaged skin then being turned, let the incision on the belly be neatly sewed up, and then we proceed to the stuffing or filling. This is done in the same way as in frogs, that is to say, you pour very fine writing sand through the mouth into the body, until it is quite filled, which is performed with greater ease in these subjects than in frogs, as they have no limbs. Should the gullet be rather narrow, so that the sand does not pass through easily from sticking to the moist parts, we may save some trouble by the use of a small funnel, through which the sand may be poured into the body. The mouth may be closed at pleasure in the manner above directed, or it may be left open if the fangs are to be exhibited, in which case it is expedient to stuff the jaws loosely with tow until the drying is finished, and also give the specimen its proper attitude, as directed in former sections. It is left to the taste of each practitioner to give it an erect attitude, or to show it in a coil, gliding along the ground, or winding around a tree or branch, &c.

Finally, when the specimen has been thoroughly dried in an oven, or in the sun, you empty out the sand through the mouth, then fix in the artificial eyes, and give it a coat of varnish. Should any of its tints have faded, or been discharged, they should be renewed by water colours before laying on the varnish.

Being then set up in glass cases, or in cabinets with glass doors, where they are protected from dust, they will keep a great while unaltered, being secure from moths, and other hostile insects, which are kept off by the varnish.

Not only all kinds of snakes, from the largest down to the most minute, are to be prepared in this way for collections, but several species of fish, such as eels, and other snake-like kinds, may be thus treated.

The old method of preserving snakes in spirits is too expensive, and requires too much attention, to be recommended, and stuffing is preferable to it in every respect. Such subjects as have been in spirits for a length of time may still be thus treated, only in this case, they, as well as other animals that have been in spirits any time, are not stripped with as great ease as more recent specimens. The skins of all amphibia, procured in travelling in foreign parts, which would take too much time to be immediately stuffed, and too much space in packing, may be dried inside out and then packed up together. The skins of snakes may be easily rolled, but those of frogs and lizards should be pressed flat. When you intend to fill them, steep them first in water, throwing it off and pouring fresh on them from time to time to guard against putrescence.

As soon as they have become perfectly pliable, you may fill them without trouble, and they will look as well as those that have been fresh skinned.

Art. XI.—*A method of detecting minute quantities of Opium, in solution;* by Robert Hare, M. D. &c.

Through the discoveries of Sertuerner, it is now well known, that opium contains an alkaline substance, called morphia, to which it owes its efficacy in promoting sleep, and relieving pain: also, that this alkali is naturally in union with an acid called meconic, which produces a striking red colour, with solutions of red oxyd of iron. Nevertheless, this property has not been proposed as a means of detecting opium; which has probably arisen from the circumstance that the meconate of iron does not precipitate. I have, however, contrived a method by which a quantity of opium, not exceeding that contained in ten drops of laudanum, may be detected in a half gallon of water.

My process is founded on the property which meconic acid has of precipitating with lead. Hence, by adding a few drops of acetate of lead to any infusion, containing any quantity of the drug in question, not more minute than the proportion above mentioned, an observable quantity of the meconate of lead falls down. The precipitation, where the quantity is small, may require from six to twelve hours, and may be facilitated by a very gentle stirring with a glass rod, to detach the flocks from the sides of the recipient, which should be conical, so as to concentrate them during their descent.

The meconate being thus collected at the bottom of the vessel, let about thirty drops of sulphuric acid be poured down on it by means of a glass tube. Let this be followed by as much of the red sulphate of iron. The sulphuric acid liberates the meconic acid, and thus enables it to produce, with the iron, the appropriate colour which demonstrates the presence of that acid, and consequently of opium.

ART. XII.—*Method of preparing Denarcotised Laudanum;* by ROBERT HARE, M. D. &c.

AGREEABLY to the observations of the French chemists and physicians, the unpleasant effects of opium reside in a principle called narcotine, and ROBIQUET has informed us, that by digestion in ether, the drug may be depurated of that noxious principle. It struck me, as soon as I became acquainted with the statement of Robiquet, that it was of the utmost importance to humanity to have it tested, and the result made known to my countrymen, if favourable.

Some opium, shaved by rubbing it on the face of a jack-plane, was subjected, four times successively, to as much ether, of the specific gravity of .735, as would cover it, allowing each portion to act upon it for about twenty-four hours.

The opium was afterwards subjected to as much duly diluted alcohol as would have been adequate to convert it into laudanum of the common kind, had it not been subjected to the ether. In the ether which had been digested on the opium, a deposition of crystalline matter soon commenced. The stopple being removed, and the mouth of the containing vessel, (in this case, a common French tincture bottle,) being covered with blotting paper, in a few days nearly the whole of the liquid evaporated spontaneously, leaving much crystalline matter mixed with colouring matter. The former is, no doubt, the principle distinguished by Robiquet, since called narcotine.

The digestion of the opium with the ether, is conveniently performed in the Papins digesters, which are sold at some of the hardware stores in Philadelphia.

The ether should be kept near the temperature of ebullition.

The first use which was made of the denarcotised laudanum, was by way of an enema of thirty drops, in the case of a child tortured by ascarides, to whom it gave early relief, inducing a comfortable, and apparently natural sleep, and causing subsequently no unpleasant symptoms.

The second instance was a case of severe head-ach, which was relieved in about thirty minutes, by ten drops taken into the stomach. A refreshing slumber succeeded, which was not followed by any of the distressing sensations to which the patient has always been subjected after taking common laudanum.

I subjoin the results obtained, with the denarcotised laudanum, by a veteran in the healing art.

"Dear Doctor,—When you presented me with some laudanum, prepared from opium deprived of its narcotine, you wished me to inform you in what it differed in effect from laudanum prepared in the usual way. I have tried it in but four cases; all of which, however, were fair ones for its employment, as each had constantly experienced the most distressing effects from opium in every way in which it had been exhibited. I will relate them in order.

"Case I. Was that of a lady who was suffering severely from a chronic affection of the uterus. In her case, opium, in some form or other, was absolutely necessary; and every form commonly known was resorted to with a view of diminishing its terrible after-effects upon her stomach and head, but without success.

"I recommended the denarcotised tincture of opium to her in a dose similar to that she had been in the habit of using of the common landanum, &c. The first two or three doses were followed by the common after-feelings, owing, most probably, to the impression of the former forms in which she used the opium not having entirely ceased; for soon after, and to the present time, a period of two weeks, she experiences the most decided relief from pain, without the slightest inconvenience following its use.

"Case II. Is one where severe after-pains followed delivery, and in which every other remedy almost, save opium, was tried without success for their relief. In this case, opium in no shape whatever could be given internally, or even employed externally, without the severest sufferings following.

"The denarcotised laudanum was given with the most entire success, and without the slightest inconvenience following its exhibition. The lady called it the 'divine tincture of opium.'

"CASES III, and IV, Were the ordinary cases of opium disagreeing in any form; the exhibition of the tincture in question, in neither case, was followed by any unpleasant feeling.

"From this experience, though limited, I am led to anticipate the great desideratum in the use of opium is obtained. With many thanks for your liberal supply of the article for my trials,

I remain, as ever, yours,

March 25th, 1827. W. P. DEWEES."

An easy mode of obtaining Meconic Acid. By R. HARE, M. D. &c. &c. &c.—If to an aqueous infusion of opium we add sub-acetate of lead, a copious precipitation of meconate of lead ensues. This being collected by a filter, and exposed to sulphuretted hydrogen, meconic acid is liberated. The solution is of a reddish amber colour, and furnishes, by evaporation, crystals of the same hue. A very small quantity produces a very striking effect in reddening solutions of peroxide of iron.

Instead of sulphuretted hydrogen, sulphuric acid may be used to liberate the meconic acid. The presence of the former, in excess, does not seem to interfere with the power of reddening ferruginons solutions. But any excess of sulphuric acid may be removed by whiting, which is not acted upon sensibly by the meconic acid. Yet the acid procured in this way, did not crystallize so handsomely, or with so much facility, as that obtained by sulphuretted hydrogen.

ART. XIII.—*General Views of the formation of Phosphuretted Hydrogen;* by LEWIS C. BECK, M. D. Professor of Chemistry, &c. in the Vermont Academy of Medicine.

IN the few observations which I shall make upon the interesting compound, forming the subject of this article, I shall carefully examine the phenomena which attend its formation, and then deduce from them some general principles, which, although they may not be new, have not occurred to me in the course of my reading.

Phosphuretted hydrogen, or the hydroguret of phosphorus, is a peculiar gaseous compound, consisting of one proportional of phosphorus, and one proportional of hydrogen. It inflames spontaneously upon the contact of the atmosphere, burning with a bright flash.

For the production of this gas, it appears to be necessary that phosphorus should be presented to nascent hydrogen. The hydrogen is almost universally obtained from the decomposition of water, although analogy would induce us to believe that it might also be derived from that of sugar, and of other vegetable products.

The elements of water have a powerful attraction for each other; we are acquainted with but few substances which can effect their separation. The splendid discoveries of Sir Humphrey Davy, made us acquainted with a class of bodies which possess this property in an eminent degree.—I mean the metallic bases of the alkalies and alkaline earths, which decompose water with great rapidity, uniting with its oxygen and liberating its hydrogen in the form of gas. These bodies, therefore, are admirably calculated for the present purpose. Some of these, moreover, unite with phosphorus, forming definite compounds, called phosphurets. And it is important to state, that these phosphurets rapidly decompose water, and evolve phosphuretted hydrogen. Such is the fact with regard to the phosphurets of potassium, sodium, calcium, &c. formed by heating these metals with phosphorus out of contact of air.

Let us now examine the circumstances which attend the more common methods of obtaining this gas.

1. One of the processes adopted in the laboratory, is to take phosphuret of lime, as it is commonly called, or phosphuret of *calcium* more properly, and to throw it into warm

water; bubbles arise and inflame upon contact of air, and they are phosphuretted hydrogen. Now in this case the explanation is very easy. Phosphorus cannot decompose water, and it must therefore be combined with a substance which has the power of doing this. *Lime* has not; calcium has. Whatever of phosphuretted hydrogen, therefore, is produced in this way, must be the result of the action of phosphuret of calcium, and not of lime.

2. Another method is to put some pieces of phosphorus into a retort filled with a solution of caustic potash, and to apply heat. In this case, the addition of heat enables the phosphorus to decompose the potash; a portion of it probably unites with the oxygen of the alkali, forming an acid, and another portion with its metallic base, forming a phosphuret of potassium. This last decomposing the water, furnishes the phosphuretted hydrogen.

3. Instead of caustic potash, the carbonate of potash and quicklime are sometimes employed in combination with phosphorus, with the same results; except that the evolution of phosphuretted hydrogen is not so rapid as in the former cases. The rationale is, in effect, the same as before. The carbonic acid of the carbonate of potash unites with the lime, and then the potash is acted upon by the phosphorus in the manner above explained.

If the above explanations be correct, we infer that for the production of phosphuretted hydrogen, it is only necessary to employ phosphorus and some metal, which metal, *per se*, possesses the property of decomposing water.

We can, therefore, obtain this gas.

1. From the phosphurets of ALL the alkaline metals; as potassium, sodium, calcium, &c.—by the mere addition of water.

2. From a combination of phosphorus and the oxides of these metals; as potash, soda, lime, &c. by the application of a sufficient heat to enable the phosphorus to effect a decomposition of these oxides.

3. From phosphorus and those metals which are usually employed in procuring hydrogen gas, as iron, zinc, &c. But as these metals do not decompose water with much rapidity, without the presence of some dilute acid, this must be added in the present case, and the process conducted in all respects as when we wish to obtain hydrogen alone, by means of these metals.—The use of the acid in both cases is the same.

Such are the general views which I have deduced concerning the formation of phosphuretted hydrogen; which I believe the most minute examination will prove to be founded in truth. L. C. Beck.

Art. XIV.—*Appendix to Caricography—Vol. XI. p.* 325. By Prof. C. Dewey.

(Communicated to the Lyceum of Nat. Hist. of the Berk. Med. Inst.)

1. C. *filifolia*, Nuttall.
Vol. XI. p. 150.
Vol. XII. Tab. P. fig. 50.

Through the politeness of Mr. Nuttall I have received a specimen of this species, since the description was printed in Vol. XI. and from which the accompanying figure is taken.

Spike simple, few-fruited, staminate flowers at the summit; staminate scale oblong, obtuse, white and membranous on the edges, tawny on the keel; fruit ovate, subglobose, fully convex above, and nearly flat on the lower surface, very slightly pubescent in maturity, slightly rostrate or pointed; pistillate scale broad-ovate, white and membranous on the edges, tawny on the back and keel, somewhat retuse in the mature state of the fruit,—lower ones distinctly ovate and somewhat acute.

2. C. *Washingtoniana*, Dewey.
Vol. X. p. 272. Tab. D. fig. 14.
C. *nigra*, Schw. and Torrey.

Since the description and figure of this species were published in this Journal, I have had opportunity to compare it with the C. *nigra*, All. from Europe, and find them, as I had stated, exceedingly different from each other. The C. *nigra* is closely related to C. *atrata* and C. *bicolor*, and the three are considered by distinguished botanists to be only varieties of the same species, viz. C. *atrata*, although Willdenow, and some others, maintain their distinctness. The difference from C. *Washingtoniana* is too palpable to require a remark. On the C. *nigra* the spikes are aggregated, the lower one sub-pedunculate, with a longer bract;—fruit ovate, apiculate, longer than the ovate, or nearly elliptic, black scale; upper spike wholly staminate, or only staminate below. On C. *atrata*, the scale is longer than the fruit, and ovate-lanceolate—the spikes larger, less clustered, pedunculate, and often cernuous. On C. *Washingtoniana*, the upper spike is staminate, sometimes the next with a few staminate florets at the apex,—the lower spikes long, cylindric, subsessile, subremote, erect, and sparsely fruited. This species is very different from C. *saxatilis*, which has *two* stigmas, while this has *three*, and is too different to be confounded with any of those now mentioned, except by those who have not the means of comparing them.

3. C. *Barrattii*, Schw. and Torrey.
Vol. XI. p. 162, and Vol. XII. Tab. P. fig. 51.

The accompanying figure of this species was politely furnished by Dr. Torrey. It will serve to make botanists better acquainted with this new and interesting species. The pistillate spikes vary from two to three. The obtuseness and even roundness of the staminate scale, and the peculiar form of the pistillate scale, are very characteristic.

4. C. *cespitosa*.
Vol. X. p. 266.
Var. *ramosa*. Vol. XII. Tab. P. fig. 52.

The peculiarity of this variety is, that one, two, or three small spikes branch from the bottom of the lowest spike — These branched spikelets have their fruit and scales like the others.

Note. Although the characters of the European plant are found on all the varieties of this species in our country, yet our plant grows to a greater size, often two feet in height, and the spikes are much longer, though they are not much larger. It is common along our streams, in large *cespitose* masses, and it seems to occur generally in this way in Europe. But in Essex county, Mass. a small variety is found in moist soil, which is not cespitose, and which closely resembles specimens from Europe. The pistillate spikes are short and thick, and much like one of the figures of this species by Schk.

Art. XV.—*Porcelain Clay?*

TO THE EDITOR.

Agreeably to your request, I send you some account of the bed of clay, of which I forwarded you specimens some time since. I am not able to ascertain certainly that it is *porcelain clay*. A box of it has been sent to the porcelain manufactory, near New-York, but no return has been made in relation to it. The account may, however, be useful to some who are interested in the manufacture of this beautiful ware.

This clay is very white, and fine like flour—melts* with ease before the blow-pipe—readily forms a paste with water—used like lime, for a wash or paint, adheres strongly to wood, and gives it a fine white—so tenacious as a thick paste, as to be employed with some success as a substitute for the common *putty*.

The bed is situated in Pownal, Vermont, on the land of Mr. Gypson, about five or six miles directly north from this College. It is in a valley, having Pownal mountain on the east and south, and is covered with a light and sandy soil, which extends to some distance about it. The principal stones about the place are masses of *rolled* quartz. Small masses of quartz, of irregular shape, are imbedded in the clay, with some pieces of feldspar, which fall to powder on exposure to the air. The whole forms a very compact and dense mass, which can be dug up only by the pick-axe. The bed has been struck only in two places, about eight rods apart, in attempts to sink wells. At the eastern place the bed was found only three or four feet below the surface; at the western, about eight feet, and was penetrated ten or twelve feet. As there seemed to be no prospect of finding water in this clay, the digging was stopped. This pit or well was about twenty feet in diameter. These facts prove that the *bed is extensive*. While the quartz on the surface appears in

* This easy fusibility will of course prevent this substance from being used as a *porcelain* clay, unless it should be corrected by the addition of siliceous matter, which would, in all probability, answer the purpose, as porcelain clay *should relent* a little in the fire, that it may acquire the same vitreous fracture and delicate translucence which distinguish porcelain from common earthen ware; but any considerable softening would destroy the symmetry of the vessels.—Ed.

rolled masses, the quartz in the bed presents no appearance of attrition. At the distance of a mile perhaps, at the west of the bed, and on a rise of land, the surface is literally covered with rolled quartz, and, where the plough has been used, is found in quantity beneath the surface. Among the quartz in this place I found a few pieces of feldspar, exhibiting a tendency to disintegration. This fact may render more plausible the supposition that this bed of clay has originated from the disintegration of feldspar, the quartz connected with it in the original granitic aggregate, still remaining, and unaffected by those causes which have reduced the feldspar to powder.

C. DEWEY.

Williams College, April 9, 1827.

ART. XVI.—*Suggestions as to the refracting power of the higher regions of the Atmosphere.*

TO THE EDITOR.

THE sight of a very large and bright halo, marked with prismatic colours, about the moon, on an evening in the early part of last month, recalled to my mind the conjectures contained in the last number of your Journal, with respect to these appearances. Since nothing better than hypothesis has been obtained, perhaps by exhausting possibilities, we may aid in eliciting the truth : with this view, I send you the following.

"Nous avons dit," says Brisson, page 76, Tome 2, Traité Elementaire de Physique, "que le gas hydrogéne s'exhale des mines, des eaux borbeuses, des marais, des latrines, des cimetières, etc. Il est aisé de concevoir qúil est la matiere des feux follets, qú on voit au-dessus de ces endroits.

"La légéreté lui permet de s'elever assez haut dans l'atmosphere ; et, comme il peut s'enflammer par une étincelle électrique, il est probable qu'il s'enflamme ainsi souvent dans les orages, et qu'il augmente alors la detonation du tonnerre. Voilà sans doute pourquoi le tonnerre est plus fréquent, et plus fort dans certains lieux. Quand ce gas detonne, ainsi, il brule ; alors sa base oul'hydrogène se combinant avec l'oxigene de l'air, forme de l'eau qui tombe en pluie. En effet, dans les orages il y a souvent des pluies violentes et subites, apres quelques coups de tonnerre."

Now, Sir, if large quantities of hydrogen occasionally occupy the upper regions of the atmosphere, since its refracting power is more than six times that of atmospheric air, (6 61436, Biot, Traite de Physique, Tome, 3 page 308,) the rays of light must be decomposed on passing into this highly refracting medium; and thus, I think, the appearances may be explained.

I acknowledge that the facts with which we are acquainted, relative to the mixture of gases, are not easily reconciled with the remarks of Brisson: but the experiments of Dalton and Berthollet on this subject, were made under the common atmospheric pressure; at least, in the account which I have seen, the contrary is not stated; and the gases were confined in contact with each other. It would be very interesting to perform the same experiments under different pressures; and even in as perfect a vacuum as we can obtain. Even granting that extremely small quantities of gas would be diffused uniformly throughout the largest vacuum that we could produce, I deem it very probable, that if small portions of two gases, contained in large exhausted receivers, were made to communicate with each other, the mixture would be much less rapid than when under the pressure of the atmosphere. Certainly the mutual repulsion of the particles, which causes aeriform fluids to diffuse themselves uniformly when external pressure is removed, must have some limit. We cannot suppose that it will exist between the particles at whatever distance they be removed from each other; but only until the repelling power, which probably exerts itself at comparatively inconsiderable distances, is balanced by the universal attraction which governs all matter. I do not know of any facts which would authorize us to make the same assertion respecting the electric fluid; (which, if there be any, seems most likely to form an exception,) yet the analogy of nature's works would lead us, even here, to the same conclusion. The levity of hydrogen would probably cause it to ascend with great rapidity, till it arrived at a region where the density of the air was equal to its own. It is possible that in such circumstances, the mixture might not take place with the same facility, as at the surface of the earth, and perhaps not at all. I fear, however, that my theory is lame: for if Mr. Dalton's hypothesis be true, the gas could expand itself with entire freedom, although surrounded by air; and therefore would not have a tendency to rise, similar to that of water in mercury, notwithstanding that the ratio of their specific gravities is nearly the same. Still, Mr. Dalton's theory is only an hypothesis; and is not acquiesced in by all chemists of note; although it appears to afford a very plausible explanation of the phenomena observed upon the mixture of gases.

As to the plausibility of my speculations, I wish to obtain the opinion of some person better qualified than myself, to decide upon these matters; and therefore send them to you.

Very respectfully yours, &c.

Oxford, Ohio, Feb. 8, 1827. Z.

ART. XVII.—*Notice of the Belmont anthracite Mines, &c. in Pennsylvania.*

COMMUNICATED TO THE EDITOR.

NEW-HAVEN, Jan. 15, 1827.

THE following facts were some weeks since verbally communicated to me by Mr. Thomas Meredith, of Belmont, Wayne county, Pennsylvania, proprietor of the Belmont coal mines. If they are at all interesting to you they are at your disposal. Yours respectfully,

THOMAS RITTER.

The Belmont mines are on the Lackawana creek, in the county of Susquehannah, seven miles above Carbondale. Carbondale belongs to the Delaware and Hudson Canal Company. The anthracite is found on both sides of the creek, nearly if not quite, upon the top of the mountains. A vein within thirty feet of the top of Moosic mountain, which bounds the coal valley on the east, is four feet thick. The range of mountains rises on the table land between the Delaware and Susquehannah rivers, and extends under various names through the counties of Wayne, Susquehannah, and Luzerne, until it is lost in the highlands of the sources of the Lehigh. Its general course is W. of S. The Belmont mines are the most northerly yet discovered in Pennsylvania. They approach within twenty five miles of the Susquehannah river, nine miles above the Great Bend bridge.

The Legislature of Pennsylvania, at the last session, granted corporate privileges to a company for making a rail-road from these mines to the Susquehannah. This company have the power to hold and work coal lands. It is contemplated to extend this rail-road down the Lackawana creek to its mouth, nine miles above Wilkesbarre.

The coal valley averages, perhaps, three miles in width. From its termination to the southern extremity of the Wyoming valley may be seventy miles. Throughout this whole region anthracite is found. The beds are of various depths, but will average perhaps fifteen feet in thickness.

In removing the shale and micaceous sand-stone, in order to uncover a vein of coal, a nodular clay iron stone was discovered in small beds interspersed among the shale. The nodules were of different sizes, from that of one's fist to that of thirty pounds weight. Mr. Meredith says that he discovered a bed of this iron ore about two feet in diameter in the midst of the shale. The nodules at the circumference and at the centre appeared to be in concentric layers and took the shape of the bed. The interstices, particularly toward the centre, were filled with ochre. A specimen of the ore and of the ochre was presented to Prof. Noyes, who thought it would make excellent paint.

The nodules in this ochre were of all sizes—in some instances very small. They had not the hardness of stones, but consisted of a shell, inclosing a dark blueish substance, sometimes of the consistence of paint, always so soft as to be cut with a knife.

A specimen of the ore above described, was sent, Nov. 1825, to Professor Keating of Philadelphia, who declared it to be genuine clay-iron stone, similar to that which is found in *bituminous* coal-fields in England and America. Prof. Noyes seemed to be of the same opinion, and thought it might contain forty or fifty per cent. of iron. Professor N. presented some to Messrs. Hart and Pond, who own a furnace in Utica. These gentlemen, (Mr. Pond in particular,) have great practical knowledge of the ores of iron ; they called it shell-iron ore, and declared it to be of superior quality for making bar-iron and for castings. They gave a certificate to this effect, and said that three tons of ore would yield two tons of iron.

ART. XVIII.—*Observations on the analogy between the Minerals of the North of Europe and of America, more particularly as connected with the uniformity of their geological situation in both countries*—*by* WILLIAM MEAD, M. D. *Addressed to the Editor.*

IT appears to me extraordinary that as yet little notice has been taken of the great resemblance that can be traced between the minerals of North America, and those which have been found in the North of Europe, particularly in Norway and Sweden. This may arise partly from the want of opportunity of comparing them; but any person who is in possession of a good collection of specimens from those countries, will be at once struck, not only with the close similarity, amounting almost to identity, but will be able to trace the same geological character and geognostic situation throughout the whole series. This similarity is observed more particularly in those specimens which are found to accompany the primitive formation at Arendal in Norway, as well as in the same class of rocks in this country. It is not confined however to the primitive range of mountains alone, as the same resemblance can be frequently traced, when we compare a number of our minerals with those of Piedmont and even of the Hartz mountains.

It would be entering at present on too extensive a field to enumerate all the minerals in which this exact resemblance may be noticed; I shall however briefly take a sketch of those to which my attention has lately been attracted; at the same time, I may observe that this subject has not entirely escaped the notice of some foreign mineralogists, whose attention has been called not only by the exact similitude of the substances, but by the more satisfactory proofs of their identity, drawn from chemical analysis.

In a letter which I have lately received from Monsieur Von Struve, a distinguished German mineralogist, with whom I have long had the honor to correspond, he makes use of the following observations.

"On examining some specimens which you have lately favored me with from North America, I have been struck with the resemblance between some of them, and the minerals which we frequently obtain from the North of Europe; for instance, I observe an apple green mineral, accompanying the condrodite* from Warwick, which I find to be a beautiful variety of pargasite, such as is found at Pargas, in Finland, where it is also accompanied with condrodite, rhomb spar, and mica ; and it is also remarkable that all these substances are found at Parga, as in America, in the same geognostic situation, and accompanied with the same minerals. The metallic substance which is found associated with spinelle in massive condrodite at Warwick, is undoubtedly titan. fer. oxidé, a mineral not unfrequently found under the same circumstances at Arendal, in Norway." The same mineralogist observes, " that, as to your Nuttallite, it seems to me to be extremely like a variety of compact scapolite, its crystalization and principal external characters being exactly like the same species of scapolite which we find in Norway."

It may now be permitted me to make a few observations in explanation of the above remarks of Mr. Von Struve. The apple green substance imbedded in carbonate of lime with Brucite, found near Warwick, had been previously noticed here, and was suspected to be a rare mineral, called pyrallolite, found also in Finland ; but its real character remained undetermined, till I received the above account, and on referring to a specimen of pargasite from Finland, in my cabinets, I find it perfectly similar, showing how useful it is to have on such occasions, an extensive variety of specimens to refer to.

* As it appears that some mistake has arisen with respect to this mineral called condrodite by the German Mineralogists, it may not be improper here to state, that it is the same mineral which was discovered many years ago by the late Doctor Bruce, at Sparta, in New-Jersey, at which period it was examined by Doctors Torrey and Langstaff, of New-York, who discovered that it contained a portion of fluoric acid, and also that it differed in many respects from other known minerals. The name of Brucite was therefore very appropriately given to it. Some years subsequent, a mineral, precisely similar in external character, which was called condrodite, was received here from Sweden, and as no regular analysis of either had been published, they were submitted to chemical investigation by Henry Seybert, Esq. of Philadelphia, who finding it to be a new mineral, also, thought proper to give it a name, to which in no respect does it appear entitled. Mr. Seybert's analysis was published at the time, in this Journal, Vol. V. No. 2, and is in every respect a most lucid and scientific specimen of chemical enquiry. It appears from it that the American and Swedish specimens are precisely similar, being a fluo-silicate of magnesia, and as such, constituting a new mineral species. Within a short period, a new and beautiful variety of this mineral has been discovered at Eden, near Warwick, imbedded in rhomboidal carbonate of lime, differing only in the unessential article of colour and lustre. This is the specimen alluded to by Mon. Von Struve. Still however, it retains the name of condrodite in Sweden, while in this country it would be unjust to deprive it of the name of Brucite from its original discoverer.

W. M.

The metallic substance to which he also alludes, as imbedded in Brucite, and accompanying the spinelle at Warwick, may be mistaken by a transient observer, for the fer oligiste of Haüy, but it is not even magnetic, and differs essentially from it in other respects. It is the same mineral which Doctor Fowler alludes to in his account of the Warwick minerals, and which he supposes to be chromat of iron; but I early determined that it did not contain a particle of chrome, and I find that Doctor Torrey has since ascertained that it is a species of titaniferous iron, precisely similar to a mineral of the same character, which is frequently found in the North of Europe. As to the specimen to which Mons. Von Struve alludes under the name of Nuttalite, I have only to observe that it is the same mineral which I discovered with others, a few years ago at Bolton, Massachusetts, and of which I gave an account in this Journal, describing it as elaolite, or a variety of scapolite. I found however, that since that, a specimen of it was presented to Mr. Brooke of London, by Mr. Nuttall, who finding it to differ in some of its crystaline characters from other minerals, gave it the name of Nuttallite.

In taking a cursory view of the principal minerals of the North of Europe, there are none which are of more importance than the ores of iron, for which Norway and Sweden are so remarkable; but I will venture to say, that every variety of this mineral which has been found there, has been found in the same class of rocks in America, in the greatest abundance and of equally good quality. The whole of those may be included under that species called magnetic oxyd of iron, or fer oxydule of Haüy, either crystalized, steel grained as in Sweden, or compact. Though not exclusively confined to the primitive formation, yet it is found generally to exist in the mountainous districts of New-Jersey, New-York and Vermont, in granite, gneiss, mica slate, and hornblende rocks. It is from this species of ore that the best bar iron of Sweden is manufactured, and if the iron of this country is not of equally good quality, it evidently arises from a deficiency of skill in the true principles of manufacturing it from such ores, as no country in Europe contains those of a better quality, or more favorably situated in every respect.

Titanium is one of those metals which has been found more particularly in the North of Europe. It is said to occur frequently in those primitive aggregates which contain beds of magnetic iron ore at Arendal in Norway, associated with augite, scapolite, epidote and hornblende, precisely the same rocks in which we find it in this country, as we shall see when we refer to details of the different localities, a few of which I shall mention.

In Sussex county, in New-Jersey, that variety of titanium, which is called sphene, or titan. siliceo-calcaire, is found in an aggregate of hornblende and feldspar, which contains abundance of magnetic iron ore. Following the same range of mountains into Orange county, in the state of New-York, sphene is of frequent occurrence in the same formation, and associated as at Arendal, with augite, sahlite, scapolite and amphibole. At Cold Springs, where a part of a mountain has been excavated to construct a road on the banks of the Hudson, specimens of sphene have been discovered, of uncommon size and beauty, imbedded in compact scapolite and sahlite ; and at West Point, similar specimens have been found with the same associations. Crystals of a rhomboidal form, of a light brown colour, and several inches in length, have been discovered in this locality, in every respect similar to those from Norway, and accompanying the same species of minerals. Titaniferous oxyd of iron, is of more frequent occurrence than is generally suspected in those districts which contain magnetic oxyd of iron ; it is said to be found associated with this mineral in Norway, and I have met with every variety of the ferruginous oxyd of titanium, wherever I have traced extensive beds of magnetic iron ore. On lake Champlain in particular, near Crownpoint, it is in abundance ; and the nigrine and iserine there have so great a resemblance to some varieties of magnetic iron ore, that I have known it mistaken for such, and even attempts made to smelt it for iron, but without success, owing to its infusibility. Indeed, some of the ores of iron in that district are so combined with titanium, that it renders them very refractory in the furnace, which may account for the frequent failures which are met with in fusing such ores.

There is scarcely any part of Europe where a greater variety of augites are found, than in Norway and Sweden ; nor could I find any class of minerals in which the similitude between the specimens from those countries and America, affords a more striking example. I shall include several of them under the name of pyroxene, as established by Haüy.

1 *Augite.* This species, which we receive from Arendal, is perhaps found in the United States in greater abundance

than in any other country. Crystals of augite of every variety of form, and of a very large size are found in those districts of the states of New-York and New Jersey, which contain beds of magnetic iron ore, but in no place more beautifully and perfectly crystalized than at Munro, in the state of New-York. These crystals are found either in groups or single, and are of an olive green colour, differing only in magnitude from such as are obtained in rocks of a similar formation, and under similar circumstances, at Arendal in Norway. But a variety of augite has been found at Kingsbridge, and at Litchfield in Connecticut, which has not been discovered in Europe, and indeed i. it scarcely noticed by any of their writers. It is found in large and small crystals, which are purely white, generally in elongated four or six-sided prisms, nearly rectangular, sometimes truncated on two edges at the extremity ; imbedded at Kingsbridge in primitive limestone, and at Litchfield in dolomite.

2. *Sahlite.* This is a variety of pyroxene, which was first observed in the primitive rocks of the North of Europe, and it affords another striking illustration of the complete identity of the minerals which are found in the same formations in America, as well as in Europe, and of the uniformity of the structure which pervades the same class of rocks in both countries ; indeed many of the specimens of sahlite from each are so similar, that it is scarcely possible to distinguish them from each other, when in the same cabinets, without reference to the labels. The finest yet discovered in America, were found in the state of New-York, particularly at Munro, and in the forest of Deane, where the specimens, generally of a dark green and bronze colour, with a fine lustre, and lamellated structure, yield to mechanical division, the result of which is an oblique four sided prism, with rhombic bases. The same specimen will also frequently exhibit a granular and a lamellated structure, and what renders the coincidence more remarkable, is, that here, as well as at Ticonderoga, it is found associated with titan. siliceo-calcaire, as at Arendal, in Norway.

3. *Coccolite.* This is another variety of pyroxene, specimens of which we receive from Norway, where it is found associated with ores of magnetic iron. Every variety, however, of this mineral, has been found in this country, and precisely under the same geological circumstances. Nothing can exceed the beauty of the specimens which are found in

the Highlands of the state of New-York, at Ticonderoga and at Wellsborough, near Lake Champlain.

4. *Amphibole.* Though it cannot be said that this very common mineral is peculiar to the North of Europe, yet some of the most distinct and perfect specimens of crystalized hornblende are found at Arendal, in Norway, intimately mixed with magnetic iron ore. Beautiful as they are, however, they are inferior to those which have been found near Warwick, in the same geological formation. A new and rare variety of amphibole, of a light brown colour, and finely crystalized, has also lately been found at the same locality, which has been examined by Mr. Brooke, of London, and, from the form of its crystals, pronounced by him a new variety of hornblende, though differing extremely from other hornblendes in its external characters.

5. *Colophonite*, or, *Grenat Resinit.* Fine specimens of this mineral are found at Arendal, in Norway, in beds of magnetic iron ore. Every variety of the same mineral has also been discovered in North America, and, in almost every instance, associated with ores of magnetic iron, as in Norway. The most interesting localities for this mineral, in the United States, are at Ticonderoga, at Wellsborough, near Lake Champlain, and at Franconia, in New-Hampshire; but those specimens which we receive from Wellsborough, have the greatest resemblance to the colophonites from Norway, presenting a greater variety of colours, many of an orange red colour, of a resinous lustre, and with an iridescent play of colours.

6. *Scapolite.* This is one of those rare minerals which is more frequently found in the metalliferous beds of Norway, than in any other country. It has, however, lately been discovered, in precisely the same formation, as well as in primitive limestone, in the United States. At Bolton, in Massachusetts, it is found beautifully crystalized in limestone; it is also found compact and foliated in the same rock, as well as at Cold Springs, on the North River, where it is associated with augite and sahlite and crystals of sphene. In general, all these specimens are perfectly similar to those which we receive from Norway. The only variety which I have not yet met with, which has been discovered in Norway, is the red, or paranthine. It may not, however, be superfluous to remark, that most of those fine specimens which we receive from Norway, exhibiting beautiful groups of radiated crys-

tals, are not in their natural state, but are detached from their matrix by submitting them to the action of nitric acid, which dissolves the carbonate of lime, in which they are imbedded, without acting on the scapolite.

7. *Epidote.* This is a mineral so generally diffused in nature, that it is not confined to any class of rocks. It has lately been discovered at Arendal, in Norway; but those fine crystalized specimens which we often receive from that country, are generally detached from their matrix by acids, in the same manner as the scapolite, when imbedded in limestone. In this state their value is greatly enhanced when in the hands of the mineral dealers. Specimens of epidote, however, equally beautiful and well crystalized, have been found in Rhode-Island and Pennsylvania. In general, these crystals are found in cavities in quartz, and are perfectly distinct without the aid of acids, which would have no effect in exposing crystals imbedded in such a matrix.

It will be observed, that in the above sketch, I have confined myself to a detail of those minerals only, which have been found in a particular district of the North of Europe, in order to point out the striking resemblance between those specimens and such as are found, under similar circumstances and associations, in North America, without any reference to a number of minerals which have been discovered, not only in the same formation, but in other parts of the United States, such as the red oxyd of zinc of Sparta, the spinelle of Warwick, and a great variety of minerals, which are either rare in Europe, or have never been discovered there. These are sufficiently uncommon and interesting to require a separate notice on some future occasion.

If the subject is found of any interest at present, these observations are perfectly at your service.

W. Meade.

ART. XIX.—*Reply to Dr. Jones, in the Franklin Journal, on the subject of the Memorial on the Upward Forces of Fluids;* by ED. C. GENET, Esq.

TO THE EDITOR.

NEW-YORK, April 26, 1827.

SIR,—Your kindness in allowing a place in the three last numbers of the American Journal of Science and Arts, for the extracts from, and the defence of, my Memorial on the applicability of the upward force of fluids to ærostatic and hydrostatic machinery, ought to restrain me from committing any further trespass upon your indulgence and the patience of your readers. I had, indeed, resolved to adhere to that prudent course, until the experiments which I am preparing on the new mode of navigating the air and the water, had attested whether man, assisted by the levity of the aeriform and the density of the aqueous fluids, could extend the limits of his faculties into the elements alotted to the birds and the fishes, or remain circumscribed within his present narrow compass.

But, Sir, I am under the necessity of deviating from that cautious line of conduct, and permit me to state, that you are yourself the cause of this new Levée de Bouclier. Your editorial note, annexed to the publication of my letter of the 12th of November last, informed me, that Doctor THOMAS JONES, editor of the Franklin Journal, and Professor of Mechanics in the Franklin Institute of Philadelphia, had once more taken the trouble to bring my theories into public notice. That hint was sufficient to excite my curiosity; expecting to find, in the Doctor's remarks, some useful lesson or merited correction, I hastened to procure a copy of the number of his Journal that you had designated. But how great was my surprise, on finding that nearly the whole of this new production of his pen, was in a great measure calculated to involve, in my proscription, our worthy friend, Doctor PASCALIS, Senior Censor of the State Medical Society, of New-York, and President of the American Branch of the Linnæan Society, and to hold us both up to public view as fit subjects of ridicule, the one for having advanced an untenable doctrine, and the other for having in some measure countenanced it, by claiming for its author, on a subject entirely new, a suspension of censure and condemnation, until the whole scheme could be matured and rectified by actual experiment.

This attack upon my respectable friend, and old fellow citizen, and his determination to observe only silence, has laid me under the double duty of assuming his defence and my own, and once more, Sir, to beg the favour, which I should not otherwise have solicted, of holding up, in your columns, my upward forces, against the efforts of those who endeavour to put them down. In fulfilling this task, however, discarding all feelings of irritation, and unwilling to retaliate illiberality, I will pass over the stigma, puns, and *bonsmots*, both in English and in French, with which Dr. Jones has ornamented his remarks; I will not even expose his faults in the French lexicography, having been brought up to consider such minute revisals as unmannerly. Rejecting, therefore, all that extraneous and offensive matter, incompatible with an impartial investigation of truth, or of a delicate detection of error, I will only select what is tangible by science, and attempt to show, that if, as Dr. Jones says, and unintentionally confirms, "*Tous les docteurs ne sont point docte*," all critics are not exempt from criticism.

There is, Sir, in Doctor Jones' last remarks, a precious confession, which in good tactics, I must place in front of my line of defence, because it will throw some light on the views and interests of my assailant, namely that the doctor is committed on certain doctrines and systems, on the steam power, which he has publicly taught, and that his impartiality, on the trial of another power, offered as a substitute for that very great, but very dangerous and expensive force, may at least be questionable.

In his present remarks, Doctor Jones, seems to have abandoned the chase of the æronaut or ærial vessel, long ago dispatched to the moon by the Boston reviewers, until my friend Eugene Robertson shall recall it to the geological crust of our own globe. The hydronaut, or water vessel, moving by combined ærostatic and hydrostatic forces, has become the pointed object of his satire, and as, according to rule, the identical matter of a libel must be laid in substance before a court and jury, I beg leave, Sir, to place before you and your readers, as an indispensable preliminary, for my vindication, the following quotation from the Franklin Journal, No. 1. Vol. III. January 1, 1827.

"Before describing his newly contrived propelling machinery, Mr. Genet gives some information relative to the steam engine, for the purpose of running a parallel between it and his newly discovered power. In doing this, he furnishes intelligence respecting the derivation of force in the steam engine, which is new to us, although we have constructed and used this instrument as well as studied and explained its operation. We are told by him, that Mr. Watt *has availed himself of the downward force of the piston and weight of the steam engine, and that the available force is created by the falling of the weight and piston into a partial and momentary vacuum, and that its power is determined by the incumbent weight and by the atmospheric presure.* Our own conclusions upon the subject were so unlike those of Mr. Genet, (called by Doctor Pascalis a philosopher of no common stamp,) that we had believed, and even publicly taught, that the weight of the piston and of the other moving parts of the engine served only to abstract from its power, and that, provided they had the necessary degree of stability, the lighter they could be made, the better they would answer in practice. Instead of supposing that the pressure of the atmosphere was necessary to the action of Mr. Watts' engine, we had taught that its structure might be simplified, and that its action would be more powerful, could it be worked in vacuo. Here is a great discrepancy, either we or the uncommon philosopher, together with his encomiast, must be in the wrong, and need in sober truth to study again our experimental and mechanical philosophy. We are giving a *fair, candid* and *ungarbled* statement of the doctrines taught by Mr. Genet, and let qualified judges decide between us.

"The following questions, with their answer, are from page 61, of Mr. Genet's Memorial. *How does the steam engine perform that operation? how does it create that vis motrix, that moving force, which is the mechanical life of the machine? By the alternate increase and decrease of temperature which produces in the cylinder two kinds of fluid, the one gaseous, the other atmospheric, by means of which the piston rises and falls.* We had fancied that the vis motrix was caused by a vacuum on one side of the piston and an elastic but condensible vapour on the other, and that the exclusion of gaseous and atmospheric fluids was of essensial importance to its action; but we really do not understand the nomenclature here used by Mr. Genet, and of course are not.

in this case, qualified judges. We never learnt *that a vacuum was a gas or that the vapour of water was atmospheric.*

"When gentlemen who are members of learned societies, and who are confessedly very clever in particular departments of science, bring the influence of their names and their talents to the support of notions the most crude, and which manifest a palpable deficiency of knowledge in the first principles of the particular branch of science to which the examination belongs, it would be a culpable neglect of duty in the sentinels of science to permit them to pass unhailed, &c."

"To talk, continues the Doctor, about condemning a man because he has ventured to depart from the Newtonian philosophy, is to talk very feebly. We know of no one who would pretend to establish a *law*, upon the opinion of that philosopher, &c. What we understand by departure from the Newtonian philosophy, is not to depart from the deductions of that philosopher, but from the mode of philosophising, by which these deductions were formed, and in this point of view the case in hand is a most fearful one."

This judgment, Sir, delivered from the chair of the professor of Mechanics of the Franklin Institute of Philadelphia, would really be appalling, if by one of the most agreeable events of the early part of my life, by my own contemplations, experiments and studies—and by frequent observations made on board of our steam boats, and in our steam engine manufactories, I had not acquired, on the origin, progress and improvement of the steam power, and the theory of the æriform fluids, a knowledge of facts that will help me out, to prove, that the aspersions of my censor and his unfair subversions, misnomers and curtailments of my text, would prevent his readers, who have not perused my Memorial, from forming a calm and unbiased opinion of my work.

In the year 1784, being then chief clerk of one of the *Bureaux*, or sections of the department of foreign affairs at Versailles, and corresponding member of the Royal Academy of Sciences, I was sent to England as secretary of Legation with the Count de Moustier, who was afterwards minister plenipotentiary of Louis XVI. to this country, and among the private instructions which I received from the ministry, I was directed to visit the English manufactories, in order to collect preparatory information, for the negociation of a treaty of commerce, about to be concluded between France and Great Britain. Arrived in London with the Count, I declined being introduced to the court, an honour which would have absorbed too much of my time, and being supplied by the Duke de la Rochefoucault, the Marquis de Condorcet, by Bailly, Lavoisier, by Sage and Brisson, my masters in chemistry, mineralogy and philosophy, and many other scientific and literary French characters, with letters for their friends and correspondents in England, I prefered the fruitful acquaintance of several illustrious members of the Royal and Antiquarian Societies, and of the other societies devoted to the promotion of the useful arts, to the vapid pleasures of the more brilliant circles of St James. By means of those dignitaries in the court of the Muses, I was favoured with uncommon opportunities of exploring all the sources of information which London offered to my admiration, particularly in the mechanic arts. The famous steam mills and other machines moved by the power of steam, did not escape my close attention, and anxious to see the place, where these wonderful machines had been invented, perfected and manufactured, I went to Birmingham, strongly recommended to Dr. Priestly, and to Messrs. Watt and Bolton.*

* My acquaintance with Mr. Bolton was not limited to my stay at Birmingham. On my return from England, I reported to the ministers, and particularly to those of the finances and of the navy, all the wonders I had seen in that astonishing country; but I stated also, as my sister Madame Campan has reported in her Memoirs, on the life of the unfortunate Queen Marie Antoinette, my apprehension, that the admirable machines, the large capitals, the extensive credits, and the superior mercantile knowledge of the English nation, would render very hazardous the liberal treaty of commerce, which the French economists were then urging with that country, unless immediate measures were taken to improve several very backward branches of industry in France. I was directed, accordingly, to invite Mr. Bolton, or Mr. Watt to repair to Versailles, and to hold up to them great encouragements for their patents and their manufacture of steam-engines. Mr. Bolton came with his son, (whom he afterwards left under my care to learn the French language, and to partake in the exercises of the young nobility, at Versailles.) I assisted as interpreter at all the interviews of Mr. Bolton with M. de Calome Comptrolleur general of the finances, and the Marshal de Castries, minister of the navy. I translated also all his communications and patents, and I remember perfectly well, that among the various enumerations which that great manufacturer made of the several applications, which could be effected of the steam power, he included navigation and the horizontal rotatory motion of mill-stones, which he had effected with many others, differing but little from the vertical motion of paddle-wheels. But as manufactures of cotton, and public pumps, were the principal objects in view at that time, that hint was not improved. It was reserved for the patriotic and persevering spirit of the illustrious Robert Livingston and his ingenious assistant, Robert Fulton, to enrich and honour the United States, with that glorious application of a power, the discovery and regulation of which, in mechanics, excepting the said successful application, is as much

In the laboratory of Dr. Priestly,* the father of the pneumatic philosophy, I witnessed those interesting experiments improved by Lavoisier and others, which have so much enlarged the circle of human knowledge. From Messrs. Watt and Bolton, and particularly from the first, I obtained, not only the most extensive information on the great improvements which had been made in applying the gigantic power of steam to almost all the arts, but also on its beginning and history, all which is perfectly present to my memory, my mental retentive faculties being preserved, by the invigorating exercises of a laborious country life, since the year 1793; when one of the billows of the French revolution, followed by many others more tempestuous, left me on these shores, without any other support than my industry, and a good stock of submissive philosophy in adversity; to the credit of which I must charge the encomiums of my friend Pascalis, which otherwise would justly deserve the sneers of Dr. Jones.

Mr. Watt, then, that immortal mathematician and mechanist, had the kindness to give me a general view of the discovery of the steam power, and with that impartiality which denotes the real friend of mankind, he allowed to France and to England what they were respectively entitled to claim. To France, the famous machine of Dr. Papin, which in the year 1698, exhibited the power of steam to procure the dissolution of bones and other substances, and was supplied with the first safety valve. But to England he attributed the discovery made, at the same period, by Savary, of the application of steam, as a power to raise water from wells by atmospheric pressure. The experiments made on the power of steam, under the reign of Charles II. by the Marquis of Worcester, during his confinement in the tower, as well as a very remarkable attempt at an anterior period, under the reign of Charles I. to propel vessels by the power of steam, were also noticed by Mr. Watt, as having concurred to give the idea of the first engine, combining the expansive force of steam and *atmospheric pressure*, which first engine was, I

the property of Great Britain, as ærostation is the inventive property of France. But if we succeed in the construction of an hydronaut, and in the steerage of ærostats, to America alone will be due the invention and promotion of those improvements.

* Dr. Black's name should not be forgotten on this occasion, and the philosophy of the steam engine is also under everlasting obligation to this great philosopher.—Ed.

believe, executed by a blacksmith, called Newcomen. None of the subsequent improvements were omitted by Mr. Watt, to show the gradual progress of the steam system of mechanics, and reach his own improvement; *in the first place*, upon the open ended cylinder, in which the expansive force of steam was employed to raise the piston, which in its fall, after the destruction of the steam, as I have stated it, became, under the pressure of the atmosphere on the piston, and the weight of the said piston, the *available force*. *And in the second place*, upon the tight cylinder which is constructed in such a manner, that on one side, instead of the atmosphere, the steam is made to press on the piston, whilst on the other side the cylinder is open to the condenser.

This summary description of the two different modes of guiding and using the power of steam, improved in succession by Mr. Watt, and which have both their respective advantages and disadvantages, and are both in use, will, I presume, Sir, satisfy you and your readers, that, in the critique of the parallel which I have drawn between the operation of the steam-piston in an open cylinder, and my hydrostat, equally placed in an open cylinder, Dr. Jones has proved either his ignorance of the difference between the open ended cylinder, practically called the *atmospheric engine*, and the tight cylinder, called the *double acting engine*, or that he has purposely selected the complicated operation of the double acting engine, in which the pressure of the atmosphere is no more used, or of the high pressure engine, (in which, on account of its overwhelming force, the pressure of the atmosphere is not taken into acconnt, and the cylinder is left open to the air,) for the *unfair* purpose of showing that my parallel was incorrect, and my definition of the steam-engine injudicious. Had he known, or *candidly* considered, that in the open ended cylinder the direct force of the steam goes only to raise the piston, and that the expansive force of that steam being condensed, the vacuum created determines the fall of the piston, under its own weight and the incumbent pressure of the atmosphere, equal, on every square inch of the area of the piston, to 15 pounds, he would not have asserted, "that the pressure of the atmosphere and the weight of the piston were not necessrry to Mr. Watts' engine, and served only to abstract from its power."

There is in reality no pressure of the atmosphere in Mr. Watts' double acting engine,* which Dr. Jones has here in

* Except on the area of the section of the piston rod.—Ed.

view. But if there is none, how can it abstract from its power? And again, if the piston works in the vacuum, as Dr. Jones has told us it did, what difference can its levity or its weight make in its power, if in vacuo, according to Newton, the gravitation of a feather and of a ball of lead, compels them to obey, with the same speed, the proportionate force that draws them towards the center of the earth?

While Prof. Jones "views my *reveries* as harmless, from their not containing any thing which could mislead an individual acquainted with the first principles of mechanical philosophy," I will admit, that from the little he has communicated, in his remarks on my book, of the doctrine taught by him in his class, on the construction of steam-engines, and his plan of making them work in vacuo, with the lightest piston and other moving parts, I do not see any discrepancy on that point, between him and what others have thought before him, namely, that the action of a steam-engine, is vastly diminished by friction and the difficulty of procuring a perfect vacuum. But that improvement, in vain attempted by several patented engineers, and, it is reported, tried again lately by Mr. Perkins, has been, to this day, and will, I am afraid, continue to be, the stumbling stone of all the steam-engineers, because it is contrary to the nature of things in the process of condensation. A perfect vacuum cannot be procured;—the air that remains, or is formed in the cylinder after the condensation, or the air which enters the steam-vessels, with the condensing water, and the gas, air, or steam, which forces itself between the piston and the sides of the steam-vessels, let the collar through which the piston-rod must work be made ever so tight and close, cannot be drawn out or excluded entirely, and will always, in all kinds of engines, oppose a gradual decrement to the descending power of the piston. In fact, the vacuum so much talked of, (at least a perfect one,) is more a fiction than a reality; but as it seems to be the main foundation of Prof. Jones' mechanical philosophy, I will abstain from any further reflection on that subject, and content myself by observing, in my own defence, that, in as much as there is a new accession of atmospheric air, and a recomposition* of water, occasioned essentially in the steam-vessels, by the injection of cold water and

* Mr. Genet evidently intends not a *recomposition*, in a chemical sense, from the elements of water, but a mechanical reconversion from the aeriform to the inelastic state.—Ed.

the departure of caloric, the denomination of atmospheric fluid, applied by me to the pretended vacuum, was not improper, any more than the two temperatures, which are the cause of the alternate motion of the piston, since the one is expansive and hot, and the other contractive and cold.

The denomination of gaseous fluids, which I have given to steam, and not to the vacuum, as the Doctor very unfaithfully quotes it, was also, I believe, Sir, correct in chemistry. That vapour was called, by the French chemists, before the adoption of the new nomenclature, *vesicular* gas, and Dr. Thomson, in his Modern Chemistry, in the classification of those elastic aerial fluids, to which the general denomination of gas, from the German, *geist*, (spirit,) has been given, assigns to steam the 12th rank among the compounds. But it is clear, that atmospheric air itself is a gas, and certainly the most compound and mysterious of all, since microscopic observations show that it contains the principles of animalisation and vegetation, and chemistry has proved that the hardest substances and minerals may be returned to its empire, that meteoric stones are formed within its strata, and that through the medium of that *chaotic spirit*, were derived the oxygenous supporters of combustion and life, together with the azotic, the calorific, electric, magnetic, and phosphoric fluids, &c. &c. all, it seems, flowing from the great source of circulation, *the sun.*

But it would be improper, in a vindication, to defend a point that the adverse party has not attacked. Dr. Jones *garbles*, when he says that I call *vacuum a gas*, and the *vapour of water atmospheric;* but he has not even suspected that I considered atmospheric air as a compound gas.—Returning then to his remarks, I find, that after having made all his efforts to prove "that I may either not understand, or or may have committed some mistake in my explanation of the steam-engine—that I may have watched its *motions* without having become familiar with its *disposition*," he changes his point of attack, and undertakes to show that "in my own child, the hydronaut, he might have expected to find me perfectly familiar with both; and that, however, he is a little inclined to suspect, that in this latter case I know more of the *disposition* than of the *motion*."

On reading this intimidating introduction to another critique, and the terrific exclamation which precedes it, "*To the hydronaut*," or *down with it*, my first impressions, Sir, were those of a novice, at the call of a master who is going to expose his errors and correct his blunders, or of a criminal, ordered to the bar for conviction. But once more I was disappointed; the Doctor, without assigning a single reason for his judgment, but his infallibility, passes sentence on this machine, warns, as a faithful sentinel, (of steam-boats,) all the mechanicians against its delusions, and affirms that "it will *stop* until it is navigated."

That decree is severe; it contains, however, a valuable admission, that I will improve, as well as a jocose comparison, made by the Doctor, of my hydrostats, "to a couple of egg-shells suspended to a scale-beam, against which passing two tumblers of water, the beam will be made to *vibrate*." The word *stop* implies a first movement impeded, and a *vibration* implies a movement continued; for no machine would stop, if it had not previously been put in motion, and no beam would vibrate on its fulcrum, if a force had not acted upon it. I accept, accordingly, the case, as the lawyers say, with these two admissions, as settled by the court below; and although the simile to egg-shells is rather derogatory, I will try to fasten to that slender link, a chain of facts and arguments that will lead less conceited appellate judges to consider my hydronaut as a more practicable machine than the Doctor would make it appear. The greatest simplicity has always been recommended by the most celebrated masters in mechanical combinations, and the nearer my system is brought to that golden rule, the more it will be exempt from the extreme intricacy of the steam-boat engines, in which such a loss of power is suffered by friction, by decrement in the fall of the piston, by the resistance of the air compressed, and by other causes, that in reality the available force applied to the paddle-wheels, does not amount to more than one-fourth of the power originally created.

I will then observe in pursuance of the above mentioned *simile*,

1. That if the two tumblers of water approached to, and put in contact with, the pretended egg shells, that I call hydrostats, make the beam vibrate, it is due to the pressure of the water, which agreeably to the property of that fluid is either upward, sidewise, or downward.

2. That in lieu of the tumblers, and of the hands that raise them against the shells, in the simile, we substitute, mentally, a basin of water, in which shall be placed, under each

of the said shells, converted now into tin hydrostats, a tin cup, open-ended, and having at the bottom two holes supplied with a sliding valve, as represented in my specifications; the one communicating with the basin of water, and the other by a pipe or leader, with a lower empty basin. It is unquestionable that the first under valve communicating with the upper basin filled with water, being opened, the water will rush from the said basin into the tin cup, and, by its upward force, will raise one of the hydrostats appended to the beam. And it is also unquestionable, that when the ascension of the said hydrostat is finished, by the elevation of the water in the cup to the level of the water in the upper basin, if the valve communicating with the lower empty basin is opened, and the valve communicating with the basin of water is shut, the water will flow from the cup into the under basin, by the same cause which makes water invariably drop from an upper level into a lower one, and which operates in the same way upon the syphon. It is also unquestionable, that the first hydrostat raised, ceasing to be pressed upward by the water rising in the cup, will return with the beam to its former state of equilibrium.

3. That if, as soon as the first hydrostat returns, the under valve is shut, and the same operation, performed on this first hydrostat, is repeated upon the second, an alternate or reciprocal vibration will be given to the beam by its ascent.

4. That if the water discharged into the inferior basin, is drawn out, or pumped out, and returned to the upper basin, or if fresh water is put into the said basin, and the water discharged into the lower basin is thrown away, the motion of the beam will be continued as long as the manual operation which promotes it, is not suspended.

5. That if to the beam is appended an apparatus calculated to open and shut alternately, at proper intervals, the two valves, and to draw out the water from the lower basin, (as is more fully explained in my Memorial,) it seems also evident that the hydrostats will rise alternately, and the valves will open and shut, as requisite, as long as water is supplied to the upper basin or drawn out of the lower one, just as well as if manual labor had contributed to raise the tumblers against the shells of Dr. Jones.

6. That if that simple machinery being enlarged and placed in lieu of a basin of water, on a vessel floating on a large and deep body of water, and if to the said machinery are

added two other apparatus appended to the beam, (as described in my Memorial,) the one calculated, as the machinery of the steam-boats, to procure a rotatory motion to paddle-wheels by means of connecting rods, cranks, &c. and the other to work alternately two pumps intended to draw the water from the lower basin, it may be inferred, that if the force procured by the pressure of the water against the hydrostats, and their own levity being filled with gas or air, is superior to the force spent on the paddle-wheels, and on the pumps, and the rest of the machinery, the hydronaut will be propelled by its own self-created power, or rather by the artificial current of water created, and the transient use made of its upward force. But, Sir, on this last hypothesis, and on this one alone, as I mentioned in my last letter, several mechanicians, who are far from being as adverse to my plan as Dr. Jones, raise doubts on the success of the hydronaut. They presume that the power of the hydrostats will be proportionate to the identical weight of the water admitted into the cylinders, and that the weight of the water discharged into the lower basin or recipient, being equal to the weight of the water in the cylinder, an equipoise, and of course a stagnation will take place, after the first ascension of the hydrostat, as Dr. Jones predicts, without assigning any reason for it.

There is, I admit, an apparent plausibility in that objection, and I had guarded in my Memoir against the possibility of a superabundance of water in the recipient, observing, that if the said recipient should retain more water than the *true air pump*, otherwise called forcing pump, could raise, extra pumps, similar to the best of those used on board ships, could, without much additional trouble and expense, be used to keep up the alternate action of the hydrostats.—I am not, however, converted to that opinion, and I am still inclined to think, that the pressure of the water rising, in what I have hitherto in this letter called the cup, and now by its proper name the cylinder, against the inferior area or foot of the hydrostat, will be proportionate to the perpendicular height of the exterior columns of water, entering into the cylinder by the under valve called in my Memoir the discharging valve; and that every foot in the elevation of the exterior columns of water will increase the pressure against the hydrostats, and multiply their force, in proportion to their area, much above the weight of the water admitted in the cylinders, and discharged subsequently into the recipients; because, I repeat it, for the information of those who are not as familiar with hydrostatics, as with the other branches of Mechanics, the pressure of fluids is directly as their perpendicular heights, without any regard to their quantity or their direction, as the well known experiment of the hydrostatic bellows and the hydrostatic press attest incontestibly: consequently, if in reference to a ship or other vessel, drawing say 15 feet water, the top level of the outside column of water, commonly called the water line, is 10 feet higher than the foot of the hydrostat at rest, allowing 5 feet for the lower basin, called in my patent, recipient, there is no doubt that the small quantity of water required in the cylinder to facilitate the ascent of the hydrostats, through the very short space which they have to travel to move the beam, will be infinitely less in weight, than the pressure of the outside columns of water, and will leave accordingly, an ample surplus of force to apply to the paddles, pumps, and other moving parts. At all events, if I was mistaken, and if the settled facts and principles, upon which I have built my system, were not in point, I have devised, besides the pumps, various very plain natural means of disposing of the water discharged from the cylinders, independently of the force originated by the pressure of the exterior columns of water on the area of the hydrostats, and I should publish those means applicable to other purposes, if the encouragement given to my plans was sufficient to extend my patent rights, as well as to defray the great expense attending those grants. But, unfortunately, Sir, my own pecuniary means are extremely limited, and having nothing to spare, but a little leisure to indulge my taste for philosophical studies, (while my eldest sons, who are now grown men, and whom, for the sake of their peace and happiness, I have brought up as farmers, attend to the business of our land and mills,) I am reduced to depend for the promotion of my improvements upon public patronage, a very precarious support indeed, particularly when the overwhelming power of high steam is poured upon them.

But, Sir, if the innumerable victims of the steam-engines could raise their heads from their earthly or watery graves, their voices would urge to investigate and ascertain, dispassionately, by actual experiment, if the cheap, simple, and safe substitute, which I offer in lieu of that expensive, intricate, and dangerous power, ought to be adopted, or considered as the eccentric dream of a philanthropist. The unfortunate passengers of the English steam-boat Comet, of our steam-boats the Oliver Ellsworth and Ætna, and many others who have perished by drowning or by scalding and other events, would warn the living to forbid high pressure engines on board of vessels, and to forbid also iron boilers, which when highly heated, produce hydrogen gas or inflammable air, as well as steam. But voices from the tombs are fanciful chimeras, and the interests and prejudices of living, rich, and influential men, holding steam stock, are efficient realities. I have experienced more than once, the *vis motrix* of that new political power, not less operative than our banks.

Would you believe it, Sir, our enlightened and patriotic governor, Mr. Dewitt Clinton, in a very judicious message, had recommended to our Legislature, measures for the safety of passengers in steam-boats and other vehicles. I appeared at Albany before the committee to whom that important concern was referred; I met there a strong representation of the steam-boat interest, determined to defeat whatever would lay their companies under any additional expense and reduce their profits, very much increased by high steam, iron boilers and short passages. I demonstrated all the dangers attending high pressure engines and iron boilers, and the authority which every Legislature had to forbid them entirely; I made also, before the committee of the house, several experiments, proving that unless all the laws of hydrostatics were reversed, the ærostatic alleviators, which are part of my patent, would prevent steam-boats from sinking, as must inevitably be the case, whenever, with their enormous concentric weight, equal to 200 tons, they spring a leak, either by the concussion of other vessels, as was the case with the Comet, or by the effect of snags or sawyers, as is so often the case on our southern and western rivers. The committee, notwithstanding the dissent of my opponents, made a very flattering report in favor of my views on the means of preventing steam-boats and vessels of all description from foundering, and recommended an appropriation of one thousand dollars, at the disposal of the governor, to make upon a larger scale, the experiments which I had made before them, upon a small one. But economy ostensibly prevailed, and our lives and property, in point of safety, on board of steam-boats, remain pretty much as they were before the message, excepting a few less important police regulations for the landing of passengers.

Having thus, Sir, in this and my preceding letter, explained what, perhaps, I had not sufficiently, or too technically treated in the specifications of my patents, I have taken the resolution henceforth to concentrate my attention entirely to the promotion of the experiments, which alone will settle, beyond the reach of criticism, the question of the utility of the upward forces of fluids; and I am warranted in the belief, by your known benevolence, that you will hear with pleasure, that I have the hope of acquiring, in this city, where I have spent the winter, associates for the essay of an hydronaut. I have also positively secured a first rate ally, for the construction and experiment of an æronaut, in the son and disciple of the celebrated Professor Robertson, of Paris, Mr. Eugene Robertson, so well known in this country by his numerous and successful ascensions in balloons, as an expert and scientific experimenter. I have forwarded to you a copy of the narrative of his eighth ascension from New-York,* which contains, besides several very interesting anecdotes and important observations, an encouraging opinion of my plans for the improvement of the navigation of the air. He does not hesitate to say, "that having read attentively my Memorial, my calculations had appeared to him to be grounded on correct philosophical principles, and that he did not see why the execution of my æronaut should not resolve the problem, which has so long remained undetermined, on the practicability of moving and steering ærostatic machines through the air, with a self-created force, instead of being, as they now are, the sport of the winds." Indeed, Mr. Robertson is so much convinced of the correctness of the idea of combining into one undivided system, ascension, propulsion, and steerage, as nature has combined them in the fishes, that he has pressed me not to let that plan lie dormant, and to do every thing in my power, by my own exertions and the concurrent efforts of all the friends of science, to excite the citizens of the United States to achieve once more, for the promotion of the useful arts, what Europe has left unfinished "The Americans," observes Mr. Robertson, "have applied the steam power, combined with mechanics, to the regular navigation of the water; let them now apply the ærostatic power to the navigation of the air,—a double conquest, which will spread an immortal fame over their national character."

* A notice of this narrative was given in the last number of this Journal, page 166.—Ed.

Indispensable engagements have obliged Mr. Robertson to visit New-Orleans; but before his departure he formed a connexion with me, and has pledged himself, in several letters from New-Orleans, where he has made a most brilliant ascension, to devote, at his return, in the course of the summer, or early in the fall, the practical experience which he has acquired in ærostation, to the execution and management of an ærouaut, which he confidently believes will succeed, and which, spirited and patriotic subscriptions, will, I hope, enable us to construct, and to launch into free space, under the proud management of man.

Respectfully, yours, E. C. GENET.

ART. XX.—*Remarks on Ærostation;* by the EDITOR.

EDMUND C. GENET, Esq. formerly minister from France to the United States, and Mr. EUGENE ROBERTSON, distinguished by his adventurous and successful aerial voyages, have issued proposals for raising funds by subscription, to enable them to ascertain by actual experiment, the practicability of navigating the air. The principles of the plan and the outline of the machinery have been already published, in Mr. Genet's Memorial on the upward forces of fluids, of which some account has been given in former numbers of this Journal. It is not to be doubted, that the experiments heretofore made, for the purpose of directing balloons through the air, have failed, and it is equally certain, that the principles upon which a ship is steered in the sea, are in a great measure inapplicable to the case of a balloon floating in the air. At least this is true of ships moved by sails, for they are operated upon by two forces, the wind and the water, and in every case, except that of a perfectly fair wind, the ship is impelled in a course, resulting from the composition of two forces.

A balloon, as usually constructed, has, on the contrary, no impelling power but the currents of the medium, in which it is immersed. The case is considerably analagous (although not perfectly so) to that of a ship without sails floating in a current. But in this case there is an adequate moving power that can be applied. It is found in oars and paddles, worked by muscular force or by steam. In principle, then, why is not the analogy perfect between a ship thus situated and a balloon? The analogy fails in this point; the balloon floats entirely in one medium, while the ship is divided between two—and what difference does this occasion in the capacity to generate and apply power? The difference is this; the power, in the case of the ship, is generated in a rare and applied in a dense medium. There is little in so rare a medium as air to impede the movement of limbs and machines, while the latter strike with great force upon the dense medium, water, and thus produce a full effect. In the case of a balloon, the medium in which the movements generating power take place, is indeed the same, as in that of the ship, but then the stroke is exhausted upon the *same* thin medium, and therefore produces little effect, unless the surface of contact or impulse be proportionably enlarged, in which case the machinery becomes very bulky, a great deal of the power must be expanded upon its mere movement to and fro, and human muscles will soon be exhausted.

The navigation of submarine boats, is perfectly analagous (respiration aside) to that of balloons. In the submarine boats, if the hidden navigator can be supplied with air and can keep out the water, he can indeed move at pleasure by the impulse of oars or paddles, upon the same medium in which his machine floats, but he cannot move rapidly, because the medium is too dense for the free motion of his wings or oars, and they cannot be applied on a great scale, because there is not adequate power within, to overcome the resistance without.* Such machines† move therefore tardily, unless impelled by the currents of the water, in which case they are acted upon precisely as the balloons are in the atmosphere.

Mr. Genet proposes to call in the aid of animal strength, and to work his balloons by horse power, applied to machinery. (See his drawings and descriptions Vol. XI. p. 346 of this Journal.)

This is a novel attempt—nothing beyond the power of human muscles having been, as yet, applied for this purpose; and experiment alone can decide whether any very considerable effect can be produced, even by horses, in modifying the direction of a balloon.

* Thus making his case to differ from that of the fishes.

† See the very interesting account, by Charles Griswold, Esq. in the second volume of this Journal, p. 94. of the submarine adventures of Sergeant Lee, at New-York during the revolutionary war.

That it is physically possible to raise horses, and even *relays* of horses, into the atmosphere, with sufficient provender to give them subsistence for a few days, is certain. But how far the agitations of tempests, may render the swing of the aerial deck too violent, and the slope too steep, to admit of the efficient exertion of animal strength, remains to be seen. Sufficient ballast will, undoubtedly, prevent the calamity of being overset, or of being thrown upon beam ends; but ballast will not prevent vibration, and vibration may become inconvenient, if not dangerous.

Supposing that the force in question can be applied, in a sufficient degree, and that the proposed modification in the direction of a balloon can be produced, another difficulty must be encountered. It arises from the frail materials of which a balloon is composed. No one probably supposes, that it will ever be possible to urge a balloon *against* the wind, or in nautical language, in the wind's eye. It is not probable, that sufficient power can be produced by any impulse upon the atmosphere, to effect this object. All that will probably ever be attempted, is to steer the balloon *upon the wind*, as ships are for the most part, actually sailed. But the question arises, will the feeble envelope that imprisons the hydrogen gas, withstand the force of the atmosphere, when the balloon takes such a direction, that it forms a considerable angle, say 90° with the direction of the wind. With gentle breezes, there might be no danger, but how would it prove in the wild gyrations of a tempest, and in the appalling vicissitudes of thunder and lightning, snow and hail, and cold, and whirlwind and tornado, in which the navigator of the air, as well as of the ocean, may be occasionally involved? Would not there be great danger that a rupture, in the gas-container, would let out the hydrogen, and let in the atmosphere, as the sea rushes into a wounded ship, and in both cases sinking would be inevitable. This difficulty, in the case of the balloon, would indeed be much diminished, by using stout materials, even canvass;* but if the balloon were large, (and no other would answer,) the weight would become enormous. Still, the question is not, what is the absolute weight of a balloon, but what is the relative weight of the whole machine, when inflated, compared with an equal volume of the atmosphere; and consequently, the heavier the envelope, the larger must be the size, in order to produce a given buoyancy.

* Say for the outside for strength, and a more delicate lining, to retain the gas within.

Before the wind, and with a wind whose continuance could be counted upon, a balloon would be a superb machine for travelling; and within the tropics the trade winds might afford that certainty. Were the aerial navigator always to sail over land, his chance of safety would be much increased, for he possesses one resource of which the sailor is destitute. He can, with ease, and at pleasure, descend to *terra firma*, and his anchors will enable him to bring up where he pleases;—but if the sailor descends, he rises not again,—all the waves and the billows pass over him, and his place is found no more. The aerial navigator can also *ascend* at pleasure, either by throwing out ballast, or by creating and injecting more gas, a thing not impracticable, even in the midst of aerial flights; thus he can avoid trees, buildings, mountains and peaks—the rocks (not however hidden ones) of the atmosphere,—and as they would be, at least in the day time, in view, he need not wait for the roaring and dashing of the breakers, to announce to him his danger. Piracy and robbery he need not fear, for were he to encounter other aerial vehicles, they might hail him with the trumpet, but they would have no artillery, and *grappling* and *boarding* would be out of the question. Musket balls might indeed wound the æronauts, or perforate the sides of their vessels and let out the gas.

After all, can balloons, supposing that they can be steered at pleasure, be used as vehicles for travelling? If their bulk and expense, compared with the amount of persons and of freight which they could transport, would be too great for common purposes, they might still be useful on particular occasions. They might, as heretofore, convey intelligence, or an important individual into, or from, a besieged place;—they might ascertain the position of armies, as at Fleurus and Jemappe;—they might aid, as was done by Gay-Lussac and others, in observing the electricity, magnetism, composition, weight, impurities, and refractive and sonorous power of the atmosphere, and the phenomena of its clouds and storms and tempests;—but could they be used for actual evry day travelling of business or pleasure?

This is the question which Mr. Genet proposes to resolve. In admitting his discussions into our columns, we only give him *fair play*. Let him be heard, although he may utter some things new and strange. He who was the companion, pupil and friend of many of the great men both in France and England, who during the latter part of the late century illuminated the world with the most splendid discoveries, directed and rectified by the severest logic of science, and who himself exhibits a vigorous and cultivated intellect, ought not to be dismissed with a sneer.

It is easy to ridicule balloons, and pert and brilliant things may be said about them, with little trouble, and with as little merit. Horace, in a beautiful ode to the safety of his friend Virgil, about to sail for Athens, inveighs against the temerity of that man, who first presumed to tempt the gods, by venturing to sea. What, (possessed of as much poetry and no more of philosophy,) would he have said, had he lived in our days, and could he have seen Capt. Hastings, or Lord Cochrane, dashing through the Mediteranean in a steam boat of war ;—a frail machine composed of the most combustible materials—urged forward by a fierce internal heat, groaning like the fires of Ætna, imprisoning a power more tremendous than the winds, but defying both winds and waves, and bearing along the thunders and the bolts of Jove ! He would have cursed the audacious adventurers by all his gods ! Could he have seen balloons actually rising into the atmosphere, till they became invisible—transporting the æronauts " swift as the winds along," or anchored aloft for months, and used by a corps of æronautic engineers,* and then descending in safety to the earth, he would have thought them little less than the gods, and would probably have changed his curse into an ode of deification.

If science then has often achieved what would have been thought incredible, let not her efforts be stinted for want of those means which an opulent country can easily supply. There is a wide difference between attempting that which is absurd, and that which is only very difficult. Perpetual motion is an absurdity, but it involves no absurdity to attempt to rise into the atmosphere, or to attempt to steer our way when we have arrived there. The latter is confessedly very difficult, perhaps impracticable, but it is not absurd.

Were the bulk of a balloon small, and the wings that might be used large, it would resemble a bird ; but as the contrary is the fact, the difficulty is enhanced in a correspondent degree. But let the experiment be made. The thousands which,

* As was done in France during the revolution, when a regular school of æronauts under a Colonel of that department, was established, for warlike purposes ; a certain number of pupils being daily exercised aloft in the atmosphere.

in great cities, are squandered upon amusements, would be much better appropriated in this manuer, and when it is fully ascertained, that balloons cannot be navigated by any practicable means, then let them remain as now, a brilliant and imposing spectacle, auxiliary to amusement, war and philosophy, but the sport of the careering winds and tempests of heaven. We conclude by wishing Mr. Genet ample suceess. Failure will involve no disgrace, but success would add another brilliant leaf to the book of discovery.

ART. XXI.—*Review of the Principia of Newton.*

(Continued from p. 35.)

THE action and laws of force in respect to particular curves, such as have the most direct application to astronomy, having been developed by our author in the preceding sections of his work, we come now to the more general principles of rectilineal and trajectory motion. The first and most simple cases comprehended in section 7, are those of bodies descending in a rectilinear direction to the centre of force, by virtue of any intensity, or law of force. To shew the analogy between this kind of motion, and that of a body descending in a curve, according to the laws of force previously investigated, it was only necessary to conceive the projectile motion to be diminished *sine limite*; the orbit then becomes a right line. The relations of space, time and velocity, of a body urged only by a force to the centre, is but one case of the general problem of a body moving in an orbit, or trajectory. This method of deriving the circumstances of rectilinear motion by its analogy to, and connection with trajectory motion, though not the most general in the physico-mathematical sciences, is certainly the best calculated to show the harmony and connection of the general and philosophical principles; the 7th section is therefore principally employed in developing that connection from principles established in the preceding part of the work. These being necessary *precognita* for the due understanding of this part of the Principia, and not having been particularly referred to in our review as being corollaries, or results of the more general principles, it may be well to exhibit to the reader in this place.

1. If a body be acted on by a centripetal force only, which is constant, it will descend with an accelerated motion in a right line toward the centre of force, as is the case of bodies falling near the surface of the earth.

2. The force is proportional to the increments of the velocity, or the accelerations of the motion are proportional to their cause, viz. the intensity of the force.

3. If the body have a motion of projection in any oblique direction, with the line of its rectilinear descent, and if the centripetal force vary in the inverse duplicate ratio of the distance, it will move in some one of the conic sections, having the centre of force in the focus of the figure.

4. The species of the conic section, and the particular curve of that species, which the body will describe, depend on the ratio of centripetal force to the velocity of projection.

5. If the force be uniform and constant, the velocity necessary for a movement in any of the conic sections, will be that which is due to $\frac{1}{4}$ of the latus rectum, when the projection is perpendicular to the direction of the centripetal force. When, therefore, that height is $\frac{1}{2}$ of the distance of the point of projection from the center of force, it will move in a circle, whose center will be the seat of force: If that height be less, it will move in an ellipsis within the circle, and the focus of the ellipsis farthest from the point of projection will be the seat of the force. It will continue to be an ellipsis until the velocity of projection, and consequently $\frac{1}{4}$ of the *latus rectum* $=0$; it will then become a straight line.

6. If the height due to the velocity be greater than that necessary for a circle, it will still move in an ellipse, having the centre of force in its nearest focus, until that height reach the limit of the ratio of 2 to 1, of that of the circle, in which case the body will move in a parabola; and if the velocity of projection be still greater, it will move in an hyperbola.

7. The periodical times of bodies moving in all ellipses, whose major axes are equal, are the same, and equal to that of a body moving in a circle, whose diameter is that major axis.

8. The periodical times of bodies moving about the same center, but in different elliptical or circular orbits, will be in the sesquiplicate ratio of their major axes, or diameters.

9. The velocities of bodies moving about the same center, in similar orbits, are in the inverse sub-duplicate ratio of their distances in similar positions of their orbits.

10. The velocity of a body moving in a parabola, is to the velocity moving in a circle at the same distance in the ratio of $\sqrt{2}$ to 1, and is equal to the velocity of a body moving in a circle at half the distance of the body in the parabola.

11. The velocity of a body moving in a parabola at its vertex, is such as would be acquired by a body falling an infinite height, by a force varying as it has been found in respect to gravitation, viz. in the inverse duplicate ratio of the distance.

12. The velocity of a body revolving in any conic section is to the velocity of a body revolving in a circle at the same distance, as a mean proportional between that common distance, and half the principal *latus rectum* of the section to the perpendicular let fall from the common focus upon the tangent of the section.

From the foregoing principles, and those previously established, it follows that bodies moving in any of the conic sections, or in a straight line, by virtue of a centripetal force, will have their positions after any lapse of time, in the same ordinate, perpendicular to the common axis; for the areas cut off by a line drawn from the focus to the perimeter of any of the conic sections, must be proportional to the times of their description, and in different sections, having one common focus and vertex, they will be to one another as their ordinates: but those conditions cannot be fulfilled, when the times are the same, except the line be drawn to the points of intersection of one common ordinate, with the perimetres of the different sections.

To show the place of a body moving in a rectilinear direction towards the centre of force, it is only necessary to find the corresponding position of it at any time moving in an ellipsis, parabola, or hyperbola, and letting fall a perpendicular ordinate on the axis of the curve. This, however, will obtain only in the cases most important, viz. when the force is such as to cause the bodies to move in conic sections, about the focus, or about the centre of the figure. The simultaneous positions of a body moving in orbicular or trajectory motion, in a curve designated by the ordinate on the axis of the curve, furnish a very ready method of ascertaining the times of bodies descending to the centre of force, supposing this (the force) to vary as before, and to be in the focus; for that time will be the same as that of a semi-revolution in a circle, whose radius is half the distance fallen through. The relation, however, of any other space fallen through, to any other time, must be determined by measuring an area corresponding to the time by a line drawn from the extremity of the diameter of the circle, or infinitely diminished ellipse to the extremity of the circular, parabolic, or hyperbolic ordinate. The area will express the times, and the intercepted part of the diameter, or the abscissa will represent the space passed over. Our author has solved this by similar and analogous principles of motion, which had been developed in the preceding sections of his work. The velocity of a falling body under the influence of a force varying as had been before investigated, has been determined with great ingenuity on the same principles, so as to form a well compacted system of rectilinear and curvilinear motion, the parts of which are mutually dependent on one another, and all founded, as is the mathematics generally, on the most simple elementary principles. The Principia has derived its celebrity from the dignity of its subjects, the almost miraculous profundity of its investigations, and its complete development of the great operative powers of nature. These great points have eclipsed the minor excellencies of the work, viz. its concise and elegant demonstrations, the logical, systematic and perfect arrangement of the principal topics, so as naturally to grow out of each other by an admirably connected chain of arguments, all tending to one object---the establishment of a new, and before the author's time, wholly unknown *system* of philosophy. Newton, in all these particulars, may justly be considered as the Euclid of philosophy, differing from him, who has had the highest reputation for more than 2000 years, in what is very remarkable, that he was the sole inventor of all the great truths he delivered, whereas the other was a compiler, and ingenious systematic manager, of materials furnished at his hand.

But to return now to the subjects under review; the rectilinear motion of bodies, acted on by a force, such as obtains in the natural world, is fully investigated in the 32d and succeeding propositions, and some curious results are from them obtained. One, in particular, is worthy of notice in this place, viz. that a body revolving in a circle, if the velocity with which it revolves, be turned directly contrary to the direction of the centripetal force, it will rise to double the height it first had from the center of force.

We come now to the rectilinear motion of bodies, urged by the only remaining law of force, previously investigated for orbicular motion, which is that of the distance directly, which would cause a body to revolve in an ellipsis whereof the center is the seat of force. A rectilinear descent of a body according to this law, will be found by the same principles as before laid down, to be in a point of the rectangular co-ordinate, but the time and velocity are represented in a very simple and elegant manner by the arc and its right sine, and the space passed over by its versed sine. As all elliptical or curvilinear motion, under the influence of such a force, will be performed in the same time, it follows, that rectilinear descents from all distances, under this law, will be performed in the same or equal times. It may seem to be a subject of mere curiosity, or a work of supererogation in our author, to have investigated the motion of bodies acted on by forces, which are not found to exist in the material creation by themselves; but every one well versed in these subjects, knows, that mixed or complicated forces, produced by the actions of different bodies in the natural world, may be the points of investigation in physico-mathematical researches, and that the resultant or joint effect of these forces may constitute a law of force very different from that of the simple forces; hence the importance of investigating movements according to any law of force. Sensible of the practical results dependent on these apparently speculative problems, and of the connection there ever must be between the movements of bodies in curves, and those which are urged in right lines by a centripetal force, our author, in his 39th proposition, proceeds to the general problem of rectilinear motion, according to any law of force. This proposition in itself is very important, but is rendered much more so by its intimate connection with those which follow in the next section. In this proposition we have, not for the first time, a complete exemplification of the fluxional or differential calculus, independent of its forms, or before it was reduced to any forms or rules as a science: indeed, the investigations of the Principia could never have been made but by one, whatever his genius might have been, who was a perfect master of its principles as well as of all its refinements. We have, also, in this proposition, an instance of a pure analytical solution, which consists in the assumption of the thing to be proved, and proceeds, by analysis from the complex or compound proposition to the investigation of its parts, and demonstrates that those parts, when resolved, are identical with those assumed, and consequently that the assumption or proposition is true.

It is demonstrated in this 39th proposition, that if the space passed over and the force be assumed as co-ordinates, the curve, which is the locus of these, will include an area, which will be as the square of the velocities in different places of the descending body. If the reciprocal of the velocities be made the ordinates of another curve, corresponding to every place of the descending body, this curve will comprehend an area proportional to the times.

If the relation of time and velocity be given, or if the velocity be as any function of the time, and the consideration of the force generating the acceleration be excluded, the relations of space, time, and velocity, may be expressed by different constructions, some of which, and those most common, are not truly mathematical. In these the time is represented by an abscissa of a curve, the velocity by a co-ordinate, which is a given function of the time, or abscissa, and the area included by the co-ordinates and curve, the locus of the co-ordinates, will be the space passed over by the descending body. This mode of showing the relations of time, velocity and distance passed over, is, as before observed, unscientific, for the quantities compared are mathematically incapable of a comparison, except by numbers whereof the units are heterogeneous, being the expressions of lines in one case, and areas or surfaces in another. But these relations may be expressed by a locus, whereof all the quantities may be homogeneous, or right lines only. For this purpose we need only to consider one of the axes of the co-ordinates to represent the time uniformly generated, commencing at the origin of the co-ordinates, or with any initial space, and the space passed over to be represented by a corresponding portion of the other axis, and the velocity will then be truly defined at any point by the trigonometrical tangent, which the tangent to the curve at that point makes with the axis representing the time; for V the velocity is always as $\frac{s}{t}$. The locus of a body acted on by a constant force, such as gravity, will be a parabola in this scheme. The first construction, however, has generally been adopted for the elementary student, and with proper explanations, referring, as Newton always does, to the lines which are the sides or boundaries of the areas, may be divested of incongruity. The 40th proposition of the Principia is the connecting link between the preceding investigations of rectilinear, and those subsequent, of curvilinear motions. The principle to be proved is very important in the theory of central forces, viz. that bodies moving in a curve, or in a straight line towards the centre of force, if their velocities at any one point be equal, at equal distances from the center, their velocities at all other equal distances from the center of force will be equal. The demonstration of this given by our author, is incomparably superior to that of Bernouilli, Simpson, or any others who have vainly attempted to improve on his manner. That of the first, who calculated the velocity from the principles of a body moving down an inclined plane, is puerile and inapplicable to a body moving in free space. These, and many other mathematicians of eminence in the solution of this problem, pretend to no inventions of their own; they only use those of our author, and think it worthy of praise, if they can exhibit his work in a different form, in some instances better accommodated to the capacities of readers, but in general much deteriorated, by the exclusion of the author's admirable and concise logic.

The corollary from this proposition, that $\sqrt{P^n - A^n}$ will be an expression of the velocity, P being the utmost height to which a body will rise under any force whose law is $n-1$ of the distance, and A any other altitude, is one of those concise and profound inferences, which more than the propositions themselves, render our author's work very difficult of comprehension to most readers. This result forms a considerable problem of his own fluxional, or differential calculus. By this science proceeding according to the *precepta*, or prescribed rules of the Principia, the work, it is certain, may be in many cases abridged, and much facilitated in complicated cases. We will endeavour to afford some examples of this *facilitation*. In uniform motion, the velocity is uniform, and its fluxion is nothing, as that is a constant quantity, but the space passed over is variable, and its fluxion is constant, and is expressed by the velocity. This will be the case, when there is no action or force to alter the velocity; but if a constant force, or one which produces constant accelerations or retardations of the motion of the body, be supposed to act on it, it is evident that these will be in proportion to the intensity of that force, as the effect must necessarily be in proportion to the cause which produces it, and by the 2d law of motion, the variation of the velocity, or the fluxion of the velocity, must therefore ever be proportional to the force. Putting, therefore, Φ for the force, and s for the space, v for the velocity, and t for the time, in uniform motion, we shall have $s \propto t\, v$, and in any kind of motion, because $\dot{s}$ and $\dot{t}$ are from their very definitions indefinitely small elements, uniformly generated, we shall have $\dot{s} \propto v\, \dot{t}$ and $v \propto \frac{\dot{s}}{\dot{t}}$. but since Φ is $\propto \dot{v}$ and $v \propto \frac{\dot{s}}{\dot{t}}$; therefore $\Phi \propto \frac{\ddot{s}}{\dot{t}}$, if we suppose $\dot{t}$ constant, but when $\dot{t}$ varies $\Phi \propto \frac{\dot{v}}{\dot{t}}$, if, therefore, instead of $\dot{v}$ we substitute $\frac{\ddot{s}}{\dot{t}}$ we shall have $\Phi \propto \frac{\ddot{s}}{\dot{t}^2}$. This very useful formula has been finely illustrated by D'Alembert, and is identical in all its principles with what had been long before delivered in the 10th Lemma and the 39th proposition of the work now before us. In this proposition, as well as in many others of the Principia, we have examples of the advantage of the author's *metaphysique* of fluxions, or the generation of quantities by motion, in its application to the solution of problems dependent on the laws and variation of motion; since these are identical with the different orders of fluxions, of which the variable motion is susceptible.

Since $\dot{t} = \frac{\dot{s}}{v}$, and $\dot{t} = \frac{\dot{v}}{\Phi}$ therefore $\frac{\dot{s}}{v} = \frac{\dot{v}}{\Phi}$, and $\Phi\, \dot{s} = v\dot{v}$, and $\Phi\, s = \frac{1}{2}\, vv$. This is precisely the first case of the 39th proposition.

If x be put for the distance of a body from the centre of force, while ascending or descending, and a be the distance from which it descended, or the utmost height to which it would rise, by a force acting according to any law of distance, or according to any function of the distance, whose exponent is n, then by hypothesis x will be the ordinate of the curve, whose area, by the 39th proposition, represents the square of the velocity, and the fluxion of this area is $x^n \dot{x}$ whose fluent $= \frac{x^{n+1}}{n+1}$ or $v^2 \propto x^{n+1}$, $n+1$ being constant, which, when $x = a$ becomes a^{n+1} and therefore the velocity at any variable distance will be truly defined by $\sqrt{a^{n+1} - a^{n+1}}$ which, if we substitute $n-1$, as the exponent of the force becomes $\sqrt{\overline{a^n - x^n}}$, which is precisely the expression given by our author in Corol. 2, proposition 40.

If $n=o$ in the former case, or the force be constant, we shall have $v^2 \propto \overline{a-x}$ or as the distance passed over, as in the case of bodies descending by the force of gravity. If $n=-1$ or the force be reciprocally as the distance the curve will be a rectangular hyperbola, and the velocity of the descending body at the center of force will be infinite. In general the curve will be of the parabolic or hyperbolic species.

Art. XXII.—*Remarks on Mr. Quinby's Demonstration of "the Crank Problem," contained in a former volume of this work, and the anonymous reply to it in the last number; with a general view of the subject of the Transmission of power by Machinery; by* E. W. Blake.

In a former volume of the Journal of Science, is a paper by Mr. A. B. Quinby of New-York, on the subject of Crank Motion; in which he attempts to prove, that in a steam engine, in which the rectilinear motion is converted to rotary by means of a crank, all the power which is exerted on the piston, tends without loss to the production of rotation in the crank. In the last number he is replied to by an anonymous writer, who points out the insufficiency of Mr. Quinby's reasoning, and attempts to prove the reverse of the proposition.

However insufficient may be the *reasoning* of Mr. Quinby, the point he aims to prove is nevertheless true. The error of his opponent has resulted from not perceiving the distinction between the different senses in which the term *power* is used both by himself and others; and I apprehend that Mr. Quinby's failure to prove his point has resulted from the same cause.

Power, in the most abstract sense of the term, can have but one definition; and if the term were always used in that sense, it would not be liable to misapprehension. Nor would the term be likely to be misunderstood, could we in the *measurement* of power always refer it to the same standard. But in measuring power, we are under the necessity of referring it to different standards, according to the nature of the effect produced by it, and according to the purpose for which the estimate is made; and thereby we obtain different quantities, which are unlike in their nature and incapable of comparison.

Power is always measured by its *effects*. It can be measured in no other way, for by its effects only do we discover its existence. When therefore the effects produced by two powers are so dissimilar in their nature as to be incapable of being referred to the same standard of measurement, the powers producing them evidently cannot be compared.

A certain power will exert a pressure of ten pounds.

Another will drive a body against a given resistance ten feet.

Another will elevate water one foot at the rate of one gallon per minute.

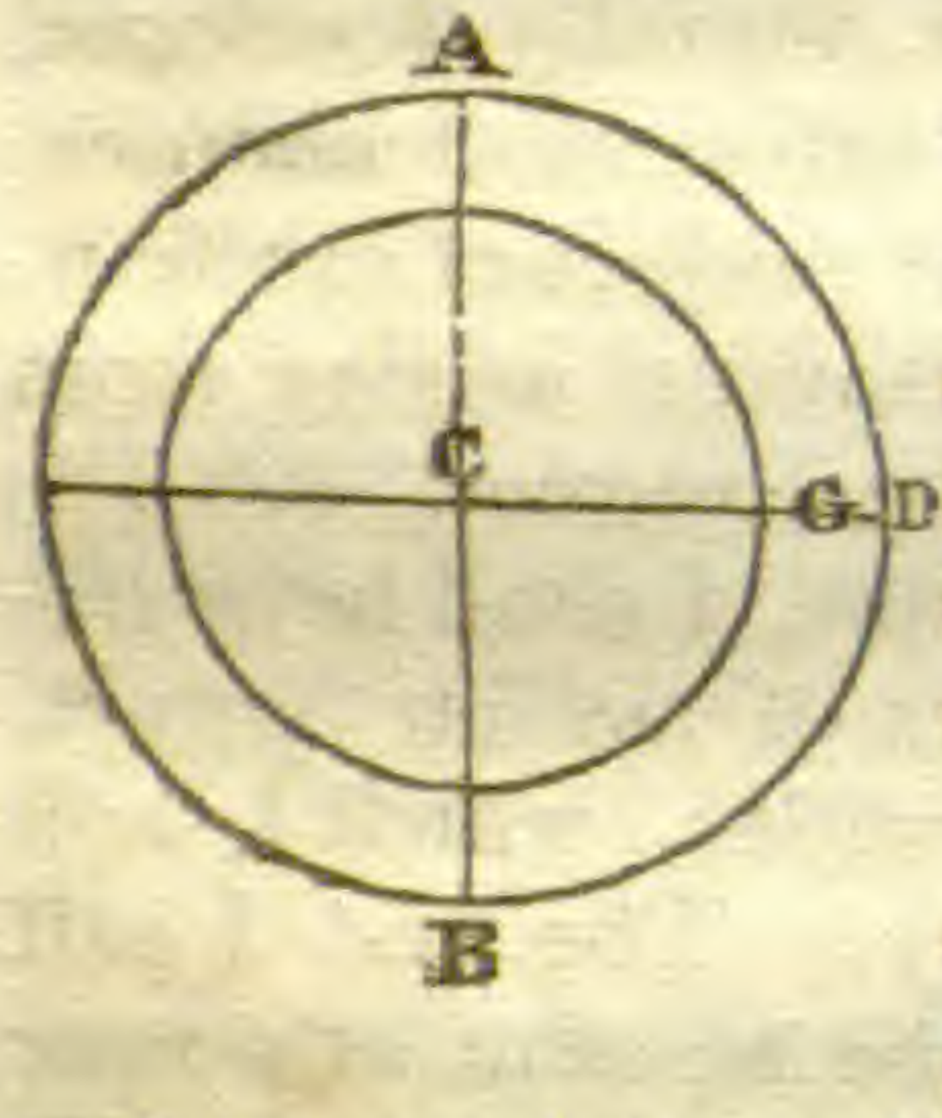
The powers necessary to produce these several effects are definite, and may be definitely measured by referring them respectively to their proper standards. But even after this is done, no one can say, that either of them is equal to, or by how much it is greater or less than, another, because they are dissimilar in their nature. The first consists of one attribute only, like linear measure. The second, of two, like superficial measure. The third, of three, like solid measure. To say, therefore, that one of them is greater or less than another, would be as absurd as to say, that a mile is greater than a square foot, or that a square foot is less than a cubic inch.

A clear apprehension of these distinctions is indispensable to a correct understanding of the subject of mechanical power; and it is to be regretted that they are not fully developed and insisted on in every elementary treatise of mechanics. So far as I have been conversant with the subject of mechanics, I have noticed more errors and disputes arising from misapprehensions here, than from all other sources.

In the few remarks which I have to make, I shall have occasion to use the term *power*, and others of similar import, chiefly with reference to two sorts of quantities, which I shall distinguish by a difference of phraseology. The first I call *Degree of Power*. By this is meant the amount of power by the standard measure of gravity, which is exerted on a fixed body at any point of time, or on a moving body at any point of its motion. The second I call *Quantity of Power*. By this is meant the *degree of power* multiplied by the *distance through which it is exerted*. *Quantity of power*, therefore, being the product of a multiplication, differs from *degree of power* as superficial differs from linear measure.

This distinction being clearly apprehended, it is manifest that no *degree* of power, however great, can ever constitute any quantity of power, however small. It is therefore evident that one cannot be compared with the other ; and that no course of reasoning with regard to one only, can justify any conclusion with regard to the other. Now here lies the cause of Mr. Quinby's failure to prove the point at which he aimed. He instituted a course of reasoning which involves necessarily only power in *degree ;* but in the conclusion, he draws an inference, necessarily involving power in *quantity.* Of precisely the same kind is the error of his opponent in the last number.

The effect of a steam engine is always motion against a resistance. Such an effect manifestly involves two attributes, viz. *degree of resistance* and *distance*, and is therefore in *quantity :* consequently, it can be compared with the power which produces it, only in quantity. We cannot therefore determine, whether, in a steam engine, the power exerted on the piston produces its due effect on the crank, by instituting a comparison between the degree of force exerted on the piston, and the mean tendency to rotation produced thereby, in the crank : for such a comparison would be in degree only ; and would leave entirely out of the account, the respective distances through which each moves ; which are as important constituents, both of the power and effect, as is the degree of force.



Now to establish the truth of Mr. Quinby's theorem, we have only to show that the quantity of power exerted on the crank, which effectually tends to the production of rotation, is equal to the quantity of power exerted by the steam on the piston.

To do this, let us suppose, with Mr. Quinby and his opponent, that the pitman, or as Mr. Quinby, perhaps with more propriety, calls it, the shackle bar, is always perpendicular ; and let the figure be constructed as by Mr. Quinby, viz. in such a manner that the radius of the interior circle shall be a third proportional to the quadrantal arc and radius of the exterior circle; the exterior circle representing the sweep of the crank.

By the construction arc AD : CD : : CD : CG. Therefore 2 AD : CD : : 2 CD : CG. But 2 AD = arc ADB, and 2 CD = AB. Consequently arc ADB : CD : : AB : CG. Hence arc ADB × CG = CD × AB. But it appears from a part of Mr. Quinby's reasoning, the truth of which is admitted by his opponent, that when CD represents the whole degree of force exerted by the shackle bar on the crank, or, which is the same thing, (the shackle bar being perpendicular) the whole degree of force, exerted by the steam on the piston, then CG represents the mean tendency to rotation in the crank which that force produces. Also it is evident, that in producing a semi-rotation of the crank, the piston moves through a distance equal to AB; and during the same time, the crank moves through the semi-circle ADB. But it has been shown that ADB × CG = CD × AB. Therefore the distance passed through by the piston, multiplied by the force by which it is driven, is equal to the distance through which the crank moves, multiplied by the mean force by which it is driven; or in other words, the power and effect are equal in quantity.

We might show with equal clearness, that the quantity of power exerted on the piston, and of direct effect on the crank, are equal in any of the parts of their respective motions, which are simultaneously performed; and also that no difference would obtain in this result, if, instead of remaining perpendicular, the pitman should take its variable position. But as I intend, before I close, to give a general course of reasoning on this subject, which will embrace every possible case, further illustrations of particular cases are unnecessary.

But if we have shown that there is no loss of power *between the piston and the effect*, by the use of the crank, we have *not* shown, nor has Mr. Quinby, that there is none *between the piston and the boiler*, resulting from the crank as a proximate cause. Mr. Quinby, therefore, had the logic of his argument been pure, could not have drawn from it the inferences which he does, viz. that there is no loss of power sustained by the use of the crank, and that the attempts to construct a rotary engine are idle. Before he could do this, he should show that steam, under like circumstances in other respects, will exert the same quantity of power, in proportion to the quantity of steam expended, on a piston which meets with a varying, as on one which meets with a uniform resistance, and also as much power in quantity on a piston, which meets with a resistance varying in the manner and ratio occasioned by the crank, as on one which meets with a resistance that varies in any other manner or ratio. Had I leisure, I think I could exhibit satisfactory reasons why there is a great loss sustained between the piston and the boiler, by the use of the crank. But I must leave this for another opportunity.

The truth above proved with regard to the crank, may be exhibited in a manner entirely satisfactory to my mind, by another course of reasoning, which is of more extensive application; and which shows, that, not only the crank, but every other species of machinery, which is, or may be contrived, to modify power, or direct it to the production of an effect, will transmit that power, if it be all applied, without loss in quantity, except the loss occasioned by friction, resistance of the air, changes in the form of the parts of the machine, resulting from pressure or collision, and other adventitious causes.

It is manifest, from the definition of *power in degree* and *power in quantity*, that mere pressure, or the effort of power in *degree*, can never occasion any *expenditure* of power in *quantity*. Thus the pressure of the steam on the internal surface of the boiler, of the cylinder and of the tubes by which it is conducted, occasions no *expenditure* of its power, because it is exerted in *degree* only: and so in numberless other cases. Now it will be admitted that all power, which is exerted in the direction of the motion occasioned thereby, produces its full and proper effect. And it is equally certain that a power which is exerted in a direction oblique to the direction of the motion which it produces, may be resolved into two components, one of which is in the direction of the motion, and the other perpendicular to it. This last component, being prevented by the construction of the machinery from producing motion, occasions no expenditure of the power. Therefore all the power expended operates directly, and consequently produces its proper effect.

From the preceding remarks, it will appear, that in mechanics, when the term power, and other terms of similar import, are used without qualification, it is of great importance that they should be correctly understood, by considering what species or measure of power the circumstances of the case necessarily imply. Thus, when we say that the power of the steam in an engine, operates to a disadvantage on the crank, if the pitman be oblique to the crank, we mean power in *degree*. But when we say, that this disadvantageous action occasions no loss of power, we mean, power in *quantity*. And when we speak in general terms of the power of the steam engine, we use the word in a still higher sense ; a sense in which it involves three attributes, and could be expressed only by the product of three unlike quantities, viz. the degree of force on the piston, the length of the stroke, and the number of strokes per minute.*

It also appears from the preceding remarks, that if we would compare the power exerted on a machine, with the effect produced by it, we must consider the *nature* of the effect produced, and the *purpose* for which the estimate is made. If the effect be that of confining a body in a fixed position, it is in degree only, and the comparison can be made only in degree. But in machinery in which the effect is produced by motion, the comparison may be made either in degree or in quantity ; according to the purpose for which the estimate is made. If, in such machinery, the object of the comparison be to determine to what extent the degree of power is modified, the estimate will be made in degree only. The lever, and other instruments, commonly called the mechanical powers, are usually constructed for the purpose of modifying in *degree*, an existing power, in order to adapt it to the production of an effect, which is either greater or less in degree, than the power. In making an estimate of the effect of such machinery, therefore, our object is usually accomplished, by comparing the power and effect in *degree* only. But in machinery which is constructed for the purpose of directing a continued power, (as that of water or steam) to the production of an effect, if we would compare the power with the effect, in order to determine whether the power be profitably expended, the comparison must always be made in *quantity*.

If in making such a comparison as that last mentioned, we take for the power, that which is actually applied to the machine in the given case, as Mr. Quinby does in his demon-

* Mr. Quinby, in one of his communications to this work, gives a "Definition of the power of the steam engine," which involves only the first of these three attributes. This evidently can be no test of the effect which the engine produces, except when the other attributes are given.

stration, we shall always find the power and effect equal in quantity, whatever be the machinery by which it is transmitted, with the exception of the difference occasioned by the adventitious causes above mentioned. But if we take as the power, that which might be, or ought to be applied in the given case, and find the effect less than the power, by a quantity, greater than is attributable to friction, &c. the result proves a failure, not in the *transmission*, but in the *application* of the power.

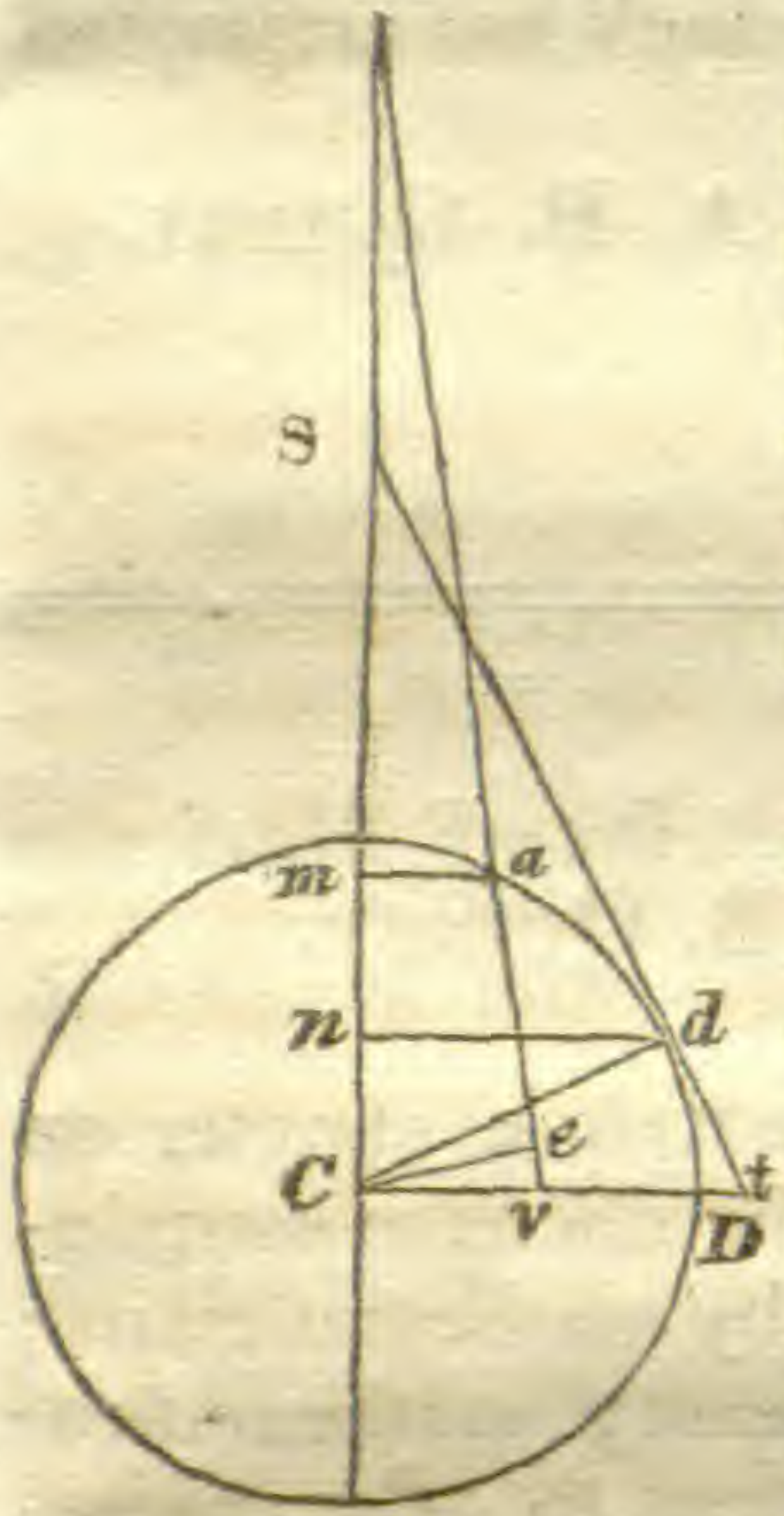
ART. XXIII.—*Reply of Mr. Quinby, to the writer of the Examination of his principle of Crank Motion.*

TO THE EDITOR.

SIR,—In the recent number of your Journal of Science and Arts, I observe what is entitled "Examination of Mr. Quinby's principle of Crank Motion." In this "Examination," the writer first undertakes to prove that if the shackle-bar moved parallel to the piston rod, "*there would be a loss of power.*" His reasoning on this subject requires but one remark. If the principle he asserts were true, a machine might be made which would be a real "perpetual motion," and which would supersede all other machines in the world, and blot out the glory of the inventors of the ship, and the steam engine, forever ;—for since P in descending, raises the *greater* weight W, (see his diagram,) through an *equal vertical space*, it is plain that with but *one pound* of power, we could drive both the hydraulic works at Marly and the steam engines in Cornwall ! !

The writer next undertakes to demonstrate, that in my reasoning to prove that the principle assumed by Mr. Ward is incorrect, I committed an oversight, which altogether destroys my "demonstration of the Crank Problem." But on this subject I may remark, that there is no connexion between my strictures on the principle assumed by Mr. Ward, and my "demonstration of the Crank Problem." This I clearly stated in the last paragraph of the strictures I offered.

But the most amusing part of this "Examination" is, where the writer falls into an error much greater than the one he is endeavouring to correct. He informs us that in the proportions *am* : *a*S : : C*c*: CS ; and *dn* : *a*S : : C*e* : C*t*, the third terms are the same ; *ie.* C*c* and C*e* are the same !* It is true that in my reasoning to show that the principle assumed by Mr. Ward is not correct, I did commit an error ; and this I now cheerfully acknowledge, notwithstanding the writer of this "Examination" has failed to prove it. My error was in assuming four lines proportional, which are not proportional. This oversight was pointed out to me last winter by my scientific friend Prof. R. M. Patterson ; and it was my determination to forward a correction for the ensuing number of this Journal. The day after Prof. P. pointed out to me the error, I shewed him the following correction.



Let the fig. represent the crank, and *sd* be a tangent to the circle : it is required to prove that "the effects of the power to produce rotation at the several points of division of the quadrant, are *not* as the perpendiculars respectively from those points to the line of force."

For the effect of P at the point *a*, we have $P \times \frac{Sa}{Sm} \times Ce = P \times \frac{Cv}{Ce} \times Ce = P \times Cv$; and for the effect of P at the point *d*, we have $P \times \frac{Sd}{Sn} \times Cd = P \times \frac{Ct}{Cd} \times Cd = P \times Ct$. And now, if the principle assumed by Mr. Ward were true, then would *am* : *dn* : : C*v* : C*t* ; or *am* : C*v* : : *dn* : C*t* ; but this we can readily perceive is not the case ; for if *a* be supposed to move forward till it be at the point D, it is plain that *am* will be $=Cv$; but *dn* is not $=Ct$; therefore the principle assumed by Mr. Ward is not correct ; and my proposition, which the writer of this "Examination" asserts is incorrect, is true.

I shall now offer a few remarks on the demonstration which the writer of this "Examination" has given of the Crank Prob. He finds the equation $\Phi R = Px$, and from this derives the

* It is possible that the writer of this Examination intended that the terms of these proportions should be taken alternately. In that case his demonstration would be true.

result that there is a loss of " more than *one third* of the power." Let us examine how he derives this result. In the equation $\Phi R = Pc$, he has two variable quantities ; and he supposes one of them to be equal to a *constant* quantity ; and then finds the value of the other by the rule for a simple equation. This method of solving an equation, requires no comment. The equation, however, which he has given, will solve the problem ; and gives the same result as that which I gave in my demonstration ;—for since $\Phi R = Pr$, and this for any position whatever of the crank, it is plain that there can be no loss of power ; for if there be a loss of power, there must be a loss at some point ; but there is not a loss at any point, and therefore there is no loss of power.

It now only remains to be remarked, that in the three things which the writer of this " Examination" has attempted to demonstrate, he has failed in all.

A. B. QUINBY.

March 28, 1827.

ART. XXIV.—*Examination of the doctrine of Maximum Effect of Machines.* By Mr. A. B. QUINBY.

MOST of the works on Mechanics, contain a chapter on the " Maximum effect of Machines." The doctrine contained in this chapter has been long received by mathematicians ; and now forms a part of the course in every mathematical school. I propose. in the paper I am about to offer, to examine this doctrine.

As it would be impracticable to refer to all the works which contain the doctrine in question, I shall confine my examination to the chapters given by three authors, viz. Dr. Gregory, Mr. Whewell, and Prof. Farrar.

In Dr. Gregory's Mechanics, vol. i. p. 320, we have the following proposition.

If R and r be the distances of the power P, and the weight or resistance W from the fulcrum F of a straight lever, (fig. 1. pl. iv.) then will the velocity of the power and of the weight at the end of any time t be $\frac{R^2P - RrW}{R^2P + r^2W} \cdot gt$, and $\frac{RrP - r^2W}{R^2P + r^2W} \cdot gt$, respectively, the weight and inertia of the lever itself not being considered.

If the effort of the power balanced that of the resistance, P would be equal to $\frac{rW}{R}$. Consequently, the difference between this value of P and its actual value, or $P - \frac{r}{R}W$ will be the force which tends to move the lever. And because this power applied to the point A accelerates the masses P and W, the mass to be substituted for W in the point A must be $\frac{r^2}{R^2}W$ (art. 310. cor. 4.) in order that this mass at the distance R may be equally accelerated with the mass W at the distance R. Hence the power $P - \frac{r}{R}W$ will accelerate the quantity of matter $P + \frac{r^2}{R^2}W$; and the accelerating force $F = (P - \frac{r}{R}W) \div (P + \frac{r^2}{R^2}W) = \frac{PR^2 - RrW}{PR^2 + r^2W}$. But (art. 228) $v \propto Ft$ or is $= gtF$; which in this case $= \frac{R^2P - RrW}{R^2P + r^2W} \cdot gt$, the velocity of P. And, because veloc. of P : veloc. of W :: R : r, we have velocity of $W = \frac{r}{R}$ velocity of $P = \frac{r}{R} \times \frac{R^2P - RrW}{R^2P + r^2W} \cdot gt, = \frac{RrP - r^2W}{R^2P + r^2W} \cdot gt.$

Cor. 1. The space described by the power in the time t will be $= \frac{R^2P - RrW}{R^2P + r^2W} \cdot \frac{1}{2}gt^2$; the space described by W in the same time will be $= \frac{RrP - r^2W}{R^2P + r^2W} \cdot \frac{1}{2}gt^2$.

Cor. 2. If R : r :: n : 1, then will the force which accelerates A be $= \frac{Pn^2 - Wn}{Pn^2 + W}$.

Cor. 2. If at the same time the inertia of the moving force P be $= o$, as in muscular action, the force accelerating A will be $= \frac{Pn^2 - Wn}{W}$.

Cor. 3. If the mass moved have no weight, but possesses inertia only, as when a body is moved along a horizontal plane, the force which accelerates A will be $= \frac{Pn^2}{Pn^2+W}$.— And either of these values may be readily introduced into the investigation.

Cor. 4. The work done in the time t, if we retain the original notation, will be $= \frac{RrP-r^2W}{R^2P+r^2W} \cdot gt \times W = \frac{RrPW-r^2W^2}{R^2P+r^2W} \cdot gt.$

Cor. 5. When the work done is to be a maximum, and we wish to know the weight when P is given, we must make the fluxion of the last expression $= o$. Then we shall have

$$rR^3P^2-2r^2R^2PW-r^4W^2=o \text{ ; and } W=P\times \left(\sqrt{\frac{R^4}{r4}+\frac{R^3}{r^3}}-\frac{R^2}{r^2}\right).$$

Cor. 6. If $R : r :: n : 1$, the preceding expression will become $W=P\times(\sqrt{n^4+n^3}-n^2)$.

Cor. 7. When the arms of the lever are equal in length, that is, when $n=1$, then is $W=P\times(\sqrt{2}-1)=.4142$ P, or nearly $\frac{5}{12}$ of the moving force.

Scholium.

If we compare the values of S and v in this proposition, and the first corollary with those in the fourth example, art. 267, which relates to motion on the axis in peritrochio, it will be seen that the expressions correspond exactly. Hence it follows, that when it is required to proportion the power and weight, so as to obtain a maximum effect on the wheel and axle, (the weight of the machinery not being considered,) we may adopt the conclusions of cos. 5 and 6 of this proposition. And in the extreme case, where the wheel and axle becomes a pulley, the expression in cor. 7. may be adopted. The like conclusions may be applied to machines in general, if R and r represent the distances of the impelled and working points from the axis of motion; and if the various kinds of resistance arising from friction, stiffness of ropes, &c. be properly reduced to their equivalents at the working points, so as to be comprehended in the character W for resistance overcome.

Now in this demonstration, Dr. Gregory has proved his proposition; but in the corollaries which he has drawn, and in subsequent parts of his chapter, he has committed the error which I wish to point out.

In cor. 4. he says, The work done in the time t is $=\frac{RrP-r^2W}{R^2P+r^2W}.gt\times W$; but this is the expression for the momentum of W. The expression for the work done is W $\times$ by the space through which it has been moved $=W\times\frac{RrP-r^2W}{R^2P+r^2W}\tfrac{1}{2}gt^2$.*

But it is in his 5th cor. that he commits the error which I wish particularly to point out. He states that, When the work done is to be a maximum, and we wish to know the weight W when P is given, we must make the fluxion of the last expression $=o$. This he does, and obtains $W=P\times\left(\sqrt{\frac{R^4}{r^4}+\frac{R^3}{r^3}}-\frac{R^2}{r^2}\right)$.

This result is true for the case which Dr. Gregory has been considering; but it is plainly not true for any case in practice; for the expression from which it is derived does not embrace the space through which P has moved; but in *all* cases in practice, the space through which P has moved must be embraced, or the formulæ derived will not be true.

There appears to have been a strange misconception, or want of information, in the minds of those mathematicians who have written on this subject. They have all taken P for the measure of the power applied to a machine; but it is plain that the measure of the power applied to *every* machine in practice, is $P\times$ by the space through which it has moved—the space through which it has moved being estimated in the direction in which the force (P) acts.†

Hence, if we wish to obtain the value of W (in relation to P) when the effect of the power applied to any machine in practice is a maximum, we must make the product of the resistance (W) and the space through which it has moved ÷

* It is true that the expression $\frac{RrP-r^2W}{R^2P+r^2W}.gt\times W$ is proportional to the work done; and in considering it a maximum, the result is the same as if the right expression had been used.

† This last clause is necessary to make this definition perfect; for the measure of the power (P) of water acting upon a wheel, is not P multiplied by the part of the circumference through which it acts, but P multiplied by the vertical height through which it acts. It will be shown that Prof. Farrar did not consider this difference in one of his problems.

by the product of the power (P) and the space through which it has moved, a maximum.*

And to do this, if we take the case of the wheel and axle, we shall have

$$\frac{W \times \frac{(RP - rW)r}{R^2P + r^2W} \cdot \frac{1}{2}gt^2}{P \times \frac{(RP - rW)R}{R^2P + r^2W} \cdot \frac{1}{2}gt^2} = \frac{Wr}{PR};$$

which is obviously a *limit*, and not a maximum.†

And this result is true for all possible cases in practice, or that can be conceived and put into practice. We therefore have the conclusion that there is no such thing as a maximum effect of machines; and all the doctrine which has been given and taught on this subject, is inapplicable to *every* machine in use.

I will now recur to the problem which Dr. Gregory has demonstrated. It is this:

A wheel and axle being given, and a power P suspended by a cord over the circumference of a wheel being given, it is required to find the weight W suspended by a cord over the axle, so that the power P, in descending by its own gravity, shall generate in the weight W the greatest momentum in a given time.

I have already remarked that Dr. Gregory's demonstration of this problem is true; but how, I will ask, could *such* a case ever be put into practice? And if *such* a case could be put into practice, where, I would ask, would be the saving of power? To put *such* a case into practice, it would be required to have an indefinite space for the machine to work in; for both P and W would have to move on continuously; and could neither ever be detached. And, again, in the extreme case of the wheel and axle, in which $r = R$, the maximum effect of *such* a machine would be $P \times .4142$;‡ but in all well constructed machines in practice the effect of P is $P \times 1$, very nearly.

* It is the ratio of the effect to the power that must be a maximum.

† The limit is, *ratio* of W to P equals *ratio* of r to R.

If the *ratio* of W to P be *greater* than the *ratio* of r to R, then will the machine move in the contrary direction.

‡ The P in this expression must be considered as representing the product of the power and the space through which it has moved.

Where, then, would be the saving of power? and where would be the maximum effect of *such* a machine?

I shall now consider the chapter given by Prof. Farrar.

In his Mechanics, p. 278, we have the following, art. 396. In proceeding to investigate general expressions for the ratio of the velocities of the impelled and working points of machines, when their performance is a maximum, let

D = the radius of the wheel to which the power is applied; or, which is the same thing, the velocity of the impelled point of the machine;

d = the radius of the axle to which the resistance is applied, or the velocity of the working point of the machine;

p = the moving force applied at the impelled point;

r = the resistance arising solely from the work to be performed;

m = the inertia of the moving power p, or the quantity of matter to which that power must communicate the velocity of the impelled point;

n = the inertia of the resistance, or the quantity of matter to be moved with the velocity of the working point before any work can be performed;

f = the quantity of matter, which, if placed at the working point, would create the same resistance as friction;

i = the quantity of matter, which, if placed at the working point, would oppose the same resistance as the *inertia* of all the parts of the machinery.

Since D and d are the radii of the wheel and axle, we shall have $D : d :: r : \frac{rd}{D}$, a weight equal to that part of the power p which is in equilibrium with the resistance. We have, therefore, $p - \frac{rd}{D}$ as an expression for the effective force of the power; and as D is the distance at which this force is applied, we have

$$pD - rd$$

to represent the force which is employed in giving a rotatory motion to the machine. The resistance which friction opposes to this force will be fd; the moment of the inertia of the power p will be as mD^2; the moment of inertia of the resistance as nd^2; and the moment of inertia of the machinery will be as id^2. Since the moving force is diminished by the resistance of friction, we shall have $pD - rd - fd$ for the moving force; and since the resistance arises from the moment of inertia of the resistance, the moment of inertia of the power, and that of the machinery, it will be as $m\text{D}^2+nd^2+id^2$. But the velocity is proportional to the moving force directly, and to the resistance inversely; therefore, the rotatory velocity will be

$$\frac{p\text{D}-rd-fd}{m\text{D}^2+nd^2+id^2}$$

Now, since the velocities of the impelled and working points are as their distances from the center of motion, or as D and d, we shall obtain these velocities respectively, by multiplying the rotatory velocity by D and d; and as the work performed is equal to the resistance multiplied by the velocity of the working point, we shall have for the velocity of the working point

$$\frac{p\text{D}^2-r\text{D}d-f\text{D}d}{m\text{D}^2+nd^2+id^2};$$

for the velocity of the working point

$$\frac{p\text{D}d-rd^2-fd^2}{m\text{D}^2+nd^2+id^2};$$

and for the work performed

$$\frac{rp\text{D}d-r^2d^2-rfd^2}{m\text{D}^2+nd^2+id^2}$$

In order to obtain absolute measures of the velocities and the work performed, we must consider, that q being the accelerating force, and qg the velocity acquired in a second, we shall have $1:t::qg:v=qgt$; and as the accelerating forces are proportional to the velocity generated by them in equal times, the preceding expressions for the velocities of the impelled and working points, may be substituted for the accelerating force q in the equation $v=qgt$, and we shall obtain, for the absolute velocities of the impelled point

$$\frac{p\text{D}^2-r\text{D}d-f\text{D}d}{m\text{D}^2+nd^2+id^2}gt;$$

for the absolute velocity of the working point

$$\frac{p\text{D}d-rd^2-fd^2}{m\text{D}^2+nd^2+id^2}.gt;$$

and for the work performed

$$\frac{rp\text{D}d-r^2d^2-rfd^2}{m\text{D}^2+nd^2+id^2}.gt$$

This is a maximum when the differential d, being considered as variable, is equal to zero, which gives

$$(p\mathrm{D}-2d(r+f))\,(m\mathrm{D}^2+d^2(n+i)) - 2d(n+i)\,(p\mathrm{D}d-d^2(r+f)) \quad =o,$$

or, by reducing

$$pm\mathrm{D}^3-p\mathrm{D}d^2(n+i)-2dm\mathrm{D}^2(r+f) \quad =o;$$

that is

$$d^2+\frac{2m\mathrm{D}(r+f)d}{p(n+i)}=\frac{m\mathrm{D}^2}{n+i}.$$

Resolving this after the manner of an equation of the second degree, we obtain

$$d=\mathrm{D}\times\frac{\sqrt{m^2(r+f)^2+p^2m(n+i)}-m(r+f)}{p(n+i)}.$$

When $r=o$, we have

$$d=\mathrm{D}\times\frac{\sqrt{m^2f^2+p^2m(n+i)}-mf}{p(n+i)}.$$

This case takes place when the resistance to be overcome exerts a contrary strain on the machine, while it consists merely in the inertia of the impelled body; as in driving a mill-stone, a fly, or in pushing a body along a horizontal plane.

When $f=o$,

$$d=\mathrm{D}\times\frac{\sqrt{m^2r^2+p^2m(n+i)}-mr}{p(n+i)}.$$

This case takes place when the friction is so small that it may be disregarded, which often happens in good wheel work, where the surfaces that touch one another are very small.

When $r=o$ and $f=o$, we have

$$d=\mathrm{D}\times\frac{\sqrt{p^2m(n+i)}}{p(n+i)}=\mathrm{D}\times\sqrt{\frac{p^2m(n+i)}{p^2(n+i)^2}}=\mathrm{D}\times\sqrt{\frac{m}{n+i}}.$$

This case takes place when the circumstances of the two preceding cases are combined.

When $n=o$, we have

$$d=\mathrm{D}\times\frac{\sqrt{m^2(r+f)^2+p^2mi}-m(r+f)}{pi}.$$

This case takes place in the grinding of corn, the sawing of wood, the boring of wooden or iron cylinders, &c. where the quantity of motion communicated to the flour, the saw dust, or the iron filings, is too trifling to be taken into the account.

When $r=o$, $f=o$, and $n=o$, we have $d=\mathrm{D}\times\sqrt{\frac{m}{i}}$.

When $m:n::p:r$, we have

$$d=\mathrm{D}\times\frac{\sqrt{p^2(r+f)^2+p^3(r+i)}-p(r+f)}{p(r+i)}.$$

This case takes place when the inertia of the power and the resistance are proportional to their pressure; as when water, minerals, or any other heavy body, is raised by means of water acting by its weight in the buckets of an overshot wheel.

When in the last case, $i=o$, and $f=o$, we have

$$d=\mathrm{D}\times\frac{\sqrt{p^2(r+f)^2+p^3(r+i)}}{p\,(r+i)^2}-\mathrm{D}=\mathrm{D}\times\sqrt{\frac{p}{r}+1}-\mathrm{D}.$$

This case often takes place, and particularly in pulleys; and making $\mathrm{D}=1$, and $r=1$, we obtain

$$d=\sqrt{p+1}-1;$$

and when $p=1$, and $\mathrm{D}=1$, we have

$$d=\sqrt{\frac{1}{r}+1}-1.$$

The preceding formulæ will be found applicable to almost every case which can occur; and the intelligent engineer will have no difficulty in accommodating them to any unforeseen circumstances.

Now in the art. I have quoted, we have

$$\frac{rp\mathrm{D}d-r^2d^2-rfd^2}{m\mathrm{D}^2+nd^2+id^2}.gt$$

for the work performed; but the work performed is

$$r\times\frac{p\mathrm{D}d-rd^2-fd^2}{m\mathrm{D}^2+nd^2+id^2}.\tfrac{1}{2}gt^2.$$

The expression here given is the expression for the momentum of r; and is the same as that given by Dr. Gregory, in his 4th and 5th cos. The problem, however, which is here considered, is different from the one considered by Dr. Gregory in his 4th and 5th cos. The problem which Prof. Farrar here considered, is this:

A power P and weight W, and a wheel being given, to determine the axle, so that P in descending by its own gravity, shall generate in the weight W the greatest momentum in a given time.

The expression which Prof. Farrar has given solves this problem; but it will not apply to *any* case in practice; for, as has already been shown, in order that it may apply to any case in practice, it must embrace the space through which Φ has moved. To make it do this we must write

$$\frac{r \times \frac{(pD-rd)\,d}{mD^2+nd^2+id^2}.\tfrac{1}{2}gt^2}{p \times \frac{(pD-rd)\,D}{mD^2+nd^2+id^2}.\tfrac{1}{2}gt^2} = \frac{rd}{pD};$$

which, as in the preceding case, is a *limit.* Hence we perceive by this case, as well as by the one which was before considered, that there is no such thing as a maximum effect of machines.

I shall now consider one or two of the demonstrations in the chapter given by Mr. Whewell.

His first problem is

A weight P, acting at a wheel, produces rotation in a mass which moves about an axis passing through the center of gravity; it is required to determine the distance at which P must act, that the angular velocity, generated in a given time, may be the greatest possible.

Here the accelerating force on P is

$$f=\frac{Pa^2g}{Pa^2+MK^2},$$

P acting at a radius a. And the velocity generated in time t, in the circumference at which P acts, is ft. And hence

$$\text{angular veloc.} = \frac{ft}{a} \;\therefore\; \frac{f}{a} = \text{max.}$$

$$\frac{a}{Pa^2+MK^2} = \text{max.} \quad \frac{Pa^2+MK^2}{a} = \text{min.}$$

$$Pa+\frac{MK^2}{a} = \text{min. whence } P-\frac{MK^2}{a^2} = o,$$

$$a=K\sqrt{\frac{M}{P}}.$$

This demonstration is true for the problem which is here considered; but there is no such case in practice; and it is obvious that no such case *can ever occur in practice.* This formula, therefore, is of no value.*

The second problem Mr. Whewell gives is

P raises q by means of a wheel and axle, as in art. 93; the axle being given, to find the wheel, that the time of q ascending through a given space, may be the least possible.

Dem. The accelerating force on q is

$$f=\frac{(Pa-qb)\,gb}{Pa^2+qb^2+MK^2}.$$

And, as this is constant, we have $t=\sqrt{\frac{2S}{f}}$, which will manifestly be least when f is greatest. Therefore, we must have

$$\frac{Pa-qb}{Pa^2+qb^2+MK^2}=\text{max.}$$

If we suppose a to vary, K will also vary in a manner depending on the form of the wheel; but if we suppose M to be small, we have, neglecting it,

$$\frac{Pa-qb}{Pa^2+qb^2}=\text{max.}$$

and differentiating, supposing a variable,

$$P(Pa^2+qb^2)-2Pa(Pa-qb)=o;$$

$$Pa^2-2qab-qb^2=o;$$

$$\therefore a=\frac{qb}{P}\left\{1+\surd(1+\frac{P}{q})\right\}.$$

If P be small compared with q, this will give nearly

$$a=\frac{2qb}{P}+\frac{b}{2}.$$

The weight P must act at a little more than twice the distance at which it would balance q.

This Prob. is different from both of those which we have before considered. The same error, however, is embraced in the expression here given, as has been pointed out in the expression given by Dr. Gregory and Prof. Farrar. To

* The K in this formula is the *radius of gyration.* See Whewell, p. 235. The expression

$$\frac{Pa^2g}{Pa^2+MK^2}$$

is the same as that given by Dr. Gregory in his 3d corollary.

make this expression apply to any case in practice, we must write

$$\frac{q\times\frac{(\mathrm{P}a-qb)gb}{\mathrm{P}a^2+qb^2+\mathrm{MK}^2}.\ \tfrac{1}{2}t^2}{\mathrm{P}\times\frac{\mathrm{P}a-qb)ga}{\mathrm{P}a^2+qb^2+\mathrm{MK}^2}.\ \tfrac{1}{2}t^2}$$

$$=\ \frac{qb}{\mathrm{P}a};$$

which, as before, is a *limit*. Hence we see that in these three cases, when the expression is reduced to a form that will render it applicable to any case in practice, the Prob. does not admit of a maximum.

It may now be remarked that the expression for either case will solve the other two; but I thought it best to exhibit the expression as it is derived by each author; and, in doing this, I hope that the space I have occupied is not unnecessary.

It remains only to remark that all the demonstrations and formulæ given by these three authors, and contained in the chapters which have been referred to, are founded on the same false principle that has been pointed out; and that not one of the demonstrations which these chapters contain, and not one of the formulæ which the authors have derived from them, will apply to *any* case in practice, *or that can be conceived and put into practice.*

Having now concluded my examination, I shall offer a few remarks on the detriment and mischief which the doctrine of "Maximum Effect of Machines" has produced.

We will suppose that some individual has constructed a machine precisely similar to that described by Prof. Farrar, in his chapter, p. 283. His words are, "Let us suppose that we wish to raise two cubic feet of water in a second, [Query. How high does Prof. Farrar mean that these two cubic feet of water shall be raised in the given second?] by means of the power of a stream which affords five cubic feet of water in a second, applied to a wheel and axle, the diameter of the wheel being seven feet. It is required, therefore, to find the diameter which we must give to the axle, in order to obtain a maximum effect. We have obviously $p=5$, and $r=2$, and since $p:r::5:2$, we have $p=\frac{5}{2}r$; but in the above table, $r=10$; hence $p=\frac{5}{2}\,10=25$. Now it appears from the table, that when $p=25$, the diameter of the axle, or d, is o .8708, D being 1; but as D=7, the diameter of the axle must be $7\times 0.8708=6.0956$.*

Now let us obtain the *ratio* of the effect of this machine to the power expended. The measure of the power expended is $25\times 7=175$; and the measure of the effect is $6.0956\times \frac{3.14159}{2}\times 2=19.1539$. Hence the *ratio* of the effect to the power is $19.1539\div 175=.115$. Consequently 100 cubic feet of water applied to such a machine, would raise $11\frac{1}{2}$ cubic feet through the same vertical height—and this is a maximum!

Thus it appears that if a machine were constructed for raising water according to the best principles given by Prof. Farrar; i.e. according to the doctrine of maximum effect of machines, the greatest effect of 100 cubic feet of water applied to such a machine would be $=11\frac{1}{2}$ cubic feet raised through the same vertical height! But there are many machines in practice in which the effect of 100 cubic feet of water is equal to 85 cubic feet raised through the same vertical height. The loss therefore in applying such a machine in practice, would be about $\frac{73\frac{1}{2}}{100}$ of the whole power!

I shall now only add that for the reputation of the gentlemen whose works I have criticised, I have the highest respect. For Prof. Farrar I have a personal esteem.

A. B. QUINBY.

April 2, 1827.

* In this Prob. Prof. Farrar has committed an oversight; for the formula

$$d=D\times\sqrt{\frac{p}{r}+1}-D$$

will not apply to the case here taken; for in the case of water acting upon a wheel, the distance from the center, at which the force (P) acts, (estimated for the direction in which it acts,) is not constant; but in the case from which the formula is derived, the distance from the center, at which the force (P) acts, is constant. The cases are, therefore, radically different: and to make this formula apply to the case of water acting upon a wheel, (disregarding the error it contains,) we must write

$$d=\left(D\times\sqrt{\frac{p}{r}+1}-D\right)\times .6365;$$

the quantity .6365 being the distance of the center of gravity of a semi-circle from its diameter, the radius being 1; and likewise the ratio of the diameter of a circle to its semi-circumference. From this it will be perceived that the result which Prof. Farrar has given in this case is erroneous; and likewise that all the numbers in the tables at page 283 and page 286 are not what he intended them to be.

ART. XXV.—*Remarks on Dr.* HARE'S *Essay on the question, Whether Heat can be ascribed to motion?* BY DENISON OLMSTED, Professor of Mathematics and Natural Philosophy in Yale College.

IT will probably be recollected by the readers of this Journal, that in the 4th volume, published in the year 1822, Dr. Hare communicated to the public an essay on heat, aiming to prove that caloric, or the cause of heat, is a material fluid. The substance of his views on the same subject, is also stated in his notes to Ure's Dictionary under the article caloric. In the "sketch" which I had undertaken to give of recent changes and improvements in the science of chemistry, the publication of which was commenced in the 11th volume of this work, it fell in my way to take notice of this essay of Dr. Hare; but as the concise manner in which, from the nature of my undertaking, I was compelled to remark upon it, appears to have been unsatisfactory to the author,* I feel bound to recur to the subject, and propose in this paper to consider the merits of his essay more at large.

I trust it will be apparent from what is said of Dr. H. in the course of my remarks, (Vol. XI. p. 357, and XII. pp. 11 and 12) that I am not wanting in that respect and deference which his great experience, his able speculations, and his brilliant inventions in the department of chemistry, so justly entitle him to claim, especially from his younger brethren; but still I am compelled to think that the arguments which he has adduced to prove the materiality of heat are not conclusive;—that he has derived consequences from Sir Humphrey Davy's hypothesis which do not legitimately follow from it, and has alleged direct arguments to support his own, which are not altogether satisfactory.

Since, in the "Reply," the author has expressed himself somewhat more concisely than in the original essay, and has no doubt exhibited those arguments upon which he principally relies, it will be proper to take these as constituting the sum of his theory, and to remark upon them accordingly.

"We concur (says Dr. H.) in disapproving of the hypothesis of Sir Humphrey Davy, but because I have met it with arguments upon its own basis, instead of briefly denouncing it, Prof. Olm-

* See Dr. Hare's "Reply," in the last number of this Journal.

sted accuses me, no less than the illustrious author, of polluting chemical science with mechanical reasonings." Reply, p. 51.

"Besides erroneously holding me up as the friend of a method of reasoning of which I am really the antagonist," &c. Ib.

From the manner in which I have spoken of Dr. H. no reader will, I think, understand me as accusing him of *polluting* chemical science, or even of being the *friend* of employing mechanical reasonings in the explanation of chemical phenomena. Yet I cannot but think that he has, in this instance, committed an oversight, both in making Davy's hypothesis wear a much more mechanical aspect than it did originally, and in applying to it mechanical principles which have no bearing on it whatever. For,

1. In the hypothesis, the motions supposed are those which occur between *particles* of matter, and at *insensible* distances. In the refutation, the principles applied are such as belong to those motions which occur between *masses* of matter and at *sensible* distances.

2. The motions contemplated by the hypothesis are either rotatory or vibratory: those supposed in the refutation are rectilinear, and in one continued direction,—for to no other does the law of percussion adduced apply.

3. The refutation takes it for granted that all the particles actually come into collision each upon each; whereas the hypothesis does not warrant the supposition that any two particles ever strike against each other at all. For it is plain that the revolutions of particles round their own axes, do not bring them into collision with each other; nor do the vibrations of the particles make it necessary to suppose that they ever hit each other; for if there be space enough between the particles to permit them to vibrate at all, it is clear that they may vibrate without coming into collision. Finally, if they did impinge against one another, it must be remembered that the motion is *backwards and forwards*, and, therefore, this is not a case to which the law of percussion, as adduced by Dr. Hare, applies.

I cannot but think, therefore, that Dr. Hare has refuted a consequence not of Sir Humphrey Davy's, but of his own creating.

The Doctor proceeds:

"The criticism of Prof. Olmsted would convey to any person who had not read my essay, an impression, that I had been so dull as to consider a disproval of the hypothesis of Sir H. Davy, as establishing that which I have myself espoused, and that I had advanced no direct arguments in favor of the materiality of heat, although to such arguments the latter part of the essay is devoted. I beg leave here to quote the reasoning, as I am still of opinion that it is unanswerable, notwithstanding the unaccountable neglect with which it has been treated by the professor."

The reasoning which the author thinks unanswerable, is then brought forward, and is as follows:

"We see the same matter, at different times, rendered self-attractive or self-repellent; now cohering in the solid form with great tenacity—and now flying apart with explosive violence, in the state of vapour. Hence the existence in nature of two opposite kinds of reaction between particles, is self-evident. There can be no property without matter, in which it may be inherent. Nothing can have no property. The question then is, whether these opposite properties can belong to the same particles. Is it not evident that the same particles cannot, at the same time, be self-repellent and self-attractive. Suppose them to be so—one of the two properties must predominate; and in that case we should not perceive the existence of the other. It would be useless, and the particles would, in effect, possess the predominant property alone, whether attraction or repulsion. If the properties were equal in power, they would annihilate each other, and the matter would be as if void of either property. There must, therefore, be a matter in which the self-repellent power resides, as well as matter in which attraction resides." Reply, p. 52.

I have found a difficulty in fully understanding the import of this passage. Does Dr. Hare maintain that the *attraction* which bodies exert, resides in a kind of matter extrinsic to the bodies themselves? Is the affinity of muriatic acid for lime, in his opinion, derived from the agencies of any *attracting* fluid, distinct from either of those bodies? And extending the same views to gravitation, does he hold that bodies fall towards the earth in consequence of an attracting fluid which brings them down? Ever since the doctrine of the "Elements and Vortices" of Descartes was exploded, and that of the "subtile ether," (which Newton barely allowed a place in the form of a query in a corner of the appendix to his Optics,) was discarded, it has been the prevailing idea among philosophers, that we know nothing of the cause of attraction; and I have met with no late writer who has taken it for granted that there is matter in which attraction resides, distinct from the bodies themselves, which exert this influence on each other. But if Dr. Hare is not thus to be understood,—if he do not mean to assert such a doctrine, then why does he conceive it necessary to suppose a fluid upon which the phenomena of repulsion depend,—" in which the self repellent power resides"—distinct from the bodies themselves, which exhibit such repulsion? Moreover, if caloric be identical with the principle of repulsion, or be the repellent principle itself, how will Dr. H. explain the fact that caloric sometimes increases the *attraction* of bodies for each other? In respect to *solids*, he might argue that repulsion operates in first overcoming the cohesion of the particles for each other, and then leaving them at liberty to enter into combination;* but what would he say of the fact that the attraction of two *gases*, when there is no cohesion, is sometimes increased by heat? I am still inclined to think, that it will be found very difficult to prove, that the phenomena of repulsion depend on the mechanical agencies of a fluid, or that that fluid is caloric.

"In support of my opinion, (continues the Doctor) I also cited the radiation of heat in vacuo, agreeably to an experiment of Sir H. Davy himself, in which a thermometer in the focus of one mirror, is influenced by a hot body in the focus of another mirror, the whole being within an exhausted receiver. I will thank Professor Olmsted to explain how heat can be transmitted under such circumstances, even with more ease than in pleno, if the cause of it be not material." p. 52.

All that can be inferred from the radiation of heat in vacuo is, that the radiation is not dependent on the presence of air. It may overthrow the doctrine of Mr. Leslie that, in radiation, heat is transmitted by aerial pulsations; but I cannot see how the fact that heat is not dependent on air for its communications, proves that it is a material substance; nor does there appear to be any more difficulty in conceiving why a heated body should communicate its influence to another body without the aid of air, than why the sun should communicate his attractive influence to Saturn or Uranus, without the aid of such a medium. I cannot tell why a heated body should act through a vaçuum, nor can Dr. Hare tell why it should act through air. We must, I think, confess our ignorance of the *modus operandi*, both of attraction and repulsion. The Doctor proceeds:

* In such case, does the repulsion *cease* at the moment when the attraction begins, or is it only *overcome* by the attraction, when this has opportunity to act without the impediment of cohesion? If the latter be true, (as I had supposed,) where is the difficulty in supposing these two forces to act simultaneously?

"'The reasoning in my essay, which Professor Olmsted has overlooked, is as follows:—As, in order for one body or set of bodies in motion, to resist another body or set of bodies in the same state, the velocity must be as much greater as the weight may be less, it is inconceivable that the particles of steam should, by any force arising from their motion, impart to the piston of a steam-engine the wonted power: or that the particles of air should prevent a column of mercury, almost infinitely heavier, from entering any space in which they may be included by beating it out of the theatre of their vibratory and rotatory movements," &c. See Reply, p. 53.

Has not Dr. Hare plainly fallen into a mistake here? It evidently is not *heat* which moves the piston of a steam-engine, but it is the elastic force of steam. "But, (it may be asked,) is not that elasticity caused by heat?" True; but the effect is not the same thing with the cause. It is difficult to see why heat should impart such wonderful power to steam, nor does our supposing it to be a material fluid diminish this difficulty. Has not the Doctor committed a similar mistake, in understanding Sir Humphrey Davy to assert, that *heat is motion*, whereas his doctrine is that motion is the *cause* of heat. The words are as follows: "The immediate cause of the phenomena of heat, then, is motion." (Ure's Dictionary, Hare's Edition, Art. Caloric.)

Finally, I beg leave to repeat that, in my view, our reasonings on physical subjects must stop when we arrive at one of those principles denominated *ultimate agents*, namely, attraction, heat, light, electricity and magnetism; that all attempts to ascertain the nature of these agents, have hitherto proved unsuccessful; and that, in the present state of our knowledge, we have no means of determining, whether they severally depend on the operations of peculiar material fluids or not. And though, to avoid circumlocution, it may be convenient to speak of these agents as fluids, yet such a use of the term ought not to be understood as conveying any opinion respecting their specific nature.

Art. XXVI.—*Meteorological Table, extracted from a Meteorological Journal of Observations, made from the thirtieth day of April,* 1826, *to the first day of May,* 1827, *at Fayetteville, Newfane, Vermont, in latitude* 42° 58′ *North, and longitude* 4° 20′ *East from Washington.* By Martin Field.

1826 AND 1827.	THERMOMETER.												WEATHER.				WINDS.								MISCELLANEOUS.			
					Maximum of temp.			Minimum of temperature.					No. of days of diff. variations.															
MONTHS.	*Mean temp. at sunrise.*	*Mean temp. at 2 o'clock, P. M.*	*Mean temp. at 9 o'clock, P. M.*	*Aggreg. of mean temp. each mon.*	*Day.*	*Hour.*	*Highest d.*	*Day.*	*Hour.*	*Lowest de.*	*Range of therm.*		*Clear.*	*Cloudy.*	*Rainy.*	*Sno & hail.*	N.	N. E.	E.	S. E.	S.	S. W.	W.	N. W.	*Inches of water in rain, snow and hail.*	*Inches of snow and hail.*	*Lightning and thunder.*	*Aurora Borealis.*
May	52.3	72.8	60.5	61.8	15	2 PM	87	1	5 AM	33	54		26	5	1		3	1	1	1		11	5	9	.9		2	2
June	60.7	75.7	65.6	67.3	4	1 "	89	14	4 "	43	46		19	11	6			2	3	2	5	5	8	5	5.9		2	
July	63.7	78.–	68.3	70.–	13	3 "	92	28	4 "	56	36		23	8	3		4	4	2	1	4	5	6	5	3.6		3	
August	62.–	76.4	66.9	68.4	19	2 "	89	10	4 "	52	37		16	15	8		1	6	3	6	6		4	5	8.2		5	1
September	57.2	69.9	60.9	62.7	5	2 30′	82	17	5 "	36	46		20	10	9			3	1	2	10	3	3	8	5.3		6	1
October	42.2	58.5	45.7	48.8	7	1 "	75	28	6 "	22	53		25	6	4			4		1	5	2	1	18	2.4		1	3
November	31.8	42.8	34.8	36.5	2	2 "	60	22	7 "	20	40		20	10	8	1	2	2		1	6	7	4	8	4.7	1		2
December	22.6	31.6	25.6	26.6	17	1 30′	52	29	7 "	–8	60		23	8	2	4	1	3	1	1	4	6	4	11	4.8	23		–
January	14.5	23.2	15.8	17.8	28	2 "	42	21	7 "	-14	56		15	16		11	5	2	1	2	4	2	4	11	7.	44		3
February	16.5	31.4	21.7	23.2	28	1 "	48	12	7 "	-10	58		18	10	1	6	1				5	6	6	10	4.–	20		2
March	24.8	41.5	29.6	32.	27	3 "	63	4	6 "	–4	67		24	7	3	3	1	3	2	2	3	6	7	7	3.6	8		–
April	40.6	56.3	45.6	47.5	11	2 "	68	2	5 "	27	41		21	9	6	1	1	2		2	4	5	5	11	7.8	1	2	3
Aggr. temp.	40.7	54.8	45.–	46.8			Recapitulation.						250	115	51	2[illegible]	19	32	14	21	56	58	57	108	58.2	97	21	17

REMARKS.

The thermometer, from which the foregoing observations were made, was suspended under a screen, upon the north side of a building, about seven feet from the earth, so that it was not affected by the direct or reflected rays of the sun.

The quantity of snow and hail was ascertained by a snow gage, and at the end of each storm a portion of it was dissolved and the water measured in a rain gage. We believe that a much greater quantity of water, in rain, hail, and snow, has fallen, in this section of the country, than is usual, within the year past; but we are unable to ascertain the fact, for we know of no accurate meteorological journal, which has been kept in this vicinity for years past, to which we can resort for information.

It will be found by the foregoing table that the highest temperature within the year was 92° above, and the lowest 14° below 0, making the difference of 106°.

The mean temperature of summer was	68°.3
,, ,, of winter ,,	22.8
Difference,	45.5

This difference is much greater than is often found in lower latitudes. M. J. De Wallestein, from his observations made at Washington, D. C. in 1823-4, found the difference of temperature between summer and winter at that place, to be only 19°.6. (Vide Journal of Science, &c. Vol. IX. page 394.)

It will, moreover, be found from the foregoing table that the least difference of temperature was between the months of July and August, and the greatest between March and April, and that the mean temperature of the month of April was nigh that of the year.

By comparing some of the observations made in the foregoing table with those made at Fort Crawford, and contained in Dr. Lovell's meteorological tables published in the Journal of Science, Vol. XII. p 152 and 153, we have the following result:

	Agg temp.	*High deg.*	*Low deg.*	*Ran. of th.*
Obs. at Fort Crawford, lat. 43d. 3m. N.	45.52	96	-28	124
" at Fayetteville, lat. 42d. 58m. N.	46.83	92	-14	106
Difference	1.31	4	14	18

Prevailing winds in both places, N. W.

Fayetteville, May 1*st*, 1825.

INTELLIGENCE AND MISCELLANIES.

I. DOMESTIC.

1. *Correction by Dr.* HARE.—I observe that my answer to Prof. Olmsted is announced in the last number of the American Journal of Science, as a "reply to a criticism of Prof. Olmsted on the *arguments respecting the materiality of heat adduced by Dr. Hare.*" The arguments which called forth the criticisms are contained in a paper entitled, "*Essay on the question whether heat can be ascribed to motion,*" and it is actually described by me as "*remarks made in answer to his* (Davy's) *hypothesis.*" I am induced to correct the above-mentioned description of my essay, because it tends to confirm, and has probably originated in, an erroneous impression given by Prof. Olmsted, that I had premised an intention of summing up, in that essay, all the proofs in favour of a material cause of calorific repulsion. Accordingly, influenced by this creature of his own fancy, he says, "*if Dr. Hare be allowed to have fully and clearly refuted the hypothesis of Sir Humphrey, his argument is still imperfect, for it by no means establishes the doctrine of the materiality of heat to prove that Davy has failed of showing that it is a product of motion.*" It seems to me that it were just as reasonable to object to the 1st proposition in Euclid because it does not establish the 45th, as to represent my arguments as "imperfect" in showing that heat cannot be motion, because I did not, in the opinion of the author, *prove its materiality at the same time.*

We have all laughed at the recipe for cooking a Carp, which begins, "First catch your Carp." I wish it were equally superfluous to say to our modern critics, when you are about to criticise an essay, first read the essay, or at least do not overlook the title!!! Before concluding, I ought, in justice, to acknowledge that, in his notice of my galvanic apparatus, Prof. Olmsted has awarded me more merit than I claim; so that as far as good will is concerned the account is more than balanced.

In the last paragraph of the 3d page of my reply, the word "*learning*" is substituted for "*reasoning.*" In another article "*corbonicometer*" for "*carbonicometer.*"

II. CRANK MOTION. *Extract of a letter to the Editor, dated, Bennington Iron Works, March* 21, 1827.

DEAR SIR—Your correspondent, in his "Examination of Mr. Quinby's Principle of Crank Motion," (Vol. 12, p. 124,) has arrived at a just conclusion, as far as his last equation, but his deduction from that equation is incorrect, as he will doubtless agree, when he considers that "the mean tendency to rotation" $= P \times .6366$ acts throughout the *demi-circumference* of the circle described by the crank; while the applied power $= P$, only acts through a distance = the *diameter* of that circle. Now it is very easy to demonstrate that:

$P \times .6366 \times$ demi-circumference $= P \times$ diameter, and your correspondent evinces too correct a knowledge of mechanics to contend that there is an absolute loss of power in its application to the crank where this equation exists. The problem is reduced to the principle of the lever with unequal arms.

I am, Sir, &c. I. DOOLITTLE.

April 4*th*, 1827.—My letter having lain over, I beg leave, before closing it, to point out an error which has crept into that deservedly popular work, "Nicholson's Operative Mechanic and British Machinist"—in page 12. Lond. Ed. in treating of the inclined plane, he says, "the manner of using it for the raising of weights, is to cause the applied force to act in a direction parallel to the plane * * * * * * * * * *the power gained is in proportion to the length of the base compared to the perpendicular.*"

Now if we suppose the angle formed by the inclined plane with the horizon to exceed 45°, the perpendicular would be longer than the base, and, therefore, according to the above theorem, it would require a greater power to raise a body along the plane, than to raise the same body vertically; hence the error is evident.

The true statement is, as laid down by most writers on mechanics, and confirmed by experiment, that the power required to raise a body up an inclined, is to the weight of the body raised, *as the perpendicular is to the length of the plane*, when the power is applied in a direction parallel to the plane; and *as the perpendicular is to the base*, only when the power is applied in a direction parallel to the base.

This is a principle now so generally known, that a repetition of it might be deemed superfluous, were it not that some persons might be inadvertently led into error by taking Nicholson as a guide, without referring to other authors. I have not at hand a copy of the Philadelphia Edition,* in which it is possible the mistake has been corrected; if so, it will be less important to notice it now, but if that should not be the case, I beg you will insert this communication. I. D.

III. *The fascination of Snakes;* by Mr. NASH.—I have often heard stories about the power that snakes have to charm birds and animals, which, to say the least, I always treated with the coldness of skepticism, nor could I believe them until convinced by ocular demonstration. A case occurred in Williamsburgh, Mass. one mile south of the house of public worship, by the way side, in July last. As I was walking in the road at noon-day, my attention was drawn to the fence by the fluttering and hopping of a robin red-breast, and of a cat-bird, which upon my approach flew up, and perched on a sapling two or three rods distant; at this instant a large black snake reared his head from the ground near the fence. I immediately stepped back a little, and sat down upon an eminence; the snake in a few moments slunk again to the earth, with a calm placid appearance, and the birds soon after returned and lighted upon the ground near the snake: first stretching their wings upon the ground, and spreading their tails, they commenced fluttering around the snake, drawing nearer at almost every step, until they stepped near or across the snake, which would often move a little or throw himself into a different posture, apparently to seize his prey, which movements I noticed seemed to frighten the birds, and they would veer off a few feet, but return again as soon as the snake was motionless. All that was wanting for the snake to secure the victims seemed to be, that the birds should pass near his head, which they would probably have soon done, but at this moment a waggon drove up and stopped. This frightened the snake, and it crawled across the fence into the grass; notwithstanding, the birds flew over the fence into the grass also, and appeared to be bewitched to flutter around their charmer, and it was not until an attempt was made to kill the snake that the birds would avail themselves of their wings and fly to a forest one hundred rods distant.

The movements of the birds while around the snake seemed to be voluntary, and without the least constraint, nor did

* The Philadelphia Edition is not at hand.—ED.

they utter any distressing cries, or appear enraged, as I often have seen them when squirrels, hawks, and mischievous boys attempted to rob their nests or to catch their young ones; but they seemed to be drawn by some allurement or enticement, (and not by any constraining or provoking power;) indeed, I thoroughly searched all the fences and trees in the vicinity to find some nest or young birds, but could find none.

What this fascinating power is, whether it be the look, or effluvium, or the singing by the vibrations of the tail of the snake, or any thing else, I will not attempt to determine; possibly this power may be owing to different causes in different kinds of snakes. But so far as the black snake is concerned, it seems to be nothing more than an enticement or allurement with which the snake is endowed to procure his food.

P. S. Since this case occurred, I have heard several respectable people, who have also seen birds charmed, observe that they have heard music occasioned by the vibrations of the snake's tail, which, they being near, could see. That snakes make music thus I know; and also that birds are extremely captivated with music—but whether this is the only means that the snake uses, or whether all kinds of snakes use it, I am not prepared to say.

In the month of June, 1823, in company with a friend, I had just crossed the Hudson river, from the town of Catskill, and was proceeding in a carriage, by the river, along the road, which is here very narrow, with the water on one side and a steep bank covered with bushes on the other. Our attention was in this place arrested, by a number of small birds, of different species, flying across the road and then back again, and turning and wheeling in manifold gyrations, and with much chirping, yet making no progress from the particular place over which they fluttered. We were not left long in doubt, when we observed a black snake of considerable size, partly coiled and partly erect from the ground, with the appearance of great animation, his eyes brilliant, and his tongue rapidly and incessantly brandished. This reptile we perceived to be the cause and the center of the wild motions of the birds, which ceased, as soon as the snake, alarmed by the approach of the carriage, retired into the bushes; the birds, however, alighted upon the neighboring branches, probably awaiting the re-appearance of their tormentor and enemy. Our engagements did not permit us to wait to see the issue of this affair, which seems to have been similar to that observed by Mr. Nash. EDITOR.

IV. ANALYSIS OF SOILS. *To the Editor.*—It is my misfortune to differ widely in opinion, on analysis of soils, from those great men, whose opinions are received as oracles by the learned world. You will please to indulge me in presenting my views on this subject for examination, as the formulæ usually presented by authors, are, in my view, *founded in mistake, and are calculated to mislead the agriculturist.*

Most soils contain more than sixty per cent. of stones, pebbles and sand, which will settle from a state of suspension in water, in less time than three minutes. Even the clay soils, as they are called, contain about fifty per cent. Dr. Beck and myself analyzed specimens of soil taken from one hundred and fifty farms in the manor of the Hon. Stephen Van Rensselaer, and obtained results as before stated, in almost every case.

Suppose, in one specimen, the soil, &c. should be quartz, in another feldspar, in another hornblende, in another sapphire, in another diamond; would there be any difference in the influence of the sand, &c. upon the productive quality of the soil, on account of the different ultimate elements of which these different minerals are composed? Should they be so far decomposed, at some future period, as to become an impalpable powder, perhaps they may then differ in their influence upon vegetation. Perhaps we may foretel the future state of the soil, a century or two to come, where such extreme disintegration is effected. But the difference in the ultimate constituents cannot possibly affect the question of fertility or barrenness, *at the time the analysis is made.* For whatever effect can be ascribable to the one, is equally a property of the other. They all hold water on their surfaces by the attraction of adhesion; they all keep the soil duly open and porous, to give passage to the roots of vegetables; they all aid alike in bracing up plants and in keeping them in a fixed position, &c. &c. Whatever is effected by one, is effected by all; size, form, quantity, and all circumstances, other than their constituent elements, agreeing.

But according to the usual analyzing formulæ, the results would be very different, and this would induce the agriculturist to search out different methods of culture, in cases where the culture should be similar. Take the examples before given, allowing each to compose sixty per cent. of the soil. The quartz would give silex about 55, alumine 5—the feldspar about 38 silex, 12 alumine, 9 potash, 1 oxyd of iron —the hornblende about 25 silex, 8 alumine, 7 lime, 1 magnesia, 18 oxyd of iron, 1 manganese—the sapphire about 54 alumine, 5 silex, 1 oxyd of iron—the diamond just 60 of the basis of charcoal. From these different results, the agriculturist would infer, that each soil should require a peculiar method of culture, when in truth all require the same.

It may be said that sapphire and diamond sand are not to be expected in soils. But it is well known that quartz, feldspar and hornblende, are all common in New-England, and in all other primitive countries. It seems to be necessary, therefore, that the stones, pebbles, and sand, should be separated at the commencement of the process, or immediately after the combined water and animal and vegetable substances are separated; and that the remainder should be analyzed by itself.

Fortunately for agriculturists, pebbles, sand, or whatever we may choose to call that hard part of soils which can act upon vegetation by surfaces only, can be entirely separated from the remainder in about three minutes. After picking out the stones, &c. according to the usual directions, we put the soil into an assay glass, or high tumbler, and pour in water and stir the mixture. The coarse pebbles immediately fall to the bottom and form a distinct stratum, from which the finer soil may be removed and washed off clean. The pebbles are weighed, and all the fine soil dried and pulverized in the usual way. We now put the pulverized mass into the assay glass. All that part of the soil before described will fall to the bottom in a distinct stratum in about three minutes. Pour off the supernatant liquid, and wash the sediment several times and weigh it. Then proceed with the part which remained in suspension in water over three minutes, in the usual way, set forth in various authors.

We here perceive that the same principle which causes this precipitation is the most important which can affect the growth of plants. It falls to the bottom because it does not strongly attract water, as the clayey part of the soil does. Being made up of hard fragments, water does not enter between its minute molecules; but merely adheres to the surface of each fragment.

I will add a circumstance which has been overlooked by the great chemists of Europe. Prof. Beck and myself, on making particular inquiry of several hundreds of Mr. Van Rensselaer's tenants, learned this curious fact; that whereever the soil was of that character which disengages the roots of winter wheat, which is called by farmers "winter-killing," it would remain very long in a state of suspension in water, before it settled so as to leave the water clear. And what is most remarkable, the time of such suspension did not wholly depend on the proportion of alumine; for where the proportion was the same in two specimens of soil, there was often a great difference in this property. By a minute attention to this subject, we were enabled to establish this rule for the direction of the agriculturist. *If his soil will settle in four hours, after being well stirred in a tumbler of pure water, he need have no apprehensions of the "winter-killing." If it remains turbid over four hours, and under twelve, the danger is not very great. But soils which remain turbid from twelve to twenty-four hours, are not safely sown with wheat.*

Yours, &c. AMOS EATON.

Rensselaer School, Troy,
April 27, 1827.

V. *Rarified Air Balloons.*—In a course of experimental illustrations, before the college classes in chemistry and natural philosophy, Prof. ABBOT and myself had occasion to prepare a rarified air balloon. We adopted two or three expedients for causing it to ascend more surely and higher, which I have not seen mentioned, and which may be worth the attention of any who may wish to construct balloons of this description.

The balloon was twelve feet in diameter and spherical, with an opening between three and four feet, at its lower pole. To the edge of this opening, which was made strong by a wooden hoop, we fastened a piece of cambric, with a tape running through the opposite edge, so that it could be almost instantly drawn or *puckered up*, when the balloon was inflated. Having effected this by burning baked straw, dipped in alcohol, we closed the aperture, and thus prevented the heated air from escaping. It rose very majestically about 400 or 500 feet, and answered our expectations, except that we had hoped to see it attain a greater elevation. To effect this object, we took off the cambric, thus lightening very much the envelop, and in the center of the opening we suspended, on cross wires, a sponge, four or five inches in diameter, which was dipped in alcohol and set on fire, just as the balloon was inflated; having attached, several feet below the sponge, a weight of a few ounces, to prevent the balloon from overturning. The consequence was, that it rose gently till it had attained the height of nearly 2000 feet; being carried, by a slight breeze, two miles horizontally, before the alcohol was exhausted.

In putting together the balloon, the gores were first cut according to a calculated pattern; we then laid them in succession upon one another, upon a table, pasting together the alternate edges, until we came to the last piece, which we pasted to that which lay lowermost. Thus was the balloon put entirely together in the easiest possible manner, and when dry, was ready to be inflated, with the exception of the hoop. E. H.

Amherst College.

VI. *Notice of the Geographical Society of Paris, and of Woodbridge's Geography.*—It is well known, that about six years since, there was instituted in Paris, a Geographical Society, for the express purpose of improving and advancing that important branch of knowledge, often too much neglected in modern education. Towards the close of the late year, we received the circular and exposè. The design of the institution is very liberal; it embraces every country, and the number of its members is unlimited. It calls upon the friends of knowledge every where, to furnish their contributions, and to write and induce others, especially travellers and navigators, and adventurers, to forward to Paris notices, memoirs and articles of intelligence and discovery, relating to every part of the world, and it promises the publication of every important and interesting fact in the bulletin which it engages to publish, and has actually published, and to forward without expense, every month. It is accumulating funds, and proposes to offer prizes in money. The list of its officers embraces many distinguished names; that of Chateaubriand is at the head as President; among the vice presidents is Cuvier, and Ferussac is secretary.

We have not seen any of the bulletins of the society, but cannot doubt, that with the zeal, perseverance and ability, which so eminently characterise the learned men of France, the society will contribute, in a very important degree, to the progress of geographical knowledge.

A respected American correspondent writing from Paris, under date of March 28, gives us some quotations from a notice of the excellent geography of Mr. William Woodbridge,* published in the Bulletin de la Société de Geographie. Tome Sixieme—no. 42, p. 178.

The notice is entitled "*Rapport sur un ouvrage Intitulé*," Geography ancient and modern, &c. 8vo. Edition of 1824. The notice speaks of the work, as being compiled with much care, and as embracing a great number of interesting facts and views. It goes on to say, "great order and method, and a classification in some degree original, in which the author embraces all the branches of geographical science, recommend it particularly to your attention. Strongly impressed with this idea, that the real essence of science consists in generalization, and in reducing all the branches of human knowledge to a few general principles and cases, Mr. Woodbridge attends only to the real truths of science, and he endeavors to bring into view only the principles most generally known and admitted. This book may be considered as a valuable guide, not only for pupils, but even for masters."

The reporter then gives an analysis of the work, in the conclusion of which he says, when speaking of the author, "He has conceived the happy idea of representing the most remarkable appearances in vignettes, or wood cuts, the greater part of which are neat, and give a just idea of facts. These vignettes are numerous, and they cannot fail of aiding essentially the memory of a pupil." The reporter commends Mr. Woodbridge for having proposed, with much sagacity, questions to exercise the judgment and memory of the pupil, and for collecting these questions at the end of the work.

He thinks that Mr. Woodbridge's work may be consulted with advantage by those who may hereafter, in France, compile treatises on geography, and throughout his entire article he treats the book in the most respectful manner.

* Of which some account was given in our eighth volume.

VII. *Sea Serpent.*—To us it seems a matter of surprise, that any person who has examined the testimony, can doubt the existence of the Sea Serpent ; the documents communicated by Dr. Bigelow of Boston, and published in the second vol. of this Journal, in 1820, were in our judgment alone sufficient, to settle the question : the following letter is an important additional document.

The American Sea Serpent.—The following letter respecting this huge animal was addressed to Robert Barclay, Esq. of Bury Hill, Surrey, by Mr. Warburton, a gentleman belonging to the house of Barclay, Brothers & Co. London. That gentleman, proceeding on his passage to America, on board the Silas Richards, Captain Holdridge, had an opportunity of beholding this sea monster, on Friday, the 16th of June, off St. George's Banks.

" *Pentonville, 20th Sept.* 1826.

" *Dear Sir,*—Having been informed by your grandson, Mr. Robert Reynolds, that you were desirous of possessing a sketch of the sea serpent as seen by me in crossing the Atlantic, and to have some account of the same, in compliance with your wishes I have annexed a rough pencil drawing of the monster, as it appeared during the time when its head was elevated above the water, and I shall state the particulars attending this novel exhibition.

" The captain and myself were standing on the starboard side of the vessel, looking over the bulwark, and remarking how perfectly smooth was the surface of the sea. It was about half past six o'clock, P. M. and a cloudless sky. On a sudden we heard a rushing in the water a head of the ship. At first we imagined it to be a whale spouting ; and turning to the quarter from whence the sound proceeded, we observed the serpent in the position as it appears in the sketch, slowly approaching at more than the rate of two miles an hour, in a straight direction. I suppose we were hardly going through the water so fast, for there was scarcely a breath of wind. I must premise, that I never had heard of the existence of such an animal, I instantly exclaimed, ' Why, there is a *sea snake.*' ' That is the sea serpent,' exclaimed the captain, ' and I would give my ship and cargo to catch the monster.' I immediately called to the passengers, who were all down below, but only five or six came up, among whom was Miss Magee, the daughter of a merchant in New-York. The remainder refused to come up, saying there had been too many hoaxes of that kind already. I was too eager to stand parleying with them, and I returned to the captain. In the same slow style the serpent passed the vessel at about the distance of fifty yards from us, neither turning his head to the right nor left. As soon as his head had reached the stern of the vessel, he gradually laid it down in a horizontal position with his body, and floated along like the mast of a vessel That there was upwards of sixty feet visible is clearly shown by the circumstance, that the length of the ship was upwards of one hundred and twenty feet, and at the time his head was off the stern, the other end (as much as was above the surface) had not passed the main-mast. The time we saw him, as described in the drawing, was two minutes and a half. After he had declined his head, we saw him for about twenty minutes ahead, floating along like an enormous log of timber. His motion in the water was meandering like that of an eel, and the wake left behind was like that occasioned by the passing of small craft through the water. We had but one harpoon on board, and the ship's long boat was, for the time being, converted into a cow house. We had two guns on board, but no ball.

"Two days after we saw him, he was seen by another vessel off Cape Cod, about two hundred miles from where he made his appearance to us. This intelligence reached New-York about four days after we arrived there, and the description given exactly corresponded with the foregoing. I dined one day at the hotel of New-York with Sir Isaac Coffin, who discredited the existence of such an animal, which was reported to have been seen by Capt. Bennet, of Boston, about five years back; but, as I assured him I had never heard, previously, even the report of such a monster, and that I was an *Englishman*, he gave full credit to it. The sketch I gave him corresponded with the description that was circulated at that time. The humps on the back resembled in size and shape those of the dromedary.

I remain, dear sir, yours respectfully,

WM. WARBURTON."

National Gazette of Philadelphia.

VIII. *Calamine in Missouri.*—Messrs. TROOST and LESUEUR have discovered in Jefferson co. at a place called Valle's Diggings, the Carbonate of Zinc, in great abundance. It has hitherto been rejected by the miners at that spot, as entirely useless.

" This ore occurs crystalized in reniform mammillary, or stalactical concretions, and is sometimes corroded, cellular, in crusts, &c.

The first, namely, the crystalized variety, we found particularly in the mines of Mr. Valles, who was kind enough to give us every facility in making a proper selection of the ores which his mines contained. These crystals form large masses of small crystals heaped together and generally colored by argillaceous oxide of iron (yellow ochre.)—Its form is an obtuse rhomboid, having mostly the edges emarginated and the solid angles truncated. These emarginations are often curvilinear, giving a globular appearance to the crystals. As the rhomboid is the primitive form, and having a laminar structure in three directions, that is, parallel to the six sides of the rhomboid, the faces, when the crystals are not contaminated by the oxide of iron, being in that case of a grayish white, have a fine pearly appearance, while the secondary faces are dull and somewhat striated parallel to its sides, forming a pretty mineral.

The other, the concreted variety, is more abundant; it occurs generally in cellular or corroded masses, which appearance has perhaps given it the name of *dry-bone*. These concretions have often an imperfect fibrous structure; sometimes it is compact—its fracture somewhat splintery, uneven, and is either dull or glistening, of an adamantin lustre, translucent, and sometimes opaque—its color is sometimes gray, sometimes yellow approaching to brown, owing to the oxide of iron.

Sometimes it is mammillary, being formed of parallel zones of a white dull color.

The mines above named are for the present the only ones in the United States where this mineral occurs in abundance, and the situation of them, being near the banks of the Mississippi, makes this ore of the highest importance.

We be lieve we have it from unquestionable authority, that the country to the southwest of lake Superior abounds in copper, found in an oxide rich in native copper. Now these two substances, namely, the zinc and copper, are (as every one knows) the ingredients which enter into the composition of *Brass*, a metal so much used in the United States (the constituents of which are at present imported from abroad.) The copper ore in the state, as mentioned, could be brought down the Mississippi without undergoing any preparation, to an establishment near the mines of the zinc, where the brass could be manufactured, and would give an additional value to the lead mines by this important branch of industry."—*New-Harmony Gazette.*

IX. *Cobalt in Missouri.*—Messrs. TROOST and LESUEUR announce the discovery of an ore of Cobalt in this state, yielding, on analysis, "upwards of seventy-five per cent. of Cobalt. If it be abundant, which there is reason to believe it is, the discovery is of very great value."—*Idem.*

X. *Localities of Minerals ;* by Dr. JACOB PORTER.—Arenaceous quartz, very white and beautiful, at Windsor. Wells.

Ferruginous quartz, resembling the variety from Lanesborough, at Windsor. Wells.

Flint, in small quantities, at Windsor. Wells.

Amianthus, at Plainfield. The color is white, and the filaments very fine and delicate.

Yellow earth, at Monroe, a new township in Berkshire county, where it is refined and sold as a paint.

Carbonate of iron, at Charlemont, Hawley and Chester.

Sulphuret of iron, in fine cubic crystals, at Braintree.

XI. *Bituminous Coal, near Harrisburg.*—We have received from GEORGE VAUX, Esq. through Mr. S. CONVERSE, a specimen of very good bituminous coal, (black slaty coal, of Werner.) It is stated to have been found ten miles north of Harrisburg, Pennsylvania, in a narrow vein, from which, however, several tons have been taken.

Harrisburg, being on the Susquehannah, in a geological region, which is peculiarly the domain of the anthracite, it is on that account the more interesting, and remarkable to find the bituminous coal there, especially if it be correctly stated, that the anthracite and bituminous coal "have been found attached to each other in the same lump." We request additional and more detailed information.—ED.

XII. *Minerals from Antigua.*—A large collection of siliceous petrifactions and agates have lately been received by the Editor, through the kindness of Mr. Wood, a resident in Antigua. Among them we notice an abundance of the wood-stone, and of calcedonified, and japerized wood : some of which are very curious from the perfect preservation of the woody texture,—the substitution of silex being so complete as to render apparent, not only the *horizontal* and *divergent layers* of the *ligneous fibre*, but even the *vascular texture* itself. A petrifaction of madrepore by a beautiful white hornstone, forms a singular specimen, and appears to be abundant. The collection contains also, many masses of *sard*, *calcedony*, ribbon-agate and jasper-agate, which, (as well as the petrifactions just mentioned,) in the hands of the lapidary, would, without doubt, afford very beautiful objects of ornament. C. U. S.

XIII. *Lead Ores of Missouri ;* by Messrs. TROOST and LESUEUR.—The lead ores which occur at the mines of Missouri, differ somewhat in every mine we visited. The first mine we examined was that known by the name of La Motte. The ore of this mine is the most complicated of any that we met in this district. It is generally the sulphuret of lead (galena, or lead glance) and occurs in masses of various size and grains, of an irregular foliated structure, approaching sometimes to small curved lamellar, and even granular ; these masses are interwoven with carbonate of lead, (white lead ore) in veins or small crystals, filling the cavities of the sulphuret of lead, and in the earthy state. These cavities are sometimes lined with sulphate of lead crystallized in small elongated octahedra. Besides these accidental mixtures, it is also contaminated with argillaceous iron ore in a pulverulent and concreted state.

The carbonate of lead occurs also, crystalized and in an earthy state ; the crystalized variety is commonly interspersed through the argillaceous oxide of iron, which is very abundant in these mines. These crystals are generally small ; and those which we collected from the rejected rubbish, undeterminable. The earthy carbonate of lead occurs in amorphous masses of an earthy and stony aspect, and incrusting the sulphuret. We found also a great number of fragments of a vein of between one and a half and three inches thick of a carbonate of lead, which had for us quite a new aspect ; and it was only after submitting it to some trials, that we found it to be a variety of carbonate. This vein is composed of two parts, one of which is the common earthy carbonate of lead, of a grayish white color, and an uneven fracture approaching to granular, interspersed with a few grains of sulphuret of lead. The other part is of a reddish brown color, of a compact somewhat resinous fracture, interspersed with yellow and black spots. I found by analysis, that the brown stony substance differed from the common earthy carbonate of lead only by containing a small quantity of peroxide of iron, the yellow spots being also ascribable to that oxide, and the black ones to partly decomposed sulphuret of lead, having still, in its interior, some undecomposed sulphuret. Judging from the pieces, we conjecture that the earthy carbonate of lead which we found among the refuse, is pretty abundant in these mines, but as the miners are not aware of its value, the greater part remains in the mines, and that which accidentally comes up with the other ore, is rejected as useless.

New-Harmony Gazette, April 17, 1827.

XIV. *Luminous appearance in the atmosphere.*—In Vol. xi. No. 2, of the Journal of Science and Arts, Mr. C. Atwater has communicated an account of a spot or spots, near the horizon, appearing as if lighted, and giving rise to a belief that there was a great fire in that direction. He remarks that he has often noticed these light spots in Ohio, but not on the east of the Alleghanies.

I would only remark that I have observed similar phenommena in New-England. I recollect one instance, when I resided at Amherst, in Hampshire County, Mass. a bright light in the North East, near the horizon, appeared as the light of a building on fire appears at night at the distance of several miles. I expected, in that instance, every hour to hear that some building in Shutesbury or New-Salem, had been burnt; and so strong was my belief of it, that I repeatedly asked my neighbors whether they had heard of any such event. At last I met a gentleman who had just come from one of those towns, who told me he had heard of no fire in that quarter, which convinced me that the phenomenon was merely atmospheric.

N. WEBSTER.

XV. *On the Fossil Remains of the Mastodon lately found in Ontario County, New-York.*—By JER: VAN RENSSELAER, M. D.

NEW-YORK, March, 1827.

Dear Sir,—The fossil remains of a mastodon having been discovered some months ago near Genesseo, Ontario County, I took means to procure correct information on the subject, and now send to you the result. In addition to the answers received by myself, a gentleman has placed at my disposal a satisfactory letter from one of his friends.

The discovery of these fossils is by no means a very extraordinary event, and yet such facts are worthy of permanent record; I therefore transmit the following short account, and remain truly yours, JER: VAN RENSSELAER.

In repairing and cleansing the village spring, and the ditches connected with it, which are dug in marl that extends two feet below the surface, it was deemed proper to deepen them; and in doing this the bones were found—about half a mile east of the court-house at Genesseo, in a small marsh, that has some elevation above the surrounding country.

The tusks were first seen, and then the head, but these, as indeed the whole skeleton, were in such a state of almost total decomposition, as to defy all attempts at preservation. The skeleton lay in the direction so frequently observed in the remains of this animal, South West and North East. The head rested upon the lower jaw. The tusks were much decayed; their points were five feet apart, and curved at least a foot from the center. They were four feet and two inches in length; the largest diameter could not be ascertained on account of their decay—but it was preserved a considerable distance and then gradually diminished, so that at five inches from the point, the diameter was three inches. The laminated structure of the tusk was rendered evident by decomposition, which had in a measure separated the laminæ, and the whole was supposed to be phosphate of lime.

Of the two (superior) incisors, no trace could be discovered, but the eight molars were in situ. The length of the largest tooth was six and a quarter inches; of the smallest three and an half; the crown of the tooth was two and an half; and the breadth of the enamel from $\frac{1}{8}$ to $\frac{3}{8}$ of an inch, as was rendered visible by wearing away of the surface. The roots were all broken and decayed. The animal could not have been old, as eight molar teeth were found; old animals have only one molar on either side of each jaw.

The pelvis was twenty two inches in its transverse diameter, between the acetabula at the inferior opening. The epiphyses of the large bones, and the patellæ were found nearly perfect, not having suffered from decay.

XVI. *Magnetism destroyed by lightning.*—The brig Medusa, Capt. Adpelt, of Jersey, while on the passage from La Guayra to Liverpool, encountered a thunder-storm in lat. 33, 38, long. 58, 12, during which the electric fluid destroyed the magnetic power of the compasses on board, two of which were on deck and two in the cabin. An optician has examined the compasses and finds they have *entirely* lost their attractive power.—*New-York Enquirer, taken from a foreign paper.*

XVII. *Medical Institution and Journal of South Carolina.*—This Institution, already established under the most favorable auspi-ces, is about to add to its usefulness, by the publication of a Medical and Physical Journal, to be conducted under the direction of the Medical Faculty of the Institution, and of other eminent professional men. We cannot doubt, that their efforts will prove eminently serviceable, especially in the southern portion of the United States, which will, hereafter, have little occasion to send its youth to the middle and northern states, for a medical education.

XVIII. *Physical and Medical Journal of Cincinnati.*—It is not among the faults of the American character to neglect the means of obtaining and diffusing useful knowledge. The *existence* of Cincinnati, now a beautiful and flourishing city, of the fourth class in the United States,* in a place, which, within the memory of many of its present inhabitants, was a wilderness, is scarcely more remarkable, than the creation there, and elsewhere in the west, of useful institutions devoted to literature, science and the arts. A Journal of Medical and Physical Science, (the first number announced for April,) is to be established at Cincinnati. Among its leading objects will be the indigenous diseases and remedies of the west, and the facts and events relating to science and art, which are peculiarly local to the trans-alleghany regions; at the same time that it will not neglect the general progress of knowledge in other countries and in other parts of this country.

It is a sufficient pledge for the zeal and ability with which this Journal will be conducted, that its principal Editor is Dr. Daniel Drake of Cincinnati.

* Population about 18,000.

XIX. *New Work on Geology.*—Mr. John Finch purposes to publish by subscription, an Introduction to the Study of Geology, containing some account of the Coal Mines of Pennsylvania, with a Geological Profile of the country between Philadelphia and Sunbury on the Susquehanna. The work is to be comprised in an octavo volume of between one and two hundred pages: price to subscribers one dollar in boards.

II. FOREIGN.

I. *Foreign Literature and Science, extracted and translated by* Prof. J. GRISCOM.

1. *Electricity. Ponderable matter transported by the electric currents.*—A variety of experiments were made by M. FUSINIERI, to ascertain whether any portion of the matter from which the electric discharge proceeds, is conveyed by the current. The discharge of two large jars was passed between balls of silver, gold, brass, &c. over polished disks of different metals. By the spots which appeared upon the disks, and also upon the balls, after each discharge, it was evident there is a real transport of ponderable matter by the electric spark. This matter is reduced to such a state of division, that it assumes the character of volatile substances. Even the gold, which was deposited in the form of an extremely thin and continuous leaf or coating, began in a few minutes to become more rare, and totally disappeared in the course of a few days. It appears therefore probable, that the light of the spark is due to the pressure of ponderable molecules, which the electricity detaches from the hardest bodies. This may also account for the various colours of the spark, for it is known to vary with the nature of the body, the reason of which has not before been stated. The light, even in a vacuum, between the poles of a voltaic pile, is owing, without doubt, to the solid particles, forced along by the electric current. The odour diffused by the electricity of our machines, and by the thunder of the clouds, is but the odour of such material particles.

Bulletin Universel, Nov. 1826.

2. *Mass of Gold.*—In the month of May last, there was sent by an express to St. Petersburgh, a mass of pure gold, weighing about 25 pounds. It was found five feet beneath the surface, in the environs of Miaeski, from which place several large pieces of inferior weight had before been transmitted. *Idem.*

3. *Grand Opal*, in the imperial cabinet of Vienna. This specimen is $4\frac{3}{4}$ inches (Viennica) in length, and $2\frac{1}{2}$ in thickness, and weighs 34 ounces (Viennica). It came from Czervenitzia, in Hungary. Half a million of florins have been offered for it, a price very inferior to the real value of this unique and magnificent specimen.—*Idem.*

4. *Precious metals.*—In a memoir communicated by M. DE HUMBOLDT to the Academy of Sciences, July 17, 1826, it is stated that mines of platina have been recently found in the Oural mountains, which are so rich that the price of platina, it is said, has been lowered thereby nearly one-third. In 1824, the auriferous and platiniferous region of Oural produced 286 *pouds;* which give 5700 kilogrammes by weight of metal, or a value of 19,500,000 francs. The united mines of all the rest of Europe, produce annually but 1,300 kilogrammes. Those of Chili furnish only 3,000, and the whole of Colombia yields only 5,000.

The Oural now affords as much gold as Brazil ever did, when its mines were the most productive. The maxim of their produce per annum in 1755, was 6000 kilogrammes of gold. At present, Brazil does not furnish 1000.

It might be natural to suppose that the prodigious increase of the Ouralian mines might produce important results, both in respect to the prosperity of Russia, and to the value of gold. But this opinion cannot be entertained, if we reflect that the quantity of this metal actually spread over the surface of the globe is so considerable, that a value of eighteen millions is in realiity insensible, and that besides, the diminution of the mines of the new world, will furnish a compensation. With respect to Russia in particular, an augmentation of eighteen millions is a trifle for so vast an empire, particularly as nearly a third will be expended in the costs of exploration and working. Nothing is so variable as the produce of mines. Those of Mexico, which in 1700 furnished only six millions of piastres in gold and silver, yielded twenty-five millions in 1809; and this immense augmentation was not felt in Europe, and produced no sensible effect, when M. de Humboldt made it known long after it had taken place. The revenue of Mexico has been maintained since that time at about eighteen millions of piastres, without any consequent modification of the price of provisions any where.

With respect to platina, the case is different. As the quantity of this metal, which has not been long used, is still very inconsiderable, an increase in the produce of the mines which furnish it, may easily lower the price of it—a circumstance which would be extensively favourable to the arts.

Idem.

5. *Hydrocyanic Acid.*—M. DUPUY had given seven drops of this acid to a horse, in order to destroy him. When he was at the point of death, he was rapidly restored by the administration of a dram of carbonate of ammonia.

Bull. Univ. Nov. 1826.

6. *Steam-Engine at Glasgow.*—The first steam-engine established at Glasgow, (Scotland) was in January 1792, in the cotton factory of Williams, Scott & Co. This was seven years after Watt & Patton established their first machine in the factory of Robinson, at Papplewick.

The number existing at the present time, is as follows:

	Numbers.	Horse power.
In Manufactories,	176	2,970
" Coal Mines,	58	1,411
" Quarries,	7	39
" Steam-Boats,	68	1,926
" Iron Mines,	1	60
	310	6,406

Tech. Repos. June, 1820.

7. *Nitrate of Soda.*—In the district of Atacama, in Peru, M. Rivero points out the existence of a bed of nitrate of soda, several feet thick, and fifty miles in length. It is three days journey from Conception and Iquiqui, in Peru. It is worked and exported.—*Bull. Univ. Oct.* 1826.

8. *Mineralogy.*—A mineral substance was discovered about nine years ago, five leagues from Madrid, and two and

a half from Aranjuez, in a place known under the name of *Salines d'Espartines*, and which was ascertained to consist of sulphate of soda, mixed with a very small portion of sub. carb. of soda. M. Casaceca, professor at Madrid, has given it the name of *menardite*, in honour of the distinguished French chemist.

This salt is precipitated from its watery solution, in a crystaline form, without retaining the least particle of the fluid which dissolved it. This anhydrous condition of a sulphate of soda is very remarkable. It may be owing to the temperature which the waters acquire that hold it in solution; or the nature of the soil on which the deposit is made, and on the salts which may remain in the mother waters.

One hundred parts of this new substance contain

Sulphate of soda	99.78
Sub-carbonate of soda	0.22

It differs from all others at present known, and particularly from the glauberite found at Villa-Rubia, in La Mancha.--*Id.*

9. *Surgery.*—M. DUPUYTREN, presented to the Academy of Sciences on the 7th of August, 1826, three persons cured of cancers of the lower jaw by the amputation of a greater or less portion of the jaw. This celebrated surgeon gave some interesting details of the history of this operation.

During a long period, the only carcinomatus affections of the jaw, within the reach of art, were those which, limited to the alveolary border, penetrate the bone only to an inconsiderable depth, and which are designated by the term *epulis*. They were attacked by the actual cautery, and often cured; a method practised for many centuries. But as to real cancers,—osteosarcoma,—which affect the whole thickness of the bone, throughout an extent more or less considerable, they have always resisted this curative process; and, on the other hand, no one had dared to attempt the extirpation of the part, when M. Dupuytren, observing that many of the subjects at the Hotel des Invalides had lost different portions of the jaw by musket balls, conceived the hope of being able to execute successfully, by well devised instruments, what had been effected by mere physical force, without destroying life. The first patient on which the operation was tried, was a man named Lezier, a coachman, 35 years of age, who had a cancer on the anterior part of the jaw. The portrait of this individual, submitted to the Academy, gave the most frightful idea of the extent and progress of the disease. The operation succeeded to a wonder; and the patient, now 48 years old, was presented to the Academy in the most perfect health. His face exhibited no deformity, but simply a cicatrice in the middle of the chin, which appeared as if it had been occasioned by the cut of a sword. Since 1812, the period of the first operation, M. Dupuytren cites twenty-five of the same kind performed by himself, by Lallemant of Montpellier, Græffe of Berlin, &c. These operations have sometimes preserved and sometimes removed the lower lip, according to its sound or diseased state. In some cases the whole of the part of the jaw which bears the teeth has been removed, and the patients have survived. Three only have died in consequence of the operation; five or six have experienced, after some years, relapses which have proved fatal; and eighteen have been radically cured. Among the latter, the patient before mentioned was not the only one which M. Dupuytren brought before the Academy: he also presented two women equally well cured: one appeared to be 25 or 30 years of age, who had sustained the operation six years before; the other, about 15, from whom the jaw had been removed about one year, had, besides the unavoidable scars of the face, some little deformity, which was attributable to her indocility. The re-union of the bones did not take place, on account of the impossibility of preventing her from speaking and eating during the time requisite to that purpose.

The consequences of the amputation of the jaw, are not only much less serious, but much less protracted than would have been supposed. A few days are always sufficient for the cicatrisation of the skin; and as to the bone, the union of its divided parts never requires a delay of more than thirty days.

I have thought, says M. Dupuytren, that it might promote the interests of truth, rather than my own advantage, to make known the chances of success of an operation, the efficacy of which has, doubtless through an unintentional error, been denied.—*Idem.*

10. *Sulphuric Acid, and Sulphate of Iron.*—M. M. Bussy, and Lecanu have arrived at the following results.

1st. That the sulphuric acid at 66° will dissolve sulphate of iron at a *maximum*, and become of a red color. 2d. That this solution passes readily to the *maximum* by the action of various oxygenizing substances, or by heat alone. 3d. That concentrated sulphuric acid does not dissolve sulphate of iron at the *maximum*, although it dissolves it when properly diluted with water. These facts explain very clearly the formation of the residue which is observed in the sulphuric acid of commerce, after its concentration: this residuum is sulphate of iron at the *maximum*, and not, as has been heretofore supposed, sulphate of lead. The small quantity of the latter, remains in solution in the acid, whilst the former, at first dissolved in the weak acid, is precipitated by its concentration. This is even a good method of depriving the *minimum* sulphate of iron of its water, when destined for the preparation of anhydrous sulphuric acid.—*Bull. Univ. Sept.* 1826.

11. *New substance which inflames on water.*—At Doulens, near Amiens, is a large cotton factory, belonging to M. Morgues, which is lighted by oil gas. This gas, after issuing from the cylinder in which it is formed, passes through a vessel of oil, in which it deposits a white liquid substance, by means of a cock in the lower part of the vessel. A workman passing, spilt some of this upon wet ground, it took fire spontaneously, and having flowed into a neighboring brook, it spread over the surface, which appeared to be on fire.—*Idem.*

12. *Lupulin* —It has been thought that this substance existed only on the saly cones of the female flower of the hop; but M. Raspail has discovered that the young leaves and buds of the plant yield it abundantly. To prove this, it is only necessary to allow these portions of the plant to dry on a seive, when, upon agitation, as much lupuline will be obtained, observing the same proportions, as from the scaly cones of the female flower. M. Raspail is of the opinion that the odoriferous principle, which is communicated to beer, is more extensively spread through the substance of the leaves than in the yellow grains, and that the latter owe their odour to the remains of the parenchyma of the scales which supported them.

This lupuline, or rather these glands, exist in a great number of vegetables. It is this, which, on the leaves of the *Myrica cerifera*, furnishes wax. It exists especially, and with all the characters of the glands of the hop, on the *Canabis Satira*, which have an odour analogous to that of the hop.

It is, however, on the *canabis*, less rich in soluble resinous substances. The solubility of the bitter principle of the hop, appears due, according to M. Raspail, to the simultaneous presence of oil and resin which exist in these glands.—*Idem.*

13. *Charcoal.*—From the experiments of CHEVREUSSE it appears that charcoal exists in two different states, dependent on the temperature to which it has been exposed. When wood is distilled in a retort, until it ceases to emit vapour, the charcoal produced is in the *first* state of carbonization. In urging the heat of the retort to a high degree, the *second* state is produced.

Electricity.—Charcoal is a good conductor only in the *second* state, or after an exposure to a violent heat. In this state it is very suitable for surrounding the bottom of a lightning rod, for the purpose of conveying the electricity into the earth. If used in this state in lieu of copper in the galvanic pile, it is very effective.

Caloric.—It is only in the second state that charcoal is a good conductor of heat.

Density.—In this state its density is considerably greater than in the first.

Hygrometry.—Coal of the same wood exposed to air, saturated with moisture, absorbs eventually the same quantity, but this absorption is more rapid in the first state. Pulverised coal preserves the same relative properties as whole pieces, but the former has less absorbent powers.

Combustibility.—Charcoal in the first state burns more easily than in the second. The author ascribes this to the unequal conductibility of the substance in the two respective states.—*Ibid.*

14. *Egypt.*—Six years ago, the pacha of Egypt, established at Baulag, a school for three hundred young people, and placed at its head Haggai-Osmann Nowreddur, who had arrived from France, and had travelled with much benefit to himself. He opened also a school in the castle of Caïro. Drawing, mathematics, anatomy and the European languages were taught, and French, English and Italian books were translated into Turk and Arabic, and a press, attached to the establishment, multiplied the copies.

The Vice Roy has recently founded an institution of the same kind on a larger scale. The school on the farm of Ibrahim-Bey, (situated between Cairo and the Nile,) will receive twelve hundred pupils. Seven hundred were entered during the last year.

Impressed with the results of his first efforts, Mohammed Ali determined to send to Paris forty two young men, selected from the city of Cairo, under the care of three Effendis, in order that they may diffuse on their return, the knowledge they have acquired, and increase the means of civilization and instruction. These young persons are now installed in the situation which has been chosen for them in the Rue de Clicky, Paris, where they are under the supervision of M. M. Jomard, Jaubert, Agoub, &c.—*Idem.*

15. *Napoleon's literary taste.*—In a biographical notice of A. A. Barbier, Napoleon's private librarian, the following statement occurs.

" The Emperor having remarked that there were wanting in his private travelling library, many important works, and that the ordinary size of the books did not allow of their being placed in it, conceived at various times, the design of having printed, for his own use, a library, the plan of which he traced with his own hand in the two following notes, which were sent to M. Barbier, by the Baron Meneval, secretary of Napoleon's port-folio.

" *Bayonne, 17th July,* 1808.

" The Emperor wishes to form a portable library of a thousand volumes, in small 12mo. printed on beautiful type. The intention of H. M. is to print these works for his own private use, without margin, in order to save space. The volumes to contain from five to six hundred pages, bound with open backs, with as thin a cover as possible. This library must be composed of about forty volumes on religion ; forty Epics ; forty plays ; sixty poetry ; one hundred Romances ; sixty history ; and the remainder, to complete the thousand, to consist of historical memoirs of all ages or periods.

" The works on *religion* must be the old and new testament, taking the best translations ; some epistles, and other of the most important works of the Fathers of the church ; the Koran ; the Mythology ; some dissertations chosen from the different sects which have had the greatest influence in history, such as Arians, Calvinists, Reformers, &c. ; a history of the Church, if it can be compressed in the prescribed number of volumes. The *Epics* are to be Homer, Lucan, Tasso, Telemachus, the Henriade, &c. The *tragedies;* insert from Corneille, only what remains in vogue; take from Racine the *Freres Ennemis, L'Alexandre* and the *Plaideurs;* take from Crebillon, only Rhadamiste, Atree et Thyeste; from Voltaire, only what are still in vogue. The *History;* insert some good work on chronology; ancient original principles; whatever may give a detailed history of France. The discourses of Machiavel on Titus Livius; L'Esprit des Lois; la grandeur des Romains; and whatever is suitable to preserve of the history of Voltaire. The *Romance;* the Nouvelle Heloise and the confessions of Rousseau; not to mention the *chefs-d'œuvre* of Fielding, of Richardson, Le Sage, &c. &c. which will naturally form a part; the tales of Voltaire.

" *Note.* Omit from Rousseau, Emile, and a crowd of letters, discourses and useless dissertations; the same with respect to Voltaire.

" The Emperor desires to have a catalogue *raisonne'*, with notes explaining the most select of these works; and a memoir of the cost of the thousand volumes, printing and binding; what each will contain of the works of each author, what it will weigh; how many cases will be necessary, what dimensions, and what space they will occupy.

" The Emperor is also desirous that M. Barbier should engage in the following work, with one of our best geographers;—to reduce from memoirs upon the campaigns that have taken place on the Euphrates, and against the Parthians, setting out from that of Crassus, to the 8th century, comprehending those of Anthony, Trajan, Julian, &c. tracing upon maps of a suitable scale, the route which each army has followed, with the ancient and modern names of the countries and principal towns; geographical observations on the territory, and historical relations of each expedition, derived from original authors."

The second note is dated Schoenbrunn, 12th June, 1809. It urges the formation of a portable library, and extends the order to 3000 volumes in 18, similar to the Dauphin collection in 18—to be printed in Didot's most beautiful type, on thin vellum paper. The 3000 volumes were to be placed in thirty cases, each containing three shelves of thirty-three volumes each.—*Rev. Ency. Dec.* 1826.

16. *Portable Library.*—There exists at Erfurt in Germany, an association which may be successfully imitated in ma-

ny other places. Its design is to instruct, while it amuses, those children whose parents have not the means of procuring books. A "Society of the friends of youth and the promotion of knowledge," has been formed for the purpose of making a collection of books, which are lent to children under the responsibility of their parents, at the rate of about one cent per volume, and two cents when the volume contains plates. The modest income arising from this source, is employed in the purchase of new books. The books are carefully chosen by the directors, who have made an appeal to parents in easy circumstances, for the purpose of increasing their means of usefulness by donations of books, &c.—*Idem.*

17. *Education in France.*—The following impressive exhibition of the state of education in France, was given by CH. DUPIN, in his introductory lecture at the opening of the normal course of Geometry and applied mechanics, on the 29th of November, 1826.

"I present to your notice a map of the kingdom, which represents by shades, more or less deep, the degrees of ignorance or information which prevail.

Those departments whose primary schools contain the tenth of the whole population, are coloured with the deep tint of no. 10; those departments whose schools contain only the 20th part of the total population, are coloured no. 20; those whose schools contain only the 229th part of the population, I have coloured in black, no. 229, &c.

What then, you will say, does France contain departments where there is but one child at school, in a population of 229 inhabitants? Yes, gentlemen, such a state of things does exist, and even still worse. But, it will be observed, this must be immediately in some corner of Lower Brittany? No, gentlemen, Lower Brittany is rather better. It has schools which contain the 222d part of its population. It must then be on the summit of the Alps or the Pyrenees, where the poor inhabitants have to struggle against eternal frosts and avalanches, in cultivating a contracted territory. No, gentlemen, the inhabitants of the Upper Alps and Upper Pyrenees are among the number of those whose popular instruction is the most diffused; because nothing gives so much moral energy to a people as to have to struggle against natural obstacles. That obscure place, where only the 229th part of the human species frequent the schools, is in the middle of the kingdom, in a wide valley, under a mild and serene sky, in the region of the vine, the mulberry and the maize, on the borders of a superb river; it is called the garden of France; it is Touraine.

Look, on the contrary, at the foot of the Pyrenees, the country of Henry the Great, *Bearn ;* it contains in its schools the 15th of the total population; and it is in the vicinity of this fine country, formerly called the garden of the Hesperides, the garden of the West, that we find the country whose deep colouring, proportioned to its present ignorance, relieves me from the necessity of pronouncing its name.

In drawing the narrow dark line, which you observe, from Geneva to Saint Malo, we separate the north from the south of France.

On the north are thirty-two departments, and thirteen millions of inhabitants; on the south fifty-four departments, and eighteen millions of inhabitants.

The thirteen millions of the north, send to school 740,846 young people; the 18 millions of the south send to school but 375,931 pupils.

Let us now observe some of the remarkable consequences resulting from this disproportion.

In the north of France, notwithstanding the rigour of the climate, the intelligent industry of the people enables them to obtain from the soil, a revenue which is sufficient to pay 127,634,765 francs of the national impost, for a surface of 18,692,191 hectares; while the fifty-four departments of the fourth, pay only 125,412,969 francs, for a surface of 34,841,235 hectares.

Thus, for a million of hectares, the public treasury receives from the enlightened portion of France, 6,820,000 francs, and from the dark portion, 3,599,700.

The superiority of the public revenue furnished by the enlightened portion of the kingdom, is also particularly obvious in the patent tax, which is levied at an equal rate throughout the kingdom.

The thirty-two departments of the north close a patent account with the public treasury of 15,274,456 francs, and the fifty-four southern departments, only 9,623,733 francs. Hence, favoured by superior industry and information, a million of Frenchmen on the north side of the line, pay for the patents of their arts 1,174,958 francs, and a million on the south only 534,662 francs.

I have examined the list of patents (brevets d'invention) from July 1, 1791, to July 1, 1825, and the following are the results:—For the 32 northern dep. 1689 patents—the 54 southern dep. 413 patents.

The University of the kingdom decrees to all the colleges of Paris and Versailles, an immense number of prizes of three grades, according to merit. The almanac of the University contains the names and birth place of all the successful candidates. After subtracting all that were born in Paris, in order to avoid giving too great an advantage to the north, the following is the result:—Rewarded pupils of the 31 northern dep. 107—54 southern, 36; that is, one third. And what is more, of these prizes 37 were of the first degree; and of these, 33 were assigned to students of the north.

Of the pupils of the *polytechnic schools*, for 13 consecutive years, I have found that of 1933 admitted, 1233 are from the north, and 700 from the south.

The *Academy of Sciences*, to which all France gives this testimony, that it chooses its members with independence, and consequently with equity, from all the savans of the kingdom, presents a result still more favourable to the inhabitants of the north. Of the 65 members who compose the academy of sciences, the 32 departments of the north have 48, and the 54 departments of the south have only 17. Consequently, to furnish one member of the academy, there must be 15,434 children at school in the north, and 22,113 in the south.

I have reserved, as the last object of comparison, those noble rewards which the government grants, at the periodical exhibition of the products of national industry. The following at the exhibition of 1819, was the proportion of the prizes.

	32 dep. of the north.	54 dep. of the south.
Gold Medals,	63	26
Silver Medals,	136	45
Bronze Medals,	94	36
	293	107

The exhibition of 1823, gives still more striking results.

Rev. Ency. Jan. 1827.

18. *The American Journal of Science*,* and the *North*

* The importance of the favourable opinion of our respected collaborators abroad, to our success at home, is our *honest apology* for inserting this commendation.—Ed.

American Review, though different in their objects, are analogous in their aims with respect to human knowledge. Both of these works appreciate it, in proportion to its utility, and never lose sight of the *cui bono?* There is no reason to fear that metaphysics will ever invade the numbers of Mr. Silliman, while natural history, mechanics, physics, chemistry, and the various applications of these sciences, supply them with materials; and if the pretended science, which is dignified with the name of *speculative philosophy*, appears in the N. A. Review, it is only for the purpose of being tried as a vagabond, arraigned before a magistrate.

In these two Journals, equally distinguished by patriotism and the love of truth, various and accurate information may be obtained in relation to the soil, the arts, and the moral and political condition of the United States. In the *American Journal of Science*, the geological discoveries made in America are an object of curiosity and instruction to European readers; and articles like that in the number for June, on the coal of Rhode Island and Pennsylvania, will, in point of usefulness, have no local limits—it will be consulted beyond the Atlantic. We shall borrow, from time to time, from both these works, materials well adapted to our *Revue*, and which our readers would reproach us for having left unnoticed. *Id.*

19. *Memoir on living animals found in solid bodies*; by M. VALLOT, physician of Dijon; read at the Academy of Sciences, Nov. 20, 1826.—The author divides his memoir into nine chapters. 1. *On living worms found in stones.* 2. *Do. found in wood.* 3. *Living fish in the earth.* 4. *Do. in stones.* 5. *Serpents in stones.* These are only ammonites. 6. *Living dogs found in stones.* This is alluded to only for the purpose of ridiculing an unauthenticated and idle tale. 7. *Living toads found in stones.* This is the most important chapter, on account of the numerous statements which have been made of such discoveries. The author's conclusion is, that there is no unquestionable evidence of the existence of such phenomena. He thinks that Ambron Paré, who states that he was an eye witness of a discovery of this nature, was mistaken; and that Bacon was also led into an error on this subject. In short, the author thinks that the term *toads (crapawds)* is only the name given by stone workers to cavities found in stones, and which by mineralogists are called *geodes*. If living toads have ever been found in such

doubtful situations, he conceives that what is called stone is only a block of earth into which the animal had entered for the purpose of hibernating. 8. *Living toads found in timber.* Cases of this nature, which have been mentioned by respectable people, the author ascribes to mere hibernation, and that the opening by which the animal entered has been unnoticed by the observer. 9. *Frogs in stones.* All the accounts of this nature are supposed to have passed from hand to hand without authority, or that frogs may have fallen into certain holes where they have found moisture enough to support life.

M. Blainville, after commending the spirit in which the memoir of M. Vallot is written, stated his belief that the author had not satisfactorily accounted for the numerous precise relations which have been made of events of this nature, such as engravings representing the animal in the stone which enclosed it. M. Blainville, declaring that he had no opinion of his own relative to the reality of the phenomenon, acknowledged that he could conceive the possibility of it.

M. Edwards, after bringing into view his researches on the same subject, stated that M. Colladon had spoken to him of a toad found in a stone, of which he was an eye witness.

Ferussac's Bulletin, Jan. 1827.

Note. As numerous occurrences of this nature have been related in our journals and newspapers, and as the facts are highly interesting in a physiological point of view, it would be rendering a real service to truth and science, if some person qualified to make a just estimate of probabilities, would embody in a single essay in this Journal, the facts most worthy of reliance in relation to the existence of living animals in situations so confined as to prevent locomotion, and to which atmospheric air can scarcely find access. G.

20. *Chemical process of Respiration.*—Prof. Mayer, of Bonn, combats the opinion of Allen and Pepys, who infer from their experiments that oxygen does not pass into the circulation from the lungs, but merely goes to form carbonic acid, which is expelled. To prove the contrary, Prof. M. killed an animal by strangulation, opened immediately the thorax and pericardium, divided the aorta and pulmonary artery, and injected into the latter distilled water, until all the blood had been washed out from the lungs, and the water re-

turned quite clear by the portion of the aorta which remained attached to the heart. This done, he injected into the pulmonary artery a green solution of the chamelion mineral which had been well protected from the air: it returned to the aorta unchanged in colour. He then tied the aorta, injected a fresh portion of the same fluid, tied the pulmonary artery, inflated the lungs, and kept up, during several minutes, an artificial respiration. The injected liquid soon acquired in the pulmonary vessels a fine red colour.—*Ibid.*

Note. This experiment only proves that when the lungs contain no venous blood, but merely a fluid which has a strong affinity for oxygen, the latter may pass through the parietes of the lungs and form the combination. It could not before have been doubted that the coats of the pulmonary blood vessels are permeable to air, since the carbon finds its way through them. The experiment of Prof. M. has too little analogy to the living function to justify any inference, we think, with respect to the absorption of oxygen by the blood; and, in our opinion, the results of Allen and Pepys stand unimpeached. G.

21. *Thermometer.*—M. Skene, a lieutenant of the royal marines, who accompanied Capt. Parry, in 1820, proposes a new division of the thermometric scale. His plan is to consider the space between the freezing or melting of mercury and the freezing of water, as one degree, and divide it into 100 minutes, and to extend this division to the higher portions of the scale. Between the freezing and boiling of water there would be about 2½ degrees. Zinc would melt at 9 degrees, &c. These numbers would be more easily retained in the memory than those in common use. The graduation of thermometers on this plan, would, it is true, be more difficult than at present; but, by being confined to the most skilful hands, greater uniformity and perfection would be secured.—*Revue Encyc. Mar.* 1826.

II. *Annales de la Societe Linneene de Paris.* The Linnæn Society of Paris has been noticed in several former numbers of this Journal. The *Annals* of the Society, a valuable scientific journal, occupied principally with original memoirs, are published in numbers every two months: the six numbers, at the close of each year, compose a volume of about eight or nine hundred octavo pages—the subscription price at Paris is 18 francs per annum. We have just received the number for January, 1827, which completes the fifth volume. The Society includes within its scope every branch of natural science, with the application of science to the arts; and the annals are diversified with an interesting variety of matter.

The fifth volume contains two elaborate memoirs on the *nectary* of plants; one by M. Soyer-Willimet, the other by M. Desvaux—the latter a prize essay. A new methodical arrangement of the known mosses, by M. G. A. Walker Arnott, of Edinburgh, with notes, &c. by M. B. Kittel, composes a very long and elaborate article.

The Paragrêle, or Hail Rod, (see this Journal, vol. x. p. 196) has for several years occasioned much inquiry on the continent, and has engaged the particular attention of the Society. In many districts, which were formerly, year after year, devastated by hail, the instrument has been adopted with complete success, while in neighboring districts, not protected by paragreles, the crops have been damaged as usual; and the Society are receiving from all quarters statements which fully confirm their opinion of the utility of the invention. The Society have made a report to the ministers of the interior, recommending that measures be adopted by the general government for protecting the country from hail; and it is estimated, from the result of experiments in numerous districts, that if paragrêles were established throughout the whole of France, it would occasion an annual saving to the revenue of fifty millions of francs.*

The Ergot is the subject of a memoir by M. Léveillé, who describes it as a parasitic fungus, under the name of Sphacelia segetum. He seems to credit the opinion that this substance will produce convulsions and dry gangrene. No mention is made of our countryman, Dr. Stearns, who first made known that property for which the ergot is now medicinally employed. (See New-York Med. Repository, vols. v. and vi.) M. de Serres has given an interesting notice of fossil bones, found in caverns of limestone situated in the environs of Lunel-Vieil, near Montpellier. In a memoir on sound, M. Gi-

* To most people of the United States, where hail storms are uncommon, and the consequent damages very inconsiderable, this estimate may seem extravagant; but in countries in which at least one-fifteenth of the whole annual crops is destroyed by hail, this subject is viewed with interest.

rose de Buzareingues maintains that sound is not generated by vibrations of the air, but depends on a peculiar fluid. An eulogy on Thomas Jefferson, who was an honorary member of the Paris Linnæan Society, delivered before the Society by M. Lemesle, is published in the annals. M. Masson-Four has announced to the Society his intention of publishing in Paris a translation of Dr. Van Rensselaer's Lectures on Geology, with Notes.

M. le Chevalier Soulange-Bodin, President of the Society, who has for several years devoted his fortune and time to the formation of a great horticultural establishment, solicits the aid of travellers, botanists, &c. of all countries, in forwarding to him roots, seeds, &c. of rare and interesting plants. Packages may be forwarded to him, directed "au Havre, à M. M. Eyries frères négocians; Jardin de Fromont, à M. le Chevalier Soulange-Bodin, à Paris, rue St. Anne, No. 44."

C. H.

III. *Notice of the Heidelberg collections of Rocks and Petrifactions.*—In Vol. X. p. 197, we mentioned that collections of minerals might be obtained, either by purchase or exchange, of Mr. Frederick Moldenhauer, of Heidelberg, Germany. We have recently received a collection from him, and it is at his request, and in compliance with the wishes of Counsellor Leonard, Professor of mineralogy in the University of Heidelberg, as well as from a desire to promote the cause of mineralogy and geology, that we publish the following remarks, communicated from Heidelberg for insertion in this Journal, with letters from Prof. Leonard and Mr. Moldenhauer.—Ed.

We flatter ourselves that we shall render a service to the study of geology, by an undertaking, in which, as every competent judge must perceive, nothing but the love of science would induce us to engage. It is well known that collections of rocks, somewhat complete, belong to a class of objects not easily acquirable; because of their offering too small a profit in business, and being therefore unfit for mercantile speculations. But the acquisition of a large number of petrifactions, which determine the geological character of a formation, was still more difficult, and even impossible until now; notwithstanding that the actual state of science requires, that of every collection of rocks, which is to be used with equal advantage, both for study and instruction, petrifactions must form a part.

This want, so severely felt by every friend of geology, we propose to relieve, by furnishing *Collections of Rocks and Petrifactions*, in complete and characteristic specimens, not surpassed for excellence in their kind.

For the accommodation of our respected correspondents, they shall be furnished within the term of six months and at the lowest freight. Each of these parcels, sent at any one time, shall consist of from 50 to 60 specimens, both of rocks and petrifactions; the first of the size of 12 square inches, the whole selected in the best manner, omitting superfluous duplicates and uninteresting varieties.

Every specimen shall be furnished with a label indicating the systematic nomenclature, in German, in French, and in English, and the locality besides. Every parcel shall contain, as far as possible, specimens of all the principal varieties of petrifactions; so that those to whom they are sent, may immediately arrange them according to the most approved geological systems of "Humboldt," "Boué," &c. Finally, towards the conclusion of a delivering,* there shall be furnished a catalogue raisonnée to the whole, which shall be furnished within 8 to 10 of the above said terms. In relation to this object, which we have long contemplated, we have made such arrangements that we do not fear any interruption.

We shall begin to dispatch our collections at the beginning of next June, (1827) and the subscription will not close before that time. The price of each parcel sent is (illegible, Ed.) and for this amount, for the convenience of the subscribers, we shall draw on them at two months from the date of the invoice. We profit by this opportunity to recommend our institute to the favor of the public. We have always on hand a great choice of minerals, in single specimens, as well as in systematical collections, at the lowest rate, and catalogues of our minerals are furnished gratis.

Heidelberg, Dec. 1, 1826.

* This is the word in the original paper, meaning what in French would be called *envoi*, viz—the things sent and the act of sending at any particular time. We have no single English word that exactly expresses the idea.—Ed.

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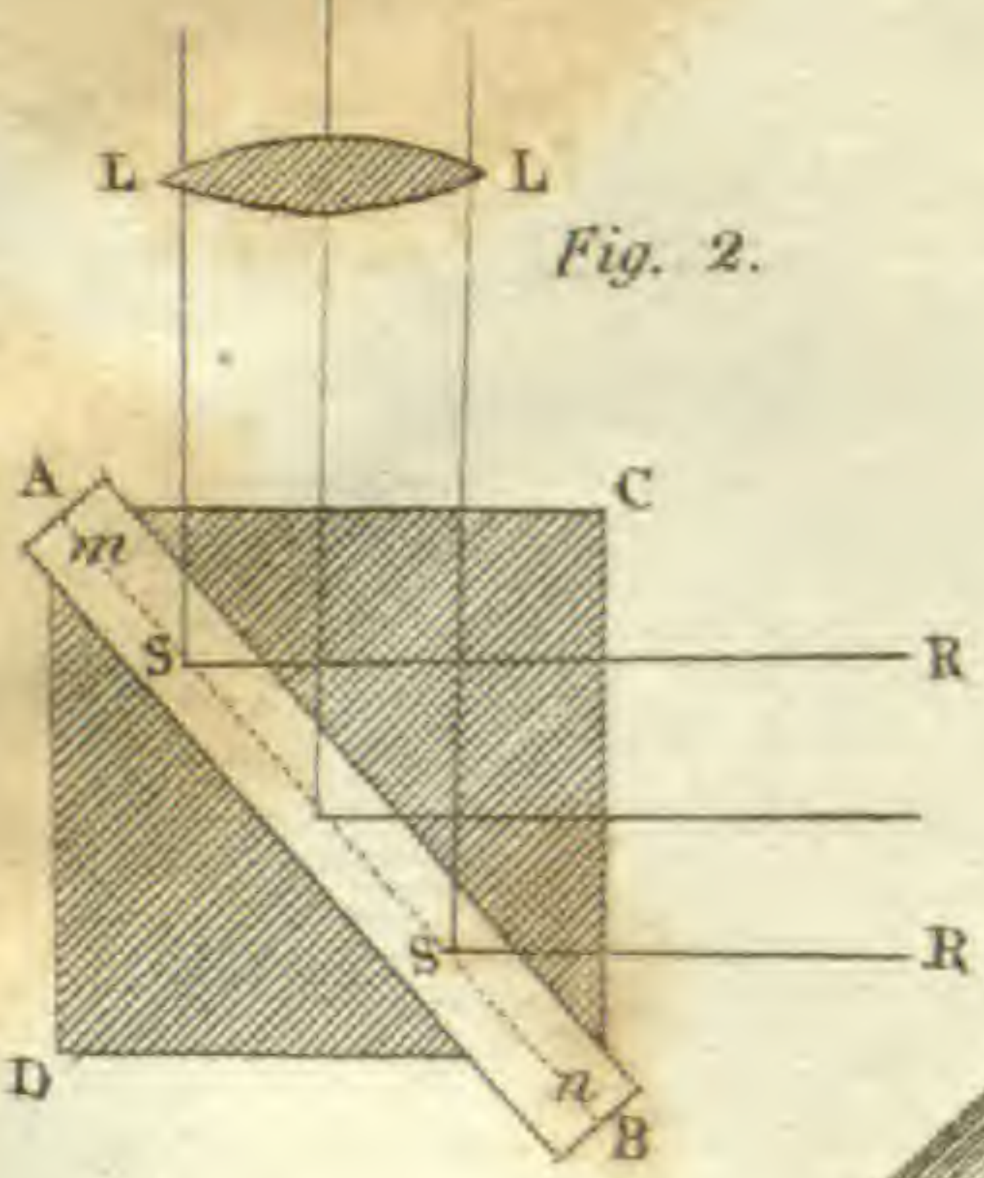

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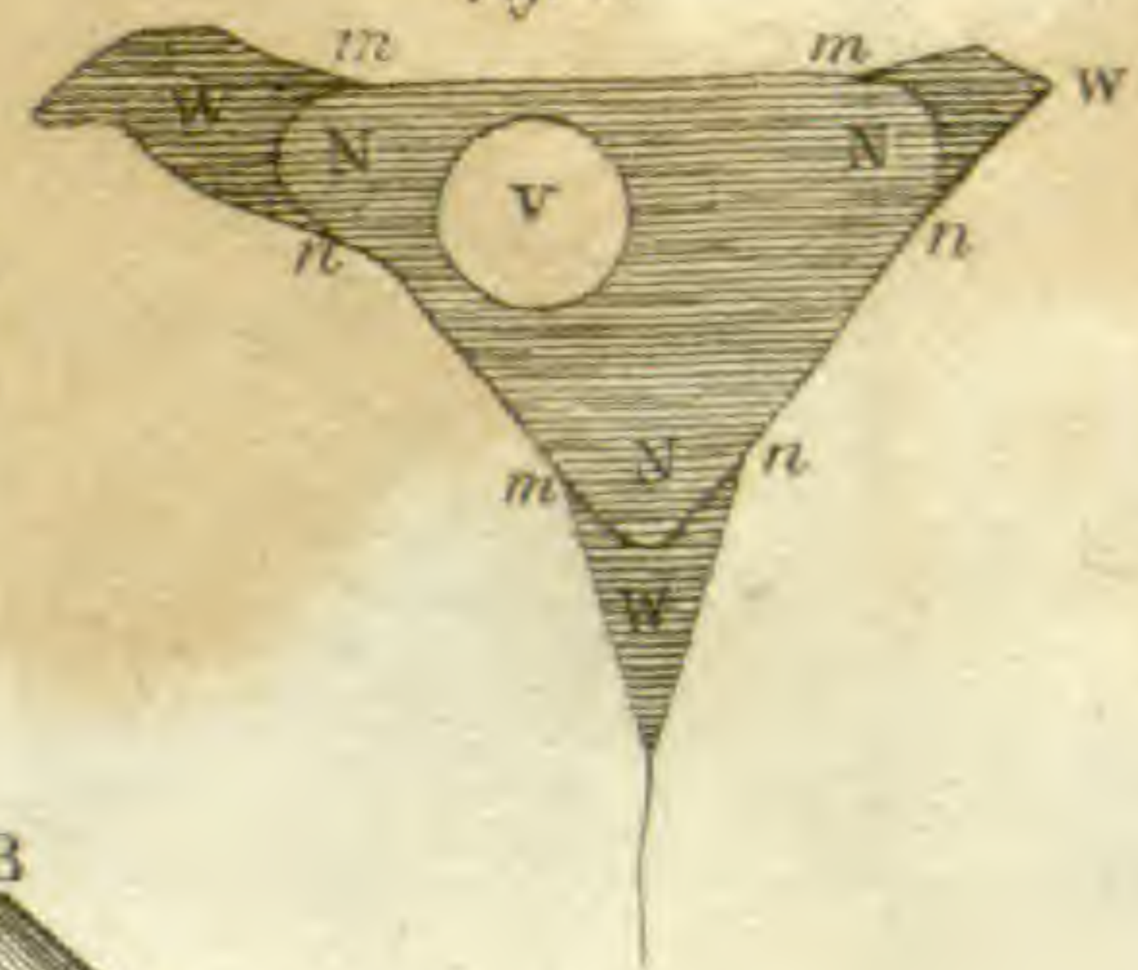

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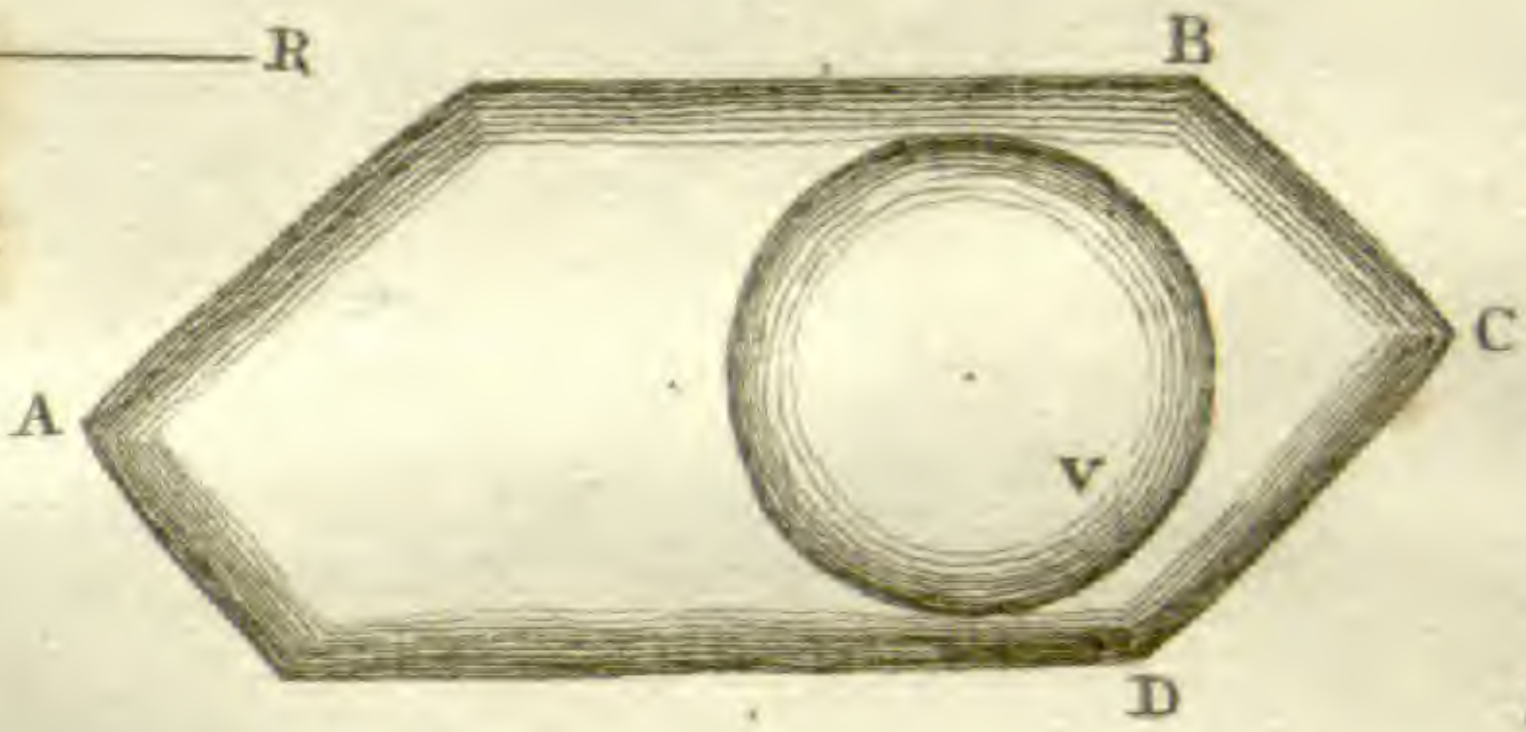

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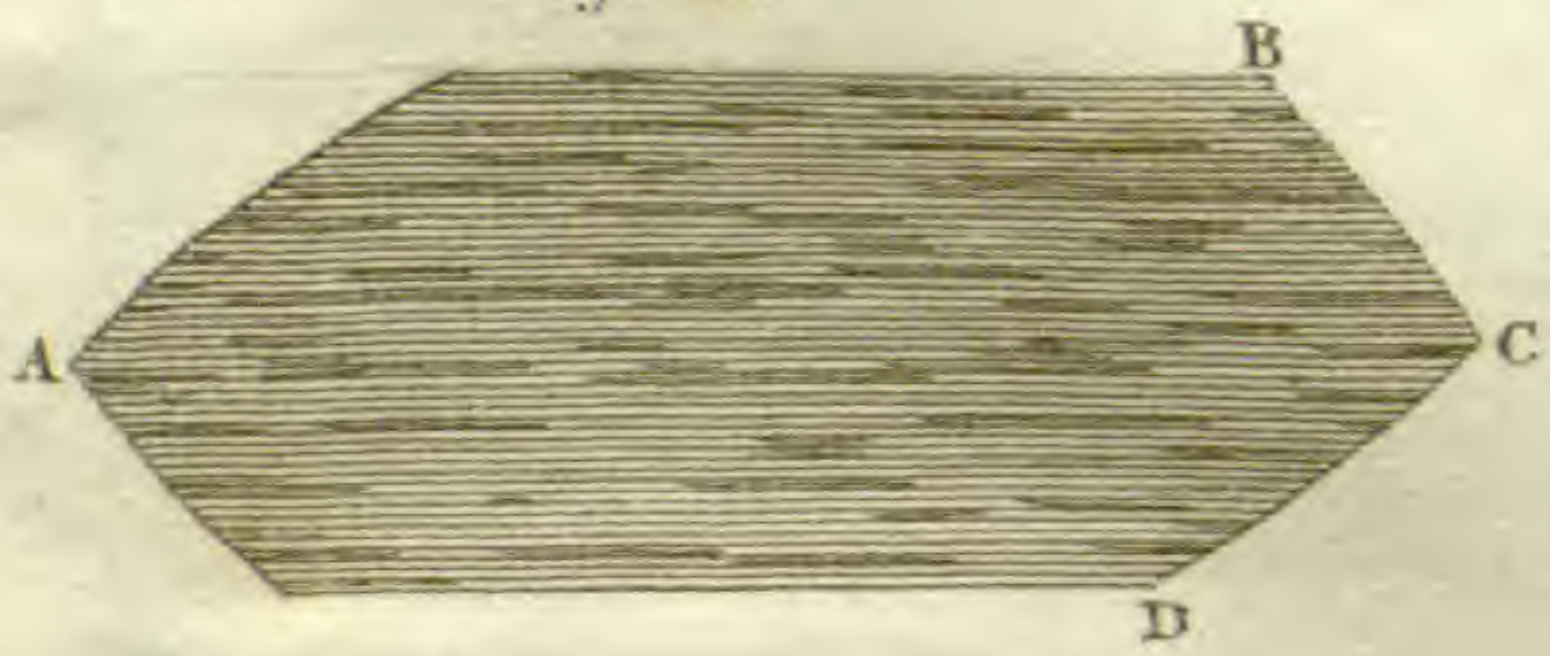

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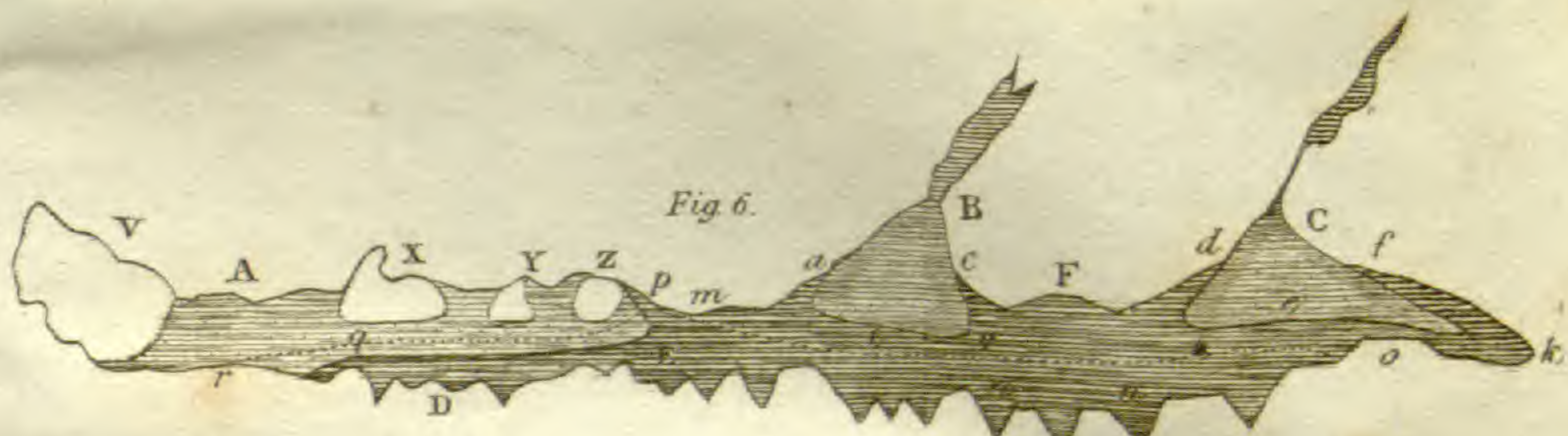

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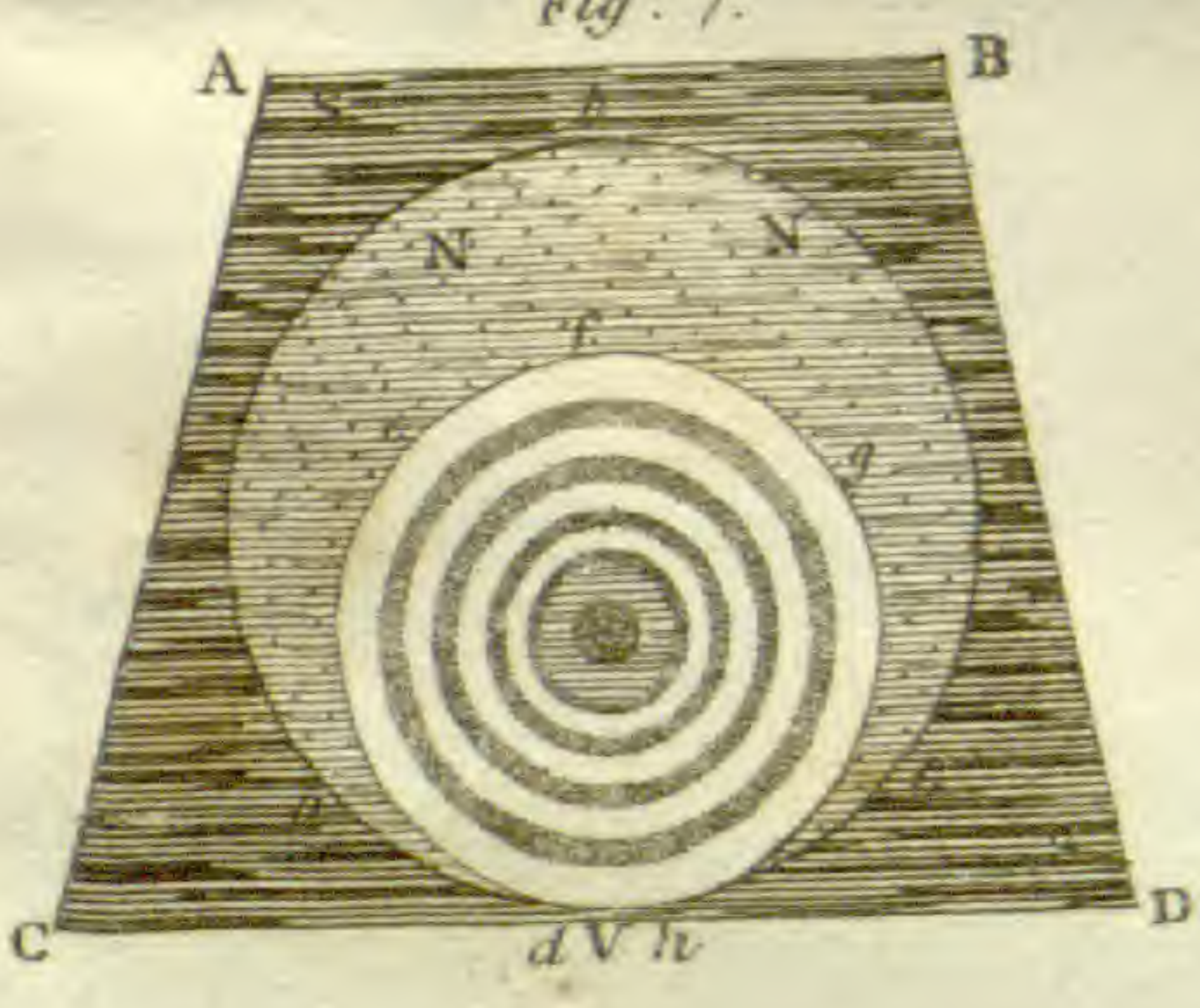

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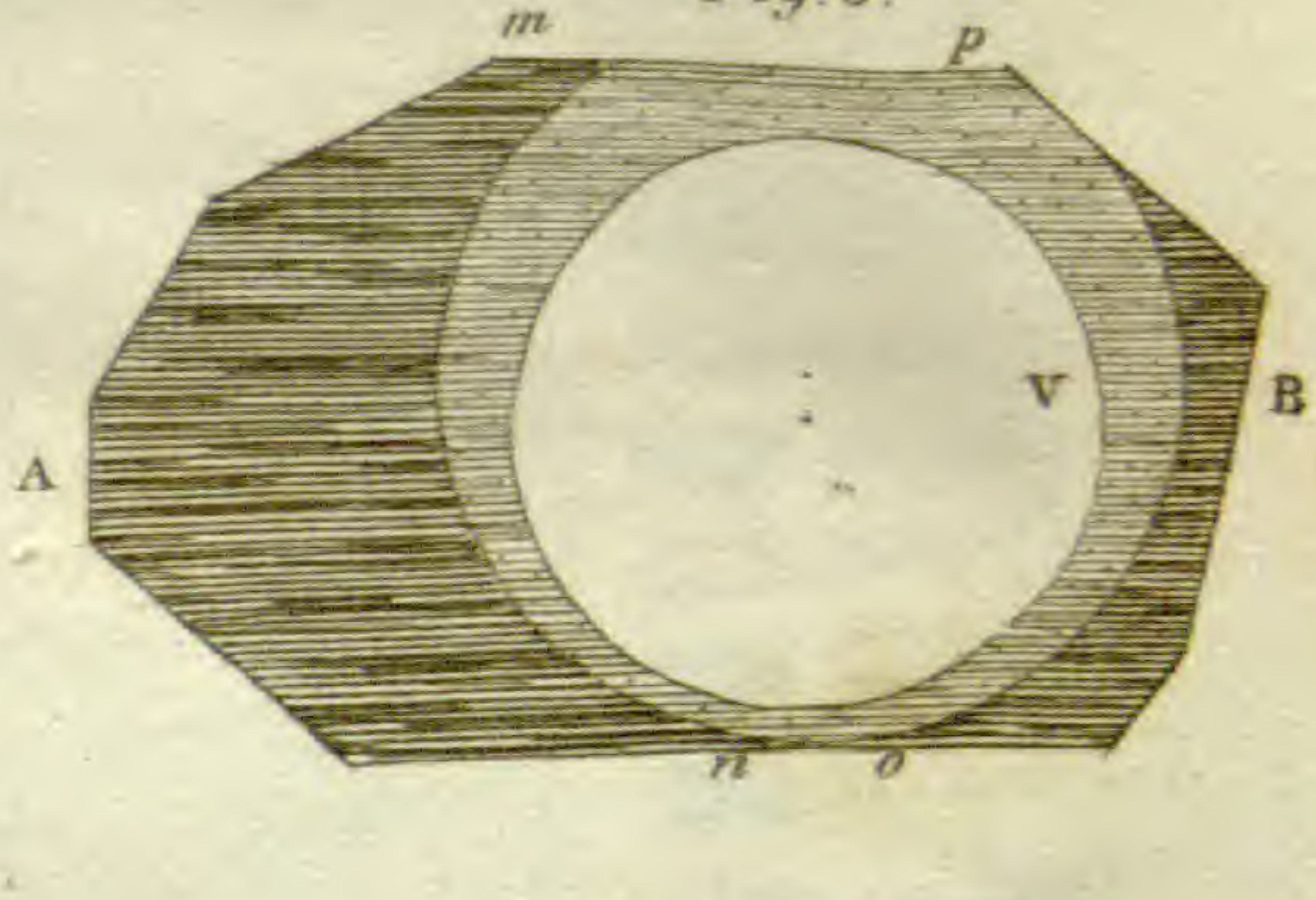

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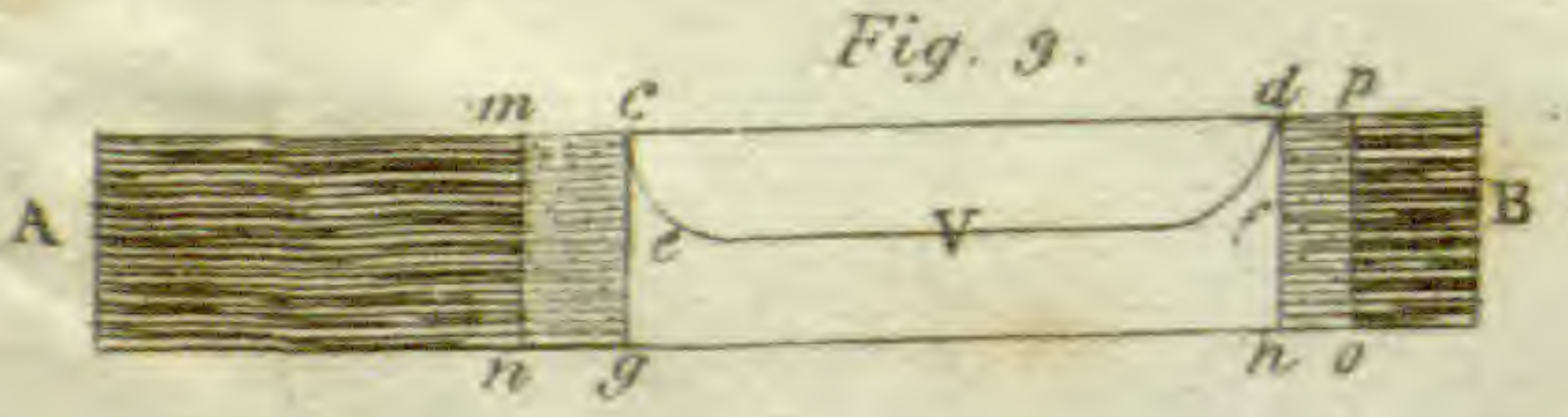

Fig. 10.


A. Doolittle sc.

GEOLOGY of the LEAD MINES and VEINS of HAMPSHIRE COUNTY, MASS.

Tab. P.

Fig. 51.

Carex Barrattii.

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Fig. 52.

C. cespitosa var. ramosa.

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Fig. 50.

C. filifolia Nutt.

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