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OF THE

QUARTERLY JOURNAL.

No. XXXIX.

ART. PAGE

TO OUR READERS AND CORRESPONDENTS.

A complete General Index to the first Twenty Volumes of this Journal is preparing for publication.

An anonymous Communication from Portsmouth cannot be inserted.

"A Chemical Student" will find the difficulty to which he alludes fully explained in our Review of Dr. Thomson's "*Attempt.*" &c.

Communications have reached us from Dr. Wilson, Dr. Mac Culloch, Mr. H. Burns, Mr. Swainson, and Mr. Wells: these have of necessity been deferred.

We are quite willing to listen to all reasonable suggestions respecting the improvement of this Journal, but the complaints of "a Mechanic" are without foundation: we must remind him, that "ex nihilo, nihil fit." Had we blazoned forth, as did some of our contemporaries, either of the schemes to which our friend alludes, we should have deservedly incurred the blame of hastiness and credulity.

The reports alluded to by F. R. S. are false, as relating to the Royal Institution, and very far from correct as concerning the Royal Society: upon *such* grounds, we decline publishing his letter: to shew that no other feeling prevails, we shall be happy to insert it in our ensuing number, provided he will allow us to attach a commentary of our own.

The review of a work inquired after by our correspondent at Warwick will probably appear in our January Number.

We are apprehensive that the communication with which we have been favoured by "a Regular Reader" respecting the manufacture of chloride of lime, would not be intelligible without a drawing.

Mr. BRANDE and Mr. FARADAY will commence the Lectures and Demonstrations in Chemistry in the Laboratory of the Royal Institution, on Tuesday Morning, the 11th of October, at nine o'clock precisely.

A Prospectus may be obtained at the Royal Institution. *(See page 203.)*

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QUARTERLY JOURNAL,

No. XL.

II. Chemical Science.

III. Natural History.

TO OUR READERS AND CORRESPONDENTS.

The coals of the vicinity of Bath are very sulphureous—perhaps it is their vapour, and not their light which bleaches Arabella's dresses. The steam of 300° must be issuing with great violence from a small aperture; *expansion* then so much increases its capacity for heat that it does not scald till the pressure is considerably diminished. Although the steam *in* the boiler may be 300°, that which *first issues* is not above 120°.

Mr. W. Jones informs us that, in 1822, he proposed to Messrs. Symons and Co., of Leman-street, a plan precisely similar to Mr. Jeffrey's, for the condensation of smoke: we should be glad to know whether there, or elsewhere, it has been successfully carried into effect?

Captain Jeremie's observations on East India opium have been received, but the sample has not yet reached us.

H. M. is informed that ice evaporates, as well as water.

The hints of our Correspondent at Dover shall be attended to. He is surely wrong respecting our general index.

In reply to a letter dated "Somerset, 22d September, 1825," we refer to what is called a Fossil Human Skeleton, preserved in the British Museum; and to Mr. König's paper on the subject, published in the Philosophical Transactions.

Mr. Van Rensselaer's communication has been received; his former one never came to hand.

We must again decline publishing the Letter of F.R.S., inasmuch as he reasons upon wrong grounds. The Library of the Royal Institution, as well as that of the Royal Society, are both easily accessible, and under certain restrictions, the Fellows of the Society are allowed to take books from the latter.

Several instruments have been invented for the destruction of calculi upon the principle of Colonel Martine's, but there are very few cases which admit of their use.

Communications have been received from Dr. Johnson, Dr. Ure, Mr. Gregory, Mr. Stromeyer, Mr. Faraday, and Mr. Horner.

A copious General Index to the Contents of the first Twenty Volumes of this Journal, which are now completed, is ready for the Press, and will be delivered with the next Number.

THE

QUARTERLY JOURNAL.

October, 1825.

Art. I.—*On the Means by which Crabs throw off their Claws. By* Dr. Mac Culloch.

[In a Letter to the Editor.]

Sir,

It is well known that the tribe of crabs, using this popular term to comprise many genera of modern naturalists, have the power of parting with their claws by a voluntary effort; and they are thus frequently taken, with one or more of these deficient, or of an inferior size to the rest, since they have also the power of reproducing them. As the nature of this singular process has never been described, and as it appears, at first sight, as it has always been conceived, a very unaccountable effect of voluntary or muscular power, I am induced to send you a sketch of the anatomy of the parts engaged; with an explanation of the mode in which the animal appears to detach the limbs.

This process appears to be effected by these animals whenever so serious an injury has been committed on any of the extreme phalanges as to render the claw useless; no provision having been apparently made to repair any injury of those parts, although there has been one established for reproducing the whole limb. It is very easy to witness this effect when the animal is recently taken out of the water and in a vigorous state; but a very short time is sufficient, in most species, to render the animal too feeble to perform that for which the utmost effort of its muscular powers appears to be requisite. Being unwilling to propagate a method of adding to the torments of animals for the mere gratification of that cruel curiosity in which physiologists have too often, and with justice, been accused of indulging, I will not here communicate the method by which the animals of this tribe may be induced to perform this voluntary mutilation; but it is rather too well known to children who have been brought up on sea-shores.

It is, nevertheless, doubtful whether the effort itself is attended with pain, whatever the previous torment may be, as it seems often to be done without any apparent motive. The whole tribe is of an extremely ferocious and irritable character; and with many, particularly of the younger animals, the mere attempt to take them, even without actual contact, causes them to drop the two hands, or larger claws. Others do it if confined in a box or a glass of water; and almost all of the smaller kinds, or half grown ones, part with any claw by which it is attempted to retain them.

To render the description of this process intelligible, it will be necessary to understand the general structure of the limbs of this tribe. The accompanying sketch, (Fig. 1, Plate I.) is from one of the small claws of the spider crab, being the species just at this moment under my eye. The structure of the several parts, as well as the form of the limb, is of course somewhat peculiar to this species; but the difference is not material among the whole, and the same explanation and drawing will, with slight modifications, apply to all the instances in which it occurs. Even if I had not accidentally had this species at hand, I should have preferred it; as the structure of the suture where the separation takes place, is rather more distinct and remarkable than in the other species which I have examined.

The limb of a crab consists of six parts, each of which has an appropriate motion in two directions, by means of a pair of muscles adapted to the nature and extent of the space to be described. In the extreme joint, the motion of direct flexion, towards the body, is considerable; but the extension is limited by the structure of the shell; and these motions are produced by two long muscles occupying the second phalanx. The motion of this phalanx upon the third, is lateral in two directions, and tolerably extensive, but less so than any of the direct motions. It is performed by a pair of muscles of considerable power, which lie in the third phalanx. The motion of this on the fourth, is a motion of extension and flexion, like that of the first; the latter being, in the same manner, considerable, while the former is limited; and the muscles that produce them are of considerable power, as occupying the great length of the fourth phalanx.

The fifth phalanx is very short, and is articulated to the fourth in an oblique manner, by a very narrow ligament; while the lower shell surmounts the upper one in such a manner as to limit the motion of the latter on the former to a very minute space laterally, which is also somewhat more considerable in one direction than another, but which varies, in this respect, in different species of crabs. These lateral motions are produced by two broad, but very short, muscles; as it will be shortly seen that but a small part of this fifth phalanx can be allowed for them, a great part of it being allotted to the arrangement provided for detaching the limb.

The sixth, or last, phalanx, is extremely short, and is articulated by a somewhat lax ligament to the preceding, which admits also of a direct motion like those of the first and third; but which is, on the contrary, much more free and extensive backwards than forwards. This phalanx is also connected with the former, by short and feeble muscles; but the principal motions in this fifth phalanx, which are amongst the most powerful in the limb, are produced by two long muscles, a flexor and an extensor, which pass quite through the sixth phalanx, and are inserted within the body of the animal. The fifth phalanx therefore moves immediately on the body, independently of the sixth, or is, at least, capable of so doing.

The last, or sixth phalanx, is connected with the body by a very wide ligament, admitting of a very considerable lateral motion, but more extensive inwards than outwards, and allowing the limbs to be folded under the body. Some short muscles are appointed to this office; but it is unnecessary to enter into further particulars; as enough is given in this sketch of the motions of the limb and the position of the muscles, to illustrate the object of this paper. The beauty of the mechanism, by which strength of articulation is combined with extent and facility of motion, cannot fail to strike the most negligent observer of nature.

It is in the fifth phalanx that the provision for detaching the limb is placed, and the accompanying sketches will assist in rendering the description intelligible.

It will be perceived that, on the exterior side of this phalanx, there is a pale ring, transversely drawn round it (Plate I. Fig. 1.); and, with a magnifying glass, it is easy to perceive, though much more distinctly in some species than in others, that a very fine line lies in the middle of it, not exactly in one plane, but slightly undulated. (Plate II. Fig. 6.) If the edge of a knife be forced upon this line, it will be found that the phalanx, at the moment it yields, flies asunder with a loud crack. In every part of the shell, the bony matter is deposited in a fibrous manner, transversely to the plate; but at this particular part, the fibres are peculiarly fine and straight, while the structure is also more brittle or tender and the colour paler, (Plate II. Fig. 8.) The division is, in fact, a natural suture; nor is it possible to separate the joint in any other place than where this exists.

If now a longitudinal section of the whole phalanx be formed, the appearance represented in Fig. 7 will be seen. It will be observed, that the suture occupies a thinner part of the shell, or that this portion is more slender than the general shell of the limb, and that it is bounded on each side by two reinforced rings. This is the case at least with the spider crab; but it is less visible in some other species which I have examined, although the peculiar structure of the suture, in other respects, is always to be distinguished. I must nevertheless remark, that in some, as in the lobster and crawfish, the external ring is by no means conspicuous, though the same provision exists in the internal arrangement, nor is it very obvious, externally, in the cancer pagurus. In the same figure, there is a rude representation of the position and insertion of the muscles, as this is necessary for understanding the nature of the action by which the limb is detached.

It will be seen that the two short muscles which produce the confined lateral motion between the fourth and fifth phalanges, or at A, (in Fig. 1, Plate I.) are inserted above the suture, (Plate II. Fig. 7,) and that the flexor, which alone is visible in this section, and which bends the joint at B, (Plate I. Fig. 1,) is attached below it; the opposing extensor, not visible in this drawing, because occupying that part of the shell which has been removed, has a similar insertion. Thus there is left a vacant space between the two sets of insertions, on each side of the suture; and this, according to the species or size of the animal, varies from a quarter to the eighth of an inch. In the living state of the animal, this is filled with a mucilaginous matter, which coagulates on boiling, so as somewhat to resemble the curd of milk; and which, after the voluntary separation of the limb, forms a protection to the ends of the last pair of flexors and extensors, and to the cavity of the joint. In Fig. 6, these muscles, by which the action of detaching the limb is produced, are represented as separated from their inferior insertions within the body; bearing here the same proportion to the fifth phalanx, as they do in the species from which this drawing was made.

It is now necessary to remark, that, in proceeding to detach the limb, the animal frequently throws the whole of the limbs into a state of violent extension, remaining perfectly rigid, as if under the operation of a tetanus or universal spasm. In other cases, the injured limb alone is so extended; and it is probable that these differences depend on the state of vigour or debility in the animal. When feeble, this action is often continued for some time, or relaxed, and again renewed, without producing the desired effect. But, when the animal is sufficiently powerful, the limb suddenly drops off at the suture, with a loud crack, in a second or two after the extension.

This singular process seems, at first sight, to be capable of explanation, by considering the structure above described, and the positions and actions of the last pair of flexor and extensor muscles; and thus it has been attempted to explain it. It is obvious, however, on a moment's consideration, that no actions of these muscles, however powerful, could alone produce the consequence in question; as they could have no further effect than that of fixing that part of the fifth phalanx, which is below the suture, more firmly on the body. To explain the mode in which their action is rendered efficient towards the separation of the limb, it is necessary to describe the forms and relations of the fifth and sixth phalanges more particularly; as, in these, the true solution of the difficulty will be found. It is from inattention to these important circumstances, that this process has appeared so mysterious; and assuredly it is difficult, on a first view of the operation as performed by the animal, to witness it, without surprise at the facility with which it is effected, and at the apparent inadequacy of any conceivable means for producing the separation. The obvious effect of muscular action, is to approximate the insertions of the muscles; yet here it appears to act as if it was to separate them.

As it is impossible to render this structure intelligible without drawings, some sketches are added for that purpose; and, as being more obvious in the common crab, (*Cancer pagurus*,) they are taken from that species. They are limited to the fourth, fifth, and sixth phalanges; being the only ones required for illustrating the subject; and these are numbered, so as to correspond to Fig. 1, Plate I. The place of the suture is indicated in these sketches, as it is not so defined externally as in the spider crab. In Plate II. Fig. 5, is an outside view of part of the limb in a state of flexion; the state of extension being performed by the approximation of the points A, B, until they meet, so as that the indicating lines coincide in a common line C. In Fig. 2, Plate I. the phalanx is in a half-extended state, and is further so turned, as to shew more distinctly the forms of the two phalanges at A and B; while, in Fig. 3, Plate I. the extension being completed, the points A, B, are brought into absolute contact at C. Another view of the meeting of those points when in a state of extension, is seen in Fig. 4, where a front view of the limb is given; and here also the coincidence of the two protuberances A, B, as the point of mutual contact, C, is indicated.

In all the figures, the position of the suture is marked; and, in Figs. 2 and 3, an attempt is also made to shew the positions of the flexor and extensor muscles of this phalanx. These, passing through the last, or the sixth, phalanx, are inserted in the body of the shell below, and beneath the suture of the fifth phalanx above, and it is by them that the action of separation is effected. But the mode of action will be more easily understood, by simplifying the appearance of the parts; as is attempted in that which is rather a diagram than a drawing, at Fig. 9, Plate II. In this, the shell is supposed transparent, to shew the directions and insertions of the separator muscles.

The limb being firmly extended, the protuberance A is thus brought to rest firmly on B at the point C, and in such a manner, that the lower portion of the suture itself just touches the sixth phalanx. If the whole of the fifth phalanx, *above* the suture, could be thus supported on the sixth, it is evident that the contraction of the flexor and extensor muscles would draw the lower portion of the former towards the shell or body, the latter phalanx being at the same time pressed against it, and thus tend to separate the lower from the upper part of the fifth phalanx. But the necessary freedom of the motion of flexion would not admit of such a construction; and the effect is therefore produced in a different manner.

For this purpose, the chief operation of the extensor muscle is, as before remarked, to fix the protuberance A firmly on B, which thus becomes a fulcrum, or point, on which the upper portion tends to revolve when the flexor is brought into action. Thus the flexor muscle acquires a lever of considerable power; and, on being caused to contract, it draws the lower portion of the fifth phalanx from the upper, and effects the separation. That this is the efficient cause, is rendered evident by the crack commencing at D; although the whole is completed so instantaneously, that it requires a quick eye to perceive a difference between the commencement and the termination of the process. It is also easy to see that a collateral provision is made for this purpose; as the suture is not only thinner but more feeble at this part; being easily separated by insinuating a knife into it there, while it is scarcely possible to enter the point or edge at the opposite side.

Those of your readers who may find a difficulty in comprehending the nature of this process, from the preceding description, may easily satisfy themselves respecting it, by examining the structure of the parts, in an animal so common. This may be done, even after boiling; when the structure and disposition of the muscles are, indeed, even more easily understood than in the living animal. But I may, I believe, add, that your London readers at least need not be surprised, if their trials of the powers of the living animals, in this respect, should fail; as they are generally too much exhausted in that market, to enable them to display this extraordinary faculty.

EXPLANATION OF THE PLATES I. AND II.

Plate I. Fig. 1, is a whole leg of the spider crab, intended to convey a general view of the articulations and motions of the legs of this tribe. The extreme, or first phalanx, has a direct motion forwards, but cannot be thrown backwards even into a straight line with the second, being checked by the form of the articulation. These motions are effected by two muscles which occupy the entire length of the second phalanx.

The second phalanx has no direct motions at all, but its lateral motions on each side are tolerably extensive, and are performed by two muscles similarly occupying the cavity of the third phalanx.

The motion of the third phalanx on the fourth is direct; but like the first articulation, this third one is so constructed, that the limb can scarcely be thrown back, even into a straight line. But the construction of the joint allows a very extensive motion forwards, and the connecting ligament is therefore lax and broad. The muscles of flexion and extension occupy the whole cavity of the fourth phalanx.

The fourth articulation is very peculiar, the ligament being so narrow as to be scarcely visible; and, in consequence of the form of the shell and the shortness of the muscles, the motion is very confined. It is lateral in either direction. The muscles, by which it is effected, are broad, but they arise from above the suture, or white ring, in the fifth phalanx, and are consequently very short.

The fifth articulation, lying between the fifth and sixth phalanges, is very free, and admits of direct motion both ways. But it differs from all the preceding in the great extent of the motion backwards; by which the whole limb admits of being thrown back, so as to be parallel to the flat surface of the animal. It has been already seen that this is the motion which prepares the limb for being detached, and the means by which this is effected will be more fully shown in the following figures. The muscles by which the flexion and extension are performed, and which also by their action serve to separate the limb, are not, as in the former cases, inserted in the next, or sixth, phalanx, but pass quite through it, to be fixed in the shell of the thorax. Their origin is below the white ring, or suture, as that of the last pair was above it.

The sixth and last phalanx is articulated to the body by a very wide and lax ligament, admitting, however, only of a lateral motion, which is performed by muscles inserted in its sides, and fixed at the other extremity to the shell of the body.

Plate I. Fig. 2, represents the sixth and fifth phalanges, with part of the fourth, in a limb of the Cancer Pagurus. The place of the suture, in the fifth phalanx, is less marked in this species; and it was for that reason that the spider crab was preferred, for the purpose of showing that part. It is, however, indicated in the figure. The limb is here thrown back by the action of the extensor; and both the muscles are introduced, as separated from the body and passing through the sixth phalanx. The protuberance of the fifth phalanx, which, in the act of detaching the limb, is brought to rest on the sixth at B, is shown at A.

Plate I. Fig. 3, in this figure another view of the same parts is given, for the purpose of shewing the complete extension of the limb at the moment when it is to be detached. At the point C, the protuberance of the fifth phalanx at A bears strongly on the point B in the sixth, or A and B coincide at C.

Plate I. Fig. 4, is a direct view of the interior of the limb at the same place, when in a state of extension. This figure is given principally to show the suture, which is very visible in the interior of the fifth phalanx; while it also represents the bearing of the fifth and sixth on each other, on the opposite side at C.

Plate II. Fig. 5. The same parts are here represented in a state of moderate flexion, for the purpose of shewing more distinctly the protuberances of the fifth and sixth phalanges at A and B, which, in the act of extension, are to be brought into contact, or to coincide in a common line C.

Plate II. Fig. 6, is the fifth phalanx, separated from all its connexions; with its flexor and extensor muscles attached at one end, and the lateral muscles, by which it is connected with the fourth phalanx, shewn at the other. The suture is here distinctly seen. This is a joint of the spider crab.

Plate II. Fig. 7, is a section of the same phalanx, for the purpose of shewing the suture distinctly, together with the insertions of the upper and under muscles. The ends of both those which lie between the fourth and fifth phalanx are shewn; but in consequence of that, only the flexor beneath is visible. It is here distinctly seen, that the shell is reinforced by a ring on each side of the suture; and that it is not only thinner, but distinctly marked by a line indicating the future division between these.

Plate II. Fig. 8, represents the dispositions of the fibres at the suture, after the division has taken place. They radiate from a common centre.

Plate II. Fig. 9, is a kind of diagram, explanatory of the action by which the separation of the fifth phalanx at the suture is effected. The flexor and extensor muscles which produce the disunion of that part, are represented as if the phalanges were transparent, so that their whole course is seen. The support or resistance which the sixth phalanx gives the fifth, by means of the meeting of the protuberances A and B at C, is also represented. When the limb is thrown into this position of extension by the action of the lower muscles, it is plain that the point C becomes a fulcrum, round which D may be caused to revolve. By means of the action of the extensor, the fifth phalanx is firmly fixed on the sixth, at a point above the suture, and somewhat beneath the upper insertion of that muscle. Thus its principal action becomes that of preserving this position; as it has little or no tendency, from the shortness of the lever thus remaining, to bring back the limb into the state of flexion. Hence that action of the flexor, which would otherwise be exerted in retaining the bent position of the limb, is so counteracted that it tends to draw the phalanx asunder at the suture D, where provision is made for that purpose. It is plain that the action of the extensor tends also to produce the same effect; the great resistance to that flexion, which would otherwise defeat this object, being that of the shoulders of the two phalanges at C.

However obscure this subject may at first appear to an observer, from the obvious tendency of the muscles to bend the limb instead of breaking it, a careful consideration of these drawings, or of the limb of the animal itself with their assistance, will, it is hoped, render it perfectly intelligible.

Art. II.—*On the Fogs of the Polar Seas.* *By* George Harvey, Esq., F.R.S., L. and E.

[Communicated by the Author.]

It has been commonly supposed, that the fogs which cover the Arctic Seas during the greater part of the summer months, are produced by *the moist air depositing its vapour, in consequence of being chilled by contact with the sea.* But this cause, it is presumed, is not adequate to the formation of mists; since it has been proved by Dr. Wells*, that dew and hoar frost are the only results which arise from air, either perfectly or imperfectly saturated with moisture, coming in contact with a body colder than itself. To produce mist or fog, as has been satisfactorily demonstrated by Dr. James Hutton†, it is necessary, that volumes of air, of unequal degrees of temperature, and holding moisture in solution, should be mingled together; and the circumstances of the Arctic Seas, during the period when these fogs generally prevail, are, it is presumed, in perfect accordance with these conditions.

Before the end of June, the shoals of ice are commonly divided and scattered; the temperature of the ocean being at that time necessarily greater than that of the icy masses floating on its bosom. This inequality of temperature will necessarily impart a corresponding influence to the air, and occasion the portions of the atmosphere, resting on the broken surfaces of the water, to become warmer than the atmosphere in the vicinity of the icebergs. The cooling influence of the icy masses also, in consequence of their being elevated considerably above the sea, will be

* Wells on Dew.

† *Transactions of the Royal Society of Edinburgh*, Vol. I.

diffused, not only by radiations from their upper surfaces to the canopy of the sky above them; but by horizontal radiations, to the air surrounding their sides. A volume of the atmosphere therefore, between two neighbouring masses of ice, will necessarily have its middle portion of a higher temperature than that of either of the portions of air between it and the icebergs *; and the consequence of such an unequal distribution of temperature must be, to cause the cold air to mingle with that of a higher temperature, and thus to produce mist or fog. The density of such mist or fog will depend on the difference between the temperatures of the mingling volumes, and on the quantity of vapour contained in the air.

The elevation of those mists above the surface of the sea will also be regulated by that of the icebergs, near which they form; since the cooling influence of the frozen mass, by rapidly diminishing above its summit, will as rapidly destroy all tendency in the portion of the atmosphere above the level of the iceberg, to assume a condition favourable to the formation of mist; thus prescribing to the mist an elevation dependent on that of the iceberg near which it forms. Captain Ross accordingly remarks, in his Account of the Polar Voyage, "that the fog was extremely thick on the surface of the sea, but at the mast-head, and at the top of the iceberg, it was perfectly clear." Captain Scoresby also, in his Paper on the Fogs of the Polar Seas, read before the Wernerian Society †, alludes to their definite elevation, and to the sky above them being perfectly clear.

It is possible, however, that two icebergs may be situated so near each other, that their reciprocal horizontal radiations will so cool the volume of air between them, as to reduce it to a tem-

* If the water in the vicinity of icebergs presents considerable inequalities of temperature, the air which reposes on it must be subject to like variations; and numerous examples of the former are to be met with in the Accounts of the Polar Voyages. Thus Captain Franklin remarks, "the temperature of the surface water was 35° when among the ice, 38° when just clear of it, and 41°,5 at two miles distant."

† *Edinburgh Philosophical Journal,* Vol. VI,

perature nearly uniform; and thereby prevent the formation of mist. The cold volume of air so formed, may, however, pass from between the icy masses, and by mingling with the air reposing on the warmer water, beyond the icebergs, produce mist at a distance from them. Nor is it absolutely necessary that *two* icebergs should exist, in order to form mist; since the horizontal radiations of *one*, by cooling the portion of air in contact with it, will cause it to mingle with the warmer air beyond the last-mentioned stratum, and thus create fog. The density of a mist when formed under the latter conditions, will be of a more variable character, than when it is formed between adjacent icebergs.

The general diffusion of fogs over the Northern Seas may also be satisfactorily accounted for, from the scattered icebergs separating the water into detached portions; and thereby creating, in innumerable directions, volumes of air, possessed of unequal temperatures. The cold air near the icebergs being blended, therefore, with the warmer air reposing on the middle portions of the broken intervals of water, must form, between most of the floating masses of ice, visible volumes of vapour, having their density dependent on the relative difference of heat between the mingling portions of air, and on the degree of humidity possessed by each.

The cause here referred to, for the production of the Polar fogs, is also one likely to promote their continuance for a considerable time; it being known, that the sea continues for many months relatively warmer than the icebergs; and therefore capable, in conjunction with the constant radiation of the ice, of producing that almost constant succession of fogs which cover the Arctic Seas during the greater part of the summer months; and which increase, in so considerable a degree, the difficulties of Polar navigation.

Plymouth, July 19th, 1825.

Art. III.—*On One of the Causes of the Movements of the Barometer, and of the South and West Winds.* By Marshall Hall, M.D., F.R.S.E., *&c. &c.*

[Communicated by the Author.]

Our inquiries into the nature and causes of the *changes* in the atmosphere will be greatly facilitated, by having first apprehended its more usual and *quiescent* state. The following observations will therefore be properly introduced by a very short description of what may be termed the natural state of the atmosphere.

If the surface of the globe were even and uniform, unchequered by mountains and valleys, and unintersected by rivers and seas, the waters of which continually evaporate and recondense, the atmosphere would remain in a comparatively tranquil state; and those movements which did take place in it, being only excited by the influences of the sun and moon, would be regular and periodical, and would consist chiefly in a constant wind from the north-east in the northern hemisphere, and in a diurnal oscillation or atmospheric tide. This movement of the atmosphere from the north-east is produced in the following manner. The sun, acting powerfully on the surface of earth within the torrid zone, heats and rarefies the superincumbent air, and causes it to rise into the upper region of the atmosphere; the place of this air is supplied from the poles, and thus, in the northern and southern latitudes, a north and south wind is produced respectively: as these portions of air have, however, little or no other motion except that in the direction towards the equator, and as the surface of the earth as we approach the equator has more and more motion from west to east, from its rotation on its axis, it follows that the air so brought from the poles, must, as it approaches the equator, have more and more of an *apparent* motion westward. These two motions being combined, the air will pursue the course of the diagonal, and to the inhabitants of the northern hemisphere of the globe, will constitute a perpetual north-east wind.

Such is the principal effect of the sun's influence on our atmosphere. A further influence of this celestial body, and of the moon, would be to produce atmospheric tides, similar to those observed in the sea, varying the *weight* of different columns of the atmosphere by augmenting and diminishing their *height*. These tides would doubtless, too, be measured by our barometers. They are obscured, in the existing state of things, by causes modifying the *elasticity* of the lower region of our atmosphere, and so cutting off, *for a time*, the influence of the height and weight of the superincumbent columns.

These regular movements of the atmosphere are disturbed chiefly by the irregularities on the earth's surface, by which the winds are diverted from their original and natural course,—being divided asunder by mountains, and made to clash violently together in the course of valleys and rivers. These effects are followed by others which arise out of the admixture of portions of air of different temperatures, and containing different portions of aqueous vapour; and it is not improbable that there are other phenomena of an electric nature, not hitherto fully appreciated and understood, which act silently and obscurely in general, though they are occasionally manifested in the phenomena of thunder and lightning.

The object of this paper is not to treat fully of the movements of the Barometer, but to point out ONE of the causes of these movements more particularly than has hitherto been done. This cause is the *transition* of vapour in the atmosphere from the transparent to the opaque and fluid form,—or the FORMATION *of clouds, and of rain*. We shall now, therefore, proceed to trace the effects of this change upon the elasticity of the atmosphere in the region of the clouds, and consequently upon the barometer.

It is well known how much the presence of water adds to the expansibility of the air in contact with it, by heat. In the experiments of Guyton and Duvernois *, the object of which was to ascertain the comparative expansibilities of the different kinds of gas, the presence of an inappreciable quantity of moisture occasioned an error of such magnitude, that, of gases which are now known to expand alike, some appeared to dilate five times as much as others by the same application of heat.

* *Journal de l'Ecole Polytechnique*, Cap. 2. † *Essai sur l'Hygrometrie.*

The rate of the expansibility of air in contact with water, is also greater as the temperature rises; for, from the experiments of Saussure†, we learn that the power of air for retaining water in a transparent state increases in a geometric progression, whilst the temperature rises in an arithmetic progression only.

From these considerations we are enabled to conceive how great the *contraction* of a given volume of atmosphere must be by the mere admixture of different portions of air charged with vapour and of different temperatures, even without the precipitation of water, but especially if, whilst its temperature is diminished, its transparent vapour be condensed and withdrawn, in the form of clouds and rain.

To these considerations must be added, that of the vast extent of country over which the changes which take place in the atmosphere are observed to diffuse themselves,—generally more than one thousand miles,—that of the extent and height of that region of the atmosphere occupied by the clouds—upwards of three miles,—and that of the immense quantity of water which must at some periods pass, in a very short space of time, from the state of transparent vapour to that of clouds or rain.

If we fully consider the extent and magnitude of these phenomena, we must conceive that the degree of contraction in volume in that part of the atmosphere which constitutes the region of the clouds, must on some occasions be great indeed. Let us now consider what must be the further effects of this contraction in the immediate and adjacent region:—a portion of the atmosphere being condensed and withdrawn, the elasticity of the remaining portion occupying the same space must be diminished; and, as further consequences, the barometer must fall, and the air of the adjacent regions must be attracted, and wind be produced;—and these effects and phenomena will *continue* as long as the atmospheric process of the formation of clouds or rain continues. When the deposition of atmospheric moisture ceases, the equilibrium in the elasticity of the air is gradually restored, the barometer rises to its *natural* level, and the wind or influx of air from the adjacent regions subsides.

The fall of the barometer is thus connected, not with the *existence* of clouds or the *fall* of rain, but with their *formation*. And we are enabled by this view of the subject to account at once for the undoubted connexion between the fall of the barometer and the fall of rain in general, and for a fact equally well established and frequently observed in the summer of 1823, that the barometer does frequently fall during fine weather and rise during rain,—the first phenomenon depending on the precipitation of water into the form of clouds, and the second on the circumstance of that precipitation having *ceased;* in this manner, when fair weather and rain succeed each other rapidly, the barometer may fall whilst *clouds* are forming, and rise when this process has ceased, though the *rain* fall.

The connexion is not less obvious between the formation of clouds and rain and the fall of the barometer, and the occurrence of gales and of changes in the wind.

It is probable, indeed, that these phenomena act and react in the atmosphere, and assume in turns the character of cause and of effect. The clash of winds is the cause of the first precipitation of moisture from the atmosphere, and the precipitation of moisture becomes, in its turn, a fresh cause of wind.

That state of the barometer which is most *generally* observed, and which usually continues longest without change, may be considered to be its most natural condition. This state approaches to its highest elevation, the barometer being far more frequently and for longer periods high than low, this state prevailing when no changes are going on in the atmosphere, and when the weather is of course calm and serene.

The causes of the movements of the barometer are to be sought, therefore, not only in such phenomena as may be supposed to add to the *weight* of the atmosphere, but in such as may temporarily diminish its *elasticity*, and concur with the disturbance of its transparency and tranquillity, or with the formation of clouds or rain, and of winds.

To shew how much the low state of the barometer is connected with the *formation* of clouds or rain, I quote a remark from the

Meteorological Observations of Mr. Dalton*. "Very dark and dense clouds," he observes, "pass over without rain when the barometer is high; whereas when the barometer is low, it sometimes rains without any appearance of clouds."

The views just given are greatly supported by the observation that the variations in the barometer are confined to the lower region of the atmosphere, and diminish rapidly as we approach the upper limit of the region of the clouds.

I now proceed to make a few observations on the influence of the condensation of atmospheric moisture as a cause of wind. The causes of the north-east wind have been already stated as given by various authors †. But I am not aware that any satisfactory cause has been assigned for the prevalence of winds from the south or from the west. It is plain, however, from the preceding remarks, that such winds must be produced by great and rapid formations of clouds and rain, the air being drawn from all the adjacent regions to that in which this condensation of atmospheric vapour takes place, and thus under particular circumstances a south-west wind or gale will be produced.

Mr. Playfair has made some observations which appear to confirm this view of the subject. He observes, "the sudden sinking of the barometer almost always indicates a gale of wind, though a gale that is sometimes at a considerable distance ‡." And further, "there is in our climate hardly any instance of rain without a change of wind, and very rarely a change of wind without rain in a greater or less quantity §." Mr. Dalton observes, "the barometer generally *rises* with a wind between the north and east, and is very low in winter when a strong and warm S. or S.W. wind blows ‖." He adds, the lowest extreme of the barometer for five years, was accompanied with a strong S. or S.W. wind and and heavy rain, and occurred just after the

* Page 196.

† See La Place, *Systeme du Monde*, t. 2. p. 174; Playfair's *Outlines of Natural Philosophy*, v. I. p. 293.

‡ *Outlines*, v. I. p. 299. § *Ibid.* p. 305.

‖ *Met. Obs.* p. 112.

highest extreme which took place during a long and uninterrupted frost.

It is impossible to show more clearly than by these facts the connexion between rain, wind from the S. or S.W., and a low state of the barometer; I merely venture to suggest one mode of accounting for this association of phenomena:—during a period of atmospheric tranquillity, evaporation goes on from the surface of the earth, and expands and forms a part of the atmosphere, to the absolute *weight* of which it must necessarily add, and the barometer accordingly takes its highest station; at length, from the collision of different winds induced by irregularities on the earth's surface, this vapour is precipitated, on the principle of the beautiful theory of Dr. Hutton *, *viz.* that, as the power of the air to imbibe moisture increases in a higher ratio than the temperature, two portions of air of different temperatures, and saturated or nearly so with moisture, cannot be mingled together without its precipitation; this precipitation is, in its turn, a fresh cause of wind, of the fresh collision of different airs, and of the renewed formation of clouds and rain,—and the contraction which thus takes place in the atmosphere diminishes its elasticity, lowers the barometer, and again becomes a cause of wind.

According to this view of the subject, the barometer may be regarded as an instrument measuring the actual *weight* of a column of the atmosphere in *serene* weather, but in serene weather only. On other occasions it measures the changes which take place in the *elasticity* of the lower region of the atmosphere, the pressure of the superincumbent part of which is cut off for a time until the equilibrium be restored. From this view, too, we observe that it is only in serene weather, and with a transparent and tranquil atmosphere, that the barometer should be employed to measure the heights of mountains. That the variations of the barometer do, in fact, depend upon changes which occur in the *lower regions* of the atmosphere, is proved by the observations of M. Laval, recorded in the *Memoirs of the Royal Academy at Paris* for 1709. During ten days the barometer was observed to vary two lines and

* *Trans. of the R.S. of Edin.* v. I. p. 41.

three-fourths at Marseilles, whilst it varied only one line and three-fourths at the top of St. Pilon, which is 960 yards above the level of the sea.

I must observe, in this place, that I am fully aware that it may be objected to the preceding observations, that the *quantity* of water in the atmosphere is not *sufficient* to account for the movements of the barometer, and for winds, from the changes in its state from transparent vapour to that of clouds or rain. To this objection I would reply, first, that it is the object of this paper to propose this, only as *one* cause of the movements of the barometer, and of wind, whilst it is admitted that there may be, and undoubtedly are, many others; and secondly, that the circumstances attending these changes in the state of the atmospheric moisture, and all the effects of these changes, are probably not yet fully understood and appreciated. Two points, at least, have been established:—the first, that there is an unequivocal connexion between the fall of the barometer, the production of wind, and the formation of clouds or rain; the second, that the transition from the elastic to the fluid state of the atmospheric moisture *does, in part*, at least, account for these phenomena—*must* be attended by a *certain* diminution of bulk and elasticity—and consequently by a movement in the adjacent regions of the atmosphere, and by a fall in the barometer. The principles here pointed out may obtain in the minor but more usual changes in the atmosphere; whilst the more extraordinary phenomena of gales and tempests may, and doubtless do, involve many other. Our atmosphere is so complicated a structure, that no simple theory will ever explain the whole of its phenomena. It has just been suggested, that as *all* the causes of the movements of the barometer are not hitherto ascertained, so *all* the circumstances attending the precipitation of the atmospheric vapour, assumed as *one* of these causes, are probably not yet fully appreciated:—one effect of the condensation of the atmospheric moisture, is an actual diminution of the *weight* of the atmosphere, and this would operate on the barometer, were the *elasticity* of the air unaffected; but as the vapour really constitutes a part of the atmosphere, and adds to its *volume*, its subtraction or precipitation must also diminish the *elasticity* of the remaining portion of the air, and, in this manner especially, lower the barometer; and this effect will be the greater in consequence of the atmospheric vapour being accumulated, and its precipitation chiefly effected, in the *lower region* of the atmosphere, in which the movements of the barometer are consequently observed to be proportionately greater than in more elevated situations.

In conclusion, I would observe, that as I should probably never have taken the pains with this subject which I have done, but with the view of laying it before our little Society, so my wishes will be fully attained if this paper either excite interest or afford instruction even to the youngest of our members, and thus tend to further the object of our meeting together. My great aim, indeed, has been to allure and aid the *beginner* in philosophical studies; and such, I imagine, to be the specific design of our institution. With this view conjectures and observations which might otherwise never have deserved attention become extremely valuable.—If any further apology were required for the preceding pages, I would say that the current of my studies has of late years flowed in a totally different channel from that of philosophy; but that it was only by recurring to the studies of former days that I could select any subject which would be appropriate to the present occasion.

Art. IV. *Description of an Instrument for destroying Stone in the Bladder. By* J. R. Griffiths, Esq.

[Communicated by the Author.]

I had long entertained the idea that an instrument might be constructed, by means of which a calculus in the bladder could be broken down into fragments small enough to pass by the urethra, and about three years since, with the assistance of an instrument-maker, I attempted to put my plan in execution; but not at that time being able to construct a drill that would work in a curve, I gave up the attempt. Some time after this I heard that an instrument for the same purpose had been invented, and used with success in France, and which was somewhat on the same principle, excepting that it had no curve, it having been found that a straight instrument might be passed into the bladder of many persons.

Notwithstanding what was said of the success of this contrivance, I conceived it would be very desirable to form one which would be better adapted to the natural curve of the urethra, and with which there would be less danger of injury to the bladder in attempting to seize the stone. Under this impression I determined to make another attempt, and for this purpose I applied to Mr. Ferguson, a very ingenious Surgeon's Instrument maker, in Castle-Street, Leicester Square; he entered fully into my plan, and was of very great assistance in carrying it into execution, and completing the instrument hereafter described.

The instrument (Plate III. Fig. 1.) for the sake of convenience, is represented somewhat smaller than the original model; it consists, in the first place, of a tube twelve inches long, A A resembling a catheter, with the exception of its being open at both ends; B is an arm coming off, which connects the whole together, and at the same time serves for a handle. Fig. 2, is a stilet, with a knob at the end, which fills up the opening at the extremity of the canula, so as to enable it to be passed into the bladder without injury to the urethra, after which it is to be withdrawn, and the apparatus for seizing the stone is then to be introduced; it consists of a tube six inches longer, and about the eighth of an inch less in diameter than the first, the anterior part of which is made flexible, to enable it to take the curve of the canula, in the same manner as the old elastic catheters were constructed, which is by means of winding up a narrow strip of silver into a spiral form; part of this is seen at D, the anterior extremity, which, in Fig. 1, is concealed in the canula, and is represented at F F, Fig. 3. The surface of this tube is divided by ridges into four compartments, which at the same time keep it steady, and allow space for the two narrow watch springs G G, to pass between the two tubes, and prevent their catching in each other; at the extremity is a collar

E, Fig. 3, which exactly fills up the interval between the tubes, and in which there are four holes for the passage of the springs, which have a small bit at one end turned down to prevent their being drawn through, and the temper of the steel is lowered, so as to admit of their being freely bent: they are passed from within outwards through two of the knobs, and brought back by the others, so as to form two loops, with which it is proposed to fix the stone; they cross each other and pass through a ring H, which rises and falls as you increase or diminish the bow of the springs. The other ends pass down in the grooves before-mentioned, and are fixed in the collar I. Fig. 1, by the screws K K, and by means of which they are slid up and down, either together or separately; L is a screw which fixes the collar when the stone is caught by the loops; *m m* is an elastic wire wound round the part of the inner tube, which is not enclosed in the canula, and serves to prevent the springs from bending outwards when pushed up; N N is a drill; the part of which that works in the curve is constructed in the same manner as the flexible part of the inner tube, but the metal is thicker, to give strength to bear the pressure necessary to perforate a stone; the other end is made of pinion wire, which passes through corresponding grooves in the pulley O, which enables it to be pushed forward at the same time that it is turned; the extremity works like a swivel in the socket P, attached to the ring R. The drill head takes off, that different sized ones may be used; they are made to cut one *way only*, as the flexible part, though quite firm enough when turned in one direction, in the opposite would have a tendency to unwind, if it met with much resistance.

To make use of this instrument, the canula, with the stilet in it, is passed; the latter is then withdrawn, and the second tube, with the watch springs attached, is pushed through it, and the end fixed in a hole in the upright S; the collar is then slid up, and the springs bow out in the manner represented in the plate: the springs may be worked up and down till the calculus is caught; it is then fixed by means of the screw in the collar, and in this way can be retained more firmly than with any forceps; by having the springs of sufficient length, a very large loop may be made, and I should think without any fear of injuring the bladder: if the calculus should be small, it may be drawn out through the canula; if too large for that, it must have holes repeatedly drilled, till it crumbles into pieces small enough.

The principal advantages that this appears to possess over the French instrument before alluded to, are,

1st. Its form is better adapted to the urethra.

2nd. It can be made of a smaller size.

3rd. There is less chance of hurting the bladder with the bent springs, than with the forceps. The loops will fix the stone more securely, and if either of them should happen to be broken, *both* the parts can be withdrawn through the canula, and a fresh spring immediately adapted to it.

4th. The fragments, or small calculi, will pass off by the canula, without injury to the urethra; this may be assisted by injecting water, and letting it out again in a full stream.

To destroy a calculus of any size would require the instrument to be used a great number of times; but however tedious, many persons, I think, would prefer it to the dangerous operation of lithotomy, and would resort to it at an earlier period of the disease, when the stone had not acquired much bulk, and the symptoms were not so urgent as to make immediate relief necessary.

Bentinck-Street, August, 1825.

ART. V.—*Outlines of Geology, being the Substance of a Course of Lectures on that Subject, delivered in the Amphitheatre of the Royal Institution of Great Britain, by* William Thomas Brande, F.R.S., Professor of Chemistry in the Royal Institution, *&c.*

[Continued from Vol. XIX., page 198.]

IV.

THERE are abundant difficulties in the way of any satisfactory theory respecting the origin of the diluvial remains of quadrupeds,

&c., which were adverted to in the last lecture. If we consider them as having lived and died in the caves, and on the spots where their remains now occur, we must presume, either that their habits and propensities were extremely different from those of the now existent species of the same tribes; or, what is yet less admissible, that the temperature of the northern regions was formerly correspondent with that of equatorial climates. Again, if we imagine the bones to have been transported thither by water-carriage, at the time of the deluge, how, it will be asked, can they have escaped attrition?—and not this only, for they actually retain in perfection all the tuberosities and processes which enable the anatomist to recognise them. To get over this latter difficulty, it has been conceived that the bones were not transported along with the pebbles and gravel, but that they came safely packed and protected in their including carcasses, floating upon the surface of the waters, and were afterwards deposited safely upon the mud or gravel, where the flesh rotted and decomposed, and the bones remained uninjured. We know how readily this kind of transportation takes place, how rapid the carriage may be, and supposing the temperature not very high, we also know that many weeks might elapse before the carcass would sink. I shall not at present venture to give any opinion upon this question, but shall refer my audience to the works of Professor Buckland, as the advocate of the one hypothesis, and to those of Mr. Granville Penn, who has defended the other.

We may now proceed with our description of the strata, upon which the former materials are deposited: these are the *supermedial rocks* of Phillips and Conybeare, and include the varieties of chalk, green and ferruginous sands, oolite or freestone, lias, and red marle or new red sandstone.

Below the varieties of clay, the position and contents of which formed the subject of the last lecture, we find the chalk which has already been stated to constitute the cavities of basins, in which the various alluvial matters are deposited. The ranges of chalk hills in the south of England are very extensive, and the landscape which they constitute, peculiar for the smooth and rounded outline of its hills, their monotony of surface, and for the singular cup-shaped concavities and deep hollows in which their sides abound. The situation and extent of the chalk in England is best shown by reference to coloured geological maps. Salisbury Plain and Marlborough Downs form a centre, as it were, from which the chalk emanates in a north-east direction, through Buckingham, Bedford, and Cambridgeshires, and terminates on the coast of Norfolk in one direction. Another branch, interrupted by the Valley of the Humber, traverses Lincolnshire, terminating at Flamborough Head in Yorkshire.

The extreme western point of the chalk is not far from Honiton in Devonshire, whence it branches off toward the south-east to the Isle of Purbeck, and again appears, forming a ridge that crosses the Isle of Wight. Near Hungerford, in Berkshire, another range of chalk commences, and passes by Alton and Rochester to the coast of Kent, forming the cliffs between Folkstone and Deal. From near Alton, another branch passes off, and ends at the lofty promontory of Beachy Head on the Sussex coast. In this chalk district there are some considerable elevations. Near Dunstable and Shaftesbury, for instance, it forms hills nearly 1000 feet above the level of the sea. Between Lewes in Sussex, and Alton in Hampshire, there are several similar elevations. Between Alton and Dover, the highest point is about 800 feet, and the Castle Hill is about 470 feet high. The chalk cliffs near Folkstone, and those near Lyme in Dorsetshire, are between 5 and 600 feet high.

Chalk, like the strata that lie upon it, abounds in organic remains, but they are of a different and more ancient character, exhibiting many new genera, and scarcely a single species quite identical with any that now exist. They are chiefly as follow:—

Remains of vertebral fish, such as teeth of a species of shark.

Among the testaceous mollusca, are ammonites and belemnites, generally in the lower strata only; a few spiral univalves and several bivalves.

Echini are very characteristic of chalk, and among them many species, and one genus, are peculiar to it. It also includes star fish, encrini, many madrepores, alcyonia, and sponges.

The forms of sponges and of the alcyonia and echini, are not uncommon in the flints; also casts of ammonites, and of nautili. These remains are not equally abundant in all parts of the chalk strata, nor are the strata themselves in all places similar. The upper beds of chalk abound in flints, which are usually disposed in regular horizontal layers, though there are cases in which, from some derangement that appears to have occurred to the strata, the flints are nearly vertically arranged, as in the Isle of Wight, and on the Dorsetshire coast. But, not the least remarkable fact, in respect to the perpendicular flints is, that they are generally splintery and broken, while those which are horizontal are in the usual state of rounded nodules. As the opposite coast of Dorsetshire exhibits a very similar arrangement, it is extremely probable that this chalk ridge has once been continuous, and that the shock or catastrophe that has broken it down, has also caused those inclinations, and that verticality of the strata once horizontal, that I have just adverted to. In the lower chalk strata, the flints become less abundant, and it frequently has a gray colour, and is *argillaceous*. These strata may be seen near Ryde in the Isle of Wight, and at Guildford and Dorking in Surrey, and the lime which such chalk affords derives certain peculiarities, as far as its use in making mortar is concerned, in consequence of its aluminous character. The greater part of the chalk hills in Cambridgeshire are also composed of the lower or gray chalk, and they gradually pass into a kind of gray clay called *gault*, and into several varieties of argillaceous loam. In the neighbourhood of Deal, and in some parts of the Isle of Wight, the flint assumes the appearance of flattened or tabular masses, which are sometimes lost in a very thin edge. Near Freshwater in the Isle of Wight, these tabular flints are arranged in diagonal layers, crossing each other in opposite directions.

Although the flinty chalk is not always found upon that without flint, it appears likely, from an examination of the neighbouring soils, that it has, at one time or other, everywhere existed; indeed the enormous quantity of alluvial flints, and the deposits of rounded masses and nodules of chalk, which fill up the hollows in many parts of the chalk strata, are abundant indications of the destruction to which the chalk has been exposed. A degradation to which its exterior situation and its softness render it peculiarly liable.

The cliffs at, and about Brighton, are particularly remarkable for the changes and devastations which they record. The town stands upon a bed of fragmented calcareous matter and flints, which, on the east, is seen resting upon shingles, consisting chiefly of flints, but mixed with rounded masses of granite, slate, and porphyry, cemented together by crystallized carbonate of lime, apparently derived from the solvent action of water upon the superincumbent chalk, thus forming a hard and durable breccia. Nodules of pyrites, and of crystalline carbonate of lime, are not uncommon in chalk. They are of a radiated texture, and the latter often unusually hard.

Having stated thus much respecting the composition and contents of the chalk strata, I have little or nothing to add, relative to their origin and that of their included fossils. The nature and characters of chalk seem to announce it as an aqueous deposit; but we must not be so bold as some geologists, who conclude that it is the detritus of coral reefs, and the dust of shells originally derived from the antediluvian ocean. The existence of various organic remains announce the existence of those animals at the time of its deposit, but we never find in it the bones of quadrupeds, or of animals of existing species; and this circumstance appears to declare it of a date anterior to that of those superincumbent beds, which we examined at our last meeting. The origin of flints, their arrangement, the peculiarities which they occasionally exhibit, and the fossils they occasionally include, are subjects that entirely baffle all theory, and it would be mere waste of time to recite the hypotheses they have given rise to. Like the chalk, however, they bear marks of aqueous, rather than of igneous origin; they contain the same fossils, echini, sponges, and other substances found in the chalk; and what is curious, many of them are hollow, and contain powdered siliceous earth, provided they have no perforations; but if hollow and perforated, they are filled with chalk.

The next beds that occur, in order of succession to the chalk, are several varieties of sand and clay; the former is often called green sand, from the fragments and particles of chlorite and green earth that it contains, and it is sometimes so compact and hard, as to be fit for a building material of no trivial durability. To what extent this formation accompanies the chalk is not quite obvious, but it occurs in many places on the western side of the chalk range extending from Dorsetshire into Yorkshire, and also upon the coasts of Dorset, Kent, and the Isle of Wight. This substance generally effervesces with acids, from the calcareous matter that it contains, and is abundant in organic remains; more especially those of the alcyonium, supposed to be a species of zoophite, and seen in a very characteristic manner at the back of the Isle of Wight, where large masses of this rock are lying upon the beach, having fallen in consequence of the washing away of the marly strata upon which they repose. This marle is of a bluish-black colour—in the Vale of Aylesbury in Bucks, and in that of the White Horse in Wilts, this stratum forms a tenacious clayey soil—and at Shotover Hill, Oxford, it abounds in oyster-shells, selenite, pyrites, and other fossils. Mr. Webster has particularly made us acquainted with the characters and peculiarities of this substance in the Isle of Wight. The sandstone strata that form the perpendicular rocks at the under cliff, lie upon a stratum of blue marle, which being soft and yielding, is occasionally washed away by land springs; the superincumbent rock of course falls, and to this cause we may attribute that ruined appearance which is so characteristic of the back of the island, and which variegated with woods and corn fields, gives a peculiar and highly picturesque character to that delightful spot. At Black Gang Chine, the washing away of the blue marle by the waters that filter from the higher land, and through the over-lying strata, is particularly well marked, and hence the cause of those *land slips* as they have been called, one of which took place to a great extent in 1799, and another about ten years ago.

The clays, gray chalk, and marles, which are common in many parts of the country that bound the chalk hills, scarcely admit of distinction into strata; they are often more or less intimately mixed with green sand and contain beds, masses, and nodules of sandstone and limestone; these all appertain to the formation we are now speaking of, and lie upon those very extensive beds of sand, commonly called ferruginous or iron sand, of the characters and situation of which it will now be right to treat more at large.

This stratum must be distinguished from the sand lying *above* chalk, as that of Blackheath and of Bagshot. It is much more extensive, and constitutes a leading feature of many of those countries which contain or border upon chalk hills. It is accompanied and often blended with some varieties of limestone, and frequently it scarcely is to be regarded as a distinct formation from *green sand*—perhaps its best character, its most marked and leading feature, is the quantity of oxide of iron that it occasionally contains, and which is so considerable in some parts of Kent and Sussex, as to have been formerly employed as a productive ore of iron. These ferruginous masses and veins are very abundant in and about Crowborough Heath, and the extensive district comprehended in a triangle, of which Dover, Beachy Head, and Alton, form the points, is chiefly composed of this kind of sandstone—here and there intermingled with other beds, but seen in characteristic masses on the coast near Hastings. Leith Hill, in Surrey, is also a good specimen of this formation; it rises to nearly 1000 feet above the level of the sea: at Bottom Head on the coast of Yorkshire, it forms an elevation of nearly 1800 feet, which is perhaps the greatest height which it attains in England. This sandstone is seen with most of its peculiarities in the neighbourhood of Tunbridge Wells, and although in many places it is almost barren, or only covered with furze and heath, in others, where it contains embedded or intermixed clay and limestone, it constitutes a soil not unpropitious to the growth of several forest trees, and even bears very stately oaks. Near Woburn, in Bedfordshire, this sand is largely planted with firs; and there, as well as at Ryegate, in Surrey, it contains large beds of fuller's earth. The organic remains which it contains in greatest abundance are nautili and cornua ammonis. This formation may be traced with little interruption from the neighbourhood of Shaftesbury in Dorsetshire, to St. Neots, in Huntingdonshire. It appears again on the east coast of Norfolk; reappears at Spilsby in Lincolnshire, and again in Yorkshire, where it reaches the north coast of that county, and covers much of its western district. In many places this sand is associated with and overlies several varieties of clay of different texture and and composition, and often assuming a slaty aspect, and containing bituminous substances, pyrites, and in some places, as about Whitby in Yorkshire, a considerable quantity of sulphate of alumina. On the south coast of England, as, for instance, in the Isle of Purbeck, this shale is abundantly bituminous, and forms what is called Kimmeridge coal. Organic remains are not rare in it; and in Wiltshire, it is associated with several varieties of limestone, in which they are extremely abundant. It is, however, doubtful, whether these substances should not rather be referred to the oolite formation, which is extremely extensive, and which is seen in the map proceeding from Somersetshire to the banks of the Humber in Lincolnshire. Carbonate of lime is its leading ingredient, and several of its varieties are used as building materials, such as Bath, Purbeck, and Portland Stone. Its texture, however, is such, that it is generally easily acted upon by the weather, and it is difficult to suggest any good criterion by which its relative durability may be judged of. It is sometimes supposed, that a comparative estimate of its value and permanency may be founded upon its absorbent powers, in regard to water, but this is not always strictly true; that which abounds in shells and other organic remains is generally very subject to decay.

In the quarries in the isle of Portland, three distinct strata are visible; the uppermost called by the quarrymen the *cap*, consists of fragmented and decomposing masses; it is immediately succeeded by horizontal seams, containing chert, flints, and some fine specimens of petrified wood; below it is the useful Portland stone. In this island, the beds dip to the south, and alternate with strata of bituminous shale. The limestones belonging to the oolite formation, are of various degrees of granular fineness, and when made up of an agglutination of small rounded concretions, they are particularly called *roestone.*

In contact with the lower beds of the oolite, we find more compact limestone or lias, which may be considered as the lowest member of this formation; its different layers present various shades of white, gray, and blue, and it is generally speaking so argillaceous, as to exhale a strong earthy odour when breathed upon. To this ingredient, and to a portion of oxide of iron, the peculiarities of lime from this lias are referrible. It accompanies the great oolite formation, and is seen extending from Ilchester in Somersetshire, by Bath and Gloster across the centre of the kingdom, terminating near Lincoln. A little to the north of Gloster, it forms eminences of more than 1000 feet high. These strata enclose a great variety of sea-shells, ammonites; and at Lyme, on the Dorset coast, they contain the skeletons and detached bones of a large animal, which has generally been regarded as the crocodile, but which Sir Everard Home has shown not to be that animal, but a peculiar extinct species, which from an analogy that exists between its spine and that of the Proteus, he has called Proteorachius. As these strata contain such remains of amphibious animals, they make it probable, in the opinions of certain geologists, that fresh water and dry land existed previous to the formation or deposition of the oolitic strata, and consequently, of course, anterior to the chalk hills and their various superincumbent substances.

V.

We have now cleared our way to the great red sandstone formation, or to the red marle of modern geologists,—a substance of very extensive occurrence, and involving the history of some important deposits, more especially, that of coal. I shall not stop here to explain the differences of opinion respecting the Wernerian phraseology applied to this rock, but it will be found that a very analogous substance occurs at greater depths, or among older rocks, and is then emphatically termed *old* red sandstone; the terms, new red sandstone, red marl, or the mere provincial phrase *red ground* being applied to that formation which follows the oolite, and which we may now proceed to examine.

In the first place, in regard to situation, the red marl extends, without material interruption, from the east of Somersetshire to the northern bank of the Tees in Durham. The map shows the occurrence of coal beds in this district, the great coal formations, or coal basins as they are often called, being apparently deposited in the inferior or mountain limestone, and covered by, and alternating with red marl, or some of the substances with which it is directly associated. It may not be irrelevant to take a general view of the positions of this rock in the several coal counties, and to notice the other substances which form its constant or occasional accompaniments; and in doing this shall I make use of Mr. Phillips's abstract of the papers in the *Geological Transactions* relating to this formation.

Red sandstone generally forms a flat and low country, or where elevated into hills, their slopes are gentle, and their outline rounded. In the midland counties, it is traversed for a considerable length by the Severn; and the Ouse and Trent, streams tributary to the Humber, take a long course through its plains in the north. In the south-east part of Durham it exhibits strata of various colours, containing coal and gypsum. In Westmoreland it covers a considerable plain on the west of the range of mountains, of which Crossfell is the highest, and which is near the south-western extremity of the great Newcastle coal-field. The southern parts of Lancashire, the north of Shropshire, and the whole of the intervening county of Cheshire, are principally characterized by their plains of red marl, and in Worcestershire it is also a prevailing rock, as also in various parts of Derbyshire, Nottinghamshire, and Staffordshire. Without, however, further enumeration of the situations in which this rock occurs, a glance at the map will show its extent, and we may employ our remaining time by shortly adverting to its contents and embowelled treasures.

The texture of red sandstone, and I had almost said its *colour*, are very various; sometimes it is very soft and clayey, but in parts much more lapideous and indurated, and it is associated with beds of a peculiar conglomerate, consisting of nodules of different substances, cemented by marl or sand, and with a rock which we shall afterwards describe under the name of amygdaloid. It is generally unfit for architectural purposes, and from its softness has been in some places extensively excavated, as near Nottingham, where it is suspected that these caves may have formed the dwellings of the aboriginal Britons.

Deposits of gypsum, or sulphate of lime, are very characteristic of red marl, and this is a substance of no small importance as an article of trade; the larger masses are occasionally manufactured either in the turning lathe or by hand into vases and various ornaments, and are sometimes used in decorative architecture, of which the columns in the hall of Keddlestone House in Derbyshire are fine specimens. The coarser varieties are employed in the potteries for making moulds, and some of the finer fibrous varieties are cut into beads and broach stones of no mean beauty. When heated, it loses water, and crumbles into a white powder, well known as plaster of Paris, and employed for casts and a variety of ornamental work. It has also been tried as a manure, but not with much success in this country, nor has any light been thrown upon the manner in which it operates as such. Sulphate of baryta, and sulphate of strontia, have also been found in this formation, but no organic remains have ever been discovered in it, although there are a few marine relics in the magnesian limestone below it.

Red marl is the last of the strata or formations, which is tolerably conformable as to position with those above it, and like them, nearly horizontal; the strata of the succeeding formations are said, in common geological language, to be *unconformably* placed with regard to the preceding, rising from under them at very various angles into lofty mountain chains, skirted by the red marl which occupies the extended plains at their base, " so that the appearance of the whole may be described by the figure of a sea composed of horizontal beds of red marl, &c., surrounding elevated islands, consisting of rocks of the coal formation or carboniferous mountain limestone, old red sandstone, slate and granite, all variously and irregularly stratified."

In the red sandstone of Droitwich in Worcestershire, and at Northwich in Cheshire, are our most considerable beds of salt: at the former place, they are inundated, and form what are called brine springs; at the latter, the solid salt is accessible, and has been excavated to a great extent both in width and depth. At Droitwich, the brine contains about one-fourth its weight of salt, and furnishes upwards of 16,000 tons annually. From this source the revenue derives an annual duty of £320,000; a fact, notwithstanding the partial advantages that accrue from it, greatly against the suspension of that equitable tax. At Northwich the quantity of solid salt annually raised exceeds 150,000 tons, about 16,000 of which are consumed at home, and 130 to 140,000 exported.

As the Cheshire salt deposits are below the level of the sea, it has been conceived that the ocean once covered the districts, and there let fall these enormous masses of muriate of soda, and that the clay, sandstone, and other substances, have resulted from the ruin of older rocks; but the salt is not such as would be obtained by the spontaneous evaporation of such water as our ocean now contains. Others regard these salt pits as the bottoms of large cauldrons, in which sea water has been boiled down by subterranean heat, and left the bed of salt like the earthy fur upon the bottom of a tea kettle: I do not know that there is much choice between either of these hypotheses.

In describing the coal-fields of England, I can only advert very generally to their arrangement and contents. The *Encyclopedia Britannica* contains two valuable articles upon this subject, and the details respecting the south-western coal-district of England, published in the first volume of the second series of the *Geo-*

logical Transactions, by Messrs. Buckland and Conybeare, may be referred to as the best geological history extant of these important formations.

The great northern coal-district of England lies between the river Tees (which separates Durham from Yorkshire,) and the Tweed, and the country slopes from Crossfell and the Cheviots slowly towards the sea, the abrupt faces of those hills being upon their western side. The beds of which this coal-field consists partake of the general slope of the face of the country, reaching the surface on Crossfell, and gradually dipping towards the east, and away under the sea. At Sunderland they are covered by magnesian limestone, and they appear to lie in a basin of mountain limestone, abounding in veins of lead.

The strata of the coal-basin are coal, sandstone, shale, limestone, and basalt, and they abound in vegetable impressions and in fresh-water shells; among the former, the varieties of fern predominate; among the latter, bivalves like those of the fresh-water muscle. It may not be improper here briefly to notice the chemical varieties of coal that are presented to us in these districts, especially as relating to their economical applications.

The coal which is generally most esteemed is that of the northern districts, Northumberland, Durham, and Yorkshire; it abounds in bitumen, softens in the fire, swells, and throws out jets of flame; it coheres, and therefore burns hollow and requires poking; it furnishes cinders, though but little ash. On the other hand, most of the coal from the west of England blazes and burns briskly, being much more easily kindled than the other; it requires no poking, because it has no tendency to cake; it affords no cinders, and leaves a dusty white ash.

A third kind of coal called culm, or stone-coal, contains scarcely any bitumen, and abounds in earthy matter; it is very difficult of inflammation. Besides these, there are some other varieties, such as cannel-coal, splent-coal, &c.

There is one important circumstance observable in stratified countries in general, already adverted to, and more especially shown in the coal-beds, which is, that they were evidently deposited before many of those unevennesses which give' rise to our present hills and dales were carved out upon the surface, and often where even a deep and extensive valley intervenes, the same strata running in the same direction, and at the same levels, are found continuous on both its sides; except in some particular cases, where the valley is very narrow and abrupt, and its sides perpendicular, and where it *appears* to have been formed by the dislocation and fracture of the ground, once continuous, in which it occurs.

In consequence of the peculiar arrangement of the coal strata, a section of them often gives the idea of a basin, or boat-shaped concavity, which has been successively filled with the various substances that occur in it; the seams of coal vary in number and in thickness, as well as in quality, and the upper seams are generally imperfect.

In consequence of the dip of the strata, it not unfrequently happens that we have an opportunity of examining and ascertaining the nature of the lowest seams, which, though deep and out of reach in one part of the coal-field, are superficial at another. This is shown at Cross-fell, where the crow-coal rises to-day, which, in consequence of the inclination of the beds, is considered to be nearly 460 fathoms below the lowest of the Newcastle beds, a little to the east of that town.

The description of one coal formation applies, in general, to others; but there are certain circumstances which give peculiarities to some of our coal districts, among which the iron-works are deserving attention. The ore which is here worked is the clay iron-stone; an ore poor in itself, but deriving value from the abundance of coal that attends it.

Some writers have amused themselves with speculations respecting the exhaustion of our coal-mines, and have calculated the number of years, or centuries, that the stock on hand is likely to last. When, indeed, we reflect upon the vast importance of this species of fuel in a country dependent not merely for its prosperity, but even for its very existence, upon its manufactures and consequent commerce; when we remember its enormous and increasing consumption; when we consider that the metropolis only swallows up annually considerably more than a million of chaldrons exclusively from the Tine and Wear districts; it might appear that the apprehensions of some worthy persons upon this score were not altogether without foundation. It is however admitted, on the other hand, that the Newcastle mines only are capable of continuing their supply for another thousand years; and if this reflection is insufficient to satisfy the disquieted minds of those who are still uneasy, they may console themselves with the reflection that there are many other districts which have only been, as it were, begun upon, and probably numerous deposits of which we are as yet ignorant, but which will be searched for and found when wanted. Besides which, it may, I think, be calculated, that of every chaldron of coals consumed in our ordinary fires, about one-eighth part is lost in the character of soot, smoke, and other unburnt matters; so that in London only, upwards of 100,000 chaldrons of coals are thus dissipated and unprofitably applied to the contamination of our atmosphere, which smoke, by improved methods of combustion, might be turned to profitable account.

In speaking of the general arrangement of the coal strata, I have said nothing of the dislocations to which they are subject, in consequence of what are called troubles, or slips, and dikes; that is, the strata are cut through, broken off, and sometimes thrown up on one side, and depressed on the other; and by the fissures and cracks thus produced are filled with broken stones and fragments of the strata, or with a hard species of rock called a dike, near which the coal is converted into a cinder, and from its cavities emits those tremendous torrents of inflammable gas, technically called blowers.

To the probable origin of these faults, or dikes, I shall afterwards advert more at full; they record one fact, namely, that the coal strata, subsequently to having assumed their present disposition and arrangement, have been subject to various disturbing causes, breaking their continuity merely in some instances, but in others affecting the whole district, and throwing it for hundreds of acres together out of its original position.

Lastly, as to the sources and origin of coal. Upon these subjects geologists, as usual, have amply indulged their inventive faculties. Every thing tends to show the vegetable origin of coal, and a regular succession might be shown, commencing with wood, little changed, and ending with coal, in which all traces of organic texture are lost. Yet even in the most perfect coal we frequently find some relic, some trace of a vegetable, or some remains of fibrous texture that announces its ligneous origin. In the leaves that occur in Bovey coal, Mr. Hatchett, to whom we owe many important observations and experiments upon this subject, found resin and extractive matter; and what is more to the purpose, he found a substance having properties intermediate between resin and bitumen, and therefore partaking partly of vegetable and partly of mineral characters; and more lately the same substance has been found in the principal coal-field of Staffordshire. Perhaps, therefore, antediluvian timber and peat bog may have been the parent of our coal strata, but then, how has its conversion been effected; is it merely by the agency of water, a kind of decay and rotting down of the wood; or has fire been called into action, torrefying the vegetable matter, and has the pressure under which this heat has operated prevented the escape of volatile matters, and caused them to assume the form of bitumen; and are those reservoirs of compressed carburetted hydrogen which I have mentioned as causing *blowers*, to be ascribed to such mode of formation? The discussion of these subjects might be prolonged, but it would end in nothing satisfactory. The theories that have been invented to account for our coal formations are full of weak and assailable points; the further we pursue them, the less do they satisfy us, and the more discordant do they seem with the phenomena they are intended to explain.

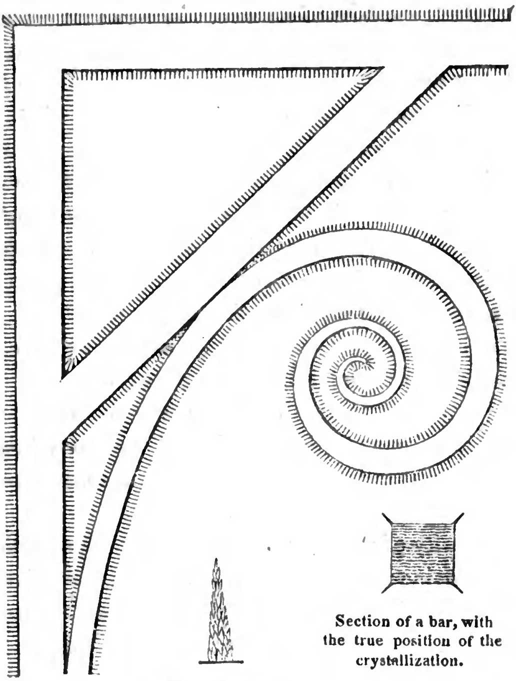
We should almost conclude, from the dogmatical air of some writers upon this subject, that they had seen the agents they speak of in active operation; that they had fathomed the depths of the globe, and measured its central heat; but if we compare our planet to an orange, and remember that we have not as yet penetrated its rind; if we compare it to the pasteboard globe of the instrument-maker, and remember that we have scarcely peeled the paper from its surface; these considerations should alone be sufficient to check the presumption of the theorist, and set bounds to the arrogance of hypothesis.

Art. VI. *On a peculiar Appearance exhibited by Hoar Frost.* *By* J. Mac Culloch, M.D., F.R.S.E., &c.

[Communicated by the Author.]

It has long been known that dew, as well as hoar frost, has a tendency to attach itself to particular bodies, rather than to others, and to rough surfaces in preference to smooth. Hence also it is found that hoar frost, which, from its permanence, is more easily examined than dew, is frequently deposited on acute edges and points, when the flat surfaces of the same bodies continue bare. I need not notice the speculations which have been entertained respecting the connexion of this phenomenon with electrical agency; but it is also obvious that the attachment of hoar frosts to strings, edges, and points, bears a striking analogy to that which occurs in many cases of the ordinary crystallization of salts from solution in water, where the preference is so often given to bodies of these forms as the first bases of attachment.

The causes which influence this mode of disposition are as much unknown as is every thing that relates to this mysterious process. It is to little purpose to form conjectures, or to propose hypotheses on this subject; but it is not useless to record any facts, which, by their accumulation and ultimate comparison, may tend to throw light on it; and, with this view, I transmit to you the enclosed sketch of a crystallization, or rather a deposit, of hoar frost, which is remarkable for its singularity, and which has not, as far as I know, been hitherto noticed by those who have paid attention to these subjects. As it will tend to save much explanation in words, and will at the same time render the appearance in question much more intelligible, I send you the sketch precisely as it was made on the iron railing of the door-way where I at first observed it.



Section of a bar, with the true position of the crystallization.

The crystallization magnified.

The temperature was little less than the freezing point, and there was a moderate fog with a high barometer, and an easterly wind; but no other meteorological phenomena were observed, nor had I any opportunity of examining the state of the atmospheric electricity, being far from home, and casually visiting in Portland-place, where this sketch was made.

Although the general effect of the distribution of this deposit of ice is sufficiently visible in the drawing, it will require a few words of explanation. The points did not consist of single crystals, but of pyramidal bodies, formed of crystals so minute and entangled, that their forms could not be discovered by the lens. They were about the sixth of an inch in length, and distant from each other by a space equal to the breadth of their bases; conjecturally, about the 30th of an inch. Where they were attached to the salient angle, or edge of a bar, they were at right angles to its line, and at the same time equidistant at the summits from each of the including planes, so as to form an angle of 45 degrees, with their surfaces on each hand. This distribution was maintained in the same manner in those parts of the iron-work which consisted of curved or circular parts; so that each group, or pyramid, was invariably placed at right angles to the tangent of the curve at that part, or in the direction of the radius of curvature. Hence it appeared, at first sight, that the effort of each pyramid consisted in an attempt to recede as far as possible on each hand, not only from the planes, but from the edge; and thus to attach itself at right angles to the latter. The same effect also took place in the interior as in the exterior of the curved parts; and thus the whole was ciliated like the leaves of some plants with a regular and beautiful fringed work.

The singularity of this appearance exciting my attention, I was induced to examine it more narrowly, for the purpose of seeing how the pyramids would dispose of themselves among the more intricate parts of the iron-work. It was found, in consequence, that where any two edges of a bar met at right angles, the crystals formed at an apex occupied the direction of the diagonal of the cube which was formed by the union of the bars, or maintained a distance equally removed from the edge of the joint on the one hand, and from the plane at right angles to it on the other. But the crystals on the two meeting edges, where nearest to the apex, did not immediately assume a rectangular position towards these edges; diverging gradually in succession from that on the angle till they assumed the regular position which they held on the remainder of the edge.

In the re-entering, or internal angle of the same joints, the crystal of the angle was also prolonged according to the diagonal of the cube; and here the crystals, intermediate between that and those which stood at right angles to the internal edges at a short distance, were so arranged as, in maintaining an equal distance at their bases, not to touch at their summits. Thus they all converged for a short space round the interior diagonal crystal, as, on the exterior, they diverged from it, in the manner represented in the sketch.

Where two planes met at a right angle, similar arrangements took place; the crystals, whether on the external, or within the internal angle, occupying a direction equidistant from the planes on each side.

This arrangement equally occurred when the angles of meeting of approximate edges or planes were greater or less than right angles, the equidistant position being regularly preserved; while, in the case of the interior angle formed by planes, any contact of the summits of the crystals was invariably avoided. Hence in those cases where very acute interior angles happened to exist, the crystals became so shortened, for the purpose of avoiding a contact between those on the neighbouring edges, that, near the extreme point, they at length vanished.

In all other more complicated cases of the meeting of the parts of the iron-work, the same general rules were found to prevail. In every part, in short, however intricate, where the crystals were formed, they seemed endued by a repulsive power, in consequence of which they tended as far as possible to recede equally from all the plane surfaces and edges in their vicinity, and, at the same time, to avoid any contact with each other.

It will be suggested that this repulsive property depended on some electrical condition, as we are not acquainted with any other power by which it can be explained; but it is not easy to assign that modification, or mode of action of this mysterious power, by which the effect could have been produced. I cannot pretend to suggest any solution of this appearance, and am better pleased to leave it thus recorded among the numerous insulated and inexplicable facts in science, of which the explanation will at some future time appear as simple as it now seems difficult.

I need only add that, on the same forenoon, all the iron railwork, which I examined for this purpose, between Portland-Place and Great George-Street in Westminster, exhibited the same appearance; but that I had never observed it before, and have never seen it since the winter of 1818, when this note was made.

The sketch only pretends to represent the general effect of this appearance to the eye. To have given the true position of the crystallization with respect to the sides of the bars, would have required a more highly finished engraving than was here admissible. A section of a bar is added for that purpose. All those intricate appearances, which it was thought unnecessary to represent, may easily be understood from the description.

Art. VII. *A Letter from* A. Copland Hutchison, Esq., *to* Sir Everard Home, Bart., *containing an Account of a successful Case of the High Operation for the Stone.*

[Communicated by Sir Everard Home, Bart.]

Dear Sir, *8th August*, 1825.

As your two successful cases of the high operation for the stone, published in the third vol. of *Strictures*, have encouraged me to adopt that mode of operating; and finding, also, that you have since that time operated in the same manner twice at St. George's Hospital; these being the only cases, I believe, that have occurred in England since the days of Cheselden*, I am induced to send you an account of the following case and operation, with full permission to make any use of it you may think proper.

* In one case upon a man 54 years of age, October 29th, 1824, in which the stone was extracted entire, weighing about 3½ ounces, though its texture was so loose beneath the external crust, that it afterwards broke to pieces in the hands of a gentleman who was examining its surface. In this case the patient died on the third day after the operation; but upon examination after death, the operation, in itself, was not the cause, the bladder having suffered so much by disease from the presence of the calculus, as not to admit of his recovery.

The other case was a boy eleven years of age; the operation was performed on the 3rd December, 1824, and the patient got well, though the wound, from his bad state of health, did not completely heal for two months. From his

Mr. C. aged 20, the son of an officer of Sheerness dock-yard, had laboured under symptoms of stone from his earliest infancy, and was twice sounded between the age of five and six, by a distinguished surgeon in London, but who was unable to satisfy himself of the presence of a calculus in the bladder. The patient and his father informed me that throughout his whole life he had never been able to retain his urine more than half an hour, night or day; the pain was occasionally so severe, also, in these frequent acts of micturition, that his life had become burthensome to him; and before this act could be effected in childhood, they were sometimes under the necessity of placing him on his head.

These symptoms continuing without any abatement, he consulted me about three months ago, when, on passing the sound, a calculus was discovered. Under these circumstances I determined to perform the high operation, and indeed there seemed to be no choice left; first, because I considered the stone to be too large for extraction by the lateral operation; and, secondly, because the bladder could not bear distention, for it never had been distended sufficiently with urine, to enable any one to perform the lateral operation with safety to the patient, and satisfaction to the surgeon.

The patient having been kept upon low diet for a month, his bowels frequently acted upon by purgative medicines during that period, and the bladder purposely made accustomed to the frequent touch, and sometimes rather rough treatment with the staff, the operation was performed on the 18th June, 1825, in the following manner, and in the presence of Dr. Lewis and Mr. Malin, of the army, and of Messrs. Brown, Cullen, and Keddell, surgeons, of Sheerness. The patient being placed on a mattress upon a table of ordinary height, with a pillow under his head; the pelvis raised considerably higher than his shoulders, with the view of removing the peritoneum as far from the parts to be cut as possible, while

birth the water had always dribbled from him; the stone was small and spherical, and its external surface made up of spiculated crystals.

I have added this note from your letter to me, with a view that all the operations of the kind which have come to my knowledge may be recorded together.

his feet rested upon chairs; standing on the right side of the patient, I made an incision, nearly four inches in length, with a scalpel, in the line of the linea alba through the integuments downwards over the front of the pubes, which latter step I found of great advantage both during the operation and in the subsequent treatment of the case. The incision was carried on down towards the bladder, between the pyramidales muscles, and through the linea alba; the latter being first punctured, and afterwards cut *transversely upon the symphysis pubis*, dividing some of the fibres of the pyramidal muscles on each side as we proceeded, so as to give a more free passage for the extraction of the stone. I then, with a director, detached this tense membrane (the linea alba) from its subjacent adhesions, and divided it upwards with a probe-pointed bistoury, introduced into the groove of the director, until I could readily insert the forefinger of my left hand, which is, on all occasions, the best director, and which was most conveniently done by standing between the legs of the patient. When about two inches and a half of the linea alba were thus divided, I thought we should have had sufficient space for cutting into the bladder and extracting the stone; but as the wound appeared deep and narrow, and there was a large calculus to extract, I cautiously extended the incision half an inch more, in the manner already described.

The bladder, very much contracted, was now clearly seen covered by fat, and was raised through the fat, upwards and forwards, to the external wound of the integuments upon the point of a silver staff, with a groove in its concave part, extending from close to its point downwards about two inches, and which I preferred to the *sond* used by the French surgeons, that instrument appearing to me both unnecessary and unscientific: the handle of the silver staff being held in a depressed position by my friend Mr. Brown, the anterior part of the fundus of the bladder was pierced from without, by a straight sharp-pointed bistoury, passing its point into the groove of the staff, and carrying the incision downwards and forwards to the pubes, until there was room for the fore-finger, which was introduced for the purpose of ascertaining the size of the calculus, that the opening in the bladder might be enlarged accordingly. Having satisfied myself on this head, I hooked up the fundus of the bladder with my finger, and enlarged the incision towards its cervix, sufficiently to admit of the free extraction of the stone. Some little difficulty here arose in disengaging the stone from its situation, notwithstanding the great assistance obtained by the introduction of the fore-finger of my right hand into the rectum, while the fore-finger of my left was in the bladder to cant or turn the stone, so as to extract it by its smaller axis; and, indeed, this was not effected until the single blade of a pair of small stone forceps was introduced. The stone being thus turned, it was easily removed with the finger and thumb. In point of fact, so firmly did the bladder grasp the calculus, that the idea was conveyed to our mind of its actual adhesion to the coats of that viscus.

One small cuticular artery was divided at the first incision, and secured at the time, to prevent the future steps of the operation being obscured by the bleeding, and the subsequent issue of blood into the cavity of the pelvis.

Two slips of linen, dipped in oil, were introduced through the external wound on each side of the bladder, but not *into the bladder*, as was done in your first case, published in the *Philosophical Transactions* *; a gum elastic catheter was passed into the bladder so as just to enter its cavity, and no farther, and was secured in this situation by tapes attached to the instrument, and to an elastic band, or retainer, in the manner described in your paper, but which in this case was made of flannel, lined with calico. The sides of the wound were brought together by slips of sticking plaster, and the parts supported by a flannel roller, passed three or four times round the pelvis and the lower part of the abdomen. The patient, by means of pillows, was placed on an inclined plane, a vessel was secured to the end of the catheter to receive the urine, and he had an anodyne draught administered to him.

* Afterwards republished in your third vol. of Strictures.

I am fully aware that there are those who may imagine the trifling difficulty we experienced in extracting the stone, arose from the depth, narrowness, and consequently from the supposed tightness, as it were, of the wound, through the parietes of the abdomen; but I can assure them that such was not the fact, for I enjoyed the most perfect freedom in that respect. The circumstance in my mind arose rather from the position of the stone, its size, and the strong contraction of the bladder upon it; for it will be borne in recollection that the bladder, in this case, had never been distended beyond the size of the calculus, and that which two ounces of urine would occasion. What the bladder wanted in capacity, it possessed in the increased thickness of its parietes, for it was fully a quarter of an inch thick at the part divided; so that its muscular power must, therefore, have been considerable. I cannot avoid, in this place, repeating the great advantage to be derived from dislodging the stone, in such cases, by the introduction of the finger into the rectum, and which appears to me a great improvement in this mode of operating.

19th June. Slept two hours during the night; about six ounces of urine came through the catheter, the rest by the wound. His pulse this morning was 120, and full; he was bled, therefore, to twenty ounces, and I prescribed saline draughts every three hours, each containing ten drops of liquor antimonialis. He was otherwise free from pain, and only complained of a smarting in the wound.

20th. Had four hours continued sleep during the night, *for the first time in his life*: urine passed equally by the instrument and the wound: pulse about 100, and soft, and he perspired profusely during the night: the draughts were continued, and he was supplied with barley-water for his common drink. An aperient draught was prescribed for the following morning.

21st. Slept nearly six hours last night: pulse 90: skin moist: no pain whatever: his bowels were well moved by the draught: urine passed as the day before, and he sat up about two hours, with his feet hanging over the side of the bed, and resting upon a chair.

No one bad symptom occurred afterwards: the urine continued for about five weeks to come occasionally by the wound in small quantities, the rest by the natural passage: the wound itself continued to look healthy from the first, and gradually closed until the 31st July, when it was completely healed, and the patient voided his urine in a full stream through the urethra.

Thus terminated this successful case; and, I am strongly inclined to believe, that the patient's recovery would have been still more speedy, had the catheter been sooner altogether withdrawn from the bladder; for it was retained almost constantly in that situation about three weeks, removing it night and morning only, for the purpose of clearing it of mucus.

The stone weighed eleven drachms two scruples and four grains. It is two inches long and one inch and half in width. A section has been made of it, and my friend Dr. Prout has had the kindness to analyze it for me. The result will be found in the Doctor's note addressed to me.

I have the honour to be,
Dear Sir,
Your very obedient humble servant,
A. Copland Hutchison.

To
Sir Everard Home, Bart.,
&c. &c. &c.
Sackville-Street.

London, July 12.

My dear Sir,

The nucleus of the calculus which you were kind enough to leave for me, consists essentially of the lithate of ammonia, mixed with some oxalate of lime, (and probably a little carbonate of lime,) the phosphates, and animal matter. The exterior laminæ are chiefly composed of the phosphates; but two or three small fragments detached from the surface were found to consist chiefly of the phosphate of lime; and this circumstance made me anxious to see the urine, as I had never before known this salt *deposited alone* from that secretion, and hence had been led to believe that calculi composed entirely of that substance were not of urinary origin, but formed in a manner analogous to those met with in the prostate gland.

The specimens of urine which you were good enough to send me arrived in safety, but I am sorry to say that they throw no light on the subject, or rather on the point in question. They are alkaline, and of a very bad character, and abound in the *mixed* phosphates, (that is, the phosphate of lime and the triple phosphate of magnesia and ammonia,) as is usual in such cases.

I am, my dear Sir,

Yours, &c. much obliged,

W. Prout.

40, *Sackville-Street.*

Art. VIII. *On Calcareous Cements.* By John White, Esq.

[Communicated by the Author.]

To the Editor of the Quarterly Journal.

Sir,

In the Miscellaneous Intelligence contained in your last number, you have remarked, that you should take notice of such facts respecting the theory and improvement of calcareous cements as were brought to light at different times, with the fair conclusions to be drawn from them, being convinced, in the highest degree, of the importance of the subject, and of the advantage which the investigation must lead to.

Having for nearly thirty years experienced, practically, the imperfections of the various cements in use in England, I have been led into a variety of examinations of them, and do not hesitate to communicate to you an account of a series of experiments which will, in my opinion, contribute essentially to a knowledge of the subject.

The first endeavour at investigation was made by a comparison of various burnt clays, obtained in the neighbourhood of London and in Kent, with the imported Pozzolano, as sold in London; but the imported material was so variable in its nature, that little resulted beyond a knowledge that it possessed more calcareous matter, and that it was more uncertain and variable in the sizes of the grains, than that of British manufacture.

The next endeavour was to ascertain what, practically, were the best sizes of the particles to be used with lime, and in what state and what species of lime entered best into the combination with them.

It appeared that either the foreign or British Pozzolano, if reduced into a very fine powder, lost considerably its power of adhesion, though it was more plastic. It necessarily was inferred, that the greater the variety of dimension of the particles, the greater would be the entanglement of the asperities, and, consequently, the greater the adhesion. Of the mortar made, it also appeared that the finer the lime could be ground, the more perfect would be the combination, and the harder the mortar obtained, because the hard particles of the Pozzolano being in a state of actual contact, no compression was likely to take place; and which, in fact, upon the subsequent investigations proved to be the case.

My reasons for trying the Pozzolano were these. I conceived that I should have two causes for the induration of the mortar; one, the disposition which many burnt materials have to unite intimately with lime; the other, the variety of form which the fracture of burnt clay produces; the real difficulty which existed was, the obtaining a perfect knowledge of the best state, and the best mode of indurating properly the clay itself. For if the burning of the clay were such as to cause vitrification, an imperfect mortar was made; perfect glass, scoria, and pumice-stone, produced very inferior mortar; perfect Pozzolano appeared to be made when a chalky clay was so indurated by fire as to put on the appearance of a commencing vitrification only.

First Experiment, tried August 9, 1824.

A pier, three feet four inches wide, one foot ten and a half inches thick, five feet eight inches high, was built on the 9th July, 1824, and was composed of hard sound London burnt stock-bricks, and mortar of one part ground lime, one and a half parts sand, and two parts rough Pozzolano. It was elevated by applying screw-jacks on each side, course by course, beginning at fifteen courses, or three feet nine inches from the bottom; this elevation from its foundation took place without any separation of the courses until the screw-jacks were placed two courses, or six inches from the top, when these two courses separated from the remainder of the pier.

The remainder of the pier was then thrown down, when it separated at eight courses from the foundation, and exhibited that the middle was not dry.

This experiment was tried in the presence of Mr. Brunel, Mr. Mathew Wyatt, Mr. Smith, and many other gentlemen.

Second Experiment, April 21, 1825.

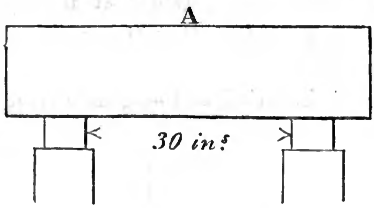
A pier of thirty-five courses of brickwork high, being six feet wide, three feet thick, and eight feet ten inches in height, was built also July 9th, 1824, of hard sound London stock-bricks, and of a cement composed of three parts British Pozzolano, ground and sifted, having particles, none being bigger than one-eighth of an inch, and one part of ground Dorking lime, unslacked, fresh mixed. This pier was first elevated from its base by a strong chain, grappling it at nine courses of brickwork from the top, again in the same way at six courses from the top, and at last the whole pier was suspended by a set of Lewis's, let into the middle of the top of the pier, about fifteen inches deep; the weight of the pier was about nine tons.

This experiment was tried in the presence of Mr. Brunel, Sir Thomas Baring, Admiral Sir Edward Codrington, Colonel Lowther, Mr. Smirke, Dr Chumside, and nearly two hundred spectators.

Third Experiment, made May 12, 1825.

It having been suggested by Mr. Brunel, and Mr. Smirke, that it would be useful to try the comparative adhesive properties of these cements by building other piers of such dimensions as might enable them to be laid horizontally, and have weights placed on them in the middle, nine piers were constructed on the 21st and 23rd April, in a vertical position, and of the following dimensions, *viz.*, six feet high, and about fourteen inches square.

The First Pier (A) which was tried was of pure fresh Roman cement, and was accidently broken in laying down where the cement had partially set in the joint of brickwork before the adhesion had taken place. A fragment of the pier, three feet six inches in length, being laid down as here represented,



was carefully loaded at A with weights half a hundred at a time; it supported eleven hundred weight, and broke under eleven and a half hundred weight.

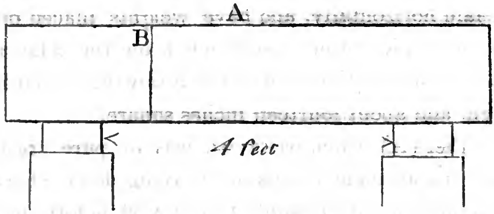
The Second Pier (B) was composed of three parts Pozzolano and one part stone lime, reduced to putty as common mortar. This pier was similarly placed in the supports, it broke in the middle, and a fragment was similarly loaded, when it supported four hundred weight, and broke under four and a half hundred weight.

The Third Pier (C) three parts Pozzolano, and one part stone lime, ground and fresh. This pier broke in turning it round, and the fragment laid by, for further experiments.

The Fourth Pier (D) three parts pure sharp sand, and one part stone lime. This pier broke into three pieces on attempting to lay it on the supports.

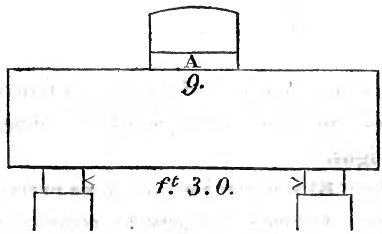
The Fifth and Sixth Piers (E F) of three parts sharp washed sand, one part chalk lime, crumbled to pieces on attempting to place them.

The Seventh Pier (G) was composed of Atkinson's Roman cement one part, pure sand one part.



Weights being carefully and successively applied at A, half a hundred at a time; it supported five hundred weight, and broke under five and a half hundred weight at B; on examining the separated parts, it was evident that the mortar had not equally adhered to every brick

The largest end being tried in the same way as in the first experiment, *viz.*

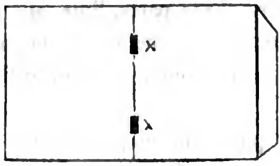


It was left nearly half an hour with one ton weight at A, and broke in consequence of a shock it experienced by the breaking of the pier subjected to the next experiment.

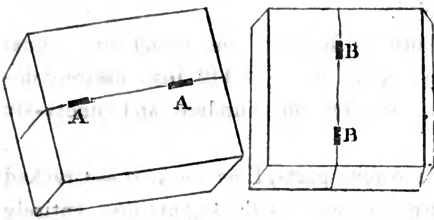
The Eighth Pier (H) four parts Pozzolano, and one part air-slacked stone lime. This broke in laying down. A second experiment was made as in the preceding pier, it supported five and a half hundred weight, and broke in two pieces.

The Ninth Pier, built of rough lumps of Pozzolano, in imitation of Roman walling, nine parts Pozzolano, and one and a half parts stone lime, was not sufficiently dry to be experimented upon.

Continuation of Experiment No. II. May 12*th*.



Two wrought-iron wedges were driven with sledge hammers at X X, for the purpose of splitting this pier; it resisted very long, and when separated, it was found that many bricks had not taken the mortar.



The mortar was not completely dry, it would require another summer to give it all the toughness and tenacity it can acquire.

The same wedges were afterwards driven with the same hammers, and greater effort at A A. It resisted and repelled the wedges several times, but when separated, the mortar and bricks appeared to have resisted alike.

A third attempt was made by driving the same wedges at B B, when a new separation took place, exhibiting the same appearance as in the first separation.

This experiment was made in the presence of the Earl of Southampton, Mr. Brunel, Lieutenant-General Sir Alexander Bryce, and many others.

Fourth Experiment, *May* 20, 1825.

The adhesion of the materials being, in some measure, ascertained by the foregoing experiments, it appeared desirable to learn how far the application of weight vertically would effect them, when the following trials were made at Mr. Bramah's, at Pimlico, by subjecting portions of the above piers to the hydrostatic press.

First Trial, Pier A. Roman cement.—Section one hundred and ninety-six inches superficial, cracked with 48,960 lbs., the compression being continued until it was entirely destroyed with 92,160 lbs. or 41 tons, 320lbs.

Second Trial, Pier (B) Pozzolano three parts, lime one part.—Section one hundred and eighty inches superficial, this being overlaid with sand at top, the same escaped compression, so that the result was uncertain.

Third Trial, Small fresh-built pier nine inches square, Pozzolano three parts, ground lime one part.—Compressed a very little with 18,720 pounds, entirely destroyed with 24,480 lbs., section eighty-one inches superficial.

Fourth Trial, (G) Atkinson's Roman cement and sand, equal measure.—Cracked on two sides with 37,440 lbs., entirely destroyed with 80,640 lbs., section one hundred and ninety-six inches superficial.

Fifth Trial, (H) Pozzolano four parts, lime one part.—Cracked with 28,800 lbs., fracture increased with 48,960 lbs., entirely destroyed with 51,840 lbs.

Sixth Trial, (C) Pozzolano three parts, Dorking lime one part.—Fractured one side with 31,680 lbs., fracture increased with 43,200 lbs.; fracture again increased with 48,960 lbs.; this experiment was not pursued until the pier was entirely destroyed, as in the preceding one.

Seventh Trial, (D) sand three parts, one part stone lime putty.—Compressed on application of weights one-eighth of an inch, cracked with 40,320 lbs., fractured in five places with 46,080 lbs., which entirely destroyed it.

Eighth Trial, Pier of Portland stone fourteen inches by 12 inches, two feet seven inches high.—A sudden fracture was produced which divided the stone into two pieces in the centre of the widest side with one hundred seventy-three and a half tons; the upper end of the stone was bedded in Pozzolano, which was compressed into a cake of five-eighths one-fifteenth thick, in a wet state; which cake remained quite solid after the fracture of the stone.

These trials were made in the presence of Mr. Smirke, Mr. Brunel, Lieutenant-General Sir A. Bryce, Mr. T. L. Donaldson, and many other gentlemen.

Inferences from the foregoing Experiments.

It may be inferred from the foregoing experiments, that an important adhesion of brickwork had taken place by the use of Pozzolano, sand and lime, in the short period of thirty days.

That from the use of Pozzolano and lime in the proportions specified, almost all the advantages required from a good building cement were obtained.

That Lord Mulgrave's or Atkinson's cement had, in the short period of twenty-three days produced an induration which was sufficient to maintain almost any weight brickwork was capable of for openings in buildings; the effect probably would have been the same in Parker's, had the material not set before the bricks were fixed in it, further, that Pozzolano had not, in that period, produced an equal adhesion, and that common mortar had produced hardly any; and it appears from the splitting of the large piers thrown down on the 21st April, that an increasing induration took place; this was evident from the nearly equal fracture of the bricks and cement.

The incompressibility of mortar being one of its most material qualities, it results that Parker's, Mulgrave's, and Pozzolano, are so far equally useful, that brickwork composed with them will bear on each superficial foot before the bricks will crack, about twenty-three tons, that fifty tons will totally crush such brickwork; and that Portland stone, of the best quality, will not split with less than one hundred seventy-three and a half tons, and that a bedding or joint of Pozzolano mortar is not destructible with that weight.

Art. IX. *Remarks on Phytolacca Dodecandra, or, the Mustard Tree of the Scriptures. By* John Frost, F.L.S., *Member of the Royal Institution,* &c.

[Communicated by the Author.]

The remark in the sacred volume*, that a grain of mustard seed should become a *tree*, must have appeared to many very pa-

* Luke, chap. 13, ver. 19, " A grain of mustard seed which a man took and cast into his garden: and it grew and waxed a great tree, and the fowls of the air lodged in the branches of it."

It

radoxical; for what we know under that name is procured from an annual plant, (sinapis nigra of Linnæus,) which has an herbaceous stem, and never attains a greater height than three feet; therefore it is quite evident that that cannot be the plant referred to, the word δενδρον, which is used, implying a shrub, or tree, and of course not any plant like sinapis nigra, with an herbaceous stem. I am not acquainted with any species of *sinapis* that can be called a shrub, much less a tree.

The author of a Theological Dictionary, of some repute at the present day, has stated under the article mustard seed, that the mustard tree alluded to by our Saviour was a species of sinapis, and asserts as a proof of the correctness of his definition, the fruit of all the members of that genus having *a cruciform corolla.*

The plant most likely to be the mustard tree of the Scriptures is a species of *Phytolacca* *, which grows abundantly in Palestine; it has the smallest seed of *any tree* in that country, and attains as great an altitude as any. This circumstance, together with that of its being indigenous to the place where the observation was made, are sufficient to convince us of the identity of the tree referred to. In addition to which I can adduce two facts which will greatly tend to confirm this opinion.

The first is, that of the Americans using the fresh sliced root of Phytolacca Decandra † for the same purpose as we use mustard seed, *viz.*, that of a Cataplasm. I have been informed that they call it (P. decandra,) *wild mustard.*

The second is, that of the seed of a species of Phytolacca affording what the seed of sinapis nigra does in great abundance, nitrogen; an element not found in many plants, excepting those which belong to the natural orders Cruciatæ and Fungi. These

It may here be proper to state that the term σιναπεως à σιναπι, in the original language of the New Testament, does not signify the seed which is obtained from any species of the genus which we know by the term Sinapis.

* Phytolacca, derived from φυτον, a plant, and lacca, or lac, a gum resinous exudation, of a red colour. The petioles of every species of Phytolacca, which I have seen, have a degree of redness, more or less.

† This is a perennial herbaceous plant, a native of Virginia, occasionally found in our gardens under the name of the American Soke-Weed.

two facts prove the analogy between the *properties* of the genera Phytolacca and Sinapis. Linnæus, in his Materia Medica, points this out very clearly.

It appears that the mustard tree of the Scriptures is undoubtedly a species of Phytolacca, most probably *P. dodecandra* of Linnæus. I cannot be quite positive as to the species, as the plant which is the subject of this communication has not yet flowered.

29th August, 1825.

Observations on an Emetic Oil termed "Pinhoën Oil."

An expressed oil has just been sent to me from the Brazils, under the name of Pinhoën oil. It is used there as an emetic, and acts powerfully in the small dose of one or two drops.

It appears to be procured from the seed of a species of Jatropha, of which there are several indigenous to South America, most likely J. multifida, the fruit of which has been long known under the appellation of the French Physic-Nut. From some experiments which I made about two years since on the seeds of several species of Jatropha, I am inclined to think that there can be but little doubt of the plant which yields this emetic oil being of the genus just mentioned. And it may here be remarked, that the expressed oil of the seed of very many species all produce emetic and cathartic effects; the former attended by a sensation of heat about the fauces, and by doubling the dose, drastic purgative effects ensue.

Mr. Reeves, of Canton, informed me that the varnish which the Chinese are so famous for making for covering paper boxes, tea chests, *&c.*, is formed by boiling the expressed oil of the seeds of Jatropha curcas, with oxide of iron. The seeds of J. curcas have frequently been mistaken for those of Croton tiglium, only, one can suppose, from there being a degree of analogy between their effects, as there is none in point of appearance.

Art. X. *A Monograph of the Genus* Tachyphonus; *a group of Birds belonging to the Tanagræ of Linnæus. By* William Swainson, Esq., F.R. and L.S., *&c.*

[Communicated by the Author.]

The new objects in the higher departments of zoology continually coming before us, have, on many occasions, confirmed the justness of those views which have led to the formation of new divisions. Several of the genera, proposed but a few years ago by the continental ornithologists, and which were then supposed to rest on the authority of a solitary example, have since been augmented by a host of others, hitherto unknown to naturalists, or indiscriminately scattered in different genera of the Linnæan system.

On the other hand, several of these groups, either from not having attracted particular attention, or from no new examples having been brought to light, remain confined within their original limits.

M. Vieillot has given the generic name of *Tachyphonus* to the fifth division of the Linnæan Tanagers, as arranged by that acute ornithologist M. Desmarest, and in which is comprised only two species, the *Tanagra nigerrima* of Gmelin, and the *Tanagra cristata* of Linnæus; both natives of South America. During my residence in Brazil, I was fortunate in procuring several other species, evidently belonging to this group, and which *seem* to be hitherto unrecorded. This latter point, however, is doubtful; for within the last few years the provinces of Brazil have been traversed in almost every direction, by many learned and indefatigable naturalists, who are now occupied, under the patronage, and even at the expense of the continental governments, in publishing ample and richly illustrated accounts of their extensive discoveries in every branch of natural science. Under these circumstances we must receive all descriptions of the productions of Brazil, so far as their *novelty* is concerned, with a certain degree of doubt. In the absence of an extensive and public zoological library, few of these costly works are accessible, and the treasures collected in many of them do not appear till after an interval commensurate with the care and expense employed to render them worthy of their immediate patrons. If this be well understood, I see no reason why we should withhold the contribution of our mite towards the general stock of knowledge, under the apprehension of doing that imperfectly, which others may have previously done well; and with the possibility that, after all, the materials for such a contribution may rest alone with ourselves.

I am unprepared to offer any detailed remarks on the situation which *Tachyphonus* may be supposed to occupy among the *Tanagræ*. By its strong, conic, and somewhat lengthened bill, and by the sinuosity of the margin of the upper mandible, this group seems to have a close affinity with *Pyranga*. Some species * in which the bill is much shorter and proportionably thicker, might, at first sight, be mistaken for *Fringillæ*; while in others †, the form of this organ is considerably modified, and resembles that of *Tanagra velia*, and its allies. But the typical species ‡ are more particularly distinguished by the elevated base of the bill, which is carried on towards the crown, and divides the frontal feathers. This character, so much developed among the *Icteri* of the new world, and, in a less degree, among the African weavers, probably induced the writers of the last century to associate the type of this group with the Linnæan Orioles.

We have little or no information respecting the manners or economy of these birds; judging from the hardness and general strength of the bill, the margins of which are frequently inflexed, we may suppose that they feed principally upon seeds; but, in some species §, the base of this organ is widened, and the bristles of the rictus are sufficiently lengthened to indicate a partially insectivorous disposition.

The colour of the plumage is frequently an obvious, though not an essential, character in natural groups. In *Tachyphonus* it is usually black, or of one uniform tint on the upper parts, unbroken by spots, and unrelieved by those beautiful colours which so ornament many of the Linnæan Tanagers. Nevertheless the head

* *Tachyphonus rubescens. T. fringilloides.* † *T. Desmaresti. T. tenuirostris.*
‡ *T. nigerrimus. T. olivaceus. T. Vigorsi.* § *T. Suchi. T. cristatus.?*

(in many species) is adorned by a crest of bright red or yellow feathers, of a thin and delicate texture, which may be erected or concealed at pleasure. A similarly-formed crest is likewise seen in some few species of the neighbouring genus *Pyranga*: but among the other *Tanagræ* it is entirely wanting.

TACHYPHONUS. *Vieillot.*

Rostrum elongato-conicum, validum, lateribus compressis, marginibus inflexis; mandibulæ superioris margine sinuato, apice emarginato.
Alæ rotundatæ, remige primo brevissimo, tertio quartoque paribus, longissimus.
Cauda rotundata, rectricibus latis.
Tarsi breviores, squamis lateralibus integris.
Caput fere cristatum.

Bill lengthened-conic, strong, laterally compressed, base with weak bristles, margins inflexed; margin of the upper mandible sinuated, the tip notched.

Wings rounded, the first quill shortest, the third and fourth longest, and of equal length.

Tail rounded; the feathers broad.

Tarsi rather short; the lateral scales entire.

Head generally crested.

1. Tachyphonus *nigerrimus.*

T. ater; tectricibus minoribus niveis (maribus) ferrugineis (fœminis.)

Deep black; lesser wing covers snowy, (male).

Entirely ferruginous, (female).

Tangara nigerrima, Gmelin. p. 899.
Tangara roux et Tangara noir. Buffon edit. Sonnini, tom. 48, p. 290.
Le Tangara noir. Desmarest. pl. 45, (mas.) pl. 46, (fœm.)
Tangara noir Pl. Enl. 179 f. 2. male.
Oriolus leucopterus. Latham Ind. Orn. (mas.) Shaw's Zool. 7, 433.
White-winged Oriole. Lath. Synopsis. Shaw's Zool. 7, 433.

Total length 7, bill from the rictus $\frac{7}{10}$, wings $3\frac{1}{2}$, tail $3\frac{1}{2}$, tarsi $\frac{8}{10}$.

This bird, considered by M. Vieillot as the type of the group, is too well known to require a detailed description. The young males of the first year are clothed in the ferruginous coloured plumage of the other sex.

T. *nigerrimus* seems to be a common inhabitant of the equinoctial parts of South America. I found it at Pernambuco, and it appeared to be not unfrequent on the table land of Bahia, associating in pairs, and never perching on the ground.

2. TACHYPHONUS *olivaceus*. Sp. Nov.

T. supra olivaceus, infra fulvido-albus; vertice cinereo; regione oculari flavâ.

Above olive; beneath fulvous white; crown cinereous; region of the eye yellow.

Description.—Size of *T. nigerrimus;* the form of the bill is typical, but its size is intermediate between that bird and *T. Desmaresti;* its colour is black. Plumage above, brownish olive; ears, crown of the head, and sides of the neck, dull cinereous; the feathers round the eye are yellow, and the sides of the head obscure olive. All the under parts are fulvous white; vent and flanks brownish, but the under tail covers are tinged with fulvous. Tail rather long, the colour brown; but the outer webs of the feathers are olive. Tarsi pale.

Total length $6\frac{1}{2}$, bill $\frac{7}{10}$, wings 3, tail $2\frac{3}{4}$, tarsi $\frac{7}{10}$.

I have ventured to describe this as a distinct species; although, from its olive-coloured plumage, I entertain some doubts whether it may not prove to be a female, in which case the specific character must be altered to suit the plumage of the male. In the proportions of the bill and tail it does not agree with any of the species here described.

This bird was originally in the collection of Mr. Hullet: Buenos Ayres was given as its locality.

3. TACHYPHONUS *Vigorsi*. Sp. Nov.

T. violaceo-niger, cristâ rubrâ; scapularibus tectricibusque interioribus niveis.

Glossy black; crest red; scapular and under wing covers snowy.

In its size and general proportions, this species resembles *T. nigerrimus*, but the bill is comparatively shorter; and the upper mandible is less arched, and less elevated between the frontal feathers. On the crown of the head is a short narrow crest of an orange-red colour, and the scapular and inner wing covers are of a snowy-whiteness; with these exceptions, the whole of the plumage is deep black, richly glossed with blue. The fourth quill is somewhat longer than the third and fifth: the tarsi are black, and rather short. Inhabits Southern Brazil. Manners unknown.

Total length 7, bill $\frac{6}{10}$, wings $3\frac{1}{4}$, tail $3\frac{1}{4}$, tarsi $\frac{3}{4}$.

I cannot designate this species (one of the most conspicuous in the group) by a name more distinguished than that of the highly-gifted ornithologist, whose essay on the "Affinities of Birds" has thrown a new light on this interesting department of nature.

4. Tachyphonus *rubescens.*

T. suprà rubro-fuscus, infrà rubescens; cristæ coccineæ marginibus lateralibus nigricantibus; rostro brevi, conico.

Above brown, beneath reddish; crest crimson, the lateral margins blackish; bill short, conic.

Fringilla cristata? Fr. spadicea, subtus rubra, vertice cristato rubro, temporibus nigris.
Gmelin Syst. Nat. 1. pars 2. p. 926.
Latham Ind. Orn. 1. 434. 2.
Moineau de Cayenne Buff. Pl. Enl. 181. f. 1.
Blackfaced Finch? Latham, Gen. 3. 253. 3.
Fringilla cristata. Shaw, Genl. Zool. 9, p. 440.

Description.—Size rather smaller than that of the goldfinch; bill short, compressed, conic; the margins of the upper mandible very slightly sinuated. The colour of the upper plumage is reddish-brown, excepting the rump, which is dull crimson. On the crown is a somewhat lengthened crest of bright crimson, margined on each side by a stripe of dark-brown; all the under parts, from the chin to the tail covers, dark red; wings and tarsi brown; tail blackish, and nearly even; inner wing covers white; bill above dark brown; beneath almost white; sides of the head brown.

Total length $5\frac{1}{4}$, bill from the rictus $\frac{9}{20}$, wings $2\frac{1}{2}$, tail $2\frac{1}{2}$, tarsi $\frac{6}{10}$.

This description has been drawn up from a specimen of the male bird sent to me from Rio de Janeiro. It seems to differ from the *Moineau de Cayenne* of Buffon, in some few particulars, which may probably arise from the difference of their locality. The black-faced finch of Pennant, said to inhabit Carolina, and the more northern provinces, cannot surely be the same as the Brazilian species.

It is not without hesitation that I have ventured to remove this bird from the *Fringillæ* of Linnæus, and to place it in the present group. In so doing, I have been guided by its general *habit*, which agrees more with *Tachyphonus*, than with any other defined genus; the bill, although short and conic, is very much compressed, while the margins are inflexed, and very slightly sinuated.

5. TACHYPHONUS *fringilloides.* Sp. Nov.

T. suprà cinereus, infrà albens, cristæ coccineæ, marginibus lateralibus nigris; rostro brevi, conico.

Above cinereous, beneath whitish, crest crimson, the sides black, bill short, conic.

This beautiful little bird is even smaller than the preceding. The colour of the upper mandible is brown, and that of the under white; the crest is of a vivid crimson, and the sides margined by a stripe of deep black; the rest of the upper plumage is cinereous-gray, and this colour tinges the white of the under plumage; the wings are brown; the tail black, and very slightly rounded; tarsi and claws pale.

Total length $4\frac{3}{4}$, bill $\frac{9}{20}$, wings $2\frac{4}{10}$, tail $2\frac{4}{10}$, tarsi $\frac{13}{20}$.

I believe this to be a bird of rare occurrence. During a month's encampment on the table land of Bahia, I failed in procuring more than two individuals, both of which were males; neither am I aware of its having been met with in any other part of Brazil. In the shortness and compression of the bill it agrees with *T. rubescens;* but the tail, which in that bird is nearly, if not quite, even, in this is more rounded; the feathers likewise are broader. The margins of the bill are inflexed, but not sinuated; and the acute angle, which constitutes a very prominent character in the *Fringillæ*, is wholly wanting in both these birds.

6. Tachyphonus *Suchii.* Sp. Nov.

T. olivaceus, infrà pallidè fulvus; cristâ flavâ; alis nigris; remigum pogoniis internis basi albis.

Olive, beneath pale fulvous, crest yellow; wings black, the internal base of the quills white.

Description.—This is of an intermediate size between *T. Vigorsi* and *T. cristatus*, but the bill is smaller and much weaker than that of the last-named bird, the upper mandible also is less convex above, and the margins, although inflexed, are not at all sinuated; the bristles of the rictus are stronger, and extend to half the length of the bill. The front and sides of the head are black; and the crown is ornamented by a half-concealed crest of straw-coloured yellow feathers; the ears are dusky black. The general colour of the upper parts of the body is obscure olive; the wings are deep black; the scapulars tinged with olive; and the quill feathers, with the exception of the two first, are all marked at the base of the inner webs by a band of pure white. All the under plumage is pale fulvous, tinged with olive on the flanks, and verging towards white on the abdomen: the tail is black, of a rounded form, and the feathers rather broad; tarsi pale.

Inhabits the southern parts of Brazil. My specimen was liberally communicated to me by Dr. Langsdorff.

Total length $6\frac{1}{2}$, bill $\frac{6}{10}$, wings $3\frac{1}{4}$, tail $3\frac{1}{4}$, tarsi $\frac{7}{10}$.

I recollect seeing this species, or one very nearly resembling it, among the valuable zoological collections made by Dr. Such, in the southern provinces of Brazil, and which have furnished materials for some interesting and valuable papers, communicated by that gentleman to the scientific Journals. I therefore embrace this opportunity of affixing his name to a native bird of that country which he has so successfully explored.

7. Tachyphonus *Cristatus.*

T. niger; cristâ rubrâ; mento uropygioque fulvis; scapularibus tectricibusque interioribus albis.

Black, crest red; chin and rump fulvous; scapular and inner wing-covers white.

Tanagra cristata. Linn. Syst. Nat. 317. Index Orn.
Tanagra cayennensis nigra cristata. Brisson Ornith. append. p. 65, tab. 4, f. 3.
Hauppette. Buffon Pl. Enl. 7, fig. 2.
——— Desmarest Hist. Nat. des Tanagres, pl. 47.
Crested Tanager, Lath. Gen. Synopsis 2, p. 221.

Description.—Bill entirely black, strong, and rather dilated at the base; the rictus furnished with a few weak bristles, and the margin of the upper mandible considerably sinuated. The crown of the head is covered by a conspicuous crest of bright orange-red, having the margins paler and more of a fulvous yellow. The general colour of the plumage, both above and beneath, is brownish black; across the rump is a broad band of fulvous, and beneath the chin is a short stripe of the same colour, extending half way down the throat: part of the scapula, and the whole of the inner wing-covers, are pure white; wings brown; tail black, and rather lengthened; tarsi brown.

This species appears subject to some variation in the colour of the crest, and in the extent of the fulvous stripe on the throat. But as these varieties have not come under my own observation, I must refer the reader to the admirable work of M. Desmarest on this family. The above description has been taken from the male bird as it usually occurs in Brazil: the female I have never seen. I have frequently had occasion to observe, that where the same species is found both in Cayenne and Brazil, the specimen from the former country is invariably larger and more richly coloured: may not this be attributed to the excessive heat and humidity of that province, which may be supposed to affect the animal productions no less than those of the vegetable world?

8. Tachyphonus *Desmaresti.* Sp. Nov.

T. violaceo-niger; cristâ uropygioque fulvis; crisso rufo; tectricibus inferioribus niveis.

Glossy black; crest and rump fulvous yellow, vent rufous; under wing-covers snowy. Pl. Enl. 301, f. 2.

Description.—Size of *T. cristatus*; but the bill is longer, more slender, and more pointed; and the upper mandible is less curved. The general colour of the plumage, both above and beneath, is deep black; glossed with blue in every part but the wings and tail. The crown has a concealed crest of buff-coloured feathers, and on the rump is a broad band of the same tint. The vent and under part of the flanks are deep rufous; tail-covers black; under wing-covers snowy white. On closely examining the tarsi of the specimen in my collection, (the only one I have the means of consulting,) I find they do not naturally belong to the bird. This artifice was formerly much resorted to by bird-stuffers, who thought it no sin to give new legs to a bird when its own were unserviceable.

In the sale catalogue of Mr. Hullet's collection, this bird is stated to have been sent from Buenos Ayres.

Bill $\frac{7}{10}$, wings 3, tail 3.

Naturalists have hitherto considered this as a variety of *T. cristatus*, but even admitting that the difference in their plumage is not very considerable, the more lengthened, pointed, and attenuated form of the bill, in this bird, at once points it out as a species intermediate between *T. cristatus* and *T. tenuirostris:* I therefore feel happy in recording it under the name of *Desmaresti*, as a just tribute of respect to that celebrated ornithologist, who has illustrated the *Tanagræ* of Linnæus in one of the most splendid publications of the present day.

9. Tachyphonus *Tenuirostris*. Sp. Nov.

T. violaceo-niger; scapularibus albis; caudæ tegminibus inferioribus rufis; rostro gracili.

Glossy blue-black; scapulars white; under tail-covers rufous; bill slender.

Description.—In its general proportions this is rather smaller than the preceding; the bill, in particular, is much more slender, the culmen more arched, and the lateral margins are more than usually inflexed. The general colour of the whole bird is a deep and glossy raven black; but the scapular, and parts of the lesser wing-covers, are pure white, and the under tail-covers deep rufous. The wings and tail agree in their structure with those of the more typical species. The bill and tarsi are black; and the latter appear rather lengthened. The dispersion of Mr. Hullet's cabinet of ornithology, some years ago, by public sale, put me in possession of this, and several other little-known birds, from the interior of Buenos Ayres.

Bill $\frac{6}{10}$, wings, $2\frac{3}{4}$, tail $2\frac{3}{4}$, tarsi $\frac{9}{10}$.

There is an evident affinity between this and the two preceding species in their general *habit*, and in the disposition of their colours; but the bill, which in *T. cristatus* is thick and comparatively short, becomes more slender and attenuated in *T. Desmaresti*, and we are thus prepared for the still greater weakness of this organ in *T. tenuirostris*; nevertheless, in all these species, the typical characters are more or less preserved. It may be added that these characters of the bill can even be traced in the Tanagra velia, and this bird may probably connect *Tachyphonus* with the genuine Tanagers.

There are several other South-American birds, arranged in the old genus Tanagra, which seem to bear an affinity to this group; but as they are only known to me by the descriptions of Dr. Latham, or the figures of Buffon, I cannot venture to give them a place in the present monograph.

Art. XI. *Supplement to a Paper on the Vibrations of Heavy Bodies, in the Fifteenth Volume of this Journal. By* Davies Gilbert, Esq. M.P., Treas. R.S., &c.

Having inserted in the XVth volume of the *Quarterly Journal*, a Paper on the vibration of Heavy Bodies in cycloidal and in circular arcs, I am now desirous of adding a supplement to the part, which relates to the variation of time, occasioned by different buoyancies of the atmosphere, as the barometer rises or falls.

My attention was first directed to the subject by the late Doctor Maskelyne; and as it appears from a very simple calculation, that each inch of variation in the height of the barometer must change the daily rate of a clock connected with a brass pendulum, about two-tenths of a second, in so far as buoyancy is alone concerned; I constructed a table for each hour, and for every tenth of an inch.

Having requested Mr. Pond and Doctor Brinkley to observe the rates of their clocks, when the barometer stood at its opposite extremes, they were so good as to do so; and I learnt, with much surprise, that neither at Greenwich nor at Dublin had the expected variation of rate taken place.

It has been demonstrated by Sir Isaac Newton, that small changes in the resistance to oscillatory bodies do not by their direct action cause any variation in the times of vibration, since the ascending and descending semivibrations compensate each other; but increased resistance reduces the arc, where the maintaining power is constant; and, consequently, it reduces also the circular excess, so that an increased resistance thus indirectly accelerates the time of vibration. This circumstance has been noticed by various writers, and had not escaped my attention; but the quantity seemed so very small as to be wholly evanescent, in comparison even with the minute change of time assignable to buoyancy. On submitting it, however, to calculation, the result proved very different.

Let h express in barometrical inches the density of the atmosphere.

Then since the resistance experienced in moving through any space, is proportionate to the space itself, to the velocity, and to the density of the medium: and in the cycloid, or in small circular arches, the space and the velocities of semivibrations are proportionate to each other:

If R be put for the resistance,

and z for the arc,

$R = hz^2$.

But resistance is evidently equal to the maintaining power, consequently hz^2 is constant; and therefore

$$z^2\dot{h} = 2hz\dot{z}, \text{ or } z\dot{h} = 2h\dot{z}, \text{ and } \dot{z} = z \times \frac{\dot{h}}{2h}$$

whence it appears that in different arcs the variation of the lengths, occasioned by a given variation of the barometer, is proportionate to the arcs themselves.

Now the circular excess in any small circular arc is $\frac{1}{16}$th part of the arc squared. (See page 10 of the paper referred to.) Whence the variation of this excess; or the fluxion of the time ($\dot{t}$) corresponding it h $\dot{h}$, will be

$$\frac{1}{16}\left(\overline{1+\frac{\dot{h}}{2h}}^{\,2} \times z^2 - z^2\right) = \frac{1}{16}\, z^2 . \frac{\dot{h}}{h}$$

But the time of descent down the arc of a cycloid, or through any small circular arc, is inversely as the square root of the moving force.

Let gravity, as acting in a vacuum, be represented by unity.

Let S = the specific gravity of the pendulum,

s = the specific gravity of air, at the height (h) of the barometer.

Then will $1 - \frac{s}{S}$ represent the moving force,

and $t = \dfrac{1}{\sqrt{1 - \frac{s}{S}}}$

But $\dot{t}$, as compared with $\dot{h}$, will be

$$\dot{t} = \frac{1}{\sqrt{1 - \frac{s}{S} \times \frac{h+\dot{h}}{h}}} - \frac{1}{\sqrt{1 - \frac{s}{S}}}.$$ The first term

of which expression, being expanded, is $\frac{1}{2} \times \frac{s}{S} \times \frac{\dot{h}}{h} = \dot{t}$.

By making this equal to the variation in the time occasioned by buoyancy,

$$\frac{1}{2} \times \frac{s}{S} \times \frac{\dot{h}}{h} = \frac{1}{16} z^2 \times \frac{\dot{h}}{h} \therefore z^2 = 8\,\frac{s}{S}$$

and $z = \sqrt{8\,\frac{s}{S}}$.

Then if air be taken at the specific gravity of

$\frac{1}{828}$ its log. 7.0819697

The spec. grav. of brass 8.8, 4 its log. 0.9242793 } 9.0757207
Ar. Com. . . . . }

8 its log. . 0.9030900

2)7.0607804

8.5303902

Degrees in radius 57°.2958 the log 1.7581226

.2885128

which gives 1°.9432, or 1° 56′ 35″ for the vibration on each side of the perpendicular, or nearly two degrees. With a mercurial pendulum the arc of semivibration must be 1° 31′ 44″, or about one degree and an half.

And it is an extremely curious circumstance, that without any reference to the attainment of this balance between opposite disturbing causes, our best clocks should have been fortuitously made to vibrate very nearly in the arc, which reduces them to an equality.

Friction is supposed to be so very small in amount, and moreover to possess such very trifling influence on these minute changes as not to require attention.

All thermometrical changes are corrected by the ordinary compensation. Nor is it of any practical importance to estimate h, (the height of the barometer from whence the variation t. is to commence,) at any other standard than 30 inches, unless, indeed, the barometer should be placed in some very elevated situation.—When other tables should be constructed from the new values of t and s.

The following table, reprinted from the former communication, gives the effect of buoyancy on a brass pendulum. These numbers multiplied by six-tenths, (.6) become adapted to a mercurial pendulum; and the same numbers multiplied by the following factors, *with their signs changed*, will give the effect arising from variations of the circular excess; for degrees, and quarters of degrees of semivibration.

°	′		°	′		°	′	
1	0	. . 0.265	3	0	. . 2.38	5	0	. . 6.62
1	15	. . 0.414	3	15	. . 2.80	5	15	. . 7.30
1	30	. . 0.596	3	30	. . 3.24	5	30	. . 8.01
1	45	. . 0.811	3	45	. . 3.72	5	45	. . 8.77
2	0	. . 1.06	4	0	. . 4.24	6	0	. . 9.53
2	15	. . 1.34	4	15	. . 4.78	6	15	. . 10.34
2	30	. . 1.66	4	30	. . 5.36	6	30	. . 11.19
2	45	. . 2.00	4	45	. . 5.98	6	45	. . 12.07

A Table, reprinted from the XVth vol. for correcting the time, as shewn by a clock, having a brass weight, or ball to its pendulum, for the variation of one inch in the height of the barometer.

Arguments.—The time elapsed since the last observation of the barometer.

And the present observed height ∽ 30 inches $\pm \frac{1}{2}$ the variation between the observations—

Additive, if the sum is Plus.—Subtractive, if it is Minus.

h	$\frac{1}{10}$	$\frac{2}{10}$	$\frac{3}{10}$	$\frac{4}{10}$	$\frac{5}{10}$	$\frac{6}{10}$	$\frac{7}{10}$	$\frac{8}{10}$	$\frac{9}{10}$	s
1 . .	00	00	00	00	01	01	01	01	01	01
2 . .	00	00	01	01	01	01	01	01	01	02
3 . .	00	01	01	01	01	02	02	02	02	03
4 . .	00	01	01	01	02	02	02	03	03	03
5 . .	00	01	01	02	02	03	03	03	04	04
6 . .	01	01	02	02	03	03	04	04	05	05
7 . .	01	01	02	02	03	04	04	05	05	06
8 . .	01	01	02	03	03	04	05	05	06	07
9 . .	01	02	02	03	04	05	05	06	07	08
10 . .	01	02	03	03	04	05	06	07	08	08
11 . .	01	02	03	04	05	06	06	07	08	09
12 . .	01	02	03	04	05	06	07	08	09	10
13 . .	01	02	03	04	05	07	08	09	10	11
14 . .	01	02	04	05	06	07	08	09	10	12
15 . .	01	03	04	05	06	08	09	10	11	13
16 . .	01	03	04	05	07	08	09	11	12	13
17 . .	01	03	04	06	07	09	10	11	13	14
18 . .	02	03	05	06	08	09	11	12	14	15
19 . .	02	03	05	06	08	10	11	13	14	16
20 . .	02	03	05	07	08	10	12	13	16	17
21 . .	02	04	05	07	09	11	12	14	16	18
22 . .	02	04	06	07	09	11	13	15	17	18
23 . .	02	04	06	08	09	12	13	15	17	19
24 . .	02	04	06	08	10	12	14	16	18	20

In the case of a mercurial pendulum, these quantities must be reduced to three-fifths (.6) of their magnitudes in the table.

Art. XII. *Correction of an Error in the "Meteorological Essays."* *By* J. F. Daniell, F.R.S.

I am indebted to an anonymous critic in the Dublin *Philosophical Journal*, no less for pointing out a mistake into which I have carelessly fallen in my Meteorological Essays, than for the flattering terms in which he has spoken of the work in general. The mistake occurs at p. 178, *& seq.* in the table for finding the specific gravities of various mixtures of air and aqueous vapour. The table consists of five columns;—the first contains the degrees of temperature from 0° to 90°; the second the alterations of volume due to each degree of heat; the third the increments of volume from the addition of vapour of any given temperature; the fourth the increase of density from the weight of the same vapour; and the fifth the correct specific gravity of saturated air. In calculating the last I have deducted or added the increase of volume from *unity*, instead of calculating the specific gravity in inverse proportion to such alteration. The mistake is so very obvious, that I could not have extended the table much further without having been aware of its commission: but within its actual limits it is of much less amount than might have been at first supposed. The facility with which errors of this description slip from the pen cannot be better illustrated than by an oversight of an analogous nature of my Reviewer himself, who observes, upon the *formula* for calculating the expansion of gas from moisture, "If the volume of the dry gas = 1, and the height of the barometer and the elasticity of steam be represented respectively by p and f, when the gas is saturated, its volume will have become $= \frac{p}{p-f}$." Now if this were the case, when the height of the barometer is 30, and the elasticity of the vapour is 30 also, the volume becomes $\frac{30}{30-30}$, which is clearly impossible; the formula upon which my calculation is founded is $\frac{p+f}{p}$; which gives, upon the same supposition, $\frac{30+30}{30}$, or double the volume.

The following corrected table should be substituted for the one already alluded to.

TABLE *for finding the Specific Gravity of any Mixture of Air and Aqueous Vapour, at Mean Pressure, from* 0° *to* 90°.—*Dry Air at* 32° *Temp. and* 30 *Inches' Pressure, being* = 1.0000.

Temp.	Alteration of Volume from Heat.	Alteration of Volume from Vapour.	Increase of Density from Weight.	Correct Specific Gravity of saturated Air.	Temp.	Alteration of Volume from Heat.	Alteration of Volume from Vapour.	Increase of Density from Weight.	Correct Specific Gravity of saturated Air.
0	−.06666	+.00226	+.00153	1.0703	14	−.03749	+.00384	+.00251	1.0373
1	−.06458	+.00237	+.00159	1.0679	15	−.03541	+.00397	+.00260	1.0350
2	−.06249	+.00247	+.00166	1.0655	16	−.03333	+.00410	+.00268	1.0327
3	−.06041	+.00257	+.00172	1.0631	17	−.03124	+.00423	+.00276	1.0304
4	−.05833	+.00267	+.00179	1.0607	18	−.02916	+.00437	+.00284	1.0282
5	−.05624	+.00277	+.00185	1.0583	19	−.02708	+.00450	+.00292	1.0260
6	−.05416	+.00287	+.00191	1.0559	20	−.02500	+.00467	+.00302	1.0239
7	−.05208	+.00297	+.00197	1.0536	21	−.02291	+.00487	+.00314	1.0215
8	−.04999	+.00307	+.00204	1.0512	22	−.02083	+.00507	+.00327	1.0194
9	−.04791	+.00317	+.00210	1.0489	23	−.01874	+.00527	+.00339	1.0171
10	−.04583	+.00327	+.00216	1.0466	24	−.01666	+.00547	+.00351	1.0148
11	−.04374	+.00343	+.00224	1.0442	25	−.01458	+.00567	+.00363	1.0125
12	−.04166	+.00357	+.00234	1.0419	26	−.01249	+.00587	+.00375	1.0104
13	−.03958	+.00370	+.00243	1.0396	27	−.01041	+.00607	+.00387	1.0082

TABLE, *&c., continued.*

Temp.	Alteration of Volume from Heat.	Alteration of Volume from Vapour.	Increase of Density from Weight.	Correct Specific Gravity of saturated Air.	Temp.	Alteration of Volume from Heat.	Alteration of Volume from Vapour.	Increase of Density from Weight.	Correct Specific Gravity of saturated Air.
28	−.00833	+.00627	+.00399	1.0061	44	+.02499	+.01094	+.00674	.9720
29	−.00624	+.00647	+.00411	1.0041	45	+.02708	+.01134	+.00697	.9699
30	−.00416	+.00667	+.00423	1.0017	46	+.02916	+.01174	+.00720	.9679
31	−.00208	+.00694	+.00439	.9995	47	+.03124	+.01214	+.00743	.9658
32	.00000	+.00717	+.00454	.9973	48	+.03333	+.01254	+.00766	.9637
33	+.00208	+.00747	+.00471	.9952	49	+.03541	+.01294	+.00789	.9617
34	+.00416	+.00773	+.00486	.9927	50	+.03749	+.01334	+.00803	.9596
35	+.00624	+.00800	+.00502	.9909	51	+.03958	+.01380	+.00839	.9577
36	+.00833	+.00827	+.00518	.9887	52	+.04166	+.01426	+.00864	.9556
37	+.01041	+.00854	+.00533	.9867	53	+.04374	+.01480	+.00896	.9536
38	+.01249	+.00880	+.00549	.9846	54	+.04583	+.01534	+.00926	.9515
39	+.01458	+.00907	+.00564	.9824	55	+.04791	+.01586	+.00957	.9495
40	+.01666	+.00934	+.00580	.9804	56	+.04999	+.01640	+.00987	.9475
41	+.01874	+.03974	+.00604	.9783	57	+.05208	+.01694	+.01017	.9455
42	+.02083	+.01014	+.00627	.9761	58	+.05416	+.01754	+.01051	.9435
43	+.02291	+.01054	+.00650	.9741	59	+.05624	+.01810	+.01083	.9416

TABLE, *&c., continued.*

Temp.	Alteration of Volume from Heat.	Alteration of Volume from Vapour.	Increase of Density from Weight.	Correct Specific Gravity of saturated Air.	Temp.	Alteration of Volume from Heat.	Alteration of Volume from Vapour.	Increase of Density from Weight.	Correct Specific Gravity of saturated Air.
60	+.05833	+.01867	+.01114	.9396	76	+.09166	+.03120	+.01810	.9087
61	+.06041	+.01923	+.01146	.9376	77	+.09374	+.03220	+.01861	.9067
62	+.06249	+.01980	+.01178	.9356	78	+.09583	+.03323	+.01916	.9048
63	+.06458	+.02050	+.01217	.9337	79	+.09791	+.03427	+.01973	.9030
64	+.06666	+.02120	+.01256	.9317	80	+.09999	+.03533	+.02030	.9011
65	+.06874	+.02190	+.01295	.9298	81	+.10208	+.03643	+.02090	.8992
66	+.07083	+.02260	+.01334	.9278	82	+.10416	+.03756	+.02150	.8973
67	+.07291	+.02330	+.01372	.9259	83	+.10624	+.03873	+.02213	.8955
68	+.07499	+.02407	+.01415	.9241	84	+.10833	+.03993	+.02277	.8936
69	+.07708	+.02484	+.01457	.9221	85	+.11041	+.04116	+.02343	.8918
70	+.07916	+.02567	+.01503	.9201	86	+.11249	+.04243	+.02411	.8899
71	+.08124	+.02654	+.01551	.9182	87	+.11458	+.04373	+.02486	.8881
72	+.08333	+.02740	+.01598	.9163	88	+.11666	+.04503	+.02549	.8862
73	+.08541	+.02830	+.01648	.9143	89	+.11874	+.04633	+.02618	.8844
74	+.08749	+.02923	+.01699	.9122	90	+.12083	+.04766	+.02688	.8826
75	+.08958	+.03020	+.01752	.9105					

To find the specific gravity of any mixture of air and aqueous vapour by means of this table, we must proceed as follows:—Note the temperature and the point of condensation by the hygrometer; if they coincide, that is to say, if the air be in a state of saturation, we shall find the specific gravity required in the fifth column opposite to the proper degree of heat in the first column. If the point of condensation be below the temperature, we must look for the amount of the alteration of volume due to the heat in the second column, and for the expansion due to the vapour in the third column. Add these together, if they have like signs; or subtract one from the other if they have different signs. As the volume corrected by this quantity is to the original volume, so is the standard specific gravity to the specific gravity, as affected by the expansion or contraction. To this must be added the increase of weight due to the vapour in the fourth column, and the result will be the correct specific gravity sought.

For example:—If we wish to know the specific gravity of a mixture of air and vapour of the temperature of 60°, and of which the dew-point is 40°; we find in the second column opposite to 60° the number +.05833, and in the third column opposite to 40° we have +.00934 : the sum of which is +.06767; therefore

$$1.06767 : 1 :: 1 : 93668.$$

In the fourth column opposite to 40° we find +.00580, and

$$.93668 + .00580 = .94248;$$

which is the correct specific gravity under the assumed circumstances.

. . . .

Art. XIII. *On the Barometer. By* J. F. Daniell, F.R.S.

The following paper requires a few words of preface. It contains, as far as I am able to recollect, the substance of a communication which I had the honour to make to the Royal Society, in the month of November last, before the commencement of their last session. As, with a former paper upon the same subject, a mistake had arisen, as I was informed, from my not having expressed a desire to have it read, the consequence of which was that it was not read, I took the precaution of placing the present manuscript in the hands of the President, with a due notification that it was presented for the purpose of being read at the meeting of the Society. I went, immediately afterwards, upon the Continent; where I was detained several months. Upon my return, in June, I found that the first part of the *Philosophical Transactions* for the year had been published, and that it contained no notice of my paper: neither could I find that any communication had been sent to me of its destination. I was naturally anxious to obtain some intelligence respecting it, and addressed a note upon the subject to the President; to which he returned me the following reply:

Dear Sir, 26, *Park-street*, *June* 5.

Your paper was read to the Royal Society many months ago *, and has been before the council. There was but one opinion expressed as to the ingenuity of the method of preventing the introduction of air, supposing the *cause* that which you have assigned. The *existence* of this cause for the appearance of elastic matter in barometer tubes, was not considered, however, as proved by the experiments you have detailed, and it was therefore agreed by the council to wait for your return, in hopes that you might be able to give them some new details or elucidations from the researches which you stated were in progress.

I am, dear Sir,

Very sincerely your's,

H. Davy.

I immediately returned the following answer, being anxious to publish the paper in the ensuing Number of this Journal; but not acquiescing sufficiently in the reasons assigned by Sir H. Davy to withdraw the paper from the judgment to which I had submitted it.

* It was read, I am informed, on the 20th January, and several papers which were delivered in after its communication were read before it.

My dear Sir,

I have no new details or elucidations to communicate to the Royal Society. I shall therefore be much obliged to you to have my paper again brought before the council for their final determination; unless indeed I am to understand that their determination has been already expressed.

I have the honour to be, dear Sir,

Very truly your's,

To Sir H. Davy, Bart., J. F. Daniell.
President of the Royal Society, &c.

In consequence of this, the paper was again laid before the council, just before the recess; when, on account of a division of opinion, their determination was postponed to next year. A member of the council then asked leave to withdraw the paper, in order to allow of its publication elsewhere, and stated that I had neglected to preserve a copy of the manuscript. The President, I am told, objected to this, and laid down the law, that the paper having been once taken into consideration by the council, could not be withdrawn. To make sure that no formality was neglected, I immediately addressed the following note to the President:

My dear Sir, *Gower-street, 25th June,* 1825.

Understanding that the decision upon my paper is postponed to next year, I beg permission to withdraw it.

I remain, dear Sir,

Your's faithfully,

To Sir H. Davy, Bart., J. F. Daniell.
President of the Royal Society, &c.

To this application I never received any answer. This is a brief statement of facts, upon which I shall abstain from making any comment. Twice before I have experienced similar treatment from the council of the Royal Society, and twice before I have appealed to the scientific public with success. I shall now endeavour, from memory and such rough notes as I have preserved, to recompose the paper, with a full conviction that the facts narrated are of sufficient importance to call for immediate publication, and satisfied, by the opinion of those who are well qualified to judge, that there is sufficient proof of their existence to satisfy all impartial minds that the subject is well worthy of that further investigation which it was my purpose to propose to the Royal Society. A new law has now been promulgated by the President of the Society, and every thing henceforward published in the *Philosophical Transactions* must be considered as proved. The council will doubtless immediately drop the notice which has always hitherto been published in the preface to the volumes, that "they do not pretend to answer for the certainty of the facts or propriety of the reasonings contained in the several papers published, which must still rest upon the credit or judgment of their respective authors;" and they will, of course, be careful that this law is administered with impartiality. In such a regulation I, as an humble individual, cannot but acquiesce; but, with many others I imagine, I shall always be found to resist the curtailment of the hitherto-acknowledged right of an author to withdraw a paper, any time previous to the decision of the council upon its publication.

In the year 1823 I presented to the Royal Society a paper upon the Construction of the Barometer, the substance of which I afterwards published in my volume of Essays, the original paper having been committed to the archives of that learned body. I therein stated my reasons for differing from the high authority of the President, upon the *cause of the existence of elastic matter in barometer tubes,* suggested by him in a paper upon "the electrical phenomena exhibited in vacuo," and published in the *Philosophical Transactions* for the year 1822. Sign. Bellani also arrived at the same conclusion as myself, from a series of experiments which he undertook, expressly to determine whether the air or vapour, the last portions of which are found to remain so obstinately in barometers and thermometers, is introduced with the mercury, or is a portion of that which originally occupied the tube before the introduction of the metal. The conclusion he comes to is, that it is always a portion of that which previously adhered to the glass, and *that mercury is utterly incapable of absorbing either air or moisture.* One of his experiments is so simple, and at the same time so conclusive, that I cannot refrain from giving a short account of it. He filled a barometer tube, and boiled it very carefully, and then prepared a funnel made of a small capillary tube, which reached through the mercury in the barometer-tube, to the closed end, and was enlarged at the top. When introduced, it had been recently made, and perfectly dry. Some mercury was then prepared by agitating it in a bottle with water and air, and dried by means of filtering paper, and afterwards passing it through paper cones, three or four times, into dry vessels. A little of this mercury was poured into the funnel-tube, and the air extracted by means of a fine wire, so that the column was continuous. So much of this prepared mercury was then poured in as fully to displace the mercury which had been boiled in the tube. The barometer was found to stand exactly at the same height as before in the same circumstances; and, when the mercury was heated, none of those bubbles appeared which arose on the first boiling *.

Still further to illustrate this subject, which I thought of the highest importance, and to ascertain the difference of capillary action in boiled and unboiled barometer-tubes, I undertook the following experiments. The apparatus, which I made use of, consisted of an upright pillar of brass, standing upon a mahogany foot, upon which two horizontal arms of unequal lengths were made to slide; at the extremity of each of these a steel needle, with a fine point, was fixed perpendicularly downwards. These points could be adjusted to the same plane, or their relative distance be measured, by means of a nut and screw upon the pillar, which carried a *nonius*; and the slightest contact of these points with the clean surface of a basin of mercury was instantly perceptible. I satisfied myself, by repeated trials, that the adjustment might be depended upon to the one-thousandth part of an

* *Giornale de Fisica,* Vol. vi. p. 20, or see *Royal Institution Journal,* Vol. xv. p. 371

inch. I made a contrivance to hold a glass tube perpendicularly immersed in a basin filled with mercury; and when one of the steel points was made to touch the surface of the fluid in the tube, and the other the surface in the basin, the depression of the former was accurately measured by the *nonius.* In this manner I determined the capillary action of several tubes, varying in their diameters from one to six-tenths of an inch. The results agreed as nearly as possible with Dr. Young's table, calculated from the experiments of Mr. Cavendish. The end of the tubes, opposite to those at which the measures had been taken, were then hermetically sealed, in such a manner as to be readily reopened under mercury: they were immediately filled with mercury, and carefully boiled. I expected to be able to ascertain the differences of depression by opening them in the basin of mercury, and proceeding as before. The experiment was performed as soon after the operation of boiling, as the mercury in the tube had cooled down to the temperature of that in the basin. At first the attraction between the mercury and the glass appeared to be perfect, and no depression could be perceived. When, however, the tubes were left some time exposed, either before or after they were opened, the air and moisture insinuated themselves between the metal and the glass, and an immediate depression was the consequence. This depression increased gradually, till at length it became fixed at the exact point of that of the unboiled tube. The progress of this effect was easily perceptible with a magnifying glass, and was rendered still more visible by heating the tube, when air-bubbles were immediately detached. This is obviously the same effect as that described by Sir H. Davy in his paper before alluded to, in which he says that, "on keeping the stop-cock of one of the tubes, used in the experiment on the mercurial vacuum, open for some hours, it was found that the lower stratum of mercury had imbibed air, for when heated in *vacuo* it emitted it distinctly from a space of a quarter of an inch of the column; smaller quantities were disengaged from the next part of the column, and its production ceased at about an inch high in the tube." Now, I believe that I should not be too presumptuous if, stopping here, I were to maintain that my experiment presented absolute *proof* that the air had insinuated itself between the mercury and the tube, and shewed that there was no " reason to believe that this air existed in mercury in the same invisible state as in water, that is, distributed through its pores." For, if the latter had been the case, the mercury, which contained no air after being boiled, would, from its greater density, have sunk in the tube, when surrounded by mercury which had not been boiled, and would have risen gradually as it became saturated with air. I am justified in drawing the conclusion from the contrary effect, that the air had insinuated itself between the metal and the tube, for the capillary depression is known to be in inverse proportion to the affinity of the fluid for the containing tube, and nothing could have affected that affinity in the case before us, but the gradual interposition of a thin stratum of air and moisture.

Having thus traced and measured the progress of the air down the sides of small tubes filled with mercury, and boiled with the greatest care, I was naturally led to suspect that the same action might take place in barometers, to their gradual deterioration. I soon saw reason to conclude that such a process actually was going on in the most carefully constructed instruments, and that, in time, air would thus insinuate itself into the best Torricellian vacuum. In the paper upon the construction of the barometer, to which I have before alluded, I gave all the particulars of the making of two barometers, in which every precaution was used to dispel every particle of air. One of these was of very large dimensions, and was fixed up in the apartments of the Royal Society, under the superintendence of the Meteorological Committee. The other was of the mountain construction, and intended for my own use. The agreement between these two instruments, when all corrections were made for the differences in their sizes and forms, was very perfect, and proved that the care which had been bestowed upon them had not been thrown away. In the latter, however, I lately remarked that a small quantity of air had ascended into the *vacuum.* I could not discover any way in which this could have obtained admission; but, attributing it to accident, I laid it aside, and thought no more of it till the present experiments recalled it to my recollection. By a singular coincidence I was, about this time, informed that the barometer of the Royal Society had assumed a very remarkable appearance, and that the mercurial column, which was originally perfectly bright and compact, now seemed dull and speckled. I immediately proceeded to examine it carefully, and I at once perceived that it was copiously studded with minute air-bubbles. As far as the mercury was exposed to view, the specks could be traced decreasing in size, from the upper to the lower part. The manner in which this instrument is fixed rendered it impossible to suspect that this air could have obtained admission by any accident; for, unlike the mountain barometer, the column of metal is exposed to no oscillations but such as arise from differences of atmospheric pressure. I was myself quite satisfied, and those who have read the account of the precautions taken in filling the tube will also, I think, be satisfied, that this air was not left at its original construction. I now leave it to the candid to judge, whether "the cause which I have assigned for the appearance of this elastic matter in barometer tubes has not *already* been proved by the experiments which I have detailed."

While I was occupied with these considerations, and sufficiently vexed to find that all my care had been thrown away to prevent my adopting that opinion without very strong grounds, it occurred to me that I had, in the course of my experience, observed a phenomenon, which was calculated to throw some light upon the present question; namely, that gases were more readily preserved from mixture with atmospheric air over water than over mercury. I was unable to refer to any notes of experiments to confirm this suspicion, but I proposed the question to Mr. Faraday, who, I made no doubt, from his great accuracy and experience, must have made the observation, if it were founded in fact. Without at the time having any knowledge of the ulterior object which I had in view, he at once answered me, that mercury, he believed, would not confine gases for a long period so well as water; and he thought, that by referring to his note-book, he could furnish me with the particulars of a case in point. He accordingly did me the favour to extract the following particulars:—

In June, 1823, he made a mixture of one volume of oxygen and two volumes of hydrogen; with this he filled five *dry* bottles over mercury, and also four bottles over water. He left the glasses inverted over mercury and water, placing three mercury and two water bottles in the windows, so as to receive the sun's rays and day-light; and two mercury and two water bottles he placed in a dark place. In July, 1824, he examined the bottles; the water bottle in the light contained hydrogen and some common air, and there was no alteration of volume; the mercury bottle in light contained common air only. The water bottles in the dark place showed no alteration of volume, and the air contained in them proved to be the *original mixture;* the mercury bottles in the same situation contained *nothing but common air.*

Now, if I had not known, from the authority of the President, that the Council of the Royal Society did not think this *sufficient proof,* I should very confidently have concluded from these facts, that a fluid which has attraction enough for glass, to enable it to wet its surface, effectually prevents the passage of gases into or out of vessels of that substance; while a fluid which does not wet the surface permits their slow penetration. I should, moreover, have ventured to affirm, that the case of the confined air is exactly analogous to that of the barometer; for its escape and the admission of the atmospheric air can only be in virtue of the law discovered by Mr. Dalton, that the gases are as *vacua* to one another. The inference is also pretty strong, that the filtration takes place along the surface of the glass, and not through the pores of the fluid.

It has been attempted, I understand, to contravert this conclusion, by the observation that gases have been preserved a considerable time by mercury; but when it is considered that the slightest film of moisture, or any foulness of the mercury, will form a connexion between the metal and the glass, the objection can be of no avail, unless these circumstances have been attended to. To ensure the maximum of the effect which I have been describing, it is necessary that both glass and mercury should be in the driest and cleanest possible state; that is to say, exactly in the state in which they exist in a well made barometer.

That another attempt has been made at explanation I can scarcely credit; namely, that some facetious person had played Mr. Faraday a trick. The particulars of the case disprove the possibility of such a circumstance, unless upon the supposition that such person foresaw the present discussion. The character, however, of Mr. Faraday for precision, renders it unnecessary to say any more, than that he informs me there is not the slightest ground for such a suspicion.

I was no sooner convinced that the most carefully constructed barometers were liable to a slow and gradual deterioration in the manner which I have indicated, than I endeavoured to find a remedy to the evil; without which, it is clear, that some of the most interesting problems of meteorology must be for ever left in a state of vagueness and uncertainty. For a long time I despaired of success; but when at length I discovered an effectual method of preserving the Torricellian *vacuum*, I flattered myself that the Royal Society would so far have taken an interest in the subject as to have ordered it to be submitted to a trial, which could not have been so satisfactorily made under any other superintendence.

I soon perceived that the only possibility of effecting the object which I had in view, consisted in discovering some method of making the mercury *wet* (if I may be allowed the term) the tube in which it is contained. I was fearful, at first, that all the substances to which its attraction is sufficiently strong for this purpose, would be so much acted upon as to become disintegrated or dissolved. I, however, fortunately recollected that, in some experiments in which I was formerly engaged, platinum, immersed in boiling mercury, became completely coated by it, and afterwards retained its coating for a long time. I repeated the experiment with some platinum foil, and found it to succeed perfectly. The mercury adhered strongly to the foil, and the latter, after a long immersion, was found to have lost none of its tenacity. I availed myself of this property in the following way:—I caused a small thin piece of platinum tube to be made about the third of an inch in length, and of the diameter of the glass tube; this was carefully welded to its open end, so that the barometer tube terminated in a ring of platinum. The tube was filled and boiled as usual, and the infiltration of air was completely prevented by the adhesion of the mercury, both to the interior and exterior surface of the platinum guard. I have no doubt that a mere ring of wire welded, or even cemented upon the exterior surface of the glass, which would be a much easier and less expensive operation, would be a sufficient protection, as the slightest line of perfect contact must effectually arrest the passage of the air: but in the first attempt I was desirous that the experiment should be tried in the most perfect manner. When a piece of glass, armed either with a ring or tube, is immersed in mercury, the effect is easily perceived; instead of any depression being visible around it, the mercury may be lifted by it considerably above its proper level. Time, of course, will be requisite fully to confirm the efficacy of the guard, and I was in hopes that the Royal Society would have attributed weight enough to the observations which I submitted to them, to induce them to give orders for the construction of a large barometer, upon the principle which I have suggested, to be placed beside the two others already in their possession. An opportunity would thus have been afforded of determining, in the most unexceptionable manner, several facts of the utmost importance to meteorological science. The further experiments which the council have called for, before they would commit themselves by the publication of the foregoing opinions, are in progress, and shall be laid before the Society as soon as they are complete; but they obviously require considerable time for their progress. I have taken the pains to re-write this paper, under the conviction that advantage will be derived to science, by sooner throwing the subject open to general observation and experiment.

I expected to have been able to find some evidence of the deterioration of barometers in the numerous registers that are kept of their oscillations: but I have not discovered any which have been continued for a sufficient length of time, with the same instruments, to answer this purpose satisfactorily. Instances abound of observers having taken the pains to re-boil their barometers, from air having obtained admission, in some unknown way which has always been attributed to accident; but the fact of their gradual deterioration cannot, in this way, be established so completely as might have been supposed.

The register of the Royal Observatory at Paris has only been published since the year 1816, in the *Annales de Chimie*; a period which is not sufficient so far to neutralize the annual oscillation as to afford the means of a satisfactory comparison. Mr. Howard, however, in his work upon the climate of London, states the mean height of the Royal Society's barometer for ten years, from 1797 to 1806, to be 29.882 inches, while for the ten succeeding years it is only 29.849 inches, which gives a depression of .033 inches in that interval.

The observations of the following ten years will not, I fear, be available in the same comparison, from the carelessness with which they have been made.

The difference in the height of the old and new barometer, which have now been placed side by side, was, in the latter part of the year 1824, .07 inch, upon a mean of twenty observations; the new barometer standing upon the average so much higher than the old one. Whether this be wholly owing to deterioration, it is not possible to say; for the old barometer does not appear to have been boiled: but from the well known accuracy of Mr. Cavendish, under whose superintendence it was constructed, it is impossible not to ascribe a great portion of it to this cause. The mercury of this instrument is now thickly studded with air-bubbles of much larger size than those of the new barometer; and when I last examined it, some of them were just upon the point of making their escape.

I now feel that I ought not to allow this opportunity to pass without making a few observations upon the new meteorological register lately published in the *Philosophical Transactions*. I am aware that, in so doing, I shall run the risk of being again designated as *hostile* to the council of the Royal Society; but I am encouraged when I consider that it is only by inferior minds that the correction of errors, and the suggestion of improvements, in the pursuits of science, can ever be considered as acts of hostility.

Great expectations, it is well known, were raised when it was announced that a committee had been appointed, consisting of the leading men of science, to take into consideration the state of the meteorological instruments and register of the Royal Society. I do not hesitate to affirm that the failure of an attempt, about this time, to establish a society to promote the science of meteorology exclusively, was wholly owing to these expectations. Many persons, who would otherwise have concurred most heartily in the plan, waited to see the result of labours which they doubted not would effect the object in view; and feared that the new society might bear too much the features of opposition:—as if any thing, which had in view the promotion of science, could be considered as opposed to the Royal Society! The unusually-protracted time of the publication of the register contributed to keep alive these expectations. At length, in the beginning of the year 1825, appeared the long-looked-for journal of 1823,—but no report from the committee! not one word of preface! not a syllable about the instruments! A most disheartening similarity in the appearance of the arrangement is the first thing to strike the eye; and it is only by a close examination that it can be inferred that changes, and those very important changes, have been made.

The register is divided as before into ten columns, and the improvements commence with the first, which contains the dates of the month. To these are now prefixed the signs of the planets, to denote the days of the week! The alteration, at all events, is harmless; which, I fear, is more than can be predicated of all the others. From the second column we may collect that the times of observation have been changed to 9 A.M. and 3 P.M., except not a few instances, in which it is to be presumed that these hours did not suit the observer's convenience. Why the change was made is left to conjecture. The title of the third column announces, for the first time, that the barometer is "corrected." But how corrected? Is it the instrument itself which is corrected for any faults in its original construction? or its indications, which are corrected for adventitious circumstances? Is it corrected for capillarity? for temperature? or for any variation in the level of the mercury in its cistern? If it be a new barometer, is it of the siphon or cistern construction. If of the latter, what are the relative capacities of its tube and cistern? what the diameter of its tube? Is there any and what difference between its indications, and those of the old instrument? A comparison of the utmost moment. The only criterion by which we may conjecture that a new instrument has been substituted for the old one is, that the height is now registered to thousandths instead of hundredths of an inch. But will it be believed that the council of the Royal Society would suffer such gross negligence to appear not only under the sanction of their "order," but after all the pomp and circumstance of a committee specially appointed to superintend the necessary arrangements? The fourth and fifth columns contain the indications of the thermometers. The latter is headed "Thermometer Without." Whence, I presume, it is fair to infer that the other is, as before, "Thermometer Within." And yet this is entitled, in some places, "Six's Thermometer," and in others, "Register Thermometer." And we are told in the first note, "Six's thermometer deranged, and a horizontal register thermometer substituted for it." But then what does it register? There is but one observation in the day recorded; it cannot be the *maximum*, because in many cases the "thermometer without" is higher. It cannot surely be the *minimum*, for who would take that from an interior thermometer? However, before three months have elapsed, note the second informs us "Register thermometer deranged." Then comes an *hiatus valde deflendus* of a month, and we return once more to Six's thermometer and two observations per day—but how repaired, and where placed, we are not informed.

The sixth column is entitled "Daniell's Hygrometer," and contains, I presume, though nobody but myself is bound to conjecture this, the dew-point.

The seventh column records the "degrees of moisture," but whether calculated from the same instrument, or from any other hygrometer, is not stated. If from the former, the situation of the thermometer by which the calculation is made should have been most particularly determined.

The eighth column contains the register of the rain; and from the greater frequency with which the amount has been lately entered, we may conclude that the soot from the old chimney-cowl, under which the gauge is situated, is more frequently removed from the pipe than it used to be.

The ninth and tenth columns, recording the direction and force of the wind, bear every mark of their former accuracy; and the only remarkable fact is the very rare occurrence of any variation of the strength from the standard 1.

The eleventh and last column rings most edifying changes upon "rain," "cloudy," "fine."

The results of all this labour are summed up at the end of the journal in one short table, containing the means and extremes of the months, and the mean results of the year. From what data, or from what part of the register, the means of temperature are collected, it is very difficult to conjecture. From the note at the foot of the last page we learn that the barometer is now 100 feet, instead of 81, above the level of low water spring-tides at Somerset-House; and that the rain-gauge is still 114 feet above the same level; but by some chance or other, six inches nearer the ground than before.

The importance which attaches to such minutiæ as these, when undertaken by such a body as the Royal Society, cannot be better illustrated than by a circumstance which has lately been discovered, in determining the length of the second's pendulum; a measure upon which depend all the late parliamentary proceedings for regulating the weights and measures of the united kingdom.

The council, by whose orders the height of the barometer above the level of the tide was determined, little foresaw at the time that this simple operation could have any reference to proceedings of such importance: and yet hear what Captain Sabine says.

"The height of the pendulums in Mr. Browne's house, in London, being here described as 92.5 feet above the level of the sea, whilst in Capt. Kater's memoir in the *Philosophical Transactions*, it is stated to be 83 feet only; it is necessary to explain that Capt. Kater's estimation of the height was founded, in part, on the understanding (*on the authority of the Royal Society*) that the elevation of their barometer at Somerset-House is 81 feet above *low-water mark;* but as the latter elevation has been since corrected by Mr. Bevan, who has determined it, by levelling, to be 90.5 feet above the *mean level*, the height of the pendulums must now be considered as 92.5 feet, and is so esteemed by Captain Kater *."

The same national work is also much affected by the want of such standard instruments as it is the appropriate province of the Royal Society to provide and preserve. Is it to be tolerated that results of such national importance should be made to depend for their verification upon a comparison with a thermometer, the property of a private individual? The uncertainty in the experiments arising from such a cause may, according to Captain Sabine, amount to " not less than $\frac{4}{10}$th of a vibration per diem; being greater, as he had reason to believe, than the sum of the uncertainties due to all other causes whatever †."

Surely these considerations, urged from so many quarters, must at length excite the dormant energies of those to whom the honour of the Royal Society is committed. If it be more consistent with the dignity of that venerable body to give up the working departments of science, and to sit as judges only of the exertions of others, let them announce such intention openly, and there will then be many come forward in the field from which they retire. In most branches of science this is the course which has been already adopted; and yet they have, perhaps, enough to do as impartial dispensers of those honours for which there are so many competitors. But if they are still determined to persevere in causing observations to be made " by their order," in the only branch of natural science which now remains to them, let them at least provide that they be made with all the care and precision which the actual state of that science demands; for upon this the honour of the Society is at stake.

* Experiments for determining the figure of the Earth. By Edward Sabine, &c. p. 343. † Idem. p. 182.

Art. XIV. ASTRONOMICAL AND NAUTICAL COLLECTIONS.—No. XXIII.

i. *A Method of Computing the* Sun's Horizontal Parallax *from Observations of the Transits of Venus. By* Thomas Henderson, Esq.

The method of computing an occultation of a fixed star by the moon, explained in No. XX. of these Collections, Art. III., may be applied with advantage to solar eclipses, occultations of the planets, and transits of Venus and Mercury. In each of these phenomena, the sun or planet occulted is to be substituted for the star, in the precepts given for occultations, and in transits, the planet is to be substituted for the moon. The orbital angle, in place of being constant as in occultations of stars, will (owing to the motion of the sun or planet) undergo a small variation equal to the change in the sun or planet's angle of position, which, when great precision is requisite, must be allowed for; and in transits, the complement of the orbital angle, and the side of the right-angled triangle, mentioned in Precept III., will have the contrary signs to those prescribed for occultations, by reason of the planet's retrograde motion. The difference of the horizontal parallaxes of the two bodies is to be employed in place of the horizontal parallax of the moon; and, while the semidiameter of the moon, or occulting body, remains without augmentation, the semidiameter of the sun or planet undergoing occultation, is to be diminished by a small quantity, obtained from this formula,

$$s \sin p \sin A,$$

where s denotes the semidiameter to be diminished, p the horizontal parallax of the other body, and A the altitude of the sun or planet. The sum or difference of the semidiameters will be employed according to the particular phenomenon to be investigated.

These modifications of the rules for occultations being observed, the sun's horizontal parallax may be thus determined from observations of the transits of Venus.

If the sun's horizontal parallax be supposed known, the horizontal parallax of Venus is ascertained from the ratios of the distances of Venus and the sun from the earth. By means of these parallaxes, the observed times of ingress and egress at those places where the beginning and end of the transit have been observed, and other astronomical *data*, the nearest distance of Venus from the sun, as seen from the earth's centre, is to be computed in the same manner as the moon's latitude is determined from observations of an occultation. See La Lande's *Astronomy*, third edition, Arts. 1970-1976. In this calculation the orbital and perpendicular parallaxes are to be adopted, instead of the parallaxes in longitude and latitude employed by La Lande, and the motions are to be referred to Venus's relative orbit in place of the ecliptic. If the assumed parallax, the observations, and other *data*, be correct, the nearest distances, deduced from the observations at the respective places, ought to be equal. But if they turn out to be different, that value of the sun's parallax should be preferred which gives for the nearest distance quantities agreeing best with each other. This is to be determined by repeating the calculation upon a second hypothesis of the sun's parallax, observing that all the parallaxes will undergo a proportional variation.

For an illustration of this method, the transit of 1769 is assumed. The times of observation at the different places, and the other *data*, are taken from De Lambre's Astronomy.

At Otaheite, the total ingress was observed at $21^h\ 43^m\ 55^s$, and the beginning of egress at $3^h\ 14^m\ 3^s$, apparent time. The sun's horizontal parallax at his mean distance from the earth being assumed $8''\cdot7$, Venus's nearest distance is found to be $606''\cdot122$; but the sun's parallax being assumed $8''\cdot5$, the same distance is found to be $606''\cdot728$. Hence an increase of one second upon the sun's mean horizontal parallax produces a diminution of $3''\cdot030$ upon the nearest distance, deduced from these observations. If D denote the nearest distance, P the number of seconds by which the sun's mean horizontal parallax exceeds $8''\cdot7$, we have the following equation,

$$D = 606''\cdot122 - 3\cdot030\ P.$$

Making similar calculations for all the places, where the beginning and end of the transit were observed, the following equations are obtained :—

Otaheite	$D = 606''\cdot122 - 3\cdot030\ P$
California . . .	$D = 606\ \cdot430 - 1\cdot180\ P$
Hudson's Bay .	$D = 606\ \cdot560 + 0\cdot915\ P$
Wardhus	$D = 605\ \cdot150 + 2\cdot985\ P$
Kola	$D = 606\ \cdot109 + 3\cdot035\ P$

Resolving these equations by the method of *minimum* squares, to obtain the most probable values of D and P, D will be found to be $606''\cdot128$, and $P + 0''\cdot0988$, making the sun's mean horizontal parallax $8''\cdot7988$, or $8''\cdot8$.

The other results, usually deduced from transits, such as the times of nearest approach and ecliptical conjunction, the difference of longitude of the various places, Venus's latitude at the conjunction, the distance from the node, and the duration of the transit, independent of parallax, may now be determined in a manner which it is unnecessary here to explain.

On comparing this method with those of La Lande, Maskelyne, De Lambre, and Biot, it appears that in the former the final result is not (as in the latter) deduced from quantities, such as Venus's nearest distance, chord described during the transit, latitude, and elongation, which cannot be known with accuracy, until the parallax be determined.

ii *Remarks on the Discordances observed between the* LUNAR OBSERVATIONS AT GREENWICH *and* PARIS. *By* THOMAS HENDERSON, Esq.

Annexed is a state of the discordances between the solar and lunar observations at Greenwich and Paris, in the years 1800-9, mentioned in *Astronomical and Nautical Collections*, No. XXI. Art. II.

SOLAR OBSERVATIONS.

Periods.	Number of corresponding observations of the Sun in A.R.	Number of Stars compared at each observatory.	Mean difference of Sun's A.R. in space.
I. From August 1800 to September 1803, when the transit instrument at Paris was changed.			″
From 29 Aug. 1800 to 19 Dec. 1801	50	117	+4·6
From 12 Jan. 1802 to 8 Aug. 1803	94	245	+3·0
II. From Sept. 1803 to Dec. 1809.			
From 4 Sept. 1803 to 21 Dec. 1804	62	154	+4·0
From 17 Feb. 1805 to 16 Dec. 1805	48	130	+0·4
From 5 Mar. 1806 to 15 Dec. 1806	42	79	+0·3
From 1 Jan. 1807 to 10 Dec. 1807	59	136	−1·0
From 6 Jan. 1808 to 6 Nov. 1808	51	128	0·0
From 11 Jan. 1809 to 21 Dec. 1809	42	100	−0·1
Total	448	1089	
Mean Results.			
From 29 Aug. 1800 to 21 Dec. 1804	206	516	+3·7
From 17 Feb. 1805 to 21 Dec. 1809	242	573	−0·1

LUNAR OBSERVATIONS.

Periods.	Number of corresponding observations of the Moon in A.R.	Number of Stars compared at each Observatory.	Mean difference of A.R. of Moon's first limb in space.	Mean difference of A.R. of Moon's second limb in space.	Mean difference of A.R. of Moon's centre in space.	Difference in measurements of Moon's diameter by the respective instruments.
(1.)	(2.)	(3.)	(4.)	(5.)	(6.)	(7.)
I. From Aug. 1800 to Sept. 1803.			″	″	″	″
From 29 Aug. 1800 to 31 Dec. 1801	66	180	+5·9	+1·8	+3·8	4·1
From 11 Jan. 1802 to 4 Aug. 1803	93	233	+4·6	+1.0	+2·8	3·6
II. From Sept. 1803 to Dec. 1809.						
From 6 Sept. 1803 to 15 Dec. 1804	40	111	+3·2	−2·1	+0·5	5·3
From 16 Feb. 1805 to 5 Dec. 1805	38	129	+5·6	−2·8	+1·4	8·4
From 2 Jan. 1806 to 21 Dec. 1806	36	82	+4·0	−1·7	+1·1	5·7
From 20 Jan. 1807 to 14 Dec. 1807	49	129	+6·9	−1·9	+2·5	8·8
From 14 Jan. 1808 to 2 Dec. 1808	41	97	+5·3	+0·5	+2·9	4·8
From 19 Feb. 1809 to 21 Dec. 1809	32	91	+3·9	+0·6	+2·2	3·3
Total	395	1052				
Mean Results.						
From 29 Aug. 1800 to 4 Aug. 1803	159	413	+5·1	+1·3	+3·2	3·8
From 6 Sept. 1803 to 21 Dec. 1809	236	639	+4·8	−1·1	+1·8	5·9

Note.—The numbers in column (6) are the means of those in columns (4) and (5), and those in column (7) are their differences, and show the quantities, by which the Transit Instrument at Greenwich appears to make the Moon's diameter less than the Transit Instruments at Paris.

The sign + denotes that the Observations at Greenwich show the Sun or Moon to be further advanced in Right Ascension than the Observations at Paris, and the sign — the contrary.

In these calculations the difference of meridians is assumed 9′ 20″·5, which has been deduced from the corresponding occultations of fixed stars by the moon, observed at both places, during the period in question.

On comparing the catalogues of fixed stars by different astronomers, similar discordances are found to subsist between the places of several stars relatively to those of the others by the respective catalogues. For instance, the relative position of Castor, in right ascension, differs 0″·31 of time, or 4″·6 of space, in Mr. Pond's and M. Bessel's catalogues. *Nautical Almanac* for 1822, p. 179. It would thus appear that the discordances alluded to are not peculiar to observations of the sun and moon, but that they are common to all the celestial bodies, and are occasioned by the unavoidable imperfections in the present state of the art of observing. It may be remarked that, while these imperfections continue, the difference of longitude determined from observations of the moon's meridian right ascension cannot be so accurate as that deduced from corresponding observations of occultations. Thus, while the difference of longitude between Greenwich and Paris, by eight occultations is 9′ 20″·5, the above series of 395 observations of right ascension makes the same difference of longitude 9′ 24″·8.

iii. *The Latitude of* GREENWICH, *as computed by Professor* BESSEL.

In the 73rd Number of Schumacher's *Nachrichten*, we find a paper of Professor Bessel on the results of the Greenwich observations for 1822, comprehending a number of altitudes obtained by reflection as well as by direct vision. The latitude, from the mean of a great multitude of observations of different stars, becomes $51° 28' 38''·343 + b'$, b' being a quantity so small that it may be neglected, though it has not been precisely determined. The mean error of each single observation is $\pm$ 0″·799, and is no greater for the observations made by reflection than for the others; a circumstance which proves the extreme care that must have been taken to avoid the effects of agitation.

The declinations of the principal stars deduced from this volume, employing Bessel's refractions, agree rather better with the Konigsberg catalogue, than with Mr. Pond's own standard catalogue.

On the other hand, it is remarkable that the determination of the latitude of Greenwich agrees much better with Mr. Pond's former computation, than with that which Professor Bessel had deduced from Bradley's observation. The former was 51° 28′ 37″·95, the latter 51° 28′ 39″·60; the differences being − 0″·393 and + 1″·257.

In speaking of the *longitudes* of places from Greenwich, as expressed in time, it was cursorily observed in the last number of these collections, that the time intended is, strictly speaking, sidereal. This expression was employed as leading to a result which is unexceptionable, although Mr. J. I. has very properly remarked that it is liable to misconstruction, and that *in questions relating to solar time* the difference in longitude may, without error, be considered as reckoned in solar time, and *not* in sidereal.

iv. Bessel's *latest Fundamental Catalogue of* Stars, *deduced from his Observations in the last five years. Schum. Nachr.* No. 78.

	R.A. 1825.			Yearly change 1825.	Secular alteration.
	H	M	S	S	S
γ Pegasi	0	4	14·059	3·0790	+ 0·0097
α Arietis	1	57	19·619	3·3566	+ 0·0201
α Ceti	2	53	8·413	3·1232	+ 0·0097
α Tauri	4	25	53·265	3·4298	+ 0·0109
α Aurigæ	5	3	46·523	4·4139	+ 0·0184
β Orionis	5	6	7·831	2·8785	+ 0·0043
β Tauri	5	15	14·128	3·7857	+ 0·0089
α Orionis	5	45	41·963	3·2453	+ 0·0031
α Canis major.	6	37	26·048	2·6441	+ 0·0004
α Geminor. med.	7	23	24·848	3·8430	− 0·0125
α Canis minor	7	30	8·172	3·1471	− 0·0044
β Geminorum	7	34	35·645	3·6851	− 0·0122
α Hydræ	9	18	59·154	2·9474	− 0·0015
α Leonis	9	59	2·552	3·2050	− 0·0102

	R.A. 1825. H	M	S	Yearly change 1825. S	Secular alteration.
β Leonis	11	40	7·577	3·0667	− 0·0077
β Virginis	11	41	34·743	3·1244	− 0·0006
α Virginis	13	15	59·118	3·1459	+ 0·0112
α Boötis	14	7	40·911	2·7323	+ 0·0012
1 α Libræ	14	41	1·368	3·3000	+ 0·0156
2 α Libræ	14	41	12·738	3·3020	+ 0·0155
α Coronæ	15	27	16·823	2·5364	+ 0·0024
α Serpentis	15	35	39·245	2·9491	+ 0·0063
α Scorpii	16	18	41·517	3·6616	+ 0·0157
α Herculis	17	6	40·286	2·7306	+ 0·0037
α Ophiuchi	17	26	48·818	2·7771	+ 0·0035
α Lyræ	18	31	0·831	2·0300	+ 0·0016
γ Aquilæ	19	37	56·371	2·8549	− 0·0008
α Aquilæ	19	42	14·637	2·9286	− 0·0014
β Aquilæ	19	46	43·025	2·9501	− 0·0015
1 α Capricorni . .	20	7	56·495	3·3331	− 0·0081
2 α Capricorni . .	20	8	20·327	3·3375	− 0·0081
α Cygni	20	35	28·088	2·0413	+ 0·0024
α Aquarii	21	56	47·581	3·0838	− 0·0043
α Piscis Austr. . .	22	47	57·812	3·3402	− 0·0217
α Pegasi	22	56	2·973	2·9812	+ 0·0053
α Andromedæ . .	23	59	21·515	3·0777	+ 0·0177

Notwithstanding this extreme accuracy in taking the mean of observations to a single *ten-thousandth* of a second in time, it *is rumoured* that between Professor Bessel and another experienced astronomer, there is a "*constant difference* in observing the right ascension," amounting to "nearly a whole second of time," as was "ascertained for some months during his stay" at that astronomer's house: and the preceding remarks of Mr. Henderson sufficiently confirm the credibility of such a rumour.

v. *Abstract of Captain* Sabine's *Experiments to determine the figure of the Earth. Printed at the expense of the Board of Longitude, 4to, London,* 1825.

"In the year 1816, an address to the Crown was moved in the

House of Commons by Mr. Davies Gilbert, praying that his Majesty would be graciously pleased to give directions for ascertaining the length of the pendulum vibrating seconds of time in the latitude of London, as compared with the standard measure in the possession of the House of Commons; and *for determining the variations in length of the said pendulum at the principal stations of the trigonometrical survey extended throughout Great Britain.* His Majesty's Ministers having requested the assistance of the Royal Society in carrying into effect the objects of the address, their accomplishment was undertaken by one of the most distinguished members of that society, and completed in 1819; and the account was published by Captain Kater, in the *Philosophical Transactions* for that year." P. xi. xii.

"The discrepancies, however, of the results obtained, by combining the lengths of the pendulums observed at the different stations in Great Britain and in France, were so great and so irregular, as to prevent any independent conclusion whatsoever, relative to the general figure of the earth, being drawn from the experiments, either of the French philosophers or Captain Kater." P. xiii.

"Such was the state of the inquiry when the present experiments were undertaken; their design was, to give the method of experiment the advantage of being tried under the circumstances most favourable for the production of a conclusive result; to extend the suite of stations previously confined to Great Britain and France, to the equator on the one side, and to the highest accessible latitudes of the northern hemisphere on the other; to multiply the stations at both extremities of the meridian, so that by their general combinations the irregular influences of local density might mutually destroy each other, and the variations of gravity due to the ellipticity alone be eliminated; and to ensure the uniformity of procedure and strict comparability of the results at all the stations, by the unity of the observer, and the identity of the instruments."

The experiments were made with a detached pendulum supported by a knife edge, of which the oscillations were compared with those of the pendulum and an astronomical clock nearly in the manner employed by Captain Kater, but with some little improvement in the mode of determining the instants of perfect coincidence. They were repeated in most cases with two different pendulums of the same form, and the greatest care was taken to preserve the identity of the circumstances in all the experiments; it was even found that the changing the pieces of agate, which supported the knife-edges, affected one of the two pendulums a little differently from the other; and allowance was made for this diversity when required. Perhaps, however, all such irregularities might have been more easily avoided by using a blunt point on each side rather than a knife-edge; the knife-edge was required in Captain Kater's experiments on the length of the pendulum, for the purpose of obtaining a precise line from which the length could be measured; but there was no such necessity where the comparative frequency only of the vibrations was concerned.

With the most laudable zeal to omit no confirmation that could be obtained for the accuracy of his results, Captain Sabine repeated his experiments at all the stations, in a manner totally different, with pendulums forming parts of actual clocks, but resting like the others on knife-edges, instead of being suspended by springs. The results agreed with those of the detached pendulum as nearly as could possibly be wished, the irregularity never amounting to more than two vibrations in a day for any one station; and the mean inference, with respect to the figure of the earth, being almost exactly the same for both series.

The grand ultimate result of all the operations at the thirteen stations, deduced by the modern method of computing the probability of least error, as well as from the most natural mode of grouping the observations, affords us an ellipticity equal to $\frac{1}{288\cdot4}$ of the earth's axis.

Taking it for granted that this is the true measure of the earth's compression, we may proceed to inquire what are the inferences from the determination, respecting the central and superficial density of the earth. It will then appear from Dr. Young's calculation of the effects of compression, inserted in this Journal, *Vol. IX.*

p. 33, that supposing the mean density to be, according to the experiments of Maskelyne and Cavendish, about 5.4; the mean superficial density must be 3·08, which is rather greater than could have been previously conjectured; the modulus of elasticity 10,800,000 feet, a little exceeding that of glass or of iron. This excess, however is not sufficient to afford any very decided argument in favour of the existence of a central fire, since it is very probable that the modulus increases when the compression becomes extreme, even without any elevation of temperature. The central density, from the same little table, is found 13·95, or somewhat greater than that of quicksilver.

The length of the pendulum vibrating seconds at the various stations of Captain Sabine's experiments is found in page 333, reduced to the level of the sea, upon a probable supposition of the general density of the superficial parts of the earth, as suggested by Dr. Young, which would only require a very slight modification from the present determination of $\frac{308}{540}$, as the comparative superficial density.

Stations.	Latitude. °	′	″		Length in a vacuum reduced to the level of the sea.
St. Thomas	0	24	41	N.	39·02074
Maranham	2	31	43	S.	39·01214
Ascension	7	55	48	S.	39·02410
Sierra Leone	8	29	28	N.	39·01997
Trinidad	10	38	56	N.	39·01884
Bahia	12	59	21	S.	39·02425
Jamaica	17	56	7	N.	39·03510
New York	40	42	43	N.	39·10168
London	51	31	8	N.	39·13929
Drontheim	63	25	54	N.	39·17456
Hammerfest	70	40	5	N.	39·19519
Greenland	74	32	19	N.	39·20335
Spitzbergen	79	49	58	N.	39·21469

The results of different combinations of these experiments with others, page 352, are expressed in this table :

	Ellipticity.
From Captain Sabine's 13 stations	1 : 288·4
From these 13 and 8 stations of the French . .	1 : 288·7
From these 13 and 7 British stations	1 : 289·5
From the mean of 5 stations near the equator and 6 in Britain	1 : 288·3
From the mean of 5 equatorial and the most northerly 5	1 : 288·4
From the 6 British and the 5 northerly	1 : 288·5
From the general combination of 25 stations	1 : 289·1
Mean	1 : 288·7

" The attempt," says Captain Sabine, p. 352, " to determine the figure of the earth by the variations of gravity at its surface, has thus been carried into full execution on an arc of the meridian of the greatest accessible extent; and the results which it has produced are seen to be consistent with each other, in combinations too varied to admit a probability of the correspondence being accidental. The ellipticity to which they conform differs more considerably than could have been expected, from $\frac{1}{306 \cdot 75}$, which had been previously received on the authority of the most eminent geometrician of the age, as the concurrent indication of the measurements of terrestrial degrees, of pendulum experiments, and of the lunar irregularities dependent on the oblateness of the earth. In further attestation of the irreconcilability of the variation of gravity now manifested, with the ellipticity inferred in the memoir in which the Marquis de Laplace has discussed the results of previous observation and experiment, it may be noticed that if *each* of the tropical stations which I have visited be severally combined with *each* of the stations within 45° of the pole, *no one result*, amidst all the irregularities of local attraction, will be found to indicate so small a compression as that of previous reception."

Taking 39·1391 for the mean length of the pendulum in London, latitude 51° 31′ 8″·4, Captain Sabine observes, that it might probably vary almost the two-hundredth of an inch above or below this length in different points of the same parallel of latitude, and on the same level of the sea; and that at the equator its mean length must be about 39·01 inches.

"The comparison of different methods of ascertaining the length of the pendulum is highly important," says Captain Sabine, page 371; "and by consequence the invention of new modes of procedure. It is understood that a *third method* has been proposed by Dr. Young, by means of a weight sliding on a rod, or bar, with a single axis of suspension, as a yet more convenient method of obtaining a correct standard, than the processes of Borda and Kater. It would be highly interesting to ascertain, by competent trial, the relative values of the three methods, and to examine the correspondence of their results; or rather to work at them until they should correspond, or until the reason of a difference should be apparent."

Without denying the justice of this conclusion, it may be remarked, that it is hardly fair either to Captain Kater, or to its inventor, to call Dr. Young's "a third method;" because it was suggested to the Committee of the Royal Society, and approved by them, *before* the date of Captain Kater's very ingenious contrivance, and therefore before the demonstration of Laplace, which showed that perfect sharpness was not necessary to perfect accuracy in the result of the convertible pendulum, and before that of Dr. Young, which proved that the effect of a temporary compression of the sharp edge would be inconsiderable, neither of which indispensable circumstances were foreseen by any person at the time that Dr. Young thought his more complicated arrangement necessary; any more than it could be foreseen with what admirable delicacy of experiment, or with what persevering industry, Captain Kater would overcome the difficulty of making his measurement from one of two opposite sharp edges to another, instead of the much more convenient process of reading off by a micrometer the distances of the fine lines only, which were to be drawn on the rod of Dr. Young's pendulum.

It is indeed doubtful whether any future experimentor would be likely to obtain a result so nearly agreeing with Captain Kater's, by his own method, as by that of the sliding weight, in which so little is left to the discretion or management of the observer. The apparatus has long been open to the public inspection in the Royal Observatory at Greenwich. The reason that no results have been obtained from it, is the failure of the clock-work by which it was to be kept in motion, and which was intended to act by a single recoiling stroke at the lowest point of the vibration, but which was not so executed as to comply with that necessary condition. But this difficulty might readily be overcome by any person who would undertake to make the experiments by means of coincidences, it being only necessary for this purpose to be provided with a clock with four different pendulums of different lengths, capable of having their coincidences observed in the different places of the weight to be employed. The mode of computation is shown in the *Philosophical Transactions* for 1818.

Captain Sabine doubts altogether of the perceptible effect of a ship's magnetism on the rate of any good chronometer, and those of Messrs. Parkinson and Frodsham he found altogether exempt from it. Page 392.

Captain Sabine has also given us the results of a multitude of experiments for determining the variation in the intensity of terrestrial magnetism, page 460. He seems to have ascertained that the agreement between the intensity and the magnetic latitude, as computed according to the approximation derived from theory, is more regular than the connexion between the dip and the intensity; but this is far from being an exception to the validity of the theory in the form first published in this Journal, as might be inferred from the manner in which Captain Sabine has stated it; on the contrary, it is more easily conceivable that local causes of disturbance should affect the direction than the magnitude of the magnetic force in any spot, as is obvious from the laws of the composition of forces; for the hypotenuse of a triangle, of which the legs are very unequal, differs very little in length from its longer side, though its direction may be considerably different.

The situation of the magnetic pole Captain Sabine finds it most convenient to place in latitude 60° N., longitude 80°, or rather 78° W. He then finds the magnetic force, as computed by Dr.

Young's theorem, and as obtained from his own very accurate experimental determination, both by the horizontal and the dipping needle.

	Exper.	Comput.
At St. Thomas	1·045	1·005
Ascension	1·02	1·01
Bahia	1·02	1·04
Sierra Leone	1·19	1·15
Maranham	1·16	1·18
Gambia	1·28	1·24
Port Praya	1·33	1·31
Teneriffe	1·49	1·45
Trinidad	1·39	1 47
Madeira	1·55	1·52
London	1·62	1·62
Jamaica	1·62	1·63
Cayman	1·63	1·65
Drontheim	1·64	1·67
Hammerfest	1·69	1·68
Havannah	1·72	1·71
Spitzbergen	1·78	1·78
Greenland	1·75	1·85
New York	1·99	1·91
Shetland	1·70	1·70
Davis Strait (*a*)	1·94	1·95
Hare Island	1·94	1·95
Davis Strait (*b*)	1·98	1·97
Baffin's Sea (*a*)	1·90	1·94
Baffin's Sea (*b*)	1·92	1·94
Baffin's Sea (*c*)	1·98	1·94
Baffin's Sea (*d*)	1·99	1·94
Davis Strait (*c*)	1·98	1·98
Possession Bay	1.95	1·96
Regent's Inlet	1·96	1·96
Byam Martin's Island	1·93	1·93
Melville Island	1·92	1·92
Winter Harbour	1·90	1·92

The greatest variation of the experiment from the computation is less than a seventeenth of the whole; an agreement perfectly unexpected in an approximation exposed to so great irregularity. Dr.

Young, in 1807, had made the magnetic pole 15° more northerly, and 8° more easterly; Biot 19° more northerly, and 50° more easterly!

In the experiments on the diurnal variation both at Hammerfest and at Spitzbergen, the needle seems to have passed its mean position about half an hour before noon and midnight.

The dip sector, employed for observing the depression of the horizon in the neighbourhood of the gulf stream, was found to afford very correct results, but less irregular than might have been expected from the actual diversities of temperature of the sea and air concerned in the refraction; the error of the tabular dip never amounting to two minutes. It was ascertained in Jamaica, by some delicate thermometrical experiments, that the heat communicated by the sun's rays is very sensibly greater in the upper regions of the atmosphere, than on the level of the sea. A number of important geographical and hydrographical notices, especially relating to the currents in the Atlantic, are contained in this volume, together with appropriate charts.

It is impossible to quit the subject of Captain Sabine's experimental labours without giving the strongest testimony of applause to his zeal and diligence and accuracy, and expressing a hope that he may find both private and public motives for continuing his exertions with equal ardour in the prosecution of further investigations connected with the advancement of physical science.

vi. *Extract from a Letter addressed by Professor* Bessel *to Professor* Schumacher, *relating to the* Greenwich *Observations.*

When I had the pleasure of being your guest at Altona, you showed me the numbers of the *Philosophical Magazine*, which contain a very severe censure of the Greenwich Observations for 1821. I saw this censure with some surprise, because I had always considered the collection of observations at Greenwich as singularly valuable, and as a rich source of astronomical truths; nor were you, I believe, of a different opinion; and we were perfectly agreed respecting the unimportance of the inaccuracies that were imputed to this work in the two papers published in the 64th volume of the *Philosophical Magazine*.

For those who are acquainted with the Greenwich observations, and who compare them with the critic's remarks, every further explanation would be superfluous; but since it may be supposed that these remarks will fall into the hands of many persons not deeply versed in astronomy, I readily comply with the request which you made, that I would commit to writing our common view of the subject. I feel, as well as yourself, the propriety of doing my best on the occasion, in order that too great importance may not be attached to this censure of an establishment, to which astronomy is indebted for a great proportion of its advancement; and that its importance cannot be very great, is sufficiently shown by the facility with which Mr. Olufsen has computed the declinations of the fundamental stars, as published in the *Nachrichten*, No. 73, from the Greenwich observations for 1822.

The greater number of the errors which have been pointed out by the censor, are merely accidental errors of the pen. Errors of this kind are certainly disagreeable, and it would be better if they could be entirely avoided; but since all collections of observations in existence do contain such errors, they clearly appear to be unavoidable.

The *first* class of errors mentioned in the *Philosophical Magazine* contains the cases in which the mean deduced from the readings of the two microscopes A and B differs from the column in which that mean is assigned. Since there must be some manifest oversight in all these cases, it may sometimes be difficult to determine whether it is in the readings or in the mean assigned; but it will, in general, be easy to distinguish, from the preceding or following observations of the same star, where the error lies.

The *second* class contains the differences between different records of the same observation. These must be errors in the copies sent to the press, and not in the readings of the microscopes; and they may generally be corrected by a comparison of the two passages: they sometimes extend to whole degrees, or to the tens of the minutes, and are then of no importance; for example, in the observations of Procyon the 23d Feb. 1821, and of β Cephei the 8th Dec. where there are errors of 30° and 5° respectively.

The *sixth* class of errors contains the intervals between the micrometer wires, as they are deduced from different observations of the same star. These are often dependent on errors of the pen, as in the observation of Capella on the 7th February, and in that of Sirius on the 8th, where there are errors of 5″ and of 40″ respectively in the fourth wire; frequently also they arise from inaccuracies of observation. In the former case they are of no consequence whatever, being easily detected at first sight; in the latter they are fundamental imperfections; but such imperfections are inseparable from the nature of observations, and it would be ridiculous to expect from an astronomer that he should perform impossibilities. *All* registers of observations exhibit inaccuracies of this kind, and if any should be produced without them, it might with confidence be asserted to be a forgery. The diligence of the astronomer is proved, not by the perfect agreement in his tenths of seconds, but by the magnitude of his mean or his probable error; and it would probably be difficult for the critic to prove that this error is much greater in the Greenwich observations, than the nature of the instruments renders unavoidable.

The errors of the *fifth* class, which comprehends the differences between the polar distances observed with two and with six microscopes, seem to me to have been introduced without the least propriety: they are either insignificant errors of the pen, as in the case of γ Draconis, 28th March, or slight accidental errors of observation, mixed with the changes of place of the stars and of the refraction, or, lastly, changes of the place of the pole on the instrument. For this last the observer can by no means be responsible. Had the critic pointed out any new method of fixing the instrument so that it should be subject to no alterations, he would have deserved the thanks of all practical astronomers; but the constant result of past experience shows that the greatest possible care, in procuring a firm foundation for the pillars, affords us only a comparative and not an absolute stability. The fixing of the instruments at Greenwich has been such as to keep them for a long time admirably firm; but at other times it has not been so successful, as may be seen in the table of the place of the pole, printed in the

Nachrichten, No. 73; the differences between the latter days of July and the beginning of August, 1821, depending on a change of this kind, so that they cannot be considered as accidental errors of observation, nor are they of material importance, as they may be readily determined by a series of observations of the pole star, so complete as those which are made at Greenwich. The accidental irregularities of the polar distances, which remain after the correction of the place of the pole, can be as little considered as an imputation on the accuracy of the observer, as those of the intervals of the micrometer wires. The truth of this remark is illustrated in the *Nachrichten*, No. 73.

The *fourth* class contains the differences between the times of transits observed with the transit telescope, and the mural circle. The latter instrument, however, not being intended for the observation of transits, nor being ever actually so employed, it would have been of no manner of use to seek for greater accuracy in the memorandums which are made merely with a view of determining its place with respect to the meridian. We ought to acknowledge the occasional insertion of these memorandums with gratitude, as they assure us that the instrument never deviates so much from the meridian as to affect the polar distances; but they are not intended for any other purpose. Neither Bradley nor Maskelyne have ever noted the times of the transits by their mural quadrant, although it was more liable to variation than the mural circle. But to correct the place of the axis of this circle continually, so as to bring it perfectly into the plane of the meridian, would certainly be of no advantage to the Greenwich observations.

Other errors which are criticized, for example, those of the names of the stars, of the hour or minute of their transits, and so forth, are of no material importance whatever; and how difficult it is to avoid errors of this kind, may be inferred from the circumstance of my having found about 1400 such errors in Bradley's observations. [The catalogue of these errors is already printed at the expense of the Board of Longitude, and is to be annexed to the publication of Mayer's original observations, which is nearly completed.]

The remark, that the observations at Greenwich are commonly concluded at midnight, would be of some weight, if it could be proved that any thing essential is omitted by this practice, which does not appear to me to be the case. The observations relate chiefly to the sun, the fundamental stars, the moon, and the oppositions of the planets; and it may easily be discovered that these different series are exhibited with an uncommon degree of perfection. Had the censor in the *Philosophical Magazine* pointed out any other series of observations which could have been combined with these, so as not to interfere with them, no doubt the Astronomer Royal would have been much obliged to him. Every thing cannot be done at once in an observatory; and if as much is effected as can be wished in one respect, something must be omitted in others. But to multiply observations, without any plan or object whatever, would be mere idleness. *Whoever is dissatisfied with the actual riches of the Greenwich observations, would do well to make the attempt to excel them; he would convince himself by such an experiment that the labour and patience required for doing so much, are fully sufficient to exhaust the powers of any one man.*

The *third* class of errors, relating to the meteorological instruments, I have not yet mentioned, because I think myself that greater accuracy is required in this department than it has hitherto been usual to observe. And if I should be allowed to suggest any improvement that could be made in the observations at Greenwich, it would be a more correct account of the meteorological instruments, and of the place in which the exterior thermometer is fixed. [It may, indeed, be expected with confidence that Professor Bessel's desire to possess a barometer and a thermometer, correctly compared with those which are employed at Greenwich, will not long be allowed to remain ungratified, though it would be a subject of much surprise on this side of the Channel, if he should detect in them such discordances as he is inclined to suspect.]

[This letter has probably appeared in Professor Schumacher's *Nachrichten*, though the 84th number of that interesting collection, for which it was intended, has not yet reached this country.]

Art. XIV. ANALYSIS OF SCIENTIFIC BOOKS.

I. *An Attempt to establish the First Principles of Chemistry by Experiment.* By Thomas Thomson, M.D. *Regius Professor of Chemistry in the University of Glasgow*, F.R.S., Lond. and Edin., &c. &c. *In Two Volumes.* London, 1825.

The well-known author of this work regards the soul and body of chemistry to consist in a knowledge of the relative weights of the combining substances. This is to form a very narrow conception of the science. The true function of the chemical teacher is announced in the following verse of the Roman poet:

> In nova fert animus mutatas dicere formas
> Corpora.

It is the characteristic of chemical genius to reveal new elementary bodies, to form new compounds of the elements known before, to discover new qualities and relations both among simple and complex substances, and to arrange the manifold and marvellous phenomena of corpuscular action, under a few general laws. The philosopher of ardent and inventive mind, content to know the general proportions, is unwilling to stop his career of discovery in order to learn the minute fractional quantities; nor will he suffer his whole faculties to flutter round the oscillations of a balance. Let none, however, hence imagine, that we desire to disparage quantitative research; we would only assign it a place of due subordination below the qualitative, conversant with new powers and forms of matter. To view, with Dr. Thomson, the first principles of chemistry as consisting in an enumeration of weights and measures, is to narrow and debase the science into an affair of addition and subtraction. This arithmetical process is, no doubt, a valuable accessory; but can never compete either in interest or utility with the knowledge of the chemical affinities, from whose play, the countless diversities of composition and unceasing successions of form in the material system, are derived. These are the grand principles of chemical action, an acquaintance with which must necessarily take precedence of the study of quantity.

It is deeply to be lamented that the latter kind of inquiry, which, as exhibited in the work before us, can hardly be deemed an intellectual operation, should have usurped, to too great a degree, in some recent publications, the place of researches into the *powers* that modify matter. Admirable specimens of this sublime study are to be found in the statics of M. Berthollet, the Bakerian lectures and "Elements" of Sir H. Davy, and in many memoirs of M. Gay Lussac and Dr. Wollaston; and the individual, who should collect and arrange these into a compendious volume, would do no mean service to science. Of such philosophical principles we can perceive no trace in Dr. Thomson's book; though the modern multiplication of chemical objects, simple and compound, loudly demands their general connexions and dependencies to be developed.

"An Attempt to establish the First Principles of Chemistry by Experiment," is the title of Dr. Thomson's book; a title which may be understood in two senses. First, it may seem to imply that the doctor modestly offers his labours as a mere attempt; or secondly, that others before him might perchance have laid down first principles of chemistry, but that the grand æra of *establishing them by experiment*, was reserved for himself. The reader is not left long in suspense by the ambiguity of the title; for the first pages show that the latter conceit has taken possession of the author's mind. From the preface, indeed, one might be led to look for some of those Herculean achievements with which Sir H. Davy astonished the world in his Elements of Chemistry, although his title-page did not blazon them forth. But *nulla fides fronti* is an adage which the reviewer has too many reasons to recollect.

We shall not, however, deal ungenerously with the Doctor; far less mete out to him with his own measure. We are ready to admit that by noting the mutually precipitating quantities of two neutral salts, he has in several cases given useful corrections of the primitive combining weights of bodies; and that he has, on some occasions, shewn errors in the second, and even first, decimal places of numbers formerly found. But, undoubtedly, his chief ingenuity is displayed in concealing, throughout the details of the book, the previous researches on the same topics of other experimenters, even when their results do not perceptibly differ from his own, which he presents as absolute perfection. Hence a young student will be led by the perusal of Dr. Thomson's "Attempt," to consider it both as the commencement and completion of chemistry, since he deigns to notice few precursors, and those chiefly for the purpose of pointing out their mistakes. He *demonstrates* his own numbers to be true, frequently to a millionth part; and rarely rests satisfied with a possible error of one part in a thousand!

Nothing places in so strong a light the vast improvement of practical chemistry during the last thirty-five years, as a comparison of modern results on chemical equivalents, with those obtained by Richter in his Researches published in 1792, and some succeeding years. He mixed together two neutro-saline solutions, in such a proportion, as to produce their mutual decomposition, as indicated by the complete precipitation of the new-created compound. This chemical operation is very simple, and with regard to many substances, susceptible of very considerable precision. Yet Richter's equivalent numbers are so erroneous, as to shew that he must either have been very careless, or have employed very impure salts. Wenzel, a much earlier writer, and the real author of those general views which Richter prosecuted concerning mixed saline solutions retaining the state of neutrality or acidity which they previously possessed, had however made far more accurate researches on the composition of salts; but these had been unaccountably neglected, though capable of furnishing excellent data for the theory of chemical equivalents.

Fischer, in the compendious table which he constructed from Richter's voluminous experimental tables, states sulphuric acid at 1000. Hence if we divide his equivalent numbers by 20, the quotients will shew their relation to sulphuric acid reckoned 50, as it is on Dr. Wollaston's scale. The following are a few of these quotients:

Potash .	80	Magnesia .	30.7
Soda .	43	Barytes .	111.1
Ammonia .	33.6	Muriatic acid	35.6
Nitric acid .	70	Carbonic acid	28.8
Lime .	39.6		

Surely nothing but the impurity of the bodies submitted to experiment, can account for the errors in these numbers; the three acids presenting the only tolerable approximations to truth. And indeed till chemicals could be procured in a state of purity, the method of research, by ascertaining the mutually precipitating quantities of saline matter, was quite nugatory. At the present day, however, the greater part of the most interesting saline compounds are prepared for sale by the manufacturer, so beautifully crystallized as to be quite free from impurities, and admirably adapted for the investigation of equivalent numbers. Such articles, made on the great scale by eminent dealers, are generally to be preferred for scientific purposes to those made in little capsules by the closet experimenter.

When the happy idea of atomic combination was broached by Mr. Dalton, chemical synthesis and analysis had become much more exact, as his collation of results exhibits. In the first volume of his "New System," published in 1808, we find the following numbers, reduced to oxygen 10, and sulphuric acid 50.

Azote .	7.1	Magnesia .	28.6
Carbon .	7.1	Lime .	33.0
Phosphorus	13.0	Soda .	40
Sulphur .	18.6	Potash .	60
Barytes .	97.1	Nitric acid .	27.1

The only articles here very erroneously given are azote, and its compound, nitric acid. Most of the other numbers do not differ much from the latest determinations; while those for soda and potash are exact.

Berzelius, familiar at an early period with Richter's speculations, was naturally prepared to embrace Mr. Dalton's views of atomic combination. He zealously set to work to determine, by the most refined and rigid methods of analysis, and synthesis, the true proportions in which chemical bodies combine. His successful labours formed the ground-work of Dr. Wollaston's valuable memoir on chemical equivalents. It is meanwhile worthy of remark, that Berzelius should very rarely have had recourse to Richter's method of mutual precipitation, in order to infer the atomic number of an unascertained salt, or of its constituents, from that of one already known.

Dr. Thomson is the first chemist who has methodically pursued this very simple and obvious route. Operating with the purer salts of modern times, he has been enabled to rectify and define the atomic numbers of a good many compounds; and thence also to deduce, on some occasions, the atomic weights of their constituents. He commenced this task in the "Annals of Philosophy, for Nov. 1820," in which he published the atomic weights of barytes, potash, soda, muriatic acid, protoxide of lead, sulphuric, nitric, and chromic acids. Of the numbers for the first four, as stated by Dr. Wollaston, he corrected the second decimal figure; the others seem to have admitted of scarcely any alteration, for Dr. Thomson's numbers nearly coincided with the previous determinations of Wollaston and Berzelius.

In verifying atomic numbers by the method of mutual saline precipitation, had Dr. Thomson been careful to ascertain that the decomposition was complete, as he might have done by suitable re-agents, much more confidence might have been reposed in his labours. But he has very frequently neglected this essential precaution, as we shall shew in the course of this examination of his work, and hence he has pretty often presented us with results, tallying well with the atomic theory, and with Berzelius, which he states as his own, though they could never have been derived from his narrated experiments. He has been also somewhat imprudent in quitting the solid track of precipitation, and of trying by *novel* methods, to demonstrate the truth of the idea suggested by Dr. Prout, that the atomic weights of all chemical bodies are multiples, by a whole number, of the atomic weight, of hydrogen.

Ceratis ope Dædalea
Nititur pennis.

The ingenious author of that proposition adduced so many examples, and analogies, as to render it highly interesting and plausible; and it has accordingly been regarded with a partial eye by the most eminent of our chemical philosophers. The beautiful law of gaseous combinations, discovered by M. Gay Lussac, and their densities to that of hydrogen being apparently in simple arithmetical proportion, naturally pointed to the principle so ably developed by Dr. Prout.

The proportion of the two elements in the composition of water, is by common consent regarded as constituting the ground-work of the atomic scale. Water is known to consist of one volume of hydrogen combined with half a volume of oxygen; and if this half volume be exactly eight times heavier than the entire volume of hydrogen, we shall have their atomic relation represented by these numbers. This half volume of oxygen, viewed as a component of concrete bodies, is estimated to weigh 1; phosphorus then weighs 1.5, and sulphur 2. But if that half volume of oxygen be contemplated in its gaseous state with reference to one volume of air as 1, or unity, its weight becomes 0.5555; and the atomic weights of the other bodies are brought into comparison with this gaseous standard, by reducing their atomic numbers in the ratio of 1.0000 to 0.5555. Thus phosphorus will become in the primitive combining volume of its vapour, $0.833 = 1.5 \times 0.5555$; and sulphur, $1.1111 = 2 \times 0.5555$.

In some cases, the concrete aspect of the oxygen atom, or prime equivalent $=1$, is convenient; in others, the gaseous aspect $= 0.5555$. They are, however, merely different forms of the same proposition; nor can the arithmetical reduction of the concrete unity to the pneumatic expression, be considered, with Dr. Thomson, as a *law of combination.* In fact, the assumption that half a volume of oxygen constitutes one atom, *versus* an entire volume of hydrogen, is altogether arbitrary, and merely a matter of convention among chemists.

We are acquainted with no body, contemplated in its gaseous state, which unites with less than its own volume of hydrogen. Most bodies, on the contrary, which combine with hydrogen, do so in the same proportion as oxygen does; that is, they take twice their volume of this inflammable gas. Now, if the densities of hydrogen and oxygen gases are as 1 to 16 exactly, then from the known relations of oxygen to other bodies, it will not be difficult to shew, that their atomic weights will be all, very nearly, if not exactly, whole numbers, or multiples of hydrogen = 1. Thus we perceive carbon to be 6, oxygen 8, phosphorus 12, nitrogen 14, sulphur 16, &c.

Dr. Thomson, in his " Historical Introduction," considers Mr. Dalton's choice of the atom of hydrogen for unity, as unhappy; asserting, that with the exception of Dr. Henry, of Manchester, and one or two chemical gentlemen in London, " this method has been rejected by almost all the British chemists, and by all the chemists without exception in Europe and America." The Doctor's reasons are thus stated: " 1. Because the atom of hydrogen is the most difficult of all to determine; and chemists are not yet all agreed about its weight. 2. Hydrogen, so far as we know at present, combines with but few of the other simple bodies; while oxygen unites with them all, and often in various proportions. Consequently, very little advantage is gained by representing the atom of hydrogen by unity; but a very great one, by representing the atom of oxygen by unity. For it reduces the greater number of arithmetical operations respecting these bodies to the addition of unity; and we see at once, by a glance of the eye, the number of atoms of oxygen which enter into combination with the various bodies. Thus, if the atom of manganese be represented by 3.5, and the weight of the various oxides of that metal be as follows:

1. Suboxide	4	4. Tritoxide	5.5
2. Protoxide	4.5	5. Manganesious acid	6.5
3. Deutoxide	5	6. Manganesic acid	7.5

It is obvious at once that the

Suboxide contains	½ atom ox.	Tritoxide	2 atom ox.
Proxide	1	Manganesious acid	3
Deutoxide	1½	Mangenesic acid	4

Whereas, if we were to make the atom of hydrogen unity, these weights would be as follows:

Manganese	28	Tritoxide	48
Suboxide	32	Manganesious acid	56
Protoxide	36	Manganesic acid	64
Deutoxide	40		

Numbers, which would not suggest the number of atoms of oxygen contained in each, with the same facility *."

The first reason given by Dr. Thomson applies with equal force to oxygen, for its relation in atomic weight to other bodies is little better known than that of hydrogen to them. The proportional weights of these rival elements are always inferred from the composition of water; and whatever relation be discovered between them, it will pervade the whole system of bodies which have for a constituent either hydrogen or water. In Mr. Dalton's original numbers, published in 1808, the atomic relation of hydrogen to oxygen in water is 1 to 7; and therefore to reduce all the numbers to the oxygen unity, they have only to be divided by 7. No error will be introduced into the resulting numbers, by having taken the hydrogen unity, provided the analyses of the oxides and oxygen acids have been exact. Again, if water be supposed to consist of 1 part hydrogen to 7½ oxygen, as on Dr. Wollaston's scale, surely this synoptic table would not have its accuracy affected by reducing the numbers to a hydrogen unity; for the relations of oxygen to the other bodies, being deduced from their proper authorities, would stand as before.

* Historical Introduction, p. 14.

Hence it is obvious that the system of atomic weights derives no immunity from error, by calling oxygen 1, and hydrogen 0.125. The following manifesto of Dr. Thomson is therefore founded on a false conception; "whereas, if we make choice of oxygen for our unity, any error respecting the atom of hydrogen will be confined to that atom, and will not affect the accuracy of the atomic weights of other bodies *." On the contrary, an error respecting the hydrogen atom will affect the atomic numbers of all the hydrogen acids and hydrates, under which title rank most of the free acids, a great proportion of the salts, and all the organic products.

The second reason assigned by Dr. Thomson, that hydrogen combines with but few of the other simple bodies, is a very singular one; for hydrogen forms a variety of most interesting compounds with all the simple bodies non-metallic, as also with some metals, while its compound, water, is of almost universal agency.

Three advantages accrue from the assumption of hydrogen, as the atomic unity. 1. We get rid of numbers less than unity in the scale of equivalents. 2. We avoid fractional quantities throughout the whole range of atomic numbers. 3. The atomic numbers in the hydrogen scheme, exhibit for the most part in reference to that gas, the specific gravities of the chemical bodies supposed to be in the aëriform state; and the combining ratios of their weights, under the same volume, are made manifest to the eye. This is a capital advantage in all researches on the gases, or on bodies which afford gaseous products in analysis. Let us consider this proposition, in reference to a few leading chemical bodies, Carbon, Phosphorus, Azote, and Sulphur.

1. Subcarburetted hydrogen gas is, as its name indicates, a compound of one volume vapour of carbon + 2 volumes of hydrogen gas; the weight of which are 6 + 2 = 8; which sum, from the 3 volumes being condensed into 1, is its specific gravity, compared to hydrogen = 1.

2. Carburetted hydrogen or olefiant gas. A volume of this compound consists of 2 volumes hydrogen + 2 volumes carbon, whose weights are 2 + 12 = 14; and as these 4 volumes become 1 volume of the compound gas, the specific gravity of this will be 14, if hydrogen is 1.

3. Phosphuretted hydrogen results from 1 volume of phosphorus = 12 + 1 of hydrogen = 1, constituting 1 volume, whose weight is 13, being the specific gravity, as well as atomic weight, of the aëriform compound on the hydrogen scale.

4. Subphosphuretted hydrogen is composed of 1 of phosphorus = 12 + 2 hydrogen = 2, constituting 1 volume, whose weight is 14. This is at once the atom and the density.

* Historical Introduction.

5. Hydrogen with azote, or ammonia. This compound consists of 1 azote = 14 + 3 hydrogen = 3, whose joint weight is 7; and as these 4 volumes compose 2 of ammonia, the resulting specific gravity will be $\frac{17}{2} = 8\frac{1}{2}$ times that of hydrogen. The specific gravity becomes here one-half of the atomic weight.

6. Hydrogen with azote and carbon; the prussic, or hydrocyanic acid. This body consists of 2 carbon = 12, + 1 azote = 14, + 1 hydrogen = 1, whose sum is 27; which 4 volumes together constitute 2; therefore the specific gravity is $\frac{27}{2} = 13\frac{1}{2}$ times that of hydrogen; while the atom is the double.

7. Cyanogen, or bicarburet of azote. It consists of 2 carbon = 12, + 1 azote = 14, whose sum is 26; and as these 3 volumes form 1, its specific gravity will be 26 times that of hydrogen.

8. Sulphuretted hydrogen. This is composed of 1 sulphur = 16, + 1 hydrogen = 1, whose sum = 17, is the specific gravity of the gas compared to hydrogen, for the hydrogen does not change its volume in combining with the sulphur; or a volume of hydrogen gas and one of vapour of sulphur compose one volume of sulphuretted hydrogen gas.

When oxygen is contemplated in the general relations of weight and volume to hydrogen, we must bear in mind the hypothetical assumption at the root of both schemes of equivalent numbers; viz., that half a volume of oxygen = 8 is the primitive combining proportion. If we regard water as consisting of 2 volumes of hydrogen + 1 of oxygen, the entire volume of oxygen becomes 16, and therefore we shall have 2 + 16 = 18 for the total weights; and as these 3 volumes of constituents form 2 volumes of aqueous vapour at 212° Fahr., we shall have its density $\frac{18}{2} = 9$, and if hydrogen be called 0.0694 in reference to air = 1, the vapour will become 0.625 = 9 × 0.06944. As the density is here compared to air of the same temperature, the relation of 0.625 to 1 will continue through the thermometric range, since vapours out of contact of their liquids, and gases, suffer the same change of volume by change of temperature. The diminished specific gravity of the vapour will be therefore simply proportional to its diminished tension or elastic force.

The preceding exemplification of the value of the hydrogen *radix*, will we trust satisfy our readers, that Dr. Thomson must have formed a somewhat crude conception of chemical equivalents, when he talks so slightingly of that atomic scale. He should reserve his dogmatic decisions for more *tangible* matters. In a word, let any practical man compare the hydrogen and oxygen scales in reference to the water of crystallization of salts, and he will readily find that 9 and its multiples, are much more manageable than the multiples of 1.125.

But the Doctor is not content with reasoning alone; he must needs exemplify, and his manganese case is a droll enough proof of the very great benefit (to himself) " of representing the atom of oxygen by unity, for it reduces the greater number of arithmetical operations, respecting these bodies, to the addition of unity." When a gentleman's arithmetic extends no farther than the addition of unity, assuredly Dr. Thomson's plan is the only safe one; and he illustrates with peculiar *naïveté* the intricacy of the other plan, in which hydrogen being called 1, oxygen assumes the formidable magnitude of 8. On multiplying the following quantities of his favourite scale, viz., 3.5, 4, 4.5, 5, 5.5, 6.5, and 7.5, by that unwieldy number 8, he has committed 3 blunders; his products being, as we have quoted them above,

28	instead of	$28 = 3.5 \times 8$
32		$32 = 4 \times 8$
36		$36 = 4.5 \times 8$
40		$40 = 5. \times 8$
48		$44 = 5.5 \times 8$
56		$52 = 6.5 \times 8$
64		$60 = 7.5 \times 8$

We shall no longer be surprised at Dr. Thomson's antipathy to the hydrogen scale; though to a student somewhat advanced in simple addition or multiplication, a short succession of 8's will not be found very difficult to link together. Were we anxious to rest our cause on the authority of names, as Dr. Thomson has tried to do, we would adduce as its patrons, Sir H. Davy, Mr. Dalton, Dr. Prout *, Dr. Henry, Mr. R. Phillips, and many other chemists, who prefer the hydrogen to the oxygen *radix*. Since the atomic theory is an indigenous plant, whose habitudes and cultivation have been but partially studied abroad, we cannot allow to the opinions of continental chemists much weight in the discussion.

We shall pass over, without further remark, Dr. Thomson's Historical Introduction, as also his Second Chapter, entitled, " Of the Atomic Theory," as presenting nothing of interest. Having got mystified, to no purpose, among the canons of Dalton and Berzelius, he strives to emerge, by a rank and file parade of acids, with 3 atoms of oxygen, with 1 atom, with 2, 4, 5, 7, and 8; an arrangement turned to no subsequent account.

His Third Chapter, " On the Specific Gravities of Oxygen and Hydrogen Gases," is held forth as " the key-stone of the building," and therefore it merits our especial attention. It is divided into three Sections, the first of which treats of the composition of

* There is an advantage in considering the volume of hydrogen equal to the atom, as in this case the specific gravities of most or perhaps all elementary substances (hydrogen being 1) will either exactly coincide with, or be some multiple of the weights of their atoms," &c.—Dr. Prout in Ann. of Phil. vii. 113.

oxide of zinc; the second, of the specific gravity of oxygen gas; and the third of that of hydrogen gas.

He sublimes ordinary zinc in an earthen retort, dissolves a given weight of it in nitric acid, and then expels the acid, by heating the nitrate to redness in a green glass retort. In this way, he obtains at once, to the minutest fraction, every thing which Dr. Prout's atomic multiples require, viz., 5.25 grains of oxide, from 4.25 grains of metal. This felicity of coincidence between his experiments and his theoretical aim is so usual with Dr. Thomson, and with him alone, as to excite no surprise in our minds. Another chemist, in quest of final precision, would have procured his zinc by reviving the metal from a pure carbonate, precipitated from a well crystallized sulphate; as he would know that it is impossible to obtain pure zinc by subliming the metal of commerce. Proust long ago sought to determine the composition of oxide of zinc in the above way. He obtained 125 grains of oxide, by igniting the nitrate formed from 100 grains of metal. This made its atomic weight 4; and it is probable that no chemist could get the number 4.25 by oxidizing zinc purified by sublimation alone, as it usually contains arsenic, cadmium, &c., which rise along with it.

We by no means assert that the atomic weight of zinc is not 4.25, for this is the mean number deducible from the experiments of Sir H. Davy, and M. Gay Lussac; and it is a number Dr. Thomson contrives to obtain in another, but very complicated, way. His atomic certainty of result springs out of the following operations. He decomposes a given weight of sulphate of zinc by carbonate of soda, he washes this carbonate on a filtre, dries it, and finally exposes it to a red heat. After these manipulations, Dr. T. gets his experimental weight to tally with his theoretic wishes, to the third decimal figure; a comfort rarely enjoyed by other practical chemists. In the last edition of his *System of Chemistry*, crystallized sulphate of zinc is declared to hold 5 atoms of water; 7 are now recognised, being 2 more than Berzelius and some other inquirers met with. We believe, indeed, that the Doctor's salts have been often in a peculiar state, as to their water of crystallization.

In the section on the specific gravity of oxygen gas, he has the hardihood to refer to his experimental results, published in the sixteenth volume of the *Annals of Philosophy*, as being perfectly accurate, though it has been fully demonstrated, that they were wide of the truth *. What renders this reference peculiarly absurd, is, that in the present book he abandons the notions he formerly held, on the weight of moisture existing in a gas standing over water, and adopts a formula for ascertaining its amount,

* See *Quarterly Journal of Science* for June, 1822.

which, though still inexact, yet being applied to his former experiments, throws them all beyond the pale of the atomic theory. To square his experiments with Dr. Prout's numbers, he, on that occasion, estimated the weight of aqueous vapour in his gases, at about one-sixtieth of what they really contained; and now, when he admits that there must be nearly fifty times more moisture in them, still he cites his old results as true. His new ideas relative to aqueous vapour, we shall presently discuss. Meanwhile, let us consider " the attempts which he has made to determine the specific gravity of oxygen gas in quite a different way."

He dissolved in dilute sulphuric acid, 100 grains of distilled zinc, contained in a small glass retort. At the end of twenty-four hours, the metal being dissolved, and the apparatus, *&c.*, having taken the temperature of the room, he obtained in each of 2 experiments, a volume of moist hydrogen gas, which, reduced by calculation, gave at 60° Fahr., 138.755 cubic inches. He concludes, that one half of this volume or 69.3775 cubic inches of oxygen gas, have entered into union with the zinc. But from his researches in the preceding section, it appears, " that 100 grains of zinc when converted into an oxide, combine with 23.5294 grains of oxygen. Hence, the weight of 69.3775 cubic inches of oxygen gas, is 23.5294 grains. Consequently, 100 cubic inches weigh 33.915 grains *. Estimating with Sir George Shuckburgh Evelyn, the weight of 100 cubic inches of dry air to be 30.5. grains, the specific gravity of oxygen comes out 1.1111, or Dr. Prout's theoretic number *exactly*. This concurrence of numbers, in the sequel of such experiments, we may venture to predict will hardly be realized by any other chemist.

Having settled this point to his entire satisfaction, he concludes the section by a formula, to shew what influence the presence of moisture must exercise on the specific gravity of oxygen gas. But his formula is rendered altogether erroneous by his adopting 0.00772, as the specific gravity of aqueous vapour at 60° Fahr. This being a matter of fundamental importance in pneumatic chemistry, and one much misunderstood, we shall devote a paragraph or two to its elucidation.

M. de Saussure took extreme pains to determine, by direct experiment, the weight of aqueous vapour contained in a given portion of moist air. He ascertained that the same volume of different gases, standing over water at a given temperature, afford, on being dried by muriate of lime, the same weight of water; which, for 100 cubic inches of aëriform matter, amounted to 0.35 of a grain. Now the subsequent researches of M. Gay Lussac, and Mr. Dalton, concur to shew that the same volume and weight

* *Thomson's First Principles*, i. 65. Taking his own data, the above number comes out 69.3765, but this error is of no consequence.

of aqueous vapour exists in moist air, as would be found, at the same temperature, in a vacuum of the same capacity. Thus, 100 cubic inches of moist air, at 60°, will contain 100 cubic inches of aqueous vapour at 60°; which, by Saussure's experiments, weigh 0.35 grain. Dividing this number by 30.5, the weight of 100 cubic inches of dry air, the quotient 0.0114 will be the specific gravity of vapour at 60°, referred to air as unity.

According to M. Gay Lussac, the specific gravity of steam at 212° is to air at 212°, under the barometer pressure of 30 inches, as 0.625 to 1.000. But vapours out of contact of liquids, and air, follow the same rate of expansion or contraction, by change of temperature. This ratio of 0.625 to 1.000, holds therefore at all temperatures; and we have therefore merely to estimate the diminution of the quantity of vapour, due to its diminished tension. Now from the latest table of elastic forces of steam, that published in the *Philosophical Transactions* for 1818, we learn that the tension at 60° is represented by 0.516 of an inch of mercury; and consequently, that the weight of vapour is diminished in the ratio of 0.516 to 30 inches. But $\frac{0.625 \times 0.516}{30} =$ 0.01075; which is the specific gravity of aqueous vapour at 60° Fahr.; and 100 cubic inches of it will weigh $0.327875 = 30.5 \times 0.01075$, or pretty nearly one-third of a grain.

Gay Lussac has also shewn that aqueous vapour, at any temperature and pressure, consists of a volume of hydrogen, + half a volume of oxygen, constituting together one volume. Now a volume of hydrogen, therm. 60°, and barometric pressure 0.516, is $0.001193 = \frac{0.0694 \times 0.516}{30}$; and half a volume of oxygen in the same state is $0.009555 = \frac{0.5555 \times 0.516}{30}$. The sum of these two quotients is 0.01075; precisely as deduced above, in a different manner.

One hundred cubic inches of air, barom. 30, standing over water at 60° Fahr., consist of 100 cubic inches of dry air supporting a barometric pressure of 29.484 inches of mercury, and 100 cubic inches of vapour, sustaining a pressure of 0.516 of an inch of mercury. But 100 cubic inches of dry air, under that pressure, weigh 29.9754 grains, and 100 cubic inches of vapour under its pressure weigh 0.3278 grains. The sum 30.3032 is the weight of the hundred cubic inches of moist air, sustaining the total pressure of 30 inches of mercury.

In like manner, 100 cubic inches of oxygen gas standing over water, at 60° Fahr., consist of 100 cubic inches, sustaining a pressure of 29.484 inches of mercury, and 100 cubic inches of aqueous vapour, bearing the remaining portion of the pressure = 0.516 of an inch; 100 cubic inches of such oxygen weigh 33.88 × 0.9828 = 33.3047 + 0.3278 vapour. The sum 33.6325 is therefore the weight of 100 cubic inches of oxygen standing over water at 60°, barom. 30. Its specific gravity, compared to moist air, or their relative weights, when taken by the same balloon and balance at the same time, will be $\frac{33.6325}{30.3032} = 1.10986$. But Dr. Thomson's wonder-working balance afforded him *directly*, in these circumstances, the specific gravity 1.1117, being Dr. Prout's theoretic number, where that number ought not to be found. Such a fictitious, or we might rather say, factitious coincidence destroys all confidence in Dr. Thomson's experiments on the specific gravity of the gases.

The above calculation may be represented by the following formula, calling *S*, the specific gravity of dry gas, (bar. 30, and therm. 60°); *v*, that of vapour at 60°; *f*, its elastic force; then the specific gravity of the gas standing over water, bar. and therm. as above, will be $S \times \frac{30 - f,}{03} + v.$

We have seen that a volume of such moist gas consists of a volume of dry gas, supporting $\frac{29,484}{30}$ of the atmospheric pressure + a volume of vapour supporting $\frac{0.516}{30}$ of that pressure. Hence, by deducting from the specific gravity of the moist gases the weight of the volume of vapour present, or its specific gravity 0.01075, the remainder will be the specific gravity of the dry gases, under a barometric pressure of 29.484 inches. Thus the following tabular view arises. Specific gravity of

	Dry at 30 Bar.	Dry at 29.484 B.		Vapour at 60°.		Moist air at 60°.
Air .	1.00000	0.98280	+	0.01075	=	0.99355
Chlorine	2.50000	2.457	+	0.01075	=	2.46775
Oxygen	1.11111	1.09199	+	0.01075	=	1.10274
Hydrogen	0.06944	0.06825	+	0.01075	=	0.07900

If moist air be called unity, then the specific gravities of the other gases standing over water will be

Chlorine . . .	2.48370
Oxygen . . .	1.10986
Hydrogen . . .	0.7951

In moist oxygen gas, the aqueous vapour forms only $\frac{1}{102}$ of the total weight; but in moist hydrogen, it constitutes more than $\frac{1}{7}$.

Many readers will probably complain of the prolixity of this developement of the influence of moisture on the specific gravity of the gases. But as Dr. Thomson, and some other chemists of name, have propagated incorrect ideas on the subject, we felt it our duty to rectify them. We shall presently apply these plain enough propositions to the Doctor's new researches, when a comical conflict will be seen, between experiment in masquerade, and a pattern theory. In his anxiety to monopolize the honour of finishing the fabric of Dr. Prout, our author has, unhappily for his fame, deranged the whole edifice, and instead of fixing the key-stone, has actually laboured unwittingly to displace it.

In order to ascertain the weight of a given volume of hydrogen gas, he dissolved a certain weight of zinc in dilute sulphuric acid, contained in a matrass, to whose orifice there was luted a glass tube, filled with dry muriate of lime. The quantity of his zinc "was unluckily very much limited by the small size of the flask," whence the temperature of the mixture was less uniform, and the heat of the effluent moist gas proportionately uncertain; for in one experiment, it rose from 50° to 87°, and in another from 48° to 81°, when the flask stood in the air. In subsequent experiments, therefore, he surrounded the flask with water at 48°, but as he did not insert the bulb of a thermometer into the matrass, the temperature of its interior was unknown, and consequently that of the gas disengaged.

On repeating Dr. Thomson's experiment, we have found that the interior thermometer stood very considerably above that of the exterior one, whose bulb was near the vessel, or even in contact with it; and that, probably, the temperature of the effluent gas may be estimated at about 11 or 12° above that of the water-bath surrounding the flask.

When 100 grains of zinc were dissolved, "the loss of weight sustained by the flask was 3 grains, and the tube containing the muriate of lime had increased in weight 0.163 of a grain *." From former experiments, he found that 100 grains of zinc afforded, during their solution in dilute acid, 136.88 cubic inches of hydrogen gas, bar. 30.1, therm. 49°. "The specific gravity of vapour at 49°, under a pressure of 30.1 inches of mercury, is 0.00533; and the weight of the vapour, contained in 136.88 cubic inches of moist gas, is 0.0533 × 136.88 × 0.305 gr. = 0.2225 grain; but the moisture retained by the muriate of lime was only 0.163 gr. It is obvious from this, that the hydrogen still retained 0.059 of a grain."

"If from the weight lost, amounting to 3 grains, we subtract this 0.059 grain for moisture, the remainder, amounting to 2.941 grains, gives the true weight of the hydrogen gas exhaled, supposing it perfectly dry. Now, from the experiments related in the last section, we know that the volume of this gas, under the

* *Thomson's Attempt,* i. 69.

pressure of 30 inches of mercury, and at the temperature of 60°, is 138.7551 cubic inches *."

There is an astounding fatuity in this passage. The flask loses 3 grains in weight; of this loss, he calculates that 0.2225 is due to moisture vaporized in the hydrogen. Hence, 3 — 0.2225 = 2.7775 is the weight of the dry hydrogen, by his own shewing. The fractional quantity 0.059 is merely the moisture, which, by his account, escaped the hygrometric action of the muriate of lime, which quantity + that in the muriate of lime = 0.2225 falls to be deducted from the loss of weight in the flask = 3 grains.

And what is the drift of this *novel* experiment? Do its results shew the *weight* of a given bulk of hydrogen, in a definite state of moisture or dryness? Certainly not. For, though his purpose was to determine *experimentally* the proportion of dry air and moisture in a known volume of the moist gas, he is obliged to have recourse to calculation, to learn that proportion. But as he does not know the temperature of the effluent hydrogen, he wants the essential *datum* of that calculation. And supposing that by a wiser disposition of the experiment, he had known this *datum*, still his result would have been wrong, because the specific gravity of aqueous vapour is very different from what he assumes it to be in his formula. The confusion of thought, discovered by the Doctor on the present occasion, can only be accounted for on the supposition, that his head got turned in planting, as he fondly fancied, " the key-stone" of the grand atomic arch. We do not consider the experiment susceptible of remarkable precision in the best hands; but at any rate it might be made a *rational*, if it cannot be made a delicate one, by causing the disengaged gas to traverse a convoluted glass tube, surrounded by melting ice. The hydrogen would then escape in a definite hygrometric state, and its weight would be exactly known from the loss of weight suffered by the apparatus.

He concludes as follows:—" Thus it appears that 138.7551 cubic inches of dry hydrogen gas (bar. 30 inches, therm. 60°) weigh 2.941 grains; consequently 100 cubic inches must weigh 2.119 grains."

On the contrary, the weight of that bulk of his dry hydrogen is, by his own statement, 2.778 grains, and therefore the weight of 100 cubic inches is 2.778 divided by 1.387551 = 2.002.

" But we have seen in the last section that 100 cubic inches of oxygen gas weigh 33.915 grains; now

2.119 : 33.915 :: 1 : 16.005.

" This approaches so nearly the ratio of 1 : 16, that it leaves no doubt that the specific gravity of oxygen gas is exactly 16 times greater than that of hydrogen gas †."

It is truly fatiguing to wade through such an inundation of

* *Thomson's Attempt*, i. 70. † *Ibid.* i. 71.

blunders. His own result, in reality, gives 2.002 grains for the weight of 100 cubic inches of dry hydrogen (bar. 30, therm. 60°), whence the following proportion :—2.002 : 33.915 : 1 : 16.9, a number which, if true, would utterly subvert Dr. Prout's theory.

But it is time to return to his estimate of the specific gravity of vapour at 49° bar. 30.1. He states it, most erroneously, at 0.00533. Let us take (as he does) Mr. Dalton's number, 0.363, for the elastic force of aqueous vapour at 49° Fahr. Now, the true specific gravity of such vapour, according to the principles so clearly established above, is $0.0075625 = \frac{0.625 \times 0.363}{30}$, air at 49° = 1.000000. But in reference to air at 60° = 1, the number becomes 0.00773; for the density of air at 49° is to that at 60° as 508 : 497; whence we have this proportion :

497 : 508 :: 0.0075625 : 0.00773.

Hence the weight of moisture in his 136.88 cubic inches (or 3 grains) of hydrogen gas is 0.00773 × 136.88 × 0.305 gr. = 0.3227. But the muriate of lime intercepted (he says) 0.163 gr.; therefore 0.3227 — 0.163 = 0.1597 of a grain must have remained in the hydrogen, assuming his experiment to have been accurately made. Thus we see that about one-half of the moisture escaped the action of the calcareous muriate, so that its presence merely complicated the result. Deducting from 3 grains, which is the whole weight of humid hydrogen disengaged from the flask, the 0.3227 of a grain of aqueous vapour, we have a remainder of 2.6773 grains, for the weight of dry hydrogen, whose bulk, he says, would have been at 60° Fahr., 138.7551 cubic inches. Hence $\frac{2.6773}{1.387551} = 1.9295$. This, therefore, is the weight of 100 cubic inches of dry hydrogen, by Dr. Thomson's *experiments* at 30 bar, and 60° therm. And 1.9295 : 33.915 :: 1 : 17.57. "This approaches (*not*) so nearly the ratio of 1 : 16, as to leave no doubt that the specific gravity of oxygen gas is *exactly* 16 times greater than that of hydrogen gas." Consequently, the specific gravity of hydrogen gas is $\frac{1.1111}{17.57} = 0.0632$; a very different result indeed "from the specific gravity already deduced by Dr. Prout (0.0694), and which I obtained experimentally, as may be seen in my paper on the specific gravity of gases*."

Of the result to which he here refers, and which is given in the 16th volume of his *Annals*, p. 168, it is sufficient to say, that he found the specific gravity of moist hydrogen, compared to that of moist air, to be by experiment as 0.0694 to 1.000†, though Dr.

* *Thomson's Attempt*, i. 71.

† The Doctor's estimate of the equation for vapour being about $\frac{1}{60}$ of the real quantity, he accommodated his experiment to this fallacious judgment!

Prout's theory, whose truth he was labouring (most unskilfully, it must be owned) to demonstrate, requires the experimental specific gravity of moist hydrogen to be to that of moist air, as 0.07951 to 1.00000, a proportion very wide indeed of Dr. Thomson's result. For, if hydrogen gas, standing over water, be *not* to air standing over water, both at 60°, as 0.07951 to 1.0000, then dry hydrogen gas will *not* be to dry air as 0.0694 to 1.0000.

We are really ashamed of the necessity for such multiform details, but the Doctor so unblushingly foists on the public his old battered brass as genuine current coin, that we were here forced to call the tilting hammer to our aid.

"The key-stone of the building" being removed by his own hands, we shall have little further to do with the rest of his pile, than to keep out of the way of the rubbish. But the real atomic theory will not be injured by his *débris*. Has Dr. Thomson yet to learn that he is not the architect of that edifice, but a very common labourer in the quarry? and that its symmetry and solidity are equally independent of his puny efforts?

The fundamental proposition, that oxygen and hydrogen are to each other by weight, in the composition of water, as 8 to 1; and that hence the other chemical bodies may be found, in their atomic numbers, multiples of that for hydrogen, had been demonstrated by Berzelius and Dulong, two of the most accurate experimenters of the age. By transmitting dry hydrogen gas over black oxide of copper, ignited in a tube, and collecting the water produced; they found, on comparing its weight with the loss of weight sustained by the oxide, that the ratio of the hydrogen to the oxygen in water was 1 to 8.009; and since water consists of two *volumes* of hydrogen + one of oxygen, the relation of their densities is, as nearly as can be, $\frac{1}{16}$; whence calling the specific gravity of oxygen 1.1111, that of hydrogen becomes 0.0694.

There is a note to the 72d page of the *Attempt*, which might puzzle posterity. He states that the weight of his *famous flask*, filled with the dilute sulphuric acid and zinc, was about 3000 grains. Yet, a few pages before, he tells us that "the capacity of the flask was about 18 cubic inches," and that it "was nearly filled with a mixture of sulphuric acid and distilled water, in the proportion of about four parts of the latter to one of the former." Now "about 18 cubic inches" of such acid, must weigh fully 5000 grains, and the glass flask itself, of this size, would probably weigh about 2000 more. Hence, with the zinc, its total weight must have been at least twice 3000 grains. Or again, if we take his weight, 3000 gr. *versus* his bulk of 18 inches, then the contents (making an allowance of only 1000 gr. for his glass) could not have exceeded 7 cubic inches. Is the experiment of the flask altogether a fable, as these palpable contradictions about its weight and capacity seem to indicate?

Dr. Thomson's 4th chapter, on the atomic weights and specific gravities of chlorine and iodine, is remarkable only for dogmatical pretension, and eulogiums of experiments of which he should be ashamed. Thus, he refers to his specific gravity of chlorine gas, given in the 16th volume of the *Annals*, because he brought out the number 2.5, so as to tally with Dr. Prout's theory, though the gas, being saturated with humidity, should have had, *experimentally*, the specific gravity of 2.4837, compared to moist air, 1.000. Then, indeed, the chlorine in its dry state would be to dry air as 2.5 : 1.0. This trick of obtaining the atomic multiple numbers, in circumstances where they cannot be found, reminds us of a star-gazer, who, furnished with a little quadrant, took it in his head to verify and correct the zenith distances of the stars. Disdaining the equations for refraction, nutation, aberration, &c., he contrived, however, to make his results come very near to the places given by Dr. Maskelyne, and hence was looked up to by his little *coterie* as a great astronomer.

We shall here introduce an example of Dr. Thomson's experimental reductions, on a very simple matter. "I find that at the temperature of 69°, one cubic inch of water is capable of absorbing 417.822 cubic inches of muriatic acid gas. The temperature of the liquid augments considerably, and its volume, when cooled down to the temperature of the air, is 1.3433 cubic inch. It is obvious from this, that 100 grains of acid of this strength contain 103 cubic inches of acid gas; and a cubic inch of this acid contains 311.04146 cubic inches of acid gas. Acid of this strength has a specific gravity of 1.1958, and I find, by saturating it with calcareous spar, that it contains 40.39 per cent. of real acid, united with 59.61 of water. In winter, I have obtained muriatic acid of as high a specific gravity as 1.212.*"

In this congeries of blunders, it is hard to say whether his experiments or calculations be most in fault.

The specific gravity of muriatic acid gas is rated by him at 1.28472.

1. The weight of a cubic inch of water + 417.822 cubic inches of muriatic acid gas is 252.5 gr. water + 163.72 gr. gas = 416.22 gr. of liquid acid; and 416.22 : 163.72 :: 100 : 39.53.

Thus 100 grains of this liquid acid manifestly contain 39.33 grains of acid gas, and not 40.39, as he asserts.

2. 416.22 grains of water occupy a volume = 1.6484 of a cubic inch; but he tells us that the 416.22 gr. of liquid acid have a volume = 1.3433 cub. inch. Hence the specific gravity of the liquid acid is $1.227 = \frac{1.6484}{1.3433}$. But Dr. Thomson *asserts* that it was 1.1958, an incompatibility which we leave him to reconcile.

* *Attempt.* Dr. Thomson may find a manufacturer of muriatic acid in Glasgow, who will supply him in winter with acid at 1.220.

3. If the water bulk of the above weight of acid be divided by Dr. Thomson's specific gravity of the acid, we shall have its volume. Thus, $\frac{1.6484}{1.1958} = 1.379$ cubic inch $= 348.1975$ grain measures; and conversely its weight in grains, divided by its bulk in grains, is its specific gravity $= 1.197 = \frac{416.22}{348.1975}$.

After floundering through this mire of miscalculation, the Doctor has the modesty to say, "All the tables hitherto published, exhibiting the strength of muriatic acid of various specific gravities, are very erroneous, because they were constructed upon inaccurate data. I conceive, therefore, that it will be worth while to exhibit an accurate table of the specific gravity of this acid of determinate strengths. My method was, to saturate a given weight of the acid with calcareous spar." This is to out-Herod Herod. "My method," as he calls it, is the old and usual one, and was that employed in constructing a table of muriatic acid, published in his own *Annals of Philosophy for October*, 1817. But the method with nitrate of silver is incomparably more delicate and precise.

What are we to think of that man's candour, who, after asserting that *all* the tables of muriatic acid hitherto published are *very erroneous*, gives as his own discovery, and as a standard of truth, a little table, of which the fundamental numbers, viz., the specific gravities corresponding to quantities of acid per cent. are *apparently* taken, without acknowledgment, from a very extensive table of muriatic acid published in this *Journal, for January*, 1822. The slight disguise that the Doctor has put on his numbers, will never make them pass for his own.

Table in Journal of Science, Jan. 1822.			*Table in Dr. Thomson's Attempt*, 1825.	
Specific Gravity.	Acid in 100.		Specific Gravity.	Acid in 100.
1.200	40.777	—	1.203	40.659
1.1846	37.108	—	1.179	37.000
1.1620	32.621	—	1.162	33.945

The fundamental strength at sp. gravity 1.20 is that from which the numbers in the tables are deduced, and it is as nearly as may be the same in both. We are confident that Dr. Thomson's small deviations from the numbers given in our *Journal for* 1822 are errors; but supposing them not so, the differences are so inconsiderable, as not to entitle the Doctor to say all the former tables were "*very erroneous.*"

In his fifth chapter, Dr. Thomson treats of the atomic weight of azote, and specific gravity of azotic gas. The usual felicity of coincidence between experimental and theoretic numbers attends his attempts here—a felicity, however, which he had not the courage to claim a very few years ago. In his long paper on the specific gravity of the gases, published in the 16th volume of the *Annals* (*Sept.* 1820), he says, "When 100 volumes of air are mixed with 42 or 44 volumes of hydrogen gas, and an electric spark passed through the mixture, the diminution of bulk ALWAYS amounts to 63 volumes.*" His "hydrogen was then prepared from pure re-distilled zinc, pure water, and pure sulphuric acid, with the requisite precautions." Mark his language now:—"A mixture of 100 volumes of air, and 42 volumes of this pure hydrogen gas, was fired by an electric spark. The diminution of bulk in three successive experiments was precisely 60 volumes.†"

What confidence can be reposed in such plastic results?

He tries to analyze nitre, by igniting it in contact with iron filings contained in a copper tube. Here "chaos is come again," but we shall keep it, if possible, out of our pages.

He favours us in this chapter with a brief table of liquid nitric acid, which seems to be taken, without acknowledgment, from a copious table published seven years ago in the 8th number of this *Journal.*

TABLES OF NITRIC ACID.

Journal of Science, Jan. 1818.			*Dr. Thomas's Attempt,* 1825.	
Specific Gravity.	Acid in 100.		Specific Gravity.	Acid in 100.
1.4855	75.000	—	1.4855	75.000
1.4530	66.948	—	1.4546	66.668
1.4189	59.775	—	1.4237	60.000
1.3945	54.993	—	1.3928	54.545
1.3630	50.211	—	1.3692	50.000
1.3001	40.647	—	1.3032	40.000
1.2765	37.459	—	1.2844	37.500
1.2402	32.677	—	1.2495	32.574
1.2212	30.286	—	1.2173	30.000
1.2084	28.692	—	1.2012	28.571

A table of the atomical relationships of acid and water, at different densities, is given at p. 24 of our Journal of January, 1819, from which Dr. Thomson's atomical table does not differ; yet he never alludes to these researches which so long preceded, and in fact superseded, his own. That nitric acid of specific gravity 1.55, consists of 1 atom of real acid + 1 atom of water, was stated in a well-known chemical publication several years before Dr. Thomson's *Attempt* appeared. His table descends no lower than to 1.2012; and it obviously ranges itself as evenly by its predecessor, as decency would permit. We are certain that the differences, however small, will do no credit to Dr. Thomson's character as a chemist.

* Page 163. † *Attempt*, i. 96

In the 6th edition of his system, oxalate of ammonia is said to consist of 4.5 oxalic acid + 2.125 ammonia. But no such compound exists. In discussing ammonia, under azotic gas, he now describes the oxalate as composed of 1 atom acid + 1 atom ammonia, + 2 atoms water=8.875. This atomic weight of the crystallized oxalate of ammonia was first given in the Philosophical Transactions for 1822.

Chapter 6th is occupied with the atomic weights of the acidifiable combustibles, carbon, boron, silicon, phosphorus, sulphur, selenium, arsenic, and tellurium.

The Doctor exposes calcareous spar to ignition in a platinum crucible, and gets at once, as if by a wishing-cap, the absolute atomic weight of carbonic acid. "The next object," says he, "which engaged my attention was to disengage the carbonic acid from 100 grains of calcareous spar, and collect it over mercury, in order to ascertain its volume*." The manner in which he tries to attain his purpose would ensure its failure, in any hands but those of Fortunatus. He treats 100 grains of calcareous spar with strong nitric acid in a retort, connected with a graduated glass receiver nearly filled with mercury, and inverted in a basin of that liquid. The beak of the retort has a stop-cock attached to it, from which a bent glass tube proceeds to the top of the receiver. The spar in three or four pieces is dropped into the tubulure of the retort; its stopper is inserted with all convenient speed, and the stop-cock opened. We, who have made many similar experiments, know well that so much carbonic acid gas would escape in the time of introducing the spar and closing the retort, and so much nitric acid vapour pass over with the gas into the receiver, as would render the experiment nugatory for all atomic determinations. It, moreover, occasions too many chemical computations for the Doctor's arithmetic. On this occasion, he acts with a prudent reserve. He takes care not to state the experimental quantities, but pronounces the following oracular response. "All the necessary reductions being made, the volume of carbonic acid gas evolved from 100 grains of calcareous spar, supposing the barometer to stand at 30 inches, and the thermometer at 60°, and the gas to be perfectly dry, amounts from a mean of two experiments, *both made, with very great care*, to 94.246 inches†." The Doctor has already drawn so freely on our credulity, that we have really none to spare for the present large demand. The result is plainly factitious.

We cannot bestow a thought on his *luminous* details *about* the compounds of carbon and hydrogen. In a paper published in the *Phil. Trans.* for 1822, there are some facts relative to naphthaline, which he has carefully concealed from the readers of his

* *Attempt*, i. 140. † *Ibid.* 143.

book, least by stating prior researches, he should stultify his own.

Under silicon, he furnishes an amusing example of the facility with which mineral analyses may be twisted into any shape that a theorist shall fancy. Thus of nepheline, he says, " Let us suppose that in this mineral, every atom of alumina is combined with an atom of silica ; and every atom of soda with an atom and a half of silica." Again under dioptase, " Let us calculate the constituents of this mineral on the supposition that it is a hydrated sesquisilicate of copper." Knebelite consists, according to him, of silicate of iron, silicate of manganese, and *trisilicate* of manganese.

How imperfectly the Doctor is acquainted with the chemical habitudes of saline bodies, on which their mutual decompositions depend, will appear from his employing sulphate of soda as a reagent to detect the presence of lead. He mixes solutions of phosphate of soda and nitrate of lead, and tries the supernatant liquid, after it has become limpid, as follows : " A drop of this liquid was put into a watch-glass, and mixed with a drop of solution of sulphate of soda. No precipitation or opalescence took place, shewing that the liquid contained no sensible quantity of lead *."

We affirm on the contrary, that a solution may contain a very sensible quantity of lead, though sulphate of soda does not occasion in it either opalescence or precipitation ; a fact which we shall state in detail presently.

Dr. Thomson gets completely bewildered in his 7th chapter, on the relation between the atomic weights and specific gravities of gaseous bodies. Here we find him describing an arbitrary convention of numbers, as a law of chemical combination. The law is thus enunciated, " The specific gravity is equal to the atomic weight multiplied by half the specific gravity of oxygen gas †."

This is a valuable piece of legislation. Dr. Thomson resolves that half a volume of oxygen, weighing 0.5555, when air=1, shall be regarded as the atomic unity, or 1 ; consequently, the atoms of all gaseous bodies may be represented numerically in reference either to that demi-volume 0.5555, or to the weight =1. Hence the reduction of weights to volumes or specific gravities, is done by multiplying by 0.5555. This instead of being a law of chemical combination, is an annoyance created by the present oxygen scale, from which the hydrogen scheme is free. But this matter has been already discussed. Yet, after all, Dr. Thomson is under a mistake in ascribing to Dr. Prout the merit of that rule for converting the atomic weight of a body into the

* *Attempt*, . 200. † *Ibid*. 241.

weight of its volume in the gaseous state. It was clearly stated by M. Gay-Lussac, in his memoir on iodine, first published in 1814, and translated by Dr. Thomson into his Annals for February, 1815. We there read, " We do not know the density of the vapour of iodine; but from experiments to be stated below, I have found that the ratio of oxygen to iodine is 1 to 15.621. Now the density of a demi-volume of oxygen being 0.55179, 0.55179 × 15.621=8.6195 will represent the density of iodine under the volume taken for unity *.

Under alkalis and alkaline earths we can perceive no new determination of any consequence.

The section on alumina, exhibits the atomic theory, dancing in masquerade among the mineral species. Thus after stating Rose's analysis of felspar, he proceeds as follows: " Let us suppose that all the bases are combined with silica, and in the state of trisilicates, except potash, which is a quadro-silicate; and let us calculate its constitution according to that supposition.

(1) 2.25 : 6 :: 17.5 : 46.666=silica united to alumina.
(2) 6 : 8 :: 12 : 16=silica united to potash.
(3) 3.5 : 6 :: 1.25 : 2.143=silica united to lime.
(4) 4.5 : 6 :: 0.75 : 1 =silica united to oxide of iron †."

In spite of all this coaxing, he is pestered with an excess of silica in the mineral =0.941, with which his four bases will have nothing to do.

He expresses astonishment at Mr. R. Phillips's number 3.375 for alumina, not perceiving it to be his own number slightly travestied in order that alum might be content with two atoms at 3.375=6.75, instead of 3 atoms at 2.25=6.75. The Doctor's distress at this discrepancy is truly ludicrous, and will meet with no sympathy.

But Doctor Thomson has reserved his grand atomic *ballet*, that it might be performed on the three new earths, glucina, yttria, and zirconia. We shall exhibit merely an *entrée* or two.

" Let us now calculate the composition of eudialite, on the supposition that all the bases are combined with silica, in the state of trisilicates, except the soda, which must be in the state of bisilicate.

" (1) 6 (atom of zirconia) : 6 (3 atoms silica) :: 11.102 : 11.102 =silica united to the zirconia.

" (2) 3.5 (atom of lime) : 6 (3 atoms silica) :: 9.785 : 16.77= silica united to the lime.

" (3) 4 (atom of soda) : 4 (2 atoms silica) :: 13.13 : 13.13=silica united to the soda.

" (4) 4.5 (atom of protoxide) : 6 (3 atoms silica) :: 8.816 : 11.75 =silica united to the protoxide of iron and manganese.

* *Ann. of Phil.* v. 105. † *Attempt*, i. 302.

"Now 11.102+16.77+13.13+11.75=52.752. This is less than the whole silica in the mineral by 0.573. As the silica in combination with the zirconia constitutes $\frac{1}{4.8}$ of the whole, it is obvious that in order to have the quantity of silica united to the zirconia in the eudialite, we must add $\frac{1}{4.8}$ of 0.573=0.12 to 11.102, which will raise it to 11.222.

"This gives us, 11.222 : 11.102::6 : 5.9358=atomic weight of zirconia *."

Does the Doctor expect any man in his senses to receive the atomic weight of zirconia, so deduced? For elaborate frivolity we know nothing comparable to the above and its companion articles. The whole components of the mineral eudialite, as well as those of the other minerals, are undoubtedly associated by a reciprocal affinity; but our author pairs them out for his atomic pantomime, with all the gravity of a French dancing-master.

In the 10th chapter, on the atomic weights of iron, nickel, cobalt, manganese, and cerium, we have sought, but in vain, for any sound sample of chemical research. What is to be thought of such a passage as the following?

"From this statement it is obvious, that when 17.375 grains of protosulphate of iron are thus treated" (with a heat progressively raised) "7.59375 grains of water, or 7 atoms —$\frac{1}{4}$th atom, will be driven off; $2\frac{1}{2}$ grains of sulphuric acid will be converted into 2 grains of sulphurous acid, which will fly off, and half a grain of oxygen, which will convert the $4\frac{1}{2}$ grains of protoxide into 5 grains of peroxide. The remaining $2\frac{1}{2}$ grains of sulphuric acid united with 0.28125 grains of water, the $\frac{1}{4}$th atom still remaining, will be driven off in the state of fuming sulphuric acid †." Though "this statement" is quite absurd in an analytical point of view, it may be made instructive; for it displays strongly the inconvenience of the oxygen scale, compared to the hydrogen. The numbers, transposed to the latter scale, run thus: 139 grains green vitriol lose first $60\frac{3}{4}$ in water ($=63-2\frac{1}{4}$); 20 grains of its sulphuric acid resolve themselves into 16 grains of the sulphurous, which fly off, and four grains of oxygen, which go to the 36 grains of protoxide, converting them into 40 of peroxide. The remaining 20 grains of sulphuric acid, united with $2\frac{1}{4}$ grains of water, the one-fourth atom unprovided for, will be disengaged in the state of fuming sulphuric acid. Will the Doctor maintain that the latter view is not far more intelligible than his, in which numbers with five decimal places of figures are required? And with regard to his method of determining the

* *Attempt*, i. 339. † *Ibid.* 347.

atomic weights of the above metallic bodies, nothing can be more unsatisfactory. We are persuaded that his numerical results would be less equivocally obtained, by a collation of the experiments of other chemists with the modern atomic theory, than by any inference from his own researches.

"I found many years ago," says he, "that when 100 parts of iron are oxidized by passing the steam of water over them at a red heat, they combine with 29.7528 parts of oxygen *." But from the experiments of M. Gay Lussac, it is known, that 100 parts of iron oxidized in this way, unite with 37.8 parts of oxygen. So much for the Doctor's experimental precision.

Now for his theoretical profundity. "Hence it would appear that this supposed oxide (of Gay Lussac), is a compound of 1 atom iron + $1\frac{1}{3}$ atom oxygen gas †."

We cannot devote much time to his second volume. Persons who can find amusement in haphazard experiments and gratuitous inferences may look into his section on uranium, particularly the 4th and 5th pages. M. Arfwedson made some good researches on the combinations of this metal with oxygen, which Dr. Thomson as usual turns to his own account, by slight modifications. "To determine the atom of chromium I dissolved a quantity of chromate of potash in water, and added tartaric acid to the solution. An effervescence took place, and the solution assumed a fine green colour, because the chromic acid was converted into protoxide of chromium. Ammonia being poured into the green coloured liquid, the protoxide of chromium was precipitated. It was collected on a filtre, well washed, and dried in the open air ‡."

Now we affirm that this experiment, from which he deduces the atomic weight of chromium, was never made, for the result is impossible. *Ammonia does not precipitate oxide of chromium from the above green solution in tartaric acid.*

When solutions of chromate of potash and tartaric acid are mixed, there is an immediate formation of bitartrate of potash, which speedily falls down; and if the tartaric acid be in considerable excess, the chromic acid will be decomposed with effervescence. But the oxide in the resulting green liquid is *not precipitable by ammonia.* Yet Dr. Thomson builds upon a pseudo-experiment, one of his usual atomic structures. *Ex uno disce omnes.*

Our readers must, by this time, be nearly as tired, as we have long been ourselves, of this illusory and fantastic attempt.

He assigns 9 for the atomic weight of crystallized oxalic acid. The number 7.875 first given in the *Philos. Trans.* for 1822, is undoubtedly more to be depended on, particularly since it has been confirmed by "a chemical friend, of whose accuracy and information I (Dr. Thomson) entertain a very high opinion §."

* *Attempt,* i. 355. † *Ibid.* ‡ *Ibid.* ii. 51. § *Ibid.* ii. 103.

It is diverting to see the pertinacious effrontery with which he still refers to his experiments on oxalic acid, after the full exposure of their absurdity, in our review of his system, (6th edition,) and in our remarks on his answer to that review *.

He now sets to work, in his usual way, on the crystalline hydrate, to ascertain, whether or not, oxalic acid contains hydrogen. This point has, however, been determined so fully by the most delicate and decisive experiments, as utterly to supersede Dr. Thomson's tardy intervention.

Dr. Thomson's experiments to ascertain the atomic weight of tartaric acid betray a rudeness in practical chemistry, unaccountable in so old a hand. He describes tartrate of potash, as containing two atoms of water, separable by heat. The anhydrous salt has, according to him, an atomic weight of 14.25, to which 2.25 = 2 atoms of water being added, the sum 16.5 will be the number of the crystallized salt. 14.25 of the anhydrous salt, corresponding to 16.5 of the crystals, were found by him exactly equivalent to 20.75 of nitrate of lead. "The mother water (of the mixed solutions of these two salts) was tested with nitrate of lead, and tartrate of potash, without being in the least affected by either. Hence it contained no sensible quantity either of tartaric acid or of lead. The *whole* of these two bodies was contained in the precipitate which had fallen."

His number for tartrate of potash is unquestionably wrong; and, indeed, though it were right, his conclusion would be erroneous. For the *mother water* (as he elegantly terms the limpid supernatant liquid) contains, under his proportions, both tartaric acid and oxide of lead. Let it be tested with sulphate of soda, and it will become cloudy; with sulphuretted hydrogen, and it will become very black; or with nitrate of lead, and tartrate of lead will fall. Thus the principle of Richter, of whose application our Doctor is so vain, becomes, under his management, quite deceptious.

His determination of the atomic weight of acetic acid is liable to the same objections. He mixes solutions of 8.875 gr. of oxalate of ammonia (its true atomic weight appropriated as usual to himself, from a prior memoir in the *Phil. Trans.* for 1822) and of 23.625 grains of acetate of lead; and tests the supernatant liquid "by sulphate of soda and muriate of lime." Now we have the pleasure of informing the Regius Professor of Chemistry, that as a test of lead, sulphate of soda is good for very little on the present occasion; and indeed no accurate chemist would trust to it. *Acetate of lead and sulphate of soda can co-exist to a very considerable extent in a clear solution;* as the youngest tyro may prove, by adding to one portion of the supernatant liquor, muriate

* *Quarterly Journal,* xi. 155, and iii. 349.

of barytes, and to another portion of the same, sulphuretted hydrogen or prussiate of potash. In the first case, a copious precipitate of sulphate of barytes will prove the presence of sulphate of soda; in the second, sulphuret, or ferro-prussiate of lead, will fall. In fact, let solutions of sulphate of soda and acetate of lead be mixed in the proportions indicated by Dr. Thomson's atomic weights of these salts, or in the most exact equivalent proportions; a portion of sulphate of lead will fall, and a corresponding portion of acetate of soda will be formed. To the supernatant liquid, (of any atomic proportion,) add carbonate of soda, and carbonate of lead will be separated in abundance. When the alkali ceases to act, let a current of sulphuretted hydrogen be passed through the supernatant liquid, and sulphuret of lead will appear. Thus also ferro-prussiate of potash will detect lead in a solution, when the proportion is too minute for the carbonate of soda test.

Dr. Thomson, from his unaccountable ignorance of these gradations of affinity, has given, as experimental results, quantities which it was impossible to obtain by the method of precipitations. And, hence, had they not been rendered *conformable* to the researches of Berzelius and other accurate chemists, as well as to the theory of equivalents, the odds would have been ten to one against Dr. Thomson's numbers in almost any case.

The above remarks apply strongly to his sections on citric, tartaric and acetic acids. And we are somewhat surprised that he should expect any attention to his experiments on the ultimate analysis of vegetables, in which upwards of nine grains of the above crystalline acids are treated with only 200 grains of peroxide of copper. No certainty of their thorough decomposition, by the oxygen of the ignited oxide, can be ensured; and the result must be destitute of all authority.

We have now adduced ample, even superfluous, evidence, of the strongest negligence or incapacity in the conduct of his researches on the atomic theory. And moreover, the perplexity into which he runs, in considering the partial and erroneous canons of Berzelius, is a decisive proof that his general views are neither clear nor comprehensive. In our *Journal* for January 1822, page 307, we endeavoured to shew the fallacy of these pretended general laws of Berzelius. This development of ours seems to have fallen under the Doctor's talons in an unhappy hour; for he tortures and disguises it most unmercifully. We request our readers to compare the passage referred to in our Journal, with Dr. Thomson's "few words respecting Berzelius's law," at p. 469, *et seq.* of his IId. volume.

The style of writing adopted by the doctor, in this new work, ill accords with the lofty panegyric pronounced by himself, on his literary attainments. *I am remarkably concise, though I hope* *always clear, and generally energetic*.*" We humbly apprehend, that more obscure, flat, and tautological phraseology, than that of which the present "Attempt" is *made up*, is not to be found within the precincts of any English book-factory. Its periodic movements are heavy and reluctant like those of a worn-out atmospheric engine.

In a prefatory address to the students of medicine and chemistry in the University of Glasgow, he advertises them, that his "future courses of lectures will be more entertaining and varied." He assures us that his first reason for publishing this book, "is the great advantage which medical practitioners will derive from a knowledge of the atomic weights of bodies, and of the weights of the integrant particles of the salts, &c., which they have occasion to employ in their prescriptions. This knowledge will be easily acquired by a *perusal of the following pages*; and it will enable those who possess it, to avoid some very awkward blunders into which I have observed too many practitioners, even of considerable celebrity, frequently to fall, to the *no little inconvenience* of their patients †."

Does Dr. Thomson know a physician, celebrated for his medical attainments, in a printed circular, *addressed by himself*, to the directors of a royal infirmary, who, to the *no little inconvenience* of his patients, prescribed the fashionable medicine prussic acid, under the form of prussiate of mercury? Fortunately, this virulent poison was rejected by the stomach before it had time to shew the power of an atomic theorist on medical prescription. We may next hear of corrosive sublimate being substituted for muriatic acid; since they have the same relation to each other as the above two bodies.

Of the merit of his work, the Doctor speaks so authoritatively as to set criticism at defiance. Having affirmed that the present publication will be of no little service to all medical men, and medical students, he says; "The tables contained in this work ought to occupy a place in every laboratory, and to lie *upon the shop* of every druggist, that he may have it in his power to have recourse to them to regulate *all* his processes ‡."

The fashion of paper roofs having gone out, we think it doubtful whether his pages will have the fortune to lie *upon* the shop of every or even of any druggist; but there is another shop where a pulverulent drug is retailed, on whose counter his pages may possibly appear.

Conceit of knowledge prevents its acquisition. Dr. Thomson, having persuaded himself that all his experiments, however ill-devised or ill-executed, are of infinite value and perfect preci-

* *Annals of Philosophy* for April, 1822, p. 245.

† *Attempt*, i. Preface viii. ‡ *Ibid.* Preface xiv.

sion, modestly tells us, that Berzelius and Dulong are in error by $\frac{1}{115}$th part, while his own results are quite correct; though in reality they are incomparably more inaccurate, when their errors are not veiled by counterbalancing errors in arithmetic.

There are a few passages of his work composed in a better spirit, and rather freer from the arrogance that blinds him. On these we would willingly have bestowed commendation, had the author not forestalled for himself every laudatory form of expression.

II. *An Account of Experiments to determine the Figure of the Earth by means of the Pendulum vibrating Seconds in different Latitudes; and on various other Subjects of Philosophical Inquiry.* By Captain Edward Sabine, F.R.S. &c. *Printed at the Expense of the Board of Longitude.* Murray.

[The following Review of the first part of Captain Sabine's Work, namely of his Experiments on the Figure of the Earth, has been transmitted to us from a Correspondent in the United States. We are glad to perceive that the Works of British Science are so quickly and so justly appreciated on the other side of the Atlantic.]

From the time of the first cultivation of science, the size and figure of the earth have been objects of inquiry. To an ignorant and superficial observer, it presents the appearance of an extended plane; to the earliest cultivators of Astronomy, it shewed an evident curvature in the direction of the meridian; and it was not long before a curvature in a transverse direction was also detected by means of a difference in the apparent time of the occurrence of Lunar eclipses in different places: hence the earth was justly inferred to be of a figure nearly spherical. Other observations have confirmed the near approach of this inference to the truth: the shape of the section of the shadow in lunar eclipses is always circular; the appearance of great expanses of water is manifestly spherical; ships in departing from the shore are hidden by the curvature of the earth, long before distance alone could render them invisible; and Humboldt, upon the Peak of Teneriffe, observed an angle of 92° between his visible horizon and the zenith. All these, and innumerable other facts, lead to the confirmation of the received opinion, that the earth is, if not an exact sphere, of a shape that differs but little from that regular geometric solid.

Were the earth at rest in space, and had it originally existed in a fluid state, its several particles would, by their mutual attraction, have arranged themselves in a spherical form; had the matter of which it is composed been incompressible and homogeneous, this sphere would have been of equal density throughout; but if its substance had admitted of compression, the outer portion would have been the most rare, and the mass would have increased gradually in density to the centre.

But the form of the earth is affected by another circumstance. When a body is made to revolve around a fixed axis, its several particles describe circles, whose planes are perpendicular to, and centres are in the axis. In this way all the particles, except those situated in the axis itself, become affected by a centrifugal force, that, did no other power oppose its action, would cause them to fly off, in tangents to the curves in which they revolve: this force is in each particle proportioned to the radius of the circle it describes. In solid bodies the attraction of aggregation is, generally speaking, sufficient to prevent any disintegration, as a consequence of the action of the centrifugal force; and in the larger masses of matter, whether solid or fluid, the attraction of gravitation produces the same effect. Our earth is a body that is in a state of rapid rotatory motion, performing a complete revolution around its axis in the space of a sidereal day. Each point upon its surface is therefore acted upon by a centrifugal force. This is greatest at the Equator, and becomes zero at the Poles; and although the attraction of gravitation is far more than sufficient to render this centrifugal force of no effect, in throwing off any portion of the matter of which the earth, or its surrounding atmosphere, is composed, it is yet rendered manifest by a diminution in the intensity of the force of gravity. This diminution of the intensity of gravitation will affect the rate at which heavy bodies fall to the surface of the earth, and the time of the oscillations of pendulums. It was first observed by Richer, a French astronomer, who visited Cayenne in 1672, for the purpose of making astronomical observations: he was furnished with a clock that marked mean solar time in the latitude of Paris; and to his surprise he found that at Cayenne, in Lat. 5° N. its rate had become 2′ 28″ per day too slow. As this was a far greater change than could be accounted for by any alteration in the length of the pendulum, caused by difference of temperature, no explanation remained, except that furnished by the opposition of the centrifugal force to the attractive power of the earth. The centrifugal force, as has already been stated, is proportionate, at any point of the earth's surface, to the radius of the circle described by that point in its diurnal revolution, or to the cosine of the latitude: but this is not the measure of the diminution it causes in the intensity of gravitation; for the latter acts in the direction of a radius of the terrestrial sphere, while the former is parallel to an equatorial diameter: on account of this obliquity of action, the diminution in the force of gravity, arising from the diurnal rotation of the earth, is everywhere proportioned, not to the cosine of the latitude simply, but to its square.

The investigations of Newton and Huygens into the laws that regulate the action of central forces, furnish us with means, by which the relation between the whole gravitating force of the earth, and its diminution at the Equator, under the action of a centrifugal force, may be determined: supposing the earth to be a sphere, this ratio is that of 289 to 1; but in consequence of the flattening of the earth at the poles and its increased diameter at the Equator, of which we are about to speak, this relation is changed, and the centrifugal force bears a somewhat higher proportion to that of gravitation. A great part of the surface of the globe is covered with water, and many of the hypotheses of geologists suppose that the earth was originally in a liquid state. The figure of the earth, and the curvature of its surface, is that which the surface of the great mass of water spontaneously assumes; and hence grew the belief that the shape of our globe cannot be that of a perfect sphere; for were the solid nucleus of the earth perfectly spherical, the waters of the ocean must have accumulated themselves, by virtue of the centrifugal force, in a zone on each side of the Equator.

In order that a mass of fluid acted upon by its own gravitation, and the centrifugal force arising from its rotation around an axis, should be at rest, and have no tendency to move towards either its Poles, or its Equator, it is necessary, that the pressure of all the columns of fluids, extending from the centre to the surface, should be equal to each other. These several columns, if enclosed in tubes, communicating with each other at the centre, would therefore be in equilibrio; but a column beneath the Equator, being formed of matter whose gravity is diminished by the centrifugal force, must be longer than one terminating at the Pole, and the lengths of the intermediate columns must vary according to their latitude. The figure that would result from such a state of equilibrium, was investigated by both Newton and Huygens, but upon two different hypotheses. Newton conceived the force of gravity to arise from the mutual attraction of all the particles that compose the earth, acting upon each other with forces inversely proportioned to the squares of their respective distances; he thence inferred, that this force was not a constant one, and that if the figure of the earth was due to the attraction of gravitation, the intensity of this force at different points was affected by the earth's figure. The earth being once flattened by the centrifugal force, this very change of figure would render the force of gravity at the Equator the least intense: applying this theory to a homogeneous spherical mass, and assuming, most happily, that an ellipsoid of revolution would fulfil the conditions of equilibrium, he inferred that the proportion between the polar and equatorial diameters was as 229 to 230, and the compression $\frac{1}{230}$th part of the greater axis.

Huygens, on the other hand, denied that the particles were mutually attractive of each other, and assumed that each particle tended towards the common centre, with a force inversely as the square of its distance from that point; by means of this hypothesis, he concluded that the curve by whose revolution the terrestrial spheroid was generated, was not a conic section, but a curve of the fourth order, although, when the centrifugal force bore but a small proportion to gravity, it would not differ sensibly from an ellipsis. He found the proportion between the greatest and least diameters of this curve to be as 577 to 578, and a consequent ellipticity of $\frac{1}{578}$. Different as are these two hypotheses and their results, there is still one remarkable accordance between them, for by both the sum of the fractions that express the ellipticity, and the excess of gravity at the pole over that at the Equator, are identical.

The hypothesis of Huygens is now exploded, inasmuch as the mutual attraction of all gravitating bodies is admitted; but his investigation is not of the less value, for it gives the flattening that would take place, under the received law of attraction, provided the earth were composed of concentric shells, infinitely rare at the surface, and infinitely dense at the centre: and as Newton's investigation gives the compression in the case of uniform density, we have thus the extreme limits between which every possible difference in the ellipticity of the earth that can arise from a difference in its internal constitution, must be comprised.

The inferences of Newton were confirmed by Clairaut, who furnished strict demonstrations of two propositions assumed by that great philosopher; these are—1. That the elliptic figure satisfies the conditions of equilibrium; and, 2. That the centrifugal force varies with the square of the cosine of the latitude. He also demonstrated a theorem that has since been of much use in determining the shape of the earth, from observations on the intensity of gravity. This important theorem is as follows, viz., *The sum of the two fractions, one of which represents the ellipticity of the earth, and the other the ratio of the force of gravity at the Poles to that at the Equator, is equal to $\frac{5}{2}$ of the fraction expressing the ratio of the centrifugal force at the Equator to the force of gravity.*

We are only acquainted with the mere crust of the globe we inhabit; but reasoning from the nature of the substances of which it is composed, we might infer an increase in its density, between the surface and the centre. The same inference may be drawn from the experiments of Cavendish with the Balance of Torsion, and the observations of Maskelyne on the attraction of the mountain Schehallion: from these different methods a mean density may be inferred of not less than four and a half times that of water, while the outer shell has a specific gravity considerably below 3. Laplace, too, assuming the density of the surface to be three times that of water, has inferred a mean density of 4.746. It will therefore be evident that the ellipticity ought to be less than $\frac{1}{230}$; but the mere application of the calculus does not furnish the measure of its true amount, for we are ignorant of the nature of the substances under investigation, and the circumstances under which they were first united in one mass. Had the earth been originally a fluid, with a compressibility equal to that found to exist in water by the experiments of Canton, the ellipticity would have been $\frac{1}{360}$; but this hypothesis is probably wide of the truth, and the inferred ellipticity consequently incorrect.

In order to ascertain the real figure of the earth, it is absolutely necessary to have recourse to experiment and observation. The method that would at first appear most obvious, is that of actually measuring a portion of one of its meridians: should its degrees be found all equal, a truly spherical figure might be inferred; should they decrease from the Equator to the Pole, an elongation would be proved; but should the degrees nearest to the Pole be found the longest, no doubt need be entertained that the earth is flattened in the direction of its axis of revolution. With these views a portion of a meridian, extending from Dunkirk to the southern frontier of France, was measured by Picard and Cassini. The apparent result of this operation led to conclusions totally different from those deduced from the theory of gravitation, and the laws of central forces. The southernmost degrees of this arc appeared to be the longest; and thus ground was afforded for the belief that the earth was an oblong instead of an oblate spheroid*.

The measure of Picard and Cassini being at variance with the received hypothesis, but the instruments and methods of the age being insufficient to discover the error, it was proposed, by way of ascertaining the truth in the most unexceptionable manner, to measure a degree under the Equator, and another as near to the Pole as was practicable. With this view, Maupertuis was sent to Lapland, and Condamine to Peru. Their measures confirmed the general theory of Newton, in manifesting the oblateness of the earth. The degree of Maupertuis, compared with those measured in France, gave for the fraction expressing the degree of oblateness, $\frac{1}{178}$; but by a recent measure of the same degree by Soanberg, an error of more than 200 toises, in excess, has been detected, and the determination of this astronomer, compared with degrees measured in France, reduces the flattening to $\frac{1}{304}$. The French arc has subsequently been extended into Spain, and as far south as the island of Formentera. In England an arc of

* The same arc has since been more correctly measured by Mechain and Delambre, towards the end of the last century, by which a different result was obtained, giving the fraction $\frac{1}{203}$ for the flattening at the Poles.

three degrees, extending from Dunnose, in the Isle of Wight, to Clifton, was measured by General Mudge, and it has since been extended as far as Unst, one of the Shetland islands. Various other arcs have been measured at different times, as, at the Cape of Good Hope, by La Caille; in Pennsylvania, by Mason and Dixon; in Italy, by Boscovich; in Hungary, by Liesganig; and in India, by Lambton. If many of the contiguous partial arcs show an elongation, still all, when compared with others at a considerable distance, to the north or south, show a flattening towards the Poles: but, although this may be considered as fully established, there yet remains very considerable doubt as to the value of the fraction that expresses the relation between the Polar and Equatorial diameters of the generating ellipsis; the results, obtained by comparing the different measurements with each other, varying so greatly as scarcely to have narrowed the question within the limits in which it had been reduced by the hypotheses of Newton and Huygens, and the demonstrations of Clairault.

This variation, particularly where recent measures are concerned, is not attributable to a deficiency either in the observers, the instruments, or the methods of observation and calculation. It appears to be occasioned principally, if not entirely, by the deflection which the plumb-line undergoes, from the unequal density of the materials near the surface of the earth, and which affects the celestial determination of the latitude at the extremities of the measured arc: no means are as yet known by which the errors thus occasioned may be avoided, or their amount ascertained and allowed for. It is to their influence that we must ascribe the fact, that by combining together the French and British surveys, whereby an arc of nearly a fourth of the quadrant of the meridian is obtained, the ellipticity deduced is much greater than would appear, from a comparison of the separate degrees of this very arc with those measured near the Equator. If a precise determination of the figure of the earth can ever be hoped for by the measurement of portions of the meridian, it can only be by the comparison of arcs of very considerable extent, certainly of not less than five degrees, accomplished at parts of the meridian extremely distant from each other.

Other methods, however, exist, that are liable to less uncertainty. The accumulation of matter in the Equatorial regions modifies the action of the earth upon the moon, insomuch that the motion of the latter is affected by two irregularities—one in latitude, and the other in longitude. The maximum effect of these equations may be determined by observation; and hence the extent of the cause may be investigated. The calculation has actually been made by Bouvard, Burg, and Burkhardt, at the instance of La Place, and gives an ellipticity of $\frac{1}{306}$. This method has a great advantage over actual measurement, for it is independent of irregularities on the surface of the earth, or of inequalities in its internal constitution.

Observations upon the length of the pendulum, beating seconds in different latitudes, also furnish a method by which the compression may be determined. The length of the seconds pendulum may be demonstrated to be exactly proportioned to the force of gravity at the place of observation. The comparison of such observations at different latitudes will afford the data for calculating the lengths of the pendulum at the Pole, and beneath the Equator: these being respectively proportioned to the gravitating forces at these places, give the numerator and denominator of a fraction, that subtracted from $\frac{5}{2}$ of $\frac{1}{289}$, furnishes an expression for the oblateness of the generating ellipse in conformity with the theorem of Clairaut. This mode of determining the figure of the earth is better, for several reasons, than that of ascertaining the same fact, from the measure of degrees, whether distant or contiguous.

It has been shewn by the investigations of Laplace, that the term of the formula in which error may arise from the causes of anomaly, has a coefficient, that is five times as great when the ellipticity is inferred from degrees of the meridian, as it is when it is determined from the lengths of the pendulum in different latitudes. The mode of ascertaining the length of the pendulum vibrating seconds has been of late years so much improved, as to have become a very simple experiment, that may be well performed by a single competent observer; while the measure of a degree of the meridian is a laborious, tedious, and expensive process.

The work, whose title appears at the head of the present article, is for the most part occupied with an account of experiments, made to determine the length of the pendulum vibrating seconds in different latitudes, and in both hemispheres. They were performed during two voyages made in public vessels, and in the employ of government: in the first, the author visited and performed experiments at Sierra Leone, St. Thomas', the Island of Ascension, Bahia, Maranham, Trinidad, Jamaica, and New York; during the second, he landed, and experimented, at Hammerfest, Fairhaven in Spitzbergen, on the coast of Greenland, and at Drontheim.

The method principally relied upon by our author, and employed by him at all his stations, is the same which was previously used by Captain Kater at the several stations of the British Trigonometrical Survey, as detailed by him in the *Philosophical Transactions* for 1819. The fundamental experiment of this method consists in suspending a pendulum alternately, from two knife edges, one in the usual position of the centre of suspension, the other near the lens of the pendulum: the vibrations of the pendulum, when suspended from these two distant points, are rendered isochronous, by a change in the position of a small weight that slides along the pendulum-rod; the point near the lens is thus rendered the centre of oscillation, in consequence of a property of the pendulum discovered by Huygens, who demonstrated that the centres of oscillation and suspension were convertible points. The distance between the knife edges may be measured with great accuracy by means of microscopes attached to accurate scales, giving thus the true length of the experimental pendulum: the number of oscillations it performs in any given time may be ascertained by comparison with the pendulum of a well-regulated clock; and hence the length of the pendulum vibrating seconds at the place of experiment may be determined, by applying the well-known proposition that the lengths of pendulums are inversely as the squares of the numbers of their respective vibrations in equal times. After the length of the seconds pendulum has been thus determined in any one place, by experiments sufficiently multiplied to ensure against any probable error, another pendulum of similar shape to the first, with the exception of its having no moveable weight, and but one knife edge, which is situated at the usual centre of suspension, is employed. This pendulum may be hung up in front of the clock, with which the original experiment was made, or of some other whose rate is known, and which is placed in the same apartment: its rate of oscillation may be thus known, and its length calculated, upon the same principle as that which we have stated as the mode in which the length of the seconds pendulum was originally determined. The length of this last-mentioned experimental pendulum being thus ascertained, and with an accuracy equal to that of the fundamental experiment, it may be carried from station to station; the number of its vibrations, in a given time, as shewn at each station by comparison with an astronomical clock, will furnish data whence the length of the pendulum, vibrating seconds at that place, may be calculated. This method is undoubtedly the best that has hitherto been proposed, and we are not prepared to say that it is susceptible of any material improvement in the theoretic part; the manner of construction or even of using the instrument may perhaps undergo change, (the latter has undergone a very important change since its first employment by Captain Kater, in the improved method suggested by Captain Sabine, and adopted by him, of observing the coincidences,) but we cannot fairly anticipate that any principle more beautiful, or more readily reduced to practice, is likely to be discovered.

The method employed by the French philosophers, in operations of the same nature, at several points of the arc of the meridian passing through France, and subsequently by Biot at Unst and Leith, is entirely different; and is the invention of Borda. He suspended in front of an astronomical clock a sphere of platinum, by means of a slender iron wire, whose length was about four times that of the clock pendulum; the wire was made of iron, in consequence of the great tenacity of that metal, which would permit it to be drawn of great fineness without rendering the wire liable to break by the weight of the ball: the coincidences of this wire, with a cross or mark upon the lens of the clock pendulum, were observed by means of a small telescope placed in front. After the pendulum was brought to rest, its extreme length from the point of suspension to the lower surface of the spherical body was measured, while it remained suspended; the distance between the centres of oscillation and suspension, or the effective length, was found by calculation, founded on well known formulæ, on the supposition that the wire was devoid of weight; and a correction finally applied for the weight of the wire. In this method, each observation is entirely independent of any other, and rests upon its own merits; whilst in the method, employed by Captains Kater and Sabine, in which the *relation* only is determined, which the length of the seconds pendulum, at the stations to which the pendulum of comparison is carried, bears to the length at the station of the fundamental experiment,—the correctness of the *absolute* length at those stations will depend upon the accuracy of the original determination. But this need not be a disadvantage, even in determining the *absolute* length at the several stations, because the fundamental experiment may be frequently repeated, until perfect accuracy may be considered as attained; and certainly is none, in the application of the results to the deduction of the figure of the earth: because in such case it is the *relation* only, and not the absolute length, which is the object of *precise* inquiry.

The method of Borda has recently been altered by Biot, who uses a pendulum of less length; the apparatus may thus be more securely and conveniently carried from place to place, enclosed in a glass-case. In spite of this improvement, the method of Kater is well entitled to the preference; the first experiment requires no greater care or precautions, and will occupy less time than every separate determination by the method of Borda; and in every subsequent trial, the British method is very much more speedy, is capable of more accurate and comparable results, and is less dependent either upon external circumstances, or upon the skill of the observer.

Besides the method which we have described, our author made use of two others, as checks upon his experiments; the first was that of an invariable pendulum attached to a clock; the second arose from the variations in the rate of the astronomical clock in different latitudes: we refer to them only as having fully confirmed the results of his other experiments, for their principle is too well understood to need any illustration.

Captain Sabine, after having recounted his several observations of coincidences of the pendulums; of the rate of the clock with the invariable pendulums; of transits and altitudes of the sun and stars for the rate of the astronomical clock and chronometers; of meridian altitudes of the heavenly bodies for the latitudes of the places of observation,—all with a fulness of detail, which will enable those who may desire to do so, to trace every step of the process from the original observations to the ultimate conclusions,—proceeds to combine his determinations, for the purpose of calculating the compression of the earth. The mode he employs, using the thirteen stations visited in his voyages, is that given by Laplace, in the third book of the *Méchanique Céleste*, founded upon the principle of the least squares.

His calculation gives the fraction $\frac{1}{288,5}$ for the compression: a very remarkable result, in consequence of its being the same that expresses the ratio of the centrifugal to the gravitating force. Not content with his own measures, he has next combined them with those of Kater, at the stations of the British, and of Biot, Arago, *&c.*, at those of the French survey; and in every combination he is led to the same result. With such a confirmation, we are warranted in saying, that we conceive this value for the compression of the terrestrial spheroid, is more entitled to confidence, than any other that has yet been given; and when we consider its remarkable agreement with the ratio of the central forces, we cannot help believing that there may be some connexion between the external figure of the earth, and its internal constitution, that still remains to be investigated, and which we consider highly deserving the attention of the few mathematicians in the world who are competent to the investigation.

This determination of an ellipticity of $\frac{1}{288,5}$ differs much from any other, whether derived from former experiments with the pendulum, from the measure of contiguous, or of distant arcs of the meridian. It is, however, entitled to the highest confidence, inasmuch, as it is the first that has been drawn from observations of the pendulum, unconnected with any other operation. It is very remarkable, that, whilst the pendulum has a right to be considered as a mode of determining the relation between the polar and equatorial axis, perhaps more certain than any other, it has never before the present day, formed the principal object of observation, but has always been confided to the same persons, who were employed either in performing geodetic operations, or in calculating their results. We would not charge these distinguished philosophers with unfairness; they are far above any such suspicion; but would merely remark the singular coincidence that has from time to time been found, between the deductions obtained by means of the pendulum, and by the actual measure of arcs, when used to confirm each other. The French commission, who reported the *Système Métrique*, inferred an ellipticity of $\frac{1}{336}$. by comparing the degrees measured in France with that measured in Peru; and this oblateness is employed by them as the foundation of that system, so beautiful in theory. Laplace, who was one of that commission, calculated the compression of the earth, from 15 lengths of the pendulum, measured at different places, and inferred that it was $\frac{1}{338}$. This, if accurate, might have fairly been received as a strong confirmation; but on examination of his calculation, as given in the third book of the *Méchanique Céleste*, an error in taking out a logarithm will be detected; and correcting this mistake, the deduced ellipticity, by his method of calculation, would be increased to $\frac{1}{319}$. Since that period, and when the compression deduced from the measure of more distant arcs, became $\frac{1}{308}$; and that which agrees with the inequalities in the lunar motion, arising from the shape of the earth, is supposed to be about $\frac{1}{306}$; the same illustrious author, by admitting the new observations of Biot and Arago, infers from the general combination of pendulum experiments, an ellipticity of $\frac{1}{310}$. No better proof can be afforded than this, of the facility with which the few observations that had been made, before the pendulum was taken up by Great Britain, as a distinct and independent method, could be made to agree with any hypothetical oblateness of the terrestrial spheroid, when examined merely for the purpose of confirming, or disproving the calculations, whose data are derived from other sources. It is now time that the pendulum should assume the rank of an independent measure of the relation between the two diameters of the earth; and the credit is due to Captain Sabine, of having been the first experimental philosopher, who has distinctly asserted its equal claim, as well as proved by his experiments its right to be so considered. His experiments have, in fact, done more than place it on an equality, as present authority, with the measurement of terrestrial degrees: in his hands, it has become the only method of deducing the figure of the earth, which, as yet, has given a precise and determinate result.

We fully concur in Captain Sabine's opinion, that the satisfactory and conclusive nature of the result, which the pendulum has afforded, when the experiments with it have thus been duly and sufficiently extended, presents a strong ground of encouragement to attempt an equally conclusive result, by the comparison of terrestrial measurements undertaken on the same decisive scale, of which, his experiments with the pendulum afford the example. He has suggested a proceeding towards the attainment of such a result which we cannot do better than lay before our readers, adding our persuasion that it is well entitled to the serious consideration of every man of science, who, either in his public or private capacity, may have it in his power to promote its execution.

"The success which has thus attended the attempt to carry into effect, under the conditions most favourable for the experiment, the method of investigating the figure of the earth by means of the pendulum, and the consistent and precise result, far exceeding previous expectation, which, under such circumstances, it has been found to afford, encourage the belief that an equally satisfactory conclusion, and one highly interesting in the comparison, might be obtained by the measurement of terrestrial degrees, performed also under the requisite conditions to give its due efficiency to the method of experiment. Experience has fully shewn, that no result of decisive character is to be expected from the repetition or comparison of measurements in the middle latitudes; and that it is only from operations carried on in portions of the meridian widely separated from each other, that such an event can be regarded as of probable accomplishment. The project of the original experimentors,—of those eminent men, who nearly a century ago, devised and executed corresponding measurements at the equator and at the arctic circle,—was of far more vigorous conception, than the steps of their successors have ventured to follow, even to the present period; and it is due to their memory to recognise that the failure on that occasion was not from insufficient extension of view, or from deficiency in the spirit of enterprise; but from the attempt having been made in the infancy of practical science, when the instruments were inferior, and the modes of their most advantageous employment less understood, than they have since been rendered.

"The discordancies, which appear in the comparison of the measurements hitherto accomplished, are not so great as those which had resulted from the comparison of pendulum experiments, previously to the present attempt to give the latter method its full and efficient trial: it has been also seen that in proportion as the arcs have been enlarged, so as to include the continuous measurement of more extended portions of the meridian, and as the processes of operation have been conducted with improved means, and increased attention to accuracy, the anomalies have progressively diminished; the prospect therefore, that they may be made wholly to disappear, by combining the interposition of the greatest interval between the measurements that the meridian of an hemisphere will admit, would seem sufficiently probable to justify and induce the undertaking.

"Through the munificent liberality and splendid patronage of the East India Company, India already presents a determination of the arc contained between the 10th and 20th parallels: and as a consequence of the political changes which have recently taken place in South America, there is reason to hope, that the impediments to a measurement between the equator and the 10th degree, in the quarter of the globe best suited for the operation, will speedily be removed.

"In regarding the polar extremities of the meridian, the attention is naturally directed in the first instance to Spitzbergen, as the land of highest convenient access in either hemisphere; its qualification, in that respect, is indeed far beyond comparison with other lands, and is a point of very principal importance; its high latitude and conveniency of access do not, however, form its only suitability; for, on due consideration, it will be found to possess many very peculiar advantages for the operations of a triangulation.

"The general geological character of Spitzbergen is a group of islands of primitive rock, the ordinary hills of which are from 1000 to 2000 feet in height, commanding generally extensive views, and unencumbered with the vegetation which presents so great an obstacle to the connexion of stations in the more genial climates. The access to all parts of the interior is greatly facilitated by the extensive fiords, and arms of the sea, by which the land is intersected in so remarkable a manner: these, whether frozen over, as in the early part of the season, or open to navigation, as in the later months, form routes of communication suited to the safe conveyance of instruments either in sledges * or in boats; the fiord, in particular, which separates the western and eastern divisions of Spitzbergen, would be of great avail; it extends in a due north and south direction for above 120 miles, with a breadth varying from ten to thirty miles, and communicates at its northern extremity, by a short passage across the land, with the head of another fiord proceeding to meet it from the northern shores of the island, and affording similar facilities for carrying on either a triangulation, or a direct measurement, on the surface

* Sledges with rein-deer trained to draft, and the Fins by whom they are managed, may be hired for the season, at Hammerfest, in any number that might be required. Spitzbergen abounds more in the food of the rein-deer, and is more plentifully stocked with the animals themselves in their wild state, than any other arctic country which I have visited. The Officers of the Griper killed more than fifty deer on the small islands which form the northern part of the harbour of Fairhaven.

of the ice at the level of the ocean. It is hardly necessary to add, that the latter operation would be unembarrassed by the inequalities of surface, and uncertain temperature of the apparatus, which occasion so much trouble, and require so much precaution in the usual determination of a base.

"The extent of the arc in the direction of the meridian, between the southern shores of Spitzbergen and the islands on its northern coast in the eighty-first degree of latitude, is between four and five degrees. At the period of the celebrity of Spitzbergen as a fishing station, in the middle of the seventeenth century, when above 200 vessels, manned by 10 or 12,000 seamen, annually resorted to its vicinity, and frequented its harbours for the purposes of boiling oil, and when the harbours were divided by convention amongst the vessels in consequence of their numbers, according to the nation and towns to which they belonged, all parts of the coast were known to and visited by the hardy and enterprising Dutch and German seamen, by whom the fishery was then principally conducted. The whales have long since deserted the haunts which their kind had enjoyed for ages before in unmolested security, and have sought retreats less accessible to man; the graves, which occupy every level spot around the harbours, contain the only and in that climate the almost imperishable memorials of the once busy scene, which has reverted to its original solitude; even the accidental presence of a whaling ship in the western harbours is an event of rare occurrence*, and it is probable that more than half a century has elapsed since any vessel has passed to the North-eastern shores; it is not surprising, therefore, that the delineation of land, represented in the charts of the period when Spitzbergen was so greatly frequented as existing to the East of the seven islands, and to extend in a northerly direction far into the eighty-second parallel, should neither have been established nor disproved by modern authorities; those persons who have had opportunities of becoming acquainted, by examination on the spot, with the remarkable correctness of the older charts in general, in the insertion and in the relative position (when not separated by much extent of ocean) of lands then recently discovered, will hesitate too hastily to reject their testimony, until it has been satisfactorily disproved; should land exist as represented in the charts of the period alluded to, even though not visible from

* During the Griper's stay of three weeks in the neighbourhood of the harbour of principal resort in earlier times, and in the middle of the fishing season, not a single whale fish or whaling ship were seen. The only vessels which now frequent the shores of Spitzbergen, are Norwegian sloops in quest of sea-horses and eider down. Their visits have been hitherto confined to the fiords and the islands on the southern and western coasts; they arrive early in March, and remain as late as November, making occasionally three voyages in a season.

Spitzbergen, its triangular connexion might be established on the surface of the ice, and latitudes yet unattained be included in the operations of the survey; nor would it be safe to assign too confidently the northern limit of such operations even in the absence of land, in our present ignorance of the facilities which the ice itself may afford for their extension towards the pole.

"The measurement of a portion of the meridian in the higher latitudes is, however, one of the many experimental inquiries, beyond the reach of individual means to accomplish, for which the advancement of natural knowledge is delayed; if its accomplishment may be hoped for by that nation which has been most forward in exploring the regions of the north,—to whom its climates and its natural difficulties are familiar,—it must still await the existence of a channel in one of the departments of the state, through which the liberal disposition of the British Government to forward every undertaking worthy of a great nation, and by which it may occupy an additional page in history, shall be rendered available to other branches of scientific research, than those which are immediately conducive to the interests of navigation.—p. 360—364.

There can be no question that the measurement of an arc of the meridian of Spitzbergen, of sufficient magnitude to render inconsequential the irregularities in the direction of gravitation at its extremities, (and such would be an arc of 4½ or 5 degrees,) would be one of the most important, as well as one of the most splendid, of those enterprises for the advancement of general knowledge, which from time to time have received the support of enlightened governments, and have commanded the admiration of all civilized nations. To those persons, to whom the climates of the North, and the difficulties presented by its icy seas and barren shores, are not as familiar as they are to our author, the natural impediments to the accomplishment of such an undertaking, may appear in a more serious light than they are viewed by him, who has had experience of the means by which they may be surmounted, and has himself proved that such extreme situations are not incompatible with the utmost accuracy of experiment. But we do not hesitate to say that the attempt, even if it should terminate in demonstrating the impracticability of accomplishment*, would do honour to the government and the country,

* We are happy to have it in our power to state, that the proposed measurement of an arc at Spitzbergen, was brought under the notice of the President and Council of the Royal Society, previously to the last recess; and that the propriety of recommending to the Government an undertaking so important to the advancement of natural knowledge, is now under consideration.—*Editor.*

by which it should be made; and, that there is no country so competent to the undertaking as Great Britain; nor any time so suitable as the present; when the experience which she has gained in her northern voyages, (which have long since ceased to have any more important practical object in view than the acquisition of such experience, and the cultivation generally of a spirit of enterprise,) may be most advantageously applied in the attainment of a purpose, of the highest rank in the advancement of science, and in the general interest of which, the nations of every quarter of the globe, and of all succeeding periods will participate. It is time that Great Britain, pre-eminent as she is in commercial enterprise, and in that of maritime and geographical discovery, with wealth at command, and a government well-disposed to "forward every undertaking worthy of a great nation, and by which it may occupy an additional page in history," should assert a like pre-eminence, (which she does not at present possess,) in enterprises of a higher character, than the mere tracing the direction of a river, or the completion of the outline of distant, and for any useful purpose, unprofitable shores.

We proceed to notice, and we shall do so as briefly as possible, the bearing of Captain Sabine's experiments upon the application of the pendulum as a standard of measure, and upon the experiments which are previously considered to have referred the British linear scale to a definite length in nature. It is in this relation that we consider his work as entitled to the greatest attention, because the pendulum furnishes in all probability the only natural standard of measure that is invariable, determinate, and easily determinable, and as such it has become the subject of legislative enactments, having been adopted in an act passed in the session of 1824, and referred to as the means of identifying the authentic legal scale of Great Britain: there can be no doubt however, after the perusal of Captain Sabine's remarks, in pages 364 to 372, that the provision made by the act is inadequate for the purpose; and there cannot be a stronger evidence of the importance of more consideration being devoted to the subject, than that the provision of an act, designed expressly for the most distant posterity, should thus be shewn to be incompetent to its purpose, even before the act itself has arrived in operation. The act declares the British imperial yard to bear a certain proportion to the "pendulum vibrating seconds of mean time in the latitude of London, in a vacuum at the level of the sea." It necessarily assumes, consequently, 1st. That the length in nature so referred to, is of an uniform magnitude, and 2d, Not only that it has been measured, but that all future measurements must conduct to an identical result.

With respect to the first point, the experiments that are contained in the present volume shew conclusively that the latitude of the place and its elevation above the mean level of the sea, are not the only, nor even the chief, circumstances, that affect the length of the pendulum; and consequently, that the measurement in any one place, even supposing it to be correctly made, and reduced to the level of the sea, by an amount which should not be arbitrarily assumed, does not determine the *pendulum of the latitude*, because the nature and density of the substances, that compose the upper crust of the earth at the place of observation, have a most important bearing, and which cannot be neglected. The clock and the experimental pendulum were found to be liable to variations of not less than ten seconds per day in the same latitude, according to the nature of the materials upon which they rested; and, as all the observations were necessarily made upon the land, it is inferred that an equal variation in an opposite direction, might be considered as likely to occur if the experiments could be performed at different points on the surface of the ocean: the whole difference, then, that might arise from the action of the different substances that are found on the surface of the globe may, in the same latitude, amount to no less than twenty seconds; and the difference in the length of the pendulum at stations differing in local circumstances, but still under the same parallel, might be equal to 0.01 of an inch; or nearly one-tenth of the whole difference of the intensities of gravity at the pole and the equator, or to $\frac{1}{2000}$th part of the absolute attraction of the earth. It would thus appear, that before the mean force of gravity, in any parallel of latitude, can be inferred with certainty, numerous observations, indeed an almost indefinite number, ought to be made in or near that parallel, to produce by their combination, a near result.

With respect also to the allowance to be applied to the length of a pendulum measured at an height above the sea, to reduce it to what it would have been if measured at the level, it is shewn that the correction which has been recently proposed, for the error arising from the *figure* of the surface, by which the regular decrease of gravity in proportion to the squares of the distances from the centre is affected, may be safely neglected; but that a far greater uncertainty than from external conformation, and for which it would be far more difficult to assign a specific correction, is involved by the variable density of the materials, on which the pendulum is raised above the surface of the sea. From these considerations, Captain Sabine concludes that the pendulum of a particular *latitude* cannot become a standard of reference, because its length is not practically determinable; that the pendulum of a particular *city*, London for example, (whereby it is implied that a length measured in one part of the city should be recoverable by a measurement made in some other part of the city,) is open to the same objections, though in a less degree; but that the more simple standard, and which is of determinate and determinable magnitude, is the pendulum of a particular *spot*; it being understood that all future repetitions, designed to produce identical results, should be made identically at the same place.

We consider, that Captain Sabine has gone far towards proving that, in this view, the pendulum is applicable to the proposed object; that with proper precautions, and by adopting the method of experimenting which he has pointed out, different observers, using different instruments, may arrive with certainty at identical conclusions: at least so nearly identical as not to differ in the fourth place of decimals of a British inch. But he has also shewn (pages 213 to 233,) that the method which was previously adopted, and employed in the experiments which are considered by the act of parliament to have determined the length of the seconds pendulum in the latitude of London, does by no means ensure identity on repetition within the limits declared in the act; because the method is not independent of individual peculiarity, or of accidental circumstance. The conviction is thus forced upon us, of how essential the experiment itself of repetition is; and that it is expedient to prove that a method will produce identical results in other hands than the original experimenter, before it is officially bequeathed for such purpose to posterity.

The selection of a spot, the pendulum of which is to supply an invariable length in perpetuity, and which will require to be referred to, not less by foreign nations of the present day, who may desire to compare their standards with that of Great Britain, than by those of more distant ages who may seek the recovery of the British measures, is by no means an indifferent consideration. It has happened accidentally that the original experiments were made in a private house in London: a circumstance which in itself, must sooner or later have obliged their repetition elsewhere. But if the length of the pendulum is affected by natural local circumstances, to the amount we have stated, (and we think the fact too clearly made out by Captain Sabine to be questioned,) may not even artificial changes in the character of the place of experiment produce a similar result, although in a less degree? Can we be assured that the vibrations of a pendulum, in Mr. Browne's house in Portland Place, are the same now, as when, not more than two centuries ago, its site was nearly a mile without the limits of the city? Nor are either of these times identical with that which would be found, were the future observer compelled to seek for the spot amongst masses of rubbish. Nor is this last view of the subject, however improbable or distant, one that is to be entirely neglected. The language, the arts, and the sciences, which are the boast of Great Britain at the present day, are founded upon a basis more secure than that of empire, and will exercise an intellectual supremacy over future ages, should even the fate that has attended the former "glories of the world," overwhelm, at some remote period, her proud metropolis. It must ever be remembered, that on the transmission of her scale, will depend the value to posterity of every attainment which she either has made, or may make, in which linear measure is concerned; and that consequently her fame and her usefulness in those distant times may materially be influenced by the provision which she may now make for its exact transmission.

For these, and for other reasons which we have not space to state, we should consider it highly expedient that, whenever Kater's original experiments shall be repeated, for the final verification of the British scale, the proceedings should take place in a public building, and at such a distance from any probable extension of dense population, as may secure a close resemblance to its present state for centuries; and that, when the pendulum of that spot shall be considered as fully and satisfactorily determined, other nations, which may be disposed to adopt a similar proceeding, should be invited to a direct comparison of the standards and measurements of the respective countries, not only for the more perfect assurance of accuracy, but in order that the places may be multiplied on the globe, at which the British measures may be hereafter reproducible.

We perceive that we have already attained our limits in the examination of the subjects contained in little more than half the work before us: the remainder consists of geographical, hydrographical, and magnetic notices of great interest, particularly the latter: the subjects however are distinct, and require in fact to be treated of separately; we shall not, therefore, however worthy they may be of notice, trespass further on the patience of our readers; but shall conclude with recommending its perusal to all persons who take an interest in such investigations, as one of the ablest works with which we are acquainted.

III. *Remarks on Professor* SPOHN'S ESSAY *De* LINGUA *et* LITERIS VETERUM ÆGYPTIORUM, *edited by Professor Seyffarth*, 4to. *Leipzig*, 1825. *In a Letter to Baron* WILLIAM *von* HUMBOLDT.

MY DEAR SIR,

I HAVE to thank you for the favour of your letter sent me by Mr. Struve: I have delayed making this acknowledgment, until I could return you some answer on the subject of Mr. Spohn, whose posthumous work you mention as having engaged your attention. We might suppose it to be almost impossible that a man possessed of any talents should spend some years of his life in a field of literature not wholly barren, without obtaining some few fruits of his labour, which had escaped the researches of others; but I have looked in vain for any one addition to what even Mr. Akerblad had made out, more than thirty years ago, that can justify the pomp and ceremony with which Professor Seyffarth's Prodromus is issued into the world.

The most satisfactory evidence on this subject is that of the papyrus of Casati, which I discovered to be the original of Mr. Grey's Greek antigraph, a little *after* I had printed, and distributed among a few friends, my attempt to translate some parts of the original, which appeared in the *Philosophical Journal* for January, 1823. You will find in it Nebonen*chus* as a proper name, twice over; Apollonius, Antimachus, and Antigenes: the three last having been read nearly in the same manner by Champollion. There is also a phrase, *et liberis ejus*, *hominibus ejus*, frequently repeated.

Of these, Professor Spohn has made out the letters *nebonen*, and *etplonies*, without marking them as proper names; and he has put down Antimaus and Antigenes as a part of his translation: but he has not attempted any explanation of the phrase, which is repeatedly rendered in the antigraph, *with his children and all his family*, nor has he rightly translated a single word besides, after the preamble, which is not in the Greek.

With respect to his mode of reading the words, by an alphabet, which, the newspapers tell us, is like the Armenian, this manuscript affords an undeniable criterion of its accuracy, as it consists almost entirely of proper names, originally Egyptian, not one of which has been read by Professor Spohn in any way at all approaching to the truth. For example, instead of Maesis Mirsios, he gives us Eumolme Nnelleme; for Peteutemis Arsiesios, Ischre pepo eepô nenee; and for Petearpocrates Hori, Nearschneoe hne. If his Egyptian dedication to the King of Saxony is equally happy with these specimens, it may happen to pass current in the other world for an address to Sesostris or to Osiris himself, or for a confession of faith in all the gods and goddesses of Ombos and of Tentyra; and thus to have procured him admission into the blessed communion of those deified Egyptian kings, who are occasionally represented, according to Mr. Bankes's drawings, as *offering sacrifices to themselves*.

London, 22 *Sept.* 1825. * * *

Art. XVI. MISCELLANEOUS INTELLIGENCE.

I. Mechanical Science.

1. Dr. Black's *Sensible Balance*.—The following description of a very delicate and, to many it may be, very useful balance, is taken from a letter written by Dr. Black, to James Smithson, Esq., and inserted in the *Annals of Philosophy*, N. S. x. 52. "The apparatus I use for weighing small globules of metals, or the like, is as follows: A thin piece of fir-wood, not thicker than a shilling, and a foot long, 3-10ths of an inch broad at the middle, and $1\frac{1}{2}$ tenths at each end, is divided by transverse lines into 20 parts, *i. e.* ten parts on each side of the middle. These are the principal divisions, and each of them is subdivided into halves and quarters. Across the middle is fixed one of the smallest needles I could procure, to serve as an axis, and it is fixed in its place by means of a little sealing-wax. The numerations of the divisions is from the middle to each end of the beam. The fulcrum is a bit of plate-brass, the middle of which lies flat on my table when I use the balance, and the two ends are bent up to a right angle, so as to stand upright. These two ends are ground at the same time on a flat hone, that the extreme surfaces of them may be in the same plane; and their distance is such that the needle, when laid across them, rests on them at a small distance from the sides of the beam. They rise above the surface of the table only one and a half or two-tenths of an inch, so that the beam is very limited in its play.

"The weights I use are one globule of gold, which weighs one grain, and two or three others which weigh one-tenth of a grain each; and also a number of small rings of fine brass wire, made in the manner first mentioned by Mr. Lewis, by appending a weight to the wire, and coiling it with the tension of that weight round a thicker brass wire in a close spiral, after which the extremity of the spiral being tied hard with waxed thread, I put the covered wire in a vice, and applying a sharp knife, which is struck with a hammer, I cut through a great number of the coils at one stroke, and find them as exactly equal to one another as can be desired. Those I use happen to be the one-thirtieth part of a grain each, or 300 of them weigh ten grains; but I have others much lighter.

"You will perceive that by means of these weights, placed on differents parts of the beam, I can learn the weight of any little mass, from one grain, or a little more, to the $\frac{1}{1200}$ of a grain. For if the thing to be weighed weighs one grain, it will, when placed on one extremity of the beam, counterpoise the large gold weight at the other extremity. If it weighs half a grain, it will counterpoise the heavy gold weight at five; if it weighs 6-10ths of a grain, you must place the heavy gold weight at five, and one of the lighter ones at the extremity to counterpoise it; and if it weighs only 1, or 2, or 3, or 4-100ths of a grain, it will be counterpoised by one of the small gold weights placed at the first, or second, or third, or fourth division. If, on the contrary, it weigh one grain and a fraction, it will be counterpoised by the heavy gold weight at the extremity, and one or more of the lighter ones placed in some other part of the beam.

"This beam has served me hitherto for every purpose; but had I occasion for a more delicate one, I could make it easily by taking a much thinner and lighter slip of wood, and grinding the needle to give it an edge. It would also be easy to make it carry small scales of paper for particular purposes."

Mr. Smithson observes, that the rings, or small weights, mentioned above, have the defect of their weight being entirely accidental, and consequently most times very inconvenient fractions of grains, and recommends instead that the weight of a certain length of wire be ascertained, and then the length of it taken, which corresponds to the weight wanted; when fine wire is used, a set of small weights may thus be made with great accuracy and ease. This is a process, the value of which is well known to the philosophical instrument maker.

2. *Tenacity of Iron, as applicable to Chain-Bridges.*—The following results have been deduced from experiments made in Russia, and detailed by M. Lamb, in a letter from Petersburgh, *Ann. des Mines*, x. 311. In the apparatus contrived for the purpose the power was applied by a hydraulic press.

The best iron tried supported 26 tons per square inch, without being torn asunder. The bars began to lengthen sensibly when two-thirds of this power had been applied, and the elongation appeared to increase in a geometrical ratio with arithmetical increments of power. The worst iron tried, gave way under a tension of fourteen tons to the square inch of section, and did not lengthen sensibly before rupture. By forging four bars of iron of medium quality together, an iron was obtained which did not begin to lengthen until sixteen tons had been applied, and supporting a weight of twenty-four tons without breaking.

Taking these results as sufficient data, it was decided by the committee appointed for the purpose, that the thickness of chains in a suspension bridge should be calculated so that the maximum weight to be borne should not exceed eight tons per square inch of sectional surface, and that before being used they should be subjected to a tension of sixteen tons per square inch, and bear it without any sensible elongation.

3. *Moving Rocks of Salisbury.*—In consequence of the interest

attached in America to the phenomena of moving rocks, described in a former page of the *Journal* *, Mr. C. A. Lee, who first called attention to them, was induced to examine them more minutely, and by noticing their situation and appearance accurately both before and after winter weather, ascertain decidedly the cause of their transportation. He says, " being fully convinced that the rocks were moved by the agency of the ice, in the month of December 1823, I took the distance of one of the largest to a tree on the shore. In the month of January 1824, there were several very cold nights, during which the ice was heard to roar not unlike the discharge of a cannon. I visited the spot immediately after, and was no longer in doubt respecting the true cause of the movement of the rocks. On most of them the ice was piled up several feet in height, projecting from the side of the rock next to the main body of the ice, towards the shore. Some which did not oppose so strong a resistance were evidently displaced, and the one in particular which I measured was moved several inches, although very firmly fixed in the stones and gravel. During the past winter the rocks have moved but very little, owing to the mildness of the season. From Dec. 1823, to Feb. 1825, the rock above mentioned has moved two feet and a half, which is much less than in former years, for the same reason; besides, it has now become more deeply imbedded in the gravel, and the full force of the expanding ice is not exerted upon it."

Mr. Lee, who dates from Salisbury, says, that since the first notice taken of them in 1822, the effect and the cause have been recognised in many places, and by many persons, and that no doubt now exists as to either. In the mountain pond of Salisbury, the rocks within reach of the ice are annually moved towards the shore, and have formed an artificial dyke of considerable extent. He objects to Mr. Wood's explication founded upon the carrying power of the ice, and states correctly that the ice generally melts *first* around the rocks; which are in this way soon loosened from it on the occurrence of a thaw.—Silliman's *Journ.* ix. 239.

4. *Etruscan Vases.*—The following are the conclusions arrived at by Professor Hausman, during an inquiry into the composition of these vases: 1. That the manufacture of earthen vases, appropriated to funeral occasions, had been widely propagated at a remote period of antiquity, with little deviation from a general plan, in so far as regards their principal circumstances. 2. That these vases have been formed with much particular diversity in regard to less important circumstances, such as the quality of the clay employed, and differences in the forms, ornaments, and paintings, not only in different countries and at different times, but also in

* Vol. xix. p. 368.

the same countries and at the same period. 3. That the finer sort of these vases are superior in regard to the preparation of the clay, and the elegance and variety of the forms, as well as the care of the paintings, to all others of the kind, whether of Roman or of modern manufacture, insomuch that the pottery of the most remote ages forms the model of that of the present times. 4. That the art of manufacturing these vases, as practised in very remote times, is much more worthy of estimation than our best performances in that way, since the ancients were not in possession of many assistances which are applied to the art by us; and because some things which are now done without difficulty, by means of certain instruments or machinery, were, in those times, perfected by means of the hand alone, by the greater dexterity of the artists. 5. That certain circumstances were peculiar to the very ancient arts of making and ornamenting those earthen vessels which have evidently been lost in later times, of which may be mentioned in particular the composition of a very thin varnish, which gave a heightening to the colour of the clay in a greater or less degree, and afforded a very thin firm black coating, retaining its lustre to the most remote ages, and capable of resisting the action of acids and other fluids; so that the modern art of manufacturing pottery ware may be materially improved, not only with regard to the forms and ornaments, but also the preparation and application of the materials, by a diligent and continued examination of those very ancient vases.—*Edin. Phil. Journ.* xiii. 62.

5. *On the Repulsion exerted by Heated Bodies at sensible Distances. By* M. A. Fresnel.—M. Libri published last year, in an Italian Journal, some curious experiments on the motions of a drop of fluid suspended on a metallic wire, of which one extremity was heated: he observed that the drop always receded from the source of heat, even when a very sensible inclination was given to the wire. This phenomenon may be explained by the changes in the capillary action of the solid surface and the liquid, caused by the elevation of temperature, and which will be different at the unequally heated extremities of the drop. It may also be admitted (which is the same thing) that the molecules repel each other more powerfully as their temperature is higher. According to this hypothesis, each liquid molecule in contact with the metallic wire will be more repelled by the small portion of surface on the side towards the source of heat, than by the contiguous portions, from which would result a sum of many small actions, all tending to impel the drop from the heated extremity.

In neither of these methods of viewing the phenomenon is it necessary to suppose that the reciprocal action of the molecules extends to sensible distances; but some other experiments of M. Libri on the same subject appears, as he has observed, to indicate repulsion at a distance. Nevertheless, I dare not affirm that they establish this mode of action, though I have observed its existence in another manner, because the calorific repulsions for intervals of some millimetres are so feeble that I can hardly believe them capable of overcoming the friction of the drop of liquid on the wire.

I had uselessly endeavoured, for a long time, for the verification of certain hypotheses, to move a small disc of foil attached to the extremity of a very light horizontal stem, supported by a thread of silk in vacuo, by the action of the solar rays collected together by a lens. Since then I proposed to try whether this mobile disc would not be repelled by a heated body brought near to it; but I should no doubt have delayed the execution of this project, if M. Libri had not communicated to me his interesting observations. They, by inducing me to consider the success as probable, caused me the sooner to make the experiment.

For its convenient performance a very fine steel wire, magnetized, and suspended by a silk fibre, had attached to its extremities a disc of foil, and a disc cut from a plate of mica, for the purpose of trying an opaque and a transparent body in the same apparatus: the fixed body, intended to repel the balance of torsion was also a disc of foil. A vacuum was carefully made within the glass jar which enclosed the apparatus; the elasticity of the remaining air indicated by the mercurial gauge was not more than one or two millimetres. The jar was then placed in the sun's rays, and so turned that the magnetized steel wire was but little out of the magnetic meridian, yet sufficient to cause one of the discs fixed at its extremity to exert a very slight pressure against the fixed disc, so that it should remain in contact with it. The apparatus thus arranged, I threw the sun's rays by a lens, sometimes on the fixed disc, sometimes on the moveable one, and immediately the latter separated quickly from the former. I retained it separate, and sometimes at the distance of a centimetre, (0.39371 of an inch,) by continuing to heat the discs. When I removed the lens, the balance of torsion did not return immediately to the fixed body, but gradually approached it, performing small oscillations. It is very probable that if I had employed thicker bodies, and such as would cool more slowly, the return to the original position would have been more gradual.

It seemed to me as if the transparent disc was not so strongly repelled as the disc of foil. I observed also that the most advantageous manner of heating the bodies, so as to retain them at the maximum distance, was to send the focus of the lens on to one of the opposing surfaces. I do not suppose that this effect is due to reflection, but merely to the facility obtained in this way of more highly heating the surface which is to exert the repulsive action.

That I might be assured these phenomena were not occasioned by the small quantity of air or vapour remaining in the bell-glass, I let the air re-enter gradually; and on repeating the experiment when the internal air was fifteen or twenty times denser than at the commencement, I found that the repulsion had not sensibly augmented in energy, as should have happened had it been occasioned by the motion of the heated air. There were, indeed, certain positions of the mobile disc relative to the fixed one in which the divergence was not so great as in vacuo.

I tried whether the interposition of an opaque screen, composed of two plates of foil separated by a small interval, intercepted the repulsive action of the fixed disc on the mobile one when either of them were heated; it appeared to me that the screen prevented repulsion. But does it entirely intercept the action? is a question difficult to answer in this way; for the interposition of the screen so that the heat should not be too rapidly communicated to it, includes the necessity of a considerable interval between the fixed and mobile discs.

In consequence of the directive force which tends to replace the steel wire in the magnetic meridian, the apparatus I have described will serve to measure the calorific repulsion of two bodies at different distances. With it also may be made other interesting experiments. I should have been desirous that this note presented such results that it might have been more worthy of presentation to the Academy; but the experiments require time, and are laborious, inasmuch as the vacuum has to be re-formed each time the apparatus is changed. I hope that philosophers, more expert, or more at leisure, will not disdain to join in these researches, which promise new and curious results, and may, perhaps, throw light on the theory of the dilatation of bodies by heat.

P. S. To complete this note I should add my reply to the objection of an illustrious geometer, who inquired if I was certain that the phenomena of repulsion which I had described to the Academy were not due to electricity developed by the heat. In my apparatus the metallic stem of the fixed disc communicated with the earth by the copper tube which passed through the glass plate on which the jar stood; so that if by throwing the focus of solar rays on to the mobile disc it had been rendered electrical, it would always have been attracted instead of repelled by the fixed disc.

Neither can we suppose it more probable that the phenomena depend on a magnetic action; for if by throwing the focus on to the fixed disc it became magnetic, it would certainly have repelled one of the extremities of the steel wire; but it would have attracted the other; whilst, in fact, it equally repelled the two generally: a *constant* repulsion in varied and even opposed circumstances excludes the supposition of an electric or magnetic action.

On repeating the above experiments with thicker discs, it did not appear that the repulsive force was sensibly augmented. If this observation be supposed correct, and the temperatures equal in the two cases, it may be concluded that the force which causes deviation of the magnetic needle depends upon the extent of surface only, and does not emanate from all the particles comprised in the thickness of the heated disc. By trying bodies of different kinds, and especially those which are transparent, and varying also in thickness, it will, perhaps, be possible to determine to what degree they intercept the repulsive action, arising from elevation of temperature.

"When the mobile disc is of some thickness, and its external surface is heated, it often happens that it remains for a long time in contact with the fixed disc, and separates from it on withdrawing the lens. This is probably occasioned by the great difference of temperature between the two surfaces of the mobile disc, from which it may result that the surface receiving the solar rays, is as much repelled by the surface of the bell-glass, as the other surface is by the fixed disc. This I offer only as a doubtful explanation, not having had time to verify it by new experiments.

M. Fresnel afterwards says, "new experiments have shewn me that the explanation at the end of the note, of the particular phenomena of thick discs cannot be admitted; for in that case the face of the mobile disc heated by the sun's rays would suffer a sensible repulsion from the neighbouring surface of the bell-glass, and by throwing the focus of the lens on to the moveable disc away from the fixed one, the needle should be deviated; this, however, does not happen.

With pieces of copper of a hundredth part suspended from the extremities of the magnetic steel wire, I have obtained very apparent effects of attraction. When the solar rays were thrown on the exterior face of the mobile disc near to the fixed disc, it approached and adhered to it as if attracted. This attraction was not occasioned by a developement of electricity, for the solar rays, reunited on the other mobile disc, produced no sensible effects, though the two suspended discs were connected by the steel wire.

I have observed actions similar to these in many other circumstances, but I have as yet too slightly studied these singular phenomena to give a general and exact description; I can only say that the experiments I have thus far made confirm me in the opinion that both the attractions and repulsions, produced by heat, do not arise from the developement of *electric tension;* and if they belong to a momentary state of emanation of the heated discs, it appears to me at least that the distribution of magnetism here follows particular laws.—*Ann. de Chimie*, xxix. 57, 107.

6. *Polarized State of Halo Light.*—M. Arago announced to the Academy, that upon examining a halo round the sun towards eleven o'clock in the morning, with an instrument of his inven-

tion, he remarked very unequivocal traces of polarization by refraction in the light of which the halo was formed. This experiment excludes all explications of the phenomenon founded upon the hypothesis of a reflexion. M. Arago thinks that the instrument he made use of in this observation will enable him more generally to ascertain when a cloud is frozen, and that it will then supply the means of studying the law of the diminution of heat in the atmosphere.—*Ann. de Chimie*, xxix. 77.

7. *Nature of Shooting Stars seen during Day-Time.*—An account by Professor Hanstein of a shooting-star seen in the day-time, has recently attracted some little attention*. Mr. Dick, however, doubts the assigned nature of the appearances, and states reasons for concluding it to be nothing more than a bird. Whilst making observations twelve years ago on Venus, when close to the sun, he, whilst looking for the planet, remarked a body passing across the field of the telescope, apparently of the size of Venus, but varying a little in this respect; at first it was mistaken for the planet, but its rapid motion corrected the error. In some instances four or five of these bodies appeared to cross the field of view, sometimes in a perpendicular, and at other times in a horizontal direction. They appeared to be luminous bodies somewhat resembling the appearance of a planet, when viewed in the daytime with a telescope of moderate power. Their motion was rapid, and inclined to a waving or serpentine form. After twelve months' observation, Mr. Dick was enabled, by observation of some which were larger than others, to decide they were *birds*, whose bodies, illuminated by the solar rays, reflected light enough to produce the appearance. In a hot summer's day, when a similar phenomenon has been observed, there was every reason to attribute it to a number of winged instruments flying at no great distance from the telescope.

Mr. Dick observes that Professor Hanstein's account of the kind of motion as being unequal, and resembling that of a rocket, corresponds to the motion of birds through the air. He remarks too, that an appearance observed by the late Mr. B. Martin, of certain bright round bodies running towards the sun, when viewed in particular circumstances, may be explained in the same manner.—*Edin. Phil. Journal*, xiii. 167.

8. *Astronomical Prize Question.*—" Method of calculating perturbations of the elliptical motion of comets, applied to the determination of the approaching return of the comet of 1759, and to the motion of that observed in 1805, 1819, and 1822." The prize a gold medal of 3000 francs value. Memoires received till Jan. 1, 1826.—*Royal Academy of Sciences, Paris.*

* *Quarterly Journal*, Vol. xix. p. 369.

9. *Prize Question; Natural Philosophy.*—1. "To determine by numerous experiments the density acquired by liquids, and especially mercury, water, alcohol, and sulphuric ether, by pressures equivalent to the weight of many atmospheres.—2. To measure the effects of heat produced by these compressions." The prize a gold medal of three thousand francs value. Memoires received till Jan. 1, 1826. *Royal Academy of Sciences, Paris.*

II. Chemical Science.

1. *On the Dry Voltaic Piles of* M. Zamboni.—The following is part of a report made by M. Ampere, on a memoir relative to the above voltaic combinations.

The energy of these dry piles ceases to diminish after two years; such, at least, M. Zamboni finds to be the case during twelve years' experience.

The diminution in the two first years varies according to the manner in which the pile is constructed.

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

The tinned paper, called *silvered paper*, with black oxide of manganese, developes an electric force very superior to that obtained when the paper is covered with a thin leaf of copper; the latter is known under the name of *gilt paper*, (Dutch gold paper.)

A pile formed of discs of paper, tinned on one side, without any interposing substance, produces electrical effects, which can result only from the circumstance that the metallic leaf, glued to the upper surface of the paper, touches it more intimately than it does the lower surface of the paper belonging to the element next placed above.

M. Zamboni has examined whether in those piles, which he calls *binary*, the action of the elements takes place as in those which are composed of leaves of tin, covered with oxide of manganese, or in the reverse order. He found that one or the other of these results could be obtained at pleasure, by imbibing the paper attached to the tin, with various substances. When oil was used the action was opposed to that produced by oxide of manganese; when, on the contrary, the paper was imbibed with honey or alkali, a solution of sulphate of zinc, or milk in a semi-coagulated state, the binary pile acts like those composed of elements powdered with oxide of manganese.

By using a dry pile of 1000 pairs, the plates not being more than five or six centimetres, (from two to two and one-third of an inch,) in diameter, M. Zamboni obtained by the condenser sparks of an inch in length, so that with such a pile an electric battery might be retained, constantly charged to a state of tension, which might be heightened at pleasure, by increasing the number of plates.

M. Zamboni thinks that a pile of 50,000 pairs of plates, of the usual diameter of leaves of tinned paper, would be a *constant* source of electricity, of which the tension would equal that of a strong common electric machine. He promises that such an instrument shall be constructed, and mentions many interesting experiments to which it may be applied.—*Ann. de Chim.* xxix. 198.

2. *New Galvanometer, by* Nobili.—The construction of this instrument is founded upon the fact discovered by Œrsted, the deviation of a magnetic needle by a wire conveying a current of electricity; and as in most other instruments of this kind, the wire is passed several times round the frame, within which the needle is suspended, that the effect may be proportionally increased. It differs, however, from all made before it, in the use of two needles instead of one; these are equal in size, parallel to each other, magnetized in opposite directions, and fixed on a straw, so that the contrary ends of the two needles point in the same direction. Their distance from each other on the straw is regulated by the construction of the frame with its covering wire, in and about which they are to move. The frame of M. Nobili is twenty-two lines long, twelve wide, and six high. The wire is of copper covered with silk, it is one-fifth of a line in thickness, from twenty-nine to thirty feet long. It makes seventy-two revolutions about the frame. The needles are twenty-two lines long, three lines wide, a quarter of a line thick, and they are placed on the straw five lines apart from each other. An aperture is made in the tissue formed by the turns of the wire on the upper surface of the galvanometer, by thrusting them from the middle towards each side; the lower needle on the straw is introduced through this aperture into the interior, in consequence of which the upper needle remains a little above the upper surface of the wire. The aperture is retained open to a certain extent, to allow freedom of motion to the needles and straw, these being suspended in the usual way from the upper extremity of the straw. The graduated circle on which the deviation is measured is placed over the wire on the upper surface of the frame having an aperture in its centre for the free passage of the needle and straw. The upper needle is the index, the lower being visible only from the sides of the instrument.

The sensibility of this instrument depends upon the addition of the upper needle. Being magnetized in an opposite direction to the lower one, it almost entirely neutralizes the influence of terrestrial magnetism, leaving only so much of directive power as shall induce the whole arrangement to return to a constant position when uninfluenced by electrical currents, and yet combining with the lower needle, to cause deflexion when an electrical current is passing through the wire.

As an illustration of the delicacy of the instrument M. Nobili observes, that it is well known if Seebeck's combination of antimony and bismuth be attached to a common galvanometer, and the point of junction be cooled, only a very slight effect is observed on the instrument; whilst, if attached to the new galvanometer, the same influence is sufficient to make the needles revolve several times. If a piece of iron wire, five or six inches long, be used to connect the extremities of the copper wire of the instrument, by twisting the ends together, and one of the points of contact be warmed by touching it with the hand, the needle will move from 0°, and in the first oscillation extend to 90°. Even the mere approximation of the hand to the junction of the metals will produce a deviation of 20°.

It is necessary for the delicacy of the instrument that the needles used be magnetized as nearly as possible to the same degree, and two indications have been observed as useful in pointing out when this is the case; the first is the position taken up by the plane of the needles, when left to the earth's influence; this should not be in the plane of the magnetic meridian, but more or less inclined to it; the second is the manner in which the system oscillates about its line of equilibrium. These oscillations should be very slow compared with those of a common needle.

In consequence of the situation of the graduated circle above, and not within the frame, the folds of the wire may be brought much nearer to each other than in the common instrument; this renders it more compact, and from the vicinity of the needle within to the wire, also more powerful. When fixing the graduation, the zero should be placed so as to accord with the position of the needles, when left to the earth's influence; this will not be towards the true magnetic north, but will not be far from it, and will always be constant.

M. Nobili then offers a very curious illustration of the powers of the instrument:—"It is known," he says, "that water usually retains itself at a lower temperature than the ambient air, the difference being sometimes two degrees, and resulting from the evaporation of the liquid. If a bar of bismuth be made to join the two extremities of the galvanometer wire, and one of the points of junction be plunged into a cup of water, the needle will immediately deviate several degrees, proving that the instrument is capable of measuring the small degree of refrigeration, produced by the evaporation of the liquid. I have actually submitted one of my galvanometers for fifteen days to an experiment of this kind, the deviation was about 15° in the morning and evening, but more considerable in the course of the day. This first attempt has made me suppose that the galvanometer might become, in the hands of an attentive and skilful philosopher, a kind of *atmidometer.* If by means of a single couple of two different metals, bismuth and copper, a deviation of 15° has been obtained, a much greater one would be produced by employing several pairs, conveniently immersed in the same vessel of water; and, perhaps, one might succeed by increasing the scale of observation, in ascertaining more exactly the diurnal rate of evaporation. I propose, also, to ascertain the effect of a current of air, excited by any means over the surface of the water used in the experiment; it would, without doubt, augment the evaporation, and by increasing the difference between the temperature of the air and the water, increase the effect on the instrument.—*Bib. Univ.* xxix. 119.

3. *On the Length of the Electric Flash producing Lightning. By M. Gay Lussac.*—The length of the flash during storms is always very great, and one may readily ascertain, in a mountainous country, that it frequently exceeds a league. This extraordinary length, and the awful sound produced by the flash, induces us naturally to admit, that the quantity of electricity which forms it is incomparably greater than that which may be accumulated in the largest electric batteries. We cannot produce explosion except at the distance of a few centimetres, (an inch or two,) and the intensity which we must suppose is required in batteries to make an explosion at the distance of a few metres, (or a few yards,) only, would be so great as to make it impossible it could be retained on a coated surface by the pressure of the air. On the other side, when lightning falls on a lightning-rod, it frequently happens that only a small portion of the point, perhaps three or four millimetres, (0.12 to 0.16 of inch,) is fused; and this effect is not very different to what may be produced by large electrical batteries.

But we cannot really judge of the intensity of electricity accumulated on our conductors, and on a thunder-cloud by the length of the spark. The electricity is retained on our conductors by the pressure of the air, the spark only occurs when this pressure can be overcome by the electricity. On the contrary, the electricity is retained on a cloud only by the resistance it affords to it as a non-conducting body, and equally pressed as it is by this fluid which surrounds it on all sides, it should obey the slightest attractive or repulsive forces by which it is affected. We may therefore conceive, that as soon as the electricity has formed a stratum, no matter how attenuated, so that it be continuous, the flash may occur and pass through considerable distances. The intensity of the flash will be produced by the quantity of electricity contained in the immense stratum enveloping the cloud. If the stratum is not continuous, which is very possible in so bad a conductor as a cloud, or if all the electricity spread over the space occupied by the cloud has not had time to disengage itself, so as to arrive at the surface of the cloud, the discharge will only be partial, and then the redoubled peals of thunder will easily be understood. It appears impossible to us, according to these observations, that the thickness of the electric stratum can ever be any thing like so great on the surface of a thunder cloud as on that of a solid conductor; for the repulsion of its molecules would dissipate it in the air. We perceive nothing to retain it but the resistance of the air as a non-conductor, and that resistance can be but very small.

As the primitive electricity spread over the space occupied by a thunder-cloud can unite but very slowly into a thin stratum, it becomes difficult, according to the theory of Volta, to attribute to it the formation of hail in particles as large as those which are sometimes observed; the phenomenon, however, is certainly connected with atmospheric electricity; and though we are not acquainted with all the circumstances which would enable us to comprehend it, we must not reject a cause because it appears to us not to have an intensity proportional to the effects we would explain. —*Ann. de Chim.* xxix. 105.

4. *On the Existence of Iodine in a Mineral Substance. By* M. Vauquelin.—The mineral in which M. Vauquelin has, for the first time, found this peculiar substance, was brought by M. Joseph Tabary from the neighbourhood of Mexico, and was labelled *virgin silver in serpentine.* It was of a whitish colour on its rubbed surface, presenting grains of metallic silver; its fracture was lamellated, and of a yellowish green colour, with some black portions and metallic silver. Twenty parts of the substance were acted upon by nitric acid with effervescence; being boiled with it for some time, and then diluted, two insoluble portions appeared; one very heavy, and falling instantly, whilst the other was light, and remained in suspension. When separated and washed, the first weighed 6.42 parts; it fused easily by the blow-pipe, producing a purple flame, and ultimately a small globule of silver appeared in the centre of a fused mass like chloride of lead. The edges of the charcoal were covered with a yellow powder. The lighter matter was brown, and weighed 2.7 parts; it burnt, producing sulphurous acid, and leaving sulphuret of lead with a little iron=1.58 parts.

A portion of the first matter, heated with muriatic acid, gave a red brown colour, and produced slight effervescence with the odour of chlorine. As the temperature rose the effervescence increased, and a beautiful violet colour was developed, in consequence of which the vessel was removed from the fire. There remained at the bottom of the acid a yellow substance, containing grey particles, which were dissolved by the hot water used for washing. This water had acquired a brown colour, and the power of colouring solution of starch of a fine blue. After many washings with water, alcohol was used, which in its turn became deeply coloured, and rendered a solution of starch of blue colour.

In consequence of these appearances the muriatic solution was diluted and distilled, when violet vapour arose, and crystals of iodine condensed in the vessel. Though the yellow matter had boiled some moments with the muriatic acid, it still contained iodine, for 2.38 parts fused with two parts of potash, the residue dissolved in water, saturated with sulphuric acid, and mixed with starch, gave, with a few drops of chlorine, a fine blue colour; 1.63 grains of metallic silver were left.

Five parts of the mineral were then mixed with two parts of caustic potash, and heated to redness for some time, after which treated with water 4.46 parts were left; these acted upon by nitric acid dissolved without effervescence, leaving a yellow substance resembling chloride of silver; when dried it weighed 0.8, and was ascertained to be iodide of silver; it gave 0.415 of silver to nitric acid.

Hence it appears that as the potash had taken 0.5 parts from the five originally used, which were iodine, and that 0.8 of iodide of silver were formed, which would contain 0.425 iodine, the whole quantity of iodine was 0.925, which divided by 5=0.185, or 18.5 per cent. in the mineral.

The alkaline solution before mentioned, saturated by nitric acid, became yellow; and added to solution of starch, with a little chlorine rendered it blue. Nitrate of mercury precipitated it red. A portion of it neutralized by sulphuric acid evaporated to dryness, digested in alcohol, and the alcoholic solution evaporated, gave quadrangular crystals of hydriodate of potash.

Besides iodine and silver, the mineral contained sulphur, lead, and carbonate of lime. M. Vauquelin considers it as probable, that the sulphur is combined with the lead and silver, and the iodine with a part of the silver. In confirmation of this it is said, that when boiled with ammonia for some time, iodide of silver is separated from it. This, however, is against the generally-received opinion, that the iodide of silver is insoluble in ammonia.—*Ann. de Chim.* xxxix. 99.

5. *Selenium in the Sulphur of the Lipari Islands.*—Amongst the volcanic productions of the Lipari Islands is a sal-ammoniac, with sulphur in alternate white and brownish orange layers. The colour of the latter has generally been attributed to iron, but the

usual tests gave no indications of that metal; arsenious acid, however, being detected. On dissolving the sal-ammoniac in water, a brownish residuum was left, which fused readily in a glass tube, and gave an orange-coloured sublimate. On hot coals it inflamed, evolving at first a mixed odour of sulphur and arsenic, and then the offensive smell of selenium. By digestion in nitric acid, till the orange colour disappeared, a solution was obtained, which, with sulphite of potash, threw down much of a cinnabar-coloured precipitate, possessing all the characters of selenium, whilst the solution evaporated gave acicular crystals of selenic acid.

This discovery by M. Stromeyer of selenium amongst the volcanic products of the Lipari Islands, renders it probable that the peculiar orange tint of the sulphur, found in those islands, proceeds chiefly from selenium, and not, as hitherto supposed, from arsenic combined with the sulphur.—*Ann. Phil.* N. S. x. 234.

6. *Natural Sources of Carbonic Acid Gas.*—Bischoff and Nöggerath, in Schweigger's Journal, mention a pit on the side of the Lake of Laach, in which they found many dead animals, as birds of different kinds, squirrels, bats, frogs, toads, and also insects. On descending into the pit, and gradually sinking the head, they experienced the same sensation as when held over a vat in a state of fermentation. The quantity of gas evolved varies at different times. This evolution of carbonic acid gas is more striking in the volcanic Eifel. On the right bank of the river Kyll, nearly opposite to Birresborn, there is a spring named Brudelreis; a provincial name for a boiling spring, and applied to this because it is perpetually agitated by large bubbles of gas, the agitation being so great as to produce a noise heard four hundred yards off. In its vicinity numerous dead birds are found, killed by the carbonic acid rising from the water; and persons who kneel to drink at the spring are driven back by the gas. As MM. Bischoff and Nöggerath approached this spring, they heard the noise of its ebullition at a considerable distance, and by approaching their faces to the surface of the turf in the vicinity of the spring, found that it was covered with a layer of carbonic acid gas. They did not observe any deleterious effects produced on the surrounding trees or grass. On emptying the basin no more water was collected, shewing that it was rain, not spring water; but the gas continued to rise through the fissures of the rock in some places, with such force as to feel to the hand like wind from a bellows. Lime-water poured into one of the fissures became turbid, and caused the appearance of ebullition again, but it was not ascertained whether the gas was pure carbonic acid or not.—*Edin. Phil. Jour.* xiii. 191.

7. *Process for the Detection of Phosphate of Lime.*—A process

is given, as one recommended by MM. Vauquelin and Thenard, for the detection of phosphate of lime, founded upon its conversion by potassium into a phosphuret, and the production of phosphuretted hydrogen, either with water or acids, by the latter body. The gas is recognised by its well-known odour, and indicates the presence of a phosphate in the matter originally used. The decomposition is to be effected in a glass tube, 3 or 4 millimetres (0.15 inch) in diameter, and about 4 centimetres (1.5 inches) long; a centigramme (0.15 grain) of potassium is to be placed at the bottom, and the substance supposed to contain the phosphate in powder is to be pressed down upon it. The tube is then to be gradually heated, the potassium sublimed through the substance, and, when cold, the excess of potassium removed by the introduction of mercury. The matter remaining, when exposed to a moist air, or when touched by muriatic acid, will evolve the odour of phosphuretted hydrogen, if any phosphate were present; or a little diluted acid may be introduced into the tube, and the gas evolved obtained. Of course, any sulphate which may be present must be removed, or the odour of sulphuretted hydrogen would seriously interfere with the delicacy of the test.—*Jour. de Chim. Méd. Jan.* 1825.

8. *Metallic Titanium in Iron Furnaces.*—Cubic crystals of metallic titanium, similar to those discovered by Dr. Wollaston in the iron-furnaces of South Wales, have also been found by Dr. Walchner, of Friburg, in the Breisgau, in the founderies of the highlands of Baden. The piece of slag examined was from the high furnace of Kandern, in which *pea-iron ore* only is smelted. Being desirous of ascertaining the presence of the titanium in the pea-iron ore, an attempt was made with the blow-pipe, and its presence, Dr. Walchner says, indicated, though in very small quantity.—*Phil. Mag.* lxvi. 124.

9. Rose *on the Separation of Titanic Acid from Oxide of Iron.*—The difficulty of separating titanic acid from oxide of iron, is well known to chemists, no process but what is very imperfect being as yet known. M. Rose, who has had frequent occasion to combat this difficulty, has discovered and published a method which not only renders analytical processes more perfect, but very much facilitates the preparation of titanic acid from its more abundant natural compounds.

A solution of titanic acid and oxide of iron being obtained in muriatic acid, if tartaric acid be added to it, and the whole be diluted with water, then a great excess of caustic ammonia may be added without the smallest precipitate of titanic acid or oxide of iron being produced. If to this solution hydrosulphuret of ammonia be added, it exerts no action on the titanic acid, but changes all the oxide of iron into sulphuret, which separates perfectly.

This precipitate is to be carefully washed with water, containing a few drops of hydrosulphuret of ammonia, until all the tartrate is removed; it is then to be dissolved in muriatic acid, heated to drive off the sulphuretted hydrogen, treated with nitric acid to peroxidize the iron, and then precipitated by ammonia: in this way the iron is procured. The titanic acid may be separated from the solution, (if it contains no fixed parts,) by evaporating to dryness, and heating red hot in contact with air, until all that is volatile is dissipated, and the charcoal is burnt off. This is best done in a small platina crucible in a muffle; titanic acid remains.

This method appears to be equally advantageous for the preparation of titanic acid from minerals containing it, combined with protoxide of iron, and which may be dissolved in strong muriatic acid, after having been pulverized. As there is then no occasion carefully to wash the sulphuret of iron, that labour is saved, and the process becomes as short, or shorter, than any other known.—*Ann. de Chim.* xxix. 130.

10. Wohler *on Tungsten, and its Combinations.*—M. Wohler prepares his tungsten by fusing together pulverized wolfram and carbonate of potash; the tungstate of potash is dissolved out by water, muriate of ammonia is added, the whole is evaporated to dryness, and heated red hot in a Hessian crucible. The mass dissolved in hot water leaves a heavy black powder, being the oxide of tungsten. It should be boiled in weak solution of potash, and washed in hot water. It is readily converted into tungstic acid by heating it in an open crucible; it takes fire, and burns vividly into a yellow powder.

Oxide of Tungsten.—Tungstic acid, heated in hydrogen gas, as Berzelius has shewn, becomes first blue, then of a deep brown colour; the substance produced has a lustre almost metallic, and when polished takes the colour of copper. Tungstic acid, in contact with zinc in dilute muriatic acid, also becomes blue, and ultimately forms films of a fine copper-colour. In this state the substance exists as an oxide, and must be retained under water; if exposed to air it becomes blue again, and ultimately yellow tungstic acid. The black powder obtained above appears almost as the metal, when compared with this substance, and when rubbed with a polisher, takes a white metallic lustre. It is, however, only oxide of tungsten, as is shewn by the increase of weight when burnt; it inflames in the air much beneath a red heat, and 100 parts absorb 8 parts of oxygen in becoming tungstic acid, the same quantity as is absorbed by the brown oxide; whilst metallic tungsten requires, for every 100 parts, the addition of 25 parts of oxygen to become tungstic acid.

A singular and, as yet, inexplicable phenomenon, occurs in the preparation of oxide of tungsten from tungstic acid, by hydrogen.

The preparation of a pure tungstic acid, when it has once contained a fixed alkali, is known to be difficult. When an acid containing a little potash or soda is used for the preparation of the brown oxide by hydrogen, this oxide is never obtained, but always *metallic tungsten*, and in this way the metal may be readily procured. It should be washed with pure potash, to dissolve the difficultly soluble tungstate with which it is mixed. *Tungsten* then appears as a white metallic powder, very heavy, which, heated in the air, takes fire, 100 parts absorbing 25 parts of oxygen to become tungstic acid.

Oxide of Tungsten and Soda.—Neutral tungstate of soda, heated in hydrogen, suffers no change; but acid tungstate of soda so treated soon acquires on the surface the colour and lustre of copper, which ultimately propagates through the mass. On cooling, the colour becomes gold yellow, and then water added dissolves neutral tungstate of soda, and leaves a heavy crystalline powder, of the colour and almost the lustre of gold. To purify the powder it should be first boiled in water, then in concentrated muriatic acid, then in solution of pure potash, and ultimately again in water. The acid tungstate of soda is prepared by adding tungstic acid to the neutral salt in a state of fusion, until no more is dissolved.

This metallic-looking substance is a compound of oxide of tungsten and soda. It is crystallized in regular cubes, which are larger as the operation has been more slowly conducted. Cavities frequently occur in the reduced saline mass, lined with very brilliant small cubes. It has a perfect metallic lustre, even when rubbed on paper; its colour is very like that of gold, and, when suspended, as in fine powder in water, and seen before the sun, like gold it is transparent, and of a green colour. No acid, or mixture of acid, except concentrated fluoric acid, will affect it, nor do solutions of pure alkalis change it. Heated in the air, it changes colour, softens, appears to fuse, and forms a transparent mass, which, on cooling, becomes a white enamel, soluble in water, and from which an acid precipitates tungstic acid. The decomposition, however, is never perfect throughout, even in oxygen gas, though combustion then occurs. In a vacuum the compound endures heat without any change. The fusible substance appears to be a tungstate of soda, but it seemed difficult to decide whether the elements of the compound were in the state of metals or oxides.

It was found that, at a high temperature, the substance was affected by chlorine; highly heated in that gas, a chloride of tungsten volatilized, and a green mass remained, which, with water, gave chloride of sodium and a green powder, the latter a mixture of a little oxide of tungsten with tungstic acid; the tungstic acid was in greater quantity than the oxide and chloride together. Hence the compound contained oxygen, which at first appears to have been distributed so as to form oxide of tungsten and soda, and, after the action of the chlorine, to have combined, forming tungstic acid; 873 parts of the compound, decomposed by chlorine, gave 157 chloride of sodium = 89 soda; consequently, 10.6 of soda per cent. in the compound.

Sulphur heated with this body decomposed it entirely, converting the tungsten into sulphuret. This was transformed by nitro-muriatic acid into tungstic acid, 45 parts being obtained from 48.7 of the compound. These correspond to 86.2 per cent. of oxide of tungsten in the compound, the residue = 13.8 being, of course, soda. Hence the compound contains,

		Calcul.	
Oxide of tungsten	4 atoms	87.81	- 86.2
Soda - -	1 ,,	12.19	- 13.8
		100.00	100.0

Attempts were made to produce this compound by directly combining oxide of tungsten with soda; when heated together metallic tungsten and tungstate of soda were produced. When acid tungstate of potash was heated in hydrogen, pure metallic tungsten was obtained.

Chloride of Tungsten.—Sir H. Davy first formed chloride of tungsten. M. Wohler shews the existence of three of these compounds. When *black* oxide of tungsten is heated in chlorine in a tube, combustion takes place, dense fumes are formed, which ultimately produce a thick sublimate of white scales, resembling in appearance native boracic acid; this is the perchloride of tungsten. In the air it gradually becomes tungstic and muriatic acids: the change is more rapid in water. It is volatile at a low temperature, without fusing previously. Heated on platina foil, it is also decomposed into muriatic and tungstic acid. As water converts it thus into muriatic and tungstic acids, it must consist of,

Chlorine	3 atoms	- 35.9
Tungsten	1 ,,	- 64.1
		100.0

166 grains of this compound dissolved in ammonia, evaporated and heated, gave 130 grains of tungstic acid = 62.65 of tungsten for 100 of chloride.

When metallic tungsten is heated in chlorine, it takes fire and burns into a chloride, with a minimum of chlorine. The compound appears sometimes as delicate fine needles, of a deep-red colour resembling wool, but more frequently as a fused deep-red compact mass, with the brilliant fracture of cinnabar. When heated, it fuses, boils, and yields a red vapour. In water it gradually decomposes, producing muriatic acid and oxide of tungsten. This compound dissolves in solution of pure potash, evolving hydrogen, forming chloride of potassium, and tungstate of potash.

Similar effects take place in ammonia. The chloride appears analogous to the oxide, and should be composed of

Chlorine	2	atoms	26.79
Tungsten	1	„	73.21
			100.00

The third compound, on the composition of which no experiment has been made, is generally formed at the same time with the maximum chloride, though in small quantity. It was once produced in larger quantity, by heating the sulphuret of tungsten in chlorine. This is the most beautiful compound of all, existing in long transparent crystals, of a fine red colour; it readily fuses, and on cooling crystallizes in long needles on the glass. It is more volatile than the others; it instantly changes in contact with the air into tungstic acid. Thrown into water it swells like caustic lime, disengages heat, a slight noise is heard, and it is instantly changed into tungstic acid.—*Ann. de Chim.* xxix. 43.

11. *Composition of Ancient Glass.*—A fragment of ancient Roman glass found near Brool, has been analyzed by Dr. Rudolph Brandes, and found to contain silica, soda, oxide of lead, oxide of manganese, oxide of iron, lime, and alumina. The silica formed about two-thirds of the mass. The glass had been so far affected by water and other agents acting upon it for a great length of time, as to have lost its transparency, except towards the centre. It had a milky white colour, with a bluish cast, and in some parts a lustre very similar to that of gold. This resulted from the thin plates into which the glass had disintegrated, and which caused it when broken, pressed, or scraped, to fall into small leaves like mica.

12. *Action of Lime upon Alcohol.*—The following experiment is one made by Dr. Menici, and described in the *Giornale di Fisica*, viii. 50. Two portions of alcohol, of three ounces each, the one being at 35° B. (s. g. 842,) and the other at 28° B. (s. g. 880,) were put into separate bottles, and to each was added three denari (about 3.5 dwts.) of caustic lime. The bottles were closed up and left for four months. At the end of that time the liquor in the second bottle had assumed a yellow colour, which, in two months more, deepened to a red. Being then opened, it was found to have a peculiar aromatic odour; by distillation unchanged alcohol came over from the clear solution, and a residue was left, which, when dry, weighed about a denaro, and resembled a red resin; it softened by heat, and burnt with a bright flame and much smoke. The stronger alcohol, on the contrary, had acquired no tint like that of the portion just described, but slowly took a light bluish tint. Hence it appears that, in contradiction

to the received notion, diluted alcohol is more readily acted upon and changed by lime than that which, by concentration, has been deprived of a part or the whole of its water.

13. *Melaina, or the Black Principle of Sepia.*—M. Bizio, during a chemical investigation of the ink of Sepia, has found reason to distinguish the black substance contained in it from all other substances, in consequence of its properties, and has called it *Melaina.* It may be obtained in a pure state, he says, by heating the black substance of sepia in a water-bath, with a mixture of 1 part nitric acid and 11 of water, until the liquor becomes of a yellow colour; it is then to be removed, to have much distilled water added to it, and to be filtered; is then to be boiled repeatedly in distilled water, washed in an alkaline subcarbonate, then again washed with cold water, and will thus be obtained pure.

This substance is perfectly black, insipid, inodorous, heavier than water, unchanged in the air. It does not affect test papers; it is insoluble in cold water, but dissolves in hot water, forming a very black solution. Alcohol and ether do not dissolve it. The aqueous solution is perfectly precipitated by sulphuric, nitric, or muriatic acid, but oxalic, citric, and acetic acids do not produce this effect; neither does alcohol or bi-chloride of mercury render the solution turbid. Cold sulphuric acid dissolves it, heat applied causes decomposition, and sulphurous acid is produced. Cold nitric acid acts upon it, liberating pure nitrogen; heat applied invigorates the action, evolving nitric, oxide, &c.; muriatic acid, either cold or hot, scarcely acts upon it. The caustic alkalies dissolve the substance readily, especially when heat is applied, and a viscid black solution is produced; acids precipitate it again, leaving a clear solution. When introduced into a flame, it burns suddenly. On a hot iron it separates, as if gaseous or vaporous matter was passing off, and when heated in close vessels, yields unequivocal indications of the presence of nitrogen.—*Giornale de Fisica*, viii. 105.

14. *Analysis of the Solanum Pseudo-Quina.*—M. Vauquelin has produced an elaborate analysis of the bark of the solanum pseudo-quina, and finds it to contain a bitter principle, purely vegetable, to which it owes its virtues, and amounting to 8 per cent., a resinous matter, about 2 per cent.; a small quantity of viscid fatty matter; a very abundant animal substance, which, in consequence of being combined with sub-malates of potash and lime, presents alkaline characters; starch, in minute quantity; oxalate of lime, 5 or 6 per cent.; malates of lime and of potash; carbonate of lime, 5 per cent.; oxide of manganese, in notable quantity, united to malic and oxalic acids; malate of iron, a minute portion of magnesia, an atom of phosphate of lime, and ligneous matter, amounting to two-thirds of the whole weight.

The animal matter, when heated, gave carbonate of ammonia, empyreumatic oil, and charcoal. It appears to form a true combination with potash or lime, or their subsalts, and seems, in part, to neutralize the alkali, whilst, at the same time the alkali confers great solubility on the substance. M. Vauquelin expresses his fear that the supposed vegeto-alkaline bodies, which have been procured from many plants of the solanum species, and received names as new substances, are only combinations of organic matter with alkalies, or their subsalts.—*Mém. du Mus.* xii. 204.

III. Natural History.

1. *Meteoric Appearance on Ben-Lomond; Ascent of Vapour.*—The following appearance is described by Mr. W. T. Ainsworth, who, with Mr. Savage, observed it on Sunday, May 8, from the summit of Ben-Lomond. At three o'clock in the morning a cold damp wind blew from the south-west, the sky there being covered with dark dense clouds, whilst, towards the east, a small extent of deep azure sky was seen, where, however, clouds were fast forming. In a short time it began to rain, and continued to do so incessantly for two hours, when, in an interval of fine weather, the travellers again resumed the ascent of the mountain. The clouds then broke, and the sun shone forth; and about this time, says Mr. Ainsworth, "having our faces turned towards the west, we observed streams of vapour rise from the earth in two or three places (at about a mile distance from us, and 400 or 500 yards apart from one another,) and ascend in a perfectly straight direction towards a heavy dark nimbus, passing over at the time. Using my hat as a level, I lay down on the ground, and found it to be rather lower than the situation I occupied near the summit of the mountain. Their bases were, I should suppose, not above three or four feet in diameter, which did not increase nor diminish till their junction with the cloud, when they assumed a more conical shape, the base of which was in the cloud. They resembled immense columns, or pillars; they had no motion forwards or backwards, and, as far as our eye could ascertain, they had no revolving motion upon their own axis. The attraction existing between the pillar and the cloud was so great, that, at the supervention of a strong breeze, though the centre of the pillar yielded, it never deviated from its columnar form, and the top remained precisely over the point from which it arose, forming, as it were, for the time, a segment of a circle. A short time after perceiving this remarkable phenomenon, we had occasion to remark the same process taking place on the lake itself. The columns, though at a great distance from us, we could plainly perceive were vapour, and not water, but they did not take on themselves so uniform an appearance. During this interesting scene, I hung two small balls hewn out of

the pith of an elder tree at the end of a stick of gum lac, a strong insulating substance, and more portable than glass; the repulsion from one another was such as to indicate that the atmosphere was in a high state of electricity. Hygrometer I had none. Thermometer stood at 45°.—*Edin. Phil. Journal*, 185.

2. *Description of an Earthquake.*—The following minute account of an earthquake is given by Professor Ferrara, of Catania, who seems to have been in the most favourable situation for the observation of such a phenomenon. On Wednesday, the fifth of March, 1823, at twenty-six minutes after five P.M., Sicily suffered a violent shock of an earthquake. I was standing in the large plain before the palace, in a situation where I was enabled to preserve that tranquillity of mind necessary for observation. The first shock was indistinct, but tending from below upwards; the second was undulatory, but more vigorous, as though a new impulse had been added to the first, doubling its force; the third was less strong, but of the same nature; a new exertion of the force rendered the fourth equal, on the whole, to the second; the fifth, like the first, had an evident tendency upwards. Their duration was between sixteen and seventeen seconds; the time was precisely marked by the second's hand of a watch which I had with me. The direction was from north-east to south-west. Many persons who ran towards me from the south-west at the time of this terrible phenomenon, were opposed by the resistance of the earth. The spear of the vane on the top of the new gate connected with the palace, and upon which I fixed my eyes, bowed in that direction, and remained so until the Sabbath, when it fell; it was inclined to the south-west in an angle of twenty degrees. The waters in the great basin of the botanical garden, as was told me by an eye-witness, were urged up in the same direction by the second shock; and a palm-tree, thirty feet high, in the same garden, was seen to bow its long leafless branches alternately to the north-east and south-west, almost to the ground. The clocks in the observatory which vibrated from north to south, and from east to west, were stopped, because the direction of the shock cut obliquely the plane of their respective vibrations, and the weight of one of them broke its crystal. But two small clocks in my chamber kept their motion, as their vibrations were in the direction of the shock. The mercury in the sismometer preserved in the observatory was put into violent motion, and at the fifth shock it seemed as much agitated as if it were boiling.—*Silliman's Journal*, ix. 216.

3. *Extraordinary Rise of the Rio de la Plata.*—This river, as is well known, is flooded at certain periods; and, like the Nile, inundates and fertilizes the country. The Indians then leave their

huts and betake themselves to their canoes, in which they float about until the waters have retired. In April, 1793, it happened that a violent wind heaped up the immense mass of waters of this river to a distance of ten leagues, so that the whole country was submersed; and the bed of the river remained dry in such a manner, that it might be walked over with dry feet. The vessels which had foundered and sunk were all exposed again; and there was found, among others, an English vessel, which had perished in 1762. Many people descended into this bed, visited and spoiled the vessels thus laid dry, and returned with their pockets filled with silver, and other precious articles, which had been buried more than thirty years in the deep. This phenomenon lasted three days, at the expiration of which the wind abated, and the waters returned with fury into their natural bed.—*Edin. Phil. Journal*, xiii. 188.

4. *Fall of a Meteoric Stone at Nantgemory, Maryland, Feb.* 10, 1825. *By* Dr. Carver.—I take the liberty of forwarding you a notice of a meteoric stone, which fell in this town on the morning of Thursday, Feb. 10, 1825. The sky was rather hazy, and the wind south-west. At about noon the people of the town, and of the adjacent country, were alarmed, by an explosion of some body in the air, which was succeeded by a loud whizzing noise like that of air rushing through a small aperture, passing rapidly in a course from N.W. to S.E., nearly parallel with the river Potomac. Shortly after a spot of ground on the plantation of Captain W. D. Harrison, Surveyor, of this port, was found to have been recently broken, and on examination a rough stone, of an oblong shape, weighing 16lbs. 7oz. was found about eighteen inches under the surface. The stone when taken from the ground, about half an hour after it is supposed to have fallen, was sensibly warm, and had a strong sulphureous smell. It has a hard vitreous surface, and when broken appears composed of an earthy or siliceous matrix, of a light slate colour, containing numerous globules of various sizes, very hard and of a brown colour, together with small portions of brownish yellow pyrites, which become dark-coloured on being reduced to powder. I have procured for you a fragment of the stone, weighing 4lbs. 10oz., which was all I could obtain. Various notions were entertained by the people in the neighbourhood on finding the stone. Some supposed it propelled from a quarry eight or ten miles distant on the opposite side of the river, while others thought it thrown by a mortar from a packet lying at anchor in the river, and even prepared manning boats to take vengeance on the captain and crew of the vessel.

I have conversed with many persons living over an extent of perhaps fifty miles square; some heard the explosion, whilst others heard only the subsequent noise in the air. All agree in stating that the noise appeared directly over their heads. One gentleman, being about twenty-five miles from the place where the stone fell, says that it caused his whole plantation to shake, which many supposed to be the effect of an earthquake. I cannot learn that any fire-ball or any light was seen in the heavens. All are confident that there was but one report, and no peculiar smell in the air was noticed.

Captain Harrison, whose account is added to this, and on whose grounds the stone fell, states, from his own observation, that the time was between twelve and one o'clock, that the explosion was sharper than a cannon; that then a buzzing noise was heard over head, first like that of a bee, but increasing till like a spinning wheel, or a chimney on fire, and that then something was heard to fall, the time from the explosion to the fall being perhaps fifteen seconds. After a while the stone was found about twenty-two or twenty-four inches beneath the surface; it had a strong sulphureous smell, and there were black streaks in the clay, which appeared marked by its descent; the mud was thrown in different directions from thirteen to sixteen steps. The stone, when washed, weighed 16lbs. It fell within 250 yards of Captain Harrison's house, and within a hundred yards of the habitation of the negroes. This account was given from memory on the 28th of the following April.—*Silliman's Journal*, ix. 351.

5. *Composition of Aërolites.*—M. G. Rose, of Berlin, has succeeded in separating crystals of pyroxene from a large specimen of the aërolite of Juvenas, and has measured the angles with the reflective goniometer: one of the crystals is of the octoedral variety, represented in the 109th figure of Haüy's mineralogy. The same rocky tissue contains microscopic hemitrope crystals, which appear to be felspar, with a base of soda, *i. e.*, albite.

M. Rose has also examined, at the request of M. Humboldt, the aërolite of Pallas, and the trachytes collected on Chimborazo, and the other volcanoes of the Andes: he has found that the olivine of the mass of Pallas is perfectly crystallized, and that the trachytes of the Andes are, in part, mixtures of pyroxene and albite like the aërolite of Juvenas. Perhaps the same is the case with those of Jonzac and Stannern, of which, as yet, the masses have not been studied mineralogically by trituration, the microscope, and the reflective goniometer.—*Ann. de Chimie,* xxix. 109.

6. *Flexible Marble of Berkshire Country, U.S.*—Dr. Dewey states that this marble, which has been known for some years, and until lately was found chiefly in West Stockbridge and Lanesborough, is now obtained at New Ashford, from an extensively wrought quarry. He had three fine specimens of it in slabs from five to six feet in length, and seven inches in width. Its flexibility and

elasticity may be shewn as it stands upon one end, by applying a moderate force to the middle or the other end. Its flexibility is seen, too, by supporting the ends of it in a horizontal position upon blocks. The marble has various colours, nearly white, with a reddish tinge, gray, and dove-coloured. Some of it has a fine grain; other specimens are coarsely granular, and have a loose texture. It is not uncommon for one side of a large block to be flexible, while the other part is destitute of this property. It takes a good polish, and appears to be carbonate of lime, and not a magnesian carbonate.

It is well known that Dolomieu attributed the flexibility of the marble he examined to exsiccation, and that Bellevue ascertained that unelastic marble might be made elastic by exsiccation. The flexible marble of this counry, however, loses this property in part on becoming *dry*. When it is made thoroughly wet by the operation of sawing, or of polishing, it must be handled with great care, to prevent its breaking; and the large slabs of it cannot be raised with safety unless supported in the middle as well as at the ends.—*Silliman's Journal*, ix. 241.

7. *Extraordinary Minerals discovered at Warwick, Orange County, N. Y.*—These *extraordinary* minerals are described by Dr. Fowler, in Silliman's Journal, ix. p. 242. They belong, he says, to the formation of crystalline limestone, which there, perhaps, has no parallel in any other region of the world, and were discovered in the township of Warwick. "What will be thought of *spinelle pleonaste*, the side of one of whose bases measures three to four inches, or twelve to sixteen inches in circumference? These crystals are black and brilliant, sometimes aggregated, at other times solitary; at this locality seldom or ever less than the size of a bullet. Some are partly alluvial, their matrix decomposing, but when unaltered, they are found associated with what has never yet been described, namely, *crystals* of serpentine, slightly rhomboidal prisms, of a magnitude parallel with the crystals of spinelle, often greenish and compact, at other times tinged yellow by an admixture of Brucite."

"In the same mass also are associated very large prismatic crystals of *chromate of iron*, at least so they appear to be, by the beautiful green colour which they impart to nitrate of potash, having a specific gravity of 4.3. Some of these prisms are an inch in breadth, and two inches in length, with two lateral faces broader than the rest."

"Not far from the same locality also is found, associated generally with a fine green and crystalline serpentine, the *red spinelle*, of various shades and degrees of translucence," &c. "These are from a line in diameter to three quarters of an inch on each side of the bases; now and then they occur in hemitrope." "At Byram, also, a few miles from Sparta, the *red spinelle* has been found by William Inglis, Esq. Some of these approaching to a chocolate brown in colour, give a base of one inch and a quarter on each plane."

"The magnitude of other crystals at this place (Warwick) is equally surprising as that of the spinelles. Crystals of *scapolite* terminated, are to be found each of the six faces of the prism, measuring four inches, or a circumference of twenty-four inches, or even more. They are, of course, rough and corroded; but the smaller prisms, often with narrow replacements on the edges, are very perfect, and almost transparent: all of these slightly tinged with green."

"Of the amphibole genus we meet with several varieties, finely crystallized; the black with six-sided prisms, each face sometimes is an inch in breadth; actynolite in short and confused prisms, and a chocolate brown finely-crystallized variety, both in large and crystals of the usual form, and also of an extraordinary form, having the obtuse angle sometimes replaced by a broad face."

"Crystals of augite abound here of gigantic magnitudes, and sometimes when smaller, of considerable perfection of form; they are generally greyish green."

"In a very singular bed, subordinate to, and, indeed, in the crystalline limestone, occurring in the form of a breccia of the old red sandstone, red graphic granite, and white felspar, I have found partly diaphanous, softish, green octoedral crystals of considerable magnitude, for which I know of no ascertained character. They appear almost similar in substance to *steatite*, being easily cut by a knife. They are not, however, found as the *spinelle* of this locality in carbonate of lime. Considering, therefore, this mineral as new, I propose to call it *pseudolite*, in allusion to its affinity to the pseudamorphous crystals of steatite."

8. *Globules of Water in Amethyst.*—Mr. Webb, of Providence, U. S., has had occasion to observe that globules of water and air were by no means unfrequent in specimens of amethyst, which came under his eye. Many of them were highly interesting from the size of the globule or portion of liquid, the form of the cavity containing it, the exhibition of double refraction through the crystal which it afforded, *&c.* He remarks that most of these specimens were found among such as had been rejected on account of being too pale for good cabinet specimens, and thinks it probable that good specimens are continually neglected for want of sufficient and close examination.—*Silliman's Journal*, ix. 246.

9. *Recent Formation of Brown Hematitic Iron Ore.*—On examining a set of cast-iron pipes, which had lain for some years in the line of one of the streets in the New Town of Edinburgh,

we were surprised to find the sand in which they had been laid, where in contact with the pipes, very compact and brown in colour. On breaking some of the masses, we found the connecting matter to be brown iron ore, and in cavities of the compacted sand this brown iron ore, exhibiting that particular lustre approaching to adamantine, and the reniform shape with the granulated surface of brown hematite. Here, then, we have an instance of the formation, by the action of percolating water on the iron of the pipes, of an ore of iron which some observers arrange with the igneous mineral formations.—*Edinburgh Philosophical Journal,* xiii. 193.

10. *On the Habits of Beavers. By* M. Geoffroy St. Hilaire.—A beaver has lived in the menagerie of the king's garden for some years; it is one of those from the Rhone, which live separately like water-rats. This animal was defended from the cold of winter only by a more abundant litter. It happened one night that the cold had increased; the shutters of the hut closed but badly, and the beaver was urged to find means of preserving itself from the effects of a very rigorous temperature. It had been the custom to give it a certain number of fresh branches to satisfy its desire of gnawing, and occupy it during the night, these were found stripped of the bark in the morning; food also was given it, consisting of greens and fruits, of an evening, before shutting it up, by closing the shutter, which was made like a penthouse. It had snowed too, and the snow had collected in one corner of its hut.

Such were the materials which the beaver, in this instance, possessed to form a wall of defence against the external air and cold; the branches were interwoven between the bars of the hut, exactly in the way that basket-makers interweave the small osiers round the principal stems, going from one to the other by contrary turns. Thus arranged, they left intervals, in which the beaver placed the carrots, potatoes, and litter which remained, each substance being cut so as to occupy and fill the spaces left. Finally, as if the animal knew that the whole should be covered with a compact cement, he employed the snow to fill even the smallest aperture which remained. The wall filled up two-thirds of the gap, and every thing which had been given to the animal, even the whole of its food, was employed in the construction.

In the morning, the snow having frozen between the branches and the shutter, the latter adhered to the new wall; when, however, removed, the work of the beaver became exposed. The boy, who attended to the animal, was so surprised at this unexpected production, that he came and informed me of it before any thing was deranged.—*Mém. de Mus.* xii. 232.

11. *Tenacity of Life in Larva.*—Dr. Yule states, that "the larva of a carnivorous beetle, sent to me from Inverary, not merely lived, but moved briskly in strong alcohol, the day after it was enclosed in a phial, filled with that liquor. Bonnet found that the larva of papilio brassicæ, frozen under a temperature of 14° Fahr., revived perfectly on being thawed.—*Edin. Phil. Jour.* xiii. 73.

12. *Argonauta Argo.* Naples.—*Academy of Science, Dec.* 14. 1824.—The Chevalier Pole read two memoires on the *argonauta argo* of Linnæus, caught on the coast of Pausillipo, near Naples, which he had the opportunity of examining whilst alive. He described the organization and parts of the animal, and has determined the most curious points of its generation. He has been able to remark, by means of the microscope, that the shell of this insect exists with the animal whilst yet in the egg; and what is still more extraordinary is, that the animal is not naturally attached to the shell. Conjectures are ventured upon the manner in which the shell is developed.—*Rev. Ency.* xxvi. 912.

13. *Recent Vegetation of Ancient Beans.*—"As I happened to be at Naples, when first Herculaneum was discovered, I should have told you that some leathern bags of beans, answering exactly to our kidney ones, were fonnd in several corners of their window-seats. The Romans were very fond of that kind of supper, as appears by a line of Horace:

'Oh quando faba Pythagoræ, &c.'

Some English gentlemen were curious enough to sow them on their return; and notwithstanding their having been, to appearance, dead for so many centuries, yet did they grow and produce. Dr. Lawson tried the experiment in a small garden of his at Chelsea, and it succeeded."—*Monthly Mag.* lx. 98.

14. *On the Origin of Ergot. By* General Martin Field.—General Field states, that his intention is not to support or oppose any theory of the orgin and nature of ergot, but simply to repeat the facts as he observed them. The field of rye examined was within fifty yards of the house, (New Fane, Vermont,) and of that kind known in the neighbourhood as the *Norway, or White, Rye,* which has been observed to be more productive of *ergot* than the English spring rye, or that said to be from the Isle of Candia. Ergot has been very abundant in this vicinity during the last season.

"The field of rye, which I very frequently examined, was in full blossom about the 30th of June, 1824, but I discovered no appearances of ergot till the 22d of July. From that time until the 12th of August, when the rye was harvested, it might be found of various dimensions. Upon minute examination I discovered that every grain of ergot, as it emerged from the glume, had attached to its apex the shrivelled rind of a grain of rye, which had the appearance of once being in a healthy state. This led me to conjecture, that a diseased state of the rye was the primary cause of the ergot. To ascertain the fact I repaired to the rye-field, where I discovered groups of flies collected upon the heads of rye, apparently in the pursuit of something within the glume. On opening the valve of the glume where the flies were thus collected, I found the saccharine juice of the grains of rye was oozing out, and would soon produce drops. I was then convinced that it was this saccharine fluid which was so inviting to the multitude of flies that collected upon those heads of rye which contained any diseased grains. Having collected a number of grains of full-grown size, and exhibiting appearances similar to those above described. I placed the same under a microscope, by which I could clearly discover a small orifice in each, near the end opposite to that to which the thread of nutrition had been attached. I could also discover, that the juice of the grain was still discharging from the orifice.

On the morning of the 1st of August, by observing the groups of flies, I found two heads of rye near each other, each of which contained a grain of punctured or diseased rye. The culms I tied to a stake drove between them, the better to enable me again to find them, and to observe their future appearances. At that time the punctured grains exhibited no symptoms of decay, otherwise than a small discharge of fluid. During the first day the flies were busily employed in extracting their delicious beverage from the orifice of each grain, and when it did not flow in sufficient quantity for their supply, they would probe it anew. On August 2nd, both grains appeared to be in a state of fermentation, and rapidly tending to decay. On the 3rd, being forty-eight hours from the time when I commenced my observations, each grain had become a rotten and shapeless mass, and exhibited very little appearance of healthy rye. Then, on carefully opening the valves of the glume, I discovered in each a small black globule, the size of which was rather larger than a pin's head. These were situate at the points of the peduncles of the diseased grain, which afterwards proved to be ergot. During the first four days, after the ergot was discovered, they grew in length very near two lines in each twenty-hours, displaying the remains of the diseased rye, from the glumes which they had occupied. On the 12th of August, the ergot had attained its full growth. The dimensions of one grain of ergot were twelve lines in length, and three lines in diameter; the other grain measured a little less.

On the 3rd of August, being convinced that the primary cause of ergot was the puncture of the healthy grain by the fly, it occurred to me, that, perhaps, it might be produced by such means as I possessed. To ascertain this fact, with the point of a needle I punctured four grains of rye in the same head, it then being in a green pulpy state, and of full-grown size. A discharge of the juice of the grains was soon discovered from the orifice of each. The flies collected as in those cases before mentioned. The result was, that on the fourth day after the operation was performed, ergot appeared in the glume, occupying the places of two of the punctured grains. The other two grains exhibited no symptoms of decay, but continued in a healthy state. From appearances I am led to believe, that in warm dry weather many grains of rye are punctured, which are not materially injured thereby. The orifice closes before a sufficient quantity of juice has escaped to produce fermentation and decay. This may, therefore, be assigned as one reason why cloudy and wet seasons are so much more productive of ergot than those which are fair and dry."

Not being able at any time to discover, by a microscope, the eggs or larva of insects in the rye, General Field concludes, that the object of the fly is simply food. The fly is of the hairy or bristly species, and deposits its eggs upon animal flesh, either fresh or putrid. The culm of rye did not seem affected by the ergot, but where there were eight or ten grains of the latter, no sound rye occurred in the head, the rye then apparently suffering a severe blight. The size of the ergot is in proportion to the number of grains in a head; where there is but one, it is from ten to fourteen lines in length, and two or three in diameter; but where there are from twenty-five to thirty grains, and that is not unfrequent, they are often not larger than sound rye.—*Silliman's Jour.* ix. 359.

15. *Action of Poisons upon the Vegetable Kingdom. By* M. Marcet.—A very interesting memoire, by M. F. Marcet, on the action of poisons upon vegetable life, has been read to the Societè de Physique et d'Histoire Naturelle of Geneva. The object of the author in the experiments he instituted was, in the first place, to ascertain the action of those poisons which act on animals by inflaming and corroding the part with which they come in contact; and in the second place, the action of such poisons, especially those of a vegetable nature, which destroy animals by their effects on the nervous system. "Until now," the author remarks, "plants have been supposed to be distinguished from animals by the absence of organs corresponding to the nerves of the latter class; but the results of the experiments tend to prove that they are capable of being affected by such poisons in a manner analogous to that in which animals are affected by them. The experi-

ments were generally made with plants of the kidney bean *(phaseolus vulgaris)*, and a comparison was always made with a plant watered with spring-water.

METALLIC POISONS. *Arsenic.*—A vessel containing two or three bean plants, each with five or six leaves, was watered with two ounces of water, containing twelve grains of oxide of arsenic in solution. At the end of from twenty-four to thirty-six hours, the plants had faded, the leaves drooped, and had even begun to turn yellow; the roots remained fresh, and appeared to be living. Attempts to restore the plants after twelve or eighteen hours, by abundant watering, failed to recover them. The leaves and stem of the dead plant put into water gave, upon chemical examination, traces of arsenic.

A branch of a rose-tree, including a flower, was gathered just as the rose began to blow; the stem was put into a vessel containing a solution of six grains of oxide of arsenic in one ounce of water, on the 31st of March. On the 1st of April, the external petals had become flaccid and slightly purple, and the leaves began to droop; 10 grains of water, or 0.12 of a grain of arsenic, had been absorbed in twenty-four hours. On the 3rd April, the petals were more flabby and faded, their colour deep purple and spotted, the odour gone, and the leaves faded. During the last twenty-four hours it had absorbed four grains of liquid. On the morrow the branch was quite dead, and there was no further absorption. Only a fifth of a grain of oxide of arsenic had been introduced. Arsenic was found, by chemical examination, in the leaves and flower. Similar stems placed in pure water had, after five days, the roses fully expanded, the leaves fresh and green, and had absorbed each, per day, 15 grains of water.

On June 1, a slit of 1½ inch in length was made in the stem of a lilac-tree, the branch being about an inch in diameter. The slit extended to the pith. Fifteen or twenty grains of moistened oxide of arsenic was introduced, the cut was closed, and the stem retained in its natural position by osier-ties. On the 8th, the leaves began to roll up at the extremity; on the 15th, they had faded and doubled up lengthwise, and the branches began to get dry; on the 28th, the branches were dry; and in the second week of July, the whole of the stem was dry, and the tree itself dead. Other trees similarly cut, but without having had poison introduced, suffered no kind of injury.

On the side of the poisoned lilac was another, the trunk of which joined the first a little above the earth. This tree became entirely dry, and with similar phenomena, in about fifteen days after the first. In another experiment, arsenic was put under the bark of a lilac-tree; in fifteen days, the two nearest principal branches were dead and dry; the rest did not suffer.

Mercury.—Two or three bean-plants, growing in a pot, were watered on the 5th of May with two ounces of water, containing twelve grains of corrosive sublimate. Next day they looked unhealthy; the leaves had drooped, and the stalk had become of a yellow brown colour; an equal quantity of the same solution was added to them. On the 7th of May the plants were quite dead; the stems were yellow, and the leaves dry and faded. The leaves, put into water, gave a solution containing mercury.

A branch of a rose-tree, with two or three half-expanded roses, had its extremity immersed in a similar solution of corrosive sublimate. On the second day, the leaves became discoloured here and there, the external petals had faded, but the flower had opened a little more; twenty-four grains of solution had been absorbed. Next day, the discoloration was more extensive, and the leaves seemed very unhealthy; on the fourth day the discoloration of the leaves was almost complete, and the branch was dry; the central petals were deepened a little in colour, but had not faded. Thirty-two grains of solution, or half a grain of the poison, had been absorbed.

On the 10th of May, a hole was made in a cherry-tree, penetrating to its pith; a few globules of mercury were introduced, and the hole closed up, so that air or water could not enter. The tree was perfectly healthy on the 10th March, 1825*, not having suffered in the slightest manner.

Tin.—April 13th, a branch of a rose-tree, with two or three half-expanded flowers, was put into a solution of muriate of tin, of similar strength with the preceding solutions; on the 15th, striæ of a yellow brown colour had appeared along the fibres of the leaves; on the 16th, the leaves were yellow, and the branch dead. The leaves, steeped in water, gave a solution containing tin. Muriate of tin affected bean plants in the same manner as muriate of mercury.

Copper.—The roots of a bean plant were withdrawn from the earth, and placed in a vessel containing a solution of sulphate of copper, in the proportion of the former solutions; in twenty-four hours the leaves of the plant had faded entirely. Reference is made to an experiment by Mr. Phillips (*Annals of Philosophy*, xviii. 76), in which a young poplar was killed by watering it with solution of copper. A knife, employed to cut a branch off this tree, had the copper precipitated on its surface.

Lead.—The roots of bean plants were introduced into solution of acetate of lead, of the preceding strength; on the second day, the lower leaves faded; on the third day, the plant was dead.

The same result was obtained with muriate of baryta.

Bean plants, introduced into solution of sulphuric acid, in three times its weight of water, began to droop in a few hours, and in twenty-four hours were entirely faded.

* Probably some mistake. The paper was read 16th Dec. 1824.

A solution of potash, similarly diluted, produced the same effect.

The roots of bean plants were placed in a solution of twelve grains of sulphate of magnesia in two ounces of water. No effects were produced at the end of twenty-four hours, and twelve grains more of the salt were added; at the end of forty-eight hours, other twelve grains were added, making thirty-six grains in two ounces of water; and yet the plants prospered. The object here was to ascertain whether mineral substances, not injurious to animals, were also innocuous to vegetables. The same results were obtained with common salt.

VEGETABLE POISONS. In these experiments the bean plants were carefully taken from the earth, and their roots immersed in the solutions used. It was ascertained that plants, so withdrawn and placed in water, would remain in excellent health for six or eight days, and continue to vegetate as if in the earth. As some of the poisons used rendered the water in which they were dissolved viscid, a comparative experiment was made in water containing enough gum to make it more viscid than any of the solutions used; the beans thus treated remained fresh and healthy for five or six days. Further, all the infusions and solutions used were filtered.

Opium.—(10th May.) The roots of a bean were placed in a solution of from five to six grains of opium in one ounce of water. In the evening the leaves had drooped, and by the middle of the next day the plant was dead beyond recovery. Extract of nightshade produced a similar effect, but more slowly.

Nux Vomica.—(May 9.) The bean plant used was put into a solution of five grains of the extract in one ounce of water at nine o'clock; at ten o'clock the plant seemed unhealthy; at one o'clock all the petioles were bent at the middle, as if broken; in the evening the plant was dead. Another plant, taken from the earth at the same time, and left without water, began also to fade in three or four hours; but the leaves only gave way in that time, and not the petioles.

A slit being made (July 15) to the centre of the stem of a lilac, nearly an inch in diameter, about fifteen grains of moistened extract were introduced, and the wound closed up. On the 28th, the leaves of the two largest branches nearest to the wound began to dry; and on the 3d of August they were quite dry. The other leaves became dry in the course of the autumn.

Opium and the nux vomica produce death in animals, according to M. Orfila, by acting on the nervous system. Opium appears to act upon the brain, and the vomica nut upon the spinal marrow.

Seeds of the Coculus Menispermis.—Ten grains of the extract of these seeds were dissolved in two ounces of water. A few moments after the introduction of the plant, the two nearest leaves became slightly wrinkled and bent, so as to become doubled. When forcibly opened, they again returned to this position. After some hours, the leaves began to droop, to twist; they then became flabby, and in twenty-four hours the plant was dead, all the petioles being bent in the middle. This kind of poison appears to act in animals on the spinal marrow, producing tetanus, and then death.

Prussic Acid.—A solution of this acid being used, the petioles began to bend in three hours, and in about twelve hours the plant was quite dead. All the petioles were as if broken in the middle.

A drop or two of strong prussic acid, put on to the end of the leaf of a sensitive plant, caused several of the neighbouring leaves to close in a few moments. A spoon containing prussic acid or the open bottle being held near the leaves, caused them to fade. In all these experiments the leaves experimented upon regained their sensibility only after some hours.

Laurel-water.—In a few moments many of the leaves became wrinkled and rolled up, in about half an hour they opened, became flabby, and in six or seven hours the plant was dead. The wrinkling of the leaves varied considerably at different times.

Belladonna.—A solution of five grains of the extract in one ounce of water was used. In a few minutes the lower leaves drooped a little, in the evening they had partly returned to the natural state; next morning, they and others had drooped; on the second day the leaves had begun to change colour; on the fourth day the plant was dead.

Alcohol.—The alcohol was mixed with its volume of water; the plant died in twelve hours. The leaves had faded and were soft. When weak alcohol, containing three grains of camphor in half an ounce, was used, the plant died in twelve hours; but, in addition to the above appearances, the petioles were bent as if broken in the middle.

Oxalic Acid.—Five grains of the acid were dissolved in one ounce of water, and a rose branch put into it. In twenty-four hours the rose began to fade, and in forty-eight hours the flower and branch were both dead. The plant had absorbed only one grain of liquid in the last twenty-four hours, and but the tenth of a grain of oxalic acid had been taken up altogether. A bean plant in a similar solution died in twenty-four hours.

Hemlock.—Five grains of extract in one ounce of water. Wrinkling of the lower leaves in a few minutes; these leaves dead in twenty-four hours; the whole plant dead in forty-eight hours.

Digitalis.—Six grains of the substance in one ounce of water. In a few moments some of the leaves became slightly wrinkled at the extremity, in the evening these extremities were dead, and twenty-four hours after the whole plant was dead.

From the whole of these experiments, M. Marcet concludes, 1. That metallic poisons act upon vegetables nearly as they do upon animals. They appear to be absorbed and carried into different parts of the plant, altering and destroying the vessels by their corrosive powers. 2. That vegetable poisons, especially those which have been proved to destroy animals by their action on the nervous system, also cause the death of plants. But as it cannot be supposed that poisons, which do not attack the organic structure of animals, should affect that of vegetables, so as to kill them in a few hours, it appears very probable that there exist in the latter a system of organs which is affected by poisons nearly as the nervous system of animals.

Then follow some experiments on the action of certain gases on the roots of plants. It is known that if a plant be taken from the earth, and its roots be introduced into a receiver of atmospheric air, containing moisture, whilst the leaves are in the air above the receiver, there will be found after some hours a small quantity of carbonic acid gas. This has generally been supposed to be formed by the combination of the oxygen of the air with the excess of carbon in the roots. In the following experiments the roots were placed in different gases, that it might be ascertained whether when no oxygen was present, and when therefore no carbonic acid should be formed, the plant died more suddenly. Six similar bean plants were selected and fixed into receivers placed over water, so that moisture should be present in the gases, the apertures by which the stems passed out being closed carefully. Different gases were then introduced into the receivers, and the following results obtained.

i. *Atmospheric air.*—The plant remained healthy for forty-eight hours, and then gradually faded.

ii. *Hydrogen.*—The plant began to fade in five or six hours, and was quite dead in fourteen or sixteen hours. The leaves were faded and the stem bent.

iii. *Carbonic acid.*—The plant began to fade in two hours, and was dead in eight or ten hours. All the leaves were faded, and the stem bent in the middle.

iv. *Nitric oxide.*—The leaves began to bend in about six hours, and the plant did not die in less than twelve hours. It appears not impossible that a little carbonic acid may have here been formed, the nitric oxide being readily decomposable.

v. *Nitrogen.*—The leaves began to droop almost immediately; in three hours the skin and upper leaves were bent and faded, and in five hours all the leaves were faded, and the plant dead. This gas seems most injurious of all those tried.—*Ann. de Chim.*, xxix. 200.

16. ***Observations on the Contents of the Digestive Canal in the Fœtus of Vertebral Animals, by*** MM. Prevost *and* Royer.—An abstract is contained in the last volume of this Journal, p. 169, of a paper by these philosophers on digestion, in which they endeavoured to establish that that function resulted from the alternate action of soda and muriatic acid, secreted by the alimentary canal upon the food. Their object in a second paper or note, the abstract of which follows, was to examine the subject as connected with that time in the existence of the animal, during which the organs were forming, or beginning to act. This examination it appears confirmed their original views.

The chick in the egg was first subjected to examination. It was only on the ninth day of incubation that the organs were in such a state as to permit of the fluid in the stomach and intestines being collected, only a few drops then being obtained from many individuals. At this time the crop or first stomach, the glandular stomach and the gizzard contained a transparent liquid, extending into threads between the fingers, and slightly alkaline; tested by acids, alcohol, and corrosive sublimate, it, from the precipitates formed, appeared to be abundant in albumen. The liquid of the intestines appeared to be of the same kind, but was in too small a quantity to be easily examined. The waters of the amnios gave much less abundant precipitates; they were clear, slightly yellow, and not extensible into threads between the fingers. The waters of the allantoides contained no albumen, and were very clear.

On the thirteenth and fourteenth day the liquid of the glandular stomach had increased in quantity, it contained much more albumen, and coagulated by heat: that portion which was in contact with the membrane containing it, was white, and had the appearance of albumen precipitated by an acid, and in fact when put upon test paper, it sensibly reddened it. On opening the gizzard, it was seen that the acid had flowed from the glandular stomach into its cavity, by the manner in which the precipitate was formed; abundant near the cardia, but very slight towards the pylorus.

The waters of the amnios coagulated by heat, and were slightly alkaline to test paper. Those of the allantoides were slightly turbid, from the presence of a portion of crystalline uric acid.

On the seventeenth day the changes were complete. The fluid of the crop was the same, but that of the glandular stomach and gizzard was entirely coagulated, and decidedly acid. Particles of albumen were found in the intestines, carried there by the peristallic motion; their surface was of a fine green colour. There was also found a substance containing globules, and of a yellowish gray aspect; it was a mixture of mucus and albumen. The waters of the amnios were denser than before. Those of the allantoides were of a yellowish white, slightly acid whilst warm from the sac, and contained both uric acid and urea; hence the kidneys were performing their functions.

On the twenty-first day, a few hours before the chick would have been hatched, the contents of the alimentary canal were in such quantity as to allow of other trials on their nature. Decided traces of mucus were found in the crop, and free muriatic acid was found in the two last stomachs. The contents of the intestines were liquid in the first part of their course, and of a dull cinnamon colour; in the rectum they were solid, and of a deep greenish-brown colour. When treated with alcohol, the latter separated the colouring principle, which was remarkable for being strongly heightened by contact with the air. The alcoholic extract exposed became in a quarter of an hour of a fine deep emerald-green colour, being at first only of a pale yellowish-green. This change did not take place in close vessels. Acids produced the same effects as oxygen; nitrogen and hydrogen no effect. The residue from the alcohol treated with dilute acids was separated into two portions; a coagulated albumen, which with certain salts remained, and mucus in considerable proportion, which dissolved.

Observations were then made upon a fœtus of the mammalia class; a calf, of the weight of four pounds and a half nearly. Its stomach contained a homogeneous liquor of a pale yellow colour, transparent, drawing into threads between the fingers, and perfectly neutral. It did not change by ebullition or by nitric acid, and only slightly by corrosive sublimate, but tannin and sub-acetate of lead precipitated it abundantly. Hence it contained much mucus, and but little albumen. The waters of the amnios were neutral, not adhesive, and gave a less abundant precipitate with tannin and solution of lead.

The small intestines contained a thick matter, formed in part of globules. It was of a yellow colour, but slightly adhesive, and contained but a small quantity of mucus, much albumen, and a colouring matter soluble in alcohol, and having the same property as that obtained from the chick. Near the cœcum, the appearance of the contents of the intestines changed; they there became solid, very adhesive, of a greenish-brown colour, and gave much mucus, but little albumen, and the colouring matter. The cœcum and rectum were filled with a white substance, containing globules without any colouring matter, and composed of a little mucus and much albumen. This observation is considered as interesting, inasmuch as it shews that the secretion of the mucous membrane of the stomach is very different from that of the mucous membrane of the intestines. It also fixes the epoch when the peristaltic motion commences.

Toward the conclusion of gestation, or about the eighth month, the liquid of the stomachs of the calf becomes thicker, more adhesive between the fingers, always perfectly neutral. Its specific gravity is 101.15. It contains no albumen; its constituents being mucus in large quantity, an animal matter soluble in alcohol and salts of soda and lime. Subjected to the pile, it loses its consistency, and deposits a considerable coagulum at the positive pole, which, though in appearance resembling albumen, has all the properties of very condensed mucus. The first portion of the intestinal canal contained a substance analogous to that found in the young fœtus, but more abundant and more highly coloured. The latter portion of the intestines, with the cœcum and rectum, contained a solid meconeum* of a greenish-brown colour, composed of mucus, albumen, and much colouring matter: there were also many hairs dispersed through it; their colour was the same as that of the skin of the fœtus: they were also found in the mucus of the stomachs. As similar hairs floated in the waters of the amnios, it appear to the author a conclusive argument in favour of the opinion that the fœtus swallows some portion of the waters in which it is immerged.

The waters of the amnios were very thick and drawing into threads; they were neutral, and resembled the liquid of the stomachs in the effects of re-agents upon them; they contained no albumen. They never gave traces of the amniotic acid described by MM. Vauquelin and Buonira. This was inexplicable, until having left a portion for two days in a hot place, it was found very acid, and then, treated according to the process described by those chemists, 170 parts gave about 1 part of pure amniotic acid.

No free muriatic acid was found at any time in the stomach of a fœtus of the mammalia class; its appearance is probably very near the moment of its birth, or otherwise it would be present before the young animal had received the milk of its mother.

It is then observed, that in a future memoir upon the manner in which the fœtus is nourished, it will be seen that the fœtal parts of the placenta form the blood of the new animal, and that no mixture takes place between this and the blood of the mother: the following observation, terminating the present paper, proves this statement. A young fœtus of a goat was procured, and its blood microscopically examined, and compared with that of its mother. The globules of the former had a diameter precisely double that of the globules of the latter, i. e., two millemetres (.079 of inch) seen with a magnifying power of 300, whilst those of the goat were only one millemetre in diameter.—*Bib. Univ.*, xxix. 133.

17. *Remedy for Effects produced by Inhaled Chlorine.*—The

* The name given to the contents of these parts.

injurious effects which result from the introduction of chlorine into the lungs are well known, and as the preparation of this substance for its application in certain manufactures is very extensive, workmen are not uncommonly suffering in consequence of its inhalation. The advantageous use of ammonia is well known in these cases, the vapour of it being inhaled, or a little of it on sugar being taken into the mouth. It does not, however, except where the effect produced by the chlorine has been slight, give full relief, probably from the formation of a portion of azotane, which is itself very injurious to the lungs; but M. Kostner recommends that at the same time the vapour of alcohol should be breathed, which will in an instant dissipate every prejudicial action. The spirit of wine is to be dropped on to sugar, and held in the mouth. In this way he has made use of it for two years with constant success.—*Gior. di Fisica.* viii. 146.

18. *Employment of Caustic to destroy the Variolous Eruption.*—M. Velpeau read a memoir to the Royal Academy of Medicine, tending to prove that if the pustules of the small pox are cauterized within the two first days of their appearance, they die away entirely; and if this be done even later, their duration is abridged, and no traces of them are left. The caustic he employs is a solution of nitrate of silver, in which he dips a probe, with which he pierces the centre of each pustule. M. Dumerel says that he has been long familiar with this practice, but instead of the solution he employed the solid caustic itself. (Archives Générales.)—*Med. Jour.*, liv. 170.

19. *Preservation of Anatomical Preparations.*—M. Braconnet of Nancy has applied the persulphate of iron, in consequence of its astringent and antiseptic properties, to the preservation of anatomical preparations, &c. It is very cheap, and combines, with the greatest facility, with all the humours and soft tissues of animals, and preserves them both from putrefaction and insects. A brain which had been plunged for three months in a solution of this salt, being put into a warm place, required a considerable time to dry it, but without shewing the least sign of putridity; placed afterwards in water, it was still preserved for some time, but did not recover its pristine softness. Portions of the liver, spleen, lungs, and muscle placed in this salt, have equally resisted destruction.—*Archives Générales, June.*

Dr. Macartney, of the Dublin University, covers his preparation jars with a thin plate of Indian rubber, which is afterwards varnished. This is found to be very superior to lead or bladder, retains alcohol when used very perfectly, and adapts itself readily to the variations of volume in the contents of the jar, from differences of temperature.

20. *Physiological Prize Question.*—" A general and comparative history of the circulation of the blood, in the four classes of vertebral animals before and after birth, and at different ages." The prize, a gold medal of 3000 francs value. Memoirs received till Jan. 1, 1827.—*Academy of Sciences, Paris.*

21. *Salt on the Shore of the Severn.*—During the wet weather in the month of July last, while walking beneath the cliff of red marl, which varies from about sixty to eighty feet in height, at Gatcomb, in the parish of Awre, Gloucestershire, on the northwest shore of the Severn, I was struck with the white appearance of the mud at low-water. On examination I discovered that, from the intense heat of the sun's rays, the stratum of mud was divided into square sections of various sizes, perfectly dried up, and the surface of the whole covered with a very fine salt. I moreover found the ledges and hollows of the cliff, where any water had lodged upon the reflux of the tide, covered to about the depth of one-tenth of an inch with a similar substance; and in both cases the saline particles did not differ perceptibly in taste, as to the degree of saltness, from a corresponding quantity of common salt. —*Letter from Rev. C. P. N. Witton, B.A., Fel. of the Camb. Phil. Soc.*

The Burmese imperial state carriage, which has been captured in the present sanguinary Indian war, is arrived in this country, and is now preparing for public exhibition. It is without exception one of the most singular and splendid specimens of art that can be imagined, presenting one entire blaze of gold, silver, and precious stones. Of the latter the number must amount to many thousands, comprising diamonds, rubies, sapphires, white and blue, emeralds, amethysts, garnets, cats'-eyes, crystals, &c. The carving is of a very superior description, the form and construction of the carriage most extraordinary, and the general taste displayed throughout so grand and imposing, yet at the same time so chaste and refined, as to defy all rivalry even from European workmanship. The enterprise and perseverance of this warlike people excite universal attention at this juncture, and the present object will prove that their skill in the arts even surpasses their prowess in arms, in both of which their proficiency appears hitherto to have been equally unknown to us. The carriage stands between twenty and thirty feet in height, and is drawn by elephants.

ART. XVII.—METEOROLOGICAL DIARY for the Months of June, July, and August, 1825, kept at EARL SPENCER'S Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

For June, 1825.

		Thermometer.		Barometer.		Wind.	
		Low	High	Morn.	Eve.	Morn.	Eve.
Wednesday	1	38	65	30.20	30.10	SW	SW
Thursday	2	50	61	29.90	29.72	WbS	SW
Friday	3	52	66	29.55	29.62	W	W
Saturday	4	49	60	29.49	29.21	WbS	NW
Sunday	5	44	55	29.33	29.60	W	W
Monday	6	38	67	29.80	29.80	W	WbS
Tuesday	7	52	65	29.77	29.73	SW	WbS
Wednesday	8	49	70	29.80	29.82	WbS	W
Thursday	9	48	72	29.88	30.03	WSW	W
Friday	10	46	74	30.10	30.10	WNW	SW
Saturday	11	49	78	30.10	30.10	SW	W
Sunday	12	51	78	30.09	30.04	W	E
Monday	13	52	74	30.01	30.10	E	E
Tuesday	14	53	72	30.18	30.23	NE	EbN
Wednesday	15	50	70	30.23	30.18	ENE	EbN
Thursday	16	47	73	30.14	30.09	EbN	EbN
Friday	17	47	66	30.10	30.10	NE	NE
Saturday	18	39	70	30.10	30.08	NE	NE
Sunday	19	45	70	29.94	29.78	W	WbN
Monday	20	47	65	29.69	29.69	W	NE
Tuesday	21	38	59	29.75	29.80	NE	E
Wednesday	22	37	66	29.90	29.90	W	NW
Thursday	23	42	66	29.96	29.96	NW	W
Friday	24	43	72	29.90	29.83	W	W
Saturday	25	46	73	29.72	29.58	SW	WbS
Sunday	26	45	69	29.62	29.62	W	WbS
Monday	27	44	66	29.66	29.66	SW	SW
Tuesday	28	47	63	29.66	29.57	SW	SE
Wednesday	29	45	69	29.58	29.60	WbS	W
Thursday	30	52	70	29.57	29.57	SW	SW

For July, 1825.

		Thermometer.		Barometer.		Wind.	
		Low	High	Morn.	Eve.	Morn.	Eve.
Friday	1	49	71	29 60	29.77	WSW	NE
Saturday	2	42	70	29.95	30.02	NW	NW
Sunday	3	41	72	30.05	30.00	W	W
Monday	4	55	71	30.00	30.08	W	W
Tuesday	5	52	71.5	30.12	30.12	W	NNW
Wednesday	6	53	68	30.06	29.99	WbN	NbE
Thursday	7	47	62	29.99	29.93	N	NE
Friday	8	50	62	29.93	29.93	NE	NE
Saturday	9	53	66	29.93	29.90	NE	NE
Sunday	10	46	75	29.87	29.85	NE	SE
Monday	11	48	78	29.83	29.83	SE	NW
Tuesday	12	55	80	29.83	29.83	W	W
Wednesday	13	58	78	29.90	29.92	W	W
Thursday	14	62	79	29.99	29.95	SW	S
Friday	15	57	84.5	29.88	29.89	S	WbS
Saturday	16	59.5	76	30.00	30.07	NW	W
Sunday	17	53	82	30.10	30.10	WbS	SE
Monday	18	56	84	30.08	30.04	SE	E
Tuesday	19	62	86.5	30.04	30.07	SE	E
Wednesday	20	62	75	30.19	30 12	SE	E
Thursday	21	54.5	68	30.12	30.12	E	E
Friday	22	43	68	30 10	29.95	E	NW
Saturday	23	50	63	29.93	29.91	NE	NE
Sunday	24	50	65	29.99	30.04	NE	NE
Monday	25	40	69	30.12	30.13	E	NW
Tuesday	26	50	67	30.16	30.16	NbW	NW
Wednesday	27	48.5	72	30.16	30.13	NE	ENE
Thursday	28	43.5	74.5	30 10	30.09	ENE	ENE
Friday	29	51	70	30.09	30.06	EbN	EbN
Saturday	30	39	73	30.03	29.94	EbN	SbE
Sunday	31	42	78	29.93	29.87	ESE	E

For August, 1825.

		Thermometer.		Barometer.		Wind.	
		Low	High	Morn.	Eve.	Morn.	Eve.
Monday	1	48	83	29.87	29.87	E	WbS
Tuesday	2	63	73	29.80	29.80	SSW	SW
Wednesday	3	52	72	29.80	29.71	SW	E
Thursday	4	59	72	29.42	29.26	SSW	W
Friday	5	51	68	29.22	29.42	W	W
Saturday	6	51.5	67.5	29.44	29.47	SW	WbS
Sunday	7	49	69.5	29.59	29.63	W	N
Monday	8	53	68	29.55	29.52	NW	NNE
Tuesday	9	56	67	29 52	29.55	W	WSW
Wednesday	10	42.5	65	29.55	29.60	S	S
Thursday	11	47	65	29 80	29.88	W	W
Friday	12	52	68.5	29.90	29.80	W	WNW
Saturday	13	55	69	29.50	29.29	NW	W
Sunday	14	56	67	29.27	29.33	W	W
Monday	15	51	62	29.30	29.45	NW	NE
Tuesday	16	56	68	29.64	29.68	NW	W
Wednesday	17	48	70	29.70	29.72	W	W
Thursday	18	54	66	29.80	29.98	NW	NE
Friday	19	52	66	30.06	30.06	W	N
Saturday	20	38.5	70	30.13	30.18	N	NW
Sunday	21	47	74	30.20	30.20	NW	NW
Monday	22	53	70	30.18	30.18	NW	E
Tuesday	23	53	72	30.06	29.98	E	EbS
Wednesday	24	47	69	29.98	30.00	EbS	NE
Thursday	25	47	68	30.00	30.00	NE	NE
Friday	26	44	70	30.03	30.03	NE	E
Saturday	27	49	62	30.00	29.93	E	E
Sunday	28	53	63	29.90	29.90	NE	NE
Monday	29	55	70	29.90	29.90	S	W
Tuesday	30	61	75	29 97	29.97	W	W
Wednesday	31	57	77	29.97	29.97	SW	SW

The First Course *of these Lectures will commence on* Tuesday, *the* 11th October, *at Nine in the Morning precisely. The* Second Course *will begin on the* Second Tuesday *in* February, *at the same hour.*

The Royal Institution.

ALBEMARLE-STREET.

PLAN

OF AN EXTENDED AND PRACTICAL COURSE OF LECTURES AND DEMONSTRATIONS ON

CHEMISTRY,

DELIVERED IN THE LABORATORY OF THE ROYAL INSTITUTION,

BY WILLIAM THOMAS BRANDE, F.R.S.,

Secretary of the Royal Society of London, and F.R.S. Edinburgh; Professor of Chemistry in the Royal Institution, and of Chemistry and Materia Medica to the Apothecaries' Company.

AND

M. FARADAY, F.R.S., &c.

These Lectures commence on the SECOND TUESDAY in OCTOBER, at Nine in the Morning, and are continued every Tuesday, Thursday, and Saturday. Two Courses are given during the Season, which begins in October, and terminates in June.

The Subjects comprehended in the Courses are treated of in the following order.*

DIVISION I.

OF THE POWERS AND PROPERTIES OF MATTER, AND THE GENERAL LAWS OF CHEMICAL CHANGES.

§ 1. Attraction—Crystallization—Chemical Affinity—Laws of Combination and Decomposition.

§ 2. Heat—Its Influence as a Chemical Agent in Art and Nature.

§ 3. Electricity—Its Laws and Connexion with Chemical Phenomena. § 4. Radiant Matter.

DIVISION II.

OF UNDECOMPOUNDED SUBSTANCES AND THEIR MUTUAL COMBINATIONS.

§ 1. Substances that support Combustion: Oxygen—Chlorine—Iodine.

§ 2. Inflammable and Acidifiable Substances: Hydrogen—Nitrogen—Sulphur—Phosphorus—Carbon—Boron.

§ 3. Metals—and their Combinations, with the various Substances described in the early part of the Course.

DIVISION III.

VEGETABLE CHEMISTRY.

§ 1. Chemical Physiology of Vegetables.

§ 2. Modes of Analysis—Ultimate and proximate Elements.

§ 3. Processes of Fermentation, and their Products.

DIVISION IV.

CHEMISTRY OF THE ANIMAL KINGDOM.

§ 1. General Views connected with this Department of the Science.

§ 2. Composition and Properties of the Solids and Fluids of Animals.

§ 3. Products of Disease. § 4. Animal Functions.

DIVISION V.

GEOLOGY.

§ 1. Primitive and secondary Rocks—Structure and Situation of Veins.

§ 2. Decay of Rocks—Production of Soils—Their Analysis—Principles of Agricultural Improvement.

§ 3. Mineral Waters—Methods of Ascertaining their Contents by Tests and by Analysis.

§ 4. Volcanic Rocks—Phenomena and Products of Volcanic Eruptions.

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* Mr. BRANDE's MANUAL OF CHEMISTRY, intended as a Text Book to these Lectures, is published by Mr. Murray, Albemarle-Street.

In the FIRST DIVISION of each Course, the principles and objects of Chemical Science, and the general Laws of Chemical Changes, are explained, and the phenomena of Attraction, and of Light, Heat, and Electricity developed and illustrated by numerous Experiments.

In the SECOND DIVISION, the undecompounded bodies are examined, and the modes of procuring them in a pure form, and of ascertaining their chemical characters, exhibited upon an extended scale.—The Lectures on the Metals include a succinct account of Mineralogy, and of the methods of analyzing and assaying Ores.

This part of the Course will also contain a full examination of Pharmaceutical Chemistry; the Chemical Processes of the Pharmacopœiæ *will be particularly described, and compared with those adopted by the Manufacturer.*

The THIRD and FOURTH DIVISIONS relate to Organic Substances.—The Chemical changes induced by Vegetation are here inquired into; the Principles of Vegetables, the Theory of Fermentation, and the Character of its Products are then examined.

THE CHEMICAL HISTORY of ANIMALS is the next object of inquiry—it is illustrated by an examination of their component parts, in health, and in disease; by an inquiry into the Chemistry of Animal Functions, and into the application of Chemical Principles to the treatment of Diseases.

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Art. I.—*On the Lignites. By* J. Mac Culloch, M.D., F.R.S., *&c. &c.*

[Communicated by the Author.]

The substances included under this term, form the link which, in some measure, unites coal and peat, so distant in geological time and place; partaking at one extreme of the mineral nature of the former, as much as at the other they approximate to the vegetable character of the latter.

The gradation of geological relations among these three combustible families is not indeed very perfect; for while peat is absolutely superficial in position, or covered with very slender deposites of recent soil, the lignites are found under alluvia of a high antiquity, or else are imbedded in the strata which succeed to the coal series. Thus, from the lignites to the proper coal, there is a regular gradation of geological relations; just as by means of the coal of the old red sandstone, we continue an analogous gradation through the primary strata. The mineralogical, or chemical transition is more perfect: and, in this respect, the lignites hold a station more nicely intermediate between coal and peat; some of the varieties approaching as nearly to the submerged wood of peat in their chemical characters, as they do in their appearance; while, in both these respects, there are varieties at the other extremity of the series, scarcely differing from coal, and finally identical with it. The term coal must therefore be taken in a geological light, as applied to that substance in a particular position, and not in a merely mineral sense. This seems the most convenient arrangement, while it also corresponds with the views of other geologists; and thus every stratum or series, generally considered as coal, which has been ascertained to lie in the strata above the magnesian limestone, or red marl, will be here considered as lignite.

While the superior antiquity of every lignite to peat, is proved by the comparative nature of its geological position in all cases, it is also inferred, from the more perfect change to bituminization which the vegetable matter has undergone. And while the production of the former appears to have ceased long ago, that of the latter is matter of daily observation.

Further, to prevent misapprehension respecting the meaning of the term, under the name of lignite are here included, together with all the coals just alluded to, those remains of trees or vegetables, which are buried under alluvial soil or very recent rocks, and which combine, with remains of the vegetable structure more or less perfect, marks of bituminization or charring; which yield bitumen, however modified, on distillation. Thus indeed, even the submerged wood which is found only under the very recent alluvia, and which is more or less connected with peat, would become necessarily included under that substance, whenever it corresponds in its chemical nature; were it not more convenient to distinguish it by geological characters, and to consider it a variety of peat.

The chemical nature of woody lignite will not perhaps be found, in every instance, to correspond as precisely with its geological position as, on this view, it should; and it is also possible that submerged wood may be so far bituminized as to be really undistinguishable from it. This indeed ought to be the case, if the gradation through the whole of these three combustible families is perfect. If it be perfect, the difficulty of distinguishing them, which would be the blemish of a mere arrangement, becomes valuable; as it establishes an important point, and constitutes a portion of the history of these substances.

Though the woody lignites may retain the marks of vegetable organization, they seldom possess the original form of the wood whence they were derived; although the monocotyledonous can frequently be distinguished from the dicotyledonous plants. They are commonly flattened, as if from the consequence of compression; while in very few cases are the forms of submerged wood altered in the same manner. But the essential chemical distinction already mentioned, generally serves to discriminate the lignites from submerged wood, whatever correspondence there may be in their forms and appearances. If, in practice, they cannot always be easily distinguished, it is a case which often occurs in mineralogy among undefined substances. In peat, essentially, no marks of bituminization are to be found, unless from the casual admixture of bitumen; and, for this reason, the chemical boundary between peat and lignite, is drawn where the commencement of bituminization is visible in the latter. As to the lignites which pass into coal, which are found in many of the stratified rocks beneath the alluvia, the mineral distinction is often nothing; and they can be classed under this head, only from their geological relations.

If the boundary of the woody or organic lignites, towards peat, is thus somewhat indefinite, it is far more indefinite towards coal. In a merely mineral point of view, however, the most obvious distinction would seem to consist in the mechanical, rather than the chemical nature of the specimens; at least, of those at the immediate point of transition. In most cases, the woody or first division of lignite, retains marks of the form of the tree whence it was derived, though it is sometimes found in a pulverulent state; whereas coal assumes the shape of a rock, losing the vegetable form, even where it still contains the remains of vegetables. But there is one modification in which it is difficult to determine whether the examples should be referred to coal, using that word in its mineral sense, or to lignite. This is the case of trap, where the specimens sometimes retain the vegetable form, though perfectly bituminized. Thus, this confusion of character, no less than the indefinite nature of lignite with respect to peat, completes that series of transitions by which the latter is connected with coal. These remarks apply solely to those substances considered as minerals. The geological distinction arising from position, is sufficiently marked.

In enumerating the different substances to which the term lignite has been most commonly applied, it becomes now necessary to add coal, without which, we should exclude some of the most important deposites of this combustible. The other varieties which have been enumerated by mineralogists, are brown coal, or Bovey coal, surturbrand, jet, and Cologne earth, or pulverulent lignite. Of these, however, the three first are not absolutely defined species; since, among various specimens of each, there are found differences sufficient to show that they tend to graduate into each other. Thus Bovey coal, as it becomes darker and more bituminous, passes towards surturbrand; while, between this species and jet, the distinctions are often very difficult to make. If those lignites which are here ranked with Cologne earth, are not always powdery, they are at least sufficiently tender to be easily reduced to that state. To these we ought to add, the casual specimens, of no decided character, which occur in many parts of the various strata; with the fragments of vegetables, the fruits, and other remains of a similar nature, that are generally described among the organic substances. The origin and theory of the whole is similar; but as there is nothing in these of sufficient importance to require a separate detail, they may be omitted from the present considerations, as casual fossils in the strata, of which the description belongs to the history of these rocks.

There are three obvious situations, sufficiently distinct in geological circumstances, in which lignite occurs; namely, in alluvial soils, under stratified rocks, and connected with rocks of the trap family. But these are insufficient for the present purpose; as the importance of some of the deposites requires that they should be distinguished by their more rigid geological connexions. Though I here omit the coal which occurs in the red marl, it ought, in strictness, to rank in this place; but being unimportant as a deposite, it may be neglected among the following more conspicuous examples of the substance.

The lowest in order is that which belongs to the lias and oolithe series, which, for the present purpose, may rank as one. Though some geologists have been unwilling to admit of more than one such deposite beneath the chalk, there can be no question that there is a second, which may be referred to the green sand. The third is situated above the chalk, in the plastic clay, and the last is that which occurs in the ancient alluvia. I might indeed refine on this division, by separately enumerating those which have been described in the red marl, in the muschelcalk, and in other positions, through the whole series above the magnesian limestone; as we might also find other deposites in the strata above the plastic clay. But such refinement appears unnecessary, except for local purposes, and it will be sufficient to have thus indicated such less important and marked examples. To these, however, must be added those lignites, which, if they do not form extended deposites, are important from their characters and positions, while they do not easily fall into any of the preceding divisions. These occur among the trap rocks. They have been called basaltic coal; but as this term has also been applied to the ordinary coal series, where interrupted by trap, the name of basaltic lignite will be preferable. Volcanic lignites, such as that of Iceland, need not be distinguished from this kind.

From the confusion which has been made between the coal beds here classed with lignite, and those of the proper coal series, it is not possible, or not safe, to quote examples in illustration of some of them; nor is it even in my power to produce adequate descriptions. A more perfect set of observations is yet wanting, and until then, this sketch must be doubtful, or imperfect. The student who may choose to adopt this arrangement, and follow the track of accurate observation, will hereafter be enabled to separate these from the genuine coal, and complete what I must leave imperfect. It must be always remembered, that as these deposites can be distinguished by the strata in which they are found, so, with strong resemblances, there is this radical distinction between them and true coal, that whereas the latter series contains no marine remains, these occur in all the lignites. Thus we are also led to a different theory respecting them, and to that theory which was once supposed to explain the great coal series itself. It is, that they have been chiefly produced by transportation; consisting of fragments of terrestrial vegetables deposited under the sea, or in lakes; though possibly also formed sometimes from marine plants. There is evidence of the former, independently of marine remains found with them, in the scattered nature of many, and in the numerous fragments of vegetables dispersed everywhere among the strata; the casually greater accumulations of these, in certain places, forming the more extended deposites.

There appears no difficulty in this theory, as it is a common case of transportation; and if certain beds or accumulations have been converted into true coal, while others preserve the characters of wood more completely, the solution will be found in an antiquity, and in circumstances, analogous to those under which the coal series itself was produced and consolidated.

It will be convenient, now, to give the geography of the chief lignites that are known, on account of the difficulty of determining to what position in the strata most of them belong. Where that appears really ascertained, these places will thus form points of reference, to stand in lieu of the descriptions which are yet wanting.

They occur abundantly along the western declivity of the Jura, in the south-west of Germany, abounding chiefly in Westphalia, and being wrought in the Buckeburg; as they are also in Coburg, and to the east of Spittelstein. Near Quedlinberg and Pirna, the same substances are scattered through the sandstones. It appears that at Coburg, they belong to the Quadersandstein, as seems also the case in other parts of the tract above-mentioned; so that we may perhaps be compelled to adopt a division prior to that of the lias and oolithe. In Istria, they occur in the oolithe abundantly, passing into coal at Carpona, and in the Island Veglia, where they are wrought for the use of the Trieste steam-boat. It is to the same series that we must refer what is called the Kimmeridge coal of England, consisting rather of bituminous shale, than of true lignite, and also that of the Cleaveland district of Yorkshire. Such also appears to be the coal field of Brora in Sutherland, of which the singular position on the granite has been noticed by myself elsewhere; and also, it is probable, that of the Western Isles of Scotland.

Of the lignite which occurs at Frankenberg, the geological position is doubtful; and it is found also in Thuringia, at Pernitz near Vienna, at Wolfseck, Wandorf, near Haagen, and in many other places, where it is wrought for coal; as is the case in Bohemia and Hungary; the mines of Buda, in particular, being remarkable. I cannot discover that the geological positions of these, and of some others which I need not quote, are clearly understood; but it is certain that some of them have been mistaken for the true coal series. The same error seems to have been committed respecting that of Bornholm, of which the correspondences extend wide over the north, as is also true of that at the foot of the Apennines, which occurs in the Vicentin and Veronese, at Castelnuova, and elsewhere, accompanied by trap. Some lignite beds occurring in certain parts of America, seem also to have been similarly mistaken; but I need not prolong the enumeration of localities among such obscure examples. If I name that lately found in the north, at Melville islands, it is chiefly on account of the presence of such a substance in regions of which the vegetation is now so cramped.

It seems now admitted by Brongniart, to whom we are indebted for a more accurate account of the following localities, that the supposed coal of the south of France is a lignite formation, occupying a higher part of the series than the last examples, and lying in the green-sand deposite. There are extensive mines of this in Provence, about Marseilles and Toulon, where twenty-eight beds are wrought; and it abounds also at Soissons, Epernay, Laon, St. Paulet, and some other places in France. That of Annecy in Savoy, which is also wrought for coal, is referred to the same position. So also is that of Putzburg and Lobsann, and that of Cologne, so well known and so often described. The principal deposite here, is thirty feet in thickness; and this locality is remarkable for its peculiar pulverulent lignite, so valuable in painting.

To the lignite above the chalk, are supposed to belong those immense deposites found in the middle of the Alps, and those of Styria, which are wrought for burning. These occur chiefly in the sands of the plastic clay, as it is thought; but it appears nevertheless certain, that some examples of this nature must be referred to a purely fresh water or lacustral origin. Those which abound in certain parts of Germany, as near Cassel and Meissner, are conceived to appertain to a formation of this nature, though lying in contact with the magnesian lime-stone; a situation not incompatible with such a geological position. Those also which are found in the basin separating the Alps and the Jura, at Vernier, Paudex, Vevay, near the lake of Zurich, at Oeningen, and elsewhere, including all the steinkohles of Switzerland, appear to be the deposites of a fresh-water lake in ancient times, as might be inferred from other circumstances attending this great locality. Those of Sheppey, the Isle of Wight, Sussex, and other analogous places in England, must, on the contrary, be referred to the marine deposite, the plastic clay. Thus the lignites above the chalk would admit of being divided into marine and fresh-water deposites; but while we must still leave the whole of the tertiary strata undivided, it is unnecessary, as it is unsafe, to attempt a more minute division here.

With respect to a theory of the fresh-water lignites, there can be no further difficulty than in the other cases; and if in any instances of such deposites above the chalk, or indeed in any other position, the magnitude may appear revolting to the theory of transportation, it must be remembered that they are easily explained by the fall of forests on sea-shores, and that such tracks of submerged wood as that of Lincolnshire, are precisely what, at a more distant period, would have furnished the very beds of lignite in question. How far peat, both terrestrial and maritime, may have aided, is almost too obvious to hint; nor is it necessary here to draw the conclusions which will follow, without even an inference, from the facts on this subject that appertain to the history of Peat, and of which I have treated in another journal.

Having thus given such localities as seemed sufficient for examples, and for indicating both the general positions of these deposites and the errors which have been committed respecting them, I may add a sketch of what little is known with regard to the general characters of each class or division.

The lowest deposite, which is called that of the oolithe, and which may include all that are found, from the magnesian limestone upwards to the green sand, is more frequently perhaps akin to coal than to the woody lignites, though the latter substances occur also in various forms. In some places it exists in the strata, in groups of different dimensions, or in scattered fragments, or else in thin and partial beds. In others, it forms regular beds of coal, of various, and sometimes of considerable thickness; and where these alternate with the shales, sand-stones, and lime-stones of the series, the superficial aspect is so much like that of the regular coal series, that it is not surprising if it should have been mistaken for that deposite. This coal is, however, generally, or perhaps always, accompanied by woody lignites, commonly in the state of charcoal, dispersed through the accompanying rocks. In the example in Sutherland, one of the beds is three feet or more in thickness, while another does not exceed an inch. The animal remains yet discovered are not numerous, but they are marine; and hence one of the essential distinctions between this and the proper coal series. It is probable that the supposed existence of marine shells in this last, has generally arisen from confounding it with the lignite coals under review. Oysters and ammonites have been found among them; and, in Sutherland, the shells, though often obscure, appear to be in addition to those, madreporites, the spines of echini, belemnites, terebratulæ, and fragments that may belong to myæ and mytili, and to cardium or some similar shell. It is obvious, however, that whatever obscurity there may be as to individual specimens, the shells to be expected are those which belong to the strata in which the lignite lies.

The lignite, as it occurs in the green sand, is said to exist rather in heaps of fragments than in proper beds, but the descriptions of individual cases are imperfect and unsatisfactory. In the Isle D'Aix the accompanying substances are sands, marls, and cherty flints, with quartz, agate, pyrites, and resinous substances. The wood is that of dicotyledonous plants, and it is said that no palms have been found. This wood is sometimes silicified, at others fibrous, or in the state of jet. Fuci also are said to occur, and the shells are all marine, consisting of nautilites, pectinites, gryphites, and others. With respect to these fossils, the same general rule may be applied as in the former case.

The deposites of lignite found in the strata which succeed to the chalk, or in the tertiary or fresh-water formations, have recently experienced considerable attention, particularly in France; though our general knowledge of them is still necessarily limited. The period of this deposite is sufficient to distinguish them from those which belong to the inferior strata, or to the alluvial formations; and their origin and causes are similar to those of the animal remains in the same situations. It is unnecessary to say that the lignite may occur in any of the strata that belong to this series, and equally unnecessary, here, to give more than this general indication, as the peculiar cases must be sought in local descriptions.

Besides the several mineral substances, or rocks accompanying them, which need not now be repeated, they are, in different places, found attended by quartz, agate, calcareous spar, sulphate of strontian, pyrites, and hydrated iron, together with amber, and imperfectly-bituminized resins, such as those of Bovey and Highgate.

The lignites themselves are sometimes fibrous and woody, while at others they pass into jet, and even into coal, being further occasionally silicified. Leaves and fruits occur, together with woody stems; and these can be referred to both dicotyledonous and monocotyledonous plants. The hitherto imperfect arrangement of fossil vegetables has referred them to terms rather than genera, under the names of carpolithes, phyllites, lycopodiolithes, palmacites, endogenites, *&c.*; nor is it here necessary to quote the more minute distinctions under which it has been attempted to divide and arrange them. The fossil animal remains, accompanying them, have been noticed by so many writers, that it is unnecessary to give a list of them.

The last division of the lignites is that which occurs in alluvial soils, and which seems necessarily limited to the more ancient alluvia. The scattered specimens, of various character, dispersed in the upper or alluvial parts of the tertiary or fresh-water strata, must be ranked in this division, but we are not well furnished with recorded examples of this nature. I must follow the prevailing opinions, in referring the well-known case of Bovey to this division, though by no means satisfied that it is a real instance of an alluvial lignite. For want therefore of satisfactory observations, we can only conjecture generally what the characters of such a deposite ought to be, and what are the accompanying substances. It is to be suspected that, though Bovey should be a real example, many of the supposed alluvia, containing amber and jet, described by authors, are rather cases of the tertiary strata beneath; and that perhaps the sands of the plastic clay, or other portions of these deposites, have been mistaken for alluvial formations.

There is another difficulty respecting the descriptions of the alluvial lignites, lying at the opposite extreme. Among the numerous examples of forests submerged under alluvial soils, it is not often that observers have inquired whether the wood possessed the characters of lignite, or was simply in the state in which the wood of peat-bogs is found, namely, little changed from its original condition, or else partially converted into peat. In other cases, however, it has been ascertained that the wood was deranged in form, or flattened, as if from the consequence of great pressure; while, at the same time, it exhibited those peculiar chemical properties by which the lignites are distinguished from peat. In such situations it has been found that the lignite varied in quality, or in the degree in which it had undergone the change by which it approximates to coal; containing more or less bitumen intermixed with the peculiar compound that constitutes the ligneous matter of wood which has partially passed from the vegetable state to that of peat. In these cases also it appears that the several kinds are found at different depths; those most deeply situated being at the same time the most highly bituminized. The strata at Bovey, just quoted, offer examples of this nature, if they are really examples; and, at this place, different strata are found separated by the loose matter which fill this valley; the more perfect lignites occupying the lowest positions.

It is plain that this and similar cases present examples of submerged forests, which, no less from the nature and depth of the alluvia under which they lie, than from their having undergone that change to bituminization which does not occur in the submerged wood of peat-bogs, are probably of a much higher antiquity than any of those deposites with which we are acquainted; or, at least, older than any of those peat-bogs which now occupy the immediate surface. It is here, undoubtedly, assumed, that antiquity alone, or time, superadded to the causes which convert wood into peat, is sufficient to convert it into the same bituminous compound that forms lignite; but it is possible that, independently of this, certain chemical agencies, of which the nature is not known to us, may have aided in the operation.

The succession of strata at Bovey, or in other situations where similar phenomena occur, might lead to the opinion that successive forests had grown and been submerged in these particular spots; and, if it be true that time alone is the agent which in these places tends to render the lignites perfect, this conjecture would receive confirmation from the more perfectly-bituminized nature of the deeper strata. But respecting this, it is impossible to come to any decision; as successive strata of wood and alluvial matter might have been deposited by the transportation of both from a distant point; while, if something more than time alone is required for the perfection of lignite, these cases might have produced an effect on the deeper strata from which the more superficial have been comparatively exempted.

The depth of the superincumbent strata, and the number of alternations in these cases, present considerable variety; but observations have not as yet been sufficiently multiplied on this subject, to admit of any general conclusions deserving of much regard. The total depth at Bovey, is said to be seventy feet; this space including all the lignite, together with the alternating beds of clay. In Iceland, it appears to be generally much more superficial, and is found in the form of boards, as if produced from the trunks of trees flattened by great pressure. This is the variety to which the name of Surturbrand has been particularly applied. That of Bardestrand is found on a hill of moderate elevation, beneath strata of sand and clay alternating with peat; and here, as at Bovey, the upper beds are imperfect, the middle intermediate in quality, and the lowest most complete. At Arnafiord, it is accompanied by shale, containing bones, with fragments of branches and roots. I need only add here that the position of Jet is sometimes analogous.

It thus appears that the species of lignite buried under alluvial soils, occur in the several forms of brown coal, or common woody lignite, pulverulent lignite or Cologne earth, surturbrand, and jet. To whatever different circumstances these variations may be owing, the differences, in a chemical view, are very considerable, and imply either a longer exposure to the causes by which the changes have been induced, or a much more energetic action of these.

It is difficult to form any conjecture respecting the date of these deposites, as they regard the disposition of the present surface of the earth. Inasmuch as they are superior to all the rocks which they accompany, they may correspond to the general or diluvian deposites of alluvia, while they may sometimes also have been formed in estuaries at later periods. Even where they have been produced by the consequences of volcanic eruptions, they must be of a comparatively recent origin. The first species, if it exists, must be ranked in point of time with the remains of extinct quadrupeds; and thus, possibly, this is also limited to particular situations, in consequence of circumstances analogous to those which have determined the accumulation of these in particular spots.

Considering the numerous examples of ancient alluvia of this nature which are known, it must appear remarkable that the occurrence of lignites among those is so rare. It is difficult to conceive that small portions only of the surface were covered with trees at the period of these alluvial formations, and equally difficult to comprehend by what means the deposition was limited to so small a number of places. The analogy of similarly limited deposites of animal remains, does not assist in explaining it; as, while these have a tendency to inhabit particular spots, forests are limited to no place. For the present, this difficulty must remain unexplained; like many others of which time is every day diminishing the number.

It may be asked, perhaps, why deposites of wood of a high antiquity do not always assume the character of coal, when antiquity alone has been supposed sufficient to produce the completely bituminized state of lignite. But, in fact, as far as relates to chemical composition, there is often little difference between such ancient lignites and coal; and it even happens among the more recent which belong to the alluvia, namely, in jet, that the chemical nature is in no way different from that of coal. In such cases, the true distinction of coal is its mechanical texture, or its rocky or stony character, if such a term may be used; and it is easy to imagine how that might have been induced under the peculiar circumstances under which beds of coal occur, and in which some of the lignites have participated. It will also be found, that many varieties of coal, marked by a peculiar fetid smell, of which the nature had not hitherto been properly examined, owe that property to their participation in the chemical nature of the imperfect lignites, still containing a portion of that unchanged vegetable matter from which the fetid smell of the lignites on burning is derived. Such coals are, in fact, mixtures of lignite and coal, if such a method of expression may be used in a case where two substances pass into each other by imperceptible transitions; the characters which belong to the form and structure justifying the name, either of coal or of lignite, as it may happen, while there is an identity or resemblance in the chemical characters of both.

The last geological situation in which the lignites occur, is among the trap rocks; and as they have either not been correctly examined, or have been the objects of misapprehension, it is necessary to be more particular in considering them. These have been called, by some, bituminized wood, by others, basaltic coal; but it is more convenient to adopt the present term.

They sometimes retain, in a considerable degree, the chemical characters of vegetables; giving out the particular volatile products on distillation, and the smell on burning, by which the other lignites are distinguished; while, at other times, they are in every respect perfectly converted into coal. It is in the former case chiefly that they retain the vegetable form and structure; but they do not always lose it when they have become perfect coal. Whether more or less distinct in character, they sometimes occur in insulated fragments or as portions of trees; at others, they are accumulated in a particular place, so as to form small deposites of an irregular nature, in which cases the vegetable form disappears, or becomes very obscure. When such deposites are of considerable size, they often put on the rocky character with the chemical nature of coal, and are sometimes partially wrought for economical uses. In one instance, which occurs in Mull, the specimens of lignite form a large portion of a trunk of a tree; in which the vegetable texture is perfectly distinct, but the substance so tender, as to fall into powder by a very slight force, resembling some of the specimens from Cologne.

The circumstances under which the rocks of the trap family have been produced, render it necessary to state very accurately the exact connexion subsisting between them and the lignites by which they are accompanied. This is especially necessary, because they have frequently been used as an argument against the igneous origin of the trap rocks on the one hand; while, by another party, they have, under equal misapprehension, been supposed to prove that origin, and further to justify the application of an analogous theory to the formation of common stratified coal. It will be seen that they prove nothing on either side; but are in a great measure independent of the peculiar circumstances under which the trap rocks have been in a state of igneous fluidity.

Lignite is often found in the conglomerates which accompany the trap rocks, and which are commonly known by the name of tufo or trap tuff. These consist of fragments, generally of trap rock alone, but sometimes admitting other substances; and they are commonly of a tender texture, or imperfectly connected by minute fragments and earth, or powder of the same nature. Such conglomerates often contain rounded materials; proving that some time and some transportation, or motion, have taken place in the parts previously to their consolidation.

It is not difficult to understand how fragments of wood may have been introduced among substances which have once been on the surface of the earth in a loose state; and how the whole surrounding mass may have been consolidated, so as to include them. It is very certain that the consolidation has not, in these cases, resulted from fusion, or from any high degree of heat; as the very fusible sand which forms the basis of such conglomerates, could not have borne such a heat, and have retained its loose texture. I need not here enter into any details of the modes in which solid trap may have been deposited on such tufaceous or conglomerate strata, as it must already be understood from the history of the trap rocks which has been given elsewhere. It is certain that such crystalline traps do lie upon loose tufas; and that as these could not, from their nature, have been subjected to any high degree of heat, the lignites which they contain, have, in the same way, escaped the effects of fire. This fact, therefore, does not prove that the solid trap rocks have not been in fusion, but merely that the tufas have not been so formed; of which, indeed, no other evidence than their own structure is necessary. Neither does it prove that the wood has been bituminized by the action of fire; from which it must on the contrary have been, like the surrounding materials exempted; while the reasons of a chemical nature will hereafter be adduced to shew that this result cannot be produced by the action of fire alone.

In other cases, where the lignites of this division are found in the same connexion, they are not contained in beds of tufa, but in veins or chasms in the solid rock. The specimen, already referred to in Mull, is a remarkable example of this nature, as well for the erect position of the specimen, as for the magnitude of the fragment. In these cases, also, it has been carelessly said, that the lignite was contained in the solid trap; but in all the instances which I have observed, it has occurred in a tufaceous conglomerate which filled the remainder of the vein, or cavity, in which it was found. And thus, I have little doubt, it will be found to have occurred in all parallel cases; in which, either from superficial observation, or from the tendency by which every one is influenced to overlook circumstances which disagree with a favourite hypothesis, this important circumstance has been neglected. Under whatever state of things such conglomerated or tufaceous cavities have been formed, it is evident that they have been exempt, like tufaceous beds, from a degree of heat capable of fusing them, and, in the same manner, the accompanying lignites have escaped total destruction, if indeed destruction be a necessary consequence of that state of things.

The last position, in which I have observed lignite in the trap-rocks, appears rather more difficult to explain on the hypothesis of their igneous origin; but, when strictly examined, it does not seem by any means inexplicable.

In these cases, the substance in question appears absolutely imbedded in the solid rock; and it must certainly be admitted, that, were this truly the case, it would present a considerable difficulty to the theory which supposes all the crystalline traps to have been consolidated slowly from a state of igneous fluidity. But when strictly examined, in all the instances of this nature which I have witnessed, it will be found that the substances in the immediate vicinity of the specimens are not crystalline, are neither greenstone nor basalt. In general, some fragment or stratum of shale will be found to surround the specimen, or else it is entangled among earthy substances differing from the including rock, and consisting of portions of those strata which have, perhaps, by their fusion, produced the principal mass, or which have been entangled in it during its state of fluidity. The unaltered or slightly modified state of those substances suffice to prove, that in the particular place in question, the heat has not been sufficient to effect the fusion and ultimate change of the original strata into crystalline trap, so as to produce those final consequences which might be expected to have resulted from its having been entangled in a fluid mass.

Even, however, if lignite should be found surrounded by a crystalline trap, or a basalt, it does not by any means follow that the rock has not been in a state of fusion, or at least softness, sufficient to enable its parts to assume a crystalline character. In the siliceous schist which lies beneath basalt, the character of the rock is sufficient to prove that it has been, in a softened state, a state capable of producing that difference which is found to exist between it and the original substance from which it was derived. Yet, in such cases, shells are often found entangled in the mass; and though sometimes deformed by pressure, as the lignites themselves are, still retaining their integrity, and a considerable portion of their original characters. Neither is the bituminous matter which is contained in strata of this nature, dissipated in such cases, since both the limestones and the shales found in these situations are often highly bituminous. Charcoal also, it is well known, is indestructible by heat, when protected from the access of air: and though altered in its chemical character by the loss of its hydrogen, in some such cases, it still retains its form. It is therefore perfectly conceivable, that, where charcoal could retain its form, and bitumen exist, the lignites might remain surrounded by a mass of fluid trap till it had cooled, in all the situations in which it has been found among these rocks; as coal also unquestionably does, where the ordinary coal strata are found in similar circumstances.

Whether this particular case has or has not occurred in trap, it is known to have happened with lava, so that the difficulty is completely removed. In Italy, trees have been found entangled in perfect lavas, having burnt out only in those superficial parts where there was access of air; and, in the Isle of Bourbon, the trunks of palms have thus also been observed wrapped in lava, so that the stony matter had penetrated the fissures formed in them by shrinking, and assumed the shapes of these.

It is thus also not difficult to conjecture why, in certain circumstances, the lignites of trap should preserve their character in a considerable degree of perfection, retaining a great proportion of the original structure and chemical nature of the vegetable; while, in other cases, they should have been entirely converted into coal. In the lignites of Meissner, the woody kinds are actually changed into coal where they are in contact with basalt. But this part of the chemical nature of the lignites will be examined immediately, when some direct experiments, explanatory of this circumstance, will be related. It is sufficient now, barely to say, that the degree of pressure under which they must have existed, may have been sometimes such as to prevent the entire dissipation of the volatile matters contained in them, and that the degree of heat and of pressure, having been present in proportions inferior to those which were required for the fusion of the mass into shapeless coal, the vegetable structure has been preserved, exactly as it would have been in charcoal, under similar circumstances; while the form has perhaps also, in many cases, remained more entire, in consequence of the protection afforded by unfused fragments of strata, or by the earthy original matters entangled together with it in the trap.

If these remarks on the association of the lignites with the trap are just, it must now be obvious that this circumstance presents no valid objection to the igneous origin of trap. But that it does not prove coal to have originated in the action of fire on vegetable matter, is a separate question, which will be examined when the chemical nature of the lignites is considered. In the mean time it will not be uninteresting to inquire under what circumstances the trap rocks can have entangled the remains of trees or other vegetable matters.

Because these rocks are often connected with conchiferous strata, and because they have been supposed never to contain air-vesicles, like the lavas, it is conceived that they are of a submarine nature, or, at least, that they have been formed under the rocky strata, or under some other considerable pressure. It is not very easy to understand how, under some of these circumstances, they could have entangled wood, though fragments of this might have been present at the bottom of the sea; either from ordinary transportation on the surface, or enclosed in the loose alluvial matters, and possibly in the previous state of lignite. I have already shewn, in other places, that the trap rocks have, like lavas, sometimes flowed on the surface of the land, since they contain vesicles, and that they are then the remaining rocks of ancient and far distant volcanoes, of which all the looser and more destructible testimonials have been removed by the lapse of time. Under these circumstances, it is not difficult to understand how they may have entangled lignites, or wood imbedded in alluvial strata; and thus perhaps it is most easy to account for these cases, and particularly for those in which the lignites are found in tufaceous strata surrounded by trap. But it is also plain, that, as in the case from Meissner just quoted, the irruption of trap into strata already containing lignites, of whatever age, might have produced similar appearances; the clays or marls undergoing the changes which commonly result from this cause, while the vegetable or coaly substances would thus become involved in the trap itself, or in the rocks which it had modified.

It is now necessary to consider the chemical nature of the lignites, and to state the circumstances in which they vary from peat on the one hand, and from coal on the other. This examination, I must premise, relates, however, chiefly to those which still partake of the nature of wood, or which are lignites, mineralogically speaking. Where they are but geologically such, and have become true coal in character, they require no distinct notice. As I have shewn that they approximate to the former substance through the intermediate stage of submerged wood, and that a sort of gradation may consequently thus be traced from the living vegetables to lignite, it will be convenient to give a slight sketch of the chemical circumstances in which peat itself differs from recent vegetable matter.

If recent vegetables be decomposed by the ordinary process of destructive distillation, they are resolved into carbon and hydrogen principally, together with certain inferior proportions of oxygen and azote; the earthy and saline matters which they may contain being here neglected, as not entering into the present consideration. These elements are not, however, obtained in a simple state by one process. Various compounds are first procured, which may, by a repetition of similar methods, or by other well known means, be at length resolved into their simple elements. Thus the hydrogen and carbon are found in the state of carburetted hydrogen of various composition, and also in a solid form; producing compounds of various kinds, into which oxygen also occasionally enters, so as to produce a substance analogous to the resins, and which I have described in the *Transactions of the Geological Society* by the name of Bistre. Acetic acid and ammonia are, in a similar manner, obtained; and, from the proportions of these different products, it is easy either to estimate, or else from their analogies to discover, the relative proportions of the several ingredients; within limits, at least, sufficient for the present objects.

Now, in examining peat, and in comparing the relative proportions of these different ingredients, it is discovered that those of the oxygen, and of the azote, which is always in very small quantity, are diminished, and that a new compound, which is the basis of peat, has been formed of the hydrogen and carbon, containing also a small proportion of oxygen. In tracing also the varieties of peat, from the first change which the vegetable has undergone to the most perfect specimens of this nature, it is discovered that the proportion of oxygen experiences a progressive diminution, but is never entirely absent. The same reasoning applies to the submerged wood of peat, which is here distinguished from the lignites.

It is sufficient to have thus briefly stated these circumstances relating to the chemical composition of vegetable matter and of peat; but it is necessary, in practice, to adopt a more compendious method of estimating the nature of the compounds subjected to investigation, particularly where the lignites are to be examined for the purpose of discovering the condition in which they stand relatively to peat, placed at one of the extremities, and coal, standing at the other. It was mentioned that a resinous matter was obtained from wood or peat, on distillation, which is distinguished by the name of bistre; but that substance is obtained, in the first instance, only in combination with such proportions of hydrogen and oxygen as materially alter its character; or, rather, it is procured as the residue of other distillations, by which the compounds of a fluid nature, first procured, are decomposed. A certain proportion of acetic acid and carburetted hydrogen being freed, according to the quantity of heat and the mode in which it is allowed to act on the first volatile products, a liquid matter resembling tar is procured; which, on subsequent distillation, gives over a volatile oil and acetic acid, the resinous matter remaining in the retort. By still urging the heat, additional quantities of tar are produced, and thus the process may be continued, until charcoal alone at last remains; the oxygen as well as the hydrogen being at length entirely dissipated, first in the compounded states already indicated, and ultimately in the form of gases.

The whole of this process presents an exact analogy to that which happens in the case of turpentine and of the bitumens; and it illustrates the nature of both these substances, of which the composition and general habits so much resemble each other. It is unnecessary to dwell on these differences with chemical minuteness, but it may be stated generally, that they appear to consist in the different proportions of the several ingredients in these analogous compounds, as far, at least, as this species of analysis enables us to discover at present. Now, as the bistre, the tar, and the volatile oil of wood, and of peat, resemble the resin, turpentine, and oil, of all the resinous compounds, so they are precisely analogous, in their relations to each other, to the asphaltum, petroleum, and naphtha of the bitumens. As it is with this latter comparison that this investigation is principally concerned, the object being to discover the differences between woody lignite and coal, it is necessary to state more particularly the circumstances in which they differ.

In the distillation of bistre, or of the vegetable compound of carbon and hydrogen, a considerable proportion of acid is always obtained, indicating the presence of oxygen; but in the distillation of bitumen, it is either not obtained, or is in a quantity so small as to bear no comparison with the produce in the former case. Thus the bitumens seem principally compounded of carbon and hydrogen, while the bistre, and the tar of vegetables, and peat, contain a notable proportion of oxygen. As the different varieties of lignite, therefore, approximate on the one hand to coal, and, on the other, to peat, or unchanged vegetable matter, so they will be found to yield, on distillation, mixed products, varying in quality according to the position of the specimen between those two extremes. The ultimate analysis of such products proves that which may easily be inferred from the preceding remarks, namely, that the greatest proportion of oxygen is found in those lignites which are the least removed from submerged wood or peat, and the least in those that approach nearest in their aspect to coal. Thus the pale lignite of Bovey yields a larger proportion of oxygen; while the analysis of jet differs but in a slight degree from that of bituminous coal.

These differences, indeed, can be rendered perfectly visible by much ruder experiments; as, without the necessity of a complete decomposition of all the products, the first results of distillation are sufficiently indicative of the relative place of any specimen towards peat or coal, or of the extent to which the process of bituminization has taken place in the lignite under trial. There is, in the first place, an easy, though a rude test, to be derived from the odour of the volatile matters: that of wood, or of peat, in burning, is well known, as is that of coal; and the peculiar smell of either of these substances is most highly sensible in the volatile fluids which may be condensed from their distillation. In the lignites, it will be found that the odour, on burning, differs in the several varieties; and that, in all, it is different from those, either of coal, or of peat, or of the ligneous fibre. If the sense of smell is really capable of thus analyzing compound odours, it appears to be a mixture of those derived from both these sources; and it is at least abundantly sensible that the odour of burning jet is not far different from that of coal; while that of the pale lignites is of a very different nature, as if from a predominant quantity of the smell of wood-tar, or the volatile oil of the distilled ligneous fibre.

The differences in the relative proportion of acetic acid, produced by the different lignites, is also remarkable, as it diminishes exactly in the degree to which they approach to coal, being greatest in the pale varieties, and least in jet; and indicating, without further analysis, the different proportions in which oxygen is contained in the several kinds. Lastly, an easy test of the degree to which the process of bituminization has advanced in the lignites, is to be found in the nature and properties of the pure volatile oil which they yield on distillation.

The distillation of coal, or petroleum, with a regulated heat, is well known to produce naphtha, a colourless volatile oil, which, as it does not act on potassium, is presumed to contain little or no oxygen. By distilling the tar of wood, or wood itself, with similar care, an analogous oil is produced, colourless also, but distinguished from the former, not only by its very peculiar odour, but by its chemical properties. Although these two oils are miscible in any proportions, yet while naphtha is a ready solvent of the asphaltum, which remains after it is separated from the petroleum, it will not dissolve bistre, or the analogous solid product which remains after separating the oil of wood from the vegetable tar. In the same manner the oil of wood is not the proper solvent of asphaltum. Both these substances are, however, soluble in the mixed oil, of which the smell is also very peculiar, and after some experience, easily recognised.

Now, in distilling the lignites, it will be found that the volatile oil differs in smell, both from naphtha and the oil of wood, and that the odour is not unlike to that produced by mixing them, while, at the same time, it is capable of dissolving a certain proportion of both asphaltum and bistre. Thus it may be inferred, as well as by a more operose and perfect analysis, that the lignites hold an intermediate state between wood, or peat, and coal, or bitumen; and the degree in which they approach to perfect bituminization may be appreciated in a manner sufficiently accurate for general purposes, by the relative power of their volatile oils in dissolving bistre or naphtha.

This view of the mixed, or intermediate nature of the lignites, is confirmed by Mr. Hatchett's experiments on the submerged resinous matter found with the lignite of Bovey, which appears, in a similar manner, to be either a compound or a mixture of vegetable resin and asphaltum, and of which other specimens, found in different places, have exhibited characters intermediate between resin and amber.

In a former essay on peat, published elsewhere, it was argued that the process of bituminization was a result of the action of water, not of fire or heat, and that time alone seemed requisite to convert peat into bituminous lignite, and ultimately into jet, if not finally into coal. Not to repeat this, I shall therefore here content myself with shewing that the presence of bituminized lignite, or even of coal, in the trap rocks, is no proof of the effect of heat in bituminizing the vegetable fibre; but that, in this case also, the charge is probably alike owing to the action of water.

It was shewn that in many of the cases in which bituminous lignite was entangled in the rocks of the trap family, it was actually contained in earthy, tufaceous, or conglomerated matters, which had undergone the action of water. It is easy to conceive that in such cases it had been bituminized before the deposition of the solid trap, having been buried in the earthy strata invaded by it. While, therefore, this circumstance offers no argument against the igneous origin of the trap rocks, neither does it prove that the action of heat is required to convert vegetable matters into bitumen. By direct experiment, indeed, the power of fire to produce this consequence is disproved, as its effects on the vegetable fibre, whether under pressure or not, is to convert it into bistre, and not into asphaltum or coal. Yet it must be admitted that, in those cases where wood is so situated in trap rocks that it must necessarily have been decomposed by the action of the heat, it has actually undergone a change of form, and has thus been converted into an inorganic substance. This substance, however, according to the direct experiments above stated, could not even then have been coal, but bistre, or a mixture of that substance with the superfluous charcoal of the wood. It is probable that it is this substance which has produced the coal now found in trap, and generally more or less connected with indications of lignite, or of a vegetable structure not totally obliterated.

Whether the long-continued action of a slow heat during the tedious process of cooling such masses of trap may not have converted such bistre into coal, is a question which, for want of power to make similar experiments, cannot be determined. But, certainly, the action of water is not precluded, even in these cases; as is proved by the quantity of water which is found mixed with the substance of trap, as well as of the stratified rocks, and which is rendered abundantly certain by the formation of some of the amygdaloidal nodules, by the existence of water in these cavities, and by the deposition of stalactitic chalcedonies in them.

Whether this view be conceived probable or not, it is not to be doubted that, if the resinous exudation of trees may, by access of moisture, be converted into retinasphaltum, and probably into amber also, bistre, first produced by the action of fused trap, may in a similar way be converted into coal. In either case, moisture alone, not the free access of water, is equally sufficient, and, in both situations, it is present.

If I have thus shewn that lignite, considered as a mineral, is a state of the vegetable matter intermediate between peat and coal, and that it occupies a certain place in the progress of bituminization, it is equally easy to account for the various approaches which that substance makes to peat on the one hand and to coal on the other; in this last case, becoming a coal as perfect as that which belongs to the proper coal series beneath the magnesian limestone. And it is also thence a natural expectation, that the deposits of these bituminous matters which are deepest situated in the earth, or which possess the highest antiquity, should present most perfectly all the characters of coal, not only in their structure and appearance, but in their chemical nature. And it will thus generally, I believe, be found, that the most deeply seated, here ranked in a geological sense with lignite and not with coal, are the most purely coal; although it is easy to understand how numerous exceptions may have arisen from other causes of a local nature which it is unnecessary to suggest. Yet among these coals, so considered in a popular view, it will often be observed that there is a peculiar smell on burning not found in the purer coal of the great series; a smell produced by a mixture of that oil just mentioned, which is obtained from peat and wood, and which indicates a bituminization not absolutely completed.

Art. II. *Observations on the expressed Oil of the Seed of Croton Tiglium. By* John Frost, F. L. S., M. R. I., *&c.*

[Communicated by the Author.]

Of all the medicines lately introduced, perhaps none has excited more attention than the expressed oil of the seed of the Croton Tiglium.

In a former Number of this Journal some experiments appeared concerning the component parts of the oil as to inert and active matter, which induced me to repeat them, as well as to make some others which I purpose giving in detail. About five years ago, when the use of this oil was revived, (for it certainly was no new medicine, in proof of which I beg leave to refer to a paper which I published in the year 1822, in the seventeenth volume of the *Medical Repository,*) it attracted particular notice on the part of the medical profession, on account of the smallness of the dose (one drop) acting with such certainty as well as violence; indeed I have lately seen a case of enteritis, in which it acted after enemata, colocynth, *&c.* had failed. It has been stated with great truth, that one drop, merely applied to the tongue, will produce violent purgative effects.

It has been recommended to administer the oil in the form of draught, by combining it with mucilage of gum arabic and mint-water by some, and an alcoholic tincture by others; both which modes of exhibition are in the highest degree objectionable, as they produce a great sensation of heat about the fauces, which can be traced by the patient throughout the alimentary canal; therefore, the assertion of Linnæus * in his *Materia Medica* appears perfectly correct. The best manner of giving it is in the form of a pill, as by that means the unpleasant feeling about the throat is avoided. The tiglium seed oil which is on sale is frequently admixed with olive, castor, or rapeseed oil, which in a medical point of view is rather an advantage than otherwise, as it tends to moderate the violence of its action.

The genuine oil is so powerful as to produce death in the dose of a very few drops, but different samples vary in point of strength, which of course depends on the rate of active matter which they may contain. It is to be remarked that all those authors, who have treated on it, have cautioned their readers concerning its use. Lewis in his *Materia Medica* has committed a strange error with regard to the dose. He says that *one drachm* must be the dose, and not a drop. We must, therefore, conclude that he was totally unacquainted with the article about which he was writing, as such a quantity would speedily terminate the life of the person to whom it might be administered. The plant is a native of the East Indies; it is a shrub seldom exceeding ten feet in height. It belongs to the twenty-first class Monœcia and the eighth order Monadelphia of Linnæus, and to the natural order Euphorbiæ of Jussieu.

The male flowers consist each of a cylindrical calyx, which is five-toothed. The corolla consists of five petals of a straw colour, and there are from ten to fifteen stamina. In the female flowers the calyx is many-cleft and reflected under the germen. There is no corolla, but there are three bifid styles. The capsule is trilocular and smooth: each loculus contains one seed. The seeds are

* Tiglii grana.
Qualitas, acerrima
Vis, emetica, drastica, exurens.—Vide *Linn. Mat. Med.*, p. 236.

somewhat concave on one side, and convex on the other; and they are of a brownish yellow colour. The leaves are pointed, nerved, serrated, supported on long petioles, and stand in alternate order *.

But, to return to the object of this communication, I would observe, that I found that the expressed oil of the seed of this plant was entirely soluble in æther and the oil of turpentine, and partially so in alcohol. One hundred grains of the seed consisted of

32	shell
68	kernel
100	

One hundred grains of the seed were digested in three drachms of sulphuric æther, sp. grav. 71, and afforded 25 grains of fixed oil.

Thirty-two grains of the oil were put into a Florence flask, containing some alcohol previously digested on olive oil (as recommended in a former Number of your Journal), to prevent (as it is stated in the experiment alluded to) the spirit from dissolving any of the oil of the croton tiglium seed. The mixture was now agitated, and then passed through a filter containing carbonate of magnesia. The filtered solution was now evaporated without heat, and yielded—

	Grs.
Active matter (soluble in alcohol and æther) combined with a very small portion of fixed oil.	8.5
Inert fixed oil	23.5
	32 grains.

It would appear from some experiments lately made in the manner above practised, that one hundred grains of the oil of the tiglium seed afforded forty-five grains of active matter, but I must confess that I cannot find any sample of oil that will furnish so great a per centage. Now as olive oil is not by any means so soluble in alcohol as castor oil, incorrect results must ensue, in consequence of the alcohol not being fully saturated with a fixed oil prior to its mixture with the expressed oil of the seed of croton tiglium. In order, therefore, to obviate this objection, I proceeded in the fol-

* Croton tiglium foliis ovatis, accuminatis, serratis, basi biglandulosis petiolis, folio brevioribrs, racemis terminalibus.—Vide *Linn. Spec. Plantar.*

lowing manner, *viz.*: Having saturated a given quantity of alcohol of the spec. grav. of 840, distilled water being 1000, I digested in it 100 grains of the tiglium seed oil; and then, after having shaken the mixture, and allowed it to rest for a time, so that the tiglium seed oil might precipitate, the supernatant liquor was poured off, and the oil remaining weighed: the loss of weight, which was 21.5 grains, of course gives the rate per cent. of active matter to a great nicety *.

It may now be stated, that out of nearly ninety species of the genus croton only one species is purgative, which is that in question. I have examined the expressed oil of the seed of three species of Jatropha, *viz.*: J. multifida, J. curcas, and J. panduræfolia; all these possess strong cathartic properties which I have no doubt pervade the whole family. The fixed oil of the seed of these species had nearly the same chemical habitudes as that of the seed of croton tiglium. I have also found a similar principle existing in the seed of several species of ricinus. The term *tigline* has been assigned by Dr. Paris to the active principle of the expressed oil of the seed of croton tiglium.

I beg to add that the plate accompanying this paper is the only correct one of the plant ever published, and which is taken from a drawing in the possession of the Medico-Botanical Society.

London, *Nov.* 13, 1825.

Description of the Plate.

a. The leaves, which are ovate, pointed, and serrate.
b. The petioles, or leaf stalks.
c. The glands, situated at the bases of the petioles.
d. Spikes of female flowers.
e. The capsule.
f. An enlarged drawing of a male flower.
g. The stamina detached, shewing their union by their filaments.
h. A female floret, shewing the calyx, germen, and three-bifid styles.
i. An outline of the capsule, with the calyx remaining.
k. A tranverse section of the capsule, shewing its three cells.
l. A seed, shewing its form.
m. A section of a seed, shewing its thickness.
n. A seed divided longitudinally, to shew the embryo.

* I have never been able to obtain more than 32 per cent. of active principle in the best sample of the oil which I could procure.

Art. III. *Outlines of Geology, being the Substance of a Course of Lectures on that Subject, delivered in the Amphitheatre of the Royal Institution of Great Britain, by* William Thomas Brande, F.R.S., Professor of Chemistry in the Royal Institution, *&c.*

[Continued from page 40 of the present Volume.]

VI.

The transition limestone of the Wernerian school appears to be the same with that usually called mountain limestone by English geologists; it forms many hilly, rocky, and mountainous districts, of singularly picturesque and romantic scenery in Britain; it is generally so compact in its texture as to break splintery—it takes a good polish—its usual colours are black, bluish, and reddish gray; it is often beautifully traversed by veins of calcareous spar, which give its surface in some instances a reticulated appearance; and it is in many parts characterized by its abundance of organic remains, more especially by those of marine animals, such as corallites and encrini. It is also rich in the metals, and as already stated, is associated with our coal strata, forming as it were the ground or basis upon which they lie; its usual geological position being under the red marl and above the old red sandstone; the latter rock, however, is by no means always in its place, and instances are not wanting in which mountain limestone lies upon slate; but in that case it is very scanty in organic remains, and differs otherwise in appearance from the rock that lies upon the sandstone. I shall first briefly enumerate the principal districts in Britain characterized by this formation, and afterwards advert to its aspects and peculiarities as constituting mountain masses.

The great patch of limestone extending from the Tweed to the Tees, bounded by the coal measures on the east and by the Cheviot Hills on the west, is commonly called the *lead measures*, in consequence of the abundant veins of that metal by which it is traversed. Indeed the rocks which we are now about to examine acquire a considerable degree of importance and interest as being the principal seats of the useful metals, and as abounding in this country in lead, copper, tin, and some other metallic bodies.

The characters of the limestone of the lead measures are very variable ; in some places it abounds in madreporites, encrinites, and other fossils, and is hard enough to receive a polish; in other places, from the prevalence of argillaceous matter, it acquires a bluish colour, softer texture, and loses its organic remains. In this great limestone district, the lead veins hitherto worked occupy a space of about fifteen miles N. and S., and twenty W. and E., and they run with little exception east and west. They present some peculiarities which are worthy of particular remark, although perfectly inexplicable, and to which I shall afterwards more explicitly allude. Among these, their varying dimensions, depending upon the nature of the beds they traverse, are particularly curious. In passing through the argillaceous strata, they become narrow, and even waste away into almost imperceptible threads, which again reunite in the limestone, thicken, and often bulge out into prolific bunches of ore ; a vein, not more than two or three feet wide in the shale, and mixed with rocky matter, shall suddenly become pure in the limestone, and widen to between seventy or eighty feet.

Though these veins contain several varieties of lead ore, the sulphuret or *galena* is their only important product. The best ores yield about two-thirds their weight of pure lead, and often contain no inconsiderable portion of silver. We learn from Mr. Phillips, that between 1803 and 1810, the greatest quantity of lead shipped in any one year at Newcastle, was 10,352 tons—the least 3,910. The average shipment at Stockton is about 3,000 tons annually. To give a general idea of the produce of these mines compared with others, it may be right to state that the whole of the lead-mines of Britain may be considered to afford an average annual produce of 45 to 50,000 tons.

This district, exclusive of its metallic mines, is penetrated and traversed by others, which, as they are only filled with lapideous substances, are of little interest except to geologists, and to them they have proved subjects of much discussion and some contention. These veins, which generally run N. and S., often disturb the parallelism of the strata; and, intersecting them, as well as the metallic veins, give rise to much confusion and disturbance. In the district we are now upon, there is an instance of the elevation of the strata to more than two fathoms upon the side of such a cross vein. The contents of these veins are very miscellaneous; and where the material they are filled with is much harder and more durable than the assailed strata, their course is often perceptible upon the weather-worn surface of the strata, sections, or precipices, they traverse. Such is the cross vein in these lead-measures, called the Devil's Back-bone, forming a ridge that may be traced a considerable distance along the strata through which it passes.

The limestone district of Derbyshire presents us with so many points of interest and importance, that much more might be said upon it than our time here admits. I shall only touch upon a few of its leading features. Castleton is at its N. point, and it extends about twenty-five miles S. to Weaver-hill. Its breadth is very irregular, but, I believe, nowhere exceeds about twenty miles. Its eastern end forms the delightful and varied scenery of Matlock, so strangely counterpoised by the dull monotony of Buxton at its western edge. Its north-western extremity is celebrated for the wonders of the peak.

In accounting for the varied aspect of this district, we may be assisted by recollecting, first, that the strata of limestone differ considerably from each other, and that beds of another species of rock intervene, which is provincially called toadstone, and that the respective edges of these strata come to the surface; and secondly, that the whole country is there traversed by a great fault or dislocation of the strata.

Mr. Farey has described the Derby district as composed of four beds of limestone and three of intervening toadstone. The upper bed of limestone is in part bituminous, and contains nodules of chert arranged nearly as flints in chalk. It contains entrochites and various shells, often exhibited in relief upon its weather-worn surface. Beneath it the rock contains beds of magnesian lime; siliceous lime, or dunstone; and towards its lower parts are the beds of black marble, which receives a good polish, and is manufactured into various ornamental articles. The lowest limestone stratum is that which forms the peak forest, the downs of Buxton and the Weaver-hills, and in it are several remarkable caverns, such as the Devil's-hole, Elden and Poole's-hole, and many others. Here also we find at Castleton those curious nodules and masses of fluor-spar, celebrated for the manufacture of vases, and a variety of other beautiful ornamental articles, as also that very singular mineral product called elastic bitumen; and the caverns abound in splendid stalactites and stalagmites, whilst their waters acquire a petrifying power from carbonate of lime.

The toadstone of Derbyshire is sometimes considered as regularly stratified between the limestone beds, but upon this subject considerable doubts may be entertained, and at all events the beds, if such they be, are liable to many extraordinary irregularities which will furnish matter for future consideration. The toadstone never contains shells, nor organic remains, which are so abundant in the alternating limestone. Nodules of chalcedony and zeolite; globules of calcareous spar, and some other substances, are not uncommon in it, and however unsatisfactory or inadequate our theories are, in respect to this extraordinary substance, it must by all be allowed to bear the marks and characters of a distinct formation.

In the cave at Castleton it forms a large, irregularly-shaped column, which has all the appearance of common basalt, and occasionally it acquires a columnar feature; so that for this, and various other reasons, I shall transfer the history of the Derby toadstone to that of those irregularly-occurring rocks that sometimes are found in primitive, and sometimes in secondary strata.

The veins in the limestone of Derbyshire contain lead, manganese, copper, and also ores of zinc and iron. The proper repository of the lead appears to be the limestone, though it also occurs in some other strata, and rarely in the toadstone, in which it is always in small quantities, and merely in strings, or very imperfect veins.

Near Bristol, the limestone hills rise from below the red sandstone, and are seen forming the edges of the coal-basin. In some places it is very bituminous, as on the Avon at Chepstow, and even exudes petroleum.

On the Welsh coast of the Bristol Channel we have another ridge of limestone, forming the basin, as it were, in which the great coal-field of S. Wales is situated. The hills that skirt Swansea Bay, and much of the delightful scenery of Neath and its neighbourhood; the sudden slope which is crowned by Knoll Castle, and the sinuosities of Pont-Neath-Vaughn, are characteristic of this formation; and again upon the banks of the Wye it constitutes scenery of a soft, but most romantic character. Upon the perpendicular and projecting precipices, lichens of various colours alternate with the gray surface of the uncovered rock. A variety of shrubs are showered by Nature's hand upon its more even, but still picturesque surface; ivy, and other creeping plants, issue in gay luxuriance from its crevices, and its steep sides are adorned by every variety of verdure. The charms of the valley of Matlock, of Dovedale, and indeed of much of the Derbyshire landscape, situated in the limestone glens, baffle all description; they have exercised the pencil of the painter and the pen of the poet, but all their addresses to the eye and to the imagination are cold and vapid, contrasted with nature's reality in these delightful spots.

I have formerly alluded to the occurrence of a red sandstone, beneath the red marle accompanying the coal and salt deposits. This is the *old* red sandstone of Wernerian geologists, and its situation upon the lowest secondary rocks appears to give it a title to that term. It often appears more as a conglomerate than a sandstone; that is, it is made up of coarse particles and pebbles, and ranges of it are sometimes seen following those of primitive rocks, where it is evidently composed of their débris. Like many other rocks, it derives its colour from oxide of iron. Geologists differ a good deal in their accounts of the position of this rock in England. One place I may mention as affording unequivocal specimens of it, namely, the N. coast of Somersetshire and Devonshire upon the Bristol channel, where it is seen recumbent upon slate, and even alternating in strata with that rock, and gradually passing into it. The red rock prevalent about Exeter, and that which occurs in the vicinity of Edinburgh, and which is seen in perfection at Hawthornden, have been referred by some geologists to this formation—the former probably does belong to it, but the latter strictly appertains to the red marl.

There occur in various parts of Britain a variety of sandstones, some approaching the nature of breccias and conglomerates; others slaty, and others almost exactly corresponding to the red marl in texture aud appearance, which, when beneath the mountain limestone, and upon grauwacke or slate, are usually, and properly enough, called members of the old sandstone family, and in this way we may perspicuously dispose of them without entering into the pedigree of the old red and of the new red, which some geologists have descanted upon with such pertinacious prolixity. I may, however, here state, that sandstone does not always intervene between mountain limestone and slate; on the contrary, numerous instances might be adduced of limestone at once recumbent upon slate, in which case it is generally very scanty in fossil remains, and often passes, by a kind of insensible gradation, into the slaty rock.

We here take leave of that series of rocks which Messrs. Phillips and Conybeare, in their excellent Outlines of Geology, have appropriately designated the *medial order.*

The next substance we have to examine has received the uncouth and unmeaning name of grauwacke; it is a rock which has often been regarded as setting definition at defiance, and sometimes specimens not referable to any other classes, and substances of doubtful origin, have been indiscriminately considered as of this family. A rock with more or less of a slaty texture, but distinguished from slate by its less-perfect lamellar fracture, and above all, by its embedded fragments, and being at the same time essentially argillaceous, appears to me to constitute legitimate grauwacke. If I found evident fragments of slate embedded in an homogeneous slate—or angular pieces of quartz; or of chlorite slate, in black slate; or here and there an imperfectly-rounded mass of any other substance, I should call such rock grauwacke. The mountainous part of Somersetshire included in its north-west district; the great slate formation of Cornwall; the ridge that follows the Malvern Hills; these have sometimes been adduced as grauwacke; but they do not come within the definition I have proposed. We must resort to Cumberland for illustrative specimens of this rock, and to the scenery of the lakes for a notion of its mountainous aspect. There is something exquisitely beautiful in the mountains that environ the southern extremity of Derwent-water; their forms, tints, and general association and outline, are perfectly peculiar—they have not the craggy summits and fragmented precipices that belong to a true slaty texture, nor do they form those bold masses and blocks which announce mountain limestone to the eye of the distant observer—they shew an union of softness and grandeur which marks them as a distinct formation; and if there be a difficulty, which there often is, in deciding respecting hand-specimens, grauwacke may, in general, soon be recognised, where the forms of its hills can be traced.

Associated with these rocks, and next in order of succession, are several varieties of slate, which, however, may, without any inconvenience or inconsistency, be referred to the great clay-slate formation, of which in England we have abundant and grand instances, especially in the northern and western parts, and which constitutes the greater portion of the mountainous district of North Wales. It is an abundant rock in Cumberland and Westmoreland, but is there so interwoven with the varieties of grauwacke, and has been so completely confounded with that rock in geological descriptions, that it is extremely difficult to draw the line of distinction between them, or to say where the one ends and the other begins. The cluster of hills, however, of which Skiddaw forms the highest elevation, may probably be referred to genuine clay-slate, and in Devonshire and Cornwall, the granitic range which traverses the promontory like a back-bone, beginning on Dartmoor, and ending at the Land's-end, has slate overlaying it on both sides. All the magnificent scenery of Falmouth, Fowey, Looe, Tintagell, and of other places too numerous to particularize, upon the north as well as the south side of Cornwall, derives its grandeur and charms from the various assemblages of slaty headlands, promontories, creeks, and islands. Sometimes its strata jet out in bold fantastic forms upon the ocean, and sometimes gradually shelve away into gentle slopes; their verdure is usually scanty and uncertain, but here and there, a clayey soil finds a resting-place, and cherishes patches of shrubs interspersed with trees of loftier growth, and attracting the traveller's attention by the sterile and fragmented surface which generally surround these insular spots of vegetation. The beauties of the coast of Cornwall are singularly contrasted by the barren exterior of its central road and great mining district, where we scarcely find a blade of grass to relieve the black and sombre hues of the ground, but where heaps of rubbish, that once was rich in embowelled treasures, give a gloomy irregularity to the surface, and where the ponderous heaving of machinery raises subterranean rivers to a level not their own, and turning them into new channels, enables the miner to arrive at those riches, which, but for the inventive genius of Watt, would have remained in inaccessible obscurity.

I have adverted to the confusion which has prevailed in the definitions of grauwacke, and have endeavoured, as far as concerns ourselves, to attain perspicuity by limiting the extent of this term. Of its unlimited application I can give no better instance than is furnished by the county of Cornwall, where all the slate has been by some geologists, and those of experience and observation, classed under that embarrassing and perplexing term; while others have entirely denied the existence of even a solitary cabinet-specimen in the whole district. Truth, however, lies between these extremes, and there are, in several parts of Cornwall, but more especially about the neck of the Lizard Promontory, certain strata, or, as they rather should be called, beds, of a rock, to which even a sceptic as to its existence elsewhere cannot deny the name: it is a rock which is slaty in its composition, and slaty in its texture, but which, from the fragments and particles distinctly embedded in the main mass, is legitimately allied to the enigmatical family of the grauwackes, while the slate of Tintagell and of Camelford is a distinctly unadulterated and admirably-defined clay-slate, and esteemed of an excellent quality as a roofing-material.

To the rocks which we have just considered, and to some others that will hereafter demand attention, belongs a remarkable feature, of which Cornwall furnishes some admirable and unrivalled instances, and which is truly perplexing to every person who looks at and examines the phenomenon unblinded by the dust of books and the fog of theory. The slate is remarkably *contorted* in some places; its strata are waved and curved for greater or less extent, and sometimes an entire bed affects a serpentine irregularity, which gradually merges into a straight line above and below. These contortions of the Cornish slate are sometimes independent of any other kind of rock—sometimes they are *apparently* caused by veins of *elvan*, to use a term of the country, by veins of a distinct species of rock which is of a porphyritic character, and which invades the slate, as it were, from below, bending it into a thousand shapes, or sometimes merely dislocating and giving a new inclination to its strata, *seeming* as if, in the one instance, it had acted upon the soft, yielding, and unconsolidated matter that surrounds it; in the other, as if it had violently protruded itself into the already-hardened mass. On another occasion, this phenomenon will come more decidedly under our notice; the concomitant effects must be studied, as well as the main ones, and thence we must endeavour to deduce an efficient cause;—at the same time, I must here remark, lest I should be thought to adopt the principles of those very theories which I am verbally contending with, that such contortions, although best observed in the slaty rocks, are not peculiar to them; that they are not characteristic of the transition-series of the Wernerians, but that they may be seen, by those who choose to see them, in gneiss, in mica-slate, in clay-slate, grauwacke, limestone, basalt, sandstone, and shale—nay, even in the newest of the new rocks, as at Purbeck in Dorsetshire, in the freestone; in the chalk at Handfast Point in the Isle of Wight; in that of Montmartre at Paris; in the pulverulent and coaly matter, on the road between Edinburgh and Leith ; and Mr. Greenough even informs us, that he found the same waving lines in sandstone, *now forming* at Port Dinnleyn in Carnarvonshire.

These curves are not less varied in their forms, than in their dimensions; they are sometimes only a few inches in extent, and the whole may be included in a hand-specimen; at other times they extend many feet and yards, and even sometimes for many miles. Saussure, in his Travels in the Alps, speaking of the shape of these contortions, compares them to the letters Z and X, exhibiting their angular and zigzag forms; to the letter C, shewing their tendency to circular flexure; and on the road from Chamouny to Geneva, a perpendicnlar surface of limestone exhibits the strata in one part bent into arches, and producing, by their association with the bendings in another part, a form not unlike the letter S.

These curvatures are regarded by Dr. Hutton and Mr. Playfair, to have originated in the elevation of the strata once horizontal, while in a flexible and ductile state, by a force acting upon them from below upwards: owing to gravity and the resistance of the mass, this direction became oblique, and the lateral force caused contortion; but this view, although ingenious, and often consistently applicable to the appearances, is sometimes, and indeed often, so at variance with them, that it cannot be plausibly entertained as a general cause; among the leading militant instances, we may advert to the existence of curved *upon* horizontal strata. Near Malvern, limestone, much contorted, rests upon strata quite horizontal. Mr. Greenough adduces, as another similar instance, the singular stratification of Mont Righi, in which highly-curved limestone rests upon strata preserving perfect horizontality: how happened it, then, that a force adequate to the flexion of one part of the limestone, elevated more than 7000 feet above the sea, should not have affected the inferior beds? A case still more opposed, and indeed, if admitted in the abstract, fatal, to Dr. Hutton's theory, may be seen upon the west side of Loch Lomond, where veins of contorted slate traverse strata which are not contorted, not even disturbed. But if the Huttonian geologists are unsatisfactory upon this point, the Wernerians are still more so; indeed, quite unintelligible. The contortion of the masses, they tell us, is the result of crystallization!

The authors of these hypotheses to account for the bendings and inflexions of strata, these contortions to which rocks are liable, have too frequently bent and distorted the facts to fit them to their own peculiar notions; and they have too often, evidently, attempted to explain appearances referable to several causes, by the assumption of one power.

If we consider the strata in which they occur to have been once in fusion, in solution, or in diffusion, the relative times of consolidation, the warping and the shrinking of one substance, as compared with others, may have had its share in producing these mysterious irregularities.

If slate be a deposit of finely-divided matter once mechanically diffused through water, its irregularities may obviously have arisen from some irregular agitation of the water during its deposition, like those undulations in the sand upon the sea-shore, which are obviously occasioned by the waves.

I have now, probably, said enough of the curvatures of strata, to excite attention to the subject, and more than enough to shew how inadequate are the theories which have been assumed to account for them. If, by saying more, I could throw more light upon the subject, I would proceed; but the further we investigate the facts, the more incongruous and inexplicable would they appear; I am, indeed, already apprehensive, that I may sometimes fatigue the attention, by asking it for opinions rather than things; but geological hypotheses are apt to run into rank luxuriance, if not occasionally trimmed, and though great authority and exalted talent has sanctioned much that I have presumed to doubt, and attempted to disprove, it must be remembered, that many of the opinions which we have been called upon to combat and to reject were advanced in the feeblest infancy of the science; and from the known candour, and scientific integrity of many of those with whom they originated, there cannot be a shadow of doubt, that if opportunity offered, they would frequently recall, modify, or resign them. It is, at all events, our particular province and duty, to direct our humble endeavours towards establishing, as far as may be, a strong line of demarcation between facts and opinions; to take care that genuine science, which aims at the interpretation of nature, is not checked by the crudities of modern scholastics—that the roses are not smothered by brambles.

Cornwall has been cited as furnishing upon its coasts favourable specimens of lowland clay-slate scenery; its mountainous aspect is seen to great perfection on the western side of Wales, where Snowdon, Plynlimmon, and Cader Idris, with many of their respectable associates, present the peaked summits, the dark and narrow valleys, the terrific precipices, and the fragmented slopes that peculiarly belong to this formation.

Slate is often traversed by veins rich in the metals, and abundant in fine specimens of various crystallized minerals. It is matter of doubt whether it ever contains organic remains, though the impressions of some bivalve shells are said to have been found in it. I am, however, rather inclined to believe that these impressions are either in grauwacke slate, or in that which borders upon and passes into limestone, and that genuine clay-slate is destitute of shells—a fact which appears opposed to its imaginary aqueous origin, and which has given rise to the presumption that the ocean was uninhabited by living beings at the time that this great deposit was produced.

Not the least-important fact in the history of the rocks which have now been considered, is their gradual, and often insensible, transition, as it were, into each other; for it furnishes us with a strong argument against many of those speculations into which some geologists have entered respecting their origin and formation. The gradual migration of chalk into lias and sandstone, and of these into oolitic deposits—of oolite into lias, and of this again into limestone, is visible in many hand-specimens, and there are equally well-marked instances of the transition of clay-slate into red sandstone on the one hand, and on the other into mountain limestone and into grauwacke; so that those rigid lines of demarcation, and sudden transitions, which the student might be led to expect from the perusal of certain systematic geological writers, are not in general to be met with in nature.

Another circumstance by which some of the rocks we have just considered are distinguished from others, and more especially from those formerly described, is the defalcation of organic relics both in quantity and perfection. The shells and corals, so abundant in the mountain limestone, are not blended with any kind of bones, nor are there are any vegetable remains; and in the underlying sandy and schistose formations a solitary shell now and then occurs, whilst in the micaceous slates, and the rocks belonging to the granitic family, which next claims attention, and will form the subject of the next lecture, there is neither shell nor coral, nor any impression or semblance of any thing that has ever appertained to the organized kingdoms of nature. The gradual transitions of one kind of rock into others on the one hand, and their sudden and abrupt lines of demarcation on the other; their verticality in one place, and their horizontality in others; their occasional resemblances, and frequent dissimilarities, are a few of the circumstances which will lead the cautious and unprejudiced observer to distrust those accumulations of hypotheses which have sometimes been dignified by the title of geological theories, but which, in assigning opposite virtues to the same subject, are alike at variance with nature and with themselves—they are systems which might pass for the inventions of those ages "when sound philosophy had not as yet alighted on the earth, nor taught man that he is but the humble minister and interpreter of nature."

VII.

We have now cleared our way to the primitive formations of the Wernerians, and have arrived at those strata, or rocks, upon which all others appear to rest, and which, towering up through the superincumbent substances, form the exposed peaks and loftiest summits of the principal mountain-chains of the world. Whether these rocks are to be regarded as original formations, upon which the other stratified materials have been successively deposited in that order of arrangement which we have enumerated; or whether the stratified rocks are to be considered, with the Huttonians, as *primary*, and the granitic and other masses that we are about to examine, as having been subsequently elevated by the expansive force of subterraneous heat, are questions that have eagerly engaged the contending geological schools, and to which too much importance has often been assigned. As we proceed in the examination of these substances, we shall notice such of their characters as have especially given rise to the igneous and aqueous sects; but, as heretofore, I shall consider hypotheses and theories as very subordinate objects.

Under the term *granitic formation*, I include, not merely granite, properly so called, but the substances into which it merges, either from the predominance of one or other of its ingredients, from the loss of one or other of them, or from the occasional addition of some new mineral; and I shall also include in this division of rocks, those substances which are their occasional accompaniments; such as serpentine, marble, and primary conglomerates: rocks always, and exclusively, associated with the granitic series, and unknown amongst those formations in which organic remains are to be found.

Under the generic term *granite*, we include all crystalline aggregates of quartz, felspar, and mica. Quartz, or siliceous earth, known in its crystalline form under the name of rock-crystal, has already been defined. (Vol. XIX. p. 80.)

Felspar is an alumino-siliceous compound, containing a portion of alkaline matter, and frequently coloured by oxide of iron, which gives it various shades of brown and red, and of which the foliated, the green, the blue, and iridescent variety of Labrador are leading sub-species. When crystallized felspar affects a four or six sided prismatic form, fusibility before the blow-pipe, and a texture somewhat softer than quartz, but yet hard enough to scratch glass, are its further distinctive properties.

Mica, the last component of granitic aggregates, contains, as its chemical constituents, silica, alumina, and a little magnesia and oxide of iron, and crystallizes in six-sided plates and prisms; it is easily recognised by its disposition to lamellar division, and by the tenuity and elasticity of the plates into which it admits of being thus mechanically separated.

The characters of granite depend much upon the perfection or prevalence of one or other of these ingredients. Their aggregation in the crystalline form is, in some specimens, distinct and well defined, in others imperfect, forming what are usually called fine and coarse grain granite; large crystals of felspar sometimes prevail, and from these the leading hue of the granite is derived; or where mica abounds, the rock acquires a lamellar and slaty fracture, and is then called *gneiss;* and where the felspar is very sparingly disseminated, or altogether wanting, or where garnets supply its place, granite is said to pass into *mica-slate*, and this again into *quartz-rock*, by the partial or entire disappearance of the mica.

Hornblende is an alumino-siliceous mineral, containing magnesia, and abundant in black oxide of iron; it forms prismatic crystals, which are sometimes blended with granite, and sometimes with felspar only, constituting syenitic granite and *Syenite*, and where crystals of felspar are embedded in massive felspar, quartz and hornblende being occasionally superadded, but not predominant, the rock is called *porphyritic*, or *porphyry*. It is right here to advert to syenitic and porphyritic rocks, as associates of the granitic formation, though we shall find that there also exists a very close resemblance between them and some of the varieties of greenstone, included in the family of trap rocks. I have also mentioned marble and serpentine, as accompanying granite. (See Vol. XIX. p. 82.) The former is here distinguished as *primitive*, in opposition to the transition marble, which abounds in organic remains and lies above the slate; the primitive is granularly foliated in its texture, and without any trace of fossil animals or vegetables; it is celebrated as a material for sculpture, when white and fine-grained, and is, in all cases, a valuable ornamental substance.

Serpentine is a rock, concerning the characters and relations of which geologists are somewhat at variance. Hornblende, or schiller-spar, and felspar, appear to be essential to its constitution, and perhaps also talc; and these three substances are sometimes distinctly visible, like the three components of granite; while, at others, the rock is so fine-grained as to be nearly homogeneous; its variety of colours recommends it as an ornamental material, but it is too soft to admit of much polish. The bright-green varieties, interspersed with white marble, form the *verde antique*, to which some of the serpentine of Anglesey bears a resemblance; and those varieties which are comparatively hard and brilliant in their colours, are called *noble serpentine*. Talc, which I have mentioned as a component of serpentine, is lamellar; but its lamina, unlike those of mica, are not elastic, and it has the soft soapy feel of a magnesian fossil. In composition it differs little from the steatite, which is found pervading serpentine in veins, and which, in many parts of the Lizard serpentine district in Cornwall, is accompanied by veins of a rock sometimes looking like the massive felspar, and at others becoming porphyritic, or even assuming the characters of granite. These are very curious facts, and it requires some forbearance to pass them over merely as such, without considering the theoretical views to which they lead.

I can hardly be induced to regard marble, serpentine, porphyry, or even syenite, as distinct formations. Sometimes they appear as beds or detached masses, varying in magnitude and extent; at others they look like modifications of some other rock. Quartz rock, too, which is said to form entire mountains, as in Jura, in Wicklow, and elsewhere, and which are characterized by their conical form and insulated appearance, is seldom free from mica, and often may be traced by regular gradation into mica-slate, and this into gneiss and granite. But I throw out these observations, not with the intent of speculating upon them, but merely to simplify the history of primitive rocks, and to shew that the occasional recession or addition of one ingredient may give the whole mass a new character, mineralogically speaking, without entitling it to rank as a new formation, geologically considered.

Having therefore pointed out the composition of these rocks, as far as necessary to their definition, it remains to notice their aspects as mountain masses, to examine the circumstances that attend their junctions and alternations, and lastly, to examine how far the received theories of their formation are consistent with the appearances which they present, and upon which the Wernerians and Huttonians must be regarded as mainly differing.

The largest granite tract of England is that of Devonshire and Cornwall, where its sides are covered by slate, but where it rises in several places to the surface, and also forms the rocky promontory at the Land's-end. There is here nothing either picturesque or sublime belonging to the granite formation. Dartmoor appears the head-quarters of dreariness and desolation, forming a large mountain tract of nearly 80,000 acres in extent, strewed with granite boulders, and fragments of rocks, and appearing to set cultivation at defiance.

This granitic district is nowhere of any considerable elevation; its highest point is the hill called Brown Willey, near Bodmin, which is about 1360 feet above the ocean's level.

The peculiarities of theWest-of-England granite are best seen at the Land's-end, where a large patch of it protrudes in a wedge-shaped promontory upon the coast; it appears formed of fragments and masses placed upon each other in the rudest disorder, and sometimes in fantastic piles and insulated blocks, which, though arising from the peculiar manner in which the rock is decomposed and dislodged by the weather, have been mistaken for monuments of ancient heroes, and for druidical remains.

These *Tors*, as they are called, have been described by Dr. Mac Culloch in the *Geological Transactions*, and some of them are depicted in the engravings annexed to his paper. One, called the Cheese-wring, at Liskeard, consists of five blocks, of which the upper are larger than the lower, the whole pile being about 15 feet high. The stones composing this, and other similar piles, suffer by the action of the weather most rapidly upon their edges and angles, which gradually become rounded, until the blocks begin to totter upon each other, and ultimately fall. This tendency of square blocks to become spheroidical, and which has sometimes been mistaken for the effect of friction, explains some of the mysteries formerly adverted to respecting granitic boulders, and shews that attrition by torrents, and transportation by streams, are not always essential to their rounded appearance. The celebrated logging-stone is also noticed in Dr. Mac Culloch's communication. It exhibits the tendency of this kind of granite to cuboidal separation, and although 17 feet long, 32½ in circumference, and weighing 65 tons, may be moved by the force of a few pounds, and visibly vibrates when blown upon by a western gale.

Though granite is in general a very durable rock, and though the permanence of the lofty peaks of the Alps, and other great granitic chains of mountains, is such as to have enabled them to weather those storms that have carried away and disintegrated much of the softer materials of other, and probably of superincumbent strata, yet there are some varieties of granite subject to moulder down, and that even with no inconsiderable rapidity. De Luc talks of the friable granite of the Hercynian forest, and Saussure describes the mouldering down of that in the Alps. The waters of the Arve are rendered milky by the pulverulent felspar that comes from the Aiguilles de Chamouny, and other points that border the Mer-de-Glace. The road across Dartmoor, from Ashburton to Chagford, traverses in one place such loosely-compacted granite as to resemble a bed of gravel. The granite of the Carglaise mine near St. Austle, in Cornwall, is so soft and pulverulent, that the excavation might be mistaken for a chalk-pit; and in the same vicinity, the immense quantities of white porcelain-earth, as it is called, is of similar origin, and seems derived from the perishable nature of the felspar, which, giving way, suffers the quartz and mica to fall out. To what the extreme proneness of some kinds of granite to suffer decay, while others are as remarkably permanent, is to be attributed, does not seem quite clear; but if I mistake not, Sir H. Davy, in his geological lectures delivered in this Institution, considered it to arise from the alkaline matter of the felspar being predominant, and yielding to the solvent agency of water.

Independent, however, of chemical composition, mere mechanical texture, and the general aggregation of mountain masses, has much to do with their respective durabilities. Where the arrangement of granite resembles that prevalent in the greater part of Cornwall, water gradually penetrating between the blocks and masses, freezes there, and thus slowly removes them, or transfers them to unstable ground: while the firmer lamellar texture of much of the Scotch, Alpine, and other varieties of granite, denying access of water to its fissures, is slower in suffering the decay that is referable to that very powerful cause.

Gneiss and mica-slate often form mountain masses in association with each other, and with the varieties of granite. The former is seen singularly contorted upon the coast of Lewis; and mica-slate rock is associated with the serpentine of Cornwall, and is seen in great perfection among the Scotch granitic scenery, more especially in the vicinity of Dunkeld, and in extraordinary magnificence in the lofty mountain of Benmore, the summits of which are above 4,000 feet above the level of the sea, and when seen in certain directions in respect to reflected light, dazzle the spectator by their extent and brilliancy. Ben Lawers, on the north of Loch Tay, as well as many of the neighbouring mountains, furnish the geological student with highly-instructive specimens of granite passing into gneiss, and this again degenerating into mica-slate and chlorite-slate. I know of no school of geology superior to this district; the transitions, junctions, and varieties of rocks are without number, and in many places so near each other, as readily to come within the eyes' grasp, and so accessible, as to furnish even the timid climber with a fine and useful series of specimens. To mention one, among the many places of this description, there is a quarry upon the high road, about three miles south of Dunkeld, in which a stratum of grauwacke is seen incumbent upon chlorite-slate, gradually passing into a fine gray roofing-slate, and this recumbent upon mica slate. The peculiar and differing dip of the respective strata, and the singular manner in which they are pierced and traversed by veins of felspar, chlorite, and quartz, and lastly their association with micaceous iron, are circumstances which will not escape the eye of the observer in his journey through this district, the beauty and magnificence of which, in regard to general scenery, is not less than its diversified geological peculiarities.

Mica-slate abounding in garnets, and often speckled with red patches, originating in their decomposition, and becoming syenitic from the interspersion of hornblende, is prevalent upon the banks of the Tay, at and about Dunkeld; but it is in Glentilt that the geologist, both practical and theoretical, will find the most ample materials for the study of the associations and junctions of the primitive series of rocks. For a detailed account of Glentilt, I must refer those who are desirous of visiting it with the advantage of previous information to the account drawn up by the late Lord Webb Seymour, and to a very able paper upon the same district, in the Geological Transactions, by Dr. Mac Culloch. He that is tinctured with the Huttonian doctrines, will here find the Plutonists upon their strong ground, and will not feel disinclined to join in that complete contempt for the Neptunian theorists, which the language of Mr. Playfair is so well calculated to inspire. But he that is above the trammels of authority, and who dares to doubt its edicts, and to refer to his own unprejudiced and unbiassed opinion, will even here, in the Plutonic fastnesses, find matter to awaken his doubts, and to teach him the imperfection and faultiness of all geological theory.

In the immediate neighbourhood of Blair, the Tilt exhibits upon its banks a deep section of the rocks that form its bed, and, what is remarkable, the micaceous strata here, and also at the falls of the Bruar, incline nearly at the same angle to several points of the compass, giving a curious interweavement and confusion to their assemblage. Ascending a few miles up the glen, we observe granular limestone, either embedded in, or interstratified with, the micaceous and gneiss rock, particularly well seen a little below Gilbert's bridge, and occasionally accompanied and disturbed by porphyritic dikes; and near Gow's bridge, a dike of greenstone disturbs and contorts the primary slate of the Huttonians, and contains a mass of embedded marble*. Hereabouts there is so much confusion and irregularity, such a variety of rocks, and such various inclinations of their strata, that neither descriptions nor drawings can give any thing but a remote and inadequate idea of their groupings and assemblages.

The bordering hills are frequently in part composed of granular quartz, and when this forms their summit, they are characteristically conical, as seen in Cairn-Toockie, and some others.

And lastly, dolomite, or magnesian marble, is here seen passing in a few places, and in detached spots, into a rock which bears all resemblance to serpentine.

I have enumerated these facts, which are shewn in the valley of the Tilt, because much of the fair and legitimate argument that may be founded upon them applies to the phenomena exhibited by the granitic or elvan veins of Cornwall; they will lead us to doubt the quiet crystalline deposition of the granite and its associate rocks, and may perhaps justify us in inferring, with Dr. Hutton, that they are of another source and parentage.

St. Michael's Mount, situated in Marazion Bay, on the S. coast of Cornwall, is also an interesting geological object, and shews us granite and mica-slate, not merely at their junction, but the latter rock is singularly traversed by granitic veins, which appear to break up the superincumbent slate, and to penetrate and harden it. Crystals of tin, quartz, and apatite, and small topazes are also found in the veins of this rock.

Exclusive of Cornwall and Devon, there is little granite in England. The Malvern Hills, Mount Sorrel in Leicestershire, and a few of the ridges of Cumberland and Westmoreland, afford us specimens of this rock, but they present nothing sufficiently remarkable to be further dwelt upon at present. In the Isle of Man, and in Anglesey, granite is associated with clay-slate; and near Gwyndy in Anglesey, the points of granite curiously protrude

* See the plates annexed to Dr. Mac Culloch's paper in the *Geol. Transactions*.

from beneath the slate, sometimes appearing as a felspar rock, and sometimes like granular quartz, with a few mica crystals.

The situation and characters of granitic rocks suggest some theoretical considerations, partly of an abstract, and partly of a general nature, to the most important of which I shall briefly allude, not so much on account of any intrinsic importance which they possess, as from the eagerness with which they have been animadverted upon by the contending geological schools.

The manner in which granite occasionally pervades the superincumbent stratified rocks; the damage which it often does to them at its veins and junctions; the new inclinations which it gives them; and above all, its occasional protrusion in considerable masses connected with veins, but above the slaty and generally superposed rocks; and its occasional embedded slaty masses, are circumstances which give the Huttonian theory of its origin a plausible aspect: while on the other hand, the delicate and well-defined line of junction with which slate and granite sometimes meet, the utter want of all fracture and violence at their contact, but more especially the slow and imperceptible transition of granite into gneiss, of gneiss into mica-slate, and of mica-slate into clay-slate, seem to favour the Wernerian view of its deposition. When, however, we fancy that we trace its crystallization from a fluid, we are suddenly recalled to the difficulty of conceiving any fluid, much less of an aqueous nature, which could have dissolved and deposited granite, which is so singular and complex a crystalline aggregate; and though the notion of igneous fusion is not without its concomitant weak and objectionable points, it is so much more consistent with the phenomena, and even with experimental deduction, as to find, with all its imperfections, a more ready access to the mind of the unprejudiced observer.

As all our strata are theoretically incumbent upon granite, which we conceive to form the lowest part of the crust that envelopes the globe, and which some have been bold enough to assert as forming its nucleus, so experience teaches us that the loftiest mountains in the world are composed of the same material, and that the alpine chains of the four quarters of the globe, where the summits are not volcanic, are either of granite or its modifications and associates. Now it is not impossible, and experience renders it probable, that these exposed peaks have once been covered, like their fellows in the lowlands, with some, or perhaps with the greater number, of the rocks which we have enumerated in the order of their superposition, but that these have yielded to the ruthless fingers of time and the elements; that they have been degraded and washed away, and that the denuded and indurated materials which they once covered, now stand the more durable, but still decaying, monuments of that former order of things; the valleys, we are told, are filled with the débris of the bordering hills, and the streams and rivers wash them into the ocean, and are gradually tending to reduce the surface of the earth to an unvaried plane. Lakes are filling up; bars are forming at the mouths of rivers; mud is gradually raising itself into dry land susceptible of cultivation, and the unfathomable depths of the ocean will one day or other become soundings, and ultimately, the waters of the sea will overwhelm the land, and man's dominion will be no longer suffered upon earth; or perhaps, to take the more cheering prospect, subterranean fires may again consolidate and elevate the detritus, and new islands and continents may arise out of the ruins of their predecessors.

Although these, and similar flighty hypotheses, deserve in themselves no serious consideration, they lead us naturally to inquire as to the cause of those irregularities which the surface of the earth exhibits—the origin of hills and dales, of mountains and of valleys: but let us first temper our minds for the consideration of this subject, by recollecting the extreme insignificance of these irregularities, and the paltryness of what we are regarding as most sublime and magnificent, compared with the bulk of our planet, to which they are but as particles of dust upon the surface of an artificial globe. In speaking of the transportation of granite boulders, I have already adverted to the probable non-existence, at the period of their lodgement in their present situations, of several of the deepest and most extensive valleys of the world; and if we examine the walls, as it were, of these valleys, or at least of many of them, and observe that the same strata in the same position, constitute the hills that bound them, we shall have little reason to doubt that they have once been continuous, and that the intervening portion has been removed. Some cases of this kind have already been pointed out (see Vol. XIX. pp. 89—90); but there are others yet more decided, and upon a much larger scale, as applying not to valleys only, but to the separation of islands and continents. Look, for instance, at the two sides of the English Channel, and the same strata, with the same peculiarities, are found upon its opposite shores: shall we then disbelieve iu the former continuity of these strata? Or when we discover the coast of Norway exhibiting the stratification of the Orkneys; Majorca composed of the same materials as Minorca; the geological analogy of Corsica and Sardinia; that of the opposite shores of the Gulf of Venice, and many other analogous cases, shall we be inclined to doubt the removal of the once intervening matter? Shall we not read in these correspondences, the list of which might be much extended, the former non-existence of the intervening chasms, let them be valleys, or rivers, or arms of the ocean? and shall we not have disproved the original existence of, or contemporaneous formation of the valley, and may we not reasonably conclude, that as the irregularities of valleys depend upon the relative hardness and destructibility of the materials that form their sides, from the nature of their soil, from the direction of their ravines, and from some of the other facts that I have already observed upon, that water has been the great agent in effecting these excavations, and that certainly the mountainous protuberances of the globe are not the invariable effect of volcanoes; that valleys have not been formed by earthquakes; and that mountains are not accumulations of sand and of mud collected by submarine whirlpools, nor great crystalline shoots, as certain German and French geologists would insist.

But if we revert to the once even and regular state of the earth's surface, and admit that impetuous torrents of water have been chiefly concerned in carving out its irregularities, where are we to look for the origin of these torrents, or how are we to account for the gigantic effect which they have left behind?

Pallas ascribes them to the effect of earthquakes and volcanoes; some have insisted upon the agency and interference of a comet, and others have attempted to accommodate their speculations to the Mosaic history of the creation, and of the deluge, without having broached any particular opinion relative to the cause of the latter phenomenon; others again have dared to bring the credibility of scripture into the field of their discussions, and have elicited nothing but the contempt and disgust of the wise and virtuous part of mankind. Surely for all these purposes, the deluge is an efficient cause; and from the evidence already adduced, as well as from that which remains to be brought forward, it will be evident that to that source we must ascribe the principal inequalities and irregularities of our present surface.

We have now reviewed in succession the various strata which incrust the nucleus of our globe, commencing with the most superficial, and terminating with those which seem to constitute their foundation, and perhaps even the bulk of our planet. They present us with a series of records singularly interesting and eventful, and will already have appeared "as a book wherein men may read strange matters:" but their history is still imperfect, and I shall endeavour to complete the outline which I have sketched, by examining the contents of mineral dikes and metallic veins, by inquiring into the causes which tend to the disintegration and decay of rocks, and by stating the general effects of certain local agents, such as earthquakes and volcanoes.

Art. IV.—*Experiments on the Action of Water upon Glass, with some Observations on its slow Decomposition.* By Mr. T. Griffiths.

[Communicated by the Author.]

It is a commonly received notion that glass is capable of resisting, to a very great extent, the attacks of active chemical solvents, and that its alkali can neither be readily separated nor exhibited in an insulated form without regularly submitting it to powerful decomposing agents. Speaking of glass, in common language, without any reference to the many soluble compounds so designated, it may be a new fact in chemistry to prove that this singular substance possesses highly alkaline properties, which may easily be shewn by the usual tests.

Upon reducing some thick flint glass to a moderately fine powder in an earthenware mortar, for the purpose of analysis, a portion of it was placed on turmeric-paper, with the view of determining if it possessed any sensible alkaline property; and, upon being moistened with water, the yellow colour of the test-paper was instantly reddened nearly as powerfully as if lime had been employed.

This effect was considered as accidental, and as probably arising from some adventitious alkaline matter, or soap, adhering to the vessels employed. Another experiment was made, with greater care, in an agate mortar, but with the same, or even a more decided result, in consequence of the more minute division of the material. When pulverized on perfectly clean and polished surfaces of iron, steel, zinc, copper, silver, and platinum, the effect took place, and apparently with equal facility; but it was found that the presence of small quantities of oxide of iron greatly diminished it, in consequence, as was afterwards proved, of the particles of glass being by them defended from the contact of water.

Since there are some saline bodies and metallic combinations which give indications of alkali to turmeric-paper, although perfectly neutral compounds, and as pure magnesia reddens this paper when moistened with water, although no solution can be shewn to take place, possibly this might be an effect of the kind, it scarcely appearing probable that any soluble matter should be abstracted from the powdered glass by the mere affusion of pure water. Litmus-paper, therefore, reddened by an acid, and paper stained with the blue infusion of cabbage, were also employed as tests; the former had its blue colour restored, and the latter was rendered green.

A portion of flint-glass in fine powder was boiled in water for some hours; upon being allowed to cool, and subside, the clear portion was decanted and evaporated, and became strongly alkaline to the taste, and to other usual tests; a drop of its concentrated solution, gradually evaporated on a glass plate, on exposure to the atmosphere, in a short time became deliquescent. Tartaric acid produced an effervescence, and afterwards a precipitate in this solution; as likewise did muriate of platinum. From these experiments, therefore, it may be fairly inferred that the alkali removed from the glass was potash in an uncombined state, and that the alkaline effect observed in the first instance did not depend upon the presence of any alkaline salts, or combination, adhering to or diffused throughout the glass.

The remaining sediment from the above solution, after having been repeatedly washed in successive portions of water, became inert as to its action on test-papers, not affecting their colours in the slightest degree; but, upon *trituration*, its alkaline power was again developed; this property being evidently dependent upon the exposure of a new or undecomposed surface. A slight application of heat to the water, was found greatly to facilitate this evolution of alkali.

In order to determine the quantity of alkaline matter abstracted from a given weight of glass, by long and continued boiling, 100 grains of flint-glass, in fine powder, were boiled nearly every day for some weeks, in two or three successive portions of water; after this process, the insoluble residue was found deficient in weight by nearly 7 grains. This result, however, must not be considered as accurate, but as a mere approximation; for, on the one hand, small portions of glass might have been carried away in the supernatant liquor; and, on the other, more alkali might have been abstracted by repeatedly triturating during the process, which, under these circumstances, would be almost unlimited.

To some pure dilute muriatic acid was added very fine flint-glass, in powder, till it was completely neutralized by its alkaline effect. Upon being allowed to subside, (which, however, was not very readily effected, minute particles remaining suspended for weeks together,) the clear portion afforded a crystalline salt on evaporation, having the characters of muriate of potash.

It may be remarked that this solution, *when perfectly clear*, contained no lead, on testing for it by sulphuretted hydrogen; but upon agitating or diffusing the fine powder of glass through water, holding the gas in solution, it was immediately discoloured, or blackened.

Flint-glass, although chosen for the above experiments, is not the only variety possessing this remarkable property, crown and plate-glass, white enamel, and what is more remarkable, Newcastle green-bottle glass, and tube of the same material, (in the composition of which there is, comparatively, little alkali,) also Reaumur's porcelain, made from the green bottle glass, possess the power of acting upon vegetable colours as alkalies.

These experiments, tending to prove that glass is a body of irregular composition, parting readily with its alkali by the action of water, it became a matter of some interest to determine how far certain natural combinations of potash with siliceous matter were equally active to the same tests, especially as in green-bottle glass, which contains little alkali, it is thus rendered evident. No analogous effect could, however, be produced by powders of felspar, basalt, green-stone, granite, obsidian, pumice, and some others, even when boiled with water, a method which never failed to produce it rapidly with glass, although cold water is perfectly sufficient.

Some interesting conclusions may be drawn from the above experiments, which may tend to explain several well-known phenomena, and may, perhaps, become matters of practical utility.

In the first place, with regard to the glasses employed in the laboratory, or for domestic uses, it must be evident that water has the power of acting upon and dissolving the alkali at the surface, and leaving an insoluble portion spread as a coating over the interior of the vessel, defending it from further immediate action.

Where, however, time can be allowed, the effect does not appear to be confined to mere surface. In collections of ancient glass, specimens may be selected, exhibiting how extensively an analogous action has been going on during the period they have remained buried in the earth. These vitreous relics of antiquity are often covered, to a considerable thickness, with opal pearly scales of beautiful appearance, consisting almost wholly of silica, whose alkali had been removed probably by the action of the water *.

A fragment of transparent ancient glass was examined with regard to its alkaline property, which it was found to enjoy in a high degree, being sensibly alkaline (when in powder) to the tongue, and its hot solution acting upon the cuticle. It appeared to consist almost entirely of potash and silica; not the smallest trace of lead being discoverable in it; several other coloured specimens of ancient glass, upon examination, were, in every case, more highly alkaline than any modern glass containing lead, that has hitherto been examined.

The specific gravity of common flint-glass was taken by way of comparison with the ancient fragments above mentioned, the result of which is here given. Flint-glass, S.G., 3.208. Ancient glass, 2.375. It may here be remarked, that the latter acted powerfully upon the test-paper, by merely moistening it, without reduction to powder. It cannot be surprising, therefore, that ancient glass, which may almost be called pure silicate of potash, should be occasionally found in states of such rapid decay, as the specimens in collections often exhibit.

Another proof of the action of water, aided by other concomitant circumstances, in producing decomposition upon glass, is an account given in Vol. I. p. 135 of this Journal, of some bottles of wine found in a quantity of black mud at the bottom of an old well, full of burned wood, supposed, upon good authority, to be of anterior date to the fire of London. The siliceous earth, in this instance, separated in films on the surface of the bottle, in consequence of the abstraction of alkaline matter, probably

* The opal is an hydrate of silica: may not its formation have taken place by a similar agency acting upon natural combinations? The removal of alkali from siliceous compounds may have left opal thus constituted.

by the action of water, aided perhaps originally by a certain degree of heat, and afterwards by the long period of their continuance, in a situation favourable to the decomposing agency.

In contact with ammoniacal, or decomposing animal matter, the disintegration of glass takes place more rapidly. Stable-windows, and bottles kept in such situations, often present a very beautiful iridescent appearance, in consequence of the siliceous matter being developed in thin plates on its surface, often amounting to a pearly, and sometimes almost metallic appearance; an effect which, it is believed, has not been hitherto investigated.

Solution of potash acts very rapidly upon glass, as the chemist often inconveniently learns by the effect produced upon the bulb of a thermometer, employed to determine its boiling point, and which is always found corroded to a considerable extent after the experiment.

It may also here be remarked (although not perhaps immediately connected with the subject), that from frequent observations by a person in the habit of using solid carbonate of ammonia, that the flint-glass bottles in which it has been for some time kept are invariably rendered much more brittle, and pieces of glass fall out upon very slight motion of its contents. This fact is merely mentioned as curious, and may probably be hereafter more fully examined.

Art. V.—*Analyses of two Mineral Waters from Springs in Windsor Great Park. By* W. T. Brande, F. R. S., Prof. Chem. R. I.

These waters were sent to me about a twelvemonth ago for analysis, and as I understand that since that period the springs have been much frequented, an account of their contents may not be unacceptable to those who have derived benefit from their use.

In taste, these waters exactly resemble each other, though one is manifestly more abundant in saline matter; their taste is salt and slightly bitter, without the smallest admixture of any chalybeate flavour.

The specific gravity of the stronger water is 1010.4; that of the weaker, 1007.7. Of the former, one pint measure (holding one pound avoirdupois of distilled water at 60°) afforded, on evaporation, 88 grains of dry saline residue; of the latter, a pint measure yielded 65 grains of residue. As the contents of these waters almost exactly resemble each other in quality, it will only be necessary particularly to describe the nature of the stronger spring.

On applying heat to the water, a small portion of carbonic acid escaped, it gradually became opalescent; and when it boiled, was turbid, and let fall a white powder, which was presumed to be carbonate of lime, deposited, as is usually the case, in consequence of the loss of the excess of carbonic acid previously holding it in solution. But on examining the precipitate, it was found to be carbonate of magnesia, the source of which was not at first very obvious, more especially as the water was slightly alkaline after having been somewhat reduced by evaporation.

A pint of the water mixed with excess of carbonate of ammonia, and boiled down so as to precipitate the earths of the earthy salts, afforded a mixture of carbonate of lime and of carbonate of magnesia, which being dissolved in excess of muriatic acid, neutralized by ammonia, and decomposed by oxalate of ammonia, yielded a precipitate, from which 1.25 grains of lime were obtained. The remaining solution, evaporated to dryness and exposed to a red heat, afforded a residue of 21.25 grains of magnesia.

The only alkali present in the water was soda, which, estimated in the state of sulphate, amounted to 10.52 grains of dry soda.

The quantity of sulphuric and muriatic acids present was determined by precipitation with muriate of baryta and nitrate of silver—the former furnished a precipitate equivalent to 33 grains of dry sulphuric acid—and the latter to 21 grains of muriatic acid. The combined carbonic acid in the water was estimated at 0.98 grains, From the preceding statement it appears, that the substances present in a pint of the stronger water are as follow:—

	Grains.
Sulphuric acid	33.00
Muriatic acid	21.00
Carbonic acid	00.98
Magnesia	21.25
Soda	10.52
Lime	1.25
	88.00

Various opinions may be formed respecting the nature of the saline combinations resulting from the union of these proximate elements. From the successive separation of the saline contents during the evaporation of the water, I am induced to consider them arranged as follows, forming the solid contents of a pint of the water, *viz.*:

	Grains.
Sulphate of magnesia	38.0
Muriate of magnesia	24.5
Common salt	9.3
Sulphate of soda	10.8
Sulphate of lime	3.0
Carbonate of soda	2.4
	88.0

The singular property of this water, that when boiled it throws down carbonate of magnesia, appears to depend upon the action of the carbonate of soda, which, though compatible with the earthy salts in a cold and dilute solution, decomposes them at a boiling heat, or when concentrated by evaporation. This has been verified by artificial imitations of this mineral water; and it has even been found that sesqui-carbonate of soda, in such excess as powerfully to redden turmeric paper, is compatible, not only in solutions containing the salts of magnesia, but also in those holding the more soluble salts of lime. In both these cases, precipitates are obtained by boiling, and by evaporation—otherwise the solutions remain transparent.

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Art. VI.—*On the Tyrant Shrikes of America.* *By* William Swainson, Esq., F.R. and L.S.

[Communicated by the Author.]

There are few groups in zoology which may be termed more truly natural, than those which are not only characterized by a general conformity in the structure and economy of the individuals, but are further remarkable as being confined within certain geographic limits. The group of birds which may be typically represented by the *Lanius tyrannus* of Linnæus, (and which form the genus *Tyrannus* of M. Vieillot,) is one of the many examples of these natural divisions in ornithology. They are altogether natives of the new world, where they may be said to represent the Drongo Shrikes (*Edolius*, Cuv.) of Africa and Asia.

The *Tyranni* are among those large insectivorous birds, who seize their prey in the air, but only devour it when again at rest. Like the true Shrikes, they are of a bold and quarrelsome disposition; unsocial and solitary in their habits, they will seldom permit other birds to come within a certain distance of those stations which they fix upon as their temporary throne. The testimony of others concurs with my personal observations to prove, that several of the larger species partake of those carnivorous habits which belong to the *Thamnophilinæ*. Indeed, on one occasion, I took from the stomach of *Tyrannus sulphuratus* a small species of lizard, which, however, was sufficiently large to excite astonishment that it ever could have been swallowed, in an entire state, by a bird not larger than a thrush. The frequent opportunities which a residence in Brazil afforded me of observing the habits of these birds, leads also to the belief, that they never use their claws either for seizing their prey, or even for holding it, when secured in the first instance by the bill. This supposition is in some degree confirmed by the structure of the tarsi, which are comparatively short, and much too feeble for such purposes; the claws are nevertheless very acute, and are probably used as offensive weapons against other birds much larger than themselves, which the Tyrants are known to attack, and even to defeat, during the season of rearing and providing for their offspring.

The *Tyranni*, for the most part, catch their prey in the air, and the organs of flight are consequently much developed; the wings are long and nearly pointed; and the tail, although but slightly divided in some species, is, in others, very long and remarkably forked. These characters are, nevertheless, considerably modified as we depart from the typical species; and totally disappear in a few individuals, whose feeble wings and lengthened tarsi seem to point them out as analogous to the *Drymophilæ* and certain other groups among the *Thamnophilinæ;* a resemblance which is, in some degree, extended to the colour of their plumage.

It may be adduced, as a proof of the accurate and comprehensive views of the great Swedish naturalist, that with little or no opportunity of ascertaining the natural habits of these birds, he should nevertheless have given them that place in his system, which more recent discoveries have shewn to be in perfect unison with nature. If the birds arranged by Linnæus under the genus *Lanius*, are considered as a family, divisible into other groups of an inferior denomination, we shall find that one of these will comprise the *Tyranninæ*. I have expressed myself more fully on this point, in some general remarks on this family recently published, wherein I have attempted to investigate the *Laniadæ* with reference to the quinary distribution of Mr. W. S. M'Leay. Since those observations were printed, I have had reason to suspect that the series in which I had conjectured the five principal groups would have followed each other in natural affinity, is objectionable, or in other words, that a relation of analogy has been mistaken for one of affinity: my suspicions on this head have been confirmed by a subsequent perusal of the elaborate and philosophic paper on the "Affinities of Birds," contained in the last volume of the *Linnæan Transactions.* And I am now induced to think that the *Tyranninæ* will be found to occupy an intermediate station between the sub-families of *Edolina* and *Ceblepyrina.* When it is considered that the study of affinities is but yet in its infancy, and that the materials by which it is to be prosecuted are few and imperfect, it must of necessity follow, that our deductions will frequently be open to correction and amendment.

My object in this paper is to arrange and describe all those birds, more particularly related to the genus *Tyrannus*, which have come under my immediate observation, distributing them into minor groups, from characters which seem to indicate a difference in their respective economy. In addition to those species which have been noticed by former writers, I have been enabled to add many others, new to the ornithologists of this country, and which are part of the fruits of Mr. William Bullock's zoological researches in the little-known provinces of the table land of Mexico.

TYRANNUS. *Brisson.* Vieillot.

Ordo. Insessores. Vigors.
Fam. *Laniadæ*. Vigors.

Section I.

Rostrum magnum.
Alæ mediocres, remigum pogoniis internis immarginatis.
Cauda æqualis.

Obs. The few birds which are placed in this section are principally distinguished from those which follow, by the more powerful construction of the bill; in this character, and also in the truncated form of the tail, they seem to make a near approach to the genus *Psaris* of M. Cuvier. The inner webs of the primary quill-feathers are entire; and the wings comparatively shorter than in the genuine Tyrants; hence a weaker power of flight may be inferred; while the superior size of the bill seems to indicate those carnivorous habits before alluded to.

Sp. 1. Tyrannus *sulphuratus*. Vieillot.
Bentevé, or Brazilian Tyrant.

T. rufo-fuscus, infrà flavus; vertice nigro aurantiacoque vario; jugulo annuloque verticem cingente albis; rostro elongato, compresso.

Rufous brown, beneath yellow; crown varied with black and golden yellow; throat and ring encircling the crown, white; bill lengthened, compressed.

Lanius sulphuratus. Lin. 1. p. 137. Ind. Orn. 1. 79.
Lanius cayanensis luteus. Brisson Orn. 1. p. 176. Pl. 16. f. 4. Pl. Enl. 296. 249.
Tyrannus sulphuratus. Vieil. Ois. de L'Am. 1. pl. 47.

Inhabits Brazil, Mexico, and Cayenne.
Total length 8½, bill from the rictus $1\frac{4}{10}$, wings $4\frac{2}{10}$, tail 3, tarsi $\frac{9}{10}$.

The lengthened and compressed bill of this bird, joined to the fact of its occasionally feeding upon reptiles, gives it some analogy to the *Thamnophilinæ,* although its economy in other respects is very different.

Sp. 2. Tyrannus *Pitangua.*

Broad-billed Tyrant.

T. rufo-fuscus, infrà flavus; vertice nigro aurantiacoque vario; jugulo annuloque verticem cingente albis; rostro maximo, depresso.

Rufous brown, beneath yellow; crown varied with black and yellow; throat and ring encircling the crown, white; bill very large, depressed.

Lanius Pitangua, Lin. (Ed. 13. Vindob. 1767.) p. 136. Gm. 303. Ind. Orn. 1. 78. Pl. Enl. 212.
Tyrannus Braziliensis. Brisson. Ois. 2. tab. 36. f. 5.
Tyrannus Bentaveo. Vieil. Ois. Am. pl. 1. f. 16. (rostrum.)

This presents one of those many examples which will occur to the ornithologist, of one bird being clothed in precisely the same coloured plumage as that of another totally distinct. The description applicable to the colours of *T. sulphuratus*, will equally apply to *T. Pitangua*. These two species, in fact, are only to be known by the different construction of their bills; in *T. Pitangua* this organ is so unusually large and depressed, as to have induced M. Temminck to arrange the bird among the *Platyrhynchi;* while the bill of *T. sulphuratus*, as before observed, is altogether compressed.

Inhabits the intertropical countries of America, but is much less frequent in Brazil than the last.

Total length 9, bill $1\frac{3}{10}$, in breadth $\frac{6}{10}$, wings $4\frac{3}{4}$, tail $3\frac{1}{4}$, tarsi $\frac{6}{10}$.

Sp. 3. Tyrannus *audax.*

T. fuscus albo varius, albus infra strigis fuscis varius; cristâ flavâ; caudâ æquali rufo marginatâ.

Brown variegated with white, beneath white, with brown stripes; crest yellow; tail even, margined with rufous.

Mus. Audax. Lath. Yellow-crowned Fly-catcher. Gen. Syn. 3. 358. Pl. Enl. 453. 2.

Size of the Benteve. Bill large, black, depressed; plumage *above* blackish brown, variegated with whitish, each feather having a narrow lateral border of that colour; *crown* brown, with a concealed crest of beautiful yellow; on each side of the head, above the eye, is a white band; and another extends from the rictus to the base of the ears: *under plumage* white, with a short stripe of blackish-brown in the middle of each feather. These stripes are most conspicuous on the throat and breast; they become faint on the abdomen, which has a pale tinge of yellow: *wings* brown, the margins of all the feathers whitish; *tail* moderate, brown, and even; the margins of all the feathers, and those of the upper tail covers, strongly tinged with rufous: *tarsi* black, rather shorter than those of *T. sulphuratus*, but less robust.

Inhabits northern Brazil.

Total length 8 inches, bill from the culmen $\frac{9}{10}$, wings $4\frac{1}{4}$, tail $3\frac{1}{2}$, tarsi $\frac{7}{10}$.

There is so little apparent difference between this bird and the Yellow-crowned Fly-catcher of Latham, figured at pl. 453 of the *Planches Enluminées*, that they may probably belong but to one species. The size of the bill is only inferior to that of *T. Pitangua*. The specimen here described came from Brazil, where the species is not common.

Sp. 4. Tyrannus *crinitus*.

T. olivaceus, infrà sulphuratus; jugulo pectoreque cinereis; remigum primorum margine, rectriciumque superficie inferiori ferrugineis.

Gray olivaceous, beneath sulphur-yellow; throat and breast cinereous; margin of the greater quills, and under side of the tail, ferruginous.

Muscicapa crinita Linn. 1. 325. Ind. Orn. 2. 485. Wilson, Am. Orn.
Tyrannus Ludovicianus. Vieil. Ois. Am. 1. pl. 45.
Muscicapa Ludoviciana. Ind. Orn. 2. p. 486.

Head with an incumbent crest; upper plumage dull olivaceous; sides of the head, ears, throat, and breast, light slate colour, which then changes into a pale sulphur-yellow; wings blackish, the two series of covers, and the margins of the lesser and scapular quills bordered with yellowish white, while the greater quills, both externally and internally, are margined with ferruginous. Tail even; the outer webs brown, the inner ferruginous; on their under surface this last colour predominates: bill brown; tarsi slender, black.

There is little doubt that Dr. Latham has inadvertently described this bird under two names. M. Vieillot concurs in this opinion, but instead of retaining the original name of Linnæus, has substitnted that of Latham. Wilson has given an interesting account of its manners, and has figured it most correctly.

Inhabits North America.

Total length $8\frac{1}{4}$, bill 1, wings 4, tail $3\frac{3}{4}$, tarsi $\frac{8}{10}$.

Sp. 5. Tyrannus *calcaratus*. Sp. Nov.
Spiny-footed Tyrant.

T. olivaceo-fuscus, abdomine fulvescente; genibus spinuliferis.

Olivaceous brown, tinged on the body with fulvous; knees armed with small acute spines.

This very singular species is in a slight degree larger than the last; and is particularly remarkable for a series of small but very acute spines, seven or eight in number, resembling the teeth of a saw, which are placed at the back of the tarsi, immediately adjoining the knee; from this part they gradually diminish in size, and unite to a series of small scales which extend to the base of the tarsi. In other respects, I can discover nothing in the structure of the bird which would justify me in detaching it from its present congeners.

The bill is black, and equal in size to that of *T. crinitus;* but the sides are more compressed, and the tip more hooked; the bristles which surround it are also longer. The colour of the whole bird is a dull olive gray, paler beneath, and tinged on the body with dirty yellow. The wings are moderate, the primary quills not emarginate, and the tail as even. The tarsi are short, weak, and blackish; and the claws small.

I met with three specimens of this bird in the province of Bahia in Brazil; it appeared of rare occurrence, and I am totally unacquainted with its habits.

Total length 8, bill $1\frac{1}{10}$, wings $3\frac{3}{4}$, tail $3\frac{1}{2}$, tarsi $\frac{8}{10}$.

SECTION II.

Rostrum mediocre.
Alæ longæ, remigum pogoniis internis emarginatis.
Cauda mediocris, ferè æqualis.
Tarsi breves.

The character which more particularly belongs to the birds of this section, is the emargination or sudden contraction in the breadth of the inner web of several of the primary quills; and which, in some species, is so considerable, as to give these feathers an appearance of being pointed; a similar construction, though in general less developed, is observed to characterize the true falcons, and in all probability has a reference to those superior powers of flight for which both these groups are remarkable. The tail, though sometimes slightly forked, is generally even; while the bill, without being particularly large, is yet comparatively strong; and the upper mandible still preserves the convex form, so conspicuous in the last group.

Sp. 6. Tyrannus *crassirostris*. Sp. Nov.

Thick-billed Tyrant.

T. griseo-fuscus, infrà pallidè flavus; mento juguloque albis; remige primo subacuminato; rostro valido.

Gray brown, beneath pale yellow; chin and throat white, first primary quill obsoletely pointed; bill strong.

This species, recently sent from Mexico, is rendered doubly interesting by the union it presents of the characters which principally distinguish the first and second sections of this group. The size and strength of the bill would associate it with the first, while the comparative length of the wings, the first quill-feather of which is obsoletely pointed, places it within the limits of the second.

T. crassirostris is equal in size to a thrush. The upper plumage is light grayish-brown, somewhat darker on the head, the tail, and the greater quill-feathers; the crown has a concealed yellow crest, and the ear-feathers are dull black; the chin and throat are white, but the rest of the under plumage is pale yellow: tail even; the upper covers tinged with rufous.

Inhabits the warm districts of Mexico, and frequents, like others of its tribe, the upper branches of trees, from which it drives away all other birds.

Total length 9¼, bill 1¼, wings above 5, tail 4, tarsi ¾.

Sp. 7. Tyrannus *vociferans*. Sp. Nov.

Noisy Tyrant.

T. olivaceo-griseus, infrà flavus; cristâ rubrâ; capite juguloque cinereis; mento albo; caudâ nigrâ, æquali; remigibus primoribus acuminatis.

Olive-grey, beneath yellow; crest red; head and throat cinereous; chin white; tail black, even; primary quills pointed.

Size rather less than the last; the bill is small in proportion to the size of the bird, but is larger than that of *T. intrepidus*. The plumage above is grayish, tinged with olive: but the head, neck, and throat, as far as the breast, are pure slate-colour: the crown has a concealed crest of beautiful orange; the under plumage from the breast is pale yellow. The wings are very long, and all the primary quills are abruptly pointed: the tail and its covers are deep black.

Total length 8½, bill scarcely 1, wings 5, tail 3¾.

From the notes of Mr. W. Bullock, it appears that this species is very common in the neighbourhood of Temascaltepec, a small village in the province of Mexico, where that gentleman has long resided. He describes it as a noisy and particularly quarrelsome bird, generally taking its station on the high branches of trees, and commencing a disagreeable chattering noise the moment any other bird presumes to alight upon its temporary domain. It is even said to attack hawks, but probably this is done only during the season of incubation.

Sp. 8. TYRANNUS *intrepidus.* Vieillot.

King-bird, or Carolina Tyrant.

T. nigro-cinereus, albus infrà; vertice caudâque nigris; cristâ aurantiacâ; rectricium apicibus albis; remigibus primoribus acuminatis.

Obscure cinereous, beneath white; head and tail black, crest orange; tips of the tail-feathers white; primary quills pointed.

Lanius Tyrannus. Lin. Gm. 302.
Tyrannus Brisson. Orn. 267.
——— pipiri. Vieill. Ois. de L'Am. 1. 44.
——— intrepidus. Id. Gal. Ois. 214. pl. 133. fem?
King-bird, or Tyrant Fly-catcher. Wilson, Am. Orn. 2. pl. 13. f. 1.

The long migrations of this well-known bird may account for its having been met with on the shores of Mexico by Mr. Bullock. I believe it has never been found on the South American continent.

The wings are long, and the exterior primary quills abruptly pointed: the tail has been described by authors as *even*, but in two specimens now before me, the middle pair of feathers are rather longer than the others.

Sp. 9. TYRANNUS *griseus.* Vieillot.

Gray Tyrant.

T. cinereus, albus infrà; auribus nigris; remigibus primoribus apicem versus acuminatis; caudâ subfurcatâ.

Cinereous, beneath white; ears black; greater quills towards their extremities abruptly pointed; tail forked.

Le Tyran de St. Domingue? Brisson, Ois. 2. pl. 38. f. 2.
St. Domingo Tyrant. Gen. Syn. 1. 185.
Tyrannus griseus. Vieil. Ois. de L'Am. 1. pl. 46.

Size rather larger than that of *T. intrepidus*, particularly the bill, which is much stronger and more convex. The general colour of the upper plumage is light cinereous gray, with a faint tinge of rufous on the tail-covers; under parts white, but grayish on the breast, and pale yellow on the inferior covers of the tail, and the inner covers of the wings: all the primary quills are abruptly pointed: tail forked, the feathers black.

Such is the description of a bird brought by Mr. Bullock from the shores of Mexico. It is probably the *T. griseus* of M. Vieillot, whose account, so far as it goes, is not inapplicable.

Total length $8\frac{3}{4}$, bill $1\frac{1}{4}$, wings $4\frac{3}{4}$, tail 4, depth of the fork $\frac{1}{2}$.

Sp. 10. TYRANNUS *crudelis*. Sp. Nov.
Gray-headed Tyrant.

T. olivaceus, flavus infrà; capite cerviceque cinereis; cristâ aurantiacâ; mento juguloque cinereo-albis; remigibus primoribus acuminatis; caudâ furcatâ.

Olive, beneath yellow; head and neck cinereous; crest orange; chin and throat cinereous-white; primary quills pointed; tail forked.

Size of *T. crinitus*, but the wings are longer, and the bill much larger. The upper part of the head and neck clear cinereous, which colour, although of a much paler hue, tinges the throat, leaving the chin almost white; the ear-feathers are dusky black, and on the crown is a concealed crest of bright orange-coloured feathers: the remaining parts of the upper plumage are dull olive, and of the under pure yellow. Wings blackish-brown; all the primaries are abruptly pointed; the covers and lesser quills have whitish margins. Tail blackish, rather lengthened, and forked to the depth of more than half an inch. Bill and tarsi black, the latter very short.

Inhabits open cultivated tracts in northern Brazil; feeding both upon insects and reptiles.

Total length $8\frac{3}{4}$, bill, extreme length, $1\frac{2}{10}$, wings $4\frac{2}{10}$, tail $3\frac{3}{4}$, tarsi $\frac{1}{2}$.

I feel some hesitation in pronouncing this to be an undescribed bird; for, as it is by no means uncommon in the northern provinces of Brazil, it appears somewhat strange that, if it be new, it should so long have escaped the observation of naturalists. Nevertheless, it cannot be referred to any of those species, either of the genus *Lanius* or *Muscicapa*, described in the *General Synopsis* of the venerable Latham. It approaches nearest to the Tyrant Fly-catcher of that author, but is near two inches longer, while the hind part of the neck, instead of being "deep brown," is cinereous.

The birds, thus far enumerated, are among the largest and most powerful of the *Tyranni*. Those which follow are rather smaller, but are placed at the extremity of this section, on account of the length and emargination of the quill-feathers.

Sp. 11. Tyrannus *leucotis.* Sp. Nov.

White-eared Tyrant.

T. griseo-fuscus, albens infrà; vertice nigricante; cristâ flavâ; temporibus albis; strigâ oculari albâ; remigibus acuminatis; caudâ æquali.

Gray-brown, beneath whitish; crown blackish, crest yellow; sides of the head white with a black stripe; quills pointed, tail even.

Le Barbichon de Cayenne, femelle. Pl. Enl. 830. f. 2?

Size of the Cayenne Fly-catcher. Colour above, dark greyish-brown, obscurely spotted on the back; under parts dull whitish, tinged with gray on the breast, and with yellow at the vent. The crown of the head, and a broad stripe on its sides, which passes from the nostrils to the upper part of the ears, are blackish-brown, but this colour only conceals a crest of beautiful yellow feathers: above the eye is a white stripe, which extends to the nape; and beneath is another, which forms a white spot at the base of the ears. The wings are pointed and brown; the covers and scapulars have white margins, and the four first primary quills are abruptly pointed. The tail-feathers are even and brown; their margins, as well as those of the upper covers, being ferruginous: inner wing-covers pale yellow; tarsi short, weak, and black: the bill is of the same size as that of *T. Savana.*

Inhabits Northern Brazil—rare.

Total length 7, bill $\frac{8}{10}$, wings $3\frac{3}{4}$, tail $3\frac{1}{4}$, tarsi $\frac{6}{10}$.

Obs.—I strongly suspect that this bird is represented in the Pl. Enl., as the supposed female of *Muscicapa barbata*, Lath. At all events this seems to be a mistake, for in specimens of the female of this last species, dissected by myself in Brazil, there is no appearance of the yellow crest so conspicuous in the male bird.

Sp. 12. Tyrannus *ferox.*

Brown-crested Tyrant.

T. griseo-fuscus, infrà pallidè flavus; jugulo pectorique cinereo-albis; capite cristato; remigibus semiobtusis; caudâ æquali.

Gray-brown, beneath pale yellow; throat and breast cinereous white; head crested; quills semiobtuse; tail even.

Tyrant Fly-catcher? Lath. Gen. Syn. 3. p. 357.
Le Petit Tyran de Cayenne? Pl. Enl. 571. f. 1.

Size of *T. leucotis*, but the bill is much larger. The bristles of the rictus are considerably developed, and reach as far as two thirds the length of the bill. All the upper plumage is dull, grayish-brown, slightly tinged with olive: the sides of the head are more cinereous; the chin and throat nearly white, changing on the breast to a very pale yellow; the crown of the head is crested, and of a uniform brownish colour. The wings are brown, having the covers and scapulars margined by greyish-white, and the first quill gradually and very slightly pointed. Tail brown and even; tarsi rather lengthened, and black.

Inhabits Brazil.

Total length 7½, bill 1, wings 3½, tail 3½, tarsi rather more than seven-tenths of an inch.

The characters which might otherwise guide us in determining the natural situation of this bird, are so slightly developed, and are of such a mixed nature, that I feel some hesitation in placing it at the extremity of this group. In so doing, I have been guided by the size and strength of the bill, which equals that of *T. intrepidus*, and by the slightly-pointed form of the first quill-feather. Should this species be the *Muscicapa ferox* of Latham, its manners (as described by that author) seem no less to bring it within the confines of the present division. The tarsi, though somewhat lengthened, are weak, and apparently not adapted for walking. It is very doubtful whether the figure which I have quoted from Buffon is really intended to represent the bird here described; for in this, and many other instances, the artist has omitted those minute but essential characters which are the indications of separate species, apparently similar. In the copy of this work now before me, the figure in question is coloured, so as to resemble *Tyrannus crinitus*.

Section III.

Alæ mediocres.
Tarsi elongati.
Cauda æqualis.

The only peculiarity by which the birds of this section seem to be marked, consists in the unusual length of the tarsi; a structure which is strongly opposed to the shortness and comparative debility of these members in all the other groups. Hence I am led to conjecture, that these birds habitually frequent the ground, and probably derive their chief support from apterous insects; or at least from tribes more generally found in such a situation. This, at least, is the economy of one species. In other respects they seem to have little *specific* affinity with each other, and the group may be considered as somewhat artificial.

Sp. 13. TYRANNUS *cinereus.*
Gray-throated Tyrant.

T. ferrugineus; capite, cervice juguloque cinereis; uropygio caudâque rufis; alis brevibus; tarsis elongatis.

Ferruginous; head, neck, and throat cinereous; rump and tail rufous; wings short; tarsi lengthened.

Muscicapa cinerea. Gm. 933. Ind. Orn. 2. 488.
Le gobe mouche roux de Cayenne. Brisson. Ois. Supp. p. 51. pl. 3. f. 3.
Rufous-bellied Flycatcher. Lath. Gen. Syn. 3. 363.

Size of *T. calcaratus;* but the bill is rather larger, and less depressed, although equally beset with stiff feathers and lengthened bristles: the upper mandible is brown, the under yellowish; the head, neck, and throat is cinereous, darkest above, while the feathers beneath have their margins nearly white: the shafts of those on the crown are black. Wings and middle of the back rufous, graduating into a bright ferruginous towards the rump. The tail is rather short, even, and rufous; the under plumage, from the breast, is of the same tint as the rump. Wings short and feeble; tarsi pale and lengthened, with a series of small, reticulated, posterior scales. Claws strong, and nearly equal in size to those of *T. ambulans.*

Inhabits Brazil: manners unknown.

Total length 8, bill 1¼, wings 3½, tail 3, tarsi 1; the outer toe connected, as far as the first joint, to the middle toe.

The figure of this species, in the valuable work of M. Brisson, is very characteristic; but that which has usually been referred to in the *Planches Enluminées*, seems to me intended for a very different bird.

Sp. 14. TYRANNUS *rufescens.* Sp. Nov.?
Yellow-rumped Tyrant.

T. ferrugineus, infrà pallidior; abdomine albente; uropygio fulvo; caudâ rufâ; alis brevibus; tarsis elongatis.

Ferruginous, beneath paler; body whitish; rump fulvous; tail rufous; wings short; tarsi lengthened.

Yellow-rumped Fly-catcher? Lath. Gen. Syn. 3. 354.

Considerably less than the last, though closely allied to it. The plumage above is rufous brown, which changes to buff-coloured yellow on the rump and upper tail-covers. Wing-covers dusky-black, tipt with rufous-brown; and this last colour forms three bands on each wing. Under plumage, from the chin to the breast, light brown; body white; vent and under tail-covers tinged with yellow. Tail nearly even, rather short, and entirely rufous. Tarsi pale and lengthened, having a small row of posterior scales similar to *T. cinereus;* claws moderate.

I am ignorant of the locality of this species, which is described from a specimen in my collection.

Total length $6\frac{3}{4}$, bill $1\frac{1}{10}$, wings $3\frac{1}{4}$, tail $2\frac{1}{2}$, tarsi $\frac{8}{10}$.

Sp. 15. TYRANNUS *ambulans*. Sp. Nov.
Walking Tyrant.

T. olivaceo-fuscus, infrà flavus; jugulo rectriciumque apicibus pallidioribus; cristâ aurantiacâ; remigibus primoribus acuminatis; tarsis elongatis, validis.

Olive-brown, beneath yellow; throat and tips of the tail paler; crest orange; primary quills pointed; tarsi lengthened, robust.

The power of locomotion, in the more characteristic examples of this family, is principally confined to the wing, but in the bird before us, this faculty is equally divided between the wings and the legs. Habitually frequenting the ground, upon which it walks and occasionally runs like a lark, it nevertheless is an air-feeding bird; for, during these excursions, it is perpetually making short, and nearly perpendicular, flights after such insects as have just taken wing. Notwithstanding every watchfulness on my part, I never could detect this bird using its feet in any other situation than on the ground.

I met with a few individuals of *T. ambulans*, during my stay at Pernambuco, frequenting the sandy pastures on the outskirts of the town; but I never again saw the bird during my travels in the other provinces.

Size rather larger than the Cayenne Fly-catcher; bill black, rather long, and resembling in form that of *T. sulphuratus*. Plumage above drab-coloured brown, beneath yellow, verging towards white on the chin and throat. Wings and tail deep brown; the latter moderate and even, having the side feathers tipt with yellowish-white: the front and sides of the head are greyish-brown, and the crest is orange-red. The two first primary quills are not emarginate, but are rather shorter than usual, and gradually pointed. The legs, as before observed, are long and robust.

Total length $7\frac{1}{4}$, bill 1, wings $3\frac{3}{4}$, tail 3, tarsi 1, middle claw nearly one inch.

Sp. 16. TYRANNUS *nengeta*.
Black and white-winged Tyrant.

T. griseus, albus infrà; alis caudâque nigris; remigum primorum basi, secundorum apicibus, rectricibusque, albis; tarsis elongatis.

Gray, beneath white; wings and tail black; base of the primaries, and tips of the secundaries and tail-feathers white; tarsi elongated.
Guiraru Nheengeta Braziliensis. Raii Syn. p. 166.
Le Guiraro? Sonnini, vol. 13. p. 324.
Le Cotiuga gris? Brisson, Ois. 2. p. 353.

This singular species is clothed in a plumage so closely resembling that of the American Mocking-bird, that even a professed naturalist would not immediately recognise the difference: This similitude is even carried on, in some degree, to the bill; which, although sufficiently depressed at the base, to remove every doubt as to the true affinities of the bird, is yet compressed on the sides, by which it assumes somewhat of that narrow form observable in the thrushes.

Size of a blackbird. The upper plumage, and also the breast and flanks beneath are of a cinereous grey; the throat and body beneath being white: across the front, from eye to eye, is a white line; between the eye and rictus the colour is blackish; and below the ears is a lengthened black stripe which joins the under mandible. The wings are rather long; the covers and scapular quills blackish with gray margins; the spurious quills entirely black; the primaries are also deep black with a band of pure white at their base; this band widens as it approaches the lesser quills, two of which are pure white, tipt with black; the rest being black, tipt with white. The two first quill-feathers are notched, and obsoletely pointed. The tail is moderate, half concealed by the covers, and slightly forked: but in old birds (the ends of the feathers being frequently worn) it appears even. The colour is deep black, broadly tipt by grayish-white. Tarsi and toes lengthened and black; claws acute.

Inhabits Brazil.

Total length 9, bill $1\frac{1}{10}$, wings $5\frac{1}{2}$, tail 4, depth of the fork $\frac{1}{2}$, tarsi 1, middle claw $\frac{9}{10}$.

I have been unusually minute in describing this bird, as a good deal of doubt and confusion has crept into its systematic history; three other birds having been mistaken for what appears to me the *Guiraru Nheengeta Braziliensis* of the old authors. The original description of Marcgrave is indeed very concise, and no doubt has been the original source of error. It is, however, probable, that this early writer intended to record the bird here described, since no other that I am acquainted with from Brazil, will so nearly fall in with his account. M. Sonnini is the only naturalist who seems to have had the same bird in view, or who has alluded to the form of the tail, which he says is *even* (*carrée.*)

Linnæus, in all probability misled by the brevity of Marcgrave, has likewise referred to the Gray Pye of Edwards, pl. 318, as belonging to the same species; and adds, "*cauda cuneiformi.*" The figure of Edwards certainly represents a bird closely resembling that indicated by Marcgrave; but as the tail is neither even nor forked, but much cuneated, it is scarcely possible to suppose that all these variations in the form of such an important part, would occur in different individuals of one species.

The third species, which has been included among the synonymes of *Lanius Nengeta*, is the *Colinga gris* of the *Planches Enluminées*, No. 699; but this bird (as M. Le Vaillant has already observed) is the *Ampelis pompadora* of Linnæus, in a young state of plumage.

I very much regret that the scarcity of *T. nengeta*, in those parts of Brazil which I had an opportunity of visiting, prevented me from noticing its natural habits. Dr. Latham observes, that in Guiana it assembles in flocks near watery places, and utters a loud cry. The unusual length of the legs, joined to the worn appearance of the tail-feathers in two specimens before me, suggest the idea, that this species, like *T. ambulans*, probably frequents the ground, from which it darts upon such insects as endeavour to effect their escape by flight. The length of the wings and the emargination of the quills, certainly point it out as an air-feeding bird; although its precise situation in this subfamily appears to me uncertain.

As we recede from the larger and more typical *Tyranni*, we are insensibly led to a group of smaller birds, likewise peculiar to the American continent, and of which the Cayenne Fly-catcher is a familiar example. They retain some of the minor characteristics of their more powerful allies, while in the general weakness of their organization they seem equally related to the Fly-catchers; to which family they have been referred by most ornithologists. I therefore refrain from offering any further remarks upon them in this place, but proceed to notice another group, which belongs more decidedly to the *Tyranni* of M. Vieillot.

SECTION IV.

Alæ longæ, remigum pogoniis internis emarginatis.
Cauda longissima, forficata.

The Muscicapa Tyrannus of Linnæus (the *Savana* of Buffon) may be taken as the type of a division strongly characterized by the tail being unusually long and very deeply forked; this structure, aided by a corresponding power in the wings, enables these birds to fly with a rapidity even greater than that exerted by those of the second division; both groups approach each other in general conformation, but the different form of their tails, and a variation in their economy, seem to warrant a separation. The habits of the *Tyranni* in general are solitary; for, although several of the same species may be seen within a limited distance, they never act in concert; but the *Tyrannus furcatus* is in some degree gregarious; it shews a fondness for the society of its own species, and frequently assembles in the air in bands of near two hundred. Whether this is for the purpose of pursuing insects, or preparatory to migration, M. Vieillot (who quotes the authority of Azara) has not distinctly stated; but the fact itself is curious, and moreover points out a considerable resemblance to the gregarious habits of the African Drongos; the typical groups of which are no less remarkable for the great length of their tails.

There are but few birds, yet known, referrible to this section. The variety of *T. Savana*, said to be found in Canada, may probably be distinct. The *Muscicapa forficata* of Latham is known to me only by the figure in Buffon, but from that it seems to possess the typical characters in great perfection; a *fourth* species, which I believe is here described for the first time, will complete the list.

Sp. 17. TYRANNUS *Savana*. Vieil.
Fork-tailed Tyrant.

T. cinereus, albus infrà; vertice auribusque nigris; alis fuscis; caudâ longissimâ, valdè furcatâ, nigrâ.

Cinereous, beneath white; crown and ears black; wings brown; tail very long, deeply forked, and black.

Muscicapa Tyrannus. Lin. 1. 325. Ind. Orn. 2. 484.
Le Tyran à queue fourchue. Brisson, Ois. 2. pl. 39. f. 3.
Fork-tailed Fly-catcher. Gen. Syn. 3. p. 355.
Tyrannus Savana. Vieil. Ois. Am. 1. pl. 43.

Size of a lark; bill black, and smaller than that of *T. intrepidus;* sides, ears, and upper part of the head deep black, concealing a crest of bright yellow on the crown; general colour of the upper plumage light cinereous, changing to blackish on the rump: wings gray-brown, rather lengthened; the two first quills abruptly emarginated, or notched, very near their extremities; the exterior web of the first quill is pale yellow: all the under plumage is pure white. The tail is black and very long; the exterior pair of feathers exceed the rest by nearly three inches, and are margined externally, to half their length, by pale yellow. The tarsi are short and black.

Inhabits Brazil, where it is, however, rare; in other parts of South America it seems to be more common.

Total length $11\frac{1}{2}$, bill $\frac{9}{10}$, wings $4\frac{1}{4}$, tail 7, depth of the fork $4\frac{1}{4}$, tarsi $\frac{6}{10}$.

Sp. 18. Tyrannus *longipennis.* Sp. Nov.
Gray fork-tailed Tyrant.

T. cinereus; mento albente; caudâ fuscâ, longâ, furcatâ.
Cinereous; chin whitish; tail brown, long, and forked.

Size of the last, but the bill is smaller and more depressed. The whole of the plumage, both above and beneath, is cinereous or slate-coloured; the feathers on the crown form an incumbent crest, and are obscurely streaked with blackish; the chin is nearly white. The wings are long, and of a uniform sooty-black; all the primaries are gradually pointed, but the second is particularly narrow, and the point acute. The tail is considerably lengthened, (though much shorter than that of the last,) deeply forked, and sooty-black: the exterior pair of feathers are nearly three-quarters of an inch longer than the others, and are margined externally with white. Tarsi short, as in *T. Savana.*

Discovered in Brazil by M. Natterer, Zoologist to the Austrian Government; but I am not aware of its being yet described.

Total length $9\frac{1}{2}$, bill $\frac{8}{10}$, wings $4\frac{1}{2}$, tail 5, depth of the fork $1\frac{3}{4}$, tarsi $\frac{7}{10}$.

In this attempt to characterize the leading groups among the *Tyranni*, it will be seen that their distinctions principally rest on characters hitherto considered of little importance. I have therefore found it necessary to give detailed, and often minute, descriptions of those species, whose identity was in any way questionable. Had this not been done, the modifications by which nature gradually passes from one form to another, could not have been traced. It must also be remembered, that the clear elucidation of species more particularly concerns the application of Natural History to the practical purposes of life; while nothing will more contribute to aid the general views of those naturalists, who study the grander operations of nature.

TYRANNI.

Tabular Synopsis of the Species.

Wings moderate. Primary quills emarginate and obtuse.		Brown, beneath yellow; bill long, compressed	*Sulphuratus*	1
		Brown, beneath yellow; bill broad, depressed	*Pitangua*	2
		—— varied with white; beneath white, with brown stripes	*Audax*	3
		Olive, beneath yellow; throat cinereous; wings and tail externally rufous	*Crinitus*	4
		Olive-brown; knees armed with spines	*Calcaratus*	5
Wings lengthened. Primary quills emarginate and pointed.	Tail even.	Gray-brown, beneath yellow; first primary quill pointed	*Crassirostris*	6
		Olive-gray, ditto bill small, chin white; tail black	*vociferans*	7
		Blackish, beneath white; tips of the tail-feathers white	*intrepidus*	8
		Small; brown, beneath whitish; temples white, eye-stripe black	*leucotis*	11
		Gray-brown, beneath yellow; throat cinereous; first primary obsoletely pointed	*ferox*	12
	Tail forked.	Cinereous, beneath white, tail forked	*griseus*	9
		Olive, beneath yellow; head and neck cinereous	*crudelis*	10
Tarsi long. Wings lengthened.		Ferruginous, head and throat cinereous, wings short	*cinereus*	13
		——— beneath paler, wings short	*rufescens*	14
		Olive-brown, beneath yellow; crest orange; tarsi strong	*ambulans*	15
		Grey, beneath white; wings and tail black, varied with white	*Nengeta*	16
Tail very long, forked.		Cinereous, beneath white; crown and ears black	*Savana*	17
		Gray, chin white, tail brown	*longipennis*	18

TYRANNI.

Specierum Synopsis.

Alæ mediocres, remigum pogoniis internis integris		Rufo-fuscus, infrà flavus; rostro elongato; compresso	*sulphuratus*	1
		Rufo-fuscus, infrà flavus; rostro maximo, depresso	*pitangua*	2
		Nigro-fuscus, albo varius, albus infrà strigis fuscis varius	*aulax*	3
		Olivaceus, infrà flavus, jugulo cinereo remigum rectriciumque margine rufo	*crinitus*	4
		Olivaceo-fuscus; genibus spinuliferis	*calcaratus*	5
Alæ elongatæ, remigum pogoniis internis emarginatis	Cauda æqualis.	Griseo-fuscus, infrà flavus; remige primo sub-acuminato	*crassirostris*	6
		Olivaceo-griseus, infrà flavus; mento albo; caudâ nigrâ	*vociferans*	7
		Nigro-cinereus, infrà albus; rectricium apicibus albis	*intrepidus*	8
		Parvus; fuscus, albens infrà; temporibus albis, strigâ oculari nigrâ	*leucotis*	11
		Griseo-fuscus, flavus infrà; jugulo cinereo; remige primo obsoletè accuminato	*ferox*	12
	Cauda subfurcata.	Cinereus, albus infrà; caudâ furcatâ	*griseus*	9
		Olivaceus, infrà flavus; cervice capiteque cinereis	*crudelis*	10
Tarsi elongati		Ferrugineus, capite juguloque cinereis; alis brevibus	*cinereus*	13
		——— infrà pallidior; alis brevibus	*rufescens*	14
		Olivaceo-fuscus, infrà flavus; cristâ aurantiacâ; tarsis validis	*ambulans*	15
Alæ elongatæ		Griseus, albus infrà; alis caudâque nigris, albo variis	*nengeta*	16
Cauda longissima, forficata		Cinereus, albus infrà; vertice auribusque nigris	*savana*	17
		Griseus, mento albo; caudâ fuscâ	*longipennis*	18

Art. VII. *Examination of the Large Achromatic of the Royal Observatory at Paris.*

Extract of a Letter from James South, Esq., F. R. S., *to* M. Schumacher.

Dear Sir, *Passy, près Paris,* 1825, *Sept.* 6.

I have the honour to transmit you the enclosed which I have received from Mr. Herschel. I have hitherto been prevented from doing much in the way of ordinary astronomy, by a resolution to complete my observations of the double stars; and I am glad to say, that my labours in this department of astronomical science are (at least for the present) fast drawing to a close. By the latter part of October next, I have little doubt but that my observatory at this place will be dismantled; and it is my intention to present the observations of 400 additional double stars to the Royal Society, at its first meeting in November.

From your Astronomische Nachrichten, and subsequently from the Memoirs of the Astronomical Society of London, I am glad to find that the Dorpat Achromatic realizes the expectations of its possessor; and I rejoice very heartily that it has fallen into such hands. Mr. Struve will not allow it to remain idle.

In the accompanying communication from Mr. Herschel, allusion is made to the achromatic constructed by Lerebours, now placed in, and belonging to, the Royal Observatory of Paris; perhaps a few lines devoted to it, will not be altogether uninteresting.

The diameter of its object glass uncovered by the cell (in English measure), is rather better than 9.2 inches, of which 8.4 inches only are in actual use; its focal length is 11 feet. The magnifying powers with which I used it, on the night of the 15th of March last, are 136. 153. 224. 240. 420. and 560. With all except 560 (which by some forgetfulness was not applied,) Venus was *extremely well* defined during *dark* night; of course, Jupiter and Saturn were *well* shewn. The two stars of Castor, of γ Leonis, of ζ Orionis, were exhibited with 240, 420, and 560 as round as possible: ω Leonis presented by its side a light blue star with

420, which could not be overlooked by the most careless observer, and with 560 both stars were *admirably* defined. Measures of position and of distance might have been gotten with the greatest facility, but for want of a micrometrical apparatus. It was my intention to have submitted the instrument, which was placed at my disposal by our amiable and ever-toiling friend, Monsr. Bouvard, in the most *unreserved manner possible*, to other and more severe tests; but in elevating the telescope to ε Bootis, the stand became deranged, and the instrument was rendered unmanageable. On a subsequent night, a similar accident also foiled us in our attempts to investigate its power.

I need not inform *you*, that a telescope having an object glass of the diameter above mentioned, which with these powers will neatly define the limb of the planet Venus, and will give to the discs of the double stars here named, images absolutely round, deserves to be well spoken of. Indeed I have no hesitation in saying, that this telescope is the best achromatic I ever pointed to the heavens; nor will I withhold my regret, or even the mortification I feel in asserting, that England, when I visited it in May last, could not produce an achromatic any thing like it. The stand upon which it is mounted is not provided with any means of giving to the telescope equatorial motion. I hope, however, that the Board of Longitude of Paris, in their accustomed zeal for the promotion of astronomical science, will, ere long, render this noble instrument more available to the purposes of scientific research, by voting to it the indispensable attributes of an equatorial. Gambeys' mechanical head would soon convert it into an instrument, which would be worthy of the French nation.

Whilst, however, I say thus much, I am far from entertaining the sentiments of Mr. Fraunhofer as to the decided superiority of refractors over reflectors; nor can I accompany Mr. Struve in his idea that the Dorpat telescope "may perhaps rank with the most celebrated of all reflecting telescopes, namely, Herschel's;" it is true, I have not had the enviable gratification of having seen the former; still I think the Paris telescope furnishes me with data, upon which to form something like a rational conjecture. Its object glass actually in use is in proportion to Mr. Struve's (if all of *it* be effective) as 70 to 92 nearly; a difference not very hard to be allowed for. I have seen the nebula in Orion; the planets Jupiter and Saturn with the Paris telescope; and with their appearances in Mr. Herschel's 20 feet reflector I am *perfectly familiar*, and the comparison is many times in favour of the latter.

The power of the 20 feet reflector at Slough is well authenticated; and if the indefatigable astronomer of Dorpat will turn his probably matchless achromatic upon some of the faint nebulæ in the constellation Virgo, or upon some others not easily resolvable into stars, he will soon satisfy himself, that his ideas of its space-penetrating power are much overrated.

James South.

P. S.—The star ζ Bootis was seen "close double" by Mr. Pond at Lisbon, perhaps 20 years ago, and, as I believe, with a Newtonian reflector of 6 inches aperture; and the circumstances mentioned in a letter written by him to Dr. Wollaston. The instrument with which I first observed it, in 1810, "close double," was a reflector of the worst possible construction, *viz.*, a Gregorian reflector of 6 inches aperture and 30 inches focal length, but a very perfect instrument made for me by Mr. Watson in the year 1809, and which is now in the possession of my friend, Mr. Frederick Perkins.

I am happy to say that I have observed Encke's comet, and also the comet of the constellation Taurus four successive nights, *viz.*, August 21, 22, 23 and 24.

Art. VIII. *Observations on* Mr. Fraunhofer's *Memoir on the inferiority of Reflecting Telescopes when compared with Refractors.*

Extract of a Letter from J. F. W. Herschel, Esq., F.R.S., *to* M. Schumacher.

Dear Sir, *Slough*, 1825, *Aug.* 15.

In the 74th Number of your Astronomische Nachrichten, you have obliged the astronomical world with Mr. Fraunhofer's account of the superb equatorial, constructed by him for the Observatory at Dorpat. The existence of such an instrument in such hands ought to be a matter of congratulation to every lover of science, and I am rejoiced to hear from Mr. Fraunhofer, that, so far from resting satisfied with what he has already achieved, he has extended his views to the construction of still more powerful instruments, and is actually employed on an equatorial of 12 inches aperture and 18 feet focal length, which will indeed deserve to be regarded as a *chef d'œuvre* of art.

I have not the least disposition to doubt the merits of the Dorpat telescope, which I have not seen, but which I am disposed to believe one of the best, probably the very best refracting telescope, ever made. The accounts which Mr. Struve has been so kind as to transmit me of its extraordinary power are quite satisfactory. A telescope capable of easily separating the stars of ω Leonis, and giving exact and coincident measures of their relative position on several nights, can admit of no question as to its excellence. I have hitherto heard but of one other refracting telescope in which this star has been seen double. It is that by Lerebours, now at the Royal Observatory at Paris, the object glass of which (like the Dorpat telescope) is 9 inches in aperture, though only $8\frac{1}{2}$ are effectively employed. Mr. Struve, however, has discovered double stars still closer than ω Leonis with his magnificent instrument.

My immediate object in writing this, however, is to obviate an erroneous impression which may arise in the minds of those who read Mr. Fraunhofer's memoir, as to the *great inferiority* of reflecting telescopes in point of optical power, to achromatics in general, and more especially to those constructed with such delicacy as his own doubtless are. Those who have witnessed the performance of Mr. Amici's beautiful Newtonian reflectors, will not readily admit this inferiority, but will rather feel disposed to wish that some attempt might be made to accommodate such admirable instruments to the more exact purposes of astronomy, an object which appears to have been too easily lost sight of.

Mr. Fraunhofer's expressions, when speaking of the loss of light by metallic reflection, are, I think, somewhat too strong. He observes that, "the most perfect metallic mirror reflects *only a small part* of the incident light, and that *the greater part* is absorbed;" and that, "in consequence, the intensity of the light entering the eye of the observer is always very small" (*ist immer sehr gering.*) A metallic mirror, however, reflects 0.673 of the incident light, or more than two-thirds, and absorbs less than one-third of the whole. Mr. Fraunhofer appears rather to have had in view the Newtonian construction, where *two* metallic mirrors are used, and where the whole effective quantity of light is only 0.452 of the incident rays. No one who has been half blinded by the entrance of Sirius or α Lyræ into one of my father's 20 feet reflectors, will say that the intensity of its light is small, nor, to take a less extreme case, will any one who uses one of M. Amici's Newtonian reflectors of 12 inches aperture (a perfectly convenient and manageable size, and of which he has constructed several,) be disposed to complain of its want of light. The ordinary reflector used by my father in his reviews of the Heavens was a Newtonian, of 7 feet focus, and barely six inches in aperture, and consequently equal (*cæteris paribus*) to an achromatic of $4\frac{1}{4}$ (4.254) English, or 3.99 Paris inches, and therefore by no means proper to be put in competition with Mr. Fraunhofer's *chef-d'œuvres* of 7 and 9 inches. Yet it will be recollected, that with this telescope, and with a magnifying power of 460, ω Leonis was discovered to be double and distinctly separated, and its angle of position measured.

In order to demonstrate the superiority of refracting over reflecting telescopes, Mr. Fraunhofer has selected the star ζ Bootis, which my father has described as a double star of the 6th class (No. 104) in his second catalogue of double stars, but without mentioning the division of the large star into two, as a double star of the first class. It might, however, be very easily overlooked in a review in indifferent weather. It is at least as difficult to resolve as η Coronæ, more so than σ, either of which, with any telescope, be its goodness what it may, requires a favourable atmosphere for its separation. From this omission, however, Mr.

Fraunhofer concludes that the power of the telescope was insufficient to resolve it, and must therefore have been inferior to that of an achromatic in the hands of Mr. Bessel, with which it was recognised by that eminent astronomer as double. It will be seen on reference to the Memoir on double stars lately published in the *Philosophical Transactions* by Mr. South and myself, that this star had been long since ascertained to be double, not only by Mr. Bessel, but by Messrs. Struve, Pond, and South, and, what is more to the present purpose, by Sir William Herschel himself. It was only by oversight that we omitted to refer in that work to his account of it, which is published in his paper "On the places of 145 new double Stars," in the first volume of the *Transactions of the Astronomical Society*, page 178. Which paper was read on June 8, 1821. It will not be amiss if we extract the account verbatim.

"(114). Journal, April 5, 1796.—7 feet reflector power 460 (ζ) *Bootis* double 1st class. Very nearly in contact; I can, however, see a small division. A little unequal, the preceding is the smallest."

"Rev. Aug. 6, 1796. ζ *Bootis.* double. Position 2 Rev.—14.5 parts + 1.1 for Zero=41° 59′.1 np. With 460 a division is but barely visible ¼ of S. Both w. A little or pretty unequal."

"Rev. July 12. 1807. ζ *Bootis.* They are fine, equal, whitish stars: the interval between their apparent discs with 460 is ⅓ of the diameter of either."

To these observations I will only add, that with the same telescope, but with a mirror much tarnished, and now used only for the most ordinary observations, I last night saw this star, as well as σ Coronæ and η Coronæ, distinctly double.

The argument, then, from the omission of this star, is untenable. ζ Orionis would have furnished a case much more in point. The very singular history of this star will be found in the Memoir on double Stars already mentioned.

In large reflectors, in which only one metallic mirror is used, the disadvantage in point of light under which they labour, in comparison with refractors, is however much less formidable.

A reflector of 18 inches aperture would be equivalent to an achromatic of 15½, and one of 48 inches to an achromatic of 41½ in aperture, a size we cannot suppose (from any thing we have yet seen) that it is possible the latter should ever attain. Reflectors of 18 or 20 inches are perfectly manageable, and, I apprehend, quite within the power of any good artist to execute, and (if intended only for use, and not at all for show) at no very ruinous expense. That which I habitually use, of the former dimension, is my own workmanship (*en amateur*), and though inferior in distinctness to the exquisite one used by my father in his sweeps, is by no means an instrument to be despised. Indeed, from the experience I have had of these telescopes, I am satisfied of their applicability even to the more exact purposes of astronomy, and that great improvements in their construction and mechanism remain to be made.

Having referred to the paper on double stars, published by Mr. South and myself, permit me (with his concurrence) to rectify some errors into which we have there fallen, and which have been recognised by him in the course of his observations at Passy in continuation of the same subject, the results of which, I hope, will, ere long, be before the public, and will afford a convincing proof of his zeal and indefatigable industry.—The first of these errors is in the case of the small star accompanying 36 Ophiuchi. It is stated in the work referred to (as the result of a single measure, probably a hasty one, or affected by an accidental derangement of the micrometer head, as having its position 19° 5′ np) and distance 3′ 0″.735. There is no memorandum of which of the close stars the position was measured from, but it must have been from the most southern, as Mr. South, by a mean of 10 measures, finds 17° 41′ np for its angle of position with respect to this star. The difference (1° 24′) is not very important, but the distance is greatly in error. Mr. S. has determined the distance of the small star from the more southern of the close stars (by a mean of 19 measures) at 3′ 15″.252, and from the more northern (by a mean of 21) at 3′ 13″.689.

The next correction I have to notice is in the case of π Aquilæ, whose position and distance are stated at 45° 27′ sf and 1″.957 respectively, while Mr. South's recent measures, which appear entitled to preference, being the results of 19 measures of angle and 10 of distance taken on 4 different nights, and agreeing well together, make the mean result as follows.

Position 33° 14′ sf; Distance 1″.447; Epoch 1825.59. It seems certain therefore that an error of 10° must have been committed in the reading off of the printed angles. Granting this, the remaining error of 2° 13′ would be pardonable in so very close a star as the result of a single set of measures.

In the remarks I have thought it necessary to make on that part of Mr. Fraunhofer's memoir which refers to the action of reflecting telescopes, I should be very sorry to have expressed myself in any way capable of being construed in a controversial sense, or as intended to give the slightest personal offence to its celebrated author, who as an artist must surely be ever regarded as a benefactor to astronomy, while optical science is no less indebted to him as a philosopher, for his beautifully delicate experiments on the constitution of the prismatic spectrum, which have given a degree of precision to optical determinations hitherto unheard of, and shewn the practicability of placing the construction of telescopes on purely scientific grounds, while they have unfolded phenomena of the highest interest in a speculative point of view. Nor can I help feeling that I should ill requite his liberal and friendly reception during a visit to Munich, which I shall ever recollect with pleasure, and in which I had ample opportunity to admire both the resources of his genius and the simplicity of his manners, by a word calculated to give pain or excite unpleasant feelings,

——— quod vitium procul abfore chartis
Atque animo prius, ut si quid promittere de me
Possum aliud, vere promitto.

J. F. W. Herschel.

Art. IX. *A Letter from* M. Gay-Lussac *to* Mr. Daniell, *on the Expansion of Gases by Vapour.*

Paris, 2 *Nov*., 1825.

" Mon cher Monsieur,

" J'ai reçu la lettre que vous m'avez fait l'honneur de m'écrire, pour me donner le renseignement que j'avois pris la liberté de vous demander :—je vous prie d'en recevoir mes sincères remercimens.

" Je profite de cette occasion pour vous prévenir que dans le dernier No. du *Journal de Science*, *&c*., p. 74, que je reçois à l'instant, vous donnez une formule pour l'expansion d'un gaz en contact avec un liquide fournissant indéfiniment de la vapeur, qui ne me semble pas exacte, et que c'est bien réellement $\frac{p}{p-f}$ qui est l'expression du volume que doit prendre le gaz en se saturant de vapeur dont la force élastique est f, sous la pression p. de l'atmosphère.

" En effet, dans un vase inextensible, le gaz en contact avec le liquide qui fournit la vapeur a une force élastique égale à $p+f$; et si on suppose que le vase devienne extensible, il se dilatera jusqu'à ce que la pression intérieure devienne égale à la pression extérieure; or, comme f est constant, le gaz se dilatera jusqu'à ce que sa force élastique soit égale à $p-f$: et les volumes étant en raison inverse des poids comprimans, on a

" v volume de l'air avant son mélange avec la vapeur, est à V, volume après le mélange, comme $p-f$ est à p: c'est à-dire

" $v : V :: p-f : p$; d'où $V = v\left(\frac{p}{p-f}\right)$ ou seulement $V = \frac{p}{p-f}$, si $v=1$.

" Le volume V devient infini lorsque $f=p$, et cela doit être ; car alors la vapeur fait seule équilibre au poids de l'atmosphère, et la force élastique du gaz doit être infiniment petite, ou son volume infiniment grand.

"J'ai pensé, Monsieur, d'après l'obligeance que vous m'avez témoignée que vous interpréteriez favorablement le sentiment qui a dicté ces observations.

"J'ai l'honneur d'être, Monsieur, avec une parfaite considération,

"Votre très-humble et très-obéissant serviteur,

"GAY-LUSSAC."

I return my best acknowledgments to M. Gay-Lussac for this obliging correction of an error. He has not been mistaken in supposing that such criticism would be most acceptable to me. I was misled, by inconsiderately supposing an analogy between the mixture of gases with one another, and their mixture with vapour, which does not exist. I shall take the first opportunity of re-calculating the table for finding the specific gravity of any mixture of air and aqueous vapour by the correct *formula.*

I must not let this occasion pass of likewise acknowledging a mistake into which I have fallen with regard to M. Gay-Lussac, in the thirty-sixth number of this Journal, by ascribing to him a critique in the *Annales de Chimie*, which wanted the usual distinctive initials of the two editors of that work. This error has been already corrected by M. Arago, in no very philosophic mood. The article in which this correction is conveyed, I should have been in no danger of mistaking, even without the A. subscribed. Nobody, who has had the advantage of any acquaintance with M. Gay-Lussac, or his works, could, for a moment, suppose that *he* would substitute personal abuse for argument, and wilful perversions for criticism.

J. F. D.

Art. X. *Proceedings of the Royal Society.*

The sittings of the Royal Society were resumed for the season on Thursday, the 17th of November, at which meeting a paper was communicated by Dr. Davy, entitled, *Observations on the Changes which have taken place in some ancient alloys of Copper.*

The author first describes the nature of an incrustation upon an ancient helmet found in a shallow part of the sea between the citadel of Corfu and the village of Castrades; the surface was of a variegated colour, mottled with spots of green, dirty white, and red: the red and green patches exhibited minute crystals of red oxide of copper and metallic copper, and were further composed of its green submuriate and carbonate; the dirty white parts consisted chiefly of oxide of tin.

These new combinations are only superficially produced: the metal was found bright beneath, and consisted of copper alloyed with 18.5 per cent. of tin.

An ancient nail from a tomb in Ithaca, and a mirror from a tomb at Samos, in Cephalonia, afforded nearly similar, but less distinctly, crystalline results. The copper in the mirror was alloyed with 6 per cent. of tin, and a minute portion of arsenic.

The examination of the incrustation upon ancient coins, consisted of oxide of tin, and of carbonate, and submuriate of copper; it, in some cases, acquires a dingy hue, from the prevalence of black oxide of copper, mixed with a little of its protoxide.

The author could discover no connexion between the perfect state of preservation of ancient coins, and their composition; but he observes, that the manner in which the crystalline structure of the incrustation is acquired, is a peculiarly interesting question. There being no reason to suspect deposition from solution, "are we not," says the author, "under the necessity of inferring that the mineralizing process witnessed in its effects depends on a slow motion and separation of the particles of the original compound; and must we not conclude that this motion is connected with the operation of attractions of different kinds, as chemical-affinity, electro-chemical attraction, and attraction of aggregation?" If this conclusion be just, the author remarks, that it opens a new field of inquiry, which may help to explain several phenomena in mineralogy and geology.

At the same meeting a paper was also read, entitled *Observations of the Apparent Distance and Positions of* 460 *double and triple Stars, made in the years* 1823, 1824, *and* 1825, *together with a re-examination of* 36 *Stars of the same description, the distances and positions of which were communicated in a former Memoir, by* James South, F.R.S.

The author prefaces these observations with a brief account of the instruments with which, and the circumstances under which, they were made. The former being the same with which the observations previously communicated to this society were made, and being fully described in the former paper alluded to in the title of this, require no further particular description; he contents himself therefore with noticing that by a different adaptation of their parts, higher magnifying powers than those formerly employed were obtained, and a series of powers from 92 to 787 used in a part of the observations.

A large portion of these observations were made at Passy, near Paris; and the author takes occasion to make honourable mention of the facilities afforded him on the part of the French government, for the ingress and regress of his instruments into and out of France, and of the attention and assistance uniformly afforded him while resident there by many distinguished individuals.

Of the stars whose measures are here presented, he states that about 160 are hitherto undescribed, and probably new. The places of these are given merely with sufficient exactness to enable any one to find them in future. The remainder are in great measure stars comprised in M. Struve's catalogue of 796 double and triple stars, and among these about 160 belong to those examined for the first time by Sir Wm. Herschel.

The observations themselves are stated in a manner somewhat different from that adhered to in the former communication already alluded to. Instead of giving all the individual micrometrical measurements on which they depend, (about 14,000 in number,) which would have swelled the paper to an enormous bulk, only the mean results of each set of measures are given; but to afford every opportunity of forming an impartial judgment of their validity, not only the number of measures on which it depends is annexed to each mean result as stated, but also the difference between the greatest and least measure taken, or the limits within which all the measures necessarily lie.

The stars themselves are arranged in order of right ascension for convenience of reference. After the statement of the mean results of the several sets of observations both of angle and distance, a final mean, with a mean date for an epoch, is deduced. In the case of Sir Wm. Herschel's stars, a comparison of the measures now obtained with those given in his catalogues, or now for the first time brought to light by a careful examination of his manuscripts, is subjoined. By this comparison several fresh instances have been found of double stars, in which the relative motion of the individuals composing them is satisfactorily proved. In one remarkable case, (that of the star δ Equulei,) this change has gone to an enormous extent, and is satisfactorily referred to proper motion in the large star. In another, not less singular, all the three stars of a triple star (ζ Cancri) are ascertained to be relatively in motion, describing orbits about each other, and forming probably a ternary system connected by the mutual gravitation of its members; thus completely justifying the views taken by Sir Wm. Herschel of this subject in his papers published in the transactions of this society in 1802 and 1804.

Annexed as an appendix to these observations, is a re-examination of about 36 stars, measured in the former paper already alluded to, and which were considered as presenting peculiar interest from the evidence then obtained of their relative motion, and of their connexion in binary systems. The results of this re-examination are in the highest degree satisfactory, as, with only two or three exceptions, these stars have been found to continue their motions in the directions, and in the greater number of cases, with nearly the velocities redicted. In the most remarkable case, that of the double star ξ Ursæ majoris, an angle of nearly 14° has thus been described by the two stars about their common centre of gravity, in an interval of less than 2 years: thus affording every probability that in a very few years we shall arrive at a perfect knowledge of the figure, elements, and position of their orbits, and be enabled by strict calculation, to answer the important question, whether the Newtonian law of attraction is confined to our own system, or obtains also in the sidereal heavens. (H.)

Thursday, Nov. 24.—A paper was read, entitled *An Account of the Construction and Adjustment of the new Standard of Weights and Measures of the United Kingdom of Great Britain and Ireland, by* Captain Henyr Kater, F.R.S.

The author, after stating that the weights and measures of the United Kingdom are founded on a standard, whose length is determined by its proportion to that of a pendulum vibrating mean time in London, which has been ascertained by him to be 39.13929 inches of Sir George Shuckburgh's scale, deems it necessary, on account of the importance of the result, to consider what degree of confidence it is entitled to. For this purpose it is necessary to compare this final result with those obtained in other experiments and by different methods. Now it appears that previous to the experiments detailed in the author's paper on the subject in the *Phil. Trans.* for 1818, on which this result rests, another series is there mentioned, made with the same instruments, but under circumstances which occasioned their rejection, and which owing to some repairs in the instruments between the two series, which occasioned a material alteration in the distance between the knife edges, have all the weight of experiments made with a different pendulum. The result of these rejected experiments, however, differed only two ten-thousandths of an inch from that ultimately adopted.

The author next compares the lengths of the seconds' pendulum at Unst and at Leith Fort, as ascertained by him by an invariable pendulum, whose vibrations had previously been determined in London, and whose length was thus known in terms of the London seconds' pendulum, and as ascertained by M. Biot at the same stations by means of a variety of pendulums, and by a totally different method of observation—that of Borda. The results of this comparison are, a difference between the determinations of M. B. and of the author, of 0.00029 inches in excess at the former station, and 0.00015 in defect at the latter.

From this near agreement of all the results, he considers that the length of the seconds' pendulum in London may be regarded as certainly known to within one ten-thousandth of an inch; while from the near agreement of the results of the French and English experiments on the length of the pendulum, he concludes that the length of the metre in parts of Sir G. Schuckburgh's scale may also be regarded as known within one ten-thousandth of an inch.

From an account recently published by Captain Sabine of his valuable experiments for the determination of the variations in length of the seconds' pendulum, he observes, doubts may be inferred of the accuracy of the method employed by him for the observations for determining the length of the seconds' pendulum in London, as well as in those which have been made with the invariable pendulum. It is asserted there, that taking a mean between the disappearances and re-appearances of the disc is a more correct method of observation than that pursued by Captain Kater, and that the intervals between the coincidences obtained, by observing the disappearances only, of the disc, would be productive of error.

In answer to this objection, the author remarks, 1st. That with respect to the convertible pendulum, or that used for determining the absolute length of the seconds' pendulum, the disc was made to subtend precisely the same angle as the tail-piece of the pendulum, so that at the moment of disappearance, its centre necessarily coincided precisely with the middle of the tail-piece, and the difference between the moments of disappearance and re-appearance is rigorously nothing; an adjustment indispensable in his method of observing, when the object is to determine the *true* number of vibrations in 24 hours.

2dly. With the invariable pendulum the disc subtended a somewhat less angle than the tail-piece, so that the inferred number of vibrations in 24 hours was diminished about two-tenths of a second. But experiments with the invariable pendulum being intended to be in the strictest sense of the word comparative, this constant difference will no way affect the ultimate result. But, as the most direct way to remove any doubts which may be entertained on the subject, the author has computed from the whole of Captain Sabine's observations, the successive differences in the vibrations at the various stations visited by him, by the two methods, viz., that of employing the disappearances and re-appearances, and the disappearances alone. The results only in one instance differ so much as a tenth of a vibration, they are indifferently in excess and defect, and the mean of their discrepancies is exactly nothing. From this he concludes that if the observations be made as nearly as possible under similar circumstances, the method of observing by disappearances alone, is productive of no perceptible error in practice, in experiments with the invariable pendulum; while in those with the convertible pendulum, the equal apparent sizes of the disc and tail-piece, preclude the possibility of any, either in practice or theory, from this cause.

The standard of Sir G. Shuckburgh having been found identical with that by Bird, in the custody of the Clerk of the House of Commons, adopted as the imperial standard unit of extension, the length of the pendulum already determined is fixed with the same degree of precision in parts of the imperial standard yard.

A repetition of Sir G. Shuckburgh's experiments on the weight of given volumes of distilled water, and a re-measurement of the cube, sphere, and cylinder used by him, were found to give no material variation from his results, these being stated in terms of the mean of several standard weights kept at the House of Commons. The troy pound nearest the mean has been adopted, and declared by the legislature to be the original unit of weight under the denomination of the imperial standard Troy pound.

The relation between this pound and the cubic inch of distilled water at 62° Fahr., bar. 30 in., has been ascertained by the commissioners of weights and measures, who find that the latter contains 252.458 gr., each grain being the 5760th part of the standard troy pound.

The avoirdupois pound is fixed by assigning its proportion to the standard troy pound, so as to contain exactly 7000 such grains.

The imperial standard gallon is defined by stating its contents under the same circumstances of temperature and pressure, at 10lbs. avoirdupois of distilled water, and the bushel by its containing 80 such pounds.

The author, having in compliance with a request of the Lords Commissioners of His Majesty's Treasury, undertaken to superintend the construction of, and to adjust, the principal standards to be deposited at the Exchequer, Guildhall, Dublin, and Edinburgh; Mr. Dollond was directed to prepare those of linear measure, and Mr. Bate those of weight and measure, the proper quality of metal for the latter purpose being determined by experiments instituted for the purpose. The experiments for adjusting them are then given in full detail. The troy pounds were first adjusted, and the exactness with which this operation has been performed may be appreciated from this, that the final errors of none of them exceeded 22 ten-thousandths of a grain. When brought so near, it was of course not thought necessary to attempt further correction.

The avoirdupois pounds and the weights of the gallon of water were then derived from the troy pounds, and finally adjusted, like them, by enclosing within the weight in hollows left for the purpose, wires equal to the errors ascertained to exist in them. The weights of these wires in each case is stated, so that should they by any accident be taken out and lost, they may be restored.

He next describes the method used in adjusting the gallon itself, the method of filling it exactly, and of weighing it when filled, together with the corrections depending on the circumstances of temperature and pressure under which the experiments were made. As a final result, it appears that one only of the gallons was ultimately found in error to a greater extent than 6 tenths of a grain, the others having their errors less than a fourth of that quantity.

The quarts and pints being next disposed of, the author describes the balance contrived by him for weighing the bushels, which proved so delicate as to turn with a single grain when loaded with 250lbs. in each scale. The resulting bushels when finally adjusted, were found to have all their apparent errors less than 6.56 grains of water; while the corrections for temperature and pressure only, amounted in some cases to no less than 138 grains; but this depending on the figure of the glass used to cover them, it is not to be understood that the contents of the vessels have actually been ascertained to this degree of precision.

The adjustment of the standard yards is next described, and the author concludes his paper by a summary of the results arrived at in the present inquiry respecting British weights and measures. The length (he remarks) of the pendulum vibrating seconds in London has been found in parts of the imperial standard yard, so that the value of the yard may at any time be known, having been referred to a natural standard presumed unalterable. The length of the French metre, a standard expressing a certain portion of the terrestrial meridian, has also been given in parts of the English scale. The weight of a cubic inch of distilled water has been determined in parts of the imperial troy pound, and thus the pound, if lost, may at any future age be recovered. The avoirdupois pound is now for the first time defined, and the measures of capacity are made to depend on the weight of water they contain; the imperial gallon, containing ten pounds avoirdupois of water, having been declared to be the unit, or only standard measure of capacity from which all others are to be derived. This, it is to be presumed, will tend to produce uniformity throughout the United Kingdom, by putting it in the power of every individual possessed of standard weights to verify his measures of capacity with the utmost facility. (H.)

Wednesday, November 30, being St. Andrew's day, the Royal Society held their anniversary meeting. After the auditors had made their report relative to the accounts of the society, the president informed the members that two Copley medals had been awarded by the council, the one to M. Arago, of the Royal Academy of Sciences at Paris, and F. R. S.; and the other to Mr. Peter Barlow, F. R. S., Professor in the Royal Military Academy at Woolwich. "The discoveries and labours," said the president, "which your council have made it their pleasure and thought it their duty to honour, by conferring on their authors the highest rewards * of this society, both belong to the same department of science—Magnetism—a department which has always claimed a considerable portion of your attention, both in its relations to philosophy and utility; to the laws and properties of natural forms; and to navigation, the great source of the power and prosperity of this mighty empire." Sir Humphry Davy then proceeded to enter somewhat in detail into the history of magnetism, and more especially dwelt upon the importance of the recent discoveries relating to its mysterious connexion with electrical phenomena. He enumerated the leading results of the inquiries and experiments of M. Arago and of Mr. Barlow; and having stated that Mr. South would receive the medal on the part of, and transmit it to M. Arago, addressed him nearly as follows:—"In transmitting this medal to M. Arago, assure him of the deep interest we take in his important researches, and inform him that we await with impatience the continuation of his labours. As a fellow of this society, his discoveries have for us the same interest that they have for his brethren of the Royal Academy of Sciences, which, for more than a century and a half, has gone on encouraging and emulating our labours. You, and our worthy secretary, Mr. Herschel, are examples of recent liberality on their part, and of the respect paid to British talent. We, I trust, shall never be behind them in dignity and nobleness of sentiment—far be from

* We are happy in being able to state that the Copley medal is no longer the highest honorary reward in the gift of the Royal Society, His Majesty having been graciously pleased to confer upon the Society two annual medals of the value of fifty pounds each, to be awarded for scientific discoveries, as the council shall deem most expedient.

us that narrow policy which would contract the minds of individuals, and injure the interests of nations, by cold and exclusive selfishness, which would raise the greatness of one people by lowering the standard of that of another. As in commerce, so in science, no one country can become worthily pre-eminent, except in profiting by the wants, resources, and wealth, of its neighbours. Every new discovery may be considered as a new species of manufacture, awaking novel interest and sagacity, and employing new capital of mind. When Newton," said the president, "developed the system of the universe, and established his own glory and that of his country on imperishable foundations, he might be regarded as giving a boon to the civilized world, for which no adequate compensation could ever be made; yet even in this, the most difficult and sublime field of discovery, Britain has been repaid, if not fully, yet fairly, by the labours of Euler, La Grange, and, above all, La Place, perfecting the theory of the lunar motions and planetary perturbations, and affording data of infinite importance in the theory and practice of navigation. Fortunately, science, like the nature to which it belongs, is neither limited by time or space; it belongs to the world, and is of no country and of no age—the more we know, the more we feel our ignorance—the more we feel how much remains unknown, *&c.*;—and in philosophy, the sentiment of the Macedonian hero can never apply—*there* are always new worlds to conquer."

On delivering the medal to Mr. Barlow, the president said, that although he had already been honoured by marks of approbation more valuable in a pecuniary point of view, he trusted that none would give him more durable satisfaction; "for this award," said Sir Humphry, "has, I believe, never been made, except after dispassionate and candid discussion; never to gratify private feelings, or to call for popular applause. We trust," continued the president, "both on account of the public good and your own glory, that you will engage in and accomplish many new labours You have not merely had scientific success, but one still more gratifying to your feelings—the idea that you have been useful to your country, and secured the gratitude of a body of men who are not tardy in acknowledging benefits."

The Society then proceeded to the election of a Council and Officers for the year ensuing, when the following members of the Old Council were re-elected.

Sir H. Davy, Bart.
Francis Baily, Esq.
William Thomas Brande, Esq.
Samuel Goodenough, Lord Bishop of Carlisle
Davies Gilbert, Esq.
J. F. W. Herschel, Esq.
Sir E. Home, Bart.
Capt. H. Kater
John Pond, Esq.
W. H. Wollaston, M. D.
Thomas Young, M. D.

The following members of the Society were chosen into the Council.

John Barrow, Esq.
John Bostock, M. D.
Sir Astley Cooper, Bart.
B. Gompertz, Esq.
S. Groombridge, Esq.
Sir A. Hume, Bart.
Daniel Moore, Esq.
Richard, Earl of Mount Edgecumbe
P. M. Roget, M. D.
James South, Esq.

Officers for the ensuing year.

President	Sir H. Davy, Bart.
Treasurer	Davies Gilbert, Esq.
Secretaries	W. T. Brande, Esq.
	J. F. W. Herschel, Esq.

Dec. 8.—A Paper entitled, Additional Proofs of the Source of Animal Heat being in the Nerves, by Sir E. Home, Bart., F.R.S., *was read.*

This paper contains the account of a repetition of the author's former experiments upon the effects of dividing the nerves supplying the velvet of the deer's horn, in which the same results have been obtained, while some exceptionable parts of the former proceedings have been carefully avoided. It was now found, as before, that immediately upon the division of the nerves of one horn, the temperature of that horn was diminished sometimes to the amount of 7°, and that in the course of ten or twelve days the disparity of temperature between the two horns began to cease, and they ultimately again attained precisely the same temperature. When this was the case, the deer was killed, and the parts carefully dissected and examined, when it was found that the interval occasioned by the recession of the divided nerves, was filled up by a newly-formed substance, which firmly connected them, and this explained the restoration in their functions which had taken place.

In further proof of the influence of the nerves over the evolution of heat, independent of mere sanguineou scirculation, Sir Everard adverts to a case of aneurism, in which he tied the femoral artery immediately below Poupart's ligament. The obstruction of this large arterial trunk, however, did not occasion any diminution of temperature in the foot, below the natural standard.

Art. XI. ASTRONOMICAL AND NAUTICAL COLLECTIONS. No. XXIV.

i. *Continuation of the Catalogue of Comets*, (Coll. No. XVI.) *By* Dr. W. Olbers. Astr. Abh. iii.

N	Date	Passage of the Perihelium in Parisian mean time.	Longitude of the Perihelium.	Longitude of the ascending node.	Angle between the Perihelium and the node.	Inclination.	Distance in the Perihelium.	Log. Dist. Perihelium	Log. mean motion.	Eccentricity.	Direction.	Name of the Computer.
		h m s	s ° ' "	s ° ' "	s ° ' "	° ' "						
126	1823	Dec. 9 10 42 29	9 4 32 59	10 3 3 22	0 28 30 22	76 11 28	0·22674	9·35554	0·927118		R	Hansen
		9 10 49 7	9 4 33 26·9	10 3 3 22·2	0 28 29 55·3	76 11 22·5	0·226670	9·3553934	0·9270382		R	Idem
		9 8 30 0	9 3 56 12	10 2 59 14	0 29 3 2	76 2 45	0·23078	9·363198	0·915331		R	Nicollet
		9 10 6 12	9 4 18 32	10 3 1 18	0 28 42 46	76 9 40	0·22801	9·35796	9·923188		R	Nicolai
		9 9 56 8	9 4 33 18·8	10 3 3 39·4	0 28 30 20·6	76 12 6·1	0·226745	9·3555383	0·9269209		R	Idem
		9 10 48 50	9 4 34 29·6	10 3 3 0·5	0 28 28 30·9	76 11 56·9	0·226502	9·3550726	0·9275194		R	Encke
		9 11 23 25	9 4 34 14·4	10 3 3 51·3	0 28 29 46·9	76 12 14·0	0·226623	9·3553934	0·9271722		R	Schmidt
127	1824	July 11 12 26 27	8 20 16 32	7 24 19 9	11 4 2 37	54 34 19	0·591263	9·7717609	0·3024869		R	Rümker
128	1824	Sept. 29 6 0 43	0 4 44 24	9 9 5 49	2 25 38 35	54 22 3	1·047238	0·0200154	9·9300602		D	Bouvard
		29 2 36 2	0 4 34 12·2	9 9 15 8·6	2 25 19 7·6	54 34 14·2	1·049538	0·020998	9·9286313		D	Hansen
		29 2 7 46	0 4 32 7·8	9 9 15 21·0	2 25 16 46·8	54 35 58·9	1·049852	0·0211281	9·9284362		D	Argelander
		29 2 11 52	0 4 33 18·4	9 9 15 48·0	2 25 17 30·4	54 34 35·5	1·049642	0·0210414	9·9285662		D	Encke
		28 23 28 57	0 4 25 57·2	9 9 15 31·6	2 25 10 25·6	54 43 7·8	1·051328	0·0217383	9·9275212	1·006046	D	Idem
		29 1 33 19	0 4 31 7·3	9 9 15 39·3	2 25 15 28·0	54 36 58·4	1·0501393	0·0212469	9·9282580	1·0017345	D	Idem
		29 1 45 2	0 4 32 6·1	9 9 16 44·0	2 25 15 22·1	54 35 31·6	1·0498353	0·0211211	9·9284467		D	Idem
129	1825	May 30 13 15 17	9 3 55 21	0 20 5 53	3 16 10 32	56 41 17	0·88912	9·94896	0·036688		R	Nicolai
		30 13 34 2	9 3 55 27·1	0 20 7 31·7	3 16 12 4·6	56 41 30	0·889127	9·948964	0·0366823		R	Hansen
		30 13 15 59	9 3 55 1·5	0 20 6 8·1	3 16 11 6·6	56 41 5·8	0·889123	9.9489616	0·0366859		R	Clausen

REMARKS.

No. Year.

126. 1823. Seen by several persons with the naked eye, in the last days of December, and pointed out by them to the notice of astronomers. Astr. Nachr. II. p. 455, III. First observed at Prague the 30th Dec. 1823, and last by Wisniewski at Petersburg, the 28th March, 1824. The comet was particularly remarkable, from the 22d to the 31st January, in having, besides the common tail opposite to the sun, another pointed directly towards that luminary. Astr. Nachr. III. p. 6, 27. Astr. Jahrb. Berl. 1827, p. 133. Gambart's Observations are in the Connaissance des T. 1827, p. 123. [For the elements as computed by Taylor, Carlini, Brinkley, and Richardson, see No. XVII of these Collections.]

127. 1824. This comet, which was not seen in Europe, was discovered by Rümker in New South Wales, and observed from the 15th July to the 6th August. The observations might be reduced with greater accuracy; and we may, perhaps, expect additional ones from Sir Thomas Brisbane or Mr. Dunlop. Astr. Nachr. IV. N. 78. p. 107. . .

128. 1824. Discovered by Scheithammer at Chemnitz, the 23d July, by Pons the 24th July, by Gambart the 27th July, and by Harding the 2nd August: observed last by Capocci at Naples the 25th December. Astr. Nachr. III. p. 244, 257. IV. N. 79. p. 123. Zach. Corr. Astr. X. vi. XII. i. This comet was difficult to be observed, from its faintness, and from the indistinctness of its nucleus: it is also somewhat uncertain whether the orbit was truly hyperbolic, as it appeared to be, from the observations of the first months in which it was visible. Encke computes the longitude of the node from the mean equinox of 29th Sept. 1824.

129. 1825. Discovered by Gambart the 19th May: observed until the end of June. Hitherto I am not acquainted with any other observations of this comet than those of Gambart, Schumacher, Nicolai, Harding, and Olbers, most of which are printed in the Astr. Nachr. IV. N. 81. The elements have some resemblance to those of the second comet of 1790, N. 93; but they differ from them too much to allow us to suppose the comets identical. Hansen and Clausen compute the longitudes from the mean equinox of 0 Jan. 1825.

Additions and Corrections for the Catalogue of Comets.

N.	Date	Passage of the Perihelium in Parisian mean time.	Longitude of the Perihelium.	Longitude of the ascending node.	Angle between the Perihelium and the node.	Inclination.	Distance in the Perihelium	Log. Dist. Perihelium	Log. mean motion.	Eccentricity.	Direction.	Name of the Computer.
		h m s	s ° ′ ″	s ° ′ ″	s ° ′ ″	° ′ ″						
(15)	1682	Sept. 14 19 26 0	10 1 49 0	1 20 44 0	3 18 55 0	17 46 30	0·58207	9·7649752	0·3136665		R	Bailey
55	1743	Jan. 10 20 29 37	3 2 57 51	2 7 31 57	0 25 25 54	2 16 16	0·838181	9·923338	0·075121		D	Olbers
124	1822	May 5 13 44 13	6 12 47 45	5 27 25 4	11 14 37 19	53 35 34	0·504194	9·7025976	0·4062319		R	Gambart
		5 14 42 0	6 12 43 51	3 27 26 56	11 14 43 5	53 37 24	0·504453	9·7027863	0·4059499		R	Nicollet
125	1822	Oct. 23 15 14 52	9 1 47 53	3 2 42 25	6 0 54 32	52 39 18	1·146389	0·0593320	9·8711303		R	Gambart
		23 13 52 27	9 1 53 46	3 2 42 10	6 0 48 24	52 39 52	1·14727	0·0596656	9·8706297		R	Bouvard
		23 18 2 39	9 1 40 32	3 2 42 23	6 1 1 51	52 40 41	1·146112	0·0592269	9·8712879		R	Rümker
		23 19 23 48	9 1 36 18	3 2 42 23	6 1 6 5	52 40 41	1·146091	0·0592191	9·8712997	0·9923022	R	Rümker
		23 18 37 50	9 1 40 16·8	3 2 44 1·8	6 1 3 45·0	52 39 9·6	1·145066	0·0588305	9·8718826	0·99630211	R	Encke

REMARKS.

No. Year.

(15.) 1682. This orbit of Halley's comet deserved to be inserted, because it rests wholly on Cassini's observations, while all the rest are derived from Flamstead's. Hist. Ac. Par. 1759. p. 162.

55. 1743. In these elements, the first observation of Grischow has been employed with the rest. The observations of this comet cannot be represented by a parabola, and inaccurate as they are, they still appear to indicate an ellipticity of the orbit. In Lacaille's elements the longitude of the node should be 2s. 18° 21′ 15″.

56. 1743. For D we must certainly read R: and the angle between the perihelium and the node must consequently be 3s. 28° 42′ 33″. Astr. Nachr. N. 48. p. 494.

124. 1822. These improved orbits of Gambart and Nicollet are found in the Connaissance des T. 1826. p. 219, 278, with the observations made at Marseilles, and at Paris. Gambart computes from the mean equinox of 12th May, 1822.

125. 1822. Rümker has deduced both his orbits from the observations made by himself at Paramatta up to the 11th November. I have assumed the longitude of Paramatta 9h. 54^m 44^s. Encke in his last orbit has comprehended both the European observations and those of Rümker; so that these elements seem to be preferable to any other. Gambart computed from the mean equinox of 12th July: Rümker 1 Jan. and Encke 25 Oct. 1822. Connaiss. des T. 1826. Astr. Nachr. II. N. 37. p. 207.. . III. N. 55, p. 107. . .

Many astronomers have omitted, after the most accurate determination of the orbit of a comet, to mention the date of the place of the equinoctial point, from which they compute the longitudes. For the sake of uniformity, and in order to avoid all uncertainty and confusion, it is much to be desired, that all computers would employ the place of the equinox for 0 January, in the year to which the perihelium of the comet belongs.

*** Professor Schumacher, the ingenious editor of these Essays, requests his Correspondents to observe, that his usual residence is at Altona, and that Professor Schumacher, of *Copenhagen*, is an anatomist, and not an astronomer.

ii. *Errors in the best* TABLES *of* LOGARITHMS; *collected from various Authorities.*

I. *Callet's Tables Portatives.*

[Those which are marked with an obelisc are corrected in some later copies.]

Log.		*for*	*read*	
910		95904136	95904139	†
25490		3998	3698	†
27602		4906	9406	
28723		2268	2298	†
28734		3461	3961	†
28800 and Title		495	459	
	Diff. 149	86	89	†
32551		5943	5643	†
32561		6677	6977	†
33450		3991	3961	
33480		8754	7854	†
34433		7499	9749	†
38052		5775	3775	†
42382		1864	1814	†
43130		7759	7795	
44400	first line	+3,40	+3,04	
47891			2539	†
56246		0196	0916	
57319			2986	
60844			2178	
64113		———	9461	†
64445		1992	1892	
66600	Diff. 66	30	39	
67200	Title	627	827	
70357			3073	†
72337		5605	3605	
77064			8515	†
78000	Diff.	59	56	
78050			3729	†
79800	Col. 1	1d.	2d.	
99018			7142	
100499		6172	6174	

T. I. Log. v. et h.	L. h.	543	*for* 33635	*read* 33935	
	L. v.	965	58538	56538	
	L. v.	1022	90700	92700	
	L. v.	1071	94708	94608	
	L. v.	1085	84548	85148	
	L. h.	1099	00211	00215	
	L. v.	1105	21129	21729	
	L. v.	1115	84179	84779	
	L. v.	1125	47381	47981	
	L. v.	1135	29141	29741	
T. II.	L. h.	101000	0320	03308	
	L. h.	101002	37909	37309	
		101014	39351	89351	
T. III.	L. v.	000132 Diff. 1.	54589	34589	
Log. Br. à 61	L.	14	12992	12922	
T. pour conv.	L. h.	50	46597	46497	
Tabl. cent.	1° 46′ sin.		4447	4347	
	46 20 Diff.		665	605	
Sinus et Log.	0·040 Cos.		57284	67284	
	0·174 L. sin.		1304	1804	
	0·197 L. cos.		0949	9949	†
	0·377 L. sin.		7183	8713	
	0·397 L. cos.		4062	8062	
	0·436 Cos.		74450	77450	
	0·449 L. sin.		4368	4308	
Log. tang.	0° 24′ 54″		7·8599831	7·8599331	
	1 45 10		8·4853297	8·4857397	†
Sin.	1 55 34		8·5264369	8·5264769	†
	2 7 3		8·7676019	8·5676019	†
	2 10 35		8·5795294	8·5795094	
	2 14 56		8·5937238	8·5937338	†
	2 39 23		8·6660184	8.6660134	
Tang.	2 50 11		8·6959981	8·6949981	
	3 12 43		8·7491027	8·7491 0	
Sin.	3 18 8		8·7604447	8.7604432	†
Tang.	3 34 20		8·7953491	8·7953791	
	3 37 16		8·8012780	8·8012980	

Log. Sin.	3°38′ 8″ *for*	8·8020567 *read*	8·8021567
Sin.	4 43 39	8·9150160	8·9160160
Tang.	4 51 14	8·9280079	8·9290079
	4 53 55	8·9330013	8·9330103
	4 36 Diff. tang.	345	245
Cot.	3 46 40	1·18525	1·18025 †
	6 40 0 under Cot.	sinus	tang.
Tang.	12 43 50	9·33395	9·35395
Sin.	12 44 20	9·36342	9·34342
Cot.	13 31 30	0·8188122	0·6188122
Sin.	24 46 20	9·92222	9·62222
Log. cos.	40 29 20	9·88111·71	9·88111·74
Cot.	42 11 30	0·04264·16	0·04264·19 †
Cos.	42 14 10	9·88945	9·86945 †
	43 47 20	9·85847·27	9·85847·37
	44 8 30	9·83589	9·85589
Tang.	44 14 50	9·9885668	9·9885868
Récueil Par. horiz.	54′ 20″ A. 56°	30′ 33″	30′ 23″

II. *Taylor's Tables*, 4. *London*, 1792.

Log. sin.	4°23′ 38″		*read* 43007
	4 23 39		43281
	6 45 52	*for* 10001	11001
Tang.	23 48 19	9·6446087	9·6445987
Cot.		10·3553913	10·3554013
Cos.	37 29 2	5503	5603

III. *Hutton's Tables, sixth edition*, 8. *London*, 1822.

P. 186	Log.	1007	*for* 6030295	*read* 0030295
187	Diff.		99	399
188	Log.	11003	5511	5111
317		75843	9255	9155
332		3330		8330
348			1930	91303
517	Title		Supply	3
532	cot.	40° 56″	*for* 1·1539754	*read* 1·1530754

iii. *Observations on Mr. Mendoza y Rios's Method of computing the True from the Apparent* LUNAR DISTANCES. *By* THOMAS HENDERSON, Esq.

It is a general opinion among seafaring persons that the method of Mr. Mendoza y Rios, for computing the true lunar distances, by means of his Tables for Navigation and Nautical Astronomy, the second edition of which was published at London, in 1809, is, for nautical purposes, the most easy, concise, and accurate. But the ingenious author not having fully shown how allowance is to be made for the variation of refraction, arising from a change in the pressure and temperature of the atmosphere, indicated by the barometer and thermometer, and for the effect on the moon's parallax, occasioned by the spheroidal figure of the Earth, it may be proper to supply this deficiency.

As explained by the Author, Tables VI., VII., VIII., and IX., showing the corrections of the apparent altitudes of the sun, stars, and moon, and the auxiliary angles for the computation of the true distances, are constructed for that state of the atmosphere, when the barometer is at 29.6 inches, and Fahrenheit's thermometer at 50 degrees. For any other weight and temperature, the variation of refraction is to be added to or subtracted from the corrections in Table VI., accordingly as the true refraction is greater or less than the mean; but to the corrections in Tables VII. and VIII., and the auxiliary angles in Table IX., the variation of refraction is to be applied in a contrary manner; that is to say, to be added or subtracted, according as the true refraction is less or greater than the mean. The variation of refraction for the auxiliary angles in Table IX. is in every case the same as the variation for the mean refraction 66 seconds, which corresponds to the apparent altitude 40° 40'. After these corrections are made, the true distance is computed as usual.

The Author, in his explanation of Table VI., has shown how the true refraction is to be calculated. A Table, such as the eighth in Mackay's Treatise on the Longitude, would be useful for exhibiting the changes of refraction at sight. Or the true refraction may be obtained from the following rule, given in the first edition of the requisite Tables. As 400 is to the difference of the thermometer from 55°, so is the mean refraction to its correction, to be subtracted, if the thermometer is higher than 55°; but to be added, if the thermometer is lower. As 300 is to the difference between the altitude of the barometer and 30 inches, expressed in tenths of an inch, so is the refraction corrected for the thermometer to the correction on account of the barometer, which added to or subtracted from the refraction corrected for the thermometer, accordingly as the barometer is higher or lower than 30 inches, gives the true refraction corrected on account of both.

Example I.

Taken from Mr. Lax's Tables, p. 278.

Barometer . . . 28·6 inches,

Thermometer . 61°.

		°	′	″		°	′	″
⊙ apparent alt.		7	38		Apparent dist.	40	36	5
☽		46	9		Hor. par.		58	25
		53	47		No. I.		16020	
Auxiliary Angle			53	21			125	
23′ 8″			39	16				
11				17				
2				25				
4				3				
23 25		55	20	22	No. II.		82832	
							150	
	App. dist.	40	36	5	No. III.		49544	
		41	18	30			160	
	True dist.	41	18	35	No. IV.		48831	
							736	
	Same as by Mr. Lax's method.						95	

In this example 25″ and 3″ are the respective variations of the sun and moon's refractions, and 4″ the variation of the auxiliary angle, all to be added, as the true refraction is less than the mean.

Example II.

Taken from Mr. Lax's Tables, p. 279.

Barometer . . . 30·4 inches,

Thermometer . . 30°.

✱ apparent alt.	36° 37″	Apparent dist.	30° 52′ 15″
☽	6 10	Hor. Par.	58 36
	42 47	No. I.	66812
Auxiliary Angle	58 43		260
2′ 44″	49 25		
3	36		
− 5	− 6		
	− 38		
2 42	44 35 0	No. II.	24172
			200
App. dist.	30 52 15	No. III.	42501
			305
	30 1 53		
True dist.	30 2 8	No. IV.	34250
			120
Same as by Mr. Lax's method .			130

Here 6″ and 38″ are the variations of the star's and moon's refractions, and 5″ the variation of the auxiliary angle; and they are to be subtracted, as the true refraction is greater than the mean.

Demonstration.—The correction of the auxiliary angle has only to be demonstrated. In the investigation the parallax is thrown out of consideration, as it is refraction alone which produces the effect.

Let a = the auxiliary angle,

M = moon's apparent altitude,

S = sun or star's apparent altitude,

r = moon's mean refraction,

r' = sun or star's mean refraction,

mr = moon's true refraction.

Then mr' = sun or star's true refraction.

By the construction of Table IX., $2 \cos. (60° + a) = \frac{\cos. (M - mr)}{\cos. M} \times \frac{\cos. (S - mr')}{\cos. S}$, Therefore

$$2 \cos. 60° - 2 \sin. a \sin. 60° = 1 - 1{\cdot}73206 \sin. a = \frac{\cos. M + m \sin. r \sin. M}{\cos. M} \times \frac{\cos. S + m \sin. r' \sin. S}{\cos. S} = (1 + m \sin. r \text{ tang. } M) \times (1 + m \sin. r' \text{ tang. } S) = (1 + m \sin. 57'') \times (1 + m \sin. 57'') = 1 + 2m \sin. 57'' = 1 + m \sin. 114''.$$

Therefore $-1{\cdot}73206\, a = m \times 114''$, and $a = m \times \frac{114''}{-1{\cdot}73206} = m \times -66''$.

Therefore the auxiliary angle, so far as it depends on refraction, is equal to the mean refraction 66 seconds taken negatively, and corrected for the given state of the atmosphere. Hence the correction of the auxiliary angle is evident.

Although this investigation involves assumptions not quite correct, yet the result will be sufficiently exact for the purposes of navigation.

Tables XI., XII., and XIII., are designed to give the corrections of lunar distances, arising from the spheroidal figure of the earth. No instructions for their use are given by the author, but as they are much better adapted to show these corrections than any others yet published, and as they are equally applicable to every method of computing the true distances, the following directions may be of service.

The moon's horizontal parallax employed in calculating the true distance, must be increased by the number taken from Table XI. The calculated true distance is to be augmented by the number taken from Table XII., and diminished by the number obtained from Table XIII., and the annexed rule, *viz.*:—From the half sum of the moon's distance from the elevated pole, the sun or star's distance from the same, and the distance of the moon from the sun or star, subtract the moon's distances from the pole and the sun or star respectively. Add the logarithmic sines of the two remainders, the logarithmic cosecant of the moon's distance from the sun or star, and the logarithm taken from Table XIII., the sum is the logarithm of the number of seconds to be subtracted from the true distance. The result is the true distance on the hypothesis of the earth being a spheroid, the polar axis being to the equatorial as 320 to 321.

EXAMPLE I.

In the first example above, the latitude is 15° 10′ S., the sun's declination 6° 49′ S., and the moon's declination 24° 41′ S. Hence the moon's horizontal parallax corrected is 58′ 26″, with which the true distance will be found to be 41° 18′ 33″. Table XII. gives 5″ to be added, and Table XIII. 7″ to be subtracted. The corrected true distance is therefore 41° 18′ 31″.

	°	′		
Moon's distance from elevated pole	65	19		
Sun's	83	11		
Moon's distance from Sun . . .	41	18	L. cosec.	0·1805
Sum	189	48		
Half	94	54		
1st Remainder	29	35	L. sine	9·6934
2d ,,	53	36	L. sine	9·9057
			Table XIII.	1·0471
Log. of 7″				0·8267

EXAMPLE II.

In the second example above, the latitude is 41° 15′ S., the star's declination 10° 13′ S., and the moon's declination 6° 48′ N. Hence the horizontal parallax corrected is 58′ 41″, and the calculated true distance 30° 2′ 1″. Table XII. gives 14″ to be added, and Table XIII. 6″ to be subtracted. The corrected true distance is therefore 30° 2′ 9″.

	°	′		
Moon's distance from elevated pole	96	48		
Star's	79	47		
Moon's distance from star . . .	30	2	L. cosec.	0·3006
Sum	206	37		
Half	103	18		
1st Remainder	6	30	L. sine	9·0539
2nd ,,	73	16	L. sine	9·9812
			Table XIII.	1·4486
Log. of 6″				0·7843

This method of correcting the lunar distances from the effect of the earth's spheroidal figure is derived from La Lande's method of computing the parallaxes on the spheroidal hypothesis, in which the latitude on the sphere, and the horizontal parallax augmented by a small quantity, are employed. The true place being deduced from the apparent in this manner, the true right ascension is at once obtained; but the equation $\frac{p \text{ sin. } a \text{ cos. } ☽ \text{ dec.}}{\text{cos. lat.}}$ (where p is the horizontal parallax, and a the angle of the vertical with the radius of the earth for the given place,) must be added to the calculated true co-declination or distance from the elevated pole, to have it correct. Therefore when the augmented horizontal parallax is employed in computing the true lunar distances, the result must be increased by this equation, multiplied by the cosine of the angle at the moon in the spherical triangle formed by the moon, sun, or star, and elevated pole. Let A denote this angle, the correction of distance is $\frac{p \text{ sin. } a \text{ cos. } ☽ \text{ dec. cos. } A}{\text{cos. lat.}} = \frac{p \text{ sin. } a \text{ cos } ☽ \text{ dec.}}{\text{cos. lat.}} - \frac{2\, p \text{ sin. } a \text{ cos. } ☽ \text{ dec. sin.}^2 \tfrac{1}{2} A}{\text{cos. lat.}}$. The value of the first part of this expression, being the quantity to be added, is contained in Table XII., and the second part to be subtracted is expressed thus, S denoting the half sum of the three sides of the spherical triangle.

$$\frac{2\, p \text{ sin. } a \text{ cos. } ☽ \text{ dec. sin. } (S - ☽ \text{ co-dec.}) \text{ sin. } (S - ☽ \text{ dist. from } \odot \text{ or } ✳)}{\text{cos. lat. cos. } ☽ \text{ dec. sin. dist.}}$$

The logarithms of $\frac{2\, p \text{ sin. } a}{\text{cos. lat.}}$ are contained in Table XIII., to which the logarithms of the other parts of the expression being added, as in the practical rule, (expunging cos. ☽ dec. from both numerator and denominator,) the sum is the logarithm of the second part to be subtracted. In Tables XII. and XIII. the mean value of the moon's horizontal parallax 57′ is assumed as sufficiently correct for computing the equation in every case. The augmentation of the horizontal parallax is contained in Table XI.

Besides the corrections taken notice of in this paper, a third ought to be considered, namely, the contraction of the semi-diameters of the sun and moon inclined to the horizon, occasioned by refraction. An easy and simple method of estimating the correction proceeding from this cause was given in the second number of the Astronomical and Nautical Collections, Journal, No. XVII. The two examples computed above, being further corrected in this manner, are reduced to 41° 18′ 18″, and 30° 2′ 28″. If neither of the several corrections had been attended to, these distances would have been found to be 41° 18′ 54″, and 30° 1′ 34″.

iv. *Correct Formula for the* NUTATION. *By Professor* BESSEL. *Astr. Nachr.* 83.

$\Delta L = \{-18'',0377 \text{ sin. } ☊ + 0'',21720 \text{ sin. } 2☊ - 0'',21633$
$\text{sin. } 2☽\}(1+i) - (1'',13640 - 2'',86868\, i) \text{ sin. } 2☉;$

$\Delta \omega = \{+9'',6480 \text{ cos. } ☊ - 0'',09428 \text{ cos. } 2☊ + 0'',09391 \text{ cos.}$
$2☽\}(1+i) + (0'',49330 - 1'',24527\, i) \text{ cos. } 2☉:$

Or, if $i = -0,069541$;

$\Delta L = -16'',78332 \text{ sin. } ☊ + 0'',20209 \text{ sin. } 2☊ - 1'',33589$
$\text{sin. } 2☉ - 0'',20128 \text{ sin. } 2☽,$

$\Delta \omega = +8'',97707 \text{ cos. } ☊ - 0'',08773 \text{ cos. } 2☊ + 0'',57990 \text{ cos.}$
$2☉ + 0'',08738 \text{ cos. } 2☽.$

v. *Remarks on the Principles of Algebraical and Fluxional* NOTATION. *By the* EDITOR.

It is extremely desirable that the mathematicians of Europe, who may have to make public any new investigations, or to reprint any of their former labours, should adopt, as much as possible, a uniform symbolical language; and in order that the language may be uniform, it is required that it should be deduced from some fixed principles, or should have at least a certain regularity of plan which would ensure its elegance and symmetry, as well as its precision and distinctness. In the Article FLUENTS, of the Supplement of the Encyclopædia, and in several parts of the Illustrations of the CELESTIAL MECHANICS, an attempt has been made to introduce some improvements, which will here be repeated, with a few further elucidations.

1. " The earlier letters of the alphabet, as far as *q*, and sometimes *r*, are commonly employed to denote constant quantities;

the subsequent letters generally for quantities considered as variable. They are here employed as relating indifferently to quantities positive or negative, and to numbers, whole or fractional, except when they are used as indices or exponents."

It may be remarked that the use of the letter *o* is generally avoided, to avoid confusion from its similarity to 0, or zero: though printers, even the most careful, are continually in the habit of printing an italic *o* instead of zero.

2. "The Italic characters are employed in preference to others, for denoting quantities in general; the Roman for characteristic marks, as d for a fluxion, or differential, sin, cos, or f, ç, for sine and cosine; and hl for hyperbolic logarithm. The long italic $\int$, however, not being otherwise used, serves very conveniently as a characteristic, to denote a fluent."

The index of a power is regularly marked by a smaller character above and to the right of the root, as a^2 for aa, x^3 for xxx; and in the same manner it is usual to write d^2, instead of repeating the characteristic d, because of the frequent occurrence of a second fluxion: but it has been more common to write $\cos^2 x$ for $(\cos x)^2$, rather than for $\cos \cos x$, since the latter expression can scarcely ever occur, though Mr. Herschel thinks the symbol would be more correctly employed in this sense. In the same manner it is usual to write $\int^2 \mathrm{d}^2 x$ for the second fluent of the second fluxion of x, and it seems to be more distinct, and therefore more elegant, to write ${}^2\!\int_\infty y\mathrm{d}x$ for the particular fluent of $y\mathrm{d}x$, taken between the values $x = 2$ and $x = \infty$, than to place the 2 on the right of the $\int$, $\int^2_\infty$, as Mr. Fourier and others have done.

3. "When the Italic letters m, n, p, q, r, or any others, are here employed as indices, they are to be understood as denoting any numbers without limitation; the Roman small letters, m, n, will be applied to whole numbers only, excluding fractions, but either positive or negative, or 0; the SMALL Italic capitals *M*, *N*, to positive numbers, whether whole or fractional, excluding negative numbers only; and the small Roman capitals, M, N, to positive integers only, including, however, 0."

Distinctions of this kind are only intended to be recollected for the particular purpose to which they are immediately applied, and it is not likely that the uses here assigned will ever be so generally adopted without express definitions, as to be worth imprinting on the memory.

4. " The characteristic Σ implies the sum of a finite number of terms, derived from all the possible variations of a quantity, which is here denoted by a small letter of the Greek alphabet."

Laplace, and others, have used the Σ in this manner; and it is easy to observe the distinction of expressing the quantity, which is variable to a certain extent by a small Greek letter, thus $\int \frac{A + Bx + Cx^2 \ldots}{a + bx + cx^2 \ldots} \mathrm{d}x = \Sigma \mathrm{hl}\,(x - \varrho)\,\frac{v}{\zeta}$; where ϱ is to be made successively equal to each of the roots of the equation $0 = a + bx + cx^2 \ldots$, $v = A + B\varrho + C\varrho^2 \ldots$, and $\zeta = b + 2c\varrho + 3d\varrho^2 \ldots$

5. " A comma , in an index, denotes or."

A comma is also used by some writers to signify *and*, in expressions of what they call functions of different quantities.

Again, Cel. Mech. p. 76. " In this country it has been usual, at least till very lately, to preserve the geometrical accuracy introduced by the great inventor of the method of fluxions, and to call any finite quantities, in the ratio of the velocities of increase and decrease of two or more magnitudes, the fluxions of these magnitudes. Thus, if we call the increments of x and y, x' and y', we have, for the fluxions, any magnitudes $\dot{x}$ and $\dot{y}$, so assumed, that $\dot{x} : \dot{y}$ shall be equal to $x' : y'$ when these increments become evanescent. On the Continent, it has been more common to write dx and dy for x' and y', considered as actually evanescent; and Euler has remarked, at the beginning of his Integral Calculus, that the language of the English is the more correct, but that the continental notation is the more convenient. His words are these:—' Quas enim nos quantitates variabiles vocamus, eas Angli, nomine magis idoneo, quantitates fluentes vocant, et earum incrementa infinite parva seu evanescentia fluxiones nominant, ita ut fluxiones ipsis idem sint, quod nobis differentialia. Hæc diversitas loquendi ita jam usu invaluit, ut conciliatio vix unquam sit expectanda: equidem Anglos in formulis loquendi lubenter imitarer, sed signa, quibus nos situ mur, illorum signis longe anteferenda videntur.' In fact, however, the English do *not* call the *evanescent* increments fluxions, any more than a mile is an evanescent quantity, when we speak of a velocity of a mile an hour. There are certainly some cases in which the fluxional notation is inconvenient; thus, when we have occasion to write $\mathrm{d}\delta x = \delta \mathrm{d}x$, it would be impossible to express this equation without deviating from that method; we might, indeed, write $(\delta x)^{\cdot} = \delta \dot{x}$, but we should still introduce one heterogeneous character " δ, in order to denote the fluxion of the quantity x, upon a different supposition respecting the generation of the quantity to be compared, which is called, for distinction, the *variation* of x; as if we supposed the ordinate x to be moved laterally along the absciss, and to assume the place of its neighbouring ordinates in succession on the one hand, or to be simply prolonged or diminished on the other, the two elementary changes of its magnitude being expressed by $\mathrm{d}x$ and δx respectively, or by δx and $\mathrm{d}x$, at the option of the writer.

" It is surely a great inelegance, to say the least, not to distinguish a characteristic mark from a multiplying quantity by a difference of type; for dx must mean, according to all analogy, the product of d and x: and it is much more intelligible to write $\mathrm{d}x$, as Lacroix, and many others, have done, instead of dx, as it is generally printed in the works of Laplace.

" It must be understood, then, that $\mathrm{d}x$, as well as $\dot{x}$, denotes a finite quantity proportional to an evanescent increment; but when we use other characteristics of variation, such as δ or Δ, it is not always necessary to limit their signification so precisely: it will, however, sometimes be convenient to employ the mark D for an element of matter, considered as evanescent, and Δx for an evanescent increment of x, corresponding to the fluxion $\mathrm{d}x$, while the larger Δ is employed to denote a finite difference, whether greater or smaller."

P. 93. " The initial value of any variable quantity u has sometimes been distinguished by a capital letter U; sometimes by a point, $\dot{u}$, especially where it is supposed to begin from nothing; but we must not altogether forget that this is the Newtonian character for a fluxion," and the most convenient symbol will, perhaps, be u'; as $\Delta u' = \Delta x \frac{du'}{dx} + \frac{\Delta x^2}{1.2} \cdot \frac{d^2u^2}{dx^2} + \ldots$ Δx^2 and dx^2 being written for $(\Delta x)^2$ and $(dx)^2$.

P. 101. "Partial fluxions are often somewhat inaccurately expressed by a circuitous notation, in which the quantity supposed to vary is set down both as a divisor and a multiplier;" thus, $\frac{dz}{dx}\,dx$, if $z = xy$, is understood to mean $y dx$, and not $y dx + x dy$: it was first proposed by Euler, who seems to have introduced the method, to distinguish a partial fluxion by enclosing it within a parenthesis, as $\left(\frac{dz}{dx}\right) dx$; and it is sometimes denoted by $d_x z$; but the addition of a comma to the d is a sufficient distinction to imply that $\frac{d'z}{dx}$ is meant to signify the coefficient of that part of the fluxion of z, which depends on the change of x only, without the verbal repetition of the remark that a partial fluxion is intended.

vi. OCCULTATIONS *of the Planets and Fixed Stars not less than the fourth magnitude, visible at* GREENWICH *in the* Year 1826. *By* THOMAS HENDERSON, *Esq.*

January 31. δ♏. Immersion at $16^h\ 59^m$ mean time. Difference of declination 15′, star south of moon's centre. Emersion at $17^h\ 36^m$ mean time. Difference of declination 11′, star south. Immersion takes place at 182° from moon's vertex towards the east or left hand; and the emersion at 121° towards the west or right hand.

February 16. Saturn. Immersion at $3^h\ 8^m$ mean time. Difference of declination 10′; Saturn south. Emersion at $3^h\ 52^m$ mean time. Difference of declination 13′; Saturn south. Immersion takes place at 175° from moon's vertex towards east, or left hand; and emersion at 119° towards west, or right hand.

February 20. 2*a* ♋. Immersion at $10^h\ 36^m$ mean time. Difference of declination 7′; star south. Emersion at $11^h\ 49^m$, mean time. Difference of declination 6′; star north. Immersion takes place at 120° from moon's vertex towards east, or left hand; and emersion at 82° towards west, or right hand.

April 12. ζ ♉. Immersion at 10^h 39^m mean time. Difference of declination 12′; star north. Emersion at 11^h 6^m mean time. Difference of declination 14′; star north. Immersion at 2° from moon's vertex towards west, or right hand; and emersion at 61° towards the same.

November 4. d ♐. Immersion before sun-set. Emersion at 5^h 24^m mean time. Difference of declination 4′; star south. Emersion at 117° from moon's vertex towards west, or right hand.

To these may be added the solar eclipse on 28th November.

November 28. Eclipse of the sun, beginning at 21^h 47^m mean time. Difference of declination between sun's centre and point of his limb where eclipse commences, 9′, the centre being south; End at mean noon of November 29. Difference of declination between sun's centre and point of limb where eclipse ends 6′; the centre being south. Moon makes first impression on sun's disc at 35° from sun's vertex towards west, or right hand; and leaves sun's disc at 68° from vertex towards east, or left hand.

Various other conjunctions of the moon with stars are marked with asteriscs in the Nautical Almanac; but they either happen when the sun is above the horizon, rendering the occultations of the smaller stars invisible, or prove to be only appulses, as visible from Greenwich. The following will be very near appulses.

March	15,	ι ♉
April	13,	ι ♊
August	30,	2α ♋
November	5,	β ♑
November	20,	1α ♋

Elements for computing the Solar Eclipses and Occultations of the Planets by the Moon, in the Year 1826.

☽	Conjunction in A.R. Apparent Time.	d	h	m	s	Diff. Dec. ☽ ′	″		Relative H. M. ′	″	Relative Orb. Ang.	°	″		A. R. h	m	s	☽'s Declin. °	′	″		Nearest approach. ′	″	Time of nearest approach. Apparent time. h	m	s	Sun or Planet's horizontal parallax. ″
♄	January	19	20	29	18	61	0	N	31	28	N	89	2	E	4	57	58	22	15	41	N	60	59	20	27	20	1
♄	February	16	4	13	20*	45	13	N	30	47	N	88	42	E	4	55	29	22	2	3	N	45	12	4	11	20	1
♄	March	15	13	47	17	18	35	N	30	9	N	89	0	E	4	58	43	21	46	37	N	18	35	13	46	38	1
☿	April	8	13	6	7	45	6	S	28	15	N	75	41	E	2	13	54	15	55	0	N	43	42	13	29	48	11
♄	April	12	0	33	22	9	40	S	29	51	N	89	52	E	5	7	15	21	35	35	N	9	40	0	33	25	1
♀	May	7	23	30	35	12	27	S	26	46	N	84	42	E	4	1	1	20	40	5	N	12	24	23	33	10	5
♄	May	9	12	16	19	34	2	S	29	54	S	88	55	E	5	19	43	21	20	32	N	34	2	12	15	2	1
☉	June	5	5	38	9	64	40	S	27	50	N	88	58	E	4	52	27	21	28	29	N	64	39	5	40	39	9
♄	June	6	0	57	31	54	35	S	30	11	S	87	34	E	5	34	28	21	23	51	N	54	32	0	52	54	1
♅	June	15	21	21	42	67	14	S	36	37	S	75	6	E	14	8	52	15	34	59	S	64	58	20	53	22	14
♂	July	13	12	34	31	3	10	N	34	58	S	76	29	E	14	29	16	16	42	17	S	3	5	12	35	47	11
☉	October	30	14	12	50	92	23	S	35	29	S	76	21	E	14	19	10	15	26	53	S	89	42	13	35	59	9
☿	October	31	8	35	21	44	42	N	34	17	S	80	25	E	15	6	42	17	56	19	S	44	4	8	48	22	6
☉	November	28	23	16	24*	72	19	N	35	45	S	85	47	E	16	19	39	20	15	8	S	72	7	23	25	20	9
☿	December	27	0	42	27	54	38	S	37	21	S	89	5	E	17	15	40	20	41	56	S	54	38	0	41	2	11
♀	December	27	22	29	39	35	46	S	39	5	N	87	5	E	17	57	4	20	23	22	S	35	43	22	32	26	33

The places of the Sun and Moon have been taken from the Nautical Almanac; those of Mercury from La Lande's Tables in Vince's Astronomy; and those of the other Planets from Professor Schumacher's Ephemeris.

The conjunctions of the Moon with Mercury and Venus on 5th January; with Venus on 5th September; with Mars on 10th August; and with Saturn on 3d July will not be occultations anywhere on the globe.

The method of computing an occultation, given in these collections, No. XX, Art. 3, is the same in principle as Du Séjour's, which is fully detailed and demonstrated in his "*Traité Analytique des Mouvemens Apparens des Corps Célestes.*" It is there shown, (Vol. I. p. 270), that the supposition of the portion of the moon's orbit, described during an eclipse or occultation, being a straight line, causes insensible errors only. An error of greater magnitude may arise from the horary motion being esteemed uniform, while it is generally variable; but this error can be easily corrected by using the horary motion corresponding to the middle of the particular interval under consideration.

In the precepts for computing an observed occultation in the article alluded to, reference is made to the denomination (North or South) of the moon's nearest approach, which is not set down in the elements published in the Nautical Almanac, as it is *always* the same as that of the difference of declinations at the conjunction.

Art. XII. ANALYSIS OF SCIENTIFIC BOOKS.

I. *Philosophical Transactions of the Royal Society of London, for the year* 1825. Part the Second.

In our quarterly reports of the proceedings of the Royal Society, we have given brief and general abstracts of the principal papers contained in the volume before us; a more detailed and accurate abridgement of their contents will form the subject of the following article.

i. *On the Anatomy of the Mole-cricket. By* J. Kidd, M. D., F. R. S., Regius Professor of Medicine in the University of Oxford.

This singular insect is described under various names; as the earth-crab, from its general appearance; vermis cucurbitarius, from the mischief it does to cucumber-beds. By the French naturalists it is called courtilière.

The best account of it is in a well-known entomological work by Rösel, published at Nuremberg, in 1749. This account is accompanied by the best engravings also of the external characters of the animal in its different states; and the value of these engravings is greatly enhanced by the accuracy with which they are coloured.

Rösel says, that about the month of June or July, rarely later, the gravid female gryllotalpa excavates a cavity, from four to five inches beneath the surface of the earth, in which she deposits her eggs in one heap, to the number of three hundred or more, and dies within a few weeks afterwards. At the end of about a month, the young mole-crickets are produced; and appear, on a hasty survey, to bear a general resemblance to the ant. Between the time of their birth and the commencement of winter, the young animals cast their skin three times; they lie dormant during the winter, deeper in the earth in proportion to the inclemency of the season; and during this period cast their skin for the fourth time. About May they leave their winter-quarters, and at this time are furnished with the rudiments of their future wings, four in number; which differ remarkably in size and form and position from those of the perfect insect, in which the inferior wings are folded in a very curious manner, while in the imperfect insect they are always open.

During the month of June or July they cast their skin for the fifth and last time; after which the wings acquire a permanent character, and the insect becomes capable of propagating its species.

It appears from Rösel's account, that while very young, these insects are gregarious, but not afterwards; that they are usually found in the vicinity of meadows and of fields of corn, particularly of barley; to which they are very detrimental by feeding on the roots, and thus intercepting the due nourishment of the plants themselves. Our author has met with the mole-cricket in one situation only, namely, in some peat-bogs, at the distance of a few miles to the west of Oxford. In the neighbourhood of these peat-bogs the insects are familiarly known by the name of croakers, from the peculiar sound which they occasionally make; a sound not very unlike, but more shrill and more soft than that of the frog. This sound, even in the case of a single individual, may be heard at the distance of some yards; but when made by numerous individuals at the same time, it may be heard at the distance of some hundred yards, provided the air be in a favourable state. The insect is usually found within a foot and a half of the surface, and in parts where the peat is neither quite dry, nor very moist; of such a consistence, indeed, as is most favourable to the mining operations of the animal.

The accounts of different authors differ as to the food of the mole-cricket. Having kept several individuals in glass vessels during some weeks, Dr. Kidd observed, that of all kinds of vegetable food, they preferred the potato, while cucumber they hardly touched; but if raw meat were offered them, they attacked it with great greediness, and in preference to every thing else. And, when they had been kept, even but for a short time, without any food, they did not hesitate to attack each other; in which case the victor soon devoured the flesh and softer parts of the vanquished. 'As I have not unfrequently found them in their native haunts, maimed in various parts of the body, I have very little doubt that, although captivity may increase their ferocity, they are not, even in a natural state, free from each other's attacks. If they are carnivorous, they probably feed on worms and various larvæ, which are abundant in the peat-bogs above-mentioned, for I have repeatedly found the horny and indigestible parts of insects within their stomachs. Similar relics I have found in the stomach of the pneumora and gryllus viridissimus. The two following facts attest, in the tribe of insects to which the mole-cricket belongs, a remarkable degree of voracity, and an equally remarkable power of abstaining from food. My friend Dr. Macartney, of Dublin, informs me that he has known a gryllus devour a portion of its own body: on the other hand, my friend Mr. Buckland, of this University, gave me, at the commencement of the present summer, a living gryllotalpa, which had been confined during nine or ten months in a tin case containing a small quantity of garden mould, without the possibility of having met with any other nourishment than such as that portion of mould might be supposed to contain.'

In the external characters of the perfect gryllotalpa, we see a perfect accommodation in form and structure to the circumstances in which the individual is naturally placed. Destined, like the common mole, to live beneath the surface of the earth, and to excavate a passage for itself through the soil which it inhabits, the gryllotalpa is furnished, like the mole, with limbs particularly calculated for burrowing; with a skin which effectually prevents the adhesion of the moist earth through which it moves; and with exactly that form and structure of body, by which it is enabled to penetrate the opposing medium with the greatest ease. At the same time, in order to prevent the necessity of its excavating a track so wide as to admit of the body being turned round in case of a desire to retreat, it is endued with the power of moving as easily in a retrograde as in a progressive direction; and, apparently to perform the office of antennæ, which warn the insect of approaching danger in its progressive motions, it has two appendages, which might not improperly be called caudal antennæ, evidently calculated to serve a similar purpose during its retrograde motions; particularly as they are furnished with very large nerves. The indifference with which the insect is disposed to move in either direction is manifested by the following experiment: if you touch it towards the head, it retreats; if towards the other extremity of the body, it advances.

The general colour of the animal is such as indirectly to serve as a protection to it, being nearly of the same hue as the vegetable mould in which it lives; so that it is not very readily distinguished upon being first turned up to view; and its safety seems to be still farther insured by the appearance of death, which, in common with many other insects, it assumes when suddenly disturbed. This stratagem, for so it may be called, appears to be most decidedly practised by the animal while in captivity; and if thrown at random out of the vessel in which it has been confined, however unnatural the posture may be into which it has been thrown, it remains as it were in a state of catalepsy during half a minute or more; the first indication which it gives of recovery from this stupor, invariably consists in a motion of the extremity of the antenna.

The general colour of the insect is a dusky brown, passing either into a reddish brown, or into an ochery yellow; those parts being of the darkest colour which are most exposed to view when the animal is moving in the open air. Every part of the body is to a greater or less degree covered by a kind of down, which seems to be the efficient cause of its capability of repelling moisture; which capability is so remarkable, that when the insect is plunged under water, it appears as if cased in silver, or some bright metallic covering; this appearance being evidently derived from a stratum of hair, interposed between its body and the surrounding liquid. This down not only serves to repel the adhesion of any moist substance to its body, but also facilitates the motion of the animal, by lessening the degree of friction which would otherwise take place; and it is owing to the same circumstance that there is an unusual degree of difficulty in retaining a sure hold of the insect, even when dead; but more especially when alive, and struggling against detention. The degree of force which it commonly exerts on such occasions is very remarkable, and, from the sensation produced, may easily be supposed to be what Rösel says it is, equal to the counterpoise of two or three pounds. The skin or covering of the insect is in some parts nothing more than a thin membrane; in other parts it resembles soft leather; and sometimes equals horn, or even shell, in its degrees of hardness.

The mole-cricket is more distinctly divisible than most other insects into three separate parts, the head, the thorax, and the abdomen. Of these, the head is not above one-twelfth the length of the whole body; the thorax three-twelfths; and the abdomen eight-twelfths.

The head is united to the thorax, as the thorax also is united to the abdomen, by means of a loose membrane, which envelops the muscles that pass respectively from one to the other; and it is in consequence of the looseness of these membranes that the animal is enabled either to separate the connected parts to a considerable distance from each other, or to contract them so closely together as to hide the interposed membranes from view; and, from the arched form of the anterior part of the thorax, it can draw in its head under that part, much after the manner of a tortoise. The same flexibility of the connecting membranes enables the animal to place either its head or its thorax at a considerable angle with the rest of the body; a movement which is very characteristic of this insect, and gives it an air of intelligence; the attitude being apparently that of watching or listening.

Dr. Kidd next proceeds to a detailed account of the structure of the external parts of the insect, and enumerates some curious and interesting particulars respecting its general anatomy. Upon the recondite subject of the sanguineous circulation of insects, our author throws out some new and important hints which deserve the attention of naturalists, and especially of the comparative anatomist.

" In meditating on this difficult problem," says Dr. Kidd, " it has forcibly occurred to me, that the tracheæ may possibly be the instruments of such a circulation; absorbing the blood or the chyle, in the first instance, from the internal surface of the alimentary canal, and thence conveying it to the various parts of the body; nor is this opinion, however improbable it may appear, entirely gratuitous. No difficulty, I apprehend, attaches to the supposition that such an absorption may take place, seeing that innumerable minute ramifications of the tracheæ penetrate the intestinal canal in every part; nor does there seem any difficulty in admitting that the insect may, by the power of exhausting the air from individual tracheæ, draw on the absorbed fluid towards those two lateral tracheal tubes, which are apparently a general medium of communication between all the other tracheæ of the body. And when once the blood has reached this supposed point of its course, it is manifest, that by whatever means the air itself is forwarded from the same point to the most distant parts of the body, by a modification of the same means, the blood may be forwarded to the same part; and the elegant proposition of Cuvier, that 'the blood being incapable of going in search of the air, the air goes in search of it,' will still remain inviolate.

"If it should be argued that the tracheæ are not found charged with blood after the death of the animal, it may be answered that neither are the arteries in the higher orders of animals found charged with blood after their death. However, I have actually seen some of the ramifications of those tracheæ which are connected with the cæca distended with a fluid of the same colour as that found in those organs; and though I have only witnessed this fact in two instances, yet such a fact, even singly taken, must be allowed to be of considerable importance.

"Of one thing I am certain, that, after careful observation, I have never found the abdominal viscera, I will not say bathed, as some authors of credit have expressed themselves, in the nutrient fluid which is supposed to have transuded through the coats of the intestines; but I have not even found them lubricated by a greater proportion of moisture than lubricates the intestines of the higher classes of animals.

"There is another difficulty which occurs to the hypothesis of the transudation of the chyle through the coats of the intestines; for, if the blood be conveyed to the several parts by previous general diffusion through the interior of the body, and then by absorption into the substance of particular organs, as the hepatic tubes, the vesiculæ seminales and the ovaries; how does it happen that the bile, for instance, does not transude through the coats of the same vessels, the pores of which have admitted the blood from which it has been formed? It may be answered, that the alteration which the blood undergoes in the several organs, changes its properties to such an extent, as to render it incapable of repassing through the pores which admitted it. I cannot of course presume to say that such is not the case; and I am aware that many entomologists will be surprised at, and perhaps disinclined to listen to, the opinion here advanced with respect to a sanguineous circulation in insects; but I nevertheless hope that the opinion will not be rejected without some previous attention to it. With regard to the dorsal vessel of the gryllotalpa, which in this, as in other insects, has been supposed to stand in the place of an arterial heart, I have very few observations to offer. It does not agree in form with the description commonly given of this mysterious organ; for though it diminishes in diameter as it approaches its head, this is by no means the case towards the other extremity of it. I have not yet completely succeeded in tracing this vessel to the anterior extremity; because as it approaches its termination in that direction, it becomes so delicate as to have hitherto broken under dissection before I arrived at the extremity of it. Towards the opposite extremity it gradually becomes larger from the centre of the body, and terminates apparently in a cul-de-sac about the last segment but two of the abdomen."

The dimensions of a full-grown mole-cricket are as follow:—

	Inches.
Length of the body, from the extremity of the lip to the extremity of the vent	2.0
Length of the head	0.165
Length of the thoracic division	0.5
Length of the abdominal division	1.33
Breadth of the thorax	0.5
Breadth of the abdomen	0.5
Length of the antennæ of the head	0.825
Length of the caudal antennæ	0.666
Length of the whole alimentary canal	2.0
Length of the œsophagus	0.5
Length from the crop to the great intestine	0.5
Length of the great intestine	1.0

We cannot take leave of Dr. Kidd without congratulating him on the perspicuous method of arrangement and description which he has pursued in this paper, and trust that he will continue to labour in a field of inquiry abundant in materials, and much in need of that elaborate, unaffected, and unprejudiced spirit of pursuit in which he has entered it.

ii. *Further Observations on Planariæ. By* J. R. Johnson, M.D., F.R.S.

Some account of Dr. Johnson's former observations upon these singular insects, will be found in the fourteenth volume of this Journal *. In the present communication, he adds another singular

* Page 387.

feature to the history of these extraordinary animals, which is, that the *Planaria Cornuta* sometimes obtains an additional head, in consequence of injury or artificial incision. Dr. J. took about a hundred *Planariæ Cornutæ*, and made in each an incision on the side of the body; in one instance only, the desired result was obtained, and after about a fortnight, a perfect head had grown out of the wound, producing a *double-headed Planaria*. In the greater number of cases, the incisions healed, or only produced wens and other irregularities.

iii. *On the Influence of Nerves and Ganglions in producing Animal Heat. By* Sir E. Home, Bart. V.P.R.S. *Communicated by the Society for the Improvement of Animal Chemistry.*

In the most simple animal structures endowed with life, large enough to admit of dissection, brain and nerves are met with, although many such animals possess no power of preserving a temperature higher than that of the atmosphere by which they are immediately surrounded. This is the case with the oyster, the fresh-water mussel, and the garden-snail: they have a brain and spinal marrow, but no ganglions.

In the leech, the earth-worm, and all the insect tribe, the brain and spinal marrow very closely resemble that of the garden snail; but in all these tribes there is a pair of nerves running down from the spinal marrow the whole length of the body of the animal, which are united together at regular intervals by what are called ganglions, composed of nervous fibres, apparently entangled and agglutinated together; and in all such animals it was proved by Mr. Hunter, in his paper on heat, that their temperature exceeds that of the atmosphere when below 56°, although in very different proportions; the excess in the leech being only one degree, while in a hive of bees it is 26°.

As the only difference between the nervous systems of those animals that have no power of producing heat, and those that have, consists in there being ganglions, Sir Everard was led to suspect that this power was derived from the ganglions with which the nerves are furnished.

"To ascertain how far there were sufficient grounds for this suspicion, I began to consider, whether any parts of animals possessed of an unusual temperature were devoid of nerves; the heat of the deer's horn while enclosed in its velvet, in June, 1824, when only one foot long, I found to be 96°, and on the 12th of July the tip of an antler was 99½; from which it was evident that these horns, during their growth, have a power of producing heat, independent of the direct influence of the brain or heart; and therefore it was only necessary to ascertain whether there are nerves accompanying their blood-vessels, which Mr. Bauer not only ascertained to be the case, but found them equally numerous with the arteries themselves.

"This discovery enabled me to institute an experiment, which at once would decide in what degree animal heat was under the influence of ganglionic nerves.

"As I might be considered too partial an evidence respecting the different results arising out of such an experiment, I contented myself with superintending it, and made over the operative part to Mr. Mayo, and his associate Mr. Cæsar Hawkins, teachers of anatomy in Berwick-street. The experiment was to consist in dividing all the trunks of the nerves that supplied the velvet of one horn, while those of the other horn were left entire; and see how far, under these circumstances, the horn would be liable to any diminution of its heat.

"An hour after the nerves were divided, which was about three o'clock, of July the 21st, the temperatures were examined, and so on once a day, as long as there was a material difference between them. This will appear, by the following diary, only to have continued for five days.

	Atmosphere.	Unnerved Horn.	Uninjured Horn.
July 21	66°	72°	84°
22	64	69	95
23	64	67	84
24	64	76	84
25	67	87	90

"Forty-eight hours after the nerves were divided, the temperature of the horn was only 3° higher than that of the atmosphere.

"From the time the experiment was made, the deer was kept in a small paddock with two companions. On the 26th of July it had bruised the horn so much, on which the experiment had been made, that the diary could no longer be continued, and that horn was then the hottest of the two.

"Upon examination after death no union had taken place between the divided trunk, but it was evident from the recovery of its heat, that some other connexion had been formed between the nerves of the horn and those of the head.

"This will not appear surprising, when I mention that the fallow deer, before they have antlers, shed their horns in June; and immediately after, they again begin to bud, and in the middle of August are completely hardened. Those with antlers mew in April or May, according to their keep, and at the end of August are at their full growth. So that in the space of four months, all the nerves that are to supply the deer's horns of a full head have not only begun to form, and arrived at their full growth, but have ceased to exist. This rapidity of growth accounts for their recovering in five days from any check that can be given to their ready communication with one another." Some other curious details upon the subject of animal heat conclude this paper, which is certainly a valuable contribution to chemical physiology.

iv. *An Essay on Egyptian Mummies; with Observations on the Art of Embalming among the Ancient Egyptians. By* A. B. Granville, M.D.; F.R.S.; F.L.S.; F.G.S.; M.R.I., *one of His Royal Highness the Duke of* Clarence's *Physicians in Ordinary, &c. &c.*

We can scarcely do justice, in the ordinary space of an abstract, to the interesting details of this essay, and shall therefore feel sanctioned in somewhat transgressing our usual limits, inasmuch as several doubtful points and obscure circumstances in the history of mummies are here for the first time cleared up and elucidated.

In the year 1821, Sir Archibald Edmonstone presented Dr. Granville with a mummy, which he had purchased at Gournou, on the 24th of March, 1819, from one of the inhabitants of the sepulchral excavations on the side of the mountain, at the back of which are the celebrated tombs of the kings of Thebes. It cost about four dollars. There was no outer case to it; and it is difficult, the author observes, to conceive how the beauty and perfect condition of the surface of the single case in which the mummy was enclosed, could have been so well preserved without any external covering.

When the case was first opened, the mummy was found covered with cerecloth and bandages most skilfully arranged, and applied with a neatness and precision, that would baffle even the imitative power of the most adroit surgeon of the present day. There was no species of bandage which ancient or modern surgery has devised, described, or employed, that did not appear to have been used in securing the surface of the mummy from external air; and these were repeated so many times, that on weighing the whole mass of them after their removal, they were found to weigh twenty-eight pounds avoirdupois, and were composed both of cotton and of linen.

The envelopes having been removed, it was at once ascertained that the subject was a female, and that no ventral incision, as described by Herodotus, had been practised to extract the viscera.

The head was closely shaved; externally the cranium appeared not to have been disturbed in any way. The eyelids were in close contact. The nose was flattened down towards the right cheek, by the action of the bandages. The lips, from being retracted, allowed the teeth of the upper and lower jaw to be seen, perfectly white, and in a sound condition. The arms were crossed over the chest, the fore-arms directed obliquely upwards, towards the extremities of the shoulders. The fingers of the left hand alone were bent inwardly, the thumb remaining extended. No papyrus, or other object of interest, was found within the grasp of the left hand, but a mere lump of rags, which had been previously dipped in the same bituminous substance observed in other portions of the envelopes.

The general surface of the body was of a deep brown colour, approaching to black, and quite dry. In parts where the larger muscles lay, as the thighs for instance, the surface felt quite soft to the touch, and the muscles yielded slightly to pressure. The cuticle appeared to have been removed throughout, except at the extreme points of the fingers and toes.

"The dimensions of the mummy appeared to me," says Dr. G., "to deserve the next consideration; and they were taken with great accuracy. Such an opportunity as that before me, of ascertaining the size and proportions of an Egyptian woman, who had probably lived before the building of the pyramids of Memphis, could not be allowed to escape; especially as no admeasurement of a really perfect female mummy has been recorded in modern times. I deemed it, therefore, an object of importance in the study of the natural history of man, to have those admeasurements ascertained with precision. It is well known, that the Egyptian form has been assumed as the type of a specific variety of the Ethiopian race, particularly by the venerable Blumenbach, from certain supposed peculiarities of outward conformation. The consideration of what follows will enable us, as far as a solitary instance can do, to judge of the correctness of such conjectural generalizations.

	Feet	Inch.
Height of the mummy from the vertex of the head to the inferior surface of the calcaneum	5	$0\frac{7}{10}$
Thus divided:		
Length of the head from the vertex to the first vertebra of the neck	0	$6\frac{2}{10}$
Length of the back-bone from the first vertebra of the neck, to the articulation of the os sacrum with the os coccygis	1	10
Length of the thigh from the centre of the head of the femur to the centre of the knee pan	1	$5\frac{3}{10}$
Length of the leg from the centre of the knee pan to the inferior surface of the calcaneum	1	$3\frac{2}{10}$
Total	5	$0\frac{7}{10}$

The dimensions of the upper extremities, and of the foot, are these:

	Foot	Inch.		
Length of the arm	1	$1\frac{5}{10}$	2	6
—— of the fore arm	0	$9\frac{5}{10}$		
—— of the hand from the tip of the middle finger, to the articulation at the wrist	0	7		
Length of the foot	0	$7\frac{6}{10}$		

"Now we find, on comparing the principal of these dimensions, with those of the Venus de Medicis, as given by Winkelman, Camper, and others, that the difference between them is so slight, as not to deserve notice. Our mummy is that of a person rather taller. The celebrated Medicean statue, which stands as the representative of a perfect beauty, is five feet in height, like our mummy, and the relative admeasurements of the arm, fore-arm, and hand, in each, are precisely similar."

Having adverted to the dimensions of the pelvis, and to those of the cranium, Dr. G. remarks, that "Cuvier's opinion respecting the Caucasian origin of the Egyptians, founded on his examination of upwards of fifty heads of mummies, is corroborated by the preceding observations; and that the systems, which were founded on the Negro form, are destroyed by almost all the recent, and certainly the most accurate, investigations of this interesting subject. It is a curious fact, which has been noticed by more than one traveller, that whole families are to be found in Upper Egypt, in whom the general character of the head and face strongly resembles that of the best mummies discovered in the hypogei of Thebes; and not less so, the human figures represented in the ancient monuments of that country."

Our author next gives a rapid sketch of the history of Egyptian mummies in general, and then proceeds to detail the appearances that occurred upon the dissection of his specimen.

An incision having been made into the parietes of the abdomen, just below the ribs, and continued down to the hip-bone, on both sides, and carried along the margin of the pubis, the whole of the integuments and muscles were removed, so as to expose that cavity completely to view. The objects which then presented themselves were a portion of the stomach adhering to the diaphragm, the spleen much reduced in size and flattened, attached to the super-renal capsule of the left kidney, and the left kidney itself imbedded in, but not adhering to the latter, and retaining its ureter, which descended into the bladder. This, as well as the uterus and its appendages, were observed *in situ*, exhibiting strong marks of having been in a diseased state for some time previously to the death of the individual. Fragments only of the intestinal tube could be found, some of them of considerable dimensions, and among them part of the cœcum, with its vermiform appendix, and portions of the ilium. Several large pieces of the peritoneal membrane were likewise observed.

There were also several lumps of a particular species of brittle resin, two or three small pieces of myrrh in their simplest and natural state, and a few larger lumps, of an irregular shape, of some compound of a bituminous and resinous nature, mixed up with an argillaceous earth. No traces of the right kidney could be found, nor of the liver or minor glands of the abdomen; although, among the many fragments of membranes and other soft parts which lay in confusion, and were removed for better inspection, the late Dr. Baillie, who was present at one of the demonstrations, detected the gall-bladder slightly lacerated, but in other respects perfect, retaining a small portion of the peritoneal covering of the liver attached to it, as well as considerable remains of its own ducts.

The cavity of the thorax was next examined; it was found that the pericardium, which adhered partially to the diaphragm, came away with it, and that a laceration had taken place at the same time in that sac.

The heart was found suspended, *in situ*, by its large blood-vessels, in a very contracted state, attached to the lungs by its natural connexions with them.

The last cavity examined was that of the cranium; for this purpose it was sawed in two, horizontally, and when thus opened, it was ascertained that the brain had been removed through the nostrils; the plates of the inner nasal bones having been destroyed in the operation by the instrument employed, as evidenced by the state of those parts.

"The eyes appeared not to have been disturbed. The tongue was preserved, and neither above nor below it was there found any coin or piece of metal, as recorded of some of the mummies, but a lump of rags dipped in pitch. The teeth were perfectly white and intact."

Dr. Granville next offers some remarks on the age of this female, and on the disease of which she died, deduced from the examination of the parts. "When we reflect for a moment," he says, "that the individual in question, according to the more generally received opinion respecting the antiquity of mummies found in the hypogei of Thebes, had probably lived upwards of three thousand years ago; it will bespeak a very extraordinary power of preservation in the mode of embalming then practised, in some cases at least, to be able to say, that the female of which we are speaking, died at an age between fifty and fifty-five years; that she had borne children; and that the disease which appears to have destroyed her was ovarian dropsy, attended with structural derangement of the uterine system generally. That such are the facts, I appeal to the state of the bones of the ilium, and of the uterus with its appendages, for proof.

"The first exhibit that peculiar degree of thinning in the centre of their osseous plates, which has been noticed in women, by Professor Chaussier, and others, in the course of a great number of observations, as an indication of their having borne children, and of their having passed the fortieth year.

"The ovarium and broad ligament of the right side were enveloped in a mass of diseased structure, while the Fallopian tube of the same side was perfectly sound and beautifully preserved."

Dr. Granville concludes this paper with some ingenious observations upon the processes adopted by the ancient embalmers, which he conceives to have been nearly as follows.

"A. Immediately after death the body was committed to the care of the embalmers, when, in the majority of cases, the viscera of the abdomen, either wholly, or partially, were forthwith removed; in some cases through an incision on the one side of the abdomen, as stated by Herodotus, and as proved by some of the mummies examined; and, in others, through the anus.

"B. The head was emptied, in all instances, of its contents, either through the nostrils, by breaking through the superior nasal bones, or through one of the orbits, the eyes being previously taken out, and artificial ones substituted in their place, after the operation. The cavity of the cranium was repeatedly washed out by injections with some fluid, which had the power of not only bringing away every vestige of the substance of the brain, but even of the enveloping membranes of it. Yet the liquid could not have been of a corrosive nature, else the tentorium, or that membranous floor which supports the brain, must have disappeared with the meninges; whereas, it is still in existence, and does not appear to have been in the least injured. A small quantity of hot liquid resin was then injected into the cranium.

"C. The next step taken in the embalming process, was to cover the body with quick lime for a few hours, and after, to rub the surface of it with a blunt knife, or some such instrument as would most effectually assist in removing the cuticle. The scalp, however, does not appear to have been touched; and care was taken also not to expose the root of the nails to the action of the alkali, as it was intended that these should remain in all cases.

"D. The operation of removing the cuticle being accomplished, the body was immersed into a capacious vessel, containing a liquefied mixture of wax and resin, the former predominating; and some sort of bituminous substance being added, not however essential to the process. In this situation the body was suffered to remain a certain number of days over a gentle fire, with the avowed intention of allowing the liquefied mixture to penetrate the innermost and minutest structure; nor can there exist any doubt, but that on this part of the embalming process depended not only its great preservative power, but also its various degrees of perfection.

"E. When the body was taken out of the warm liquid mixture, every part of it must have been in a very soft and supple condition, wholly unsusceptible of putrefaction. The next steps, therefore, to be taken, with a view to convert it into a perfect mummy, must have been those, which, had they been taken before that part of the process that has been just described, would have exposed the body to inevitable putrefaction, in a climate like that of Egypt, namely, the tanning of the integuments, and the exposing of their surface to the preservative action of certain salts. The body was then partially dried, and, lastly, the bandages, previously steeped in a solution of tannin, were applied, some lumps of myrrh, resin, and bitumen, having been previously thrust into the abdomen."

The efficacy and correctness of this view of the process, Dr. Granville has experimentally verified. He concludes with the following observations in reference to ancient descriptions of the art.

"The preceding explanatory description of what appears, from the unquestionable facts collected in the course of my inquiry, to have been the best, and, in my opinion, the primitive mode of preparing mummies by the ancient Egyptians, differs from that found in Herodotus, as well as from those accounts which we read in other writers who came after him. It does not, however, appear that the eminent historian just mentioned had ever been present at the embalming of a mummy, or that he ever had an opportunity of examining one of them. He must, therefore, like many other travellers, have noted down what he had collected from hearsay, in which, amidst much that was surmised, there was something approaching to the truth. It is in evidence that the art was kept a profound mystery among those who professed it, so that the different modes of embalming described with such orderly minuteness of details by Herodotus, could only have been conjectural. It is a curious fact, that, with the exception of the lateral incision, and immersion into a saline solution, mentioned by that historian, we find no confirmatory evidence of the other steps of the supposed processes of embalming detailed by him in any of the various mummies that have hitherto been examined. And in the one now submitted to the inspection of the Society, by far the most perfect that has yet been publicly described, we have none of the characteristic features of the three several modes of embalming which we are told were followed by the ancient Egyptians; while, on the other hand, some of the lesser features of each process are strikingly apparent. We have, in fact, the presence of that which Herodotus asserted was invariably removed in the better prepared mummies, and some of those parts are absent, on the other hand, which he stated never to have been touched in the inferior class of those singular preparations. These facts will be duly valued by the scholar, and the commentators of that historian; and the explanation now given of the real mode of mummifying, will enable the lexicographer to advance with confidence, that the name *mummy* was given to such preparations from the circumstance of *wax* (*mum* in the Coptic language) being the really preservative ingredient employed in their preparation."

v. *On the temporary Magnetic Effect induced in Iron Bodies by Rotation. In a Letter to* J. F. W. Herschel, Esq., *from* P. Barlow, Esq., F. R. S.

An abstract of the contents of this letter has appeared in our thirty-eighth Number, (Vol. xix., page 263.)

vi. *Further Researches on the Preservation of Metals by Electrochemical Means. By* Sir Humphry Davy, Bart., Pres. R. S.

In two former papers (see Vol. xvii. pp. 253, 279) the President has described the effects of small quantities of electro-positive metals in preventing the corrosion or chemical changes of copper exposed to sea-water, and has stated that the results appear to be of the same kind, whether the experiments are made upon a minute scale, and in confined portions of water, or on large masses, and in the ocean.

The first experiments proved, that the copper sheeting of ships might be preserved by this method; but another circumstance was to be attended to, how far the cleanness of the bottom would be influenced by this preservation.

The use of the copper sheathing on the bottom of ships is two-fold: First, to protect the wood from destruction by worms: and, Secondly, to prevent the adhesion of weeds, barnacles, and other shell-fish. No worms can penetrate the wood as long as the surface of the copper remains perfect; but when copper has been applied for a certain time, a green coating forms upon it, to which weeds and shell-fish adhere. When this green rust has *partially* formed, the copper below is protected by it, and there is an unequal action produced, the electrical effect of the oxide, submuriate, and carbonate of copper formed, being to produce a more rapid corrosion of the parts still exposed to sea-water; so that the sheets are often found perforated with holes in one part, and comparatively sound in other parts.

In his last paper, the President has referred to the circumstance of the carbonate of lime and magnesia forming upon sheets of copper, protected by a quantity of iron above $\frac{1}{12}$ parts, when these sheets were in harbour and at rest.

"The various experiments," says Sir Humphry, "that I have caused to be made at Portsmouth, shew all the circumstances of this kind of action, and I have likewise elucidated them by experiments made on a smaller scale, and in limited quantities of water. It appears from these experiments, that sheets of copper at rest in sea-water, always increase in weight from the deposition of the alkaline and earthy substances, when defended by a quantity of cast-iron under $\frac{1}{150}$ of their surface, and if in a limited or confined quantity of water, when the proportion of the defending metal is under $\frac{1}{4000}$. With quantities below these respectively proportional for the sea, and limited quantities of water, the copper corrodes; at first it slightly increases in weight, and then slowly loses weight. Thus a sheet of copper 4 feet long, 14 inches wide, and weighing 9 lbs. 6 oz., protected by $\frac{1}{100}$ of its surface of cast-iron, gained, in ten weeks and five days, 12 drachms, and was coated over with carbonate of lime and magnesia: a sheet of copper of the same size protected by $\frac{1}{150}$, gained only 1 drachm in the same time, and a part of it was green from the adhering salts of copper; whilst an unprotected sheet of the same class, both as to size and weight, and exposed for the same time, and as nearly as possible under the same circumstances, had lost 14 drachms; but experiments of this kind, though they agree when carried on under precisely similar circumstances, must of necessity be very irregular in their results, when made in different seas and situations, being influenced by the degree of saltness, and the nature of the impregnations of the water, the strength of tide and of the waves, the temperature, &c.

"In examining sheets which had been defended by small quantities of iron in proportions under $\frac{1}{250}$ and above $\frac{1}{1000}$, whether they were exposed alone, or on the sides of boats, there seemed to me no adhesions of confervæ, except in cases where the oxide of iron covered the copper immediately round the protectors; and even in these instances such adhesions were extremely trifling, and might be considered rather as the vegetations caught by the rough surface of the oxide of iron, than as actually growing upon it.

"Till the month of July, 1824, all the experiments had been tried in harbour, and in comparatively still water; and though it could hardly be doubted, that the same principles would prevail in cases where ships were in motion, and on the ocean, yet still it was desirable to determine this by direct experiment; and I took the opportunity of an expedition intended to ascertain some points of longitude in the north seas, and which afforded me the use of a steam-boat, to make these researches. Sheets of copper, carefully weighed, and with different quantities of protecting metal, and some unprotected, were exposed upon canvas, so as to be electrically insulated upon the bow of the steam-boat, and were weighed and examined at different periods, after being exposed in the north seas to the action of the water during the most rapid motion of the vessel. Very rough weather interfered with some of these experiments, and many of the sheets were lost, and the protectors of others were washed away; but the general results were as satisfactory as if the whole series of the arrangements had been complete. It was found that undefended sheets of copper of a foot square lost about 6.55 grains in passing at a rate averaging that of eight miles an hour in twelve hours; but a sheet, having the same surface, defended by rather less than $\frac{1}{500}$, lost 5.5 grains; and that like sheets defended by $\frac{1}{70}$ and $\frac{1}{100}$ of malleable iron were similarly worn, and underwent nearly the same loss, that of two grains, in passing through the same space of water. These experiments (the results of which were confirmed by those of others made during the whole of a voyage to and from Heligoland, but in which during the return the protectors were lost) show that motion does not affect the nature of the limits and quantity of the protecting metal; and likewise prove, that independently of the chemical, there is a mechanical wear of the copper in sailing, and which, on the most exposed part of the ship, and in the most rapid course, bears a relation to it of nearly 2 to 4.55."

The author next proceeds to detail a series of experiments having for their object "to ascertain the extent of the diminution of electrical action in instances of imperfect or irregular conducting surfaces."

With single sheets or wires of copper, and in small confined quantities of sea-water, there seemed to be no indications of diminution of conducting power, or of the preservative effects of zinc or iron, however divided or diffused the surface of the copper, provided there was a perfect metallic connexion through the mass. Thus, a small piece of copper containing about 32 square inches, was perfectly protected by a quantity of zinc which was less than $\frac{1}{4000}$ part of the whole surface; and a copper wire of several feet in length was prevented from tarnishing by a piece of zinc-wire which was less than $\frac{1}{1400}$ part of its length. In these cases the protecting metal corroded with great rapidity, and in a few hours was entirely destroyed; but when applied in the form of wire and covered, except at its transverse surface, with cement, its protecting influence upon the same minute scale was exhibited for many days. A part of these results depend upon the absorption of the oxygen dissolved in the water when its quantity is limited, by the oxidable metal, and of course the proportion of this metal must be much larger when the water is constantly changing; but the experiments seem to shew that any diminution of protecting effect at a distance, does not depend upon the nature of the metallic, but of the imperfect or fluid conductor.

This indeed is shewn by many other results.

A piece of zinc and a piece of copper, in the same vessel of sea-water, but not in contact, were connected by different lengths of fine silver wire of different thickness. It was found that whatever lengths of wire of $\frac{1}{300}$ of an inch were used, there was no diminution of the protecting effect of the zinc; and the experiment was carried so far as to employ the whole of a quantity of extremely fine wire, amounting to upwards of forty feet in length, and of a diameter equal only to $\frac{100}{98742}$ of an inch, when the results were precisely the same as if the zinc and copper had been in immediate contact.

Pieces of charcoal, which is the worst amongst the more perfect conductors, were connected by being tied together, and made the medium of communication between zinc and copper, upon the same principles, and with the same views as those just described, and with precisely the same consequences.

Sir H. then details some experiments to determine the requisite extent and nature of the contact or relation between the copper and the preserving metal. He could not produce any protecting action of zinc or iron upon copper through the thinnest stratum of air, or the finest leaf of mica, or of dry paper; but the action of the metals did not seem to be much impaired by the ordinary coating of oxide or rust; nor was it destroyed when the finest bibulous paper was between them, being moistened with sea-water.

Sir H. concludes this communication with some practical inferences and theoretical elucidations arising out of its experimental details. The very first experiment made on harbour-boats at Portsmouth, proved that a single mass of iron protected many sheets of copper, so as to make them negatively electrical, and in such a degree as to occasion the deposition of earthy matter upon them; but observations on the effects of the single contact of iron upon a number of sheets of copper, where the junctions and nails were covered with rust, and that had been in a ship for some years, shewed that the action was weakened in the case of imperfect connexions by distance, and that the sheets near the protector were more defended than those remote from it. Upon this idea, Sir H. proposed, that when ships, of which the copper sheathing was old and worn, were to be protected, a greater proportion of iron should be used, and that, if possible, it should be more distributed. The first experiment of this kind was tried on the Sammarang, of 28 guns, in March, 1824, and which had been coppered three years before in India. Cast-iron, equal in surface to about $\frac{1}{80}$ of that of the copper, was applied in four masses, two near the stern, two on the bows. She made a voyage to Nova Scotia, and returned in January, 1825. "A false and entirely unfounded statement respecting this vessel," says Sir H., "was published in most of the newspapers, that the bottom was covered with weeds and barnacles. I was present at Portsmouth soon after she was brought into dock: there was not the smallest weed or shell-fish upon the whole of the bottom from a few feet round the stern-protectors to the lead on her bow. Round the stern-protectors there was a slight adhesion of rust of iron, and upon this there were some zoophytes of the capillary kind, of an inch and a half or two inches in length, and a number of minute barnacles. For a considerable space round the protectors, both on the stern and bow, the copper was bright; but the copper became green towards the central parts of the ship; yet even here the rust or verdigris was a light powder, and only small in quantity, and did not adhere, or come off in scales, and there had been evidently little copper lost in the voyage.

"I had seen this ship come into dock in the spring of 1824, before she was protected, covered with thick green carbonate and submuriate of copper, and with a number of long weeds, principally fuci, and a quantity of zoophytes, adhering to different parts of the bottom; so that this first experiment was highly satisfactory, though made under very unfavourable circumstances."

At Liverpool several ships have been protected, and have returned after voyages to the West Indies, and even to the East Indies. The proportion of protecting metal in all of them has been beyond what Sir H. recommended, $\frac{1}{90}$ to $\frac{1}{70}$; yet two of them have been found perfectly clean, and with the copper untouched after voyages to Demerara; and another nearly in the same state, after two voyages to the same place. Two others have had their bottoms more or less covered with barnacles; but the preservation of the copper has been in all cases judged complete.

In cases when ships are to be newly sheathed, some of the experiments detailed in this paper render it likely, that the most advantageous way of applying protection will be under, and not over the copper: the electrical circuit being made in the sea-water passing through the places of junction in the sheets; and in this way every sheet of copper may be provided with nails of iron or zinc, for protecting them to any extent required. By driving the nail into the wood through paper wetted with brine *above* the tarred paper, or felt, or any other substance that may be employed, the incipient action will be diminished; and there is this great advantage, that a considerable part of the metal will, if the protectors are placed in the centre of the sheet, be deposited and re-dissolved: so there is reason to believe that small masses of metal will act for a great length of time. Zinc, in consequence of its forming little or no insoluble compound in brine or sea-water, will be preferable to iron for this purpose; and whether this metal or iron be used, the waste will be much less than if the metal was exposed on the outside: and all difficulties with respect to a proper situation in this last case are avoided.

"The copper used for sheathing should be the purest that can be obtained; and in being applied to the ship, its surface should be preserved as smooth and equable as possible: and the nails used for fastening should likewise be of pure copper; and a little difference in their thickness and shape will easily compensate for their want of hardness.

"In vessels employed for steam navigation the protecting metal can scarcely be in excess; as the rapid motion of these ships prevents the chance of any adhesions; and the wear of the copper by proper protection is diminished more than two-thirds."

vii. *On the Magnetism of Iron arising from its rotation. By* Samuel Hunter Christie, Esq., M.A., *of Trinity College, Cambridge; Fellow of the Cambridge Philosophical Society; of the Royal Military Academy. Communicated April* 20, 1825, *by* J. F. W. Herschel, Esq., Sec. R. S.

The principal heads of this paper have already been laid before our readers, (see Vol. xix. p. 264,) and its length prevents our doing further justice to it, in the form of an abstract.

viii. *Some account of the Transit Instrument made by* Mr. Dollond, *and lately put up at the Cambridge Observatory. Communicated April* 13, 1825. *By* Robert Woodhouse, A.M. F.R.S.

ix. *On the fossil Elk of Ireland. By* Thomas Weaver, Esq., Member of the Royal Irish Academy, of the Royal Dublin Society, and of the Wernerian and Geological Societies.

x. *Microscopical Observations on the Materials of the Brain, and of the Ova of Animals, to shew the analogy that exists between them. By* Sir Everard Home, Bart., V. P. R. S.

For an account of the contents of these three communications we refer to our account of the proceedings of the Royal Society, at the time they were read. (Vol. xix. p. 268.)

On New Compounds of Carbon and Hydrogen, and on certain other Products obtained during the Decomposition of Oil by Heat. By M. Faraday, F.R.S., Corresponding Member of the Royal Academy of Sciences.

The principal object of this paper is to describe two new compounds of carbon and hydrogen. It commences with a description of the liquid which is deposited during the compression of oil-gas, as practised by the portable oil-gas company.

It is a thin, light fluid; sometimes transparent and colourless, at others opalescent, being yellow or brown by transmitted, and green by reflected light. It has the odour of oil-gas. When the bottle containing it is opened, evaporation takes place from the surface of the liquid; and it may be seen by the striæ in the air that vapour is passing off from it. Sometimes in such circumstances it will boil, if the bottle and its contents have had their temperature raised a few degrees. After a short time this abundant evolution of vapour ceases, and the remaining portion is comparatively fixed.

The specific gravity of this substance is 0.821. It does not solidify at a temperature of 0° F. It is insoluble, or nearly so, in water; very soluble in alcohol, ether, and volatile and fixed oils. It is neutral to test colours. It is not more soluble in alkaline solutions than in water; and only a small portion is acted upon by them. Muriatic acid has no action upon it. Nitric acid gradually acts upon it, producing nitrous acid, nitric oxide gas, carbonic, and sometimes hydrocyanic acid, *&c.*, but the action is not violent. Sulphuric acid acts upon it in a peculiar manner.

This fluid is a mixture of various bodies, which may, by their difference of volatility, be separated in part from each other. Some of it drawn from the condenser, after the pressure had been repeatedly raised to 30 atmospheres, and at a time when it was at 28 atmospheres, then introduced rapidly into a stoppered bottle and closed up, was, when brought home, put into a flask and distilled, its temperature being raised by the hand. The vapour which came off, and which caused the appearance of boiling, was passed through a glass tube at 0°, and then conducted to the mercurial trough; but little uncondensed vapour came over, not more than thrice the bulk of the liquid; a portion of fluid collected in the cold tube, which boiled and evaporated when the temperature was allowed to rise; and the great bulk of the liquid which remained might now be raised to a comparatively high point, before it entered into ebullition.

A thermometer being introduced into another portion of the fluid, heat was applied, so as to keep the temperature just at the boiling point. When the vessel containing it was opened, it began to boil at 60° F. As the more volatile portions were dissipated, the temperature rose: before a tenth part had been thrown off, the temperature was above 100°. The heat continued gradually to rise, and before the substance was all volatilized, it had attained 250°.

With the hope of separating some distinct substances from this evident mixture, a quantity of it was distilled, and the vapours condensed at a temperature of 0° into separate portions, the receiver being changed with each rise of 10° in the retort, and the liquid retained in a state of incipient ebullition. In this way a succession of products were obtained; but they were by no means constant; for the portions, for instance, which came over when the fluid was boiling from 160° to 170°, when re-distilled, began to boil at 130°, and a part remained which did not rise under 200°. By repeatedly rectifying all these portions, and adding similar products together, our author was able to diminish these differences of temperature, and at last bring them more nearly to resemble a series of substances of different volatility. During these operations he remarked, that the boiling point was more constant at, or between 176° and 190°, than at any other temperature; large quantities of fluid distilling over without any change in the degree; whilst in other parts of the series it was constantly rising. This induced him to search in the products obtained between these points for some definite substance, and he ultimately succeeded in separating a new compound of carbon and hydrogen, which he calls *bi-carburet of hydrogen.*

This substance was obtained in the first instance in the following manner: tubes containing portions of the above rectified products were introduced into a freezing mixture at 0°; many of them became turbid, probably from the presence of water; one, received at 176°, (by which is meant that that was the boiling point of the contents of the retort when it came over) became partly solid, crystals forming round the side, and a fluid remaining in the centre; whilst two other portions, one received at 186°, and the other at 190°, became quite hard. A cold glass rod being introduced into one of these tubes, the mass within was found to resist considerable pressure; but by breaking it down, a solid part was thrust to the bottom of the tube, whilst a fluid remained above: the fluid was poured off, and in this way the solid portion partly purified. The contents of the tube were then allowed to fuse, were introduced into a larger and stronger tube, furnished with another which entered loosely within it, both being closed of course at the lower end; then again lowering the temperature of the whole to 0°, bibulous paper was introduced, and pressed on to the surface of the solid substance in the large tube by the end of the smaller one. In this way much fluid was removed by successive portions of paper, and a solid substance remained, which did not become fluid until raised to 28° or 29°. To complete the separation of the permanently fluid part, the substance was allowed to melt, then cast into a cake in a tin foil mould, and pressed between many folds of bibulous paper in a Bramah's press, care having been taken to cool the paper, tin foil, flannel, boards, and other things used, as near to 0° as possible, to prevent solution of the solid substance in the fluid part to be removed. It was ultimately distilled from off caustic lime, to separate any water it might contain.

The process, which Mr. Faraday considers the best for the preparation of this substance only, is to distil a portion of the fluid deposited during the condensation of oil-gas, to set aside the product obtained before the temperature rises to 170°, to collect that which comes over by 180°, again separately that which comes over by 190°, and also the portion up to 200° or 210°. That before 170° will, upon redistillation, yield portions to be added to those of 180° and 190°; and the part obtained from 190° upwards will also, when re-distilled, yield quantities boiling over at 180°, 190°, *&c.* Having, then, these three portions obtained at 180°, 190°, and 200°, let them be rectified one after the other, and the products between 175° and 195° received in three or four parts at successive temperatures. Then proceed with these as before described.

Bi-carburet of hydrogen appears, under common circumstances, as a colourless transparent liquid, having an odour resembling that of oil-gas, and partaking also of that of almonds. Its specific gravity is nearly 0.85 at 60°. When cooled to about 32° it crystallizes, becoming solid; and the portions which are on the sides of the glass exhibit dendritical forms. By leaving tubes containing thin solid films of it in ice-cold water, and allowing the temperature to rise slowly, its fusing point was found to be very nearly 42° F.; but when liquid, it may, like water and some saline solutions, be cooled much below that point before any part becomes solid. It contracts very much on congealing, 9 parts in bulk becoming 8 very nearly; hence its specific gravity in that state is about 0.956. At 0° it appears as a white or transparent substance, brittle, pulverulent, and of the hardness nearly of loaf-sugar.

It evaporates entirely when exposed to the air. Its boiling point in contact with glass is 186°. The specific gravity of its vapour, corrected to a temperature of 60° is nearly 40, hydrogen being 1: for 2.3 grains became 3.52 cubic inches of vapour at 212°. Barometer 29.98. Other experiments gave a mean approaching very closely to this result.

It does not conduct electricity.

This substance is very slightly soluble in water; very soluble in fixed and volatile oils, in ether, alcohol, *&c.;* the alcoholic solution being precipitated by water. It burns with a bright flame and much smoke. When admitted to oxygen gas, so much vapour rises as to make a powerfully detonating mixture. When passed through a red-hot tube, it gradually deposits carbon, yielding carburetted hydrogen gas.

Chlorine introduced to the substance in a retort exerted but little action until placed in sun-light, when dense fumes were formed, without the evolution of much heat; and ultimately much muriatic acid was produced, and two other substances, one a solid crystalline body, the other a dense thick fluid. It was found by further examination, that neither of these were soluble in water; that both were soluble in alcohol—the liquid readily, the solid with more difficulty. Both of them appeared to be triple compounds of chlorine, carbon, and hydrogen; but the author reserves the consideration of these, and of other similar compounds, to another opportunity.

Iodine appears to exert no action upon the substance in several days in sun-light; it dissolves in the liquid in small quantity, forming a crimson solution.

Potassium heated in the liquid did not lose its brilliancy, or exert any action upon it, at a temperature of 186°.

Solution of alkalis, or their carbonates, had no action upon it.

Nitric acid acted slowly upon the substance, and became red; the fluid remaining colourless. When cooled to 32°, the substance became solid, and of a fine red colour, which disappeared upon fusion. The odour of the substance, with the acid, was exceedingly like that of almonds, and it is probable that hydrocyanic acid was formed. When washed with water, it appeared to have undergone little or no change.

Sulphuric acid added to it, over mercury, exerted a moderate action upon it, little or no heat was evolved, no blackening took place, no sulphurous acid was formed; but the acid became of a light yellow colour, and a portion of a clear colourless fluid floated, which appeared to be a product of the action. When separated, it was found to be bright and clear, not affected by water or more sulphuric acid, solidifying at about 34°, and being then white, crystalline, and dentritical. The substance was lighter than water, soluble in alcohol, the solution being precipitated by a small quantity of water, but becoming clear by great excess.

With regard to the composition of this substance, Mr. Faraday's experiments prove it a binary compound of carbon and hydrogen, two proportionals of the former element $(6 \times 2) = 12$ being united to one of the latter $= 1$. The absence of oxygen is proved by the inaction of potassium, and the results obtained when passed through a red-hot tube.

Of the various other products from the condensed liquor, the next most definite to the bi-carburet of hydrogen appears to be that which is most volatile. If a portion of the original liquid be warmed by the hand, or otherwise, and the vapour which passes off be passed through a tube at 0°, very little uncondensed vapour will go on to the mercurial trough; but there will be found after a time a portion of fluid in the tube, distinguished by the following properties. Though a liquid at 0°, it, upon slight elevation of temperature, begins to boil, and before it has attained

32°, is all resolved into vapour or gas, which may be received and preserved over mercury.

This gas is very combustible, and burns with a brilliant flame. Its specific gravity was between 27 and 28, hydrogen being 1. Hence 100 cubic inches weigh nearly 57.44 grains.

When cooled to 0°, it condensed again, and enclosed in this state in a tube of known capacity, and hermetically sealed up, the bulk of a given weight of the substance at common temperatures was ascertained. This compared with water gave the specific gravity of the liquid as 0.627 at 54°. *It is, therefore, among solids or liquids the lightest body known.*

This gas, or vapour, when agitated with water, is absorbed in small quantities. Alcohol dissolves it in large quantity; and a solution is obtained, which, upon the addition of water, effervesces, and a considerable quantity of the gas is liberated. The alcoholic solution has a peculiar taste, and is neutral to test-papers.

Olive oil dissolves about six volumes of the gas.

Solution of alkali does not affect it; nor does muriatic acid.

Sulphuric acid condenses the gas in very large quantity; 1 volume of the acid condensing above 100 volumes of the vapour. Sometimes the condensation is perfect, at other times a small quantity of residual gas is left, which burns with a pale blue flame, and seems to be a product of too rapid action. Great heat is produced during the action; no sulphurous acid is formed; the acid is much blackened, has a peculiar odour, and upon dilution generally becomes turbid, but no gas is evolved. A permanent compound of the acid with carbon and hydrogen is produced, and enters, as before mentioned, into combination with bases.

By detonation with oxygen it appears that 1 volume of the vapour or gas required 6 volumes of oxygen, consuming 4 of them in producing 4 of carbonic acid gas, and the other 2 by 4 of hydrogen to form water. Upon which view, 4 volumes or proportionals of hydrogen=4, are combined with 4 proportionals of carbon=24, to form one volume of the vapour, the specific gravity of which would, therefore, be 28. Now, this is but little removed from the actual specific gravity obtained by experiment.

As the proportions of the elements in this vapour appear to be the same as in olefiant gas, it became interesting to ascertain whether chlorine had the same action upon it as on the latter body. Chlorine and the vapour were, therefore, mixed in an exhausted retort: rapid combination took place, much heat was evolved, and a liquor produced resembling hydro-chloride of carbon, or the substance obtained by the same process from olefiant gas. It was transparent, colourless, and heavier than water. It had the same sweet taste, but accompanied by an after aromatic bitterness, very persistent. Further, it was composed of nearly equal volumes of the vapour and chlorine: it could not, therefore, be the same as the hydro-chloride of carbon from olefiant gas, since it contained twice as much carbon and hydrogen. It was, therefore, treated with excess of chlorine in sun-light: action slowly took place, more chlorine combined with the substance, muriatic acid was formed, and, ultimately, a fluid tenacious triple compound of chlorine, carbon, and hydrogen was obtained; but no chloride of carbon. This is a remarkable circumstance, and assists in shewing, that though the elements are the same, and in the same proportions as in olefiant gas, they are in a very different state of combination.

Mr. Faraday next proceeds to detail the experiments by which he ascertained the tension of the most volatile part of the condensed oil gas liquid; it appears to be equal to about 4 atmospheres at the temperature of 60°. He also adverts briefly to the properties of the other portions of the liquor, and gives an outline of such facts as others have determined respecting them. In conclusion, he observes, that the importance of these vapours in oil gas, as contributing to its very high illuminating powers, will be appreciated, when it is considered that with many of them, and those of the denser kind, it is quite saturated. On distilling a portion of liquid, which had condensed in the pipes leading to an oil gas gasometer, at Apothecaries' Hall, he found it to contain portions of the bi-carburet of hydrogen. It was detected by submitting the small quantity of liquid which distilled over before 190° to a cold of 0°, when the substance crystallized from the solution. It is evident, therefore, that the gas from which it was deposited must have been saturated with it. On distilling a portion of recent coal gas tar, as was expected, none could be detected in it, but the action of sulphuric acid is sufficient to shew the existence of some of these bodies in the coal gas itself.

With respect to the probable uses of the fluid from compressed oil gas, it is evident, in the first place, that being thus volatile, it will, if introduced into gas which burns with a pale flame, give such quantity of vapour as to make it brightly illuminating; and even the vapour of those portions which require temperatures of 170°, 180°, or higher, for their ebullition, is so dense as to be fully sufficient for this purpose in small quantities. A taper was burnt out in a jar of common air over water; a portion of fluid boiling at 190° was thrown up into it, and agitated; the mixture then burnt from a large aperture with the bright flame and appearance of oil gas, though of course many times the quantity that would have been required of oil gas for the same light was consumed: at the same time there was no mixture of blueness with the flame, whether it were large or small.

The fluid is also an excellent solvent of caoutchouc, surpassing every other substance in this quality. It has already been applied to this purpose.

It will answer all the purposes to which the essential oils are applied as solvents, as in varnishes, *&c.*, and in some cases where volatility is required, when rectified it will far surpass them.

It is possible that, at some future time, when we better understand the minute changes which take place during the decomposition of oil, fat, and other substances by heat, and have more command of the process, that this substance, among others, may furnish the fuel for a lamp, which remaining a fluid at the pressure of two or three atmospheres, but becoming a vapour at less pressure, shall possess all the advantages of a gas lamp, without involving the necessity of high pressure.

xii. *Account of the repetition of* M. Arago's *experiments on the Magnetism manifested by various substances during the act of rotation. By* C. Babbage, Esq. F.R.S., and J. F. W. Herschel, Esq. Sec. R.S.

Though this paper is merely stated to refer to a repetition of M. Arago's experiments, it may in fact be considered as a distinct and somewhat elaborate investigation founded upon a report of the very singular results obtained by that celebrated physical inquirer. Having already laid before our readers an outline of its contents, and intending, upon an early occasion, to give a connected view of the very singular discoveries in magnetism which every day is bringing forth, we shall at present merely recommend those who are experimentally engaged on the subject, to study Messrs. Babbage and Herschel's paper.

xiii. *On the Magnetism developed in Copper and other Substances during rotation. In a Letter from* Samuel Hunter Christie, Esq., M.A., *&c., to* J. F. W. Herschel, Esq., *Sec. R. S. Communicated by* J. F. W. Herschel, Esq.

xiv. *On the Annual Variations of some of the principal fixed Stars. By* J. Pond, F.R.S. *Astron. Royal.*

xv. *On the Nature of the Function expressive of the Law of Human Mortality, and on a new Mode of determining the Value of Life Contingencies. In a Letter to* Francis Baily, Esq., F.R.S. *&c. By* Benjamin Gompertz, Esq., F.R.S.

In respect to these which are the concluding papers of the volume before us, our limits prevent any more extended notice than that already given in our abstract of the proceedings of the Royal Society, (Vol. xix. p. 277.)

II. *Considerations on Volcanoes; the probable Causes of their Phenomena, the Laws which determine their march, the Disposition of their Products, and their connexion with the present State of, and past History of the Globe; leading to the establishment of a New Theory of the Earth. By* G. Poulett Scrope, Esq., *Sec. Geol. Soc.* London, 1825. Phillips.

This work forms what is commonly called a complete treatise, though the author has given it a more moderate title; and although, like every work on geology, it cannot fail to contain disputed points and controversial matter, we do not know that any thing new can now be said on these subjects, and shall not therefore indulge ourselves in discussing them. Thinking it equally unnecessary to enter into any discussion on the merits of the work, as such, since, on all subjects which, like this, may be called technical, the particular readers into whose hands it must fall, are fully as competent to judge of it as ourselves, we shall be content with giving such an analysis of it as is practicable, conceiving, that in so doing, we shall be best able to gratify and interest our readers.

Describing generally what is meant by volcano, and by lava, in which the author properly includes all volcanic rocky erupted matter, under whatever form it may be disposed on the surface, Mr. S. proceeds to state the known volcanoes at 200; afterwards showing reason to think this to be much less than the number probably existing in the world. The arguments for this opinion consist in our ignorance of the interior of great continents, in the probability of unknown marine volcanoes, and in the fact that, for want of observers and records, many which have broken out at distant times, are unknown or forgotten.

To the terrestrial volcanoes he has given the term subaërial, and to the marine the appellation of subaqueous.

In the first class, the character of the appearances vary according to the incidental fact of the volcano being new, or appearing through an ancient vent. But it is not very certain that we know of any rigidly new; as even the eruptions of Yorullo are considered as coming from vents subsidiary to former ones. Auvergne, however, and the well-known country connected with it, in this respect, as well as the volcanic territory of the Rhine, preserve the records of volcanoes which have been once of this character, and where therefore the circumstances can be studied with facility.

With respect to the other case, the author proceeds to investigate the phenomena of active volcanoes, dividing them into three classes, which he calls phase of permanent eruption, phase of moderate activity, and phase of prolonged intermittences.

In the first class, Stromboli is an unquestioned example; and the same appears true of the volcano in the Lake of Nicaragua. The second class includes the great mass of volcanoes known as such; and among those, Vesuvius and Ætna are the most familiar and the best studied, from the free and frequent access which they have permitted, for so many years, to persons endowed with the capacity for observation. This is also the character of the volcanoes of the Pacific, of those of Kamtschatka, and the Molucca and Philippine Islands, and indeed, generally of many more whose histories are to be found without end in works, and of which the enumeration here would be too long for our analysis.

In the third class, or under the phase of prolonged intermittences, the number is even greater than in the preceding; and as novelty, in the case of eruptions of this nature, is added to the naturally terrific circumstances attending them, these are the eruptions which have excited the greatest attention, and which make the greatest figure in history. The author proceeds to sketch the general appearances attending an eruption of this nature.

They are commonly preceded by earthquakes of different degrees of intensity and duration, and with loud sounds or detonations resembling the noise of ordnance and musketry, apparently produced by the disengagement of aëriform fluids, and the increase of bulk in the fluid rocks; and their sounds are conveyed through the solid earth; not by means of the air. The atmosphere at this time is remarked to be in a peculiar state of stillness, attended by a sense of oppression.

During this period also, springs are apt to disappear, so that wells become dry; and it is known that the extent of this affection is sometimes very considerable.

When the eruption first appears, it is generally with sudden and great violence. Explosions, apparently from confined air, take place with loud noises, and succeeding each other with rapidity, and often with increasing force; the vent being, commonly, the central point or crater of the mountain. And in its attempt to escape, this air throws up fragments of rock, which sometimes fall back into the crater, and are again repeatedly projected, together with clouds of aqueous vapour. And as the fragments also are often broken into small pieces, and even into dust, this, uniting to the vapour or mixing with it, produces dense black clouds, or smoke, often assuming the form of a column of entangled or successively formed clouds.

Having arrived at a certain height, this column generally spreads laterally or horizontally, forming, if the air is calm, a shape resembling that of a pine-tree, or if there be wind, a horizontal stream. Out of this cloud proceed lightnings of great vividness, while the falling of the dust, added to the density of the cloud, produces darkness over the surrounding country. The melted rock or lava now boils up in the crater, and is often so thrown up into jets by the extricated air, as to resemble flames; and at length it either boils over the edge of the crater, so as to run down the mountain, or else finds an issue laterally, by some crevice, equally flowing down in a stream, which holds its course as circumstances permit, down to the lower grounds.

In the night this current is luminous; but in the day, it is generally obscured by vapours, or loses its light by the cooling and blackening of the surface. There are cases, however, in which no torrent of lava occurs, and where no other rocks than scoria are erupted. The greatest period of violence is generally over when the lava has flowed for a little while, or this is the crisis of the volcano. But commonly, the explosions of fragments and dust continue for some time, gradually diminishing, till the whole falls into a state of quiescence, and is, finally, extinguished. Lastly, it must be noticed, that from the action of the volcano on the atmosphere, clouds are generally formed in it, which produce falls of rain, often causing torrents, or even inundations.

We must refer to the work itself, pp. 13, 14, for a catalogue of eruptions which we could not conveniently introduce. But the intervals of repose are various, reaching in some cases as far as to many centuries; so that cultivation and population is renewed, to be dispersed again at some future day. In these intervals of repose, however, it is common for vapours to continue to be produced, either from the craters, or in the courses of the currents of lava; and when these are sulphureous, they deposit sulphur; and in other cases, from their acid nature, they corrode and decompose the rocks through which they find a vent. What are called solfataras and souffrières are the result.

The author now proceeds to the Theory of Volcanic Eruptions.

The force of the eruption consists in aëriform fluids extricated and expanded by heat, and maintaining the lava in a state of ebullition; as is evinced by the case of Stromboli, described so accurately by Spallanzani, and as is further proved by facts occurring at Vesuvius, and in the Isle of Bourbon. But the liquidity of the lava is imperfect, in our author's estimation, resembling that of mud, or arising from a freedom of motion among solid particles, mixed with one or more perfect fluids which form their vehicle. At the moment even of emission, he considers it as radiating very little caloric, though of a brilliant white heat, its fluidity, even when greatest, not exceeding that of honey, and being often much less considerable. In flowing, the crust consolidates and cracks, emitting vapours, and thus resembling the scoria of furnaces. He considers that the low degree of this heat is proved by the slight effect which a current of lava produces on a thermometer held near it; and Dolomieu's opinion is adduced to confirm the supposition, that the mobility of the particles of lava arise from some other cause than their complete combination with caloric.

The author considers this opinion to be further proved by experiments on lava, in furnaces. Here, it becomes a glass; whereas in its natural state of flowing and consolidation, it is an aggregate of crystals or separate substances, and thus possesses a lithoidal character. Hence also, he concludes that the crystals found in it existed previously as solid substances in this apparent fluid, and have not been formed during the process of cooling. His conclusion, to quote his own words, is, "that this substance is seldom in a state of igneous fusion, but owes its partial and imperfect liquidity to some other cause, by which a certain mobility is communicated to the solid crystalline particles of which it consists."

He considers, therefore, that its liquidity will depend, not upon the quantity of caloric with which it is combined, but on the quantity of fluid which it contains in proportion to the size and form of the solid particles; and that its condensation is effected, not by parting with its heat, but by the escape or condensation of the fluid which caused its mobility. And as it discharges much elastic vapour on cooling, he considers this matter to be the cause of its fluidity; while the fissures then formed, allowing more elastic vapour to escape from the interior parts, the same process goes on through the whole mass.

The elastic vapour in question he considers to be also that which is the cause of all the violence and explosion in the volcano itself, or steam; whatever other substances in less quantity may be united to it. Bitumen, as it appears by Mr. Knox's experiments, is one of those substances. The mobility and ebullition of lava are therefore the produce of water in union, disengaged in the form of vapour; and thus, when this is disengaged, the solid parts, held in mixture rather than fluidity, unite and become rock. And, consequently, as the volume of vapour generated on any point will depend on the ratio of the temperature and pressure, so, under the same circumstances in these respects, the fluidity of the lava will vary according to the degree of comminution of its component crystalline particles.

The general conclusions flowing from these considerations are, that the volume of vapour generated by any change in the circumstances of a lava, depends on the ratio of the temperature to the force of compression—that the intumescence or dilatation of lava, varying with the volume of vapour produced, must follow the same law—that the proportion of the vapour generated, which is liberated and "unites into parcels," to that which remains fixed, will vary with the comminution of the grain—and, that the force with which these parcels or bubbles tend to traverse the liquid lava and rise to its surface, varies directly with the specific gravity of the solid lava rock; and the degree to which they are enabled to obey this impulse must depend on the liquidity of the mass immediately above them.

Under an increase of pressure or a diminution of temperature, part of the vapour which would otherwise escape tends to the re-consolidation of the lava. It becomes water, and the enclosing crystal which was disintegrated by the ebullition of the lava or the action of its vapour is re-aggregated. And the separate crystals are also re-aggregated, in a confused manner, into a solid rock, more or less porous in proportion to the quantity of vapour disengaged. Such is the author's view of the formation of lava rocks, and of the crystals in them, as far as we can condense it into our narrow space.

A wider general view follows. It is concluded that the interior of the earth, at "no great vertical distance," is of an intense temperature; that the accumulated caloric is continually tending to an equilibrium, but that this is opposed by the imperfectly conducting powers of the rocks or strata, while he also supposes that "the crystalline, and particularly the compact granitoidal rocks," are much better conductors than the "secondary limestones, clays, shales, and sandstones." This caloric thus retained is supposed to act especially on the previous masses of lava, producing all those effects which give rise to the phenomena of volcanoes. Here the author enters into a long and minute detail on the formation of fissures, for which we must refer to the work itself, at page 32 and onwards, as we find it unsusceptible of abridgment.

At page 37, he proceeds to investigate the laws by which the violence and duration of the eruption must be determined.

What he calls the general force of repression, consists in the external pressure, whether from the atmosphere, the ocean, or whatever else, in the weight of the column of liquefied lava, and in the reaction of the vapour generated from the confining surfaces.

And proceeding to apply these rules, he further concludes that the energy of the eruption will vary directly with the temperature of the lava and the width of the aperture, and inversely with the thickness of the solid rock which must be broken through, or with its resistance generally as formed by that thickness and its specific gravity, or with the external pressure. Thus also the duration of the eruption will vary directly with the rate at which the temperature increases downwards, and inversely with the specific gravity of the lava, and the width of the aperture. It equally follows that the expansion and the volume of the dilated mass will be directly proportioned to the energy and duration of the eruption.

It is also plain that as these several circumstances may vary during an eruption, so will variations take place in the appearances and effects; the chief circumstances subject to such changes being the breadth of the aperture or the freedom of escape, and the varying quantity of the superincumbent weight. Thus it is that, during the eruptions themselves, the size of the aperture enlarges by the abrasion of its sides, depending on the violence of the expansive force, on the nature of the rocks traversed, and on the depth from the surface of the focus of expansion. Thus also the consolidation of ejected lava within communicating fissures, tends to increase the restraining power, and proportionally to modify the results.

A general law is hence deduced, namely, that the developement of the volcanic action necessarily and universally tends to its own extinction, by increasing the restraining forces. This follows necessarily from the solidification which takes place within the vent or vents, and from the quantity of ejected scoria which fall back into the crater, and aid in choking it.

Now when these powers have so acted as absolutely to consolidate and fill the passages, it may happen that a feebler point or less resistance will occur at some place which had not formerly afforded any passage. Thus an entirely new vent is formed; and as it is a probable inference that such weak parts will be found in the line of former fissures produced by the previous actions of the volcano, we are led to explain the fact of continued chains of volcanic apertures, such as are notedly found in America.

As the volcanic aperture is a vent for the caloric contained within the earth, when the eruption has not been such as to carry off the whole then disengaged, or that the restraining forces have suspended it, the consequence is, that the gradually increasing expansion will break out into earthquakes; and should a fissure be formed, there will be what the author calls a paroxysmal eruption. Thus he considers that some volcanoes have a double system of operations, "productive both of paroxysmal and minor eruptions;" the first proceeding from interior foci, and having a deep and wide crater, while the others occur in higher secondary foci, throwing up cones within those craters, and tending to obliterate them. These are the moderate or tranquil eruptions and actions, which proceed for a time till they are swallowed up by one from the primary seat, of more intense power. Thus are explained the well-known phenomena of Vesuvius, Teneriffe, Bourbon, and other places, described by numerous observers.

We must here remark, before proceeding further, that the author has given many explanatory drawings to illustrate the minuter details into which he enters; and as it is here in particular that we feel the difficulty of giving a view in analysis of these details, for want of the same aid, so here particularly we must refer our readers to the original work.

At page 58, he makes some observations of the power of atmospheric pressure separately considered, in modifying the volcanic action on the principle of a restraining force, and concludes that the effect is sufficiently marked to render the lava in the crater subject to an influence varying with the barometric pressure. The action will be analogous to that which affects the boiling point of water under similar circumstances; and, in proof of this effect, Stromboli is used as a barometer by the natives. The author even considers that this pressure exerted upon the volcanic aperture, may act so powerfully, as to affect the interior of the earth, so as to regulate the appearance or otherwise of earthquakes; it being supposed, in this case, that a similar pressure is acting on those vents "contemporaneously over a vast extent of the earth's surface."

Thus he also considers that hurricanes may be the cause of volcanic eruptions and earthquakes, instead of being the effects, as has been supposed; their action consisting in diminishing, as is well known, the pressure on the barometer, and thus in removing a portion of the restraining force. He admits, however, that the volcanic eruptions re-act upon the atmosphere, producing various meteoric phenomena, such as hurricanes themselves, as well as the lightnings and rains formerly mentioned.

We proceed now to the third chapter, which is entitled "Disposition of Volcanic Products."

The obvious result of eruption, in the manner formerly described, is to produce a volcanic cone, containing at the apex, which is truncated, an inverted hollow cone, which is the crater. The form of the cone is modified by various causes, such as the sizes of the fragments, their cohesion, *&c.*, while that of the crater will also depend on such causes, and further on the form of the fissure, which is the vent. And thus when the fissure is prolonged, the crater may be elliptical; or there may be more than one volcanic cone; or, lastly, a ridge may be the result. Thus, also, an original irregularity of the ground may modify the forms, as is also found to happen from violent winds occurring during the period of the eruption. Lastly, the emission of lava from particular points is a frequent cause of irregularity in the forms of volcanic mountains. Very obviously this happens when the lava in ebullition fills the crater, and then breaking down by its weight the feeblest side, flows down the surface of the hill. The cone, thus broken at the summit, sometimes remains in that state; while at others, eruptions of scoria following the flow of the lava, replace it, and even sometimes so as to conceal the original orifice.

Such are the external accidents of a simple volcanic cone. The internal structure is stated as being conglomerated, and the substances described are scoria and fragments. The scoria are portions of the liquid lava projected into the air, and which being consolidated in their transmission, fall down in various fantastical shapes, resembling in their general aspect the scoria of furnaces, and often called volcanic cinders. The rounded masses, called volcanic bombs, often contain in the interior a fragment of some rock, as a sort of nucleus, and they are generally more solid than the others. Very liquid lava, of a low specific gravity, is supposed to form pumice; and if the specific gravity is higher, some other species of cavernous structure; while an inferior liquidity produces scoria, with few, or with rough and angular cells; and it is considered that the size and regularity of the air-bubbles vary directly with the fineness of the grain.

Scoria of all kinds are sometimes broken, or even ground into dust, by the collisions, it is here supposed, which they undergo in the air; and different degrees of fineness give lapelli, pozzolana, and volcanic sand and ashes.

The other class of fragments consists of pieces of the hard rock broken from the sides of the fissure by the force of the explosion; and these, of course, present great diversity of character, because they may belong to many different strata. These fragments have generally undergone considerable changes during their passage through the fire, or their contact with it. Thus, some are incrusted with lava; others are partially fused or burnt: while, in some cases, they are supposed to have been more materially altered in character by a re-combination of their elements. Such fragments are rare, from obvious causes, in ancient volcanoes erupting from old vents. The explosive force is so powerful as to throw such fragments sometimes to the height of 4000, and even of 6000 feet.

The conglomerate structure of the volcanic cone is sometimes irregular; while at others, the different substances are stratified, and in modes successively parallel to the surface of the hill. But it also necessarily happens that this stratification must be often disturbed by commotions within the mountain, resulting from other eruptions, with their consequent fissures and other causes; while further, the fragments are sometimes consolidated into a rock by currents of lava.

The fourth chapter examines the disposition of lava when ejected. Felspar, titaniferous iron, and augite, are considered as the chief constituents of these rocks. Their names are, according to the author, trachyte, phonolite, trachytic porphyry, compact felspar, pitchstone, obsidian, basalt, dolerite, greenstone, basaltic, clinkstone, and gallinace, to which he adds another, called graystone. The specific gravity of felspar to the other substances, is considered as four to five; and all other circumstances being equal, it is presumed that the fluidity will be proportioned to the specific gravity.

The force of expulsion by which lava is projected from the vent is called the ratio of production, "varying directly with the rapidity of the internal progress of expansion through the focus, *i. e.*, with the energy of eruption." And "if this term be fixed, it varies inversely with the fluidity of the lava, since the degree of intumescence by which it is occasioned preserves this proportion." We use the author's own words, that we may convey an idea of the mathematical precision of his language, so superior to any that we could produce.

Thus again: "The external circumstances that affect the disposition of lava upon the earth's surface, are divisible into, first, those which tend to diminish its fluidity; and secondly, those which favour or impede the lateral extension to which it is urged by that fluidity."

Thus, if it is fluid it flows, at least if there are no obstacles to stop it, "until the cessation of the force of expulsion and the progress of consolidation in the mass already expelled gradually diminish its velocity, and finally arrest its further progress." Thus it assumes on cooling various forms, and becomes a sheet, a nappe, or a plateau, or a stream, or a current, or a coulée, or a hummock, or a dome.

If it contains much iron, it has a tendency to spread in sheets, and if the reverse, producing trachyte, it forms domes. And here, the author observes, that geologists have committed a serious error in asserting that the trachytic rocks had not flowed on the surface, but had been elevated in the forms in which they are now found; equally controverting Von Buch and Humboldt, who affirm that they had been blown up like bladders. Trachyte is, however, found also in sheets; but these are always thick, and have a tendency to form hummocks.

The author having again reminded us, that "the quantity and escaping force of the vapours evolved from lava in ebullition varies directly, and the quantity of lava protruded *en masse* by its internal intumescence, inversely with its fluidity," concludes, among other things, that the trachytic rocks have been elevated by the same causes and in the same manner as basalt, and that the difference of their bulk arises from a lower degree of fluidity arising from an inferior specific gravity.

Here, at page 100, follows a very minute description of the "mode of procedure of lavas in general when poured out upon the earth's surface." Veins or dikes, such as those of the North of Ireland, he considers, have been formed from such currents descending into previous fissures, in some cases, while in others they have been the volcanic vents. And he considers also, that by the friction of the lava on the sides of such fissures, its grain must have been comminuted; by which means we must explain the greater degree of fineness which these possess at their sides. Currents of lava "progress" even after the original vent is closed. And sometimes the lava escapes from the upper crust, leaving a cavern, as happens in Iceland. But we must here refer to the author himself for a variety of curious but well-known particulars, which do not well bear abridgment. We prefer selecting what is new whenever we can find it.

And, therefore, we will here notice that, as the crystals or parts of lava in a current proceed by a "sliding and slipping motion of their plane surfaces over each other, facilitated by the intervention of the elastic fluid, rather than by the rotatory movement which actuates the globular molecules of most other liquids," the crystals of trachyte will arrange themselves in the currents, so as to have their longer axis in the direction of the motion. Thus a very small quantity of vapour interposed between the parallel surfaces of proximate crystals will allow them to glide past each other, while the motion in a lateral direction will become proportionately difficult. We felt it necessary to give this view thus clearly, because we think that Dolomieu and Kirwan had both failed in explaining the cold fluidity of lavas, particularly when at the white heat formerly noticed.

This also, it is important to remark, accounts "for the extreme difficulty with which a stream of lava is induced to alter the direction it has been once led to assume; as it also does for the great perfection of the angles of the crystals; a fact which those two philosophers had equally failed in explaining." In this case, the quantity of vapour which communicated the "sliding or glissant motion" in one direction, may be insufficient to act when the position of the crystals is unfavourable; and the friction from obstacles, explains also why the particles and crystals are finer at the extremity of a current than higher up, or in the middle; a circumstance which others had attempted to explain by imagining a more slow and more perfect crystallization as the necessary consequence of greater fluidity and a longer period of cooling.

The consolidation of lava occupies the fifth chapter.

Lest the author, and we after him, had not clearly explained the state of lava in fusion already, it is repeated here once more, and we shall, therefore, give the explanation. When the component crystals of lava have been disintegrated by the vapour, part of it still remains "imprisoned between the crystalline laminæ," creating, by its elasticity, a sort of repulsion between them, without altering the positions of their axes, the remainder being free, or having liberated itself from the points on which it was first vaporized by displacing the laminæ and disintegrating the crystal, the parts becoming smaller crystals are conformably disposed towards each other, while the liberated vapour remains among them in "irregular parcels." Therefore when the increase of pressure or diminution of temperature arrives, the forces that tend to bring together gain an ascendency over the power of the vapour, or else the forces remain "unaugmented," while the elasticity of the vapour diminishes; and then by the gradual condensation of the fixed vapour, the "disaggregated" laminæ are brought together again, and "without any derangement in the positions of their axis, are re-aggregated into whole crystals precisely such as they were before." In the mean time the free vapour "allows the approach of the disintegrated crystals," but as their poles do not now correspond, and their proximate surfaces are not parallel, a confused crystallization, or a granitoidal and more or less porous rock is the result: and in case of extreme slowness on the part of the vapour, other kinds of reversal and mobility take place among the crystals, and a partial re-crystallization occurs, from many "particles uniting into larger crystals."

This is, as clearly as we can abridge it, our author's theory, both of the fluidity or liquefaction, and the consolidation of lava, with all their phenomena; while he concludes with remarking how it is confirmed by the experiments of Mr. Watt and Sir James Hall on the fusion and refrigeration of rocks.

The author proceeds to explain the extrication of air, forming air-bubbles in lavas; but as we do not perceive any views of the same novelty and interest in this particular portion, we shall refer our readers to the original, and take the liberty of passing it over; and for the same reason we shall refer them to the book itself for the account of sublimation and amygdaloidal infiltrations, as well as of hot springs; all of them being very interesting facts connected with this curious subject.

The sixth chapter treats of the divisionary structure of lavas, which, in a broad view, is attributed to contraction from the escape or consolidation of the included vapour, and to the establishment of certain centres of attraction. Here, too, we must allow the author, in a great measure, to speak for himself, by a reference; as the various detailed views under which he has handled the subject, would scarcely receive justice in any abridgment that we could here give. The divisions which he has established, however, are thus termed, first, the prismatic; second, the tubular, lamellar, and schistose; third, the rhomboidal and cubiform; fourth, the globiform; fifth, the angulo-globular.

The causes which give rise to the different mineral compositions of different lavas are also stated in this chapter; and the author is rather inclined to consider them as arising from circumstances taking place in the erupted lava itself, than from any original difference in the rocks by the fusion of which it was formed. Thus, a mass of granite, under particular circumstances, might form a basalt; while it is esteemed a fact in support of this opinion, that very different lavas are produced from the same vent. Here the author quotes many references, and as we have not remarked it before, we must not let this pass without saying that wherever there are facts of this nature required, the references are sufficiently numerous for any useful purpose.

The seventh chapter treats of volcanic mountains; and though we might, perhaps, esteem it, in some measure, a repetition of much that has gone before, as had also struck us in the previous parts of the work, we have no doubt that the author has perfectly good reasons for his arrangement, and as little doubt in accusing ourselves of want of care and attention in examining the valid causes which have led to this mode of disposing his materials.

Mr. S., like preceding writers, attributes the well-known circular lakes found in various places, to the destruction of the upper parts of volcanic cones by eruption, and to the consequent lodgment of water in the crater, or to the action of an eruption on the sides of the fissure through which it has made its way; which last process, in particular, he dwells upon with some detail; and he also thinks that such lakes, formed in volcanic craters, may, by their pressure, have burst through the sides, and thus have been one cause of the demolition of volcanic cones. The aqueous eruptions of South America, and the torrents of fish, are, of course, enumerated among the rest of these phenomena. Those waters which he calls eluvial eruptions, necessarily entangle and carry down stones and mud, forming deposites, which he calls eluvial, and which sometimes are so consolidated as to produce rocks. In these rocks the "divisionary" structure takes place, and he considers that to be regulated by the same laws as the similar structures occurring in lavas or rocks which appear to have been in fusion; but which, of course, according to the proofs which he has given, are only rendered fluid by being suspended in hot water and vapour, being, like the others, but mud; red-hot mud, would, we suppose, express his meaning: but we must refer our reader to this chapter itself; as we fear that we at least should be guilty of repetition, if we were to attempt an abridgment of it, and thus, perhaps, appear to throw blame on the arrangement.

The eighth chapter on subaqueous volcanoes follows. The phenomena have been so often described, that we need not repeat them here. The most important new conclusions drawn are, first, "that when the expansive force of the confined lava has, at length, overcome its antagonist, the tension of the mass, and its temperature, must be proportionately intense;" and, second, "that the vapour which escapes from the lava, owing to its excessive tension, will be speedily refrigerated," *&c.*; in short, that the cold ocean will condense the hot steam; though we must beg the author's pardon for substituting such vulgar language; and, therefore the eruption will be invisible, till the erupted matters are elevated above the surface of the water; while "the mineral and saline compounds which, in greater or less quantity, always accompany the aqueous vapour evolved from a volcanic vent, will, on the condensation of this vapour, mingle with the waters of the ocean, and add to the ingredients of the same nature with which it is impregnated." We think it extremely probable.

With equal accuracy it is concluded, that some of the fragments will accumulate round the vent and form a cone, while those which are "scattered to any height by the gaseous explosions, and particularly the lightest and finest of the fragmentary matters," will render the ocean muddy, or "turbid," and finally be deposited so as to form "sedimentary strata;" and thus, as pumice floats on water, it may be driven by winds and currents, and deposited at distant points. Also, if the waters are impregnated with calcareous matter, these tufas will have a calcareous cement; and those which form a thick paste, will produce a tufaceous stone more readily than the more dispersed floating dust.

As to the subaqueous lavas, "they will spread laterally beneath the cover of a scoriform envelope, with a rapidity and to an extent proportioned to the propulsive force, their fluidity, and the permanence of that fluidity, and the accidents of level in the surrounding surfaces." The author also considers, that submarine lavas will preserve their fluidity longer than subaërial ones, in consequence of the superincumbent weight of water preventing the escape of the interior waters, which he has already proved to be the cause of its fluidity; and, showing that the extension of such beds of lava must be proportioned to the depth of ocean above, he thus accounts for the great extent of the generality of flötz-trap formations. This pressure explains two other phenomena occurring in these rocks; first, why they possess no air vesicles; and, secondly, why they do possess them, as in the case of the amygdaloids. The first consequence arises from the "instantaneous consolidation" of the surface of the lava, aided by the superincumbent pressure, preventing the escape of the vapour bubbles "by the ascending force of their inferior specific gravity," a great cause of "perplexity to geologists." In the second consequence, vesicles must be expected in the interior of the rock "whenever its liquidity was sufficient to permit the agglomeration of the vapour into parcels; the extreme tension of the elastic fluid causing the expansion of the bubbles as the lava flows on," while, from the consolidation and pressure, as before, few can escape by "rising outwardly."

The author notices, of course, how volcanic cones, at first merely eruptive under the sea, may be raised above it by the elevation of the strata beneath; and this portion of the chapter is accompanied with a list of useful references, for which we must, ourselves, refer to it.

"Systems of volcanoes" forms the title of the ninth chapter; and this is assuredly one of the most interesting departments in the history of volcanoes. In this case, also, the linear arrangement is a very remarkable fact; and as the author's collection of examples is too long for us even to abridge, we must again refer to the original, which is accompanied by a sketch. It is probable that the open volcano, on any one line of extinct ones, is a spiracle to a large portion of the internal fissure; and a casual obstruction of it thus leads to earthquakes and new eruptions. Safety-valve, our author's term, is very explicit.

In Venezuela and the adjacent country, after each set of earthquakes, fissures are produced in the rocks, and after each the levels are observed to be raised. And reasoning on the probability, that the "transmission of caloric," as the author expresses it, from the centre to the circumference of the globe, is coeval with creation, he concludes, as others have concluded before him, that the phenomena of elevated and fractured rocks are thus explained; or that volcanoes are but the continuation of that force, now operating partially, which once, more energetic and acting through longer periods, has elevated all the mountains of the globe.

Another general conclusion is rather more purely the author's own. It is this, if we can contrive to explain in a few vulgar words the matter of pages, that wherever the volcanic power has found vent, there the strata are least elevated, and that, reversely, where it has not escaped, we find the highest mountains. For the proofs themselves, being given at some length, and not a little controverted, and again answered by the author himself, we *must* refer once more to the book, as they are far beyond abridgment. But considering that he has demonstrated it, he concludes, as others also have supposed, that, as he expresses it in his own peculiar language, "a subterranean bed of intensely heated crystalline rock, from whose gradual increase of temperature and consequent expansion" "volcanic phenomena arise, must extend generally beneath the surface of the whole globe." And the successive expansive shocks of this subterranean bed, which have elevated the continents, incidentally gave rise to contemporaneous eruptions, or "partial outward intumescences of this matter" on the prolongation of the elevated strata, "more or less distant from the line of maximum elevation, *&c.*

Now, we presume, we have arrived at a general theory of the earth, in the tenth chapter, though that title does not occur till some time afterwards. If it is not very new, it is so much the better, because it will have the support of other opinions. The crystalline rocks, granite, *&c.*, intumesce, or are forced upwards, either in a solid or fluid state, by the internal expansion; breaking the superincumbent strata, and producing fractures, elevations, and so forth. And when the crystalline rock has passed through the strata, we have granite summits; and when it has not, why then denudation wears away the strata at the summit, and the crystalline rock becomes exposed just as well as if it had passed through.

A detailed account of flexures and fissures, and of all the possible and probable modes in which these have been produced, and varied, and modified, is also given; but those who wish to see how the author has treated all this matter must consult the work; as whatever appearance of variety in the explanations, as compared to former writers, might appear in consulting the book itself, any abstract of it that we could make would appear a mere repetition of matters a thousand times discussed, and thus, perhaps, diminish whatever credit may be due to the present writer for his industry, ingenuity, language, or whatever else. Similarly, it is here shewn, how valleys, as well as ridges, must have been produced by the elevations of the strata, how the retiring waters must have acted on them so as to widen them; and, moreever, how such waters may have produced valleys, even if there had been no previous channel of this nature prepared for them. And also, the author shews that other valleys have been excavated by the slow action of waters, " among which, the fall of water from the sky, and its abrasive power as it flows over the surface of the land from a higher to a lower level is the principal."

And here, also, he promises, in a future work, to prove that the quantity of water thus flowing and making valleys is much less now than it was once, and that the waters of the sky or the earth are diminishing; a theory, however, which is not much newer than what we have already quoted, but of which, nevertheless, we shall be very well pleased to see his new proofs.

As to the period of these continental elevations and other phenomena, it is left in doubt; but the author considers that the elevation of the " Colossal European chain, and perhaps, therefore, of the whole of Europe," " took place at a comparatively recent geological epoch." We do not, however, discover to what the word " comparatively" here alludes, so that, on this head, perhaps the author has not yet completed his investigations. Here also he insinuates in a note, that the elevation of the mountains may have had something to do with the near approach of " an erratic planetary body or comet;" as this would act in a barometrically inverse manner by diminishing attraction, and thus allow the interior rocks to expand. Here also he considers that granite was the " original, or mother rock," converted by circumstances into all others. " A great degree of comminution, occasioned by the friction of the crystalline particles on one another, may have sometimes reduced the granite to a porphyry, and so on."

And thus having disposed of the crystalline rocks, we proceed to the strata and their origin. Here we are in the same dilemma as before, on the subject of abridgment. If we refer the readers to the book, the author will receive the credit which he deserves for a theory of strata, as of unstratified rocks, which is doubtless his own discovery, deduced from his prior reasonings on volcanoes, because he has not borrowed it from prior authors on the same subjects, and arguing highly in favour of his ingenuity. Whereas, if we attempt an analysis of it, we fear by using language of that ordinary kind to which our ears have been so much accustomed, it might lead to the supposition that there was really no novelty in the views, either of this or the preceding chapter; which would be an injustice, since the originality of the views cannot be questioned, when the author announces " a few theoretical opinions naturally suggested by the phenomena he has been investigating;" as we just remarked.

However, we must attempt it. Whenever a portion of the globe was elevated by the intumescence of the crystalline rocks, the waters raised by them must have possessed an abrasive force, wearing away the surface and depositing the detritus at a lower level. " But this is not all; for by the absolute elevation of such a mass, the radius of the globe is dilated at this point," and a proportionate body of water flowing to the opposite extremity, to restore the equilibrium, produces an " antipodal tide" and " oscillatory movements." And these waves will wear away the rocks also: and thus denudations were produced; and thus great fragments will have been left in the longitudinal valleys, while the finer detritus would be carried further on. And as the finest sedimental deposits subsided, bituminous, vegetable, animal, and other substances would subside also, and concretionary attraction would divide them into strata, and so on.

" The water of the ocean has two sources," land springs and rain; which last, also, has two sources, the one in common evaporation, and the other in volcanic vapours. And these carried into the ocean, originally, mineral matters; while pressure, the heat of the ocean, and all the other circumstances, discovered by the author, which we cannot detail, will assist in accounting for the consolidation of strata, and so forth. And the author is " of opinion" that the primitive ocean, " locally supersaturated with silex or carbonate of lime, was sufficient, with the aid of consolidation and pressure, to explain the formation of crystalline rocks of these two characters. Mica was sedimental or crystallized in such rocks as mica slate; and asbestos, epidote, and garnet, were produced by the "capricious play of the forces of chemical affinity."

The theory of gneiss is, however, much more unquestionably original. We must imagine, what in fact has been proved long ago in the first part of the work, a general intumescence of an intensely heated bed of granite, forming the original surface of the globe, succeeded by a period in which the predominance was acquired by the repressive force, occasioned by the condensation of the waters on its surface, and the deposition of sedimental strata forming the transition series. This "simply, at once," explains the lamellar structure of the gneiss formation. "And this structure may have been subsequently increased by the friction of the different laminæ against one another, as they were urged forwards in the direction of their plane surfaces towards the orifice of protrusion, along with the expanding granite beneath; the laminæ being elongated, and the crystals forced to arrange themselves in the direction of the movement."

Hence the author proceeds to give what he calls a "conjectural rough sketch of the theory of the globe," which, with the exception of an appendix, terminates the present volume. We were not wrong, then, in considering all that we have already quoted as matter utterly new, since he concludes, that however imperfect this theory may be, it is deduced from the views which he has laid down, and is "more accordant with the general and constant processes of nature than any other which the spirit of geological inquiry has yet started."

The mass of the globe, or at least its external zone to a considerable depth, was originally of a granitic composition with a very large grain.

On reaching its actual orbit, or before, "it enjoyed a great diminution" of the pressure which had previously crystallized, or preserved it in a state of crystallization, at an intense temperature, perhaps as an integrant part of the sun.

Part of its atmosphere was lost in its passage from the sun, on the principle of a comet's tail; and the remainder formed the present atmosphere and the ocean, or the main reservoir of superficial waters.

The rotatory motion and the liquefaction of the surface produced the oblate form. And as the "process of expansion proceeded in depth," the original granite beds were liquefied; "the crystals being merged in the elastic vehicle produced by the vaporization of the water contained between the laminæ." And as the superior pressure forced it out, a quantity of aqueous vapour was produced on the surface of the globe, while its expansion lowering its temperature, it fell back again and produced water.

Thus there were alternations, and consequently rain, and torrents, and many other things; and the waters of the ocean dissolved silex, and such other minerals as water under such circumstances could hold in solution. And then the disintegrated crystals that were suspended subsided; the felspar and quartz first, and afterwards the mica, as being more buoyant, and the pressure produced a parallelism gradually increasing, and thus were formed the gneiss beds.

A period at length arrived when the force of expansion was checked by the gravitating column, and thus it was stopped by this pressure "from progressing further inwardly." The lowest stratum was granite disaggregated, the next was granite disintegrated, reconsolidating into granite, the third became gneiss, and the fourth, zone, consisting of turbid and heated water, containing the other earths and minerals, formed mica and slate, carbonate of lime, and so on. The fifth stratum was aëriform, and formed the atmosphere.

And then as the evaporation of the ocean went on, more precipitations took place, and there were formed the transition formations: and beneath this crust a new process was commenced. The outer zones of crystalline matter were refrigerated, and abstracted caloric from the nucleus; and by some other operations, which, we grieve to say, we cannot abridge, the process of consolidation "progressed downwards" with the increase of the expansive force in the lower strata, and the upper zone of crystalline matter which had intumesced was re-solidified, and the gneiss formation once more was the result. And beneath this, the granite was again solidified and returned again to its former condition; but the increasing expansive force overcame the resistance, and produced fissures, and thus the inferior crystalline zones came out of them in a "solid or nearly solid state," together with the intumescent granite, and extravasations in the form of lavas.

And the reason why the foliated rocks protruded so easily, was their peculiar structure, which allowed them to slide, while the crystals also were elongated in the direction of the motion, as happened in the "pearlstones" of the trachytic formation. And the rapid expansion of the crystalline rock in the fissures while the water was boiling, ground the crystals into powder, and produced "porphyries or serpentine;" while if the "particles were comminuted to an extreme degree by the friction attendant on the intumescence," they may have "effected entirely new combinations," and so produced diallage rock, hornblende rock, *&c.* And when the heat was not sufficiently great to boil the water, the aqueous vapour exuded through the walls, bringing with it silex in solution, together with other mineral and metallic matters sublimed from the lower part of the fissure.

Then all this commotion and breaking up produced the waves as before, which broke up and triturated the projecting angles, and these were again deposited by subsidence. And then the ocean became peopled; and as portions of the strata had been elevated above the waters, vegetables were produced, and their fragments becoming carbonized, were washed into the sea, and became entangled among the subsiding strata, forming coal-beds. Subsequently, the temperature diminished, and the present creation appeared.

The final conclusion (since we fear that we are exceeding our bounds,) is, that the " grand mineral masses of every age, composing the known crust of the globe," are attributable to three primary causes: first, solution of certain minerals in water; second, suspension of fragments of various kinds and sizes; and third, the elevation of crystallized matter by intumescence or expansion, in a solid or fluid state.

And these causes, being perfectly original and new, " have an immense advantage over most, perhaps over all, of the hypotheses that have as yet been brought forward to explain the same appearances, and which speaks volumes in their favour: and this is, that *they are still in operation*—with diminished energy, it is true, but this is the necessary result of their nature."

The author concludes by remarking, that other geologists have erred by considering rather what might be than what is; that the laws of nature are invariable; that other geological theories have chiefly been dictated by a love of the marvellous, as the dark ages had recourse to supernatural causes; that this " wonder-working spirit" had inspired most of the geologists who have hitherto published theories to account for the mineral appearances of our planet; and that though the theory of the globe which he has himself proposed will require " much ulterior developement," " its truth will be established on the soundest possible basis;" because it proceeds by the application of those modes of operation, which nature still employs, and on that law on which all our knowledge rests, " on every subject whatsoever," namely, that similar results always are, have been, and will be produced by similar preceding circumstances.

Art. XIII. MISCELLANEOUS INTELLIGENCE.

I. Mechanical Science.

1. *Effects of an Anti-Attrition Preparation.*—A Munich journal reports the following results relative to the advantage obtained by employing a mixture of hog's-lard and plumbago for the diminution of friction in machines. The composition consisted of ten and a half parts of pure hog's-lard, fused with two parts of finely pulverized and sifted plumbago. The lard is first to be melted over a moderate fire, then a handful of the plumbago thrown in, and the materials stirred with a wooden spoon until the mixture is perfect; the rest of the plumbago is then to be added, and again stirred till the substance is of uniform composition; the vessel is then to be removed from the fire, the motion being continued until the mixture is quite cold.

This composition is to be applied to pivots, the teeth of wheels, &c., in its cold state by means of a brush, and is seldom required oftener than once in twenty-four hours. It was found that the machines employed in certain iron works, cost, in oil, tallow, and tar, as much per week as six florins twenty-nine kreutzers; but, upon replacing these substances by the composition described above, the expense was diminished to one florin thirty-eight kreutzers for the same time, 5 lbs. being required for the week's service. Economy to this extent is not to be neglected in manufactories where many machines are in use.—*Ann. des Mines*, xi. 79.

2. *Strength of Leaden Pipes.*—Experiments upon this subject have been made at Edinburgh by Mr. Jardine at the Water Company's yard. The method followed was to close one end of a piece of pipe, and then throw water into it by a forcing pump attached to the other end, the force or pressure being measured by a gauge belonging to the pump. When the water from the injecting pump first begins to press out the pipe, little or no alteration is observed on it for some time. As the operation proceeds, however, the pipe gradually swells throughout its whole length, until at last a small protuberance is observed rising in some weak part, which increases until the substance of the pipe becoming thinner and thinner, is at last rent asunder, when the pipe bursts with a crash, and the water issues with great violence.

In the first experiment, the pipe was of 1½ inch bore, and the metal, which was remarkably soft and ductile, one-fifth of an inch in thickness. This sustained a power equivalent to that of a column of water 1000 feet high, equal to 30 atmospheres, or 420 lbs. per square inch of internal surface, without alteration; but with a pressure equal to 1200 feet of water it began to swell, and with 1400 feet, or 600lbs., on the square inch, it burst. When measured after the experiment, it was found to have swelled until of a diameter of 1¾ of an inch. The edges of the fracture were not ragged, but smooth and sharp like a knife.

In a second experiment the pipe was two inches in diameter, and one-fifth of an inch in thickness. It sustained a pressure equal to that of a column of water 800 feet in height with hardly any swelling, but with 1000 feet it burst. The fracture here was not so fine as in the former pipe, the metal being much less ductile. —*Caledonian Mercury.*

3. *Method of curing Smoky Chimneys.*—There is a way of building a vent, which was found to succeed in the huts which were erected by the British army in America during the war of the revolution; and even in the underground vents which were built to their tents when out at a late period of autumn, or rather the beginning of winter. In the writer's own house, where the principal vents were altered upon this plan after the house was finished, and in which there have been fires for nine months, the purity and cleanliness of the rooms sufficiently testify its efficacy; but he has a still farther proof in the testimony borne to it by Mr. Elliot, who built the house and made the alterations, and who was so convinced of the improvement effected from what he saw, while the vents were damp, that in the two houses which he has since built in Melville-street, Edinburgh, he has constructed all the vents on the same principle. *The method* is simply to contract the vent as soon as possible, then gradually to widen it for 4 or 5 feet, and then again contract it to the usual dimensions, and carry it up in any direction. No register grates are necessary.—*New Monthly Mag.* xv. 456.

4. *Suggested Improvements in Light-houses.*—Although great improvements have of late years been made through the British dominions upon light-houses, yet it is possible to make further progress in so useful and necessary a building. They may be so constructed as not only to ascertain the situation of head-land, harbours, *&c.*; but also to determine the distance the observer may be from them in the following manner, *viz.*—Suppose the light-house to be erected of a conical form, the great light at the top may have what tinge it shall be thought proper to give it; underneath, at a distance of from 100 to 150 or 200 feet, three more smaller lights to be seen a few leagues at sea. So long as these last-mentioned are not seen, the observer may conclude he is a considerable distance from the light-house; but as soon as any one of them is perceivable, he need only take the angle of altitude between it and the great one, and in a table calculated on purpose

beforehand, he will find the distance he is from the light-house by an easy and expeditious method, sufficiently exact for his purpose.—*N. M. Mag.* xv. 503.

5. *Cuthbert's Reflecting, Compound, and Single Microscope.*—The instrument made by Mr. Cuthbert, of Bishop's-walk, Lambeth, has been described by the Editor of the *Technical Repository*, who gives it high commendation. Having also witnessed the powers of this instrument, we have no hesitation in bearing testimony to its value, as much for its estimated economy and the facility with which any one may use it, as for the faithful and perfect manner in which it exhibits objects.

The reflecting part of this instrument consists of a concave elliptic speculum, of six-tenths of an inch focus, and three-tenths diameter, mounted at the farther end of a short conical tube, which is screwed into the cylindrical body of the microscope; near to the concave speculum an aperture is made in the side of the conical tube, through which the image of the object to be viewed is received upon a very small plane diagonal speculum, and by it reflected into the concave speculum, from whence it is transmitted through the body and eye glasses, to the eye of the spectator in a greatly magnified state. Upon the conical tube a triangular bar is fitted at right angles to it, having a sliding carriage upon it, with a very fine rack and pinion to adjust it accurately: and to the carriage a stage, fitted up with a springing plate to hold the sliders, with objects in them to be viewed, can be adjusted; or the stage can be removed, and an improved forceps substituted in its place. Upon the triangular bar an illuminating concave speculum, mounted in a swinging adjusting frame, can be slided when required to concentrate the light in viewing transparent objects. And also upon the conical tube, an illuminating lens, mounted in a swinging frame can be fitted, to increase the light in viewing opaque objects; and which, as the object, however highly magnified, never approaches nearer to the conical tube than the tenth part of an inch, can be readily effected.

The cylindrical body of the microscope is received into the cleft-jointed socket of the usual telescope-stand, fitted up with a binding screw, mounted upon three folding legs, and furnished with a turning joint for changing the position of the body from a horizontal to a vertical one, or any intermediate one at pleasure; the joint is also mounted upon a cylindrical tube, sliding in another to which the legs are screwed, thus permitting the body of the instrument to be elevated or depressed, or turned in any horizontal or other direction at pleasure; and as it can also be turned round in the cleft-socket, so it admits of every possible variety of position; and can either receive the direct light of the sun, day-light, or of a lamp or candle, as may be eligible, or their reflected light from the speculum.

The magnifying power of this reflecting microscope is considerable, and it is varied and very greatly increased by applying different eye-glasses, as in the astronomical telescope; and, therefore, to render the instrument also equally useful for lesser magnifying powers, Mr. Cuthbert has contrived to remove the reflecting part from the body of the microscope, and to substitute for it a short tube, into which the usual buttons with object-glasses can be screwed, as in the common compound microscope; the adjusting stage, and forceps, and the illuminating speculum being also equally applicable to the microscope in this state; and to render the microscope an universal one, he has fitted a ring with a screw inside of it to the triangular bar, and the latter to the body of the microscope, and he thus converts it into a single microscope, or megalascope; the buttons with the object-glasses of the compound microscope screwing into the ring, or lenses of less magnifying powers being substituted in place of them.

The powers of this instrument, when applied to difficult *test-objects*, were very evident; the minute ribs on the feathers of a moth's wings were shewn with the utmost accuracy and detail, as were also the lines upon the coloured scales of the diamond beetle, by which the colours are produced. In the hair of a mouse also, where usual microscopes only shew dark-coloured spots, longitudinal wavy lines or furrows were most distinctly visible.—*Tech. Rep.* viii. 285.

6. *On the alteration in the bulk of successive portions of Air introduced into a Torricellian Tube.*

To the Editor of the *Quarterly Journal of Science.*

Sir,—Wishing to know the effect on the power of a vacuum of introducing into it certain portions of air, and not being able to meet with any information on the subject, either in the books I consulted or from several men of science with whom I conversed on the subject, I made the following experiment. As its result was different from what was anticipated, I think it may be useful to others, and trouble you with this communication, with a view to its publication in the Journal so ably conducted by you, if you think it deserving of a place in it. The experiment being made for a practical purpose, no more accuracy was attempted than was necessary for that purpose. The following statements, however, do not deviate much from the truth.

A tube about thirty-two inches long was filled with mercury, and inverted in it; the mercury in the tube standing at twenty-nine inches above the surface of that in the basin, I introduced into the former a certain portion of air, having previously ascertained, by means of water, what space in the tube was occupied by the contents of the measure I employed. The air, of course, expanded as it rose through the mercury, and at the surface it occupied about four and a half or five times the space it occupied under the whole pressure of the atmosphere. It is to be recollected, it was still exposed to some part of this pressure, the mercury having descended several inches: one portion of air being introduced after another, it necessarily occupied less and less space as the mercury descended. When the mercury had descended to about eighteen inches above the surface of that in the basin, the addition which the measure of air made to the space between the mercury and the upper end of the tube was only about equal to the bulk of the air before its introduction. When the mercury had descended to fifteen inches, this addition was one-fourth less than the bulk of the air before its introduction; and when the mercury had descended to about five inches above that in the basin, the addition was only about one half of this bulk. The cause of this apparent shrinking of the air introduced, is evidently the increased condensation of the air previously in the tube, in consequence of the descent of the mercury exposing it to a greater degree of atmospherical pressure; but I had expected to find that the result, on the whole, would have been expansion, not contraction.

I wished to ascertain what was the actual expansion of the air introduced when the mercury stood at fifteen inches; for this purpose, I introduced the same measure of water; this I found enlarged the space above the mercury by one-half of the bulk of the water. Here the degree in which the air in the tube contracted, was pretty nearly the same as when the air was introduced; the mercury standing only about a fifth or sixth of an inch higher than in the latter case. If we overlook this slight cause of greater compression, we shall have the following statement:—the contraction of the air previously in the tube equal to half a measure; the additional space above the mercury, after the introduction of the air, three-quarters of a measure. Thus, it would appear, that when the air is relieved from about one-half of the pressure of the atmosphere, it occupies only about one-fourth more space than under the whole of its pressure. I am, Sir,

Your obedient servant, A. B.

We do not understand our Correspondent's conclusion.—Ed.

7. *On the Attraction manifested at sensible Distances by solid Surfaces moistened by, and immersed in, a Liquid. By* M. P. S. Girard.—Experiment has long since proved that the surface of certain solid bodies may be moistened by liquids which have not the property of moistening the surfaces of other bodies. Thus, mercury, which has the power of moistening many metals, and adhering to their surface, has no such property with regard to glass, wood, and many

other substances. It is equally known, from experiment, that the surface of the same solid body susceptible of being moistened in succession by different liquids is, in similar circumstances, variously wetted, according to the nature of the liquid: thus, at the same temperature, a plate of glass wetted with alcohol will retain a thicker layer of the fluid on its surface than it would have done if wetted with water.

This property, possessed by solid bodies of retaining on their surfaces a stratum more or less thick, according to the fluid by which they are wetted, is particularly evident when the solid is reduced to the state of fine powder, and is diffused through the liquid in such quantity that, by their vicinity, the atmospheres adhering to them mutually penetrate each other. Experiment then shews that through the intervention of these atmospheres, the particles tend to approach each other with a force which is greater, as the particles are nearer to each other. Hence mutual actions and re-actions, which transmitted to the interposed fluid, subject it to new forces and pressures, of which the intensity may be appreciated by means of the hydrometer.

I have shewn, in a preceding memoir *, that the degrees of this instrument then differed, and should differ, from those indicating merely the specific gravity of the mixture of fluid and solid matter. In the latter case, the specific gravity, indicated by the instrument, expresses merely the force with which eacd molecule of the mixture considered as homogeneous gravitates towards the centre of the earth; whilst, in the first case, the instrument not only indicates the specific gravity of the liquid interposed between the solid particles, but also the force with which the liquid gravitates on these molecules.

Although the experiments by which these facts have been determined leave no doubt, yet their importance, and the importance of their consequences is such as to require, if possible, other proof; I have, therefore, endeavoured to render the attraction exerted through the intervention of a liquid sensible upon large surfaces, and, if possible, to measure rigorously the intensity of this action with reference to the distance at which it is exerted.

If two plane surfaces be imagined, suspended vertically in a liquid by which they are wetted, the stratum of liquid adhering to them will form on each a kind of fixed envelope. If, then, the surfaces be approximated to each other until the distance between them is so small as to allow of their liquid envelopes penetrating each other, these surfaces should mutually attract each other with a force increasing as the distance is diminished. To render sensible, and appreciate the effect of this force, conceive that moving the moistened surfaces from the perpendicular position in which

* On Liquid Atmospheres, and their influence on the solid particles they envelope.—*Mém. de l'Académie Royale,* iv. 1819 and 1820.

they are supposed to be freely suspended, they are made to approach until within a determinate distance of each other; if this distance be greater than the double thickness of the liquid layer adhering to each, the two envelopes will be merely placed opposite to each other, but will not mutually penetrate; and then the two moistened surfaces obeying the gravity which they retain in the fluid will, like insulated pendulums, re-assume the vertical position from which they have been moved. Now this will take place in a certain interval of time depending on the length of the thread or wire by which the surfaces are suspended, and the resistance which the fluid opposes to their motion. It is also evident that, on abstracting this resistance, which can always be done when the motion of the pendulum is extremely slow, the duration of the oscillations will be the same, whatever the interval through which they have been moved from the perpendicular.

But if, assuming another case, the two solid surfaces immerged are brought so near to each other that the fluid layers which adhere to them mutually penetrate, they will then mutually attract each other; the action of their weight in the liquid will be in part counteracted by this attraction; and when they are left to themselves, the time they will require to attain the original perpendicular situation, *i.e.*, the duration of the first half oscillation, will be the greater as their mutual attraction increases, or as they are placed at a smaller distance from each other at the commencement of the oscillation.

The effort of gravity on the moistened surfaces may be rendered feeble to any degree, either by giving to the surfaces in one way or another, a specific gravity, but little different from that of the liquid in which they are immersed; or in diminishing the angle formed by the suspension wires when they are removed from the perpendicular, and made to approach each other. Then by interposing and fixing between the surfaces a metallic wire of a certain diameter, and by means of pressure, bringing the surfaces in contact with the opposite sides of the wire, it is evident that the diameter of the latter becomes a measure of the interval between the surfaces. If, then, the surfaces be left to the contrary actions of their mutual attraction, and their gravity in the liquid estimated parallel to that attraction, the duration of their first oscillation will necessarily be a certain function of the difference of these contrary forces. If then this duration be observed whilst the forces are varied, *i. e.*, in varying either together or separately the diameter of the interposed metallic wire, and the amplitude of the movement of the surfaces, the observations will indicate the mutual variations of the distance of the two surfaces at the commencement of the oscillation, the amplitude of the oscillation itself, and also its duration.

M. Girard then proceeds in his memoir to describe an apparatus constructed with great care according to these views.

The surfaces he used were of glass; each was attached to a disc of cork, so as to render the whole of one mass or piece, of a specific gravity very little greater than that of the water in which they were to be immersed, and the prisms formed in this way were each accurately of the same size, weight, form, and surface; they were suspended by silk threads so as to form two pendulums, the weight of each being in the water 1 gramme (15.44 gr.). The thickness of each pendulum was 2 centimetres (.787 of inch), and the surfaces of glass opposed to each other, 10 centimetres (3.9371 inches) long, and half that height. The length of the suspension threads was 18 centimetres (7.1468 inches). It is not our object here, however, to describe the apparatus, but simply to give a view of the author's expectation, and generally of the results he obtained. Operating with his apparatus, he found that when the surfaces were at any greater distance from each other at the commencement of an oscillation than 2½ millimetres (.0984 of inch,) the time required for the first half oscillation was the same, amounting to 7 seconds. Hence it followed, that at this distance the two liquid layers on the moistened surfaces did not penetrate each other, and that therefore the thickness of this layer must be less than $\frac{5}{4}$ of a millimetre (.0492 of inch). As, however, the object in view was general confirmation rather than particular estimates of the strength of action, this point was not pursued, and M. Girard went at once to distances so small as to be within the limit of interference. These distances were five in number, measured by the diameters of five different wires used; the smallest being .0563, the second .1127, the third .1579, the fourth .1917, and the largest .2481 of a millimetre in diameter. Then the gravity of the pendulums exerted in a direction opposite to their attraction, or in a horizontal line, was varied by moving the points of suspension to different equal distances on both sides, from the points at which the pendulums would have hung when just in contact. The following is part of a table expressing the results. Nos. 1, 2, 3, 4, and 5, are the wires, or the distances between the surfaces at the commencement of the oscillation. The figures in the first five columns express the gravitating power, in grammes, opposed to the attraction, and tending to separate the surfaces; and the figures in the last five columns express the time, in seconds, required in the corresponding experiments to complete the first half oscillation.

No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
0.02763	0.02747	0.02740	0.02725	0.02710	832″	585″	380″	273″	163″
0.05544	0.05532	0.05520	0.05510	0.05495	440	261	217	128	91
0.08386	0.08319	0.08318	0.08313	0.08286	296	177	145	79	64
0.11164	0.11154	0.11136	0.11133	0.11416	225	131	110	56	51
0.14023	0.14022	0.13980	0.13972	0.13956	184	106	90	44	37

In looking down the last five columns, it will be readily observed how the time diminishes as the separating force of the pendulums (expressed in the first five columns) increase, the original interval between the surfaces being the same; this is a natural consequence of known mechanical laws. But in looking along them horizontally, it will be observed how rapidly the times of the oscillations diminish as the original interval between the surfaces, as measured by the intervening wires, is increased, although the separating power is but very little altered.

Without following M. Girard further, we may at once quote his conclusion, and refer our readers for further details to the memoir itself, in vol. xxix. of the *Annales de Chimie*. "Those surfaces which, being entirely immersed in a fluid, are susceptible of being wetted by it, when brought sufficiently near to each other to allow of the mutual penetration of their moistening layers, exert an attraction on each other by means of the interposed liquid, at distances which are sensible and capable of accurate measurement, and which increases as the distance is diminished."

It appears that attention to temperature is of great importance in these kind of experiments. M. Girard found that other circumstances being equal, a variation of temperature from 5° to 20° centigrade (40° to 68° F.) caused a variation in the time from 783″ to 520″ only.

8. *On an unnoticed Mechanical Principle—Explanation of the cutting of Steel by Iron.*—The observation made by Mr. Barnes, in America, of the action of soft iron upon steel, has called forth the experimental remarks of many persons, and numerous explications of it have been given, none of which, however, have been satisfactory to all persons.

The following is an abstract of parts of a paper by M. Allou: it has been inserted in the *Bibliothèque Universelle*, xxix. p. 192. It is remarked, that if the disc of iron be moved with a velocity continually increasing, it is at first acted upon by the steel plate or piece presented to it; with a certain velocity, no action either way takes place; and with a still greater velocity, the steel is cut by the iron. In explanation of this fact, M. Allou states, that if two bodies equally hard and elastic, such as two balls of ivory, strike each other with velocities nearly equal, each of these will experience a similar blow, and the change of form which results is instantly corrected by the elasticity of the bodies. But if the first is moved with a velocity incomparably greater than the second, and if the latter, though harder than the former, is susceptible of being cut or traversed, it will suffer either the one or the other effect, *without receiving any sensible velocity*, all the action impressed upon it being employed in *piercing or penetrating* the ball or body struck.

In support of this *principle* are cited various well-known effects, amongst which are, the penetration of an open door by a candle shot from a gun, no motion being communicated to the door; the legerdemain trick of breaking a stick resting on the edges of two glasses filled with water, by striking it sharply in the middle with another stick, the glasses not being moved or the water spilt; the cutting off of twigs and flowers in a garden or the fields, by a sharp blow on the stems with a light switch; the separation of a snake into portions when struck with a hazel-tree rod; the mode of blasting introduced by the engineer Jessop, in which, after the introduction of the powder into the hole, the upper part is filled merely by sand: in all these cases M. Allou considers the effect as due to the *want of reaction* in the passive body, as regards the attack made upon it by the active body. "All bodies resist motion," says M. Francœur, in his *Treatise on Mechanics*, "and it is in resisting that they receive it. Those which are instantaneously pierced or destroyed, not having offered appreciable resistance, should not therefore receive motion, and this is confirmed by experiment." Speaking of the branches, flowers, and the snake, cut in pieces by the switch, "the shock is so rapid and unexpected, that the muscular fibres of the reptile, as well as those which form the tissue of the flowers and branches, *have not time to react.* Now it is this want of reaction which constitutes the phenomenon;" and in the experiment of Mr. Barnes, "is it not natural to think that the shock on the steel by the cutting edge of the disc is so sudden and unexpected, that the molecules of the first *have not time* to react on those of the latter, and are thus rapidly removed at each contact."

Other facts of a similar nature with the former are then cited in support of this view of the matter, but they add no new proof.

The author thinks that the mechanical principle of which the phenomena described are the effects, has not had sufficient importance attached to it. We may be permitted to observe that we think if any thing else was intended in the explication than was previously well known, it has been left in too obscure a state to persuade us of its importance, or indeed of its accuracy, It seems difficult to comprehend how, when two similar bodies meet, they should possess different properties dependent *merely* upon the superior velocity of one over that of the other; and taking the cases of the two ivory balls, we doubt whether any difference would be observed in the effects occasioned by their coming together with a certain great velocity, whether the one or the other ball had all the velocity, or whether each had half. Again, if in all the cases quoted, including the curious experiment of Mr. Barnes, of cutting steel by iron, things could be changed, so that the body in the experiment actually in motion could be at rest, and the body previously at rest could have the motion transferred to it, would not the effects be the same? If in place of making the iron disc revolve and holding a steel file to it, the disc were at rest and the steel file revolving with equal rapidity round it, the same part being always in contact with the disc, is there any reason to expect a different result than that now obtained? We think not; and, indeed, going at once to the principle sought to be established, we cannot think that action and reaction are distinguished from each other by any difference in the times which they respectfully require, or that where there is time for the one there is not time for the other.

9. *Magnetic Rotation.*—M. Arago's beautiful experiment is now well known, and, as it deserves, attracts attention every where The following are some results obtained by MM. Prevost and Colladon, which, as they vary slightly in certain points from those as yet published in this country, will be interesting to such as pursue this branch of science.

A disc formed of a thick copper wire rolled in a spiral, produced much less effect than a perfect disc of the metal of the same weight and size.

A disc of glass covered with lead, or a single leaf of tin glued on to wood, sensibly deviated the needle. Wood alone, or sulphur, or a disc of peroxide of iron, had no appreciable effect.

A disc of hammered copper deviated the needle more strongly than the same disc annealed.

A screen of copper, or copper and zinc interposed, diminished the effect without destroying it. The diminution was greater as the screen was thicker, or placed nearer to the needle. A screen of glass had no influence. If the interposed metallic screen were pierced by an aperture equal in diameter to the length of the needle, its effect was very nearly the same.

A vertical magnet suspended in the centre of a cylinder of copper remained unmoved, whatever the direction or rapidity of rotation of the ring.

When two needles were fixed together in a similar direction, the effect increased; when they were placed with their opposite poles together, it ceased entirely.

A needle magnetized, so as to have similar poles at its two extremities, was the apparatus most sensible to the motion of the discs. It was one of this kind which the authors used in their delicate experiments.

The conclusion arrived at by MM. Prevost and Colladon is, that the effects are due to a transient magnetization of the discs, which, not being able to modify itself with a rapidity proportional to that by which the different points of the disc are displaced by rotation, are transported to a small angular distance from the needle before they are changed, and draw it after them. This is the same explanation in effect as that of MM. Herschel and Babbage.

Experiments made with care to determine the influence of the velocity and the distance of the discs, indicated that the angles of deviation, and not their sines, augmented proportionally with the velocity, at least, within certain limits, and that the sines of the angles of deviation increased in an inverse ratio of the power $2\frac{2}{10}$ of the distance. They were careful to employ, in this determination, discs having diameters very great in comparison to the length of the needle.—*Bib. Univ.* xxix. 316.

10. *On the Formation of a Society for the Cultivation of Naval Architecture.*—It is remarkable in a country which owes its opulence to commerce, and its political power to its naval superiority, that no association among the learned should exist, for the purpose of cultivating, in an especial manner, the important subject of naval architecture; and the circumstance is the more to be wondered at, from the establishment of philosophical societies forming so striking and peculiar a characteristic of the age.

There are so many advantages to be derived from the co-operation of numbers in the prosecution of scientific pursuits,—advantages first pointed out by the prophetic mind of Lord Bacon, and which experience has in every way confirmed, that it would be regarded as an unnecessary waste of words, to attempt, at the present time, to advance any new arguments in favour of a system, which has tended in so splendid and triumphant a way, to ennoble and dignify the exertions of man.

At the present moment, besides the Royal Society, which may be said to embrace the whole range of the physical sciences, we have societies devoted to the exclusive cultivation of particular departments of natural knowledge, such as the Geological Society, the Astronomical Society, &c. Now there is perhaps no subject which requires more essentially the aid and co-operation of numbers than naval architecture, involving as it does, so extensive a field for inquiry, and so beset as all its elements are with difficulties of so peculiar and intricate a kind. At the present moment there is a spirit of inquiry abroad respecting ship-building, which no antecedent period ever exhibited; and which, if taken at the flood, and before the causes that have awakened it again subside, must produce consequences of a very important kind. What seems to be wanting, is a sort of focus, or common point of union, to rally the disjointed and insulated speculations now afloat respecting it, and to concentrate the efforts of those who feel interested in its advancement. This might be most readily and effectually done, by instituting a society; the object of which should be, to encourage theoretical and experimental inquiries, connected with naval architecture, and to publish from time to time, in its transactions, such papers of approved merit, as might be laid before it at its meetings,

There is not time at the present moment, to go into the subject with all the generality which its merits demand; and the hint is thrown out in a crude and imperfect state, for the consideration of those, who, from their talents, and the interest they feel in the subject, would be likely to promote the formation of a society of the kind.

December 1*st*, 1825. Z.

II. Chemical Science.

1. *Influence of Solar Light on the Process of Combustion. By* Dr. T. M'Keever.—Induced by the general opinion prevalent in this and other countries, respecting the power of sun-light in diminishing the combustion of a common fire, Dr. M'Keever instituted a set of experiments on the actual rate of combustion of well-known bodies in the bright rays of the sun, and in dark places; they were made during the past fine summer.

Exp. i. Two portions of green wax taper, each weighing 10 grains, were both ignited at the same moment; one of them I placed in a darkened room; the other I exposed to broad sunshine in the open air: thermometer in sun 78° F.; in room 67° F., loss as follows *:—

In five minutes, that placed in sunshine, lost	.	8½ grains.
" " darkened room, lost		9¼ "

Exp. ii. Again, two portions of taper, each weighing 23 grains:

In seven minutes, that placed in sunshine, lost	.	10 grains.
" " darkened room, lost		11 "

Exp. iii. A common mould candle, fourteen inches in length, and three in circumference, was accurately divided into inches, half inches, and eighths, and exposed, in the first instance, to strong sunshine: thermometer 80° F.; atmosphere remarkably calm.

To consume one inch, it took	. .	59′	0″
In darkened room (temp. 68° F.)	. .	56	0
In ordinary light of day (temp. 68° F.)	.	57	10

Exp. iv. A piece of taper 7 inches in length, and six-eighths of an inch in circumference, was carefully divided into inches; and, as in former experiments, submitted to bright sunshine; thermometer 79°.

* In all these experiments the snuff was carefully removed with a sharp scissors, whenever a quarter of an inch of taper was consumed.

To consume one inch, it took . . .	5′	0″
Transferred to darkened room (temp. 67°)	4	30
In ordinary light of day (temp. 67°) . .	4	52

Exp. v. In order to vary the experiment, and guard as much as possible against the agitation of the surrounding atmosphere, two lanterns were procured, one of them was coated with black paint, the other left naked. In these were placed two portions of taper of precisely equal weights, and both were then exposed to a strong glare of sunshine.

In 10′ that placed in painted lantern, lost .	16.5	grains
,, ,, uncoated lantern, lost .	15.	,,

The diminished rate of consumption in both was probably owing to the want of a free current of air through the lanterns.

Exp. vi. With the view of ascertaining whether similar results were to be obtained with the light of the moon, lanterns were prepared, and the experiment tried, as last described, when a very brilliant moon was shining, but no difference in the loss sustained by the two portions could be perceived.

Finally, Dr. M'Keever constructed an apparatus similar to that used by Dr. Herschel in his researches on the power of the prismatic colours to heat and illuminate, and was thus enabled to carry on the combustion of the taper in any of the coloured rays of the spectrum. A piece of green taper being accurately marked, and then ignited, was submitted to different portions of the spectrum, with the following results:

To consume 2 in. of taper, it took in the red ray	8′	0″
,, ,, ,, green ray	8	20
,, ,, ,, violet ray	8	39
,, ,, verge of violet ray	8	57

Commencing with the violet ray, the loss of one inch of taper was in the following times:

At the verge of the violet ray	4′	36″
In the centre of the violet ray	4	26
,, green ray	4	20
,, red ray	4	16

Ann. Phil. N. S. x. 344.

2. *On the Simultaneous Action of Gaseous Oxygen and Alkalis on Organic Substances. By* M. Chevreul.—The following are some of the facts from a memoir by M. Chevreul, upon the combined action of oxygen and alkali, upon substances unaffected by either agent separately. The experiments were made by pouring mercury into a jar or tube, about a centimetre (.03937 of an inch) in diameter, and graduated into cubic millimetres, until it was nearly full; the organic matter was then introduced, the tube filled with boil-

ing water, closed by a glass stopper, and inverted over mercury. The alkaline solution was then introduced; it should be previously boiled, and if of potash or soda, freed from carbonic acid by baryta water, ultimately the oxygen gas is introduced, and the whole agitated from time to time. The same alkaline mixture, with the exception of the oxygen, is preserved in a neighbouring tube, as a standard, by which to appreciate the action of the oxygen gas. The following are some of the results.

Hematine. The solution of hematine or extract of log-wood of a yellow orange colour, becomes blue by union with potash, which may be preserved for six months without alteration, even in sun-light; but if it be in contact with oxygen, its blue colour instantly changes, and gives place to a reddish yellow. No hematine remains. 0.2 grammes of hematine in 3 volumes of solution of potash, when in contact with 26 volumes of oxygen, absorbed in 10′, 14 volumes of the gas; in 25′, 24 volumes; in one hour 15′, 25.5 volumes; and in 2 hours, 25.6 volumes. Muriatic acid disengaged 3 volumes of carbonic acid, and probably 3 volumes more were retained by the liquor.

The alkaline combination of hematine attracts oxygen with such force, that 0.1 gramme of extract of log-wood dissolved in 2 cubic centimetres of potash water, reduced 25 cubic centimetres of atmospheric air to pure azote, in 12 minutes. It may therefore be used eudiometrically.

The colouring principle of Brazil wood with potash water, forms a purple combination, unalterable in years out of the contact of oxygen; but that gas admitted, is absorbed, the solution becomes of a reddish brown colour, and the substance is decomposed; 0.1 gramme of extract of Brazil wood dissolved in 2 cubic centimetres of potash water, absorbed 7.5 cubic centimetres in a quarter of an hour, and 8.5 in 17 hours. Muriatic acid disengaged carbonic acid; the whole quantity being perhaps 3.5 cubic centimetres.

Cochineal forms with potash water a fine purple solution, sensibly unaltered in a twelvemonth. Contact of oxygen makes it pass to yellow, and the colouring principle is destroyed.

Colour of violets also appeared to act in a similar manner; but it was impossible to free the solution from air, and, consequently oxygen, inasmuch as it would not sustain ebullition.

Flax put into contact with potash water gave colour to it, which, in contact with oxygen, caused absorption of the latter. The ligneous part of the flax did not produce this effect.

Gallic acid. M. Chevreul shewed, in 1814, that permanent gallates of potassa, soda, baryta, strontia, and lime, could not be obtained, operating in contact with the air, because the oxygen re-acted, rendered the solutions blue, green, and purple, and destroyed the acid. Gallic acid forms soluble colourless salts with potash and soda. Insoluble white and crytalline salts with baryta, strontia, and lime. These salts are permanent out of contact of the air, and when decomposed by muriatic acid, yield pure gallic acid and muriates; 0.2 grammes of gallic acid, dissolved in 2 cubic centimetres of potash water, and placed in contact with 21 cubic centimetres of oxygen, became green and absorbed oxygen. After a quarter of an hour the absorption was 7.5 cubic centimetres; after three-quarters of an hour 12 cubic centimetres; in 15 hours 14.5 cubic centimetres. Muriatic acid then disengaged 10 cubic centimetres of carbonic acid, including that held in solution, and the acid liquor precipitated gelatine.

When the alkali is in excess, the colour developed is red, and the absorption of oxygen more rapid and greater. By the successive addition of portions of potash and oxygen to 0.2 grammes of gallic acid, it ultimately absorbed as much as 58 cubic centimetres of oxygen.

The same phenomena took place with the neutral gallate of baryta, or the gallate with excess of baryta.

Ox gall with alkali out of the contact of air, undergoes no change in colour; with access of oxygen the gas is absorbed, and the colour disappears.

Colouring matter of blood and albumen. The colouring matter with potash water absorbs oxygen, and becomes of a green yellow colour; out of contact of air, it was, after six weeks, still of a reddish brown colour. Serum of blood and white of egg also caused absorption of oxygen.

Empyreumatic oil. An empyreumatic oil, from the distillation of a fat body, at first of a light yellow colour, slowly became brown by absorbing oxygen; but mixed with potash, it absorbed oxygen rapidly, and quickly took a deep brown colour.

Lignine Sugar and Starch. The experiments of M. Braconnot * on the action of alkali on lignine, and the production artificially of ulmin, are well known. M. Chevreul mixed wood with solution of caustic potash in a retort, the beak of which plunged into mercury, and then applied heat. The water was first evaporated, then action took place, and an inflammable gas was liberated, containing very little carbon; the residue was of a light yellow colour, and by the addition of water freed from air, no deeper colour was communicated; when oxygen was admitted, there was rapid absorption, the colour became dark, and the ulmin or matter described by M. Braconnot appeared.

Many important considerations are derived from these facts, such as the necessity of considering the probable action which will be induced by the use of caustic alkalies in the separation of proximate vegetable principles, such as gallic acid, vegeto-alkalis, colouring matter, &c. The following questions are also suggested,

* Quarterly Journal, viii. p. 392.

and put by M. Chevreul. Are not the alkaline liquids of the animal system in a different condition with regard to oxygen gas to the acid liquids, even when the latter contain the same principles as the former?

Has not the alkali contained in the blood some influence on respiration? consequently, is there not in the organs of animals, inorganic bodies possessed of an activity much greater than has as yet been recognized?

Is the alkalinity of the fluids destined during life to receive the impression of the air essential to respiration, or is it simply a concomitant phenomenon with the combustion of the carbon and the hydrogen in the blood? "If," says M. Chevreul, "the necessity of the alkaline nature of the blood during respiration be confirmed, it will establish an important difference between the blood of animals and the sap of vegetables, which is always acid."—*Mém. du Muséum*, xii. 367.

3. *Thorina, not a distinct earth.*—M. Berzelius has ascertained that the substance which he described 10 years ago, as a new earth, does not merit that distinction, being merely a sub-phosphate of Yttria. We are glad of this correction, and think, that in the present state of chemistry, the man who strikes an earth or metal off the list, deserves more thanks than he who puts one on.

4. *On the Distillation of fatty Bodies, by* M. Dupuy.—M. Dupuy is a student in pharmacy, who, having occasion, in 1823, to distil some oil, observed that the temperature exerted an important influence on the nature of the products. When the oil was not raised to ebullition, a *solid* product was obtained equalling three-fourths of the quantity employed; whilst, if a temperature higher than ebullition was used, a liquid product was obtained during the whole of the operation. These results were communicated to M. Thenard, who recommended the author to submit them also to M. Chevreul, in consequence of his peculiar interest in researches on fatty bodies. The conduct of M. Chevreul was so honourable and delicate on this occasion, as to deserve the marked approbation of all scientific men. Seeing that the experiments were highly interesting, he advised the student to continue them, supplied his want of means and experience by giving him the use of his laboratory and his advice; and, though he knew of all the facts before the publication of his work on animal fatty bodies, would not, from delicacy, refer to them; nor could he be persuaded by the author himself to take up the subject for quicker and better investigation, because as a man of honour he considered it the inviolable property of M. Dupuy.

The following are the results of five distillations:

100 parts of	i. Olive Oil.	ii. Olive oil.	iii. Lard.	iv. Tallow.	v. Tallow.
Solid product	76.470	100.00	74.609	——	——
Liquid product	23.529		12.307	87.499	83.033
Charcoal . . .	3.676	2.650	3.846	3.124	4.052
	103.675	102.650	90.762	90.623	87.085

In Nos. 1 and 2, the temperature never rose to ebullition. The increase is probably from absorption of oxygen from the atmosphere. No. 3 was made with less care, and the matter sometimes boiled; hence more gas was produced—more vapour carried off by it, and less absorption from the atmosphere took place. Nos. 4 and 5 were made at a higher temperature. It follows, however, from the experiments, that if oil be distilled at a temperature a little below ebullition, with the contact of air and necessary precaution, the product equals the weight of the oil employed plus a portion of carbon.

By receiving the products in different portions, it is found that besides water, *&c.*, two kinds of matter are obtained; the first, forming three-fourths of the oil employed, is of a consistence approaching that of lard; yellowish and soft at first, but by degrees acquiring consistency, and becoming very white. At a certain point of time, a yellow tint is again evident, which indicates that the process will soon cease, *unless* the temperature be increased: this product has a disagreeable, penetrating odour, and is very acid. The second kind of matter is obtained by heating the residue to ebullition; it is liquid, of a yellow amber colour at first, but by contact with air absorbs oxygen, and becomes of a deep brown colour; its odour is less disagreeable than that of the first product. It has an analogy to the empyreumatic oil of amber, and is sensibly acid. Further examination indicated the presence of margaric, oleic, and sebacic acid; a volatile acid; a volatile odoriferous principle not acid, and a fatty matter not acid.

The first product was agitated, and left in contact with cold water. The filtered liquor was acid and odoriferous when distilled; the product was colourless, and contained the volatile acid and the odorous principle. Being saturated with baryta and distilled, the odorous principle passed into the recipient, and the baryta salt being decomposed by phosphoric acid, gave the volatile acid; it somewhat resembles that obtained by M. Chevreul from rancid fat, and by its volatility and oily aspect, when a hydrate resembles the phocenic and butyric acids.

The residue left by distillation of the washing water was, upon evaporation, found to contain sebacic acid, and an extractive matter becoming brown by contact with air.

The portion insoluble in water was washed in hot water, and treated with magnesia, which combined with it: boiling water then separated an efflorescent white salt, containing an acid resembling the volatile one described above. The insoluble salts, treated with alcohol, yielded to it a portion of fluid fatty matter, not acid. Ether completed the separation of this substance.

The residue decomposed by muriatic acid, gave a substance composed entirely of margaric and oleic acids.—*Annales de Chimie*, xxix. 319.

5. *Variations in the Composition of the Atmosphere.*—Mr. Dalton states that he has found the oxygen in the atmosphere vary from 20.7 per cent. to 21.15 per cent. The latter was the case on the 8th of January last, the barometer being 30.9, wind N. E.. and very moderate, after three days of calm and gentle frost. The general state of the atmosphere yields only 20.7 or 20.8 per cent. of oxygen.—*Ann. Phil. N. S.*, x. 304.

6. *Action of Carbonic Acid on Hydrosulphurets. By* M. Henry, jun.—Although M. Chevreul had shewn that carbonic acid is capable of decomposing the hydrosulphurets; yet, when M. Henry advanced the opinion that the sulphuretted hydrogen, disengaged from the mineral waters of Enghien, was owing to the action of free carbonic acid on the hydrosulphurets contained in those waters, it met with considerable opposition; in consequence of which he resumed the subject, and undertook a series of experiments with a view to elucidate it, from which he has deduced the following conclusions:—

1. Carbonic acid, in contact with the alkaline or magnesian hydrosulphurets, is capable of decomposing them completely, if the action be continued for a sufficient length of time.

2. The decomposition is effected either by boiling a hydrosulphuret in water impregnated with carbonic acid; or by placing the mixture, without heat, in the vacuum of an air-pump; or by passing a current of carbonic acid gas through a diluted solution of the hydrosulphuret.

3. The hydrosulphurets, obtained by converting sulphates into sulphurets by carbonaceous matter, are less readily acted on.

4. The result of the decomposition of all these salts is the production of carbonates, or rather bi-carbonates; and the quantity of sulphuretted hydrogen disengaged is proportionate to that of the carbonate formed.—*Ann. Phil. N. S.*, x. 381.

7. *Inspiration of Inflammable Gas. (Hydrogen?) By* Signor Giacomo Cardone.—This experiment was made in consequence of the difference of opinion on the effects of this gas on the

lungs, entertained by Scheele, Fontana, and others. The air was expelled from the lungs as much as possible, the mouth-piece of a bladder containing 30 cubical inches of the gas applied to the mouth, and the gas inhaled at two inspirations. An oppressive difficulty of respiration and a distressing constriction at the mouth of the stomach were the first sensations; these were followed by abundant perspiration, a general tremour over the whole body, seeming to commence at the knees; an extraordinary sense of heat, slight nausea, and violent head-ach. My eyes beheld things but indistinctly, and a deep murmuring sound was in my ears. After a short time, all these effects ceased, except that of heat, which increased in an alarming manner; but ultimately, by the abundant use of cold drinks, I was restored to my original state of health.—*Giornale di Fisica*, viii. 295.

8. *In a Mixture of Muriate of Potash and Muriate of Soda to determine the Proportions of each.*—As the nitrate of silver is the most delicate and convenient precipitant, by which we can ascertain the quantity of muriatic acid in combination with these bases; and the quantity united to each being certain, but different (relative to their atomic weights,) which difference is sufficiently great for experimental purposes; hence the quantity in the mixture varies relative to the proportions of each.

The test may be rendered more commodious for analytical experiments, by using it in the state of solution, of known strength or specific gravity, and by means of a graduated tube.

Then let W = the number of grains of the mixed salt operated on.
a = the grain measures of the test which W grains of (p) would require.
b = the grain measures of the test which W grains of (q) would require.
c = the grain measures of the test which W grains of the mixture operated on *have required.*

Suppose x = grains of (p) contained in W grains of the mixture.
$W-x$ = grains of (q) ditto, ditto.

$$\text{Then } \frac{W.p}{W.q} = \frac{a}{b} = \frac{p}{q}$$

$$\text{And } \frac{x.p + \overline{W-x}.q}{W.p} = \frac{c}{a}$$

$$\therefore x + \overline{W-x}.\frac{q}{p} = W.\frac{c}{a}$$

$$\therefore x - x\frac{b}{a} = W.\left\{\frac{c}{a} - \frac{b}{a}\right\}$$

$$x.\frac{a-b}{a}=W.\frac{c-b}{a}$$

$$x=W.\frac{c-b}{a-b}=\text{grains of } p$$

$$W-x=W.\frac{a-c}{a-b}=\text{grains of } q.$$

Say that 200 grain measures of the test are equivalent to 10 grains of *muriate of soda*, then 10 grains of *muriate of potash* would require 157.9 grain measures (nearly) of the same test.

Suppose that we have 178.95 grain measures exhausted in one experiment in 10 grains of a mixture of the two salts—

$$\text{Then } x=W.\frac{c-b}{a-b}=10\times\frac{178.95-157.9}{200-157.9}=10\times\frac{21.05}{42.1}$$

$=5$ grains of muriate of soda.

$$\text{And } W-x=W.\frac{a-c}{a-b}=10\times\frac{200-178.95}{200-157.9}=10\times\frac{21.05}{42.1}$$

$=5$ grains of muriate of potash.

9. *On the Solution of Steel and Iron in Acids, and on the residua which remain. By* M. Karsten.—M. Karsten observed, as others have done, that the action of acids on *steel* depends on the hardness of the metal; hard steel being dissolved with great difficulty and slowness in diluted acids. *White raw iron* shows the same habitudes as steel, but in a more striking manner. Diluted muriatic, or sulphuric acid, has scarcely any effect on it; and the black powder does not appear until weeks have elapsed: strong hot muriatic acid dissolves it, leaving no residuum. Sulphuric acid, in the same circumstances, leaves some carbon, black and of a metallic appearance. Cold nitric acid separates black flakes, which, by long exposure to the acid, become brownish-red.

Gray raw iron exhibits very different appearances. Diluted muriatic and sulphuric acids act but slowly, and leave, after months, a carbonaceous residuum in very different conditions; one part is in thin leaves, or scales, lustrous, metallic in appearance, capable of resisting acids and alkalis, not attracted by the magnet, and very slowly combustible in a red-hot crucible: these are *graphite*. Another part has a similar appearance; but is magnetic, and resembles the residuum from soft steel. A third part is black, not magnetic; colours alkaline solutions black, and is readily consumed by heat and air. Of these three bodies, the *graphite* is never missing; the others seldom occur together. Strong muriatic acid does not leave so much graphite, part being carried up by the gas. Strong sulphuric acid leaves graphite, and easily combustible carbon. Nitric acid of specific gravity 1.3, whether urged by heat or not, seems to act irregularly; leaves of graphite being evolved, which as they fall off, or are separated from, the iron, allow the action to be renewed, they seem to form an actual mechanical impediment to the solution; a part of the carbon is dissolved by the acid, and the residuum is mostly graphite mixed with carbon in the state of brown powder.

The graphite thus obtained is insoluble in acids, and alkalis, and quite pure. Heated to redness in the atmosphere, it disappears slowly, leaving *no* residuum: 18 grains of it placed in a muffle heated to whiteness, required 4 hours for its combustion; it gradually decreased in bulk, produced no flame, and left a minute film of white silica. When the process was interrupted, the only difference seen between the calcined and uncalcined graphite was, that the leaves of the former, if held against the light, appeared transparent in some places, and exhibited a peculiar fibrous structure, which the uncalcined portion did not possess: melted with nitre, it was slowly consumed, and the salt remaining, left no residuum in water: heated with sulphate of potash, it did not convert it into sulphite.

Thus, then, the graphite in gray raw iron is not what it has been supposed to be, a combination of carbon and iron, but pure carbon, or its metallic base. Whether natural graphite be also a pure carbon metal, or really a combination of carbon and iron, is yet to be determined.—*Phil. Mag.* lxvi. 290.

10. *Preservative against Rust. Stockholm*, 9*th Sept*., 1825.—M. the Councillor of State of Loevenhielm, Swedish Ambassador in France, during his residence in Paris, received certain propositions from the house of Mazet and Co., tending to give to the proprietors of the mines in Sweden, for the sum of 300,000 francs, a secret; the use of which is, by means of a metallic composition, to preserve all iron goods from rust. The colleges of mines and of commerce, with the Academy of Sciences, and the delegates of the iron-office, were directed to examine these propositions, and they thought fit to adopt them.—*Rev. Ency.* xxvii. 899.

11. *Combinations of Antimony with Chlorine and Sulphur.*—The following estimations of the composition of these bodies is by M. Rose. The crystallized compound of antimony and chlorine, obtained by distilling the pulverized metal with corrosive sublimate, was supposed, from theoretical views, to be a compound of three proportionals of chlorine and one of the metal. It was analyzed by bringing it into solution in water by tartaric acid, preci-

pitating the antimony by sulphuretted hydrogen, and afterwards the chlorine, by nitrate of silver; the results were—

Antimony . . .	53.27	by theory	54.85
Chlorine . . .	46.73	„	45.15
	100.00		100.00

By passing dry chlorine over heated antimony, combustion of the metal is caused, and a very volatile liquid is formed, being another chloride of antimony; it is white, or of a light yellow colour, and in its external properties resembles Libavius' liquor. It has a strong disagreeable odour, and fumes in the air. Exposed to air, it absorbs water, and becomes a solid crystalline mass; with more water, it heats, and precipitates oxide of antimony. Analyzed in a manner similar to the former compound, it gave—

Antimony . . .	40.56	by theory	42.15
Chlorine . . .	59.44	„	57.85

supposing it to contain 5 proportionals of chlorine to 1 of metal.

When chlorine is passed over the sulphuret of antimony, containing three proportionals of sulphur, the first, or crystalline chloride, is obtained, mixed with chloride of sulphur; upon applying heat, the former dissolves in the latter, and as the solution cools crystallizes again in large crystals.

Sulphurets of Antimony. Having made many experiments on these compounds, M. Rose has found *but* three; these corresponding with the oxides of the same metal. The compound, with 3 proportionals of sulphur, is the native mineral, and corresponds to the oxide with 3 proportionals of oxygen. It dissolves entirely in muriatic acid, disengaging only sulphuretted hydrogen. It is also formed by passing sulphuretted hydrogen through solution of emetic tartar, or through butter of antimony dissolved in water and tartaric acid. It is, in this case, of an orange colour, but is a true binary sulphuret; as is also kermes mineral; both these being, according to M. Rose, compounds with 3 proportionals of sulphur.

The next compound is obtained by dissolving antimony in nitro-muriatic acid, evaporating to dryness, heating the mass to redness, fusing it with caustic potash, by which it is said to form what is called antimonious acid, and then acting on the fuzed mass with water and muriatic acid until a clear solution is obtained. This solution, precipitated by sulphuretted hydrogen, yields the body in question of an orange colour: by analysis it gave—

Antimony . . .	66.35	by theory	66.72
Sulphur	33.65	„	33.28

supposing it to contain 4 proportionals of sulphur.

The next compound is the golden sulphuret of antimony; the methods of preparing which are well known. It is considered as containing 5 proportionals of sulphur, and by calculation, therefore, is composed of—

Antimony	61.59
Sulphur	38.41

The results of experimental analyses differed too little, it is said, from the calculated results, to require that they should be given.

The native compound of sulphuret of antimony, with oxide of antimony, was found to contain—

Sulphuret of antimony . .	69.86
Oxide of antimony . . .	30.14

or, according to M. Rose's views, 1 proportional of oxide of antimony, with 3 proportionals of oxygen, and 2 proportionals of sulphuret, with 3 proportionals each of sulphur.—*Ann. de Chim.* xxix. 241.

12. *On the Detection of Arsenic by Lime Water.*—The paper from which the following extracts are made is by M. Aug. Ludw. Giseke, and has been published in Schweigger's Journal. We are induced to notice a part of it in consequence of the importance which attaches to any circumstance affecting the indications of arsenical tests. The following process for the detection of arsenic in cases of poisoning, is the joint production of Rose and Berzelius:—"Cut up the coats of the stomach, and place them in the liquid, which is boiled with a few drachms of caustic-potash, in order to dissolve any arsenious acid that might be contained in it. The solution obtained is filtered, heated till it boils, and during the boiling mixed with nitric acid, which is added in small portions as long as any thing separates, and till the liquid has become strongly acid, clear, and of a bright yellow colour; it is filtered while hot; afterwards nearly, not completely, saturated with carbonate of potash, and made to boil, in order to expel the carbonic acid; then it is boiled with clear lime-water as long as any precipitate is formed; the lime-water first saturates the excess of acid, and then precipitates with the arsenious acid as arsenite of lime, and with the phosphoric acid and other animal substances decomposed in the nitric acid. If, instead of saturating the acid with lime-water, you add first caustic alkali till the liquid becomes alkaline and then add lime-water, no precipitate will be formed, because the arsenite of lime is held in solution by the alkali."

This statement of the solubility of arsenite of lime in a solution of alkali, being in contradiction with certain facts, M. Schweigger was induced to examine the circumstances more minutely, and was ultimately led to the following explanatory experiment:—prepare an arsenical liquid, pour it into three glasses, and add to one portion an excess of caustic-potash; to the second, excess of caustic-soda; and to the third, excess of caustic-ammonia. On adding lime-water, a deposit of arsenite of lime will be formed equally in each of the glasses. Now add to each a few drops of acid, (for instance, nitric acid,) yet so that in all the alkali shall predominate; whilst no solution of the precipitate will take place in the glasses that have the potash and soda in them, it will immediately begin in that with the ammonia; and all the arsenite of lime will be finally dissolved, although the ammonia be not saturated by the acid which has been added. Of course, the solution will take place in the three glasses when any acid is in excess; yet, on saturating the acid with alkali, the precipitate will be re-formed immediately in those glasses that contain the potash or soda, but not in that which holds the ammonia, however one may neutralize the liquid.

By putting muriate or nitrate of ammonia into a liquid containing arsenic, and adding lime-water in any quantity, no precipitate will be formed, even though heat be applied. Thus it will be seen, that it is not the ammonia, as *caustic alkali*, which retains the arsenite of lime in solution, but it is the presence of a soluble salt of ammonia which prevents the formation of the deposit; and if, instead of ammonia, caustic potash or soda be used in the process described by Berzelius, then lime-water will instantly form the precipitate of arsenite of lime.—*Phil. Mag.* lxvi. 253.

13. *Artificial Gold, a new alloy.—Hanover.* M. Dittmer has described in the Hanoverian Magazine the following compound of different metals, prepared by the privy-counsellor Dr. Hermstadt, and which may supply the place of gold, not only as to colour, but also for its specific gravity and ductility. The materials consist of 16 parts, by weight, of virgin platina, 7 parts of copper, and 1 part of zinc, equally pure; these metals are to be mixed together in a crucible, covered with powdered charcoal, and perfectly fused so as to form a homogeneous mass.—*Rev. Ency.* xxvii. 900.

14. *Simple mode of obtaining Meconiate of Morphia.*—The following process is by Dr. Giuseppe Meneci: reduce good opium to powder, put it into a paper filter, add distilled water to it, and slightly agitate it; in this way wash it till the water passes through colourless; then pass a little diluted alcohol through it; dry the insoluble portion (now diminished to one-half), in a dark place; digest it, when dry, in strong alcohol for a few minutes, applying heat; separate the solution, which, by cooling and after-

evaporation, will yield well crystallized meconiate of morphia of a pale straw-colour.—*Gior. di Fisica*, viii. 218.

15. *Rectification of Alcohol at common Temperatures.*—M. Paget Descharme proposes to rectify alcohol in the large way by exposing to its vapour deliquescent salts; thus he puts into a vessel with a flat bottom a given quantity of weak alcohol; he then puts a portion of pulverized muriate of lime into a dish, and places it upon feet, or otherwise, over the alcohol in the first; this is closed hermetically, or a cover fastened on by pasted slips of paper, and the whole left for four or five days, in which time the alcohol strengthens, and the muriate deliquesces; the muriate is then removed, and a fresh portion introduced, and this is repeated until the alcohol is sufficiently concentrated. This is an operation common enough in our laboratories: it remains to be seen whether it can be economically adopted in the large way.—*Ann. de Chim.* xxix. 328.

16. *Hygrometric Property of Sulphuric Acid.*—The quantity of water that sulphuric acid sp. gr. 1.840 is capable of absorbing from an atmosphere saturated with vapour, has lately been determined, by exposure of 50 grains of acid of the above strength to such an atmosphere. In the course of four months, it gained 423.2 grains of water, considerably more than eight times its original weight, its sp. gr. being diminished to 1.0706.—T. G.

17. *Simple method of exhibiting the Deflection of the Magnetic Needle by the Electric Current.*—A striking method of exhibiting the deviation of the magnetic needle, whilst under the influence of the voltaic conducting wire, consists in employing one of M. de la Rives' floating spirals, in the centre of which, and parallel with it, is placed a suspended magnetized needle. The moment the plates of an apparatus, so constructed, are dipped into an acid, the needle places itself at right angles to the spiral wire: thus shewing one of the principal facts of electro-magnetism, although upon a small scale, yet in a very decided manner.—T. G.

18. *Necessity of Water in the preparation of Lead-plaster.*—Attempting to form lead-plaster, the *Emplastrum Plumbi* of the *Pharmacopœiæ*, without the use of water, steam being the source of heat, I was surprised to find after several hours, during which time the litharge and oil had been kept at a temperature of 220°, or thereabout, and constantly stirred, not the slightest appearance of combination; upon the addition of a small quantity of boiling water, the oil and oxide immediately saponified; water appeared, therefore, to be essential to the formation of the plaster. It also appeared probable the oxide might be in the state of hydrate; to

ascertain if such were the case, I precipitated, by potash, the oxide from a quantity of acetate; the precipitate, when washed, was dried by a heat of 220° until it ceased to lose weight: 100 grains, heated to redness in a tube, gave off nearly 8 grains of water, and assumed the orange colour of litharge; the recently-precipitated oxide was no doubt, therefore, an hydrate; part of which, with somewhat less than two parts of olive oil, without any addition of water, at a temperature of 212, formed, in half an hour, perfect plaster. Each of these experiments has been repeated with precisely the same results. I am induced to mention this fact, because all pharmaceutical writers limit the action of the water to that of keeping down the temperature. H. H.

III. Natural History.

1. *On the Insalubrity of the Air of Marshes in communication with the Sea. By* M. Gaetano Giorgini.—The observation of M. Giorgini has been drawn to the state of the atmosphere in the neighbourhood of certain marshes on the borders of the Mediterranean; and by reference to historical data, and various documents, he has proved the great importance which attaches to the circumstance of their being, at times, in communication with the sea, so as to have a mixture formed between their waters and that of the sea. Both ancient and modern authors have announced the fatal effects produced in the neighbourhood of marshes by such mixture, and a local belief of the same thing is very common and strong; the opinion has, however, never been supported by any well ascertained and public fact, until the present paper, which contains a case so much in point, and so interesting, as to induce us to insert it at some length.

On the south of the Ligurian Apennines, is a marshy shore, bounded on the west for twelve miles by the Mediterranean, on the south by the River Serchio, and on the north by the River Frigido, a torrent commencing at the foot of the Apennines, in the state of Massa di Carrare, running three or four miles over the land, and then falling into the sea. The plain is from two to four miles wide, and is traversed by a few short torrents or streams; among these are the rivers Camajore and Pietra-Santa, which divide the plain into three separate basins.

The rain and spring-waters, which flow into the three basins mentioned, are slowly discharged into the sea by natural or artificial canals, penetrating the sand-bank, which exists on the sea side; these are, first, for the principal basin of the Lake of Massaciuccoli, the ditch of Burlamacca; second, for the smaller Lakes of Torre and Montrone, the ditches of Montrone and Tonfalo;

thirdly, for the small lake of Perrotto, and its neighbouring marshes, the ditch of Cinquale. The level of these stagnant waters is between that of high and low water in the neighbouring sea; there being but little difference between these two points in this part of the Mediterranean. In this state of things, formerly, when the waters of the sea rose from any circumstance, (unless the waters of the marshes were very high,) they used to return up the ditches, fill the basins, and inundate the country to the foot of the mountains; and with a north-west wind the waves used to penetrate with force to the interior. The mixture of fresh and salt water, thus formed, and which, in summer, was rarely changed, became corrupt, and spread infection over the neighbourhood of the most destructive kind.

In this way the effects of the mal-aria were re-produced annually, in the neighbouring country, with all their peculiar horrors: the population, though small, presented feeble infants and diseased men, old age being unknown there. All attempts to avoid the scourge, by living on the hills, or in the interior, and frequenting the plain when the business of cultivation essentially required it, were vain; they fell victims to the extensive influence, and such being the effects upon the inhabitants of the country, much more rapidly did a stranger suffer from the deleterious atmosphere; one single night, in the months of August or September, causing inevitable death to the incautious traveller who should stay so long in this infested country.

Such was the state of things until 1741: previous to that time, Gemignano Rondelli, Eustache Manfredi, and Bernardino Zendreni, had successively insisted upon the necessity of excluding the sea from these marshes, and in 1740—41, a sluice with folding-doors, competent to give emission to the waters of the marsh, but prevent the sea from entering, was constructed at the mouth of the Burlamacca. The most complete and unexpected success immediately followed upon, and has continued with, this work. The year after its completion, there were no appearances at Viareggio, Massaciuccoli, Quiesa, nor in parts more distant from the basins of Montrone and Perrotto, of the terrible maladies which previously appeared every year. The inhabitants soon recovered health; and the land being very fertile, the population rapidly increased, and is increasing at this moment. Viareggio has become a considerable town, and so completely has all suspicion of its insalubrity disappeared, that the first families of the city of Lucca have for years built their summer seats there.

To these strong proofs of the good effects of the means taken may be added others, deduced from the neglect of those means. In the summers of 1768 and 1769, Viareggio and the neighbouring parishes of the Lakes of Massaciuccoli were again ravaged by the old diseases. It appears, from the registers, that in these two years, Viareggio had 170 deaths in a population of 1330, making nearly 1 in 15 for each year, whilst in the year following, the deaths were 32, or 1 in 40. The cause was found to exist in the damaged state of the gates in those two years, which permitted the passage of the sea; they were repaired, and the evil disappeared.

A similar circumstance happened in the years 1784 and 1785; in the first year, the deaths were 92 out of 1898 inhabitants, or 1 in 20; and in 1785, they were, 103 in 1834 inhabitants, or 1 in 18. The government reports state, that in this population of 1898, there were 1200 sick persons. The epidemic was stopped, in 1769, by repairing the gates.

Notwithstanding the success of the precautions taken at this part of the coast, the neighbouring parts were long left a prey to the destroying influence of the mixed marsh-waters; and the inhabitants around the basins of Montrone and Perrotto were not considered until the year 1804. In the years 1809, 1810, 1811, similar means were taken with the best effects to the inhabitants of Montignosini, and the vicinity, and in 1812, a sluice was constructed on the Cinquale, which perfected the arrangements in this part, and made a large portion of country equally healthy with Viareggio. To complete the arrangement, it was now only required to guard the ditches of Montrone and Tonfala with sluices; the former was finished in 1819, and the latter in 1821.

Since that time the diseases of mal-aria have ceased so entirely at all points, that no other dangers are now incurred, regarding the insalubrity of the atmosphere, than such as may arise from neglect of these sluices, which the inhabitants of the country should regard as their palladium.

Now that it has been well ascertained that the exclusion of the sea from the marshes ensures salubrity and *vice versâ*, a vast field is opened to researches, which, though difficult, delicate, and expensive, are of the highest importance and utility. The following are three points put by the author:—

1. Is the developement of the pestilential miasmata due to the mere mixture of soft and sea water, or is it occasioned by the destruction of vegetable and animal species in the marshes by the introduction of the latter?

2. In the one or other case, what are the chemical changes effected by the mixture, the nature of the deleterious emanations, the degree of heat requisite for their production, the influence of the sun, and the mud of the marsh, *&c. &c.?*

3. What was their action on the animal system? To what distance may they extend? and in general, by what circumstances are they modified? *&c.*—*Ann. de Chim.* xxix. 225.

2. *Effects of Lightning on the Animal System.*—In reference to the case described, page 367 of our last volume, Dr. Fusinieri writes, that during the winter of 1824, no particular effects were perceived by Sig. Tomiello in the arm struck by lightning, but that, as the spring of 1825 advanced, it again became affected; the same sensation of heat, and want of motion, taking place, when the weather became stormy; the change in the weather being pre-indicated for several hours, or, at times, even days. Dr. Fusinieri remarks upon the circumstance, that these effects were not perceived in the winter season, though the weather might be stormy, and the temperature as warm, at times, as on occasions when the arm was affected. He considers the cause as existing in a morbid sensibility of the nerves of the arm to atmospheric electricity.—*Gior. di Fisica*, viii. 219.

3. *Investigation of supposed Electric Currents in the Nerves.*—Il Sig. Nobili has endeavoured to ascertain, by means of his delicate Galvanometer (p. 170), the existence of those electric currents which are supposed by some persons to take place through the nerves, in the animal system. The means adopted, were to introduce the wires of the instrument into different parts of the nerves, in which case, if currents existed, part should be diverted through the instrument, and rendered sensible by its effect upon the compound needle. Notwithstanding the extreme delicacy of the instrument, no traces of such currents could be perceived.

In consequence of the observations of Amici and Herschel, Il Sig. Nobili also examined the circulation of the chara * in a similar way, but could detect no electrical current. He found also that a current of electricity, sent through the circulating system of the chara, deranged and injured it.—*Gior. di Fisica*, viii. 269.

4. *Relation of a case of Poisoning caused by the Honey of the Lecheguana Wasp. By* M. Auguste de St. Hilaire.—" After having crossed the plains of the Rio de la Plata, I coasted the thinly-inhabited borders of the Uruguay, and arrived at the camp of Belem, on the locality of the town of the same name destroyed by Artigas. There I was told I should have to pass a desert destitute of habitations or roads, but that, in case of necessity, I could have recourse to two detachments of Portuguese soldiers posted on the banks of the river, and also that I could have a guide to the first detachment near the mouth of the Guaray. At the borders of this river I took another guide, who was to conduct me to the rivulet S. Anna, where I was told the second detachment was placed. Arrived at the rivulet, I and my men searched for

* Quarterly Journal, xvi. p. 388.

the detachment for two days; but our exertions being useless, I sent back the guide (he never having been farther), with one of the soldiers of my escort, to the river Guaray, directing the latter to return with another guide. In the mean time we waited on the borders of the rivulet, in a place inhabited only by a multitude of jaguars, immense troops of wild mares, stags, and ostriches, opposite the right bank of the Uruguay.

"For four days we were inconvenienced in this desert place, by heavy rains, and multitudes of annoying insects, with no other shelter than my cart. On the fifth day the weather became fine, and I went to botanize in the country about the river, accompanied by two of my men, all well armed against the attacks of jaguars. After some hours, hunger sent us back to the rivulet, and we ate of our usual food, the flour of the manhioc, and cow-beef, roast and boiled.

"During a short walk, the evening before, we perceived a wasps' nest suspended at about a foot from the earth, from one of the branches of a small tree. It was nearly oval, about the size of the head, of a gray colour, and of the paper-like consistence of European wasps' nests.

"After our breakfast, my two companions went to destroy this wasp's nest, and they took out the honey. We all three tasted of it. I ate most, but the quantity did not exceed two spoonfuls. The honey had a mild agreeable taste, and was quite free from the physic-like taste so often belonging to our bee-honey.

"After having eaten, I felt a pain in the stomach, not violent, but inconvenient. I lay down under my cart, and went to sleep; during which, those objects dearest to me were present to my imagination, and I awoke deeply affected. I rose, but felt such extreme weakness, that I could not take fifty steps. I returned to the cart, lay down on the grass, and felt my face bathed in tears. Blushing at my weakness, I laughed at myself, and notwithstanding my efforts, this laugh became lengthened and convulsive. Nevertheless, I had power to give some orders, and during the time my chasseur, one of the two Brazilians who had eaten with me, arrived.

"This man united, to rare intelligence, a light and fantastic character. Often, after long periods of amusing gaiety, he would, without any reason, fall into a dull melancholy state, continuing for weeks, and then he would find sources of irritation in the most innocent words and delicate attentions. His name was Jozè Mariano; he approached me, and said, with a gay but wild appearance, that for the last half hour he moved about the place without knowing where he was going. He sat down under the cart, making a place for me; I had much difficulty in reaching it, and feeling my extreme weakness, rested my head upon his shoulder.

"It was then that I experienced the most cruel agony, a thick cloud obscured my eyes, and I could perceive nothing more than traces of my men, and of the azure of the heavens traversed by a few light vapours. I did not feel much pain, but weakness of the extremest kind. Concentrated vinegar was placed near my mouth and nostrils, and rubbed on my face and temples; it re-animated me a little, and then I felt the pains of death. During this time, I preserved my memory perfectly, and remembered all that I had said, and that had been said; my recollections agreed perfectly with the recital of a young Frenchman who had accompanied me. A violent combat then passed in my mind, which continued, however, for but a few instants. I triumphed over my weakness, and resigned myself to death. That which affected me most was the fate of my Indian Botocudo, whom I had drawn from his forests, and who, I believed, would, after my death, be condemned to slavery. I conjured those around me to have pity on his inexperience, and to tell my friends that my last thoughts had been for that unfortunate young man. I felt an anxious wish to talk in my native language to the Frenchman who was earnest in his cares for me, but I was unable to find a single word in my memory which was not Portuguese, and I cannot express the shame and contradiction I felt at this want of recollection. I had at first endeavoured to take water and vinegar, but finding no relief, I requested warm water: each time that I swallowed it, I felt the cloud removed from my eyes for a few moments, and I began to drink it in very large quantities. I continually requested a vomit of my young Frenchman, but he could not find one. He searched in the cart, and I, being beneath it, could not see him. I nevertheless seemed to have him before my eyes, and reproached him for his slowness. This was the only error into which I fell.

"Whilst this was passing, the chasseur had risen without my perceiving it, but my ears were quickly struck with the dreadful cries he uttered. I was a little better at this moment, and none of his motions escaped me. He tore his clothes furiously, threw them far from him, took a gun, and discharged it. The piece was taken from him, and then he began to run about the place, calling the Virgin to his help, and crying out that all was on fire about him, that we two were abandoned, and that they were going to leave our portmanteaux and cart to be burnt. A Guarina man, (one of my suite) having endeavoured in vain to restrain him, was seized with fright, and fled.

"Until this time, I had had the attentions of the soldier who had joined us in eating the honey, but he was now very ill; he however soon vomited, and being of a robust temperament, he quickly recovered strength, though it required some time perfectly to re-establish him. I have understood since, that during the whole time he was extremely pale, his figure being frightful. On a sudden he said, "I shall go and tell what is passing at the Guard of Guaray." He mounted a horse, and galloped over the country, but the young Frenchman soon saw him fall; he rose, galloped a second time, fell again, and was found by my men profoundly asleep, some hours after, in the place where he fell.

"I then found myself almost dying, with no other than a man still furious, my Indian Botocudo, who was merely an infant, and the young Frenchman, who was almost distracted by these extraordinary events. All the morning we had perceived insurgent Spaniards on the other side of the river, and some even in the distance on the same side; they would probably have attacked us, had they known how small was our number. The dangers of my situation affected my spirits, and I felt myself worse.

"I had calculated the soldier would return with a new guide from Guaray on that day; I hoped to obtain help from them, and my imagination was divided between the desire of seeing them, and the fear of surrounding dangers. At one time I thought I saw their dogs, but I was mistaken, and returned to my former state. The dogs I had seen were some almost without master, in the deserts, which had been attracted by our food. The chasseur, Jozè Mariano, now came and sat by me; he was more calm, had fastened a cloth round his loins, but had not yet recovered his reason. "Master," said he, "long have I accompanied you, I have always been a faithful servant, I am on fire, do not refuse me a drop of water." Full of terror and compassion, I took his hand, and endeavoured to console him.

"The warm water, of which I had drunk a prodigious quantity, now produced the desired effect, and I vomited. I felt relieved, a numbness occurred in the fingers, but it was of short duration. I distinguished the cart, the pasturages, and the trees, and the cloud left my eyes, so that I could see all but the upper part of objects, or if it came on again, it was only for a few instants. The state of Jozè Mariano continued to cause me much alarm. I was also fearful I should not, myself, recover the entire use of my faculties. A second vomiting began to dissipate these fears, and procured me fresh ease. I saw objects more clearly, could talk French or Portuguese at will, my ideas became more clear, and I directed the young Frenchman where to find an emetic. I divided it into three parts, vomited abundantly, evacuating the food and honey I had taken in the morning, with torrents of water. Until I had taken the third portion of the emetic, I felt pleasure in long draughts of water; but after that time, I disliked it and took no more; the cloud disappeared, and after some cups of tea, I took a short walk, and, with the exception of strength, was almost in my natural state.

"Nearly at the same moment, reason returned suddenly to Jozè

Mariano, without his having vomited; he took fresh clothes, mounted a horse, and went in search of the soldier, with whom he shortly returned.

"It was about 10 o'clock in the morning when we had taken of the honey, and the sun was setting before we had recovered. The momentary absence of the Frenchman and the Indian Botocudo had prevented them from eating any of it. The soldier had offered some to the Guarina man, but he, knowing its deleterious quality, had refused to eat of it. The soldier laughed at him, and had not mentioned the circumstance to us.

"On the morrow I was still weak, the soldier complained of deafness, Jozè Mariano had not recovered his strength, and said his body seemed covered with glue. As our guide had arrived the evening before, we parted, and continued our journey, glad to leave the place."

Having told his soldier that he should be glad of some wasps of the kind which had produced this honey, M. St. Hilaire was called, on the day following the memorable one, to look at a wasps' nest, exactly resembling that of the preceding day. It was recognised by the Guarini and the Indians the guide had brought with him, to be of the kind known in the country by the name of *Lecheguana.* Some of the animals, with fragments of their habitation, were secured, and have been deposited in the king's cabinet. The honey was red and liquid, like that of the preceding evening.

It appears, that notwithstanding the events of the preceding day, the Indian Botocudo, the Guarina man, and another, ate of this honey without the knowledge of M. St. Hilaire, but none of them suffered from it.

After-inquiry, in the more inhabited part of the country, elicited that two kinds of *Lecheguana* were known there, the one yielding honey white and innocuous, the other, such as is red; and this, though not always, yet often caused serious injury, occasioning a kind of drunkenness or delirium, which could be relieved only by vomiting, and which sometimes occasioned death.—*Mém. du Muséum*, xii. 293.

5. *Loss of Memory.*—"A singular remark, and which, I believe, has never been made, is that in cases where the memory has been lost, without any change in the reasoning faculties, it is always the last syllables of the words which are forgotten. It was thus, that Alexander Selkirk, an English sailor, who was found after the lapse of 25 years on a desert island, still spoke English quite well, excepting the last syllables, which he had forgotten. I have remarked the same phenomenon in a person who was young, but blind for 14 years, to whom, as I shall hereafter relate, I restored the faculty of writing."

Madame de Genlis makes this remark, in consequence of a note which had been written by the Duchess of Cerifalco, after confinement in a subterranean cave for nine years, in which note she observed, that almost all the last syllables were wanting.—*Mem. of M. de Genlis*, iii. 37.

6. *On a peculiar Blue Matter obtained from certain Urines. By* M. H. Braconnot.—Dr. Castara, of Luneville, sent a small flask of urine to M. Braconnot, of so deep a blue colour, that it appeared almost black. It was voided by a person who had also vomited a matter of a similar colour. This peculiarity had existed for two years. The blue matter retained in suspension was deposited but very slowly; it was, however, readily separated by a filter. When well washed, this peculiar substance was pulverulent, of extreme tenuity, insipid, inodorous, of a tint deeper than Prussian blue, adhering readily to the fingers or paper, and making blue marks. Cold water had no action upon it; boiling water took a slight brown tint from it, and then became red by acids, indicating a little solubility. Boiling alcohol had more power over it, and became greenish; when filtered and allowed to cool, it deposited a slight deep-blue sediment, which had a crystalline appearance; the solution evaporated left the blue matter, which, when dissolved in a feeble acid, deposited a portion of fatty matter.

The alkalies have no affinity with this substance, and change it before they dissolve it. Heated with weak solution of carbonate of potash, it did not appreciably dissolve. A hot weak solution of caustic potash dissolved a part of it, forming a brown liquor, reddening feebly by acids; but then with ammonia forming a flocculent precipitate and a colourless solution.

Weak acids readily unite to the blue matter; if the smallest possible quantity of acid be used, the solutions are brown; with more acid they are of a fine red colour. Alkalis, earths, *&c.*, precipitate the blue matter unchanged. It being supposed that the matter possessed some of the characters of a salifiable base, it was heated with water slightly soured with sulphuric acid, in order to separate mucus if any were present. In this way, a yellow-brown solution was produced, nearly neutral, which, by further addition of acid, became bright red. The brown solution, evaporated slowly, gave a residue of a very fine carmine colour, which by solution became yellow-brown, but by evaporation again assumed its fine tint. This compound, when put into the mouth, had a slight acrimonious taste, and coloured the saliva blue from the action of the soda in it.

The red compound, applied in drops on porcelain, left spots, carmine red by transmitted light, but of a golden metallic colour by reflected light. They ultimately became blue, probably from the presence of ammonia in the air. When a solution of this compound was sufficiently concentrated, and left to itself, it yielded a multitude of small acicular crystals of a ruby-red colour. These, however, proved to be sulphate of lime, which was left colourless by the application of alcohol; the alcoholic liquor, on evaporation, gave an imperfect crystalline result; and these crystals, dissolved in water, and acted upon by magnesia, gave the original blue substance.

Strong sulphuric acid, diluted muriatic acid, and oxalic and other vegetable acids, dissolve this substance with facility; the solutions have generally the properties already described, and are decomposed by ammonia, magnesia, *&c.*, *&c.* The solutions leave blue stains on the skin, which, when touched by a little alkali, are very permanent. Silk dipped into them, and then into a weak solution of alkali, became of a grayish-blue colour. Acetic acid combines with the most difficulty with this substance, and the brownish solution obtained by boiling, if evaporated, lets the acid go, and the blue matter separates. Strong nitric acid, when heated with it, permanently alters it.

The blue matter precipitated from the weak sulphuric acid solution by magnesia, appearing to be in its purest state, a portion of it was heated in a glass tube, in the upper part of which was some reddened litmus-paper; this became blue by the action of the fumes; some empyrematic oil also rose, and much carbon with a little oxide of iron remained behind. The matter, as obtained from its alcoholic solution, also gave ammonia when decomposed by heat: hence the substance certainly appears to contain nitrogen.

It appears from these experiments, that this sediment is composed almost entirely of a peculiar blue substance, being accompanied by small quantities of fatty matter, phosphate of lime, mucus, and perhaps oxide of iron. Considering its slight solubility in water or alkalis, and its aptitude, on the contrary, to combine with acids, also the large quantity of carbon which it contains, M. Braconnot seems disposed to assimilate it with organic salifiable bases, and proposes for it the name of *cyanurine.*

The urine from which this blue matter was separated, was then limpid, of a yellow brown colour, and with the usual odour; it distinctly reddened blue litmus paper. Heated, it lost colour, and deposited a very black sediment, which was soluble in weak acids, and in the concentrated urine itself. Alkalis had but little action on this black matter: it agreed in many points with the blue substance, but its solutions did not become bright red by excess of acid, but deep brown. M. Braconnot proposes the name of *melanurine* for it, and he thinks it probable that it is this substance which colours certain urines black.

The urine gave the same quantity of urea and other substances as an ordinary secretion, with the exception of uric acid, of which not a trace could be found; from which it would appear that the peculiar substances present are the result of some change in the principles of that body.

M. Braconnot had also another specimen of urine for examination. It, by standing, deposited a minute quantity of blue matter. He thinks also, that the specimen examined by M. Julia * must have been coloured by this peculiar matter.—*Ann. de Chimie*, xxix., 252.

7. *Analysis of an Urinary Calculus from a Hog. By* M. Würzer, of Marburg.—This concretion had been cut, in July, 1824, at Fulda, from the urethra of an emasculated pig, under a twelvemonth old. It had the size and shape of a small bird's egg. It weighed 91 grains. Its specific gravity was 1.964. It was covered by a thin light-gray crust, and when broken, presented a radiated and concentric crystallization. Upon chemical examination and analysis, it was found to contain the following substances and proportions.

Phosphate of lime and ammonia .	51.787
Muriate of potash	2.625
Protoxyde of iron	0.169
Slimy matter, with a urinous smell .	1.648
Water	43.573
Loss	0.198
	100.000

Phil. Mag., lxvi., 288.

8. *Chinese Manner of forming Artificial Pearls.*—Mr. Gray's observations upon the artificial production of pearls are inserted at page 167 of our last volume. In a late visit to the College of Surgeons, Mr. Gray observed some pearls in the same species of shell before spoken of (Barbala Plicata,) which had the external appearance of having been formed artificially. These pearls are described as of a very fine water, and nearly orbicular; their base is supported by a small process, which separates at the end into two short diverging processes, which stand off at right angles to the central rib. On more minute examination, it appeared that they had been produced by the introduction between the mantle of the animal (while yet alive) and the shell, of a small piece of silver wire, bent into a peculiar form; that is to say, so as to form a right angle, with one arm ending in two diverging processes, so as to make the simple end always keep its erect position. These wires must have been introduced in the same manner as the semi-orbicular pieces of mother-of-pearl in the other method of forming artificial pearls, as there is no appear-

* Quarterly Journal, XVI. p. 117.

ance of any external injury. The pearls are solid and nearly orbicular, with a small pedicel, which is continued so as entirely to cover the wire. They may be perforated and used so as to show their whole surface, which I did not expect could ever be the case with any artificial pearls; but they must, doubtless, unlike the artificial pearls formed by the other means, be a considerable time in coming to any useful and valuable size.—*Ann. Phil. N. S.* x. 389.

9. *Flying-Fish in the Channel.*—Sir—In going down channel on the 23d of August last, with light winds from the E.N.E., inclinable to calm, when off Portland, we were surprised by the appearance of a rather large shoal of what is commonly called the *flying-fish.* They were evidently closely pursued by some one of their numerous enemies, from the frequent and long flights which they took; but it was impossible to discover what he was, though passing close to the vessel.

I am not aware that I have ever heard of this fish having been seen in the English Channel before; and the fact may possibly interest some of your numerous scientific readers.

Sunderland, Dec. 2, 1825. J. C. W.

10. *Age of a Yew-tree.*—In the original charter for building the church of Peronne, in Picardy (now the department of Somme), dated in the year 634, a clause was inserted, directing the proper preservation of a yew-tree, which was in existence in 1790, about 1150 years after this notice of it in the charter.—*M. M.*, lx. 359.

11. *Phosphate of Lime accompanying Iron-Stone in Coal-beds. (Allier.)*—MM. Manby and Wilson sent various specimens of minerals, which were proposed to be worked for iron, to the school of Mines; and one of them, being examined by M. Berthier, proved to be a new variety of phosphate of lime. This observation is of considerable interest as relating to the source of phosphorus sometimes found in iron obtained from the argillaceous iron-ore.

"Among these specimens," says M. Berthier, "one was found which contained very little iron, and which I soon ascertained to be principally composed of phosphate of lime: this specimen had exactly the same appearance as the argillaceous carbonate of iron, and the ticket accompanying it indicated that it was found in the same circumstances, *i. e.*, in lumps in the bituminous schist accompanying the coal. It was lenticular, about the size of the fist, homogeneous, and fine-grained, having some degree of lustre in a bright light, and of a deep gray colour. The argillaceous carbonate of iron of the coal-formations often contains phosphoric acid, even in considerable proportions; but pure phosphate of lime has never before been observed in these circumstances."

"The specimen of the phosphate of lime of Fins gave by analysis

Lime	.353	or	Phos. of lime	.670
Phosphoric acid	.310		Carbonate of iron	.157
Protoxide of iron	.096		Alumine	.090
Alumine	.090		Water and bitumen	.060
Water, bitumen, & carbonic acid	.120			
	.979			.977

Heated in a crucible lined with charcoal, it fused into a compact mass, opaque, stony, covered on its surface with grains of a brittle metallic substance. Fused with half its weight of borax, it produced a vitreous enamel-like scoria, and very brittle grains, almost unacted upon by the magnet."

A note by M. Guilleman on the same substance, describes it as occurring in globular nodules, sometimes flattened, and generally of small size. They are found in abundance in the black argillaceous schists; they are not homogeneous, but are enveloped in carbonate of iron; sometimes they are internally divided by laminæ of carbonate of lime or of coal: at other times they are enveloped in a zone of compact sulphuret of iron. In the centre is a dirty yellow, or gray nucleus, compact, finely granular, and traversed by graminous impressions; it is this nucleus which contains the phosphate of lime. One specimen of sp. gr. 2.65 was composed of

phosphate of lime	0.863
carbonate of iron	0.117
alumine	0.006
coal, water and loss	0.014
	1.000

but the proportions vary in different specimens.—*Ann. des Mines*, xi. 143.

12. *Geology of the Severn.*—The Rev. C. P. N. Wilton, F.C.P.S., *&c.*, has lately been engaged in making a geological survey of the shores of the Severn, in that part of its course which passes through the parish of Clure, in Gloucestershire, to an extent of about 7 miles. In this examination several interesting discoveries were made, of which detailed accounts will shortly be laid before the public.

In one place a stratum was found of a sort of carbonized wood, much resembling Bovey coal, in which occurred, disseminated in small pieces, a white substance not hitherto met with in that matrix, and which, upon examination by Mr. Brande, was found to be sulphate of barytes.

A fossil species of alcyonium was met with in blue lias. A circumstance regarded as extremely curious, when mentioned to that zealous naturalist, Mr. Miller of Bristol, (author of the *Natural History of the Crinoidea*, &c.) With this were found immense specimens of cornu ammonis, *&c.*

Near the same spot a great number of bones were met with in diluvial gravel; a large fragment of a gigantic stag's horn: seven fragments of immense jaw-bones, and teeth in great quantities.

At no great distance some remains of antiquity were discovered. A sort of burying-place has been distinctly traced out. Wood ashes, iron nails, and rude implements, with portions of red and black pottery, were dug up, and at about 2 miles' distance, fragments of the same pottery were found mixed with a quantity of iron-slag.

In another spot, on the banks of the Severn, in a bed of clay, 9 feet below the surface, was dug up, a sort of iron shovel, much corroded, accompanied by fragments of red pottery, and *wood in a state resembling coal.*

13. *On the "Extraordinary rise of the Rio de la Plata," and on the Storms of that part of South America.*

Sir,—In the last Number of the *Journal of Science and the Arts*, p. 183, my attention was arrested by an article extracted from the *Edinburgh Phil. Journal*, which, to most nautical or scientific men, and particularly so to those who have visited the part of South America alluded to, cannot but appear somewhat astonishing. I mean that headed "Extraordinary Rise of the Rio de la Plata," which is thus described:—

"This river, as is well known, is flooded at certain periods; and, like the Nile, inundates and fertilizes the country. The Indians then leave their huts and betake themselves to their canoes, in which they float about until the waters have retired. In April, 1793, it happened that a violent wind heaped up the immense mass of waters of this river to a distance of ten leagues, so that the *whole country was submersed; and the bed of the river remained dry, in such a manner that it might be walked over with dry feet.* The vessels which had foundered and sunk were all exposed again; and there was found among others, an English vessel which had perished in 1762, *&c. &c.*

Now, supposing that water has always a natural tendency to find its own level, it will appear rather strange how the adjacent lands could possibly be submersed to such an extent as is here described, or to any extent at all; and the bed of the river at the same time be left dry, which certainly possesses a much lower level, as also a greater declivity towards the sea than the face of the neighbouring country. The first part of the paragraph will convey very unexpected and pleasing intelligence to the inhabitants of the right bank; for so far from the river being flooded at *certain* periods, and then, "*like the Nile, fertilizing the country* *which it inundates,*" this event occurs at very *uncertain* periods, sometimes carrying away with it, or spoiling not only the accumulated produce, but also the growing harvest of the peasant; and even then, I conceive, the fact of its being any advantage to the soil more than doubtful. That some few of the natives may, on such occasions, be obliged to retire with their families to their boats, is possibly true; but generally speaking, and particularly so in the villages, the houses are of two stories, and when washed out of the basement, they coolly retire to the one above: and so situated, all communication between the different houses is in course effected by canoes or boats. This is particularly the case at Ensinada; a spot perhaps the most liable of all others in the Plate to inundation; and where, as if the genius of folly and stupidity was destined to control every step of that ill-fated expedition, from its arrival in the river to its departure, the quondam General Whitelock, thought proper to effect his landing. The natural consequence of which step was, that he lost all his carriage-guns in the intervening marshes between that village and the Barraccas.

The fact is simply this; the tides in the Plate being extremely weak, the stream is powerfully acted upon by the prevailing winds: and the waters increased or otherwise, in proportion to their strength and duration. Thus, a strong and long-continued easterly wind will, by assisting the flow from the ocean, and proportionably retarding the body of fresh water from the many tributary streams on the upper part of the river, always occasion a considerable rise: and after heavy rains, when the freshes come down strong, will frequently inundate most of the low lands on the right bank. The left bank of the river is higher; in some places, (Monte Video, for instance,) almost mountainous: I should, perhaps, rather say hilly: and is, consequently, less subject to inundation.

The westerly winds, on the contrary, which are the most remarkable and powerful in this part of South America, impelling by their violence the body of water from the upper part of the river towards the sea, much faster than its place can be supplied from the tributary streams, will cause a sudden fall of the water, frequently of several feet. And, even after the first violence of the storm has passed away, should the wind continue from the same quarter, it will, perhaps, be days before the river attains again its usual depth. It was, in consequence of a storm of this description in 1793, (a most violent pamparro,) that the extraordinary fall alluded to in the paragraph above quoted, occurred; and which I have frequently heard described at Buenos Ayres by those who had witnessed it; but I never before heard that the country was at the same time inundated. Indeed it usually happens on such occasions, that the small streams which empty themselves into the Plate are left entirely dry; and I have frequently found the little harbour of Ensinada rendered difficult of access even by a boat; while the Barraccas, which with easterly winds I have known to receive a light ship of five hundred tons, after a strong westerly wind, scarcely possible to be entered with a canoe. It should be observed, that the river between Buenos Ayres and Monte Video is excessively shallow, having barely three fathoms with ordinary tides, in the channel, which is also narrow. The middle of the stream is filled with extensive, dangerous, and, I should think, increasing banks, having only a few feet water upon them, and with low tides are frequently dry.

The wind above alluded to, is called by the natives *Pamparro*, from *Las Pampas*, those immense plains which it traverses in its course; throughout which it takes up, and carries with it through the air large quantities of sand, soil, and vegetable refuse. Meeting with nothing throughout the immense space, and to the traveller, apparently interminable level, capable of arresting its progress, or diverting its direction; it rolls on, gathering increased strength, density, and velocity in every league; throwing down houses, and frequently scattering ruin and destruction upon the villages over which it passes. To the seaman, who is unacquainted with the coast, these storms are particularly dangerous, as they generally come from the opposite direction to that from whence the wind is blowing; and are seldom preceded by any warning calm, or threatening appearance of the atmosphere, but burst at once with terrific violence upon the unsuspecting mariner, who may thus, if unprepared to resist its fury, see in one moment his lofty ship a wreck upon the face of the waters. One circumstance alone gives the experienced seaman or landsman warning of its approach. A slight arch of dusky white is first discerned in the horizon; this increases, and expanding as it approaches, the clouds, of which it appears composed, roll gradually towards you. Meanwhile the atmosphere is clear, even perhaps within the circumference of the threatening arch, and the wind steady from the opposite point from which the danger is to be apprehended. At length the hollow murmuring of the approaching storm is heard; and in a moment it bursts upon you in all its violence, covering the decks with sand and dirt, even when many leagues from the land. Happily, the extreme violence of these winds is seldom of long duration, scarcely ever lasting more than half an hour, when they subside; frequently, however, ending with a strong gale from the same quarter of the compass.

I remember in 1808, when in his Majesty's late ship the Agamemnon, in going down the coast from Rio de Janeiro, we experienced, for the first time, the effects of a Pamparro. The writer was at that time a very young man, but the circumstances are full in his recollection; and the attendant phenomena were nearly as I have described above. None of the officers were previously acquainted with the coast; the ship, bound to the Plate, was running down before the wind, going between nine and ten knots through the water, with a steady, moderate breeze between the north and east; studding sails on both sides, and, with the exception of the threatening cloud above-mentioned, which none on board then suspected indicated danger, a perfectly clear atmosphere. In one instant the gale burst upon the ship; she was taken a-back; the studding-sail-booms snapt in pieces, and she was forced with such velocity a-stern, that, notwithstanding her previous head-way, the sea came half-way up the ward-room windows. Fortunately, she paid sound off, the sails filled, the sheets and halyards which had been let go, rendered; the sails were quickly taken in; and no injury, save the loss of studding-sail booms, was sustained. This occurred in the middle watch,—the whole was but the work of a minute; and in course there was only the regular watch on deck: but the Agamemnon possessed at that time one of the very best ship's companies in the service.

Sunderland, Nov. 19th, 1825. J. C. W.

14. *Remarkable Phenomena observed in the Island of Melida, Province of Ragusa.*—The phenomena referred to were described by Dr. Stulli to the Editors of the *Bibliothèque Universelle*, under the date of June 4, 1825.

The Isle of Melida is situated in the Adriatic Sea, opposite Ragusa, in the latitude of 42° 30′ N. Its length is 7 leagues, its greatest breadth about 1 league. Towards the middle of it is the Valley of Babino-poglie, about half a league in extent, and surrounded by mountains of considerable elevation. A village of the same name is situated in the centre of the valley.

At break of day on the 20th of March, 1822, there was heard for the first time at Babino-poglie, a noise resembling the sound of a cannon; though it appeared to be the result of distant explosions, still it occasioned a degree of vibration in the doors and windows of the houses. From that time the noise was heard daily. For the first three months, it was supposed by some to be produced by a vessel practising in the open sea, or in one of the Dalmatian ports; others thought it the noise of Turkish artillery practising at some frontier village: a proof that no local movement of the earth or atmosphere accompanied the sound. After a time, persons were posted on the neighbouring heights, but they could not make out the direction of the sound; it seemed to come from all points. The effect, however, was found to be most sensible at Babino-poglie, and almost null at the extremities of the island. The pastor of the island descended into various deep and spacious grottoes, but perfect silence reigned there.

The number of detonations per day varied from four to six, and even at times, to a hundred; their force increased now and then until they resembled the noise of large artillery. They occurred in all seasons, at all hours of the day, in fine and in stormy weather, during the flux and the reflux of the sea, and whether it were calm or agitated. The most violent were in August, 1823; no rain had fallen for the preceding four months, the rivulets were dry, and the rivers very low.

The noises occurred until February, 1824, when they ceased for seven months; the detonations recommenced in September of the same year, and continued until the middle of March, 1825, though more feeble and rare of occurrence; since which time they had ceased, though perhaps only for a limited period. There were during the phenomena intermittences of several months; these never took place except after very strong detonations, and the sounds recommenced suddenly and with intensity. The present cessation, however, was after sounds more and more feeble. The last sounds were profound and dull, like the echo of the report of a cannon, and gave an idea of the gradual diminution of the cause of this extraordinary effect.

The detonations were never accompanied by any luminous phenomena of a meteoric nature, nor was any local modification of the atmosphere observed during their continuance, nor accumulation of clouds, nor impetuous winds, nor great claps of thunder, nor abundant snows, nor adverse winds; the barometer and electrometer never exhibited any peculiar movement. The nature of the sound did not indicate a subterraneous cause, but rather an explosion in the ambient atmosphere.

No probable explanation of this curious natural phenomenon has yet been suggested.—*Bib. Univ.* xxix. 267.

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15. *Edinburgh Prize Essay.*

Royal Medical Hall, Edinburgh, 22*d Nov.* 1825.

The Royal Medical Society of Edinburgh propose, as the subject of their prize essay, the following questions:

1st. What is the respective agency of the veins and the lymphatics in the process of absorption?

2nd. By what means or mechanism do these vessels accomplish this process? What are the proofs which shew that the substances absorbed are taken up by open mouths or orifices, or pass through the coats in the manner of imbibition or transudation?

3d. Is there any reason to believe that the individual animal tissues possess a distinct power of absorption, or that this process is influenced by the nature of the animal tissues?

The sum of twenty guineas, a medal, or a set of books of that value, will be given to the author of the best dissertation on the subject proposed by the society, for which all men of science are invited to compete.

The dissertation may be written in English, French, or Latin, and must be transmitted to the secretary on or before the 1st of December, 1826, and the adjudication of the prize will take place in the last week of the month of February following.

To each dissertation must be prefixed a motto, which must likewise be written on the outside of a sealed packet, containing the name and address of the author. No dissertation will be received with the author's name affixed; and all dissertations, except the successful one, will be returned, if desired, with the sealed packet unopened.

NICOLSON BAIN, *Secretary.*

Copy of a Letter from John Pond, Esq., A.R., *to* Mr. South, *relative to the Star ζ Bootis*

"Dear Sir,—I first observed ζ Bootis to be a close double star at Lisbon, in the year 1795, with a seven-feet reflector, which the late Sir W. Herschel made for me expressly, with the greatest care. I immediately communicated the circumstance to him, and he wrote me in answer, that in consequence of my information, he had examined the star, and found it as I described, but reckoned it at that time among the most difficult. I brought the same telescope to England, and used it afterwards in Somersetshire for several years, but could never again see the same star distinctly double, which I attributed to change of climate; the star is now much more easily seen; our transit telescope separates it with ease. I recollect at Lisbon, that it was only on very favourable nights that I could see it double. Yours very truly,

"*Royal Observatory,* "J. POND."

"*Thursday, 22nd December.*

ART. XIV.—METEOROLOGICAL DIARY for the Months of September, October, and November, 1825, kept at EARL SPENCER's Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

For September 1825.

		Thermometer.		Barometer.		Wind.	
		Low	High	Morn.	Eve.	Morn.	Eve.
Thursday	1	61	74	29.97	29.97	SW	N
Friday	2	57	68	30.01	30.05	NW	NNE
Saturday	3	58	66	30.07	30.03	N	NW
Sunday	4	48	62	29.92	29.92	NNW	NW
Monday	5	45	60	29.92	29.95	NW	NNW
Tuesday	6	39	66	29.90	29.80	NNW	WbN
Wednesday	7	46	66	29.68	29.56	W	W
Thursday	8	51	65	29.47	29.52	W	W
Friday	9	44	68	29.56	29.56	SW	SbW
Saturday	10	55	68	29.54	29.39	SSE	SW
Sunday	11	56	68	29.40	29.43	SE	SE
Monday	12	56	67	29.50	29.73	SSE	SE
Tuesday	13	50	65	29.70	29.52	E	SE
Wednesday	14	50	63	29.45	29.33	ENE	NNW
Thursday	15	60	65	29.40	29.53	NNW	WNW
Friday	16	44	71	29.63	29.64	SE	SW
Saturday	17	60	72	29.63	29.62	SSW	SW
Sunday	18	55	67	29.63	29.58	SW	SW
Monday	19	61	71	29.58	29.60	SW	SW
Tuesday	20	57	69.5	29.60	29.60	S	SW
Wednesday	21	61	67	29.44	29.44	SW	WbS
Thursday	22	55	63	29.41	29.70	W	WbN
Friday	23	40	61	29.84	29.88	W	SW
Saturday	24	54	69	29.83	29.86	SW	WbS
Sunday	25	62	69	29.87	29.84	SW	SW
Monday	26	57	65	29.80	29.85	SW	SW
Tuesday	27	49	62	29.97	30.08	W	NW
Wednesday	28	40	62	30.20	30.20	NW	EbS
Thursday	29	45	59	30.18	30.03	E	E
Friday	30	44	59	29.90	29.81	E	SE

For October, 1825.

		Thermometer.		Barometer.		Wind.	
		Low	High	Morn.	Eve.	Morn.	Eve.
Saturday	1	50	63	29.79	29.68	SE	SSE
Sunday	2	58	65.5	29.56	29.54	SE	SE
Monday	3	54	62.5	29.54	29.50	SE	SE
Tuesday	4	54	63.5	29.60	29.79	SW	W
Wednesday	5	51	65	29.87	29.90	SE	SW
Thursday	6	55	64	29.88	29.70	SW	ESE
Friday	7	54	56	29.37	29.80	S	WsW
Saturday	8	42	60	29.88	29.73	S	S
Sunday	9	46	59	29.78	29.83	SW	W
Monday	10	54	62.5	29.93	30.08	W	W
Tuesday	11	58	65	30.15	30.18	SW	WNW
Wednesday	12	47	61.5	30.07	29.90	SE	EbS
Thursday	13	55	60	29.96	30.08	W	WbN
Friday	14	39	59	30.04	30.04	SW	W
Saturday	15	38	53	30.24	30.37	W	NW
Sunday	16	35	57	30.35	30.29	NW	WSW
Monday	17	41	54	30.12	29.97	SW	W
Tuesday	18	39	53	29.97	29.40	W	W
Wednesday	19	45	50	29.30	28.88	W	W
Thursday	20	34.5	40	28.90	29.20	NNW	NW
Friday	21	35	41	29.27	29.50	NW	NW
Saturday	22	37.5	48	29.73	29.70	NW	NW
Sunday	23	31.5	53	29.98	29.98	NW	WNW
Monday	24	47	56	29.88	29.78	W	W
Tuesday	25	40	47	29.78	29.82	W	W
Wednesday	26	29	45	29.90	29.87	W	W
Thursday	27	40.5	50	29.87	29.90	NNE	W
Friday	28	48	58	29.87	29.87	W	W
Saturday	29	46	59	29.90	29.80	W	W
Sunday	30	43	58	29.80	29.62	SW	SW
Monday	31	42	53	29.78	29.84	W	W

For November, 1825.

		Thermometer.		Barometer.		Wind.	
		Low	High	Morn.	Eve.	Morn.	Eve.
Tuesday	1	45	56	29.80	29.60	SW	WbS
Wednesday	2	42	53	29.61	29.50	W	SW
Thursday	3	45	50	28.80	28.60	WbS	W
Friday	4	36	45	28.84	29.60	W	NW
Saturday	5	29	55	29.80	29.64	W	SW
Sunday	6	44	49	29.06	29.06	W	W
Monday	7	33	42	29.10	29.10	W	WbS
Tuesday	8	29	45	29.20	28.85	W	SW
Wednesday	9	36	43	28.83	29.03	W	SW
Thursday	10	38	39	28.90	28.83	NE	NE
Friday	11	35	41.5	29.26	29.40	NW	NW
Saturday	12	29	38.5	29.63	29.73	NW	NW
Sunday	13	22	38	29.87	29.78	W	W
Monday	14	33	42.5	29.72	29.78	W	NW
Tuesday	15	32	41	29.94	30.00	NW	W
Wednesday	16	29	42	30.00	30.02	W	W
Thursday	17	33	42.5	30.00	29.80	SW	SW
Friday	18	31	49	29.80	29.59	SW	SW
Saturday	19	36.5	45	29.70	29.73	W	W
Sunday	20	30.5	44	30.00	29.95	W	SW
Monday	21	44	47	29.62	29.60	W	WbS
Tuesday	22	37	43	29.60	29.83	W	W
Wednesday	23	30.5	49	30.08	30.00	W	SW
Thursday	24	42	50	30.60	29.90	W	SW
Friday	25	37	44	30.04	30.10	WbS	SW
Saturday	26	32	52	29.90	29.58	W	SW
Sunday	27	35	50	29.78	29.60	SW	SW
Monday	28	40	50.5	29.13	29.00	SW	WbS
Tuesday	29	41	45	28.82	28.70	SSW	SSW
Wednesday	30	37	42	29.26	29.50	N	NW

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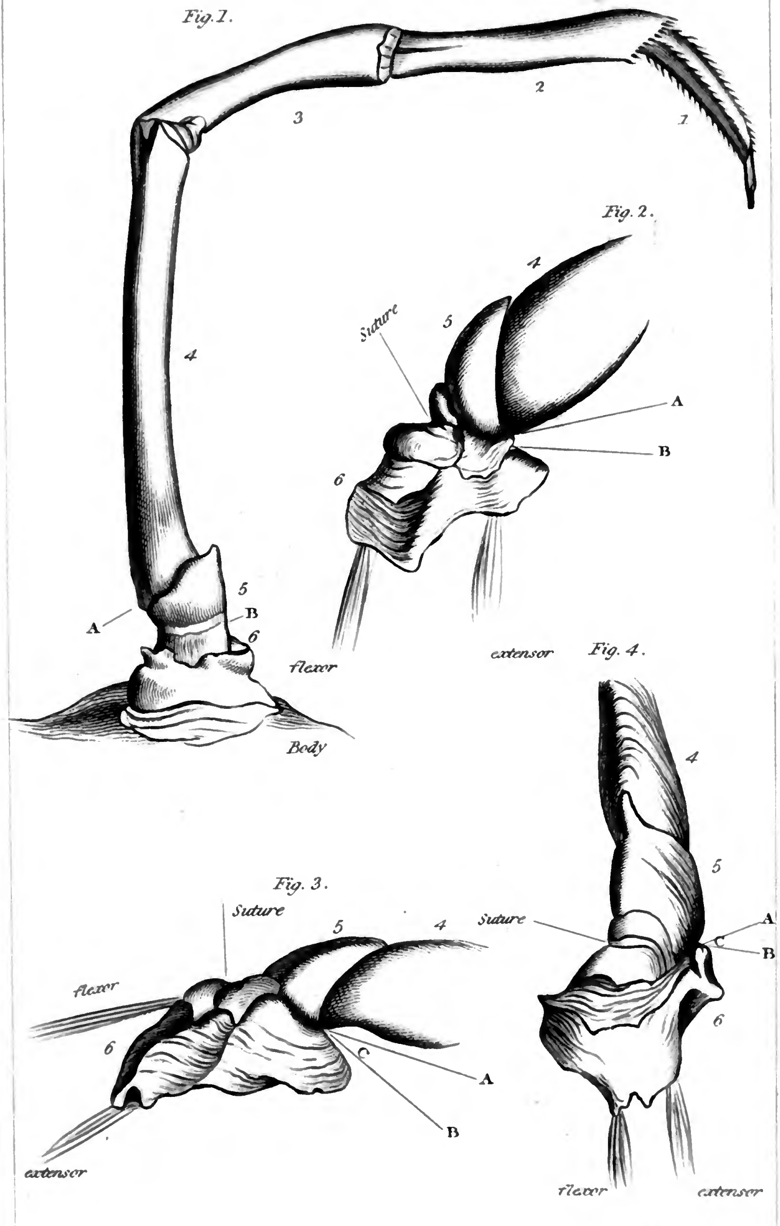

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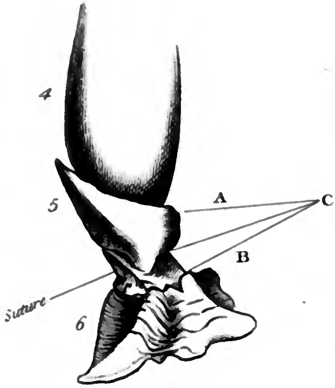

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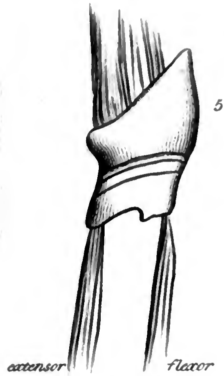

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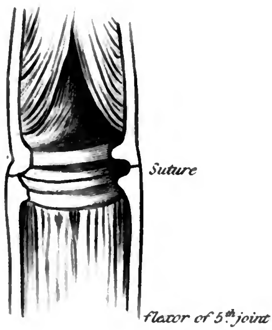

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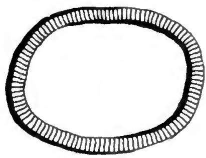

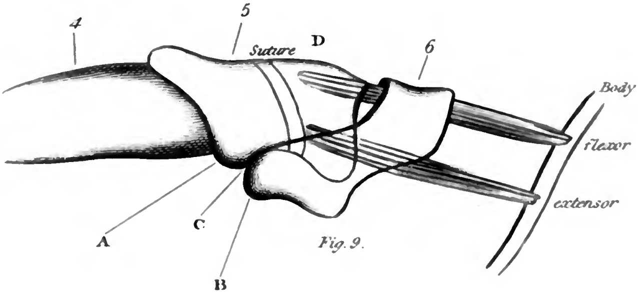

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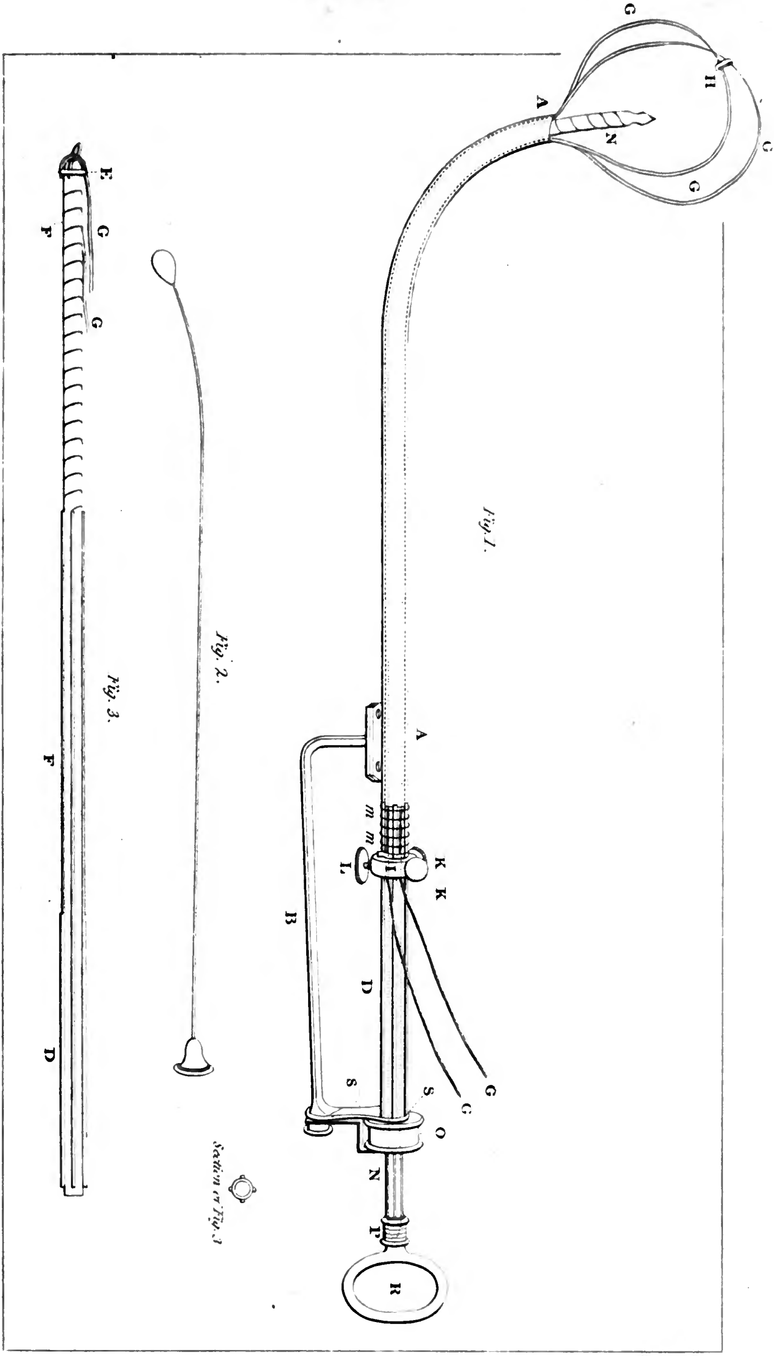

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