THE

EDINBURGH NEW PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

CONDUCTED BY

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CONTENTS.

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EDINBURGH NEW PHILOSOPHICAL JOURNAL.

On Double Stars. By M. Arago.

Many of our readers are probably aware that *L'Academie des Sciences* has recently conferred its prize-medal on Sir John Herschel, for his investigations concerning double stars. This occurrence naturally directs attention to many curious investigations. Thus it may be inquired, what are we to understand by double stars, and triple and quadruple? and how is it that these multiple stars have suddenly become so much the object of assiduous research in the observatories of both hemispheres? Finally, what results do astronomers expect to deduce from the observation of these stars? These are some of the many inquiries which are involved in this subject; and we shall now proceed to discuss them in a popular form, freeing them as much as possible, if not from all mathematical technicalities, at least from such calculations as cannot be pursued without a familiar acquaintance with the formulas of spherical trigonometry, and the elliptical motions of the planetary system.

1. *What is to be understood by double Stars?*

Astronomers give the name of *double, triple, quadruple*, &c. *stars*, to groups of two, of three, or of four stars, which appear very near each other.

When we look at the heavens with a telescope, even in those quarters where the stars are most abundant, as in the *milky way*, the stars which are embraced in the field of vision are usually distributed in a manner sufficiently uniform. The intervals which separate them are nearly equal and vastly great. The more this rule was general, the more the exceptions were likely to attract the attention of astronomers. How, for example, was it possible to avoid observing the star *Castor* (α Geminorum) which to the naked eye appears single, and which, in truth, had been so regarded by the Greek and Arabian astronomers, but which is now found, when examined by a glass of sufficient magnifying powers, to be composed of two stars of the third and fourth magnitudes?

Among the double stars which are now known, there are some, the component parts of which are exceedingly near to each other. Before they can be observed or separated, it is necessary to be supplied with first-rate glasses, of the largest dimensions, and to be favoured with conditions of the atmosphere that are but seldom found in these changeable climates. Of this kind we may name ϵ of Aries, γ of the Crown, and π of Hercules.

The late Sir W. Herschel, who first bestowed continued attention on the double stars, has divided them into four classes, not according to their intensity, but according to the angular distance, greater or less, of the two component stars. The *first class* includes all the groups in which the centres of the two stars are at least four seconds of distance the one from the other. The *second class* includes the angular distances between four and eight seconds; the *third* those between eight and sixteen; and, finally, the *fourth class* is composed of all the groups which are not comprehended in the preceding classes, and where the angular distance of the two stars does not exceed thirty-two seconds.

Herschel's first catalogue contained—

97 double stars in class	1
102	2
114	3
132	4

Making a total of 445 double stars.

Some time before his death, Herschel increased the number to above 500. Since that time it has been very considerably increased. M. Struve, after making a general examination of the heavens with an immense telescope of Fraunhofer, after conducting his investigations to all the stars of the first eight magnitudes, and even to the most brilliant of those of the ninth, which are comprehended between the north pole and 15° south of the equator, has distinguished and catalogued (those of Herschel included),

987	double stars of class 1
675	 2
659	 3
736	 4

Making a total of 3057 double stars.

This number of more than 3000 is the result of the examination of about 120,000 different stars; and thus we find that about *one star in forty* may be considered as double.*

Those observers who are favourably situated for the examination of the southern hemisphere, the astronomers of the Cape of Good Hope and of Port Jackson, have also begun to direct their attention to these multiple stars. Hence we may reasonably infer, that, in a very short time, the number of these stars that will be subjected to an annual examination in the principal observatories, will not be under 5000 or 6000.

The division of double stars into four classes, proposed by Herschel, and adopted by his successors, besides being completely arbitrary, has a defect which makes it necessary it should be wholly abandoned. In fact, we shall presently see, that according to the period of observation, we should be led to place the same group, now in the first class, then again in the second, and sometimes again in the third class.

The triple and quadruple stars do not appear to be very nu-

* This proportion, as M. Struve has observed, changes with the brightness of the stars. Thus, of 2374 stars of the first to the sixth magnitude which Flamsteed had examined in the region explored by the astronomer of Dorpat, 230 double ones were discovered; that is somewhat less than 1 in 10.

In the same region of the heavens, Piozzi has catalogued 3388 stars, which the English astronomers had in part disregarded on account of their inferior lustre. This group presents only 134 double stars; that is 1 in 25.

In repeating the process on still smaller stars, only 1 in 42 is found.

merous. The catalogue of M. Struve, for example, does not include above 52 triple stars within the limits of the scale of angular distances which compose the four classes of double stars of Herschel. Of this number we may cite ζ of the Crab, and ξ of the Balance, in both of which the component stars are all three sufficiently brilliant.

The two several stars of which the double star is composed, have, in general, very different intensities. It even very often happens that they are remarkable for a distinct difference of colour. Often the brightest of the two is of a reddish or yellowish colour; and more frequently still, the other is of a greenish or bluish shade. We shall arrange in the following table the names of a certain number of double stars which exhibit differences of colour, with the purpose of shewing that this kind of star is by no means rare, and also because they furnish to the curious, one of the most interesting subjects of examination. The observations respecting the stars of the southern hemisphere have been derived from the labours of Mr Dunlop, the astronomer at Port-Jackson and New Holland; the others are derived from the catalogues of Herschel and South.

The 35th* of the Fishes.	Larger star white;	smaller blue.	
... λ of Aries.	......	white; ...	blue.
... 13th of the Whale.	......	yellow; ...	blue.
... 26th of the Whale.	......	white; ...	greenish blue.
... γ of Andromeda.	......	orange; ...	emerald green.
... 59th of Andromeda.	Both are bluish; nearly equal in size.		
... 32d of Eridanus.	Greater star a straw colour; lesser blue.		
... η of Perseus.	......	red;	... dull blue.
... ι of Perseus.	......	white;	... bluish.
... φ of the Bull.	......	red;	... bluish.
... 1st of the Camel-leopard.	......	yellow;	... blue.
... ω of the Charioteer.	......	granite colour;	... blue.
... 62d of Eridanus.	......	white;	... blue.
... β of Orion.	......	white;	... bluish.
... δ of Orion.	......	white;	... purple.
... ξ of Orion.	......	yellowish;	... bluish.
... 8th of the Unicorn.	......	yellow;	... purple.

* It may be useful to give here the signification of the cyphers and of the letters which are employed in distinguishing the stars.

When Bayer, in 1603, published his Celestial Charts, he affixed in each constellation a letter to every star. The first letter of the Greek alphabet α was placed near the most brilliant star *of the constellation*; the second, β, at the next most brilliant; and so on to the last letter ω. The Greek alphabet being thus exhausted, the letters of the Roman alphabet, a, b, c, and always in the same order, were next employed. More lately, the number of the stars of all the constellations having been so prodigiously increased by telescopic observations, we are obliged to place them in the catalogues with a common numeral. The numbers employed in this article, where nothing to the contrary is specified, are those of the old catalogue of Flamsteed, well known under the name of the British Catalogue.

The 38th of Gemini.	Greater star white;	lesser blue.
... ζ of Gemini.	...... white;	... blue.
... α of the Lion.	...... white;	... bluish.
... 2d of the Canes Venatici.	...... red;	... blue.
... ι of Cancer.	...... a beautiful yellow;	... indigo blue.
... ε of Bootes.	...... yellow;	... greenish blue.
... ζ of Bootes.	...... white;	... deep blue.
... ζ of the Serpent.	...... both blue.	
... ξ of the Crown.	...... white;	... blue.
... β of the Scorpion.	...... white;	... blue.
... κ of Hercules.	...... white;	... reddish.
... 43d of Hercules.	...... red;	... bluish.
... α of Hercules.	...... reddish;	... green.
... ο of Opiachus.	...... red;	... blue.
... 53d of Opiachus.	Both blue; very unequal in size.	
... ν of the Dragon.	... bluish; have the same intensity.	
... α of the Serpent.	Greater star white;	lesser blue.
... 12th of Berenice's Hair.	...... white;	... red.
... 24th of Berenice's Hair.	...... reddish;	... beautiful green.
... ξ of the Great Bear.	...... white;	... bluish.
... 55th (Bode) of Berenice's Hair.	Both bluish; and of the same intensity.	
... ξ of Lyra.	Greater Star white;	lesser blue.
... β of Lyra.	...... white;	... blue.
... ο of the Dragon.	...... deep red;	... blue.
... θ of Lyra.	...... white;	... blue.
... β of the Swan.	...... yellow;	... intense blue.
... ψ of the Swan.	...... white;	... clear blue.
... 28th (Bode) of Andromeda.	Both bluish; very nearly equal.	
... κ of Cepheus.	... white;	... blue.
... γ of the Dolphin.	... white;	... yellowish.
Anonymous *.	Both blue; nearly of the same intensity.	
Anonymous †.	... blue;	have the same lustre.
The 47th of Cassiopeia.	Greater Star white;	lesser blue.
... η of Cassiopeia.	...... red;	... green.
... ι of the Painter's Easel.	...... white;	... blue.
... κ of the Ceutaur.	...... white;	... blue.
... ε of the Flying Fish.	...... white;	... blue.
... κ of Argus.	...... Blue;	... dull red.
... θ of the Centaur.	...... yellow;	... blue.

2. *Why is it that these double Stars have suddenly become an object of such assiduous attention?*

It has already been remarked, that the two distinct stars which go to compose the double star, have generally intensities which are exceedingly dissimilar. Each group in which these great inequalities of intensity occur, along with a considerable difference in the distance of two stars, would furnish, as we shall presently see, a very simple means of observation by which to judge of the distance of the more brilliant star from the earth. Even

* Right ascension, 19 h. 19′; declination 20° 40′ north.

† Right ascension, 19 h. 21′; declination 36° 10′ north.

Galileo had proposed this method. Dr Long put it into practice. A little later Dr Herschel applied it to those binary groups that were catalogued in his time, and which seemed to promise the greatest prospect of a successful result. But, as often happens to every one, though all have not the candour to avow it, in seeking for one thing, the celebrated astronomer of Slough found another. He discovered that usually the stars of unequal sizes forming groups are not, as had been previously imagined, *independent* stars, *accidentally* placed in two closely approximated visual lines;—that their proximity within a very limited space is not a simple effect of projection or perspective; he discovered that these stars are associated with each other;—that they form true systems;—that their relative position is ever changing;—*that, in short, the smaller stars revolve round the greater*, precisely as the planets Mars, Jupiter, Saturn, &c. revolve round the sun.*

In virtue of these circulatory movements, the smaller star is sometimes precisely to the east, and sometimes exactly to the west, of the greater. At certain epochs this moving star will be found exactly to the north of the more brilliant one, which appears to be its centre of motion. And at other epochs, it will be seen in the opposite position, or in the south.

These simple remarks will be sufficient to verify the relative displacement of the two stars. But after that we have seen the motion, we have naturally a desire to know by what law it is effected. For the acquisition of this knowledge, we must multiply observations, and give them a precise accuracy, by the help of a plan which we shall now endeavour to explain.

Two very fine threads must be stretched through the focus of a

* Mathematically speaking, the two stars, the one as well as the other, move round the *common centre* of gravity. And yet, the usual astronomical observations exhibit only the successive positions of the smaller star *in relation to the greater*. But if we reflect on it, we shall perceive that, practically, the elements of a *relative motion*—the orbit to which the discussion of these elements will conduct—cannot also be other than a *relative orbit*. In a word, it will be the curve along which an observer situated in the greater star, and who imagines himself motionless, will perceive the lesser to move. Besides, we do nothing more when we wish to determine the orbits of Jupiter, Saturn, &c. In truth, we every day report the position of these planets in reference to the sun, without considering if this luminary has or has not an especial motion of progress in space.

telescope. The one must pass across the centre of the circular space, which is denominated the field of vision, and *must be fixed*, immovably fixed, to the tube. The other thread must turn round the same centre, so as to coincide, when we wish it, with the stationary thread, or to make with it, to the right or to the left, upwards or downwards, every imaginable angle. These angles are to be measured upon a graduated circle, placed either interiorly or on the outside of the tube.

In making an observation, the more brilliant star is first placed, as accurately as possible, at the point of the intersection of the two threads. We then turn the moveable thread till we get it accurately over the centre of the second star. Thus, in reading off the degree at which it has stopt, we know the angle which the direction of the fixed thread forms with the visual line drawn from the centre of the greater star to the centre of the less.

According to this method of preparing the telescope, and owing to the particular direction which is given to the immoveable thread, whatever the hour is at which the observation of the angle is taken, the same number is invariably found.* Moreover, if the telescope is turned towards the star at the moment when it has arrived at its greatest elevation in its nightly course,

* This result must needs astonish those who have remarked the apparent changes which the constellations undergo betwixt their rising and their setting. Nevertheless, we cannot think for a moment of doubting it. It is even the peculiar characteristic property of those instruments, known in our observatories under the name of *Parallactic instruments*. In these, the movement of the telescope is effected around a cylinder parallel to the axis upon which the starry sphere appears to execute its revolution. Well, then, turn the telescope, *thus mounted*, towards any constellation at the moment of its rising. Let us suppose that, at this moment, one of the threads which it incloses is parallel to the line which would join two of the stars contained in the field of vision. This parallelism will exist at any hour whatever that the observation may be repeated. It will be so at its rising,—it will be so at its setting;—at the moment of its passing the meridian, and at all intermediate epochs. Without doubt, betwixt the time of rising and that of setting, the line connecting the two stars will take, in relation to the horizon, positions having very different inclinations; but it will be also identically so with the thread with which we compare it, since the starry sphere and the telescope of the parallactic instrument move round one and the same axis.

We trust that these details will suffice to exhibit the possibility of the species of observations which are alluded to in the text.

that is to say, when it has arrived at the meridian, the immoveable thread is horizontal.

The instrument, then, which is employed, gives for the moment in question, and for the moment of the passing the meridian, the *angle* which a *horizontal* line proceeding from the greater star, forms with the straight line that joins this same star to the smaller. This is what is called the *angle of position.*

According to this method, the observations of different astronomers, of different days, of different years, become objects of comparison amongst themselves. The table of the successive values of the angle of position, teaches at a glance whether the lesser star revolves round the greater from west to east, or from east to west; if the movement is uniform or otherwise; and what are the points of the greatest and the least rapidity.

Another instrument composed of two threads, the one fixed, the other moveable in parallelism with the former, an apparatus which bears the name of a *micrometer*, enables us to ascertain if the apparent distance of the stars be constant or variable, and, when there is a variation, to what extent this variation is confined.

This is all that observation supplies. But these data are amply sufficient to enable us to determine, by the assistance of calculation, the form of the curve described by each star, also to know whether this curve is circular or elliptical; and in the latter case what is the extent of the eccentricity.

Four values of the angle of position, and of the apparent micrometrical distances corresponding to known epochs, are, in general, necessary to determine the form and the position of the curve which the lesser star describes around the larger.

When it happens that the plane which contains the curve passes through the earth, the movement of the satellite star *seems* to take place along a straight line; and then there are not successive angles of position to be measured; all is reduced to micrometric observations of distances, and *five* of these observations are necessary to arrive at the results which the *four* supply in the preceding hypothesis.

Finally, if the observer is not supplied with a micrometer, and can thus observe only angular displacements, *six* angles of position corresponding to known epochs, will be indispensable in calculating the form of the orbit of the lesser star.

It was not meant to give in this place even the slightest idea of the algebraical calculations which serve for the solution of the problems relative to the form and the position of the orbits of double stars. We must be content with reporting the results. The first to which we have arrived, viz. the elements of the orbit of the stellary satellite of ξ of the Great Bear, have been obtained by M. Savary, of the *Bureau des Longitudes*, by methods which are peculiar to himself. The others are the results of the labours of MM. Bessel and Encke, and of Sir John Herschel.

Names of the Double Stars.	Period of a Revolution of the lesser Star round the greater.	Half the Greater Axis, as seen perpendicularly from the Earth.	Eccentricity of the Orbit.*
η of the Crown, . . .	43 years.	"	"
ξ of the Great Bear, . .	58 ...	3.8.	0.42.
70 of Ophiucus, . . .	88 ...	4.4.	0.47.
Castor, ...	253 ...	8.1.	0.76.
ς of the Crown, . . .	287 ...	3.7.	0.61.
61 of the Swan, . . .	452 ...	15.4.	
γ of Virgo, ...	629 ...	12.1.	0.83.
γ of the Lion . . .	1200 ...		

Amongst these stars, there is one, the consort of η of the Crown, which has accomplished the entire circuit of its orbit, since Sir W. Herschel determined, for the first time, its angle of position. It is already considerably advanced in its second revolution. The oldest observations of the Great Bear, re-

* This column contains the ratio of the eccentricity (that is the distance of the *centre* of each *ellipse* from the *focus*) to *half the greater axis*. In our solar system, the greatest values of these ratios are, for Mercury, 0.21 ; for Pallas, 0.24 ; for Juno, 0.25. In the other eight planets, the eccentricity is in each less than 0.1. The orbits of the seven stars exhibited in the above table are, then, much more elongated, much more different from a circle, than those of the eleven known planets. This result is certainly worthy of remark, but it ought not very much to astonish us. The masses of the planets of our system are but very small fractions of the mass of the sun ; whilst in the double stars, the satellite star and the central one may be bodies of equal dimensions, or at least of the same class of magnitude. We may add that agreements of this sort may one day become the true touchstone of cosmogonic theories.

garded as a double star, are of 1782. The duration of the period being fifty-eight years, the stellary satellite of ξ will have accomplished its entire revolution, under our eyes, in 1840.

It has just now been remarked (p. 1.), that if it happened that the plane in which the orbit of the lesser star is contained should pass through the earth,—that if this orbit, to use a mechanic's term, should present itself to us, by *its edge;* the satellite star would appear to move, sometimes in one course, and sometimes in the opposite, but always *along the straight line* passing through the greater star. This variety has offered itself to the attention of astronomers.

According to Sir W. Herschel, the star τ of Serpentarius is double. At the epoch at which this great astronomer formed the first catalogue of multiple stars, the two distinct stars which compose τ were sensibly separated. At present they are so thoroughly confounded,—they lie so exactly the one over the other, that Struve himself, though using the great telescope of Fraunhofer, has not been able to discover the slightest semblance of its being double. What would Bradley, Lacaille, and Mayer have said, if, in their day, any one had taken upon him to assert that, in that firmament which they had so thoroughly examined, there were occultations of some stars by others.

ζ of Orion has presented the counter-part to π of Serpentarius. At present it is a double star very easily cognizable. Herschel formerly put it down in his catalogue as decidedly simple.

In γ of Virgo, the plane of the orbit has such an inclination to the visual line proceeding from the south, that the distance of the satellite star from the central star which, in 1756 was 6″. 5, has been reduced, in 1829, to 1″. 8. Since this latter date, the distance is again sensibly increased.

The branch of astronomy which treats of the displacement of the stellary system, is but of yesterday. We are not, then, to be astonished, if little be known concerning the relative movements of *triple* stars. Already, however, observations have shewn, that in ζ of the Crab, the two inferior stars revolve round the principal one. Regarding ψ of Casiopeia, which is composed of one star rather brilliant, and of two smaller ones extremely near each other, it will probably be discovered that these latter revolve round each other, and at the same time revolve together round their more brilliant companion.

3. *The consequences which result from the nature of the motions which are observed in the Double Stars. And first, As they relate to the universality of the Newtonian attraction.*

The algebraical formulas, by the aid of which we have succeeded in elucidating all the curious elliptical movements of the double stars, have been wholly *based upon the hypothesis*, that the greater and lesser stars attract each other in the inverse ratio of the square of their distances. The determination of the orbit of each star requires only four, five, or at most six measurements of the angle of position, together with the apparent distances. With regard to any observations which have not been employed in these primary calculations, whether they be anterior, posterior, or intermediate, they become so many means by which to submit to a delicate and decisive proof, the hypothesis with which we started. It is sufficient to investigate if they be in agreement with an orbit which cannot be the true one, if we have deduced its form from an erroneous supposition. We may moreover add, that many of the comparisons have been made betwixt the really observed positions of the satellite stars, and the positions inferred from calculated ellipses. And any discrepancies observed, have only been of that minute and trifling character which is nearly inseparable from this difficult kind of measurement.

Hence, it follows, that in admitting that even to the apparent confines of the visible world, there exists an attractive power which operates in the inverse ratio of the square of the distance, those who calculated the orbits of the double stars only assumed what was true;—it follows, that the stars are governed by the same power which, in our solar system, presides over all the motions of its planets and their satellites;—finally, it follows, that this celebrated Newtonian attraction, the *universality* of which has not hitherto been established beyond the limits included within the revolution of the planet most removed from the sun, that is to say, of Uranus, *becomes universal*, even to the grammatical acceptation of the word.

It is not to be supposed, that without any hesitation we might have given this indefinite extension to the discovery of Newton. The existence of attraction in all parts of the system, formed by the sun and the planets which surround him, was a cardinal fact whose laws we had discovered, and whose consequences we had followed with a success truly wonderful. But it did not thence follow that an attractive power was inherent in matter, and that large bodies might not exist in other regions and in other systems, without mutually attracting each other. Reason, however boldly strong, had no right to pronounce upon the universality of the law of the square of the distances. Now, however, we repeat it, thanks to the investigations concerning double stars, this hesitation is for ever removed. And this alone would suffice to justify the lively interest which the relative displacements of the stars have excited among astronomers.

We now proceed to exhibit, in the succeeding chapters, some of the other results which this new branch of the science promises to disclose.

4. *When the distances of the Double Stars from the Earth shall have been determined, then the masses of those of the Stars whose relative motions are known, may be easily compared with the mass of the Earth or Sun.*

Of all the results which constitute the glory of modern astronomy, none so much strikes the imagination of those who are not acquainted with the laws of celestial mechanics, as the determination of *the masses* of the stars. Thus, if it happen that a professor, whose business it is to analyze the various wonders of the firmament to his auditory, is guilty of the mistake at the commencement of a discourse, of citing the numerical values of the planetary masses;—if, for example, he says, we shall now prove that, supposing we could place the sun in one scale of a balance, to establish the equilibrium, it would require us to place in the other scale 337,000 globes equal in size to the terrestrial globe, —a lively feeling of incredulity would immediately pervade the auditory; and if subsequently there were any listeners, it would be only to judge of the lecturer's ability to develope a sophism. This is nevertheless the subject to which the natural order of ideas inevitably conduct us. And we feel, without having recourse to any algebraical formulas, that we cannot deny ourselves the gratification of furnishing to our readers an idea, sufficiently correct, of the method by whose help we are enabled to *weigh* the planets. And were we here to reveal all our mind, it would be seen, although we have really to discuss all the fundamental principles of the theory of attraction, that we less fear not being understood, than to hear it remarked by those who shall have the patience to follow the demonstration to its close, " Why? is that all!"

A body left to itself falls to the earth; but an inert body, that is to say, one having no will of its own, cannot move, cannot fall, or rise, or stir in any direction, unless some power forces it. All the elements of this power emanate from the material particles which compose our globe. Their whole effect, their result, is what is called attraction—gravitation—weight.

The *total* force which solicits any individually attracted particle, being the sum of the actions *of each* material particle of the attracting body, it will be, so far as its intensity is concerned, proportional to the number of their ultimate particles. Thus suppose that the earth, without any change of its dimensions, were to become *one hundredth part* more compact,—that it were to come to include *a hundredth part* more matter in the same volume, its attractive force over the bodies placed on its surface would become *one hundredth part* greater than it was before.

There is now, then, no difficulty in comprehending the expression so often used—*that the attraction is proportional to the mass!*

Inquire we now, how a variation in the mass, or, which is the same thing, in the attractive power of our globe, will manifest itself? We answer—by a correspondent variation in the velocity of falling bodies. This velocity (which during exceedingly short periods may be considered as uniform) ought in effect to be proportional to the power which produces it; in other words—the power is as the mass. The velocity, then, will also be proportional to the mass. Under present circumstances, a heavy body, at Paris, passes, in the first second of its fall, through ($4\frac{9}{10}$) 4·9 metres. But if the mass of the earth were to be augmented *one hundredth part*, the space gone over in this first second would be increased by *a hundredth part:* instead of 4.9 metres, we should find it measured 4·9 and .049; which together make 4·949. Is there now, then, any difficulty in perceiving, how the velocities will lead to a valuation of the masses? But let us continue.

The space through which a body falls, by the earth's attraction, in the interval of a second, diminishes in proportion as we are elevated above the surface. It is sensibly less at the summit of a high mountain than at the level of the ocean. The power which engenders this velocity, or, we would rather say, the attractive force inherent in the material particles, *diminishes* then *when the distance increases.* And the law, according to which this diminution operates, was desiderated. But Newton has made this grand discovery; he demonstrated, that, at twice the distance, the attractive power of a body is two multiplied by two, or four times less than before; that, at thrice the distance, it becomes three multiplied by three, or nine times less than it was; and that, at ten times the distance, it has only one hundredth part (10 multiplied by 10) of the attractive power. As, then, in arithmetic, we call *the square* of a number the product of a number multiplied by itself, we combine all the individual results in this general formula: *The attractive power of a body diminishes in proportion to the square of the distances.*

We shall presently perceive, that the measures of velocities may lead to the determination of the masses. In the mean while, let us recognise that it is necessary not to forget, how far the experiment regarding velocity has been conducted.

We shall now retrace our steps for a moment, to remove a difficulty which may occur to the mind of the reader, as to the mode of taking the value of distances, in those cases where the attracting bodies are of very large dimensions.

When any small body, after having been elevated, we shall say to the height of 30 feet, is abandoned to itself, and so falls, we are convinced that this happens in virtue of the *individual* action exercised by *each* of the material particles of which the earth is composed. But these particles are not, one and all, at the same distance from the falling body. The particles at the surface, to which it vertically corresponds, are, according to our hypothesis, removed only 30 *feet.* In addition to this, there is a distance of 1600 *leagues* to the central particles, and nearly twice as much to those situated at the opposite side of the globe. It truly seems next to impossible to draw any thing like a simple result from the aggregate of the action of so many millions of particles so differently placed. The problem is, in truth, insoluble, when the attracting body is of an irregular form. But when, on the contrary, this form is spherical, the calculation becomes of a remarkable simplicity. For Newton has proved, that *the material particles, when uniformly distributed in the shape of a sphere, act, on the whole, upon a point exterior to them, as if they were all united in its centre.*

Thus, then, so far as we have to do with bodies which are accurately spherical, or nearly so, we shall have no need to occupy ourselves with the distances, whether great, lesser, or least, of the different attracting particles, to the point attracted. All will then turn out accurately, as if the sum-total of these particles were actually at the centre of the sphere; there will only be, in virtue of an abstraction which the theorem of Newton legitimately involves, a single distance to consider, viz. that from this centre to the point that is attracted.

There is yet another point, which, before proceeding to the question of celestial physics, which is the proper object of this chapter, it is necessary for us to examine, viz. How the attractive force of the earth exerts itself, not so much on a body in repose, as on a body in motion.

Let us suppose that a cannon, placed at a certain height, has been pointed in a direction perfectly horizontal. The bullet would fly from the piece horizontally. But every one knows, that it would very soon leave this direction,—that it would gradually descend,—that, at length, it would fall to the earth. Nor is it doubted by any one, that this gradual descent of the bullet is the effect of the attractive power of the earth. It is not, however, so generally known, whether this attractive power is modified in its effects by the rapidity of the course of the bullet. A very simple experiment will teach us.

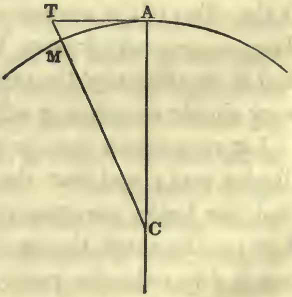
Let us suppose that, in front of the cannon, there is a perpendicular wall; and that the distance of this wall shall be such, that the bullet occupies *exactly a second* in flying to strike it. Let us also mark the exact point to which the axis of the cannon is directed—the point which the bullet would strike, if it moved in a straight line—if, during its course, the earth did not attract it. The vertical distance between the point thus marked, and that point, somewhat lower, at which the ball would really penetrate the wall, is the measure of the effect which gravitation produces in the interval *of a second,* upon a body which moves with so great a horizontal velocity. The experiment gives for this distance 4 metres 9 c.;—precisely the distance which the bullet taken up so high, and then abandoned to itself, falls vertically in the same time.

Let us now place the wall at a somewhat greater distance from the cannon. Let us suppose that the bullet does not reach it till at the end of *two seconds.* The point which this bullet will strike, we shall find much farther below the fixed point than in the preceding experiment. But the distance betwixt the two points will be exactly equal to the vertical descent of a body, which, left to itself, is, for *two seconds,* subjected to the attraction of gravitation.

In general terms, *the attractive power of the earth produces precisely the same effect upon a body at rest, and a body in motion, when this effect is measured in the direction corresponding to that in which the attraction is excited.*

The moon will now furnish us with an additional opportunity of verifying this last law, and that of the diminution of the attractive force in the ratio of the square of the distances. The moon, in truth, in the eyes of an astronomer or geometrician, is nothing more than a projectile, which, at the creation, has been launched with a force sufficient to circulate indefinitely around the earth, as would now happen, without the presence of an atmosphere, to a bullet projected horizontally from off the surface of our globe with sufficient velocity.

Let C, for example, be the point occupied by the earth, round which the moon revolves from right to left, and A be the position of this luminary. At the instant of quitting the point A, the moon is moved in the direction of a small *element* of its curvilineal orbit, which passes through the point A, that is to say,



in the direction of the straight line, the tangent A T. It is not, however, in the point T that the moon will meet the radius CT, (in place of the radius we had almost said the perpendicular wall CT, as in the case of the bullet), it is in M that the two meet. But the moon could not quit the direction AT, according to which it was moving, unless some power had turned it aside from this first course.

But it is to be remarked, that this power is the energy of the attraction of the earth placed at C;—that this power, in acting upon our satellite during the time it required to transport itself from the radius CA to the radius CMT, has attracted it,—has made it to fall the length of TM,—the distance, so to express it, from the fixed point T to the point M, which is really struck by the *projectile moon.*

To demonstrate the proposition, is to make the following observations and calculations. By the help of a direct experiment, we determine the angle which the radius CA, directed from the earth to the moon at a certain epoch, forms with the radius CM carried towards the same luminary, *a second of time* afterwards. The radius CA, that is the distance from the moon to the earth, is known in leagues and yards. Hence it ought to be, or rather it in fact is, easy to calculate for the angle ACM, the measure of the angular displacement of the moon in the interval of a second, how much the point T, the extremity of the tangent, is distant from the point M, situated upon the small arc of the circle AM, that is to say, by what fraction of a yard the moon has fallen towards the earth in a second of time.

The space through which a body falls in a second, when it is left to itself at the surface of the earth, when, in other terms, it is 1600 leagues from the centre, is 4.9 metres. That we may obtain the distance it would fall, if it were removed from this same centre, even to the distance of the moon, we reduce the preceding number in the ratio of the squares of the distances.

The result of this very simple calculation is found to be, with an astonishing degree of accuracy, the numerical value of the distance MT, such as it has been deduced from the velocity of the moon, and the dimensions of her orbit. Thus it is nothing but the power whose effects we daily observe at the surface of the earth,—the power to which the falling of a body is owing, that maintains our satellite in the curve which it describes around our globe. This power alone, compared with its intensity at the surface of the earth, shews itself there diminished in the proportion of the squares of the distances; and we repeat it, without our being required to take into consideration the state of motion of the moon.

With these preliminary ideas, we may now address ourselves to the question of the determination of the masses of the celestial bodies.

Suppose that we set about finding how much more of a mass the sun is,—how much more matter it includes—than our globe. We shall take the space 4.9 metres, which a body falls at the surface of the earth in the interval of a second; we shall reduce it in the proportion of the squares of the distances, so as to know what would be (always by the action of the earth) the fall of this same body, if its distance should become equal to that of the sun. The result of this simple calculation will be proportional to that of the mass of the earth. A luminary which, *at the same distance*, would induce, towards its own centre, a fall double, triple, or a hundred-fold—would evidently be a mass double, triple, or a hundred-fold that of the earth. The question is thus brought to this, How much does the sun, in the interval of a second, cause to fall, towards its centre, a body which is removed from it as far as our globe? Moreover, this last question, which, at its announcement, might appear unanswerable, since we are not able to transport ourselves to the surface of the sun, there to make experiments on the falling of heavy bodies, finds its solution direct and immediate in the circumstances of the annual motion of the earth.

In virtue of this motion, our globe describes round the sun, in 365¼ days, an almost circular curve, the radius of which is 39,000,000 of leagues. Let us divide the 360° comprehended in this circle, by the number of seconds contained in 365¼ days. The quotient will be the very small fraction of a degree, which the earth goes round in its orbit in a second of time. Let us now look back to the figure on page 16. Let us suppose the sun in C; the earth in A; let us consider the angle ACM equal to the angular displacement which the earth undergoes in a second; the radius of the orbit CA of the length of 39,000,000 of leagues, and we can then easily calculate in fractions of a league or in yards, the distance TM, which the sun, by his attractive power, causes the earth to fall in a second. We have recently determined this space for our globe. We have seen how much it would cause to fall in the same interval of time, a body which would also be at the distance of 39,000,000 of leagues. The distances in these two cases being equal, the falls must needs be proportional to the masses. In searching, by a simple division, how many times the fall towards the earth is contained in the fall towards the sun, we should learn how many terrestrial globes it would require to make a mass equal to that of the luminary which illuminates it. It is thus fundamentally, if not formally, that the number 337,000, already cited in page 12, is discovered.

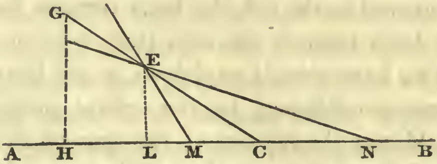
What elements, then, have we employed to arrive at this result? Only the quantity of the angular motion of our globe around the sun in a second of time, and the value in leagues of a radius of the terrestrial orbit,—none other. But the direct observation of the double stars supplies us with the angular velocity of the lesser star round the greater; if we had, *in leagues*, the radius of the orbit which this lesser star runs, we might easily find what is, in the fraction of a league, or, in yards, the distance which it falls in a second towards the central star. This distance, compared to the fall of a body towards the earth, or to the fall of a body towards the sun, since previously the three numbers, as we have already explained, would have been reduced to a common distance, by the inverse proportion of the squares, would give the relation of the mass of the greater star to the mass of the earth, or to that of the sun. Hitherto, unfortunately, we have known, respecting the radii of the orbits of the stellary satellites, only the angles which they subtend, as seen from the earth. To transform these angles into measures of length in leagues and yards, it would be necessary to have the value of the distances which separate us from the stars. When these distances shall have been determined, the radii of the orbit in leagues may be deduced, and the remainder of the calculation will be accomplished without difficulty.

The science, in being enriched with a knowledge of the movements of the double stars, has made an immense stride in the solution of a problem which seems far removed above the intelligence of man. The day in which the distance of a double star shall be determined, will be the day in which it may be weighed, in which we shall know how many millions of times it contains more matter than our globe. We shall thus penetrate into its internal constitution, although it may be removed from us more than one hundred and twenty billions (120,000,000,000,000) of leagues.

We now hope, then, however dry this chapter may have appeared, that it will be allowed, that we have at least endeavoured to give, and without calculations, some idea of those recondite and fruitful principles, from which astronomers and geometricians deduce results altogether so astonishing*.

5. *The Observations upon the Binary Groups, composed of independent Stars, may serve to determine the distance from the earth of one of the Stars of which these Groups are composed.*

We now proceed to endeavour to furnish, in this chapter, an elementary view of the methods which astronomers have employed to ascertain the distances of the stars from the earth. We can thus bring under review the advantage of that one of those methods which is grounded upon the observation of the double stars.



Let us suppose AB an indefinite horizontal straight line, upon all the points of which, a person making observations may transport himself, furnished with an instrument which astronomers employ in measuring the angles contained in vertical planes,

* Mathematically speaking, the velocity with which a bullet falls towards the earth, depends *on the sum* of the masses, both of the earth and bullet. The fall of the earth towards the sun is also determined *by the sum* of the masses of the sun and the earth; it is from the result of these *sums* of masses, and not merely the effect of an isolated mass, that the calculation is effected. But it is evident, that, on account of the extreme smallness of the bullet compared with the earth, and of the earth compared with the sun, that we may, without any appreciable error, adopt the hypothesis insisted upon in the text. It is not, however, the same with regard to the double stars. Sometimes the satellite star differs but very little from the central one (at least if we may judge from the intensity of the lustre), so that we could not regard the result of the calculation given above as giving the *sum* of the masses of the two stars.

viz. one having a gráduated circle furnished with a plummet-line, or a level, and a moveable telescope. Let us suppose that above the line AB, there is found at the unequal heights EL, GH, two small objects E and G.

When the observer stands in C, the two objects E and G will be situated, as an effect of perspective, on the same visual ray. In the glass of the instrument, the nearer will cover the more distant; but as soon as the station C is abandoned, as soon as the observer advances or retires, this state of things will be wholly changed. If he remove to M, the object E will no longer cover the object at G: it will appear *more elevated*, since, after having seen it, it would be necessary to depress the telescope to find G. A removal, on the other hand, towards N, for example, would give an exactly opposite result; the object E, as the diagram clearly shews, would then exhibit it as underneath G.

It thus, then, is clearly established, that *the relative positions of two objects, at different distances, necessarily change when the observer alters his position.*

But it is not enough for the science to have established the existence of the *relative apparent motions*, which are determined by a change in the position of the observer; it is requisite, moreover, to know what is, in the *total apparent motion*, the part which each of the two objects plays; what is numerically the share, in the phenomenon, which the distances of these two objects from the observer bears, to the distance which he himself has gone over in the course of the horizontal line.

All this results immediately from the simple inspection of the analytic processes employed by astronomers in their calculations; but we must abstain from quoting any formulas, that we may preserve for this contribution that popular character which we have designed. We must be contented with pointing out, as clearly as it is possible, the results which follow from them.

To avoid troublesome circumlocutions, it will be proper, at once, to designate *the angular elevation of an object*, the angle which is formed by the horizon, and the visual line proceeding from the eye of the observer to that object. This is, besides, the technical expression.

Let us consider the observer again placed in C, that is to say, in part of the line AB, where the two objects C and G have *the same angular elevation*, where these objects are covered the one by the other. If he proceeds, for a certain space, towards the left, these two heights will *increase*, at the same time, but unequally. If, on the contrary, he transport himself from C towards the right, the angular heights will both *diminish*, and the diminutions will not have the same value for the two objects.

Well, then, calculation and experience agree in demonstrating, that, with regard to each angle of elevation, the variation depends, solely, *on the proportion there is between the distances of the object observed from the line AB, along which the movement is effected, and the extent of this movement.* When the extent CN, along which the observer moves, is a considerable aliquot part of the distance to the object E, the change of elevation is considerable between the station C and the station N. If, on the contrary, the line CN is almost infinitely small, compared to the distance of the points to be marked, the angle of elevation will be found to have sensibly the same value at the two points C and N. Hence it may be understood, that if two objects, E and G, which, seen from C, cover one another, the second is at an immense distance, their *relative* changes of position will depend only on the variations which, by the movements of the observer, will be effected on the angular elevation of the object which is nearest. These variations may thus be almost appreciated by the naked eye, or, at least, without the assistance of a large graduated instrument. Particular attention is requested to this remark, as we shall presently again refer to it.

It has just been remarked, that the change which the angle of elevation of an object E experiences, was dependent on the *space* over which the observer has moved, and on the *distance* EL, from the object to the line NCL, passing through the two stations. But such is the intimate connexions of these three quantities, that any two of them being given, we can always from them very simply deduce the third. Thus, when the observer, in moving from C to N, has measured with precision, by the assistance of his graduated circle, the diminution to which the apparent elevation of the object E has been subjected, two lines of calculation enable us to pass from the numerical value of this diminution, to the determination of the number of times that CN is contained in LE; that is to say, to the knowledge of the distance of the inaccessible object E, for CN is always measureable in leagues.

And now is the reader put in possession of the principle of the method which astronomers habitually employ for the determination of the distances of the celestial bodies, and which they denominate the *method of parallaxes*.

The method of parallaxes, every one may perceive, must give results so much more precise, as in moving from the first to the second station, the angular elevation of the objects shall have *more sensibly varied;* or rather, for it is the same thing in other terms, as the traversed base CN, is a larger aliquot part of the distance that is sought after, EL.

When the object E is a star; where it is the stars, the distances of which we wish to measure, we take for our first station N, one of the extremities of a diameter of the almost circular ellipse which the earth annually describes round the sun; and, for the second station, C, the other extremity of this same diameter. But the distance which then separates these two points, C and N, is about 20,000,000 of leagues. Notwithstanding, so vast a displacement does not sensibly change the angular elevations of the star. The visual rays CE and NE, extended to this star, from the two places separated by 20,000,000 of leagues, form angles very nearly equal with the line which joins the two places.

We have said the angles were *nearly equal*, for it rarely happens that there is not found, between the angular elevations measured at the two extremities of the base, discordances, often it is true, irregularities, to the extent of one, two, and even three seconds. These quantities are unquestionably very small; scarcely do they exceed the errors of the observations; and yet they are of vast importance. If any one, for example, had succeeded in assuring himself, that for a star little removed from a visual line perpendicular to the diameters of the terrestrial orbit, the angular elevation, from the extremity of one of these diameters, really exceeded by three seconds the elevation from the other extremity, calculation would give for the distance of this star from the earth (5,000,000,000,000) five millions of millions of leagues.

If the difference of the two angles was only two seconds, we should find a distance one fourth greater than the preceding.

And, finally, if any one succeeded in establishing a difference in the two angular elevations of a single second, the star would be (16,000,000,000,000) sixteen billions of leagues from the earth *.

But some who have heard of the extreme accuracy of modern observations, will be ready to exclaim against our admission of

* According to the notion, in general very plausible, that the most brilliant stars ought to be the least distant from the earth, astronomers formerly agreed to examine the parallaxes, especially of the stars of the first and second magnitude. Latterly there has been some reason to suppose that certain stars, which are little remarkable for their intensity, might probably be found amongst the nearest. We shall here mention a few of the considerations which indicate this.

Formerly the stars were called *fixed stars.* But assuredly they do not merit this appellation. All in fact progress,—all have an individual motion. Nor are we here speaking of the revolution of a smaller star around a greater, with the consideration of which we have been so long engaged ; but of a motion which, since it has been observed, has always been going forward in the same direction ;—of a motion apparently destined, in the long run, to mingle together the stars of the different constellations. It is natural to suppose that the more rapid this individual movement is, the nearer will the star in which it is observed be to ourselves. According to this principle, the 61st of the Swan, which has a proper annual motion of more than 5 seconds, naturally presents itself as probably offering a sensible parallax. With this in view, we, along with M. Mathieu, have observed it with extreme care during the month of August 1812, and during the following month of November. The angular height of the star above the horizon of Paris at the second epoch, did not exceed its angular height at the first, but by the small fraction of $\frac{6}{100}$ part of a second. An absolute parallax of a single second would have necessarily inferred a difference of 1″.2 between these two elevations. Our observations then indicated that the diameter of the terrestrial orbit,—that 39,000,000 of leagues could not be seen at the 61st of the Swan under an angle of more than *half a second.* But a base, seen perpendicularly, subtends an angle of half a second, when it is elongated 412,000 times its own length. The 61st of the Swan, then, is *at least* at a distance from the earth equal to 412,000 multiplied by 39,000,000 leagues. The number that results from this multiplication, indicates a distance which light could not penetrate in less than *six years,* though it flies, as every one knows, 80,000 leagues in a second.

Another word, and we have done. The 61st of the Swan moves every year in a right line more than five seconds. At the distance we are removed, a second corresponds *at least* to eight billions (8,000,000,000,000) of leagues. *Every year,* then, the 61st of the Swan moves at least 4,000,000,000,000 of leagues. And this is what was called a fixed star !

the possibility of errors such as two or three seconds in the differences of the measurements of the angular elevations of the same star. And, it is freely granted that, with first-rate instruments and long experience, such errors may be avoided in the mean of a great number of observations; in such, for example, as the planetary diameters. But the observations of parallaxes are a very different thing.

Let it be first remarked, that these observations require instruments of very considerable dimensions. Without this condition, a second will not be visible upon the graduated scale. It must be added, that the earth occupies *six whole months* in passing from one point of the terrestrial orbit to the point diametrically opposite;—that, if the angular elevation of a star has been measured in the first station *in winter*, it can only be measured *in summer* in the second;—that if the whole apparatus is not kept in exactly the same state *during the six months*, it will be impossible to compare the observations;—and, finally, it seems most difficult to avoid the slight bendings and smaller alterations which this instrument, so large, so massive, and composed of so many separate parts, which, in the two epochs, must be in thermometrical conditions entirely dissimilar, must necessarily undergo, &c. &c. But in spite of all these obstacles, what with consummate skill on the part of the artists, and care and patience on the part of astronomers, we can now answer for a difference of the angular elevations of the same star, observed at the distance of six months, within nearly two or three seconds.

This space, seen through the focus of the great telescopes of our graduated circles, does not equal the thickness of the thread of the spider! Can we, after this, be astonished that there was little prospect of surpassing this limit of precision by the usual modes of procedure?

We have, however, to add, that, *in certain circumstances*, which we proceed to point out, the double stars would enable us to value the change of the angular elevation, not only by three whole seconds, but even to the accuracy of *a tenth of a second*; that is to say, thirty times more accurately than has hitherto been done.

And now is the time to revert to a remark which was put in reserve in page 22, concerning the relative change in position of two objects at different distances.

This change, we then said, depends entirely on one of the two objects,—on that which is the nearest; whensoever the other is at such a distance, compared with the space over which the observer can move, that these variations of angular elevations are insensible. Then the second object becomes the most exact of marks with which we may compare the first, to recognize and to measure its changes of elevation. Then, too, there is no need either of a large mathematical instrument, which shall be unchangeable during its transport from the first to the second station, or of a level or plummet;—then to know if there have been a change of position, a simple *coup d'œil* is sufficient, when the objects are nearly in contact; and then, besides, the extent of this change is measured with the help of a little instrument known under the name of a micrometer, and which, enclosed in the telescope, is simply composed of two threads, the one fixed and the other moveable, with the help of a screw.

If it be supposed, as is natural, that the difference of brilliancy between the two stars depends, *in general*, on the difference of their distances from the earth;—that stars of the seventh, eighth, and ninth magnitude, are much more distant than stars of the first, second, and third, we shall find in the heavens many binary combinations which will satisfy the required conditions. Every amateur who is supplied with a powerful glass, may henceforward work for the determination of the distance of the stars, with as much prospect of success as those who are astronomers by profession.

This is in fact the method of observation of which we have formerly spoken. It is now evident, then, that no one should apply himself to the observation of stars which almost seem to touch. In the groups of this sort, the difference of the lustre of the two stars avails so little as it regards the difference of the distance, that, in describing its orbit, the lesser star, without ceasing to be the lesser, interposes itself betwixt us and the greater. As to the binary combinations, where a star of the first, second, or third magnitude is remarked at the angular distance of three, four, or five *minutes*, from a star of the sixth, seventh, or eighth magnitude, there are, without doubt, a considerable number where the lesser star appears so, only on account of its much greater distance. Whoever is fortunate enough to discover such a combination, if he can furnish himself with a perfect micrometer, of a somewhat considerable magnifying power, will determine the real distance of the greater star from the earth; provided, always, that this distance does not surpass that which will be traversed by the light in thirty years. It is then indeed possible, in spite of the accuracy of the method, that we then only obtain a limit *on this side of which* the star will not be situated. But, when it is remembered that each year contains 365¼ days, that each day is composed of 86,400 seconds, that during each of these periods light moves 80,000 of leagues, we shall perceive how prodigious this inferior limit of distance is, when a ray of light will not traverse it in less than thirty years!!

6. *The observations of Double Stars, properly so called, may serve one day to determine either the distances of these binary groups from the earth, or to fix a maximum or minimum limit beyond which they cannot be placed.*

The method of parallaxes has not hitherto determined more than the limit of distance (minimum distance) on this side of which the observed stars are not. Thus the angular elevations of 61 of the Swan, stated in page 20, have placed the two stars which compose this group of 412,000,000 times, *at least,* more distant than the sun. But that which it is necessary to add to this *inferior limit* to know the real distance, remains wholly unknown. If any one, for example, were to choose to suppose that the true distance of 61 of the Swan were equal to 100,000,000 of times more than the inferior limit deduced by the method of parallaxes, no one could contradict him, for this number is not more incompatible with the observations than a number a million of times smaller, or than a number a million of times greater! In this state of the science, it was exceedingly desirable to discover a method of placing a superior limit by the side of the inferior one previously discovered. And this method may, sooner or later, be deduced from the observations upon the double stars, according to a process we now proceed to disclose.

When the curve (we shall suppose it exactly circular) which the lesser star of a binary group describes round the greater, presents itself exactly in front, that is to say, when the plane which incloses it is perpendicular to a line drawn from the earth to the central star, the satellite star, throughout the duration of its revolution, continues constantly at the same distance from the earth. The satellite star proceeds, in fact, to occupy successively, in virtue of its individual movement, all possible positions on the circumference of the small circle. And no one doubts that all the points of a circumference of a circle, viewed exactly in front, are equally distant from the eye of the observer.

Through the centre of the circular orbit of the stellary satellite, let us draw a horizontal diameter, which will divide this orbit into two equal parts,—one superior, the other inferior. Let us then turn the plane in which the circuit is contained round this horizontal diameter, and in such a way, for example, that the lower part shall come to the front, or towards the observer, whilst that the upper will be carried to the back part. Viewed perpendicularly, the orbit of the lesser star was circular. Viewed in its new oblique position, it will appear elongated; and it is of especial importance to remark, that these different parts *will no longer be found at the same real distance from the observer.* In the half circle which, on assuming the perpendicular position, shall have come towards the front, there will necessarily exist a point nearer to the earth than all the others. The point diametrically opposed to this will be the most distant one. In moving from the first point to the second, the satellite star will then gradually *recede* from the observer. In returning from this second point to the first, it will *approach* towards him. This double circumstance, *on account of the appreciable velocity* of light, may bring along with it sensible differences in the manner in which the star *shall seem* to move along the two halves,—the one the ascendant, the other the descendant half of its course. Let us now proceed to examine in what forms we shall perceive a luminous star, which is endowed with a proper motion.

Let us take this star in a certain determinate position. From this position it will in all directions dart rays which will propagate themselves in straight lines, and the prolonged directions of which, whatsoever may be the place and the time in which they are observed, will indicate *the place which the radiant body occupied at the moment of their departure.*

One of these rays will arrive at the earth. Suppose it has taken a considerable time in coming,—say a month. During this time the star will not have remained immoveable; it will have quitted its former station. Thus we shall see it in this first position, when it is there no longer.

Let us now admit, that our ideas may be specific, that the star has moved, at the same time *withdrawing itself from the earth*, along the arch of a curve of a certain extent,—an arc, we shall say of a circle, which, placed obliquely in space, is nearer to us at one of its extremities than at the other.

We perceive the star moving, on this arc, at the extremity nearest to the earth, thirty days, we have supposed, after that it has quitted this spot. It follows from this, that it would require *more* than thirty days for the rays of light to reach us from that extremity which is most distant from us. The star will have reached this more distant extremity—it will have left this position *for a longer period than thirty days* at the time when, from the earth, we see it as placed in that spot. When, then, *from the date* of this latter observation, which is thus found posterior *by more than thirty days* to the date of the *real arrival* of the star at the extremity of the arc, we subtract the date of the observation of its departure, the error of which, by the hypothesis, was only, and exactly, *thirty days*, the difference will be *greater* than that which we find by subtracting the one from the other, if *the* REAL *dates of the transits* of the same star through the observed points were known.

If, instead of making the moving star to start from the point nearest to us, and so tracing it to the most distant, we had given it the reversed course: if the point of the first observation had been the more distant, it is evident, that the difference between the observed transits, that is to say the transits as influenced by the propagation of the light, instead of being greater, *would be smaller* than the difference between the real transits.

In general terms, if, in its curvilinear course, a star is gradually removed farther from the earth, the luminous rays which emanate from it come more and more tardily, to shew us in what positions it is successively placed. To go from one of these positions to the other, it will appear then to occupy more time than in reality it requires. The reverse of this necessarily happens, when, during its course, the star is approaching nearer to us. But the two halves of the orbit of a double star are in precisely the condition which we have been describing, when the plane which includes them is oblique, as it regards the visual ray proceeding from the earth to the central star. Mathematically speaking, the stellary satellite, viewed from the earth, will employ then more time to run through the ascendant half of its orbit,—the half in which it is continually removing itself from us, than to run through the other half,—that in which it is approaching towards us. Well, then, we proceed to shew, that the distance of the satellite from the earth may be deduced from the difference observed between the direction of the ascendant semi-revolution, and the duration of the opposite semi-revolution, whenever this difference shall have been observed with precision.

If we revert to the preceding explanations, it will be easily perceived, that the duration of the ascendant semi-revolution of the satellite *surpasses* the duration of the real semi-revolution, by the number of days and the fractions of a day which the light may employ to traverse the number of leagues by which the distance of the satellite from the earth is increased during this semi-revolution. It is not less evident, that the duration of the descendant semi-revolution *is less* than the duration of the real semi-revolution by the same number of days, and fractions of a day, since, in its retrograde course, the satellite approaches us quite as much as previously it had removed itself. Finally, the two observed semi-revolutions differ from each other *the double* of the time which the light takes to pass through the number of leagues by which the distance of the satellite from the earth varies in these two extreme positions.

Let us, then, subtract the duration of two observed semi-revolutions, the one from the other; let us take the half of the difference; let us, then, transform this half into seconds, at the rate of 86,400 seconds for a day; let us multiply the total number of seconds thus obtained by 80,000, the number of leagues which light moves in a second, and the product will be *the value, also expressed in leagues, of the quantity which the satellite star re-*

moves from the earth in its passage from the point of the orbit, which is the nearest to the point which is the most distant.

The position and *the dimensions* of the orbit of a stellary satellite, are connected in a necessary way with the total quantity which the satellite removes from the earth, and again approaches, during each of its revolutions. When the dimensions of the orbit are known, we may thence easily infer, by calculation, the value of the changes of the distance. And, reciprocally, from the value of these changes, we can mount up to that of the dimensions of the orbit. But we have just demonstrated how, in certain cases, the astronomer experimentally determines, in leagues, the changes which the distance of the satellite star undergoes from the earth. In these same cases, the greater axis of the elliptical orbit which the star seems to describe, may also be expressed in leagues. The inclination under which this axis presents itself to us, is deduced from the position of the plane of the orbit. The micrometer enables us also to know its apparent extent, or how many seconds it subtends. But after this, there is not a land-surveyor who does not know how to determine the distance at which he stands from a certain base, as soon as he has learnt the inclination of that base to the visual ray, its absolute length, and the angle at which he examines it. The astronomer has exactly the same calculations to make. He only operates on numbers to a vast extent greater. His base is the diameter of an orbit which is traced by a star; but still, that which he is seeking, and that which he will find, is the distance of this star from the earth.

M. Savary, to whom we are indebted as being the first to point out the important part which the successive transmission of light might one day play, as it respects the phenomena of the double stars, fearing, without doubt, that it would be only with great difficulty, on account of the slowness of the motion of the satellite stars, that we could determine, with accuracy, the difference of the duration of their ascending and descending semi-revolutions, had contented himself with presenting the observations of these durations, as a mean of arriving, not at an absolute distance, but only at a limit. We shall now show how it is necessary to regard the method, if it be not wished to carry it farther than this.

Suppose that it shall have resulted, from the minute examination of a series of measures of angles of position, that the duration of the ascendant semi-revolution of a stellary satellite *only surpasses, by twenty days,* the duration of the descendant demi-revolution, hence it follows, that the total quantity which the star removes from, or approaches to, the earth, in proceeding from the one of its extreme positions to the other, *cannot,* in its turn, *be greater* than the number of leagues traversed by the light in ten days.

Let us, for a moment, adopt *this superior limit* as the real value of the total change of the distance of the star, and let us enquire, as is done every day, for the extent in leagues, of the axis of the stellary orbit. In parting from a limit, it is a limit which we must find. Thus, the calculation will give us a number of leagues which the real length of the diameter in question cannot surpass. In other words, it will conduct us either to the real length or to a length greater than it.

Now, if we inquire, by the known methods of land-surveying, to what distance must be extended a right line of a length equal to that number of leagues which constitutes *the superior limit,* that it may appear to us under the angle which the *micrometrical* direct observations have assigned to the axis of a stellary orbit, what will be found will be, without other alternative, either the truth or a quantity greater than the truth; the truth, if the number of leagues employed has happened to be exactly equal to the diameter of the orbit, a quantity greater in every other case, since then the number on which we have operated will itself have been too great; but that it may be brought to subtend a certain determinate angle, a line must evidently be transported further, as it is made longer. We are thus, then, brought to the determination of a distance, *beyond which* we cannot suppose the star situated, without placing ourselves in opposition to the facts.

If, from another part of the investigation, the discussion of the angles of position should permit us to affirm that the duration of the ascendant semi-revolution of a stellary satellite is superior to the duration of the descendant semi-revolution, by *at least* a certain definite number of days, the calculation applied to this new result, instead of a superior limit, would lead us to *an inferior one*, that is to say, to a distance *on this side*, of which the star assuredly cannot be placed.

Every one may now understand how brilliant those discoveries may be that will reward the astronomer, who, in modifying the means of observation of the double stars actually known, shall assign, with a new accuracy, the durations of the ascending and descending semi-revolutions of the stellary satellites. The discovery of the distance of the stars, and the determination of their masses, will become the prize of such merit!

7. *Concerning the Colours observed in the Multiple Stars.*

When we remark, in the catalogue of double stars, that there are so many binary combinations of red and greenish-blue, and of yellow and blue, it naturally occurs, that the blue and green tints *of the lesser star* are not the real shades, but the result of an illusion, the simple effect of contrast. This opinion might be supported by the observations which are found in all treatises on optics, concerning *accidental colours*. In these works it is stated, that *a feeble* white light appears green on the approach of *a strong* red light; and that it passes into a blue when the neighbouring strong light is yellowish. These combinations were, so frequently, those which exhibited themselves between the more and the less brilliant parts of the double stars, that we might think ourselves authorised to regard the coincidence of the two phenomena that which really happened. It is, however, also true, that a great number of exceptions occur, and they certainly ought not to be neglected. Thus it often happens, that a lesser blue star may be the companion of a brilliant white one. Number thirty-eight of Gemini is an example of this; α of the Lion is a second, and there are others. In these instances there is no red star, and, therefore, the phenomenon of contrast is out of the question. Consequently, the blue tint cannot be considered an illusion. *Blue, therefore, is the real colour of some stars.* This consequence flows also directly from the observation of δ of the Serpent; for, in this group, both the greater and the smaller are blue. Hence, nearly ten years ago, we had many doubts whether the notion of *contrast* satisfactorily accounted for all the facts.

Sometimes, unquestionably, it is the real cause; the brilliant star being red or yellow, the smaller one appears of a green or blue tint. A very simple experiment is sufficient to distinguish these cases from the others; we have only to conceal the principal star from our view by a thread or a small patch placed on the glass. If, during the occultation of the greater star, the smaller, which then appears alone, ceases to be coloured; if it becomes white, the green or bluish tint, with which it seemed overspread when the two were seen simultaneously, was only an illusion. When, on the other hand, the contrary happens, we cannot refuse to consider these tints as real. Thus, then, the occultation of the greater star produces a disappearance of colour in the smaller, only in a certain number of cases; and most commonly, this occultation leaves the tint of the smaller star unaltered, or, at least, induces only modifications that are insensible to us.

The existence of so may *blue* or *green* stars in the binary groups, known under the name of *double stars*, is a fact the more worthy of attention, because betwixt the 60,000,000 or 80,000,000 of isolated stars, whose positions the astronomical catalogues make known to us, we believe there is not one to which there is attributed any other characters, in regard to tints, than white, red, and yellow. The inherent physical conditions, then, respecting the emission of a blue or green light, seem to be met with only in the multiple stars.

This phenomenon has been observed for too short a time *

* We have been anxious to find out what observer it was that first recognised *the existence of blue stars.* The ancients only speak of *white* and *red* stars. In this last class they placed Arcturus, Aldebaran, Pollux, Antarus, and α of Orion, and all these remain red to the present day. To this list, and this is a circumstance which is worthy of remark, they add Sirius—whose whiteness is now remarked by every body. It would seem then, that with time certain stars change their colour. We shall now quote the first passage known to us where mention is made of *blue stars.* It will be found in *Le Traité des Couleurs de Mariotte*, which was published in 1686.

"Il y a des étoiles," &c. "There are some stars that are very red, as the eye of the Bull, and the heart of the Scorpion; there are also others that are yellow and *blue*:" and again—"The stars that appear red and yellow must needs be very luminous, but having their vivacity obscured by exhalations which are spread over them; and those which *appear blue*, have a feeble light, but pure and without exhalations."

In the catalogue which Mr Dunlop published in 1828, there will be found in the Southern Hemisphere, notice of a group that has three and a to warrant us even to hope for a plausible explanation. We must look to time and accurate observations, to inform us if the green and blue stars be not suns which are already in the process of waning;*—if the different shades of these stars does not indicate that their combustion is proceeding with different degrees of intensity; and, finally, if the tint, with the excess of the most refrangible rays, which the lesser star often exhibits, be not owing to the absorbent power of an atmosphere, which the action of the star, usually much more brilliant than that which it accompanies, may develope. In the study of phenomena, wherein, we must, without doubt, take into marked consideration the action which two suns, equally luminous, and of unknown physical constitutions, exert upon each other, we have no longer

half minutes of diameter, and which is composed of a great number of stars, all blue. The same astronomer speaks of a real nebulosity, that is to say, a confused mass of radiant matter, the tint of which is also blue. Nothing of this sort seems to have been observed on this side of the equator.

* If the *new star* of 1572 possessed the physical constitution of the permanent stars, the explanation of the blue colour, by the enfeebling of the combustion, ought to be discarded. This star, which at the time of its sudden appearance, on the 11th of November 1572, so far surpassed the most brilliant stars of the firmament for lustre, that it was seen with the naked eye in full day light: it was then of a *perfect whiteness.* In January 1573, its light, considerably enfeebled, had become *yellow;* somewhat later it assumed the *reddish* colour of Mars, Aldebaran, or α of Orion: to the red, as reported by the observers of the time, succeeded the *livid white* of Saturn, and this last shade continued till the entire disappearance of the star. In all this there is no mention made of blue. The new star of 1604, in the same way, did not exhibit this colour. It is therefore established, by two striking examples, that a star may appear to come into existence, possessing the highest degree of incandescence, and then apparently diminish to its entire disappearance, without ever becoming *blue!* It is however to be remarked, that the disappearance of the stars 1572 and 1604 having been observed with the naked eye, it might be plausibly maintained, that the blue might have shown itself, but only when being so far enfeebled, they could be found only in the class of telescopic stars. And besides, there remains always this question—Are the *new* stars, and the *permanent* ones of the same nature? Perhaps the permanent stars, such as our sun, only shine in virtue of a gaseous atmosphere which surrounds them, and it is a property of a gas when combustion becomes feeble, that it should then become blue. The absence of the principal prismatic shades, during the different phases of the new and the changing stars, is a remarkable phenomenon, whence important consequences regarding the velocity of the luminous rays of the different colours may be deduced. This inquiry, however, must be reserved for another occasion.

even analogy for our guide. In truth, the observations of naturalists could only put in comparison with the solar rays, things terrestrial, and these at temperatures but little elevated. It is then more than probable, concerning this question of the colour of the star, that the great object of observers for a long time to come, must be simply to collect facts. The satisfaction of associating them with the known laws of physics, may probably be reserved for our great-grandchildren. But this is only a reason why we should redouble our efforts and our zeal. In the astronomical phenomena, the accuracy of observations has often compensated for want of time. And besides, when, after having arrived at the termination of extensive labours, the hope of some important generalization has not been realized, our disappointment may find consolation in remembering, that the discovery of a single fact, well observed, well described, and well appreciated, is unquestionably an advance in science; whilst ingenious and seducing theories which may be received with general enthusiasm, are often nothing more than a retrograding.

Fontenelle, Huygens, Gregory, and others, have described, in works which are very well known to the public, the appearance and the movements of all the stars of the firmament, as they present themselves to observers who might be placed on the surface of the Sun—of the Moon—on the planet of Jupiter, followed by his four satellites—of Saturn, surrounded by his prodigious ring, and on Comets, with their eccentric orbits. Those who delight in these contemplations, have only to transport themselves, in thought, to the planets with which the double stars are unquestionably accompanied, and the united actions of two of these suns with ellipses very eccentric, will then become the occasion of a multitude of interesting researches. The article has already extended itself so far, that we must be contented now with simply directing the attention of those explorers of distant worlds, to the binary and ternary groups of coloured stars; to the simultaneous or successive presence of these different suns upon the horizons of the neighbouring planets; to the various combinations of white days, and red, and green days; and to the thousand curious optical phenomena which must be the consequence of all this. There is here something, which, for a time at least, may occupy the attention of the most fervid imagination.

8. *The double stars have become a means whereby we may judge of the excellence of Telescopes.*

The distinguishing of the stars is for those astronomers who may be called upon to pronounce upon the excellency of telescopes, whether of a more common, or of the most superior kind —a touchstone more sensible and precise, in certain respects, than the observation of the disc of planets has hitherto been. The expressions that the glass *distinguishes well*,—that with it we *distinctly* see the belts of Jupiter and of Saturn,—that the spots of Mars are clearly perceived, &c. &c. are vague, and possess a different value, as they are pronounced by an astronomer more or less accustomed to use powerful and well-made instruments. These expressions, whoever employs them, always imply, in the mind of the speaker, a confused idea of comparison. But if it be said, that with a magnifier of 200 times, for example, a glass completely separates the two stars, now so near each other, which together form σ of the Crown, there is supplied, to any one who tries a similar experiment, the means of ascertaining, without any hesitation, if his instrument be *inferior* to the other. If we, in a word, recall to recollection the fundamental principle of every telescope, the advantages of this kind of test will at once become evident.

A telescope is composed of two glass lenses. The one large, and turned towards the object, is called the *object-glass;* the other, quite small, and placed near the eye, is denominated the *eye-glass.* The former lens forms, in a certain part more or less distant from its surface, called the *focus*, an aerial image,—a true picture of all the objects in view. It is this image—this picture, which is enlarged by the help of the magnifying eye-glass, just as if it were a natural object.

When the focal picture is distinct,—when the rays which, proceeding from *a point* of the object, are again concentrated in *a single point* in the image, the observation then made with the eye-glass gives results most satisfying. If, on the contrary, the rays which emanated from a point do not reunite at the focus in a single point; if they there form a little circle, the images of two contiguous points of the object necessarily encroach upon one another; their rays are confounded together, and it will be impossible for the ocular lens to dissipate this confusion. The office which it alone fulfils is to magnify; it magnifies every thing in the image, the defects as well as the rest. The telescope, we mean the united lenses, cannot then represent the objects well defined.

This defect of distinctness exists, in different degrees, in telescopes, according as the artist has succeeded in giving to the two faces of the object-glass a regular curvature, more or less in approximation to the geometric form, which theory has established as most suitable, and towards which the optician is always approximating, but which still always remains an abstraction. It often happens, that with a single glance, whatever be the object at which we look, we can know if the object-glass has been successfully finished; but this is not always the case. Astronomers themselves, when called upon to compare two telescopes, however experienced they may be, sometimes feel difficulties, if they have only examined large bodies, such as Venus, Jupiter, Saturn, and Mars. In these cases, the double stars put an end to all dubiety.

It is proved that the stars have not sensible angular diameters. Those which they appear to have, depend, for the most part, on the imperfections of our instruments, and also on certain defects, certain aberrations of the eye itself. The more a star appears small, every thing being equal, as it regards the diameter of the object-glass, the magnifying power employed, and the brilliancy of the star observed, the more perfect is the telescope. But the best means of judging whether the stars appear very small, if points, such as these, are represented at the focus by simple points, is evidently, to examine stars which are excessively near to each other, and to observe if their images are confounded, if they encroach upon one another, or otherwise, if they be perceived to be distinctly separated. We subjoin a list, from the known double stars, of a certain number of those in which the best glasses alone, furnished with strong magnifiers, succeed in effecting a separation.

36th of Andromeda. The distance between the two centres was 0″.7 in 1831.
η of the Crown. . . . . 0″.8 in 1830.
σ of the Crown. . . . . 1″.8 in 1830.
γ of the Crown. One of the most difficult to distinguish, as much on account of the extreme approximation of the two stars, as on account of their great difference of intensity.
ι of the Ram. Very difficult to separate.
η of Hercules. Do. do. do.
τ of Serpentarius. The telescope of Dorpat itself does not *now* distinguish them. From older observations, however, we have learnt that this is a double star.

That these observations might be complete, it was important not to forget to point out the great advantage which may now be derived, by the examination of double stars, in the trying of great telescopes. On all accounts, we are persuaded that the importance of the application will be recognised, when it is considered that the kind of these instruments which is indispensable for all the great observatories, costs eight hundred, a thousand, or even sixteen hundred pounds, and this independent of the expense of mounting.

9. *Of the part which the doctrine of Probabilities has fulfilled in the question of double stars.*

The calculations of probabilities has enriched astronomy with a great number of very remarkable results. Hitherto, however, the doctrine has not assumed, either in teaching or in elementary works, the place that is due to it. It would appear that there has been a dread of darkening the truths of the science, whose demonstration rests on the immediate union of direct observations, by associating them with deductions, which, without possessing altogether the same certainty, nevertheless merit, in the mean time, to be held in high estimation. We may add, that we do not know a question more fit than that of double stars, to demonstrate how far observers would err in despising the lessons taught by the calculation of probabilities.

As far back as 1767, a distinguished astronomer, John Mitchel, struck with the irregular distribution of the stars in the firmament, examined if it were possible to believe that this distribution was the effect of accident. He took, as an example, the constellation of the Pleiades.

This group is composed of *six* principal stars. On the whole expanse of the heavens, it is almost impossible to count more than fifteen hundred which have an intensity which can be compared to theirs.

The problem for solution, then, was this: Fifteen hundred stars being thrown *at hap-hazard* over the extent of the firmament, what probability is there that six of them will be united in the confined space which the constellation of the Pleiades occupies. Mitchel found for this probability $\frac{1}{500000}$; that is to say, that it was 500,000 to *one* that such an approximation of six stars should occur. But since this concentration exists, in spite of the one probability out of the five hundred thousand which might lead to it, *we ought to conclude* that there has been something erroneous in the ground-work of the calculation. But on closer examination, we find there is only one hypothesis, viz. that the stars are distributed over the heavens *at hap-hazard.* Hence our hypothesis, the *probable* consequences of which are so little in accordance with the facts, itself becomes improbable. It is, therefore, the directly opposite theory which ought to procure suffrage. And thus the six stars of the Pleiades are not so closely concentrated by chance; thus a physical cause has regulated their union within so narrow a space; and thus they are in a mutual dependence upon one another! But is not this precisely the same conclusion which, much later, has been deduced from the tedious labours of astronomers on the double stars? Here, it will be perceived, the theory of probabilities had taken the lead of direct observations.

In applying the same doctrine, remarks the English philosopher, to the stars *which appear double or triple only through the telescope*, their connexion would appear established upon infinitely greater probabilities. And what would Mitchel have said, if in his time certain binary groups had been known, such as η of Hercules, and γ of the Crown, in which the two constituent parts can scarcely be separated with the help of the best telescopes and the strongest magnifiers? With a little more confidence in the results of the calculations of probabilities, practical astronomers would have commenced observations on double stars in 1767. This confidence was possessed by the ingenious author of the remarks to which we have been alluding, to such a degree, that he even then spoke, in his memoir, of the existence of *stars which* *were revolving round each other*, as one of the means by which to resolve some of the difficult questions of physical astronomy.

Although the principles of probabilities begin now-a-days to be more extended, we would even say more employed, and although, on the other hand, the intimate union—the mutual dependence of the two constituent parts of a considerable number of binary stars, is the result of direct and incontestable observations, yet it is difficult not to concur in the remark of M. Struve, that this union—this dependence, the fruit of so many delicate researches, might have been inferred by all who have eyes to see, from the simple inspection of the table, wherein is enumerated the different classes of the double stars.

The four classes of Herschel, we ought on no account to forget, have nothing to do with the intensity of the stars; they have only a relation to their angular distances. The first is composed of all the binary groups, in which the constituent elements are at least separated by a distance of four seconds. The second contains those in distances of more than four, and less than eight, seconds. The third commences with eight seconds, and ends with sixteen. Finally, the fourth mounts up to thirty-two seconds. Now every one will understand, that in seeking after the probability in which the stars scattered over the firmament without any rule would present themselves in groups of two,—that this probability, we say, would be less in proportion as the groups in question would be confined within smaller limits. It is, in short, as if we were to calculate what probability, in throwing a certain number of barleycorns on a chess board, there would be of finding them collected, *in the squares,* in groups of two. The probability must evidently diminish in the same ratio as the sizes of the squares. In the proposed problem, the barleycorns are the stars; the chess-board is the firmament; the squares, for the first class of Herschel, are those with distances of four seconds at most of diameter; for the fourth class, the dimensions of the squares extend to thirty-two seconds. In the hypothesis of an absolute independence amongst all the stars with which the heaven is sprinkled, the first class of double stars would be much less numerous than the second, still more so than the third, and yet more than the fourth. But the reverse of all this really occurs, as may be seen by a reference to the table (page 3.). We are thus then again led, by the simple consideration of probabilities, to conclude, that the stars which approximate to each other, do so, not only in appearance, that is to say, not only according to the laws of perspective or of optics, but, on the other hand, that they really form individual systems.

I. *Some Observations on a Note of M. A. Van Beek, purporting to point out an Error in the Bakerian Lecture of the late Sir Humphrey Davy "On the Relation of Electrical and Chemical Changes."* II. *Some Observations on Euchlorine, relative to the Question of its Decomposition.* By JOHN DAVY, M.D., F.R.S., Assistant Inspector of Army Hospitals. Communicated by the Author through Sir JAMES MACGRIGOR.

THE Note referred to above, occurs in a paper of M. Van Beek, published in the 38th volume of the Annales de Chimie et de Physique, and is as follows:—

"Dans le cours de mes expériences sur la préservation des métaux, je me suis aperçu d'une erreur grave que le célébre chemiste Anglais Sir Humphrey Davy a commise, dans le Bakerian lecture du 8 Juin 1826, 'On the relation of Electrical and Chemical Changes,' publié dans les Transactions Philosophiques de 1826: il recommande d'employer le zinc ou *l'étain* pour la preservation des chaudières à vapeur, surtout celles des bateaux à vapeur où l'on fait souvent usage de l'eau de mer.

"Des expériences décisives m'ont appris que l'étain, bien loin de préserver le fer, est au contraire préservé par ce dernier métal, et qu'ainsi un morceau d'étain introduit dans le chaudiere, au lieu de preserver le fer de l'oxidation et de diminuer par la les dangers d'explosion, devrait puissamment contribuer à sa prompte destruction.

"Si l'on veut faire usage de cette application utile du principe de la preservation reciproque des métaux, le zinc seul devra être employé."

This statement is made so strongly by M. Van Beek, that the majority of those who have read it, have probably been inclined to receive it as correct. Such at least was the impression on my own mind, on a hasty perusal. But when I reflected on the subject, doubts arose of its accuracy. It appeared very unlikely that an inquirer such as my brother, who had devoted so much time to electro-chemical research, and had just then brought to a conclusion the investigation of the protection of copper in sea-water by electro-chemical means, should have fallen into so grave an error; and considerations respecting the relation of tin to iron in the scale of chemical affinities, came in confirmation of this opinion. To endeavour to satisfy myself, as far as possible to demonstration, on which side the truth was, I had recourse to experiments; and I now beg leave to communicate some of the results, with the conclusions to which they led.

If M. Van Beek were correct, it appeared as a necessary consequence, that tin would be negative in its electrical relation to iron. To ascertain the relation of these two metals in this particular, I connected wires of them with a galvanometer, and plunged them successively into the mineral acids, more or less diluted with water, into sea-water, solution of potash, and lime water. The result in every instance was the same; the tin proved positive and the iron negative.

Tin thus being positively electrical in relation to iron, it appeared a necessary consequence, that it should exert a protecting influence on the iron, and defend it from chemical action. To ascertain this, three equal portions of tin wire were taken, and three of steel wire, each weighing 1.6 grain, and muriatic acid of the same dilution, in about equal quantities, was poured into similar vessels.

Into one of these a tin wire alone was put, into another a steel wire alone; into a third, a steel wire and tin wire twisted together.

In less than twelve hours, the steel wire unattached was dissolved. After three days, the tin wire was found to have lost six-tenths of a grain, and the same both in the instance in which it was alone, and in that in which it was connected with the steel; but, the steel in this latter instance had lost nothing, it retained its lustre unimpaired, and, in brief, was perfectly protected by the tin. And the same result was obtained when the tin and steel wires were bound together by a fine silver wire.

The next necessary consequence appeared to be that tin would protect iron, in contact with water, either cold or boiling, or fresh or salt as my brother inferred it would, but which M. Van Beek denies, maintaining, on the contrary, that the iron protects the tin, and the tin accelerates the corrosion of the iron. To determine this experimentally, portions of steel wire, and of tin unattached and attached, were kept in rain-water and sea-water boiling for several hours. Neither the tin or steel had lost ought; the tin wire was not tarnished, and the steel very slightly so; and, as well as I could judge, not more or less when attached or unattached to the tin. This, then, was altogether a negative result, seeming to shew, as I believe is true, that neither tin nor iron has the power of decomposing water at its boiling temperature; and, therefore, that no protection is required for an iron boiler used for converting water into steam.

Wires similar to the preceding were next put into fresh water and sea-water and left exposed to the atmosphere, in a room of about the temperature of 60° Fahrenheit. Examined after twenty-four hours, the steel wire exhibited rust, and in about the same degree, whether attached to the tin wire or not attached, the tin wires both remaining bright. The experiment being continued, the rusting of the steel proceeded, and in a few days a deposit of yellow rust had taken place, which augmented in quantity from day to day. But not so the tin wires; even after some days they continued pretty bright; no deposit of oxide was distinctly formed on them, excepting in the instance of the solitary wire in salt-water, which in two or three places was very partially incrusted with white matter most probably oxide of tin, its surface remaining bright.

These results were different from what I expected: The steel certainly was not protected by the tin. To what was this peculiarity owing? The rust on examination appeared to be hydrated peroxide of iron. No disengagement of gas accompanied its formation. Reflecting on this, and the negative results in the experiments in which steel had been kept in boiling sea-water, I was led to the conclusion that the oxide in question was formed not by the decomposition of water, but by the union of the iron with the oxygen of the air dissolved in the water.

To put this to the test of experiment, a portion of sea-water was deprived as much as possible of air by means of the air-pump, with steel wire immersed in it; and, after the exhaustion, the bottle was corked and inverted in water to prevent the re-admission of air. The result now was different; after twenty-four hours only a stain just perceptible appeared at the end of the wire, where in contact with the glass; and no more appeared after several days, the wire generally remaining bright: Whence it appears to me, it may be inferred, that the oxidation of steel in water, is analogous in theory to its oxidation in moist air, or in heated air, or in acid vapour, in all which circumstances the contact of a more electrically positive metal, even of zinc, does not defend it; no more than iron, tin or zinc defend copper completely when similarly situated.

The property of iron to rust in water and in moist air more rapidly than tin, as is well known, and even than zinc, may perhaps depend on the powerful affinity of the protoxide for oxygen, to form the peroxide,—promoted by the predisposing affinity of the latter to form a hydrate, a compound containing about 16 per cent., in two proportions of water, and in which the oxide and water are united so firmly, that a temperature approaching to a red heat is required to expel one proportion, and that of a red heat to drive off the other.

Relative to the statement with which M. Van Beek concludes his note, that zinc should be used in every instance in which it is desirable to afford protection to iron in the boilers of steam-engines, it is necessary to be cautious in giving an opinion. That zinc is capable of defending iron in water, and even in salt water, from rusting, I have satisfied myself by experiment. But as gas is disengaged from the iron, whilst the zinc oxidates, it may be a question whether danger may not arise from the inflammable gas mixing with the steam, and more than counterbalancing any little saving of the iron from rusting, owing to the action of the air on the water previous to boiling. In regard to theory, this instance of the protection of iron in water by zinc, may be considered analogous to that of iron by tin in a weak acid, such as the diluted muriatic which is capable of dissolving the oxide of tin as it forms.

After what has been brought forward, little comment need be made on M. Van Beek's assertion, that tin instead of preserving iron, is on the contrary preserved by the latter metal, and the destruction of the latter accelerated by it. I shall merely remark, that though my experiments are not in accordance with his, I can conceive circumstances in which the result may be as M. Van Beek maintains, such for instance, as water capable of depositing matter in the tin, so as to cover it with a closely adhering crust. In illustration of my meaning, I may offer the instance of the action of very dilute nitric acid, on these two metals in contact, in which the iron though negative compared with the tin, yet dissolves first, in consequence being positive in relation to the insoluble oxide that speedily envelopes the tin.

I shall conclude with the mention of a few particulars which bear on the theory of voltaic electricity, and which have come under observation in the course of this inquiry.

In the Bakerian lecture already referred to, my brother has adduced an instance of decided electrical effect, indicated by the galvanometer, under circumstances excluding chemical action, and in which the sole cause appears to have been the contact of the connecting platina wires with an acid and alkali, separated by a neutral, imperfect, conducting fluid. This he brought forward with many other facts in support of his views relative to the primary production of voltaic electricity, views nearly accordant with the original theory of Volta, and which he adopted in preference to the purely chemical hypothesis after careful consideration of the leading facts, a large number of which were the fruits of his labours.

Latterly, other experiments have been published in favour of the same theory, two of which in particular have been considered demonstrative by their authors; I allude to those of M. Pfaff, and of Matteuci, both given in Annales de Chimie et de Physique,* one, in which electricity is excited by a pile of zinc and copper with cloth, moistened with a solution of sulphate of zinc deprived of air; the other, in which it is produced, as indicated by the most delicate of all galvanometers, a prepared frog, by zinc and copper and distilled water purged of air, circumstances in which it is supposed that no chemical action can take place.

The objection to these instances appears to me to be, that proof

* Tom. xli. p. 246, and xlv. p. 106.

is wanting of the absence of chemical action. In experiments in which zinc is concerned, much caution is required not to fall into error relative to its effects. Though incapable, I believe, of decomposing pure water, whether cold or boiling, like iron, yet it has this power, when the water is strongly impregnated with saline matter, as I have witnessed, on immersing polished zinc in a strong solution of its sulphate, hydrogen being disengaged and oxide of zinc formed ; and, farther though incapable of decomposing water in a weak saline solution, such as sea-water, yet if associated with another metal negative to it in electrical relation as platinum, it acquires the power. This I have ascertained by several experiments, using sea-water containing air, and also deprived of air, both by the air-pump and by the action of iron. Immediately, on the immersion of two wires of these metals, joined, minute bubbles of gas appeared about the platina wire, the disengagement of which rest on rather increasing in rapidity, and which when collected in sufficient quantity for examination, in the course of two or three days, proved to be hydrogen.

The application of these results to the instances in question is obvious, especially to the experiment of M. Pfaff, in which were conjoined the two circumstances imparting power to the zinc, namely, a strong saline solution and the contact of a negative metal. In the other experiment, that of M. Matteuci, it is possible that the saline mater in the muscles of the frog, though washed with distilled water, or even merely the contact of the muscles themselves, might have exercised a similar influence.

The only example I have met with in my own experience, of electrical action, indicated by the galvanometer, referrible merely to contact, independent apparently altogether of chemical change, or change of temperature, has been afforded by the immersion of silver and iron wires in a solution of protomuriate of tin, saturated with tin. Steel wire and platina wire previously introduced, had no effect on the galvanometer, indicating therefore the absence of chemical action ; but, substituting a silver wire for the platina, a distinct though slight effect was produced on the needle ; and as the silver wire was negative, it may be inferred to have taken place without chemical change.

The same result has been obtained, using a neutral solution of muriate of zinc. Silver wire and steel immersed in this, afffected sensibly the galvanometer, the silver being negative; while neither of these wires immersed in conjunction with platina wire, produced any sensible effect on the needle. The solution of zinc was formed by means of zinc wire and muriatic acid, the former greatly in excess. After the violent effervescence was over, a very gentle one continued, accompanied by the deposition of oxide of zinc, to which there appeared to be no limit, depending on the decomposition of water. Before the solution was used it was filtered, to rid it of any particles of the oxide or of the metal, which present might have vitiated the result.

The effect of the contact of other metals applied to this solution was hardly less decisive than the preceding, in exhibiting electrical action, through the medium of the galvanometer without apparent chemical action. Thus in every instance in which a bright wire of zinc was immersed in it, together with another metal, as platinum, gold, silver, copper, iron, tin, the needle was moved, the effect being greatest with the most negative metal, as platinum, and least with the one approaching nearest to zinc in its electrical relation, as tin; and, at the same time, not the slightest tarnish could be perceived on the bright polished surface of the zinc.

Even had a chemical effect taken place on the zinc, as the electrical action varied according to the other metals employed; it may be considered a fact in favour of the theory of contact, and belonging to the same class of phenomena as those lately brought forward by M. A. Bouchardat*; a class, probably, which will become greatly extended, and will embrace a great variety of effects, which at present appear so mysterious, as the inflammation of hydrogen mixed with oxygen from the contact of porous platinum, the conversion of starch into sugar and mucilage, under the influence of a minute portion of sulphuric acid; the formation of nitric acid under the influence of carbonate of lime in rocks and soils, containing potash, on exposure to the atmosphere, and many other chemical changes not less obscure, which hitherto have commonly been considered as instances of predisposing affinity.

MALTA,
1*st March*, 1834.

* Annales de Chimie et de Physique, tom. liii. p. 284.

II.—*Some Observations on Euchlorine, relative to the Question of its Composition.*

When my brother, the late Sir Humphry Davy, in 1815, discovered that combination of chlorine and oxygen which has since been called Deutoxide of Chlorine, he was in doubt whether to consider euchlorine a true chemical compound, or a mixture of the new gas and chlorine. He remarked, " that two in volume of this gas, and three in volume of chlorine, would produce by explosion the same products as euchlorine." The only facts he was aware of opposed to this idea was, that Dutch foil, which burns in a mixture of two volumes of common air, and one of chlorine, remains unaltered in euchlorine, which could hardly be expected unless the latter were a definite compound. But, he adds, " The force of this argument is suspended, till it be supported by an experiment, shewing that Dutch foil inflames in a mixture of two of the deep yellow gas, and three of chlorine." This experiment he had not an opportunity of making at Rome, where he discovered the new gas, no foil of the kind being procurable there; but, on his arrival in England, a few months after, he did not delay trying it. The result was negative; the mixture, in the proportions above stated, had no effect on Dutch foil; and hence he concluded, " that the deep-coloured gas and chlorine have a chemical action on each other, and that euchlorine is not a simple mixture of them." However, he drew this conclusion merely on the ground of probability; he intended to prosecute the subject, and hoped soon to present some new results *. But this he did not accomplish, his attention, almost immediately, having been directed to more important objects of research, especially fire-damp and the safety-lamp.

Since that time, the nature of euchlorine has remained an unsettled question. The majority of chemists, and some of the highest authority, as Berzelius and Gay Lussac, have considered it a definite compound;—whilst a few have more inclined to the idea, that it is merely a mixture, founding their opinions chiefly

* Philosophical Transactions, 1815, p. 219.

on the fact, that the condensation of the gases in euchlorine is not in accordance with the rule of condensation witnessed in the combination of the gases generally.

The latest and the ablest advocate of this latter opinion is M. Soubeiran, who, in his elaborate and excellent paper, entitled "Recherches sur quelques combinaisons du Chlore," published in the 48th volume of the Annales de Chimie et de Physique, has brought forward some facts of an instructive kind, the results of new experiments, and which are strongly in favour of the view he takes.

Yet, even M. Soubeiran's researches have not removed all doubt; this I felt in reading his paper, and I perceived from Mr Johnston's Report on Chemistry, published in the Proceedings of the Oxford meeting of the Association for the Advancement of Science, that he has been similarly impressed. I have been induced, therefore, to make some new experiments on the subject, a few of the results of which I now beg leave to communicate, trusting that they may be of some use in deciding the question of the nature of this curious gas.

The first point on which I wished to satisfy myself, was the degree and kind of action of mercury on euchlorine, both being doubtful. As a preliminary, I thought it right to try the action of chlorine on this metal, for the sake of comparison.

A retort charged with black oxide of manganese and muriatic acid, had its beak plunged under mercury, a receiver full of mercury being placed above it. As the gas was disengaged on the application of heat, it was absorbed as it rose, so that, when pure, very little of it reached the top of the jar, and that was speedily absorbed, the thick crust which formed not preventing it; so rapid, indeed, was the absorption of the gas by the mercury, that the mouth of the retort was several times clogged, rendering it necessary to introduce a wire, to clear the passage. The thick crust, the result of the absorption, of a very light-gray colour, and not soiling the fingers, was a mixture of calomel and corrosive sublimate.

To try the effect of euchlorine on mercury, the same apparatus was used, excepting that the retort was very much smaller, and charged with chlorate of potash, and muriatic acid diluted with an equal volume of water. A large quantity of gas was disengaged, with the occasional aid of a gentle heat, and collected in different receivers over mercury. After standing twenty-four hours, there was very little absorption of gas. It possessed all the characters of euchlorine, as described by my brother: on agitating it, and transferring it from one receiver to another, the effects were different; sometimes it appeared to be pretty rapidly absorbed, at other times very slightly, or, indeed, hardly perceptibly, the surface of mercury retaining its lustre. The drier the mercury was, and the vessels, the less tendency there was to absorption. In some experiments, by repetition of transfers of the same portion of gas in mercury not carefully dried, the whole was absorbed; and, in other experiments, stopt, when about half the volume of the gas was absorbed. The residue had less intensity of colour, coloured water less strongly when absorbed by it, and had ceased to be explosive on the application of heat. The compound formed in the jars was bulky, of a darkish-gray hue, and had somewhat the appearance of an amalgam. I could obtain no euchlorine from it, either by the action of heat or of acids; it soiled the fingers like black oxide of mercury, and appeared to consist of calomel, corrosive sublimate, and this oxide of the metal.

The inference I drew from the experiment was, that euchlorine may remain in contact with dry mercury without alteration; and that, when it is absorbed, it is in consequence of its decomposition, the liberated oxygen combining with the mercury at the same time that the chlorine did.

These results may tend to reconcile the apparently contradictory statements of my brother and of M. Soubeiran. The former using dry mercury, and making his experiments, as well as I recollect, at a cold season of the year, and being chiefly intent on procuring the gas as pure as possible, for examination, was most struck by its want of action on mercury, and justly so. M. Soubeiran, on the contrary, viewing the gas as a mixture of chlorine, and deutoxide of chlorine, rather looked for its absorption. He supposes that its action on mercury, "a échappé à H. Davy, sans doute parce que le protoxide de chlore peut être conservé assez long-temps sur le mercure à la faveur de la croûte superficielle qui recouvre bientôt le metal, et le preserve d'une alteration plus profonde:" An explanation hardly to be admitted, as there appears no reason why a crust should defend the metal more from euchlorine than from chlorine.

M. Soubeiran states, that by agitating a small quantity of mercury in euchlorine, under very cold water, the whole of the chlorine is absorbed, and the oxygen left. In some trials, in effecting the absorption by agitation over mercury, I have also witnessed a residue of oxygen. Why, in some instances, the oxygen should be absorbed, and in others not, or only partially, I have not been able to ascertain.

The next point it appeared to me desirable to ascertain was, whether chlorine could be added to euchlorine, without materially altering its character. If it could not, the obvious inference would be, that euchlorine is a definite compound. A mixture over mercury was made, of about two volumes of euchlorine and one of chlorine. The colour of the mixture was a little lighter; silver-leaf immersed in it was slightly tarnished; bright rolled zinc did not appear to be acted on; and after two hours, it was not visibly absorbed by the mercury, and only very slightly after twenty-four hours.

This result, then, is decidedly in favour of the conclusion, that euchlorine is rather a mixture of the deutoxide and chlorine than a pure compound; for the inference is obvious, that if euchlorine can bear dilution with one-third its volume, without being materially changed, the deutoxide, *a fortiori*, may admit of being diluted with chlorine, so as to reduce it to the strength of euchlorine. And, the variable proportions of oxygen in the different specimens of euchlorine which I have examined, whether made with a weak or a strong acid, is in accordance with the same conclusion.

My brother's sole argument, as already mentioned, for considering euchlorine a definite compound was, that the deutoxide deprived chlorine of the power of acting on the common metals. The argument was apparently a good one, but far from conclusive; nor did he consider it so. As euchlorine has the same effect in a less degree, this argument now can hardly be admitted. It is more reasonable to refer the effect either to the electro-chemical influence of the deutoxide, or to chlorine, similar to that of iron (in a cast iron pneumatic trough) in preventing mercury being acted on by muriatic acid gas standing over it, or to the miscellaneous class of obscure chemical facts daily augmenting, which, in the present state of the science, baffle explanation.

Besides the objection last mentioned, which, if I do not deceive myself, may be considered removed, I know of no other, to the opinion that euchlorine is merely a mixture of the deutoxide and chlorine;—all its sensible, as well as all its chemical properties, so far as they have been ascertained, agree well with this view. The colour of euchlorine, and its odour and taste, are the same as those of the mixture; and its effect on the skin, and its explosive power, &c. are similar. To those who have any doubt on the subject, I would recommend M. Soubeiran's able paper, in which arguments of a different kind are adduced, and which, it appears to me, are not easy to resist, supposing the difficulties I have considered removed.

Relative to the name of the deep-coloured gas, as deutoxide is manifestly improper, I would beg to propose for it that of Euchlorine, on the same principle that it was applied to the mixture before the true compound was discovered; it is equally suitable (the colour of the gas being greenish-yellow), and may be considered now free from all danger of change. For permanency of nomenclature, it would be an immense advantage could names such as this be generally used.

Remarks on the Remains of a very large Oak Tree dug from a Peat-Moss near Lanfine, Ayrshire; and on the Ancient Caledonian Forest in the West of Scotland. By T. Brown, Esq. F. R. S. Ed., M. W. S. Communicated by the Author. *

I. *Remarks on the Oak Tree.*

When cutting drains through an insulated peat moss of about five acres in extent, at Barhill, near Loudoun Hill, in Ayrshire, the workmen laid bare the trunk of an oak tree. As this appeared to me to be interesting, from its large size, and from other circumstances connected with it, I had it carefully removed and preserved.

The tree measured 48½ feet in length. The upper part of it, for 16 feet, was, externally, quite entire. It was even nearly

* Read before the Royal Society of Edinburgh, April 7. 1834.

wholly covered with thick undecayed bark. There were a number of stumps of large branches issuing from the lower part of this entire portion. The upper part of it divided into several massive branches.

The remainder or lower part of the trunk, for 32½ feet, was much decayed; it varied in shape and in dimensions till it gradually tapered to a point at *its lower* extremity. In about one-half of this decayed part, the woody fibres were straight and uniform in their direction, as if no branches had arisen from it; but in the lower portion of it, in five different parts, the fibres were twisted and assumed a circular direction, appearing distinctly to shew that large branches had issued from these. Below this there was not even the least trace of root. The upper entire part of the trunk was completely immersed in moist peat earth, but around the decayed portion the peat was more dry and scanty. The lowest tapering part of the stem was nearly out of the peat, and lay on the edge of the bog, in contact with a dry gravelly soil of considerable depth, sloping rapidly upwards from the moss. The tree had fallen to the south.

Sixteen feet from the upper extremity it measured ten feet in circumference, even although from this portion the bark and the soft wood had been decayed. Immediately below this several large branches had arisen.

We could only form a probable conjecture regarding the dimensions of the decayed part of the tree, 32½ feet in length, but from its breadth at one part, and from the branches which in five different parts appeared to have sprung from it, it must have been very large. In fact it actually measured ten feet in circumference at the height of 32½ feet from the ground.

I had this tree examined, separately, by an experienced forester, and by a gentleman possessed of large plantations in England, and well qualified to judge of the value of timber. The former calculated that, when entire, it would have contained 534 feet of measurable timber. The latter formed even a higher estimate of its size when entire, and calculated that thirty years ago it would have sold for L. 300.

Since there was no appearance of any root, for the lower part of the trunk was gradually wasted to a point, it is most likely that the stem had originally been of greater length than we found it. But of course this is only conjecture.

As I was anxious to preserve this memorial of the ancient forests of Scotland, I had it cut into pieces and removed to near my residence, at the distance of three miles. The upper entire part was then again united, by enlarging the hollow in its centre and fitting into it a strong square piece of timber. We attempted to raise it on its end by means of pullies and proper tackle; but from its great weight, and from the unfavourable position in which it lay, we failed till we first raised it to the sloping posture by means of levers. It was then elevated by the strength of above twenty men, assisted by pullies fixed to a neighbouring large tree. The entire part of the tree now stands erect. The decayed part is placed extending horizontally from it.

I am quite sensible that we have authentic accounts of trees being found in mosses in Scotland, of greater length and circumference than the one which was found at Barhill, but I am not aware of any record where a trunk of large size is said to have been so entirely and so extensively covered with bark as this one was. The entire state of one part of this tree affords, therefore, a proper basis for reasoning; at all events, I conceive that several conclusions may be fairly formed from this decayed tree, and from the situation in which it was found.

The peat moss formed a *flat* circular surface, containing about five acres, surrounded by dry gravelly ground. This margin gradually rose from the level surface of the moss on all sides, except at one point where a most abundant stream of water issued. Around the edge of the moss there were several other trunks of oak trees covered with the peat, but no one of these was at all equal to the one which has been described. It appears probable, therefore, that this oak tree, along with others, originally grew on the dry margin of a small shallow lake; that, from various causes, it, along with the rest, had fallen into the lake; that it had gradually become covered and embalmed in aquatic plants, so as ultimately to fill up completely the hollow with peat moss. We cannot account for the entire preservation of the tree immersed in the moss, on any other principle than that it had fallen into water, and become gradually covered with aquatic herbaceous plants and shrubs. The moss could not have existed at the time of the fall, for if this had been the case, the tree could not have sunk into it so completely as to have been preserved even with its bark. The part of the tree covered with the deeper water, and afterwards with the moss, was quite entire; but at the sloping edge of the lake, where the covering was less complete, the trunk was more or less decayed, till on the dry land no traces of it could be seen.

In the centre of the moss there were numerous trunks and branches of birch, but although I sought with care, I could not discover any appearance of oak. This was entirely confined to the margin. The birch, most likely, had grown long after the fall of the oak, on the moist mossy soil which had covered it, and had, along with various shrubs and herbaceous plants, at last filled up the cavity of the lake.

We may conclude, also, that this tree grew surrounded by others, or in a wood, since we never see an oak, or, indeed, any forest tree, attain a great height when it is solitary. The old oaks at Chatleherault, near Hamilton, are of great girth, but, from being in some respects solitary, their stems are short when compared with the one found at Barhill. Their trunks do not exceed from 12 to 18 feet in length, whereas the other was near 50. Most likely a wood covered the dry rising grounds which surround this swamp; and my impression is, that this tree could not have grown quite close to the water, which, at one time, filled the lake, but at some little distance. If this view be correct, the tree must have originally been of greater height than we found it.

As the entire part of the tree was covered with bark, it proves that it must have fallen before it had been long dead. The hollow in the centre had certainly existed long before it fell down, and, as this extended to the top of the stem, it proves that the tree must have been in a ruinous state, though alive, when it fell.

The situation in which this oak was found is elevated upwards of 500 feet above the level of the sea. I have ascertained this by actual measurement of a part of the height, and by calculation of the remainder. There are other trunks of oaks of large size found in a moss about two miles from this, nearer the source of the river Aven, where the altitude is probably 800 feet at least above the level of the sea. In this situation I measured one trunk out of several, 45 feet in length. This one was much decayed, without any trace of either branches or root.

The remains of these trees are only found in situations more or less hollow, covered and preserved by moist peat earth. In some of these bogs, especially in such as are situated in deep depressions, we may suppose that, in former times, lakes of greater or less depth had existed; but that, in other situations, in consequence of the obstruction occasioned by fallen trees, water had become stagnant, aquatic plants had vegetated, and, in the peat earth produced by their decay, the fallen trees had been preserved. Accordingly, these moss trees are not found, where, either from the slope of the ground, or from the porosity of the soil, water does not stagnate.

We found, too, that where the trunk of our moss tree reached the dry soil at the margin of the bog, it became completely decayed and removed. Undoubtedly, however, these dry and porous situations, from being much more favourable for the growth of timber than moist soils, would in former times be covered with trees even larger than are found in the mosses; but when these were laid prostrate on a dry soil, they would be speedily decayed and removed from sight. They were only preserved where they chanced to fall into a lake, or where they formed a morass.

II. *On the Ancient Caledonian Forest in the West of Scotland.*

These trees almost to a certainty were individuals of that part of the Caledonian forest which by old records extended up Avondale by Strathaven, passed over the high ground near Loudoun hill, and covered a large portion of the upper part of Ayrshire. Here, as well as in Avondale, there are many extensive and bare districts, which, we are informed, were at one time covered by trees; and, from the few specimens which have been preserved in small insulated bogs, we must form the conclusion, that these were of much larger size than those of modern times. I am not aware if there is any oak tree in Scotland at present alive equal in size to that which this fragment must have been; and as it is surely very improbable that the largest, or nearly the largest, tree of the forest should have grown at so high an elevation above the sea, in a soil, we may conceive, rather too moist for vigorous growth, and should have had the great luck to fall into a situation so favourable for its preservation, we may, without impropriety, conjecture that many larger trees grew in soils and at elevations more favourable for great increase of size.

I was at some pains to examine the dimensions of the annual layers, in order to form an idea of the comparative rapidity of growth of trees in former times with those of the present. It is difficult, from the interlacement of the woody fibres in the oak, to be certain of the limits in each annual layer; but in several parts of the transverse section of this tree I could distinctly observe a few zones. These did not exceed a line in breadth. In other parts the layers were so indistinct, that I could not distinguish them from each other. Certainly, however, there was no reason from this single specimen to suppose that the growth of the oak had been more rapid in those times than in our own.

We must suppose that these forests, which existed 500 years ago in many other parts of Scotland as well as in Ayrshire and Lanarkshire, afforded shelter and warmth to districts where these are now very much needed. The wind, sweeping from the sea over a bare country and a moist soil, though its temperature is perhaps not actually lower, yet it is not nearly so kindly to animal, and even to vegetable life, as when its force is broken by wood. We are informed, too, that in those times Scotland, from its forests of oak, afforded shelter and food to the wild *boar*, to the *beaver*, to the *wolf*, and to other animals which are now extinct in Britain.

Although we have no very certain means of ascertaining at what period of Scottish history the woods in Avondale and Ayrshire were chiefly destroyed, yet there are many reasons for believing that this was in a great measure effected during the times which followed the death of Alexander III. in the year 1285.

After this event, wars and various other causes continued to ruin the woods, and even the agriculture and resources of Scotland, for nearly two centuries. In fact this country appears to have been declining in prosperity even till the middle of the last century. We believe, however, with some confidence, that the destruction of this part of the Caledonian forest commenced nearly about the year 1300, and that from various causes the ruin was soon completed, so that the country at length became almost quite deprived of wood.

About the year 1300, Scotland was harassed by the selfish and brutal policy of Edward I, when the succession to the Scottish crown was disputed by Bruce and by Baliol. These "wars of the succession," as they were properly named, continued in various ways to injure this country for nearly two centuries. Our impression, however, is, that this forest was destroyed nearly about the beginning of this period, that is, early in the 14th century.

From the local situation in which the family estates of Bruce in Galloway and in Ayrshire were placed, these districts were more attached to his fortunes than any other part of Scotland, and of course these were more contested than any other. To destroy the shelter afforded to the Scots by their forests, the English soldiers would certainly, by cutting or burning, destroy as many trees as they could. We are informed that this was the custom in former invasions of Britain. For instance, under the Emperor Severus, the Roman soldiers in great numbers were employed in destroying those woods which afforded protection to the natives of Scotland. Many proofs of this destruction were seen some time since in the extensive mosses situated on the river Forth above Stirling. Roman roads and other antiquities were found below the moss. Even the marks of the hatchet were observed on the moss oaks which were deeply covered with peat *. A similar policy would assuredly influence the Kings of England during their wars in Scotland; but through this dark and gloomy period of Scottish history, the written records are so few and so very deficient, that we cannot obtain much information, or place any great confidence in them.

Let us, however, make the probable supposition, that these forests were partly destroyed by the invading enemies of Scotland. The wind thus admitted to tall and densely growing trees would soon destroy the wood of a country. I had some years since an opportunity of witnessing an event of this description, where a plantation of above 100 acres, consisting of trees of fifty years of age, was totally blown over or otherwise

* Statistical Account of Scotland, vol. xviii. p. 321.

ruined, merely by cutting down those which formed a barrier against the west wind. These trees covered a hill whose top was about 800 feet above the level of the sea.

It is rather a remarkable circumstance, and we cannot refrain from attaching importance to it, that within 300 yards of the place where the large oak and other smaller ones were covered by the moss, two small deposits of silver pennies, in excellent preservation, were discovered; one of these on the south side, about twenty years ago, and the other on the north, within these few years.

A few of these coins were acquired, immediately after they were found, by my predecessor in the country, and since that I have been able to procure a few more from farmers in the neighbourhood. I have now above twenty in my possession. These are all silver pennies, either of Edward I. or of his unfortunate son Edward II. There is one exception in a coin of Alexander III. of Scotland. As Edward II. was murdered in 1327, and as no coins of a later reign were found, it becomes most probable that these coins were deposited during the reign of Edward II, soon before the battle of Bannockburn, which was fought in 1314.

I have understood that the remainder of the coins which were found in these situations, were exactly similar in appearance to those in my possession. Almost the whole of these are in such good preservation, they are in general so sharp in the impression, and of course they have been so little worn or used, that we may be allowed to conjecture they had in part been recently issued from the mint. It is by no means likely that so many fresh English coins, almost without any mixture of Scotch ones, should have been in the possession of the poor and half naked natives of Scotland, so far removed from the towns where they had been coined. I am inclined to conjecture, that these had been deposited by the English officers or soldiers during the destructive and doubtful skirmishes of those times, and that they had been hid soon after they had been acquired as pay. As the English had but little footing in Scotland after the decisive battle of Bannockburn, it is likely, as we have said, that these coins were deposited previous to this event.

It is perhaps not out of place to mention here, that the country around Loudoun hill, about a mile distant from where the coins were found, though at present bare and exposed, appears in old times when the *Caledonian wood* existed, to have been the field of several fierce contests.

A well formed Roman camp or station, containing about two acres of ground, is situated about half a mile to the south of Loudoun hill, close by the road to Ayr, on an insulated rising ground. It is named by the farmers near this, "Wallace's camp;" but it is distinctly a Roman station, with its square form, and with its usual enclosures and barriers. It is not mentioned, as far as I know, in any account of the Roman antiquities in Scotland; but that it was a Roman camp is distinctly proved by its form and by other circumstances. This is also confirmed by a silver coin which was found on the field about a year ago. The farmer who found it gave it to me. I was much gratified to find that it was a coin of Augustus Cæsar in excellent preservation. I have also a silver coin of Antoninus, which was found along with several other Roman coins at Torfoot, about two miles nearer Strathaven.

Close by the Roman camp, to the north, (the high road lies between them), a very large cairn of bulky stones existed thirty years ago, which was named "Wallace's cairn." I do not find, however, that any exploit of our great national champion is stated in history as having occurred on this field; but I observe it is mentioned by Barbour, that close by Loudoun hill Robert Bruce defeated the English commander Aymer de Vallence in the year 1307,—that is in the first year of the reign of Edward II, who was at that time in Scotland. Although no coin nor any other remains of antiquity were found below this cairn, yet, from its situation, I have little doubt but it was intended as a memorial of this battle.

It is well known that for several centuries, both before and after the time of Robert Bruce, the records of Scotland are very barren and unsatisfactory. In fact this country, instead of advancing in the arts, and in agriculture and population, was really declining in all of these. The natives, in every sense of the word, were barbarians, perpetually fighting, and exposed to the attacks of the English, and even of neighbouring lairds and clans. The justly popular name of "Wallace," who, in the midst of these times, terminated his patriotic career in 1305 on a scaffold, is naturally, though erroneously, associated with many more than his share of the antiquities in Scotland; and, among many others, we find that both the Roman camp and the cairn near Loudoun Hill have been named after him.

From the immediate neighbourhood of the Roman camp, and from its insulated and commanding situation, it is exceedingly probable that Robert Bruce may have occupied it previous to the skirmish at Loudoun Hill; and in this way the Roman name may have been lost. In fact, as we have already mentioned, the field of battle, as marked by the cairn, is close beside the Roman camp, and is not 200 yards distant from it.

This cairn was of great size, and the stones so large and useful, that they were employed in building a stone-wall near the place about 30 years ago. I have lately, however, had a new collection of stones heaped on the spot, and, at present, though small when compared with its former size, yet it is a well marked object, about fifty yards to the north of the road close by Loudoun Hill. It is another monument which connects this neighbourhood with the eventful fortunes of Robert Bruce.

I have already hinted at the idea, that Scotland, before the wars of the succession, had a better climate, and, in some districts, a greater number of inhabitants than for several centuries afterwards, and, as far as we can judge from these relics, this appears actually to have been the case. At all events, this district seems to have been more fought for than its appearance at present would merit. In these days, the wild look of the country around Loudon Hill certainly would not deserve a Roman camp, nor much military contest; and surely no one would now dream of hiding a treasure of silver coins in the midst of what was lately a bare uninclosed country. Five centuries ago, however, when it was in a great measure covered by a forest of gigantic oaks, the aspect and the shelter must have been very different from the present *.

* It may be mentioned, however, that even in much later times, the country around Loudoun Hill has been a field for contest. The celebrated skirmish of Drumclog fought between the Covenanters and Graham of Claverhouse, took place within a mile of Loudoun Hill, in the year 1679.

We are quite sure that many of our peat bogs in Scotland existed long before the period when the forests were destroyed, and were, and indeed are still, produced by the decay of herbaceous plants, or of small shrubs alone, without requiring the destruction of any trees for their origin. I have merely wished to bring forward some facts to assist in rendering it probable, that some of our mosses originated, when the forests, which, five or six centuries ago, covered large districts in Lanarkshire and in Ayrshire, were destroyed.

I have also wished to form some probable idea of the immense oak trees which at one time were so abundant in Scotland.

This mossy piece of ground, which has been so frequently mentioned, is now drained, and, last autumn, it produced an excellent crop of oats. It is nearly surrounded by extensive plantations, about 25 years old, and it is agreeable to see, that among all the trees which have been planted, no one is more vigorous than the oak. It is remarkable for its silvery grey polished bark, and for the length of its shoots. In future times these oaks may rival those of the Caledonian Forest, which, in that of Robert Bruce, surrounded the small lake; and the chance is, that their growth, in consequence of the improvements in draining, will be even more rapid in future than in those early times.

These monuments of the oaks of former periods, which our peat-mosses furnish, ought undoubtedly to have a considerable influence in regulating our future plantations; but it is really to be regretted that they do not appear to have attracted so much attention as they deserve. At present, how rarely do we observe an oak tree in Scotland even of six or eight feet in circumference. We have a few large ashes, sycamores, beeches, yews, and lime trees, but we have scarcely any oaks worth noticing, and these few, as those near Hamilton, are chiefly the remnants of the ancient Scottish woods.

We are informed that our climate is so bad, that even acorns do not arrive at maturity, and that, therefore, this country is not suited to the oak. But, surely, no one will say that the Caledonian Forest was planted, or that the new trees required for its supply for thousands of years, were obtained from a nursery, or had been grown from English acorns. The succession of oak trees was decidedly continued by those acorns which fell, and which were covered with the soil, and, in this way, escaped from becoming the food of the wild boar, or of other animals.

In the present day, in Scotland, indeed, we have hardly any oaks sufficient in maturity to produce a crop of ripe acorns, and, of course, we must bring our seed from England, where adult trees are abundant; but this is no proof that our climate is incapable of producing acorns sufficiently ripened for growth. It merely proves, that our adult trees are so few, that acorns can be collected more easily in England, and that a greater proportion of these, in consequence of superior climate and shelter, are productive. We have a number of large plantations of oak in different situations, but these are professed to be almost entirely intended for copse wood, yet no tree seems to be more admirably suited to the moist atmosphere of the west of Scotland than the oak; nor more calculated to thrive almost on every variety of soil, and even at a great height above the sea.

In these high situations, if they be even moderately sheltered, we find this tree growing freely on dry gravel, as where the large oak was found, on a spot consisting entirely of the diluvium formed from the decay of trap rocks; but, in the immediate neighbourhood, it grows almost equally well on very different soils, on decomposed whin, on earthy loam, and on stiff clay, and even at a height not less than 800 feet above the sea.

In common circumstances, the oak, for a few years after being planted, chiefly in consequence of a bad mode of management, is slow in its progress. It is soon overtopped by the quick-growing Scotch fir, the larch, or the spruce fir, which are very properly mixed with it. It is neglected, and very often dies. With moderate care, however, it soon begins to grow rapidly, and it keeps pace, nearly, with any other forest tree.

In this essay, I am well aware that conjecture may appear to have been too frequently employed; but, at all events, our large moss tree proves how favourable the climate of Scotland was, and probably still is, for the growth of the oak.

Historical Account of Experiments regarding the Influence of Colour on Heat, the Deposition of Dew, and Odours. By JAMES STARK, M. D. Edinburgh. Communicated by the Author.

DESCARTES was one of the first—if not the very first—of the modern philosophers who turned their attention to the subject of *colour* in connection with *light* and *heat.* It is not my intention to take any notice here of his theory of light. But in the course of his investigations, he observed that a black colour suffocates or extinguishes the rays which fall upon it, while white on the contrary reflects them *. Kepler, as quoted by Dr Priestley, had indeed previously asserted, that the reason why *black* objects grow hot sooner than *white* ones, is not properly owing to any difference in the colour, but because those substances which are black are of a more dry and inflammable nature. † But his conclusion from the known fact was but little calculated to promote the advancement of scientific inquiry.

It remained for a philosopher of our own country, and one to whom science is perhaps more indebted than to almost any other individual of that age, for its subsequent progress, to make the first correct experiments on the influence of colour over heat. This was the Hon. Robert Boyle. His experiments and observations upon colours were first published in the year 1663, and form the basis of all that has since been written on the subject.

To try whether white bodies reflect light more than others, he held a sheet of white paper in a sun-beam admitted into a darkened room, and observed that it reflected a far greater light than paper of any other colour. *Green*, *red*, and *blue*, were then compared together; the *red* gave much the strongest reflection, the *green* and *blue* almost the same. The *yellow* compared with the two last, reflected somewhat more light. *Red* and *purple* compared together, the former seemed to reflect

* Dioptrics, p. 50.

† Priestley's History of Discoveries relating to Vision, Light, and Colours, p. 141. Lond. 1772.

a little more light. *Blue* and *purple* compared, the former seemed to reflect a little more light. * Mr Boyle also found that common burning glasses will not for a long time burn or discolour white paper exposed to their action. When he was a boy (he says) he took great pleasure in making experiments with those glasses, and was much surprised at this remarkable circumstance. He observed also, that the image of the sun was not so well defined upon white paper as upon black; and that when he put ink upon the paper, so as to blacken its surface, not only was the moisture quickly dried up, but the paper which he could not burn before would presently take fire. Mr Boyle also found, that, by exposing his hand to the sun with a black glove upon it, it was suddenly and more considerably heated, than if he held his naked hand to the rays, or put on a glove of white leather.

Mr Boyle, struck with these results, put the matter to the test of further experiment. He took a large and broad tile, and having coated one-half of its surface *white* and the other half *black*, he exposed it to the rays of a summer sun. After exposure for some time, he found, that while the whited part remained cool, the portion that was black had grown very hot. He further varied the experiment, by leaving part of the tile of its native *red;* and after exposing the whole to the sun for a certain time, observed that this part grew hotter than the *white*, but was not so hot as the *black* part.

The same fact as to the absorption and reflexion of caloric had been previously observed in regard to black and white marble; and Mr Boyle relates, in further proof of its accuracy, that a friend of unsuspected credit informed him, that in a hot climate, he had seen eggs well roasted in a short time, by first blacking the shells, and then exposing them to the sun †.

That indefatigable experimenter, Dr Hooke, afterwards remarked, that black and white marble being exposed equally to the fire, the black will be found much hotter than the white, because the white reflected back the rays which the other did not; and that a piece of white marble or stone, if one half of it

* Boyle's Works by Birch, vol. i. p. 725. Lond. 1772.

† Boyle's Works, vol. i. p. 706-7.

was coloured black, would, when exposed to the fire, be much hotter on the black part than on the white *.

The subject of light and colours was now taken up by the greatest of names in modern science, Sir Isaac Newton. His discoveries were communicated to the Royal Society in a letter early in the year 1672. He had ascertained by experiments with the prism, that a beam of white light, as emitted from the sun, consists of several different colours, which possess different degrees of refrangibility. These primary and simple colours are *red*, *orange*, *yellow*, *green*, *blue*, *indigo*, and *violet*. He observed that the red was refracted least, and the violet most powerfully; and was thence led to the conclusion, that the same degree of refrangibility always belonged to the same colour, and the same colour to the same degree of refrangibility; that whiteness or white light is a compound of all the colours of the spectrum; that blackness was a privation of all colour; and that the colours of natural bodies are not qualities inherent in the bodies themselves, but arise from the disposition of the particles of each body to stop or absorb certain rays, and thus to reflect more copiously those rays which are not absorbed.

As my object, however, is not the investigation of the abstract nature of colours, or their production, as connected with light, but only to trace some of the modifications which coloured bodies, as they exist in nature, or are produced by art, exert over heat, I leave, for the present, the brilliant experiments of Newton. As connected, however, with Sir Isaac's analysis, it may be mentioned, that Sir David Brewster has very recently discovered that the prismatic spectrum consists of three different spectra, viz. *red*, *yellow*, and *blue*, all having the same length, and all overlying each other; and that the seven assumed colours are all compounded of these three simple and primary ones †.

The more immediate subject of these observations was now taken up by the celebrated Dr Franklin, who seems to have repeated most of Boyle's experiments, and detailed the results in a letter to a friend in 1761. He observed that different parts of dress, coloured black and white, imbibe the rays in different

* Birch's History of the Royal Society, vol. iv. p. 175.

† Edin. Trans. vol. xii. p. 123.

degrees; and that while the black part of the dress is quite hot to the touch, the white will be quite cool. "Again," he says, "try to fire paper with a burning-glass. If it be white you will not easily burn it, but if you bring the focus to a black spot, or upon letters written or printed, the paper will immediately be on fire under the letters." He further remarks, that fullers and dyers find black cloths, of equal thickness with white ones, and hung out equally wet, dry in the sun much sooner than the white, being more readily heated by the sun's rays; and that before a fire, heat penetrates black stockings sooner than white ones.

This sagacious observer now made an experiment with patches of different coloured cloths from a tailor's pattern-book. These were laid out on the surface of the snow in a morning of bright sunshine. The colours used in the experiment were *black*, *deep blue*, *lighter blue*, *green*, *purple*, *red*, *yellow*, *white*, and other colours, or shades of colour. In a few hours the *black* had sunk so much as to be out of the reach of the sun's rays; the *dark blue* almost as low; the *lighter blue* not quite so much as the *dark;* and the other shades of colour less as their tint was lighter; while the *white* cloth remained on the surface of the snow, "not having entered it at all." *

This experiment demonstrated, like all the previous ones, that the calorific rays were absorbed much more abundantly by the black than by any of the other colours; that the intermediate colours possessed a power of absorption in proportion as they receded from the black colour; and that the white portion of cloth had reflected nearly the whole rays.

An experiment similar to this, but with coloured metals, was made by Sir Humphry Davy, and the result published in 1799. This celebrated chemist took six pieces of copper (each an inch square, and two lines thick), of equal weight and density, and coloured one of their surfaces *white*, one *yellow*, one *red*, one *green*, one *blue*, and one *black*. On the centre of the under surfaces was placed a portion of a mixture of oil and wax, which became fluid at 76°. The plates were then attached to a board painted white, and the coloured surfaces of all the pieces equally

* Franklin's Works, vol. ii. p. 109. Lond. 1816.

exposed to the direct rays of the sun. The result was, that the cerate on the *black* plate first began to melt; then that on the *blue;* next the *green* and *red;* and lastly, the *yellow.* The square coated with *white* was scarcely affected by the heat, though the *black* had completely melted. *

The last of these experiments, though confirming as to coloured metals what Franklin had ascertained as to coloured cloth, does not seem to have led to any further investigation as to the modifying effect of colour on the absorption or emission of heat.

Though I have mentioned the experiment of Sir Humphry Davy in connection with, because it seemed more illustrative of, Dr Franklin's experiment, yet it is proper to remark, that there were previous inquirers into the subject of heat and colour.

The effect of a coating of black in raising the temperature of substances induced Dr Watson, afterwards Bishop of Landaff, to apply it to the thermometer in 1772. He exposed the bulb of an excellent thermometer to the direct rays of the sun, when the sky was perfectly free and clear. The mercury rose to 180° Fahrenheit, and remained stationary. He then covered the bulb, by means of a camel-hair pencil, with Indian ink. The mercury sunk a few degrees during the application of the coating, and the evaporation of the water; but immediately afterwards rose to 118°, evincing a rise of ten degrees from the application of the black coating. Dr Watson, though he does not seem to have carried the investigation farther, concluded from this experiment, that " if the bulbs of several thermometers were painted of different colours, and exposed to the sun at the same time for a given period, some conjectures respecting the disposition of the several primary colours for receiving and retaining heat might be formed, which could not fail of being, in some degree, interesting †."

Count Rumford was the next writer of note who drew the attention of scientific inquirers to the subject of heat. In a paper read before the Royal Society in 1792, he detailed numerous experiments on various substances to ascertain the cause of the conducting and nonconducting powers of bodies with re-

* Beddoes's Contributions to Physical Knowledge, pp. 44-5. Bristol 1799.

† Phil. Trans. Abridg. vol. viii. p. 371.

gard to heat. He used for this purpose a mercurial thermometer, the bulb of which was about $\frac{55}{100}$ of an inch in diameter, and its tube about ten inches in length. This was suspended in the axis of a cylindrical glass tube about three quarters of an inch in diameter, ending with a globe of $1\frac{6}{10}$ inch in diameter, in such a manner that the centre of the bulb of the thermometer occupied the centre of the globe. The space between the internal surface of the globe and the surface of the bulb of the thermometer was filled with the substance whose conducting power was to be determined. The instrument was now heated in boiling water, and afterwards, being plunged into a freezing mixture of pounded ice and water, the times of cooling from 70° to 10° of Reaumur were observed and noted down.* The matters operated upon were raw silk, sheep's wool, cotton wool, linen in the form of the finest lint, fur of the beaver, fur of a white Russian hare, and eider down, sixteen grains in weight of each. The difference in the results from these articles was extremely small, and much less than the Count expected to find them, probably from all the articles being nearly of the same colour, and though of the same weight of different bulk, or occupying a larger space. Of the seven substances, ***hare's fur*** and ***eider down*** were the warmest; after these came ***beaver's fur***, ***raw silk***, ***sheep's wool***, ***cotton wool***, and lastly *lint* or the scrapings of fine linen.†

Hare's fur, . . .	1315″	Sheep's wool . . .	1118″
Eider down, . . .	1305″	Cotton, ...	1046″
Beaver's fur, . .	1296″	Linen, ...	1032″
Raw silk, ...	1284″		

In the year 1804, Count Rumford communicated to the Royal Society another interesting series of experiments on the nature of heat and the mode of its communication. These experiments were made with hollow brass cylinders filled with water, and coloured with various substances. A thermometer was attached to the cylinder, and the rate of cooling marked. Numerous experiments with the various substances used are detailed; but the most striking one, in the Count's opinion, was that in which the cylinder was blackened, by holding it over

* Rumford's Essays, vol. ii. p. 430. Lond. 1798.
† Ibid. p. 437.

the flame of a wax candle. In one instrument in which the surface was naked, or of its natural colour, the time of cooling through the standard interval of ten degrees was $55\frac{7}{8}$ minutes; while in another with the surface blackened in the manner mentioned it was only $36\frac{1}{8}$ minutes. The black matter carefully wiped off with a piece of linen rag, which was accurately weighed before and after the experiment, was found to be $\frac{1}{18}$ of a grain Troy weight. It had covered a surface of polished brass equal to fifty superficial inches. "How this very thin covering (says the Count) which, if the specific gravity of the black matter were only equal to that of water, would amount to no more than $\frac{1}{4309}$ of an inch in thickness, could expedite the cooling of the instrument in the manner it was found to do, is what still remains to be shown."*

Count Rumford afterwards varied this experiment by covering the cylinders with an animal substance—gold-beaters' skin. "Having covered," says he, "the two large cylindrical vessels No. 3. and 4. with gold-beaters' skin, I painted one of them black with Indian ink, and filling them both with boiling hot water, I exposed them to cool, in the manner already described in the air of a quiet room. No 4, which was *blackened*, cooled through the standard interval of ten degrees in $23\frac{1}{2}$ minutes; while the other (No. 3.) which was not blackened, took up 28 minutes in the same interval †."

The result of these and numerous other experiments was, "that those substances which part with heat with the greatest facility or celerity, are those which acquire it most readily or with the greatest celerity ‡."

Count Rumford seems to have rested in this conclusion, and does not appear to have extended his inquiries as to the effects of heat or cold on other colours. This appears the more remarkable, as he asserts, after relating the unexpected results of these experiments, "that nothing should prevent him from making the experiment of blackening his skin, or at least of wearing a black shirt," if he were called upon to live in a very hot country.

In connexion with this idea of Count Rumford, and leading

* Philosophical Transactions, 1804, p. 95, 96. † Ibid. p. 59.
‡ Ibid. 1804, p. 128.

to the same conclusion, I may here notice a series of experiments made by Sir Everard Home at a much later period (1820), to ascertain why the *rete mucosum* of the Negro formed a defence against the scorching effects of the sun's rays. He exposed the back of his hands to the rays of the sun, one hand being covered with black cloth, the other naked. A thermometer was placed upon each. After ten minutes the degree of heat of each thermometer was marked, and the appearance of the skin examined. This was repeated three different times with the following results.

1*st*, Thermometer under *black* cloth	91°	—— the other 85°
2*d*, ...	94°	.... 91°
3*d*, ...	106°	.... 98°

The skin where uncovered was scorched; the other hand had not suffered in the slightest degree. In another experiment, the back of a Negro's hand was exposed to the sun with a thermometer upon it which stood at 100°; at the end of ten minutes the skin had not suffered in the least.

An experiment with the rays concentrated by a lens for fifteen minutes singed the *black* kerseymere with which Sir Everard's arm was covered, but made no impression on the skin; while with *white* kerseymere in the same circumstances a blister was formed. Even the rays concentrated on the back of the hand of a Negro for fifteen minutes produced no sensible effect.*

Nearly about the period when the last experiments of Count Rumford were made public, the celebrated Essay on Heat of Sir John Leslie appeared. This work was published in 1804, and included a series of experiments which have formed the basis of what has since been written on the subject of heat. Though the results of most of Sir John's experiments seem to have been powerfully influenced by the black colouring of part of the apparatus employed; and though the differential thermometer and photometer are constructed on the absorbent power of a black-coloured ball, yet he expresses himself doubtful of the influence of colour in producing these results. "The last quality," says he, "which may perhaps have some influence in modifying the power of a substance to emit or absorb heat is its colour. On colour the disposition to imbibe the rays of light principally depend. *Black* is most absorbed of

* Phil. Trans. 1821, 3–5.

light, *white* discharges it most copiously, and scarlet is next in order, its emissions being very bright and dazzling *."

"In illustration of this," says Sir John, "I painted three sides of a square tin cannister with lamp-black, whiting, and minium,—each being worked up with as little size as would give consistence to the pigment. Being presented to the reflector, the effect of the black surface was 100°, that of the white only 85°, and that of the red 90° †."

Sir John made other experiments on the rate of cooling of a globe of planished tin filled with warm water, in which a thermometer was inserted. The air of the room was at 35° centigrade, and perfectly steady, and the progress of the ball in cooling was carefully remarked. "From the station of 35° till the internal thermometer sunk to the middle point or 25°, the time elapsed was 156′." Sir John now painted the surface of the ball with a coating of lamp-black, and again filling it with warm water, scrupulously repeated the experiment. "The same effect," says he, "was now produced, or one-half of the heat expended, in the space of only 81′ ‡."

Sir John appears evidently to have been struck with this result, though in perfect accordance with all the experiments which had been previously made, of which, however, he does not seem to have been aware. "The application of a coat of pigment to a metallic surface, instead of retarding the effect, almost doubles its discharge of heat. This fact," says he, "equally curious and important, is most contrary to the prevalent notions, and seems not to have been observed. Had the reverse taken place, we should have readily satisfied ourselves with attributing it to the slow conducting power of the superficial crust §."

Notwithstanding these results, Sir John rather inconsistently concludes his remarks on the subject of colour, by saying, that, "on the whole, it appears exceedingly doubtful if any influence of that sort can be justly ascribed to colour. But (says he) the question is incapable of being positively resolved, since no substance can be made to assume different colours without at the same time changing its internal structure ."||

* Essay on Heat. p. 93. † Ibid, p. 93, 94. ‡ Ibid. p. 268.
§ Ibid. p. 270. || Ibid. p. 95.

Four years previous to the appearance of Sir John Leslie's work, and the later experiments of Count Rumford, the investigations of the late Sir William Herschel brought out some new facts regarding the modification of heat by colour. This celebrated astronomer, when viewing the sun through large telescopes by means of differently coloured glasses, sometimes felt a strong sensation of heat with very little light, and, at other times, a strong light with little heat,—differences which appeared to depend on the *colour* of the glasses used. This observation led to his researches on the heating power of the prismatic colours, which were published in the Philosophical Transactions for 1800. The experiments made with a view to ascertain the heating power of the different rays are detailed in the volume now referred to, and afford very interesting results *.

In this unsatisfactory state the subject of heat, as connected with colour, was left. Subsequent inquirers seem to have adopted the sentiments of Sir John Leslie, and regarded farther inquiry as hopeless. Even Sir Humphry Davy, though he had instituted an experiment with coloured pieces of metal similar to that of Dr Franklin with coloured pieces of cloth, and ascertained the comparative powers of different colours in absorbing heat, did not pursue the subject further. He appears to have acquiesced in the general idea, that the thing was incapable of being demonstrated, though, in one of his works, he distinctly states it as probable that the colour of bodies is connected with their power of absorbing heat. "The manner (says he) in which the temperature of bodies are affected by rays producing heat, is different for different substances, and is very much connected with their colours. The bodies that absorb, as it is called, most light, and of course that reflect least, are most heated when exposed either to solar or terrestrial rays. Black bodies in general are more heated than red; red more than green; green more than yellow; and yellow more than white. Metals are less heated than earthy and stony bodies, or than animal and vegetable matters. Polished surfaces are less heated than rough surfaces †."

The elementary writers on chemistry, since the date of Sir

* Phil. Trans. 1800.

† Elements of Chemical Philosophy, vol. i. Lond. 1812.

Humphry Davy's work, merely mention the subject in similar terms to his; and even Dr Edward Turner, one of the latest and most acute chemical investigators, remarks, "that the absorption of luminous caloric, whether proceeding from the sun or a common fire, is very much influenced by colour; it is most considerable in black and dark coloured surfaces, while it is much less in white ones. The influence of colour, on the contrary, in the absorption of non-luminous caloric, is exceedingly slight: it remains to be proved indeed whether any effect can be fairly attributed to this cause. *"

Such is the present state of our knowledge of the influence of colour upon the absorption and radiation of heat. The influence of colour, at least of a black surface, though noticed by Boyle, Dr Watson, and more lately by Count Rumford, and remarked as an unaccountable circumstance in his results, failed to induce that active experimentalist to make a trial of other colours. Sir John Leslie was equally surprised at the result of a nearly simultaneous experiment of the same nature, which he supposed not to have been before observed, and remarks, not only that it is contrary to the prevalent notions on the subject, but that the question is incapable of being resolved. Dr Thomas Thomson, in his late work, says, that "hitherto it has been impossible to ascertain the efficacy of hardness and softness, or of *colour*, upon the radiation of heat †."

I am far from thinking that the experiments I have made ‡ will solve all the phenomena observed, where *colour* may be supposed to influence the results. All I maintain is, that colour exerts a powerful influence over the absorption and radiation of caloric, either luminous or non-luminous. Future experiments may determine the extent of this modifying principle, which, till now, has been doubted or denied by most of the writers who have alluded to the subject.

I.—*On the Absorption of Heat by differently Coloured Substances.*

My first experiments were made with wool variously coloured, and as nearly as possible of the same degree of fineness. These

* Elements of Chemistry, fourth ed. p. 18.

† An Outline of the Sciences of Heat and Electricity. Lond. 1830, p. 147.

‡ See Phil. Trans. 1833, p. 285. *et seq.*

coloured parcels of wool, of which equal weights were taken, were wrapped severally round the bulb of a delicate thermometer, graduated on the tube; the thermometer was then placed in a glass tube, the tube plunged into boiling water, and the time which elapsed during the rise of the thermometer from one given point to another noted. The apparatus was in fact nearly the same as that employed by Count Rumford. With twenty grains by weight of each wool, the result was that,

Black wool rose from 50° to 170° Fahr. in	6′ 35″
Green, ...	7 43
Scarlet, ...	8 3
White, ...	8 45

The next set of experiments were made with the common air thermometer, graduated to tenths of an inch in a descending series. The bulb was coated with the various colours as mentioned, and heat thrown on the ball by means of planished tin reflectors about three inches in diameter, from a gas Argand burner. At the commencement of the experiments the coloured fluid always stood at 1°. Coated with blacking from a wax candle, and the heat reflected as mentioned:—

The fluid descended in a mean of four experiments to	83°
Dark brown, a mean of three experiments to .	74
Orange-red, ...	58
Yellow, ...	53
White, ...	45

Though the above experiments, and others detailed at length in the paper alluded to *, particularly those with the coloured wool, do not coincide exactly in minute particulars, the general result of the whole is nearly the same as to the ratio of the absorbing powers. Minute differences in experiments made at intervals may be easily accounted for, from the various states of aggregation of the wool, and its being placed equally, or not, all round the bulb of the thermometer. The experiments with the air thermometer are not liable to the same objection, and the mean of a number of experiments made with this instrument would afford a pretty correct estimate of the absorbent powers of different colours.

The experiments decidedly shew that colour, independently

* Phil. Trans. 1833.

of the nature of the substance employed, has a powerful influence over the absorption of caloric, and agree in a striking manner with the results of the experiments made by Dr Franklin and Sir Humphry Davy, upon bodies of very different qualities.

II.—*On the Radiation of Heat by differently Coloured Substances.*

It has been stated, as a general principle, that the radiating powers of bodies in regard to heat bear a proportion to their absorbing powers; that is to say, that the more quickly a body is heated, the more quickly does it part with its heat. On the contrary, it is known, that bodies which are the most powerful reflectors of heat, are those which, when heated, retain that heat the longest. This has been more particularly noticed with regard to the metals, in which, by diminishing the polish of the surface, it was found that their reflecting powers were much reduced, while their power of radiating or giving out heat was increased. No experiments have, so far as I know, been made to prove the influence of colour in modifying the radiation of heat, except with regard to metal balls and cylinders coated with black.

In these circumstances, it struck me that it would be important to ascertain whether colour, which exerts an influence so powerful over the absorption of heat, might not exert an equal influence over its radiation. To ascertain this point was now the object of my investigation, and my anticipations as to the result were fully realized, as the following experiments demonstrate. If they are not so complete as could be wished, or the best that could be devised, they prove sufficiently the general principle, and may pave the way for more accurate investigations.

The first experiments which I made on the radiation of caloric were with differently coloured wools. The colours were black, red, and white, of each thirty grains weight. Having rolled each round the bulb of the thermometer, as formerly mentioned, and placed them in the tube, it was heated in boiling water to the temperature of about 190° Fahr.; and when the mercury in the thermometer began to descend, and had fallen to 180°, it was plunged into water at 45°, and the rate of cooling accurately noted. The following were the results:

Black wool fell from 180° to 50° Fahr. in .	21′ 0″
Red, ...	26 0
White, ...	27 0

Another experiment with black, red, and white wools, twenty grains of each, and with the temperature raised to 173°, gave the following results:

Black wool fell from 170° to 60° Fahr. in .	15′ 45″
Red, ...	17 0
White, ...	18 30

The next experiments were made with wheat-flour, coloured black, brown, yellow, and white. The black colouring matter was lamp-black; the brown was the umber of the shops; and gamboge powder was the yellow. I took 100 grains of each coloured flour, and placed them separately in a tube of about three quarters of an inch in diameter, then sunk the bulb of the thermometer into the middle of the flour, and heated the tube to about 190° Fahr. When the mercury began steadily to descend, and arrived at 180°, I plunged the tube into water at 45°, and observed the rate of cooling. The following were the results:

Black flour fell from 180° to 50° Fahr. in .	9′ 50″
Brown, ...	11 0
Yellow, ...	12 0
White, ...	12 15

A third set of experiments was made by coating the ball of the air-thermometer with various pigments; but for these I must refer to the paper alluded to *.

These experiments demonstrate, that differently coloured substances possess a specific influence on the absorption of heat or caloric, both luminous and non-luminous, and that they give off their caloric in the same ratio as they absorb it.

The experiments may be varied to any extent, by using different substances; and even water in coloured vessels cools more or less quickly, according to the colour of the vessel in which it is held. For instance, to ascertain this, I filled glass balls about an inch and a quarter in diameter with water at 120° Fahr. and placed a thermometer in the fluid. The time which elapsed during the fall of the mercury through 25° was accurately noted; and the results were, that the ball coated with Prussian blue fell through that interval in seventeen minutes; the ball coated

* Phil. Trans. 1833, p. 294, et seq.

with orange-red in eighteen minutes; and that coated with white in nineteen minutes. The temperature of the air in the room was 50°.

The demonstration of the influence of colour on the absorption and radiation of caloric, may tend to open up new views of the economy of nature, and perhaps suggest useful improvements in the management and adaptation of heat. Dr Franklin *, who never lost sight of practical utility in his scientific investigations, from the result of his experiments with coloured cloths on the absorption of heat, drew the conclusion, " that black clothes are not so fit to wear in a hot sunny climate or season as white ones; that white hats should be generally worn in summer; and that garden-walls for fruit-trees would absorb more heat from being blackened."

Count Rumford and Sir Everard Home, on the contrary, come to a conclusion entirely the reverse of this. The Count asserts, that if he were called upon to live in a very warm climate, he would blacken his skin, or wear a black shirt; and, in point of fact, in the decline of his life, he astonished the Parisians by appearing in the streets in winter entirely dressed in white. Sir Everard, from direct experiments on himself and on a negro's skin, lays it down as evident, that the power of the sun's rays to scorch the skins of animals is destroyed when applied to a dark surface, although the absolute heat in consequence of the absorption of rays is greater †. Sir Humphry Davy explained this fact, by saying, that " the radiant heat in the sun's rays is converted into sensible heat." With all deference to the opinion of this great man, it by no means explains why the surface of the skin was kept comparatively cool. From the result of my experiments, it is evident, that if a black surface absorbs caloric in greatest quantity, it also gives it out in the same proportion; and thus a circulation of heat is, as it were, established, calculated to promote the insensible perspiration, and to keep the body cool. This view is confirmed by the observed fact of the stronger odour exhaled by the bodies of black people.

The different shades of colour by which races of men inhabiting different climates are distinguished, equally possess, there

* Dr Franklin's Works, vol. ii. p. 109. Lond. 1806.

† Phil. Trans. 1821, p. 6.

is reason to believe the quality of modifying the individual temperature, and keeping it at the proper mean. This adaptation of colour may perhaps be traced in the inhabitants of every degree of latitude, and may be found to correspond with the causes which limit the range of plants and animals. The effect of the exposure to the sun in our own country in warm seasons is temporarily to change the colour of the parts submitted to its influence, and to render them less susceptible of injury from the heating rays.

The influence of colour, as modifying the effects of heat, is also strikingly illustrated in other classes of the animal kingdom. The quadrupeds, for instance, which pass the winter in northern latitudes, besides the additional protection from cold they receive in the growth of downy fur, change their colour on the approach of the cold season. The furs of various hues which form their summer dress are thrown off, and a white covering takes its place. Hence the white foxes, the white hares, and the ermine of the arctic regions. Even in more temperate climates, and in our own country, the hare, in severe winters, often acquires a white fur; and the stoat or ermine is found with its summer dress more or less exchanged for a winter clothing of pure white. Some writers on natural history state these changes as means of protection to the animals from their enemies, by assimilating their colour to the winter snow. Without denying that this may be one final cause for the periodical change of colour, I am rather disposed to consider it as accommodating the animal to the changes of season it undergoes. The white winter coating, as is evident from my experiments, does not throw off heat so rapidly as any of the other colours; and hence its use in preserving the animal temperature.

The feathered tribes which inhabit northern latitudes, afford still more remarkable instances of the adaptation of colour to the changes of temperature. The summer dress of many families is so different from their winter plumage, as to have led many ornithologists to multiply species as the animal was described in its winter or summer plumage. The ptarmigan is a familiar example. Mr Selby remarks, that "the black deep ochreous yellow plumage of the ptarmigan in spring and summer gradually gives place to a greyish-white; the black spots become broken, and assume the appearance of zig-zag lines and specks. These, again, as the season advances, give place to the pure immaculate plumage which distinguishes both sexes in winter." *

The display of colours in the plumage of the birds of tropical climates is also in strict accordance with the observed facts of the influence of colour over the absorption and radiation of heat. The metallic reflexions and polished surface of the whole family of humming-birds is admirably suited to their habits; and the colours of the wings of the Lepidoptera in the class of Insects, there is little doubt, serves some similar purpose, in maintaining the temperature of the animals at a proper mean. In proportion to the diminution of temperature, and the distance from the equator, a corresponding dilution of colour in animals takes place, till, in temperate countries, it is almost uniformly of a sober grey. In the arctic regions, all colour except white and black disappears,—modifications of which, with very little variety of other colours, form the summer and winter clothing of most of the northern tribes of birds.

In the vegetable kingdom, I am disposed to believe that the colours of the petals of flowers serve some useful purpose, in regard to preserving the temperature of the parts necessary for reproduction at the proper mean, and that the varied pencilling of Nature has thus an object beyond merely pleasing the eye. In this view, the quality of colour, so widely extended, and so varied and blended in every class of natural bodies, acquires a further interest, in addition to its ministering to the pleasures of sight, and affords a new instance of that benevolence and wisdom by which all the arrangements of matter are calculated to excite and gratify the mind directed to their investigation.

Even in the inorganic portion of Nature, and in northern climates, the portion of heat imbibed by the soil during a short summer, is prevented from escaping by the covering of snow which falls in the beginning of winter, and thus the temperature necessary for the scanty vegetation is kept up. By this white covering, vegetables are enabled to sustain a lengthened torpidity without suffering from the injurious effects of frost, and the ground is preserved from partial alternations of temperature,

* Selby's Illustrations of British Ornithology, part i. p. 312.

till the influence of the sun at once converts the northern winter into summer, without the intervention of spring.

III.—*On the Influence of Colour on the Deposition of Dew.*

As connected with the preceding investigations, it may be mentioned, that I had projected a train of experiments to ascertain the proportions in which dew was deposited on variously-coloured substances. Dr Wells found the deposition of dew influenced by the radiating power of the substance employed; but neither he nor any of the philosophers who have treated of the subject, seem to have been aware of the modifying effects of colour on the absorption and radiation of heat. My avocations have hitherto prevented me from following out these experiments; but to shew that the influence of colour may be extended to moisture, and of course to dew, I shall give from my notes the result of two experiments made to ascertain this point.

Jan. 16. 1833.—I exposed last night (the temperature ranging from 28° to 30°, and with a dense fog) ten grains of black wool, the same quantity of scarlet wool, and an equal weight of white wool, on a black board, which was placed on the leads on the top of the house. When taken in this morning, and carefully weighed, the black wool had gained 32 grains, the scarlet wool 25 grains, and the white wool 20 grains, deposited on the wool in the form of hoar-frost.

A few nights afterwards, after a slight thaw, and when towards night the temperature fell to 31°, I again exposed four colours of wool, ten grains of each, in the same manner. By next morning, the black wool had gained 10 grains, the dark green $9\frac{5}{10}$ grains, the scarlet 6 grains, and the white 5 grains.

Dr Wells had indeed made experiments on the deposition of dew, with equal quantities of black and white wool; and in four out of five experiments, the black wool was found to have acquired a little more dew than the white: " whence," says he, " I concluded that it had also radiated a little more heat. But I afterwards remarked, that the white wool was somewhat coarser than the black, which might have occasioned the difference of their attraction for dew." On another night, he made an experiment with pasteboard covered with white paper, and pasteboard covered with paper blackened with ink. At daylight," says he, " I observed hoar-frost upon both pieces, but the black seemed to have a greater quantity than the white." *

These facts, it might have been supposed, would have led Dr Wells to further experiments with different colours. But the reverse was the case. He quotes Mr Leslie as to the hopelessness of success, without making a farther attempt; " since a black body almost always differs from a white one in one or more chemical properties, and this difference may alone be sufficient to occasion a diversity in their powers of radiating heat."

IV.—*On the Influence of Colour on Odours.*

If the influence of colour over heat attracted but little the attention of philosophers employed in the investigation of the absorbing and radiating powers of different substances, even when presented to their notice in anomalous facts, which could not easily be explained on any other principle, it is not to be wondered at, that the apparently far less appreciable influence of colour on odours should have totally escaped notice. In point of fact, I am not aware that the subject has hitherto been investigated, and know of no recorded facts in which the influence of colour over odours has been pointed out. In attempting to shew from experiment, that the colour of bodies in imbibing odours is correlative with the power of colour over the absorption and radiation of heat, I state a fact, which, though new to science, is in admirable correspondence with the known properties of light and heat. And though I may not be able, from the nature of the substances subjected to experiment, absolutely to determine the amount of this connexion, I trust my imperfect investigations may form the basis of new and better devised experiments, by directing the attention of men of science to this hitherto untrodden field of inquiry.

Odour is that quality of bodies which is distinguished by the sense of *smell*. This sense is nearly connected with that of *taste*; so much so, indeed, that they have sometimes been considered as modifications of the same sensation. It has been remarked, that what is agreeable to the olfactory organs, is in general a

* Wells on Dew, p. 106. Lond. 1814.

good index of what is pleasant to the taste, and fit for the purposes of nutrition. In the animal kingdom, and among many of the *Mammalia,* it is the sense of smell that guides the still blind young to the mother's teat; and many of the beasts of prey seem to be led to the haunts of the animals which form their food, by intimations conveyed through their olfactory apparatus. According to Scarpa, this sense, so acute in rapacious birds, is very obtuse in the orders *Gallinæ* and *Passeres.** Reptiles possess the sense of smell; and it is said that particular odours, such as that of rue (*Ruta graveolens*) is disliked by serpents, and that the emanations from the *Aristolochia anguicida* is even capable of killing the rattlesnake. Perhaps the jugglers of Egypt and India may avail themselves of such odours as enable them to handle with impunity the reptiles which they display.

Odours also powerfully affect fishes, as is well known from the different preparations of bait in use among anglers; and facts are stated, which indicate that the odours of substances in many cases guide the insect races to their proper food. In the vegetable kingdom, odours are sensible in the leaves, roots, and particularly the flowers and seeds of many families; and in the mineral kingdom, many species are principally distinguished by the odours they emit, when rubbed or breathed upon,—the heat and moisture in this last case seeming to disengage the odorous particles.

It may, therefore, be laid down as a general rule, though not without some apparent exceptions, that every body in nature possesses, in addition to its other distinctive qualities, an odour or effluvium particularly its own.† In the animal kingdom, for instance, not only has every species a characteristic smell, as the horse, the ox, &c., but every individual may have an atmosphere around it of emanations peculiar to that individual alone. In the human race, not only is this the case, but even the differences of age and sex are marked by peculiar odours. These emanations may be produced or modified by food, occupation, climate, the passions, personal cleanliness, or the reverse; but it is not

* Osphrésiologie, ou Traité des Odeurs, &c. Par Hippol. Cloquet, D. M., 2d ed. Paris, 1821.

† Haller, Elementa Physiologiæ, v. 154.

the less true that they do exist.* It is by means of this special odour, this individual atmosphere, that the dog follows the traces of wild animals hours after they have passed; and it is by this that the same animal is enabled to find the track of his master, and to distinguish him at once among a thousand individuals.

Further, besides the odours or emanations of health, there exist particular odours, characteristic of animal bodies in a state of deranged action or disease. Thus in fever, small-pox, and other eruptive diseases, a particular odour is exhaled from the patient, typical of the affection; and this, in many cases, is so well marked, that the nature of the ailment may often be distinguished by the odour alone. The particular emanation which arises in the ill-ventilated and often crowded apartments of the poorer classes of society, and in prisons, is well known, when concentrated, to be extremely dangerous; and modifications of this and similar effluvia, when more attention shall be directed to the subject, may often afford the physician an indication of lurking disease.

The remarkable diffusion of odorous particles through the air, without the odorous body adding perceptible weight to the medium by which it is communicated, was observed by the celebrated Mr Boyle. This philosopher had turned his attention to odours, or *effluviums*, as he has termed them, and made experiments on certain odorous bodies, which are detailed in his essay on "the strange subtilty of effluviums." This sagacious experimenter, and almost the only writer on the subject previous to Cloquet in 1815, treats the subject of effluviums under the following heads:—"1. The strange extensibility of some bodies while their parts remain tangible. 2. The multitude of visible corpuscles that may be afforded by a small portion of matter. 3. The smallness of the pores at which the effluvia of some bodies will get in. 4. The small decrement of bulk or weight that

* "Les habitans du Quercy et du Rouergue se nourissent de froment, d'oignons, d'ail, et boivent habituellement du vin. Ceux de la Haute-Auvergne ne vivent au contraire que de lait, de fromage, de seigle, de sarrasin, et ne boivent que de l'eau. Lorsque la saison des moissons rassemble ces peuples dans un même canton, on distingue facilement les Quercinois et les Rouergats à l'odeur fetide et ammoniacale qu'ils répandent autour d'eux, tandis que celle des Auvergnats rappelle le petit-lait aigri et tournant à la putrefaction."—*Osphresiologie*, p. 65.

a body may suffer by parting with a great store of effluvia And, 5. The great quantity of space that may be filled, as to sense, by a small quantity of matter, when rarified or dispersed."

In illustration of these divisions of his subject, Mr Boyle instances the extreme extensibility of gold; and the expansion of half a grain of gunpowder, which, when exploded, filled a space 500,000 times greater the powder. The dissolution of a grain of copper in sal-ammoniac is still more surprising, communicating a tincture to 28,534 its weight of water; a single grain gave a blueness to above 256,806 pints of limpid water, and even a perceptible colour to double that quantity.*

The results of these experiments with gold, gunpowder, and the colouring matter of dissolved copper, seem to be stated for the purpose of gaining credence as to the still more wonderful dissemination of the particles of odorous bodies. As Mr Boyle's experiments on odours or effluviums are, however, at the distance of nearly two hundred years, the only one which have been made, I give the details in his own words:—

"Having, for curiosity sake, suspended in a pair of exact scales, that would turn with a very small part of a grain, a piece of ambergris bigger than a walnut, and weighing betwixt an hundred and six score grains, I could not, in three days and a half that I had opportunity to make the trial, discover, even upon that balance, any decrement of weight in the ambergris, though so rich a perfume, lying in the open air, was like in that time to have parted with good store of odoriferous steams. And a while after, suspending a lump of assafœtida five days and a half, I found it not to have sustained any discernible loss of weight, though, in spite of the unfavourable cold weather, it had about it a neighbouring atmosphere, replenished with fetid exhalations. And when, twelve or fourteen hours after, perhaps upon some change of weather, I came to look upon it, though I found that in that time the equilibrium was somewhat altered, yet the whole lump had not lost a quarter of a grain, which induced me to think that there may be steams discernible even by our nostrils, that are far more subtile than the odorous exhalations of spices themselves." †

* Boyle's Works, vol. iii. p. 661, 668. † Ibid. p. 672, 673.

Keill, in his Introduction to Natural Philosophy, founding upon these experiments of Mr Boyle, makes a calculation of the number of odorous particles, which may thus be in a sphere of five feet radius. He supposes that there is but one particle of the odorous body in every part of that space, which part is equal to the fourth of a cubic inch; and though it is probable that effluvia so rare would scarcely, if at all, affect the sense of smell, yet upon this principle he calculates that in a sphere of the semidiameter of five feet there will be 57,839,616 such spaces, and of course just so many odorous particles.*

In Mr Boyle's experiment with assafœtida, the mass exposed to the air lost, in six days, the eighth part of a grain in weight; but since the flux of effluvia from an odoriferous body is continual, it is manifest it ought to be proportionate to the time; and therefore, in one minute's time, the weight of the effluvia flowing from the assafœtida would be equal to $\frac{1}{69120}$ of a grain, and the magnitude of the particles equal to $\frac{8}{10000000000000000}$ parts of a cubic inch.†

Haller,‡ for more than forty years, preserved 400 dissertations, of forty pages each, which a single grain of ambergris had perfumed, and at the end of that period they had lost nothing of their odour. This learned physiologist has calculated that each inch of the surface of the papers had been impregnated with $\frac{1}{2691064000}$ of a grain. The surface of the paper was estimated at 8000 feet, and it lay in a bed of air at least one foot thick for 14,600 days. Mr Boyle also mentions his having a pair of Spanish perfumed gloves which preserved their odour for nearly thirty years.§

Mr Boyle further remarks, in proof of the prodigious diffusion of odorous effluvia through vast tracts of air, that a friend of his sailing along the coast of Ceylon for almost a whole day, at the distance of twenty or twenty-five miles, the wind blowing from the shore, felt the air "manifestly odoriferous."‖ Lord Valentia, a late traveller, remarks of the same island, that at nine leagues distance, the aromatic odour of the spices was dis-

* Keill's Introduction to Natural Philosophy, p. 48, 49. Lond. 1758.

† Ibid. p. 49, 50.

‡ Elementa Physiologiæ, tom. v. p. 157. Lausannæ, 1769, 4to.

§ Boyle's Works, vol. iii. p. 677.

‖ Ibid. vol. iii. p. 677.

tinctly felt.* Diodorus Siculus says something analogous of the coast of Arabia; and Bartholin states, that the odour of rosemary was recognised forty miles from the coast of Spain.†

Certain odours, or odoriferous particles, it is well known, combine with, or adhere in preference to, different bodies, and the presence or absence of heat, light, moisture, &c. are known to modify their action. Some odours are found to be more easily retained by spirituous fluids; other are attached by oils; alcohol is the best vehicle for balsamic substances; and fat bodies absorb best the odour of the *Liliaceæ.* Gloves and paper, as mentioned before, preserve for a long time the odour of ambergris; paper and cotton that of musk; while wool is said to retain fetid odours strongly. It is upon the knowledge of these facts that the fabrication of essences, pommades, pastilles, &c. is founded; and it is by this means that the fragrance of spring and summer is perpetuated through the winter months.

There is one quality in odours which requires to be particularly mentioned, and that is, their capability of being acted upon by moisture and heat. By whatever means this is accomplished, whether by setting free or dissolving the latent odorous particles, or by merely increasing their intensity of action, the fact is certain, and matter of ordinary observation. Every one must have perceived the increased intensity of odorous emanations in a garden or field after a summer's shower; and the almost insupportable fetid exhalations which arise from stagnant water in ditches in the same circumstances, is equally well known. The emanations from ditches, indeed, show the existence of moisture in the atmosphere before rain, and afford the husbandman an indication of the coming shower.

It is not my object here to enter into details concerning the nature of the different odours, their classification, or particular effects. Linnæus has classed vegetable odours in seven sections‡; Fourcroy has divided them into five genera §; Haller attempted to class them in two divisions, as they were agreeable or the reverse; and others have classed odours, as they were produced

* Lord Valentia's Travels.

† Osphresiologie, p. 51.

‡ Amen. Acad. tom. iii. p. 195.

§ Annales de Chimie, tom. xxvi. p. 232.

from the animal, vegetable, or mineral kingdom. My aim is only to show, that odours, like heat and dew, are influenced by *colour*, and that the same laws which regulate the absorption and radiation of heat and light, may be applied with equal propriety to the imbibition and emission of odorous particles.

My first experiments on odours, made some years ago, were with articles submitted to the action of odorous substances, by being inclosed with them in drawers or boxes for a certain period, and the amount of attraction ascertained by the intensity of smell. In all these experiments, however, reliance had to be placed upon one sense alone, viz. that of smell, as none of the substances employed had gained any appreciable weight. I was therefore desirous that, if possible, at least one experiment should be devised, which would show, by actual increase of weight, that one colour invariably attracted more of any odorous substance than another; and upon considering the various odorous substances which could be easily volatilized without change, and whose odour was inseparable from the substance, I fixed upon camphor as the one best suited to my purpose. In an experiment of this nature, it was necessary that the camphor should be volatilized or converted into vapour, and that the coloured substance should be so placed as to come in contact with the camphor while in that state. It was therefore of the first importance to prevent currents of air within the vessel in which the experiment was conducted, and with this view I used a funnel-shaped vessel of tin-plate, open at top and bottom. This rested on a plate of sheet iron, in the centre of which the camphor to be volatilized was placed. The coloured substances, after being accurately weighed, were then supported on a bent wire, and introduced through the upper aperture. This was then covered over with a plate of glass. Heat was now applied gently to volatilize the camphor; and when the heat was withdrawn, and the apparatus cool, the coloured substances were again accurately weighed, and the difference in weight noted down.

Proceeding on this plan, I arrived at the most satisfactory and conclusive results. The deposition of the camphor in various proportions on the coloured substances submitted to experiment, offered evidence of the particular attraction of colour for odours, resting on ocular demonstration; and when to this is added a positive increase of weight as ascertained by the balance, the conclusions previously drawn from the sense of smell are confirmed in a singular and very satisfactory manner. I have in this mode repeated all the former experiments with differently coloured substances; but shall here only detail a few, as sufficient to show the general results:—

1. I took ten grains of *white* and the same quantity of *black* wool, and having suspended them in the manner stated, vaporized the camphor. When the apparatus cooled, I found, on weighing the wool, that the *white* had gained $1\frac{5}{10}$ grains in weight, and the *black* $1\frac{8}{10}$ grains.

2. In a similar experiment, but using three colours of wool, *white, red,* and *black,* I found the *white* wool had gained $\frac{3}{10}$ of a grain; the *red* $\frac{8}{10}$; and the *black* $1\frac{4}{10}$ grains.

3. In another, where the heat was applied for about ten seconds, the white had gained no appreciable weight, and but little smell; the red had gained $\frac{1}{20}$ of a grain; while the black had acquired $\frac{2}{20}$ of a grain.

4. In an experiment with *black, red, green,* and *white,* the results were:—

Black	gained in weight	$\frac{3}{10}$ of a grain.
Green	. . . . .	$\frac{2\cdot5}{10}$
Red	. . . . .	$\frac{2}{10}$
White	. . . . .	$\frac{1}{10}$

5. In an experiment with *black, blue, green, red,* and *white,* 10 grains of each, the result stood thus:—

Black	gained in weight	$1\frac{2}{10}$ of a grain.
Dark Blue	. . . .	$1\frac{2}{10}$
Scarlet Red	. . . .	1
Dark Green	. . . .	1
White	. . . . .	$0\frac{7}{10}$

In repeating this experiment the *dark green* was $\frac{7}{10}$, while the *red* was only $\frac{6}{10}$; the others in the order as before.

I now varied the experiment by employing square pieces of card of equal size, coloured with different preparations of lead. This was done with the view of ascertaining whether smooth surfaces of equal density, and coloured as nearly as possible with a pigment of the same nature, would absorb odorous particles with the same facility as loose portions of wool. The colours were mixed up with a solution of gum-arabic, and laid on the cards as equally as possible with a camelhair pencil.

6. Pieces of card of equal size being coloured as mentioned with various preparations of lead, viz. *red*, *brown*, *yellow*, and *white*, and previously weighed, were exposed to the vapour of camphor in the vessel before described. After exposure for some time, and when cool, it appeared on weighing, that the

Red had gained	1 grain in weight.
Brown . .	$\frac{9}{10}$ of a grain.
Yellow . .	$\frac{5}{10}$
White . .	a trace.

The whole of the upper surfaces of the *red*, *brown*, and *yellow* cards were covered with a fine light downy deposit of camphor. The *white* had an extremely fine deposit on its surface, but inappreciable by a very fine balance.

7. Another experiment with cards coloured *black*, *red*, *brown*, *yellow*, and *white*, exposed to the vapour of camphor, gave the following results:

Black had gained	1 grain.
Red . .	$\frac{9}{10}$ of a grain.
Brown . .	$\frac{7}{10}$
Yellow . .	$\frac{5}{10}$
White . .	$\frac{4}{10}$

8. In a similar experiment with cards coloured *black*, *dark-blue*, *dark-brown*, *orange-red*, and *white*, the attractive powers were as follow:

Black gained . .	$\frac{9}{10}$ of a grain.
Dark Blue . .	$\frac{8}{10}$
Dark Brown . .	$\frac{4}{10}$
Orange Red . .	$\frac{3}{10}$
White . . .	$\frac{1}{10}$

In all these experiments, it was invariably found that the *black* attracted most, the *blue* next; then followed the *green* and *red*; after these the *yellow* and *white*. The heat was never continued so long as to warm the apparatus, else the whole camphor would have been driven off. Neither was such a quantity of camphor used as would have given a thick coating to the substances employed, as then the attraction of the coloured surfaces might have been diminished.

1. The next set of experiments were intended to ascertain the comparative attraction of animal and vegetable substances. The first of these was upon equal weights of black wool and black silk, ten grains of each exposed to the vapour of camphor in the manner already stated. The *black wool* gained $1\frac{5}{10}$ grains, the *black silk* $1\frac{7}{10}$ grains. From this experiment, it would appear, that of these two animal substances, *silk* possesses the greater attraction for odours.

2. In equal weights of *white wool* and *white cotton*, the *cotton* gained $\frac{3}{10}$ of a grain; the *wool* $\frac{4}{10}$.

3. In another experiment with *white silk*, *white wool*, and *white cotton*, ten grains of each, the result was,

Silk had gained	$3\frac{5}{10}$ grains in weight.
Wool	$2\frac{4}{10}$
Cotton	$2\frac{2}{10}$

4. In a similar experiment, with the usual weights of the same articles,

Silk had gained	$1\frac{4}{10}$ grains in weight.
Wool	$\frac{5}{10}$ of a grain.
Cotton	$\frac{4}{10}$

5. Another experiment, in which *black silk*, *black wool*, and *black cotton*, were exposed in equal quantities of the usual weight to the vapour of camphor, as before described, gave this result.

Black silk gained	$\frac{2}{10}$ of a grain.
Black wool . .	$\frac{1}{10}$
Black cotton . .	$\frac{1}{20}$.

6. In an experiment with *white silk*, *white wool*, *white cotton*, and *white card*, each weighing ten grains, and exhibited as before, the following were the results:

White silk had gained	$1\frac{9}{10}$ grains in weight.
White wool . . .	$1\frac{1}{10}$
White cotton . . .	1
White card . . .	$\frac{4}{10}$ of a grain

These last experiments tend to shew, that different substances attract odours in different proportions, and this independent of the texture or fineness of the substance employed. Wool, though generally coarser in the fibre than cotton, has yet a greater attraction for odours; and silk more than wool. The general conclusion would appear to be, that animal substances have a greater attraction for odours than vegetable matters; and that all these have their power much increased by their greater darkness or intensity of colour. These experiments seem also to establish, that the absorption of odours by coloured substances is regulated by the same law which governs the absorption of light and heat. The analogy goes still farther, for in other experiments which I made with a view to ascertain this point, I invariably found, that the power of colour in radiating or giving out odours, was in strict relation to the radiation of heat in the same circumstances. My first experiments in this branch were with differently coloured wools, inclosed for a certain time in a drawer along with assafœtida and camphor, and afterwards exposed for a specific period to the action of the air. Though one can easily judge by the sense of smell alone the different intensities which these articles have acquired immediately on being taken out of the drawer, yet, after exposure for some time to the air, the difference of intensity is much more difficult to be perceived. In general, it seemed to me that the whole of the substances lost their sensible odour in nearly the same space of time, though the odorous particles given out by the black were, of course, much greater in quantity than in the others. To demonstrate this, I took pieces of card, coloured as before, *black*, *dark blue*, *brown*, *orange-red*, and *white*, and, after having exposed them to the vapour of camphor, in the usual manner, they were taken out of the vessel, weighed, and exposed in the apartment for twenty-four hours. Upon carefully weighing the cards at the end of this period, it was found that the *black* had lost one grain; the *blue* nearly as much; the *brown* $\frac{9}{10}$ of a grain; the *red* $\frac{8}{10}$; and the *white* $\frac{5}{10}$. In about six hours afterwards, the *black* and *blue* had completely lost their camphor; the *brown* and *red* had the merest trace, inappreciable by a delicate balance; while the *white* still retained about $\frac{1}{50}$ of a grain.

In another experiment with cards coloured *dark blue*, *dark* *brown*, *orange-red*, *yellow*, and *white*, they had gained in weight, after exposure to the vapour of camphor,

Dark blue . .	$\frac{9}{10}$ of a grain.
Dark brown . .	$\frac{8}{10}$
Orange-red . .	$\frac{6}{10}$
Yellow . . .	$\frac{5}{10}$
White . . .	$\frac{4}{10}$

After lying in the apartment for twenty-four hours, the cards were again carefully weighed, when the camphor remaining was found to be, in the

Dark blue . .	$\frac{1}{30}$ of a grain.
Dark brown . .	$\frac{1}{10}$
Orange-red . .	$\frac{2}{10}$
Yellow . . .	$\frac{1}{10}$
White . . .	$\frac{3}{10}$

Hence in the same space of time the loss in each was blue $\frac{22}{30}$; brown $\frac{21}{30}$; red $\frac{12}{30}$; yellow $\frac{12}{30}$; white $\frac{3}{30}$.

The influence of coloured surfaces upon the absorption and emission of odours, having, I trust, been satisfactorily shewn, it only remains to state shortly some of the practical conclusions which may be drawn from the experiments detailed.

If it be thus certain that odorous emanations have not only a particular affinity for different substances, but that the colour of these substances materially affects their absorbing or radiating quality, the knowledge of these facts may afford useful hints for the preservation of the general health during the prevalence of contagious diseases. From their minute division and vast range of action, latent poisonous exhalations or effluvia, inappreciable by the balance, may no doubt exist to a dangerous extent, without being evident to the sense of smell. But in most cases it will be found, that, when contagious diseases prevail to such extent, the emanations from the sick will, if attended to, give the surest indications of the contamination of the surrounding air. Besides, even if we allow that infectious emanations have no necessary connexion with odours, the preceding experiments will afford the strongest possible presumption, that the emanations of an infectious nature, in common with odours, vapours, and emanations generally, are emitted on the one hand, and on the other received, according to the same general laws.

Experience has sufficiently proved, that emanations once generated in, or communicated to, the human body, may be conveyed from one individual to another, and even, through the medium of clothing or merchandize, from one place to another. This has been particularly observed in plague; and hence in countries where this disease is liable to occur or be imported, the institution of quarantine establishments, to prevent personal intercourse or the dispersion of goods, till a certain number of days have elapsed, during which the disease, if existing, should appear;—articles of merchandize and clothing being at the same time purified by exposure to the air, or fumigated. Though this transport of disease has been more particularly observed in plague, yet instances of the same nature have occurred in other diseases, more particularly small-pox, and more recently it has by many been supposed in cholera.

It is unnecessary to detail the means of purifying infected goods, or fumigating the apartments of those who have been known or suspected to labour under diseases supposed to be communicated by contagious effluvia. It is sufficient to state, that exposure to a high temperature, fumigation with chlorine and sulphur, and free exposure to the air, are found amply sufficient for the first; and apartments are more generally recommended to be purified with chlorine, and washed with caustic lime. As to fumigations with chlorine, it cannot be denied that this will destroy the effluvium floating in the air exposed to its action; but unless this fumigation be frequently repeated, it can have but little effect, as the walls and furniture will be constantly contaminating the air, by giving out the deleterious particles which they had previously absorbed. Lime washing has generally been supposed to act in the same manner as fumigations, viz. by destroying the contagious emanations; but from the experiments of Guyton Morveau, it would seem that caustic lime, and indeed lime in any state, has no such effect. It merely absorbs the gases which disguise the odour, but neither changes its deleterious properties nor alters its real smell. He, therefore, disregards lime-washing, except as a general mode of cleaning walls, and attributes no other beneficial effect to it than as contributing to cleanliness.

The result of my investigations has led me to form a very different opinion. It is to white-washing that I should attribute much of the good effects that have been found to follow the purifying means generally employed. In such cases I should trust more to white-washing the walls, personal cleanliness, and free ventilation, for destroying or diminishing the effects of supposed pestilential or hurtful effluvia, than any other measures. Acid and other fumigations, except chlorine, only disguise, but do not destroy, the property of animal effluvia to produce disease.

In the late epidemic cholera here, it is well known that this disease broke out in the village of the Water-of-Leith, situate a little to the north-west of Edinburgh, and lying on both sides of the stream of that name. Many of the inhabitants were seized with the disease, and fell victims to its severity. If a damp and low situation, with accumulated filth of all kinds, render disease more fatal, this was certainly a place likely to suffer severely, and at first it did so. But the Board of Health, with that promptitude for which they were distinguished, quickly got all the filth, so far as practicable, removed, the houses fumigated, and the walls white-washed outside and inside. By these means the disease seemed at once to be arrested, its virulence was much abated, and it gradually declined. The fumigations in this case could only act upon the deleterious emanations in the air at the time; but unless constantly renewed, could not affect the fresh emanations generated from those labouring under the disease. The necessary ventilation must also have speedily carried off the chlorine. In white-washing, on the other hand, although it had no specific influence upon the contagious effluvium, yet, by constantly presenting a reflecting surface, prevented the absorption of the emanations by the walls, and thus tended with moderate ventilation to keep the air of the apartment pure. Dirty-coloured walls, on the contrary, would readily, as has been demonstrated, absorb the noxious odours, and as soon as the effect of the fumigation was over, gradually give them out again.

The good effects of white-washing appeared strikingly in another instance at this particular time; for I venture to assert, that if human means had any influence on this disease, Edinburgh owes much of the mildness of its attack to the white-washing of its steep and narrow lanes and closes, the walls of the common-stairs, and most of the hovels inhabited by the lowest classes of the community,—not to the partial fumigations and sprinklings with chloride of lime, which the first breath of wind carried off. The white-washing of the walls prevented them from absorbing the deleterious emanations, and the currents of air were thus enabled to sweep them away, before they had accumulated to such a degree as to become an active source of disease.

Next, therefore, to keeping the walls of hospitals, prisons, or apartments occupied by a number of individuals, of a *white* colour, I should suggest that the bedsteads, tables, seats, &c. should be painted white, and that the dresses of the nurses and hospital attendants should be of a light colour. A regulation of this kind would possess the double advantage of enabling cleanliness to be enforced, at the same time that it presented the least absorbent surface to the emanations of disease.

On the same principle it would appear that physicians and others by dressing in *black*, have unluckily chosen the colour of all others most absorbent of odorous and other exhalations, and of course the most dangerous to themselves and patients. Facts have been mentioned which make it next to certain, that contagious diseases may be communicated to a third person through the medium of one who has been exposed to contagion, but himself not affected *; and in fact the circumstance of infectious effluvia being capable of being carried by medical men from one patient to another, I should conceive one of the means by which such diseases are propagated in the ill-ventilated and dirty habitations of the poor exposed to their influence.

Even in my own very limited experience, I think I have observed some melancholy instances of the effect of *black dress* in absorbing the hurtful emanations of fever patients in a public hospital; and many facts are incidentally noticed by medical writers and referred to other causes, which I should not hesitate to ascribe chiefly to exposure of this nature. Not to mention individual cases, in the Sessions held at Oxford in July 1577, " there arose amidst the people such a damp that almost all

* See Treatise on the Epidemic Puerperal Fever of Aberdeen, by Alexander Gordon, M. D. Lond. 1795, p. 63-4.

were smothered." Lord Bacon attributes this effect to the smell of the jail, where the prisoners had been close and nastily kept; and mentions it having occurred twice or thrice in his time, "when both the judges that sat upon jail, and numbers of those who attended the business, or were present, sickened and died." * A similar occurrence related by Sir John Pringle, happened at the Old Bailey sessions in 1750, when four of the judges were attacked and died, together with two others of the counsel, one of the under sheriffs, several of the jury and others, to the amount of about forty in the whole. My explanation of the peculiar fatality of these emanations to the judges, counsel, and jurors, was the peculiar attraction of their official black for the putrid effluvium, as Sir John calls it; and the escape of two of the judges who sat on one side of the Lord Mayor, to the current of air that was in the room not sending the baneful odours in their direction.

On finding the Dew-Point, &c. from the Cold induced by the Evaporation of Water. By H. MEIKLE, Esq. Communicated by the Author.

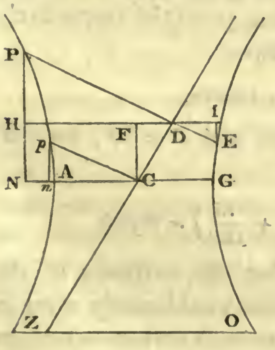
I WAS happy to see this interesting subject taken up, and so much forwarded, in this Journal for September last. The formula and table there given for obtaining the dew-point from the temperature of the air and depression of the moist thermometer, agree so well with observation, so far as the comparison has been made, that they appear to be by far the most accurate that have yet been given for the purpose; but it is much to be wished that observations were furnished for verifying them, for temperatures above 92° and below 56°, the limits of the range compared. A perusal of that article, and of the observations or experiments adduced in it, led me to revise and compare with them a scheme, which had occurred to me about seven years ago, for obtaining the dew-point from the same data, by merely laying a ruler across certain lines or curves delineated on a plane. From some rude sketches which I had then made, it appeared that this object could be effected by means of straight lines together with curves, which very much resembled hyperbolas; but I had not then ascertained that the common or conic

* Pringle's Observations on Diseases of the Army, p. 296.

hyperbolas would answer so well for the purpose as I shall now shew they do.

In this scheme, of which I shall at present only give an outline on a small scale, the Fahrenheit temperature is estimated nearly in the same way as latitude is reckoned from the equator; that is, by the perpendicular distance from a straight line ZO parallel to AG, the transverse axis of the two hyperbolas PAZ, EGO. Thus the distance of a point P, in the hyperbola PAZ, from ZO, which is at the zero of the scale, denotes the temperature of the air; while the distance of a point D, in the asymptote CD, from the same line ZO, represents the temperature of the moist thermometer; and if a straight line, representing the ruler above mentioned, be applied to P and D, so as to cut the hyperbola EGO in any point E, the distance of E from ZO denotes the temperature of deposition, or the dew-point.

Through D draw DH parallel to AG, and upon DH let fall the perpendiculars PH, CF, EI; produce CA and PH to meet in N. Through the centre C draw C*p* parallel to DP, and meeting the hyperbola AP in *p*; on CN let fall the perpendicular *pn*. Then it is a known property of the hyperbola, that the semidiameter C*p* is a mean proportional between the segments PD and DE; and, therefore, because the three triangles PHD, DIE, *pn*C, are obviously similar, *pn* is a mean proportional between PH and EI. By means of these properties we may find an expression for EI in terms of PH, and other known or constant quantities. It is evident that PH represents the depression of the wet thermometer below the temperature of the air, and that EI denotes the farther depression of the dew-point below that again.



Put $a = AC$ the semitransverse axis, and b for the semiconjugate; then, from the known properties of the curve, $PN^2 = \frac{b^2}{a^2}(CN^2 - a^2)$, and $CN^2 = \frac{a^2}{b^2} PN^2 + a^2$; also $DF = \frac{a}{b} CF$ and $DH = DF + CN = \frac{a}{b} CF + \sqrt{\frac{a^2}{b^2} PN^2 + a^2}$. Again, $pn^2 = \frac{b^2}{a^2}(Cn^2 - a^2)$, and $Cn^2 = \frac{a^2}{b^2} \times pn^2 + a^2$; and because the triangles PHD, pnC, are similar, we have

$$\frac{DH^2}{PH^2} = \frac{Cn^2}{pn^2} \text{ or } \frac{\left[\frac{a}{b}\, CF + \sqrt{\frac{a^2}{b^2}\, PN^2 + a^2}\right]^2}{PH^2} = \frac{\frac{a^2}{b^2} \times pn^2 + a^2}{pn^2},$$

and therefore $pn^2 = \dfrac{b^2\, PH^2}{[CF + \sqrt{PN^2 + b^2}]^2 - PH^2}$

Hence, because pn, as already mentioned, is a mean proportional between EI and PH, we have

$$EI = \frac{pn^2}{PH} = \frac{b^2\, PH}{[CF + \sqrt{PN^2 + b^2}]^2 - PH^2}.$$

Since a has entirely disappeared, this result is independent of the value of the transverse axis, which may therefore be taken of any magnitude most convenient; but, in order to apply this general expression, we must first, supposing EI to be known, find, by means of the same equation, the value of b^2, which, after going through the necessary steps and reductions, will be

$$\frac{4PH \times EI \times CF}{PH - EI} \times \left(1 + \frac{CF}{PH - EI}\right).$$

From examining the large Table, page 284 of this Journal for September last, it appears that when t', the temperature of the wet thermometer, is 54° F, it is always nearly the arithmetical mean between t the temperature of the air, and t'' the dew-point. Now, when the straight line or ruler PE, in the graphic scheme, passes through the centre C of the hyperbolas, we have always $PH = EI$, or t' equal the arithmetical mean between t and t''. Hence, the transverse axis cuts this scheme in about the 54° of Fahrenheit; and therefore $CF = t' - 54°$; $PN = t - 54°$. Also, when $t = 56°$, and $t' = 45°$, we find $t'' = 26°$ in the Table; and consequently $CF = 9$; $EI = 19$; $PH = 11$, which values being substituted for these quantities respectively in the general expression for b^2, it becomes

$$\frac{4 \times 11 \times 19 \times 9}{8} \times \left(1 + \frac{9}{8}\right) = 1998{\cdot}56,$$

and $b = 44°.7$. Again, when $t = 92°$, and $t' = 70°$, we find $t'' = 61°{\cdot}2$ in the Table;

$$\text{Hence } b^2 = \frac{4 \times 22 \times 8.8 \times 16}{13.2} \times \left(1 + \frac{16}{13.2}\right) = 2076.5,$$

and $b = 45°.57$. It thus appears, that the extremes of the range, as yet compared with observation, would nearly agree in making $b = 45°$; so that the conjugate axis corresponds to an interval of about 90° on Fahrenheit's scale. Putting, therefore, $PH = t - t' = D$, we get

$$EI = t' - t'' = \frac{2025D.}{[t' - 54 + \sqrt{(t-54)^2 + 2025}]^2 - D^2}.$$

So far as the Table has been compared with observation, this expression keeps very close to it; and even in its extreme case, where $t = 110°$, and $D = 34°$, this formula makes the dew-point 66°.9, which is just 1° higher than in the Table. At low temperatures, the difference is greater, though only on greater values of EI; but, in such cases, it would require experiments to decide how far any of them may be right *. But, independently of its use for comparing the two methods, this formula, since it does not require the aid of any Tables of the force, &c. of steam, will, (where it is wished to use a formula), be found a convenient one for directly computing the dew-point; and will be still more easy, if, in place of the difference of the squares in the denominator, we use the equivalent product of the sum and difference of the quantities without squaring. A similar formula, as also a similar diagram, would give the temperature at which vapour of the same density as that in the air would be at its maximum density, or in a state of saturation; but a method of obtaining both that and the dew-point from one diagram or plate will be noticed presently.

Several of the lines in the diagram being only for illustrating the foregoing investigation, would fall to be omitted altogether on a large scale. But to render the project more complete, the space between the two hyperbolas should be occupied by lines parallel to AG; and at the distance of a degree one from another, or each marking a degree of temperature. Close to the right side of the hyperbola EO, a set of divisions might be put, to

* Although it were turning out, from more extensive observations, that the hyperbolas would not exactly answer for all temperatures, yet, from their answering so well, as we have just seen they do, throughout the range compared with observation, there seems little reason to fear, that no curves whatever would suit, or even that the proper curves would differ much from hyperbolas. The effect could no doubt be considerably modified, by using two hyperbolas not exactly equal; as also, by shifting a little the position of the straight line CD, and of the parallel marked 54°, &c., and perhaps by slightly curving these lines. But it will be soon enough to have recourse to any such expedients when they may be seen to be necessary.

note the force of vapour at the respective temperatures, and almost close on the right of that, again, might be placed another curve pretty similar to the hyperbola, and nearly equidistant from it, for the purpose of the ruler marking on it the temperature to which, if the air were cooled without any diminution of volume, it would be just in a state of saturation with moisture. Or, if close on the right side of this new curve, a set of divisions were put, to note at once the density of vapour at the respective temperatures, the labour of finding the actual density, or weight of vapour in the air, would be still farther abridged, or almost avoided. On the left of the other hyperbola PZ, and as it were parallel to it, several curves might be placed, and the more distant, respectively from it, as the barometric pressure was smaller. These are meant to be used in place of PZ, when the pressure falls short of 30 inches. When it exceeds 30, there is scarcely any need for correction for pressure; but that, too, could easily be provided for by a similar curve on the other side of PZ.

Were this project realized, that is, if, by merely laying a ruler across a plate, in the manner above mentioned, the force, density, &c. of the vapour in the air, could be indicated at once, and without computation, it would obviously be the most convenient method yet employed for the purpose, and would supersede the use of troublesome and expensive instruments for directly observing the dew-point—often with a considerable degree of indistinctness and uncertainty.

The ingenious author of the article above quoted, holds some opinions which differ materially from my own. I cannot, for instance, conceive how the wet bulb could continue to furnish any heat for the formation of vapour, after the process has fairly commenced. When the instrument is first exposed to the drying influence of the air, evaporation takes place, and lowers the temperature of the bulb, by abstracting heat from it for the formation of vapour; but a limit is soon set to the fall of temperature by the warmer air, which, in successively touching the wet and colder surface, bears a share of the loss of heat, or, in other words, imparts heat by contact to the colder bulb, and also, no doubt, by the warmer surrounding bodies throwing in a very little heat upon it by radiation. The heat thus imparted and thrown in, is, next to all that I can think of, as being spent in the formation of vapour. It might indeed be supposed, that the stem of the thermometer should convey a little heat to the bulb; but this being of glass, is a very bad conductor, and any heat which it supplies must be quite inappreciable, as it makes no sensible difference whether we apply a wet covering to the bulb alone, or continue it along a part of the stem. To me, at least, it is as difficult to comprehend how the same piece of coal could continue to burn for an indefinite period, as that the wet bulb could continue to furnish any heat which it does not receive from other bodies. But Dr Anderson certainly carries this idea to perfection, in supposing the wet bulb alone to furnish, that is, to create all the heat expended on the formation of vapour.

Without pretending to give a complete theory of the rate at which the depression varies in air of different densities, it does not seem so very difficult as the other gentleman supposes, to explain why the depression should be greater as the air is more attenuated. When air is dilated, its capacity for heat, or the absolute heat which a given mass of it can contain at a particular temperature, is no doubt increased; but I am rather at a loss to see what concern this has with the question. It is surely not the total or absolute heat of a given mass, but rather the specific heat of a given volume, or the heat which that volume of air can give out in being cooled through a small range that is to be imparted to the bulb, and spent in saturating the same, or an equal volume, with vapour*. Now, although the rarer the air is, the fewer of its particles will touch the cold surface,

* Nothing is known, or likely ever to be known, of the total or absolute quantity of heat contained in any body, or of the ratio which the absolute heat of one body bears to that of another; yet many chemical writers take it for granted that both are perfectly ascertained, as also that the specific heat is exactly proportional to the absolute heat, and that the former is the same at every temperature. Of the fallacy of such assumptions we have abundant evidence in the many examples which Mr Dalton has collected of the ridiculously different positions which computations founded on them give to the absolute zero. In one case, however, a notable exception is made with respect to the ratio between the absolute and specific heats: the absolute heat of steam is obviously greater than that of water, and yet the specific heat of the former, notwithstanding its vastly greater volume, is only reckoned about a fourth of that of the latter; which affords a strong presumption that enlargement of volume does not increase the specific heat, however much it may enlarge the capacity for the absolute.

yet a cubic inch of rare air is, *cæt. par.* just as capable of containing vapour as an inch of it ever so dense. Hence, if neither more nor less air be cooled down by its touching the wet ball than is to be saturated with vapour, it should follow, that when one cubic inch of air has only half the density of another, and consequently only half the specific heat *, it must be cooled down twice as far to give out heat sufficient to form as much vapour; and therefore the depression, if affected by no other circumstance, should vary inversely as the density or barometric pressure.

Such I presume to be what chiefly regulates the depression in air of different densities, supposing the temperature of the moist bulb constant; but, to render the explanation a little more complete, some reason should be given, why, in comparing cases in which the temperature of the air is the same, the depression should increase so much more slowly than the reciprocal of the pressure, as we shall afterwards see from observation it does. It is obvious that the greater the depression or excess of the temperature of surrounding bodies over that of the wet bulb, the greater propensity will these bodies have to radiate or throw in heat upon it; and, consequently, the greater will be the supply of heat from radiation. At first sight, this might be supposed to afford the reason of which we are in quest; but evaporation, as is well known, goes on more quickly in rarer air, which implies a corresponding acceleration in the circulation of that air over the evaporating surface; and this acceleration again, which no doubt is owing to the greater difference of temperature, and increased force of evaporation, implies a correspondingly more rapid influx of heat from the greater volume of air which, in a given time, passes close over the cold moist bulb; for, as was already noticed, the greater difference of temperature makes up

* In this Journal for September 1826, page 339, in the Annals of Philosophy for November 1826, p. 368, and in Brande's Journal of Science for March 1829, page 65, it is shewn necessarily to follow from admitted data, that the specific heat of a given mass of air, under a constant pressure, is the same whether that pressure be great or small. The proof of this is the more satisfactory, as it neither depends upon a particular scale of temperature, nor requires the true scale to be previously known. Hence, at the same temperature, the specific heat of a cubic inch of air under a constant pressure, is as its density. The same thing has been more recently advanced, and as quite new too, by MM. De la Rive and Marcet.

for the smaller specific heat of rarer air. Hence radiation, being unaided by mere circulation, probably furnishes almost as small a proportion of heat to the wet bulb, in rare air as in dense *, compared with what the air imparts to it by contact; so that there is little reason to think that radiation makes any material alteration on the rate of the depression †.

Since, then, something farther seems necessary to explain why, in comparing cases in which the temperature of the air is the same, the depression should increase in a slower ratio than the reciprocal of the pressure. It occurs to me, that it is principally owing to the following, which, as seems conform to observation, would cease to have that effect on the comparison of cases where the temperature, not of the air, but of the moist bulb, is the same, namely, the smaller capacity which a given volume of air is known to have for moisture, at a lower temperature: so that less heat must be expended in forming vapour sufficient to saturate that volume at a lower temperature, and therefore a less cooling of the air, or a depression less than in the ratio of the reciprocal of the pressure will suffice for furnishing that heat; especially if, as is extremely probable, the specific heat of air is greater at a lower temperature than a higher. For if a given mass of air, under a constant pressure, expand in geometrical progression, for equal increments of heat, the increments or decrements of heat for the degrees of Fahrenheit, and which are usually called the specific heats, will be inversely as their respective distances from — 448°; but when the volume and pressure are given with a variable mass, the increment, decrement, or specific heat for one degree of Fahrenheit will be inversely as the square of the distance of that degree from — 448°, viz. in the ratio compounded of the value of a degree of Fahrenheit, and the density of the air under a given pressure, each of which varies inversely as the temperature reckoned from — 448°.

Our views likewise differ a little respecting the correction for

* From the experiments of Dulong and Petit, it appears that the quantity of heat propagated by radiation, is, *cæteris paribus*, independent of the presence, density, or movements of any gaseous medium: such being, of course, free from opaque or gross particles floating in it.

† That any heat supplied by radiation to the moist bulb, must, in ordinary cases, be very inconsiderable, appears from the depression being so little different in the wind and in the calm.

B, the barometric pressure. In this Journal for December 1826, I had proposed to multiply D, the depression of the wet thermometer, by $\frac{B+27}{57}$; from observing that, when the temperature of the air was constant, this reduced the different depressions to one value, or made them likewise as if constant; and therefore it was obvious that, by making $t' = t - \frac{B+27}{57}D$, the several cases became the same as if the pressure too had been constant. There can be little doubt that this was nearly correct, for the particular series of experiments from which it was deduced, though perhaps for it alone, rather than cases in which the air is more humid. But when we alter the value of t, the whole concern is unhinged, not one of the quantities continues constant throughout that series of experiments, and we have no evidence that the multiplier $\frac{B+27}{57}$ is at all near the truth. That gentleman's view of the matter, I readily grant, would have been quite correct with respect to that series, had it been the temperature of the moist bulb, and not of the air, that was constant; but then a much greater correction would have been required, as I find the following experiments, where t' is constant, would give $\frac{B+6}{36}$ for the multiplier.

B	t	t'	D.
30	46°.1	35°. 1	11°
10	59 .9	35 . 1	24.8

It is farther to be observed, that the actual temperature of the air is much higher in the second of these cases than in the first; and it appears from Professor Daniell's experiments, that when two equal vessels, the one containing water and the other sulphuric acid, were put under a receiver on the plate of an air-pump, at the temperature of 52°.3, the dew-point was 35°.7, but when the temperature was 60°.7, the dew-point was no lower than 48°.4 *. Hence there is reason to suspect, that the air at

* *Meteorological Essays*, 2d edit. p. 498. There were seven experiments of each sort, of which I have taken the mean; and, in seeking for the mode in which the dew-point varies with the temperature in the given circumstances, I find that the *evaporating force* (as Mr Dalton would call it, though perhaps he would only name it so in air of the ordinary density), is the same

the temperature of 59°.9, contained more moisture than at 46°.1; and that, had it contained as little, the depression would have exceeded 24°.8. This renders it probable, that the proper multiplier should nearly equal $\frac{B}{30}$, or follow the ratio of the density or pressure, agreeably to what we deduced above from theoretical considerations. In adopting this multiplier, I would of course follow the author of the article in this Journal for September last, in applying the correction to the temperature of the air, and not to that of the moist bulb. But the result so obtained would still differ widely from that of Dr Anderson, who, though he uses the same multiplier, yet in effect applies the correction to the temperature of the moist bulb, and not to that of the air, which generally renders the correction too great, and often considerably so.

Professor Daniel has also given a series of experiments on the rate at which the depression of a wet thermometer varies in air of different densities, (*Essays*, 2d edit. p. 499). But, in these, the wet bulb was evidently not so fully exposed to the drying influence of the sulphuric acid as in my experiments; because the Professor not only kept a vessel with water under the receiver, but placed it directly between the wet bulb and the acid. His depressions are therefore much smaller than mine; and they likewise increase at a slower rate, though perhaps both might have observed more nearly the same rate had the temperature of the moist bulb been constant *.

On another point still we differ. In this Journal for September,

in both cases; that is, the excess of the maximum force of vapour for 60°.7, over that for 48°.4, equals the excess of the maximum force for 52°.3, over that for 35°.7. This, however, can scarcely be the law of nature; for, if the temperature of the air were lower than 31°, the force of vapour at the dew-point would need to be negative, which looks rather paradoxical. The air was exhausted, through every different pressure, to ·15 inch, but the dew-point may be said to have remained the same, at least it did not follow the exhaustion.

* To increase the cold in a wet thermometer, Dr Lardner recommends exposing it to the sun; but whoever properly tries this, will find that the sun, as well as the fire, has quite the contrary effect. Nay, it is obviously just the same delusion, only on a greater scale, to expose, as the doctor directs, bottles of wine, wrapped in wet cloth, to the sun, as a source of greater cold.

the gentleman remarks, p. 286, " that t, t'' and B being given, the actual weight of moisture contained in a given volume of air, is *nearly* the same as is sufficient to *saturate* the same volume at the temperature $t'' - (.062\,(t - t'') + 1.12\,(30 - \mathrm{B}))$, and under a pressure of 30 inches."

This will be found a very convenient and pretty close approximation, when the actual pressure is 30 inches; but, in other cases, it may deviate widely from the truth, and yet nothing is more easy than to render it alike correct for every pressure. To accomplish this, we have only to cancel the superfluous term $1.12\,(30 - \mathrm{B})$, which is at best of no use: for, when the actual temperature t, and dew-point t'' are given, the weight of moisture in a cubic foot is in effect given, because it depends on no other condition, being precisely the same whatever be the atmospheric pressure B. The experiments of Deluc, Dalton, Gay-Lussac, Daniell, and many others, have completely settled this point, and placed it for ever beyond any doubt. In this our author seems to have followed Dr Anderson, in his habit of making the barometric pressure an ingredient in almost every formula and computation connected with the subject, and without appearing to be aware that such procedure was quite incompatible with the theory of Dalton, to which he professed to adhere. Thus in the *Edinburgh Encyclopædia*, Art. Hygrometry, p. 578, Dr Anderson, in giving a table of the force and density of steam, says it is adapted to a pressure of 30 inches, and that when the pressure is different, the numbers in the table (none of which denote atmospheres but grains and inches), must be altered in the same ratio as the pressure. Now, nothing can be more obvious than that steam *in vacuo* has no concern with the external pressure. But it is as well known that the maximum force or density of steam in air has nothing to do with the density of that air, being the same as *in vacuo*, and yet Dr Anderson almost every where assumes the force or density of steam in air to be proportional to the barometric pressure. I readily grant that whilst air undergoes a change of pressure, the force and density of the vapour in it are altered in that ratio, provided the temperature is not altered, and none of the vapour has been liquefied; but this is obviously a case essentially different from those to which I refer, where the pressure, of whatever intensity, is understood to be constant during the time of observation. The Doctor also employs a formula to reduce the force of vapour, at the dew-point, to the force which it has in the air, at the actual temperature, although these, being under the same pressure, and each of them forming the same proportion of it, are precisely equal, and require no reduction whatever.—(*Ibid.* p. 581; and in this Journal, vol. xiii. p. 226, first series). Neither Mr Dalton nor Mr Daniell ever think of using any such reduction.

It has become a standard doctrine in almost every scientific compilation, that a given weight of aqueous vapour or steam, in a state of saturation, contains the same quantity of heat, whatever be its volume or temperature. But a doctrine's being generally received, is not always a proof of its soundness; and so it happens in the present instance: for, that a given *weight* of saturated steam really contains more heat at a lower temperature, and consequently under a larger volume, than at a higher temperature, and of course under a smaller volume, appears from this:—If we open a cock in the cover of a steam-boiler, when the pressure within amounts to only one atmosphere, the vapour which issues preserves its transparency till it get to some distance from the orifice; but when the pressure within amounts to two or three atmospheres, the steam issues cloudy and opaque from the very orifice. This clearly shows that the same quantity of heat which had been amply sufficient to maintain the steam in the elastic, and consequently transparent form, within the boiler, is quite inadequate to do so under a larger volume and lower temperature. M. Clement found that, on condensing three equal weights of steam, having the unequal forces of one, two, and three atmospheres respectively, in three equal quantities of cold water, the rises of temperature were equal; and it is upon this complex and fallacious experiment that the doctrine I have questioned has been founded. It is, however, to be observed, that when the stream of steam is very small, as it must have been in his experiments, it has, while in the act of dilating from under a pressure of two or three, to that of one atmosphere, the opportunity of absorbing heat from the metal of the very slightly open stop-cock and pipe, then much hotter than 212°, the temperature to which the dilating is supposed to reduce the steam; for this propensity of the steam to absorb heat, or rather of the metal to part with it, must obviously be greater the higher the temperature of the boiler and its appendages, that is, the higher the original pressure of the steam. No wonder, then, that equal weights of steam, at first so different in density and temperature, should, when equally dilated, and received in equal quantities of cold water, produce equal rises of temperature; as is shown somewhat differently in the *Philosophical Magazine* for July 1826, p. 38. It would, however, be running still farther into the opposite extreme to suppose that equal *volumes* of saturated steam should, at different temperatures, contain equal quantities of heat. There is no reason to doubt that the more dense volume will always be found to contain more heat, though, from what we have just shown, this cannot increase in so high a ratio as the density does, because that would be precisely Clement's doctrine over again.

Observations on the Loamy Deposit called "Loess" of the Basin of the Rhine. By Charles Lyell, Esq. F. R. S. Foreign Secretary to the Geological Society, &c.* Communicated by the Author.

During the last summer, I had opportunities of examining the remarkable deposit called by the Germans "Loess," in several parts of the valley of the Rhine, between Cologne and Heidelberg, and also in some parts of the country of Baden, Darmstadt, Wurtemberg and Nassau. The observations made during this tour have caused me to modify some of the opinions which I formerly entertained and published respecting the probable origin and mode of deposition of this formation, and its relation to the newest volcanic products of the Lower Eifel. As much has been already written on this subject, I shall confine myself in this notice to what I saw during my late excursion, and shall give my observations nearly in the order in which I made them, pointing out afterwards the general conclusions to which they appear to me to lead.

* Read before the Geological Society of London, May 7th 1834.

It may be as well to state, that the Loess consists of a pulverulent loam of a yellowish grey colour, containing a certain quantity of carbonate of lime, according to Leonhard about a sixth part. When not associated with gravel it exhibits no signs of stratification. It contains almost everywhere imbedded terrestrial and aquatic shells of species still living in Europe, which have usually lost their colour, but are for the most part entire.

The Loess is found with its usual characters reposing here and there upon the gravel of the plains of the Rhine at Bonn, where I first examined it with attention, and patches of it are seen of much greater thickness on the flanks of the Siebengebirge, on the right bank, and at a corresponding height near the summit of the low hills which border the plain on the opposite bank. In all these localities terrestrial shells, chiefly Helix and Pupa, are by far the most abundant.

I employed a collector for a fortnight in obtaining shells from a deposit of Loess of considerable thickness, which is laid open on the right bank of the Rhine about a mile and a half below Bonn. The individual shells procured in an entire state amounted to 217 in number, not a seventh part of which were of aquatic species. The proportions were as follows:

Terrestrial—Helix 167, Pupa and Clausilia 18; 185 individuals; *Aquatic*—Lymnea 17, Paludina 10, Planorbis 5; 32 individuals;—217.

In order to compare these fossils with such shells as are now drifted down by the Rhine, I made a collection of the latter at low water from the mud and sand of the shore of the river for several miles above and below Bonn. Along the beach is a line of rubbish composed of small pieces of drift wood, leaves, weeds, sand, and other matter, cast up principally by the large waves raised by the steam-packets, as they cut through the water. Here the greater number of drift shells occur, and I collected 273 individuals which were in the following proportions.

Terrestrial—Helix 133, Pupa and Clausilia 12, Bulimus 2; 147 individuals; *Aquatic*—Paludina 48, Planorbis 34, Neritina 28, Lymnea and Succinea 5, Unio 6, Ancylus 3, Cyclas 2; 126 individuals;—273.

If I may be allowed to draw any general conclusion from this comparison, it would appear that, in the waters of the Rhine, as in the loess, the drift-shells belong chiefly to terrestrial species, and in both the great mass of the shells are referable to the same genera, the principal difference consisting in the absence from the loess of species of the genera neritina, ancylus, and unio. The only bivalve-shells I ever happened to meet with in the loess, were Cyclas fontinalis, *Drap* *.

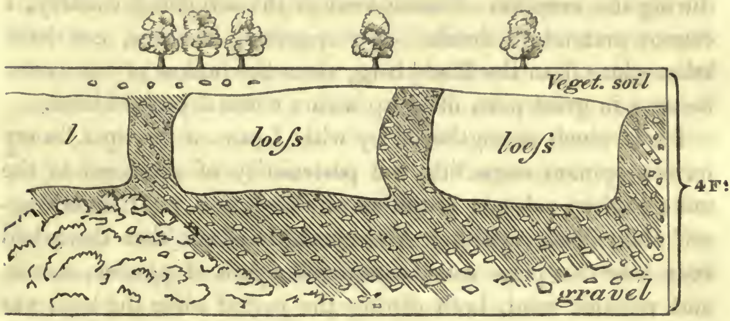
It may be well to observe here, that, in some places where the bank of the river is wholly or partly composed of loess, the fossil shells are often washed out, and may be found entire on the shore; and they might, in such cases, unless great caution were used, be confounded with the more modern shells drifted down by the Rhine. I was careful to guard against this source of error, by collecting chiefly from spots far from the loess, and by rejecting those which, by their want of colour, or by the circumstance of their being filled with loess, resembled the general characters of the fossils. The colour of the more modern specimens affords in general a safe criterion for distinguishing them from the fossils; and I feel sure that there was scarcely any intermixture in the sets above compared, or only two or three lymnea, at least, were doubtful.

The greater part of the shells drifted by the Rhine agree specifically with those which are buried in the loess; and if I had enlarged my collection, the correspondence would no doubt have been much more perfect, for the shells of the loess vary in different localities, and those now brought down by the Rhine probably vary equally at different seasons. As the drift shells of the Rhine agree with those of the loess, so the sediment of that river bears a very close resemblance to loess. This was first pointed out to me by Professor Noegerath, and it has lately been confirmed by Mr Horner's experiments on the quantity and nature of the solid matter brought down in the waters of the Rhine at

* I found several specimens of this with both valves entire, together with Valvata piscinalis, in the interior of an individual of the Lymnea ovata, in loess at Odenau, near Bruchsal. Hard calcareous concretions, in the same loess, contained shells of recent helix and clausilia, which were thus embedded in solid limestone. In the third volume of my Principles of Geology, Appendix, p. 58, I included Cyclas palustris, and C. lacustris, Drap. in a list of loess shells; but I afterwards ascertained that they had been brought to the spot in mud used to fertilize the soil. Probably they are to be found in loess.

Bonn *. The circumstance must, in part, be ascribed to the rapid degradation of loess, which is constantly going on throughout the valleys drained by the Rhine and its tributaries, but it also shews that the waste of other rocks in the same districts produces a sediment very similar in its nature to loess.

It is well known that the loess rests on the gravel of the plain of the Rhine. This superposition is well seen on the left bank of that river, about a mile above Bonn, where the loess fills up hollows in the gravel, and presents the appearance represented in the annexed sketch.



I conceive, that in this instance, small rills or torrents must first have furrowed the upper beds of gravel, leaving small trenches with vertical and occasionally overhanging walls, and then the waters holding loamy sediment in suspension must tranquilly have overflowed the spot and thrown down the loess until it first filled up the cavities, and then formed a continuous overlying mass.

The next subject to which my attention was called on my way from Bonn to Mayence, was the relations of the loess to some of the more modern volcanic formations of the Lower Eifel.

The volcanic hill called the Roderberg, situated on the left bank of the Rhine, about four miles above Bonn, and immediately opposite the celebrated Drachenfels, is well known. From the perfect form of the crater at its summit, and the appearance of its scoriæ, it has always been supposed to owe its origin to one of the most modern eruptions of this country. In the mid-

* See Proceedings of Geological Society 1834.

dle of the crater is a farm-house, where a well was sunk in July 1833: at that time I visited the spot in company with Mr Horner, and we found, to our great surprise, that the materials passed through were loess, covered by a small bed of cinders and cindery loam. The mass of pure loess was 65 feet in thickness. How much deeper it extended was not ascertained. We did not find any shells, but we were only able to examine a small quantity of loess which had been taken from the well. The usual calcareous concretions were in abundance. Whether the overlying cinders were alluvial or showered down from the air during the eruption of some vent in the adjoining country, I cannot pretend to decide. No eruption, however, can have taken place from the Roderberg, since the hollow of the crater became, in great part, filled up with a dense deposit of loess.

I was much strengthened by what I saw on this spot, in my former opinion respecting the posteriority of the loess to the more modern volcanic eruptions of the Eifel; yet I found myself obliged, on revisiting Andernach, to admit that there had been near that place some considerable falls of pumice, scoriæ, and volcanic sand, both during the period when the loess was forming, and since its formation. I am aware how easily pumice and other light volcanic matters may be drifted during heavy rains, and that the waters capable of depositing the loess might easily have washed away such transportable matter, had any of it been already strewed over the land before the loess was formed. In that case some alternations of volcanic cinders and loess might undoubtedly have been caused, even though all volcanic eruptions had ceased before the deposition of loess began. With due regard to these views, I compared with attention the appearances near Andernach with those which I had seen in the neighbourhood of active volcanoes, and concluded, contrary to my original idea, that some volcanoes must have been in activity while the formation of loess was still going on. In the hollow way called the Kirchweg, immediately above Andernach, the loess, having its usual characters, is still seen, with here and there an included fragment of pumice, or a small quartzy pebble. I collected several shells from it, and Mr Steininger gave me a list of species which he procured from the spot. The thickness of the loess in this and other adjoining places (as in the high road from Andernach to Mayen) is from 15 to 30 feet. In one place the loess is clearly exposed, resting upon volcanic matter, and at the junction it alternates with it. Small portions of pure loess are there entangled in black volcanic matter. In many other sections, the same loess is seen covered with beds of pumice, trassy pumiceous sand, and small dark volcanic cinders, forming upon the whole a mass from 10 to 15 feet in thickness, and very like that which covers Pompeii. There is, in this instance, no loess intermixed, nor any alternation at the point of junction, as might have been expected, if the volcanic matter had been washed over the loess by running water. At one place in the Kirchweg I observed, in a perpendicular section, an aggregate of small fragments of pumice resting on loess. The latter had wasted away, so that the incumbent mass of pumice was undermined and overhanging. It thus exhibited its under surface, projecting several feet from the face of the precipice, and it appeared flat and even like the ceiling of a room, shewing that there was an abrupt passage from loess to the pumice. It may also be seen, on comparing several sections, that before this shower of pumice fell, the loess already formed the slope of a hill descending towards the valley of the Rhine, just as it does now where no volcanic superstratum has been spread over it. I conceive, therefore, that the valley of the Rhine had assumed its actual shape, and that the loess had been considerably denuded, before the occurrence of the eruption which produced the great bed of pumice near Andernach.

I think it unnecessary to give more details respecting the sections near Andernach, because some of them have been faithfully described by Mr Steininger, Dr Hibbert *, and others; and these geologists have declared their conviction that some of the volcanic eruptions were contemporaneous with, and others subsequent to, the deposition of the loess.

On descending the hill to the village of Plaidt, on the road from Ochtendung, at the distance of about four miles from Andernach, I saw loam resembling loess covered with eight feet of volcanic matter, consisting of stratified beds of pumice, dark volcanic sand, lapilli, &c. I found no shells in this loam.

* See Hibbert's extinct Volcanoes of Neuwied, p. 221.

I conjectured that its height was about 600 feet above the level of the Rhine.

From Andernach I proceeded to Neuwied, and from thence across a plain covered with pumice to Sayn. Near the latter place, I saw the loess forming a terrace on the flanks of the hills composed of greywacke, and at a lower level the country is covered with volcanic ejections, which, according to M. von Oeynhausen, are clearly seen in some sections to overlie the loess, a fact which I had not time to verify.

From Sayn I proceeded to Mayence, where the country on the left bank of the Rhine is composed of tertiary limestone, with green and white marls. This formation is overtopped by loess, and both are cut off abruptly in the escarpment which the high land presents towards the Rhine at Mayence, Oppenheim, and other places.

The tertiary formation must here have undergone considerable denudation since the loess was superimposed. In an excursion through part of the Duchy of Darmstadt by Mayence, Oppenheim, Alzey, Flonheim, Eppelsheim and Worms, I found the loess spread almost everywhere over the country, and the inferior tertiary strata and secondary red sandstone only exposed to view in valleys, or where the country begins to rise towards the base of the Donnersberg.

At Heidelberg Professor Bronn, who has devoted much time to the study of the loess, told me that he is persuaded that the loess was not formed suddenly by a transient flood, but gradually by successive deposition. The absence of all appearance of stratification, which formerly led me and others to a different conclusion, is owing, he thinks, chiefly to the homogeneous nature of the loamy deposit. In some places he has seen calcareous concretions forming horizontal lines, marking the greater quantity of calcareous matter which was thrown down when some of the layers were accumulated. I had formerly imagined that the loess must have subsided suddenly from a flow of muddy water, like the Moya of the South American volcanoes, in the same manner as I believe the unstratified trass of the Rhine volcanoes to have been formed; but I am now convinced that Professor Bronn's view of the subject is more correct. Among other places, the signs of successive deposition are well seen in the deep gravel pits at the Manheim gate of Heidelberg, where the following section is exposed.

1. Vegetable soil with gravel.
2. Loess without any appearance of stratification, and with land and fresh-water shells.
3. Loess and gravel in alternating layers.
4. Sandy loess with shells.
5. Coarse gravel and loamy sand in horizontal strata, from one to two feet in thickness.

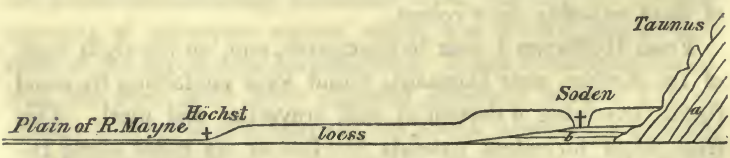
This section shews that, after the loess with shells (No. 4.) had been deposited, alternate strata of gravel and loess accumulated to the thickness of 12 feet, and then pure loess.

In travelling from Heidelberg to Heilbronn, by Wiesenbach and Sinsheim, a country composed of the bunter-sandstein, muschelkalk, and keuper of the Germans, I found the loess at various heights filled with both land and fresh water or amphibious shells,—the Succinea elongata generally equalling or surpassing in the number of individuals all the accompanying land-shells. I collected 158 shells from the loess between Heidelberg and Heilbronn, of which 80 belonged to Succinea minuta, 68 to the genus Helix, and 10 to the genus Pupa. Heilbronn is nearly 500 feet above the level of the sea, and M. Titot of Heilbronn informed me, that some of the loess on the hills near Heilbronn lies about 300 feet above the Neckar. If this is the case, the height of the loess must sometimes be more than 800 feet above the sea. Part of the district here alluded to, is within a few miles of that elevated table-land above the Bergstrasse between Wiesloch and Bruchsal, which I had visited the year before, where the loess attains the thickness of 200 feet and upwards, and contains a great variety of recent shells, many of them retaining their colour.

From Heilbronn I went to Stuttgardt, and, on the right bank of the Neckar, near Canstadt, found loess containing its usual fossils, overlying a deposit of tuff, travertin, and marl. This fresh water formation extends for five or six miles along the Neckar, by Canstadt and Münster, and in part of it Professor Jäger has found the remains of a tortoise, and some plants which appear to be of extinct species.

Whether the overlying loess is connected in age with the travertin, I was unable to determine; but I was told by naturalists at Stuttgardt, that the land-shells of the travertin were of recent species, and the same as those in the loess. From Stuttgardt I went to Göppingen and Boll in Wurtemberg, and between the last two places saw loess resting on lias, after which I met with no more of it in the course of a tour by Heidenheim, Steinheim, Wasser Alfingen, Nordlingen, Solenhofen, Pappenheim, Ellingen, Nuremberg, Pegnitz, Bayreuth, the cave-district round Muggendorf, and thence to Forcheim and Bamberg. Between Bamberg and Wurtzburg, in the valley of the Mayn, I again found the loess, at Dettelbach, of a somewhat redder tint than in Wurtemberg, but exhibiting the same want of stratification, and containing the same terrestrial and aquatic shells, especially Pupa and Succinea. The loess near Dettelbach is seen not only in the Valley of the Mayn, but on the hills of muschelkalk, five or six hundred feet above the valley, where its redder tint is probably, in part, derived from the degradation of the red bunter sandstein.

In the Spessart, and in the country immediately around Aschaffenburg, I observed no loess. The road which leads from Frankfort to the foot of the Taunus, passes first over the low flat plain of the Mayn, which is covered with yellow sand, for the most part very barren. (See section, No. 2.) At Höchst, on the Mayn, is a higher platform, composed of loess, and here the soil is extremely fertile. This platform afterwards rises to a still greater height between Höchst and Soden, which last town is situated in a valley cut through the loess, at the bottom of which the subjacent tertiary strata of the Mayence formation are laid open. On quitting Soden, I ascended the steep flanks of the Taunus mountains, and saw no loess. (See diagram, No. 2.)

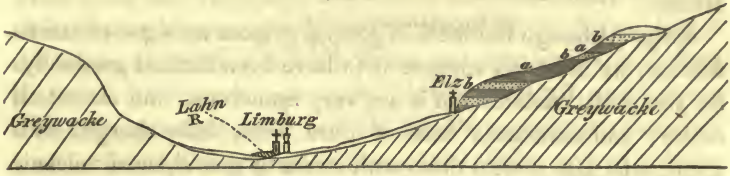


b Miocene tertiary. *a* Schist of the Taunus.

I then crossed the highest part of the Taunus, where the greywacke passes into crystalline schists, and from thence descended towards Esch and Walsdorf, where the more ordinary greywacke of the Rhine, a yellow argillaceous and sandy rock, is very generally concealed under a deep covering of loam, which appears to have resulted from the decomposition of this greywacke, and not to have been transported from any distance. This loam has precisely the ordinary colour of the loess, and contains a great quantity of quartz pebbles.

The same alluvium is very general in the Westerwald, especially on the surface of that high table-land around Altenkirchen, Uckerath, and between that place and Siegburg, a district lying immediately behind the Siebengebirge.

No. 3.



a a Loess with shells. *b b* Beds of quartz pebbles.

The principal river which intersects the table-land of Nassau is the Lahn, which I crossed at Limburg, about twenty miles above its junction with the Rhine. The road from Limburg to Freilingen passes first by Elz. On the north of this village is a hill, which forms one boundary of the valley of the Lahn, and here loess is seen with all its usual characters, with many land and fresh-water shells; and alternating, as at Heidelberg, with gravel. I observed, in particular, a horizontal layer of white quartz peebles, a foot and a half in thickness, resting on a mass of loess fifteen feet thick, and covered by another bed of loess five feet in thickness; the loess, in both situations, including in it entire shells. Following the road, I found the slope of the hill above to consist of horizontal beds of quartz pebbles, which have a base of loess. Hence it appears that the valley of the Lahn, which is excavated through highly inclined greywacke, has, at some period since its excavation, been partially filled up with beds of gravel, alternating with loess, a great part of which has since been removed by denudation. (See Section, No. 3.) It appears that, during the accumulation of the mass, fine loam was sometimes thrown down, containing unbroken shells, then gravel, and then again the shelly loam.

On a review of the observations above mentioned, it appears to me that the following conclusions may be legitimately deduced:—

1*st*, The loess is of the same mineral nature as the yellow calcareous sediment with which the waters of the Rhine are now commonly charged.

2*dly*, The fossil shells, contained in the loess, are all of recent species, consisting partly of land and partly of fresh-water shells.

3*dly*, The number of individuals belonging to land species usually predominates greatly over the aquatic, and this seems now to be the case with the modern shells drifted down by the Rhine.

4*thly*, Although the loess in general evinces no signs of stratification, we must yet suppose it to have been formed gradually, for the shells contained in it are very numerous, and almost all entire; and sometimes beds of pure loess, fully charged with shells, alternate several times with strata of gravel, or of volcanic matter.

5*thly*, Although, in general, the loess overlies every formation, including the gravel of the plains of the Rhine, and the volcanic rocks, which have the most modern aspect, yet in some cases, as at Andernach, the volcanic matter is so interstratified as to indicate that some eruptions occurred during the deposition of loess.

These inferences seem to me sufficiently clear; but if asked to account for the manner in which the loess, considering it as a fluviatile or lacustrine formation, was brought into the places which it now occupies, I must confess that the more I have studied the subject the more difficult I have found it to form a satisfactory theory.

If we begin to study the loess near Strasburg, we see large masses of it at the foot of the Vosges, on one side of the great plain of the Rhine, and at the base of the mountains of the Black Forest, on the other side. The intervening plains exhibit here and there remnants only of the same formation resting on gravel, for the loess has evidently suffered great denudation; valleys having been hollowed out in it, and small ridges of intervening hills formed, much like those seen on the surface of older horizontal tertiary formations. On following the loess from Strasburg to Mayence, we may trace it covering the rocks of every age along the borders of the great plain of the Rhine, and we naturally incline at first to suppose that a vast lake has existed, of which the barrier may have been somewhere near Bingen, formed by the union of the mountains of the Hundsruck and Taunus, before the deep and picturesque gorge of the Rhine, between Bingen and Bonn, had been opened, or occasioned by the choking up of that gorge by lava or ejections from the volcanoes bordering the Rhine below Bingen. Of this lake, the valleys of the Neckar and the Mayn would have formed two great bays. According to this hypothesis, the depth of water must have been sufficient to have allowed a loamy sediment to be thrown down not only on the gravel of the Rhine, but at the height of 600 feet or more above that level, on the boundary heights. Afterwards, we must suppose that an opening was made through the barrier, and the lacustrine sediment denuded, until at length the original valleys of the Rhine and its tributaries were re-excavated, and small patches only of loess left here and there.

But this explanation is not sufficient, for when we pass from Bingen to the country of Neuwied, we find masses of the same loess rising to considerable heights above the Rhine, so that we require another lake, or we must remove the barrier of the great lake farther down than Andernach. If we then suppose it to have been in some of the narrowest parts of the great gorge between Andernach and Bonn, we again encounter a similar objection; for, on examining the Siebengebirge, we discover the loess at great heights on its flanks, as also on the opposite hills behind Poppelsdorf; and we are then under the necessity of constructing an imaginary dam, many hundred feet in height, which should stretch across a wide part of the plain below Cologne. Even if we are prepared to assume the former existence of one or more such barriers, we have still to assign adequate causes for their removal.

It is clear that no theory can account for the position of the loess, which does not admit great revolutions in the physical geography of the country now drained by the Rhine and its tributaries, within a very modern geological period, when all the existing testacea inhabited the country.

It seems also indispensable to assume that some barriers have existed, for those waters must have been at rest from which the loess, with its unbroken shells, was thrown down as sediment. Probably the relative levels of different parts of the country now covered by loess, have been altered by the elevation of some tracts, and the depression of others, since the loess was formed. In order to possess data for speculating on this point, we must have more accurate observations on the highest levels which the loess ever attains above the Rhine and above the sea.

This singular formation is so homogeneous in its mineral character, whether it rests on gravel, volcanic matter, granite, red sandstone, or any other rock, that it cannot be compared to the local alluviums which different rivers and torrents may have produced in various parts of the same hydrographical basin. It may not all have been deposited at one time, or in one vast lake; but it seems to have been derived from some common source, as from the sediment of one great river like the Rhine, continuing to overflow a certain district, and always bringing down the same kind of sediment.

I subjoin a list of the fossil shells which I collected myself, from the loess of the various districts which I have mentioned in this paper.

Helix fruticum,	Drap.	Pupa tridens,	Drap.
arbustorum,	ib.	lubrica.	
pomatia,	ib.	Clausilia bidens,	ib.
nemoralis,	ib.	plicata,	ib.
hortensis,	ib.	Achatina acicula,	ib.
ericetorum,	ib.	Succinea amphibia,	ib.
carthusianella,	ib.	elongata,	ib.
plebeium,	ib.	Planorbis marginata,	ib.
obvoluta,	ib.	carinata,	ib.
pulchella,	ib.	Lymnea auricularis,	ib.
Pupa muscorum,	Mont.	ovata,	ib.
(marginata, Drap.)		Valvata piscinalis,	ib.
dolium,	Drap.	Cyclas fontinalis,	ib.
frumentum,	ib.		

On the Theory of the Elevation of Mountain Chains, as advocated by M. Elie de Beaumont. By Dr Boue. Communicated by the Author.

In Brochant's excellent French translation of De la Beche's Geological Manual, M. de Beaumont has given a new and modified exposition of his theory and opinions, in regard to his twelve or thirteen epochs of elevations or revolutions. His essay may be considered as consisting of two parts, viz. an explanation of the theory, and an account of the application of his system.

It is only to Mr Lyell that M. de Beaumont seems to stand opposed in the first part of his essay. Every one knows and acknowledges that most of the upheavings *(redressemens)* were produced by a series of violent and rapid movements; and this view is rendered more probable by the extent of the effects of elevation. In treatises and lectures on Geology, the Pyrenees, the primary chains of Scotland, and some chains of Scandinavia, have sometimes been brought forward as examples of this kind; and M. de Beaumont acknowledges this very fairly (p. 167). On the other hand, Mr Lyell has shewn very well the vagueness of the limits assigned by M. de Beaumont to his revolutions. (See Principles of Geology, vol. iii. p. 343.) I believe that few geologists would blame M. de Beaumont for rejecting Mr Lyell's hypothesis, by which he endeavours to explain the upheavings, by the unlimited repetition of local and slow movements; although this view may be correct in the case of the elevation of whole continents.

It must always be difficult to trace limits between a very quick and a very slow movement; as, for example, when one speaks of an elevation which has taken place rapidly, does he mean an instantaneous upheaving? or are we to understand an effect produced in a space of some months, or some years, or even some centuries? An upturning of strata may have taken a considerable time for its completion; and even such a period as to have admitted of the deposition of strata on a part of the earth different from that where the elevating force was in action. In this way the geological demonstrations of the great revolutions produced by elevation, would not be everywhere present, a position which is just what M. de Beaumont admits.

It is probably to Mr Lyell that M. de Beaumont alludes, as considering "les dislocations de couches qui caracterisént les pays de montagnes, comme les résultats de phénomènes locaux, qui se seraient répétés d'une manière successive et irréguliere." (p. 260.) Unfortunately M. de Beaumont has not always attended enough to the mean directions (directions moyennes) of the various mountain ranges; and, on the other hand, leaving these natural guides to the labyrinth of dislocations, he has recourse to the indications afforded by maps, which are very often erroneous. He has committed this error in regard to the Apennine chain.

The study of all the possible intersections of M. de Beaumont's twelve or thirteen systems of elevation, would be most useful in order to get a standard point of departure; and to see if, in the known parts of the earth, there are no similar *accidens* which do not belong to any of the cases established *a priori*, and which would render necessary the establishment of some additional epoch of revolution.

According to M. de Beaumont, a parallelism of direction in the dislocations of various countries had been long remarked. Amongst the older writers, I shall rest satisfied with mentioning Stenon, who wrote in 1667 (De Solido, &c.), and Bernhard Varenius, who, in 1712, published his Geographia Generalis in quâ affectiones generales Telluris explicantur (Cambridge, 8vo). Werner, and after him Schmidt (Theorie d. Verschiebungen älterer Gänge, Frankfurt, 1810), applied the idea to the distinction of metalliferous veins; Humboldt did so to various chains of Europe; Jameson to the mountain ranges of Scotland; Hausmann, in 1808, to the mountains of Scandinavia, (Denkschriften d. Acad. v. München.); M. Brochant to the Jura range, and also to the Alps; Heim to the hills of central Germany, and Von Buch, in 1824, to the chains of central Europe. (Leonhard's Taschenbuch). "Cette notion de la contemporanéité des fractures paralléles entre elles et de la difference d'âge des fractures des directions différentes," (p. 621), was also an axiom of the school of Freiberg; "rien n'était plus naturel," adds M. de Beaumont, "que de Songer à la généraliser, et à l'étendre à toutes les dislocations que présente l'écore minérale de notre globe."

A well marked distinction must be made between those who reject totally the foundations of M. de Beaumont's doctrine, and those who admit them, but at the same time do not think it possible to push the consequences so far as he does. As a supporter of the latter view of the subject, I am fully aware of all the importance of the parallelism in the directions of mountain chains, as well as of longitudinal and transverse valleys, when I have limited countries under examination. These phenomena afford important indications, when the geognostical positions have furnished the key to the upheaving and upturning of the strata. But I confess that in the yet infant state of the science, I dislike travelling round the globe between the parallel lines of the same elevation, without taking into consideration the incorrectness of maps, and our complete ignorance about the stratification and the nature of the strata in most chains of the earth. M. de Beaumont may be right in his assumptions, but in this state of uncertainty I prefer abstaining entirely from such speculative subjects.—M. de Beaumont says, " le nombre des dislocations dans le sol de chaque contrée serait à peu près egal à celui des directions de chaînes de montagnes nullement distinctes et independantes les unes des autres, qu'on pourrait y distinguer." (p. 621.) For my part I cannot admit this definition, because I take into consideration the sinkings as well as the upheavings and upturnings; and, besides, I do not exactly understand the force of the expression, " à peu pres." The upraisings in a country are indicated by the altered positions of the various series of beds, and by the different directions of these changed positions. Every upraising produced, either separately or simultaneously, elevations, upturnings of the strata, depressions, and rents; thus the dislocations of a country will be marked by different *accidens*; first, by the variety of forms presented by chains of hills, and by peculiarities in the position of the mineral masses on their declivities; secondly, by the upturning of beds even on level plains, and at the level of the sea; and, thirdly, by the occurrence of depressions of the soil, rents, faults, veins, dykes, open rents, and valleys. Ordinarily all these *accidens* of upraising and disturbing power can be classified into a certain number of groups according to their directions. This is a longer definition than that of M. de Beaumont, and it is even not so well limited, but I think it is more conformable to the phenomena presented in nature, which, indeed, are of such a description as to baffle very strict classification.

On the other hand, M. de Beaumont contends, that "le nombre des dislocations n'est jamais très grand, qu'il est à peu près du même ordre que celui des changements de nature et de gisement que présentent les dépôts de sédiment de chaque contrée, changements qui les ont fait distinguer depuis Fuchsel (J. de Geolog. v. ii. p. 190) et Werner, en un certain nombre de formations, et qui ont été considérés comme étant chacune le resultat d'un grand phénomène physique," (p. 621.) I agree with Mr Lyell (Princ. vol. iii. p. 341,) that we should come to some agreement as to the meaning of the words formation and dislocation. If the first term were extended so far as that we should consider as formations the gypsum of Montmartre and the coarse marine limestone of Paris, we should not be able to understand one another; but yet I suppose M. de Beaumont is of this opinion.

In regard to the word dislocation, it is synonymous with separation and disjointing; and, taking this general designation, it appears to me that the dislocations of the ground are not nearly numerous enough to correspond with the directions of the chains. But I enter into the abstractive idea of M. de Beaumont, who, in this way, has only indicated those great phenomena which have raised up ranges of hills, while he has omitted the minor changes which have taken place. I believe that, in the present state of our knowledge, the twelve revolutions, or systems of elevation, are too few even for Europe, small as it is, in proportion to the whole surface of the globe.

M. de Beaumont acknowledges that the number of systems of elevation is by no means fixed for ever, (p. 123); and especially in so far as the older formations are concerned. Indeed if we include the whole surface of the globe, there seems to be nothing against there being double or three times the number. Yet there must be a limit to the greatest upraisings which have agitated the crust of the earth, and M. de Beaumont has expressed himself well on this subject (p. 661): it is only in regard to the number of elevations that we differ, and I confess I have less faith in the present state of our knowledge. To those philosophers who, on the other hand, suppose an unlimited number of epochs of elevation, we would, with M. de Beaumont, oppose the logical deduction of Saussure and M. Brochant, viz. that "La constance de direction des couches redressées dans un certain ensemble de montagne ou de terrain doit probablement resulter de ce que toutes ces masses ont eté deplacées, en même temps par la même operation naturelle."—(*Bullet. Univ. de Sc. Nat.* vol. xxi. p. 344.) Now, as these groups of deposits are limited in number, in proportion to the small surface of our planet, the opinion of an unlimited number of very great elevations would seem to be excluded.

M. de Beaumont believes that, in his system, he has given more than *des apercus generaux et vagues,* in regard to the mutual relations of elevations and geological formations (p. 621); but I believe with my excellent friend Mr Lyell, that he exaggerates the results of his views; for, speaking only of Europe, he considers merely the chains of mountains, and what he calls boutonnieres. He has not yet ventured to trace, upon a map of Europe, his twelve systems of elevations, although the facts for such a generalization are not wanting. But, as such a map is more striking than long descriptions, errors are more easily perceived even by those who are not initiated in all the details of geological geography, and in such a case retractations are more painful.

In order to have given something more than limited views, M. de Beaumont should have added to his ideas on the formations of mountain chains, and their directions, some considerations on the formation of table-lands and plains or flat countries, and also a greater number of geological and geogenical expositions, similar to those I offered on the nature and origin of the European formations, (Memoires Geolog. et Paleontol. 1832). By following this plan, his essay would have been complete, while, as it is, it cannot be considered as more than "des aperçus generaux et vagues;" for the respective geographical limits of his systems are not all traced; whereas, according to my way of considering and describing the geogeny of the crust of the earth, that of taking formation after formation, every tolerably accurate map gives a very fair idea of the changes which Europe has undergone at different times.

Some will say that I put too much weight on details; and I certainly do consider details as of great consequence, as it is in this way that it is most easy to point out an error, which might otherwise escape in the midst of generalizations. When M. de Beaumont shall have classed the whole of the European ranges and chains of mountains and the rest of that continent under his twelve lines of elevations, then every one will recognise with ease the truths as well as the errors of his doctrine. To give a striking instance of this, it is only necessary to recall to mind the line drawn by M. de Beaumont from north to south, on Corsica, Sardinia, and Istria; and for what reason? probably because their pointed extremities are turned to the south, as those of almost all continents and large islands are; for the direction of the strata is quite different, and indeed nearly the reverse. Now, every person who had traced upon a map the direction of the stratification of the mineral masses of these countries, would distinguish at once the important error committed by the ardent imagination of the professor. In this way, I am naturally induced to mention, as the greatest imperfection of his theory, the not taking sufficiently into consideration the general direction or strike of the beds. This omission, and the horoscopical interpretations of imperfect maps, are the chief objections I have to the application of the theory.

The formulary of M. de Beaumont's system was and still is "l'indépendance des systèmes de montagnes diversement dirigées, (Recherches sur quelques unes des Revolutions du Globe, &c. p. 303; and Manuel, p. 622); that means that every system of elevation has taken a different direction, sui generis. Starting from that proposition, which I believe is only correct within certain limits, he tells us of parallel lines of hills from the Cape Ortegal to the Persian Gulf, from Tenessee in the United States to Cape Comorin in India, (Bull. Univ. de Sc. Nat. vol. 21. p. 355). From this he considers himself entitled to conclude, what is in fact merely hypothetical, that "l'ecorce minerale du globe presente une serie de vides dont le parallelisme semble indiquer que la production est instantanée," (Bull. p. 356.) In repeating last year my objections to these propositions of M. de Beaumont, I concluded from the facts we possess, that one could not admit *the general coincidence between the directions of the beds and the chains, the constant parallelism of the dislocations of the same epoch, and of contemporaneous chains, and the constant non-parallelism of chains and upraised strata of different epochs.* (See my Resumé des Progrès de la Geologie for 1832 in the Bullet. de la Soc. Geol. de France, p. cxxii.)

M. de Beaumont still continues to explain the foundations of his theory as formerly, and one would think either that he must be correct, or that he kindly endeavoured to spare disgrace to me and others who think as I do. In a case of this kind, the interests of science should get beyond such trifling considerations. But it strikes one with astonishment to find that his views, when unfolded to us, do not at all correspond with his programme.

First, he thought it necessary to warn us, that, owing to the spherical form of the earth, the lines of elevation must have described sections of circles, and that they exist upon the tangents of these last. In regard to small sections of circles, this information was hardly necessary, but for those which are considerable, as, for instance, that of the Apennines, the Carpathians, (see Bullet. de la Soc. Geol. de France, vol. iv. p. 73), and some chains of Asia, it is essential that M. de Beaumont should explain himself clearly, and discuss the objections made to his opinions; a course which he has not followed.

Afterwards, when comparing his sections of circles with the lines of the meridian, he contends, in regard to the one, that he has in view only small sections of great circles, (p. 633); and as to the other, that he is not able to see the " limite à la distance à laquelle il serait possible de suivre des accidens de soulevement constamment soumis à une même loi," (p. 622.) Now M. de Beaumont himself says, " deux grands cercles se coupant nécessairement en deux points diametralement opposés ne peuvent jamais etre paralleles dans le sens ordinaire de ce mot." (p. 622.) But I leave this discussion, fearing lest it should be considered as belonging to the chicanery of words; and I now come to the facts and assertions by which it appears that M. de Beaumont reconciles his doctrine deduced from the parallelism of direction. I rest satisfied with transcribing the following four citations. "La direction du septieme système, celui du Mont Pilas, court en general à peu près du N. E. au S. O. Cependant il y a quelquefois des déviations suivant la direction de fractures plus anciennes; ainsi dans la Haute Saone, dans le midi de la Côte d'Or, et dans le departement de Saone-et-Loire, on voit un grand nombre de fractures de l'époque qui nous occupe suivre la direction propre au système du Rhin;" (at the end of page 638.) In explaining his system of the Pyrenees, the ninth revolution, the following decisive paragraph occurs: "Dans le nord de la France et le sud de l'Angleterre, la denudation du pays de Bray et celle des Wealds du Surrey, du Sussex, du Kent, et du Bas Boulonais, ils (les mêmes caractères de composition et de direction) paraissent avoir pris la place de protubérances du terrain crétacé dues à des soulèvements opérés immediatement avant le dépôt des premières couches tertiaires, suivant des directions générales parallèles à celles du Pyrénées, mais avec des accidens partiels, parallèles aux directions d'autres soulèvements plus anciens;" (p. 648.) Speaking of his tenth system that of the isles of Corsica and Sardinia, "Il est assez curieux," says M. de Beaumont, "de remarquer que les directions du système du Pilas et de la Côte d'Or, de celui de Pyrénées, et de celui des îles de Corse et de Sardaigne, sont respectivement presque parallèles à celles du système du Westmoreland et du Hundsruck, du systéme des ballons et des collines du Bocage, et du système du nord de l'Angleterre. Les directions correspondantes ne diffèrent que d'un petit nombre de degrés, et les systèmes correspondants des deux séries se sont succédé dans le même ordre, ce qui conduit à l'idée d'une *sorte de recurrence périodique des mêmes directions de soulèvement, ou de directions très voisines*;" (646.) I think that this is clear enough; but he adds farther, in considering the objections made to him on the same subject by Mr Conybeare (Phil. Mag. 3d Ser. August 1832, p. 118), "La direction des dislocations de l'île de Wight étant sensiblement parallèle à celle du système des Pays Bas et du sud du Pays des Galles, on aurait un quatrième exemple du retour à de longs intervalles des mêmes directions, des dislocations dans le même ordre; (voyez plus loin les remarques de M. de la Bèche à ce sujet.) Le système des Alpes Occidentales comparé au système du Rhin, dont il partage la direction à quelques degrés près, pourrait fournir un cinquième tèrme à la série de rapprochements qui indique cette singulière periodicité des directions des dislocations;" (p. 647.) Now I ask any one, if, with these true propositions, one can still talk of the "Independance des systèmes de montagnes diversement dirigées?" Does it not confirm those who had seen only a misconception in that abstraction? This important part of M. de Beaumont's system is thus completely modified; and we must take the retractation in the details, although in the generalization the contrary view is given.

Besides this, M. de Beaumont has not taken the trouble to answer the objections made to his opinions by various geologists; as, for example, in regard to the possible formation, by elevation, of much curved or contorted chains (see Bullet. de la Soc. Geol. de France, v. iii. p. 51.); the difficulties presented by chains of hills composed of horizontal beds, or beds elevated together, without being upturned, as in the German Jura in Wurtemberg and Bavaria, a chain which presents also to M. de Beaumont the difficulty of describing a curve from Schaffhausen to Ratisbon, and thence to Cobourg; the difficulties presented by cavities filled by upraised masses; and, lastly, the occurrence of chains in which the strike of the bed is not parallel to the direction of the mountains, as, for instance, in the Thüringerwald, where Heim described the fact in 1798. (Geol. Beschreib. v. ii. p. 18. See my Resumé des Progrès de la Geologie pour 1832, p. cxviii to cxx, in the Bull. de la Soc. Geol. de France.) To all these objections M. de Beaumont answers nothing, and he does the same in regard to those made by Thürmann; (Mem. de la Soc. d'Hist. Nat. de Strasbourg, vol. ii.); Schwatz, (Natürliche Geographie von Würtemberg, 1832, T. S. Min. 1833, cah. i.); Pasini, (Ann. delle Sc. del regno Lombardo Veneto, vol. i. fasc. 1); and Hibbert, (History of the Extinct Volcanoes of the Basin of Neuwied, &c. 1832.)

With regard to the cataclysms and destruction of creations produced by the elevation of the chains, that idea was mentioned by many old writers, of whom I need only allude to Fuchsel. Mr Sedgwick was the first to object that some philosophers had wished to separate in too decided a manner by revolutions, creations which would seem to be connected with the other great relations.

M. de Beaumont only answers to Mr Sedgwick, that " Lorsque deux formations semblent passer insensiblement l'une à l'autre, il n'y a jamais qu'une très petite épaisseur de couches, dont la classification puisse rester incertaine, et lorsque certaines espèces de fossiles sont communes à deux formations successives, elles ne forment, en général, qu'une fraction, souvent même peu considérable, du nombre total des espèces de chacune des deux formations ;" (p. 619.) Now, this uncertainty in the classification remains to us not as a consequence of a want of exactness in the system, but as a consequence of the gradual operations of nature. Besides, the whole reasoning of M. de Beaumont reposes upon the acceptation given to the word formation. Does it mean a deposit, a mass of various deposits, or a group of particular beings? I suppose M. de Beaumont adopts the second definition, but in that case, descending without fear from the generalizations to examples, we shall easily demonstrate that, for instance, some people have too hastily separated by general revolutions the zechstein and the coal formation ; and again, the variegated sandstone, the muschelkalk, and keuper, three deposits, which, on a large scale, form only a single geological mass.

M. de Beaumont, foreseeing the objections, agrees entirely with our views, for he acknowledges that "entre les périodes de diverses formations, il y a eu pour le moins des deplacements considérables dans les lieux d'habitation de certains groupes d'êtres organisés, en même temps que dans les lieux de depôt de certains sédiments ;" (p. 619.) Every one will admit this kind of anodyne revolution is widely different from those general cataclysms which were said to have produced such a change on the surface of the earth, that new creations were necessary to fill up again the spaces of the earth and seas, which were without beings to inhabit them. The door remains in this way open to every future correction, or to any addition to the actual system of the paleontological distribution into epochs.

Let us now review the twelve systems of elevation of M. de Beaumont.

The *oldest system of elevation* is that of *Westmoreland* and the *Hundsruck*, and consists of what I consider to have been islands, which had emerged before the formation of the carboniferous and

Dudley series of rocks. (See my Memoires Geologiques et Paleontologiques, v. i. p. 18.) In that system, the strata have been elevated in a line running a little to the E. of N. E., or a little to the W. of S. W., or h. 3 to 4 of the miner's compass. It includes the older chains of the British isles; those of the N. W. of Germany; the Erzgebirge; the Sudetes; a portion of the Black Forest, of the Vosges, of Mount Pilas, and of Brittany; the Montagne Noire in Southern France; the Mount Bigarre, and Mount Canigau in the Pyrenees; also a part of the centre of France, of the Maures, of Corsica, of Scandinavia (Westmanland, Jemtland, Lappmark), and of Finland. Gneiss, mica-slate, clay-slate, quartz-rock, and greywacke, constitute these chains or first continents.

The elevation of these mineral masses must certainly have taken place before the formation of the old red sandstone, and I believe with M. de Beaumont, that it was anterior even to the formation of the newer transition rocks. The horizontal or gently inclined position of the limestone containing orthoceratites and trilobites in Sweden, in Baltic Russia, and Podolia, are much in favour of that idea. The beds with trilobites at Dudley and Tortwork, would also have been horizontal had they not been affected by more recent dislocations. The same might be said of the arenaceous and calcareous slates containing anthracite of Southern Iceland. I may add, that, in regard to Canada, excepting the information we have as to directions, the only data we have to go upon in determining the age of the first elevations of the older rocks of that country, are derived from some horizontal or slightly inclined masses of shelly limestone. The horizontality of the system in Northern Europe and in America, forms the type of a peculiar geological zone. Before going further, it is as well to remark, that there are in Europe other elevations which have taken place in lines parallel to those which we have already mentioned, and which, as we shall see, are of a different age. On the other hand, there are in Europe upheavings in completely different directions from that of which I have spoken, and which must nevertheless have been produced at the same period as the system which we are now considering. As an illustration of this remark, I may instance the primary schistose rocks of the Riesengebirge and the Eulengebirge, where the direction of the strata is N.N. E. to S.S. W., or N.N. W. to S.S. E., and sometimes W. N. W. to E. S. E. Another example is the primary part of the Böhmer-Wald-Gebirge, where the direction is most generally E. N. E. to W. S. W. It was thus that the Continent was produced, which was afterwards covered with the insular vegetation whose remains are now buried in the coaly beds of Silesia and Bohemia. This, then, would be an example of a variety of directions in the same system of elevation, and it will be perceived, that, with my mode of reasoning, one can go farther, and with more certainty, than when guided only by the doctrine of directions of chains, and the parallelism of elevations.

If M. de Beaumont is skilful enough to decompose all these various directions into as many systems, it remains for him still to prove the chief point, viz. that all the upheavings besides that from a little to the E. of N.E., to a little to the W. of S.W., have taken place at a subsequent period; and this is the question upon which I insist.

I believe that every epoch has resulted from several limited movements, which have taken place in the same or in different directions; but as yet our collection of facts as to directions and inclinations is not sufficient to enable us to subdivide each period, but this may possibly be afterwards accomplished.

In sketching out the position of the European Islands before the formation of the carboniferous series, I enumerated, after the isles or emerged and upheaved masses, a series of submarine chains or rocks placed at an inferior level, and composed of the newest transition rocks, (Memoires Geologiques, et Paleontologiques, vol. i. p. 19). M. de Beaumont has taken a part of these last to construct his *second system of elevation*, or that of the Ballons (Vosges), and the hills of the Bocage (Calvados). This system would also comprise a part of the interior of Brittany, a portion of the south-eastern part of the Vosges and of the Lozere, the anthracitiferous rocks of Southern Iceland, and some hills of greywacke and slate in Devonshire and Somersetshire. Lastly, M. de Beaumont has included in this system the hills of greywacke to the N.W. of Magdeburg, the hills of Sandomirz in S.W. Poland, and the formation of the N.N. E. escarpment of the Hartz.

This elevation, anterior to the old red sandstone formation, would present anomalies in the direction of the dislocations. "La plus marquée probablement produite immediatement après le dépôt des roches suposées redressées court suivant des lignes dont l'angle avec le meridien varie de 90° à 67° 30′ (vers l'ouest), mais qui sont toujours très près d'être exactement parallèles à un grand cercle qui passerait par le Ballon d'Alsace (dans le midi des Vosges), en faisant avec le meridien du lieu un angle de 74°, ou en se dirigeant de l'O 16° N., à l'E., 16° S." Now, Mr Weaver assigns to the anthracitiferous rocks of Southern Iceland, a general direction from west to east, with an inclination to the south and north. In Devonshire and Somersetshire, the direction is W. 10°, N. to E. 10° S. We thus perceive the pliableness which M. de Beaumont has given to his system, a quality which makes it agree still less with systematic ideas. On the other hand, he has thus been enabled to answer skilfully the objection made by Messrs Sedgwick, De la Beche, and Connybeare, in regard to the parallelism of elevation of the older rocks in western England, and in the south of Ireland, where these gentlemen think they have observed contemporaneous upheavings from the E. of N.E. to the W. of S.W., and from E. to W. I have pointed out dislocations in Hungary which run from E. to W., and which are of a more recent age than the preceding. (See Bullet. de la Soc. Geol. de France, vol. iv. p. 75.)

The third system of elevation, that of the North of England, has been founded on the able observations of Mr Sedgwick, who has shewn that England is traversed by a hilly carboniferous axis, which runs from S. to N., but bends a little to the N.N.W. "Les forces soulevantes auraient agi (non toutefois sans des deviations considerables), suivant des lignes dirigées à peu près du S. 5° E., au N. 5° 0;" (p. 630). The consequence of this is the occurrence of great faults in Derbyshire, at the foot of Crossfell and the Craven hills, as well as in the anticlinal line of the western moors of Yorkshire. All these fractures have preceded the formation of the old red sandstone, and indicate a violent and momentary action with which Mr Sedgwick connects also the eruption of the trap rocks and the toadstones.

M. de Beaumont thinks that traces of these dislocations are to be found in the Malvern Hills, the neighbourhood of Bristol, on the western coast of the department de la Manche, perhaps in the hills of Tarere, the chain of Les Maures, and the primitive hills of Corsica.

I have also given examples of fractures in the direction from north to south in Hungary, Styria, and Carinthia, but of an entirely different date; certainly more recent than the greensand. (See my Resumé for 1832, p. 121, and the Bulletin de la Soc. Geol. de France, vol. iv. p. 75.) But my objections have become useless, since M. de Beaumont now acknowledges the possibility of the parallelism of upheavings (redressemens) which have taken place at different epochs.

The fourth system of elevation is that of the Netherlands and the Southern part of Wales. Freisleben and other geologists have pointed out in the beds of the red secondary sandstone and zechstein of Mansfeld, faults and inflections in a direction nearly from east to west. These *accidens* seem to M. de Beaumont merely a peculiar case of those irregularities in stratification which are common to all the sedimentary deposits not posterior to the zechstein, from the river Elbe to Wales. In this way he attributes to this system all those singular bendings of the carboniferous strata of the Netherlands and the Bristol Channel. These movements were anterior to the formation of the secondary conglomerates of Malmedy; as well as that of the magnesian conglomerate of England, a rock to which Sedgwick has assigned a date posterior to that of the magnesian limestone of the north of England.

The coal measures of Sarrebruck covered by the horizontal beds of the Vosges sandstone, must have been affected by that disturbance, and this subject deserves the further consideration of geologists. Now, let us compare what M. de Beaumont tells us about Belgium and the Sarrebruck country, with the objections I made to his first paper in 1830 (J. de Geol. vol. ii. p. 347.), and the examples I gave of recent elevations in the direction from east to west, (Bull. de la Soc. Geol. de France, vol. iv. p. 76.) M. de Beaumont gets over the difficulties by admitting completely his first system, and a return of the same direction in the upheavings. Yet I still contend, that in the coal measures there are irregularities in the stratification which have had their origin in the mode of formation of the deposit, and which are not to be confounded with dislocations. (See Mem.

Geol. et Paleont., p. 28,–31 et 35). Professor Merian mentions directions of stratification from east to west in the older formations of the Black Forest, and there are similar examples in Southern Silesia, in Sudermanland and Smoland in Scandinavia, and I do not see well how these *accidens* can be made to agree with the epoch of elevation of which we have now spoken.

The fifth system of elevation is that of the Rhine. The Vosges, the Hardt, the Black Forest, and the Odenwald, form two symmetrical groups, which present two long steep acclivities which are rather sinuous, but parallel to each other and also to the bed of the Rhine, and having the direction of N. 21° E. to S. 21° W. These lines are the type of Von Buch's Rhine system.

The escarpments of the Vosges are composed chiefly of sandstone of the Vosges (grès vosgien), and variegated sandstone. Muschelkalk and keuper come in contact with these rocks in an unconformable position, a peculiarity which shows the epoch of formation of this system of fractures. While pointing out this fact in regard to the Vosges, M. de Beaumont does not extend the observation to the Black Forest, where the variegated sandstone is found upon the inclined table land as well as at the foot of the escarpment. (See Journ. de Geol. vol. iii. p. 349.) We must also in this case compare Beaumont's views upon the formation of the Vosges and the Black Forest, with those very different ones adopted by my active friend M. Roget. The latter maintains that these mountains are mere central masses with diverging ramifications. (See Bull. de la Soc. Geol. de France, vol. iv. p. 129, and his work on the Vosges now in the press.)

M. de Beaumont thinks he can find traces of these dislocations in the directions of some chains, as in the hills between Saone and Loire, in the hills of the centre and the South of France, and in the Mediterranean part of the Var department, although the deposits between the coal measures and the variegated sandstone are not present in these districts, (p. 635.)

The sixth system of M. de Beaumont is that of the Thüringerwald, of the Böhmerwaldgebirge, and Morvan. I shewed some years ago that the Jurassic rocks have been deposited in seas or great gulfs, (Mem. Geolog. vi. p. 48.) These deposits have been formed in a horizontal or gently inclined position, and while some of them have been subsequently upheaved, the rest remained in their original position either in the form of a flat tract of country, a low table land, or pretty high truncated hills as in Bavaria.

M. de Beaumont assigns to this system a direction W. 40° N. to E. 40° S.; and the formations which were disturbed by its elevation are the beds of the variegated sandstone, of the muschelkalk, and of the keuper as well as the older rocks, and these formations must have formed the steep walls (falaises) at the base of which the Jurassic beds were horizontally extended. The movement took place between the period of the keuper and that of the inferior lias sandstone. As examples, M. de Beaumont mentions the north-eastern part of Germany, the Thüringerwald, the Western Böhmerwaldgebirge, the neighbourhood of Autun and Avallon, and in Greece the Olympic system of MM. Boblaye and Virlet. In north-east Germany the floetz formations, from the variegated sandstone to the Jura limestone, form curved beds or inclined masses; and it might be a subject of discussion whether these *accidens* are original, or if all these deposits were at first horizontal. But entering into M. de Beaumont's views, it would be at least necessary to date the upheaving from the epoch of the middle part of the Jurassic formation.

With regard to the Thüringerwald, the Jurassic deposits and even the lias beds do not touch its base at the west end, and they do not exist between its eastern extremity and the Hartz ; and the keuper only approaches its neighbourhood. If the zechstein covers in nearly horizontal beds the secondary sandstone of Eisenach, we can observe near Ilmenau and elsewhere disturbances, and especially singular faults (failles), which extend from the older coal measures to the variegated sandstone. Voigt, Von Hoff, &c. have described these minutely, and the last mentioned distinguished observer, in a recent work on Thuringia, has expressed a suspicion that the muschelkalk has been dislocated and contorted after its formation, and before the formation of the succeeding deposits which occur in its valleys (Hohenmessungen von Thuringen, 1833, in 4to.) In the Coburg country, the lias marls and sandstone, as well as the lower Jura limestone, are placed horizontally upon the keuper, which seems to occur in cavities of muschelkalk, and this last is itself covered here and there by the keuper. Near Blumenroth the upper part of the keuper, with a subordinate bed of magnesian limestone, has been upheaved.

The Fichtelgebirge and Böhmerwaldgebirge are connected with the Thuringerwald by the Frankenwald; and Von Hoff mentions that the strike of the beds of this last chain is from N.E. to S.W., a fact which is also shewn by all geological maps. (See Taschenbuch d. Mineral. of Leonhard, vol. vii. part i. p. 151. et 159.) In the Fichtelgebirge and Western Böhmerwaldgebirge, the direction of the strata is from E. N.E. to W. S.W., and the upheaving and upturning seem to have been antecedent to the deposition of the older coal formation in Bohemia and Bavaria. On the other hand, the Olympic system is the oldest in Greece, and, according to MM. Boblaye and Virlet, it has affected only the primary rocks.

The direction nearly from N.W. to S.E., is that of many other chains of hills, as the Bleking in Scandinavia, a part of the Hartz, the hills of Alvensleben, the floetz chain in Westphalia, the hills in Lusatia, a part of the Riesengebirge, the hills of Southern Silesia, the chain of Southern Poland, the older part of Sicily according to Hoffman, &c. The epochs of these ranges of hills are very different from the epoch which, in the opinion of M. de Beaumont, is characterised by this particular direction.

The seventh system of elevation is that of Mount Pilas in the Forez, of the Cote d'Or, and the Erzgebirge. It would also include the Cevennes and the table-lands of Larzac. M. de Beaumont finds traces of it from the Elbe to the Dordogne, and investigates its influence on the peculiar distribution of cretaceous deposits, and in so doing points out considerations similar to those offered in my Geological and Paleontological Memoirs (vol. i. p. 48–50, and p. 53–56.)

The direction of this system is from N.E. to S.W., or from E. 40° N. to W. 40° S., pretty similar to the direction of the first system. This *boulversement* is supposed to have taken place between the deposition of the Jurassic formation and the commencement of the cretaceous epoch.

Upon this point I must again urge my objections in regard to the Erzgebirge, although I cannot now bring forward for this purpose the position of the coal measures upon the older upheaved strata of that chain, because M. de Beaumont now admits these traces of his first system. As the direction of N.E. to S.W. is frequent in the Erzgebirge, I do not see why this chain should be regarded as belonging to the seventh rather than to the first system. But I must still add, that the celebrated Professor Naumann of Freyberg has given, as the mean direction of the slaty rocks of that district, h. 7.4, or from W. N.W. to E. S.E.

After the primary slaty rocks were raised up in various directions from N.E. to S.W., and from W. N.W. to E. S.E. in the Erzgebirge; from N.W. to S.E., from N. N.W. to S. S.E., from N. N.E. to S. S.W., and from W. N.W. to E. S.E. in the Reisengebirge; and from E. N.E. to W. S.W. in the other chains of Southern Bohemia; that last country must, at a very early period, have formed a great cavity or Caspian Sea, in which were deposited the old coal formation, red secondary sandstone, chalky rocks, and some tertiary argillaceous beds, with lignite. Of all these formations, the greensand and inferior chalk are the only ones which extend beyond the basin to the flat country surrounding the circular mass of hills. These peculiar circumstances of position prove as completely the antiquity of the annular mass of mountains, as does the circumstance of its not having been anywhere cut through before the deposition of the chalk; so that none of the formations between the secondary sandstone and greensand could have been deposited there. The only other supposition which suggests itself, is to imagine that the Bohemian cavity which received the chalk strata was formed by a sinking down before the cretaceous period, and that it had previously been an undulating tableland,—an hypothesis which would lead us to consider the coal measures and the secondary red sandstone as fluviatile and terrestrial deposits. But the presence of a trilobite limestone would render an additional hypothesis necessary, viz. the repetition of a sinking down, during which process an upheaving *en masse* must have happened, and this is certainly a very complicated explanation. (See my Mem. Geolog. vol. i. p. 71.)

At the period of the deposition of the greensand formation, a great rent running N. and S. separated the Erzgebirge from the Riesengebirge; and an immense quantity of quartzy debris accumulated there in the form of horizontal or gently inclined beds, and corresponding to the configuration of the surface on which they were deposited. Now, some geologists believe that sienites made an eruption through the chalk and covered this formation, and it is likely that these *accidens* were accompanied by some disturbance. To such a cause I attribute the upheaving of some inferior beds of the Jurassic system described by Count Munster. (Deutschland of Keferstein, vol. vii. cap. i. p. 1.) Mr Naumann endeavours to connect with these igneous phenomena the local dip of 45°—70° of the greensand at Mariaschein, Liesdorf, and Weilzen near Augsig, which would also be partly the effect of a slipping down. At least we cannot yet draw general conclusions from the facts, as the perfect, or nearly perfect horizontality of the greensand upon the older rocks, is the predominating feature. Such seems to me to be the state of the question. M. de Beaumont explains it according to his own views, but it would be regarded in quite another light by those who like Cordier, Naumann, Rozet, &c. have peculiar ideas on the subject of slaty rocks.

If M. de Beaumont had not given up his belief in the constancy of the parallelism of direction for every system, I would here point out to him the Western Carpathians running N. E. and S. W., and composed chiefly of greensand beds, which are upheaved in such a manner as to shew that they ought to belong to his eighth and not to his seventh system.

The *eighth system* is that of *Mont Viso.* M. de Beaumont admits with me, and in opposition to other geologists, that most of the Alpine summits owe their height to a series of successive elevations, (p. 640). The direction of the dislocation now under consideration, is from N.N.W. to S.S.E.; and the examples are found in the French Alps, the S.W. extremity of the Jura from Nice to Lons le Saulnier, from Noir Moutiers to the southern part of the kingdom of Valencia in Spain; and in the Pindic system, in Greece, pretty nearly parallel to a great arc of a great circle passing through Mont Viso. These disturbances are supposed to have happened between the deposition of the greensand and chloritose chalk, and that of the marly and white chalk.

As I have already given examples of a direction from N.N.W. to S.S.E., as in the Riesengebirge, one would thus have specimens of the errors to which we are led by the theory of the parallelism of elevations in the same epoch, unless M. de Beaumont can include such cases under his Mont Viso system.

The *Pyrenees* are the type of his *ninth system* of elevation, which appeared between the chalk period and the commencement of the tertiary deposits. All geographers have recognised a uniform type of structure in the range of hills extending from Cape Ortegal in Gallicia, to Cape Creuss in Catalonia. On the great scale, it is to be regarded as a congeries of parallel ranges running W. 18° N. to E. 18° S., and in an oblique direction in relation to the line which joins the two extremities.

Pareto (See Bull. de la Soc. Geol. de France, vol. i. p. 64), and I (T. de Geol. vol. iii. p. 353, andResumé pour 1832, p. cxviii.) maintain the union of the Apennines with the system of the Pyrenees, while M. de Beaumont persists in his first opinion on the subject. We tell him, that in Italy the direction of the upheaving, and also that of the igneous dikes and veins, is from S.W. to N.E.; while he finds, upon geological or geographical maps, indications of fracture, which those who have been on the spot have not been able to discover. On the other hand, if M. de Beaumont would admit that his ninth system of elevation has, like some of the other systems, taken place in different directions, we should very nearly agree as to the respective position of the tertiary rocks. M. de Beaumont lays much weight upon his preconceived line of the igneous rocks, but their position does not prove much, for, from their fluid or pasty state, they have naturally filled up rents produced during elevations, and yet these rents would be transverse to the axis of the principal movement. Besides, M. de Beaumont himself acknowledges that the ophites are in that particular case, and " qu'ils ont suivi les directions de toutes les anciennes fractures et de tous les clevages plus ou moins oblitères du sol," (p. 656).

The other examples given by M. de Beaumont, are the steep wall of the Southern Alps; the Julian Alps; a part of Croatia, Dalmatia, and Bosnia; the Achaic system in Greece; the eastern part of the Carpathians; some parts of the Hartz; and the denudations of the Bray country, of the Wealds of Surrey, of Sussex, and of Kent. I shall not repeat here the distinction it is necessary to make between the denudations and upheavings of beds produced by elevation, and those denudations which result only from convex surfaces covered by gently inclined beds. (See Bull. de la Soc. Geol. de France, vol. ii. p. 23; or Resumé, pour 1832, p. cii. et. cxvi). To give an extreme example, I may remark, that the denudations, elevations, or chalky craters of elevation at Beine near Grignon, and at Meudon near Paris (p. 655), have not yet been admitted by geologists.

Other examples correspond exactly with what I have said upon this subject in my *Memoires Geologiques;* for every where the greensand has been dislocated and elevated to a great height. But the direction of these *redressemens* does not seem to be the same in different countries; and I do not see that coincidence which M. de Beaumont perceives. For instance, in the eastern Carpathians, the dislocations, as well as the longitudinal valleys, run N.W. and S.E., and these disturbances are contemporaneous with those which have lifted up the other or western part of the Carpathians from N.E. to S.W. (See Bullet. de la Soc. Geol. vol. iv. p. 73.) M. de Beaumont has concluded, from the examination of maps, that the chain of the western Carpathians is parallel to the Western Alps; and hence draws the conclusion that their upheaving coincides. Now, if he had been on the spot, he would know that the stratification of the beds forms a kind of diagonal line with the direction of the higher part of the chain; so that if the last corresponds with the direction of the Western Alps, the upheaving of the beds has taken place in a different line. One of two things must be the case; either the strata in the Western Alps and Carpathians not being parallel, must belong to two different elevations, or, what is more probable, these two ancient table-lands were at the same epoch upheaved and upturned in directions somewhat different. At the foot of the Carpathians alpine blocks are unknown, and the old alluvium, as well as the newest tertiary deposits, are horizontal, but the molasse has been upheaved, as along the Western Alps.

I do not see how it is easy to get rid of the *disagremens* of partial *accidens* parallel to the directions of other older elevations (p. 643.); but with regard to the pretty sinuous line from London to the mouth of the Danube, the southern border of an immense sea, M. de Beaumont does well to remark that the line is undulating, for I think it is so much so, that, leaving the gulfs out of consideration, and supposing it parallel to his Pyreneo-apennine direction, I do not see the conclusion he can draw from it, unless he supposes that the configuration of the whole of Europe was modelled at that period according to that system, a proposition which has still to be proved. And M. de Beaumont himself tells us, that "ce grand espace présentait aussi des irrégularités résultant de dislocations plus anciennes et dirigées autrement," (p. 644).

The *tenth system* of elevation, is *that of the islands of Corsica and Sardinia*, and its period of formation that between the deposition of the inferior tertiary strata of Paris and the second tertiary formation, commencing with the Fontainebleau sandstone. The valleys of the Loire, of the Allier, and the Rhone, are supposed to have owed their origin to this system of dislocations. Like Sickler (Idien zu e. Vulcanischen Erdglobus, Weimar, 1812, 8vo.) M. de Beaumont connects with it some basaltic cones of Northern Germany; while Keferstein arranges the basalts of the same country in parallel zones, running east and west (Die Basalte von Nord Deutschland 1820).

The tenth system seems to have been established on grounds too unsatisfactory; for the Islands of Corsica and Sardinia have been too little examined to allow of their being taken as its type. (Journ. de Géol. v. 3. 355, and Resumé for 1832, p. cvi.)

The direction N.S. is found also in the beds of a part of Scandinavia (Wermeland, Dalecarlia); in the Oural; the Aldan hills in Siberia; in the S.W. Hartz; in the upper part of the Leine valley in Hanover; on the borders of the Weser and Fulda; in some hills near Paderborn, &c. Now, the epoch of all those upheavings does not accord with that at which M. de Beaumont supposes that Corsica and Sardinia assumed their present configuration.

In speaking of the *eleventh system*, or *that of the Western Alps*, M. de Beaumont agrees with the geologists who preceded him, in believing that this chain has been formed by a certain number of elevations, repeated at long intervals of time, and mostly in different directions. One of the most recent must have produced the Mont Blanc chain, as we there find the erratic blocks placed upon the uppermost molasse. Von Buch, in 1811, and Von Raumer, in 1817, opposed this fact to the false name of Protogine, which Jurine gave to the rocks of Mont Blanc. (See my Memoir. Geolog. p. 357. and following.)

M. de Beaumont thinks it is easy to trace in the Alps the intermixture of the systems of elevation, and he mentions in it some circular elevated cavities like that at Lorieche, at Derbarrens, and round Mont Blanc. If M. de Beaumont deciphers so easily the intermixtures (entrecroissemens) of systems, Professor Studer would be desirous to have an answer in regard to a range of hills in the Canton of Berne, where the upheaved beds alter their directions without fracture or perceivable intermixture of system. Studer is a well informed geologist, and one who seeks truth, and is not afraid of being contradicted when he is wrong.

The upheaving of the Western Alps has taken place in a direction from N.N.E. to S.S.W., or more exactly from N. 26° E. to S. 26°W. In the interior of the Alps the *rides* having been produced on ground already raised out of the water and hilly, the dislocations have extended only to the chalk formation (compare my Memoires Geologiques, p. 60.); but on the borders of the Alps the middle tertiary rocks have been upheaved, as at Superga near Turin, at the foot of the great Chartreuse in Provence, in Entlibuch, &c. M. de Beaumont finds a relation between the position of the cones of phonolite at Howentwiel and the small island of Ilion; but I think the asserting this is at least hazardous.

M. de Beaumont also thinks that this system is connected with the direction of the eastern coast of Spain, a chain in Morocco, &c.; and he terminates his account of it by some considerations on the configuration of Europe after this elevation. (See my Memoires Geologiques, p. 61—75.)

According to M. de Beaumont, the hyæna, the ursus spelaeus, the Siberian elephant, the mastodon, the rhinoceros, and the hippopotamus, perished during that cataclysm,—an hypothesis which, in regard to some of the animals at least, requires confirmation.

The molasses of the Entlibuch are upheaved and inclined, but I do not know that they include the upper part of this deposit, where shells are so abundant, and in which the strata are ordinarily horizontal, or very little inclined, as near Zurich, between Thun and Berne, in Argovia, and elsewhere.

M. de Beaumont still considers the nagelfluh of the Rigi as part of the molasse; but the very height of the mountain (1875 metres), an elevation which the molasse nowhere else attains, shews that the rock belongs to the inferior cretaceous series. M. Bertrand Geslin has for a long time been of my opinion, and found in it even the fucoidal sandstones of the greensand (see J. de Geologie, vol. i.): and the section given of the Rigi by De la Beche confirms my suspicion (p. 268.)

M. de Beaumont repeats the error of Messrs Murchison and Sedgwick, who maintain that tertiary rocks occur in the valleys at the northern foot of the Eastern Alps (p. 650.). The Lignite of Hering, in the Tyrol, would be the only case of the kind, but I have already explained why that fluviatile or delta formation is found at the outlet of the valley. (See my Memoires, p. 7.) On the other hand, the observations made on the base of the Southern Alps near Coma and Vicenza, in the Netherlands, and in the Pyrenees, go to confirm the objections I made to the opinion that the Gosau deposit belongs to the tertiary series. (See my Memoires, p. 185.)

If direction alone were to be attended to, we should include in this eleventh system a part of Scandinavia (Upland, Smoland), Northern Russia, a portion of the Riesenbirge, &c. Now, in Scandinavia there are no rocks newer than the older transition formation; and Mr Ermann believes that the elevation in Russia took place after the formation of the first floetz deposits, an opinion which does not correspond with Beaumont's views.

The *twelfth system* is that of the *great chain of the Alps from the Valais to Lower Austria*; it has the direction E. ¼ N.E. to W. ¼ S.W.; and was formed between the period of the tertiary deposits, or the *terrain de transport ancien*, the old alluvium of M. de Beaumont, and the older true alluvium. This elevation caused the dispersion of the rolled alpine blocks, by the sudden melting of snow on the Western Alps; but the bodies of water thus formed, and which transported these blocks, were " des courans diluviens qui n'ont rien de commun avec le deluge de l'histoire." (P. 653.)

It is a singular fact, that, in going from west to east, these blocks are not found further than the outlet from the Alps formed by the valley of the Inn; and their size diminishes exceedingly beyond the Rhine valley. In Austria, I have met with no blocks—there are merely pebbles.

M. de Beaumont ascribes to a more ancient catastrophe the dispersion of the rolled masses of northern Europe (p. 655.); and here compare my Memoires, p. 77 and p. 359.

As examples of his *twelfth system*, M. de Beaumont mentions the hills of Sainte Baume, of Sainte Victoire, of Leberon, of Ventoux, Mount Pilate, the two Mythens near Schwitz, &c.; the lines of the higher hills of Spain, and the northern chain of Sicily. He also connects with it the gypsum and the salt deposits, and the salt spring, together with the eruptions of the ophite or diorite in the Pyrenees and in Spain.

The shores of the seas of these early periods produced lines pretty nearly parallel to the direction of the great chain of the Alps. (P. 656.)

M. de Beaumont acknowledges that this elevation produced in the south-east of France a double inclined plane, on the one side ascending from Dijon and Bourges to the Forez and Auvergne, and on the other from the shores of the Mediterranean to the same districts. He then makes out a line of culminating points from Hungary to Auvergne, which would explain some of the anomalies in the geodesic measurements. Lastly, he connects with it very well the formation of rents or great valleys in the Cantal and Mont Dore; giving to some parts of the latter the name of craters of elevation. (P. 654.)

Taking the direction as a guide, one would include in the same system with the Alps the chain of Fogares in southern Transylvania and the Balkan; yet in the former it is only the greensand which is upheaved, and in the latter only the alpine Jura limestone.

With regard to the parallelism or coincidence of origin which M. de Beaumont has established between his epochs of elevation, and the formation of the various chains, I shall rest satisfied with a few observations, as we yet possess too limited a collection of facts, and our maps are so imperfect.

I do not see reasons for believing that the Alleghanys and the Gaults of Malabar were elevated at the same time as the Pyrenees. No one has ever observed greensand on the summit of the Alleghanys, a range of mountains composed of slaty rocks which are more or less crystalline and arenaceous, or of older schistose rocks. Some old coal measures occur at their base, and at some distance, some red saliferous sandstones. The sections of Maclure, Brown, Taylor, Hitchcock, and other American mineralogists, show that this chain was elevated before the deposition of the old coal strata, but it has, perhaps, subsequently been subjected to some dislocations. The Gaults of Malabar have, according to Dr Hardie and other geologists, a direction from north to south, or rather a little to the west of north to a little to the east of south. They are composed chiefly of granite, crystalline slates, and trap-rocks, a geological constitution which at once excludes the idea of correspondence with the Pyrenees. They are probably a continuation of hilly ranges, elevated before the deposition of the old coal measures, and before that of the red saliferous sandstone of India; and regarding them in this light, one does not see what relation they can have with the present configuration of the Pyrenees. It is more likely that the mountains of the Crimea and the Caucasus were connected with the elevation of the Carpathians and Pyrenees.

The connecting the upheaved northern chain of Norway with that of the Western Alps (an idea founded on the direction of the chains given in maps) remains a mere hypothesis, as there is a total want of floetz and tertiary rocks in Norway. From the North Cape to the White Cape in Africa, the general line of the European shores had, according to M. de Beaumont, the direction of that elevation. The great Alps would be represented by the Atlas and by the central chains of the Caucasus and the Himalaya. "Toutes ces chaines courent parallèlment à un grand cercle qu'on representerait sur un globe terrestre par un fil tendu du milieu de l'empire de Maroc, au nord de l'empire des Birmans," (p. 659.)

The Himalaya range has not the direction of the Eastern Alps, as is well seen in the excellent map of Professor Ritter *(Abh. d. k. Acad. Wissensch, Berlin* 1832.) According to that learned geographer, this great range runs N.W. and S.E. *(Entwurf* *zu e. karte von ganzen Gebirgssystem d. Himalaya*, 1832, p. 10.) Dr Hardie gives it more accurately perhaps a direction N. 25° W. to S. 25° E. If it were parallel to the chain of the Alps, it would have a more easterly direction. The cretaceous shelly deposits on its summits, and the gently inclined molasse strata at its southern base, would lead us to suppose that its last upheavings took place at the tertiary epoch, or perhaps after the molasse. Excepting in the valleys, primary blocks have been observed only on the sides of the Indau Kooh.

The analogy of position and fertility in Lombardy and the valley of the Ganges, pointed out by Professor Ritter and M. de Beaumont, is accidental, and is the consequence of the direction of the waters of the Po and the Ganges in longitudinal valleys at the base of high ranges of hills. It has been supposed that the valley of the Indus communicates with that of the Ganges by a narrow and strait neck; and yet Dr Hardie found, to the south-west of Delhi, between the two valleys, a considerable chain of hills, the Neilgerrhi, 60 miles in length, and sometimes attaining a height of 5000 feet.

M. de Beaumont now gives up his *Deluge Historique* mentioned in his first edition, and, like Mr Sedgwick, believes that it was only a local event (p. 661). He thinks with Mr Lyell, and other geologists of the old and the modern schools, that "les causes qui ont produit les phénomènes géologiques, subsistent encore, et que la tranquillité dont nous jouissons aujourd'hui est due à leur sommeil bien plutôt qu' à leur aneantissement," (p. 662.) He differs, in this respect, from Brongniart's opinion (Tableau des Terrains); but, in the mean time, he returns to the ideas of this last geologist, and to my own, in supposing that the creating or modifying causes have formerly shewn an energy superior to that with which they have been acting since the establishment of actual societies, and that there have been periods of comparative tranquillity, (p. 663.) In this respect he is far from agreeing with Mr Lyell.

The elevation of mountain chains cannot be ascribed to the continued operation of Plutonic action, but we must rather, with Cordier, and other philosophers, seek for the cause in the "refroidissement séculaire," that is the gradual diffusion of the primordial heat to which our planet owes its spheroidal form, and the generally regular disposition of the layers of the globe from the centre to the circumference, in the order of their specific gravity. The refrigeration would tend to establish a relation between the capacity of the solid crust and the volume of the interior mass still in a fluid condition; and the upheavings or prominences would be the consequence of a diminution of the capacity of the solid crust, in consequence of the "retrait" produced by the gradual refrigeration of the masses in the interior, (p. 665). These are the views which M. de Beaumont entertains.

AN ATTEMPT AT A NEW ARRANGEMENT OF THE ERICACEÆ. *By DAVID DON, Esq., Libr. L. S., &c. Communicated by the Author.*

AMONG the numerous families which compose the vegetable kingdom, few surpass the *Ericaceæ* in the diversity of their forms, beauty of their flowers, or in the extent of their geographical distribution, which verges upon the ultimate limits of vegetation in both hemispheres. The direction of mountain-chains, and especially of particular strata, such, for example, as siliceous and micaceous deposits, appears to exercise an equally important influence on the distribution of this family with the circumstances of latitude and elevation. Species of the groups of *Andromedeæ* and *Vaccinieæ* traverse the Andes from one extremity to the other; and in Asia they extend from the Frozen Ocean to within the Tropics; colonies of them being found in almost every branch of the Indian Alps. The similarity of the vegetation of North America and Central Asia is strikingly exemplified in the groups of this family which are peculiar to both regions, such as *Rhodoreæ*, *Monotropeæ*, *Pyroleæ*, *Vaccinieæ*, and the aberrant *Ericeæ*. Some species are common to both continents, such as *Pyrola picta*, *Monotropa Morisoniana*, *Bryanthus Stelleri*, *Cassiope tetragona*, and *Andromeda polifòlia*; the two last forming likewise part of the European Flora. Europe and Africa alone contain the normal *Ericeæ*,* well characterized by their persist-

* I ought, perhaps, to except the *Calluna vulgaris*, samples of which were contained in a collection of dried plants from Newfoundland, given to me by Mr Cormack, who assured me they had been collected in that country.

ent corolla, the maximum of which is at the Cape of Good Hope, a spot where so many families of plants are found huddled together in strange confusion, as if Nature had at length deprived herself of sufficient space for their more equal distribution. The most easterly point to which this last group extends is the Mauritius, where the various species of *Salaxis* are found. The maximum of *Rhodoreæ*, *Vaccinieæ*, *Pyroleæ*, *Monotropeæ*, and the aberrant *Ericeæ*, is found in North America; these tribes, as I have before stated, being also common to Asia. Van Diemen's Land may be regarded as comprehending the majority of the *Epacrideæ*. Of all the genera of *Ericaceæ*, that of *Gualtheria* is, however, the most extensively diffused, being met with in almost every region of America, in New Zealand, Van Diemen's Land, and other islands of the South Pacific, and in the East Indies. The greater development of the calyx in this genus, and its more or less adherence to the ovarium, considerably lessens the importance of the discriminating character of the *Vaccinieæ*, and most satisfactorily shews that they constitute but a group of the *Ericaceæ*, rather than a distinct order. As happens in other very natural families, the characters of the generic groups in the *Ericaceæ* are not so strongly marked as in those that are less so; but we are not on that account to give up the idea of dividing them, and to retain three or four hundred species in one genus, as has been done in the case of *Erica*, which I have here attempted to subdivide into a number of minor groups; and, whatever opinion may be formed of their title to rank as separate genera, the arrangement of the species will, I trust, be found more natural than any hitherto proposed.

The examination of this interesting family was undertaken with the view of assisting my brother in the laborious undertaking * in which he is now engaged; and as a complete account of the species will appear in the forthcoming volume of that work, I have omitted most of them in the following pages, as they would have extended the present paper beyond the limits admissible in a periodical journal.

* General System of Gardening and Botany. By George Don, F.L.S. Vols. 1. & 2. London, 1831–32. 4to.

ERICACEÆ.

FLORES hermaphroditi, subsymmetrici, regulares.

CALYX 4- v. 5-divisus.

COROLLA rariùs 5-partita.

STAMINA definita, corollæ laciniis alterna, insertione variâ.

STYLUS et STIGMA indivisa.

CAPSULA libera, v. calyce adhærenti aucto carnoso baccata: *loculis* plerumque polyspermis.

SEMINA albumine carnoso.

EMBRYO erectus, axilis.

Plantæ (per terrarum orbem ubique sparsæ) polymorphæ, plerumque fruticosæ.

Obs.—Ordo in phalangibus sex sequentibus optimè dispositus.

Tribus 1. ERICEÆ. Antheræ biloculares. Ovarium liberum. Discus hypogynus, nectariferus, nunc rarò squamis ornatus. Gemmatio nuda. Folia sæpiùs margine revoluta.

2. RHODOREÆ. Antheræ biloculares. Ovarium liberum. Discus hypogynus, nectariferus. Gemmatio squamis imbricata, strobilina. Folia plana, costâ extremitate callosâ.

3. VACCINIEÆ. Antheræ biloculares. Ovarium adhærens. Discus perigynus, nectariferus. Fructus baccatus. Gemmatio nuda.

4. PYROLEÆ. Antheræ biloculares. Ovarium liberum. Discus hypogynus, nudus. Semina peltata, samaroidea. Embryo dicotyledoneus. Plantæ foliatæ, terrestres.

5. MONOTROPEÆ. Antheræ uniloculares. Ovarium liberum. Discus hypogynus, nudus. Semina peltata. Embryo indivisus. Herbæ aphyllæ, parasiticæ.

6. EPACRIDEÆ. Antheræ simplices, uniloculares, longitudinaliter dehiscentes. Ovarium liberum. Discus hypogynus, sæpiùs lobatus v. squamis 4 v. 5 ornatus. Folia plana.

Subtrib. 1.—Corolla Persistens. Ericeæ normales.

Gen. 1. ERICA. *Calyx* 4-partitus, basi nudus. *Corolla* globosa v. urceolaris, limbo 4-loba. *Stamina* inclusa: *filamenta* capillaria: *antheræ* bifidæ: loculis abbreviatis, foramine oblongo hiantibus, basi aristatis v. cristatis, rarò muticis. *Stigma* peltatum. *Capsula* 4-locularis, polysperma. Frutices (Europ. et Africæ) *foliis sparsis v. verticillatis, acerosis.* Flores *terminales, fasciculati v. racemosi.* Pedicelli *squamati.*

Typus. E. cinerea, *L.*

* *Antheræ basi aristatæ v. cristatæ.* Species normales.

2. Arborea; 3. pubescens; 4. persoluta; 5. articularis; 6. obesa; 7. australis; 8. physodes; 9. absinthoides; 10. guttæflora; 11. gracilis; 12. regerminans.

** *Antheræ basi muticæ.* Sp. aberrantes.

13. Ciliaris; 14. glutinosa; 15. cerinthoides.

2. GYPSOCALLIS. *Calyx* 4-partitus, glumaceus, basi nudus. *Corolla* campanulata v. breviter tubulosa, ore dilatata, 4-loba. *Stamina* exserta: *filamenta* complanata: *antheræ* bipartitæ: *loculis* basi muticis, distinctis, substipitatis! foramine obliquo hiantibus. *Stigma* simplex. *Capsula* 4-locularis, polysperma.

Fruticuli (Europ. et Africæ) *foliis subverticillatis, acerosis.* Flores *laterales v. terminales conferti.*

Typus. G. vagans, *Salisb.* (E. vagans, *L.*)

* *Antheræ basi omninò muticæ.* Sp. normales.

2. Multiflora; 3. purpurascens; 4. carnea; 5. mediterranea; 6. manipuliflora; 7. umbellata; 8. nudiflora.

** *Antheræ basi corniculatæ.* Sp. aberrantes.

9. Nigrita.

3. PACHYSA. *Calyx* profundè 4-partitus, coriaceus. *Corolla* subglobosa, coriacea: *ore* coarctato, 4-lobo. *Stamina* inclusa: *filamenta* valdè dilatata: *antheræ* bifidæ; *loculis* abbreviatis, basi cristatis, foramine obliquo hiantibus. *Stylus* basi dilatatus. *Stigma* simplex, obtusum. *Discus hypogynus* elevatus. *Capsula* 4-locularis, polysperma.

Fruticuli (capenses) *erecti.* Folia *laxè imbricata, compressa.* Flores *terminales, subcorymbosi, pedicellis bracteolatis.*

Etymol. Παχυς, crassus. Corolla substantiâ crassa.

Typus. P. ardens (E. ardens, *Andr.*) 2. baccans; 3. vernix.

4. CERAMIA. *Calyx* 4-partitus, glumaceus. *Corolla* urceolaris, limbo 4-dentata. *Stamina* inclusa: *filamenta* dilatata, plana: *antheræ* bifidæ: *loculis* abbreviatis, basi corniculatis. *Stigma* capitatum. *Capsula* 4-locularis, polysperma.

Fruticuli (capenses) *erecti.* Folia *sparsa, obtusiuscula, plana, subtùs glauca.* Flores *terminales, subcorymbosi.*

Etymol. Κεραμιον, urceolus. Corolla urceolaris.

Typus. C. urceolaris (E. urceolaris, *Soland.*) 2. marifolia.

5. DESMIA. *Calyx* 4-dentatus. *Corolla* globosa, ore coarctato, 4-dentato. *Stamina* exserta: *filamenta* complanata: *antheræ loculis* abbreviatis, foramine oblongo hiantibus, basi omninò simplicibus, in filamentum confluentibus. *Stigma* capitatum. *Capsula* 4-locularis, polysperma. *Semina* scrobiculata.

Fruticuli (capenses) *erecti.* Folia *sparsa, patula, subulata.* Flores *terminales, glomerati.*

Etymol. Δεσμη, fasciculus. Flores glomerati.

Typus. D. conferta. (E. conferta, *Andr.*)

1. D. *conferta*, umbellis sessilibus fasciculatis, filamentis angustis.
   Erica conferta. *Andr. Heath. v.* 2.

2. D. *æqualis*, umbellis pedunculatis aggregatis, filamentis dilatatis.

3. D. *polifolia*, foliis ternis aristatis, floribus fasciculatis, corollis oblongis fauce dilatatis, filamentis dilatatis, stigmate subsimplici.
   Erica polifolia. *Salisb. in Herb. Lamb.*

6. EURYLEPIS. *Calyx* 4-partitus, coriaceus, basi bibracteolatus. *Corolla* tubulosa, coriacea, basi ventricosa, limbo erecta, 4-fida. *Stamina* inclusa: *filamenta* dilatata, canaliculata: *antheræ* bipartitæ: *loculis* coriaceis, foramine oblongo hiantibus, basi auriculatis. *Stigma* clavatum, disco 4-tuberculatum, annulo crenulato. *Capsula* 4-locularis, polysperma. *Placentæ segmentis* bilobis. *Semina* ovalia, ventricosa.

Fruticuli (capenses) *diffusè ramosissimi.* Folia *sparsa, margine revoluta.* Flores *terminales, solitarii, magni.*

Etymol. Ευρυς, latus, et λεπις, squama. Squamæ calycinæ dilatatæ.

Typus. E. Halicacaba (E. Halicacaba, *L.*)

* *Antheræ basi muticæ.* Sp. normales.

2. Thunbergii; 3. albens; 4. sexfaria (stigma obtusum); 5. triflora.

** *Antheræ basi aristatæ.* Sp. aberrantes.

6. Massoni.

7. EURYSTEGIA. *Calyx* 4-partitus, amplus, glumaceus. *Corolla* urceolata, ore coarctato, 4-dentato. *Stamina* inclusa: *filamenta* dilatata, complanata: *antheræ* bipartitæ, foramine oblongo hiantes, basi biappendiculatæ: *appendiculis* complanatis, decurrentibus, cristatis, erosè crenatis. *Stigma* capitatum. *Capsula* 4-locularis, polysperma.

Fruticuli (capenses) *densè ramosi.* Folia *laxa, subulata, margine revoluta.* Flores *subsolitarii, penduli, albi v. rosei.*

Etymol. Ευρυς, latus, et στεγη, tectum. Calyx amplus.

Typus. E. glauca (E. glauca, *Andr.*) 2. pomifera; 3. andromediflora.

8. LOPHANDRA. *Calyx* 4-partitus, basi 4-bracteolatus: *segmentis* scariosis, rotundatis, extùs ventricosis. *Corolla* campanulata, 4-loba. *Stamina* inclusa: *filamenta* dilatata, complanata: *antheræ* bifidæ: *loculis* apice rostratis, medio foramine oblongo hiantibus, lateribus alatis, cristatis, crenulatis. *Stigma* truncatum. *Capsula* 4-locularis, polysperma.

Fruticuli (capenses) *erecti, ramosissimi.* Folia *patentia, brevia, obtusa, glauca.* Flores *terminales, subterni, rosei.*

Etymol. Λοφος, crista, et ανηρ, ανδρος, mas. Antherarum loculi cristati.

Typus. L. pyramidalis (E. pyramidalis, *Andr.*); 2. cubica.

9. LAMPROTIS. *Calyx* 4-partitus, amplus, glumaceus, coloratus, basi bibracteatus. *Corolla* urceolata: *limbo* parvo, 4-lobo. *Stamina* inclusa: *filamenta* capillaria: *antheræ loculis* abbreviatis, longitudinaliter dehiscentibus, basi muticis v. cristatis. *Stigma* capitatum. *Capsula* 4-locularis, polysperma. *Semina* subrotunda, scrobiculata.

Fruticuli (capenses) *ramosissimi.* Folia *opposita, adpressa, subulata, glabra.* Flores *copiosi, terminales, subsolitari.*

Etymol. Λαμπροτης, splendor. Calyx nitidissimus.

Typus. L. calycina (E. calycina, *L.*) 2. lutea; 3. tenuifolia; 4. taxifolia.

10. CALLISTA. *Calyx* 4-partitus, foliaceus. *Corolla* hypocrateriformis: *limbo* dilatato, patenti, 4-fido. *Stamina* inclusa: *filamenta* capillaria: *antheræ loculis* abbreviatis, longitudinaliter dehiscentibus, basi muticis. *Stigma* capitatum. *Capsula* 4-locularis, polysperma.

Fruticuli (capenses) *ramosissimi.* Folia *acerosa, laxè imbricata.* Flores *terminales, subsolitarii v. plures fasciculati.*

Etymol. Καλλιστος, pulcherrimus.

Typus. C. pellucida (E. Walkeri, *Andr.*). 2. denticulata; 3. fragrans; 4. comosa; 5. ventricosa.

11. EURYLOMA. *Calyx* 4-partitus, foliaceus. *Corolla* hypocrateriformis: *tubo* elongato, filiformi, v. ventricoso: *limbo* 4-partito, dilatato. *Stamina* inclusa: *filamenta* dilatata, membranacea, canaliculata: *antheræ* bipartitæ: *loculis* membranceis, longitudinaliter dehiscentibus, basi in calcar breve tumidum productis. *Stigma* disco elevato, 4-lobo. *Capsula* 4-locularis, polysperma.

Frutices (capenses) *diffusè ramosissimi.* Folia *adpressa, semicylindrica, peripheriâ minutè denticulata.* Flores *terminales, solitarii v. terni, breviter pedunculati, magni, speciosi.*

Etymol. Ευρυς, latus, λωμα, margo. Corollæ limbus dilatatus.

Typus. E. Aitoni (E. Aitoni, *Willd.*) 2. jasminiflorum.

12. CHONA. *Calyx* 4-partitus, foliaceus. *Corolla* infundibuliformis, limbo 4-loba, revoluta. *Stamina* exserta: *filamenta* capillaria: *antheræ* truncatæ: *loculis* elongatis, parallelis, basi aristatis. *Stigma* simplex, obtusum. *Capsula* 4-locularis, polysperma.

Fruticulus (capensis) *diffusus.* Folia *terna, linearia, aristata, margine revoluta, setoso-ciliata.* Flores *terminales, corymbosi, sanguinei.*

Etymol. Χωνη, infundibulum, ob corollæ figuram.

Typus. C. sanguinea.

13. SYRINGODEA. *Calyx* 4-phyllus, glumaceus. *Corolla* longè tubulosa, limbo brevi, 4-lobo. *Stamina* plerumque inclusa: *filamenta* capillaria: *antheræ* bipartitæ: *loculis* abbreviatis, obtusis, basi muticis v. aristatis, foramine oblongo hiantibus. *Stigma* simplex v. capitatum, in *aliis* annulatum, disco elevato. *Capsula* 4-locularis, polysperma. *Semina* ovalia, compressa, lævia.

Frutices (capenses) *erecti.* Folia *laxa, acerosa.* Flores *magni, speciosi, in ramulorum apicibus conferti, undique versi, subspicati.*

Etymol. Συριγξ, fistula, ob corollam longè tubulosam.

Typus. S. vestita (E. vestita, *Thunb.*)

* *Antheræ basi muticæ.* Sp. normales.

2. longifolia; 3. coccinea; 4. filamentosa; 5. sessiliflora; 6. phylicifolia; 7. versicolor; 8. bicolor; 9. Linnæana.

* * *Antheræ basi aristatæ.* Sp. aberrantes.

10. cruenta; 11. coronata; 12. abietina; 13. Leæana.

14. DASYANTHES. *Calyx* 4-partitus, basi bibracteolatus. *Corolla* tubulosa, hispida: *limbo* erecto, 4-lobo. *Stamina* inclusa: *filamenta* capillaria: *antheræ* bipartitæ: *loculis* basi muticis, longitudinaliter dehiscentibus. *Stigma* amplum, peltatum. *Capsula* 4-locularis, polysperma.

Fruticulus (capensis) *erectus.* Folia *laxè imbricata, setoso-hispida, margine revoluta.* Flores *terminales, fasiculati, lutei.*

Etymol. Δασυς, pilosus, et ανθος, flos. Corolla pilosa.

Typus. D. Sparrmanni (E. Sparrmanni, *L.*)

15. ECTASIS. *Calyx* 4-phyllus, glumaceus. *Corolla* tubulosa, basi paululùm ventricosa, limbo 4-dentata. *Stamina* longè exserta : *filamenta* valdè dilatata : *antheræ* bipartitæ : *loculis* elongatis, tubulosis, fissurâ longitudinali dehiscentibus, basi in filamentum omninò continuis ! muticis. *Stigma* clavatum, truncatum. *Capsula* 4-locularis, polysperma. *Semina* ovata, compressa, lævia, nitida.

Frutices (capenses) *ramosissimi.* Folia *laxè imbricata, margine revoluta, supra plana.* Flores *terminales, solitarii v. plures, laterales.*

Etymol. Εκτασις, extensio. Stamina longè exserta.

Typus. E. Plukenetii (E. Plukenetii, *L.*)

* *Floribus lateralibus, calycibus ebracteatis.* Sp. normales.

2. Petiverii ; 3. bruniades.

* * *Floribus, terminalibus, calycibus squamis pluribus basi arctè imbricatis.* Sp. aberrantes.

4. Banksiana ; 5. Sebana ; 6. imbricata.

16. ERIODESMIA. *Calyx* amplus, 4-partitus, basi bibracteatus. *Corolla* campanulata : *limbo* 4-lobo, revoluto. *Stamina* exserta : *filamenta* dilatata, complanata : *antheræ* bifidæ, obtusæ, undique papilloso-scabræ : *loculis* foramine oblongo dehiscentibus, basi in filamentum continuis ! *Stigma* capitatum. *Capsula* 4-locularis, polysperma. *Semina* angulata, nitida.

Fruticulus (capensis) *diffusè ramosissimus.* Folia *terna, obtusa, pilosissima.* Flores *terminales, solitarii v. terni, globosi, capituli hirsutissimi instar.*

Etymol. Εριον, lana, et δεσμη, fasciculus, atque ad florum similitudinem refert nomen.

Typus. E. capitata (E. capitata, *L.*)

17. OCTOPERA. *Calyx* 4-partitus, reflexus, basi nudus. *Corolla* globosa : *ore* coarctato, obtusè 4-lobo. *Stamina* inclusa : *filamenta* complanata : *antheræ loculis* brevissimis, foramine amplo hiantibus, basi appendiculâ solitariâ lanceolatâ acuminatâ auctis. *Stigma* peltatum. *Capsula* 8-locularis ! loculis polyspermis.

Fruticulus (capensis) *procumbens, pubescens, foliis verticillatis, floribus terminalibus subumbellatis, pedicellis squamatis.*

Etymol. Οκτω, octo, et πηρα, saccus, ob capsulam octolocularem.

Typus. O. Bergiana (E. Bergiana, *L.*)

18. EREMIA. *Calyx* 4-partitus, basi bracteis imbricatus : *segmentis* latè orbiculatis, ciliatis, coriaceis. *Corolla* urceolaris : limbo parvo, 4-lobo. *Stamina* inclusa : *filamenta* capillaria : *antheræ* bipartitæ : *loculis* abbreviatis, basi muticis, foramine oblongo hiantibus. *Stigma* capitatum. *Capsula* 4-locularis : *loculis* monospermis ! *Semina* grandiuscula, elliptica, ventricosa.

Fruticulus (capensis) *diffusè ramosissimus.* Folia *patentia, undique hispidè setosa.* Flores *glomerati.*

Etymol. Ερημος, solitarius, ob semina in quoque loculo solitaria.

Typus. E. Totta (E. Totta, *Thunb.*)

19. SALAXIS. *Calyx* 4-phyllus, irregularis. *Corolla* campanulata, 4-fida. *Stigma* peltatum. *Capsula* drupacea, 3-locularis, 3-sperma.

Frutices (mauritiani) *foliis ternis subsenisve margine revolutis, floribus in apice ramulorum subracemosis.*

Typus. S. arborescens, *Willd.*

Obs. Genus distinctissimum a cl. Salisburio primùm conditum fuit.

20. CALLUNA. *Calyx* 4-partitus, membranaceus, coloratus, basi 4-bracteolatus. *Corolla* calyce brevior, campanulata, 4-loba. *Stamina* inclusa: *filamenta* dilatata: *antheræ* bipartitæ, basi biappendiculatæ: *loculis* mucronulatis, longitudinaliter dehiscentibus. *Stigma* capitatum. *Capsula* septicida dehiscens. *Semina* ovoidea, lævia.

Frutex (europæus). Folia *trigona, obtusa, brevissima, quadrifariàm imbricata, margine revoluta, basi sagittata.* Flores *terminales, spicato-racemosi.*

Typus. C. vulgaris, *Salisb.* (E. vulgaris, *L.*)

21. BLÆRIA. *Calyx* 4-partitus. *Corolla* brevis, tubulosa, limbo 4-fida. *Stamina* 4!: *filamenta* linearia, complanata, glabra: *antheræ* bipartitæ: *loculis* basi attenuatis, muticis, apice foramine oblongo hiantibus. *Stigma* simplex, obtusum. *Capsula* 4-locularis, polysperma.

Fruticuli (capenses) *ramosissimi.* Folia *verticillata, margine revoluta.* Flores *terminales glomerati.*

Typus. B. ericoides, *L.*

Obs. 1. Genus Gypsocallidi affine, sed abundè differt staminum seriei interioris defectu. Semina matura in pluribus adhùc inquirenda.

Obs. 2. In Herbario Lambertiano plantæ hujus tribus maximè singularis conservatum est exemplar, in Promontorio Bonæ Spei a Johanne Roxburgh lectum, cui calyx 4-phyllus, corolla altè 4-partita, ferè 4-petala, filamenta capillaria, antheræ bifidæ (loculis abbreviatis basi muticis), stigma capitatum, capsula 4-locularis, semina compressa, caulis erectus, folia conferta subulata obtusiuscula suprà planiuscula, flores terminales subsessiles glomerati. Anne genus sit distinctum, vel potiùs alicujus speciei descripti varietatem singularem?

Subtrib. 2.—Corolla decidua. Andromedeæ.

22. ANDROMEDA. *Calyx* 5-fidus: *laciniis* acutis, basi simplicibus. *Corolla* globosa: *ore* coarctato, 5-dentato. *Stamina* 10, inclusa: *filamenta* barbata: *antheræ loculis* abbreviatis, uniaristatis. *Stigma* truncatum. *Capsula* loculicido-dehiscens. *Placenta* 5-loba: *lobis* simplicibus. *Semina* elliptica, compressa, nitidissima, hilo lineari laterali.

Fruticulus (Europ. Asiæ et Amer. boreal.) Folia *lineari-lanceolata, mucronulata, margine magis minùsve revoluta, integerrima, subtùs glauca, costâ elevatâ, venulisque reticulatis.* Petioli *brevissimi, callosi.* Flores *terminales, umbellati, pulcherrimi, rubicundi v. nivei, bracteis ovatis semifoliaceis imbricatis muniti.*

Typus. A. polifolia, *L.*

23. CASSIOPE. *Calyx* 5-phyllus: *foliolis* basi imbricatis. *Corolla* campanulata, 5-fida. *Stamina* 10, inclusa: *filamenta* glabra: *antheræ loculis* ab-

breviatis, tumidis, uniaristatis. *Stylus* basi dilatatus. *Stigma* obtusum. *Capsula* loculicido-dehiscens: *valvis* apice bifidis. *Placenta* 5-loba: *lobis* simplicibus. *Semina* oblonga, compressa, nitida.

Fruticuli (Europ. Asiæ et Amer. boreal.) *ericoides.* Folia *parva, imbricata.* Flores *solitarii, pedunculati, rosei, laterales v. terminales.*

Etymol. Cassiope Andromedæ mater.

Typus. C. tetragona (Andromeda tetragona, *L.*)

* *Foliis planis.*

1. C. *hypnoides,* foliis acerosis laxis.
   Andromeda hypnoides, *L. Pall. fl. ross. p.* 55. *t.* 73. *f.* 2.
2. C. *lycopodioides,* foliis ovatis adpressis quadrifariàm imbricatis.
   Andromeda lycopodioides, *L. Pall. l. c. p.* 55. *t.* 73. *f.* 1.

** *Foliis adpressè imbricatis, margine revolutis, tumidis, subbilocularibus.*

3. C. *tetragona,* foliis obtusis muticis periphæriâ minutè ciliatis, pedunculis glabris.
   Andromeda tetragona, *L. Pall. l. c. p.* 56. *t.* 73. *f.* 4.
4. C. *ericoides,* foliis aristatis periphæriâ setoso-ciliatis, pedunculis glabris.
   Andromeda ericoides. *Pall. l. c. p.* 56. *t.* 73. *f.* 3.
5. C. *fastigiata,* foliorum periphæriâ apiceque elongato scarioso-membranaceis, pedunculis lanatis.
   Andromeda fastigiata. *Wall. pl. asiat. rar.* 3. *t.* 284.

24. CASSANDRA. *Calyx* 5-phyllus, basi bibracteolatus: *foliolis* basi imbricatis. *Corolla* oblonga: *ore* coarctato, 5-dentato. *Stamina* 10, inclusa: *filamenta* glabra, basi simplicia: *antheræ loculis* apice elongatis, tubulosis, muticis. *Stigma* annulatum, disco 5-tuberculatum. *Capsula* loculicido-dehiscens. *Placenta* 5-loba: *lobis* simplicibus.

Frutex (Amer. Europ. et Asiæ borealibus communis) *sempervirens:* ramulis *recurvatis, pubescentibus.* Folia *brevissimè petiolata, elliptico-oblonga, denticulata, coriacea, avenia, utrinque squamulis peltatis lepidota!* juniora *subtùs argentea.* Flores *axillares, in ramulorum apicibus racemi modo dispositi, brevissimè pedicellati, cernui, nivei.*

Etymol. Nomen poeticum. Cassandra Priami et Hecubæ filia.

Typus. C. calyculata (A. calyculata, *L.*)

25. ZENOBIA. *Calyx* 5-dentatus. *Corolla* campanulata: *limbo* revoluto, 5-lobo. *Stamina* 10: *filamenta* brevissima, glabra, basi dilatata: *antheræ loculis* elongatis, tubulosis, apice biaristatis! *Stigma* truncatum. *Capsula* loculicido-dehiscens. *Placenta* 5-loba: *lobis* cuneatis, crassis, subarcuatis. *Semina* angulata, hilo oblongo, laterali.

Frutices (Amer. boreal.) *sempervirentes.* Folia *sparsa, dilatata, margine sæpè dentata.* Flores *racemosi, pedicellis solitariis v. aggregatis.*

Etymol. Zenobia Palmyrensis regina perillustris, virtute, doctrinâ infortunisque celeberrima.

Typus. Z. speciosa. (A. speciosa, *Mich.*)

26. LYONIA. *Calyx* 5-partitus. *Corolla* ovata v. tubulosa: *ore* coarctato, 5-dentato. *Stamina* inclusa: *filamenta* complanata, dilatata, brevissima, puberula: *antherarum loculis* membranaceis, longitudinaliter dehiscenti-

bus! omninò muticis. *Stylus* robustus, 5-gonus. *Stigma* simplex, obtusum. *Capsula* 5-gona, 5-locularis, loculicido-dehiscens: *valvularum marginibus* valvulâ externâ tectis! *Semina* acicularia.

Frutices (Amer. boreal.) *foliis sæpiùs membranaceis pubescentibus, floribus plerumque terminalibus racemoso-paniculatis.*

Typus. L. paniculata, *Nutt.* (A. paniculata, *L.*); 2. frondosa; 3. ferruginea; 4. racemosa; 5. mariana; 6. marginata; 7. arborea; 8. jamaicensis.

27. LEUCOTHÖE. *Calyx* 5-phyllus: *foliolis* basi imbricatis. *Corolla* tubulosa, 5-dentata. *Stamina* inclusa: *filamenta* dilatata, complanata, puberula: *antherarum loculis* abbreviatis, truncatis, muticis. *Stigma* amplum, capitatum. *Capsula* loculicido-dehiscens.

Frutices (Amer. boreal.) *sempervirentes.* Folia *coriacea, dentato-spinulosa.* Flores *racemosi, albi, axillares v. terminales.*

Etymol. Nomen poeticum.

Typus. L. axillaris (A. axillaris, *Soland.*); 2. floribunda.

28. PIERIS. *Calyx* altè 5-partitus. *Corolla* tubulosa v. ovata: *ore* coarctato, 5-dentato, revoluto. *Stamina* inclusa: *filamenta* dilatata, apice bisetosa! *antherarum loculis* abbreviatis, incumbentibus, longitudinaliter dehiscentibus. *Stylus* robustus, 5-gonus. *Stigma* truncatum. *Capsula* loculicido-dehiscens. *Semina* scobiformia.

Arbores *v.* frutices (nepalenses) *foliis coriaceis, floribus terminalibus racemosis.*

Etymol. Pieris una Musarum.

Typus. P. formosa (A. formosa, *Wall.*); 2. ovalifolia; 3. lanceolata.

29. PHYLLODOCE. *Calyx* 5-partitus. *Corolla* globosa: *ore* coarctato, 5-dentato. *Stamina* 10, inclusa: *filamenta* gracilia, glabra: *antheræ loculis* abbreviatis, truncatis, muticis. *Stigma* peltatum, 5-tuberculatum. *Capsula* 5-locularis, septicido-dehiscens. *Semina* compressa, nitida.

Fruticuli (Europæ, Asiæ et Americæ, regionibus borealibus communes), *sempervirentes.* Folia *linearia, obtusa, patula.* Flores *terminales, solitarii, v. plures aggregati, subumbellati.*

Typus. P. taxifolia, *Salisb.* (Andromeda cœrulea, *L.*)

1. P. *taxifolia*, foliis margine denticulatis, pedunculis aggregatis glandulosis, laciniis calycinis lanceolatis acuminatis, antheris filamentis ter brevioribus.

   Phyllodoce taxifolia. *Salisb. parad. t.* 36.

   Menziesia cœrulea. *Swartz in Linn. Trans.* 10. *p.* 377. *t.* 30. *f. a. Engl. bot. t.* 2469.

   Andromeda cœrulea. *Linn. sp. pl. p.* 563.

   A. taxifolia. *Pall. fl. ross. p.* 54. *t.* 72. *f.* 2.

   Erica cœrulea. *Willd. sp. pl.* 2. *p.* 393.

   In Europâ et Asiâ boreali. Ad Udæ fontes. D. Laxmann, ♄ (v. v. c. et s. sp.)

2. P. *Pallasiana*, foliis margine denticulatis, pedunculis aggregatis tomentosis, laciniis calycinis ovato-lanceolatis acutis membranaceis, corollis oblongis, antheris filamentis dimidio brevioribus.

   Andromeda cœrulea β. viridiflora. *Herb. Pall.*

   In Insulis Curilis. *Steller.* ♄ (v. s. sp. in Herb. Pallas. nunc in Mus. Lamb.)

Frutex erectus, rigidus, spithamæus. *Folia* præcedentis, sed breviora, margine copiosè denticulata. *Flores* in ramulorum apice numerosi, aggregati. *Pedunculi* breviores, ut et calyces, pilis sericeis ferrugineis adpressis undique copiosè vestiti. *Calycis laciniæ* ovato-lanceolatæ, acutæ, membranaceæ. *Corolla* oblonga, vix calyce longior, profundiùs 5-dentata. *Antheræ* apice truncatæ, biforaminulosæ, filamentis dimidio breviores.

3. P. *empetriformis*, foliis margine denticulatis, pedunculis aggregatis parcè glandulosis, calycis laciniis ovatis obtusis, antheris filamentorum longitudine.

Menziesia empetriformis. *Smith in Linn. Trans.* 10. *p.* 380 ; *Spr. syst.* 2. *p.* 202 ; *Bot. Mag. t.* 3176.

In Americâ boreali. ♄ (v. v. c. et s. sp. in Herb. Smith. et Lamb.)

30. BRYANTHUS. *Calyx* 5-phyllus, imbricatus. *Corolla* profundè 5-partita, patula. *Stamina* 10, corollâ breviora : *filamenta* complanata, glabra : *antheræ loculis* abbreviatis muticis v. posticè aristatis, foramine terminali dehiscentibus. *Stigma* obtusum. *Capsula* 5-locularis, septicido-dehiscens, polysperma. *Semina* ovoidea, nitida, raphe carinatâ.

Fruticuli (Asiæ et Amer. boreal.) *humifusi.* Folia *conferta, patentia, planiuscula.* Flores *terminales, solitarii v. subracemosi.*

Obs. Genus a Gmelino primùm conditum.

Typus. B. Gmelini.

1. B. *Gmelini*, ramulis pruinosis, foliis margine denticulatis, pedunculis plurifloris glandulosis, antheris muticis, stylo filiformi.

Menziesia Bryantha. *Swartz in Linn. Trans.* 10. *p.* 378. *t.* 30. *f. b.*
Andromeda Bryantha. *Linn. mant.* 238 ; *Pall. fl. ross. p.* 57. *t.* 74. *f.* 1.
Erica Bryantha. *Thunb. diss. n.* 8 ; *Willd. sp. pl.* 2. *p.* 386.
Bryanthus repens, serpyllifolio, flore roseo. *Gmel. sib.* 4. *p.* 133. *t.* 57. *f.* 3.

In Kamtschatkâ, circa portum Ochotensem, et in Insulâ Beringii. *Steller.* ♄ (v. s. sp. in Herb. Pallas. nunc in Mus. Lamb.)

2. B. *Stelleri*, ramulis glabris, foliis margine obsoletè crenulatis, floribus solitariis subsessilibus, antheris posticè biaristatis, stylo conico.

Andromeda Stelleriana. *Pall. l. c. p.* 58. *t.* 74. *f.* 2. (bona).
Menziesia empetriformis. *Pursh, fl. amer.* 1. *p.* 265, nec *aliorum.*

In plagâ occidentali Americæ borealis. *Menzies.* A cl. Stellero primùm detecta, sed de loco incertus sum.

31. DABŒCIA. *Calyx* 4-partitus. *Corolla* ovalis, ventricosa, limbo 4-dentato. *Stamina* 8, inclusa : *filamenta* dilatata, glabra : *antheræ* lineares, basi sagittatæ : *loculis* parallelis, apice solutis, longitudinaliter dehiscentibus. *Stigma* simplex, truncatum. *Capsula* 4-locularis, septicido-dehiscens.

Fruticulus (Hiberniæ et Pyrenæor.) *sempervirens.* Folia *elliptica, plana ! subtùs niveo-tomentosa.* Flores *terminales, racemosi, purpurei.*

Typus. D. polifolia. (Andromeda Dabœcii, *L.*)

Obs. Menziesia ferruginea et globularis genus omninò diversum constituunt et ad Rhodoreas referendum.

*On Malaria.**

An English naturalist, Dr MacCulloch, maintains that plants, and also water, give out the malaria as a peculiar poison; and that this matter can be transported, and consequently that the sickness it causes may be produced in districts where there are no plants, or where, after the harvest, there is only stubble remaining. If, in considering this subject, we place together the marsh fever and the real malaria, we find that all the assertions made regarding them turn upon this view, that putrid marshes render impure the air which previously did not contain the contagious poisonous ingredient which produces these diseases. As for several centuries the malaria has actually prevailed to a great extent in Rome during the summer season, and as, in later periods, quinine has been employed as a remedy, the quantity of that medicine consumed has been taken into consideration, and the conclusion has been drawn that Rome is becoming more marshy every year. Those who saw the absurdity of this opinion, for there is no district more dry than that of Rome, had recourse to the Pontine marshes. But as it was opposed to this view of the subject, that these marshes are two days' journey from Rome, and that several healthy towns and a ridge of hills intervene, it was found necessary to maintain that the malaria reached its destination by a circuitous route; then it was thought that the Tiber, by the diminution of its waters caused by the heats of summer, must become impure, which, however, is by no means the case. The idea of forming another bed for the Tiber, had no other ground than that which has

* The guide books for Italy carefully point out suspected districts, and enjoin travellers to drive rapidly through them, and not even to close an eye, and much less to sleep, in them. The clamour upon this subject is still greater near such places, but it has its origin chiefly in the interested motives of innkeepers. The Neapolitan scientific men gave me whole lists of suspected places, all which, however, lie near the great roads. About districts in the interior I could learn nothing; and I was unable to obtain more minute or solid information upon the subject, although I gave myself much trouble for this purpose.

been so often renewed since the time of Cæsar, to conduct it through the Pontine marshes, and so to remove it to a distance. The opinion has now become prevalent, that the best remedy for the evil is the growth of plants.

Whether the Pontine marshes had their origin after the time of Tarquinius Superbus, by the sinking of the land and destruction of the twenty-three towns caused by an earthquake, is of no consequence for our purpose. The attempts at improvement made by Appius Claudius, Julius Cæsar, Augustus, and Trajan, afford no important facts. When Decius, under the Gothic viceroy Theodoric, undertook the draining, many workmen became ill and died, so that it was found necessary to give up the work several times. The same happened under Pius the Fifth, in 1585.

When, in the midst of the difficulties which oppressed his territories, Pius the Sixth began the work with the greatest vigour, new epidemic disorders broke out amongst the labourers. Many died speedily, and others recovered as rapidly. A large part of the district was drained, and in this respect there was cause for triumph. Pius founded there a considerable colony, with a parish church and a capuchin monastery. Of these marsh diseases, no further trace shewed itself; but, on the other hand, the real malaria appeared when the ground was thoroughly drained. Many of the colonists and capuchins gradually became pale, almost lost the power of speech, acquired a corpse-like aspect, and at length died. Thus the whole colony was in a short period destroyed. The handsome church has been converted into the hay magazine of the present post-house. When the crops are cut, the proprietor of the post-house and his family remove to a more healthy situation; and, paying high wages, give the establishment in charge to their servants, of whom some always fall a sacrifice to the malaria. I saw two such individuals; I took the cook with me in my carriage to Rome, where he found aid in the hospital. He had not been at all aware that the disease had seized him, and remained nearly two years in service, until his voice became as weak as that of a child, and he was seized with a cough, accompanied by symptoms of great debility, under which in all probability he sunk. On the other hand, several houses stand in the middle of the marsh; and I learned that the real malaria had never appeared amongst the people who inhabited them, but that violent fevers are of frequent occurrence.

It is well known that marshy districts, abounding in putrid water, are very unhealthy, and that in these intermitting fevers become epidemic; so that the ships from Manfredonia, and some other places on the Adriatic Sea, are obliged to serve quarantine on arriving at other ports. I saw marshy districts of this description at Basiento; in the province of Otranto; at Crati; near Miastro, Catania, Lentini, Agosta, &c. The real malaria tracts of country are characterised by entirely different features. In the year 1669, four villages were overflowed by the lava which issued from the eruption of the M. Rossi on Etna. The inhabitants built a new village on a beautiful height between Paterno and Motta. To the east and south basalt may be observed, and to the north we meet with the old seas of lava of Etna; but the chief rock is a vesicular basaltic tuffa. The village was called Castellino, and is now entirely deserted; and the houses appear as if they had suffered from fire. Though excessively dry, the district abounds in rich fields of corn. At a greater distance round this remarkable elevated point, there is no marsh or other similar collection of water. The first year the malaria showed itself after the harvest, and under its influence the whole colony suffered; so that at last not an individual lived on the spot, while the neighbourhood was abundantly inhabited. Notwithstanding this, however, a few years ago, a rich inhabitant of Palermo built in that very district some farm-houses, and a beautiful country house. The farmers unfortunately heard the fate of the colony, and again every thing was deserted, and the wood of the houses carried away. One of the least moist districts is that round the village of Fiorida, which lies on an extensive platform composed of basaltic tuffa, and, like all other malaria tracts, is characterised by the absence of vegetation. Soon after leaving Syracuse no more trees are to be seen. In the long narrow limestone valley bushes are to be met with, but when the height is reached all vegetation disappears after the harvest; there is nothing but dried up corn fields, in which nothing thrives amongst the stubble. The village, which contains the only houses of the district, is visible at a distance of fifty Italian miles; but in the upper and more beautiful part of it all the houses, and in the remainder many of them, were deserted. When the harvest is over, the richer part of the population remove, and again take possession of their property in autumn. As I several times wandered through the village, I saw a number of children from five to twelve years old creeping about, or lying, corpse-like, in the burning rays of the sun. Amongst the grown up there are fewer who are attacked by malaria; but still, at church, I saw a considerable number. Those who have passed their fourteenth year are less liable to the disease; but many are seized by it, and especially, as is asserted, every newly arrived stranger. At the inn there were three children and a person advanced in life, who had been ill for several years, and were apparently near dissolution. I saw several similar districts in Sicily, and particularly between Caltanisella and Sulera, in the centre of the country. In Calabria, Cosenza is especially notorious, and is deserted in summer by nearly all the respectable inhabitants; and it was there where I saw the fear for the disease carried to the greatest extent. Neapolitans and foreigners ascribe the malaria to the marshy districts; and yet Cosenza is a very dry place. There are certainly two rivulets which unite below the town, but these run exceedingly rapidly over the pebbles. It is only at a distance of many miles, and again where Crati approaches the ancient Sibaris, that the ground becomes marshy, but still not to such an extent as to produce fevers. Besides, malaria does not exist in a single place in this valley of seventy miles in length, except at Cosenza, its highest point. To the west of Cosenza there are conical hills of granite and gneiss; to the south, there are extensive strata of sandstone, under which appear fragments of limestone beds, and traces of basaltic tuffa and mud-like masses; to the east, there are traces of old mud volcanos, and tuffaceous tower-shaped masses; and to the north begins the highest part of the valley, which descends for two days' journey, and then terminates near ancient Sibaris. The valley of the Negro presents similar features; the river flows into the Silaro, and in the lower region renders marshy the district of Palla, and makes that of Basizza so noxious, while in the highest and driest part, at the source of the river, and farther down on a hill at La Palla, the malaria appears. When in the province Basilicata, we advance from Molitano to the river district of Agri, we meet with large gypsum craters, and more towards the Tarentine sea, white marly mud-like masses, which, near Craco and Ilice, form innumerable mud-hills or rather cones, which in summer resemble the ordinary soil, but in winter become mud volcanos. In the most fruitful part of that district, which is now an extraordinary desert, for there is nothing human to be seen for a whole day's journey, I found an old wall with an inscription announcing that here *Carolus dei gratia Hispaniorum Romanorum et Neapolitarum Imperator* had built a church in 1729, in order to afford religious consolation to the inhabitants. But the malaria and time have destroyed the church and the other buildings. During my geognostical observations there, my servant, who was an exceedingly strong and healthy individual, was attacked by the malaria, and, according to the last letter I received, is likely to sink under it. Rome seems to have much analogy to the district I have described. The whole region is composed of basaltic tuffa, which is continued as far as Naples, and there unites with the pasilipo tuffa of the Pflegrean fields. To the north this formation still continues; and, as at Basilicata, terminates with mud deposits. Over this whole extent of country basalt or lava masses are frequent, and volcanic products are accumulated in hills. Even the limestone of the Apennines has in some places suffered alterations. The neighbourhood of Rome is also very hilly, dry, and entirely without vegetation. For days together, one sees nothing but desolate dried up corn fields without trees, bushes, or wood of any description. In early times Rome was surrounded by extensive sacred woods, which were not suffered to be destroyed. At that period malaria was unknown, though intermitting fevers were well known in the Pontine marshes The avarice of the Popes, however, converted these sacred woods into gold, and so desolated the region that not a tree or wood are to be met with around Rome. With the commencement of this system of extirpation the malaria appeared; and has at length reached such a height that, yearly, many are carried gradually off by it; and in the summer months strangers and respectable inhabitants quit Rome, and thus the gigantic city is half depopulated.

From all my observations, which at present I have not time to develope, I believe I may deduce the following conclusions: First of all, it is necessary to distinguish malaria from marsh fevers; and this we may do, either by considering the form or the cause of the disease. To throw every thing together without further proof, is to give rise to uncertainty, and to form a chaos in the present state of our knowledge. When we take into consideration all the phenomena of marshy districts, the conclusion does not lie far distant, that the atmosphere is in different degrees rendered unfit for human organisation, not by the passage of the water to the air, but by the decomposition and solution of vegetable substances; that thus those various intermitting fevers, and even the plague itself, are produced by the Adriatic Sea; that the removal of those diseases, though they certainly are most frequent in summer, is not connected with any particular period; and that, consequently, though marshy regions are avoided, yet in these emigration does not invariably take place. In the case of real malaria, in opposition to marsh fevers, the circumstances are different. So long as the earth is covered with living vegetables, as for example with corn, the air of the most suspected district is pure and healthy, and no one fears being attacked by the disease; but when the prodigious crops, which in those volcanic, loose-soiled districts are speedily brought to maturity, are removed, does the surface of the earth become dead at the warmest and most energetic period of its functions? or does not rather a portion of those substances, which were consumed by the leaves and roots of plants, now go to the atmosphere and render it unfavourable for the breathing of man, until all is again restored to an equilibrium in higher or more distant regions. That carbonaceous matter is beneficial to the vegetable kingdom, is as well known as that it is prejudicial to the breathing process in animals. That in Rome the higher parts of the town, as the *Trinita del Monte*, the *Capitol*, &c. are free from malaria, while low-lying districts, as the *campo vaccino*, &c. are very dangerous, is certain, and confirms the view we have given. On the appearance of malaria the Pope leaves the low lying Vatican, and inhabits for a certain period a palace placed on a higher elevation. No educated person in Germany doubts the organic function of he earth, to which also the cholera itself has been ascribed; and when a more general regard to nature advances to the south, the sacred woods will again gradually surround Rome, large vine branches entwine themselves round the elms, the hills be thus again covered, and the malaria reduced within limits. The fact is not without interest, that all real malaria districts are of volcanic formation, and that they are often to be found at the boundary of volcanic and non-volcanic rocks. That the district of the Aderner sea was formerly exceedingly unhealthy is certain; and the same was the case with the *Monte Gauro* or *Barbaro,* where at that period the best wine was produced, but since the formation of the Monte Nuovo by a volcanic eruption of 1538, between the sea and the hill, the spot has become healthy; but, at the same time, since that event it has been found impossible to grow even tolerable wine in a place where such nectar was formerly obtained. On the other hand, it is known that it is only at a recent period that *Monte Fiascone* has produced its nectar. Whoever may make the malaria the subject of his investigation will find a host of facts which he may collect, and from them make out a history of this difficult and little known subject.

Observations on Ground-Ice. By the Rev. Mr Eisdale. (Communicated by the Author *.)

On the 28th of December 1831, I read a paper at the meeting of this Society, directing the attention of the members to a particular kind of ice, which seems to be formed in direct opposition to the ordinary laws of congelation. The ice to which I allude commences at the bottom of the water, and extends upwards to the surface, and it is produced only in the most rapid and most rugged streams. This is exactly the reverse of the usual process of congelation, which takes place in stagnant water, commencing at the sides of the river or pond, and gradually extending over the surface; when it thickens downwards towards the bottom, and if the frost is sufficiently intense, con-

* Read before the Philosophical Society of Perth, on the 28th Dec. 1831.

verts the whole water into a solid mass of ice. The phenomenon did not seem to have attracted any attention in this quarter; and the facts which I stated, and the speculations which I advanced, seemed to excite some surprise: the ice in question, however, is perfectly familiar to every person in this country, though I never had seen any attempt to account for its formation, and had long puzzled myself in vain to form any plausible theory on the subject. I was not ashamed to confess this at the time, and I expressly declared that the theory which I proposed was intended to elicit, rather than impart, information, and especially to direct attention to the *alleged* facts which had been communicated to me, and on which I founded my explanation of the process.

This kind of ice is well known in all northern climates, from its annoying effects in obstructing all works which are carried on by the impelling power of water. When ice collects on the surface of mill-leads it is easily managed: it needs only to be broken and floated down the stream: but when the ice of which I am speaking forms, the case is perfectly hopeless; the leads are gorged up from the very bottom, and it is in vain to attempt to remove the obstruction. This kind of ice is called in Germany *grund eis;* in France it is known by the name of *glace de fond*; and in the south of Scotland, it is called *lappered* ice, an epithet which the common people apply to the natural coagulation of milk. I am happy, however, that the phenomenon has now attracted the attention of some eminent philosophers, particularly of the celebrated Arago in France, who has been at great pains in collecting a variety of facts, and has proposed a theory for the explanation of the appearance, which I shall shew to be utterly inadequate for the purpose; and which, with the modesty that characterises genuine philosophy, he admits does not thoroughly satisfy himself.

I beg leave to call the attention of the meeting, for a moment, to the phenomenon itself. Every inhabitant of Perth who has witnessed the setting in of a severe frost, must have observed that before the *true* ice, as I may call it, has made much progress in advancing from the sides to the centre of the river, nearly the whole body of the stream above the bridge is occupied by large irregular masses of floating ice of very considerable thickness, far beyond any thing that could be effected by the natural operation of the frost in surface freezings. I believe it has seldom occurred to any observer to inquire how these masses of amorphous ice were formed: they all come down the river from a great distance; and being stopped, at last, by the flow of the tide, and closely compacted together, they are agglutinated by the frost, and present great obstacles to navigation. Now these masses are precisely the ice in question: they are formed in the most rugged currents, adhering to the projecting rocks and rough inequalities at the bottom, and increasing upwards, till their bulk and smaller specific gravity, as compared with water, enable the stream to tear them from their fastenings, and hurry them down the river.

I shall mention a few of the facts which M. Arago has collected on this subject; and it is curious enough to observe from his statement, that, what is perfectly well known to every peasant, is still called in question by the majority of the natural philosophers of France: they deny the existence of *ground-ice.*

M. Beaun, in 1788, wrote several dissertations chiefly to establish the existence of *ground-ice*, from observations made by himself and by the fishermen on the Elbe. He informs us, that the latter declared that the baskets which they let down into the river, for the purpose of catching eels, were often, when brought to the surface, incrusted with ice; that the anchors used for mooring their boats, when lost during the summer, again appeared in the following winter, being raised by the ascending force of the ice at the bottom, with which they had been covered to such an extent as to render them buoyant; and that this ground-ice often raised up the large stones to which the buoys were fastened by chains, and caused the greatest inconvenience by displacing these useful signals.

Desmarest, a member of the French Academy of Sciences, was among the first who made observations on the formation of *ground-ice;* but he advances no theory on the subject. He says he had seen flakes of this ice formed at the bottom of running streams, increasing to the thickness of five or six inches in a single night. A more extraordinary fact than this was communicated to myself about two years ago, when my first paper was announced in the newspapers. A miller, in the western part of the country, wrote me a letter containing a theory of his own, ascribing the phenomenon to the prevalence of particular winds; in confirmation of which he mentioned, that, during a severe frost, when his mill-lead was entirely free of any kind of ice, he had occasion one day to lop some branches from a tree which overhung the lead; one of them fell into the water and was left there, as he did not apprehend any consequences from such a trifling occurrence. Next day, however, to his astonishment, the water was turned entirely out of the lead, and had overflowed a large portion of an adjoining meadow. On proceeding to ascertain the cause, he found that a solid barrier of ice had been formed across the lead where the branch had fallen in, so as completely to prevent any water from passing, whilst the rest of the lead was free from ice. He ascribes this to the prevalence of a very sharp north-east wind which had blown during the night. There can be no doubt that this is converting into a cause, what is merely an accidental concomitant, as I shall shew hereafter.

On the 16th February 1827, M. Hugi, President of the Society of Natural History at Soleure, while standing on the bridge of the Aar, and when the river was perfectly clear of ice, observed in these circumstances, large icy tables continually rising from the bottom of the river, in a vertical direction, and with such buoyancy, as to rise considerably above the surface, when they immediately sunk into a horizontal position, and floated down the stream. A great many facts of the same kind may be found in M. Arago's paper, which is given in the Edinburgh New Philosophical Journal for July last; which is the first paper on the subject of ground-ice that I have ever seen.

Let us now attend for a little to the cause of these singular phenomena, and I will be bold to say that no adequate cause has yet been assigned for them; unless the hints which I formerly threw out on the subject as queries, rather than as ascertained facts, shall be considered sufficient for the purpose. M. Arago gives his theory as to the cause at great length. It is simply this, that the different strata of water, in a running and shallow stream, being all mixed together by the agitation caused by the inequalities of the bottom, are all cooled down during an intense frost to the freezing point, and that the stones there form proper points of attachment to facilitate the formation of icy crystals. This is, in fact, the same answer that was given to my theory, in some of the newspapers, two years ago; but it is altogether inadequate, for this plain reason, that, according to it, the phenomena of ground-ice ought to appear in every hard frost, when the water reaches the requisite temperature. But so far is this from being the case, that in the hardest frosts which we have ever seen, not a particle of ground-ice was found in the river. Take, for instance, the very severe frost of 1813-14, when the Tay was frozen over for many weeks, yet no ground-ice was to be seen. Some gentlemen present may remember to have skated down the stream and through below the arches of the bridge, whilst the ice every where was clear as crystal, and the bed of the stream entirely free from the white spongy ground-ice.

Some now present will probably recollect that the theory which I proposed, as a solution of these phenomena, was founded on information which I had received from country people, and others, whose operations depended on water-wheels, and whose interests forced them to attend to appearances, which might pass unheeded by others. The sum of their information was, that the ground-ice was never formed but after a heavy *rione*, or hoar-frost. If this is the fact, the explanation is obvious. The hoar-frost, which is congealed moisture, precipitated from the atmosphere, and falling into the river when the water is cooled down to the freezing-point, cannot be dissolved. It retains in the water the very shape in which it descends from the air. When these small crystals fall on a deep unfrozen pool, the water being above the freezing-point, the particles melt and are incorporated with the water; but in a shallow and agitated stream, almost the whole water is brought, in succession, into contact with the intense frost, and may thus be cooled down to the freezing-point to the very bottom of the stream, before even a pellicle of ice is formed on the stagnant pool. All the particles of hoar-frost, then, or frozen vapour which fall on such a stream will remain unmelted; and being tossed in all directions by the agitations of the current, will be brought into contact with the rocks, or other substances projecting from the bottom, to which they will readily adhere, and form a *nucleus* for that strange accumulation called *ground-ice*, which is found nowhere but in streams.

I would not have brought forward this theory a second time, had I not met with some facts collected by M. Arago, which afford the strongest confirmation of the theory which I had advanced, though he himself scarcely seems to have had a glimpse of their importance. He mentions an observation by Desmarest, that in a cloudy sky the ground-ice accumulates uniformly, but is interrupted when the sun shines. Now, what he calls a cloudy sky I conceive to be an atmosphere loaded with hoar-frost, and rendered hazy by its condensation; for I do not think it possible that a *genuine cloud* can exist in the atmosphere during a keen frost. Here, then, this observer furnishes a fact in perfect accordance with the information on which I proceeded, viz. that the ground-ice is formed only during a hazy state of the atmosphere, in other words, during a hoar-frost; whilst he tells us that the process was interrupted when the sky was clear.

But M. Arago quotes a passage from a paper of Mr Knight, the celebrated botanist, in the 106th vol. of the Phil. Trans., which brings the matter nearer, if not altogether, to a demonstration, though Mr Knight himself proposes no theory. The passage is as follows:—"In a morning which succeeded an intensely cold night, the stones in the rocky bed of the river appeared to be covered with frozen matter, which reflected a thread of silvery whiteness, and which, upon examination, I found to consist of numerous frozen *spicula* crossing each other in every direction, as in snow, but not having any where, except near the shore, assumed the state of firm compact ice. *The river was not at this time frozen over in any part*, but the temperature of the water was obviously at the freezing point, for small pieces of ice had every where formed upon it in its more stagnant parts near the shore; and upon a mill-pond, just above the shallow streams, in the bottom of which I had noticed *millions of little frozen spicula floating upon the water*. At the end of this mill-pond, the water fell over a low weir, and entered a narrow channel, where its course was obstructed by points of rock and large stones. By these, numerous eddies and gyrations were occasioned, which apparently drew the floating *spicula* under water; and I found the frozen matter to accumulate much more abundantly upon such parts of the stones as stood most opposed to the current (where that was not very rapid), below the little falls, or very rapid parts of the river."

These are by far the most important observations that have been made on the subject of ground-ice. M. Arago is so much struck by them, that after having concluded his own theory he says, " It is not certain that the little particles, mentioned by Mr Knight, do not play an important part in this phenomenon, which I have entirely overlooked." I verily believe they do; only one element is wanting in the catalogue of Mr Knight's observations to decide my opinion, and that is, the state of the atmosphere during the preceding night. I am persuaded that it had been loaded with hoar-frost, and its precipitation into the river formed the floating *spicula* which he observed; they could have no other origin; and their being brought into contact with the stones by the gyrations of the stream, is exactly what I had given two years ago as the theory of the formation of ground-ice, by the congelation and precipitation of the moisture of the atmosphere.

It is always delightful to explore the mysteries of nature, and the Author of our being has provided in such researches unbounded exercise for the highest powers of our understanding and reason. Even brute matter gives us some idea of the immensity of its Creator; for notwithstanding the immense strides that have been made in investigating the properties of matter, we may be said to be at this moment only on the threshold of science; and future generations, if the mind goes on to improve, will look back on our most profound researches merely as forming the rude elements of that more perfect knowledge which they will have reached. Perhaps much remains to be known even with regard to the common phenomena to which I have this day directed the attention of this meeting; and although I think we have nearly reached the solution of our problem in the process of freezing, yet that you may not think the mysteries of congelation exhausted, I conclude with mentioning a fact, which the illustrious Frenchman, whom I have so often quoted, leaves without even attempting an explanation. " During the congelation of the bottom of the Aar, M. Hugi immersed pitchers filled with hot and cold water; the first, on being brought up, was covered with a layer of ice *one inch thick;* the other had no marks of congelation. Bullets covered with cloth, warm as well as cold, afforded similar results."

Notice of an Earthquake at Saena in Peru. By JOHN REID, Esq. Communicated by the Author.

SIR, SAENA, 11*th November* 1833.

THE place from which I write is situated forty miles N.NW. of the port of Arica, on the coast of Peru, and twenty-five miles inland from the point of the bay of the same name, laid down in our maps as the Morra de Sama. The surrounding country is part of that hopeless waste which reaches along the coast from Tumbey to the confines of Chili, on which nature in denying it rain has set the impress of eternal sterility.

The Cordillera of the Andes, which runs nearly the whole extent of this side of South America, parallel with the coast, is distant only about twenty miles, and presents the sublime summits of Tacora, and three other nameless mountains, covered, for several thousand feet, with perpetual snow, glittering under the pure sunshine of a tropical sky. The climate, from our proximity to the Cordillera on the one hand, and the Pacific Ocean on the other, is one of the finest in the world: seven years of almost constant observation have given me a medium of 63°, as the general average temperature of day and night. Rain, in the proper sense of the word, is unknown in winter. We have sometimes a drizzling mist during the night; but even this is rare, and wind, except in the slight southerly trade breeze, which sets in about mid-day, and calms at sunset, is utterly unknown. A small stream, dignified by the name of River, descends from the Cordillera, and by its careful distribution, supports the luxuriant vegetation which environs the town, but these advantages are more than counterbalanced by our exposure to earthquakes.

On the night of the 8th of October 1831, at a quarter past 9 o'clock, the first great "terremoto," for a period of nearly a century, took place here. Its approach was announced by a hollow rumbling subterraneous noise, not unlike, but much louder, than distant thunder. This lasted for about ten seconds, and was followed by a violent vertical movement of the earth, which continued for nearly seventy seconds more. Many of the houses were thrown down, the walls of others shattered in every direction, and in some cases pieces of building were detached from the middle of walls, leaving the rest of the edifice uninjured. This earthquake ruined the unfortunate town of Arica, was felt at the very extremity of the republic to the south, and as far north as Camana, a line covering seven degrees of latitude along the coast. It was also felt at sea, at a distance of 100 miles from Arica, and at Chuquisaca, 400 miles inland, shaking to its centre not only the immense breadth of the main Cordillera, but the lateral chain of Portosi, on the eastern extremity of which Chuquisaca is situated. The great shock was followed by two others at 11 P. M., and 5 of the following morning, and the earth continued sensibly trembling for at least a fortnight afterwards, up to the 7th of February 1832. I counted ninety-seven distinct shocks, and from that day the greater part of the earthquakes we have had have taken place without the noise which used formerly to precede them.

On the morning of the 18th September last, precisely at 6 o'clock, another dreadful convulsion of the earth occurred here, which entirely destroyed at least 1000 of the 1200 houses of this unfortunate city, besides completing the utter ruin of Arica and the other small towns of the province. This earthquake commenced in its full force without any preceding noise. It lasted forty-three seconds, and the movement of the earth was horizontal, with two or three undulatory *oscillations*, the most alarming and dangerous of all. Those who, like myself, may have frequently experienced similar things, will easily agree with me, that it is no time for exactness in calculation, nor could I speak with precision to the duration of this convulsion in which my personal safety was in many ways threatened; but for the fact of having had my watch at the moment of its occurrence in my hand, and having preserved presence of mind enough to note the instant when it ceased, I am inclined to believe that three oscillations occurred every second. The subterranean noise was dreadful, infinitely louder than any thunder I ever heard, and I have been in many thunder-storms on the summits of the

Andes. Many poor people lost their lives, and all were driven to the surrounding desert, to seek safety in distance from the dangerous vicinity of walls and houses.

On the evening preceding the two earthquakes, of which I have spoken, the atmosphere was very dense, an ominous inexplicable stillness seemed to prevail, broken only at intervals by the breathing of an air of wind, which appeared to have no determined direction, and was felt within doors the same as in the street. The atmosphere appeared to be in a highly electrical state, and many people taking notice of these things, were in some degree prepared for the coming calamity. Nor was the howling of the dogs and braying of jack-asses during the night disregarded. In countries exposed to earthquakes people acquire a habit of observing any thing considered as an indication, as well as a delicacy in the perception, of the slightest shake, which appears to a stranger ridiculous timidity. Two or three circumstances came under my own observation, which seemed to prove that some powerful agent is at work in the atmosphere besides the hidden one below the surface. A great number of empty glass phials I found next day standing where they had been left, but the stoppers were scattered in all directions about the room. A few others, containing different liquids, were thrown from the shelves and broken, but no empty one had even fallen on its side. On a highly varnished new table, at which I had the night before been reading, the varnish became so fluid that it passed through the boards of several books, and they next day appeared as if glued to the mahogany. From several large earthen jars sunk in the earth, the water was thrown in considerable quantity over the mouths, although in none of them was it nearer to the top than from 3 to 4 feet. One singularity in the dog is remarked here, and it is, that immediately after a shock, whether strong or weak, the whole dogs of the place run to drink at the nearest water.

I had got this length when a messenger arrived from Arica to inform us, that the vessel which takes our letters to England positively sails in the morning, and they must leave this immediately. I proposed giving a short historical view of the principal earthquakes which have happened on the coast of Peru since the conquest, along with a brief description of the principal volcanoes in our neighbourhood. This I shall do by the first opportunity, in the hope that it may not prove altogether uninteresting. I have greatly to regret the having, many years ago, broken my barometer in the interior, but I am in daily hopes of receiving one I long since ordered from England. I am, Sir, your most obedient servant, JOHN REID.

On some of the Cetacea. By Professor TRAILL. Communicated by the Author.

It has, I believe, excited considerable surprise in this country, to observe the keen discussions which have lately taken place between Geoffroy St Hilaire and other French naturalists on the nutrition of the Cetacea.

British naturalists have long considered the existence of mammæ, the secretion of milk, and the lactation of their young, as among the best established facts in the natural history of that order of animals; but it would appear that, on the recent capture of a considerable number of a large species of Delphinus, Geoffroy St Hilaire has endeavoured to throw doubts on the received opinion on this subject, on grounds chiefly derived from an observation some time ago made by *Baër*, that the mammæ of the Cetacea were analogous to the abdominal glands of the ORNITHORHYNCHUS and ECHIDNA, two animals which, from the common termination of the fœcal, urinary and generative organs, Geoffroy had proposed the generic name of *Monotrèmes*. This assertion of Baër seems to have been the origin of the scepticism of that distinguished naturalist.

The questions which have been discussed in France are involved in four propositions :—

1. Have the Cetacea *mammæ?*
2. If they have, do they secrete milk ?
3. Have they a nipple ? And,
4. Do the young derive their nourishment from the teats?

The three first have been long ascertained by various naturalists.

The ancients appear to have had very correct notions respecting these points, particularly in one genus of cetaceous animals, the *Delphinus.* Aristotle had remarked the striking affinity of the Cetacea to the Mammalia inhabiting the land; and states, " that young dolphins are nourished by the milk of their mothers as they swim in company with them." Pliny is more express when speaking of the same genus: " *Nutriunt uberibus, sicut Balænæ.*" " *Quin et adultos diu comitantur; magnum erga partum charitate.*"

The fact of whales possessing teats is so well known to our Greenland sailors, that I never heard it doubted by any of them whom I have examined on the subject; and I have repeatedly heard them describe the milk which flows from the udder of the female whale when it is pressed. Mr Scoresby, one of our most accurate observers, thus describes the lactiferous system of the great whale, *Balæna mysticetus:* " Two paps in the female afford the means of rearing its young. They are situated on the abdomen, on each side of the pudendum, and are two feet apart. They appear not to be capable of protrusion beyond a few inches. In the dead animal they are always found protruded.

" The *milk* of the whale resembles that of quadrupeds in its appearance. It is said to be rich and well flavoured." (Arctic Regions, i.)

I have had opportunities of examining several species of Delphinus, and can positively assert that the females have mammæ, which are furnished with *teats* or *nipples*, and which secrete milk.

It is known to some of my friends, that, in 1809, I described, chiefly from the drawings and notes of the late Mr James Watson of Orkney, a new species of *dolphin*, to which I gave the name of *Melas* from its glossy blackness; but for which I afterwards proposed the trivial name of *Deductor*, from its gregarious disposition and propensity to follow a leader. The first description was published in the 22d volume of *Nicholson's Journal;* the second in *Scoresby's Arctic Regions.* In both, the fact of the young being nourished by sucking the dams is noticed, but shortly, because it was considered as too well established to admit of any doubt; but I am enabled, from Mr Watson's notes, now in my possession, to state more fully this fact, as witnessed by him in the herd of 92 individuals of that species stranded at once in the Orkney islands; from which, the drawings and descriptions published by me were originally derived.

In a letter, dated in 1807, Mr Watson thus writes: "On the 13th of December last, ninety-two whales of the Delphine genus were killed in the Bay of Scapay." "They were of various sizes, from five to twenty feet in length; the smallest were destitute of teeth, and *sucking their dams.*"

Another individual, who was then also present, and is now with me while I write, gives the following account of that extraordinary scene:—"When the whales were driven on shore in Scapay Bay, the young ones continued to swarm round their dams, until the receding tide left them also dry. During this interval I observed some of them clinging to the teats of their mothers. When separated the milk flowed from the teats in great quantity. It was white; and, as it flowed, of the consistence of thin cow's milk; but, on standing, it seemed to throw up cream, or to become more rich in appearance. As I walked round the animals, pools of milk were here and there distinctly visible. The moans of the mothers were most piteous, especially when their young were removed. This induced one of my servants to lift a small whale, and apply it to the mother's teat, of which it immediately laid hold. I cannot recollect that I saw it absolutely *sucking;* but it grasped the teat with its toothless mouth, and my impression was that it sucked."

These facts were witnessed by hundreds of persons, among whom I never met with one who thought differently on the lactation of the *Delphinus Deductor.*

Among Mr Watson's papers I have found several measurements of this species, besides those already published by me.

"The eye, 2 inches long, and $\frac{5}{8}$ths broad, is placed about 18 inches from the point of the snout.

"On the back of the head, and in a line with the eyes, was placed the spout-hole, of a semilunar shape, $4\frac{1}{2}$ inches in length by 2 broad.

"Length of the animal" (from which the drawing was made) "20 feet, and girth $11\frac{1}{2}$ feet. Pectoral fins $5\frac{1}{2}$ feet by 15 inches. Tail $5\frac{1}{2}$ feet wide, by 2 feet deep.

"Teeth conical, about $1\frac{1}{2}$ inches long, bent a little inwards;

and in those *under* the largest size, there were 24 in each jaw; those *full grown* seemed to have lost some of their teeth."

It seems to me probable, that the animal lately described as a new species by the French *savans*, under the name of *Delphinus Globiceps*, is only my *D. Deductor*, which has a semiglobular snout.

I may here remark, that, in my original description, by a misprint, the pectoral fins were said to be *from* 6 *to* 8, instead of *from* 4 *to* 6 feet long. Another, of which the measurements were published in Scoresby's work, had a length of $19\frac{1}{2}$ feet, and the free part of its pectoral fins was only $3\frac{1}{2}$ feet by $1\frac{1}{2}$ foot.

In this species the spiracle has its cornua pointing forward. Mr Scoresby has also published a figure and admeasurements of *Balæna Rostrata*, taken by Mr Watson from a stranded specimen. In Mr Scoresby's work, the circumference is stated at 20 feet; but in another set of admeasurements, in Mr Watson's handwriting, I find the girth given at 10 feet only.

This species of whale may be readily discriminated from the young of the allied species, by the colour of its palatal laminæ, which are in it *whitish*, while they are dark brown in the other species.

A fine specimen of the *B. rostrata* was, about two months ago, caught in the Firth of Forth, and exhibited in this city. It agreed exactly with the Orkney specimen in its external appearance; and a good account of its anatomical structure may be soon expected from *Dr Knox*, who has dissected the animal.

On Workman's Correction of Middle Latitude Sailing. By WILLIAM GALBRAITH, A. M., Teacher of Mathematics, Edinburgh. Communicated by the Author.

IT has lately been the practice to introduce into several elementary books which treat of the principles of navigation, a table by Mr Workman to adjust the arc called the Middle Latitude, so as to produce the same course and distance as by Mercator's sailing, when the earth is considered to be a sphere. This correction is, indeed, generally small, and by the nature of this problem does not, in a practical point of view, confer material accuracy on the results, even supposing the earth to be a sphere. Besides, from the use of the log as generally constructed, and the instability of the compass in an irregular sea, it is not possible to steer with great nicety, seldom to so much as within a quarter of a point of the tenth. The errors, therefore, to which a ship's place is liable by these causes are much greater than by any small inaccuracy in ordinary cases, arising from the common method of computing the course and distance. In addition to these, however, the earth is not a sphere but a spheroid, of about $\frac{1}{300}$ of compression, and since this is the case, it appears to me to be an unnecessary refinement to take into account corrections which proceed on the spherical hypothesis. On this account I think it needless to encumber a process with any additional complexity, intended for the use of ordinary seamen.

Instead of entering upon any lengthened discussion on this subject here, I shall merely refer to the third volume of Delambre's Astronomy, chapter xxxvi, for a demonstration of the methods to determine the meridional parts on the spheroid, and to Mendoza Rios' tables for the proper table. It is shewn by the celebrated astronomer first named, that the meridian parts, answering to the reduced latitude for a given ellipticity, are those on the spheroid. Strictly speaking, then, the meridian difference of latitude found in this manner ought to be employed in determining the course and distance by Mercator's sailing, and to the results from these, those by Middle Latitude sailing ought to be made conformable. Suppose, for example, that the middle latitude is 55°, and the difference of latitude 6°; then, on the spheroidal or true hypothesis to $\frac{1}{300}$ compression, the correction of the middle latitude is — 7′. But by Workman's Table this correction on the spherical hypothesis will be + 6′. The results would therefore have been more accurate in this case, by omitting the correction than by applying it.

It may, consequently, be remarked, that, in Mercator's sailing, the meridional parts should strictly be taken for the reduced or geocentric latitudes, which are always smaller than the observed or apparent latitudes. The reduction of the latitude is greatest at 45°, and diminishes towards the equator and poles. The meridional difference of latitude may therefore be augmented or diminished by correcting the latitudes for the spheroidal figure of the earth. If they are both less than 45°, the meridional difference of latitude will be diminished ; if the one be less than 45°, and the other greater, the meridional difference of latitude may be the same on the spheroid and on the sphere ; and if they are both greater than 45°, the meridional difference of latitude may be increased.

These remarks will be rendered more palpable to practical seamen, by an example wrought at length by both methods, and then comparing their results, than by a direct demonstration. Thus, taking Mr J. R. Young's example, page 126 of his Trigonometry, lately published, and employing Mendoza Rios' Table of Meridional Parts,—

1. App. Lat.	51°18′	M. P. 3597.50	Red. Lat. Lat. 51° 7′	M. P. 3579.94
2.	37 0	2392.63	36 49	2378.87
Diff. 858 =	14 18	1204.87		1201.07

Hence the course is 33°5′, to distance 1024 miles.

As radius, . . .	10.000000		10.000000
Is to tan. 33° 5′ . .	9.813899		9.813899
So is Mer. D. Lat. 1204.87 .	3.080939	1201.07	3.079568
To differ. of Long. 784.94 .	2.894838	782.50	2.893467

Now, by middle latitude sailing, not corrected by Workman's table, the difference of longitude is . . 779 miles
Hence difference of longitude on the spheroid, 782.5

Error by the common method, — 3.5

Difference of latitude by Workman's table, . 786.6 miles
The same on the spheroid, . . . 782.5

Error by Workman's proposed correction, + 4.1

greater in excess than the uncorrected method is in defect.

The same results would nearly arise from a recomputation of the example given by my friend Mr Riddle of Greenwich, in page 175 of the second edition of his excellent treatise on Navigation, published in 1831.

These remarks are given, not in the spirit of acrimonious criticism, but purely to shew the inutility of making unnecessary corrections in our best works, usually in the hands of ordinary practical seamen.

54. South Bridge.

Observations on the Structure of the Brain, &c.

The "Annalen der Physik und Chemie" von Poggendorff, No. 7, 1833, contains an essay by Professor Ehrenberg of Berlin, entitled, " The necessity of a minute mechanical examination of the brain and nerves in preference to the chemical analysis illustrated by observations," in which there is given an account of some observations recently made by him with regard to the minute structure of nervous tissue, as seen by the aid of a very powerful microscope.

Many attempts of a similar nature to examine the structure of that fibrous-like texture which is in general seen in some parts of a fresh brain, and which becomes more obvious when the brain has been artificially hardened by steeping in alcohol or a solution of the muriate of mercury, or by boiling in oil, have been made ever since the microscope came into use, but these attempts have led as yet to very unsatisfactory results.

A hasty repetition of Professor Ehrenberg's observations has not shewn us the appearances described by him, but the well merited character for accuracy and skilfulness in the use of the microscope which that observer has acquired by his interesting researches on the structure and functions of Infusoria, makes us hope that they may be found to be correct, and satisfies us that a short account of them will at all events be interesting to anatomists and physiologists.

The discordance in the accounts given of the structure of the brain and nerves by Leewenhock, Della Torre, Monro, Barba, Home, and others, and the unsuccess which has generally attended this investigation, may in some degree have proceeded from unskilful management of the microscope on the part of some,—from different modes of examination having been adopted by others,—from a total ignorance with regard to the disposition of the elementary texture in which the nervous matter of the brain has been generally believed to be contained,—from the supposition that has prevailed that a fluid or mucous matter might constitute the matrix in which the nervous filaments are deposited,—and from the circumstance that fibres of very different magnitude have been looked for in the nervous texture by different observers.

Professor Ehrenberg has shewn that the proper nervous substance of the Brain and Nerves does actually consist of very minute fibres; and he informs us that these fibres can only be discovered by the aid of a magnifying power of 300 diameters, and that he was sometimes obliged to have recourse to a much greater magnifying power, as 800 diameters, in order to bring them into view. He examined thin slices of the recent brain, and states that the fibrous structure was in general most obvious at the thin margins of the slices, when these were simply laid on the object glass-holder of the microscope, and that gentle pressure of the nervous substance between two thin plates of glass generally rendered the fibres more apparent.

The great mass of the Cerebrum and Cerebellum consists, according to Professor Ehrenberg, of very minute fibres irregularly disposed in the cortical part, and there interspersed with globules and plates, converging as they pass inwards from the surface towards the centre of the brain. The greater number of these fibres have not a regular cylindrical shape, but present the appearance of strings of pearls, the swelled portions being situated at some distance from one another, and united by narrower parts which are continuous with them, and are formed apparently of the same material. Besides these fibres, which Professor Ehrenberg calls *articulated*, from their knotted appearance, this observer states that towards the base of the brain and crura cerebri, other somewhat larger fibres, of a regular cylindrical form, are to be observed, interspersed among the articulated or knotted ones. These two sets of fibres are not held together by cellular tissue, nor fluid, nor mucous matter, but appear to be nearly in juxtaposition with one another, except where they are penetrated by the net-work of minute bloodvessels which are every where distributed through the brain. The cortical substance seems, according to Ehrenberg's observations, to differ from the medullary or white substance chiefly in the want of the straight cylindrical fibres, and in the articulated fibres being contained in a denser net-work of bloodvessels, and being covered by a layer of free granules larger than the dilated parts of the knotted fibres.

In the brain, the fibres run for the most part parallel to one another; they are sometimes seen to cross, and, in a few instances, Professor Ehrenberg states that he has observed two fibres uniting into one, but never any distinct anastomosis.

The larger straight Cylindrical fibres, he states, are manifestly tubular, because it is possible to see the inner paries of the tube, and on dividing some of these fibres and gently pressing them between plates of glass, a granular medullary matter was made to issue from them. In the Knotted or Articulated fibres he never was able to discover a distinctly tubular appearance, nor could any matter be pressed from their interior; but notwithstanding this, Ehrenberg considers these also as tubular.

Professor Ehrenberg has observed a remarkable difference in the minute structure of some of the Nerves of special sensation, the great sympathetic nerve, and the compound spinal nerves. He finds that the olfactory, the optic, and the auditory nerves, as well as the branches of the great sympathetic, are entirely composed of knotted or articulated fibres, similar in size and appearance to those forming the great bulk of the nervous matter in the cerebrum; while the nerves of motion and the regular spinal nerves, are entirely composed of the straight cylindrical tubular fibres.

The cylindrical tubular fibres of the spinal nerves and of the nerves of motion coming from the brain, are considered by Professor Ehrenberg as prolongations of some of the articulated fibres of the brain itself, for he has observed at the origin of a nerve of motion, that the articulated fibres gradually lose their knotted appearance as they pass into the root of the nerve, and increasing slightly in diameter, become the straight tubular cylindrical fibres proper to nerves of this description.

The net-work of the retina affords an excellent opportunity of viewing the articulated cerebral fibres, but in order that these may be well seen, there must be removed from their surface, a layer of coarse granules, nearly of the diameter of the nuclei of the bloodglobules, and similar to those which cover the flattened extremities of the articulated fibres, at the surface of the cortical substance of the brain.

It remains still to be investigated, whether the knotted kind of fibres are only to be found in the nerves above mentioned, or are peculiar to all sensory nerves, while the cylindrical tubular fibres are peculiar to motory nerves.

Both the cylindrical and the articulated fibres, as they pass from the brain into the roots of the nerves, receive a nervous covering or neurilema, which invests each individual fibre, and each bundle of fibres, as well as the whole trunk of the nerve, with a dense cellular and vascular coat.

The cylindrical fibres are stated to be about $\frac{1}{120}$ of a line in diameter.

It must not be supposed, that Professor Ehrenberg has confounded the tubular appearance of the nervous fibre with that of the neurilema, for he professes to have accurately distinguished the limits of both these parts.

The Ganglia are described by Professor Ehrenberg as somewhat resembling the brain, in respect to the nature of the fibres composing their nervous substance. They are formed by reticulated collections of both articulated and cylindrical fibres, interspersed with granules and cellular texture. In some places in the ganglia, he has also remarked a greater than ordinary enlargement of the swellings of the articulated fibres.

These observations have been made on the human brain and on that of some quadrupeds, of birds and reptiles, with nearly the same results in all.

Chemical Analysis of an Indian Specimen of Mesolite. By Robert D. Thomson, M.D. H.E.I.C.S. Communicated by the Author.

This specimen of mesolite I obtained in Caranja, one of the picturesque insulated islands in the harbour of Bombay. In this locality, as well as in Salsette, it appears to occur in considerable abundance in amygdaloid, which bears a striking resemblance to the rocks at Dumbarton, from which so many beautiful zeolites have recently been extracted. It may be frequently observed in these islands, detached from its native rock, among the debris with which the sides of the high lands are always profusely supplied. This mineral has hitherto been procured in considerable abundance in Scotland and Germany, and specimens from each of these situations differ slightly in their composition. It is from this circumstance, that the present analysis is now submitted to the reader, as I find that the Indian variety is characterised by an additional quarter atom of water, a fact which will be more distinctly perceived in the formulæ.

The Indian specimen resembles the mesolites of Scotland in its mineralogical characters, but has a lower density. Its specific gravity, by a mean of two trials, is 2.262. Before the blowpipe *per se* curls up; fuses with borax into a colourless bead with difficulty. Ten grains reduced to a fine powder were boiled with some pure muriatic acid, to which a little nitric acid was added; by this means the powder was converted into a jelly. Distilled water was now poured upon it, and the whole solution evaporated to dryness. Very dilute acid being then added to the dried powder, by digestion the silica alone remained undissolved, which was thrown on a filter and well washed. When dried, heated, and weighed, it amounted to 4.27 grains. Before the blowpipe, with carbonate of soda, this product fused into a colourless glass, shewing that it consisted of pure silica. The liquid which passed through the filtre was concentrated, and caustic ammonia was added. The alumina, which fell in fine white flocks, was thrown on a filter and washed with warm distilled water. The precipitate, after being dried and heated to redness, weighed 2.75 grains, which dissolved completely when boiled in muriatic acid, demonstrating that no silica existed mixed with it. Caustic potash, when boiled with the solution left no precipitate, shewing that no iron was present in the mineral. The washings of the alumina were concentrated on the sand-bath, and the solution rendered neutral. A solution of oxalate of ammonia produced a copious white precipitate, which was thrown on a filter. The carbonate of lime produced 1.36 grains, which is equivalent to .761 lime. The remaining liquor was now evaporated to dryness, and the residue subjected cautiously to a red heat, to dissipate the ammoniacal salts. A white substance remained, which weighed 1.5 grains. When dissolved, it yielded a precipitate with nitrate of silver, but none with muriate of platinum, or when boiled with carbonate of soda. It was therefore chloride of sodium, equivalent to .70 soda. Ten grains of the mineral were rendered opaque by a red heat, and lost of their weight 1.471 grains. The Indian specimen of mesolite thus afforded,

Silica, ...	4.270	42.700
Alumina, . . .	2.750	27.50
Lime, ...	.761	7.61
Soda, ...	.700	7.00
Water, ...	1.471	14.71
	9.952	99.52

The formula to represent its constitution most nearly, will be

$$3\,AS + (\tfrac{1}{2}\,C + \tfrac{1}{2}N)\,S^3 + 3\tfrac{1}{2}\,Aq.$$

which approaches closely to a Bohemian specimen analyzed by Dr Freysmuth, with a specific gravity of 2.333.* The results of three analyses of mesolite afford formulæ differing in the relative proportions of soda and lime, and in the quantity of water. These are in the order of their increase,

$3\,AS + (\tfrac{2}{3}\,C + \tfrac{1}{3}N)\,S^3 + 2\tfrac{2}{3}\,Aq.$ Iceland. Fuchs.
$3\,AS + (\tfrac{1}{2}\,C + \tfrac{1}{2}N)\,S^3 + 3\ \ Aq.$ Bohemia. Freysmuth.
$3\,AS + (\tfrac{1}{2}\,C + \tfrac{1}{2}N)\,S^3 + 3\tfrac{1}{2}\,Aq.$ India.

which it is obvious may be resolved into the general expression,

$$3AS + \binom{N}{C}\,S^3 + 3\,Aq.$$

These formulæ are calculated according to Dr Thomson's atomic weights, reckoning the atoms of silica 2, and that of alumina 2.25. The mineral belongs to the V. Genus Alumina, of his 2d Class Alkalies and Alkaline bases, in his new chemical arrangement of minerals.

* Schweigger's Journal, xxv. 426. An. of Phil. xvi. 405.

Description of several New or Rare Plants which have lately flowered in the neighbourhood of Edinburgh, and chiefly in the Royal Botanic Garden. By Dr Graham, Professor of Botany in the University of Edinburgh.

10*th July* 1834.

Alstrœmeria oculata.

A. *oculata;* caule terete, volubile, glabro; foliis ovato-oblongis, obtusis, utrinque glabris; petiolis tortis; umbellis multifloris, pedunculis subbifloris, pedicellisque glabris; bracteis bracteolisque obovato spathulatis, crispatis; petalis nonconformibus, longitudine subæqualibus.

Alstrœmeria oculata, *Loddiges*' Bot. Cabinet, t. 1851.—*Cuming's* Herbarium, No. 345.

Description.—*Stems* numerous, flexuose, and voluble, green, glabrous and shining, simple. *Leaves* ovato-oblong, many-nerved, crispid at the edge in our specimen, glabrous on both sides, bright green above, glaucous below, petioled; petioles twisted, illustrating that beautiful arrangement of nature to correct that lusus so common in this genus, by which the upper and lower surfaces of the leaf are originally reversed. *Umbel* terminal, several rayed, the rays generally bifid, and supporting two flowers. *Bracteæ* and *bracteolæ* corresponding in number to the primary and secondary divisions of the umbel, obovato-spathulate, crispid and generally coloured in the edges. *Corolla* (9 lines long, 7½ across) campanulate, red; *petals* subequal in length, the outermost the broadest, nerved, ovate, narrow so as to resemble a claw nearly in the lower half, notched at the apex, somewhat revolute in their edges; inner ones sandglass-shaped, pubescent within on their lower half, connivent in the middle, so as to close the throat which is whitish and surrounded by a broad dark purple semilunar band, especially on the two uppermost (which are the broadest) of the three inner petals. *Stamens* shorter than the corolla, decumbent, filaments glabrous at their origin and near the apex, pubescent and slightly swollen in the centre, immediately above which they are sprinkled with small lilac tubercles. *Anthers* ascending, reddish-leaden coloured, oblong, flat, bursting in the edges, when, as in the genus, they become flattened in the opposite direction; pollen granules minute, greenish-leaden coloured. *Pistil* about as long as the stamens; stigma trifid; style glabrous, with some small scattered lilac tubercles on its upper part. *Germen* dark green, turbinate, triquetrous, angles rounded.

This extremely beautiful, though small flowered species, we received at the Botanic Garden, Edinburgh, from Mr Knight in the beginning of this year. It flowered in April. No native station was communicated to us, but the specimens in Cuming's herbarium are from Valparaiso. My specimen from this source differs from the cultivated plant only in being more drawn out, in the peduncles being occasionally 3-flowered, in the leaves being more oblong, less glaucous, and free from undulation in the edge; but in every essential particular they seem the same.

Gastrolobium retusum.

G. *retusum;* foliis cuneatis, truncatis, utrinque lanato-pilosis, breve petiolatis, nervo medio in setam deciduam producto; stipulis setaceis pilosis, persistentibus; capituliss tipitatis, terminalibus axillaribusque.

Gastrolobium retusum, *Lindley,* in Bot. Reg. 1647.

Description.—*Shrub* erect; branches long, slender, round, pubescent, dotted with green. *Leaves* (1 inch long, ¼ inch broad) verticillate, cuneate, truncate, reflected in the sides, covered above and below with long subappressed somewhat woolly hairs, shortly petioled, middle rib prolonged into a deciduous bristle. *Stipulæ* bristle-like, hairy, more than twice as long as the petiole, persisting. *Capitula* dense, terminal or axillary, in the latter situation 8–12-flowered, on peduncles (half an inch long) solitary in each axil. *Calyx* bilabiate, upper lip bifid, lower 3-partite. *Corolla* twice as long as the calyx, orange-yellow, of deeper and richer colour before expansion. *Petals* slightly unequal, the vexillum rather the longest, the carina rather the shortest, on long claws; vexillum kidney-shaped, striated, reddish at its base; alæ elliptical, striated with red at the base; keel red, rather straight, emarginate and blunt at the apex, its petals separated at the base. *Stamens* free, inserted into the torus, imbedded within the keel; filaments red, glabrous; anthers yellow, elliptical, pollen-granules yellow, very minute. *Pistil* as long as the stamens, included within the keel; germen hairy, green; style glabrous, red, falcate, compressed laterally; stigma simple, dorsal, white.

This pretty little shrub was first raised, at the Botanic Garden, Edinburgh, in 1831, from seed brought home by Dr Lang; and again in 1832, from seed communicated by her Grace the Duchess Countess of Sutherland. It first flowered in December 1833, and the same plant much more freely in March 1834.

Lysinema pentapetalum.

L. *pentapetalum;* corollis pentapetalis; unguibus longitudinaliter distinctis, extus glabris, calyce longioribus, cumque segmentis ejus et bracteis lanato-ciliatis.

Lysinema pentapetalum, *Br.* Prodr. 552.—*Rœm. & Schult.* Syst. Veget. 4. 383.—*Spreng.* Syst. Veget. 1. 630.

Description.—*Shrub* erect, with loose straggling branches, which are villous when young. *Leaves* very numerous, scattered, glabrous, shining, bright green, fleshy, bluntly angled behind, flat in front, on short adpressed petioles, linear-subulate, connivent at the apex, spreading, except near the flowers, where they are suberect, often reflected at the lower part of the branches. *Flowers* sessile, axillary, solitary, collected into psuedo-spikes at the extremities of the branches. *Bracteæ* numerous, imbricated, linear, adpressed, brown, glabrous, lanato-ciliated, gradually larger to the calyx. *Calyx* pentaphyllous, closely embraced at the base by the bracteæ, from which it cannot be distinguished but by being rather longer than the longest of these. *Petals* 5; laminæ slightly reflected, white, oblong, glabrous; claws longer than the calyx, linear, brown, closely applied to each other at the apex, but without any organic union, glabrous on the outside, woolly within. *Stamens* 5, alternating with the petals, included; filaments hairy, brown; anthers oblong, somewhat pointed at both extremities, attached by the backs above their middle; pollen yellow, granules globular. *Hypogynous scales* 5, ovate, green, glabrous, alternate with the filaments. *Stigma* obscurely lobed, fringed with hairs. *Style* erect, rather longer than the stamens, red, hairy, green and clavate immediately below the stigma. *Germen* round, sessile, tomentous, 5-locular. *Ovules* numerous, in two rows in each loculament, fixed to a green central receptacle.

This plant was raised at the Botanic Garden from seeds collected at King George's Sound, and communicated to me by Colonel Lindesay along with other seeds, and a valuable collection of dried specimens, in July 1830. It flowered freely in the greenhouse in March 1834.

Sphærolobium medium.

S. *medium;* calycis tubo labiis dimidio breviore (corollis rubris).—*Brown.*

Sphærolobium medium, *Br.* in Hort. Kew. ed. 2d, vol. iii. p. 14.—*De Cand.* Prodr. 2. 108.—*Spreng.* Syst. Veget. 2. 350.

Description.—*Stem* slender, branched from the base; branches ascending, cylindrical, green, glabrous, rush-like, tough and woody. *Leaves* (3½ lines long) linear-subulate, subappressed, glabrous, green, and slightly spotted with black, opposite alternate or in verticels, caducous, the short adpressed petiole remaining. *Flowers* in verticelled terminal spikes, springing singly or in pairs, or three together from the axils of *bracteæ* resembling the leaves. *Pedicels* (about one line long) nodding, bibracteate at the apex; *bracteolæ* ovate, adpressed, caducous. *Calyx* twice as long as the pedicel, bilabiate; limb twice as long as the tube; upper lip bidentate, teeth large and divaricated at the apex; lower lip 3-partite, segments linear; the calyx, bracteolæ, and pedicels green, and closely covered with minute dark spots. *Corolla* orange-red, all the claws short; vexillum twice as long as the calyx, broader than long, notched, yellow at the base, reflected over the upper lip of the calyx; alæ as long as the vexillum, projecting forward in the centre of the flower, and spread horizontally, their upper (inner) edges being in contact, and the lower (outer) slightly turned upwards; keel blunt, shorter than the alæ, claws, and limb as far as the angle, free, in the fissure and at the teeth on the upper edge slightly ciliated, emarginate at the apex, lobes blunt, teeth blunt and equal in length to the claws. *Stamens* 10; filaments free, glabrous, the upper ones somewhat flattened near the base; anthers greenish-yellow; pollen granules small, oblong. *Pistil* rather longer than the stamens, like them included within the keel, green, glabrous; stigma small, capitate; style ascending, expanded into a thin colourless sickle-shaped edge on its upper side under the stigma; germen oblong, stipitate; ovules 2.

This plant, native of the SW. coast of New Holland, was introduced into Britain, according to Mr Brown, by Mr Peter Good, in 1803, but has never been common in collections, and perhaps was subsequently entirely lost. It was raised at the Botanic Garden, Edinburgh, from seeds communicated by Mr Newmann in 1830, and flowered for the first time in April 1834. Its habit is very similar to *Sphærolobium vimineum*, but its spikes are more clustered, its flowers are twice as large, and their colour is much finer. It should of course receive in the greenhouse the same treatment as *S. vimineum.*

Proceedings of the Royal Society of Edinburgh.

(Continued from Vol. XVI. p. 194.)

1833, *Dec.* 2.—SIR THOMAS MAKDOUGALL BRISBANE, President, in the Chair. At this Meeting the following communication was read:—

On a New Species of Coloured Fringes developed between certain pieces of plate-glass, exhibiting a new variety of polarization, and a peculiar property which renders them available for the purposes of Micrometry. By Mungo Ponton, Esq.

The author, when he first observed these fringes, found that they presented the appearance of three rectilinear bands, each consisting of black, white, and coloured stripes; but the central band was afterwards found to be composed of two united into one. There is thus a band for each of the four surfaces of the plates, the two side ones, appertaining to the uppermost and undermost surfaces, and the centre ones to the surfaces which are approximated. The peculiarities by which they are distinguished are as follows:—

1*st,* They are confined to certain specimens of glass, but not to perfectly parallel plates.

2*d,* They are exhibited while the plates are at a considerable distance from each other, provided their surfaces are preserved as nearly parallel as possible; and they are not affected even by the interposition of another plate between those by which they are formed.

3*d,* They are destroyed by the application of Canada balsam or oil of turpentine to any one of the four surfaces.

4*th,* They are of uniform breadth and appearance, so long as the disposition of the plates remains the same, and are not affected by pressure in whatever manner it may be applied. The Newtonian fringes, on the other hand, are affected, both in breadth and direction, by the manner in which the plates are pressed together, so that they can be produced at right angles, or in any other position, with respect to the new bands.

5*th,* The fringes under consideration are produced by the light which is returned inwards upon the plates by reflection from their anterior surfaces, so that the rays suffer three reflections and four refractions before reaching the eye.

6*th,* They present phases of revolution which follow a different order according to the surfaces that are placed together, and varying, as the revolution is made, from right to left or from left to right. The bands revolve only at half the rate at which the plates move during the first semi-revolution; and during the last quarter of revolution, when certain faces are together, there is a complete breaking up of the rectilinear fringes, which spread themselves in curvilinear forms over the whole surface of the plates.

7*th,* When viewed by homogeneous light, the fringes appear as light and dark stripes, covering the entire surface of the plates, and of uniform breadth which cannot be altered, except by changing the arrangement of the plates; but by turning one of these, both the breadth and direction of the stripes are changed. Their number varies from 10 or 12 to nearly 2000.

8*th,* The plates which exhibit the fringes do not display any symptoms of possessing the doubly refracting structure, when viewed by polarized light. The appearance of the bands, however, is the same as that of the fringes, produced by crossing wedge-shaped plates of sulphate of lime, and passing polarized light through them, as described by Dr Brewster. It is therefore probable, that the new fringes are occasioned by the intersection of oppositely polarized pencils of light, whose polarity is induced by the repeated reflections and refractions which they undergo in passing and repassing through the plates—and it would seem as if each surface exerted an independent and peculiar polarizing effect on the rays,—a hypothesis which appears necessary to account for the phenomena attending a change in the disposition of the surfaces.

9*th,* They possess a peculiar property, which the author conceives will render them available for the purposes of micrometry. When the surfaces of the plates are parallel, two of the bands are united into one at the centre; but if a film be introduced between the plates, so as to cause them slightly to diverge, the two bands in the centre will be separated, and move laterally from each other, still preserving their perfect parallelism. A film, $\frac{1}{300}$th of an inch in thickness, causes the central bands to separate to a distance of an inch, so that every $\frac{1}{20}$th of an inch of separation is equivalent to $\frac{1}{15000}$th of an inch of thickness. When smaller thicknesses are to be measured, recourse must be had to the side bands, which are affected by a much slighter degree of divergence than the centre ones. A thickness so minute as that of gold leaf may be rendered sensible by the side bands, and a scale for micrometry might be found, by introducing successive leaves of gold of a known thickness.

1833, *Dec.* 16.—Sir Henry Jardine in the Chair. The following communications were read :—

1. " A General View of the Phenomena displayed in the Neighbourhood of Edinburgh by the Igneous Rocks in their relations with the Secondary Strata ; with reference to a more particular description of the section which has been lately exposed to view on the south side of the Castle Hill." By the Right Hon. Lord Greenock.

The author, referring, in the introductory part of his paper, to the views taken by Hutton of the structure of the earth's surface around Edinburgh, explained,—That the prevailing rocks are strata of sandstone and shale of the coal formation, with occasional beds of limestone ; and interrupted by insulated as well as grouped hills of igneous origin, rising abruptly through them,—That the latter or trap-rocks, seem in many quarters interstratified with the former, as if they had burst while in a state of fusion between the strata of the secondary rocks,—That fragments of the secondary rocks are often seen imbedded in the trap, as if they had been broken away from the strata to which they belonged, and been hurried along by the fused erupted mass,—And that the trap-rocks often present very different appearances in the same hills, shewing that they were erupted under varying circumstances at different periods of time. The author farther explained, that the environs of Edinburgh seem to constitute a great basin, surrounded by trap-rocks, which dip outwards in all directions from a common centre,—the Pentland Hills forming the southern boundary, the rocky coast of Fife at Burntisland the northern, and Salisbury Craigs and Corstorphine Hill the eastern and western limits.

The paper then proceeds to describe the appearances presented by a late section of the southern face of the Castle Hill, where several of the phenomena referred to above are very well illustrated. The great mass of the Castle Hill rock is a dark compact greenstone. Towards the south-west point, altered rocks are seen resting on the trap in a highly inclined position ; and within the Castle wall, fragments of sandstone are imbedded in the greenstone, shewing that the latter must have burst in a state of fusion through the strata of the former. But at the south-east point of the rock, beyond the walls, the section lately made in cutting the new south-west road has displayed appearances, which, in the author's opinion, supply strong evidence that, subsequent to such eruption, the secondary and trap-rocks had been uplifted together by a common cause, probably acting on a great extent of the face of the country. This section shews five or six beds of sandstone, with alternate layers of slate clay or marl. Signs of great confusion are found in these strata, more especially as they approach the point of junction with the trap-rock,—their eastern extremity being thrown upwards, while their western portion is cast down, so as to lie unconformably on the upturned strata; and near the point of junction with the greenstone, the ends of the strata of sandstone and slate-clay are shattered, and have actually fallen over, so as to come obliquely in contact with the tabular masses of the greenstone. Yet it is remarkable, that the sandstone and shale present no appearance of semi-fusion or intermixture, where they are in contact with the greenstone; nor does the greenstone here present any imbedded masses of the secondary rocks, nor send out any veins among their adjacent strata. At the time, therefore, when the dislocation of the sandstone strata occurred at the point of junction with the greenstone, the latter could not have been in a state of fusion; and the only rational explanation which occurs is, that both rocks were raised, *in a solid state,* by some common cause, above the level of the waters under which they were originally formed; and that the fault or dislocation in the sandstone strata was produced by some subsidiary disturbing power acting at the same period.

2. Researches on the Vibrations of Pendulums in Fluid Mediums. By George Green, Esq. Communicated by Sir G. Ffrench Bromhead, Bart.

The author proposes in this paper to resolve a particular case of the motion of fluids, not previously noticed, and susceptible of practical application, namely, the circumstances of the motion of an indefinitely extended non-elastic fluid, where agitated by a solid ellipsoidal body moving parallel to itself, according to any given law; always supposing the body's excursions very small compared with its dimensions. The question here stated is considered by the author to admit of an easy general solution. As the principal object of his paper is to determine the alteration produced in the motion of a pendulum by the action of the surrounding medium, he insists more particularly on the case where the ellipsoid moves in a right line parallel to one of its axis; and endeavours to prove that, in order to obtain the correct time of a pendulum's vibration, it will not be sufficient merely to allow for the loss of weight caused by the fluid medium, but that it will likewise be requisite to conceive the density of the body augmented by a quantity proportional to the density of this fluid. He determines the value of the quantity last mentioned, when the body of the pendulum is an oblate spheroid, vibrating in its equatorial plane; and finds, that when the spheroid becomes a sphere, the quantity is precisely equal to half the density of the surrounding medium. Hence in the last case, the true time of the pendulum's vibration is obtained, if it be supposed to move in vacuo, and its mass be simply conceived to be augmented by half that of an equal volume of the fluid, while the moving force with which it is actuated is diminished by the whole weight of the same volume of fluid.

3. Observations on the Fossil Fishes lately found in Orkney. By Dr Traill.

The geologist has been for some time acquainted with the occurrence of Fossil Fishes in Caithness, and they have been more lately found also in Orkney, especially near Skaill, the seat of W. G. Watt, Esq. in Pomona.

The author describes these fishes as imbedded in a dark coloured flag, which lies beneath three feet of soil and loose stone, and eleven feet of solid beds of similar flag, but destitute of organic remains. The fishes are contained in two strata, measuring together about two feet in thickness. The upper stratum contains only fishes belonging to the *Cartilaginei,* and seemingly the genus *Raia* ; the lower contains numerous fishes that belong to the orders Thoracici and Abdominales, most of them with distinct scales. Almost all of them lie on their bellies or sides, none on their backs, and their attitudes generally bespeak the energy of their final struggles. The fishes of these two contiguous strata are never intermixed. The strata dip about one foot in seven to the north-west. The author found only a single specimen of a petrified vegetable with the fishes. It was the leaf of a canna or a reed.

The Orkney Islands have much uniformity in their geological structure. The principal rock is this sort of slate, which is connected with sandstone, and has occasionally interposed thin beds of limestone, that seldom contain any organic remains.

The only primitive rocks in Orkney are in a limited district around Stromness, and in the contiguous small island of Græmsey. There granite, and gneiss approaching to mica-slate, appear in the surface, and have resting on them a coarse sandstone conglomerate. This last is in immediate contact with the slaty rock described above. The highest ridges in Orkney are the mountains of Hoy, which are composed of thick beds of sandstone, in which the author lately discovered a vast bed of trap. This sandstone, as well as that which occurs in the other islands, belongs to the old sandstone ; and the slaty rock is probably a newer part of the same formation.

There are not any distinct traces either of the mountain limestone or of the coal formation in Orkney, unless we are disposed to consider this slaty rock as the oldest member of the mountain limestone. But from its connexion with the sandstone, it is safer to reckon it a member of the old red sandstone series.

Specimens were exhibited to the Society illustrative of the author's statements.

4. Notice of further Discoveries at Burdiehouse. By Dr Hibbert.

The author announced that, since his former paper was read, on the organic remains of the limestone quarry at Burdiehouse, discoveries of still greater interest had been made. These chiefly consist of the remains probably of a large animal of the Saurian tribe, namely, what appears to be the epiphysis of one of the vertebræ, presenting, when broken across, the cancellated structure of bone—several large scales obtained by Mr Connell,—and, in particular, a large and beautifully perfect tooth, two inches and a quarter long, and covered with its enamel, which is quite entire. The remains here alluded to were exhibited and presented by the author and Mr Connell to the Society's Museum.

1834, *Jan.* 6.—Sir T. M. Brisbane in the Chair. The following Communications were read.

1. On the Investigation of Magnetic Intensity, by the Oscillations of a Horizontal Needle. By William Snow Harris, F. R. S.

The chief disturbing causes by which the magnetic intensity, as ascertained by the oscillations of the horizontal needle, are affected, are 1. Variations in the air in which they are performed; 2. The influence of changes in the mechanical conditions incidental to the mode of suspending the needle; 3 Changes in the disposition and intensity of the magnetism of the needle from heat and other causes.

These causes of disturbance the author proceeded to investigate.

I. He compared the oscillations of the needle vibrating in air, with those of the same needle oscillating *in vacuo*; and he minutely described the apparatus which he had contrived for allowing the needle to vibrate freely in an exhausted receiver, and his mode of determining the arcs of vibration. This apparatus enabled him to appreciate the resistance of air to the oscillations of the needle, and its effect in rendering *unequal* the duration of vibrations performed in long and in short arcs. Hence he inferred the impossibility of ascertaining the alleged diurnal changes of magnetic intensity by the common apparatus.

II. The second source of disturbance he endeavoured to obviate by a more accurate mode of suspending the needle; by which its centre of gravity and point of magnetic neutrality should be made to coincide. This the author proposes to accomplish by greater care in finding its true centre, and in adjusting its horizontality by means of small sliding counterpoises of platinum on each arm.

III. The influence of increase of temperature on the magnetic needle has generally been considered as *lowering* the tension of its magnetism; and it has been represented as again restored by cold: but the author's experiments seemed to prove the contrary, when the comparative experiments were made *in vacuo.* He considers, however, that if the needle be prepared, by being previously exposed to a variation of temperature from 212° to 0° of Fahrenheit, its tension will not afterwards be affected by ranges of temperature within these limits.

One of the most interesting parts of Mr Harris's paper is his mode of determining changes of magnetic tension in a particular magnet. It is well known that if a needle be made to vibrate within a ring of copper, it will be more speedily brought to rest, than if vibrating in open space. The influence of the ring of copper, therefore, might be employed to detect changes in tension, provided the force which induced motion in the needle, and that force by which it would eventually be reduced to rest *without* the ring, were both constant quantities. This, however, is not the case; but the author proposes to reverse the experiment, and cause the *ring to vibrate round the needle,* placed within it, so as just not to touch the ring. This will afford a comparative measure of the force of the needle at its poles, if we observe the influence of the needle in reducing the ring to a state of rest. A convenient mode of doing this he has given, and has deduced a general formula for estimating the differences in magnetic tension thus detected.

The author has also examined the influence of bright sunshine on the suspended needles; and has shewn that the difference observed in the oscillations of the needle in sunshine and in the shade, may be made nearly to vanish in the exhausted receiver; and he has rendered it probable, that the slight differences observed in bars oscillating in the sun's rays, are not altogether dependent on magnetism.

Lastly, the author endeavoured, by an artificial electric *aurora* in a *luminous conductor,* six feet long and four inches wide, to ascertain whether there was any effect produced on a finely suspended needle, placed within eighteen inches of the conductor; but the oscillations of the needle were not affected by a stream of electricity procured for twenty minutes from a powerful machine in this apparatus,

2. Experiments on Magnetic Intensity made at Liverpool and Manchester. By Dr Traill.

Dr Traill made a report to the Society of experiments made by him in 1832 at Liverpool and Manchester on magnetic intensity, measured by the oscillations of the horizontal needles belonging to the Society, which had been sent to him for that purpose. The reporter also stated the result of a series of experiments made by Professor Oersted and himself in Liverpool in 1823, which is important, as having been made use of by Professor Hansteen in constructing his *isodynamic magnetic lines* for Great Britain.

The result of Dr Traill's experiments is, that Hansteen has estimated the magnetic intensity of England a little too high, as Mr Dunlop found he had that of Scotland; and the reporter concluded that this arose from the experiments on which Hansteen founded his calculation being affected by some degree of local attraction, from the confined spaces in which the experiments were made.

The magnetic intensity, as deduced from the time of 300 vibrations in the reporter's experiments with the Society's needles, is, with Hansteen's cylindrical needle,

At Liverpool, mean of three series = 798″.21
At Manchester, from one series . = 798.82

With Dollond's flat needle,

At Liverpool, mean of three series = 1052.83
At Manchester, from one series . = 1051.76

The reporter also stated, that the magnetic dip at Liverpool, as ascertained by several experiments made there by Lieutenant Allen, R. N. and himself, with a needle furnished by the Board of Admiralty, for the late expedition up the Niger, is = 72° 2′ 24″.

The experiments on the dip, as well as two other series on magnetic intensity with a horizontal needle belonging also to the Admiralty, were made on the same spot as those with the Society's needles, viz. an open space in the Botanic Garden at Liverpool.

3. Description and Analysis of a Mineral from Faroe, not before examined. By Arthur Connell, Esq.

The mineral in question was put into the author's hands by Mr Rose, mineral-dealer of this city, as a substance supposed to be a variety of mesotype. Mr Rose obtained it from Count Vargas Bedemar of Copenhagen, who had brought it from Faroe.

It has a pure white colour, with some opalescence and translucence, a glistening vitreous lustre, and somewhat greater hardness than fluor. Its texture is imperfectly fibrous; but the fibres in some places diverge with considerable regularity, shewing an approach to a crystalline structure. The specific gravity is 2.362; it is remarkably tough and difficultly frangible, so as to require much time and labour to separate a mass of it into smaller fragments.

It gives off water at a red heat; and is fusible *per se* before the blowpipe only on the edges, without any swelling up. With soda it fuses with effervescence into a semi-transparent glass; with borax and salt of phosphorus, gives colourless glasses; and, with nitrate of cobalt, presents no alumina reaction. It gelatinizes readily with muriatic acid when reduced to powder. The analysis was effected by this reagent. After separating silica, the metallic oxides were thrown down by ammonia, and the lime by carbonate of ammonia. The alcalies were separated from one another by chloride of platinum, and the water was determined by ignition. Its composition is as follows:

Silica	57.69
Lime	26.83
Water	14.71
Soda	.44
Potash	.23
Oxide of Iron	.32
Oxide of Manganese	.22
	100.44

This composition differs from that of all other minerals, so far as the author's knowledge extends; and shows the substance under analysis to be a hydrated quatersilicate of lime, conformably to the formula, $9\ S^4\ C + 16\ Aq$.

Sir David Brewster, who possesses a mass of the mineral which he received from Count Vargas Bedemar, has observed crystallized faces, but so near the general surface, that they cannot be separated. He has also found that it possesses double refraction; that it reflects a bluish light, and consequently transmits a yellowish one; and that it possesses no pyroelectricity. He has no doubt that it is a new mineral.

The author proposes to distinguish it by the name of *Dysclasite*, [δυσ κλαω], as expressive of its remarkable tenacity and difficult frangibility. It will, of course, be arranged with the Zeolites.

The Secretary read an extract from a letter, giving a short description of the Stalactitic Caves recently discovered in the county of Tipperary, and exhibited various illustrative drawings.

Several additional specimens were exhibited from Burdiehouse Quarry; and Dr Hibbert read a short notice relative to the position of the limestone there, and the relation it bears to the mountain-limestone of Muirhouse and the neighbourhood. His observations were to the effect that, by examining some sections of the strata between Burdiehouse and Loanhead, he had now satisfied himself that the limestone of Burdiehouse lies *beneath* the great bed of mountain-limestone formerly described by him as traversing the country from Joppa towards the Pentland Hills. The order of the strata between them is as follows:—*Burdiehouse limestone*—shale and thin beds of the same limestone,—sandstone and shale,—sandstone, coal blaes, ironstone bands, and thin seams of coal—*Mountain limestone*—limestone blaes—*Coal measures*.

Premiums offered by the Wernerian Natural History Society.

EDINBURGH, 10*th May* 1834.

The WERNERIAN NATURAL HISTORY SOCIETY offers the following Honorary Premiums; open unconditionally to all scientific Naturalists:—

1. Twenty Sovereigns, or a suitable piece of Plate of that value, for the best Geological Account, with a Geognostical Map, Sections, and Specimens, of the Three Lothians, with as much of the neighbourhood as may be required for the elucidation of the districts.—To be given in against December 1835.

2. Ten Sovereigns, or a piece of Plate of that value, for the best Natural and Economical History of the Fishes—marine, fluviatile, and lacustrine, of the River District of the Forth. A collection of Specimens of the rarer Fishes will be desirable.—To be given in against December 1835.

3. Ten Sovereigns, or a piece of Plate of that value, for the best Account of the Entomology of the Three Lothians, and River District of the Forth; with a Collection of Specimens, and Map of the distribution of the Insects.—To be produced against December 1836.

4. Ten Sovereigns, or a piece of Plate of that value, for the best Essay on the Botany of the Mountains of Scotland, in connection with their Geological Structure and Composition; with Specimens, and a Map of the Distribution of the Plants. In this Essay, the range of elevation, and the northern and southern limits of the different species, should be attended to, and any facts tending to illustrate the geographical distribution of plants carefully recorded. It would also add greatly to the interest of the communication if it were accompanied with a coloured Geognostical Map of the mountainous districts examined.—To be produced against December 1837.

5. Ten Sovereigns, or a piece of Plate of that value, for the best Account of all Avertebrate Animals, (with the exception of Insects and their larvæ), inhabiting the River and Firth of Forth, their tributary streams, and the lakes included in the basin of the Forth; with a collection of Specimens.—To be given in against December 1837.

Communications may be addressed either to Professor Jameson, the President, or to Mr Neill, Secretary of the Society, Edinburgh.

Discovery of the Bones of the Iguanodon in a quarry of Kentish Rag (a limestone belonging to the lower greensand formation) near Maidstone, Kent. Communicated by Gideon Mantell, Esq. F.R.S. &c.

Some time since a paragraph appeared in the London papers, stating that some gigantic antediluvian bones had been found in a stone-quarry on Rockhill, near Maidstone, belonging to a Mr Prinsted; that they had been inspected by the curious in the neighbourhood; but that no one could guess their origin, or to what sort of animal they could have belonged. Mr Prinsted had the politeness to inform Mr Mantell of Brighton (late of

Lewes) of the circumstance, and that gentleman visited Maidstone a few days since, and has communicated to us the following remarks on those extraordinary osteological remains.

"The mass of stone in which the bones are imbedded will, when united, form a surface of 8 feet by 7, its thickness varying from a few inches to 2 feet: it consists of the hard variety of the limestone called *Kentish rag*, which is well known in the south-east of England as an excellent building stone. It abounds in the marine shells characteristic of the deposit, viz. *Gervilliæ*, *Trigoniæ*, *Terebratulæ*, &c. The surface exhibits a confused layer of large bones, more or less broken, and all of them, with the exception of a few vertebræ, lying without any order or connexion. The following were sufficiently exposed to admit of their characteristics being determined.

"Two femurs or thigh-bones, each 33 inches in length.

"One tibia and fibula, about 30 inches in length.

"Several metatarsal and phalangeal bones.

"Two *unguical* or *claw-bones* (the *distal phalangeal*): these very closely resemble the nail-bones of a very large land tortoise, and differ essentially from the claw-bone figured in the Geology of the South-East of England.

"Several caudal, and a lumbar vertebræ.

"Fragments of many ribs.

"Two of those extraordinary bones figured in the Geology of the South-East of England, plate iv. figs. 1, 2, and which probably are *clavicles*.

"A fragment of one tooth, and the impression of another, decidedly of the Iguanodon.

"There are portions of many other bones visible, and should the proprietor clear away the surrounding stone with care and skill, there can be no doubt that much additional light will be thrown on the osteology of the Iguanodon. Anxious to transmit this hasty notice in time for the Edinburgh New Philosophical Journal, I must not indulge in any remarks on this solitary and interesting fact of the occurrence of the remains of the gigantic lizard of the Wealds in the marine arenaceous beds of the chalk.

BRIGHTON, *June* 12. 1834."

*Earthquake in South America**.

An official account of the late dreadful earthquake in America was received on 7th June 1834. By this it appears that not only was the city of Pasto, with a population of 15,000 souls, destroyed, but that that of Popayan, with double the population, suffered the same fate. In Pasto all the religious houses, the churches of Jesus de Roi and San Andrew excepted, which escaped with the loss of their steeples, were destroyed. The cathedral and the churches of San Francisco, San Sebastian, Santiago, with their convents, and Santa Domingo, Mared, and Monjas had been completely dashed to pieces. Only three or four houses escaped, and those with much damage; and in most of the buildings not a vestige even of the foundation remained. The country around presented one scene of desolation, and the houseless and wretched people were exposed by day to the scorching sun, and by night to the chilling frosts peculiar to the climate. It appears the earthquake commenced at seven o'clock on the morning of the 20th January, and that for four hours the motion of the earth continued. A repetition of the shocks occurred on the 22d, and completed the ruin. All the villages in the neighbourhood of Pasto, Laguna, Mocondino, Buesquillo, Pejimdino, Puerres, Cunehalla, Tamondino, Tongovito, Gualmatan, Pandraco, and Tescuel, had been much injured, and the churches all destroyed. In the districts of Malatuy, Vacuanquir, Tambo, Bucaco, Funds, and the neighbouring parishes, great injury had been sustained. The commissioners appointed by the Government had reported that on the right of the large lake in the district of Sibundoy, a small rising ground had been observed that vomited forth from its bosom large pieces of rock, and that huge and perforated caverns had appeared in the neighbourhood; that of the surrounding desert of Bondoniella half had been swallowed up, and the other part so raised above the surface that it had formed a mountain of great height, like that lofty height between Sibundoy and Ajuadrico, which, in its formation, overspread a great deal of the original soil. The commissioners further state that this mountain has, from further convulsions of the earth, mouldered away, and covered the high roads, causing the formation of immense marshes in the neighbourhood; that portions of

* The above account is from one of the London periodicals.

the earth had precipitated themselves into the bed of the river Baldayaco, and obstructed its course, the sudden and impetuous overthrow of which had destroyed the lands and houses of the people of Santiago, forming its waters even as far as Putumac, having been in its course increased by tributary streams to the number of 90. The inhabitants had fled in great terror to the highest mountains. Almost the whole of the canton was overspread with large abysses, and the extreme of wretchedness prevailed throughout the country.

NEW PUBLICATIONS.

1. *Mathematical and Astronomical Tables for the Use of Students in Mathematics, Practical Astronomers, Surveyors, Engineers, and Navigators.* Second Edition, greatly enlarged and improved. By William Galbraith, A.M., Teacher of Mathematics, Edinburgh. Edinburgh: Oliver and Boyd; Simpkin and Marshall, and J. W. Norrie & Co. London.

On the publication of the first edition of this work, we recommended it strongly to the notice of those for whose use it was intended, and its success has been in accordance with our anticipations. The second edition is greatly enlarged by the introduction of much valuable matter, seldom to be met with in the usual publications on corresponding subjects. The author appears to have spared no pains in improving and enlarging his work. The ordinary Logarithmic Tables are much superior to those usually employed, by having proportional parts attached to the greater part of them, and almost always differences when the former are wanting. The table of sines, tangents, and secants, is in accordance with the views of the committee of the Astronomical Society of London, who have recommended double arguments in both arcs and time to be subjoined to tables for Nautical Astronomy, an advantage of great importance in many computations where time is involved. The best edition of Mr Ivory's Table of Astronomical Refractions is given by our author, the most accurate perhaps of all others hitherto published. The table of reduction of altitudes to the meridian, of the reduction of the sun's observed declination to the solstice; tables for reducing mean solar to sidereal time; for correcting the mean places of the stars, of which a pretty extensive catalogue of 160 is given, comprehending almost all those to the third magnitude inclusive; and many other tables too numerous to be particularly noticed here, are all of the best description, and will be found to be highly useful in every case of accuracy where the aid of such tables is required.

In short, this work contains one of the best small collections of useful tables any where to be found, printed in a neat manner; and the permanent accuracy of the greater part is secured by stereotype plates. We, therefore, confidently recommend it to the notice of the public for the valuable matter which it contains, for the accuracy of the tabular part of its contents, and for the general utility of the whole, and that too at a very moderate price.

2. *Illustrations of the Botany and other Branches of the Natural History of the Himalayan Mountains, and of the Flora of Cashmere.* By J. F. Royle, Esq. F.L.S., F.G.S., &c. No. 2. Folio. Parbury, Allen and Company. London, 1834.

We have great pleasure in announcing the publication of the second Number of Mr Royle's beautiful and valuable work; which contains, independently of nine well drawn and accurately coloured plates of new plants, and a geological section of the Himalayan mountains from Sidowra to the Shatool Pass, also a general description of the physical features of our Indian Empire, and the commencement of a series of observations on the botany of the Himalayan mountains, which cannot fail to interest the botanist and instruct the general reader.

3. *The Natural History of Animalcules; containing Descriptions of all the known Species of Infusoria, with instructions for procuring and viewing them, &c.: Illustrated by* 300 *magnified figures on steel.* By Andrew Pritchard, Esq. London: Whittaker and Co. 8vo. Pp. 104. 1834.

The discoveries of Professor Ehrenberg in regard to the form, structure, functions, and distribution of the Infusoria, first made known to the British public through the Edinburgh Philosophical Journal, excited a very general interest among the cultivators of the philosophy of zoology. The present volume of Mr Pritchard on the Infusoria will, we doubt not, be read with pleasure, and constantly consulted by all those who may engage in the study of this curious branch of Natural History.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Remarks on the Theory of the Elevation of Mountains. By GEORGE BELLAS GREENOUGH, Esq. F.R.S., &c. &c. &c. President of the Geological Society of London *.

AMONG the subjects which have for some years past engaged the thoughts of geologists, none perhaps has excited so general and intense an interest as the Theory of Elevation. I shall avail myself, therefore, of the present occasion to lay before you a connected statement of the scattered facts and opinions upon which it rests.

On entering upon this subject, it is necessary to understand distinctly what is meant by Elevation. Definitions have recently been decried, I think unwisely. The formation of definitions, it has been said, and the establishment of unerring distinctions, are among the last, and not the first steps of systematic knowledge. Equally true, and far more salutary, is the lesson that science cannot be advanced by equivocation. As in trading concerns fixed weights and measures are necessary guards against fraud, so in philosophical investigation words of definite meaning are indispensable securities against sophistry and self-delusion. Euclid did not end, he began with defining. Mathematical certainty has no other basis than mathematical precision, and the greater part of those absurdities which from time to time attach themselves to all other branches of knowledge, derive their subsistence from ambiguity of language and a dearth of definition.

* These *Remarks* form part of Mr Greenough's lately published Address, delivered before the Geological Society of London.

A torrent brings down a quantity of alluvial matter, and the plain on which it rests is said to be *elevated.*

An opening occurs in the earth; ejected ashes, scoriæ, and lava accumulate around it; a Monte Nuovo is formed; and the area it occupies is said to be *elevated.*

By the persevering labour of polypi, a coral reef gradually attains the surface of the ocean; and the fabric so constructed is said to be *elevated.*

A porous rock covers a rock that is not porous; the rain filters through the superincumbent bed; springs break out in the subjacent; and at last, for want of support, the porous rock, originally horizontal, acquires an inclined posture, one end being directed upwards, the other downwards; and the whole is said to be *elevated.*

An earthquake takes place at the mouth of a river; the sea is violently affected; a bar is formed at the entrance of a harbour from the washing in of new alluvion, or from some obstruction to the escape of the old; where a ship floated, a barge is a-ground; and the land is said to be *elevated.*

Such instances of elevation are common and incontestible; but elevation of this kind is quite different from that which forms the subject of my present inquiry.

By the term *Elevation*, I mean only the removal of any given object from a lower level to a higher level; consequently it is necessary, before I speak of an object as *elevated*, that I should be prepared to shew two things: first, the level at which it has stood; secondly, the level at which it stands.

That I might form a right opinion of the theory, the merits of which I am about to investigate, I have endeavoured to determine the site, the number, and the magnitude of those multifarious objects to which the attribute of elevation is continually applied. The attempt has proved unsuccessful: they are indefinite in place, in form, and in dimension. That mountains should be elevated is not surprising, but we are familiarized also with valleys of elevation.* In ancient times an island (Delos, for example), would alternately emerge from, and plunge be-

* Valleys of this nature are properly called by Mr Scrope "valleys of elevation and subsidence," or more concisely, "anticlinal valleys." See Scrope on Volcanoes, p. 213.

neath, the sea. Extensive provinces, nay, entire kingdoms, now perform the same feat. The existence of craters of elevation is by some still considered doubtful; but it is an accredited fact that mountains and mountain chains have risen, either *per saltum* or *per gradus*. All the strata have been raised; and all unstratified rocks would doubtless have been raised also, but that some have risen of themselves. The bed of the sea has been elevated again and again. Continents, too, have been raised, though "by an operation distinct from that which raised the primary strata."

The arguments advanced in favour of these doctrines are derived either from observation, or from induction.

It is stated by Von Hoff, that, in the year 1771, several tracts of land were upraised in Java, and that a new bank made its appearance opposite the mouth of the river Batavia. The authorities cited for the effect of this and several other earthquakes mentioned in the same place by this author, are Sir Stamford Raffles, John Prior's Voyage in the Indian Seas, and Hist. Gen. des Voy. tom. ii. p. 401. Mr Lyell has cited the first of these only, but no such fact is noted in either edition of the work of Sir Stamford Raffles. The other authorities adduced by Von Hoff I have been unable to consult; but from the Appendix to the Batavian Transactions (which contains an apparently authentic account of all the recorded earthquakes that have taken place in Java during a century and a half), it would seem, that, in the year 1771, in which the uprising is said to have happened in that island, there was no earthquake at all.

The earthquake of Chili in 1822 has been so much* insisted on, that it requires detailed consideration. Of this event an account by Mrs Graham is inserted in our Transactions. I am deeply sensible of the honour that lady conferred on the Society by her obliging compliance with the request which elicited her narrative, and it is only the importance of its contents which could induce me to subject them to the test of rigid examination.

According to this account, "it appeared, on the morning after the earthquake, that the whole line of coast from north to

* Bakewell's Geology, edition 4, pp. 98, 504. Lyell, vol. i. pp. 401, 455. De la Beche's Manual, edition 2. Scrope on Volcanoes, p. 209.

south, to the distance of above 100 miles, had been *raised* above its former level." But by what standard was the former level ascertained? Who, on the morrow of so fearful a catastrophe, could command sufficient leisure and calmness to determine and compute a series of changes, which extended 100 miles in length, and embraced (according to a statement in the Journal of Science), an estimated area of 100,000 square miles? How could a range of country so extensive be surveyed while the ground was still rocking, which it continued to do on that day, and for several successive months? What was the average number of observations per square mile? Who made, checked, and registered them? By what means did the surveyors acquaint themselves with what had been the levels and contour before the catastrophe took place, by which, as we are told, all the landmarks were removed, and the soundings at sea completely changed?

Mrs Graham states, that, by the dislodgment of snow from the mountains, and the consequent swelling of rivers and lakes, much detritus was brought to the coast; and further, that sand and mud were brought up through cracks to the surface. Amid so many agents it should not be easy to assign to each its share in the general result.

That fishes lay dead on the shore may prove only that there had been a storm. In her published travels, Mrs Graham represents them as lying on the beach, which may very well have been thrown up, as the Chesil bank has been, by a violent sea. Some muscles, oysters, &c. still adhered, she says, to the rocks on which they grew; but we know not the nature or dimensions of these rocks, whether fixed or drifted. The occurrence of a shelly beach above the actual sea-level is an observation which must not be lost sight of. I propose to speak of it hereafter: in the mean time be it recollected, that these beaches are said to occur along the shore at *various* heights, along the summit of the highest hills, and even among the Andes.

Neither in the paper of Mrs Graham, nor in the anonymous account published about the same time in the Journal of Science, can I find any paragraph to justify the position (which, from the seductive character of the work * in which it appears, may,

* Lyell, vol. i. p. 473.

if not now assailed, soon be deemed unassailable), that a district in Chili, 100,000 miles in area, "was *uplifted* to the average height of a foot or more; and the cubic contents of the *granitic mass* added in a few hours to the land." By what means we get the average I do not know. Mrs Graham says the alteration of level at Valparaiso was about three feet; at Quintero about four feet; but *the granitic mass!* has the geological structure of Chili been sufficiently examined to assure us that granite extends over 100,000 square miles?

In the well known work of Molina, a Jesuit who passed the greater part of his life in Chili, and wrote a natural history of that country, I find no ground for supposing that in any earthquakes which took place there from the time the Spaniards first landed on its shores to the date of his publication, any similar phenomena had been noticed. Moreover, the statement of Mrs Graham, and of the writer before alluded to, respecting the *elevation of land* which occurred during the earthquake of 1822, has not been confirmed by Captain King, nor by any naval officer or naturalist who has since visited that region, though many have visited it who had heard the circumstance, and who would willingly have corroborated it if they could. But they saw no traces of such an event; and the natives with whom they conversed, neither recollected nor could be induced to believe it.

The 16th number of the "Mercurio Chileno," a scientific journal, contains an account of this earthquake, by Don Camilo Enriquez, which I have not been able to procure. A later number refers to this account, and to another published in the Abeija Argentina, a work of considerable reputation, which, by the kindness of Mr Woodbine Parish, I have been enabled to consult. The account there given of the earthquake of 1822 is strongly recommended to the reader, "as a sensible straightforward description of what actually took place, without the high colouring in which ignorance, and terror, and exaggeration are apt to indulge."

No notice is here taken of the permanent *elevation of the land*, and the account concludes thus:—

"The earth certainly cracked in places that were sandy or marshy. I saw cracks, too, in some of the hills, but mostly in the low nook where much earth had run together; the sea was not much altered, it retired a little, but came back to its old place. Don Onofri Bunster, who, on the night of the earthquake, was walking on the shore at Valparaiso, in front of his house, had a mind to go up on the hill, but could not, so great was the quantity of falling dust and stones: he repaired to his boat therefore, and with some difficulty got aboard; this done, he made observations on the motion of the sea; on sounding, the depth was thirteen fathoms; he heaved the lead a second time, and the depth was no more than eight fathoms: this alternate ebbing and flowing lasted the whole night, *but did not the slightest harm on shore.*"

These are the only cases I remember to have met with in which the testimony of eye-witnesses has been adduced to prove the rise of land by earthquakes. That such rise may have taken place at different times without being recorded, perhaps even without being observed, is not very improbable; but if I am to pronounce a verdict according to the evidence, I believe there is not as yet one well authenticated instance in any part of the world, of a non-volcanic rock having been seen to rise above its natural level in consequence of an earthquake.

Before I quit this subject, it may not be amiss to mention, that, on comparing the times at which the successive shocks took place in Chili, as given by Mrs Graham, and the other authorities to which I have had occasion to refer, the discrepancy is extraordinary.

I have already intimated in a few words, my opinion as to the sense in which land can be said *to be elevated by means of volcanoes.* Of these, Vesuvius is, perhaps, the most constantly observed; and among the innumerable authors who have described its effects, from the time of Pliny down to the present day, not one pretends that the Apennine limestone, close at hand, has been in the least raised by that volcano. We shall do well to bear this in mind, when we have occasion to consider the height at which tertiary shells are found on Etna. That those shells belong to beds thrown up by Etna, is a doctrine founded upon induction, not upon experience. As far as experience goes, we have no reason to think that Etna, in its most violent paroxysms, will ever raise those tertiary strata above their present level.

Leaving these scenes of paroxysmal violence, let us next inquire whether there may not be going on, in the calmest seasons and in the stillest countries, a *chronic and almost imperceptible impulsion of land upwards.*

As early as the time of Swedenborg, who wrote in 1715, it was observed that the level of the Baltic and German Oceans was on the decline. About the middle of the last century an animated and long-continued discussion took place in Sweden, first as to the cause of this phenomenon, and then as to its reality. Hellant of Tornea, who had been assured of the fact by his father, an old boatman, and who afterwards witnessed it himself, bequeathed all he had to the Academy of Sciences, on condition that they should proceed with the investigation: the sum was small, but the bequest answered the purpose. Some of the members of the Academy made marks on exposed cliffs and in sheltered bays, recording the day on which the marks were made, and their then height above the water. The Baltic affords great facility to those who conduct such experiments, as there is no tide, nor any other circumstance to affect its level, except unequal pressure of the atmosphere on its surface and on that of the ocean: this produces a variation which is curiously exemplified at Lake Malar, near Stockholm. As the barometer rises or falls, the Baltic will flow into the lake, or the lake into the Baltic. The variation resulting from the inequality of atmospheric pressure, however, is trifling. In sheltered spots mosses and lichens grown down to the water's edge, and thus form a natural register of its level. Upon this line of vegetation marks were fixed, which now stand in many places two feet above the surface of the water.

In the year 1820–1, Bruncrona visited the old marks, measured the height of each above the line of vegetation, fixed new marks, and made a report to the Academy. With this Report has been published an Appendix by Halestrom, containing an account of measurements made by himself and others along the coast of Bothnia. From these documents it would appear:—1. That along the whole coast of the Baltic the water is lower in respect to the land than it used to be. 2. That the amount of variation is not uniform. Hence it follows, that either the sea and land have both undergone a *change of level*, or the land only; a change of level in the sea only will not explain the phenomena.

A quarter of a century has now elapsed since Mr von Buch declared his conviction that the surface of Sweden was slowly rising all the way from Frederickshall to Abo; and added, that the rise might probably extend into Russia. Of the truth of that doctrine the presumption is so strong, as to demand that similar experiments and observations should be instituted and continued for a series of years in other countries, with a view to determine whether any change of level is slowly taking place in these also. The British Association for the Advancement of Science have already obeyed the call. A committee has been appointed to procure satisfactory data to determine this question as far as relates to the coasts of Great Britain and Ireland, and I cannot but hope that similar investigations will also be set on foot along the coasts of France and Italy, and eventually be extended to many of our colonial possessions.

The inductive arguments in favour of the *elevation of land*, whatever the size, and whatever the amount of rise, are founded chiefly on the following circumstances:—1. The height of sedimentary beds and marine bodies, whether corresponding or not to those of adjacent seas, or of the actual globe. 2. The height of terraces resembling sea-beaches. 3. The height of ripple-marks. 4. The change of posture which horizontal strata undergo in the neighbourhood of "unstratified rocks." 5. The various heights at which the same rocks occur in different parts of their course. 6. The anticlinal posture of strata frequent in, though not confined to, mountain chains. 7. The arched or domed configuration of some strata. 8. The occurrence of coral, apparently recent, high above the present surface of the sea. 9. The position of ancient buildings, viz. the temple of Serapis at Puzzoli, &c. I have not time to consider these arguments in detail; each deserves to form the subject of a separate treatise. Some of them prove, not elevation, but only change of level, which subsidence would explain equally well. Some prove local disturbance, whereby one portion may have been thrown up, the other down. Some, again, afford a fair presumption of real *local* elevation or ascent. Most of them are good to a certain point: all are continually overstrained; and I am frequently astonished to observe how prodigious the weight,—how slender the string that supports it.

The assigned *causes of elevation* are exceedingly various. One author raises the bottom of the sea by earthquakes; another, by subterranean fire; another, by aqueous vapour; another, by the contact of water with the metallic bases of the earths and alkalis. Heim ascribes it to gas; Playfair, to expansive force acting from beneath; Necker de Saussure connects it with magnetism; Wrede, with a slow continuous change in the position of the axis of the earth; Leslie figured to himself a stratum of concentrated atmospheric air under the ocean, to be applied, I suppose, to the same purpose.

It is impossible, within the narrow limits of this discourse, that I can enter into the merits of these and other hypotheses seriatim. I must therefore throw them into two classes, the first of explosive forces, the second of sustaining forces; they are one and the same in Plutonic language, but still it will be convenient to separate them.

That explosive forces exist, or may exist, under the surface, no one can deny; but I cannot adopt the opinion (however high the authority from which it comes) that "in volcanic eruptions we find a power competent to raise *continents* out of the ocean." The force we find in volcanic eruptions is limited in time, place, and action; it fuses bodies of easy fusibility; it tosses up those that are refractory, and thus forms either a current of lava or a shower of stones, scoriæ, and ashes. What resemblance is there between this operation and the rise of a continent? With more propriety might it have been said, that in a mole-hill we behold the action of a cause competent to raise mountains.

If by *continent* is meant a whole continent, and nothing but a continent, its rise, provided this happened only once, would seem difficult to understand; but to me still more incomprehensible is the confident assurance we continually receive from writers of high and deserved reputation, that this event has happened again and again. Before we admit the submersion of a continent, we must admit either that, at a period immediately preceding that catastrophe, there existed under the land a cavity large enough to contain the continent about to be submerged, or that, during the process, the subjacent beds shrunk in consequence of a reduction of the temperature, and to such an extent that the contraction in a vertical line equalled the distance from the level of the highest tops of the continent to that of the surrounding ocean. In like manner, before we can admit the elevation of a continent, we must admit either that, at a period immediately preceding that catastrophe, there happened an inroad of sustaining matter equal in thickness and in extent to the continent about to be uplifted, or that, during the process, the subjacent beds expanded in consequence of an increase of temperature, and to such an extent that the expansion in a vertical line equalled the distance from the level of the highest tops of the continent to that of the surrounding ocean. These, therefore, are the events which we are taught to credit, as having taken place again and again, notwithstanding the tendency which caloric has to diffuse itself, and the apparently unaltered dimensions of the fissures and local caverns by which the strata are so often separated or intersected.

I will not expend more of your time in arguing against such doctrines. All men are more or less lovers of the marvellous, but few, I think, will upon reflection approve such marvels as these.

Solids, fluids and aeriform substances exist, we know, in the interior of the earth, and expand by heat, which exists there likewise. All of these, therefore, are fit *agents of elevation*, subject to certain conditions.

Dr Daubeny attributes the liquefaction of lava, the throwing up of ashes, and all other phenomena of disturbance attendant on volcanic eruptions, to the action of water upon the metallic bases. This cause is not opposed to experience, and appears well proportioned to the effect, which is sudden, violent, occasional, temporary, accompanied by heat and by flame. To me, at least, it seems far more satisfactory than the explanation of those who ascribe the effect to the elastic power of subterranean fires, repressed in one place and relieved in another, or to the undulations of a heated nucleus.

A heated *central nucleus* is a mere invention of fancy, traceable, I believe, to no other source than the hope of obtaining a good argument from the multiplication of bad ones. To the

Huttonian and every other geological sectary who relies on this postulate, I say, be cautious: "*incedis per ignes dolosos.*"

The only observation I recollect to have met with in favour of central heat is, that the deepest mines are the warmest—be it so! Might not a geologist by parity of reasoning argue thus? In travelling from Rome to Chamonix, the country becomes continually more and more mountainous; some of the peaks of Chamonix are from ten to fifteen thousand feet above the level of the sea. Imagine, therefore, what they must be at Hamburgh!!

If mines derive their temperature from heat lodged in the centre of the earth, the temperature ought to vary with their distance from the centre; and, therefore, since the earth is an oblate spheroid, the mines of Scandinavia ought at the same depth from the surface to be proportionally warmer than those of tropical countries; a result which has never been, I believe, even suspected.

The existence of *central heat*, in the sense and to the extent assumed in the Huttonian theory, is contrary to all our experience. If heat there be in the centre of the globe, it must have the properties of heat and none other. I ask not how the heat originally was lodged in that situation, for the origin of all things is obscure; but I ask why, in the countless succession of ages which the Huttonian requires, the heat has not passed away by conduction, and if it has passed away, by what other heat it has been replaced?

Dr Chalmers, in speaking of Sir Isaac Newton, observes, that it was a "distinguishing and characteristic feature of his great mind, that it kept a tenacious hold of every position which had proof to substantiate it; but a more leading peculiarity was, that it put a most determined exclusion on every position destitute of such proof. The strength and soundness of Newton's philosophy was evinced as much by his decision on those doctrines of science which he rejected, as by his demonstration of those doctrines of science which he was the first to propose. He expatiated in a lofty region, where he met with much to solicit his fancy, and tempt him to devious speculation. He might easily have found amusement in intellectual pictures; he might easily have palmed loose and confident plausibilities of his own on the world. But no, he kept by his demonstrations, his measurements, and his proofs."

Gentlemen, let us, as far as is consistent with the nature of geological investigation, show the strength and soundness of our philosophy in the same manner.

That heat of considerable intensity prevails occasionally, in certain places, at some depth, is all that we have as yet clearly established. Whether that heat is permanent, whether it is generally diffused, whether it is central, are questions of mere speculation.

Intimately connected with the hypothesis of *central heat* is that of *refrigeration.*

It has been observed by one of our members, that "the remains, both of the animal and vegetable kingdom, preserved in strata of different ages, indicate that there has been a great diminution of temperature throughout the northern hemisphere, in the latitudes now occupied by Europe, Asia, and America; the change has extended to the arctic circle as well as to the temperate zone; the heat and humidity of the air, and the uniformity of climate, appear to have been most remarkable when the oldest strata hitherto discovered were formed. The approximation of a climate similar to that now enjoyed in these latitudes, does not commence till the æra of the formations termed tertiary; and while the different tertiary rocks were deposited in succession, the temperature seems to have been still further lowered, and to have continued to diminish gradually even after the appearance of a great portion of existing species upon the earth." The little knowledge we have of the fossil productions of countries south of the temperate zone, induces me to believe that these observations are as applicable to the southern hemisphere as to the northern.

This *refrigeration,* one of the most undoubted facts in geology, is supposed by the Huttonians, and, if I mistake not, by M. Elie de Beaumont and others, to arise from a decrease of the *central heat;* an opinion, however, which cannot I think be supported.

We know of one method only by which central heat, if it exists, can pass from the earth, viz. by radiation. It cannot pass by conduction. Conduction implies conductors, which in empty space are not to be procured *; but the radiation of heat, at low temperatures, is so slight that it is scarcely sensible at 100° of Fahrenheit's thermometer, a temperature twice as great as the medium temperature of the surface of the globe at this time. The temperature of the earth's surface has been shewn by Fourier to be as constant as are the dimensions of its orbit, and the period of its annual revolution. Laplace observes, that our planet has undergone no contraction of size during the last 2000 years; consequently there has been no sensible *refrigeration* during that period, and the last seculum of M. de Beaumont has already extended to more than twice the length of a millennium.

Another argument, or rather postulate, has been adduced in favour of *central heat*,—the fusion of unstratified rocks, and their forcible injection into the stratified.

Gentlemen, I have confessed to you again and again, that I am not aware, nor has any one as yet informed me, by what test stratified and unstratified rocks can be distinguished; the only test I know is the good will and pleasure of those who make the distinction. The followers of Pluto seize and appropriate to his use as many rocks as they think proper. By virtue of such seizure, these rocks become necessarily unstratified: why so? because if stratified they would be no longer Plutonic. Stratification I know is a question to be determined not by the senses but by the fancy; otherwise, I would say, that the magnificent range of basaltic cliff, which extends from the county of Derry along the coast of Antrim as far as Fairhead, is as distinctly stratified as any mountain-limestone, oolite or chalk, in Great Britain.

However, I waive this objection, as it leads me away from my subject, and return to the consideration of *central heat*. Have those who believe in this agent ever taken into their account the nature of the substances said to have been fused? Many of the trap rocks, not all of them, (for the family is large, and many of its members have been introduced into it, not by nature but by adoption), I attribute to the agency of the causes which have produced lava, causes which, comparatively speak-

* See Comparative View of the Huttonian and Neptunian Systems of Geology.

ing, I do not believe to be very deep-seated. These rocks I put out of consideration for the present; the remarks about to be offered apply to granite and its congeners, under which head I would give to every one full liberty to include or reject quartz-rock, gneiss, mica-slate, eurite, cipollino, hornblende-rock, serpentine, &c. Some or all of these, it is the bounden duty of *central heat* to fuse and to eject.

Such and so limited are the means of chemistry, that of many substances thus brought within the sphere of our inquiries, the point of fusion is at this day unascertained. The author of the masterly publication before adverted to, brought together many useful observations upon this subject. He observes that "Lavoisier could not melt a particle of carbonate of lime by the intense heat of a burning mirror, and that quartz, according to Saussure, requires for its fusion a temperature = 4043° of Wedgwood's pyrometer, glass requiring at a medium only 30° of the same scale."

That the difficulty which here suggests itself, of providing, in the absence even of imaginary fuel, a supply of imprisoned heat sufficient to fuse the substances I have mentioned and others scarcely less refractory, may be mitigated by extending the time employed in the process, or by the aid of compression and other circumstances, I am ready to admit; but, in the most favourable view of the case, the heat wanted (when we consider the thickness and extent of these rocks, comprising entire mountains and mountain chains), must be prodigious; and I cannot but admire the singular taste of those geological speculators, who, enjoying the free range of the globe, have deposited their caloric exactly in that spot in which it can be of least use to them. The inconvenience of this distribution becomes still more apparent, when it is recollected that fusion is not all that is necessary, but that, when fused, these substances must be propelled in a determinate direction and with sufficient force, in many instances, to raise the bed of the sea to the height of an alpine chain. I will not attempt to point out to you the way in which this is accomplished, but confess at once that I do not understand it.

And yet it appears certain that the surface of our planet has become cooler and cooler, from the period when organic life commenced to the tertiary epoch. If this cannot be explained by the escape of heat, there remains only one other mode of explaining it,—a continually diminishing supply. The latter is the explanation offered by Mr Lubbock. Sir John Herschel also has brought into view causes within the range of physical astronomy, which, independently of a loss of internal heat, produce a slow but certain diminution of temperature on the surface of our globe*. These auxiliaries, however, are insufficient.

Mr Lyell has offered another solution of the problem, depending not on celestial but terrestrial causes. The chapter that contains it abounds in valuable information and ingenious reasoning; but when the author tells us that † in every country "*the land has been in some parts raised, in others depressed, by which, and other ceaseless changes, the configuration of the earth's surface has been remodelled again and again since it was the habitation of organic beings, and the bed of the ocean lifted up to the height of the highest mountains*," I cannot but wish that he had stated this as an opinion, not as a fact.

All these theories have one defect in common; they do not meet the whole of the case. We have to explain not only the *cooling gradual* during the long interval that occurred between the formation of the carboniferous beds and the chalk, but also the *sudden chill* which followed, and seems to have continued from that time to this. There is yet another element to be taken into account. The coal-beds of Melville Island contain various plants, natives of the country where they are found, and which, if we may trust analogy, require for their healthy growth, or for their growth at all, not only tropical heat ‡, but a tropical apportionment of the periods of exertion and repose. It is a botanical impossibility that such plants could have flourished in a region in which they must have been stimulated by months

* The Baobab tree of Senegal is supposed by Adanson to have attained the age of 5150 years, and De Candolle attributes to the *Cupressus disticha* of Mexico a still greater longevity. (Lyell, vol. iii. p. 99.)

If these opinions be correct, it seems improbable that any great change either of level or climate can have taken place at these spots within the last 5000 years.

† Principles of Geology, vol. i. p. 113.

‡ Since this passage was written, doubts have been expressed whether the specimens of these plants preserved at the British Museum are sufficiently distinct to warrant the inference.

of continuous light, and paralysed by months of uninterrupted darkness. The distribution of light, therefore, as well as of heat, must formerly have been different from what it is at present.

To meet this further difficulty, recourse is had to physical astronomy, which gives us the *precession of the equinoxes, and a shifting axis of rotation:* but the periodical changes of astronomers are insufficient to explain the phenomena to which I have just drawn your attention. It has therefore been suggested that a greater change may, in the course of ages, have been produced on the axis of the earth's rotation by some foreign cause, say the *collision of a comet.*

Such change is undoubtedly possible, but of possibilities there is no end, and we must circumscribe our researches to render them useful. Sir John Herschel gives us no encouragement, therefore, to proceed with this speculation. Mr Conybeare also dissuades us from it, but by an argument which to me at least appears inconclusive.

His argument, founded upon the lunar theory, is this,—that the internal strata of the earth are ellipses parallel to its external outline, their centres being coincident, and their axes identical with that of the surface. The present axis of the earth must therefore have been its axis from the beginning. It may have been so, yet I should like to be told by what process the form of the internal strata of the earth had been so nicely determined. Possibly, however, I may not understand the expression "internal *strata.*" All I believe to be ascertained is, that of corresponding sections of the interior the density is nearly the same, and if so, my inference is, not that the earth has never changed its axis of rotation, but that, if it has done so, the interior was then sufficiently pliant to accommodate itself to the change.

A much more formidable objection to the employment of such a cause is, that, if once called in, we must take it with all its consequences. The effects produced by it will not be what we wish performed, but what its nature obliges it to perform. In explaining the phenomena of Melville Island, it might render inexplicable those of the rest of the world. If we choose to change the axis upon which the earth revolves, let us at least fix upon the best time for doing it; now, What is that time? Immediately after the formation of the carboniferous series? The reduction of temperature at that epoch was inconsiderable; tropical plants and animals are found in the lias, in the oolite series, in the chalk. A much more convenient time would be on the first appearance of the tertiary rocks; but however satisfactory it might be to trace to such a cause the violent changes and disturbances which appear to have taken place about that period in all other parts of the world, I am afraid our satisfaction would be greatly diminished on finding that Gosau and Maestricht * escaped unhurt.

Be the cause what it may, the effect is certain. The temperature of the crust of the earth must have been higher when the coal-measures were deposited than now, and we have reason to think it was still higher at antecedent periods. That a considerable degree of heat still exists, either partially or generally, at no great distance from the surface, appears from thermal springs and volcanoes.

I am aware that the doctrine of *internal cavities* has been regarded as visionary; and in the extent to which it was carried by some of the old Cosmogonists it was so; but that comparatively near to the surface there are, I do not say vacuities, but large spaces unoccupied by solid matter, is not only probable, but almost proved. It seems indeed to be a necessary consequence of the structure of the crust of the earth. No miner has ever got to the bottom of a vein, and a vein itself is often a half empty pipe or fissure. The correspondence of the phases of distant volcanoes, the continuous ranges of their eruptive openings, the vast extent of territory shaken simultaneously by their convulsions, are so many proofs of communication below the surface. The bulk of the ejected matter cannot be less than that of the concreted ejections which we see; for at the temperature of fusion it is greater than at a lower temperature, and for every foot of matter ejected it is necessary to provide a substitute in the place which it occupied.

The continuous streams of lava which issued in Iceland, on one occasion, attained the length of forty or fifty miles. But the bulk of volcanic matter presented to view does not enable

* See the descriptions of these in Geol. Trans.

us to form a correct estimate of the quantity of matter ejected; we must take further into account the combustible substances which have vanished, the gases which have escaped, the dust and ashes which, projected into the air, have fallen many miles distant from the place of explosion *. Then only can we entertain a just idea of the cavities that must have been created in the interior of the earth by the escape of a mass of matter competent to produce an Etna or a Chimboraço. Such cavities are ill suited to support such mountains; La Metherie therefore supposes cavities to be at a distance, and volcanic matter to flow from these through long galleries and fissures of communication. Nor have we in volcanic countries alone decisive evidence of the existence of subterranean cavities. No rock is exempt from fissures: in thick beds of limestone, fissures and caverns are exceedingly abundant; and the extent of these last is sometimes prodigious. Who has not heard of the grotto of Antiparos? of the caverns of Carinthia and Carniola, the content of which amounts to some hundred thousand cubic feet? of the Kingston Cave recently explored near Michelstown in Ireland?

To the frequency of caverns and openings, by whatever name designated, I ascribe many of the inequalities which vary the surface of the earth; such openings, I conceive, produce phenomena sometimes of subsidence, sometimes of elevation. I cannot entertain a doubt that many of the tilts and contortions of strata usually ascribed to *soulèvement* have been occasioned solely by want of adequate support.

The Duchy of Finland exhibits an endless series of lakes filling up the hollows of a granitic surface. Let me be allowed a similar series of subterranean lakes occupying similar basins beneath the level of the Baltic, and receiving, by means of fissures extending up to the summits of the Scandinavian chain, a continual supply of water which has no outlet; in other words, let me be allowed the use of hydrostatic pressure; and without having recourse to central heat or secular refrigeration, I think I shall be able to account, without difficulty, not by a general and uniform rising, but by a number of unequal and partial

* In 1783, a submarine volcano off the coast of Iceland ejected so much pumice that the ocean was covered to a distance of 150 miles, and ships were considerably impeded in their course.

risings, for the phenomena observed along the shores of the Baltic.

Steam is often referred to as capable of producing the same result, nor will I deny that it might do so under favourable circumstances; but I apprehend steam rarely does act in nature under such circumstances; for its existence depends on the access of heat, and its force on close confinement, contingencies not very likely to occur in the porous and fissured strata of the earth. Any of the various gases, if compressed, might also become agents of elevation, but only under the same conditions as steam.

I have reserved for the last the popular theory which accounts for elevation by the forcible *inroad of igneous rocks into sedimentary.*

To put this theory to the test, it is natural to inquire, what igneous rocks are? My answer is, whatever geological speculators think proper to call so. The late Professor Dugald Stewart cautioned us strongly, though, alas! in vain, to avoid the language of theory. "Appearances," he observes, "should always be described in terms which involve no opinion as to their causes. These are the objects of separate examination, and will be best understood if the facts are given fairly, without any dependence on what should yet be considered as unknown; this rule is very essential where the facts are in a certain degree complicated."

In dealing out to rocks the appellation of *igneous*, some geologists are more liberal than others. I have not time to enumerate the various rocks which enjoy this title, still less to investigate their respective claims to retain it. I will therefore content myself with observing, that in the scantiest catalogue they are many in number, and consequently, if ejected in a state of fusion, must have been ejected from different reservoirs and cauldrons, not from a *central* cauldron.

That any rock whatever was originally igneous, is a gratuitous assumption. Lavas themselves may be, and probably are, in very many cases, rocks not originally igneous, but rocks which have been exposed at one time or other to the action of fire.

Granite is one of the rocks most usually considered as an *agent in elevation,* for what reason I am at a loss to discover.

Solid granite has no inherent principle of motion; if it move, it can only be by virtue of the impulsion it has received from some other body, not in consequence of its igneous origin or its want of stratification. The disturbances of strata that adjoin granite are not more constant, nor more striking, nor more extensive, than those of strata far remote from it, as for instance the limestone shales of Derbyshire or the coal-beds of Liege. Granite veins are too small to raise mountains, and the changes or anomalies that take place at the junction of granite with other rocks, whatever else they may prove, appear to me to have no bearing on the question of *elevation.* On the other hand, the arguments adduced against the doctrine that granite while fluid has been forcibly injected from beneath into its present position, are, to my mind, conclusive, especially that which is founded on the frequent transition which takes place from granite to the rocks that adjoin it. We find a continuous series from granite through gneiss and mica-slate to clay-slates and the fossiliferous slates; and it is not possible to stop at any point of this progress, and to say in which direction the tendency is strongest. If the gradation were single, the difficulty would be great, but what shall we say to a repetition of such gradations? In Mr Weaver's paper on the East of Ireland, two detailed sections are given, in one of which, more than six layers of granite alternate with as many of mica-slate, and in the other five alternations of the same kind occur, the rocks in each instance forming bands from three to seventy fathoms in thickness.

The reliance which some authors place on granite and other unstratified rocks, as *agents of elevation*, is to me very extraordinary; let one instance suffice. At Castrogiovanni in Sicily, the Pleiocene beds attain an altitude of 3000 feet; hence it has been inferred, that *since these beds were deposited, there has been formed and introduced into the beds subjacent, a body of granite, sienite, porphyry, or other crystalline and unstratified rocks, 3000 feet in thickness.* This supposition is said to be necessary, but since I do not see the necessity, I will venture another supposition, viz. that Etna has not risen to the height of 10,000 feet, without occasioning large cavities in its neighbourhood, some of them submarine; that Castrogiovanni is situate over one of these; that the Pleiocene strata have closed the cavity and rendered it water-tight, except on the side of Etna, from whose lofty flanks and cloud-capped crater the caverns beneath are regularly supplied by fissures with rain-water and melted snow. Let the author grant me so much,—I ask no more. The hydrostatic paradox has tripped up the hills of the geological one, and I behold my pleiocene beds mounted at once on a pedestal 3000 feet high, and capable of still further promotion.

If the explanation here offered meets the case of Castrogiovanni, it will equally account for the height of the tertiary beds in different parts of the Val di Noto, and for similar phenomena in every country which is or has been formerly the site of volcanic eruptions.

To the appearances on the Gulf of St Lawrence, described by Captain Bayfield, I have already adverted.

My predecessor directed your attention, last year, to the existence in the Morea of four or five distinct ranges of ancient sea cliffs, marked at different levels in the limestone escarpments by lithodomous perforations, lines of littoral and sea-worn caverns, and other striking proofs of former tidal action. Similar terraces have been observed in Sicily, in Chili, in the Gulf of St Lawrence, and various other places. At Uddevalla in Sweden, are ancient beaches with shells of living species, 200 feet above the level of the Baltic, a height strikingly disproportionate to the very moderate rise ascertained to have taken place in other parts of the Scandinavian coast: many examples of similar phenomena have been found in Great Britain. It would be rash to offer a solution of these phenomena in the gross. Every individual case deserves separate examination. All I undertake at present is to put a new key into the hands of the decipherer.

It was my intention, on commencing this address, to have discussed, at some length, the theory of M. Elie de Beaumont, but there is not time now to do it justice. He belongs to that class of authors whose opinions, right or wrong, always instruct me. There is no part of his theory which does not evince thought and diligence, a habit of correct observation, and an enlarged mind. In some respects I differ from him, and it will not be difficult to infer, from what I have already said, wherein the difference consists. Should these observations engage his notice, I would beg him to consider whether the disturbances in the

Alps and elsewhere have not been generalized rather more than they will bear, whether the tilts and upliftings may not have taken place bit by bit at various epochs, and whether, if the *secular refrigeration of the globe* cannot be established, and *central heat* be an *ignis fatuus*, his attention may not be usefully directed to more partial but better authenticated sources of disturbance and elevation.

Allow me, in conclusion, to say a few words upon a subject in connexion with which my name has of late been brought forward much more prominently than I could have desired,—I mean *Diluvial Action.*

Some fourteen years ago I advanced an opinion, founded altogether upon physical and geological considerations, that the entire earth had, at an unknown period, (as far as that word implies any determinate portion of time), been covered by one general but temporary deluge. The opinion was not hastily formed. My reasoning rested on the facts which had then come before me. My acquaintance with physical and geological nature is now extended; and that more extended acquaintance would be entirely wasted upon me, if the opinions which it will no longer allow me to retain, it did not also induce me to rectify. New data have flowed in, and, with the frankness of one of my predecessors, I also now read my recantation.

The varied and accurate researches which have been instituted of late years throughout and far beyond the limits of Europe, all tend to this conclusion,—that the geological schools of Paris, Freyberg, and London, have been accustomed to rate too low the various forces which are still modifying, and always have modified, the external form of the earth. What the value of those forces may be in each case, or what their relative value, will continue for many years a subject of discussion; but that their aggregate effect greatly surpasses all our early estimates, is, I believe, incontestibly established. To Mr Lyell is eminently due the merit of having awakened us to a sense of our error in this respect. The vast mass of evidence which he has brought together, in illustration of what may be called *Diurnal Geology*, convinces me, that if, five thousand years ago, a deluge did sweep over the entire globe, its traces can no longer be distinguished from more modern and local disturbances. The first sight of those comparatively recent assemblages of strata, which he designates the *Eocene*, *Meiocene*, and *Pleiocene* Formations, (unknown but a few years ago, though diffused as extensively as many which were then honoured with the title of universal), shews the extreme difficulty of distinguishing their detritus from what we have been accustomed to esteem diluvium. The fossil contents of these formations strongly confirm this argument. M. Deshayes has shewn that they belong to a series unbroken by any great intervals, and that, if they be divided from the secondary strata, the chasm can have no relation to any such event as is called the Flood.

Further, the elephants and other animals once supposed to be exclusively *diluvial*, are now admitted to be referrible to two or three distinct epochs; and it is highly probable that the blocks of the Jura Mountains, of the north of Germany, of the north of Italy, of Cumberland, Westmoreland, &c. are not the waifs and strays of one, but of several successive inundations.

It is, Gentlemen, a well-known rule of such institutions as ours, that the "authors alone are responsible for the facts and opinions contained in their respective productions." Under that feeling have I spoken on the present occasion, and having freely set before you what has occurred to me on some points of general interest to our science at this time, I think it my duty, in concluding this address, to disclaim and deprecate any attempt to connect what I have here expressed, with the general sentiments of the Geological Society. The opinions I have uttered are my own, and I should be sorry that more importance should be attached to them than they intrinsically deserve, from the accident of their having been delivered from this Chair. Had not the whole responsibility fallen on myself, I should have hesitated, or perhaps altogether forborne, to bring before you opinions, several of which, I know, are little in accordance with those of some of the most distinguished members of our Association.

Remarks on a paper by Dr Stark, On the Influence of Colour on Heat, &c. in the Philos. Trans. 1833, part ii.; *and on an Historical Account of Experiments relating to the subject, in the Edinburgh New Philosophical Journal,* No. 33. By the Rev. BADEN POWELL, M.A. F.R.S., Savilian Professor of Geometry, Oxford. Communicated by the Author.

ON the appearance of the part of the Philosophical Transactions just referred to, my attention was immediately drawn to the contents of Dr Stark's paper, as belonging to a subject to which, some years since, I had paid particular attention. Several remarks occurred to me on the first perusal of it, which I conceived it might not be unserviceable to the cause of scientific truth to bring forward, but which circumstances have prevented me from putting into a form for publication till now. Since the greater part was written, I have seen the same author's paper in the Edinburgh New Philosophical Journal, and in adding some remarks on that paper also, my observations may perhaps be found to have assumed an irregular form, which I have not now time to correct. To the general doctrine maintained by Dr Stark I confess I entertain considerable objections; though I most readily acknowledge the interest of his researches, and the skill and ability displayed in them. The subject is one which certainly no previous experimenter has attempted, though I believe such a conclusion as Dr Stark's has often been vaguely stated by writers on the subject of heat; but this portion of the science of heat seems to have been in an especial degree abandoned to inaccuracy. It, in fact, is hardly yet recognised as a separate science; and it is so slightly connected with one or two other branches, that it seldom meets with any original or precise discussion from systematic writers on those branches. I observe, however, that Dr Stark's conclusion has received the sanction of Dr Prout in his Bridgewater Treatise; and as it certainly possesses much plausibility, I conceive it the more necessary to explain freely the nature of my doubts in regard to it. I can only hope that nothing will be found in the manner of stating my objections, which can reasonably give offence to the author;

and that he will perceive I have no other object than the endeavour to promote the cause of philosophical truth.

Without further preface, then, I will commence with some general remarks, bearing on the nature of the whole inquiry, before I proceed to consider the experimental evidence in detail.

After referring briefly to the labours of preceding experimenters, on the relations of substances to heat, the author remarks, that they have all stopped short as it were at the point where the influence of colour comes into consideration; and this appears to have been in several cases owing to a conviction which Dr Stark regards as unfounded, that the inquiry presents difficulties of a kind absolutely insuperable, and which are essentially inherent in it. A quotation which he gives from Sir J. Leslie tends to put these difficulties before us in the clearest point of view:—

"On the whole, it appears exceedingly doubtful if any influence can be justly ascribed to colour. But the question is *incapable of being positively determined*, since no substance can be made to assume different colours without at the same time changing its internal structure." Notwithstanding this explicit statement, the author expresses some surprise that Sir J. Leslie and Count Rumford should have stopped short in their researches at this point, when so wide a field of unknown properties lay in view.

The experiments of Sir J. Leslie have, I conceive, distinctly established the general fact, that there is *some peculiarity* of *texture*, or arrangement of the particles of the surface, which gives an increased power of absorbing, and reciprocally of radiating, *simple* heat. In my report on Radiant Heat, published in the Reports of the British Association (i. 203), I have collected in one point of view all the principal results. It is true that these cannot be looked upon as possessing any high degree of precision; chiefly from this circumstance, that the thickness, density, &c. of the coating could not be in all cases precisely equalized; and thus other circumstances than the nature of the *surface* must have affected the results. In some cases I believe the distinguished author took particular pains to equalize the coatings where their nature admitted of it; as by taking precisely equal quantities in the first instance, which were afterwards dissolved, or spread on the surface. But as *approximate* results, in the absence of any better, they are of the highest value. Now these results extending through a considerable range of substances (as I have remarked in my report) do seem to afford one or two principles of classification as to increased energy of effect; but *colour is evidently not one of them.* We have some *white* substances near the head of the list, and some dark ones very low in it. But to complete our view of the matter, let us only contrast the differences observable among such coatings in reference to *simple heat*, with those which subsist when we expose them to the heat accompanying, or belonging to, or excited by, *light.* Here the case is clear, and not capable of misapprehension; the *dark coloured* substance (unless the effect be completely disguised by its badly absorbing *texture*, as metallic polish, &c.) absorbs the *light*, and thus greater *heat is excited or developed.* I believe the most accurate method we at present possess for making such comparisons, is that which I have adopted, viz. to coat two thermometers with the substances to be examined, and to observe the *ratio* of their risings when exposed to luminous, and to nonluminous heat, or the *comparative* effects of the radiation from a *luminous source* upon them, and their comparative rapidity of *radiating* again the heat they have acquired. An extensive series of such comparisons (which I have never yet had leisure to make with the requisite accuracy,) would put us in possession of most important data on a subject of which at present we know hardly any thing. The main difficulty is that of determining *precisely* the circumstances and properties wherein the coatings differ. A very extensive induction perhaps, is the only means open to us of ascertaining this, considering how totally ignorant we are of the intimate nature and structure of bodies, and above all, of the peculiarities on which their colour depends.

In fact, this consideration has long appeared to me to present a formidable, perhaps insuperable difficulty, in all experiments of the kind; and I will merely say further, that so fully have I been myself impressed with its formidable character, that though at one time I had devoted much labour and attention to extensive inquiries of this kind, I felt compelled to relinquish them as hopeless. Yet, as far as my observations have extended, they exactly corroborate what all the previous results of Franklin, Davy, and other philosophers lead us to expect, viz. that the effects of the radiation from *luminous* sources, are in a totally different *ratio* from those of *simple* radiant heat; the former always following the order of *darkness* of colour, *even in the midst of considerable diversity of other properties*; the latter certainly not exhibiting any such constant or general relation: but, as far as we can at present conjecture, the relation which they do follow being one of an entirely different kind; though, of course, instances may occur when it presents an apparent, and possibly in some cases a real, coincidence with the order of colour.

Indeed, the reason assigned by Sir J. Leslie appears to me perfectly satisfactory against the prospect of success in such experiments. I would even go further, and maintain that if we have any substance, in the first instance of its natural colour, and then proceed to dye or tinge it in any way with another, one of two things *must* take place: either, 1st, Some heterogeneous matter is now mixed up with it, and consequently the surfaces which it is the object of experiment to compare are *not of the same material;* or, 2dly, The mechanical disposition of the particles of the original body is altered, and it has *no longer precisely the same texture.* Either circumstance would totally invalidate a conclusion as to any change in its radiating power being due to its *colour as such.**

Upon these grounds, I feel most forcibly the philosophical caution of the remarks of Dr T. Thomson and Dr Turner, which the author has cited; and, as the former has observed, the investigation in question "has hitherto been impossible," so, I am inclined to think, that its impossibility is rendered nearly certain on the ground first stated; or rather, that though we may trace approxi-

* It should be observed, that Professor Forbes has pointed out the necessary connexion between absorbing and radiating power, as dependent on the arrangement of the particles of the surface to be one of the mathematical consequences from Fourier's researches on the subject. This circumstance, perhaps, may be regarded as diminishing still further the probability of any relation to colour.

See Translation of M. Maurice's Abstract, &c. Lond. and Edin. Journal of Science, Feb. 1833. Note p. 108.

mate relations between differently coloured bodies in their absorptive and radiating powers; yet it will be impossible to distinguish whether such relations are dependent on *colour, as such*, or only on *certain states of the body* which are the concomitants of a particular colour.

In some cases such a distinction is readily made, even with the degree of knowledge we already possess on the subject. For example, coatings of lamp-black, or of the smoke of a candle, have been used by all experimenters as highly efficacious; but in the case of simple heat it is more than questionable whether it is the *blackness* which is the cause of the increased energy. Where *light* is concerned, we have evidence of another kind that it is so. But after the distinctions which, I conceive, I have established (Phil. Trans. 1825) in the case of terrestrial heat, we must recognise the highly absorptive *texture* as acting an important part, and in the instance of *simple* radiant heat, without light, the *whole* increased efficacy is due to this property.

Perhaps the most valuable information yet obtained on this point is the conclusion of MM. Nobili and Melloni, when qualified by the considerations which I have ventured to suggest (Report, p. 266), that *the radiating powers are inversely as the conducting*. If this remarkable conclusion be admitted as sufficiently established, it will go far to explain many effects apparently connected with colour. In particular, all those *very* numerous cases in which *carbon* enters as an ingredient into black or dark coloured pigments. This being one of the worst known conductors, will, by the above law, be one of the best radiators; and thus, lamp-black, soot, &c. &c. radiate and absorb simple heat with great energy, *not as being black*, but as being *carbon*.

I cannot help observing, that, throughout this paper, its able and ingenious author does not appear sufficiently to bear in mind the distinctions just referred to between the *different species* of heating effects, if I may so term them; or, more properly, the different modes or channels by which, as it were, the same common effect, estimated by us in producing the same sensation of heat, is *conveyed*. All these have been confounded together under the one term Radiation, or radiant caloric, and, as I conceive, no small confusion of ideas has sometimes resulted. In my report on radiant heat, I took particular pains to place these distinctions in the most prominent light possible. If Dr Stark had honoured that report with a perusal, I think either he would not have fallen into one or two such misconceptions (as I esteem them), or, if he considered my positions faulty, could not have done less than have noticed and criticised them. The neglect of such distinctions appears in several instances in the introductory part of the paper, and I shall notice some others in the sequel. Thus, for example, (p. 286), he speaks of the experiments of Franklin and Davy as referring to *heat* in general, whereas they refer simply to the *light* of the sun, which, in the act of absorption (whatever that be), in some way or other, of which we are wholly ignorant, produces, excites, or sets free, a certain quantity of heat.

This subject, especially when the rays are subjected to prismatic analysis, is involved in much uncertainty; but the researches of Sir D. Brewster, and of MM. Nobili and Melloni, are probably those which will tend most to elucidate it. (See my Report, p. 293). Before entering upon the detail of Dr Stark's experiments, I may here take the opportunity of referring briefly to the distinction between the two *kinds* of terrestrial heat, because I am aware that my view of the matter, though I conceived I had explained it, both in my several papers and in the report alluded to, with sufficient perspicuity, has not even yet been correctly understood. My fundamental experiment there referred to, (see p. 279), if the numerical results are considered entitled to any confidence, and if no adventitious cause of error can be pointed out, involve, as a *mathematical* consequence, the conclusion that *two* distinct heating causes or agents emanate, *at one and the same time*, from a luminous hot body, which are marked by possessing different properties. It is certainly remarkable that this result should have been entirely overlooked, or palpably misunderstood, in the various works embracing the subject (which I have happened to see), which have appeared since 1825; and yet no experimenter, as far as I have been able to learn, has refuted the investigation, or even suggested any objection which might invalidate the result. Yet it is certainly of importance, that if that result be worth nothing, it should be *proved* to be so. In a word, both De la Roche's *experiments* and *his theory* have been maintained by nearly all writers since the publication of my experiments; whereas, if those experiments are worth any thing, his *theory* is entirely overthrown, while his experiments are verified, but explained on another principle. Yet it is remarkable how little some of the few authors who have alluded to my researches seem to have apprehended this. And so long as this branch of science continues as it were disowned, equally by all the departments with which it stands more or less connected, and elementary writers are content to copy one from the other, without any, or only the most superficial examination, it is not surprising that the most erroneous notions should obtain currency, and this in a tenfold degree when the sanction of any great name has unfortunately once been extended to a mistaken view of any point. I cannot forbear referring to the striking instance of this afforded by the misstatement of my experiments given in Dr Thomson's work on Heat, which I endeavoured to correct in a paper in the Annals of Philosophy, November 1830. I am not aware whether, since that time, any amended edition has appeared; but certainly, views of the subject scarcely less vague and misconceived, (as appears to me), have emanated from several quarters from whence their appearance has greatly surprised me.

Having deviated a little from the precise line which I had proposed to follow in this communication, I may perhaps be excused in diverging a little further, to introduce another remark connected with the same subject, which is hardly worthy of a distinct paper, and which may tend to render the present argument more complete to those readers who may not have the inclination or opportunity to seek further for elucidation of my views. I refer more particularly to the inferences made from De la Roche's experiments, and my explanation of his results, though denying that inference.

The results I allude to, and the inferences from them, are as follows.

When the hot body was *non*-luminous, the effect when a *plain* glass was interposed was *greater* than with a *blackened* glass; but since the transmission of heat by *absorption* and subsequent radiation must have been *at least equal* in the latter case to that in the former, and most probably *greater*, the effect through the plain glass must have been, in part at least, a *direct* continuance of *radiation* through the glass.

Finding in the subsequent luminous cases that this greater effect through the plain glass increases in relation to that through the coated glass, M. de La Roche infers that this is only an extension of the same phenomenon as in the non-luminous cases, and thence adopts the general conclusion of increasing transmission.

On this point I would first remark, that, to whatever cause the former phenomenon be owing, and if it be a direct transmission of simple radiant heat, it must not be confounded with the subsequent phenomena, and the explanation of it must not be extended to the latter effects.

The experiments given in my paper in the Philosophical Transactions 1825, are, I think, sufficient to prove that, in the instances of luminous bodies, if the same partial transmission still continue, yet the experimental conclusion above established must still be admitted with respect to the principal heating effects, and there still remains the same distinction between the two heating agents to be maintained.

It is by no means improbable, or incompatible with any thing I have advanced, that there may be a small direct transmission of simple heat through glass at high temperatures; or, on the other hand, it may with equal probability be conceived, that even from hot bodies which are non-luminous to our organs of sight, a certain degree of light may emanate, and being transmissible through glass, may exert its heating influence on a black surface like other light.

In the next place, I would observe, that the supposition itself, that the effect through the *blackened* glass *ought to be* equal to, or greater than, that through a *plain*, does not appear to me by any means a *necessary* one. It is indeed from particular instances, in which, from the nature of the surfaces employed, the absorption and subsequent radiation were greater than when the screen presented a plain surface, that Sir J. Leslie has mainly established the doctrine of the effect of screens; but, without at all interfering with that doctrine, it is very possible to conceive a screen so constituted as to be incapable of radiating heat on one side in the same degree that it acquires it on the other; it is possible that some peculiarities of surface may produce greater lateral and less direct conduction of heat: or, again, if some portion of the absorbing surface be not exposed to the direct rays of heat, from that portion, a radiation of acquired heat will take place on the same side. Now, on this principle I formerly offered an explanation of the results.

De la Roche's apparatus consisted of conjugate reflectors: in the foci were a heated ball and a thermometer, between them the screen, having its coated side towards the ball. The portions of the screen which fall *without* the area of the rays will not radiate their heat so as to produce much sensible effect on the thermometer: and they will at the same time give out their heat more rapidly on the coated side, by virtue of its better radiating power, and consequently abstract more from the other parts on the same side, than when the glass was plain; and the same to a certain extent may take place even within the section of the rays, since the central point of the screen will be that most heated by the additional direct action of the hot ball. With the plain glass there was neither so great an excess of heat (from its less absorptive texture), nor such a tendency to radiate it on one side rather than the other.

It would, however, be satisfactory to see whether experiment will shew any instance of a screen having a more absorptive surface exposed to the radiating body, and yet not acquiring a corresponding increase of temperature on its other surface; or whether that surface would, even in any case, shew a less acquisition of heat than when the exposed surface was plain, so that the relative temperatures of the outer side in the two cases should at all correspond with De la Roche's result of less transmitted effect with the more absorptive surface; and thus the dependence of the latter effect on the former as its cause, be in any degree rendered more probable.

With this view, I may be permitted to mention a few experiments which I tried long ago, comparing the temperature of the outer surface of a plate of glass when the inner, or side exposed to the source of heat, was respectively plain, or coated with Indian ink. The hot body was an iron ball heated and then cooled to just below visible redness in the dark. A small thermometer was attached to the outer side of the glass, and its bulb kept in contact by a wire spring; before experiment, the thermometer remained in contact long enough to acquire the temperature completely.

The following is a view of the results :—

Iron heated below visible redness, three inches from glass plate. Thermometer in contact with outer surface at its centre.

		Glass coated with Indian ink towards the Hot Iron.		Glass plain.	
Exp.	Min.		Differ.		Differ.
1.	0 3	17.5 21.	3.5	17.5 21.75	4.25
2.	0 3	21.5 26.	4.5	21.5 26.75	5.25
3.	0 3	21.5 26.	4.5	21.5 27.	5.5
4.	0 3	21.25 26.	4.75	21.25 26.	4.75

In these experiments, the outer surface of the glass, when *plain*, acquires a temperature at least equal to, and in most cases greater than, when coated with Indian ink. To whatever cause this may be owing, we must hence admit that it may also have been the case in M. De la Roche's experiments. And a secondary radiation from the outer surface of the screen, which would thus have a greater temperature, would sufficiently account for a greater effect transmitted when the screen was used plain, compared with that when the glass was coated with Indian ink; and thus the supposition of a direct continuance of radiation through the glass is rendered unnecessary. It is true the difference here exhibited is not near so great as that of the transmitted effects in M. De la Roche's experiments, but it is sufficient to confirm the probability that some such cause may have operated in producing them.

While upon the subject, I will add a brief remark on the subject of *extremely thin* screens. It has been argued that heat radiates *directly* through them, because no difference is perceptible with a change of distance in the screen from the hot body. Now, it appears to me, that if the effect arise from secondary radiation, the time of transmission of the effect, until it be as great as if no screen were interposed, will depend upon the thickness, conducting power, and state of surface. If the thickness be of sensible magnitude, the time of conducting through will of course vary with the distance, *i. e.* the intensity; but if it be insensible, the time, in any case, will also be insensibly small; and hence effect will not vary with the distance.

I have wandered far, however, from the proposed subject of this communication, to which I must now return.

Dr Stark's first set of experiments (p. 287), is designed to try the influence of the *colour* on the absorption of heat. He employed equal quantities of wool, dyed of different colours, filling up the interior of a glass tube, containing a thermometer, and noted the rapidity of communication of heat when the tube is immersed in hot water; the greatest effect was found with the *darkest coloured* wool.

Here I would ask (independently of what I have just observed relative to the change in the material employed), is there not some confusion between the absorption of *radiant* heat (even in any sense of the word), and its communication by *conduction?* At any rate, it is the latter property which is here shewn to be affected by that change in the nature of the wool (whatever it may be), which is effected by the circumstance of its being dyed black. I am aware that it is perhaps impossible to mark the transition from one species of effect to the other, when we come to insensible distances; but nevertheless so many distinct properties characterize them, that we ought surely, in any investigation, carefully to point out whether we mean our conclusion to apply to conduction or to radiation.

The next set of experiments would seem to be regarded as referring to exactly the same class of phenomena as the last; but it does in fact belong to a totally different sort. The effect tried is that of the compound radiation of *light*, with its power of exciting heat, and of *simple heat* together (agreeably to my conclusions), upon the bulb of a thermometer, coated with differently coloured pigments. The *light*, of course, is most absorbed by the *dark-coloured* coating, and therefore more heat generated. Results of this kind, I think, must be considered quite distinct from those which bear upon the absorption of *heat*, as such.

In the experiments (p. 291), the tube containing coloured wools as before, being heated to a given point, the time of cooling a certain number of degrees was noted. The difference was but small, but the effect more rapid with the darker wool. This would be a consequence of the better *conducting* power which the former experiment establishes.

Similar experiments, with a similar result, were tried with the substitution of flour differently coloured.

These results, considered as referring to the *conducting* power, and described with reference to the peculiarity, whatever it may be, in the physical character of the substance connected with its darkness of colour, appear to me an interesting addition to the very few instances on record in which the *conducting power* has been shewn to have any precise connexion with any one determinate physical characteristic of bodies.

The results (p. 292), with differently coated bulbs, and (p. 294) glass bulbs containing water, evincing a greater rapidity of cooling with the darker coloured pigments, and greatest of all with the smoke of a candle, are explicable in regard to the last mentioned case by the consideration of its *texture* as well as its *blackness*. In regard to the different-coloured paints, though I am far from denying that the general agreement of so many different experiments is a strong circumstance, yet I conceive the remark before made ought here to be borne in mind; and the conclusion can hardly be regarded as strictly verified, unless we could be sure of all the circumstances attending the laying on, as well as the nature of the paint, so as to ensure an absolute and perfect *equality* of thickness, conducting power, &c. But supposing the validity of the conclusion admitted, it appears to me to amount to this: That certain paints, in proportion as they contain some colouring matter of a darker hue, sustain an increase in their power of *radiating* heat. Is this substance carbon? At any rate, I conceive the inquiry still remains, Whether such increase has *any real analogy* with the increased tendency to absorb light, which constitutes darkness of colour, and which, as such, is really and constantly accompanied by a greater excitation of heat when the substance is exposed to the action of light.

Some experiments of Sir E. Home are next referred to as contradictory to those of Franklin and others, while the author deems the explanation given by Sir H. Davy as insufficient. Here again I may be allowed to observe, that if he had referred to my report (p. 288), he would have found what appears to me a satisfactory explanation of the point, in accordance with Sir H. Davy's suggestion. It was originally given in a paper in the Annals of Philosophy. Sir E. Home's experiment, I believe, shews only this much, that the *scorching* effect on the skin which sometimes takes place by the sun's rays through semi-transparent white cloth or linen, is prevented by the absorptive power of black cloth; or, again, that it strikes through the transparent skin of a white, but is absorbed by that of a Negro; and thus, in either case, by the absorption of the black substance, the effect is gradually converted into that of heat of temperature.

With regard to the highly interesting remarks of the author, bearing on the *final causes* of the change to white, during winter, of the hair and feathers of some animals, I would merely observe, that what I before said with respect to the want of data for discriminating between the "*causa*" and "*non causa*" to be found in the circumstance of colour, or of some peculiarity in substance and texture, will, I think, fully apply here. If, indeed, the difference of colour in hair depend upon any peculiar colouring matter secreted, more or less copiously, or not at all, according to circumstances, surely such difference in the actual component matter of the hair will afford a sufficient cause to account for a greater or less radiating or conducting power in a way strictly analogous to a variety of well understood cases, which it would be more philosophical to adopt as an explanation, rather than have recourse to a principle so little capable of distinct apprehension, and so little referrible to any classes of facts of which we have a satisfactory analysis, as the influence of a disposition to reflect certain rays of light upon the emission of heat.

It would be a mere repetition of what I have already urged, to express my hesitation as to the same conclusion which the author deduces respecting the influence of snow in preserving the temperature of the earth. It is a badly *conducting* substance by virtue of its peculiar *texture* of flakes and spiculæ; and this being accordant with a known general law of the effect due to such texture, I cannot conceive we are reasoning philosophically, in having recourse to so remote a property as its *white colour*, to account for the effect.

Similar remarks appear to me to apply with equal force to the experiments on the deposition of dew; and I must confess myself entirely disposed to concur in the truly philosophical hesitation displayed by Dr Wells in prosecuting such an inquiry, notwithstanding the author's implied censure of it, and to regard the objection cited in the words of Sir J. Leslie as almost decisive against the validity of any conclusion, until we shall have a much closer insight into the actual structure and intimate nature of bodies than we at present possess. "A black body almost always differs from a white, in one or more chemical properties, and this difference may alone be sufficient to occasion a diversity in their powers of radiating heat." (P. 300.)

I have here restricted my observations to that portion of Dr Stark's inquiry which relates to *heat.* The other part of it, in which he endeavours to establish similar conclusions with respect to odours, miasma, &c. refers to subjects in which I cannot pretend to be so conversant. But I cannot help thinking that many of the same cautions which I have ventured to suggest, as far as they regard the philosophical character of the reasoning, might be found not less applicable in these cases also. The same want of due distinction between the different sorts of heating effect runs through Dr Stark's historical Sketch in the Edinburgh New Philosophical Journal, (No. xxxiii, p. 65). The earlier experiments to which he refers, of Des Cartes, Boyle, &c. all refer not to *heat* in general, but to the *sun's rays.* Those of Hooke and Franklin specially establish the analogy between the solar rays and those from *flame.* Those of Bishop Watson do not, I would submit, refer to "the effect of a coating of black in raising the temperature of substances;" but to its effect in increasing that particular development of heat which takes place when the sun's *light* is absorbed, and which is *necessarily* greatest in dark coloured surfaces. Count Rumford's experiments refer to two totally distinct inquiries: one the *conducting*, the other the radiating, powers of different substances. To the experiments of Sir E. Home, (here more fully stated), I have already referred; and the same remark will apply to the suggestion of a black dress as most suitable, *not* for a *warm* climate, as such, but for exposure to the *sun*, and the avoidance of *scorching*. Whether the accumulation of the same heat, in a more equable manner, be more desirable, would be another question.

With reference to the often recited experiments of Sir John Leslie, it appears to me that since his blackened balls were used for the effects of the *solar rays*, there is none of that occasion for surprise which the author seems to evince at his expressing himself doubtful as to the influence of colour in *simple heat*. Nor when he had tried the experiments referred to, does his cautious inference from them seem to me more than is fully justified by the considerations I have already referred to. So far from perceiving any of that "inconsistency" with which Dr Stark taxes him, I confess I am disposed to think his hesitation was grounded on the most philosophical views. But even if Dr Stark (as I presume) is an opponent of Leslie's peculiar theory of the radiation (or rather, as he would have termed it, *pulsation*) of *simple heat*, yet surely he must allow the philosopher to be consistent with himself, in doubting the influence of colour in the latter case, which, according to his view, was not connected even by any sort of analogy with the radiation of *luminous heat*.

The researches of Sir W. Herschel, I would suggest, were not directed to the "modification of heat by colour," but to the heating property of the sun's rays, as analyzed either by prismatic refraction, or absorption by coloured glasses. Again, when Sir H. Davy speaks of "the temperature of bodies being affected by *rays* producing heat," it is evident, from the whole tenor of the passage, and of the experiments referred to, that he means rays from *luminous* sources. The remark of Dr E. Turner, quoted at length (p. 75), I regard as the most perspicuous statement of the matter which can be given in a few words. In short, upon the whole of this historical review, I venture to remark, that the deficiency pointed out as existing in all previous researches, seems to me rather inherent in the nature of the subject, when distinctly considered, than one which could have been or probably will be, supplied by any extension of experiments of the same kind as those referred to; which can only be attempted with a prospect of definite results when we shall have attained a far more intimate knowledge of the structure of bodies than we now possess, and in which every consideration of probability, as far as we can judge, would rather discourage the idea of any extension of the analogy of the relation which subsists between the colour of surfaces, and the effects of *luminous* heat upon them, to the case of *simple* heat unaccompanied by light.

Notice of some Experiments on Silicated Fluoric Acid Gas.

By John Davy, Esq. M. D. F. R. S. Assistant Inspector of Army Hospitals. Communicated by the Author.

The results which I now beg leave to communicate to the Royal Society, were obtained in experimenting on silicated fluoric acid gas, with the hope of acquiring further information respecting the fluoric principle.

Of the unsuccessful experiments the slightest notice may suffice,—such as of the sublimation of phosphorus and sulphur, and of iodine, in the acid gas; the fusion of zinc,—the heating to redness of iron and charcoal in it, and also of the chloride of calcium; the exposure of it to the sun's rays mixed with hydrogen; and the decomposition by heat, in a retort filled with this gas, of the chlorate of potash. In each of these instances, no effect whatever was produced on the gas, as had been before found in several of them, both by MM. Gay Lussac and Thenard, and also by my brother, the late Sir Humphrey Davy.

On the most probable hypothesis, that silicated fluoric acid gas is a compound of a principle analogous to chlorine, and of silicium, it appeared not unreasonable to expect, that the fluorine, even in combination with silicium, might expel oxygen from lime and the other earths, for the bases of which it appears to have a powerful affinity; or, if not, that the acid gas might combine with these bodies directly.

The first trial I made was on lime; the result was remarkable. When perfectly caustic, lime was introduced into a warm dry tube, and filled with clean and dry mercury; on the admission of the silicated gas, the combination of the two was instant, and accompanied by the bright ignition of the whole mass. If the experiment was less carefully made, there was no immediate action on the introduction of the gas; it was either not absorbed at all, or very slowly, and to a small extent; and yet, when heat was applied by means of a spirit-lamp, the combination was sometimes effected rapidly with ignition. Occasionally, however, even a dull red-heat did not effect an union. In the instances of failure, it appeared to be owing either to a superficial crust formed on the line, connected with humidity, which defended the interior from the action of the acid, or to the presence of some carbonate of lime, or even of its hydrate, which had a similar effect. In the best experiments, I carefully sought for oxygen in the residual gas; but never found it; the little gas that remained proved to be either silicated fluoric acid gas in mass, or common air that had adulterated it. This absence of oxygen proved, that the silicated fluoric acid gas had united directly with the lime.

The compound of the silicated fluoric acid and lime, was tasteless; had no effect on litmus or turmeric paper; appeared to be insoluble in water; before the blowpipe it phosphoresced, emitting a brilliant bluish-white light,—and when urged by the flame, softened, and the particles were agglutinated into a mass of such hardness as to scratch glass. With strong sulphuric acid it effervesced powerfully, almost like carbonate of lime,—giving off silicated fluoric acid gas. Muriatic acid acted on it slowly, converting it, I believe, into subsilicated fluate of lime (the fluate of silica and lime of Berzelius), which was dissolved, a little silica remaining undissolved in a gelatinous state.

From the quantity of anhydrous sulphate of lime which has been obtained from it, when decomposed by sulphuric acid, it appears to consist of two proportions of lime and of one of silicated fluoric acid; thus, in one experiment, 1.9 grains of it gave 2.35 of sulphate of lime; and, in another, 1.65 of it afforded 2.04. The latter result I consider the best: however, the quantity being so small, at best it can only be received as an approximation. As the idea of this composition, and considering fluorspar composed of one proportion of fluorine and of one of calcium

—and, silicated fluorine acid, of two proportions of fluorine and one of silicium, according to my brother's early views,* this silicated fluate of lime will contain the same proportion of fluorine as fluor-spar.

Similar trials were next made of silicated fluoric acid gas on magnesia, alumine, and barytes. With magnesia it united readily, both when cold and heated, but without ignition. The compound, judging from the slight examination which I made of it, is analogous to that of lime; without taste, insoluble in water, infusible before the blowpipe, decomposed by the action of the sulphuric and muriatic acids—the acid gas being expelled by the former, and by the latter the compound resolved into a diliquescent subsilicate and free silica. With alumine and barytes, it also united, but to a less extent; and it was again expelled by sulphuric acid.

On the action of the oxides of the common metals on silicated fluoric acid gas, I have yet made but few experiments. In the instance of peroxide of iron, of black oxide of manganese,—the fusible oxide of antimony, the red oxide of mercury, on the contact of the gas, a portion of it was absorbed; and probably combinations were formed superficially like the preceding. They may be deserving of particular inquiry. The peroxide of iron absorbed most gas, and gave it off most readily when acted on by concentrated sulphuric acid.

M. Berzelius, in an elaborate and able paper on the fluoric compounds,† expresses it as his opinion, that silicated fluoric acid gas is merely a fluate of silica, and states, that it is capable of entering into combination only with neutral fluates, without suffering decomposition,—and that, when one portion of its silica has been separated, it can be replaced only by an alkali, an oxide, or by water.

The conclusion at which I have arrived from my own experiments, is necessarily in opposition to that of this distinguished chemist, and in accordance with the commonly received opinion, namely, that the gas is an acid, and capable of entering into direct union with certain oxides, as it was well known before to

* Phil. Trans. 1814.

† Annales de Chimie et de Physique, tom. xxvii.

have done with ammonia. M. Berzelius expressly states, that he failed in his attempts to combine it directly with lime. To what circumstance this failure was owing, I shall not attempt to point out. The negative result, no doubt, led him to adopt the idea that the gas is not an acid.

In farther support and illustration of its acid nature, I may mention, that it instantly reddens litmus paper introduced into it, carefully dried; and that, though the silicated fluate of ammonia is decomposed by muriatic acid gas, silicated fluoric acid gas being disengaged, and muriate of ammonia formed, it is not decomposed by carbonic acid gas; but, on the contrary, the carbonate of ammonia is decomposed at the temperature of sublimation by silicated fluoric acid gas.

From the similarity of properties of silicated fluoric and fluoboracic acid gas, it seemed probable *a priori*, that the latter also might be capable of entering into union with lime, magnesia, &c. directly; and the single experiment I have made on the former earth has confirmed the conjecture. As soon as the fluo-boracic acid gas was introduced into a tube over mercury, containing some quicklime, its absorption commenced, and was promoted by the application of heat; but, though pretty rapid, it was not attended with ignition. The compound formed was very similar to the silicated fluate of lime,—but rather more easily fusible. Acted on by concentrated sulphuric acid, it emitted the peculiarly dense fumes characteristic of fluo-boracic acid vapour.

As regards the question of the nature of the fluoric principle, the facts adduced are but of little weight; however, their bearing seems to be most in favour of the hypothesis which is now most generally received,—the one already alluded to, that the fluoric principle is analogous to chlorine.

MALTA, *April* 2. 1834.

Address to the British Association for the Advancement of Science, delivered on the occasion of the Opening of the Fourth General Meeting at Edinburgh, 8th September 1834. By James D. Forbes, F. R. SS. L. & E., Professor of Natural Philosophy in the University of Edinburgh, and one of the Secretaries of the Association. Communicated by the Author.

It having been suggested that the general view of the progress of the affairs of the Association, so ably executed last year by Mr Whewell, should annually be continued by the Secretary for the time being, I have undertaken this portion of the duties which devolve upon the Secretaries for Edinburgh, at the desire of my learned colleague Mr Robison, who, on the other hand, has engaged briefly to state the nature and motives of the practical arrangements for the present meeting, of which he has had the kindness to superintend by far the most laborious part.

I felt anxious that such a periodical report as I have mentioned should be continued, because of the necessarily fluctuating state of our Body, and the small number of persons who, by circumstances, have been enabled to attend all the meetings, and to become acquainted with the actual operation of a somewhat complicated machine; and I was ready to undertake that duty, because I hoped that I might be able, by an appeal to facts, in the *first* place, to put in a clear point of view, what has not perhaps been enough insisted on, and has therefore been very generally misunderstood,—the perfectly *unique* character of this Association, and the high aims to which its efforts are directed; and, in the *second* place, to demonstrate that these aims and objects are in the due course of attainment, that the members, and especially the *projectors* of this institution, are fulfilling the pledges, of no common character, which they gave to the public, and this more especially in relation to the proceedings of the past year.

The character of the Association, I have said, may be considered as *unique*. It is not to be confounded with those numerous and flourishing institutions which have sprung up, especially of late years, for the simple diffusion of scientific truths. Such *diffusion* does not even, properly speaking, include any attempt at *extension* or accumulation: if in many cases it does promote such extension, it is indirectly, and beyond a doubt has sometimes had the opposite tendency. The intellectual wealth of mankind is no more increased by this operation, than is the weight of the precious metals under the hand of the gold-beater. A greater display may indeed be attained, and a more commodious application to the useful and the elegant purposes of life; but for actual increase of the stock which may hereafter be fashioned with ease and expedition by the hands of a thousand artificers, we must recur to the miner toiling in his solitary nook, and to the labourer who painfully extracts some precious grains from the bed of the torrent. It is the furtherance of this species of productive energy that the British Association claims for its capital object. The diffusion of a taste for science amongst its numerous members is no doubt also one of the most necessary and most desirable consequences of the principles upon which it is founded; but it is not the basis of these principles. To teach those who have never pursued natural knowledge but as an occasional amusement,—to feel that for them a field lies open which to-morrow they may call their own,—to lend them such aid as may promote the success of their exertions, by removing the preliminary difficulties, and pointing out the existing boundary betwixt the known and the unknown,—to stimulate these exertions and those of others who have already become, to a certain degree, familiarized with the labours and with the results of intellectual toil, by enabling them to mix with the veterans in each department who have gained, and who still continue to gain, the highest rewards which the investigation of nature confers,—who will point out the methods which they pursued, the disappointments which they met, and the difficulties which they surmounted, thus affording at once the gratification which every generous mind feels in personal communication with those who have signalized themselves by intellectual achievement, and the instruction and encouragement for the pursuit of a similar course which words and words alone can impart,—*these* we hold out as amongst the first and the most valuable objects proposed to be attained by the institution of this Association.

No doubt societies for the promotion of Natural Knowledge have been in existence for near two centuries, and these have done much to the due advancement of science itself, as well as the promotion of a more general taste for its cultivation. They were admirably adapted to the period of their institution, when the difficulties of ordinary communication, and the want of scientific journals, made the Royal Society of London the great centre of philosophical information,—when new experiments were there first repeated,—when new theories were there first discussed,—and when its transactions, and those of the other academies of Europe—fraught with the literary treasures which Hooke and Wren, and Boyle, and Leibnitz, and the Bernouillis loved to display, and which Newton alone loved to conceal—were the couriers which published to Europe the intelligence of the successive intellectual victories of that mighty age. Rarely even then, however, and latterly still less, did these societies attempt to guide in any specific direction the investigations of their members, or to form any school of science for the initiation of fresh inquirers. The formation of such schools of disciples who voluntarily combined under some philosopher of eminence, partly did away with the necessity of this on the Continent; whilst the total want of any thing similar in our own country, and the less specific objects of those honorary rewards which from time to time have been given by learned societies in all countries, and which have occasionally drawn forth all the powers of some master mind to the solution of a specific difficulty proposed as a prize question, necessarily produced a greater want of systematic co-operation amongst scientific men in Britain than is to be found in several countries not her political superiors.

The migratory Scientific Associations of Germany and Switzerland—to which we gratefully acknowledge that our British one owes its rise,—embrace only one class of the objects to which we have above alluded as characterizing this Body. Their aim was simply to promote the intercourse of scientific men, and to diffuse a taste for the prosecution of science. Their existence is not permanent,—they execute no functions but for the moments during which their members are once a-year assembled,—they regard not the past, and have no cares for the future,—they merely receive and consider the communications which the zeal of individual members places in their way. Such was proposed to be the character of the Body this day assembled,—an imitation of the foreign meetings having been suggested by some individuals engaged in scientific pursuits, amongst whom Sir David Brewster was conspicuous; but the original idea, and the much more signal merit of bringing that idea to bear, of establishing a permanent society, of which these annual reunions should simply be the meetings, but which, by methods and by influence peculiarly its own, should, during the intervals of these public assemblies (whilst to the eye of the world apparently torpid and inactive), be giving an impulse to every part of the scientific system, maturing scientific enterprize, and directing the labours requisite for discovery;—the clear perception of the practicability of all this, and the discovery and suggestion of methods for its fulfilment, were due to one individual, and to one alone; and I shall be borne out by all those who have closely watched the progress of this Society from its birth to the present hour, when I say, that not only for the idea generally, and the modes of carrying it into effect, but for the actual construction of the machinery in its whole details, we are indebted to the almost single-handed exertions of Mr William Vernon Harcourt.

If we now turn from the professions to the *acts* of the Association, we shall find gratifying proof that these sanguine anticipations were not chimerical; and that this primary machinery, not destined itself to do the work desired, but to construct the *tools* requisite for its performance, was wanting neither in efficiency nor in permanence. The first and most signal proof which we can cite, is the production of those Reports on the Progress of Science, which appeared to the founder of the Association one of the most important objects of such an institution, and one which, beyond all dispute, no existing society could have attempted. To require of persons whose time was in all cases more or less valuable, such a devotion of it as was required for a systematic and precise detail of the recent progress of the sciences which they respectively cultivated, was to make a demand, the boldness of which cannot perhaps well be appreciated, but by those who have had experience in the labour of bringing together the substance of detached, though often profound, papers in the extensive range of scientific periodicals and academical collections. Yet so obvious was the utility of the proposed undertaking, that, in the very infancy of the Association, there were found several distinguished individuals, and chiefly from the University of Cambridge, who had not even been present at the first meeting, but who volunteered to undertake some of the most valuable of those reports which appeared in the first volume of the proceedings of the Association. As Mr Whewell enumerated these in his last year's address, I will not farther allude to them. It ought, however, specially to be observed, that these reports differ entirely from the short systematic treatises on scientific subjects with which the press teems. They are not primarily intended for the general reader —they are not meant for the purpose of popularizing technical subjects; their main object is so to classify existing discoveries as to lead the individual who is prepared to grapple with its difficulties, to start with the most complete and accurate knowledge of what has already been done in any particular science, not intended itself to contain that knowledge, but merely to serve the purpose of a *catalogue raisonnée*, by means of a lucid analysis and arrangement, at the same time (and here is the great necessity of securing the co-operation of persons distinguished in the several departments) that the report should point out the most important questions which remain for solution, whether by direct experiment or by mathematical investigation.

The second volume of Reports has amply justified the expectations with which it was hailed; and whilst the first was chiefly occupied with reports upon great and leading divisions of science, we have here several happy specimens of a still greater division of labour, by the discussion within moderate limits of some particular provinces. Thus Mr Taylor has treated of one particular and most interesting question in Geology, the formation of Mineral Veins,—one of the most important, in a theoretical point of view, which could have been stated, and which, from its intimate connection with commercial speculation, might have been expected in a country like ours to have been more specifically treated of than it has been. It strictly belongs to the dynamics of the science, to which, since the time of Hutton, but little attention has been paid until very recently. By the exertions, however, of Mr Carne, of Dr Boase, and Mr Henwood of Cornwall, whose researches are to form one point of discussion in the Geological Section at the present meeting, the question of the origin of Mineral Veins, though probably by no means decided, has been brought prominently forward.

That Electric Agency was concerned in the disposition of metalliferous veins can scarcely be doubted; and the connection between electricity and magnetism, now so fully established,—the connection between metalliferous veins and lines of elevation, and between the latter and the isodynamical lines of terrestrial magnetic intensity, as suggested by Professor Necker of Geneva,—point out a bond of union between this subject and that of terrestrial magnetism, on which we have a report by Mr Christie, where the very interesting direct observations of Mr Fox of Falmouth, on the electro-magnetic action of mineral veins, are particularly noticed. Mr Christie's theory of the diurnal variation of the needle, which he is desirous should be submitted to the test of a laboratory experiment, is likewise intimately connected with the actual constitution of our globe *. The whole subject of Terrestrial Magnetism, is one of the most interesting and progressive of the experimental sciences. The determination of the *direction* of the magnetic energy by means of two spherical co-ordinates, termed the variation and the dip, and the measure of the *intensity* of that force, are the great objects of immediate research, as forming a basis of theory. The existence of four points on the earth's surface, to which the needle tends, has long been known; and the position of two of these (in Northern Asia and America,) has recently been elucidated by the persevering efforts of Professor Hansteen and Commander Ross. The precise numerical determination of the elements just alluded to, acquires a deep and peculiar interest from the multiplied variations which they undergo. Not only

* Report, p. 122-3.

are these elements subject to abrupt and capricious changes, which Baron Humboldt has termed *magnetic storms;* but gradual and progressive variations are undergone at different hours of the day, at different seasons of the year, and throughout longer periods, which may even perhaps bear a comparison with the sublime cycles of Astronomy.

Natural History forms a more prominent subject in this volume than in the last, though the reports of Professor Lindley "on the principal questions at present debated in the Philosophy of Botany;" and of Dr Charles Henry "on the Philosophy of the Nervous System," refer only to particular departments of widely extended subjects, which are again to be resumed in more general reports, undertaken for the present meeting,—that by Mr Bentham, on Systematic Botany, and by Dr Clarke of Cambridge, on Physiology in general. We cannot but remark with pleasure, that one of the points for inquiry particularly insisted on by Professor Lindley, that of the influence of the chemical nature of soils, and of the excretions of plants, was taken up at an early period of the existence of the Association, by one of its most zealous supporters, Dr Daubeny; and that, in reference to the review by Dr Henry of the labours of European physiologists, we may quote, as a national honour, the discoveries of our distinguished Associate, Sir Charles Bell.

On the general connexion and occasional apparent opposition of Theory and Practice, I would refer to some very pertinent remarks in the Address of Mr Whewell, at the last meeting. The importance of carrying on both simultaneously and independently, and of looking to our increased knowledge of both as the only sure means of ultimately reconciling discrepancies, has been manifested, by the desire of the Council of the Association to procure two distinct reports on the Theory and Practice of Hydraulics, which have been drawn up with remarkable perspicuity, and within a small compass, by Mr Challis and Mr Rennie. Both of these gentlemen have shewn their zeal in the objects of the Association, by promising to continue their valuable labours. Mr Rennie, on that part of his subject which relates to the motion of fluids in open channels, and Mr Challis on some of those exceedingly interesting branches of theory altogether modern, which physically, as well as in their mathematical methods, have the closest analogy to that case of the motion of fluids treated of in the present volume, namely the Theory of Sound, and the intimate constitution of liquids. When, in addition to these reports, we shall have received that undertaken by Mr Whewell upon the mathematical theory of Magnetism, Electricity, and Heat, we shall undoubtedly possess the most complete outline extant, of a department of knowledge entirely of recent date.

In the science of Hydraulics, indeed, some progress in theory has accompanied the increase of practical information, at least since the time of Newton; but in the other strictly *practical* report of the present volume, that of Mr Barlow on the very interesting subject of the strength of materials, little or nothing has been done of much theoretical importance since the days of Galileo. Circumstances, which it would be easy to point out, prevent our setting out, except in rare cases, from unimpeachable data; but several very interesting conclusions of general application are derivable from well conducted experiments, and the Association may claim some credit for having brought into general notice the ingenious investigations of Mr Hodgkinson of Manchester, more particularly alluded to in this paper.

One report, and that the longest which has ever been printed by the Association, remains to be mentioned. It is by Mr Peacock on the present state of Mathematics. When we consider the vast extent of the subject, and the extremely limited number of persons, even in the whole of Europe, capable of undertaking it, we must consider the production of a work of so much labour as the present, which, as yet, is incomplete, but which the author has promised to resume, as the best trophy to which we can refer in proof of the entire efficiency of the Association, according to its original plan,—as a proof of the ability and the indefatigable industry which it has enlisted in its service,—as a proof that its aim is not the dissemination of superficial literature, stamped with the effigy of science, and lowered for the demand of the indolent and the careless,—but that it is intended to refine the precious metal until it reaches a state of chemical purity, not to alloy and coin it for the purposes of a promiscuous and debased currency. Mr Peacock undertook his report in the early days of the Association, when its friends were yet few and its success dubious; its execution has been delayed by the extent of the subject and labour of the task. The report on the differential and integral calculus, which was intended to form the basis of it, is delayed, and the present one is devoted to a discussion chiefly of algebraic methods, and a close examination of the metaphysical principles upon which this interpretation of analysis is founded. The author has thus been led to extend the views which, in his recent systematic treatise, he had developed in regard to the signs of affection of algebraic quantities, including those of imaginary quantities, of discontinuous functions, and the interpretations of zero and infinity. The author has then treated of Series, as regards their fitness for giving directly conclusive results, particularly when such series are *divergent*, leaving to the other part of the report a detail of the progress in the application of series, which is more practical than metaphysical. The author then treats historically of the elementary works in use on Algebra and Trigonometry; and devotes the last part of the report, consisting of above fifty pages, to the Theory of Equations, in which he has minutely analysed some of the most remarkable papers on this abstruse subject. Altogether this report (especially when completed), cannot fail to fulfil, in a striking manner, the two great objects of such works; *first*, to supply those engaged in collateral branches of science with the means of referring to and obtaining the information they may require upon methods which perhaps are of daily utility in physico-mathematical inquiries; but with which, from the vast extent of the science of pure mathematics, the shortness of human life prevents the possibility of a complete and systematic acquaintance, unless it be made the special object of study; and, in the *second* place, to point out, where chasms of reasoning occur, what mathematical methods are impregnable, and what rest upon a still dubious basis, in a metaphysical point of view, several of which are very specifically treated of in Mr Peacock's report. It is much to be desired that nothing may longer postpone the conclusion of a work which cannot fail to reflect honour upon the Association.

Were these annual Reports the only fruits of the labours of this Society, there would be no reason to complain. But yet more specific results of its impulsive action on science may be quoted. The questions suggested by the reporters and others, recommended for investigation, have met with ready attention from several individuals capable of satisfactorily treating them. Professor Airy has himself investigated, from direct observation, the mass of Jupiter, suggested as a desideratum in his report on Astronomy ; and, since the last meeting of the Association, has confirmed his first results by new observations, which give almost the same mass by the observed elongations of the satellites, as had been deduced from the perturbations of the small planets by Jupiter. Hourly observations of the Thermometer in the south of England have, in two instances, been commenced ; and we are assured that the same desirable object is about to be attained by the zeal of the Committee in India, where the Association has established a flourishing colony. A series of the best observations, conducted for ascertaining the law which regulates the fall of Rain at different heights, has been undertaken at the suggestion of the Physical Section, by Messrs Phillip and Gray of York, which have been ably discussed by the former gentleman, in last year's Report, and have since been continued. A regular system of Auroral observation, extending from the Shetland Isles to the Land's-End, has been established under the superintendence of a special committee, and specimens of the results have been published. Observations on the supposed influence of the Aurora on the Magnetic Needle, have likewise been pursued in consequence of this proceeding. The conditions of Terrestrial Magnetism in Ireland have been experimentally investigated by Professor Lloyd. An important inquiry into the law of Isomorphism has been undertaken by a Special Committee, which has likewise reported progress ; and an elaborate synopsis of the whole Fossil Organic Remains found in Britain is in progress, under the hands of Professor Phillips. Many specific inquiries are besides going forward, under particular individuals, to whom they were confided ; whilst it is not to be doubted that numberless persons, many of them perhaps new to the world of science, are at this moment pursuing investigations recommended in general terms, in one or other of the publications of the Society.

To others the Association has not scrupled to commit a portion of the funds at their disposal, for the purpose of pursuing objects which required an outlay, which might be deemed unreasonable by individuals. Among the most important of these is the collection of the Numerical Constants of Nature and Art, which are of perpetual recurrence in physical inquiries, and which has been confided to the superintendence of Mr Babbage. When objects of still more peculiar national importance presented themselves, the Association has fulfilled its pledge, of stimulating Government to the aid of science. Five hundred pounds have been advanced by the Lords of the Treasury towards the reduction of the Greenwich Observations, at the instance of the Association; and more recently the observations recommended by the Committee on Tides, have been undertaken by order of the Lords of the Admiralty, at above 500 stations on the coast of Britain.

Individuals, as we have said, have been stimulated by the influence of the Association, but so may nations and great bodies of men. Its published proceedings have found their way into every quarter, and are tending to produce corresponding efforts in distant lands. Our reports on science have produced some very interesting counterparts in the literary town of Geneva. America has taken the lead in several departments of experiment recommended by the Association; and the instructions for conducting uniform systems of observation have been reprinted and circulated in the New World. We must likewise consider it as an especial proof of the influence and importance of the Association, that a Report on the Progress of American Geology has been undertaken and executed by Professor Rogers of Philadelphia. Similar contributions from some other foreign countries have been promised, which will extend the utility of the Association, by making us acquainted with the more characteristic state of science in the various parts of Europe. Nor can we fail, on the present occasion, to consider, as a most auspicious promise of the future success of the Association, that the distinguished Secretary of the Institute of France has not only honoured this meeting by his presence, but has promised to interest that powerful Body on behalf of the important objects contemplated by the Association, which its co-operation might effectually secure. The formation of a Statistical Section at Cambridge was the prelude to the establishment of a flourishing society, which acknowledges itself the offspring of this Institution, and which promises, by a procedure similar to that introduced by the Association, to advance materially the greatly neglected subject of British statistics.

Gentlemen, I shall be satisfied if, in the preceding hasty review, I shall have given you some direct and tangible proof of the working of a system, the excellence of which may best be appreciated by such statements. Did it come within the scope of these observations (which it does not), I could quote examples, equally specific, of the powerful moral influence of the Association. Yet, in conclusion, I will call upon you to remark, because I believe that it comes home to the breast of every one who has habitually attended these our annual reunions, what a spirit is infused into otherwise isolated and perhaps ineffective exertions, when many minds, conversant with one class of objects, and aiming at one great end, unite in friendly and intellectual converse. There is an impulse there which no system of cold calculation can estimate. There is a bond in the sense of community of purpose, which is the cement of society. There has been, we fear, a general but most erroneous impression abroad, that philosophers are incapable of enjoying, and stoically superior to, the ordinary sociabilities of life,—that scientific ardour dwells only in the mind of the solitary, and gives place to narrow-minded jealousy, when another attempts to share the prize. If, in a few cases, such allegations have not been without a colouring of truth, it is to meetings like these that we should look for a cure which no mere reasoning can effect. The most striking feature of these meetings has ever been, the pervading sense which has thrown a peculiar character over them, of the one great and exalted object which united so many distinct and unconnected individuals,—which not less has drawn into this great assembly, the single and unaided labourer in the cause of science, from the solitudes of the country, or the still greater intellectual solitude of some noisy and commercial city, and the phalanx of scholars who have shared the advantages, and sustained the reputation, of the great academical foundations of the country.

True it is that, looking merely to the moral influence of the Association, some there are whose zeal for the promotion of science places them above the necessity of such an external stimulant. But we must not legislate for individual and such rare cases. Those who have once trode the higher walks of science, need perhaps no inducements to revisit these sublime elevations. The footway may be sharp and narrow, surrounded with precipices and occasionally enveloped in mists,—but they have there breathed that pure and elastic air, which descends not to lower regions,—and through the cloudy openings they have caught rich and extensive views, shewing at once the configuration and the bearing of the country, which less daring spirits must painfully and partially explore. Such men are independent of any reward but that which the exertion itself bestows; yet, let it not be called an ignoble motive, if the traveller, embarked on the discovery of a new, and hitherto untrodden, path, which leads to the point to which he aspires, feels fresh vigour infused into his frame, by the consciousness that, in the valley beneath, a thousand eyes are watching his progress, and that a shout of applause unheard except perhaps in imagination by him, will announce the arrival of the adventurer at the summit of the alpine chain.

We look forward without anxiety to the future fate of the Association. So long as it continues to be guided by the same principles as heretofore, it cannot fail to confer a substantial benefit upon the science of Britain. We have enough of energy in action to communicate to the many the knowledge of the few, but it is to prevent the stagnation of the stream at the fountain-head, which should be our especial object. True it is that but a few are able or disposed to devote themselves unreservedly to those great enterprises which require the whole man; yet, though it is morally impossible that any others should undertake the highest generalizations to which we have just alluded, a division of labour is as practicable in intellectual as in mechanical science. If one designing mind direct the whole, distinct labourers may be engaged, un-knowing each other's tasks, yet happy in the consciousness of being more usefully and more honourably employed than in imperfectly attempting the execution of works which they might individually complete. The exquisite piece of mechanism which, in the form of a watch, issues from the manufactories at Paris or Geneva, has its various elements of its wheels and pinions, its balance and fusee, collected from the detached cottages of the peasantry of the Jura.

To combine individual effort, to render parts capable of combination into a whole, to economize time, and thus virtually to lengthen the lives of those whose exertions are valuable in the cause of science, may be considered as humble, yet surely most important, contributions to its advancement. We shall have little reason to regret the want of a National Institute, whose existence is the just subject of pride to our continental neighbours, so long as individual exertion can supply the stimulus which even the sunshine of wealth and patronage has sometimes failed to excite.

New Genera of Plants. Communicated by G. A. Walker-Arnott, Esq. A.M., F.L.S., &c. Communicated by the Author.

Since the publication of the paper on Indian plants in this Journal, No. 29, by Dr Wight and myself, we have received specimens in fruit of *Bragantia Wallichii* (p. 181.), which enable us to state that the pod-like capsule is not terete, as we described it, on the authority of Rheede's bad figure, but tetragonal, with the angles very sharp: this plant, moreover, is precisely the same as *Trimereza piperina*, Lindl., in his observations under *Aristolochia cymbifera*, t. 1443 of the Botanical Register. I may also here remark, that I have now ascertained that the genus *Bhesa*, described by me also in this Journal, No. 32, p. 315, is the *Kurrimia* of Wallich; that *B. Moja*, Ham. is *K. pulcherrima*, Wall.! List, n. 4334; and that *B. paniculata* is *K. paniculata*, Wall.! L. n. 4336. I had no suspicion of *Kurrimia* (which I only lately saw in Dr Hooker's herbarium) being the same as Dr Hamilton's genus, partly from Wallich not referring to Hamilton's specimens, and partly because *Itea macro-*

phylla of Wallich! in Roxb. Fl. Ind. (ed. Wall.) 2. p. 419, is placed also in *Kurrimia* (*K.? macrophylla*, Wall. List, n. 7200), while the structure and shape of the fruit is totally different from our species: this last, indeed, I still suspect to belong to Saxifrageæ, and not to be distinct from *Itea;* it is scarcely distinguishable from *I. chinensis*, Hook. and Arn. in Bot. of Beechey's Voyage, p. 189, t. 39.

Having lately occasion to re-examine the species of Asclepiadeæ of the Peninsula of India, I observed among Dr Wight's specimens of the order, a rather remarkable plant, collected by Roxburgh at the Cape of Good Hope, probably on his final return to England. The following character is derived both from Dr Wight's notes and my own observations.

I. ONCINEMA.

Ord. Nat. ASCLEPIADEÆ. *Brown.*

Corolla campanulata, 5-partita, tubo brevi. Corona staminea 5-phylla; foliolis tenuibus membranaceis planis obtusis intus simplicibus. Antheræ membrana terminatæ. Massæ pollinis compressæ, anguste oblongæ, apice ad curvaturam affixæ appendiculorum in apiculum adscendentem desinentium, pendulæ. Stigma conico-rostratum elongatum, apice subbiapiculato: corpuscula elongata apice capitellata. Folliculi . . . .

Frutex *volubilis, glaber.* Folia *lineari-lanceolata, attenuata.* Cymæ *diffusæ, dichotomæ, pauciflore, interpetiolares.*

1. O. *Roxburghii.*—Periploca Capensis, *Roxb.! mst.*

Hab. Ad Caput Bonæ Spei; *Roxburgh.* (v. s. spec. in herb. Wight. ab amiciss. Dom. N. B. Ward communicatum.)

This appears a very distinct genus, much resembling in general aspect *Secamone emetica*, except in the umbels or cymes having fewer, much larger, and very differently shaped flowers.

The following descriptions of some new genera of Cyperaceæ are by Prof. Nees von Esenbeck, of Breslaw.

II. COURTOISIA, *N. ab E.*

Ord. Nat. CYPERACEÆ, *Juss.*—Trib. Cypereæ, *N. ab E.*

Spicula subbiflora, disticha. *Squama* inferior minor, sterilis; secunda et tertia majores alato-carinatæ, subæquales; secunda fertilis hermaphrodita; tertia sterilis includens quartam hermaphrodito-masculam minorem inversam! *Stamina* tria, filamentis filiformibus; antheris linearibus, erectis, parvis, luteis. *Ovarium* triquetro-subulatum, longitudine filamentorum; *stigmata* tria, filiformia, contorta, in apice ovarii subsessilia. Perigynium nullum. Caryopsin non vidi.

Inflorescentia. Capitula *composita e capitulis seu spiculis minoribus* 3-5-*stachyis subsessilibus bracteolatis, disposita in umbellam Cyperoideam, (in nostra specie simplicem,) involucratam, ochreatamque.* Involucella *bractealia angusta capitula fulciunt et interstinguunt.* Bracteolæ *seu* squamulæ *propriæ membranaceæ, acuminatæ, sub singula spicula singulæ, spicula breviores.* Spiculæ *ad bracteolarum situm relatæ, transversales; inferiores globuli singuli imperfectæ.*

Est genus, quod ad formam spiculæ attinet, *Kyllingiam* omnino referens, quod ad habitum et inflorescentiam *Cyperum*, quod ad situm spicularum ratione bracteæ, *Lipocarpham* et *Hypolytrum*. Sed restat differentia gravissima, communi harum plantarum structuræ adversaria, scilicet: Squamulam infimam sterilemque ab utroque latere spiculæ a bracteola magis aversa positam et in centrali perfectiorique spicula haud raro deficientem si demis, restant squamæ duæ, alternæ quidem, sed ad apicem æquales naviculares membranaceæ, alte carinatæ, semiovatæ, longe mucronatæ, trinerves, purpureo-maculatæ. Harum inferior florem in angulo foret perfectum, e staminibus tribus cum pistillo intermedio constantem. Stamina lateralia lateribus ovarii incumbunt, anterius angulo ejus antico. Altera squama, ejusdem structuræ quidem, sed per se sterilis, oppositam sibi includit squamam tertiam breviorem angustioremque extrorsum versam et florem triandrum pistillo minus (ut puto) completo continentem. squamarum fertilium steriliumque. Diu autem hæsitavi, nec hodie omnino certus sum, numne aliter sese res habeat; movet autem præsertim ovarii, quod dixi, figura insolita in hoc ordine, elongato-triquetra, ac si valvulam graminis superiorem ostenderet cum ovario in unum corpus concretam. Quod si ita esset, geminum quasi florem, ad Graminearum typum accidentem, haberes, quorum alter inferiorque nudus, alter terminalisque squama tertio sterilique suffultus foret. Fatendum autem, me frustra dissolvere ovarium et partes involucratas ab ovario proprie sic dicto discernere tentasse.

Genus R. Courtois Professori Leodiensi dedicatum.

1. C. *cyperoides* (N. ab E.)—*Wight.! Cat. n.* 1853.—Kyllingia cyperoides, *Roxb. Fl. Ind.* 1. *p.* 182; *(ed. Wall.) I. p.* 187.—Cyperus glomeratus, *Herb. Klein et Heyn.*

Hab. In Peninsula Indiæ Orientalis.

Radix annua? fibrosa pallida. Culmi ½–1 pedem alti, crassitie pennæ passerinæ, triquetri, læves, erecti, plures ex uno radice cum foliorum fasciculis intermixti, basi foliosi. Vaginæ laxæ, membranaceæ, glabræ, mollissimæ; 1–2 infimæ aphyllæ; 1–2 elongatæ, pallide lutescentes, foliiferæ. Folia longitudine culmi, linearia, acuminata, plana, lævia, lineam unam lata, viridia, laxa. Involucri folia 3–4, alterna, erecta, culmi altitudine, singula radium simplicem in angulo ferentia, culmeis similia. Umbella nonnihil cernua, tri-4-radiata cum media sessili. Involucelli foliolum lineare, margine scabrum, capitulum æquans aut superans. Ochreæ laxæ, truncatæ, membranaceæ, pallidæ. Capitula densa, pisi majoris magnitudine, globosa. Spiculæ majores 1¼ lineam longæ, ovales, valde complanatæ. Bracteola et squamula infima albidæ; reliquæ squamæ in fundo pallido sanguineo lineolatæ et punctatæ, membranaceæ, dorso viridulæ acute carinatæ trinerves, carina in mucronem viridem excurrente. Bracteolæ pleræque in acumen gracile setiforme laxum extenuantur.

III. ANOSPORUM, *N. ab E.*

Ord. Nat. CYPERACEÆ, *Juss.*—Trib. Hypolytreæ, *N. ab E.*

Spica composita. Bracteæ undique imbricatæ, singulæ suffulcientes spiculam multifloram a basi fertilem. Squamæ distichæ. Perigynium cum basi ovarii concretum et in ejus tunicam externam transiens, spongiosum, per strias veluti in lacinias divisum. Stylus simplex, longus, retortus, basi subulata ovario continua. Stamina tria; filamenta linearia, persistentia; antheræ lineares, mucronatæ. Caryopsis compresso-trigona, cuspidata, lævis, basi spongiosa pallida, superne seminifera. Semen pedicello in centro pericarpii surrecto et cum eodem hac parte concreto sublatum, pericarpio adhærens.

Inflorescentia: Spicæ *aggregato-capitatæ, involucro foliaceo cinctæ.*

Proximum genus *Melancrani* Vahl, sed diversissimum stylo simplici et perigynio cum pericarpio ita confluente, ut dubites, anne deficiat omnino, ovarii basi sterili atque tumenti locum cedens.

1. A. *monocephalum* (N. ab E.)—*Wight.! Cat. n.* 1855.—Cyperus monocephalus, *Roxb. Fl. Ind.* 1. *p.* 188; (*ed. Wall.*) *I. p.* 193; *Wall. Cat. n.* 3441.—C. monocephaloides, *Roxb. in Cœt. Merc. Ind. Or. mus. tab.* 1318 (ex *Arn.*).—Schœnus triceps, *Herb. Heyn.*—Kyllingia triceps, *Herb. Russell.*

Hab. In locis udis depressis Bengaliæ; *Roxburgh.*—Bengalia inferior, et Silhet; *Wallich.*—Peninsula; *Wight.*

IV. HEMICARPHA, *N. ab E.*

Ord. Nat. CYPERACEÆ, *Juss.*—Trib. Hypolytreæ, *N. ab E.*

Spicula undique imbricata squamis basi cuneatis apice rotundatis demum deciduis unifloris. Flosculus sub singula squama singulus, univalvis. Singulare sane videtur ejusmodi exemplum alternantium inter sese squama propria communi opposita ovarium a tergo tegente et demum cum fructu cohærente. Stamen unum, anterius magisque laterale. Stylus ad basin fere bifidus. Caryopsis oblonga, biconvexa, styli basi mucronulata.

Spica *densissima ad speciem lateralis ob involucrum culmum continuans.* Habitus *omnino* Isolepidis setaceæ *et affinium, sed spiculæ adspectus qualis in* Lipocarpha, *cui genus hoc nimis propinquum.*

1. H. *Isolepis* (N. ab E.)—*Wight. Cat. n.* 1856.—Scirpus hæmisphericus, *Roth. ? N. pl. sp. p.* 29.

Hab. In Peninsula Ind. Or.; *Wight.*

Culmi fasciculati cæspitosi, erecti, 1–2½ poll. longi, setacei, compresso-tetragoni, striati, glabri, glauci. Radices fibrosæ, crispæ. Vagina ad basin plures culmos amplectens, scariosa, aphylla, ovato-oblonga, obtusiuscula. Vagina propria ad basin singuli culmi una, 4–5 linearis, oblique truncata, striata, glabra, foliolo prædita vix 3 lin. longo lineari angusto obtuso canaliculato glauco. Spicula una, 1–1½ lin. longa, elliptica, obtusa, teres, ad angulum fere rectum patens. Involucri monophylli foliolum 2–4 lin. longum, culmum continuans, incurvum, canaliculatum, obtusum, basi profunde concavum margineque scariosum. Squamæ densissime multifariam imbricatæ, ex obovato cuneatæ, obtusissimæ, uninerves, nervo in mucronulum exiguum album desinente, basi albidæ, apicem versus fusco-purpureæ, membranaceæ, punctulatæ. Valvula propria communi opposita et eadem paulo brevior, ovalis, cucullata, ovarium a dorso involvens, membranacea, pallida, in juniori statu solubilis et tum vero apice quandoque emarginata, maturitatis tempore caryopsi ceu tenue integumentum adherens. Squamula anterior nulla omnino, cujus loco stamen filamento latiusculo, laterale. Stylus brevissimus, profunde bifidus, stigmatibus recurvis capillaribus; ovarium oblongum. Caryopsis ovali-oblonga, obtusa cum mucronulo, utrinque, præsertim extrorsum, convexa, confertim punctata, matura cinereo-nigra, tegumento accessorio detergibili.

V. MALACOCHÆTE. *N. et Meyen.*

Ord. Nat. CYPERACEÆ, *Juss.*—Trib. Scirpeæ, *N. ab E.*

Spiculæ undique imbricatæ, squamis omnibus fertilibus. Perigynium 3–6-phyllum, foliolis linearibus membranaceis hirtulis deciduis. Stamina tria, antica. Stylus compressus, bifidus, deciduus. Caryopsis lenticularis hinc planiuscula.

Inflorescentia *corymboso aut cymoso paniculata*; involucro *communi recto, ad speciem culmi apicem efficiente.* Culmus *triqueter, aut teres, nudus, basi vaginatus*, vagina *brevi lamina terminata.*

Setarum seu potius foliolorum perianthalium indole hoc genus abunde differt a *Scirpo,* ubi setæ cartilagineæ retrorsum hispidæ persistunt, et revera perigynii vices supplent. In nostro autem genere vera stamina abortiva sunt ista foliola, linearia, mollia, pubescentia, siccando corrugata, fragilia facileque decidua. Quod quidem optime in specie nostra Chilensi cernitur, ubi stamina tria antica cum phyllis totidem posticis, æque longis filamentisque textura simillimis, at pubescentibus, seriem duplicem staminum sex, plantis his debitam, vix unquam autem tributam, explent. Aliud argumentum præbit *Malacochæte scirpoides,* quæ *Pterolepis scirpoides* Schrad. Etenim in hac setæ tres exteriores cum staminibus tribus interioribus alternantes apicem versus plumolosæ in antheris apice simili modo barbatis parem utrorumque produnt originem.

1. M. *pectinata* (N. ab E.:) culmo tereti apice trigono, panicula composita, laminis hypogynis cuneiformibus pinnatifido-ciliatis.—*Wight. ! Cat. n.* 1895; *Royle. herb. n.* 56.—Scirpus pectinatus, *Roxb. Fl. Ind.* 1. *p.* 218; (*ed. Wallich*) 1. *p.* 220.—S. campestroides, *Roxb. in Cœt. Merc. Ind. Or. Mus. tab.* 744 (ex *Arn.*)—S. plumosus, *R. Br. Prodr. Fl. Nov. Holl.* 1. *p.* 223?

HAB. In locis turfosis arenosis Peninsulæ Indiæ Or.; *Roxburgh*; *Wight.*—Himalaya; *Royle.*

Culmus 2–4 pedalis teres, spongiosus, apice obtuse trigonus. Vagina ad basin culmi una et altera, truncata, aphylla. Squamæ spicularum latæ, ovatæ, membranaceæ, ciliolatæ, apice emarginato-bidentatæ et nervo excurrente mucronatæ, inferne albidæ, apice medio totove testaceæ et fusco-testaceæ. Squamulæ duæ laterales, duæ vel una tantum posticæ, membranaceæ, fuscæ, apicem versus pulchre ciliolatæ, basi cuneatæ. Stylus longus bifidus. Caryopsis obovata, lenticularis, lævis, mucronulata, pallida.

Variat *α*, involucro (culmi apice) umbellam subæquante aut breviore, toto trigono aut triquetro, spiculis obscurioribus, perigynii squamulis quaternis. *Wight. l. c.*—*β*. Involucro umbella duplo longiori, incurvo, basi tereti apice triquetro, spiculis pallidioribus, perigynii squamulis ternis. *Royle, l. c.*

Scirpum plumosum R. Br. hujus loci esse haud improbabile quidem, id autem dubia movet quod cl. Auctor cum *Scirpo valido* Vahl, similem dixerit, cui in nostris speciminibus setæ sunt filiformes, rigidulæ, nonnihil flexuosæ et setulis reversis hispidissimæ.

VI. HYMENOCHÆTE. *P. de B.*

Ord. Nat. CYPERACEÆ, *Juss.*—Trib. SCIRPEÆ, *N. ab E.*

Spicula undique imbricata, squamis omnibus fertilibus aut infimis sterilibus. Perigynium 3–6-phyllum foliolis elongatis filiformibus membranaceo-mollibus hirtulis tempore maturitatis deciduis. Stamina tria, antica. Stylus trifidus, filiformis, basi bulbosus, bulbo cartilagineo in fructu superstite. Caryopsis trigona aut triquetra, ovalis, styli basi conica coronata, matura basi nuda.

Inflorescentia: Corymbus *compositus aut supradecompositus, involucratus.* Gramina *alta,* culmo *crasso triquetro, basi folioso.* Folia *et* Involucri *pleraque foliola longa, latiuscula, margine scabra.*

Proximum genus *Malacochæte,* a quo differt: stylo trifido bulboso, setis perigynii filiformibus, caryopsi trigona, culmo folioso, *Scirpi sylvatici* habitu.

1. H. *grossa* (N. ab E.:) spiculis decomposito-corymbosis, involucro subtriphyllo culmi angulis lævibus.—*Wight. Cat. n.* 1896.—Scirpus grossus, *Retz. Obs.* 5. *p.* 15.—*R. et Sch. S. V.* 2. *p.* 141; *Vahl, En.* 2. *p.* 270; *Roxb. Fl. Ind.* 1. *p.* 231; (*ed. Wall.*) *I. p.* 230.—Scirpus giganteus, *Roxb. in Cœt. Merc. Ind. Or. Mus. tab.* 764. (fide *Arn.*)—Scirpus, *Wall. Cat. n.* 3470.

HAB. In aquis dulcibus stagnantibus profundioribus; *Roxburgh.*—Nathpure; *Hamilton.*—Wallajabad; *Wight.*—Gongachora; *Hamilton.*

VII. MORISIA. *N. ab E.*

Ord. Nat. CYPERACEÆ, *Juss.*—Trib. RHYNCHOSPOREÆ, *N. ab E.*

Spicula monoica biflora, squamis senis distiche imbricatis, 4 inferioribus sterilibus, quinta hermaphrodito-fœminea, sexta mascula, minore inclusa. Stamina 3. Stylus longus, simplex, tortus, a basi bulbosa deciduus. Perigynium nullum. Caryopsis biconvexa, basi styli tuberculiformi obtusa coronata.

Inflorescentia: Spiculæ *terminales, capitatæ,* capitulo *involucrato.*

Habitus *Haplostylis,* a qua defectu perigynii et fructu apice tuberculato abunde differt. In uno flore autem setulam inveni brevem scabram caryopsi ad latus adstantem eademque multo breviorem. Inter spiculas majores fertilesque minores occurrunt et steriles, probabiliter quandoque masculæ.

Morisius Professor et Academiæ Turinensis membrum, Floræ tam cæteræ quam patriæ studio, operibusque pluribus botanici argumenti editis præclarus.

1. M. *Wallichii* (N. ab E.)—Rhynchospora, *Wall ! Cat. n.* 3422. *a.*

HAB. In Nepalia; *Wallich.*

VIII. HAPLOSTYLIS. *N. ab E.*

Ord. Nat. CYPERACEÆ, *Juss.*—Trib. RHYNCHOSPOREÆ, *N. ab E.*

Spicula monœca biflora, squamis distiche imbricatis, inferioribus sterilibus: squama fœminea univalvis; mascula terminalis, bivalvis. Stamina 3. Stylus longus, simplex; deciduus. Perigynium duplex: exterius minimum, membranaceum, ciliato-lacerum vel in setulas solutum; interius e setis 4–6 basi cohærentibus constans. Caryopsis biconvexa vel concavo-convexa, calloso- aut membranaceo-marginata, basi setis persistentibus cincta apice tuberculata vel in formam rostri compressi coangustata.

Inflorescentia: Capitulum *terminale, involucratum, e pluribus spicularum fasciculis sessilibus conflatum.* Habitus *Kyllingiæ,* sed textura spicularum rigidior.

Differt a plerisque Cyperaceis stylo simplicissimo indiviso; a *Kyllingia* insuper perigynio setuloso; a *Carpha* setis brevibus, non triquetra, spiculis monœcis.

1. H. *Meyenii* (N. ab. E.:) involucro capitulum superante foliisque glabris, styli basi pileiformi caryopsin biconvexam tuberculo claudente.—*Wight. Cat. n.* 1903.—Rhynchospora, *Wall. Cat. n.* 3428.—Scirpus retusus, *Kœn.*

HAB. In China, *Meyen; Vachell, n.* 65.—Ceylona; *Rottler; Klein; Koenig; Macrae.*—Amboina; *Lesson.*

IX. CEPHALOSCHŒNUS. *N. ab E.*

Ord. Nat. CYPERACEÆ, *Juss.*—Trib. RHYNCHOSPOREÆ, *N. ab E.*

Squamæ distiche imbricatæ, duæ inferiores vacuæ, supremæ abortivæ. Perigynii setæ 6, denticulis antrorsum spectantibus scabræ, caryopsi cum rostro breviores. Stylus simplex, ovario articulo conjunctus; medioque articulo partibiles. Caryopsis compressa, basi styli longa angustaque aristata.

Inflorescentia *capitata,* capitulo *terminali solitario vel pluribus corymbosis.*

Differt ab *Haplostyli* perigynio setoso longiori et styli basi longa angusta rostriformi.

1. C. *Zeylanicus* (N. ab E.:) capitulis subtristachyis, axillaribus simpliciter corymbosis, terminalibus duplicato-corymbosis paucifloris, radiis foliisque linearibus elongatis margine scabris.

HAB. In Ceylona insula; *Macrae.*

Affinis C. *articulato*, sed diversus spiculis subternis, nec pluribus, alternatim approximatis et adeo veluti spicatis fasciculatisque, corymbo multo laxiori paucifloro. *Rhynchospora aurea* Vahl, utrum ad *R. corymbiferam* N. et. Mey., an ad hance nostram speciem referenda sit, in dubio relinquimus.

2. C. *articulatus* (N. ab E.:) spiculis, subcapitatis, capitulis fasciculatis corymbosis, corymbis axillaribus decompositis terminali superdecomposito multifloris erectis, ramulis tenuibus, bracteis inferioribus ramos æquantibus setaceis, rostro caryopsi longiore, foliis lato-linearibus carina et margine scaberrimis.—*Wight! Cat. n.* 1904.—Schœnus articulatus, *Roxb. Fl. Ind.* 1. *p.* 184; (*ed. Wall.*) 1. *p.* 189.—S. umbellatus, *Roxb. in Cœt. Merc. Ind. Or. Mus. tab.* 703. (ex *Arn.*)—S. corymbosus, *Heyne.*—S. Surinamensis, *Heyne.*—Rhynchospora, *Wall. Cat. n.* 3424.—R. aurea, *Herb. Heyne* (minime *Vahl.*)

Hab. In locis uliginosis regionis montanæ superioris Penins. Ind. Or.; *Roxburgh; Heyne; Wight.*

C. *Zeylanico* persimilis, differt statura multo majori, foliis latioribus, caryopsi duplo grandiori, rostro crassiori caryopsi duplo fere longiori, squamis spiculæ mollioribus.

X. CYLINDROPUS. *N. ab E.*

Ord. Nat. CYPERACEÆ, *Juss.*—Trib. Sclerieæ, *N. ab E.*

Spiculæ diclines, monœcæ, fœminea uniflora. Squamæ distichæ, inferiores vacuæ. Stamina tria. Stylus trifidus. Nux nitida, obtusa cum papillula, basi perigynio cylindrico truncato constricta.

Differt a *Scleria*, cui omnino proxima, perigynio non lobato, basin nucis in formam pedunculi cylindrici brevis constringente, et spiculis tam masc. quam fœm. distichis. Culmi apice longo tractu nudi.

1. C. junciformis (N. ab E.)

Hab. In Ceylona insula; *Macrae* in Herb. Lindl. (inter specimina Scleriæ tessellatæ.)

Culmus triqueter, ad angulos retrorsum scabriusculus; ad basin diphyllus (in uno exemplo). Vaginæ trigonæ, villosæ: lobulus oppositifolius, obtusus. Folia linearia, 1–1½ lin. lata, plana, hirsuta, (in nostro apice mutila). Involucrum monophyllum, erectum, culmum continuans, carinatum, hirtulum, 2½ pollices longum. Spica basi subdivisa, dein simplex, ad speciem lateralis, longitudine involucri. Rachis triquetra, hirsuta, scabra, flexuosa. Spiculæ glomeratæ, ternæ, quaternæ, quarum centralis una vel ultera fœminea, exteriores masculæ et una earum pedicellata, reliquæ sessiles. Bracteolæ lineari-setaceæ, hirsutæ, spiculas duplo superantes. Spiculæ florentes lanceolatæ subdistichæ, 3 lineas longæ. Masculæ multifloræ, squamis duabus inferioribus vacuis, omnibus ovato-oblongis acutis carinatis fusco irroratis glabris. Stamina tria, squamas excedentia. Spiculæ fœminæ squamæ ejusmodi, sub fructu majores. Nux terminalis, fere bilinearis, ovalis, papillata, alba, nitida, striata, costis interjectis angustis obtusis remote obiterque tuberculatis. Basis nucis dimidio angustior ¾ lineæ longitudine, cylindrica, hypogynio concreto truncato margine parum tumidulo lutescente tecta.

Hujus generis, ni forte ejusdem speciei, esse existimo *Scleriam poœformem* Retzii non nisi fortunato aliquo jactu divinandam.

XI. HYPOPORUM. *N. ab E.*

Ord. Nat. CYPERACEÆ, *Juss.*—Trib. Sclerieæ, *N. ab E.*

Spiculæ androgynæ, trifariam imbricatæ; fœminea infera, masculum terminalem amplectens bivalvis; mascula compressa, quadrivalvis, disticha, triflora, fœmineæ opposita ceu valvula spiculæ universalis tertia. Stamina duo, vel unum. Stylus trifidus, deciduus. Perigynium nullum. Nux basi contracta, supra partem angustatam trifariam porosa vel saltem depressa ibidemque angulato-sulcata aut punctulata.

Inflorescentia. Spica *terminalis.* Spiculæ 2–4 *glomeratæ, bracteolis brevibus suffultæ.* Squamæ *membranaceæ, coloratæ, carinatæ, sæpe hirtæ.*

1. H. *pergracile* (N. ab E.:) culmo erecto filiformi simplici triquetro, glomerulis spicatis alternis paucifloris bracteam membraceam æquantibus, nuce depresso-globosa mucronata tuberculato-echinata alba, subtus sulcis tribus eporosis impressa.—Scleria, *Wall. Cat. n.* 3406.

Hab. Silhet; *Wallich.*

Simile *Hypoporo* (*Scleriæ*) *interrupto.*

2. H. *capitatum* (N. ab E.:) culmo erecto simplici, spiculis capitatis foliisque hirsutis, nuce rugosa tuberculata basi trifariam biporosa, squamis masculis apice fimbriato-laceris.

Hab. Ceylona; *Macrae* in Herb. Lindl.

Radix fibrosa, sanguinea. Squama ovata obtusa, sanguinea, tomentosa, ad basin culmi et fasciculorum. Culmus digitalis, erectus, acute triqueter, circa genicula hirsutus, ad apicem foliosus. Folia linearia lin. 1 lata, obtusa, plana, hirsuta, superiora culmum superantia, supremum involucrale erectum reliquorum forma et magnitudine. Capitulum basi nudum, in pedunculo (seu potius culmi apice) nudo pollicari triquetro hirsutoque, nucis avellanæ magnitudine, ex aliquot spicularum fasciculis oligostachyis constans, depresso-subglobosum. Spiculæ in fasciculo ternæ, lanceolatæ, compressæ, aliis omnes fœmineæ, adjecta in aliis mascula minore. Fœmineæ 3 lin. fere longæ, e squamis ovato-lanceolatis subulato-cuspidatis rigidulis hirsutis persistentibus exstructæ, quarum duæ inferiores duplo minores, duæ terminales florem includunt. Stylus longus, trifidus. Nux globosa, obtusa, opaca, alba, tuberculis exasperata, seminis brassicæ ambitu, basi ad ¼ constricta trigono-sexangularis et supra stricturam inferius transversim trifoveolata singula foveola biporosa. Perigynium nullum, nisi costæ hypopodii. Spicula mascula 1½–2 lin. longa, linearis, squamis angustioribus minus hirsutis; duabis infimis vacuis lanceolato-acuminatis, reliquis membranaceis convolutis apice in lacinias lineares plerumque 6, filamenta castrata mentientes et fortasse ex eorundem coalescentia derivandas, fissis. Superiores fertiles staminibus tribus, antheris angustis flavis. Color spicularum pallide rufescens, acumine squamarum virente.

XII. TRILEPIS. *N. ab E.*

Ord. Nat. CYPERACEÆ, *Juss.*—Trib. Elyneæ, *N. ab E.*

Spiculæ androgynæ, undique imbricatæ. Gluma (squama) univalvis, uniflora. Perianthium masculinum fœmineumque bivalvia, valvulis glumæ parallelis superiori inferiorem amplectente; quandoque univalvia, sola inferiori valvula obvia. Stamina duo-tria. Stylus trifidus. Perigynium longe rostratum, ore truncato coarctato.

Inflorescentia. Spiculæ *pedicellatæ, fasciculatæ, vaginis pedunculos alte coercentibus.*

Differt a *Cobresia* perianthio masculino præsente, omnique habitu.

1. T. *Royleana* (N. ab E.:) triandra, spica composita densa, spiculis apice masculis, squamula propria subsolitaria, foliis latiusculis falcatis culmique trigoni angulis scaberrimis.—*Herb. Royle. Cyp. n.* 119.

Hab. In Himalaya; *Royle.*

In flosculis fœmineis quandoque occurrit altera valvula magisque interior, extrorsa, duplo minor, linearis, primariæ a latere glumæ opposita, et ab illa una cum pistillo inclusa. Vides igitur in hoc stirpe non modo perigynii *Caricum* communis originem, sed etiam setæ accessoriæ *Unicaricum.*

2. T. *Lhotzkiana* (N. ab E.); diandra, spiculis simplicibus fasciculatis axillaribus, squamulis propriis binis, foliis lineari-subulatis.—Carex Lhotzkiana, *Herb. Endl.*

Hab. Brasilia, ubi in Corcovado *Lhotzky* invenit.

Memoir on the Inquiry, Whether any Terrestrial Animals have ceased to exist since Man's creation; and whether Man was cotemporaneous with Species which are now lost, or which at least do not appear to have representatives now upon our globe. By M. Marcel de Serres. (Continued from vol. XVI. p. 289.)

Concerning real Beings, now existing, depicted on Antique Monuments, whose Species we can recognise.

We have, on a former occasion, brought under the observation of our readers, not a few instances of existing animals depicted in antique monuments; and we have taken leave largely to insist on the extreme accuracy which the ancients displayed on these memorials of their skill. As, however, we learn that the details into which we entered have not appeared sufficiently numerous to many antiquarians, we now proceed to supply some additional ones; at the same time remarking, that the circumstances in which we are placed does not afford facilities for the examination of the original monuments themselves. All, then, that we can now do, is to allude to those examples which are contained in the works to which we have access.

Previous to entering into these details, and in order to abridge them, we may remark that there is a very considerable number of animals which abound so largely upon the monuments of antiquity, that we shall point out, only in the general, the works in which they are represented.

Of these we may enumerate among the terrestrial mammalia which are most frequently figured, the various races of dogs, of horses, of oxen, and of wild boars; and, along with these, lions, panthers, leopards, elephants, stags, and antelopes. Amongst the birds we may mention the eagle, the hawk, the vulture, the raven, the crow, the ostrich, the swallow, the lark, and titmouse, the partridge, the pigeon, the peacock, the domestic cock and hen, the swan, and the duck. As it regards reptiles, the crocodile, especially that of the Nile, and the various varieties of serpents and tortoises, are most frequently depicted. And, in respect to fishes, it may be remarked, that they are found not nearly so frequently as the other classes. They are seen chiefly upon the monuments of Pompeii and Herculaneum ; and they might be reconciled with the living varieties, if it were quite certain that they had been represented with the whole of their characters, especially with all their fins, and these in their real positions.

An important branch of this subject consists in the determination of the various kinds of vegetables that are represented on antiques; and these are much more numerous than we had at first imagined. These plants are generally figured with a degree of accuracy quite sufficient for their classification, though it often requires a somewhat minute examination. In truth, the same kind of uncertainty which prevails whilst determining fossil plants, exists here in a still greater degree; the mutual bearings of forms having neither the same importance, nor being equally necessary to vegetables as to animals. But, in spite of this difficulty, which is inherent in the subject, we believe we can demonstrate that the plants so depicted are much more numerous, not only as it regards individuals, but even species, than has been hitherto supposed. This is a point to which, at another time, we shall direct the attention of the geologist and the antiquarian.

Respecting the animals above alluded to, they may be found engraved in a great number of works upon antiquities, both Greek and Roman. Of these we name the following, because they contain the greatest number. First, *Antonii Augustini Antiquitatum Romanorum Hispanarumque, &c. Antverpiæ*, 1617. Some very rare animals are here represented with great accuracy, such as the antelope, oryx, and bubalis (the Barbary cow, tab. 58 and 60), and also the hippopotamus. There is another work of the same author, entitled *Regum et Imperatorum Romanorum Numismata*, in which there is also a great number both of wild and domestic species, equally well engraved. The antique medals copied in this work demonstrate that the ancients had minutely distinguished the different races of the horse, and the crosses betwixt the jack-ass and mare, and the horse and ass. The *mules* represented in plates 26 and 32 are sufficient evidence of this. Other examples may be seen upon a Mosaic, engraved in a work with the following title, *Li Antichi Sepolcri ove Mausolii Romani ed Etruschi di Pietro Bartoli*, and printed at Rome 1696. Others will be found upon the medals dedicated to Tiberius, and which are described in the work of Bellori, entitled *Adnotationes nunc primum evulgatæ in XII primorum Cæsarum numismata.* Romæ, 1730. Finally, upon the reverse of a medal dedicated to Julia Pia Augusta, we observe two mules harnessed to a car. There is the same design on a medal of Agrippina's, which is to be found, as is the former, in the work of John Vaillant, entitled, *Numismata Imperatorum Romanorum Præstantiora*, and published at Rome in 1743.

We see, then, that the ancients were acquainted with the different crosses of the horse and ass. That between the jackass and the mare, they denominated ὀυρεος *mulus*, mule, and that between the horse and the ass, ἵννος *hinnius*, or γἵννος *ginnulus*. It is not less certain they distinguished the several races of the horse. Judging from the statues, and the descriptions they have left us, we perceive they recognised four principal varieties of the race-horse, the war-horse, and the draught horse. These races are, 1. the African; 2. the Apulian; 3. the Thessalian; and, 4. the Sicilian. From these principal races proceeded the various secondary varieties, which are so abundantly figured on the monuments of antiquity.

Their acquaintance with the genus Equus, *horse*, was very extensive, and probably they attached much importance to it on account of the great use they made of these animals. The *onager*, or wild ass, was also well known to them, which is a proof that the ancients had penetrated into the interior of Asia, and to the west of Africa. This species, so well depicted by the ancients, is now found to exist in its wild state in Persia. The zebra, another species of the same kind, by Dion denominated simply *hippotiger*, was also familiar to the ancients, consequently they had also opened up communications with the countries of South Africa, the native country of this variety. The zebra which was exhibited in the games of the circus, astonished the Romans as much by its agility as by the colour of its skin, whence among them it received the name of Tiger-horse.

The *Dziggetai*, or *Equus hemionus* of naturalists, was also known to them, for this species had been domesticated by the Greeks in many provinces in Asia. If we are to regard the Mosaic of Palestrina as authoritative, the ancients were also acquainted with another species of the same genus, intermediate between the preceding and the couagga (Equus quaccha). If this species, as every thing seems to indicate, has really existed, it must have totally disappeared and become extinct, as has happened with so many other races, the former existence of which we know only by those remains of them that are found in the various strata of the earth.

But the ancients have bestowed not less attention to the various species of the *dog* than to the several races of the horse. They appear even to have had dogs so large and powerful, that they could harness them to their chariots. Thus Heliogabalus made himself be driven in his chariot by four dogs of a prodigious size; whilst, at other times, he preferred four stags, or it might be lions or tigers*. It is not less true that their monuments exhibit a crowd of other varieties of dogs, amongst the most common of which we mention the greyhound, the mastiff, the pointer, the harrier, the setter, and the spaniel. This last variety is found upon a carnelian stone, and may be found represented in a work of Agostini, published at Rome in 1686, under the title of the Antique Gem. The other varieties may be seen upon divers monuments which are copied in numerous works, among which we shall only mention *Le Rovine della cittá di Pesto detta ancora Posidonia* (Roma, 1784); also *Le pitture del Museo in Portici trovate, incise da Baltassare Probst* (Augusta 1795); and, lastly, *Le Antiche lucerne Sepolcrali, figurate da Bellori* (Roma, 1691) †.

It will not be questioned that lions, tigers, panthers, leopards, and bears, which were exhibited in such wonderful profusion in the amusements of the Circus, or in the triumphal processions, are found in abundance on the antique monuments. These, in fact, are the most common. We shall mention, then, only a very few

* L'Antiquité expliquée, de Montfaucon, tom. iii. part ii. p. 271.

† The author last quoted, in reference to the various races of dogs, as well as of other animals brought under his review, furnishes a list of forty-two archeological works, which is here omitted.

of the principal works where copies of them may be found. In doing otherwise we should soon become tedious. We may, however, in passing, remark, that the number of the larger carnivorous animals which the ancients were in the habit of putting to death in their public games, was so immense, that all the sovereigns of Europe, and of the whole world, would attempt in vain to bring together as many. Thus Trajan, after his victory over the Parthians, exhibited games in which were produced 11,000 wild beasts, all of which were put to death. Pompey, even at the opening of his theatre, exhibited to the people a one-horned rhinoceros, 410 panthers, and more than 600 lions, more than 300 of which had manes, and were of course males.

A knowledge of the immense number of animals that were thus destroyed in the games of the circus, is, on more accounts than one, interesting to the naturalist. It clearly shews that the various savage races, and more especially the various carnivorous animals, were formerly much more numerous than they are now. Moreover, as finally all these animals were put to death, whether in the circus or after their triumphal feats, the anxiety with which the emperors, and other ambitious citizens, collected such prodigious numbers, must have contributed rapidly to diminish the noxious animals it was so much man's interest to destroy.

The effect thus produced was so much the greater, inasmuch as those public shows of the destruction of animals, which at first had only a political object, became ere long a favourite subject of the almost inconceivable luxury of the rich. Among other nations, as for example some of the Asiatic, a religious character was given to the destruction of wild beasts. A favour, more familiarly known under the name of an *indulgence*, was the reward. Whoever destroyed a tiger or a rhinoceros procured an indulgence for a hundred years, whilst he who slew a lion was rewarded with one of a thousand years' duration. This difference was, without doubt, owing to the greater importance and difficulty connected with the destruction of a lion. According to this view, it may be supposed, that the same religious ideas would allow only a month's indulgence to the killing of a fish or a tortoise, and three months to the destruction of a crocodile *.

We now remark, that in the *Thesaurus antiquitatum Roma-*

* See Asiatic Researches, vol. v. p. 371.

norum, a J. Grævio, (Lugd. Bat. 1694–99), and in the supplement entitled *Nova Supplementa congesta ab Joanne Paleno* (Venitici, 1737), as also in his dissertation on the games of the circus, may be seen a considerable number of the larger carnivorous animals, as depicted by the ancients, and combating with men.

The medals called *Antonian*, which are figured in the work of Morellianus, or of Sigebert Hauercampi *(Familiarum Romanorum numismata omnia; Amstelodani*, 1734*)*, present us with a great number of *lions*. This is also true of the work of Gorius *(Tresor des pierres gravées Antiques)*, which we formerly had occasion to quote when speaking of the chimæra. The lion is also very often represented as an accompaniment to Bacchus on antique monuments. Thus he is found on various bas-reliefs and precious stones, noticed in the greater number of works which speak of him, such as those of Montfaucon, d'Augustini, of Gessner, Gorius, and of Bartoli.

It would appear that lions were so abundant at Carthage and in Rome, that they succeeded in subduing and taming them. Thus, Hanno had a lion at Carthage which was so tame, that it followed him every where like a dog. Some years before the Christian era, Anthony had tame lions harnessed to his chariot; whilst, on the other hand, Domitian exhibited to the people a woman combating with a lion; and, later still, in the same games of the Circus, a tiger overcoming another lion. Quintius Scævola, as is well known, was the first who exhibited lion-fights in the Circus to the people of Rome.

The true *tiger*—the *Felis tigris* of naturalists—is more rarely represented in antiques than the lion, the leopard, and especially the panther. The first which was brought to Rome was exhibited in a cage, at the dedication of the temple of Marcellus. Claudius exhibited four at a later period at the opening of the Pantheon. A Mosaic which has come down to our days, represents these tigers of the natural size, so that they can be compared with the existing species.

The royal tiger is also very accurately figured on many engraved stones, which are represented in the works we have already named; and also in those of La Chausse, of Mariette, of Montfaucon, and of Ciampini.

As to *Panthers* and *Leopards*, they were more frequently represented on antiques than the tiger. They were almost as common as the lion. Very many of them were brought to Rome. The first that were seen, were exhibited in the Circus by Marcus Fulvius, 156 years before the Christian era. This example was followed by Scipio Nasica and Publius Lentulus. The latter succeeded in collecting sixty-three of these animals. But this number was soon exceeded, first by Pompey, who, as we have already noticed, sent four hundred and ten into the Circus; and afterwards by Augustus, who exhibited three hundred and twelve of these animals to the people. Still later, Gordian succeeded in increasing the number that appeared in the games even to a thousand, although Probus, more than any other of the Roman Emperors, collected a prodigious number of wild beasts for the Circus shows.

Regarding the *Elephant*, it seems worthy of especial notice, that the ancients appear to have had more accurate ideas concerning it than our great modern naturalists, including even Buffon and Linnæus. In truth, Aristotle knew the organization of the elephant better than Buffon, and what he has written of its history and manners, is also more accurate. Nor is this all; neither Buffon nor Linnæus had distinguished the two species of the elephant; which, however, the ancient authors and statuaries were well acquainted with. In the dissertation of Cuper, inserted in the *Novus thesaurus antiquitatum Romanorum* of Sallengre, may be seen some details as interesting as curious, concerning the games in which the ancients employed the elephant, and concerning the two species with which they were acquainted. Cuper also informs us, that Seleucus Nicator, King of Asia, possessed no fewer than five hundred of the Asiatic elephants; whilst the Ptolemies, on the other hand, never employed, either in their wars or feasts, any other than the African variety. It would also appear, that it was one of these Ptolemies, probably Ptolemy Philadelphus, who introduced the art of hunting and catching these animals.

This art speedily arrived at great perfection, for the number which the Emperors and other grandees of Rome soon exhibited, is quite astonishing, especially when we think of the difficulty there is in catching them. The first that were brought to

Rome were put to death in the Circus by order of the Magistrates, 136 years A. C. At a later period they were used in battle, and also, when tamed, in domestic life. Thus, in a *fête* given by Cæsar, twenty elephants encountered, first five hundred foot soldiers, and afterwards an equal number of cavalry. After the conquest of Macedonia, Metellus had one hundred and forty two elephants conducted to Rome, all of which were killed with arrows. During the night of that day on which Cæsar gave his grand fête, in which there was the combat with the elephants, he went home, illuminated by elephants carrying lanterns. Domitian exhibited to the people an elephant which, after having vanquished a bull, came to prostrate itself on its bended knees before the Emperor in token of respect. Before this period, Germanicus, on the occasion of his triumph over the Germans, exhibited elephants which had been taught to dance, and which were not loth to exhibit their accomplishments.

Be this as it may, however, the two species of the elephant are most accurately represented in the Roman and Greek medals, particularly on those of Alexander, of Commodus, of Antoninus Pius, of Antiochus, and Alexander Severus. The African species is easily distinguished by the round form of the head, the prominence of its forehead, and the size of its ears, in the medals of Regulus, and in some of those that are dedicated to Julius Cæsar. In a word, we may say that both the species of the elephant, are drawn and engraven on an endless variety of monuments. Sometimes they are represented as partially clad, or laced with a variety of cords and nets.

The same general remark may be made of the other pachydermata—as the rhinoceros, the hippopotamus, and the wild-boar, as also of the varieties of the *hog*, which, as is generally believed, is derived from the last of these animals. There is especially one of these races of the hog, which must have been very common at Rome, if we may judge from the frequency of its appearance on the monuments. It is from Guinea, and is easily distinguished from any other by the remarkable bristles with which it is covered on the neck and back, and which are continued even to the loins. This variety has always been very common in Africa, with which continent the Romans had the freest intercourse. In proof of the great frequency of this animal on antiques, we may adduce the medals of Antoninus and other Emperors, which may be seen in the works of Montfaucon, of Patin, and Sallengre. We may also allude to the various discoveries of Herculaneum. Some other races are scarcely less frequently depicted upon these same monuments, as, for example, that like to the hog of China, which is characterised by limbs so short, that the abdomen of large dimensions touches the ground. This race is very well figured in the 45th plate of the fourth volume of the *Antiquités d'Herculaneum*, which was published in Rome in 1729.

It is quite ascertained, that the *wild-boar*, as well as the various races of the hog, was well known at Rome. The first wild-boar presented entire appeared at a repast given in Rome, by Servius Rullus. Anthony, at the period of his triumvirate, caused eight of them to be served up, and all entire. The *rhinoceros* also was well known to the Romans, and they represented it on many of their monuments. The first double-horned rhinoceros (Rhinoceros Africanus), seen at Rome, was in the reign of Domitian. Decidedly before that period, and fifty-five years before the Christian era, Pompey, at the opening of his theatre, had exhibited a single-horned rhinoceros (Rhinoceros Indicus, Cuvier) to the multitude. This species which is engraved on the mosaic of Palestrina, is also represented upon other mosaics and medals, and particularly on a stone engraved in the work of Thomas Mangeart, which has been already quoted.

Nearly the same remarks might be made of the *hippopotamus*, which, though very inaccurately described by Latin authors, has nevertheless been represented with much fidelity by the statuaries of the same nation. It is designed with fidelity upon the mosaic of Palestrina, and upon other monuments, such as the medals which are dedicated to the Emperor Julius Philippus. These medals have been copied in the works of Vaillant, Patavini, and Mangeart. It is, moreover, known, that the first hippopotamus which was seen at Rome, was brought thither under the direction of Emilius Scaurus, who, whilst Edile, took every pains to exhibit to the people animals that had never before been seen in the Circus. It was he who also presented to them the bones of the animal to which it was said that Andromeda had been exposed, and as one of these bones was thirty-six feet in length, it is probable it was the lower jaw of a whale.

The ruminating animals had also attracted the attention of Egyptian, Greek, and Roman statuaries. The *Antelope Gazella*, which has been seen in Europe only within these few years, is well represented on the monuments of ancient Egypt. This is also the case with the *Oryx*, or the antelope with straight horns, which, figured in profile, and in the usual rude style of their artists, would probably be the origin of the fable of the unicorn. We say the fable; for an animal with cloven feet, and the middle of whose frontal bone is divided by a suture, cannot have a horn springing from an osseous tissue in the mesial line of its head. The *Irish elk*, so long regarded as a fossil species, was also known to the Romans. At least Hibbert, if he has not found its representation in the delineations of Pompeii and Herculaneum, has at all events discovered it in an ancient painting and sculpture, which has been found in Rome. This species, which the drying up of lakes and morasses has contributed to extinguish, appears to have lived within the times of historical record, since Opian has very well described it, as has been previously observed by Aldrovandus. Another circumstance that very distinctly proves how recent the extinction of this species must be, is that Hart has observed a callus on one of its bones which was discovered in Ireland,—a callus which appeared to have grown up after the infliction of a wound with some pointed and cutting instrument. The *Cervus euryceros* of Aldrovandus, or the stag with gigantic horns, is, then, a species which has become extinct on the surface of our globe, within the period of historical record, as is demonstrated not only by the painting discovered at Rome, but also by the descriptions which are found in Opian, in Munster, and also in Jonston. But moreover, the bones of this elk being found mixed in the same muddy deposits with the bones of the rhinoceros, the elephant, the hippopotamus, and the hyena, ought it not to have been the same with them? By no means; and assuredly there is nothing contrary to the usual laws of nature in the contrary supposition. Can it be regarded as contrary to the usual order of things, that an animal that was continually pursued by the Romans, and great numbers of which they consequently destroyed, both in their games and in their triumphal exhibitions, has finally disappeared, through the effect of causes, which, though slow, were not less continued and sure? This species, which, on account of the size of its horns, could not easily find a safe retreat, has the more promptly disappeared, inasmuch as the swampy marshes where it was wont to dwell, have themselves dried up and disappeared.

Besides these species, the ancients have represented a great number of the other species upon their monuments. Thus we recognise the common stag and the hind, the fallow deer and the roebuck, the various gazelles, as well as the more common members of this family, viz. the goat and the kid, the ram and the ewe. Goats in an especial manner attend upon the satyrs, the fawns, and all the rural divinities; and as these are often represented upon antique cameos and medals, it is the same with the animals peculiarly devoted to them.

The ancients have also well distinguished the two species of the *camel*, viz. that with two humps (*Camelus bactrianus*, Linn.), and that which has only one, and known under the name of the dromedary (*C. dromedarius*, Linn.) We are soon satisfied of this by glancing the eye upon the medals consecrated to Adrian, to Commodus, to Caligula and Caracalla, all of which may be seen in the work of Patin, to which we have previously alluded. These animals are not less abundantly found upon the medals copied into the work of Hauercamps, and principally on those which go under the name of *Æmilia*.

It appears that the two species were presented to the people in the fête which Ptolemy gave in honour of his father Ptolemy Soter. It was in this fête, the triumph of Bacchus, was represented, and in which a very great number of animals were exhibited. Among these Athenæus has distinguished the elephant, the stag, the antelope, the oryx, an hundred and thirty sheep of Ethiopia, three hundred of Arabia, and twenty of the island of Eubœa; also white stags of India, and twenty Indian oxen, remarkable for their brilliant whiteness, and eight others of Ethiopia. There were, besides, a great number of leopards, panthers, and white bears, of ostriches and parrots, as well as a great crowd of Ethiopian birds. It also appears that there were four lynxes, an Ethiopian rhinoceros, and a cameleopard. What appeared still more remarkable was a pack of two thousand four hundred dogs, and which was followed by twenty-four male lions of consummate beauty.*

We have already spoken of the astonishment which was manifested by modern naturalists, on the assertion that Ptolemy had exhibited a *white bear* in the feast which he gave to the people of Alexandria, on his accession to the throne. The same naturalists have appeared equally surprised that Megasthenes has stated in his voyages, fragments of which are preserved by Strabo, Arrian, Ælian, and Athenæus, that *bears* were to be found in the south of India. In truth, for a long period, it was not known that they really existed in that country; but some little time since, many species have been discovered there, among others the jungle bear, and it is only fair to do justice to the accuracy of Megasthenes, as well as to other naturalists and writers of antiquity.

Megasthenes has affirmed that most of our domestic animals are to be found in a wild state in India, an assertion which has more lately been confirmed by Ælien, and the accuracy of which is now becoming more apparent and specific, as it regards many animals. But, without dwelling on this, it is at all events certain, that the bear is found on antiques, quite as often as the several large carnivorous animals of which we have already spoken. They are also extremely well depicted, and can easily be recognised. Respecting the species of the kind, they must have been well known at Rome; for Scipio Nasica and Publius Lentulus exhibited more than fifty individuals at a time, and Caligula alone caused 400 to be slaughtered in the Circus.

The *buffalo*, the *bison* of the ancients, has been also represented upon many of their monuments. It would even appear that the Romans had succeeded in training them; at least the Emperor Domitian harnessed them to his chariots. At a later period this animal appeared in the games of the Circus, and amongst more than 400 animals that Septimus Severus presented, as issuing forth from an immense machine, in those fêtes

* See Athenæus, lib. v. pp. 196, 203. Also the description of this Feast in the History of the Commerce and Navigation of the Egyptians, under the Ptolemies. By Ameilbon. Paris, 1766, p. 70.

which he gave in honour of the marriage of Caracalla, many bisons were observed, as were also wild asses. It may easily be recognised that it is of no small importance in relation to the question which now engages us, to know exactly the precise epoch in which such and such an animal was known, from which country he was brought, and in what number. Very many are the inquiries that are made on these points, their solution can be given only by entering into such details as these; but their further prosecution may properly be excused.

We shall here introduce another remark regarding one of the Ruminantia, of which we have already spoken. We allude to the *sheep*, and the fact before stated, that the ancients have supplied but few representations of the ram and ewe, in comparison of those they have given of the goat, the kid, and the she-goat. And this is the more astonishing, if we are to believe Varro and some Greek philosophers, who maintain that the sheep was the first animal man succeeded in domesticating. From this opinion Buffon, and most of the modern naturalists, dissent, and contend, on the contrary, that the dog was the first animal he subdued; and the immense number of representatives of it on antiques seems rather to countenance this supposition. The comparative rarity of the sheep in the ancient sculptures is the more remarkable, as, from the time of Varro, this animal has been found in very many countries in its wild state. Thus, in Phrygia and in Lycaonia, many wild sheep are found, and it is the same of the goat in Samathracia. The details which Varro has left us on this subject merit the greater confidence, as his assertions have very recently been verified. Thus he had informed us, that Thibet was the native region of the *onager* or *wild ass;* and, in fact, they are now found in the Mountains of Taurus and the lower Kurdistan, in those mountains which separate Persia from the Afghans. This animal still exists there in its wild state, and hunting it is one of the most common amusements of the Persian princes. It is also known that oxen are found in their wild state in Mysia, Dardania, and Thrace, and horses in certain parts of northern Spain. The wild ass is figured not unfrequently on ancient monuments. In proof of this we only quote the twenty-seventh plate of the works of Micali, and the twenty-ninth of those of Caylus. To these engravings we might add a host of others, which have been copied from antique medals.

A great number of the Rodentia have likewise been handed down to us. We clearly recognise the *rabbit* and common *hare*, and also that of Egypt, so remarkable for its long ears. This species, which is very common on all the monuments of Egypt, is scarcely less common on many antique stones, which are to be found represented in the works of Micali and Montfaucon, as well as on many of the bronzes which have been discovered at Herculaneum. This is also true of the *beaver*. This class of animals attracted the larger attention of the ancients, inasmuch as many of them are such delicate articles of food. It was for the purpose of perpetuating them, and augmenting their numbers, that they conceived the idea of preparing parks for the rearing and feeding of them. These parks seem first to have been used by Fulvius Hirpinus, about the time of the Second Punic War. They were with propriety denominated *Leporina*, because there were reared at least three varieties of the hare,—the common one, the original one, or that from Spain, and also the Alpine, a variety which, now-a-days, is almost extinct. In them also were reared the greater number of the fallow deer, which were found in the ancient forests, and with them were also bred the mouflon or wild sheep.

The animals which were reared in these parks were all but domesticated. They were taught to assemble at a given call. Thus, Hortensius having invited a number of visitors to his country residence, at the sound of a horn, a number of stags, roebucks, wild-boars, and fallow-deer, speedily assembled. These animals collected in crowds close by the apartment in which they were dining. It is easy to perceive that the care which the ancients thus took in rearing so many animals in their parks, and those which they designed for the games of the Circus, must have necessarily extended their knowledge of wild animals, and led them to remark their predominant characteristics. We may here remark, that there are many which they have very well described, and which have often been regarded fabulous, till they were afresh discovered. This was the case, for example, among the Rodentia, with the *prickly mouse*, stated by

Aristotle and by Ælian as being found in Egypt and Lybia, and which, till very recent times, has not been found in either of these countries. In fact, this mouse had not been met any where till the time of the French expedition into Egypt; and we may almost say that this expedition has fully confirmed the account of Aristotle, and proved that the ancients were very careful never to advance any thing without being sure of its truth. So has it been with the boar with the two tusks, or the *Babiroussa*, of which Ælian has spoken so largely in detail: it was not discovered in any of the most distant countries of India till after the revival of letters; and previous to that time it was regarded as wholly chimerical and fabulous.

Finally, among the Rodentia, which were well known to the ancients, we have still to name the *grey dormouse*, which was highly esteemed among the Romans. This animal was with them an object of particular care; and they fed it with much attention, that it might be presented at the tables of the great. It is well known how far the Romans carried this sort of luxury. It was for its indulgence that, after the introduction of the parks we have above alluded to, Lucius Strabo introduced aviaries, and Lucinius Murena fish-ponds. In their grand repasts, such was their sensuality, that they had dishes of the brains of ostriches, the tongues of the flamingo, the grouse of Phrygia, the cranes of Milos, and the pheasants of Colchis. Hence too it was that the gourmand Hortensius constructed fish-ponds of salt water, in which he fattened the most delicate fish, such as soles, whitings, lampreys, gold and silver fishes *(Dorades)*, and the shell-fish of the ocean. Other ponds intended for trouts, pike, and salmon, were supplied with fresh water.

The luxury and ingenuity of this kind which was exercised by the ancients is great indeed; and excites our astonishment at the number of fish they were accustomed to feed in their fish-ponds. The quantity was so great that, according to Pliny, Cæsar borrowed from Irrius no fewer than six thousand lampreys for a feast which he gave to the Roman people. The number which Irrius lent to Cæsar was, according to Varro, not so great as that specified by Pliny, but by the smallest calculation it would amount to two thousand. Even this number is so prodigious, that it enables us to form an estimate of the height this species of luxury had reached, among all those whose object it was to purchase minions, or to procure friends.

The luxury which led the Romans to collect so many fish in their ponds, directed them also to assemble an immense number of different birds in their yards and aviaries. These aviaries were at a later period used for the rearing of the peacocks which Alexander had imported from Greece, where they were merely considered as an object of curiosity on account of the beauty of their plumage. Hortensius, however, judged differently, and he ordered several of them to be served up in a splendid banquet, which he gave to his friends. From this time peacocks multiplied prodigiously in Rome; and Ptolemy Phocion was astonished with the number he saw there. This number became latterly so great, that, if we may believe the ancient authors, Antidius Lucero made an income of nearly L. 600 by feeding this beautiful bird.

Nor should we be surprised at this, at all events, if we were to judge of it by the immense quantity of figures of this bird which the ancients have left us. Nor are there fewer of a great many others, among which it will suffice to specify the different species of cranes, storks, herons, parrots, titmice; of eagles, vultures, hawks, owls, and ducks. Many of these birds are represented with extraordinary perfection. In proof of this, we particularize the bas-relief copied in the sixth plate of the work of M. D'Agincourt*, which represents the eagle of Jupiter carrying Ganymede away, and being conscious, as the bas-relief itself expresses it, of what he carried, and for whom (*Sentiens quid rapiat et cui ferat*). In accuracy and truth it is really admirable, and allied to a sublimity which it is easier to feel than to define.

As the number of birds here referred to is very considerable, we will not endeavour at present to describe them, intending to return to the subject at a subsequent time. And what we have said on birds, we might state with equal safety of fishes and reptiles. These, and especially the latter, are usually very accurately represented; and to be known, they only require continued attention, and a sufficient number of objects of compari-

* Recueil de Fragmens de Sculpture en terre cuite. Par S. d'Agincourt. Paris, 1814.

son. It is the same also with certain insects, for it would be a mistake to suppose that the ancients had confined themselves to supply representations of a few beetles (principally the *Ateuchus sacer et impius*), to which they had devoted particular attention on account of their utility. On the contrary, their attention was equally directed to a great variety of kinds, and to nearly all the orders. So is it with the Crustaceæ. The ancient mosaics, as well as the paintings found in Pompeii and Herculaneum, include a great number, as we shall prove at another time.

This review will assuredly be sufficient to prove with what minute attention the ancients studied the various productions of nature, inasmuch as they have left us such faithful representations of them. They appear, however, to have neglected some of them; and among these we may chiefly mention shells and their inhabitants. In fact, with the exception of the fretted *helix* and the *buccinum* (*Triton nodiferum*), which they have often applied to the mouth of their tritons and naiades, and other sea divinities, shells are but rarely represented upon the antiques. The number of varieties is very inconsiderable, especially when compared with the *Articulata*, which appear to have attracted the attention of their artists nearly as much as the vertebral animals of the most complicated organization. Before bringing these observations to a close, we cannot resist the temptation of alluding to the beautiful cameo, engraved in the fourth volume of La Galerie de Florence, in which we observe a triton blowing the shell. The head and breast are those of a man, whilst the feet are those of one of the palmipede birds, and the rest of the body corresponds to that of a fish. Every thing in this cameo indicates the lot of this divinity, who, like certain birds, palmipede and fishy, was to dwell upon the waters of the ocean.

These details demonstrate that the real beings depicted upon antique monuments, are thus as numerous as they are accurate; that is to say, that each maintains its general characters and distinctive traits. If it were necessary to subjoin additional proofs to a fact which no one, we hope, will dispute, our attention might be directed to the crocodiles of the Nile, the first five living specimens of which were exhibited to the Romans by Emilius Scaurus. Let the sculpture of these animals be examined, and it will be found, that, like those which are discovered from time to time in the catacombs of Egypt, they represent, point for point, the crocodiles which still inhabit the Nile, that river of high and ancient renown. Before that beautiful monument, the naturalist and the antiquary must stand astonished, and render homage to the genius of the ancients, which led them to throw as much sublimity as accuracy into their works.

On the Seiches of the Lake of Geneva. *

The term *Seiches* is an appellation which is given in the neighbourhood to certain sudden elevations and depressions of the surface, to which the water of the Lake of Geneva is subject. The phenomenon itself has been long and generally known, whilst no satisfactory explanation, nor even any accurate account of the singular circumstance, is to be found in the works that allude to it. Persuaded that further inquiry was desirable, Professor Vaucher of Geneva undertook the investigation; and in the years 1803 and 1804 read to the Société de Physique of that city the result of his labours. Much general attention to this interesting paper was not excited at the moment; and we are convinced that a complete publication of the document will be received with interest. We shall here then supply as much as our space admits, referring for the more minute details to the memoir itself. (Memoires de la Société de Physique et d'Histoiré Naturelle de Genève, t. iv. part 1.)

The waters of the Lake of Geneva, as well as those of all the other lakes that are formed by the rivers which derive their sources from the higher Alps, are subject to a variation, the extreme limits of which usually correspond with the months of August and February, the times of the greatest heat and greatest cold of the climate.

But independent of this regular and annual increase, the waters of the lake are also exposed to sudden rises and falls, viz. the *Seiches*.

This phenomenon, which is well known to all those who dwell on the borders of the lake towards its western extremity,

* From the Bibliotheque Universelle.

where it is chiefly remarkable, at an early period excited the attention of the naturalists of Geneva. About the commencement of the eighteenth century, Fatio de Duilliers, a mathematician and accurate observer, described it in a memoir which was inserted in the second volume of Spon's History of Geneva, and was entitled *Remarques sur l'Histoire Naturelle des Environs du Lac.* Shortly after, Professor Jallabert alluded to it in the *Mémoires de l'Académie des Sciences.* Finally, M. Serre in the Journal des Savans, Professor Bertrand in an academic dissertation which has not been published, and M. de Saussure in the fifth volume of his *Voyages aux Alps*, have successively alluded to it.

Three of these observers have attempted to furnish an explanation. The first is Fatio de Duilliers, who imagines that strong breezes of wind driving the waters towards the town of Geneva, there more or less alter their level, which they do not at once resume, but only after many oscillations. Jallabert having remarked that the hypothesis of Fatio could not explain those *Seiches* which occurred in calm weather, attributes the phenomenon to sudden rises of the river Arve, retarding the course of the Rhone, and consequently forcing it back on the waters of the lake. Professor Bertrand having refuted both of these suppositions, proposes a third, in which he alleges that the *Seiches* are produced by electrical clouds, which, attracting the waters of the lake, produce pulsations, the effect of which is more apparent where the opposite sides of the lake approach the nearest.

The contrariety of these explanations, which moreover were not based on any series of observations, only excited (says Vaucher) my curiosity. In truth, I thought less of discovering the causes of the phenomenon, than of appreciating the value of the different solutions which had been given of it; whilst, at the same time, it appeared to me almost a disgrace to our city, and more especially to our scientific Society, that so singular a circumstance should occur every day under our eyes, without an endeavour to determine its true cause; and sometimes I regretted that Saussure himself, to whom the explanation almost of right belonged, had never undertaken it.

There is almost an invincible interest in seeking after causes, which often leads us away almost in spite of ourselves; and there was in this singular phenomenon even more than the usual attraction. It might depend on some obscure law of physics or of atmospheric electricity; and the discovery of the cause could not fail to be highly gratifying. I prepared myself then seriously for the work; and was really in that state of mind which philosophers recommend when in the search after truth. I had not as yet formed any hypothesis upon the cause of the *Seiches*, and my only knowledge about them consisted in the descriptions of authors, and in the recollections of what I had seen of them in my early youth.

My first observations bear date of the beginning of November 1802. Prepossessed with the idea that the *Seiches* did not occur but at some short periods of the year, and especially at the time when the waters were most abundant, I went often to the shores of the lake, less for the purpose of observing the phenomenon, than of establishing its absence at this season of the year. But as on every occasion, instead of being stationary, the waters had a marked rise and fall, I saw it was essential to give my observations the necessary accuracy. I shall here remark, before going further, that the movement of elevation and of depression of which we are now speaking can never be confounded with that of the waves. It does not at all exhibit itself by any agitation on the surface of the water; on the contrary, it is a calm and simultaneous movement of the entire liquid mass. It alike takes place when the water is agitated, and when it is calm; but to observe it accurately in the first instance, without any fear of confounding it with the motions of the waves, I directed a portion of the water into a small pool, where its tranquillity could not by possibility be so affected.

I was not satisfied with observing in general the movements of the water, whether rising or falling; I was also solicitous of marking the duration of their movements, and the laws they followed. I therefore provided myself with a French foot-rule, which I placed perpendicularly in the water, several inches above its surface, so that I could perceive each minute, the changes of the level. I thought it equally necessary to note, at the commencement of every observation, the condition and meteorological circumstances of the atmosphere. With regard to the height of the barometer, I followed the tables of the *Bibliotheque Britannique*, which supplies two diurnal observations.

The thermometer and hygrometer were also examined, and their indications marked.

In the memoir, we are presented with the details of ten observations made by Mr Vaucher in the manner he has described above. The first, which was made only at one station, viz. at *Eaux-Vives*, on the left bank of the lake, quite close to the town, went to establish the almost unceasing continuity of the oscillatory movement of the waters of the lake, which never ceased either to ascend or descend. In the second observation, Mr Vaucher established a second station at Pâquis, upon the right bank of the lake, opposite to the former. We shall copy the details of this second observation, that we may exhibit, by a striking example, the progress of the phenomenon.

It was made 30th of December, commencing at 1 hour 10 minutes.

Barometer was 26.10.8. Thermometer +3. Hygrometer 82.

It is to be observed that the two watches employed were in exact correspondence; also, that in the following table the letter r. stands for *rose*; the letter f. stands for *fell*, and s. stands for *stationary*.

At Paquis.		At Eaux-Vives.		At Paquis.		At Eaux-Vives.		At Paquis.		At Eaux-Vives.	
Min.	Lines.	Min.	Lines.	Min.	Lines.	Min.	Lines.	Min.	Lines.	Min.	Lines.
11.	r. 1	The observations at this station did not begin till the 22d minute.		31.	f. 1	...	s. 0	51.	f. 3	...	f. 3
12.	r. 1			32.	r. 2	...	r. 4	52.	f. 5	...	f. 2
13.	r. 2			33.	r. 5	...	r. 4	53.	f. 7	...	f. 4
14.	s. 0			34.	r. 7	...	r. 7	54.	f. 3	...	f. 3
15.	f. 3			35.	r. 8	...	r. 6	55.	f. 3	...	f. 4
16.	f. 4			36.	r. 12	...	r. 7	56.	f. 2	...	f. 4
17.	f. 2			37.	r. 10	...	r. 6	57.	f. 2	...	f. 3
18.	f. 3½			38.	r. 5	...	r. 8	58.	f. 2	...	f. 3
19.	f. 3½			39.	r. 3	...	r. 4	59.	f. 1	...	f. 4
20.	f. 2			40.	r. 2	...	r. 5	60.		...	f. 7
21.	f. 2			41.	r. 2	...	s. 0	1. 2d hour		...	f. 3
22.	f. 2½	...	f. 3	42.	r. 0½	...	r. 1	2.		...	s. 0
23.	f. 1½	...	f. 2½	43	r. 0½	...	r. 3	3.		...	r. 1
24.	f. 1	...	f. 1½	44.	r. 4	...	r. 2	4.		...	s. 0
25.	s. 0	...	s. 0	45.	r. 4	...	r. 4	5.		...	r. 2
26.	f. 1	...	f. 1	46.	r. 1	...	r. 5	6.		...	r. 2
27.	f. 1	..	f. 1	47.	s. 0	...	f. 1	7.		...	s. 0
28.	f. 1	...	f. 1½	48.	f. 0½	...	s. 0	8.		...	r. 2
29.	f. 3	...	f. 1½	49.	f. 4½	...	r. 2	9.		...	r. 1
30.	f. 2	...	f. 2½	50.	f. 3	...	f. 3	10.		...	r. 2

The correspondence (Mr Vaucher remarks) betwixt the observations made in the two experiments struck me as remarkable; for, except in a single instance, that, namely, of the 49th minute, in which the water fell in the one place, whilst it rose in the other, it habitually ascended and descended at the same instant on the opposite banks of the lake. Besides, the space over which it moved in each minute is usually the same, with some trifling exceptions, owing, it might be, to the observer. And, finally, the sum-total of the rise and fall at the two stations is, within a line or two, precisely the same.

It is of importance to remark, that the kind of movement might itself produce some anomalies; for, as may be observed, it was not accomplished in a regular and continuous way, but, on the contrary, was effected rapidly, and as it were by fits and starts. In the middle of a rapid rise, there would all at once be a stop or a slow ascent, and then speedily another rapid rise. Sometimes there was even a momentary fall in the middle of a rise, or a rise in the middle of a fall; and within the one minute the water might both ascend and descend, one or more lines, so that, in this case, it was only possible to mark the difference, and this circumstance is no farther exhibited in the table than by putting the letters *r* or *f* just as the rise or the fall, upon the whole, predominated.

In the 3d, 4th, 6th, 7th, and 8th observations, still maintaining the station of Eaux-Vives, the author removed the other one to points more and more distant from the town, sometimes on the left bank, and sometimes on the right, till he reached Genthod, at the distance of nearly five miles. He observed, that as he removed towards the eastern extremity of the lake, the amount of the seiches was less and less considerable: and other observations which were subsequently made at Céligny, ten miles from the town, confirmed the conclusion.

In the 5th observation, he compared with the station of Eaux-Vives another which was chosen on the Rhone, below the town, at the distance of nearly a mile from Eaux-Vives, following the course of the stream, and he there found the oscillatory movements much less. At the confluence of the Arve and the Rhone, about a quarter of a mile further down, they could not be at all detected.

The 9th and 10th observations were more particularly designed to demonstrate the influence of the atmosphere upon the phenomenon. Unfortunately, however, during the year in which the observations of Mr Vaucher were made, the weather was uncommonly dry and free from storms. And as it is well known that this settled state of the atmosphere is that in which the seiches are the least considerable, the author had not an opportunity of observing any in which the oscillation was greater than a foot.

The exact mode in which this rise and fall takes place merits observation. It is more of a swelling up than any thing else, without any agitation or any thing stormy: where the lake communicates with any neighbouring pool, as for example in the various ditches which surround the fortifications of the town, the waters in these latter neither rise nor fall cotemporaneously with the other, but they flow uniformly from the lake to the ditch, or from the ditch to the lake, according as the waters of the latter are rising or falling.

It may be remarked in passing, that a very erroneous idea would be entertained of the phenomenon, were it conceived to be owing to any transportation of the water from the large lake into the contiguous pools; for, as we have already said, the water being often in a state of perfect repose during the occurrence of the largest *seiche*, it would be impossible, according to this explanation, to assign a reason for the many momentary variations, and especially to conceive how the *seiches* should not accurately correspond, and manifest themselves at the same moment upon the opposite banks; and, finally, how the transport of so much water could be effected without shewing itself in some rapid current, flowing from the greater mass of waters into the smaller, or the reverse.

It was not enough, continues Mr Vaucher, for the accomplishment of the task, to learn only the principal circumstances connected with the seiches at the western extremity of the lake; it was of importance to ascertain also what happened at the other extremity, that we might either confute or confirm the various explanations which had been given of the phenomenon, and finally to arrive at a discovery of the true cause.

I applied then to an inhabitant of Vevey, who had the kindness, whenever his time permitted him, to make those observations which I requested. But he was never able to perceive a sudden variation of the level of the waters of the lake to a greater extent than a few lines. This result surprised me the more, as it was opposed to the opinion of Saussure, who states that the *seiches* are observed at both ends of the lake. Hence I took two journeys to the eastern extremity, to ascertain the truth with my own eyes; and I was never able to perceive variations which extended beyond a line or two, even at the time that corresponding observations in the neighbourhood of Geneva shewed as many inches. The boatmen, and others who lived on the shores, of whom I subsequently made inquiry, and who were quite familiar with the phenomenon, as it is seen at Geneva, have unanimously affirmed that they never observed any thing similar, at either Evian, Villeneuve, or Vevey.

It appears then certain, that the *seiches* are at their maximum in the narrowest part of the lake; that they go on diminishing to the distance of from three to six miles; that thence to the eastern extremity they are not seen to a greater extent than a few lines; that their appearance, which may be at any season of the year, is never more marked than during great atmospherical vicissitudes, and that they almost disappear when the barometer is high, when the north-east wind blows, and when the clouds are uniformly spread over the sky.

When I felt I had thus accomplished all that could be done on the Lake of Geneva, I then thought of examining, with the same object, the other lakes of Switzerland. I began with Zurich. It was sufficiently large, and also, like Geneva, narrow at one of its extremities. I had, moreover, in that Canton, a friend who was as intelligent as obliging, to whom I could apply for assistance. It was only, however, at my urgent solicitation that he would comply with my request; for he had previously interrogated so many intelligent persons in the district, and their answers were so uniform, that he was persuaded there was no such occurrence on Lake Zurich.

It happened, however, that when he was at one time examining, at the beginning of January, the time at which the margin of the lake was frozen, he was astonished to see that the water was alternately rising and falling. At this time he witnessed air-bubbles issuing from, and air rushing underneath, the ice. Being, after this time, satisfied of the existence of the phenomenon, he began to examine it with much care and interest, and the following is the result of his researches:—The movement of the waters, which is denominated *Seiche*, is much less considerable in the Lake of Zurich than in the Lake of Geneva. It often happens that it cannot be perceived at all; but, in the months of February and March of 1803, it was often sensible, though its maximum did not reach much above half an inch. It was more considerable near the town than elsewhere, whilst it could also be observed at a great distance. It was always most considerable in summer, and more at the commencement of a storm than in calm and serene weather. Under the former circumstances, the movement has been observed to the extent of eighteen lines, and the rises and falls succeeded each other rapidly, and in an irregular manner.

As the Lake Annecy is the nearest of all those in the neighbourhood of Geneva, I wished also to examine it, and I visited its banks on the morning of Sunday, May 8. 1804. The weather, which had been changeable and rainy the previous evening, had become beautiful and very mild. The barometer was at 26.10.8.; the thermometer in the shade + 12; the wind was at south-east; the sun was shining at intervals, and there was a perfect calm. This lovely lake, about ten miles by three, is embosomed in lofty mountains, which are not, however, covered with perpetual snows: it is supplied by the waters of a multitude of little torrents which descend from the neighbouring heights; and consequently it has not a period of regular annual increase and decrease, like the greater number of the Swiss lakes. It, however, rises and falls irregularly many times a-year, according as the season is wet or dry. The river which issues from it is but small, and has scarcely any current at its origin. It is called The Fiers, and falls into the Rhone, a little above Scissel.

At the time of my arrival, at half-past seven in the morning, I could perceive no appearance of the seiche upon its banks. I then walked a couple of miles from the town, where I watched for it a whole hour, without any success. At another station I selected, somewhat nearer, I perceived the waters to fall two lines in less than five minutes. Half an hour afterwards, at the end of the lake, it fell three lines, and had again risen four and a half at the time I finished my observations. It was then two o'clock; the sun was very hot. Two hours afterwards it began to rain, and continued to do so till I reached Geneva.

I had previously taken the trouble accurately to inquire of the inhabitants if they were acquainted with the phenomenon of the *seiche*, which I had used all pains to explain to them. They unanimously answered, that the surface-level of the lake never varied suddenly, but that the waters rose and fell on account of rain or drought. It is notwithstanding certain, according to the observations just alluded to, that this lake exhibits them, like the others that have been noticed. They are undoubtedly less than those of Geneva, but they also resemble them in that they take place in the same intervals of time, and that they appear more conspicuous towards the exit of the river than farther up.

The last lake which, in the year 1803, was examined at my request, was that of Constance. This lake was of great importance in the question, both on account of its size, and of its narrowing, which gave it a resemblance to Geneva; and I had heard, in a casual way, that the *seiches* had been observed upon it. I requested Mr Roux-Bordier, a distinguished amateur in natural history and physics, who was at the time making a journey in the eastern parts of Switzerland, to have the kindness to bestow some attention on the subject. This he promised; and, on his return, informed me that there were in truth *seiches* on the Lake of Constance, and that the Messrs Macaires, respectable merchants in the town of Constance, and on the banks of the lake, would supply me with all the information I could desire. I wrote then, to these gentlemen, and received an ample and precise answer.

They stated that there were *seiches* in the Lake of Constance, but that they were more moderate and less frequent than those of the Lake of Geneva. They believed that they could only be seen close to the town, and at a small distance from it, and that the rise never exceeded from 4 to 8 inches. They thought, too, that their continuance was for a longer period than at Geneva; but, on this point, they state that they have not made any accurate observation. They also state, that there is a current in the Lake Constance, which begins to be perceptible at about two-thirds of a mile from the town. I have consulted other people who lived in Constance, and who all remember having seen *seiches* there. They could not accurately assign their maximum, but they must have been to the extent of several inches;

to be remarked at all. They also say that the lake has a fall, or marked declivity near the town, and that this declivity has not hitherto been measured. Mr Escher, the same friend who had the kindness to make the observations on the Lake of Zurich, also undertook a journey to Stein, where the Rhine, after being for a time broad, begins to grow narrow, and properly to form a river; but there he could discover no *seiche*, nor could he learn that it had ever been observed. He only remarked that the current near to this town was so strong that it was with difficulty a powerful man could maintain himself erect, and not be carried down the stream.

In the year 1803 I also procured some friends to make observations on the Lake of Neuchâtel. The individuals to whom I applied had the prejudices to which I have already alluded; they imagined that the seiches were peculiar to the Lake of Geneva; but after having examined attentively, and under the circumstances which I recommended, they also found variations quite appreciable in the middle of the lake, and more considerable as they approached the spot where the waters discharge themselves by the Theilla into the Lake of Bienne.

Finally, with the same object in view, in July 1804, I took a journey to Italian Switzerland. I had especial hopes that the lake, whence issues the Tessin, and which is nearly of the same shape as that of Geneva, would present the same phenomena, viz. the seiches, and more considerable in proportion as I approximated towards its southern extremity. Accordingly, I clearly perceived at Baveno, near to the islets Borromée, a variation of two or three lines; but when I had descended as far as Sesto, the waters were so strongly agitated, that I could not make any observation; and the persons of whom I inquired, whether owing to the absence of the occurrence, or rather to their not understanding me (for they did not know French better than I spoke the Italian), seemed to have no knowledge of the phenomenon. At a later period, however, I renewed my endeavours at Sesto, at the spot where the Tessin leaves the lake, and I then noticed very marked variations. I may, in general terms, make the same remarks concerning the lake of Lucerne, near to the Taun, as satisfactory testimony has been supplied on the point, by intelligent travellers.

On the Lake of Como also, I had the misfortune of being disappointed. The lake was tempestuous, and, accordingly, I could not make any investigation. It was otherwise at Lugano, where I had a favourable opportunity of observation in August 1804. The weather was hot, and a storm threatened, and I saw four or five times, during the interval of two hours, the waters rise several lines, and at one time in particular, at least five lines. From all this I thought myself authorised to conclude, that the phenomenon of the seiches takes place upon all lakes whatever; and if it be more remarkable on the Lake of Geneva than on others, that this was owing to the cause which produced it, and which exists every where, though not exercising all its power except on the surface of the Lake of Geneva.

Here terminates the recital of the principal observations which were made by Mr Vaucher, during the interval of two years, upon the seiches of the Lake of Geneva, chiefly at its western extremity, and also of those occurring on the lakes of Zurich, Annecy, Constance, &c. &c. In a second part of his memoir, the author proceeds to the conclusions which may be deduced from his observations, and to the investigation of the cause of the phenomenon.

He remarks, From all the facts which I have adduced, and from others of which I have said nothing, I think I am entitled to conclude in a general way,

1*st*, That there are seiches more or less considerable upon all lakes; and regarding those on which they have not hitherto been observed, I should conceive that this was owing to their not having been sufficiently examined.

2*d*, That these seiches may take place at all seasons of the year, and indifferently at all hours of the day, but that they are in general most frequent in spring and autumn.

3*d*, That nothing appeared to exert a greater influence upon the phenomenon than the state of the atmosphere, so that the steadier the weather the less remarkable the seiches, and the more unsettled the weather the more striking the seiches. All my observations, moreover, proved, that the movement was feeble, if it at all existed, when the wind was from the north, or when the weather was very steadily excellent, or when there was an universal mist, or when rain or snow were falling widely; whilst, on the contrary, the seiche was always considerable when the atmosphere was studded with formidable clouds, or when the weather, otherwise serene, just preceded a storm, and when the barometer was falling.

4th, That, all other things being equal, the seiches are more considerable in the Lake of Geneva in proportion as we approach its *debouchement* into the Rhone; that they are most of all considerable at this part, and from this spot decrease to the confluence of the Rhone with the Arve, where they nearly wholly disappear. That the same is true in proceeding towards the other extremity of the lake, as far as Coppet, where they occur to the extent of only an inch or two, and as far even as Rolle, where they are still less, but yet nowhere they do entirely disappear.

5th, That the eastern extremity of the Lake of Geneva does not exhibit seiches more remarkable than other lakes, though the opposite opinion seems established in the works of Saussure.

6th, That although the seiches are more *frequent* in spring and autumn than at other seasons, yet they are to a *greater extent* in summer, and especially towards the end of that season. The largest that have been ever observed have always occurred in the months of July and August, or at the beginning of September.

7th, That the minimum of seiches has no limit, but that their maximum never exceeds five feet. Amongst the most considerable are the four particularized by Fatio de Duilliers in his *Mémoire sur l'Histoire Naturelle de Genéve*, inserted at the end of Spon's History of Geneva, and those which Messrs Serre and De Saussure observed together on the 3d of August 1763.

8th, Finally, that the duration of the seiche is very variable, its higher limit seldom exceeding twenty or twenty-five minutes, usually much less, whilst its lower is nothing.

And now to explain these different particular circumstances of the phenomenon :—

1st, There must be a cause which is capable of disturbing the level of the surface of lakes which may act at all times, and with different degrees of intensity, whose influence increases with atmospherical variations, and especially at the approach of storms.

2d, This cause, which appears to be always acting, must be rendered more powerful by local circumstances, which exist in a remarkable degree on the Lake of Geneva, and in such a way as to produce its maximum of effect at the neighbourhood of the town, or the exit of the Rhone; and that from that spot it continually diminishes, on the one side to the confluence of the Arve and the Rhone, and on the other to the widest expanse of the lake, where it does not extend beyond a few lines.

It is concerning the nature of this cause, as we believe, that those naturalists have deceived themselves, who have attempted to give explanation of the phenomenon; and who, with a paucity of observations, or rather without any accurate observations at all, have assigned causes for the seiches which could never have at all produced them. I do not here allude to the hypothesis of Jallabert, who attributed them to the melting of the snows, nor to that of Fatio, who imagined that a breeze of wind, acting obliquely or vertically upon the surface of the waters at some distance on the lake, thus retarded their flow; but I allude more particularly to the opinion of M. Bertrand, who thought that the seiches were produced by certain electrical clouds, attracting and repelling the waters, and alternately producing those risings and fallings which constitute the phenomenon. But independent of the fact, that it is difficult to understand how the power of an electrical cloud could attract or repel a mass of water, it must be conceded, that if this were the true explanation, there would never be seiches when the sky was not charged with clouds, nor would there be any in winter or autumn; nor would they be so peculiar as they are on the lake of Geneva. Besides, the water which was thus attracted and repelled by the clouds, instead of remaining, as it often does, in a state of quiescence on the surface of the lake, would, on the contrary, be agitated and repelled with violence: suppositions, these, which are wholly at variance with the observations which we have detailed. I do not deny that, in certain circumstances, electricity may produce some effects somewhat similar; that electrical water-spouts, for example, which are observed on the sea, and sometimes also on the lake of Geneva, neither agitate its surface, nor elevate and abase its waters; but the method in which these water-spouts act, so far at least as it is known, has no resemblance to the gentle and periodic movement of the seiches; and it assuredly would not be correct reasoning to deny the action of electricity, in explanation of the seiches which occur during serene weather, and then to admit this same cause in explanation of those exhibitions of them which are observed when clouds are scattered over the skies.

In reflecting upon the different agents which might produce the seiches, according as we have described them, I have been able to discover only one which explains their different appearances in a satisfactory manner, and this is the agency of the atmosphere. Theory and observation alike agree in teaching us, that many causes with which we are acquainted, and others of which we are ignorant, contribute to change, almost continually, the weight of the different columns which compose it. Let us only suppose that clouds are unequally spread over the skies, and that some of these intercept the solar rays from the lake, it will result from this simple supposition, that there will be irregular refrigeration among the different columns, and consequently an inequality of density, so that they will press unequally upon the surface of the lake; but the liquid thus unequally pressed, and ever having a tendency to maintain its equilibrium, will fall in one place, and rise in another; there will thus be alternating risings and fallings, which in all accuracy being effected on the water of the lake, independent of all agitation of the air, can thus be scarcely ever on an exact level. And if, instead of supposing a simple refrigeration occasioned by the interception of the solar rays, we suppose such a state of the atmosphere that it rains at one place whilst it is fair at another; and if, moreover, we recognise the sudden and local variations which so often happen in the air at the approach of storms, and which are so violent that they may produce hail in the higher regions of the atmosphere, we may easily conceive how the waters of the lake should be so unequally pressed upon, as to rise and fall.

These suppositions are far from being gratuitous. Saussure, in his *Hygrométrie*, and principally in the chapter entitled *Des Variations du Baromètre*, after having discussed the causes of general variations, then proceeds to more particular ones; and he mentions, that whenever a partial shower refreshes the air, it is found that, on the spot where it fell, the barometer immediately rises half a line or even a line, without the existence of any general cause to produce this rise; and according to his calculation (Essai IV. chap. iii. p. 476), he finds that the simple refrigeration of three degrees, in the whole extent of a column of air, suffices to explain a variation of 0.85 of a line in the barometer.

But independent of these greater local variations, which occur but seldom, there must exist lesser ones every moment, so to speak, in the various regions of the atmosphere. And, in truth, it is but seldom that we can suppose that the different columns of which it is composed can have exactly the same temperature; and whenever there is this variation, it is likewise impossible to suppose that the liquid upon which they rest, shall be accurately on a level.

Observation comes in here in support of reasoning; for Mr Senebier, to whom we are indebted for so many valuable works, and who has bestowed a great deal of attention upon every thing which affects the variations of the thermometer, has often assured me, that when he observed this instrument, at ten minute intervals, it seldom happened that he found the mercury at exactly the same height, and it almost continually was varying the sixteenth of a line and more. He had the kindness to observe this instrument at the time that I was marking the variations on the lake. I may also mention, that whensoever, in a variable atmosphere, I attentively observed a very admirable barometer which Mr Jurine had the kindness to lend me, I have scarcely ever failed to discover variations as considerable as those specified above.

With the intention of carrying conviction on this point as far as possible, I frequently transported this barometer to the shores of the lake, to certify that its variations corresponded with those on the surface of the water; but a little reflection soon convinced me that this experiment was not at all calculated to supply the result I wished to obtain. In truth, when the water was rising and falling at one spot, it was impossible to ascertain whether the change was effected in virtue of the variation of that column of the atmosphere which was perpendicular to it, or of some of the neighbouring strata. Besides, there was nothing to prevent the column which supported the barometer undergoing a change, which change would not manifest itself on the surface of the water which supported it; and finally, the change might even in certain cases be the very contrary to that which the variation of the mercury in the same place, and at the same moment, might require. In truth, no one can pronounce any thing upon the possible movements of the water in the spot where the observations are making, without at the same time knowing the condition of the neighbouring columns. And even were we to suppose that many individuals were observing in different places at the same moment, still their number could never be so multiplied as to obtain in this particular precise and accurate information. We ought, then, to be satisfied with the knowledge, that the weight of various columns may vary, and frequently in reality do vary, and that it is impossible for them so to vary, without disturbing the level of the water; so that, could we even account for the occurrence of the seiche from some other cause than the atmospherical variations, we should still have to explain how the different columns of air could vary unequally in weight, whilst the water on which they reposed did not itself vary in height.

However, it at one time did happen that I was more fortunate in this particular than could have been expected. During the tempestuous weather which brought the month of November 1803 to its close, and during which the barometer and the temperature varied nearly equally alike, Sunday the 13th was of whole time the most remarkable. At three o'clock in the afternoon of that day there had been lightning, to which succeeded, in about half an hour, rain, accompanied with tremendous wind. I happened to be at Eaux-Vives at the moment, and I observed one of the most remarkable seiches which I have ever seen; the water gradually lowered itself about a foot, and it was still descending when I left the spot. This happened exactly at the same time that certain of my friends had been observing the barometer, and who had previously observed it to vary considerably during the course of the day: at this precise time they saw it descend suddenly almost a line, and continue its oscillations for a considerable time afterwards.

The local and instantaneous variations of the barometer being, under ordinary circumstances, to the extent of about $\frac{1}{16}$ of a line, and reaching to a line, or even more, at the approach of a storm; in explaining by their means the phenomena of seiches, we must consider the waters of lakes as forming a syphon with an infinite number of branches, some one of which communicates with all the others. We must also suppose that this central branch, communicating with all the others, shall be for the moment charged with a column of air, the weight of which varies. If, then, this column experiences an augmentation of weight or of tension, which corresponds to a line of mercury, and consequently to fourteen lines of water, the subjacent water will have a tendency to fall fourteen lines; and this quantity which it will fall in the central branch of the syphon, will be the measure of the extent it will rise in the other branches which have not been subjected to any change of pressure, that so the equilibrium may be maintained.

If, at the same time that there is an augmentation of pressure on one column, there is also a diminution of pressure on other columns, so that the mercury falls in one place, whilst it rises in another, it is clear that in all these cases, and in others of the same kind which may easily be imagined, the effects would be compounded and increased; whilst at the same time they would be diminished or even destroyed when the neighbouring columns are also augmented in weight, a circumstance which occurs whenever, in the middle of a regular ascension, the water becomes stationary or even descends.

These barometrical variations are sufficient to account for the differences which are observed in the level of the most part of lakes. True it is, it is usually imagined that these lakes do not present the appearance; and this arises from the circumstance, that in fact they are not very apparent. Thus it has always been supposed, that the lakes of Zurich, Annecy, and Neufchatel, and many others, never exhibited them; but I have shewn, that, with attentive observation, there might be detected differences of the level of the surface, which, in ordinary circumstances, extended to two or three lines, and which, at the approach of storms, might extend beyond an inch.

Upon the whole, then, it is difficult not to agree with M. Vaucher; and to perceive, in the ordinary and continued variations of the atmospherical pressure, the cause of the oscillations, not less continual, which affect, in different degrees, the waters of many of the Swiss lakes, and more remarkably that of Geneva. But it still remains to explain how this cause should produce at the western extremity of the lake just named, effects which are so much greater in their extent than those which are observed in the others; and it is of this fact, that M. Vaucher endeavours to give an explanation in the last part of his memoirs.

M. Vaucher remarks, that if we cast our eyes upon the map of Switzerland, we shall find that, with the exception of the Lake of Constance, the Lake of Geneva is that in which the narrowing is most considerable at its extremity. Its width at Thun is nearly eight times as much as it is at Eaux-Vives, and it insensibly gets narrower all the way from Nyon to Geneva. Independent of this narrowing, it presents another phenomenon which, according to the author, does not occur in any of the others, at least to the same extent; it is, that those waters at the place where they begin to acquire the movement by which they escape, have already a sensible declivity, and this at about the distance of three miles: According to some observations which are contained in the memoir already quoted of M. Fatio de Duillier, and after a new measurement of the levels executed by himself, M. Vaucher concludes, that the declivity of the lake from Genthod to Geneva, being three miles of distance, is when the waters are low about a foot; that it is more than double of this in the month of June; and that it attains its maximum in the month of August, when it may extend to about four feet, when at its usual high level, or at all events, on extraordinary occasions.

Moreover, continues M. Vaucher, so great a declivity is not to be found in any other lake with which I am acquainted; that of Annecy has not even a sensible current where the river runs out from it; that of Zurich is under the same circumstances, and that of Geneva itself, at its other extremity, has scarcely any declivity; for the Rhone, at its entrance, does not, on its current, carry the boats farther than three hundred yards. The only lake which seems to be an exception is that of Constance, concerning which M. Macuaire informed me that the declivity is apparent, and that it commenced about two-thirds of a mile above the town of the same name. But this fall, which has never, I believe, been measured, and which I have witnessed when the waters were at their height, is far beneath what occurs in the Lake of Geneva. Another circumstance which is peculiar to this lake is, that at both the right and left sides of the current which issues from it, there are very considerable expanses of water, which have absolutely no movement whatever. Such, on the side of Paguis, is the ditch which goes to form the Fosse-Vert, and those which extend to the adjacent fortifications; and on the side of Eaux-Vives particularly, such is the whole of the margin of the shore to a considerable distance from the town; such is the Harbour of La Furterie, and the Wooden Harbour, and such the extensive ditches of the fortifications which surround the town on that side. I do not believe that any of the lakes of Switzerland, or of the higher or lower parts of Italy, presents, at the issuing of its waters, any thing of a similar configuration.

And, finally, to come to an explanation of the phenomenon as it occurs at the extremity of the lake, I suppose that, in the circumstances of the atmosphere, which I have already pointed out when rain threatens, or considerable barometrical changes rapidly succeed each other, the waters of the lake, at some distance from the town, are depressed by a column of air which has become heavier than those around it: at the same time, this pressure raises the level of the neighbouring waters, which it may raise, we shall say, to two inches. But if these waters, instead of being at the horizontal level, on the contrary, consist of those which form the rapid fall of the stream, they will then be subjected to two forces, viz. that of the fall, which is carrying them along, and that of the atmospheric column which impels them from their level. Under these circumstances, they will follow the diagonal of the two forces, and in a direction which it is not difficult to determine; and they will thus be more or less elevated in a considerable extent of the current. But they cannot be thus elevated, without the current being at the same time diminished, and the waters retarded in their course, and of course augmented in quantity. And as the waters of the current are in equilibrium with the stiller waters that surround them, they will swell them up, not truly by running over them, but by forming on all sides interior currents, such as M. Jurine formerly mentioned he had seen at the Fosse-Vert, and such as every one may observe at all times under the same circumstances. These flows will continue so long as the pressure shall subsist with all its weight; they will go so far as to elevate the level of these still waters to the extent of two feet and a half in the combination of circumstances that is most favourable for their operation; and then the pressure diminishing more or less rapidly, the swells moving along in some other direction, it will speedily happen, that the waters may fall as far as they before rose, if, at the same time, the amospheric columns diminish in weight as much as previously they had increased.

This explanation accounts for all the appearances which the seiches exhibit. Here we perceive why they are at their maximum at the place where the current is the strongest, and are provided with reservoirs to receive their superabundant waters;—that they diminish in intensity in proportion as the declivity is less considerable, and that at Genthod, where there is scarcely any fall, the *seiche* also is scarcely sensible. It may, moreover, be remarked, that those of the ditches that are fed by the waters of the Rhone, below the bridges, such as *Porte-Neuve*, partake of the phenomena of the seiche in the same way as the others.

From this explanation we also learn, how the seiches are much more considerable in the Lake of Geneva than they are in others which have not the same configuration at their lower extremity; and why in these different lakes, they are more remarkable in proportion as the waters have a swifter current at their exit, and have in their neighbourhood a greater number of reservoirs which can receive their superabundance.

When the atmospheric columns press unequally upon rivers, and running waters of a considerable expanse, they do not produce seiches, at least any that can be perceived; but they, at the same time, retard or accelerate their course, according as the waters are more or less pressed upon; and they also produce, without doubt, those refluxes, or counter currents, which cannot often, as I believe, be explained on any other principle. And whenever the waters of a river, without any other probable cause, are elevated or fall, we may without hesitation conclude that they are subjected to unequal pressures from different atmospheric strata.

Here, then, I finish the explanation of the singular phenomenon of the seiches. Subsequent observations will either destroy, confirm, or perhaps modify, the causes which I have assigned for them; but the more I reflect upon them, the more do I find it difficult to conceive any others which can furnish so satisfactory a solution of the different appearances which they present. I should certainly have wished to have examined with greater care the several lakes of higher Italy, as also Lucerne and Constance, which so much resemble Geneva; and I would venture to solicit travellers, who may have favourable opportunities, to examine the subject, and so to correct and to confirm the solution which I have ventured to propose.

Observations on the Origin of Mouldiness. By M. Dutrochet, Member of the Institute.

Water, which holds organic substances in solution, very frequently developes living beings, viz. *the infusores*, which belong sometimes to the animal and sometimes to the vegetable kingdoms. These substances, which have been regarded by certain naturalists as the product of spontaneous generation, ought to be considered, with greater propriety, as owing their appearance to the development of certain invisible germs which are scattered throughout nature with profusion, and which only require favouring conditions to assume their being, and to develope themselves. We may place among the vegetable infusores that kind of white *byssus* which is composed of minute branching threads, sometimes articulated, and sometimes not, which frequently exhibits itself in water, holding various organic substances in solution. It is to this vegetable production that the observations made by M. Amici refer, and which are expounded in his memoir entitled *Observations sur l'Accroissement des Végétaux*.* M. Amici having observed, in those little wounds by which the vine, in spring-time, pours out an abundant sap, a kind of yellowish byssus, examined this substance with

* Annales des Sciences Naturelles, l. xxi, p. 92.

the microscope, and found it composed of branching threads and articulations. He considered it as a kind of *conferva.* Anxious to discover what might be the origin of this vegetable production, he observed that it appeared in the sap of the vine when collected in vessels, and that in them it developed itself with rapidity. He was thus led to consider this vegetable production as owing its origin to a tendency which the sap of the vine had to become organized, consequently as being the result of a spontaneous generation. Starting with this supposition, M. Amici is led to allow that it is by means of this tendency to spontaneous organization, that in general the sap produces wood, the increase of which it is continually effecting. Passing by this hypothesis, I wish to discover the class, and the conditions in which this kind of vegetable filament exhibits itself, and of which M. Amici only discovered a single variety. For the most part they present themselves under the appearance of a white or rather transparent kind of felt, composed of a number of branching filaments of the minutest delicacy; they never exhibit the green colour peculiar to the confervæ and to the vaucheriæ. Besides, these vegetable filaments which we are now considering, do not require the influence of the light that they may live and thrive; they grow as well in the dark as in the light. They are seen to grow in water containing certain organic matters. I have seen, as M. Amici did, their development in the sap of the vine, also in gum water; but they especially appear in abundance in water in which a little isinglass is dissolved. In water, which holds in solution a small quantity of the gelatine of strong glue, they do not appear so frequently; and when the water holds in solution a little albumen from the egg, they do not appear at all. I am quite satisfied of this last fact from the many experiments which I have made. And this fact will presently be of use when investigating the conditions under the dominion of which these infusores vegetables appear. We must first, however, determine their nature.

The vegetable filaments which we are here considering, exhibit themselves, as I have just said, under the appearance of a kind of felt, composed of branching threads. It is especially at the bottom of the vessel, which contains the liquid in which they appear, that they accumulate; whilst we very frequently observe them also appear adhering to the sides of the glasses containing the liquid in which they grow. Soon after their first appearance, we observe their filaments radiating as from a common centre; at a later period, their ramifications cross and intersect each other, and in every sense form a sort of felt. When the liquid in which these infusores vegetables grow has but little depth, and when consequently they rapidly attain to the surface of the liquid, we perceive they are soon covered in the free air, with a kind of white effervescence, which, examined in the microscope, is found to be entirely composed of mould of the minutest dimensions, but composed of different varieties.

It was important to know if these moulds were parasite vegetables, accidentally implanted upon the filamentous infusores vegetables which filled the water, and occupied its surface; or whether these same moulds were the production, in the air, of these aquatic vegetables. To satisfy myself on this point, I put small portions of these aquatic vegetables into small menisca, that is to say, small glasses of the same shape as watch glasses, not above four or six lines in diameter, and very flat. Seizing one of the small menisca with common pincers, I plunged it into the water which contained in suspension the small portions of the filamentous vegetables above alluded to, and by this means I got hold of them without in any degree injuring them; they remained in the meniscus with the very small quantity of water which it could contain. I then placed this meniscus under a small glass bell, shut close by water, above which the meniscus was elevated, by being placed on a small support. The filamentous vegetable thus placed on the surface of the water, and in a very humid atmosphere, was constantly covered with mould, at the end of three or four days; and it thus became easy for me to transport them under the microscope without injuring them.

In this manner I have completely satisfied myself that mouldiness is the growth in air of the aquatic filamentous vegetables, which we are now considering. I have seen, in the most distinct way, the aerial filaments of the mould springing from the stalks of the aquatic filamentous vegetable; sometimes by a production from the side, and sometimes by the sprouting out of the extremity of one of these aquatic filaments, which, in coming into the air, become, by that alone, a filament of mould, and then assumed an opacity which it had not so long as it continued an aquatic filament. It is thus demonstrated that the aquatic filamentous vegetables, now under review, are the *thallus* growth-stalks of mouldiness. These *thalluses*, when they are entirely under the water, grow indefinitely in this state. Their development is commonly radiated towards the commencement, but frequently it advances in a way that is wholly irregular, so that it truly produces a kind of felt, by the crossings of the filaments. These filaments are sometimes provided with articulations, but more frequently they have none.

The moulds which I have seen produced from the aquatic thallus now under consideration, have all appeared to me to belong to the genera described by Persoon under the names of *Monilia* and of *Botrytis.* I have observed that all the thallus, whose filaments have joints like the Confervæ, give origin to the monilia, whose aerial filaments are also furnished with articulations. It is, without doubt, to a thallus of this kind that the observation of M. Amici refers, concerning the alleged conferva which he saw grow in the sap of the vine. All the monilia, however, are not thalluses with jointed filaments; when the filaments of these monilian thalluses are destitute of joints, the aerial filaments of these microscopic vegetables are equally destitute of them. As to the filaments of the Botrytis thallus, they are never articulated.

But one important question still remains for solution; viz. What are the qualities which a liquid must possess, ere it will develope the thallus of mould? * I have previously remarked, that water holding a small quantity of albumen in solution never produces these thalluses. From this fact I started, to discover what chemical qualities it was necessary to give to this same liquid, to make it produce the thallus of mouldiness. In these experiments I used only distilled water, that I might be the more certain of the results. I dissolved a drop of the most liquid portion of the albumen of a new laid egg in an ounce of

* The term *mould* is here employed in the sense given to it by Bulliard, that is to say, in the usual sense. Persoon has divided the genus mould (*Mucor*) of Bulliard into many genera, retaining the name mould to one of them alone.

distilled water, and put it into a flask. This liquid was preserved for a whole year, both exposed to the light and put into the dark, but it never exhibited the least trace of the thallus of mould. It did not even develope a single atom of *greenish matter.* Thus, it has been indisputably demonstrated, that this albuminous liquid is wholly unfit for the production or the nutrition of the vegetable infusores. I then took six flasks, into each of which I put an ounce of water, mixed with albumen as above, and to each of them I added one drop of an acid. The acids employed were the sulphuric, nitric, muriatic, the phosphoric, acetic, and oxalic. In less than eight days, the thallus of mould appeared in the whole six flasks. These thalluses were simultaneously produced at the bottom and sides of the vessels, and were observed to develope themselves in concentric rays. I then took some of each of these thalluses, and I proceeded in the way described above, so that I might produce their aerial moulds, and they all, without exception, produced *monilia* of different kinds.

Into separate flasks, containing the same albuminous water, I put severally equal quantities of caustic potash, and soda, in the proportion of 0.005 to the weight of the water. In both the thallus of mould appeared, but it was not till about the end of three weeks. In the aerial growths of these thalluses I could only discover *Botrytis* of various kinds.

It would result from these experiments, that the acids exclusively favour the production of the monilia, whilst the alkalies conduce only to the production of the botrytis. But these results are not constant. They are altered by the employment of other organic substances than albumen. Thus, if to distilled water there be added a small quantity of aqua potassa, in which a little of the fibrin of the blood is dissolved, this liquid gives origin to thalluses, which produce *monilia.* I have also seen, that when phosphoric acid is added to the distilled water of lettuce, the liquid gives origin to the thalluses of *botrytis.* In this last experiment, there was in the water no other organic substance but that which had passed over with it in the distillation. I have observed that this distilled water of lettuce, pure and abundant in itself, deposits in the bottom of the vessels in which it is contained a white substance, which is entirely composed of microscopic globules, and which appear to me to be a vegetable infusorus. But this water never produces the thallus of mould, and this, because it contains neither acid nor alkali, conditions which are indispensable, as it would appear, to the production of these thalluses. But this distilled water of lettuce produces these thalluses when a little of any acid is added. And when the distilled waters of plants contain an acid which passes over with them during the distillation, they never fail to produce and deposit at the bottom of the vessels which contain them thalluses of mould. It is thus that I have observed them in the distilled laurel (*Prunus Laurocerasus*) water, which contains, as is known, hydrocyanic acid.

These solutions of organic substances, which produce the thalluses of mould without any addition of acid or of alkali, assuredly owe this power to their naturally containing a free acid or alkali, as also to this, that they have become acescent. This last alternative is probably the case with the watery solution of isinglass, which produces in such abundance the thalluses of the monilia. I have, moreover, found that this solution, in which these thalluses were developed, did not turn the vegetable blues to red. But this is no sufficient proof that they did not contain a free acid in a trifling quantity, sufficient, however, to determine the appearance of the thallus. I have, in fact, seen these thalluses produced in albuminous water, to which I had myself added a quantity of nitric acid, but so small that it did not redden the vegetable blues.

The subcarbonate of potash, which exists in almost all vegetable products, is alkaline, and probably contributes to determine the development of the thallus of mould in certain solutions of vegetable substances. This alkaline salt being added to albuminous water, it then readily produces this thallus. I have proved that the bi-carbonate of potash produces the same effect; but it is to be remarked that this salt is scarcely ever neutral, the alkali invariably slightly predominating. It may be asked, how it happens that the albumen of the egg, which contains a small quantity of soda, does not, in virtue of this ingredient, provoke the production of the thallus of mould in the water to which it may be added? To this it may be answered, that the soda in the albumen is not in a free state; but that, according to the opinion of M. Dumas, it forms with the albumen a kind of compounded neutral—an albuminate of soda. I here repeat, that the free condition of an acid or an alkali in solution in the water, containing an organic substance, is absolutely necessary for the determining of the growth of the thallus of mould. The quantity of these chemical agents necessary for the production of the effect, cannot be determined as it respects its minimum, for this, in reality, appears to be inappreciable; it can, however, be determined as to its maximum. It is well known that no living being can exist in a liquid which is too acid or too alkaline. I have found that the thalluses of mould are produced in albuminous water, to each half ounce of which a drop of the concentrated sulphuric, nitric, and muriatic acids, is added. This is nearly the maximum of the acidity which allows the production and the growth of the thallus. As to the maximum when the alkalies are concerned, it appears to me it has been reached when the liquid contains an hundredth part by weight of caustic soda, or potash.

When a neutral salt is added to the albuminous water, it does not promote the appearance of the thallus. I make this statement as the result of a great number of experiments.

When I made my first experiments upon the thalluses of mould, I was ignorant of their nature; and witnessing these filamentous infusores vegetables appearing constantly in the albuminous water, rendered slightly acid or alkaline, and never appearing in the pure albuminous water, I was tempted to think that this living vegetable being was the product of spontaneous generation, as M. Amici had done, as before stated. It appeared to me probable that the invisible *germs* of the filamentous vegetable were created by a chemical action of the acid or alkali upon the organic matter dissolved in the water, and that they then developed themselves in virtue of the *vital action* which would have been the necessary attribute of that compound *chemico-organic moléculaire*, or of that *germ*. Such were the ideas that led me astray, previous to my having discovered that these filamentous infusores vegetables were the thallus of mould. Before this discovery, all that had the appearance of the marvellous disappeared, viz. the appearance, in certain liquids, of hese infusores vegetables, which, as it would seem, I could produce at pleasure. The various moulds have seeds, whose diminutiveness is excessive, and which, scattered every where in the atmosphere, and perhaps even contained in animal and vegetable liquids, develope themselves under the forms of filamentous thallus, when they are placed in circumstances necessary for their development. The presence then, of an acid or of an alkali in an aqueous liquid containing some organic matter, is thus nothing more but a condition of the development of the thallus of mould. Experiment has proved the accuracy of this theory. I have taken a small portion of the thallus of mould, produced in an aqueous solution of isinglass, and I have transplanted it into pure albuminous water, where it ceased to grow. I have also put into pure albuminous water small portions of the thallus of mould, which were taken from the albuminous water somewhat acid or somewhat alkaline; and they continued in it without any increase. These experiments have proved to my conviction, that pure albuminous water is wholly improper for the development of the thallus of mould; and that it is on this account that it never appears in this liquid when left to itself. This is also true of the albuminous water united with the neutral salts.

Mercury, whether in the state of a salt, or of an oxide, completely obstructs the appearance of the thallus of mould in liquids where it is found. Thus, for example, the solution of isinglass, which so abundantly produces it, will produce it no longer, if to the solution we add the smallest quantity of red precipitate or of corrosive sublimate. This property of mercury is very remarkable, and gives rise to some useful applications in the arts.

Mercury in its metallic state, added to the water which holds a little isinglass in solution, does not hinder the prompt production of the thallus of mould; the same is also true of the (æthiops mineral) oxide of the black sulphurate of mercury. The proto-sulphate of mercury (turbith mineral), completely hinders the appearance of the thallus, as, in fact, do all the salts of mercury.

Observing how efficacious the oxide of mercury was in preventing the production of the thallus, even when used in a dose of the most minute quantity, I wished to try if there were not other metallic oxides which were capable of producing the same effects. I added, then, various oxides to a watery solution of isinglass. I subjoin the results of these experiments. The oxides of lead and tin appeared to quicken the development of the thallus; it appeared so soon as the second day. The oxides of iron, antimony, and zinc, did not appear to me to exercise any influence upon the development of these thalluses, which appeared, as they usually do, at the end of four or five days. The oxides of copper, nickel, and cobalt, considerably retarded their appearance, for they did not show themselves until the twelfth or fifteenth day. Thus the oxide of mercury, appears to be the only one which prevents the appearance of mouldiness.*

On the Change of Colour in the Chameleon. By H. Milne Edwards, Esq.

The little reptile which is known under the name of the Chameleon, has been long celebrated for the sudden changes which the colour of its skin undergoes. Hence it has become the popular emblem of man's versatility; and were we to believe ancient writers, it possesses the marvellous power of successively assuming the colour of every object with which it comes in contact. It is long, however, since naturalists have stript the history of this little animal of all the fables with which it was adorned; but it is at the same time true, that, in denying it the property of changing its colour almost without limit, it has decidedly been established, that it undergoes truly remarkable variations; and is sometimes nearly white, sometimes yellowish, and sometimes again almost black, according as it is asleep, or in its ordinary waking condition, or is excited by anger.

This singular phenomenon was, without doubt, abundantly calculated to excite curiosity; and zoologists, of course, endeavoured to discover the cause of it. On examination, we find that they have furnished a variety of hypotheses more or less plausible; but they have not, so far as I know, grounded their opinions upon physiological experiments, or anatomical

* Annales des Sciences Naturelles. Janvier 1834.

researches, and consequently we nowhere find the guarantee of strict accuracy.

According to Hasselquist, these changes of colour depend on a kind of disease; more expressly, a kind of jaundice to which the animal is unfortunately subject; and especially when it is put in a rage.

Another author has more lately explained the phenomenon, by stating that the blood of the chameleon is of a violet blue colour, whilst the coats of the bloodvessels, and of the skin itself, are yellow, so that the colour of the skin changes accordingly as it is abundantly supplied with blood, or the reverse.

Cuvier regards the changes of colour as probably owing to the immense size of the reptile's lungs; and that according as these organs are full or are empty of air, they make the whole body more or less transparent, by forcing a larger or smaller quantity of blood into the integuments, and even colouring this fluid itself with hues more or less brilliant.

There are other naturalists, who, whilst they attribute these changes to the respiration, explain the effect of the pulmonary distentionon the skin in a different way. The common integuments of the chameleon, as is generally known, is furnished with a great number of small scaly granules which make it to resemble shagreen: it is said then, that these granules are of a yellowish colour, whilst the deeper portion is of a dark red tint, and if this membrane is much contracted, the granulations alone are seen; whilst, on the other hand, if the skin be disturbed by the enlargement of the lungs, these same granules separate the one from the other, and thus exhibit the natural colour of the skin, from which results all the various hues which are ever presented.

Mr Spittal, to whom we are indebted for some interesting observations on these changes, regards them as connected with the state of the lungs; and Mr Houston, who has enriched science by his researches upon the structure and motions of the tongue in these strange animals, considers this phenomenon as dependent on the vascular turgescency of the skin.

Finally, it might moreover to be inquired, whether these variations in colour were not owing to some peculiar construction of the scarf-skin, which, by itself changing, might act on the light in various ways, and might, in turn, reflect different rays, in the same way as very slender plates of the metals produce a succession of colours, according as their thickness is increased or diminished.

Now, regarding all these explanations, it must at once be seen, that they could be regarded in no other light than mere hypotheses; and the subject appeared to me so curious as to demand a more complete solution. I accordingly anxiously availed myself of the opportunity which was presented me by M. Savart, of examining anew these changes of colour, and of making researches as to the causes of so singular an occurrence.

About the month of June 1833, Mr Savart received two chameleons from Algiers, which he preserved alive till towards the end of October. One of these reptiles, which we shall designate No. 1. was usually of a purplish grey colour; but during the night, when it was in profound sleep, it appeared of a whitish-grey; and occasionally it exhibited along its sides, spots of a dirty yellow hue, aud sometimes there appeared on different parts of its body, other spots which were red, or even of a deep violet colour. Finally, some days previous to its death, it had assumed a yellowish hue, and was covered with small black miliary points, which by degrees extended, so as to form continuous spots, and to cover almost the whole body.

The chameleon No. 2. was usually of a deep bottle-green colour, approximating to black. When it was sound asleep, it became like the other of a pale dirty yellow; and, during the course of the day, there was often observed upon its sides, spots of a lettuce-green colour, while the rest of the body was of a bottle-green shade. When it was placed near to the window, and a hope of escaping seemed to be excited, the tinge of lettuce-green extended itself over the whole body. Finally, when it sickened, some yellowish spots appeared; but it maintained till death the general colour of azure-green, which was habitual to it.

The chameleon No. 2. changed colour more easily than the other; but in both, these changes were effected only gradually. Besides, we can distinctly state, that they were entirely independent of any distention more or less considerable of the body of the animal. These chameleons often swelled up exceedingly, without exhibiting any change of colour, and at other times these variations occurred, without their being preceded by any change of its size.

Thus then, direct observation destroys in a moment all the hypotheses, by the help of which naturalists had endeavoured to explain the changes in the chameleon's colour by the effects of the distention, more or less considerable, of its lungs; but they had hitherto thrown no light upon the real cause of the phenomenon. To obtain information on this point, I had recourse to anatomy.

Immediately after the death of chameleon No. 1., I detached a portion of the skin, upon which were to be found both the dark and red colour already described, and a large yellowish-grey spot, which I submitted to examination under a powerful magnifying glass.

The surface of the skin is, as is well known, thickly set with an immense number of small rounded tubercles, amongst which much finer granulations are to seen. Some naturalists have thought, as before stated, that the changes in the colour in the chameleon depended on the circumstance that these tubercles were yellowish, and the deeper parts of the skin of some other colour; and that when the skin was contracted, they alone were seen, whilst by the distention of the integuments, their points extended themselves, so to speak, over the inferior surface thus brought into view. But the real fact is opposed to this supposition; for, in those portions of the surface of the body which were most deeply tinged, as well as in those that were clearest, it was exactly underneath these tubercles that the local tint, whatever it was, was the most distinct.

In those parts of the skin which were of a dark red colour, it was easy to satisfy one's-self with the help of the magnifying glass, that the yellowish-grey colour peculiar to the neighbouring parts, had not entirely disappeared, but it was, as it were, somehow marked by a countless number of minute points of a purplish red, more or less deep; each tubercle seemed covered with a net-work, and, examined with the naked eye, these points seemed to cover all the surface. Between the several tubercles, points of the same colour were also visible, but of a much lighter shade. Finally, on the inner surface of the skin, this deep hue appeared still more intense.

In those parts of the skin which did not exhibit this purple colour, there was only to be seen, on the external surface, a yellowish-grey tint, more decided over the cutaneous tubercles, than in the intervals by which they were separated; and in some patches along the sides, and on the under part of the body, it was whiter than elsewhere; whilst on the upper parts of the back it inclined more to pure yellow. When the skin was extended so as to stretch the tubercles with which it is studded, no apparent change was produced on the colour; but, on examining its inner surface, there was universally found the same violet-red colour, approaching to black, which, in certain places, was seen from without as well as from within.

It appears then evident, that there exists throughout, in the integuments of this animal, two pigments which are quite distinct, viz. one of a grey colour, more or less yellowish or white, according to the parts on which it is observed, and another of a violet or blackish red; and that the differences of colour which I have noticed were owing to this circumstance—that sometimes the latter membranous pigment was seen at the surface through the scarfskin, more or less intermingled with the former, whilst, again, at other times, it was hid under the greyish membrane.

This fact being established, it became probable that the formation of the purple spots of varying hues which were observed in a transient way during the life of the animal, and afterwards vanished altogether, depended on certain alterations in the position of the lower membrane. The mixture of its tint with the hue of the more superficial layer, can in truth easily explain all the phenomena which are observed during life. And I was only confirmed in this opinion by the change which was exhibited a little while after death. As we have previously noticed, the dark red hue extended itself nearly over the whole surface of the body; but on placing the animal upon a marble slab which was somewhat cold, I observed that the spots became paler, and in certain places vanished altogether. In those points where the colour had undergone this change, the darker pigment was no longer visible under the scarf-skin, and was only to be found under the greyish pigment, by which it was now again completely covered over.

When to the skin of the animal which had just died, I applied some concentrated alcohol, the dark purple colour almost immediately disappeared, and in its place was to be seen that tint, which, during the life of the chameleon, unusually pervades it during profound sleep. The greater part of the stronger acids produced the same effect; but when I applied a solution of alkali to a part of the skin which presented the natural yellowish-grey colour of the superficial pigment, the exactly opposite change was effected, the colour immediately changed to a dark red.

Finally, in small shreds of the skin which I detached from the body, I could make the colour change from the yellowish-grey to the violet-red by mechanical means alone, viz. by pressing the deeper pigment towards the surface of the skin; and when, after this, I examined these portions with the microscope, I found them of the same appearance with those similar tints resulting from the physiological actions respecting which it was my object to discover the nature.

The results being the same, it might be presumed that the causes were analogous, and it would then follow, that during life, the deeper pigment, according as it was hidden in the substance of the skin, or shewed itself in a greater or less degree in the midst of the more superficial pigment, occasioned the phenomena of coloration or of decoloration of which we have already described all the appearances. But another inquiry yet remains, viz. How this inferior pigment could now protrude into, and now retire from, the more superficial one? For the solution of this question I again had recourse to an examination of the structure of the skin.

Having digested for some time a portion of the skin in a strong solution of alkali, for the purpose of dissolving or making transparent the parts which concealed the position of the pigment, I then dissected it under a strong magnifying glass, and I distinctly perceived that the dark colouring matter was contained in a great number of small cavities which were situated in the substance of the skin, from each of which originated very minute ramifications which extended to the scarf-skin, so traversing the superficial bed of greyish pigment, which appeared extended over the whole surface of the skin, and very well represented the coat which by anatomists is denominated the rete mucosum.

This structure once recognised, it became an easy matter to understand how the deep pigments could now mount up through the middle of the superficial, and exhibit its own colour more or less completely, or at another time might wholly be hidden beneath it. For the production of the first of these appearances, it would suffice that the lower part of these vesicles should contract, or be compressed by the contraction of the deeper parts of the skin, so as to cause the fluid contained in their interior to flow into the minute ramifications with which their surface is supplied, so as to render the fluid visible from without. For the skin thus coloured to return to its yellowish-grey tint, all that would be required would be the contraction or the compression of these same superficial ramifications, which thus emptying themselves into the vesicle below, would lose their colour, and more or less completely disappear.

It may now be remarked that this phenomenon is not altogether unique in nature. Many of the mollusca cephalopoda exhibit something analogous. The skin of these animals is furnished with a number of differently coloured spots, which alternately appear and disappear; and if a portion is put under a microscope, it may be perceived that these changes depend on the contraction of small vesicles filled with a coloured liquid, which reach from the surface of the skin to a considerable depth. When one of these spots appears, the liquid corresponding here to the pigment in the other case, is propelled towards the superficial part of the vesicle, and there displays itself; whilst during its disappearance it is forced into the deeper parts by the contraction of this superficial point itself, which then becomes almost invisible.

The dissection of the second chameleon confirmed the results obtained by the researches we have just been describing. For here we found two very distinct pigments; the one superficial, yellowish or white, according to the portions of the surface which were examined; the other deeper, and of a bottle-green colour, verging to the black. It is evident that the mixture of these two colours, and the predominance of the one over the other, could not but produce all the variations which were exhibited during the life of the animal.

This greenish pigment appeared, moreover, to have the greatest analogy with the purple one which was found in No. 1. It underwent the same changes with the various chemical agents, and as the light happened to strike upon it exhibited a very deep green colour, or one which approximated to the violet.

It is known that there are many colouring substances, which, viewed by the transmission of light, thus being transparent, or by the reflection of the light, or, finally, examined in masses more or less dense, change their hues. The reddish green colour of Carthamus presents a remarkable example of this phenomena; and it appeared probable that the difference which existed between the hues of the deeper pigment in these two chameleons, depended on some slight difference in its state of cohesion. If this happens, the same individual might present not only those changes which we noticed, but also a change from green to purple.

However this may be, we see—

1*st*, That the change in the colour of chameleons does not depend essentially either on the more or less considerable swelling of their bodies, or the changes which might hence result on the condition of their blood or of their circulation; nor does it depend on the greater or less distance which may exist between the several cutaneous tubercles; although it is not to be denied that these circumstances probably exercise some influence upon the phenomenon.

2*dly*, That there exists in the skin of these animals two layers of membranous pigment, placed the one above the other, but arranged in such a way as to appear simultaneously under the scarf-skin, and sometimes so that the one may conceal the other.

3*dly*, That every thing remarkable in the changes of colour which manifest themselves in the chameleon, may be explained by the appearance of the pigment of the deeper layer to an extent more or less considerable, in the midst of the pigment of the superficial layer; or from its disappearance underneath this layer.

4*thly*, That these displacements of the deeper pigment can in reality occur; and it is probably a consequence of them that the chameleon's colour changes during life, and may continue to change even after death.

5thly, That there exists a close analogy between the mechanism by the help of which the changes of colour appear to take place in these reptiles, and that which determines the successive appearance and disappearance of coloured spots in the mantles of several of the cephalopode mollusca.

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*First Essay, preliminary to the Series of Reports on the Progress of the Useful Arts, ordered by the Society of Arts for Scotland.**

When we contrast the conditions of man at remote periods of his history, we perceive an immense disparity between his attainments; and this disparity becomes the more remarkable when we consider, that of all the species of animals which exist on the surface of the earth, man alone is liable to this fluctuation.

Each individual animal attains the complete use of all its faculties; and this, even though the successive generations of the tribe be separated from each other by a long lapse of time. With many animals nothing in the shape of instruction is needed. The insect tribes at once proceed in the course that nature has designed for them. No sooner does the egg burst than the larva sets itself about the business of its existence; it swims expertly through the water, and seeks out its appropriate food. Led by an unerring instinct, it approaches the surface of the pool or climbs the stalk of some aquatic plant, and, ere the spectator has time to mark the change, it launches off in an untried element, and is undistinguished amid the thousands that have had the long experience of an hour. Some again wake to life in the tough bark, and eat their vermicular way through the sapwood; till, when the metamorphosis draws near, they seek the outer rind, cut it with altered mandibles, elevate their elytra, unfold from beneath them their delicate wings, and use, with the utmost ease, their newly acquired powers and senses.

Ascend, as we call it, in the scale of existence, and the ele-

* Read before the Society, 5th September 1834.

ments of tuition appear. The birds, for the most part, educate their young; they lead them by short flights to seek their food, and only abandon them after their powers are fully developed. The same remarks hold of many of the quadrupeds. In all cases, however, the powers arrived at are nearly the same with each individual of a species. But when we reach the top of the scale, how different! The young of the human species receives not merely that tuition which is common to all the mammalia, but also a distinct kind of education, which conveys the fruits of the experience of all the preceding generations. Man lives to add to that experience, and though his physical powers reach to their full development, the entire man knows nothing of maturity. Powers of which our ancestors were ignorant, are now wielded by us, while we, in our turn, may be opening the way for other processes to be employed by our descendants.

The burrowing bee still uses the same instrument to pierce the downright shaft, and to cluster round it the beautifully smoothed cells. Still she selects the hard beaten soil, whence the wind may sweep the dust that otherwise would betray her labours. The sand-spider still uses the same cement to form the walls of her retreat, and to weave her branchy net. But man is found at one time burying himself in the ground, at another tearing the rocks asunder to rear the magnificent palace. Here he draws his subsistence from the ocean, there he cultivates the ground; here he clothes himself in the skin of the wild beast, there he weaves the delicate web, and prides himself in the sleekness of his coat.

With man there is no permanence. Every thing is changing, and each season adds to his powers and comfort. He seems to possess an endless variety of appetites, that are only called into action as opportunity offers for their gratification; there seems to lurk within him an immense variety of powers, of which only a few are called into active use by any single individual. Among the animals, the history of an individual is almost the history of the tribe; but the story of the life of man is ever changing; and the mode of living of one nation appears incredible to another.

In the present paper I intend to take a rapid view of the general nature of those processes by which man gradually changes his condition; to examine into their tendencies, whether for or against the increase of human happiness; and to glance at the method of encouraging their development.

Man is possessed of a highly muscular and pliable form, capable of enduring long continued and vigorous exertion; yet the tenderness of his limbs prohibits the direct employment of his powers. The animals are invariably supplied with instruments fit for the various operations they have to perform. The bee has the proboscis wherewith to reach the bottom of the nectary; the burrowing animals have claws for digging the earth, and the beasts of prey for tearing their food. But man works entirely by *tools.* Does he wish to throw the stone, he uses the sling; the spade enables him to dig the ground. The capability of employing inanimate matter, of making it, as it were, a part of himself, is almost peculiar to man; only very faint traces of that power are to be perceived among the animal tribes; in man it is completely developed; for, on reflection, we will at once perceive that almost every operation which we perform, is done by the assistance of tools of one kind or another. When we walk we protect our feet against the sharpnesses of the road, and when we attempt to change the form of any hard body, we arm our hands with something harder still.

It is then not merely possible for man to employ tools in his operations; it is indispensable. His hands are admirably formed for the wielding of such tools; while with them alone he could neither dig the earth nor fashion the softest timber. Taken in the most general sense then, the *arts* are eminently useful to our race. Some of them are necessary to our very existence.

Let us conceive man in his rudest stage, ignorant of every latent power, and using only his own limbs. View him falling before every attack of the lion, the tiger, or even of the wolf; and then fancy that some fortunate individual hurls the stone to crush his adversary. What rejoicings through the tribe! with what eagerness would they practise the new *art!* what honours would be heaped on its discoverer! Suppose then, that chance had revealed to them the nutritious qualities of some root; and that patiently they dug it with their fingers, till some one fatigued, perhaps tortured, with continued exertion, seizes the flat stone or the broken bough, and renders the operation at once more rapid and more agreeable. It is clear that the discovery of these simple tools, the *missile* and the *spade*, would increase the security and augment the power of the race; it is undeniable that with these assistants man would be happier and more comfortable than before. The labour expended in providing sustenance and in defending life would be diminished, and more time would be allowed for the gratification of higher appetites. The spade, the sling, the club, or the bow, would begin to be ornamented, and man would delight himself in the symmetry and propriety of the ornaments.

On the first glance one might allow that the same principle would extend itself to every subsequent discovery, and that every means of enabling man to produce a greater effect than he could have produced before with the same labour, must augment the enjoyments of the race. Such a general statement, however, must be received with the utmost caution, since the circumstances of human society are so varied that some of them may even happen to convert the benefit that otherwise would have accrued from such improvements, into an actual evil.

So long as each individual family provide for their own wants, the principle has undoubted scope; but when different individuals have betaken themselves to different crafts, and when the community depends on the barter of good offices, an improvement in any particular art may be highly injurious to some one of the various classes. It seems clear that every improvement is ultimately beneficial to the whole commonwealth; but it is impossible to shut our eyes to the misery which is, at times, inflicted on individuals. In the present highly artificial state of society, these sinister effects are unfortunately too frequent and too severe, and it is no easy matter for the sufferers to reason themselves into the belief, that what has been productive of their own ruin can at all be beneficial to the community.

The rapidity with which changes in the modes of operating are now introduced; and the competition which exists, not merely between the fabricators of the same material, but between those of different kinds of commodities, give an unprecedented variability to the distribution of comforts and enjoyments; so that when contemplating the advantage of same new process, one shrinks from a view of the mass of misery which its introduction may occasion. Many arts are acquired only after long practice; the power of producing certain effects with rapidity and precision, forms the capital in trade of many industrious men. But when some new mode is discovered, that capital is completely annihilated, and the unemployed workman can hardly look with any thing but jealousy on the innovation. In the old method of manufacturing paper, great dexterity was required in lifting a sheet, so that it might be of equal thickness throughout, as well as similar to others of the ream. Good workmen were scarce and received high wages; but when the thickness came to be regulated by machinery, these workmen were either thrown idle or compelled to betake themselves to some inferior employment; a number, indeed, readily found employment in the continental paper-works, yet still the distress must have been great among that class of men. The members of that craft, however, formed a small proportion of the population, so that the elastic constitution of human society has already accommodated itself to the new circumstances. But the hand-loom weavers have suffered more severely and for a greater length of time; their number was more considerable, and of course they experienced a greater difficulty in finding a new employment; and the circumstance, that the finer and more expensive looms were often their own property, has perhaps rendered them unwilling to desert their old employment.

Painful though the contemplation of such sufferings may be to the philanthropist, it is impossible that he can disapprove of the cause. A small class of the present generation indeed suffers, but a benefit is conferred not only on the population at present existing, but on the whole future generations of the race. The distress occasioned by the substitution of steam for hand-spinning, was intense; that, however, has passed away, and the effect now is, that the peasant is able to purchase fabrics which, a hundred years ago, would have excited envy among the wealthy. The machines for forming the various parts of watches, threw, no doubt, out of employment for a time, the handicraftsmen, but a watch is not now considered as worthy the attention of a Rupert. I might cite examples without number, for in almost every instance, a decided improvement has been accompanied by temporary inconvenience, while the permanent effects have invariably been an increase in the number of hands employed, and an advance in the general condition of society.

There are, however, some exceptions. There have been improvements beneficial throughout to all parties concerned. Such are those improvements in agriculture which augment the fertility of the soil; or those in mechanics which have rendered practicable what before was impracticable. Mines and quarries which could not have been profitably wrought, had the water and the rubbish been removed by hand, have become at once productive on the application of water, wind, or steam. And remote pastures, whence the cattle could not be brought but at an expense almost exceeding their value, now regularly supply our cities. On such improvements the mind rests with unalloyed pleasure; not so when the improvement consists in the substitution of one process for another.

Since the distress of one class of workmen is often the effect of a general improvement, we might consider it fair that the community charge itself with the relief of the sufferers. The projectors of a canal or of a railway are taken bound to make up any loss that may be sustained by the trustees on the ordinary road; and it really appears that, with equal propriety, the inventor of some new process might be made liable for the damage thereby occasioned to those who practised the old ones. Such an enactment, however, imagining it capable of being put in execution, would crush every innovation, and produce that stagnation which we see in China; it would prevent even the changes of fashion, and the shawl-weavers would at present be prosecuting actions against all the weavers of printed crape. Since distress, as serious as that produced by any improvement, is often occasioned by the caprice of the leader of fashion, with as much propriety would we define the form and material of dress, as restrain the most perfect freedom in the progress of discovery. Relief need not be sought from legal enactments; yet the case is a hard one, and demands alleviation.

The principal source of inconvenience lies in the difficulty with which the workman acquires a new craft. Those workmen who are accustomed to a great variety of processes, and to a frequent change of tool, are almost secure; but the situation of those who know only one process, is deplorable. He who has never gone beyond the fixing of a pin's head, or the sharpening of its point, would be rendered completely destitute by the discovery of a method of forming a pin by machinery; but the engine-maker has never felt any inconvenience from the general introduction of slide-rests and planing-engines. The philanthropist then, should rejoice to see the workman, not contented with a single occupation, render himself master of several crafts. A great deal of stress has been laid on what is called the division of labour. Long continued practice in a single art will indeed give at length great expertness; yet I question if one accustomed to vary his work, would not much sooner reach proficiency. In passing from the grinding of knives to the grinding of scissors, some little time indeed is lost, and a few minutes elapse ere the workman's hand has got, as he calls it, into the way. But this only at first—after he has had frequent occasion to make such transitions, his mind has stored up the little incidents to be attended to, and no more time is needed at a change of employment, than is necessary every morning at the beginning of his day's labour. Not only would this custom of acquiring several trades give security to the workman, it would at the same time greatly facilitate the march of improvement. An interchange of methods and ideas would at once take place, and each craft would adopt some of the methods practised by another. The wright, instead of forcing an obstinate screw-nail, would use the chisel of the die-cutter to dress the destroyed cut—thousands of applications would be made of principles that otherwise would never be brought home to the attention of the artizan.

If we run over the history of the great improvements that have been made, we will find that almost all of them have come from the minds of those who have attended to a considerable variety of subjects. The great improver of the steam-engine was not an engine maker, nor did the contrivance of the stocking-frame come from one tired with the monotonous manipulation; and I question much if any of the users of the distaff ever entertained the idea that it could be dispensed with. The method of co-ordinates of the higher geometricians, after improving astronomy and geodetics, descended to plan the slide-rest, and the planing-engine; and had no intercourse existed between the art of working iron, and the abstruse science of applied algebra, we might yet have wanted those excellent instruments. An extensive knowledge, in fact, of what can be done with materials is essential to the contrivance of new modes of operating; or even necessary to the successful repetition of operations already known.

From the general cultivation of the minds of the workmen, we might therefore expect an increase to the stability of human society, an improvement in the style of execution of the various manufactures, and a more fertile inventiveness. We are apt to imagine that the improvement of our manufactures goes on by starts, and that a few names only need be mentioned in their history. The course of improvement is indeed marked by a few more considerable steps; but beneath these, and concealed from the mere casual glance, there flows a calm but rapid current. Minute facts are communicated from one workman to another, and the craft proceeds by insensible gradations; the skill of the workman gradually advances, creates a taste in the consumer for a better article, and receives again from that improved taste a reflected stimulus. Unless we had workmen skilful to execute, our best arranged schemes would fall to the ground; and unless the artizan himself were able to appreciate the beauty of execution, we would be shocked, at every turn, by deformity and imperfection. But when the mason plies the mallet, he delights in contemplating the finished cornice, and not merely, as we are apt to imagine, the prospect of gain, but also the anticipation of a higher enjoyment cheers him through his toil. And perhaps the gratification of the architect himself is inferior to the delight felt by his better workmen when they retire to contemplate the finished edifice.

While seeking the means for encouraging improvements in the arts, we must not then imagine that our only business is to reward the contriver of some important change. Of that task the anomalous patent laws have effectually relieved us. The principal part of our employment should be to render public each minute piece of detail, and to encourage the able workman to communicate the results of his experience. We should endeavour to improve his taste by the exhibition of finer specimens of workmanship, and to excite his emulation by rewarding superior merit. Variations almost insensible, in the modes of operating, lead to important changes in the character of the effects. Thus we attempt in vain to file a small surface flat while it is held firmly in the vice, yet the operation becomes easy when a small vibratory motion is allowed. Again, if we attempt to hone a penknife with the thumb towards the back of the blade, we soon produce a rounded edge, but on reversing the position of the blade, a thin edge is preserved. The turning-graver when held in one position, makes criddled work; yet, when placed in another scarcely different, it produces a clean surface. The spokesheaver, when used by a beginner, is apt to round the end of a piece of wood, but a very little deviation from the ruder way of holding it preserves the proper outline. Thousands of niceties such as these would form the workman's manual. Those workmen who have constantly resided in an isolated district, are often found ignorant of the simplest artifices of their craft; while those again who have had communication with numbers of their own trade become, from that intercourse, expert operators. Keeping facts such as these in view, our Society should gladly receive the statements of the artizan, and should lend a willing ear to communications, though their subject might be of less importance than a substitute for steam. By rendering this a place where the workman might hear the different processes described, and the principles of them explained; where he might learn the cause of the advantages of one method over another, and the source of the imperfections of all operations, we would provide an inexhaustible fund of instruction for ourselves, and, at the same time, would give a powerful assistance to the progress of invention.

Those improvements which seem likely to produce a decided effect, and those inventions that have required the concurrence of great ingenuity and severe exertion, merit our peculiar attention; and considerable difference of opinion exists as to how we ought to proceed in our attempts at encouragement. We may either propose prizes for distinct objects, or may generally offer rewards for such improvements as appear best deserving of them. In the one way we attempt to lead the mind, in the other, we only approve of what has been done. Of the first, we have had a notable example in the parliamentary offer of a reward for a perpetual motion,—a reward, the hope of receiving which, has led many ingenious men to ruin. The last approaches as nearly as possible to the natural state of things, where a meritorious action works out its own reward. We have resolved on an amalgamation of the two, so as to combine all the advantages, but all the disadvantages too, of both. This course will be attended with one important result, that in a few years our own records will furnish us with data wherefrom to draw a decision; it will then be proper to return to this important subject.

That encouragement, however, which we can give aggregately is trifling in comparison with that which flows from our individual exertions. It is in the power of each to recommend a better article, or a more commodious instrument; and even though the newer commodity be a little more expensive than the old, it is the duty of every one to give the improved process a fair chance, in order to accelerate, as much as possible, the yet remote maturity of the human race.

Sept. 4. 1834. EDWARD SANG.

Observations on the Hygrometer.

IN a former paper *, a sketch was attempted of the theories which have been proposed of the Hygrometer by evaporation; and a modification of Sir John Leslie's formula for finding the dew-point was suggested.

The writer is gratified to find, that one so familiar with the subject of that communication as Mr H. Meikle, has expressed a favourable opinion of the Table of Dew-points which it contains †. With respect to some of the errors which that gentleman has pointed out, they are readily acknowledged; especially that of supposing a given volume of air, of given temperature, to require more water for its saturation under a great, than under a small pressure.

Mr Meikle, in the same paper, explains a mode, which he has discovered, of expressing geometrically the relation between

* Edinburgh New Philosophical Journal, vol. xv. p. 273.

† Ibid. vol. xvii. p. 98.

the temperature of the air, of the wet thermometer, and of the dew-point. This ingenious method seems to correspond remarkably with the results of the formula, except when the air is supposed to approach to dryness. The error then rapidly increases, as will be apparent from the following example. Suppose that in air absolutely dry, the wet thermometer indicated 54°; the temperature of the dew-point would then be infinitely low, and the line DE in the diagram would coincide with the asymptote. But this would give to the air a temperature infinitely high; whereas, by the formula, it ought only to be about 114°.

The author regrets, that, before writing his former observations, he had not seen Sir James Ivory's paper "On the Hygrometer by evaporation," contained in vol. lx. of the Philosophical Magazine. That investigation seems to be so comprehensive, that, if the result does not entirely correspond with experiment, the cause of the non-accordance must be sought for in the inaccuracy of some of the data. But the only two data not derived from experiment are, that the air in contact with the wet bulb is fully saturated; and, that the heat by which the water is converted into vapour is entirely derived from the air. The writer had at one time some difficulty in reconciling the latter supposition with the phenomena, but now he sees that they are strictly consistent; and that, while, at the commencement of the evaporation, the bulb supplies nearly the whole heat, and the air scarcely any, the case is perfectly reversed by the time the maximum cold is established. When the maximum has been once attained, the film of water may be conceived to act as a perfect non-conductor between the air and the bulb of the thermometer; for, instead of transmitting to the latter the heat which it receives from the former, a portion of it combines with that heat, and both are dissipated in the form of vapour. With respect to the other supposition, namely, that the air in contact with the wet bulb is fully saturated, it will be considered at a subsequent part of the paper. In the mean time, an attempt will be made to trace the nature of Sir James Ivory's investigation.

It commences with the supposition, that, in a given volume of air, of given temperature, and under a given pressure, vapour of some unknown tension is contained. The volume of combined air and vapour is then supposed to sink in temperature, each successive portion of heat disengaged in so doing being entirely employed in adding to the quantity of vapour contained in the air, until the saturation be completed.

The equation deduced from these data is somewhat complicated. The complexity, however, arises chiefly from the condition that the mass to be cooled is composed of variable proportions of air and vapour, having different specific heats, and also from taking into account the difference of volume corresponding to the difference between the original temperature and that of the dew-point. But as these conditions, from the minuteness of the quantities which they introduce, do not sensibly affect the result of the formula, Sir James Ivory adopts a more simple equation, which may be investigated as follows, on the supposition that the air from which the heat is evolved is dry, and that its original volume remains unchanged.

Suppose a given volume of air at the temperature t, and under the pressure B, to contain a quantity of moisture which would saturate it at the temperature t''. Further, that the temperature of the given volume of air is reduced from t to $t - D = t'$, and that the water converted into vapour by the heat thus evolved, is exactly sufficient to raise the point of saturation of the given volume from t'' to t'. It may be shewn, that if $\frac{B}{30}$ represent the weight of the given volume of air, the weight of water to be converted into vapour will be very nearly $\frac{f_{t'} - f_{t''}}{48}$, where $f_{t'}$ and $f_{t''}$ denote the tensions of vapour at the temperatures t' and t'' respectively.

Again, the specific heat of air under a constant weight, and when the pressure is B, being $= a'$, and the latent heat of steam being $= l$, then a weight of dry air $= \frac{B}{30}$, in cooling D degrees, will communicate to a weight of water $= 1$, sufficient heat to raise its temperature $\frac{BDa'}{30}$ degrees. Further, the same weight of water $= 1$, will have its temperature raised $\frac{l(f_{t'} - f_{t''})}{48}$ degrees by the heat required to convert into vapour a weight of water $= \frac{f_{t'} - f_{t''}}{48}$. But, by supposition, $\frac{\text{BD}\,a'}{30} = \frac{l\,(f_{t'} - f_{t''})}{48}$; consequently, when $f_{t''}$ is the only unknown quantity, it may be found by the equation

$$f_{t'} - \frac{\text{B}}{30} \times \frac{48\,a'\,\text{D}}{l} = f_{t''} \qquad \text{(1.)}$$

As Sir James Ivory considered the above formula chiefly in its application under the ordinary pressure, he substituted 1 for $\frac{\text{B}}{30}$, and the specific heat of air under a pressure of 30 inches for a'. But supposing that B differed considerably from 30, it would be necessary to retain it in its original form; or, instead of a', to substitute $a =$ the specific heat under a pressure of 30 inches, and to multiply it by a coefficient varying with B.

This being a subject upon which there is considerable diversity of opinion, the author is anxious to state the grounds on which he proceeds in drawing any conclusion. With this view, he takes the liberty of making the following quotation from a paper by La Place *. "On peut déduire des rapports précédens divers theorêmes sur les gaz: tel est le suivant, qui s'accorde avec les expériences faites sur cet objet, autant qu'on doit l'attendre d'expériences aussi delicates. La quantité de chaleur dégagée par un volume de gaz, en passant, sous une pression determinée, d'une température à une autre inférieure, est proportionelle à la racine carrée de cette pression." According, then, to the theorem here enunciated, if c be the specific heat of air *under a constant volume*, when the pressure is 30 inches, and c' the specific heat when the pressure is B, it will follow that $c\sqrt{\left(\frac{\text{B}}{30}\right)} = c'$. But, denoting the specific gravity of air under the pressure of 30 inches by s, and that under the pressure B by $s \times \frac{\text{B}}{30} = s'$; and having reference to the values of a

* Annales de Chimie et de Physique, tom. xviii. 185.

and a' formerly assigned, it is evident that $\frac{c}{s} = a$ and $\frac{c'}{s'} = a'$.

Hence, by substitution,

$$\frac{c'}{s'} = \frac{c\sqrt{\left(\frac{B}{30}\right)}}{s \times \frac{B}{30}} = \frac{c}{s}\sqrt{\left(\frac{30}{B}\right)} = a\sqrt{\left(\frac{30}{B}\right)} = a'.$$

Introducing the above value of a' into equation (1.) it becomes

$$f_{t'} - \frac{48\,a\,D}{l}\sqrt{\left(\frac{B}{30}\right)} = f_{t''} \text{ .... (2.)}$$

or $f_{t'} - .011448\ D\sqrt{\left(\frac{B}{30}\right)} = f_{t''}$

by making $a = .2669$, and $l = 1119$.

With respect to the correction for difference of pressure, the author stated in his former paper, that the formula there proposed gave correct results between the pressures of 30 and 22.9, when D was multiplied by $\frac{B+27}{57}$. Although, as it appears, this coefficient was adopted through a misunderstanding of what Mr Meikle had stated, yet, if $\sqrt{\left(\frac{B}{30}\right)}$ be the coefficient which is truly deducible from theory, the accompanying Table will explain how the other agreed so well with observation; for it is evident that, at least as low as 22 inches, it would be indifferent whether the Table of Corrections were calculated by means of $\sqrt{\left(\frac{B}{30}\right)}$ or $\frac{B+27}{57}$. And here it ought to be remarked, that the numbers in the small table formerly given are rather too large, an additional correction having been joined with them, with the view of obviating a source of error, which, upon more careful consideration, it is found can have no sensible effect. The subjoined is the correct Table: the manner of using it is explained in the former paper.

B.	$\sqrt{\left(\frac{B}{30}\right)}$	$\frac{B+27}{57}$
30	1.000	1.000
29	0.983	0.982
28	0.966	0.965
27	0.948	0.947
26	0.931	0.929
25	0.913	0.912
24	0.894	0.895
23	0.875	0.877
22	0.856	0.859

Difference between the Wet and Dry Thermometers.

BAROM.		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34
30.5	29.5	.01	.02	.02	.03	.04	.05	.06	.07	.08	.09	.09	.10	.11	.12	.13	.14	.15	.15	.16	.17	.18	.19	.20	.20	.21	.22	.23	.24	.25	.26	.27	.27	.28	.29
31.	29.	.02	.03	.05	.07	.08	.10	.12	.13	.15	.17	.19	.20	.22	.24	.26	.27	.29	.31	.32	.34	.36	.38	.39	.41	.43	.44	.46	.48	.50	.51	.53	.55	.57	.58
	28.	.03	.07	.10	.14	.17	.21	.24	.27	.31	.34	.38	.41	.45	.48	.51	.55	.58	.62	.65	.69	.72	.76	.79	.82	.86	.89	.93	.96	1.0					
	27.	.05	.10	.15	.21	.26	.31	.36	.41	.46	.52	.57	.62	.67	.72	.77	.82	.88	.93	.98	1.03	1.08	1.13	1.19	1.24	1.29	1.34	1.39	1.44						
	26.	.07	.14	.21	.27	.34	.41	.48	.55	.62	.69	.76	.82	.89	.96	1.0	1.1	1.2	1.2	1.3	1.4	1.4	1.5	1.6	1.6	1.7	1.8	1.8							
	25.	.08	.17	.26	.34	.43	.52	.60	.69	.77	.86	.95	1.0	1.1	1.2	1.3	1.4	1.5	1.5	1.6	1.7	1.8	1.9	2.0	2.0	2.1	2.2								
	24.	.10	.21	.31	.41	.52	.62	.72	.82	.93	1.0	1.1	1.2	1.3	1.4	1.5	1.6	1.7	1.8	2.0	2.1	2.2	2.3	2.4	2.5	2.6									
	23.	.12	.24	.36	.48	.60	.72	.84	.96	1.1	1.2	1.3	1.4	1.6	1.7	1.8	1.9	2.0	2.2	2.3	2.4	2.5	2.6	2.8	2.9										
	22.	.14	.27	.41	.55	.69	.82	.97	1.1	1.2	1.4	1.5	1.6	1.8	1.9	2.1	2.2	2.3	2.5	2.6	2.7	2.9	3.0	3.2											
	21.	.15	.31	.46	.63	.78	.94	1.1	1.2	1.4	1.5	1.7	1.9	2.0	2.2	2.3	2.5	2.6	2.8	2.9	3.1	3.3	3.4												
	20.	.17	.34	.52	.69	.86	1.0	1.2	1.4	1.5	1.7	1.9	2.1	2.4	2.2	2.6	2.7	2.9	3.1	3.3	3.4	3.6													

In deducing the correction for difference of pressure, the writer has been led to adopt views regarding the specific heat of air, as affected by pressure, differing from those entertained by Mr Meikle. He has been influenced in doing so by the consideration, that while the experiments of Clement and Desormes are opposed to the opinion that the specific heat of a given volume of air is directly as the density, they corroborate the law theoretically deduced by La Place.

With reference to the experiments made by Professor Daniell and Mr Meikle, for the purpose of determining the variation of D with the pressure, it seems evident, that when a thermometer with a wet bulb is placed in the receiver of an air-pump, the air cannot be perfectly dry, even when exposed to the action of sulphuric acid; for the very circumstance of there being a portion of moisture absorbed at the surface of the acid, proves that it pervades also the rest of the receiver. Supposing, therefore, in the two experiments mentioned by Mr Meikle at p. 106, of his paper, $t'' = -14°$ and $f_{-14°} = .031$, then the value of D, calculated by means of the formula

$$(f_{t'} - f_{t''}) \times \frac{175.4\, f_t}{.66372 + f_t} \times \sqrt{\left(\frac{30}{B}\right)} = D, \text{ will be}$$

$$(.22255 - .031)\, \frac{175.4 \times .32667}{.66372 + .32667} = 11°.08, \text{ 1st experiment; and}$$

$$(.22255 - .031) \times \frac{175.4 \times .5215}{.66372 + .5215} \times \sqrt{\left(\frac{30}{10}\right)} = 25°.6, \text{ 2d ex-}$$

periment; the errors being + .08 and + .8. It appears from the above formula how much the value of D may be affected by the presence of a portion of vapour probably too small to be detected by means of Daniell's hygrometer.

The author is still unable to show that the modification of the dew-point formula which he proposed, is theoretically deducible from the data. However, as one of these data is only hypothetical, namely, the supposition that the air in contact with the wet bulb is fully saturated, he would venture to propose in its stead the condition, that the saturation of the air is not altogether perfect, but that, for the same value of D, it is more and more complete, as the temperature of the air increases. It is easy to see, from a consideration of Sir James Ivory's formula, that, on such a hypothesis, the value of D would need to be increased as *t* diminished. A small Table of comparative observations is subjoined.

No. of Experiment.	Temperature of the Air.	Wet Thermometers.	Difference between the dry and wet Thermometers.	Observed Dew-point.	Calculated Dew-point.	Error.
1	68.25	61.75	6.5	57.25	57.5	+ .25
2	56.25	54.5	1.75	53.25	52.75	— .5
3	64.5	59.0	5.5	54.5	54.8	+ .3
4	67.5	61.25	6.25	55.75	57.0	+ 1.25
5	67.25	61.0	6.25	56.75	56.7	— .05
6	63.0	59.0	4.0	56.25	55.8	— .45
7	62.25	57.75	4.5	55.25	54.0	— 1.25
8	68.0	61.75	6.25	57.25	57.65	+ .4
9	63.25	59.0	4.25	56.5	55.2	— 1.3
10	69.5	63.0	6.5	58.25	59.0	+ .75
11	68.75	61.0	7.75	56.25	55.8	— .45
12	63.5	58.0	5.5	54.75	53.6	— 1.15
13	63.75	58.0	5.75	54.5	53.4	— 1.1
14	68.0	61.25	6.75	56.25	56.75	+ .5
15	65.5	59.75	5.75	55.25	55.6	+ .35
16	69.0	62.0	7.0	57.25	57.5	+ .25
17	66.5	61.0	5.5	57.5	57.2	— .3
18	66.25	61.0	5.25	57.5	57.5	0
19	67.0	59.5	7.5	54.5	54.6	+ .1
20	64.5	58.75	5.75	54.25	54.3	+ .05
21	64.75	58.75	6.0	53.75	54.2	+ .45
22	59.0	54.0	5.0	50.0	49.1	— .9
23	63.75	57.75	6.0	53.75	52.9	— .85
24	63.5	57.25	6.25	52.25	52.1	— .15
25	59.75	54.75	5.0	49.25	49.9	+ .65
26	62.5	56.25	6.25	51.25	50.9	— .35
27	61.25	56.5	4.75	53.25	52.3	— .95
28	60.75	56.5	4.25	53.25	52.8	— .45
29	62.75	57.75	5.0	53.75	53.7	— .05
30	65.5	60.5	5.0	55.75	56.9	+ 1.15
31	64.75	61.0	3.76	58.25	58.35	+ .1
32	61.25	57.25	6.0	49.75	49.7	— .05
33	62.5	57.25	5.25	52.75	51.7	— 1.05
34	62.25	56.75	5.5	52.75	51.5	— 1.25
35	60.25	56.5	3.75	53.75	52.6	— 1.15
36	57.25	53.75	3.5	51.5	50.2	— 1.3
37				51.0		— .8
38	58.5	54.0	4.5	50.75	49.5	— 1.25
39	57.25	55.25	2.0	54.0	53.4	— .6
40	67.5	62.25	5.25	58.75	58.85	+ .1

There was no barometer at hand when the observations were made; but the variations of pressure at the surface of the earth are too small to occasion any sensible error, except when the air is of a low temperature, and very dry.

From an inspection of the table, it appears that the maximum errors in excess and defect were + 1.25 and — 1.3. Also by comparing the 4th and 5th observations together; the 6th and 9th; the 15th and 30th; the 22d and 25th; the 29th and 33d; the 36th and 37th,—it will be seen that the observed dew-point varied considerably, without a corresponding change in the dry and wet thermometers, sufficient to account for the difference. It is true that this circumstance does not determine on which side the error lies; but if the comparative difficulty of making an observation in the two cases be considered, it seems fair to attribute it to the uncertainty attending the use of Daniell's hygrometer. While the writer makes this remark, he is of opinion that the original form of Daniell's hygrometer is decidedly preferable to those proposed by Jones and Adie, from the circumstance that the dew-point is confined to a zone of the bulb only—the contrast between the clear and dim surface enabling the eye to mark the commencement of the deposition much more accurately. The invention of M. Pouillet, noticed by Professor Forbes in his Report on Meteorology, seems likely, from its construction, to combine the separate advantages of the above-mentioned instruments.

In a note at p. 107. of Mr Meikle's paper, already alluded to, there is a remark regarding the effect of exposing a wet thermometer to the sun. It happened, that, just before seeing that observation, the writer had been making one or two experiments on the same subject. Two thermometers, similar in every respect, and having their bulbs covered so as to possess the same power of absorbing and radiating heat, were exposed to the sun. The bulb of one of them was then moistened, and when the maximum cold was established, the temperatures of the dry and wet thermometers, and also those of the air and of the dew-point, were observed; the latter by means of Daniell's hygrometer. The results are exhibited in the accompanying Table.

Temperature of the dry Thermometer in the Sun.	Temperature of the wet Thermometer in the Sun.	Difference between the wet and dry Thermometers in the Sun.	Observed Dew-point.	Calculated Dew-point.	Error.	Temperature of the Air.	Calculated value of D.	Difference between the 3d and 8th Columns.
94.25	74.5	19.75	58.0	67.3	9.3	66.5	5.0	14.75
88.25	71.5	16.75	58.	64.8	6.5	66.0	4.6	12.15
80.0	69.75	10.25	58.25	65.3	7.05	66.5	4.9	6.35
76.25	67.5	8.75	58.75	62.7	4.05	66.5	4.7	4.05
83.0	69.5	13.5	59.0	65.3	6.3	67.5	5.2	8.3

According to these experiments, if the temperature of the dry and wet thermometers exposed to the sun be respectively denoted by T and T′; then the ratio of $T - T'$ to $t - t'$ becomes greater as $T - t$ increases. This, it is presumed, is what Dr Lardner intended, and not, as Mr Meikle supposes him to mean, that T′ is absolutely less than t'. It may also be concluded from these observations, that $T - T'$ is not proportional to $f_{T'} - f_{t''}$, but that it is more nearly so as $T - t$ decreases. The temperatures T′ and t' differ remarkably in this respect, that the latter is not affected by a current of air, while the former is considerably reduced by fanning the wet bulb for a few seconds. This might have been anticipated; for, in the former case, the increased circulation of the air, while it promotes evaporation, provides exactly in the same proportion the requisite heat; whereas, in the latter case, the source of heat being constant, the cold increases with the rate of evaporation. Besides, when T′ is greater than t, the air abstracts instead of contributing heat, and that in proportion to the quickness of its circulation.

Before concluding this rather desultory paper, the writer will make one remark regarding the process of evaporation. According to Dr Dalton, its intensity varies as $f_t - f_{t''}$ under similar circumstances. Now, if, as has been supposed, the air in contact with the wet bulb of the thermometer is saturated at the temperature t', there seems no reason to doubt that it is also saturated at t, when exposed to the surface of a body of water of that temperature. But it is evident, that, supposing the dew-point to be the same, and the circulation of air equally rapid, the rate of evaporation in the former case to that in the latter would be as $f_{t'} - f_{t''}$ to $f_t - f_{t''}$. Perhaps, therefore, the intensity of evaporation has been found to be as $f_t - f_{t''}$, from the depth of water generally used in making the experiments having been sufficient to preserve the temperature nearly at t. Or, it may be, that the difference of temperature $t - t'$, by increasing the circulation of air over the colder surface, compensates for the smaller quantity of moisture which each portion of air contains, and thereby renders $f_t - f_{t''}$ the true measure of the quantity evaporated in both cases. However, from the result of one experiment, in which two moist surfaces were exposed, one in the sunshine, and the other in the shade, the writer is led to think, that the temperature of the evaporating surface affects the process.

Marine Insects destroyers of Wood.

My dear Sir, Leith, 1*st August* 1834.

In my paper on the Limnoria terebrans, published in the Edinburgh Philosophical Journal for April last, I stated that the ravages of that animal were first noticed at the Bell-Rock, by Mr Stevenson in 1809: and this, I believe, has been the general understanding among zoologists for many years past. But I find, in the twenty-second volume of the Journal de Physique (1783), an account by the Abbé Dicquemare, of the destructive effects produced at Havre by the agency of a minute crustaceous animal, which, from the description and figures given by the author, I have no doubt was of the same species as that now called Limnoria terebrans. It is only with the view of adding something to our acquaintance with the history of an animal which is so interesting to us on account of the peculiarly destructive effects of its habits, that I now direct your attention to the paper by Dicquemare, above referred to. I enclose a translation of part of it, which you will oblige me by inserting in the next number of your Journal, as a correction of the misstatement contained in the account of the structure and habits of the Limnoria formerly published. I am, my Dear Sir, yours most faithfully,

John Coldstream.

To Professor Jameson.

"*Marine Insects destroyers of Wood* *.

* * * * "I have already described some of those marine animals, which, by destroying the surface of stones, oblige us to repair our harbour-sluices, and other works of the same kind. Let us now attend to those which perforate, nearly in the same manner, all sorts of wood. For a long time it had been the practice at Havre, to keep the fir-logs, of which masts are made for the ships of war, in a basin called La Bare-floride. Some years ago, it was observed that the surface of these logs was destroyed by marine insects, which had bored in the softest parts to the depth of an inch and a half, so that the diameter of each log was diminished by three inches: it was feared that the timber, being gradually softened by the action of the sea, would be entirely destroyed by these insects, or at least rendered useless. I know not whether any other measure was taken to remedy this evil; but soon after it was discovered, the Bare-floride was abandoned, and the logs removed to the ditches of the fortifications, and to another basin, where the mast-yard now is. Having heard, by chance, some time after the removal of the masts, of this destruction of such valuable materials, I sought for the enemy secretly, in places to which I had greater freedom of access than to the dockyard, and I discovered an old fishing-station which was infested by it, where ash, elm, and even oak, had long before been attacked, and in which the animals could be brought to view by removing the surface of the wood. I took a few from their holes, and placed them in sea-water, on different kinds of wood, recently cut. They lodged themselves in these within twelve hours. I had then no doubt of these being the agents in the destruction of the mast-timber. The animal is semicylindrical in form, or nearly so, covered with a scaly crust, divided unequally into fourteen segments, of which the largest are near the head, the smallest towards the tail, which is truncated, and perforated by the anus. The head is rounded, and terminated beneath by a kind of blunt beak, which, apparently, serves the animal in boring the wood, as it inserts its head first, and the *debris* which it forms passes between its feet under its belly. This debris sometimes covers the back under the form

* Extract from a paper entitled, "Extraits du portefeuille de M. l'Abbé Dicquemare ; Insects marines, destructeurs des bois."

of a moist white dust. The head bears four antennæ. There are seven feet on each side, and a double appendage. The general colour is a dirty white. I have obtained from some individuals five young ones completely formed; from others fewer; but a peculiarity worthy of notice here presents itself, namely, that I have seen some bearing eggs in different stages of development. The eggs and the young pass out by a triangular opening, situated about the middle of the ventral surface. The transparency of the crust admits of the young and the eggs being seen before they leave the body of their parent.

"To know our enemies is the first step—to determine the means of ejecting or destroying them is the last. To the latter point I have directed my attention, and have made some experiments bearing upon it. It would be very useful to have a police established in our ports, with the view of watching and preventing the devastations of these animals."

Critical Notices of various Organic Remains hitherto discovered in North America. By RICHARD HARLAN, M. D. * &c.

THE author of the following observations has been led to the undertaking by the urgent requests of many of his scientific friends in Europe.

It will appear in the ensuing pages that many eminent American naturalists have occupied themselves in the successful prosecution of this most interesting department of human knowledge; and yet very recent inquiries have satisfied us, that but a small fraction of what has been published on this subject in this country is adequately known to foreign naturalists. With the exception of some few of our scientific journals, the limited circulation in foreign countries of our scientific publications, is a subject of just complaint among Europeans, who interest themselves in works of this nature. We have been honoured with the personal acquaintance of hundreds of transatlantic savans, to whom we are well assured the following pages, imperfect as they necessarily are, will prove an acceptable offering;

* From the sheets of the volume of the Transactions of the Geological Society of Philadelphia, at present in the press, sent to the Meeting of the British Association at Edinburgh.

a motive in itself more than sufficient to impose upon us a more difficult task, and the end satisfactorily attained, is more than adequate compensation for the labour bestowed.

CLASS MAMMALIA.

ORDER PACHYDERMATA.

GENUS MASTODON, CUV.

M. *giganteum* or *maximus* of Cuv.

Recherches sur les Ossemens Fossiles, vol. i. 3d edition; S. L. Mitchill's edition of Cuvier's Theory of the Earth; Harlan's *Fauna Americana;* Cooper's Notice of Big-bone Lick, Am. Monthly Journal of Geology; Peale's Account of the Skeleton of the Mammoth, 4to.; Trans. Am. Philos. Soc.; Ann. of Lyc. Nat. Hist. N. York; Syn. TETRACAULODON, of Godman, Trans. Am. Philos. Soc. vol. iii. new series; MAMMOTH of the Anglo-Americans, "*Father of the Buffaloes*" of the Indians, *Animal d'Ohio* of the French.

Locality.—Confined to North America, principally in the valley of the Ohio, Big-bone Lick, Kentucky, but occurring in every state of the Union. Specimens of the teeth and bones in most cabinets of natural history. A skeleton nearly complete, both in the Philadelphia and Baltimore museums.

Place in the Geological Series.—Not yet ascertained with sufficient accuracy. According to De la Beche, "Geolog. Manual," occurring not later than his "Erratic Block Group," which also includes the elephant or mammoth, and five other species of mastodon, together with the genera Hippopotamus, Rhinoceros, Tapir, Cervus, Bos, Hyena, Ursus, Megalonyx, Megatherium, &c.

M. De la Beche remarks, p. 169:—"The relative age of the deposit in which the American Mastodons are found, can not be considered as satisfactorily ascertained. Some geologists are of opinion that these animals have disappeared more recently than is commonly supposed; that is, previous to the commencement of the modern group."

In most instances, there is sufficient evidence that these animals died, and left their bones to become fossilized in the precise situations in which they are now found; and that they have been brought from a distance or exposed to the action of running waters, which proves clearly that they have been destroyed subsequently to the action of those causes which formed the beds of gravel or diluvial detritus, in and upon which they are frequently found.

Not only are the teeth and bones of this animal unworn by the action of running waters, but the skeleton is not unfrequently discovered in a standing position, just as the animal has sunk into the marsh or mud, clay and sand. Such were those from Great Osage river.—Cuv. An. Foss. vol. i. p. 222, and in the skeleton noticed by Dekay and others. Ann. N. Y. Lyceum.

In some instances, it would appear that the stomach itself, with its vegetable contents, has been preserved. In a letter addressed to Cuvier, by the late Professor B. S. Barton, there is an account of the discovery of the remains of a mastodon in Withe county, Virginia, five feet and a half beneath the soil, on a bank of limestone. "But what renders this discovery peculiarly curious," continues M. Cuvier, Anim. Foss. vol. i. p. 219, "is that they collected from amidst these bones, a mass of semimasticated small branches, grasses, leaves, &c., among which it was thought a species of brier, still common in Virginia, was recognisable; the whole of this being enveloped in a kind of sack, which was regarded as the stomach of the animal, so as to leave little doubt that it consisted of the identical substances which the animal had devoured."

M. Cuvier further remarks, p. 222, "Indications of the sojourn or passage of the sea over the remains of these animals appear to be more rare than in the case of the elephant bones; I have never seen any remains of shells or zoophites on the bones of the great mastodon which I have examined."

During the exploration made by Lieut.-Col. H. S. Long, at Big-bone-lick, in 1824, great quantities of the remains of the elk and bison, both recent and fossil, were disinterred along with the bones of the mastodon.

From the facts and observations above detailed, together with others of a similar nature, that might be produced, we are led to the conclusion that the great mastodon, and other similarly situated animals, must have ceased to exist, at a period much more recent than is generally supposed. There are no evidences of its existence prior to the last general cataclysm. They may even have disappeared, together with the fossil elk, or moose, of Ireland, since the creation of man, though long previous to his earliest historical records.

Much has been written of late by inexperienced individuals, containing romantic descriptions of the remains of monstrous extinct quadrupeds, disinterred in various parts of our country, and which are calculated to produce much confusion when they attract the attention of the uninitiated. Thus, in excavating the canal around the falls of Ohio, the remains of portions of several individual skeletons of the mastodon were exhumed from the river banks, several feet beneath the surface of the present soil. Several pairs of tusks were arranged in a circle, within which were the remains of a fire and Indian tools; various other bones of the same were scattered about this focus, which had no doubt at some distant day been so arranged by the native Indians. A writer in one of the Kentucky papers presumed that all the bones were the remains of a single individual, with its immense mouth filled with enormous teeth, and armed with several pairs of huge tusks, and the whole animal of course sufficiently large to swallow a forest at a meal.

Another account of a huge animal disinterred at Big-bone-lick, 60 feet long and 25 feet high! has gone the rounds, being first published in our western papers, republished in those of the Atlantic cities, and finally transferred to those of Europe.

Of a character somewhat analogous are the descriptions of similar organic remains published by individuals supposed to possess higher claims to science, in the Trans. of the Am. Philos. Soc. vols. iii. and iv. At page 478. of the volume first referred to, there is a description of the under jaw of a young mastodon, with a figure. This relique was found in Orange county, New York, and is now in the New York museum.

The author of these remarks took an early opportunity to forward plaster casts of this jaw to the Geological Society of London, and to the Garden of Plants at Paris; and on his recent visit to the Jardin des Plantes, he was somewhat surprised to observe that he had already been in some measure anticipated by a foreign naturalist; this museum already containing the plaster cast of a portion of the lower jaw of a mastodon, sent from Germany to Baron Cuvier, soon after the completion of the last edition of his Animaux Fossiles. This specimen also contained the inferior tusk, about which so much has been subsequently written on this side of the Atlantic. The circumstance, however, elicited very little attention from the French professors. Yet it is on the existence of this inferior tusk in the jaw of the young individual from Orange county, above referred to, that the author has attempted to found a new genus of fossil quadrupeds, under the name of "TETRACAULODON."

Admitting that the genus had been established on a solid basis, the name is not a proper distinction, as it is equally applicable to the camel, hog, horse, deer, hippopotamus, fossil tapir, &c. all of which possess "*four tusks*," or tusks in each jaw.

It further displays inattention at least, if not ignorance of established usages among naturalists, to found a genus on the existence or absence of tusks in the lower jaw, independently of any other specific differences in the organization of other portions of the body. It is well known that the *males* of some species of animals possess tusks in one or both jaws, whilst the females of the same species are destitute of these teeth; just as some male animals possess *horns* whose females are destitute of them.

On the first appearance of this pretended "tetracaulodon," the inferior tusks were characterised by the best authorities on this subject to characterise the young of the mastodon; a subsequent examination, however, of numerous jaw bones of the mastodon in our various cabinets, soon demonstrated these inferior tusks to be mere sexual peculiarities; a goodly proportion of the jaws of the adult mastodons being found to be thus characterized, but in no one single instance were specific differences observable in the jaw teeth, maxillary bones, or any other portions of the skeletons.

Vol. iv. p. 317. of the Trans. Am. Philos. Soc. contains the lucubrations of a neophite in these matters, whose laborious observations as historian of the pretended "Tetracaulodon," would lead us to believe that he had clearly elucidated this subject, and had ended the dispute in question. The author occupies twenty-three pages of this quarto volume in letter-press, besides ten plates (with numerous figures). With a critical acumen and depth of research peculiarly his own, he has "actually discovered," from the same materials previously examined in vain by naturalists of less penetrating zeal, three "new species of mastodon," and two or three new species of "Tetracaulodon!!"

We repeat, that with others, upon whose judgment reliance is to be placed, we have repeatedly examined all the specimens of fossil bones noticed in the memoir above referred to, and have searched in vain for any *specific* differences, not to speak of *generic* distinction. The jaw-bones, together with the various teeth connected with them, or separately existing, display no peculiarities or varieties of structure, but such as are found to exist in similar portions of the skeletons of any other species of animal, recent or fossil, provided specimens are selected from individuals of different sexes, and different ages; no peculiarities or differences, in fine, worthy of notice, not fully described by Cuvier, in his Ossemens Fossiles, where he has given seventeen figures of the teeth and jaws of this species, and which are thus noticed in vol. i. p. 226, of his last edition:—"The differences of teeth of the '*Grand Mastodonte*,' consist principally in the number of their points, and in their length and breadth.

"I recognise three kinds of them: those nearly square, with three pairs of points.

"Rectangular, with four pairs of points. Others still longer, rather contracted posteriorly, with five pairs of points, and an odd spur.

"The first are generally found among those most used; I have observed many about half used, and several others worn down even to the neck of the tooth.

"The latter, on the contrary, are very rarely used, and are almost always, their posterior parts at least, entire.

"This circumstance at once indicates their relative position. The teeth with six points are anterior, and are the first to appear; those with eight and with ten points come after, and are situated behind. Direct observation has confirmed this induction."

Again, at page 227:—"The disposition then of the jaw teeth in the adult animal is as follows: Two with six points and two with eight points above, and two with six points and two with ten points below.

"But besides these eight molars which remain in the adult, there are others placed anteriorly to them in young individuals, which are shed successively.

"Thus the number of *effective jaw teeth*, which can be brought into action at one time, is eight in the young animal, and four only in the old.

"The *roots* of these teeth, like those of other animals, are not formed until after the *crown* is perfected. They are found complete only in such teeth as are already somewhat used."

After reading the above quotations from Cuvier's "Ossemens Fossiles," let any one attentively examine the *specific* characters of the "new genus and species" in the memoir above referred to, and judge for himself of their validity. But for such readers as may not have it in their power conveniently to refer to the memoir in the Transactions of the American Philosophical Society, we will now quote a paragraph in the author's own words, which affords a fair specimen of his notions of *specific* characters.

"The cabinet of *our* society (Am. Philos. Soc.) contains a portion of an inferior maxillary bone, which differs in its form from any of those hitherto described. This fragment consists of the chin, the right ramus, with the posterior molares, and a portion of the left ramus. The anterior molar has three denticules, with two points each; and a ridge posteriorly. The ramus of this jaw is *straiter* and more *cylindrical;* the *height* from the base to the edge of the alveolæ is less; the groove for the tongue *broader* and *shallower*, and the *direction* of the teeth less diverging than in the maxillary figured in plate xxiv.; the crowns of the teeth are also less elevated in the former than in the latter."—*Vid.* vol. iv. *Trans. Am. Philos. Soc.* p. 323.

"*Height, breadth, depth, direction*"*!* &c.

On comparing a number of human jaws together, scarcely two will be found to correspond exactly in these particulars.

The author of "Tetracaulodon" renown appears to pay no regard to the principles of classification; yet he ought to have been aware, that, whether "labouring for bread, or doing something for fame,"* writers on natural science are not permitted to swerve from established laws.

* Vid. "Tetracaulodon" Memoir. Trans. Am. Philos. Soc. vol. iv. p. 318.

We shall now close our observations on the remains of the *Mastodon giganteum*, by one more quotation from an authority which our author appears to esteem as conclusive in such matters; we allude to Mr William Cowper of New York, whom our author states "has been long engaged in the investigation of the history of the mastodon; has visited Big-bone-lick for the purpose of obtaining materials; and who, upwards of a year since, communicated to the Lyceum of Natural History of New York, some observations on the dentition of that animal."*

The conclusion to which Mr Cooper arrived after the fullest and most complete investigation of the most extensive collections of the mastodon bones, in this country, of the famous "Tetracaulodon" inclusive, will be found in the following paragraph, and needs no comment:—

"The '*Tetracaulodon*' of the late justly lamented Dr Godman, appears to me, after a careful examination of his specimen, to be another young individual, also of the common mastodon, but older than mine. I have stated my reasons for this opinion, in a paper on the dentary system of the mastodon, which I read to the Lyceum of Natural History, in April 1830. It appears, however, from recent observations, that the lower tusks which I supposed all the species to have possessed in their youth, were in some instances permanent during the advanced age of the animal. But whether this was a sexual characteristic, or merely an individual case of anomaly, of which I have seen other curious examples, *I cannot recognise more than one species of mastodon among the great quantity of their remains found in the United States, which have come under my observation, those just alluded to included.*"—*Vid.* "Notices of Big-bone-lick, by Wm. Cooper," Monthly Am. Journ. of Geology and Natural Science, conducted by G. W. Featherstonhaugh, vol. i. p. 158.

Finally, in the original memoir, descriptive of this supposed new genus, the author has himself expressed doubts of the validity of the characters on which it is proffered. He admits that the specimens he has described are the remains of a *young*

* Ut Supra, p. 336.

individual, and that "in every view, this animal so strongly resembles the mastodon, but for the singular difference of organization presented by the lower jaw and its tusks, *we could not avoid concluding we had obtained a young animal of that species.*" As regards this jaw itself and *molar* teeth, they certainly do resemble those of the MASTODON *giganteum*, as closely as the same parts in any young animal resemble those of the adult individual.

Note.—MASTODON *angustidens*, Cuv. and M. *tapiroides*, Cuv. Indications of the existence of these species in North America, were given in the *Fauna Americana*, pp. 212, 213. Subsequent observations have not yet further confirmed this indication.

GENUS ELEPHAS.

E. *primogenius*, Blumenbach and Cuvier.

Ossemens Fossiles, 2d edition, t. 1, p. 75, pl. 2; Harlan's *Fauna Americana*, and Journal of the Philadelphia Academy of Natural Science; Mitchell's edition of Cuvier's Theory of the Earth.

Locality.—In Europe these remains abound in the northern countries, also in France, Germany, and Italy. They are scattered over a vast range of country in North and South America. The frozen bodies of these animals have been found enveloped in ice on the north-west coast of America, as well as in Siberia. (*Vid.* Kotzbue's Voyages.)

Place in the Geological series.—The fossil bones of the elephant, although they are found to exist contemporaneously with those of the mastodon, rhinoceros, megalonyx, ox, deer, &c. would appear to have belonged also to a geological period more ancient than the last-named animals. According to Cuvier, "the isolated bones which are met with every where, are often observed to have marine animals attached to them, which establishes, in an incontestible manner, that since their dispersion they have been covered by the ocean, under which they have been buried a considerable time."

These remains are most generally discovered in the diluvial deposits which fill valleys, or on the borders of rivers.

It is probable that the immense mass of the fossil bones of the elephant scattered throughout the world, include the remains of several species; they are generally found in a state of decay, too imperfect for specific comparisons, the only perfect skeleton of this animal known, being that in the museum of St Petersburg, Russia. From observations that we have made on the fossil elephant teeth, several years ago, and published in vol. iii. of the Journ. Acad. Nat. Sciences of Philad. there can be little doubt but that two distinct species at least once existed in North America.

Specimens of the teeth and fragments of the skeleton of this species abound in our cabinets both public and private; more particularly in the cabinet of the Academy of Natural Sciences of Philadelphia, of the Philosophical Society, &c. &c. The Geological Society of Pennsylvania possesses an enormous fossil *os femoris* of this animal, found near Moorestown, New Jersey.

I have observed several specimens of elephant teeth, with the enamel arranged like that of the African elephant, which appeared to be fossilized; two of these are in the museum at Liverpool, one in my own collection. Their origin is uncertain, and all such are considered as apocryphal by Baron Cuvier.

Genus Tapirus.

T. *mastodontoides*, Harlan.

Fauna Americana, page 224.

Locality.—Big-bone-lick, state of Kentucky.

This fossil molar tooth displays considerable analogy to that of the "small fossil tapir" of Cuvier, differing only in the obliquity of the transverse eminences of the crown, and in the form of the disks of these, produced by detrition; but as subsequent and more extensive observation on the tapirs, in the museum of the "Jardin des Plantes," at Paris, has convinced us that similar differences in the form and direction of the transverse eminences are displayed in the different teeth of the same individual, we admit that little reliance is to be placed on them, when regarded as *specific* characters.

The molar teeth of the tapirs, kangaroo, and manatus, bear considerable analogy with those of the mastodon; they are covered in a similar manner with enamel, and furnished alike with transverse mamillary eminences in the young animal, which by detrition present disks, more or less resembling each other in the teeth of these different animals. Thus, a superficial observer might readily confound our fossil tooth with that of a young mastodon, was not its size at least one-half smaller than the smallest of the milk molars of the mastodon that have come under the observation of naturalists. Mr Cooper has casually remarked (vid. Notices of Big-bone-lick. Am. Monthly Journ. of Geology, p. 163, in a note), " Among these (the molars of the mastodon), I include one similar to the tooth, also from Big-bone-lick, described by Dr Harlan, as having belonged to an extinct species of tapir. That it is a young mastodon's tooth, is evident, I think, from the milk teeth still remaining in the head on which the supposed genus Tetracaulodon is founded, as well as from the small jaw above described."

It is difficult to conceive in what manner " the milk teeth remaining in the head" of this or that animal, could prove any thing concerning the nature of the tooth in question. Mr C. probably means to say that he compared the tooth of my fossil tapir with those in the jaws of young mastodons; I also have made similar comparisons, and have carried comparisons still farther. Taking the disputed tooth in question to Paris, I compared it in presence of naturalists skilled for their observation, with the teeth of the various tapirs preserved in Cuvier's collection of comparative anatomy. The tooth in question proves to have belonged to the anterior socket in the upper jaw of a tapir. The size of the tooth and the form and structure of its roots, distinguish it from those of the mastodon.

Place in the Geological series.—Contemporaneous with the fossil remains of the rhinoceros, elephant, mastodon, and other pachydermatous quadrupeds. Hitherto the fossil tapirs have been found only in Europe, whilst the recent species inhabit only South America and Mexico, the peninsula of Malacca and the isle of Sumatra.

Genus Equus.

E. *caballus.* The Horse.

The fossil remains of this quadruped are sparingly found both in North America and in Europe. The late Dr S. L. Mitchell, in his edition of Cuvier's Theory of the Earth, alludes to the fossil teeth and vertebræ of the horse, found near Neversink Hills, state of New Jersey.

The cabinet of the Academy of Natural Sciences, Philadelphia, also contains specimens, from the Valley of the Ohio or Mississippi, and we have to acknowledge the receipt of others from Col. I. J. Abert, of Washington, which were found in excavating for the Chesapeake and Ohio Canal, near Georgetown, D. C., not far from the Potomac River.

GENUS RHINOCEROS.

RINOCEROIDES *Alleghaniensis.*

Vid. Am. Monthly Journal of Geology, &c. where, under this name is figured and described a petrifaction which displays a considerable resemblance to the bony snout of the rhinoceros. The original specimen was sent to London, and the geologist who there examined it considered it of too doubtful a character to be admitted as a fossil remnant.

For ourselves, we are disposed to wait for further discoveries of this nature, previous to admitting the present specimen as a part of our fossil fauna. The specimen is no less singular or interesting to geologists, as demonstrating the very close analogy of a mere *lusus naturæ* of the mineral kingdom, if it be nothing else, to a portion of the animal skeleton. One argument applied to this and other similar specimens, in order to prove that it could not be considered as an organic relic, viz. the total absence of bony material, I conceive to be by no means conclusive; it being quite possible that the skeleton of an animal might be so circumstanced as to become completely mineralized, or changed from its original structure, just as we observe some vegetable structures to have changed. In ordinary instances, we are well aware the very reverse of this, as regards bones, is the fact; even the animal matter in fossil bones would appear to be, under some circumstances, as indestructible as the rock in which they are entombed, some of which are comparatively ancient, such as the saurian bones contained in the cuperose schists of Europe, and which were found on analysis to contain animal matter.

ORDER EDENTATA.

GENUS MEGATHERIUM, CUV.

M. *Cuvieri*, of authors.

Cuv. Ossemens Fossilis, 3d ed. vol. v. part 1st, p. 174, pl. 16; *Megatherium*, S. L. Mitchell, Ann. of the Lyc. N. York, vol. i. p. 58, pl. 6, and Wm. Cooper, ut supra, vol. i. p. 114, pl. 7, and vol. ii. p. 267; Harlan's *Fauna Americana*, p. 200.

Syn. *Animal du Paraguay: Animal incognita.*

Locality.—In South America, Paraguay, Lima, and in the vicinity of the River Luxan, three leagues south-west of Buenos Ayres, whence was obtained the skeleton nearly entire in the Madrid Museum. In 1823, remains of this fossil animal were first discovered in North America. Specimens from Skidaway Island, Georgia, in the cabinet of the New York Lyceum, a detailed account of which will be found in the volume of the Ann. of the Lyc. of N. York, above referred to, by the late Dr Mitchell, and by William Cooper, Esq.

Place in the Geological series.—The entire skeleton in the Madrid Museum was obtained on the borders of the river Luxan, South America, in 1789. The bank in which it occurred is only elevated about ten yards. These remains occur most commonly in the great plains of South America, particularly in the vicinity of Buenos Ayres; in that flat country, washed by the Panama and its tributaries, the bones being found sunk in the sand of the ancient alluvion, and sometimes, during very dry seasons, when the waters are low, they appear elevated above the surface; such was the position of those fine and valuable specimens of this fossil animal, recently brought to London, and presented to the Royal College of Surgeons, by Woodbine Parrish, Esq. The inhabitants of a remote district, we are informed, saw the pelvis of the animal appearing above the water, and throwing the lasso drew it on shore, carried it to the authorities of Buenos Ayres, from whom Mr Parrish obtained it, and subsequently sent 100 miles into the country, and with great exertions in dredging and turning off the water, succeeded in obtaining the greater portion of the skeleton, including the massive scaly cloak of the animal, with which it was covered somewhat in the manner of the Chlamyphorus and Armadillo, together with the caudal vertebræ, neither of which had ever been previously found. The os femoris is more than twice the thickness of that of the elephant. The bones of the feet are more than a yard long and twelve inches wide.

As regards the position of the remains of this animal discovered in North America, we are indebted for all the information we possess on the subject, to the observations of Mr William Cooper.—*Vide* Ann. of the Lyceum of New York, vol. i. p. 124.

"My inquiries have not, as yet, enabled me to give any very precise information respecting the locality of these bones, or the character of the formation in which they were found: their appearance, however, indicate that they have been overflowed by the sea; and they appear to have had one side imbedded in the earth or mud, while the other was washed by the salt water. They are thinly incrusted in some places with *Flustræ* and other zoophites, and have recent shells of the genus Balanus and Ostrea adhering to them. All are remarkably hard and heavy, and of a deep black colour; they do not retain any of their animal matter."

It is further stated: "These bones are still to be procured in great quantity, by some labour and expense, at the same place. Bones of the same kind may be obtained at two other places: one called *White Bluff*, also on the sea coast of Georgia, the other at some distance up the Savannah river."

We have only to remark, that the relative possession of the bones above referred to, as regards the waters of the ocean, appears to be due to accident, or recent exposure; the fractured surfaces of the bones still retain their angles, and in other respects display sufficient evidence that they have not been exposed to the action of running water; they apparently occupy the situation in which they were originally deposited.

GENUS MEGALONYX, Jefferson.

M. *Jeffersonii*, Harlan, Fauna Americana, p. 201.
M. *Jeffersonii*, Desm. Mammalogie, p. 336.
MEGALONYX, Jefferson.

Trans. of the Am. Philos. Soc. vol. iv., old series, p. 246, and Wistar, same vol. p. 526, pl. 1 & 2; Cuvier, Ossemens Fossiles, vol. v. part 1, p. 160, pl. 15, 3d edition.

The characters of the genus, being founded on a single molar, the only one known when Cuvier wrote his notice of this animal, will require revision.

Locality.—As yet only three localities for the remains of this interesting fossil are known—all in North America, viz.—Greenbriar county, state of Virginia, Big-bone-lick, and White Cave, Kentucky.

Place in the Geological series.—The first notice we have of the existence of this fossil genus is due to the late Mr Thomas Jefferson, former President of the United States of North America, who made them the subject of a memoir published in the Transactions of the Am. Philos. Soc. of Philad. vol. iv. p. 246.

Mr Jefferson compared these fossils with similar parts of the lion, to which he considered his animal congeneric. Plaster casts of these bones were sent to Baron Cuvier, who was thus enabled to estimate them at their true value, and to arrange them as pertaining to an animal of the tardigrade family, and as allied to the Megatherium.

The bones forwarded by Mr Jefferson to the American Philosophical Society, consist of " a small fragment of the femur, or humerus, a complete radius, a cubitus complete, broken in two, three claws, and a half dozen other bones, belonging to the foot or hand." A tooth and some other small fragments were subsequently obtained by Palisot de Beauvois, from the same cave.

Similar caverns to that in which these bones were found, exist in great numbers throughout the western part of Virginia, Kentucky, Tennessee, and other portions of the great valley of the Mississippi river, in the cavernous limestone, which here constitutes the surface rock for hundreds of miles. Some of these caverns, such as the mammoth cave of Kentucky, are several leagues in extent, and appear to have once been the channels of subterranean rivers, a circumstance which may possibly explain the comparative rare occurrence of organic remains within them.

Nitre, or saltpetre, is not unfrequently found adhering to the surfaces, and in the soil of clay, mud, or stalagmite formed at the bottom. The cave in Greenbriar county, Virginia, in which the Megalonyx bones of Jefferson were found, is thus situated, and was formerly extensively worked for saltpetre; the bones were buried two or three feet beneath the surface of the floor of the cave; they are completely fossilized, very dense and heavy, of a white colour, and are still very well preserved.

MEGALONYX *laqueatus*. HARLAN.

Journal of the Academy of Natural Sciences of Philadelphia, vol. vi. p. 269, pl. 12, 13, 14. Also, in the American Monthly Journal of Geology and Natural Sciences, 1831 and 1832. "Description of the Jaws, Teeth, and Clavicle of the MEGALONYX *laqueatus*." By R. HARLAN, M. D. vol. i. p. 74. pl. 3.

Locality.—"White Cave," Edmondson county, Kentucky, on the southern bank of Green river, fifty miles in a direct northern line from the Ohio river, and about half a mile from the mouth of "Mammoth Cave."

The specimens of this highly important organic remain are at present the property of John Price Wetherill, Esq., and have been by him liberally deposited in the Cabinet of the Academy of Natural Sciences of Philadelphia. They consist of the following portions of the skeleton, viz.: two claws of the fore-feet; a radius, humerus, scapula, one rib, and several remnants of ribs, os calcis, tibia, portion of the femur; four dorsal and one lumbar vertebræ, a portion of a molar tooth, together with several epiphyses, the bones being portions of the skeleton of a young animal, all occasionally imperfect at their extremities.

Recent bones of the bison, the deer, the bear, and a metacarpal bone of the human finger, accompanied the specimens, and were stated to have occurred in the same cave with those of the Megalonyx; the latter, strictly speaking, are not fossilized; they retain a very considerable portion of their animal matter, are much more brittle and lighter than recent bones; most of the articulating surface are still covered, more or less, with cartilage, tinged of a yellow colour. One of the unguial phalanges still retains the horny covering or nail, also tinged of a yellow ochreous colour. These bones are stated to have been found on the surface of the floor of the cave, uncovered by earth or stalagmite. Not only the teeth, but other portions of the skeleton, were found, on comparison with similar parts of the Jefferson Megalonyx, to present differences estimated of sufficient importance to constitute distinctive specific characters.

In the same collection there are a humerus, nearly perfect, nineteen inches long, and a metacarpal bone of an adult animal of the same species, subsequently disinterred at Big-bone-lick; these are of a deep black colour, of a dense and solid structure, like the soundest of the mastodon bones.

Still more recently, a large collection of fossil bones obtained from Big-bone-lick, have been exhibited in the city of New York; among them were observed, the jaw, teeth, clavicle and a tibia of the right leg of the *Megalonyx laqueatus*, the same that are described and figured in the American Monthly Journal of Geology and Natural Science of Philadelphia.

Place in the Geological series.—Contemporaneous with the Big-bone-lick fossils, and probably also with bones of the caverns of Germany, England, France, &c.; but judging from the appearance of the Megalonyx bones from White Cave, Kentucky, they are still more recent than those of any extinct fossil species hitherto discovered, with the probable exception of the "Elk of Ireland." I have seen, in the museum of the Dublin Society of Natural History, the lower portion of the fore-leg of a cervine animal, with the skin, hair, and hoof simply desiccated, found in the peat-bogs of Ireland, along with the bones of the fossil elk, of which animal it was supposed to form a part; it bears the closest analogy to the same part of the North American moose-deer, (*Cervus alces*, Linn.)

Most of the original specimens of the fossil bones of this extinct species are in the cabinet of Mr J. P. Wetherill, deposited in the Hall of the Academy of Natural Sciences, Philadelphia. Plaster casts have been taken, and the specimens thus multiplied are contained in many European cabinets; among others, we have furnished the "Jardin des Plantes," Paris, and the Geological Society of London.

ORDER RUMINANTIA.

Genus Cervus.

C. *Americanus*, Harlan,

Fauna Americana, p. 245; Wistar, Trans. Am. Philos. Soc. vol. i. new series, p. 375, pl. 10, fig. 4. *Fossil Elk* of the United States of North America.

The present fossil species was first established on a mutilated skull in the cabinet of the American Philosophical Society, presented by the late President Jefferson; the species appears to be nearest related to the common elk of North America (*Cervus Canadensis*, Briss.), although it displays several characters which distinguish it from all other species, living or fossil, hitherto introduced into the systems. Judging from the skull, the animal was larger than our common elk.

Locality.—The bones of this fossil elk are not unfrequently found in the celebrated morass near the Ohio river, Big-bone-lick, in company with the bones of the mastodon. Some fossil bones were observed by Dr Bigsby in Canada, which, from designs in his possession, are judged to have belonged to the fossil elk.

Place in the Geological series.—Such as indicated by the above mentioned locality.

Genus Bos.

B. *bombifrons*, Harlan,

Fauna Americana, p. 271; skull of a fossil Ox, Wistar, Trans. Am. Philos. Soc vol. i. new series, p. 379, pl. xi. figs. 11 and 12.

This fossil species displays considerable analogies in such portions of the skeleton as are known, to the bison (*Bos Americanus*) or common buffalo of the United States; but the form of the skull, and peculiar disposition of the horns in the fossil, distinguish it as a nondescript species.

Locality.—Big-bone-lick and other similar morasses. The fossil teeth of this species are very common.

B. *latifrons*, Harlan,

Fauna Americana, p. 273; Cuv. Anim. Foss. 1st ed. vol. iv. pl. 3, fig. 3, *Broad-headed Fossil Ox.*

This specimen, a mutilated skull of large dimensions, is in the cabinet of the Am. Philos. Soc. Philad. It resembles in many respects the skull of the *Auroch*, (*Bos urus*, Cuv.) The horn is twenty-eight inches in circumference at its base.

Locality.—State of Kentucky. According to Cuvier, similar fossil skulls have been found in Europe, on the borders of the Rhine, near to Cracovie in Bohemia, &c.

B. *pallasii*, Dekay,

Ann. of the Lyc. of Nat. Hist. N. York, vol. ii. p. 280, pl. 6.

Among a large collection of fossils presented to the Lyc. of

Nat. Hist. of N. York, by the late Dr Mitchell, is a bovine skull, which Dr Dekay has minutely described as above referred to, and compares it with the skull of the *Bos moschatus*, which it most nearly resembles.

Similar fossils have been occasionally found in Siberia, which it is supposed were probably carried there in ice from the American continent. *Vid.* Cuv. Anim. Foss. vol. iv. pl. 3, fig. 9 and 10; also, Ozeretskovsky, Memoirs of the Royal Academy of St Petersburg, 1809-10.

Locality.—" New Madrid, on the banks of the Mississippi river, ejected by the earthquake of 1812."

ORDER CARNASSIERS, Cuv.

Genus Trichecus, Lin. The Walrus.

T. *rosmarus*, Lin.

Cuv. Recherches sur les Ossemens Fossiles, tom. v. Cooper, Ann. Lyc. Nat. Hist. of N. York, vol. ii. p. 271.

Only slight indications of the existence of a fossil morse or walrus have been hitherto observed in any country; a few molar teeth, and some fragments of bone found in France, have been referred to this species. In the work above alluded to, Mr Cooper has given a lucid account of a mutilated fossil skull in the cabinet of the Lyceum, which, without doubt, belonged to the walrus; the skull is remarkably hard and heavy, the tusks having become almost agatized. On comparison with similar portions of the T. *rosmarus* of Linn., it displayed strong specific affinity.

Locality.—Accomac county, Virginia.

Place in the Geological series.—Atlantic tertiary? along with the fossil bones of Cetacea.

Captain Beechey brought home with him from the north-west coast of America the fossil vertebræ of an unknown extinct mammalia; on comparing the fossil casts of these vertebræ with the amphibious tribe of Carnassiers in the museum of the Garden of Plants, the fossil appeared referrible to one of this family.

ORDER CETACEA.

GENUS MANATUS.

The fossil ribs and vertebræ of a large species of manatus are contained in the cabinet of the Academy of Natural Sciences of Philad. *Vid.* Journ. Acad. Nat. Sciences of Philad. vol. iv. p. 32, "Notice of Plesiosaurus," &c., by R. Harlan, M. D.

Locality.—Eastern coast of the United States, Atlantic Tertiary, Georgia, New Jersey, western shore of Maryland, &c.

The cabinets of the Academy of Natural Sciences of Philadelphia, and the Lyceum of Nat. Hist. of N. York, contain ribs and vertebræ, &c. of fossil whales, or

CETACEA *proper.*

Such remains are by no means of rare occurrence in the Atlantic tertiary.

In the estuary of the Mississippi river, numerous remains of recent whales are daily discovered, the bones being observed projecting from the mud. The skull, jaws, and teeth of a very large spermaceti whale were thus obtained by the fishermen some few years since, and carried to New Orleans, where they were palmed on the public as the fossil remains of some enormous nondescript monster. Numerous theories and ingenious speculations arose on the subject, and were gazetted from one end of our country to the other. The bones were purchased at considerable expense, and exhibited through the United States. The late Dr Godman produced a memoir on the occasion, and announced to the American Philosophical Society "the discovery of the remains of the largest '*saurian* fossil' ever heard of," and proposed to designate it by the name "Megistosaurus," which stands at the present day registered on the minutes of the Society. The animal was represented as possessing a long horn several feet in length, projecting from the side of its head. The fame of this wonderful monster found its way even into the European newspapers—when lo! and behold! on the first examination of these remains by a naturalist, they were immediately perceived to form a portion of the skeleton of an immense recent spermaceti whale; the pretended horn being nothing more than one of the intermaxillary bones sawed of, and fitted on the jugal bone of the right side.

Thus the remains at last met with an honourable burial, on the eve of departure for England, where they would no doubt have astonished the natives, both as to the gigantic fossil productions of the New World, and as specimens of the critical acumen of our scientific observers.

The articulating surface plates, or epiphyses of the vertebræ of whales, are not unfrequently found separate, both fossil and recent; they have occasionally given rise to false notions, and to the dissemination of error. The "New Fossil Genus" of Raffinesque, named "*Nephrosteon*," (*Vid.* Atlantic Journal,) and the bone on which the genus is constructed, and which this author considers as a portion of the head-plate of a fossil saurian, has no other foundation than one of these epiphyses from the remains of a recent spermaceti whale.

CLASS AVES.

The fossil remains of birds are of rare occurrence in any country, but particularly so in America; only one specimen clearly ascertained has fallen under our immediate inspection. This consisted in a femur, imperfect at its upper extremity, of an individual allied to the genus *Scolopax*, obtained by the late S. W. Conrad. The bone appears to be perfectly mineralized. Cab. of the Acad. Nat. Sciences, Philad.

Locality, from a "marl-pit" in New Jersey.

CLASS REPTILIA.

ORDER CHELONIA.

Fossil bones and breastplates of turtles are not unfrequently discovered in the Jersey "marl-pits," but are too imperfect to admit of any satisfactory arrangement into genera or species; they occur principally in the Atlantic secondary. Specimens preserved in the Cab. of A. N. S. and Lyc. Nat. Hist. N. York.

(To be concluded in our next Number.)

On the Structure and Uses of the Mammary Glands of the Cetacea. By Professor Traill. Communicated by the Author.

In perusing M. Geoffroy Saint Hilaire's interesting *Fragment* on the structure and uses of the mammary glands of the cetacea, it occurred to me that there was an obvious and easy method of ascertaining how far we must admit the distinction attempted to be established by that eminent naturalist, between the process of lactation in terrestrial and aquatic mammalia.

M. Geoffroy Saint Hilaire describes the formation of the void within the mouth of the young animal, and the flow of the milk, in the usual manner; but he conceives, that in the act of swallowing, it is essential that the air enter by the nostrils to supply the place of the mouthful of milk that is passing into the stomach. His words are, "*Pendant que l'air ambiant, libre désormais de traverser la route des narines, s'en vient remplir l'arrière-bouche, et rendre à la langue et à ses parties accessoires leur première aptitude à la deglutition du bol alimentaire,*" p. 74. The want of this supply of air, he contends, must prevent animals immersed in water from continuing the reiterated efforts of sucking and swallowing without quitting the teat; and he arrives at the conclusion, that "*Le cetacé ne tète donc point.*"

On reading these remarks, I immediately tried whether I could not suck and swallow with my nose closed; and found that the process was not attended with any difficulty. I also ascertained that the same could be done when the face was immersed in a basin of water.

But in order to render all the circumstances of the experiment as similar as possible to those affecting the lactation of aquatic mammalia, I furnished myself with a bladder, containing half an English pint of milk, and connected it with a short glass tube, surmounted by a cow's teat. I entered a bath, and plunging the apparatus and my whole body below the water, I found that I could suck and swallow, in successive efforts, as readily as in the open air. There was so little difficulty, that, on removing the cow's teat, I sucked up and swallowed all the milk during four immersions, without any violent effort.

When such is the case with man, whose power of submersion rarely exceeds half a minute, why should we doubt that cetacea, accustomed to remain submersed in their own element for a period of not less than fifteen or twenty minutes, without the necessity of respiration, are able to reiterate the action of sucking and swallowing while adhering to the mother's teat?

I may add, that to avoid any accidental chance of error, I was assisted in these experiments by my friend Dr Cumming of Chester.

10 Albyn Place, *September* 18. 1834.

Astronomy.

The Professorship of Practical Astronomy in the University of Edinburgh, which became vacant by the death of Dr Blair in the year 1828, has now been filled up by the appointment to that office of Thomas Henderson, Esq. lately astronomer at the Cape of Good Hope: he has also the title of *Astronomer-Royal* to Scotland; and is directed by his commission to carry on a series of observations in the Edinburgh Observatory on the Calton Hill, expressly with a view to the improvement of astronomy, geography, and navigation, the results to be reported twice in the year to his Majesty's Government. From the zeal and intelligence manifested by Mr Henderson when at the Cape observatory, where he made at least 10,000 observations in the course of fourteen months, and the fact, that he has obtained his present appointment expressly by the recommendation of the Astronomical Society of London, and eminent individual members of that body, we anticipate most favourably for the reputation of our Edinburgh Observatory. Mr Henderson has an assistant, and has at his command a *mural circle* six feet in diameter, a *transit instrument* about eight feet in length, with an object-glass six and a-half inches in diameter, and an *azimuth and altitude circle* of which the vertical circle is three feet in diameter, and the azimuth circle two feet. These three instruments, the first and last of which were made by Troughton and Simms, and the transit instrument by Repsold of Hamburgh, (the object-glass by the late Frauenhofer), are inferior to no instruments in the world.

On the Upper Greywacke Series of England and Wales.

TABLE of the Order of Stratified Deposits which connect the Carboniferous Series with the older Slaty Rocks in the Counties of Salop, Hereford, Montgomery, Radnor, Brecknock, Caermarthen, Monmouth, Worcester, Stafford, and Gloucester. By R. I. Murchison, Vice-Pres. Geol. Soc. and Royal Geog. Soc., F.R.S., F.L.S., &c. &c.

The history of the nature and succession of the Upper Transition Rocks has long been a desideratum among geologists, and no attempt has hitherto been made to point out their order. Having discovered, in the above-mentioned counties of England and Wales, a series of deposits replete with organic remains, which seemed to afford a complete key to a large portion of the rock formations hitherto much neglected, the author has been led to devote himself specially to the study of this branch of the science during the last three years, during which he has coloured geologically all the sheets of the Ordnance Survey which relate to this region, and has collected a great number of organic remains which were previously unknown to naturalists. Struck by the nature and objects of these operations, and their importance to the completion of the geology of England and Wales, many noblemen and gentlemen have requested the author to publish his views in a separate work, accompanied by a map, sections, and figures of the organic remains in each formation. He has consented to prepare such a work, and Mr John Murray, Albemarle Street, has offered to receive the names of all persons who will join as subscribers. This work will describe not only the formations *beneath* the coal measures, but also all those yonnger deposits which overlie them in the country under review, as well as all the associated Sienitic and Trappean Rocks and their effects. But the chief value of this work will consist in the establishment of complete types of the succession of these deposits in the descending order, commencing with those strata which are already well known, and terminating downwards with those slaty rocks in Wales and Cumberland, with which Professor Sedgwick has rendered himself so familiar, and of which he is about to undertake the illustration; the author having, conjointly with the Professor, examined such portions of the country as have enabled them to connect clearly the upper system of the one, with the more deep-seated rocks investigated by the other. The annexed tabular view is offered, in the mean time, as a synopsis of part of the labours of Mr Murchison.

	Formations.	Maximum approximate thickness.	Subdivisions.	Lithological Characters.
	Carboniferous Limestone.	Feet. 500 ?	Limestone. Shale.	
	Old Red Sandstone.	10,000.	*a.* Red conglomerate and sandstone. *b.* Cornstone and argillaceous marls. *c.* Tile stones, &c.	*a.* Quartzose conglomerate overlying thick-bedded sandstones. *b.* Red and green, concretionary limestones, with spotted argillaceous marls and beds of sandstone. *c.* Flaggy, highly micaceous, hard, red and green sandstone.
UPPER GREYWACKE SERIES.	I. Ludlow Rocks.	2000.	*d.* Upper Ludlow rock. *e.* Aymestry and Sedgeley limestone. *f.* Lower Ludlow rock.	*d.* Slightly micaceous, grey-coloured, thin-bedded sandstone. *e.* Subcrystalline or grey and blue argillaceous limestone. *f.* Sandy, liver, and dark-coloured shale and flag, with concretions of earthy limestone.
UPPER GREYWACKE SERIES.	II. Wenlock and Dudley Rocks.	1800.	*g.* Wenlock and Dudley limestone. *h.* Wenlock and Dudley shale.	*g.* Highly concretionary grey and blue subcrystalline limestone. *h.* Argillaceous shale, liver and dark gray-coloured, rarely micaceous, with nodules of earthy limestone.
UPPER GREYWACKE SERIES.	III. Horderley and May Hill Rocks.	2500.	*i.* Flags. *k.* Sandstones, grits, and limestones.	*i.* Thin-bedded, impure, shelly limestone, and finely laminated, slightly micaceous greenish sandstone. *k.** Thin-bedded, red, purple, green, and white freestones. Conglomeritic quartzose grits. Sandy and gritty limestones.
UPPER GREYWACKE SERIES.	IV. Builth and Llandeilo flags.	1200.		*l.* Dark-coloured flags, mostly calcareous, with some sandstone and schist.
	V. Longmynd and Gwastaden Rocks.	Many thousand feet.	Comprising all the slaty system of South Wales.	*m.* Hard, close-grained, gray greenish and purple sandstone. Red and gray quartzose conglomerate. Slate-coloured and purple schists. Coarse slates: little or no calcareous matter.

* The sandstones (*i*, *k*, and m,) pass into quartz rock in the vicinity of certain trap rocks (Wrekin, Caer Caradoc, Blaen Dyffrin garn, &c.,) as will be explained in the Work alluded to.

Characteristic Organic Remains.	A few Localities.
Corals differing in species from those of the formations below. Producta hemisphærica. P. Martini. Spirifer triangularis, &c. (Defence and teeth of fishes. Clee Hill, Salop.)	Lilleshall, Steeraways, Orleton, south end of Clee Hills, and Llanymynech, Shropshire. The edge of the South Wales Coal-basin.
a. No organic remains observed.	*a.* Caermarthen and Brecon Fans, SE. part of Black Forest, Brecknockshire; flanks of the Brown Clee Hill, Shropshire.
b. Fishes of undescribed genera.	*b.* Central and north. parts of Herefordshire: eastern part of Brecknockshire: Whitbach near Ludlow, and base of the Clee Hills, Shropshire: Tenbury and Alveley, near Kidderminster, Worcestersh.
c. Avicula, n. s. Pileopsis, n. s. Small Orthocera. Small Ichthyodorulites?	*c.* Pontarlleche, Cwmdwr, Caermarthenshire: Clyro Hills, Brecknocksh.: Tinmill Copse, near Downton Castle, Herefordsh.: Clun Forest, Shropsh.
d. Avicula, n. s. A. retroflexa, Hisinger. Atrypa (Dalman), n. s. Cypricardia, n. s. Homonolotus Knightii, new genus, Konig. Leptæna lata, V. Buch. Orthis, several new species. Orbicula, 2 new species. Orthocera, several new species. Pleurotomaria? 2 new species. Turbo, n. s. Gigantic serpuline bodies, &c. &c.	*d.* Ludlow Castle, Whitcliffe, Munslow, Diddlebury, Larden, Shropshire: Croft Castle, Mortimer's Cross, Titley, Kington, Fownhope, Stoke Edith, Herefordshire: West flanks of Malvern and Abberley Hills, Worcestershire: West flank of May Hill: Presteigne, Pain's Castle, Radnorshire: Trewerne Hills, Corn-y-fan, Brecon, Usk Castle.
e. Pentamerus Knightii, M. C. Pileopsis vetusta, M. C. Bellerophon, n. s. Lingula, n. s. Atrypa, n. s. Terebratula Wilsoni, M. C. Calamopora fibrosa, Goldf., and a few other corals.	*e.* Aymestry, Croft Ambry, Gatley, Brindgwood Chase, Downton on the Rock, Herefordshire: Yeo Edge, Shelderton, Norton Camp, Dinchope, Caynham Camp, Shropshire; Sedgeley, Staffordshire.
f. Phragmoceras, new genus, Broderip, 3 sp. Asaphus caudatus. Ichthyodorulites? small. "Cardiola," Brod., new gen. 2 sp. Nautilus, n. s. Spirulites, 2 n. s. Pentamerus. Atrypa galeata, Dalm. n. s. Pleurotomaria, n. s. Orthocera pyriformis, n. s.; and several others.	*f.* Escarpments of Mocktree and Brindgwood Chase, Gatley, and valley of Woolhope, Herefordshire: Marrington Dingle, Westhope, Hopedale, and Long-Mountain, Shropshire: west side of Abberley and Malvern Hills: escarpments in Montgomery, Radnor Forest, Brecknock and Caermarthen shs.
g. Corals and Crinoidea in vast abundance. Bellerophon tenuifascia, M. C. Euomphalus rugosus. Eu. discors. Conularia quadrisculata, M. C. Pentamerus, n. s. Natica, n. s. N. spirata, M. C. Leptæna euglypha, Dalman. Spirifer lineatus, M. C. S. n. s. Terebratula cuneata, Dalm. Producta depressa, M. C. Orthocera, sev. sp. Asaphus caudatus. Calymene Blumenbachii. The Bar Trilobite and others.	*g.* Lincoln Hill, Benthall and Wenlock Edge, Shropshire; Burrington, Nether Lye, near Amestry, Nash, near Presteigne, Old Radnor: Pwll-Calch, Caermarthenshire: valley of Woolhope, Ledbury, and west side of Malvern Hills: east side of Abberley Hills, Dudley, Worcestershire: Long Hope, near May Hill, and Tortworth, Gloucestershire: Prescoed and Cil-na-Caya, near Usk.
h. As. caudatus variety, C. Blumenbachii. Lingula, n. s. Orthis, n. s., and others. Cyrtia trapezoidalis, Dalm. Delthyris, n. s. Orthocera, n. s. O. annulata, M. C. Crinoidea, &c.	*h.* Buildwas, Hughley, Wistanstow, and Clungunford, Salop: escarpments in Montgomery, Radnor, Brecknock, and Caermarthen shires: west flank of Malvern Hills, Alfrick, Worcestershire: centre of Wren's Nest, Dudley, &c. &c.
i. Pentamerus lævis, M. C. P. oblongatus, n. s. Leptæna, n. s. Pileopsis, n. s. Orthis Callactis, Dalm., and several new species. Terebratula, n. s. Tentaculites and Crinoidea, abundant, } Corals rare.	*i.* Banks of the Onny, near Horderley, Acton Burnell, Chatwall: the Hollies near Hope Bowdler, Cheney Longville, Acton Scott: east flank of Wrekin and Caer Caradoc, Salop: Eastnor Park, Obelisk, and centre of Woolhope Valley, Hereforshire: May Hill, and Tortworth, Gloucestersh.
k. Nucula, n. s. Pentamerus, n. s. Trilobites of underscribed species, including the genus Cryptolithus of N. America, and 14 species of the genus Orthis have been found, including O. aperturatus, Dalm., all differing from those of the overlying formations.	*k.* Horderly, Hoar Edge, Long-Lane, and Corton, Shropshire: Ankerdine Hill, Old Storridge, Howlers Heath, SW. of Malvern Hills, Worcestersh.: May Hill, Gloucestersh.: and the same localities as *i* in Shropshire: Powis Castle, Guilsfield, and Alt-y-maen, Montgomerysh.: Castell Craig, Noeth Grug, and Llandovery, Caermarthenshire.
l. Asaphus Buchii. Agnostus, Brongn., undescribed Trilobites of three species; differing from those of the overlying formations.	*l.* Rorington and Hope, near Skelve, Shropshire: Llandrindod and Wellfield, near Builth, Radnorshire: Tan-yr-Alt to Llandeilo, Caermarthenshire.
m. Few organic remains have yet been observed in this great system, but it is underlaid by fossiliferous strata and limestones, which will be described by Professor Sedgwick.	*m.* The Longmynd, Linley, Haughmond, Lyth, Pulberbatch Hills, Salop: Gwastaden, east of Rhayader, Radnor, &c. &c.: hills west of Llandovery, Caermarthenshire.

N.B.—No vegetable remains, except the *Fucus serra* (Brongn.), and some very imperfect fragments of Fucoids? have been found in any portion of the deposits below the carboniferous limestone, nor has any coaly matter been detected, except small nests of Anthracite. In this list of organic remains, only such individuals have been mentioned as are characteristic of each subdivision. Others, as, for example, the *Terebratula affinis*, M. C. (*Atrypa reticularis*, Wahl.), which occurs in several formations, have been omitted in this short Table, but will be given hereafter in a full and descriptive account of all the organic remains. None of the species of corals or shells are identical with those found in the true carboniferous limestone.

[We recommend this important tabular view, and consequently the projected work of Mr Murchison, to the attention of geologists of every country. His investigations have been conducted in a manner worthy of general imitation. To the geologists of Scotland, who know the different greywackes to the north and the south of the Frith of Forth, as described by Professor Jameson in his lectures and writings, the tabular view will prove very useful.]

CORRECTIONS AND ADDITIONS IN THE LAST NUMBER.

On Mr Meikle's Paper on finding the Dew-point, &c.

Page 103, line 3 from bottom, *for* a fourth *read* four-fifths

On Mr Don's Paper on Ericaceæ.

Pag. 152, lin. 15, Gemmatio nuda. *lege* Gemmatio sæpè nuda.
...... 152, ... 21, Gemmatio nuda. *lege* Gemmatio imbricata.
...... 154, ... 14, Massoni. *lege* Monsoniana.
...... 158, ... 26, *adde* Semina obovata, lævia, raphe elevatâ callosâ.
...... 160, ... 12, Synonymon *adde* Menziesia cœrulea, *Pursh, fl. Amer.* i. p. 265.
...... 160, ... 6, a basi *adde* Capsula oblonga, submembranacea. Placenta axilis, angusta, prismatica. Semina subrotunda, ventricosa, lævia, gilva, chalazâ parvâ, intensiùs coloratâ.

PROCEEDINGS OF THE BRITISH ASSOCIATION AT EDINBURGH IN SEPTEMBER 1834.

President.

SIR THOMAS MAKDOUGALL BRISBANE, BART., K. C. B. F. R. SS. L. & E., &c. &c. &c.

Vice-Presidents.

SIR DAVID BREWSTER, and the REV. J. ROBINSON, D. D. Astronomer-Royal, at Armagh.

General Secretary.

REV. W. VERNON HARCOURT, F. R. S. & G. S.

Treasurer.

JOHN TAYLOR, ESQ. F. R. S., M. G. S.

Assistant Secretary.

PROFESSOR PHILLIPS.

Local Secretaries.

JOHN ROBISON, ESQ. Sec. R. S. E., and PROFESSOR FORBES.

Monday, 8th September.

THE meeting was opened in the George's Street Assembly Rooms, at eight o'clock in the evening, by the President of the former year, Professor Sedgwick of Cambridge, with an address.—The Association, he said, had exalted him to a position of great honour, which, at the time, he prized as above all other power, and to which he would ever look back with the greatest delight. From this situation he was now on the point of retiring but he did so, however, with feelings of exultation, if such feelings could be deemed appropriate on such an occasion, as indeed they were, inasmuch as the trust he held was about to devolve upon a gentleman of great eminence, and who was more equal to the undertaking than the person who now addressed them. The Association was not one which was in a bankrupt state, or which was falling off in either power or members, but one which, on the contrary, was going on increasing in strength and in power, and producing effects on the philosophic world which would be felt in generations yet unknown, and promote the best interests of humanity. Perhaps what he had said might be considered enough, and if he concluded here by expressing his gratitude for past honours, it might be as well. But he hoped they would not think he was needlessly clinging to that Chair, if he still detained them for a few moments longer, by touching on one or two topics connected with the Institution. He begged, then, to congratulate them on their increasing numbers. Even were it to be supposed that motives of vanity had brought many of them together, yet under that view there was reason to rejoice that the public feeling was with them; for unless they had the public sympathy, it was impossible, even for a philosophical body, to go on with success. The professor then proceeded to advert to the original institution of this Association; which, he said, had been started by a set of independent men, with the best intentions, and with the most sober views of future good and of the success of their scheme, hardly knowing what constitution to give it, and never dreaming of the glorious success which it had now obtained. The first meeting had been held at York; the second at Oxford, where a large accession of numbers was experienced; the third at Cambridge, where the numbers still further increased; and now it had reached the Scottish capital, where an addition had been made to their number beyond all precedent. He then went on to remark on the many circumstances connected with this city, which tended to endear it to himself and others,—especially that of its having given birth to so many illustrious philosophers, men who had investigated the obscurest relations of physical science, and disentangled its phenomena.

The learned Professor then proceeded to expatiate on the advantages of an association of this nature. On his way hither he had the good fortune to meet with M. Arago, the perpetual Secretary of the French Institute, and Dr Vlastos from Greece. M. Arago, in the departments which he had cultivated, was inferior to none in Europe. To meet with men like these,—to breathe the same atmosphere,—to partake of the same sentiments, and enjoy their conversation and their friendship, were enough to justify the institution of that Association, were there no other advantages. But there were many other circumstances which pointed out the use of these associations, among which was the power of combination. How feeble and how powerless was man when alone; and, on the other hand, how powerful and how forcible was he when acting in combination! The brute elements could then be brought fully into subjection, and himself raised in the scale of intellectual beings,—for as he gained knowledge he gained power. Thus great good arose from combination, and from collision with men of even conflicting opinions, and a power of concentration was obtained which was unknown to a person acting by himself. It was said in opposition to this, that the greatest philosophic works had been achieved in private This was so far true; but the first germ of such works was not suggested in private, but originated from the authors' having mingled with men of similar pursuits. He instanced La Place, whose intercourse with men of letters and science must have greatly aided him in disentangling the phenomena of nature,—for in all such cases when a point of experiment was reached, it was always necessary to call in experimental men. The learned professor next adverted to the published Transactions of the Association, in illustration of the uses of the Association. Last year a discussion had arisen on the aurora borealis, which had been found to be connected with electrical phenomena, thus becoming a link in physical science. Soon after that a beautiful arch across the heavens was seen simultaneously at various parts by, he believed, most members of the Association; and experiments having been made by Dr Dalton of Manchester, as to the altitude of the arch, it was found to be about forty miles above the surface of the earth.

The Association, at last meeting, had also recommended that experiments should be made on heated bodies long kept in fusion: in pursuance of which certain bodies were at present in the furnace, and would probably be uncovered for examination in the course of ten years. Now, but for this Association, these experiments would never have been attempted. He also alluded to certain observations which had been made at Greenwich, which were in a raw unreduced state, but which, on application being made to Government by some members of the Association, some hundred pounds had been obtained to assist in preparing for the benefit of the world. Observations on the tides were also in progress, from which great good was expected. The Professor next proceeded to combat the objections which had been urged against such associations. They were said to be dangerous in their tendency, but he denied that the investigation of truth could ever be injurious to mankind;—this was a libel on the God of Nature; for, instead of impugning any of the grander truths, they would, on the contrary, be more and more corroborated. He urged most strenuously upon the Association the necessity of keeping in mind the objects of its institution, and to confine their researches to dead matter, without entering into any speculations on the relations of intellectual beings; and he would brand as a traitor that person that would dare to overstep the prescribed boundaries of the institution. If the Society should ever be broken up—which God forbid—he would predict that it would happen by some members imprudently and daringly passing its boundaries. Before concluding, he made some complimentary remarks on the fame which this city had always enjoyed as a seat of learning and science; and in allusion to the monuments of Burns, Playfair, and Stewart, which had been erected on the Calton Hill since last he (Mr S.) had visited this city, said, that, although he did not disapprove of monuments to warriors who had fought the battles of their country, yet he viewed with more pleasurable interest such monuments as those — memorials as they were of peace—and with which was connected neither shrieks nor wailings, heart-breakings nor blood—they were the visible representation of those feelings in which they participated.—He then moved, that, in accordance with the resolution of the General Committee last year at Cambridge, Lieut.-General Sir Thomas Brisbane do take the Chair.

Sir Thomas Brisbane having taken the Chair, addressed the audience in the following terms:

After the distinguished Nobleman who first filled the situation I have now the honour to hold, and after the two celebrated Professors who successively followed him—men of pre-eminent talent and gigantic intellect, and who are recognised as such all over Europe—I must confess, I appear before you with the utmost diffidence, and must claim your indulgence; for I feel that I am quite inadequate to discharge in a becoming manner, the various and important duties belonging to the situation which I now occupy,—which would require almost universal knowledge,—and for which I am indebted to the kind indulgence of the Association, and not to any merit of my own,—but for which mark of high distinction I beg to express my unbounded gratitude. I must also acknowledge the great obligations I feel towards my learned and eloquent predecessor, for the kind, though unmerited compliments, he has been pleased to bestow upon me. Although Edinburgh cannot boast of the accommodation, or ever attempt to rival the boundless hospitality, the Association experienced at the English Universities, still I feel confident my countrymen will yield in no degree to them in giving the Association the best possible reception, with a desire to uphold the national character for hospitality, as all ranks must hail with enthusiasm and much gratification, men who have done so much towards the extension of the boundaries of human knowledge and comfort, as those who are now assembled in this ancient capital, which has given birth to individuals who have done honour to human nature, and amongst whom many could have been found who would have adorned this Chair, in the place of the humble individual who has now the honour to address you—indeed I need not go farther than my nearest learned friend on the right (Sir David Brewster), one of our Vice-presidents. It is but justice to the Principal and Professors of the University to say, they have done all in their power to afford every accommodation, and the free use of the class-rooms and other public rooms in the College, which are admirably adapted for the sectional and other meetings. Other public bodies have not been backward in the same offers. The noblemen and gentlemen in the neighbourhood have expressed their desire to promote the objects of the Association.—After the luminous *exposé* we have just heard from my learned predecessor, he has left me no subjects to touch upon. Professor Forbes has kindly undertaken the task of detailing the labours of the Association since our last meeting; and I need not say it could not be in better hands. I shall therefore not waste the time of the meeting, but conclude by congratulating the Association on its prosperous condition, and I have no doubt it will go on progressively until its beneficial effects shall be felt, not only over the whole of the united empire of Great Britain, but even throughout Europe, or the globe we inhabit.

Mr Robison (one of the Secretaries), to whom the Association is deeply indebted for its triumphant success in Edinburgh, next gave a detailed account of the arrangements which had been made for the accommodation of the members, and the general order of the business for the week.

Professor Forbes (the other Secretary) then delivered an address on the occasion of the opening of the fourth general meeting of the British Association in Edinburgh, which is printed in the preceding pages of this number of the Edinburgh Philosophical Journal.

Tuesday, 9th September.

MORNING—IN THE UNIVERSITY.

The different sections having been organized, business commenced in each of them at 11 A. M. The following abstract will convey to our readers a short but correct account of their proceedings.

SECTION A.—MATHEMATICS AND GENERAL PHYSICS.

Chairman—Rev. W. WHEWELL.

Deputy Chairmen—Rev. Dr LLOYD. Rev. Dr ROBINSON.

Secretaries—Professor FORBES. Professor LLOYD.

Committee.—M. Arago. Mr Baily. Sir David Brewster. Sir Thomas Brisbane. Rev. Mr Bowstead. Mr Cooper. Lieutenant Drummond. Professor Forbes. Rev. Mr Greswell. Professor Hamilton. Mr Henderson. Mr Hopkins. Dr Jackson. Dr Knight. Rev. Dr Lardner. Rev. Dr Lloyd. Professor Lloyd. Professor Moll. Mr Murphy. Lieut. Murphy. Rev. Mr Peacock. Dr Pearson. Professor Powell. Mr Ramage. Mr Rennie. Rev. Dr Robinson. Mr Robison. Professor Stevelly. Professor Thomson. Professor Wallace. Mr Wharton. Mr Wheatstone.

The Section having met, and the Reverend Dr Lloyd, Provost Trin. Coll. Dublin, having been called to the Chair, Mr Whewell read the report of Mr Challis on the theory of capillary attraction.

After some observations from Dr Robinson on the subject of the report just read, Professor Moll noticed the experiments of M. Lenck, published in Poggendorff's Annalen, and which appeared to have been overlooked by Mr Challis.

Mr Whewell made some observations on the subject of the same report, particularly with reference to the constitution of comets, and to the conclusions of M. Poisson respecting the variation of density of the fluid near the surface in capillary phenomena, and the atomic constitution of bodies generally. With reference to the latter part of the subject, Professor Hamilton stated, that the atomic discontinuity, considered by M. Poisson as necessary in order to the physical explanation, did not appear to him mathematically requisite to the investigation of these laws.

Mr Sang made some remarks on the effect of changes on the surfaces of bodies, in illustration of the principles adverted to.

M. Arago spoke on the theories of Laplace and Poisson on molecular action, and observed that the conclusion of M. Poisson, representing the change of density near the surface of fluids, could be put to an experimental test by the observation of the angle of complete polarization at these surfaces.

Professor Powell then read a paper on the repulsion produced by heat, as established by the contraction of Newton's rings, when heat was applied to the glasses.

Professor Stevelly mentioned some familiar facts of a different kind, in confirmation of the result obtained by Professor Powell. Mr Sang also made some remarks on the same subject, and Professor Forbes stated the result of his repetition of the experiment of Professor Powell, and alluded to the explanation given by himself of the vibrations of heated metals in connexion with the same subject.

Mr Addams described certain phenomena of mobility in the particles of precipitated silica when heated, which he seemed to think were due to the same repulsive force.

Mr Whewell read a letter from Mr Hailstone, accompanying a table of barometrical observations, taken at short intervals.

Professor Forbes remarked, that the momentary oscillations of the barometer, adverted to by the author, had been already noticed by other observers, and made some observations on the atmospheric waves, whose existence was doubted by the author.

Professor Forbes read a short communication from Mr Christie, on a remarkable meteorological phenomenon observed by him at Woolwich.

Mr Baily mentioned a similar phenomenon observed by him, and described by Mr Faraday; and Mr Whewell and Professor Powell noticed the fact of the observation of the same phenomenon by other observers.

Section B.—Chemistry and Mineralogy.

Chairman—Dr Hope.

Deputy-Chairmen—Dr Dalton. Dr Thomas Thomson.

Secretaries—Professor Johnston. Dr Christison.

Committee.—Dr Daubeny. Dr Turner. Dr Lloyd. Rev. W. V. Harcourt. Thos. J. Pearsall, Esq. William Hatfield, Esq. Dr Traill. Dr Gregory. Dr Thomas Clark. Thomas Graham, Esq. Arthur Connell, Esq. Luke Howard, Esq. Dr Apjohn. Charles Tennant, Esq. Charles Macintosh, Esq. William West, Esq. Richard Phillips, Esq. George Lowe, Esq.

The Chemical Section having met at 11 A. M., the proceedings of the Committee were read over, and Dr Hope took the chair, in conformity with the request of the Committee.

The recommendations of the Chemical Committees of the former meetings of the Association were then read over and severally considered.

In regard to the specific gravities of the gases, Dr Dalton stated that he was not prepared with any results on this subject in a state to be laid before the Section.

Dr Turner made some remarks on his experiments on atomic weights, published in the Transactions of the Royal Society of London, and on the conclusion he had come to that the atomic weights of bodies cannot be represented by whole numbers. On this subject a discussion of some length took place, in which many members took part.

Mr Johnston and Mr Harcourt gave an account of the state of the experiments they have respectively undertaken, on the comparative analysis of iron in the different stages of its manufacture, and on the effects of long-continued heat.

In regard to the purity and specific gravity of mercury, Dr Thomson stated that he considered the mercury as imported into this country to be pure, and that he believed the determination of the specific gravity of mercury, as given by Mr Cavendish, to be correct, as it agrees with that of Mr Crichton of Glasgow, lately deduced from a very great number of careful experiments continued throughout a whole winter.

Dr Daubeny, on the subject of the seventh recommendation, viz. an inquiry into the nature and quantity of the gases given off from thermal waters, and the effects of season and other circumstances on them, referred to his late paper in the Philosophical Transactions, of which he gave an account, and announced his expectation of continuing his researches.

Mr Low made some observations on the products collected in the chimneys of smelting and other furnaces, and promised some farther remarks on the recommendation of the Chemical Section of last year relative to that subject.

Section C.—Geology and Geography.

Chairman—Professor Jameson.

Deputy-Chairmen—Major-General Lord Greenock. G. B. Greenough, Esq. President of the Geological Society of London.

Secretaries—Professor Phillips. T. Jameson Torrie, Esq. Rev. J. Yates.

Committee.—Dr Buckland. Dr Boase. J. Bryce, Esq. Major Clerke. Professor Sedgwick. Colonel Silvertop. H. T. M. Witham, Esq. William Smith, Esq. J. Taylor, Esq. W. C. Trevelyan, Esq. Rev. J. Yates. R. I. Murchison, Esq. William Hutton, Esq. Charles Lyell, Esq. L. Horner, Esq. J. B. Pentland, Esq. R. Griffith, Esq. William Copland, Esq. Dr Hibbert. R. Stevenson, Esq. Lieutenant Murphy. William Clift, Esq. Sir Thomas Dick Lauder. Sir George Mackenzie. Rev. Dr Fleming. Dr Traill. Captain Maconochie. Henry Woolcombe, Esq. Dr E. Turner. S. P. Pratt, Esq. M. Agassiz. William Nicol, Esq. Rev. Mr Turner.

Professor Jameson in the Chair.

The recommendations of the Committee of Geology at Cambridge were read, and the researches undertaken in compliance therewith reported to the meeting. The communications presented to the Section were enumerated, and, in consequence of a resolution of the Section, the discussion on some views advocated by Dr Boase, relating to Primary Rocks, was commenced by this gentleman stating some of these views, and finally limiting the subject of the discussion to the question, whether Primary Slates are, or are not, stratified?

In proposing this specific question, Dr Boase stated the difficulties which occurred *in limine*, as to the meaning of the term stratification, and noticed the various definitions of different geological authors, depending on considerations of the parallelism of certain surfaces of division, on the curvatures and contortions existing in them, on the alternation of beds of different mineral characters, and on the circumstances observed with regard to the inclination of layers. He stated, from his own observations, and referred to a recent publication for detailed descriptions, the fact, that all the characters usually considered as characteristic of stratification in primary slates, do also occur in granites, and that the essential structural characters of these slates are continued into the neighbouring granites, thence inferring, that no real structural distinction existed between the granites and the primary slates.

Professor Sedgwick entered on the question at considerable length, and stated that, sixteen years ago, after a visit to Cornwall, he had been led to adopt the opinions now held by Dr Boase, but that his subsequent experience, and more especially his investigations in North Wales and Cumberland, had produced a considerable change in his views. He discussed the principles upon which questions of this nature ought to be considered, and particularly stated the impossibility of giving definitions which would be applicable to all cases. The Professor stated as his belief, that it is impossible to separate the lower and higher parts of the slate series; and is of opinion, that it is generally quite practicable to distinguish between true stratification and laminar structure. He mentioned particularly the assistance to be derived from the striped appearance so common in the slates of Wales, and noticed the distinctions between certain structures of rocks, and the characteristic marks of true stratification. Professor Sedgwick expressed it as his opinion, that the laminar structure has been produced at a period subsequent to that of the formation of the slaty rocks in which it occurs.

Mr Greenough directed the attention of the Section to cases where the lines of structure are not parallel to the seams of stratification, and instanced the sandstone rocks at Crichton Castle and in the neighbourhood of Roslin. He expressed his conviction, that Lehmann and Arduino's definition of primitive rocks should still be adhered to, and regretted that the innovations in the terminology of geology had increased the difficulties of the present discussion.

Mr Lyell explained the definition of stratification he has given in his Principles of Geology, and made some farther remarks on the essential characters of stratification.

Professor Phillips observed, that the views of geologists on the defects of the stratification and other characters of primary strata, were commonly tinctured with peculiarities depending on the limits of their inquiries, and that while the symmetrical system of division common in primary rocks of all kinds, was the most attended to by one class of observers, and traces of stratification were regarded by others, results apparently conflicting were drawn from the examination of the same country. It was important to attend to the real distinctions between the two systems of structure, because each was due to a proper cause; but it was absolutely essential to the production of a right general conclusion, that partial truths thus disclosed should be contemplated together.

Mr Yates noticed some localities in Cornwall, where the separation of the schistose rocks and granites is very marked.

Dr Buckland expressed his acquiescence in the views of Professor Sedgwick and Professor Phillips, and referred the Section for a fuller account of his opinions on the subject to his work now in the press.

Dr Boase shortly replied, and referred to his recently published treatise on Primary Geology for a more extended account of his views.

A portion of Dr Rogers' Report on the Geology of North America was then read, and a general account of the contents of the remainder given by Professor Phillips. Illustrative maps were exhibited.

Lord Greenock presented for distribution copies of a view of the Castle Hill section.

Section D.—Natural History.

Chairman—Professor Graham.

Deputy-Chairman—Sir William Jardine, Bart.

Secretaries—William Yarrell, Esq. Professor Burnett.

Committee.—G. A. Walker-Arnott, Esq. Monsieur Agassiz. Dr Adam. C. Babington, Esq. Robert Brown, Esq. D. C. L. W. Christie, Esq. Dr Coldstream. Allan Cunningham, Esq. J. Curtis, Esq. David Don, Esq. J. P. Duncan, Esq. Dr R. Dickson. Dr Daubeny. Rev. L. W. P. Garnons. Dr Greville. B. D. Greene, Esq. Boston, U. S. Professor Henslow. Dr Hooker, Professor Jameson. Rev. L. Jenyns. Dr Richardson. J. F. Royle, Esq. P. J. Selby, Esq. F. R. S. E. Colonel Sykes. W. Spence, Esq. Richard Taylor, Esq. Dr Wasse. James Wilson, Esq. William Thompson, Esq.

A report on the recent and present state of Zoology, by the Rev. Leonhard Jenyns.

An account of excursions in the neighbourhood of Quito, and towards the summits of Chimborazo and Pichincha, by Colonel Hall.

Professor Agassiz next delivered some very interesting observations upon the different species of the Genus Salmo which frequent the various rivers and lakes of Europe, of which the following is an abstract:—

The genus Salmo, as it has been established by Linnæus and Artedi, or, I ought rather to say, by Rondeletius, has supplied Cuvier with the type of a peculiar family, in which he has retained the generic characters of Linnæus, viz. one dorsal fin with soft rays, and a second one, which is rudimental and only adipose. Cuvier places this family in his order *Malacopterygii Abdominales*, between the *Siluridæ* and the *Clupeæ*; and he subdivides it, on just grounds, into a great number of generic sections, which comprehend a vast variety of exotic species. In my work on the fishes of Brazil, I have added several new kinds to those which Cuvier established; and am of opinion that, in the natural classification, it is now absolutely necessary to unite the family of the Clupeæ to that of the Salmones, since the only difference we find between them consists in the presence or absence of an adipose fin; an organ assuredly too insignificant to constitute the distinctive character betwixt two families, and the less so, as there are some genera of the family which possess it, whilst in others it is completely awanting, as for example, in the Siluridæ. We may with equal truth affirm, that all the real Salmones of Cuvier have not this adipose fin, for in many species of the genera *Serrasalmus*, *Myletes*, &c. it is composed of rays which are truly osseous.

Restricted to the limits which Cuvier has assigned to it, the genus Salmo comprehends all the species of which the body is somewhat lengthened, the mouth large, and supplied with teeth, which are conical, pointed and formidable, implanted into all the bones of the mouth, that is to say, into the interior maxillary bones, both superior and inferior, into the vomer and palate bones, into the tongue itself, and into the branchial arches. The margin of the upper jaw is formed by the interior and superior maxillary bones, and constitutes only a single continuous arch, as in the higher classes of animals; a conformation which in the class of fishes is found only in the Clupeæ. It is also singular that the number of branchial rays is seldom exactly the same on the opposite sides of the head, the number varying from ten to twelve. The pectoral and the ventral fins are of a middling size; the latter placed about the middle of the belly, opposite to the dorsal; at their base, and along their insertion, there is a fleshy fringe, somewhat similar to the long scales which are found on the most part of the Clupeæ. The caudal fin is attached to a very fleshy root, and is moved by very powerful muscles.

This elastic spring is to these fishes a most powerful lever; when wishing to leap to a great height, they strike the surface of the water with a kind of double stroke. By this means they overcome obstacles which appear insurmountable, and leap over nets which are intended to confine them. The most formidable waterfalls can scarcely arrest them. The several species of this genus are found in the northern and temperate regions of Europe, Asia, and America.

The fishes of this family are very ravenous, and feed principally upon the larvæ of aquatic and other insects, and of the small crustacea; they also devour fishes of a smaller size. Their alimentary canal is short, but the stomach is proportionally long and strait. At its pyloric extremity may be observed a great number of appendices, which are connected with the pancreas, and to which is generally, but erroneously, applied the name of cæcum. The swimming-bladder of the whole of them is very large, and opens into the œsophagus near the bottom of the gullet. Though I cannot here enter into the subject very fully, I may in a word state, that I am persuaded that this organ ought to be regarded as the lungs of fishes;—that the circulation of the blood in these animals has been inaccurately interpreted, when it is supposed that in their heart there may be traced a pulmonary course; also, when their branchiæ have been identified with the lungs of other animals; and, finally, when their great dorsal artery has been considered as analogous to the aorta of the mammalia.

Most of the salmon varieties reside in fresh waters; in summer they pay a visit to the sea, and do not mount up again to the rivers, unless for the purpose of there depositing their spawn. It is sufficiently remarkable that most of our species deposit their ova in November and December, and that the young fry of course comes into existence in the coldest season of the year. From this circumstance we may suppose that it is owing to this habit of enduring intense cold in the first days of their existence, that they can subsequently support all that variety of temperature to which they are soon to be exposed.

In proportion as the genus Salmo is now circumscribed within its natural limits, so much the more is it difficult to characterise the various species; and I have no fear of being contradicted when I affirm, that since no one has devoted himself to their history, so no one has yet succeeded in determining, with any degree of precision, their distinctive characters. The greatest obstacle to the solution of this problem arises from our ignorance of the accuracy of the characters hitherto employed to distinguish the several species, the one from the other. We have especially attached ourselves to the form of the head, and to the arrangement of the colours; but these two particulars are much too variable to supply precise characters. As to the variation in the colour, we may say it is infinite. There are, however, two circumstances which especially modify the tints of the salmon tribes, namely, their age, and the season of the year. The younger fish are, in general, much more spotted than the older ones, whose tints become more and more uniform. The Salmo *Hucho*, for example, with violet spots more or less distinct, has, when young, large black transverse bands upon the back, down to the middle of its sides. In the second and third years of its existence, these bands break up into black spots, less deep in colour, and they disappear more and more, till in its latter years the fish acquires a colour which is almost uniform. The Salmo *lacustris* of Linnæus, when young, has large black and ocellated spots upon all the superior parts of its body; but from the third year they diminish, and ere long they entirely disappear. The Salmo *Umbla*, so long as it is young, is of a uniform greenish-yellow colour, with the abdomen white, and at a later period of life these tints assume a deeper hue—of a more lively green, and finally pass into a blackish-green. The abdomen soon becomes silvery-white, afterwards yellow and orange coloured, and then of a golden lustre. Its flanks are very soon adorned with ocellated yellow spots more or less distinct, but ere long there are no spots at all. In the Salmo *Fario*, the spots vary even more. In the young they are found yellow, green, brown, even black and violet, also black and red, but in the long run they all entirely disappear. I have also noticed that the seasons have an influence on the colours of the different kinds of Salmo. It is during the autumn, and at the time of the greatest cold, that is to say in October, November, December, and January, that their tints are most brilliant, and the colours become more vivid by the accumulation of a great quantity of coloured pigments. We might almost say that these fishes bedeck themselves in a nuptial garb, as birds do. The colour of their flesh varies according to the nature of their aliment. This family of fishes feed, as we have said above, especially upon the larvæ of aquatic insects, and of small crustacea. It is in the waters which contain the most of these last, that the most beautiful salmon trout are found. Direct experiments which were made in lakes, have proved, to my satisfaction, that the intensity of the colour of the flesh arose from the greater or smaller quantity of gammarinæ which they had devoured.

As to the structure of the head, it offers, in the opercular bones, in the surface of the cranium, and in its proportions relative to the whole body, very excellent characters; but those, on the other hand, which are taken from the proportional length and size of the jaw-bone, are of no value at all, since the lower jaw is longer or shorter than the upper, according as the fish opens or shuts its mouth; and this consideration introduced into the characteristics of the family, has very considerably contributed to multiply the institution of species. The hook which forms the jaw of the Salmo *Salar* is not even a peculiar characteristic of this species, since the full grown males of all the species of the genus present a crooked prolongation of their lower jaw, to a greater or less extent.

It results, then, from these observations, that the different species of the salmon family, far from being confined within the narrow limits of some small bodies of fresh-water, are, on the contrary, very widely distributed. They also thrive in all climates, at least in all elevations above the surface of the ocean, whether in fresh water or in salt. It is also true, they prefer those situations where the water is limpid.

Possessed of these facts, which I had collected with the most minute and jealous precautions, I have tried to determine the various species which are found in the fresh waters of the Continent; grounding my examination upon the study of the interior organization, and upon the particulars already determined which the integuments present concerning the structure of the scales. I have also introduced the shape of the body, and the proportional size of its internal parts, as important accessaries to the description of the species. Of course I cannot enter at present into the details of a minute description. This, in fact, is the investigation of which I propose to give an account in my treatise upon the fishes of the fresh waters of central Europe. I must here confine myself to a short statement of the results I have obtained.

It is a very singular fact, that those fishes which are the most widely distributed, and those which are most highly prized, are precisely those whose natural history is the most perplexed. The opinions, too, which are so widely extended concerning their geographical distribution, are not at all in unison with the real state of things. There scarcely exists a country to the which some peculiar species of salmon has not been assigned, and I may add, that even in the *Regne Animal* of Cuvier, we find many nominal species, which are not even local varieties, as I purpose ere long to demonstrate. The cupidity of the fishermen, the rivalry of epicures, and the fastidiousness of the palate of salmon eaters, have, without doubt, contributed to spread these opinions upon the narrow limit assigned to the haunts of the species of the Salmon. There is especially a famous variety, in the annals of epicurism, over which the greatest possible obscurity has been cast,—it is *l'ombre chevalier*, the char, or alpine trout. After having attentively examined the continental varieties, I with eagerness availed myself of the opportunity I have lately enjoyed, of examining near their native haunts several species of this genus which are found in England. Through the kindness of Sir William Jardine and of Mr Selby, I have also had an opportunity of examining all those which they have collected from the Scottish lakes; and the result has been, that I have succeeded in determining the perfect identity of many of them with the species found in other countries in Europe; while, on the other hand, I am convinced, by the observations of these naturalists, that there are species peculiar to Scotland. Nevertheless, it is true, that systematic authors, from having allowed themselves to fall into error by the prevailing opinions circulated concerning the vast multitude of species of this genus, have been investigating the characters of a great number of merely imaginary species. But to the philosophical naturalist, the distinctions upon which they support themselves in establishing the differences of species, are quite insufficient, and the comparative examination of these pseudo species admits of very different results.

I am convinced that all the fish belonging to this family, on the Continent, may be reduced to the six following species:

1. Salmo Umbla, *Lin.* the Char of England,—the Ombre Chevalier of the Lake of Geneva,—the Rötheli of Swiss Germany,—and the Schwarz Reutel of Saltzburg.

Synonyms: Salmo Salvelinus, *Lin.* Salmo alpinus, *Lin.* Salmo Salmarinus, *Lin.* (but not the Salmo alpinus of *Bloch.*)

This fish is found in England and Ireland, in Sweden and Switzerland, and in all the southern parts of Germany.

2. Salmo Fario, *Lin.*—the Trout of brooks,—Common Trout, —Gillaroo-trout,—and Par.

   Synonyms: Salmo sylvaticus, *Schrank.* Salmo alpinus, *Bloch.* Salmo punctatus, *Cuvier.* Salmo Marmoratus, *Cuv.* Erythrinus, *Lin.*

   It is found as extensively as the first species.

3. Salmo Trutta, *Lin.* Sea-trout,—Salmon-trout. It is the same as the Salmo Lemanus of Cuvier; and the Salmo albus of *Rondeletius.*

   It is found as extensively as the two preceding.

4. Salmo lacustris, *Lin.* The same as the Salmo Illanca, and the Salmo Schiffermulleri of *Bloch.*

   Found in the lakes of Lower Austria, and in the Rhine above Constance.

5. Salmo Salar, *Lin.* *The true Salmon.* The Salmo *Hamatus* of Cuvier is the old fish, and the Salmo *Gadeni* of Bloch the young fish.

   Found in the Northern Seas, whence it ascends the rivers even as far as the Swiss Lakes.

6. Salmo Hucho, *Lin.* Of the same species as the preceding. Peculiar to the waters of the Danube.

It results then, from these observations, that the different species of the Salmon family, far from being confined within the narrow limits of some small bodies of fresh-water, are, on the contrary, very widely distributed. They also thrive in all climates, at least in all elevations above the surface of the ocean, whether in fresh water or in salt. Nevertheless, they prefer those situations where the water is limpid.

I may state, that it is not upon vague data that I have drawn these several conclusions; but upon the actual examination of living specimens of all the species that have been named, and that I have myself studied them in the localities where they were caught.

SECTION E.—ANATOMY AND MEDICINE.

Chairman.—Dr ABERCROMBIE.

Deputy-Chairmen.—Sir CHARLES BELL. Professor CLARKE.

Secretaries.—Dr ROGET. Dr WILLIAM THOMSON.

Committee.—Dr Alison, Dr Arnott, Sir G. Ballingall, S. D. Broughton, Esq., Dr J. Campbell, Professor Clark, William Clift, Esq., Dr Davidson, Dr Hodgkin, Dr Holme, Dr Home, Dr Maclagan, Dr Roget, James Russell, Esq., Dr Thomson, Dr A. T. Thomson, Dr William Thomson, Prof. Treviranus, Dr Turner, Dr Yelloly.

The Medical Section met this day at eleven o'clock, when, on the motion of Dr Yelloly, Dr Abercrombie was requested to take the Chair. Dr Yelloly then proposed that the Section should appoint Dr Abercrombie Chairman for the ensuing meeting, and Sir Charles Bell, and Professor Clarke of Cambridge, Deputy-Chairmen; which proposal was unanimously agreed to, and Dr Abercrombie took the chair accordingly.

A letter from Dr Roupell was laid before the meeting, expressing his regret, that circumstances had interfered with his pursuing the subject of the Operation of Poisons introduced into the animal economy, of which he had been appointed at the last meeting to undertake the investigation, in conjunction with Dr Hodgkin; but assuring the Section that he had not neglected the subject, and that he trusts next year to exhibit the drawings he has made, and to lay before the Section a joint memoir on the subject. It was proposed and unanimously agreed to, that Dr Roupell and Dr Hodgkin should be requested to prosecute their investigations, and lay the results of them before the next meeting of the Association.

Mr Broughton read to the Section the results of the experimental inquiry respecting the sensibilities of the nerves of the brain, which, at the last meeting of the Association, Dr Marshall Hall and he had been requested to undertake. The most important new fact stated in this paper was, that the 8th pair of nerves, in several experiments on horses and asses, appeared quite insensible to ordinary irritations, and no muscular contraction was observed to succeed its irritation; but when the trunk of the horse was *compressed* with the forceps, or when it was cut through, and the lower portion of the upper segment compressed in that way, a struggle, an inspiration, a cough, and effort to swallow, were always observed. At the conclusion of the paper, Sir Charles Bell made some observations on the impartial spirit in which such inquiries ought to be conducted, and on the importance of their being prosecuted, under the guidance of a minute knowledge of anatomy, and complimented the authors of the memoir on the evidence it bore of these two qualifications.

Dr Alison read a notice of some experiments by Dr J. Reid, illustrating the connection of the irritability of muscles with the nervous system, with observations by himself. The result of these experiments was (in confirmation of the doctrine of Haller and of Wilson Philip, and in opposition to that lately propounded by Mr J. W. Earle), that the irritability of muscles, after it has been exhausted, or at least greatly diminished by galvanic irritation, may be restored by rest; although all their nerves be divided, and they be rendered incapable either of sensation or of voluntary motion. Some remarks having been made by Dr Allen Thomson on the sources of fallacy to which he conceived experiments of the kind detailed are exposed, it was resolved to appoint the following gentlemen a Committee, to witness the repetition of these experiments by Dr Reid, viz. Dr Bright, Mr Clift, Dr A. Thomson, Dr Hodgkin and Dr Alison.

Dr Alison stated that he had a communication to lay before the Section, on the vital powers of arteries leading to inflamed parts; but that he was desirous, as Mr Dick had at present some horses which would afford an opportunity of shewing the phenomena, to which he wished to call the attention of the meeting, that a committee should be appointed to witness these phenomena previously to the reading of the communication. A committee was accordingly proposed and appointed, consisting of Mr Broughton, Mr Bracey Clark, Mr Dick, Dr Fletcher, Mr Clift and Dr Alison.

The meeting then adjourned till Wednesday the 10th, at eleven o'clock.

Section F.—Statistics.

Chairman—Sir Charles Lemon, Bart.

Deputy-Chairmen—Col. Sykes. Benj. Heywood, Esq.

Secretaries—Dr Cleland. C. Hope Maclean, Esq.

Committee.—Howard Elphinstone, Esq. Rev. E. Stanley. J. E. Drinkwater, Esq. Rev. W. Whewell. The Earl Fitzwilliam. Sir John Sinclair, Bart. Sir Thomas Acland, Bart. John Kennedy, Esq. Captain Churchill. R. I. Murchison, Esq. John

Wishaw, Esq. Dr Chalmers. L. Horner, Esq. John Marshall, Esq. Neil Malcolm, Esq. Francis Clark, Esq.

The Statistical Section assembled. The report of the Third Meeting of the British Association, held at Cambridge in 1833, contained two recommendations applicable to this Section, in reference to which this Section has to state, that Col. Sykes has now in progress some statistical returns, collected by himself in India, relative to the Deccan, but that they are not yet in a sufficiently advanced state to be laid before the meeting.

This Section has also to report, that Professor Jones has, in pursuance of the second recommendation of the Association, applied for leave of access to the archives of the East India Company, and that that Body, with its accustomed liberality, has afforded him every facility in promoting his researches. This section has also great satisfaction in reporting, that a Statistical Society has been formed in London, the present condition and progress of which will be laid before the meeting in the course of this week.

Mr Benjamin Heywood attended to announce the formation of a Statistical Society in Manchester, which has been established since the last meeting of the British Association; and appeared on behalf of that body.

An important communication issuing from that Statistical Society, "On the condition of the working-classes in certain districts of the town of Manchester," was presented by him.

From this document it appeared that the number of families visited by three persons appointed by that Society, amounted to 4102, composing nearly 20,000 persons, occupying 3110 houses, and 1002 cellars and apartments, of which only 689 were well furnished, 1551 were comfortably furnished, and the very large number of 2551 were described as uncomfortable. It further appears, that, out of the above number of 20,000 persons, 7789 receive wages, and only 158 pay a rent exceeding four shillings a-week. The same paper stated that there were in the above district 8121 children under the age of twelve years, of whom only 252 attended day-schools, while 4680 attended Sunday-schools, and nearly half the children were without education. The number of parents who stated themselves to be able to read amounted to 3114.

The next communication to which this Section directed its attention, was a lecture on the science of Agriculture, and the means of promoting its improvement, by the Right Hon. Sir J. Sinclair, Bart. This Section, considering it to be the first and most essential rule of its conduct to confine its attention rigorously to facts, and, as far as it may be found possible, to facts which can be stated numerically, and arranged in tables, did not feel justified in entering upon the consideration of the contents of this paper.

EVENING—GEORGE'S STREET ASSEMBLY ROOMS.

In the evening, at 8 o'clock, the general meeting of all the Sections, with a brilliant display of the fair sex, took place in the Assembly Rooms.

Sir Thomas Brisbane having taken the Chair, said, that, before commencing the regular business of the evening, Mr Taylor, the treasurer, would report on the progressive increase of the Association, from the commencement to the present period.

The Treasurer then stated, that the number of tickets issued to new members on the present occasion, amounted to upwards of 800, and that probably 150 would be added to-morrow. The Association, at the commencement at York, numbered 350 members; at Oxford they increased to 700; and at Cambridge, last year, to about 1400.*

Sir Thomas Brisbane then requested the Association to attend to the reports of the Presidents of the different sections as to the proceedings of the day.

Professor Whewell read the report of the Physical Section; Dr Dalton, in absence of Dr Hope, the report of the Chemical and Mineralogical Section; Professor Jameson the report of the Geological and Geographical Section; Dr Graham the report of the Natural History and Botanical Section; Dr Abercrombie the report of the Medical Section; and Sir Charles Lemon reported the proceedings of the Statistical Section.

After the Presidents of the Sections had thus reported proceedings, Dr Robinson read a letter from Mr Rumker of Hamburgh, which was accompanied by an ephemeris of the track of the comet of 1682 and 1759, whose return is expected at the end of this year. The Vice-President of the Association, Professor Robinson of Armagh, then gave an interesting account of our

* We have been informed, that on the last day the number enrolled in Edinburgh is 1298.

knowledge of the history and nature of comets. Professors Whewell and Hamilton also took a discursive view of the cometary world in their usual characteristic manner.

The Association then adjourned, and the President intimated that the next evening meeting would take place in the same room and at the same hour.

Wednesday, 10th September.

MORNING—UNIVERSITY.

SECTION A.—MATHEMATICS AND GENERAL PHYSICS.

The Rev. Dr Lloyd, Provost of Trinity College, Dublin, having been called to the Chair, Professor Lloyd read to the Section a portion of his report on physical optics.

M. Arago offered a few observations on some statements contained in that report, chiefly with reference to the hypothesis of transversal vibrations, and advocated the claims of Dr Thomas Young, as the first to propose it.

Mr Whewell read a paper by M. Challis, entitled "Theoretical Explanations of some facts relating to the composition of the Colours of the Spectrum." Mr Whewell also offered some suggestions regarding Sir John Herschel's explanation of dispersion, according to the undulating theory. Sir D. Brewster objected to the validity of this explanation; his objections being grounded on the phenomena of the dark bands in the light transmitted through nitrous acid gas, and their alteration with the increase of temperature.

Professor Powell handed in a short paper "On the Achromatism of the Eye," in continuation of a paper contained in the last volume of the British Association.

Professor Powell then gave a brief view of the explanation of dispersion of light in the undulatory theory, as afforded by the mathematical analysis of M. Cauchy, and stated a condition which seemed necessary to the validity of this explanation.

Mr Whewell, Professor Hamilton, and Dr Robinson also made some observations on the same subject.

Professor Phillips then read the second report of the result of twelve months' experiments on the quantity of rain falling at different elevations above the ground, made by himself and Mr Gray.

Sir Thomas Brisbane made some observations on an anomaly in the quantity of rain registered under different circumstances of observation. Mr Howard likewise made some observations.

It was then moved by Dr Robinson, that the Section should return its thanks to Mr Phillips for the careful manner in which he had executed the task entrusted to him by the Association, and request him to continue his observations during another year, with the view of obtaining such results as might furnish the means of deciding on the theories that had been proposed for the explanation of the phenomena.

Mr Phillips thanked the meeting for the kind manner in which his communication had been received, and professed his readiness to comply with the request. Mr Phillips gave some further account of his mode of observation, with reference principally to the remarks of Mr Howard.

Professor Stevelly read a paper, entitled "An attempt to connect some well known phenomena in Meteorology, with well established physical principles." The questions discussed in this paper were,—1. The nature, origin, and suspension of clouds, and the immediate effect of their formation; 2. The manner in which *rain* is produced, and the immediate effect of its production; 3. The manner in which *wind* results from the formation of cloud and rain; 4. The origin of *hail.*

Section B.—Chemistry and Mineralogy.

The Chemical Section proceeded to consider the recommendations of the Sections of the former meetings, having reference to Mineralogy and Crystallography.

Mr Whewell made a communication on the progress of the inquiries entrusted to Prof. Miller on the forms of crystals, and to the Committee appointed to examine the subject of isomorphism. He stated that the German chemists and crystallographers are ardently engaged on this important subject. In regard to the properties of substances similar in constitution and form, he directed the attention of the meeting to the important fact, that the optical properties are often very different in substances considered to be of the same species. And on this subject reference was made to the case of topaz, on which some discussion took place, chiefly on the observations made as to the relation of their optical axes at different temperatures.

A paper was read by Dr Charles Williams, *On a New Law of Combustion.* In this communication the author shewed, that many organic substances exhibit in a dark place a pale lambent flame, similar to that exhibited by dry phosphorus when heated in the air to a temperature below incandescence, (as low as 300°), and that this flame bursts out into that known to attend ordinary combustion, when the substance is plunged into oxygen gas. This feeble combustion commences in organic substances when vapours begin to be evolved. This feeble flame has little heating power, and passes to ordinary flame by a rapid transition, accompanied by a feeble detonation. Some metals, as zinc and potassium, shew the same phenomenon, though of shorter duration, probably from the formation of a coating of oxide.—As a practical result from his observations, he remarked on the danger of many manufactures, as those of soap and candle-making, in which vapours are driven off from organic substances, and this low combustion actually goes on during the whole process of manufacture.

Dr Daubeny next brought before the meeting the economical employment of coal-tar in connection with water as fuel, according to the method lately suggested by Mr Rutter. A discussion then arose as to whether the water in this case acts chemically or mechanically, or both, in facilitating the combustion of the tar. Mr Macintosh stated, that by repeated experiments he had found, that coal-tar gave no more heat when burned than an equal weight of *splint* coal, the kind preferred, where a long continued heat is required. Mr Low also stated, that from long experience he could affirm, that the use of water along with coal-tar was productive of no benefit whatever, and that 3 gallons, or 33 lb. of coal-tar, give an equal amount of heating effect fully to 40 lb. of coke, made from the Newcastle coal of the Hutton seam.

From the discussion on this subject, which was protracted for some time, it appears to be established, 1. That tar may be used as fuel, but that it does not give much more heat than the same weight of the best coal. 2. That when mixed with water, it flows more easily through tubes, but does not appear to evolve more heat than when used alone.

The next communication was an abstract of the discoveries of Reichenbach, in regard to the products of the destructive distillation of organic substances. In this paper Dr Gregory detailed the properties of Paraffine, Eupion, Kreosote, Pittakall, Picamar, and Kapnomor, and exhibited specimens of several of them. He also made some observations regarding the more common products of destructive distillation, and stated that several of these substances are found in the naphtha of Rangoon; the petroline found by Dr Christison in that substance, being the substance afterwards named Paraffin by Reichenbach. After a short discussion regarding the products of the distillation of caoutchouc, the meeting adjourned to Thursday 11th, at 11 A.M.

Section C.—Geology and Geography.

Professor Jameson in the Chair.

Mr George Rennie communicated observations on the principle of construction, and the practical employment, of an instrument for taking up water at great depths. It was tried by Mr Rennie at the estuary of the Tamer, near Plymouth, and found to succeed completely.

An interim report by Mr Stevenson, "On the State of our knowledge respecting the relative level of Land and Sea, and the waste and extension of Land on the east coast of England," illustrated by charts and sections of the German Ocean, was read to the meeting. Remarks were made by Professor Phillips, and a discussion ensued, in which Mr W. Smith related the results of his personal investigations on this subject.

Mr Greenough remarked, that the question proposed by the Association to Mr Stevenson, related to changes in the relative level of land and sea on the British coasts; whereas the memoir of that gentleman, which they had just heard, was confined to a description of alterations of the coast due to the gain of land by new deposits, or losses referable to the encroachments of the sea. Mr G. then observed that, with reference to alterations in relative level, Mr Lyell would probably be able to communicate some interesting information to the Section, as he had just returned from a tour in Sweden, purposely undertaken with a view to investigating that point.

Mr Lyell stated that he was most willing to answer to this call, though it would be impossible for him to give in detail all the proofs of a change of level which he had observed. It was his intention soon to read a paper on this subject to the Royal Society of London, but he had no objection to give orally an outline of the principal results at which he had arrived. It would be necessary to preface his statement with a brief sketch of the state of the controversy touching the gradual rise of Scandinavia, at the time of his visiting that country. It was more than 100 years since the Swedish naturalist Celsius had declared his opinion, that the level of the waters, both of the Baltic and the ocean, were suffering a gradual depression. In confirmation of this phenomenon, Celsius had appealed to several distinct classes of proofs; 1*st*, The testimony of the inhabitants on the northern shores of the Gulf of Bothnia, that towns formerly sea ports were then far inland, and that the sea was still constantly leaving dry new tracts of land along its borders. 2*dly*, The testimony of the same inhabitants, that various insulated rocks in the Gulf of Bothnia, and on some parts of the eastern shores of Sweden, then rose higher above the level of the sea than they remembered them to have done in their youth. 3*dly*, That marks had been cut on the fixed rocks on the shore some thirty years or more before, to point out the level at which the waters of the Baltic formerly stood when not raised by the winds to an unusual height, and that these marks already indicated a sinking of the waters. On the whole, Celsius concluded that the rate of depression amounted to three or four feet in a hundred years. To this conclusion it was objected, that there were many parts of the Baltic where the level of the sea had not fallen, as could be proved by ancient pines and castles standing close to the water's edge, and other natural and artificial monuments. It was remarked that the new accessions of land were chiefly where rivers entered the sea, and where new sedimentary deposits were forming; and that the marks were not to be depended upon, because the level of the sea fluctuated in consequence of the action of the wind.

Von Buch, in the course of his tour in Sweden and Norway, about twenty-five years ago, found at several places on the western shores of Scandinavia, deposits of sand and mud containing numerous shells referable to species now living in the neighbouring ocean. From this circumstance, and from accounts which he received from inhabitants of the coasts of the Bothnian Gulf, he inferred that Celsius was correct in regard to a gradual change of relative level. As the sea cannot sink in one place without falling every where, Von Buch concluded that certain parts of Sweden and Finland were slowly and insensibly rising. Mr Lyell, together with Von Hoff and others, still continued to entertain doubts with regard to the reality of this phenomenon, partly on grounds stated by former writers, and above enumerated, partly because Sweden and Norway have been, within the times of history, very free from violent earthquakes, and because the elevation was said to take place not suddenly and by starts, according to the analogy of the intermittent action of earthquakes and volcanos, but slowly, constantly, and insensibly.

Mr Lyell had visited some parts of the shores of the Bothnian Gulf, between Stockholm and Gefle, and of the western coast of Sweden, between Uddevalla and Gothenburg, districts particularly alluded to by Celsius. He had examined several of the marks cut by the Swedish pilots, under the direction of the Swedish Academy of Sciences in 1820, and found the level of the Baltic in calm weather several inches below the marks. He also found the level of the waters several feet below marks made seventy or a hundred years before. He obtained similar results on the side of the ocean; and found in both districts that the testimony of the inhabitants agreed exactly with that of their ancestors recorded by Celsius. After confirming the accounts given by Von Buch of the occurrence on the side of the ocean, of elevated beds of *recent* shells at various heights, from ten to two hundred feet, Mr Lyell added, that he had also discovered deposits on the side of the Bothnian Gulf, between Stockholm and Gefle, containing fossil shells of the same species which now characterize the brackish waters of that sea. These occur at various elevations, from one to a hundred feet, and sometimes reach fifty miles inland. The shells are partly marine and partly fluviatile; the marine species are identical with those now living in the ocean, but are dwarfish in size, and never attain the average dimensions of those which live in waters sufficiently salt to enable them to reach their full development. Mr Lyell concluded by declaring his belief that certain parts of Sweden are undergoing a gradual rise to the amount of two or three feet in a century, while other parts visited by him, farther to the south, appear to experience no movement.

Lord Greenock, in name of the Highland Society, communicated the desire of that body to give its assistance to geological investigations; and announced that, from information lately received from the Treasury, it is now certain that the Geological Map of Scotland will speedily be published.

Lord Greenock then read a paper on the coal formation of the central district of Scotland, which was illustrated by specimens, sections, and maps. Lord Greenock is inclined to believe that organic remains such as those found in the coal strata at Stoneyhill, near Musselburgh, will be found to become more rare, if they do not entirely disappear, as we descend in the series, and approach the limestone containing marine shells and encrinites, although the reappearance of such remains in an inferior position at Burdiehouse is a circumstance not easily to be accounted for. The author remarked that the flat seams contain the most valuable coals in the district; but that they occur only partially in the Mid-Lothian coal-field, and they are not to be found to the southward or westward of the road from Edinburgh to Dalkeith, as they are said to have been thrown off by a dyke near Sheriff-hall, beyond which some of the edge-seams appear to have been brought up and flattened:—and these are worked as flat seams at the Dalhousie, Polton, and Eldon collieries. There seem to be sufficient reasons to warrant the supposition that the coal district of the opposite coast of Fife was originally connected with that on this side of the Firth. At the same time, with the knowledge we possess respecting the two districts, it would be difficult to prove their exact correspondence either by their lines of bearing or by the quality of the coals. But it is not improbable that disturbing causes may have operated to produce derangements and dislocations in those parts of the coal formation now beneath the water, even to a greater extent than in those on the shores of the Firth. From the reports of Mr Landale and Mr Bald, it appears that the number of coal-beds is nearly the same in the two coal-districts, and that the total thickness of the coal in them is also nearly alike.

In the Fifeshire District.	Edinburgh District.
29 beds of coal.	26 beds, and probably 29.
119 feet.	109 feet.

The two seams of coal, the workings of which have lately been resumed, on the estate of Captain Boswell at Wardie, have apparently been thrown out of their natural position by some disturbance.

The nodules of ironstone, of which there is a great abundance in the bituminous shale of Wardie, are very remarkable; for there is scarcely one to be found that does not contain an organic nucleus, either a coprolite or some portion of a fossil fish. Similar nodules have been found on the opposite coast, and on Inchkeith.

A notice by Mr Menteath on the Closeburn limestone was read, in which an account was given of the geological, mineralogical, and chemical characters of that deposit.

Professor Sedgwick spoke of the services rendered to our knowledge of the geology of the north of Scotland, by the late Mr Macculloch junior, and expressed the hope that the results of his investigations would be employed and acknowledged in the Government Map.

A notice was read by Mr Trevelyan on fossil wood from a bed of clay lying above coal in Suderoe, the most northern of the Faroe islands.

Dr Hibbert read a paper on the ossiferous beds contained in the basins of the Forth, the Clyde, and the Tay.

He pointed out, in a general manner, the order of succession observed by such beds as were deposited later than the primary and transition schists. These were the peculiar grey micaceous sandstone, principally to be found on the north of the Tay, known by the name of the Arbroath pavement; the red sandstone, into which the Arbroath pavement passes; and the stupendous masses of conglomerate materials, formed by rolled fragments of primary and transition rocks, which repose at the foot of the Grampians. It was incidentally stated that, near Cratown, the conglomerate strata were traversed by a trap rock, containing large crystals of glassy felspar, which gave to it the exact character of one of the modern trachytes of the Siebengebirge. The conglomerate rocks were supposed to have been formed at two distinct epochs. The author expressed a suspicion only that certain patches of sandstone, occurring both on the east and west coast of Scotland, might be considered as New Red Sandstones.

That the greywacke schist and its associate beds of limestone contain organic remains, has not yet been shewn. The author exhibited a specimen of the Arbroath pavement containing vegetables, and he stated that Mr Lindsay Carnegie of Kinblythmont in Angus, had presented to the College Museum some striking specimens of remains inclosed in the Arbroath pavement, one of which appeared to belong to a crustaceous animal. [These were subsequently exhibited to the Geological Section by Professor Jameson.]

But it was shewn that organic remains had been most abundantly found in the later deposit of the carboniferous group, which the author had previously described at the meetings of the Royal Society of Edinburgh. Certain limestones, for instance, namely, those of Burdiehouse, East Calder, Burntisland, &c. which he conceived to be of fresh water origin, and belonging to the lower members of the carboniferous group, severally contained both vegetable and animal remains.

The limestone of Kirkton, near Bathgate, was remarkable for its mammillated and ribbonned structure; which last peculiarity was produced by thin layers of pure flinty matter alternating with other distinct layers, which were severally calcareous, argillaceous, or bituminous. This rock had a striking resemblance to the tertiary limestones of Auvergne, which exhibit a similar character when they come in contact with volcanic eruptions. And hence, as the limestone of Kirkton is alternated with tufa, and is in the immediate neighbourhood of trap-rocks, it probably owed its peculiar geological character to similar circumstances. This limestone contained numerous plants as well as the remains of a most remarkable crustaceous animal, a nearly complete specimen of which the author was enabled to exhibit to the Society, through the kindness of Dr Simpson of Bathgate, into whose possession the relic had fallen. The author remarked, that a larger head of the same animal had been described by Dr Schouler; but as this naturalist had unfortunately not seen the extremity of the animal, the description was of necessity imperfect.

[Incidental to this notice, Mr Smith of Jordanhill, near Glasgow, exhibited to the Society the more perfect head of the animal described by Dr Schouler. And Mr Jameson Torrie placed in Dr Hibbert's hands a memoir just published by Dr Harlan of America, in which fossil remains are figured of a similar character, but of the diminutive size of five inches only. The generic name of Eurypterus has been given to the American specimen. Dr Hibbert announced that drawings, accompanied by a description of this singular animal, would be shortly published.]

The limestone quarry of Burdiehouse was very briefly described. as many details regarding it had already been published by the author. This limestone was a very deep-seated bed in the carboniferous series. Above it were alternating beds of sandstone, shale, and thin seams of coal. A limestone containing marine shells and corallines followed, while the whole was surmounted by the coal-measures of Loanhead. The Burdiehouse limestone contained a variety of plants, minute Entomostraca and Conchifera (among which there appears to be a Cypris and a Planorbis), various undescribed fish, the bones of gigantic animals, large scales and coprolites. Among the bones are pointed teeth of the extraordinary length of three and three-quarter inches, and of the width of one and a half inches at their base, which resemble those of Saurian reptiles. These teeth were adorned with a most beautiful brown enamel, as well as the large scales which are so plentifully found in the quarry. There were also exhibited some bony rays of the extraordinary length of fifteen inches, which must have belonged to an huge fish.

The author announced that all the relics of fish hitherto discovered at Burdiehouse would be submitted to the inspection of M. Agassiz, who had studied fossil ichthyology with such splendid results, and who, in the invaluable work which he was publishing, promised to fill up, with the success of a Cuvier, this great blank in natural history.*

Dr Buckland, after making many comments on the relics of the Burdiehouse quarry, introduced to the Geological Section M. Agassiz, who was one of the distinguished savans present at the British Association, and requested his opinion on the specimens displayed on the table.

M. Agassiz, agreeably to this invitation, explained the character of the fish in a most luminous manner, which excited a very general feeling of satisfaction. And after an examination, on the following day, of the whole of the Burdiehouse specimens in the possession of the Royal Society of Edinburgh, he announced, that it was one of the richest acquisitions which he had ever made in fossil ichthyology, as it contained several wholly undescribed genera.

With regard to the large teeth which had been conceived to be Saurian (for such was the opinion entertained of them by some of the first naturalists of London and Paris), M. Agassiz, after a renewed examination of them, became disposed to pass a different opinion from that which he had at first view embraced. A close inspection of the minute jaw of one of the fry of this animal, led him to the conclusion, that the relic rather belonged to a fish of a new and extraordinary genus, which will probably prove to be more Sauroid than any of the ancient fish which he has classified in his "Recherches sur les Poissons Fossiles."

This naturalist now considers, as parts of this animal, all or most of the large bones discovered in Burdiehouse quarry, including the very large bony rays, as well as the large enamelled scales which have been so abundantly found. These have been entrusted to his care by the Royal Society of Edinburgh, for the description which he purposes to give of them in his invaluable work. M. Agassiz has named the animal Megalichthys Hibberti.

The judgment thus pronounced upon these relics is calculated, in the opinion of this naturalist, to excite questions of the most lively interest respecting the characters possessed by the races of beings

* Dr Hibbert likewise displayed the teeth and other relics of a large fish, which he had recently discovered in the black limestone of Ashford, in Derbyshire.

which lived during so remote an epoch. It would be a new era for paleontology, as he observes, if it could be demonstrated, as it is his belief it may be, that the fish of this period unite, in their particular organization, the characters of reptiles belonging to that class of animals which only appear in great numbers during later times.*

Several members of the Geological Section, particularly Dr Buckland, took this opportunity of acknowledging the eminent services rendered to geology by the Royal Society of Edinburgh, in preventing the relics of Burdiehouse quarry from being dispersed, and thus lost to science. Nor was it forgotten that the object was mainly accomplished by the indefatigable and truly scientific exertions of their General Secretary Mr Robison.

M. Agassiz and Dr Buckland have subsequently urged the importance of the Royal Society continuing these exertions in the cause of paleontology; and it is grateful to think how much this object has been seconded by the co-operation of Mr Torrance, who at present possesses the lease of Burdiehouse quarry.

A letter from R. Allan, Esq. inviting members of the Section to inspect his mineralogical collection, was read.

The two first numbers of the extensive work of M. Agassiz on Fossil Fishes were exhibited, and were particularly recommended to the attention of the members of the Section by the Chairman and Dr Buckland.

Section D.—Natural History.

On the plurality and development of Embryos in the seeds of Coniferæ, by Robert Brown, Esq.

The earliest observations of the author on this subject, were made in the summer of 1826, soon after the publication of his remarks on the female flower of Cycadeæ and Coniferæ. He then found that, in several coniferæ, namely, Pinus strobus, Abies excelsa, and the common larch, the plurality of embryos in the impregnated ovulum was equally constant, and their arrangement in the albumen as regular as in cycadeæ; and similar observations,

* This investigation of Agassiz is confirmatory of the opinion, founded on zoological and geognostical characters, of Professor Jameson, delivered in the Wernerian Society, viz. that the limestone of Burdiehouse, Burntisland, &c., contains the remains of fishes, but none of Saurian animals, and that the strata of these districts are not of fresh-water origin.

made during the present summer, on several other species, especially Pinus sylvestris and pinaster, render it highly probable that the same structure exists in the whole family.

The first change which takes place in the impregnated ovulum of the coniferæ examined, is the production or separation of a solid body within the original nucleus.

In this inner body or albumen, several subcylindrical corpuscula of a somewhat different colour and consistence from the mass of the albumen, seated near its apex, and arranged in a circular series, soon became visible.

In each of these corpuscula, which are from three to six in number, a single thread or funiculus, consisting of several, generally of four, elongated cells or vessels, with or without transverse septa, originates. The funiculi are not unfrequently ramified, each branch or division terminating in the minute rudiment of an embryo. But as the lateral branches of the funiculi usually consist of a single elongated cell or vessel, while the principal or terminal branch is generally formed of more than one, embryos in coniferæ may originate either in one or in several cells, even in the same funiculus.

A similar ramification in the funiculi of the Cycas circinalis has been observed by the author.

Instances of the occasional introduction of more than one embryo in the seeds of several plants belonging to other families have long been known, but their constant plurality and regular arrangement have hitherto only been observed in Cycadeæ and Coniferæ.

Section E.—Anatomy and Medicine.

Dr Sharpey read a communication on certain peculiarities in the circulation of the porpoise, which he illustrated with preparations.

The principal facts established by Dr Sharpey's investigations, were, 1st, That the veins as well as the arteries of the porpoise, present, in several regions of the body, a remarkable plexiform arrangement; and, in some places, plexures of both kinds of vessels are combined or mingled together.

2d, That the arterial plexures are not confined to the thorax and vertebral canal, (the situations in which they were particularly described by Mr Hunter as occurring in whales, in his observations on the structure and economy of these animals, published in the Philosophical Transactions of 1787,) but several are formed by the rteries of other parts.

3d, The description given by Cuvier (Leçons, vol. iv. p. 258), of the division of the caudal portion of the aorta, requires to be corrected, inasmuch as the vessel is not wholly and suddenly resolved into small branches, which unite to form it anew, but is only diminished in size, and concealed in the midst of a plexus of small vessels, from which it again comes out.

4th, Several arteries shew a tendency to divide into long parallel branches, of which the arteries of the ploric viscera offer a striking example.

5th, The brachial artery, as in tardigrade animals, divides into many long branches, which unite again into a few larger vessels, after forming a plexus which is not convoluted.

Lastly, The internal carotid, which at its origin is as large as in man, diminishes in a tapering manner, and without giving off branches till it enters the skull, when it is scarcely thicker than a pin.

Some remarks were made by Dr Roget, Mr Clift, Mr Dick, Dr Cooke of Durham, and Mr Mackenzie.

A communication from Mr Murray of Hull, on the change of colour in the Chameleon, was next submitted to the Section.

After pointing out that there are other animals besides the chameleon, such as the *agama* or Mexican chameleon, and the *polychlorus*, which display a change of colour, or variable intensity in the tint on their skin, and noticing some of the more striking circumstances in the natural history of the chameleon, particularly the manner in which it casts its skin every six months, the author proceeded to describe the circumstances in which he conceives the changes of colour in the skin of this animal to depend. These he conceives to be the electro-chemical action of the sunbeam on the blood, through the cutaneous surface, as modified by its more or less accelerated impulse, conjoined with the greater or less dilatation of the investing membrane. According to the author, the skin, when narrowly inspected, seems to be covered with small granulations of variable size, and ever varying convexity, and which are capable of receiving, through the agency of a plexus of contractile and expansive fibrillæ, a variable quantity of blood. As confirming the opinion that the change of colour is intimately connected with the circulation of the blood, Mr Murray refers to experiments which appear to him fully to prove, that the various shades of colour displayed in patches on the skin of the chameleon, exhibit corresponding changes of temperature, the thermometer indicating, according to his observation, a difference of 2° Fahr., viz. from 73° to 75° Fahr., when the ambient air was 72° Fahr.

This communication called forth some remarks from Dr Spittal and Mr Edmonstone, on observations they had themselves made on the chameleon; from Mr Dick on the tongue of that animal; and a notice from Dr Allen Thomson, of some experiments made by him on the change of colour observable in the cuttle-fish.

Dr Allen Thomson then exhibited to the meeting a series of preparations made by Dr Sharpey and himself, of injections of the lacteal and lymphatic absorbent vessels of the seal, porpoise, turtle, and man—preparations which appear to them to support the conclusions of Professor Panizza of Pavia, in opposition to the opinion of Fohmann, Lippi, and others, as to the non-existence of numerous communications between the absorbents and veins. It was resolved that the connexions of these two systems of vessels should be proposed to the General Committee as a subject for experimental inquiry.

Section F.—Statistics.

The Statistical Section having met at eleven, a discussion arose on the paper presented by Mr Heywood yesterday, in which several members from the large manufacturing towns in England, and from the mining districts of Cornwall and Wales, took a part.

The discussion turned principally upon the advantages of Sunday and Infant Schools, especially as shewn in the reaction on the habits of the parents of children attending them. Many instances of this were stated to be within the knowledge of members present, who had directed their attention to this subject.

The possibility of ascertaining, by direct inquiry, the amount of the earnings of the poorer classes was discussed, and the difficulty of obtaining information of sufficient accuracy on this very important head was strongly pointed out by the fruitless attempts which had been made by the Statistical Society of Manchester, as also of Dr Cleland in similar enquiries undertaken by him for the use of Government.

Mr Taylor and his brother undertook, in conjunction with the Committee, to draw up a series of questions upon the condition and habits of the mining population in Cornwall and Wales, from which they hope to draw a complete account of the statistics of that class for the next meeting of the Association.

The Secretary next read a very interesting paper relating to the statistics of Glasgow, drawn up by Dr Cleland for the British Association. In this paper was pointed out the great inaccuracies of the parochial registers, and the instances here stated shewed that these are wholly inadequate to form the basis of a correct census.

In the portion of this paper which relates to the probability of human life in Glasgow, Dr Cleland stated his belief that that city is a place of average health, and that no material variation in the rate of mortality in that city has occurred between the years 1821 and 1831.

Dr Cleland found, as has been universally observed elsewhere, that in Glasgow there are more males *born* than females ; but that, in every period above fifteen years of age, the proportion of living females always preponderates.

It is unnecessary to allude at greater length to this valuable paper, as Dr Cleland has printed it for the use of the Association, and members may obtain copies of it by applying to the Secretaries of this Section.

A letter from Professor Malthus was read, stating his regret that his avocations at the East Indian College prevented him from attending this meeting of the Association.

A letter from Mr Quetelet of Brussels to Professor Whewell was read, respecting his necessary absence from this meeting, and announcing the speedy publication of a work by that gentleman, the most interesting portion of which will be devoted to an examination of the law of population. Mr Quetelet states his belief that he has succeeded in reducing the examination of this law to the discussion of mathematical formulæ, and that those at which he has arrived are in fact exactly similar in form to those employed in the planetary theory. Mr Quetelet is aware how visionary this announcement may possibly appear, but requests that it may be tested by the close accordance between the calculated results and those furnished by observation in England, the United States, and elsewhere.

The Committee took this opportunity of reminding the meeting, that, in consequence of the recommendation of the Statistical Section at the last meeting, a society has been formed under the name of the Statistical Society of London. Several members of the council of that society are present in Edinburgh, with power to elect new Fellows. Those members of the Association who may be desirous of joining that society, are requested to give in their names to the

Secretary of the Section, who will communicate them to the council of the society. The Secretary can also furnish copies of the prospectus issued on the formation of the society, its regulations, and an imperfect list of the present members.

EVENING—GEORGE'S STREET ASSEMBLY ROOMS.

The reports of the different Sections having been read by the Presidents, Sir Thomas Brisbane requested Dr Lardner to explain the principle of Mr Babbage's celebrated calculating machine. The learned Professor explained, in as far as possible without exhibition of models or the machine itself, its various properties. The meeting adjourned till next evening.

Thursday, 11*th September*.

MORNING—UNIVERSITY.

Section A.—Mathematics and General Physics.

The Rev. Mr Whewell having taken the Chair, Mr Rennie gave the second part of his report on Hydraulics, containing the application of the principles of that science to the subject of rivers.

Professor Hamilton then gave an account to the meeting of a new method in Dynamics. After a brief review of the several leading steps which had been successfully made in the advancement of dynaminal science, and more particularly the labours of Galileo, Newton, aud Lagrange, Professor Hamilton proceeded to the consideration of the problem to be solved, which may be stated to be the determination of the three co-ordinates of each point by the moving system, as a function of the time; and after attending to the limitations of the solutions of the problem, as hitherto given, he proceeded to explain his own method, and to shew that the solution may be made to depend upon a certain function of the initial and final configurations, analogous to that which he has denominated a characteristic function in optics. In this mode of treating the subject, the whole problem is reduced to the research of this function. The author concluded by stating the degree of success which had attended his own investigations respecting the form of this function, in the case of a binary system in the problem, by the determination of the orbit of a comet from the initial and final co-ordinates, and the intervening time; and, lastly, in the problem of three bodies.

Professor Phillips communicated a paper on a new form of the Dipping Needle, constructed so as to afford the means of correcting the error of the centre of gravity.

Colonel Sykes read a short paper entitled, Notes on the mean temperature in India, which called forth some remarks from Sir David Brewster and Professor Forbes.

Professor Lloyd gave an account of magnetical observations undertaken in Ireland in pursuance of the recommendations of the Association; and of a new method of observation which he had employed.

Dr Robinson stated to the meeting various objections to the site of the present Observatory of Edinburgh, and suggested the propriety of transferring that Observatory to some more suitable position; and of converting the existing building to the purposes of a magnetical observatory. Dr Robinson concluded by stating the proposition which he intended to submit on this subject to the Committee of the Section. Sir David Brewster also stated the result of his own observations on the progress of destruction now going forward in the object-glass of the transit instrument.

M. Arago made some remarks on the accuracy which may be attained in usual methods of observing the dip of the magnetic needle. He stated that, when the instrument was furnished with a micrometer, and the necessary cautions observed, he found it adequate to the determination of the diurnal variation of the dip, contrary to the opinion expressed by Professor Christie in his report on terrestrial magnetism. M. Arago also stated the results of some observations of his own on the decay of the object-glasses of telescopes.

Professor Forbes read a short communication from Mr Jordan, describing a mode of suspending the magnetical needle, which it is supposed by the author would possess some advantages for observing variations in the direction and intensity of the earth's magnetism.

Sir David Brewster described to the meeting a remarkable coloration which he had observed in the space included between the interior and exterior rainbow.

Mr Saumarez read a paper on light and colours, containing some peculiar views respecting their nature and origin.

SUB-SECTION.

M. I. Brunel, Esq. in the Chair.

Mr Dent exhibited a chronometer with a glass balance-spring, and presented an account of its rate kept at the Royal Observatory, Greenwich, since the last meeting of the Association. He also shewed a chronometer in motion, with a pure palladium balance-spring, a table of the variations of gold, steel, palladium, and glass, from 32° to 100° Fahr.; and another table shewing the qnantities respectively due to direct expansion, and to loss by elasticity in steel and palladium.

Mr Adam gave a description of a Sextant Telescope of his invention, furnished with a spirit level attached to its eye-tube, for taking vertical angles or altitudes on sea or land, when the horizon is invisible.

Mr Ramage read a proposal for constructing a reflecting telescope, of greater magnitude than has yet been attempted, and exhibited a model of the proposed instrument. Mr Cooper stated, that a superior reflecting telescope had been constructed by Mr Grobe of Dublin, at one-fifth of the usual cost, and expressed his perfect confidence in the ultimate success of the invention.

Mr Alexander Gordon exhibited Muretz's modification of Fresnel's polyzonal lens, which (with a common Argand flame) Mr Gordon proposes as an economical light for ports and harbours; and to be adopted when a more intense flame is used for coast light-houses, in situations where the use of parabolic reflectors is not absolutely necessary.

SECTION B.—CHEMISTRY AND MINERALOGY.

The Chemical Section met at 11 A.M.

Mr West gave an account of his experiments on bar iron, shewing that when dissolved in muriatic acid, sulphuretted hydrogen is given off from bar iron of the best quality. He inferred that sulphur is present as a deteriorating substance in most malleable irons; and he suggested that the quantity of sulphur in such irons should be determined more accurately. On the mode of ascertaining this point, some discussion arose.

A notice by Sir David Brewster was then read regarding a large specimen of amber from Ava, intersected by thin layers of carbonate of lime.

Mr Van der Toorn next gave a determination of the amount of water in crystallized sulphate of zinc. The total amounts to 7 atoms, of which 6 are given off at 110° C., the other atom remaining as a necessary constituent of the salt. From this result he concluded that sulphates, which at a red heat give off sulphuric acid, contain an atom of water as an essential constituent. On this subject some discussion arose, and Dr Clarke stated as a general law, that when salts effloresce, they always form compounds containing a definite number of atoms of water, which compounds may be obtained regularly crystallized by submitting solutions to evaporation at different temperatures.

Some considerations were then submitted by Mr Johnston on chemical notation, which gave rise to a discussion in which many members of the section took part. The result was, that the subject of notation was referred to the Committee of the Section with the view of introducing into this country a uniform system of notation.

A model of a spirit of wine lamp of considerable power, by Mr Trevelyan, was exhibited. After which a paper was read by Mr Henry Hough Watson, on the amount of carbonic acid in the atmosphere of the town of Bolton, and the country around.

His results were in 10,000 parts,

	Max.	Min.	Mean.
In the country, [12 observations] =	4.74	3.89	4.135
In the town, [19 observations] =	8.62	4.19	5.30

He could not discover any connexion between the variations of its quantity, and season, and weather, as suggested by *De Saussure.* The method adopted by Mr Watson to determine the point, did not appear, however, entirely free from objection. In the course of the discussion which arose, Dr Thomson stated as his opinion, that the difference in the results obtained at different times, and by different experimenters, arose from errors of experiment.

An extract of a letter was then read from Professor Hare of Philadelphia, suggesting the propriety of appointing agents in the different ports to assist the members of the Association in communicating with foreigners.

It was also stated by the Secretary, that an able report had been received from Dr William Henry on the present state of our knowledge regarding contagion.

An analysis of the oxichloride of antimony or crystallised powder of Algaroth by Mr Johnston, was then read, after which the meeting adjourned to Friday the 12th, at 11 o'clock.

Section C.—Geology and Geography.

Professor Jameson in the Chair.

Lieutenant Murphy exhibited some sheets of the Ordnance Survey Maps of Ireland.

Mr Nicol read a paper on the structure of recent and fossil woods, in which he described the general results of his investigations. He exhibited an extensive series of specimens illustrative of his observations, and explained his method of obtaining thin sections of recent and fossil woods.

Professor Traill communicated some remarks on the geology of the Orkneys. These islands consist chiefly of sandstone, and of a sandstone flag much charged with clay, belonging to the old red sandstone. Granite also occurs very like that of Sutherland, close grained, and often approaching to gneiss; and is covered by a conglomerate. The fossil fishes occur near Skaill in Pomona, about two miles from the junction of the granite with the slate.

Mr Murchison spoke of the fossil fishes of Caithness, and said he had formerly sent specimens to Cuvier, who had been disposed to refer the families to which they belong to those of Mansfield and the Thüringerwald. Mr Murchison afterwards visited the spot with Professor Sedgwick, and came to the conclusion, that the strata containing the fishes are referable to a formation as old as the old red sandstone, and perhaps ascending as high as the carboniferous series.

Professor Sedgwick entered at considerable detail into the nature of the sandstone deposits of the north of Scotland.

Dr Hibbert considers the fossil fishes in question as belonging to a formation analogous to that of Mansfeld.

M. Agassiz expressed his opinion that the fossil fishes of Orkney and Caithness belong to a period more ancient than the coal measures.

Mr Lyell agreed with Professor Sedgwick and Mr Murchison in their opinions, and made some remarks on the geology of Forfarshire.

Mr Milne read a paper on the geology of Berwickshire, and described the different formations of which that district consists, viz. the greywacke, old red sandstone, coal measures, and trap. He detailed the mineralogical and fossil characters, marking their different formations, and in particular alluded to the question as to the place in the geological series to which the red sandstones, &c. of Berwickshire are to be referred, and gave it as his opinion that these strata are developments of the lower beds of the coal-field. He also pointed out in detail the various elevations which have successively taken place on the land, by eruptions of trap at successive epochs; accompanying his observations by a reference to specimens, map, and sections.

Remarks expressive of the high opinion entertained of the value of Mr Milne's paper were made by Professor Sedgwick, Professor Jameson, Mr Greenough, and Mr Murchison.

The Secretary laid before the Section Dr Harlan's paper on the fossil organic remains of the United States.

Section D.—Natural History.

On the functions and use of the orbital glands in birds of the orders Natatores and Grallatores, by P. J. Selby, Esq.

On the birds observed and collected during an excursion in Sutherlandshire, by P. J. Selby, Esq.

On the fishes obtained during the same excursion, by Sir William Jardine.

On the insects obtained, by James Wilson, Esq.

On a collection of insects recently received from Java, by James Wilson, Esq.

On the change of colour of the fruit in a certain species of elder, by the Rev. James Drake.

On the cultivation of Phormium tenax in Scotland, by John Murray, Esq.

On the progress made in researches on the secretions from the roots of vegetables, by Dr Dunbar.

On the distribution of the phenogamous plants of the Faroe Islands, by W. C. Trevelyan, Esq.

Professor Jameson exhibited a splendid collection of coloured drawings of the vertebrate animals of Great Britain and Ireland, executed by Mr William Macgillivray, Curator of the Museum of the College of Surgeons. The Professor remarked, that their peculiar excellence consisted in their combining, with great beauty of pictorial effect, very accurate representations of the shape of the head, and of the structure, form, and modes of combination, of feathers in birds, and scales and plates in fishes, amphibia, &c. The drawings exhibited form part of a great collection, intended for publication, under the title of "The Mammalia, Birds, Reptiles, and Fishes, of Great Britain and Ireland."

Mr Graham Dalyell next read a very valuable memoir on the propagation of Scottish zoophytes, of which the following is a very short abstract.

All the following animal products are aquatic, and, excepting the last, inhabit the sea. Those less conversant with their formation may be referred to the general aspect of the *actinia* now ranked among the *radiata* ; and to the *hydra*, as consisting of a soft fleshy body with a dilatable mouth and stomach in the centre, surrounded by tentacula serving the office of fingers or hands.

1. A specimen of the *Actinia equina* preserved by me, produced above 276 young in six years. The embryos are first exhibited to the observer in the tips of the tentacula, whence they can be withdrawn and returned, and are finally produced by the mouth, during great compression of the parent. A tip with its embryo having been amputated, the latter began to breed in fourteen months, and survived five years. The actinia is erroneously defined in the *Systema Naturæ*, as having only one aperture. Streams may be seen spouting from the tentacula of the *Crassicornis*, and each of the thirty or forty tubercles of the *equina*, open to discharge purple flakes after feeding.

2. The *Hydra tuba* or trumpet polypus, a new Scottish species, is the largest of the *Hydræ* proper, extending about two inches in whole, with its long white tentacula waving like a beautiful silken pencil in the water. It propagates by an external shapeless bud issuing from the side of the parent, and withdrawing, though very long connected by a ligament, on approaching maturity. In thirteen months a single specimen had eighty-three descendants. Singular and distorted forms appear from the successive and irregular evolution of the buds, during subsistence of the connecting ligament. Observations were protracted during five years on the same group and the young.

3. The *Tubularia indivisa* is rooted to rocks and shells, by a stalk above a foot high crowned by a scarlet head, resembling a beautiful flower, with numerous external and central tentacula. Splendid groups occur of fifty or even of 100 specimens. The ovarium of this product consisting of several clusters like bunches of grapes is borne externally on the head, from whence the ovum or advancing embryo separates and falls to be developed below.

Prominences soon indicate the evolution of tentacula, then with enlarged instead of acute extremities as in adults; as they extend the nascent animal elevating itself on them as on so many feet, but with the body inverted, enjoys the faculty of locomotion. Apparently selecting a site, it reverses itself to the natural position with the tentacula upwards, and is then rooted permanently by a prominence, which is the incipient stalk, originating from the under part of the head. Gradual elongation of the stalk afterwards continues to raise the head, and the formation of the *zoophyte* is perfected.

It is obvious, therefore, that this product is primarily of animal nature exclusively.

4. The finest specimens of *Sertulariæ* resemble luxuriant shrubs in miniature, with stems, boughs, branches, and twigs, with thousands of cells and their polypi. One species, however, provisionally designed *Sertularia Uber*, rises towards three feet high from the root, thus infinitely exceeding the dimensions hitherto ascribed to the Sertulariæ. Certain specimens of this and various others bear *vesicles* or small vascular bodies, three or four times the size of the cells containing white, pink, green, or yellow corpuscula, of a spherical form, in their earlier stages. All preceding naturalists have conceived the vesicle, the ovarium, and these spherules, the ova whereby Sertulariæ are propagated. But a long series of observations, greatly diversified and continued throughout many successive years, has not led to this immediate result.

The vesicle contains from one to thirty corpuscula, according to species, which are spherical on the earliest recognition, through the refining transparency of its sides. Their shape alters on approaching maturity; it elongates, becomes elliptical, next prismatic, and at length each corpusculum issues as a perfect animal from the orifice of the vesicle. Now in figure and in motion, together with the exhibition of certain peculiarities, it bears much resemblance to the planaria. The colours are the same as were exposed by the transparency of the vesicle, which remains empty; the dimensions of none exceed a line in length. These animals may constitute a new genus, to be provisionally denominated *Planula*. Eight or ten species of Sertulariæ have afforded them; nor has any thing else been ever obtained from the vesicle.

The animal crawls very actively at first; but in some days its motion relaxes, it becomes stationary, contracts, and dies, though without speedy decomposition, as is incident to the planaria. Shortly afterwards, if many white or yellow planulæ occupied a vessel, a number of white or yellow spots, circular or spherical segments, and of about what might be grossly computed of equal superficial area to planulæ, may be discovered in nearly the same place. Next, the summit of the segment rises in an obtuse spinous prolongation, which, swelling into a cell as it advances, soon displays a living polypus in full vigour. Other cells are formed by further extension of the stalk, and by the divergence of the buds which constitute them, to right and left. Meantime the original spot breaks into divisions like radicles; it is gradually attenuated, and at last disappears. The first animal is quite as large as any of those succeeding it in the growing product; but probably the figure of later cells of some species undergo modifications. Plantations of hundreds of Sertulariæ may be easily obtained. According as the dying planula is white or yellow, so is the circular root invariably white or yellow. Although some important obscurities remain for elucidation, I have been hitherto unable to recognise any other elements of the nascent Sertularia.

5. The *Flustra Carbasca* resembles a leaf divided into subordinate parts, one of the surfaces being studded with cells, and the other exhibiting elevations or convexities corresponding to their bottom; and the whole product is of a yellowish colour. Each cell, of a shuttle or slipper shape, level with the surface of the leaf, is inhabited by a vivacious polypus, exercising a percussive faculty both of the tentacula individually, and of the whole head. Some of the cells are occupied occasionally by large, bright yellow, irregularly globular, solid ciliated animalcula, subsequently quitting them to swim heavily below. In several days they become motionless like the former, and die also without immediate decomposition. Next, there appears in just about the same spot below occupied by the motionless animalculum, a yellow nucleus with a lighter diffusing margin. This in its farther diffusion assumes a shuttle or slipper form; it becomes a single cell, which afterwards displays a polypus under the wonted figure and action. The adult flustra was *vertical*, for the leaf is always erect, but here the new cell is *horizontal*. By a singular provision of nature, as only one side of the adult is cellular, the original cell is necessarily a root, sole, or foundation, to admit subsequent enlargement, which in such zoophytes is always from a single cell. One end of the cell next rises vertically, wherein a second cell with its polypus is soon displayed overhanging the first, and at right angles to the plane of its position.

But as if the purposes exacted by the existence of the latter were now fulfilled, it dies while the existence of the second has scarcely attained maturity, and as a third cell, beside the second, is forming for a basis to further incerment.

All the preceding inhabit the sea, and propagate though solitary.

6. The *Cristatella mirabilis*, an inhabitant of the fresh waters of Scotland, is the most remarkable of polypiferous products, and perhaps it should constitute the type of a distinct genus. Specimens are of a longer or shorter oval figure, flattened, extending from six to twenty-four lines in length, by two or three in breadth, and resembling the external section of an ellipsoid. The whole of the under, and the middle of the upper, surface are smooth, the latter environed by a triple row of 100, 200, or 300 polypi, rising from within the margin. This product is of a fine green colour, and soft fleshy consistence.

Each polypus, though an integral portion of the common mass, and incorporated with it, is a distinct animal, endowed of itself with individual sensation and action. It consists of a fleshy stem issuing from the mass, crowned by a head like a horse-shoe, which is bordered by about 100 tentacula. Floating particles attracted by the mouth are conveyed into the stomach and intestinal organ, which are exposed within the body The common mass enjoys the faculty of very slow locomotion, either extremity indifferently being in advance; and thus are 300 animals or more subjected to its volition, by bearing along the whole in progression.

On dividing a specimen asunder, each portion receded, as if by mutual consent.

Twenty, thirty, or more, lenticular substances, imbedded in the flesh, are exposed through the translucent green of the animal, which may escape while it is vigorous, but which are liberated to float on the water toward the end of autumn, by its decay and decomposition. These are ova with a hard shell, and yellowish fluid contents. Their surface is brown, and the circumference yellow, begirt by a row of projecting double hooks.

In five or six months, one side of the liberated substance gapes as an oyster-shell to protrude an originating polypus, which, by a remarkable provision of Nature, floats with the head downwards, for absorbing the aliment below. When it is enabled to affix itself on quitting the egg, a second polypus appears beside the first, then a third, and thus of others, while the common base remains disproportionately large. Perhaps the earlier perfect formation is as a row of polypi around the smooth fleshy centre. Breadth seems diminished in proportion to the length of a specimen.

Thus it appears that the most luxuriant zoophyte—one composed of a thousand animals—originates by a single polypus only; and that the earliest recognition of its elements is as a circular spot or spherical segment.

Awaiting some future opportunity of illustrating the mode of increment peculiar to zoophytes, I shall only observe that, in the first stage of the Sertularia polyzonias, for example, a single enlargement forms the summit of the stalk. It is invested by a delicate membrane, which, instead of including a solitary head, covers a twin bud also. As the former increases, the latter forks off from it; next another from that which is the more mature, and thus with the rest. The increment of the Tubularia ensues only during the subsistence of the head. But the head is deciduous, falling in general soon after recovery from the sea. It is regenerated at intervals of from ten days to several weeks, but with the number of external organs successively diminishing, though the stem is always elongated. It seems to rise within this tubular stem from below, and to be dependent on the presence of the internal tenacious matter with which the tube is occupied. A head springs from the remaining stem, cut over very near the root; and a redundance of heads may be obtained from artificial sections, apparently beyond the ordinary provisions of nature. Thus 22 heads were produced through the course of 550 days, from three sections of a single stem. The reproductive powers of some animals are very great. It would be worthy of investigation whether, in some of the *Annulosa*, as they are now denominated, the whole elements of the entire animal do not reside in each segment. Fragments from the lower extremity of the largest specimens I could procure of the *Amphitrite Ventilabrum*, and others of that genus, have regenerated both the complex and beautiful plume, forming the branchiæ before and the secretory glands behind, as they may be conjectured, affording the glutinous matter for fabricating the tube. But neither can the singular *mechanical* properties of the former be used, nor do the latter seem of any avail under the artificial state of the redintegrated fragment.

All the preceding results, together with many others alike singular, are illustrated by drawings from the pencil of skilful artists.

Section E.—Anatomy and Medicine.

The Section met this morning at eleven o'clock, when the Chairman, Dr Abercrombie, after noticing the death of Dr Joseph Clarke of Dublin, which had occurred since his arrival in Edinburgh to attend the meeting of the Association, read a communication which Dr Clarke had left behind with his friend Dr Jackson, State Physician for Ireland, and himself, containing an Abstract of a Registry kept in the Lying-in-Hospital of Great Britain Street, Dublin, from the year 1758 to the end of 1833, and which illustrates in a very striking manner the importance of thorough ventilation in such establishments, and the great diminution of mortality among the children since this object has been attended to.

From that communication it appeared, that during the 75 years mentioned, relief has been afforded to upwards of 129,000 poor women; that in 1781, every sixth child died within nine days after birth, of convulsive disease, and that after means of thorough ventilation had been adopted, the mortality of infants, in five succeeding years, was reduced to nearly 1 in 20.

Dr William Thomson read a paper on the infiltration of the lungs with black matter, and on black expectoration, particularly as occurring in coal-miners, iron-moulders, and other workmen exposed by their employment to the inhalation of carbonaceous gases and powders; and he requested the co-operation of those medical gentlemen, who may have opportunities of observing the diseases of these classes of workmen in different parts of the country, in investigating the causes and nature of these morbid appearances.

Dr Thomson referred to a case of this kind, which had fallen under the observation of his father and himself in the year 1825, in a collier residing at Tranent; to a case described by the late Dr James Gregory, in the 109th number of the Edinburgh Medical and Surgical Journal; to two cases described by Dr William Marshall of Cambuslang, near Glasgow, in the number of the Lancet for 17th May 1834; and to various cases and observations contained in two communications in the number of the Edinburgh Medical and Surgical Journal now in the press, the one by Mr Graham, lecturer on chemistry in Anderson's Institution, and the other by Dr G. Hamilton of Falkirk, as well as in a number of hitherto unpublished communications received by his father and himself from professional gentlemen in different parts of the country. The author exhibited also a number of preparations and drawings illustrative of the appearances, nature, and seat, of this singular infiltration.

Dr C. J. B. Williams of London made a few observations on the subject.

Mr Dick next read a short notice on the use of the omentum or caul, which he illustrated by a comparison between the structure of this organ in the horse and in the sheep, shewing that in the horse, in which the omentum is small, the intestines are fixed, and undergo comparatively little change of place. The facts adduced by Mr Dick seemed to him to corroborate the opinion that the omentum served, by interposition between the intestines and abdominal parietes, to facilitate motion.

Sir Charles Bell then delivered a discourse explanatory of his views of the functions of the Nervous System, and of the manner in which this department of physiology should be studied. The further consideration of this subject was postponed till Friday at one o'clock.

Section F.—Statistics.

The Section met as usual at 11 o'clock. In the absence of Sir C. Lemon, Col. Sykes, V. P. took the chair.

The Secretary read a paper, communicated by Mr Gordon, the Secretary of the Committee of the Society for the Sons and Daughters of the Clergy, on the origin and the objects of the new Statistical Account of Scotland, now publishing under the superintendence of that society.

Earl Fitzwilliam suggested the expediency of furnishing more minute details with respect to the agricultural part of the reports. His lordship wished the statements to shew not only the total amount of land in cultivation, but also the quantities allotted at the time of the inquiry to the various kinds of produce, the number and value of agricultural implements, the number of draught and other cattle, and other similar details. His lordship stated that he had succeeded in obtaining the returns from some parishes in his own neighbourhood, and suggested that accurate and minutely detailed information from only a small number of places, would furnish more safe general inferences than could be obtained from a much more widely extended, but less precise, inquiry.

Mr Stanley thought there would be considerable difficulty in procuring such minute details, chiefly arising from the jealousy of the land occupiers; but undertook to prosecute such an inquiry in his own parish, and to furnish the results at the next meeting.

Dr Brunton thought that no such difficulty would occur among the tenantry of Scotland; and stated, that the thing proposed had been already done with commendable minuteness, as might be seen on turning to any of the parochial accounts already published.

Col. Sykes stated, that the returns which he had himself collected in the Deccan, embraced the stock and implements, and land in cultivation, and that the village constitution in India afforded peculiar facilities for obtaining minute information on all these points.

Mr Holt Mackenzie remarked, it was most desirable that statistical statements should be always the result of accumulated facts rather than of computation; for that statements of computation were only approximations to the mark, and in many cases lead to absolute error. The discussion on the subject was continued, branching into a variety of details, until the Section separated; in the course of which it was suggested by Lord Jeffrey, that a recommendation from the Section to the Society superintending the Statistical Account of Scotland, will not fail to meet with attention from the Society; and that a practical result of great value might thus immediately follow from the discussion that had taken place. The committee undertook to communicate with the Society in the manner suggested.

EVENING—GEORGE'S STREET ASSEMBLY ROOMS.

The President having taken the chair, and the reports of the various Sections having been read, Dr Buckland delivered a very animated and instructive lecture on fossil amphibia and fishes; after which the meeting adjourned till the following evening.

Friday, 12th September.

MORNING—UNIVERSITY.

SECTION A.—MATHEMATICS AND GENERAL PHYSICS.

Rev. Mr Whewell in the Chair.

Dr Knight exhibited to the Section a method of rendering the vibrations of heated metals perceptible to the eye.

Mr Russell gave an account of some recent experiments on the traction of boats on canals, at great velocities.

Sir David Brewster communicated to the Section the results of a series of experiments on the effects of reflexion from the surfaces of crystals, when some surfaces have been altered by solution, and exhibited a number of singular forms, produced by different crystals, or by the same crystal under different circumstances.

Mr Graves presented a paper on the theory of exponential functions, in further illustration of a memoir on the same subject, which he had laid before the Royal Society, and which had been printed in its Transactions.

Professor Hamilton explained a new method of conceiving imaginary quantities, and the principles of a theory which he denominated " the theory of conjugate functions." Professor Hamilton stated, that he had confirmed, by the aid of the theory, the results obtained by Mr Graves.

Mr Sang stated the results of some theoretical and experimental investigations which he had made on the nature of those curves traced by the extremities of vibrating wires fixed at the end, and he exhibited drawings of the forms of the curves thus produced.

Mr Sang noticed a property of the successive integer numbers, intended to facilitate the discovery of those that are prime.

Dr Williams read to the Section, a paper on the production and propagation of sound.

Mr Campbell gave an account of his views respecting the antilunar tides.

Professor Forbes having explained to the Section the principles of the sympiesometer of Mr Adie, and the objections to which it was subject, arising from the want of identity of temperature of the mercury in the air; then stated a modification which he had introduced in the construction of the instrument, whereby the correction for temperature might be effectually obtained.

Mr Dick explained a new construction of an achromatic object-glass, in which, by the refraction of the cementing fluid substance, he had been enabled to correct the secondary spectrum. The author exhibited a telescope with an object-glass formed on this principle.

Sir T. Brisbane made some verbal remarks on the subject of a siliceous sand procured at New South Wales, and which has been supposed to afford glass for optical purposes of a superior quality.

Mr Murphy stated the result of some recent observations of Mr Snow Harris, on the retention of electricity on the surface of bodies in vacuo.

Professor Lloyd took the advantage of the absence of other business, to allude to an unintentional omission in his report; and he stated some important observations of M. Arago, which had not been generally known.

Dr Robinson read a paper on the visibility of the moon in total eclipses.

Mr Whewell submitted to the section a paper on collision by Mr Hodgkinson.

SUB-SECTION.

M. I. Brunel, Esq. in the Chair.

Mr Murray gave a description of his apparatus for communicating between a stranded vessel and the shore; with a method of illuminating by night the path of the arrow and the vessel.

Mr Addams exhibited a new case of the interferences of sound, produced by waves excited by a tuning-fork in two tubes placed at right angles to each other.

Mr Dick communicated a description of a new elevated or suspension railway, which he proposes to construct, and illustrated the subject by numerous explanatory drawings.

Mr Brunel exhibited a model, and gave a description, of his new mode of constructing arches. Having explained his plan at the last meeting of the Association, he brought the subject forward on the present occasion merely for the purpose of reporting the results of his subsequent experiments. He stated, that the structure has now stood two winters and two summers, without any sensible alteration; that he is adding to his experimental model, with a view to demonstrate how far it can be extended, not only in the formation of arches, but likewise in other situations, such in particular in the great descents to the Thames Tunnel.

Mr Whewell exhibited and described a new instrument invented by Mr Saxton, for measuring minute variations of temperature in metal rods, &c.

A view of the weather, drawn from a register of ten years kept at Edinburgh, was laid on the table by Mr Adie, optician, in which the state of the barometer and thermometer were shown by undulating lines. The depth of rain was shewn on the day in which it fell, by the height of a broad red line; the thunder storms by a scarlet mark; the aurora by a blue one; and part of the space allotted to each day was tinted of a particular colour, to represent the direction of the winds, so that the views of the weather for the different years had only to be compared together, and it could immediately be seen which of them had been remarkable for heat, rain, steadiness, or the contrary.

A paper was read by Mr Alexander J. Adie, civil engineer, Edinburgh, on experiments made with a pyrometer heated by a current of steam on the expansion of stone and other substances. The quantities were measured by a micrometer, which reads the thirty-thousandth part of an inch, and given in decimals of the length for 180° F. He found that when a rod of straight-grained well seasoned oak was kept dry, it only expanded at about the fifteenth part of the rate of platinum; that the expansion of black marble was about half as much as that of glass; and that a rod of sandstone from Craigleith Quarry expanded very nearly at the same rate as cast-iron.

The Rev. G. Tough exhibited and described his celestial glass sphere, containing the earth, sun, and moon, with their relative motions.

Mr Badnall made some observations on the friction upon railways.

Section B.—Chemistry and Mineralogy.

The Chemical Section met at eleven A. M.

The Secretary announced that a set of standard thermometers by the first makers in London, Edinburgh, Paris, and Glasgow,—one of which had been compared with the standard thermometer of the Observatory at Paris, are at present placed by the Royal Society of Edinburgh in the hands of the Council of the Association; and that an opportunity will be afforded to the members of the Association to compare their own thermometers with these, during the week after the close of the meeting, on application being made to the Secretaries of the Chemical or Physical Sections.

Mr Harcourt then described the objects of the experiments now in progress under his superintendence, for determining the effect of long continued heat on various mineral substances, and the various methods adopted by him in disposing them beneath the iron furnaces of Yorkshire.

Dr Clark gave an account of Mr Nixon's process for smelting iron by the aid of the hot-blast, and exhibited numerical results of the advantages derived from the new process. The saving is so great, that the total amount of coal now necessary to produce one ton of iron, amounts only to 2 tons 14 cwt., whereas formerly, it required 8 tons $1\frac{1}{2}$ cwt., being a saving of 5 tons 8 cwt., for each ton of iron produced. This subject was discussed at considerable length.

Dr Christison then gave an account of some observations in regard to the action of various waters on lead, and of some practical results deducible from them, relative to the use of lead in the construction of water-pipes and cisterns, and the manufacture of carbonate of lead.

A communication was then read from Sir David Brewster, on the optical characters of minerals, which gave rise to considerable discussion.

Mr Graham gave an account of an investigation made by him into the constitution of certain hydrated salts. He stated that he had found,—That certain salts of sulphuric acid which crystallize with 5, 6, or 7 atoms of water, contain 4, 5, or 6 of these as water of crystallization, which are expelled at or below 212° under atmospheric pressure, and at 60° in vacuo: That one atom is left as essential to the constitution of the salt: That this remaining atom of water is expelled by a stronger heat, and is in general recovered on exposure of the anhydrous salt to the air; and that in every instance of a sulphate so constituted, the essential atom of water may be displaced by sulphate of potass, which, in the proportion of one atom, occupies the place of the expelled water, constituting a crystallizable sulphate, with a double base and six atoms of water of crystallization. The salts possessing these properties are the sulphates of zinc, iron, nickel, manganese, copper, lime, magnesia, cobalt.

Some applications of these and other facts were made by the author, to the doctrine of Isomerism; which led to a long and interesting discussion between Drs Dalton, Thomson, Turner, Clark, and Professor Johnston.

Mr Kemp next gave an account of a paper on a new mode of liquefying the gases, by which they may be obtained more easily, and in much larger quantity. He detailed the properties of several of the gases in the liquid state, illustrating more particularly the independent bleaching power of chlorine, and sulphuretted hydrogen when in a state of liquid, the power of some of the condensed gases as conductors of electricity, and the phenomena resulting where the condensed gases are brought in contact with one another, as well as with other substances.

The Section then adjourned till the 13th at half-past ten A.M.

Section C.—Geology and Geography.

Professor Jameson in the Chair.

Mr Dunn exhibited and described his new clinometer.

Mr James Bryce read a notice of some caverns containing bones near the Giants' Causeway.

Mr Horner, in reference to the same subject, read a communication from Mr Thomas Andrews, of Trinity College, Dublin, who had recently discovered some extensive caves in the Island of Rathlin, situated four miles from the Antrim coast, with a sea of thirty fathoms between. From the situation of the caves in Rathlin, it is evident that the sea must have once entered them at a much higher elevation than its present level.

Professor Phillips, in reply to a recommendation of the meeting at Cambridge, gave a general statement of his views of the relation of joints and veins. Rocks are too commonly considered merely as stratified or unstratified, though certain remarkable parallelisms in slate rocks, and the pillar form of divisions of trap-rocks, are too obvious to be left unnoticed by those who attend to the structure of rocks. Professor Phillips states it as a general law, that besides the planes of stratification, there is in stratified rocks another structure—a series of divisions more or less regular, often called joints. These are of various kinds.

1. Cracks which do not go through a whole bed of stone. Some of these called by the workmen dry cracks, have their surfaces often marked by dendritic appearances, occasioned by infiltrated oxide of iron, manganese, and other substances. Generally the sides of a crack are in apposition, but sometimes separate.

They are sometimes empty, sometimes filled with carbonate of lime, or other substances. That they have been produced since the bed was deposited, is easily proved; for the cracks sometimes divide shells, that must have been included in the limestone rock before the crack was produced. In the Rigi also, on examining the conglomerate, we find pebbles of various kinds, which are crossed by fissures or cracks, splitting them through and through. The stony matter interposed must be of later origin than the formation of the crack, and still more subsequent to the deposition of the rock. Metallic substances of the same kinds as those which occur in mineral veins, are often found in these cracks.

2. Joints which go through a whole bed, or through several beds, and even through the whole of the conformable beds. Where a

joint goes through limestone and shale, the opening is wider in the limestone, narrower in the shale.

3. Fissures which go through a great variety of strata, though very different, as sandstone, limestone, shale, &c. Viewing the matter on a horizontal plane, we find the divisions to be generally arranged in parallel lines. This is well seen in the vast limestone scars which begird the mountains of Yorkshire; certain of these, which may be called *master-joints*, are seen to be much more regular than the other joints, sometimes empty to an immense depth, sometimes filled by clay, holding a great proportion of pebbles. The sides are sometimes lined by calcareous spar and sulphate of barytes, &c., and in some cases the deposition of calcareous spar has gone to such an extent, that the whole cavity is filled with it. If a bed of shale is interposed between beds of limestone, the fissure is much wider in the limestone than in the shale. Carbonate of copper, oxide of iron, and other substances, also occur in the joints. A certain direction of the joints is common to the slate, to the limestone, to the carboniferous series, and even to the oolitic rocks of the north of England. Through the whole of the country examined by Mr Phillips between the Tees and the south of Yorkshire, there is the most general agreement, the direction of nearly all the master joints being to the NN. W., which is now the direction of the magnetic needle. Some general and long continued cause appears to have been in operation, which has produced this constancy of direction. Other joints very extensive, but less so than those ranging NN. W., belong also to the division of master-joints.

In mineral veins in the north of England, a direction passing from the east, but a little to the north of it, is the most common. The miners call these right running veins. Other veins and rock dykes and faults occasionally cross them, and these generally coincide with the direction of the great master-joints. There is in one part of Yorkshire an immense quantity of basalt. The metallic veins pass nearly east and west through this, as they do through sandstones, limestones, shales, &c. Other portions (connected in the opinion of Professor Sedgwick with this great mass) go off from it in directions so nearly straight, that a surveyor might use them as lines for the basis of his operations.

There is an analogy between the direction of the great mineral fissures and the lines of convulsive movement in the north of England. There is a general line of dislocation north by west, from which passes another to the east, and another to the west south-west. Mineral veins are exceedingly numerous between these lines, and especially near to that having the easterly direction. In the valley of the Tyne they are parallel to the northern line, and the cross courses, on the other hand, are parallel to the primary line of dislocation.

Mr John Taylor states that this east and west direction of mineral veins is common not only to the whole of Britain, but also to Mexico; and Professor Phillips, reasoning upon the whole of the facts at present known to him, expressed his conviction that they seemed to indicate the operation of important agencies as yet not brought into account in geological reasoning.

Remarks were made by Mr Smith, Mr J. Taylor, and Dr Boase.

Mr Maclaren exhibited sections of the Pentland Hills, and made some remarks on their structure. These hills, he stated, are about fifteen miles in length, and from three to six in breadth. The fundamental rock is transition slate accompanied by greywacke in vertical strata, which are covered unconformably by conglomerate and various felspar and claystone porphyries, in beds dipping to the south-east, at angles varying from 10° to 35°. Beds of conglomerate, alternating with greywacke, abound in the western part; in the eastern, the greywacke is accompanied chiefly by felspar, claystone porphyries, and amygdaloids. A vast mass of sandstone forms the termination of the chain on the west, and rises to the height of nearly 1800 feet in the two Cairn Hills. The age of the hills, or the period of their elevation, is indicated by the position of the secondary rocks on their flanks. The sandstone of the Cairn Hills inclines against the transition rocks, at a considerable angle, on the north side, and at Craigintarrie appears in beds almost vertical. On the south side, the older strata of the coal formation are found at various places, in a position highly inclined or vertical, while a newer portion of the same series is found in horizontal beds, or dipping in towards the hills at a low angle, and in juxtaposition with the former. It follows that the elevation of the transition rocks took place at a period subsequent to the deposition of the older, but previous to the deposition of the newer, part of the coal formation.

Mr Murchison gave an abstract of Dr Rogers' report on the geology of North America, and read extracts from this valuable and elaborate memoir. The following are the conclusions drawn by the author.

1. The deposits of New Jersey differ from those of the southern states, in being chiefly arenaceous, and in containing an immense quantity of the pure chloritic mineral called green sand.

2. The organic remains hitherto discovered are nearly all, with the exception of one or two species, peculiar to this continent.

3. The existence of great quantities of lignite, of the remains of scolopax a shore bird, and the position of these beds in New Jersey contiguous to the primary boundary or ancient coast, all indicate that these beds were deposited in a comparatively shallow sea, analogous in position to the present extensive line of soundings which skirts the coast. The obvious shallowness of the portion of the secondary ocean where these beds were formed, may perhaps help to explain the remarkable discordance alluded to between the American and European marine species of this period.

4. The calcareous masses of Alabama, at least the upper beds, are possibly different in age from the marls and arenaceous beds of New Jersey.

5. The marl formation of New Jersey is perhaps most nearly represented by the European green sands. The limestone deposits of the south, on the other hand, resemble more the upper members of the cretaceous group, for example, the formation of the plateau of Maestricht.

6. Thus far there is no evidence of the existence of true chalk in North America. Genuine flints have not yet been found in any bed.

7. Volcanic forces, during this period, seem to have been nearly dormant, which may perhaps assist in accounting for the absence of the chalk.

8. The want of coincidence both in organic remains and mineral character, between these beds and the cretaceous group of Europe; the difficulty of deciding their identity at present, from the want of a sufficient knowledge of the structure and superposition of our formations; and above all, the importance of pursuing our geology free from the shackles of a nomenclature originally adapted to another continent, render it desirable that we reject the terms in use, and appropriate to this group of formations a name which shall be independent of old associations, and yet express their position in the geological series.

Mr Lyell expressed the high opinion he entertained of the labours and theoretical views of Professor Rogers. As it appears that a very small number of the tertiary fossils of North America agree specifically with those of Europe, Mr Lyell coincides with the author in thinking, that the only approximation that can at present be attempted towards ascertaining the relative age of the tertiary groups of the two continents, is that derived from a comparison of the relative proportion of recent to extinct shells. At the same time, Mr Lyell fully concurs with Mr Rogers in his opinion, that such a correspondence ought not to be insisted upon, as affording any positive test of exact contemporaneous deposition, since the rate of change in species cannot be assumed to have been always equal, especially in remote regions, during equal periods of time.

Mr Lyell, in speaking of the adoption by Professor Rogers, of his (Mr Lyell's) nomenclature of the leading divisions of the tertiary formations, remarked, that the principles of his classification had been sometimes mistaken by geologists. Mr Lyell had used the numerical proportion of recent to extinct fossil shells, as a useful term of comparison between distinct tertiary groups, but never as the principal character of a particular epoch. Thus, for example, the important character of the Eocene strata in Europe, was not the circumstance that those strata contain about three per cent. of living species of shells, but that they contain 800 or more species peculiar to the Eocene period, occurring in formations of that era in distant regions, and not found in other tertiary groups.

Captain Maconochie, Secretary to the Royal Geographical Society, gave an account of the origin and progress of that Association. He then communicated some details relative to the late expedition to the Niger, and to the expeditions which are about to be sent out to the interior of Africa, and to British Guiana.

Lieutenant Allan, the fellow-traveller of Lander, exhibited some panoramic views of the scenery of the Niger.

Mr Murchison presented a tabular view of the order of succession of various formations of great thickness, and distinct from each other in their organic remains and mineralogical characters, which rise from beneath the old red sandstone of England and Wales. He then dwelt on the series of fishes occurring throughout the old red sandstone of England, and pointed out Dr Lloyd of Ludlow as the person who had first called his attention to them. These fishes, it now appears, are common to the central portion of the old red sandstone of England, and the strata occupying the same geological position in Forfarshire and other counties in Scotland. Mr Murchison further expressed his opinion, that the Arbroath pavement is the equivalent of the Tile stones, or lower member of the old red sandstone of England.

Professor Jameson exhibited a fossil fish, the *Cephalaspis* of Agassiz, which he had found in the old red sandstone (Forfarshire) several years ago, long after he had determined that the sandstone of Caithness, Orkney, Shetland, and of whole tracts of country on the east and west of Scotland, were of the same geognostical age: and Mr Blackadder exhibited a fossil fish from Glammis millstone quarry, in the same district.

M. Agassiz made some observations on the fossil fishes of Scotland, and the following is a summary of the general conclusions he has formed as to these remains.

The high geological antiquity of the greater part of the stratified mountains of Scotland, gives a peculiar interest to the investigation of their organic remains; as they lead us to the knowledge of the condition of our planet, at a period in regard to which we possess only a few insulated fragments of information. The mollusca, zoophytes, &c. of these formations, have been examined by many; but the remains of vertebrate animals have been but little investigated, and of fishes, we are acquainted with those only which have been described and figured by Messrs Sedgwick and Murchison, and which have also been noticed by Cuvier and Pentland. The occurrence of a large number of these was known, but no particular information as to their nature was communicated. For a long period I have been anxious to have an opportunity of examining these interesting fossils; and this has been afforded me by the meeting of the British Association at Edinburgh. I have now to offer a short notice of the results of my examination, and at the same time to thank geologists for the kindness and liberality with which they have assisted me.

The collections which have afforded me the most important materials are the following: That of the Royal Society, which, through the unwearied exertions of the Secretary, Mr Robison, contains many remarkable remains from Burdiehouse; the collection of Dr Hibbert, which also is rich in fossils from the same locality; Dr Traill's collection, containing many interesting fishes from Orkney; Lord Greenock's extensive series of ichthyolites, from the coal formation, and especially from Newhaven. In Professor Jameson's possession I saw a large head of a fish from the old red sandstone of Forfarshire, and of which Messrs Murchison and Sedgwick have shewn me a less perfect specimen, but one which exhibits the other parts of the body. Mr Torrie submitted to my examination an extensive collection of fossil fishes from Caithness, similar to those described by Messrs Sedgwick and Murchison ; and also some fishes from Gamrie, of which Mr Murchison possesses a very perfect collection.

Of the fossil fishes not from Scotland, which I have seen on the present occasion, I shall take another opportunity to speak.

As to the determination of the Scottish fishes, I must remark, generally, that they all belong to two orders of the Class, viz. some to the order of Placoidian fishes, Agass. (Cartilagineux, Cuv.); but the larger number to the division Ganoidian fishes, Agass., and two to the section Heteocerei, in which the upper lobe in the caudal fin is longer than the lower.

In the old red sandstone, there are two species from Glammis, Forfarshire, viz. one species of the genus Cephalaspis (Ganoidian), which has hitherto been found in this formation only. The most remarkable characters of this genus are the shield-like covering of the head, and which is prolonged backwards in the form of two horns as in the Trilobites, and the manner in which the eyes are placed near each other on the head. The other species belongs probably to the genus Hybodus (Placoidian), but of this I have seen only an Ichthyodorulite.

The fishes from *Caithness* and *Orkney* approach one another most nearly; though amongst the latter there are several new genera, and in all eight species. Those from Caithness seem to belong to two species only. Amongst the Orkney fishes there are two very remarkable genera resembling the *Acanthodes* of the coal formation, also having very small scales; but the new *Cheiracanthus* is furnished with a spine in the pectoral fin only, and the other, the Chirolepis, instead of having the spine is provided with a row of small scales. I have been convinced by the examination of many specimens, that the genus Dipterus has two dorsal fins. In Orkney there are also species which have two dorsal fins and two anal fins, which sometimes are opposite one another, and sometimes alternate; and these are types of two genera, the *Diplopterus* and the *Pleiopterus*.

The fishes from *Burdiehouse* are also very numerous; in their characters they agree with those of the coal formation, but are more removed from those of Saarbrück than are the remains found at Newhaven. The most remarkable amongst them is an animal which, from the structure of its teeth, might be considered as a reptile, and which must have been of very considerable dimensions; but which, from its skeleton and its scales, is decidedly a fish. This animal forms a new genus under the name *Megalichthys*, and confirms the opinion I formerly expressed, that we observe in older deposits organic remains which, with the usual characters of their family, unite the characters of the types which have made their appearance at a more recent period. Unfortunately no perfect specimen of the Megalichthys has been found, and it has not been possible to bring together all the different parts of the skeleton. Another new genus, related to the Amblypterus, has a long dorsal fin extending beyond the ventral fin and the anal fin, and may be named the *Euronotus*. The other species belong to the genera *Pygopterus* and *Amblypterus*. Very large Icthyodorulites occur not unfrequently, and seem to belong to the genus Hybodus.

At *Newhaven* eight species occur, of which some bear a considerable resemblance to the fossil fishes of Saarbrück; though still distinguished from them by some characters. They belong to the genera *Pygopterus*, *Amblypterus*, and *Palaeoniscus*; and there is one species which will in all probability form a new genus, as it differs considerably from the genus *Acrolepis*. Placoidian fishes are also found, but only in fragments, so that I have not been able to determine them; and there are two other species of which small traces only have been obtained.

In the coal formation of *Fifeshire* a new specimen of *Palaeoniscus* has been found.

It may appear strange that I should consider the Gamrie fossil fishes as belonging to the coal formation, but they seem to be so nearly related to that deposit that I cannot regard them as of much more recent origin. There are three species, viz. one Cheiracanthus, one Palaeoniscus, and a third of which perfect specimens have not yet been obtained.

From this short notice, it must be evident how important the study of fossil fishes of Scotland is for advancing our knowledge of the beings which existed before the oolitic period, and how much we may yet expect from future careful investigations.

Dr Knight of Aberdeen read a notice on the Flints found in various parts of Aberdeenshire, and more especially in the vicinity of Peterhead. He particularized the fossils found in them, and exhibited an interesting series of specimens.

Mr Saul exhibited drawings of the incisors and canine teeth of the fossil hippopotamus, from a gravel pit near Huntingdon.

Mr Hall's model of a part of Derbyshire was exhibited.

The Secretary exhibited an impression of a fossil plant, supposed to be new, from Ayrshire, and sent by Dr Thomson of Glasgow; also an unusually large skull of the ox from a marl pit in Caithness, sent for inspection by Mr Robison.

Dr Buckland laid before the Section a drawing by Mrs Turner of Liverpool, of a large fossil marine plant, found in the new red sandstone of that neighbourhood in 1829.

Although the paper of which the following is an abstract, was in part read in the Natural History Section, yet, as its contents are chiefly geological, we deem it preferable to give an account of it in this place. It is entitled, "Account of the Natural History of the central portion of the great mountain range of the south of Scotland, in which arise the sources of the Tweed." By W. Macgillivray, Esq.

The mountains forming the most elevated part of this range are situated in the parishes of Tweedsmuir, Megget, and Manner, which form the southern and south-eastern parts of the inland county of Peebles, and are continuous with the high land forming the celebrated pastoral districts of Yarrow and Ettrick in Selkirkshire, and with the higher parts of the parish of Moffat in Dumfries. This region was described as composed of uniform, smooth, rounded hills of greywacke, scarcely ever precipitous or even abrupt, clothed to the summits with junceæ, cyperaceæ, grasses, heath and pasture plants, and separated into groups or ridges by long, narrow, straight valleys, which, although generally green, seldom present any natural wood, even along the clear streams that flow into the valley of the Tweed. The valley of Manner water was taken as a characteristic specimen of the numerous depressions by which the country is grooved. Whitecoom, Hartfell, and other mountains, were described, and the alpine plants observed on them enumerated, with the view of contrasting this region with the Grampian range. The vegetation of the cleuchs or ravines was also described. The Tweed was then followed from its sources, to Peebles, and finally to the mouths of the Gala and Ettrick, in the whole of which space the rocks are composed of greywacke, greywacke-slate, clay-slate, slate-clay, and occasional small beds of limestone, none of which, however, are wrought. The districts of Yarrow and Ettrick, which are of precisely similar geological structure, were then described, with reference to their scenery, vegetation, and animal productions.

Perhaps few districts in Scotland of equal extent present less varied geological phenomena than that which contains the sources of the Tweed. The general direction of the strata is from S.-W. to N.-E.; they are usually highly inclined, sometimes vertical, and often horizontal, but present every degree of inclination; the general dip is to the north-west. The composition of the greywacke exhibits considerable variety.

In form, the hills approximate in a considerable degree to many of the granite masses of Aberdeenshire, but they never present the precipices and corries, which characterize the more elevated of the latter. The whole district, with its rounded, smooth-sloped mountains, connected in elongated heaps, its long, narrow, straight, or slightly tortuous valleys, its argillaceous and pebbly soil, its clear and rapid streams, and its grassy vegetation, with the absence of natural and the scarceness of planted wood, forms a strong contrast with the mountainous districts of the middle and northern divisions of Scotland, in which peaked and serrated and ridgy mountains, with precipices and corries, rugged and winding valleys, slopes covered with debris, and patched with heath and bracken, brown or limpid streams fringed with birch and alder, rivers and lakes with cataracts and islands, dark forests of pines and thickets of briars, with other remarkable features, still, and will for ages, give interest to the ancient land of the Gael.

The object of this paper, was principally to shew the propriety of taking the geology, botany, and zoology, of a district in connection, the limited views which botanists too often take of the economy of nature teaching them to consider as entirely destitute of interest the rocks, the soil, the elevation, the temperature, and the animal productions of the country whose vegetation they pretend to describe.

[The following short notice by W. Gilbertson, Esq., which, with many others, owing to want of time, was not read, we now communicate to our readers; and at the same time we request the author to send to Edinburgh the specimens alluded to.]

I beg leave to lay before you a few fossils for the purpose of illustrating an elevation of the strata of this country since the creation of the present existing race of animals. The situation is in the county of Lancashire, and betwixt the Lune and the Mersey; the greatest elevation at which these fossils have yet been found is 350 feet above the sea, in the excavations made by the Preston Water Company, at the foot of Langridge Fell, and from whose measurements this elevation was ascertained. The shells are interesting, as being of the same species as those now found on our shores; and shewing, therefore, that this elevation has taken place since the creation of existing species; and the stations and roads of the Romans being upon this deposit, prove that no change has taken place in the form of existing species during the period that has elapsed since they occupied this country.

GEOLOGICAL EXCURSIONS.

Owing to the unfavourable state of the weather, the shortness of the period of meeting, being only six days (it ought to have been twelve), and the pressure of business in the Sections, the extensive geological excursions previously planned by Professor Jameson, and which he was to have led, were not carried into effect. A few short geological walks, however, were made in the immediate neighbourhood of the city.

Section D.—Natural History.

On the Coculus Indicus of commerce, by G. Walker-Arnott, Esq.

On the head of Delphinus deductor, on the laryngeal sac of the rein-deer, and on a new species of thrush from Nepaul, by Dr Traill.

Dr Allen Thomson exhibited some specimens of the following reptiles:—

Ampiuma means (didactylus of Cuvier), Menopoma (of Harlan), Menobranchus lateralis, and Proteus anguinus; and made some remarks upon the place which these animals and the Cæcilia hold among the other Batrachian reptiles.

Dr Thomson then exhibited a few specimens and drawings of the young of the common thornback, at the period when the external branchial filaments exist. He described the connexion of these filaments with the internal gills, and the circulation of the blood in the single vessel running through each of the fifteen filaments that project from the side of the neck; which he had observed in the animal, kept alive for some days.

Information regarding the progress made towards the publication of the posthumous works of Cuvier, by J. B. Pentland, Esq.

On the transformations of the crustacea, by J. O. Westwood, Esq.

Mr Pentland, in continuation of the observations which he offered at a preceding meeting of this section, on the physical configuration of the Andes of Peru and Bolivia, and on the distribution of organic life at different elevations, on the declivity of these gigantic chains, entered into details on the reasons which have led him to conclude that there existed at a comparatively recent period, and between the 14° and 19° degrees of S. Lat. a race of men very different from any of those now inhabiting our globe, characterised principally by the anomalous form of the cranium, in which two-thirds of the entire weight of the cerebral mass is placed behind the occipital foramen, and in which the bones of the face are very much elongated, so as to give to these crania more the appearance of certain species of the ape family, than that of human beings. Mr Pentland entered into details to prove that this extraordinary form cannot be attributed to pressure, or any external force, similar to that still employed by many American tribes, and adduced, in confirmation of this view, the opinions of Cuvier, of Gall, and of many other celebrated naturalists and anatomists.

The remains of this extraordinary race are found in ancient tombs of the mountainous districts of Peru and Bolivia, and principally in the great interalpine valley of Titicaca, and on the borders of the lake of the same name. These tombs present very remarkable architectural beauty, and appear not to date beyond seven or eight centuries before the present period.

The race of men to which these extraordinary remains belong, appears to Mr Pentland to have constituted the inhabitants of the elevated regions, situated between the 14° and 19° degrees of South Lat. before the arrival of this present Indian population, which, in its physical characters, its customs, &c. offers many analogies with the Asiatic races of the old world.

Mr Pentland took occasion to defend M. Humboldt from some accusations of inaccuracy in his measurements of the heights of several points in the Andes of the neighbourhood of Quito, contained in Colonel Hall's paper read on a previous day.

On some peculiar secretions and elaborations, viewed in connexion with the ascent of the sap. By John Murray, Esq.

On a new species of pecten. By T. Brown, Esq.

On the progress of successive vegetation, at various heights, on the Hymalayan Mountains. By J. F. Royle, Esq.

Some observations on the structure of feathers. By Sir David Brewster.

Section E.—Anatomy and Medicine.

Dr Alison read a notice of some observations made by himself and Mr Dick, veterinary surgeon, on the vital condition of arteries leading to inflamed parts, referred to at Tuesday's meeting, and one of which had been repeated by the Committee then appointed for the purpose, with a short statement of inferences, as to the essential nature of inflammation, thence deducible. The result of these observations was, that the contractile power of the larger arteries supplying inflamed parts,—both their elasticity, and that additional power which they possess during life, and retain for some hours after apparent death,—appears to be decidedly *less* than that of the corresponding arteries of sound limbs. The members of the Committees resident in Edinburgh were directed to repeat and vary the experiments detailed in both the papers by Dr Alison, and to report the results of their observations to the Section at next meeting.

Dr Macdonnell communicated the results of a long continued series of observations made by him since the year 1784, on the influence which posture exercises on the quickness of the pulse, and on the connexion between the frequency of the pulse and of the respiration both in health and in disease, and suggested certain practical applications of these views to the distinction of diseases.—Dr W. Thomson suggested, that as the pressure of business rendered it impossible to enter so fully on the consideration of this very important subject as was desirable, Dr Macdonnell should be requested to prepare a fuller account of his experiments and observations, to be laid before the Medical Section at the next meeting of the Association, which proposal was approved of.

Dr Bushnan exhibited some specimens of small animals which he had found in human blood; when some remarks were made by Dr Anderson, suggesting doubts as to whether these animals had not found their way into the blood subsequently to its being drawn from the body. The Chairman suggested the necessity of farther observations for determining whether the animals in question can be regarded as entozoa.

Dr T. J. Aitkin communicated the result of his inquiries into the varieties of mechanism by which the blood may be accelerated or retarded in the arterial and venous systems of the mammalia. After stating that his attention had for some time been directed to this subject, which he conceived to be of great interest to the physiologist, he particularly drew the attention of the Section to four modifications of arterial distribution, as indicated, 1*st*, By the angle at which a branch comes off from its trunk; 2*dly*, The direction of the vessel; 3*dly*, The subdivision; and 4*thly*, The formation of plexus. In illustration of the first, or angle of origin, he exhibited a preparation of the aorta of the tiger, in which the superior intercostals arose at an acute, the middle at a right, and the lower at an obtuse angle; from which he inferred that the force and velocity of the blood are rendered equal through the whole series. In speaking of the second circumstance, or the direction, he adverted to the tortuous entrance of the internal carotid and vertebral arteries into the skull in the human subject, and shewed that it is still more remarkable in the horse, which in feeding requires to have the head for a considerable time in the dependent posture. But the best examples of the tortuous or serpentine course, are to be seen in the spermatic arteries of the mammalia. This mechanism, he contended, adapts the circulation to the various positions in which organs may be placed, and to their states of action and repose. In speaking of the third modification, or the subdivision into numerous long branches, he particularly alluded to the observations of Sir Anthony Carlyle with respect to the arteries of the sloth, and shewed that a similar ramification is found in the hedgehog, both in the arteries of the panniculus carnosus and of the mesentery. Of the last modification, the plexus, he shewed examples in the rete mirabile of Galen in the internal carotid, and of Hovius in the ophthalmic artery, of the ruminantia. He inferred that this structure prevents vascular turgescence, which would otherwise occur, during the long period when these animals keep their head in the dependent position while browsing. He also shewed that a rete mirabile exists in the ophthalmic artery of the seal and goose, and considered it probable that in them it is conducive to the alternate adaptation of the eye to vision in air and water. He described the remarkable plexiform arrangement which exists in the mesenteric arteries and veins of the hog; and instituted a comparison between these vessels in carnivorous and herbivorous mammalia, concluding that these modifications are in conformity with the transmission of the blood through the liver, the rapidity of the peristaltic motion, and the power of nutrition.

Dr Hodgkin read a part of the history of the results of the experimental inquiry respecting the action of poisons, the prosecution of which was committed by the Association at its last meeting to himself and Dr Roupell, so far as he himself had been able to prosecute the inquiry. Conceiving the object which the Association had more particularly in view in calling for this report, to have been to facilitate the recognition of the effects of acrid poisons, with a view to aid in judicial inquiries, Dr Hodgkin considered it as an essential preliminary, to obtain an accurate and definite knowledge of the different appearances which are presented by each part of the alimentary canal within the limits compatible with health; and accordingly, in the first part of his report, he entered at considerable length into the consideration and description of these. Dr Hodgkin's memoir was illustrated by wax models and drawings of beautiful execution.

Dr Yelloly made some remarks on the importance of distinguishing in morbid investigations between the redness produced by simple vascularity and that which results from inflammation, and illustrated the subject by a drawing of a portion of the spinal cord of a person who met a violent death, when in the enjoyment of perfect health.

Dr Allen Thomson made some remarks on the development of the human fœtus, and exhibited a preparation of a very small human embryo, which was contained in an ovum about a month old; but which, from its structure, appeared to have been blighted at the twelfth or fourteenth day. The structure of this fœtus was illustrated by a reference to specimens of the fœtus of the cat, sheep, and birds, at early periods, the object of Dr Thomson's remarks being to shew that the human embryo, in the early stages of development, passes through changes similar to those ascertained to take place in other vertebrated animals.

Sir Charles Bell continued his discourse on the principles which should guide physiologists in investigating the functions of the nervous system, referring particularly to the necessity of a knowledge of the minute anatomy of the brain, to the ascertainment of the functions of its several parts.

Professor Syme made some remarks on the operation of removing portions of the joints, and combated the objections urged against this operation, as insufficient for the removal of the whole of the diseased parts, and as leaving the patients in possession of a limb rather cumbersome than serviceable. Professor Syme exhibited to the Section several patients on whom the excision of the elbow and shoulder-joint had been performed some time previously, in order to prove that neither of the objections adduced was valid.

SETCION F.—STATISTICS.

The Statistical Section met at 11 o'clock. Col. Sykes, V. P. in the chair.

Mr Drinkwater gave an account of the origin and present state of the Statistical Society of London. He stated that this society, which was only founded in March last, already contained nearly 450 members, and that it was actively employed in encouraging the establishment of similar societies in every part of the United Kingdom, with a view, through their means, of conducting extensive inquiries on a general and systematic plan.

Captain Maconochie gave an account of M. Guerry's Essai sur la Statistique Morale de la France, and pointed out some of its most striking results, illustrated by several maps of France, coloured with different shades, so as to indicate the comparative amount of instruction, and of crime against persons and property. He also explained from what data these maps had been constructed, bearing testimony to the absolute freedom of M. Guerry from any bias towards particular systems.

It appears by a comparison of six following years, if the whole of France be divided into five several divisions or regions, that the proportion of all the crimes committed in France which belong to each region is very nearly the same from year to year; in no case differing from the mean by more than $\frac{4}{100}$ in crimes against persons, and $\frac{2}{100}$ in crimes against property.

Captain Maconochie then went through a great many very interesting details with respect to the various districts of the country, the sex, the ages, and the season of the year, at which different crimes are found to prevail. M. Guerry confirms what has been already remarked by Mr Quetelet in Belgium, that the summer months are much more productive of crimes against persons, and the winter of crimes against property. It appears that crimes against property are three times as numerous as crimes against the person. M. Guerry sees no reason to believe that crime is increasing in France, but justly remarks, that a more vigilant police, and greater publicity given to those crimes which are committed in later times, may have given rise to this opinion.

It is also necessary, especially in comparing distant epochs, to notice changes in the institutions and laws of the country.

M. Guerry mentions the large and increasing number of second accusations, but observes, that a man once condemned to the galleys seldom renders himself liable a second time to that punishment.

Almost every crime is committed more frequently by men than women: crimes against children are equally divided between the two sexes. In 100 crimes against persons, men commit 86, and women only 14: in crimes against property, men commit 79, and women 21. Two-fifths, or nearly half of all the crimes committed by women against the person are infanticides.

The greatest ignorance in France is on the west coast and in the centre, and not in the south, as has been supposed; the same districts shew the least amount of crime. The greatest amount of crime is in Corsica and Alsace.

In both sexes, the greatest number of crimes is committed between the ages of 25 and 30, which short period embraces nearly one-fifth of the whole.

It is impossible to give an accurate notion of the various interesting comparisons given in this work, without extending this report too far.

M. Guerry concludes by warning his readers not to be too hastily led away to the conclusion that education has a tendency to develope instead of repressing crime, remarking, that the utmost limit warranted by his observations is, that education is a mighty instrument, powerful either for good or evil according as it is directed, and that, unless, whilst we inform the intellect, we also take pains to cultivate the moral sentiments, and to touch the affections of the heart, we bestow only a doubtful advantage on its object.

Mr Auldjo read an account of Potindo's work on the revenue and population of the kingdom of Naples, referring to that part of the kingdom lying north of the Straits of Messina, giving details of the state of the population, and the public institutions. These statements shew the amount of population, the extent of the country, the quantity cultivated, uncultivated, in forest land, and capable of being cultivated; giving comparisons in these respects with the Grand Duchy of Tuscany, the kingdom of Sardinia, and the dominion of the Church. They also shew the number of landed proprietors, renters of land, labourers, and paupers. An examination of the revenue shewed it to be in a prosperous state, and that the funded debt of the nation would probably be redeemed in fifteen years.

The Secretary read a paper by Mr Murray, on the different rates of mortality in the higher and lower classes of society, shewing that the author's observations agreed with those of Dr Villerme in representing the most opulent classes as the longest lived. Mr Murray hoped to lay the details of his observations before the next meeting of the Association.

These remarks gave rise to some discussion, in the course of which Mr Humby observed that in Lancashire and Cheshire those receiving the highest wages in manufacturing towns were often improvident and dissipated, and consequently short-lived.

The Secretary read a paper by Mr Grut, on the tables which have been recently published by the Equitable Insurance Office in London, pointing out the vast importance of the results that might be obtained from the experience of other similar societies, and suggested schedules of inquiries that might with advantage be submitted to them; adding a list of insurance offices in London and various parts of the country, with the dates of their establishment.

EVENING—GEORGE'S STREET ASSEMBLY ROOMS.

The President having taken the Chair, the business commenced by reading the reports of the proceedings of the Sections. Dr Abercrombie, in concluding the report of his Section, went on to say, "that, in thus concluding the reports of the Medical Section, he found it necessary to state, that the whole business of that Section had been conducted in the most satisfactory manner, and that a great variety of most important communications had been laid before them; but, considering these as not adapted for a mixed assembly, he had alluded to them in very few words. Having, therefore," he continued, "intruded but little upon your attention, I trust you will indulge me for one minute while I express, in the name of the Medical profession of Edinburgh, the high satisfaction we have received from the meeting which is now drawing to a close, especially by having been brought into personal intercourse, and I trust personal friendship, with so many distinguished individuals, whose names have long been familiar to us as holding the highest rank in physical science. From their combined labours we expect the most important results to every department of human knowledge. I am none of those who anticipate from the researches of physical science, any thing adverse to the highest interests of man as a moral being. On the contrary, I am convinced that those who have made the greatest attainments in true science will be the first to acknowledge their own insignificance, when viewed in relation to that incomprehensible One who guides the planet in its course, and maintains the complicated movements of ten thousand suns and ten thousand systems in undeviating harmony. Infidelity and irreligion, I am satisfied, are the offspring of ignorance united to presumption; and the boldest researches of physical science, if conducted in the spirit of true philosophy, must lead us to new discoveries of the power and wisdom, and harmony and beauty, which pervade all the works of Him who is Eternal."

Professor Whewell then delivered a lecture on several curious phenomena connected with the tides; and concluded his remarks by expressing, in very warm terms, the feelings of gratitude entertained by himself and other strangers of the Association, for the kind and hospitable reception they had met with in Edinburgh.

Professor Sedgwick gave a general account of the proceedings of the Geological and Geographical Section, and expressed his conviction that geology would benefit much from the discussion which had animated the Geological and Geographical Section. He concluded by re-echoing the sentiments of Dr Abercrombie, that the pursuits of science, instead of leading to infidelity, had a contrary tendency,—that they went rather to strengthen religious principles, and to confirm morals.

The President announced that the concluding General Meeting of this Association would take place in the College Library at three o'clock to-morrow.

Saturday, 13th September.

Section B.—Chemistry and Mineralogy.

A communication from Mr Fox was read on the electro-magnetic condition of mineral veins, and the Section agreed to recommend the continuation of these experiments.

Mr Stevelly explained the construction of a new vernier, and its application to Dr Wollaston's scale of chemical equivalents.

Some observations by Mr Henry H. Watson on Sir John Leslie's hygrometer, were then read by the Secretary.

Professor Johnston gave a notice of the results of a paper he communicated to the Section, on the dimorphism of the sesquiiodide of antimony.

Mr Low exhibited some interesting products of gas flues and retorts, and of long continued heat.

Dr Gregory exhibited a series of specimens of organic principles,—after which the Section closed its meetings.

Section E.—Anatomy and Medicine.

The reading of Dr Hodgkin's memoir on the action of poisons was concluded.

In the second part of this report, Dr Hodgkin began with describing the general and local effects produced by injecting hot water into the stomach of the dog. He next proceeded to direct attention to the inferences which may be drawn from the situation of the principal lesion of the stomach in poisoning, shewing, that where the agent is intensely active, this is observed in the greater curvature immediately opposite the orifice of the œsophagus, rather than precisely at the cardiac extremity, where, in other cases, the most intense injection is generally met with; whilst, on the other hand, if the poison be not sufficiently strong at once to destroy the powers of the stomach, its effects will be found most conspicuous in those parts which, under ordinary circumstances, are the most frequent seats of injection. Another circumstance on which Dr Hodgkin particularly insisted, was the occurrence in cases of inflammation of the stomach, of an interstitial deposition of lymph, producing the appearance of small, irregular, opaque whitish spots in the substance of the mucous membrane itself. This appearance he considered as of importance, as furnishing, in some instances, a ground of distinction between the effects of decided inflammation and mere congestion. The character of the secretion found on the surface of the mucous membrane also, Dr Hodgkin considered capable of throwing considerable light on the condition of the membrane before death.

Professor Clarke read to the Section a portion of the report which, at the previous meeting of the Association, he had been requested to prepare on the present state of Physiological Science. Dr Clarke explained that it had been drawn up in the idea that it was intended to be laid before a general meeting of the Association, and not before the Medical Section in particular, and that he had consequently endeavoured to give it a more popular character than he otherwise would have felt justified in doing.

A similar explanation was given by Dr Yelloly, respecting the report on the state of Pathology, which he had been requested to prepare.

Dr Yelloly, on the part of the Strangers of the Medical Section of the British Association, proposed that a resolution should be entered on the minutes, expressive of their warmest acknowledgments for the very kind manner in which they have been received in Edinburgh, which was unanimously agreed to.

Dr Abercrombie concluded the business of the Medical Section by an address, in which, after some allusion to the subjects which had more particularly engaged their attention, he expressed his confidence in the zeal of the members in following out the investigations which had been recommended to them; and he impressed upon them the importance of a zealous cultivation of pathology, as the only foundation of certain knowledge respecting the phenomena of disease. Dr Abercrombie then proceeded to make some observations on the interest and importance to the medical profession of the study of Mental Philosophy. In alluding to this subject, he said he was aware of the objections which had been brought against admitting the philosophy of mind as one of the regular Sections of the Association; and to a considerable extent he admitted their truth, as it might be difficult to preserve such discussions from those hypothetical speculations by which this important science had been so much obscured and retarded in its progress. But, by treating it as a branch of Physiology, he trusted this might be avoided, by rigidly restricting the investigation to a careful observation of facts, and the purposes of high practical utility to which they might be applied. Keeping in view the importance of these rules, he earnestly recommended the subject to medical inquirers, as capable of being cultivated, on strict philosophical principles, as a science of observation, and as likely to yield laws, principles, or universal facts, which might be ascertained with the same precision as the laws of physical science. For this purpose, however, inquirers must abstain from all vain speculations respecting the nature and essence of mind, or the mode of its communication with external things, and must confine themselves to a simple and careful study of its operations. Some of these Dr Abercrombie alluded to under the following heads:—the laws of the succession of thoughts, and the remarkable influence of association;—the voluntary power which we possess over the succession of thought, the due culture of which lies at the foundation of all sound mental discipline;—the influence of habit upon mental processes, and the means of correcting injurious habits;—the important relation between voluntary intellectual processes and moral emotions, and between such intellectual processes and the result of evidence in producing conviction;—the laws of reason or judgment—the means of cultivating it—and the ruinous effects which result from the neglect of such culture. In concluding these observations, Dr Abercrombie alluded briefly to the moral phenomena of the human mind, and the impressions which we derive from them, with a feeling of absolute certainty, respecting the moral attributes of the Creator.

Respecting the means of cultivating the Philosophy of Mind as a science of rigid observation, Dr Abercrombie alluded to the study of mental phenomena and mental habits in ourselves and in other men; and the whole phenomena of dreaming, insanity, and delirium, and the mental conditions which occur in connection with diseases and injuries of the brain. The subjects of dreaming and insanity, which have hitherto been little cultivated with this view, he considered as capable of being prosecuted on sound philosophical principles, and as likely to yield curious and important results respecting the laws of association, and various other processes of the mind.

The practical purposes to which mental science may be applied, Dr Abercrombie considered briefly under the following heads:—(1.) The education of the young, and the cultivation of a sound mental discipline at any period of life. In all other departments, we distinctly recognise the truth, that every art must be founded upon science, or on a correct knowledge of the uniform relations and sequences of the essences to which the art refers; and it cannot be supposed that the only exception to this rule should be the highest and most delicate of all human pursuits, the science and the art of the mind. (2.) The intellectual and moral treatment of insanity, presenting a subject of intellectual observation and experiment, in which little comparatively has been done, but which seems to promise results of the highest importance and interest. (3.) The prevention of insanity in individuals in whom there exists the hereditary predisposition to it. He gave his reasons for being convinced that, in such cases, much might be done by a careful mental culture, and that irremediable injury might arise from the neglect of it. (4.) Dr Abercrombie alluded to the importance of mental science as the basis of a Philosophical Logic, but did not enlarge on this part of the subject. He concluded his address by some observations on the dignity and importance of medicine, as one of the highest pursuits to which the human mind can be directed; as it combines with the culture of a liberal science, the daily exercise of an extensive benevolence, and thus tends at once to cultivate the highest powers of the understanding, and the best feelings of the heart.

COLLEGE LIBRARY.

The last meeting of the Association was held in the large and splendid hall of the College Library, the galleries of which were set apart for the accommodation of ladies. The doors were thrown open about half-past two o'clock, when a great rush was made for admission, and, a little after three o'clock, when the business commenced, the hall was filled. A short time before this, the Lord High Chancellor Brougham made his appearance on the platform.

It was announced to the meeting by the President, Sir Thomas Makdougal Brisbane, that invitations to the British Association had been received from the Bristol Institution, the Literary and Philosophical Society of Liverpool, the Royal Dublin Society, the Royal Irish Academy, the Geological Society of Dublin, and the University of Dublin. That the general committee, having considered the circumstances under which the invitations were brought forward, had unanimously resolved that the invitations of the constituted scientific authorities in Dublin should be accepted, and that the next meeting of the Association should be held in Dublin on Monday, 10th August 1835. That the thanks of the Association had been voted to the Bristol Institution, and to the Literary and Philosophical Society of Liverpool.

The Officers and Council appointed for the ensuing year were then announced, viz.

Dr Lloyd, *President Elect.*
Lord Oxmantown and Rev. W. Whewell, F.R.S., *Vice-Presidents Elect.*
Professor Hamilton, F.G.S., Professor Lloyd, F.G.S., *Secretaries for the Dublin Meeting.*
Treasurer to the Dublin Meeting, Dr Orpen, Treas. R.I.A.
Treasurer to the Association, John Taylor, F.R.S.
General Secretary, Rev. E. V. Harcourt, F.R.S.
Assistant Secretary, Professor John Philips, F.R.S.
The Secretaries for Edinburgh, Professor Forbes and J. Robison, Esq., were requested to continue their valuable services till the Dublin Meeting.

The Council consists of—

Trustees.—Professor Babbage, Mr R. I. Murchison, Mr John Taylor.
Members elected.—Mr Robert Brown, Dr Bentham, Dr Buckland, Professor Airy, Mr W. Clift, Mr Drinkwater, Dr Hodgkin, Mr S. Christie, Mr Greenough, Mr Lubbock, Dr Roget, Rev. G. Peacock, Mr G. Rennie, Mr Yarrell.
Ex-officio Members.—The Officers of the Association.
Secretaries to the Council.—Dr Turner, Rev. J. Yates.

The Rev. Vernon Harcourt, general secretary, stated the results of the proceedings of the general committee on the subjects brought before them for consideration by the sectional committees, as to grants of money, requests for reports on the progress of science, and recommendations of special subjects of scientific inquiry. They had authorised the appropriation of part of the funds of the Association for the purpose of prosecuting particular researches in physical, chemical, geological, zoological, botanical, and medical science, to the extent of L.830. They had authorised the application for the continuation of reports on various branches of science to Rev. G. Peacock, Rev. J. Challis, Rev. R. Willis, Mr George Rennie, Professor Rogers, and Mr Stevenson; for a report on the application of mathematical science to the phenomena of heat, electricity, and magnetism,—on electro-chemistry and electro-magnetism to Dr Roget,—on the zoology of North America to Dr Richardson,—on the botany of North America to Professor Hooker,—on the geographical distribution of plants to Professor Henslow,—on the geographical distribution of insects to Mr J. Wilson,—on the pathology of the nervous system to Dr C. Henry,—and on the effect of circumstances of vegetation on the medicinal virtues of plants to Dr Christison.

Various recommendations of special subjects for inquiry were sanctioned by the general committee, and ordered to be printed in the next volume of the publication of the Association.

Dr Buckland then rose to propose the thanks of the Association to the patrons and officers of the University, for the handsome and liberal way in which they had given up to them the use of the rooms in the University. He said that throughout the week they had been received in the metropolis of Scotland, in a way which was worthy of the far-famed hospitality of the nation. They had been welcomed to the houses and to the tables of the inhabitants—nay, the very rocks of the country had welcomed them by opening before them their valuable treasures; they had seen that the Grampians had formerly had spices waving on their tops, while at their bases the crocodile swam; and a thousand fishes had started from their rocky sepulchre, to bid welcome to the members of the British Association for the Advancement of Science. After some farther remarks, the learned Doctor concluded by proposing his motion, which was seconded by Dr Lloyd, Provost of Trinity College, Dublin.

Votes of thanks were also voted to the College of Physicians, Highland Society, and Proprietors of the Assembly Rooms, for their great liberality in accommodating the Association.

Votes of thanks were next voted *seriatim* to the Presidents and other office-bearers of the Association.

The vote of thanks to the Presidents was proposed by Mr Whewell, and seconded by Professor Hamilton of Dublin. The latter addressed the meeting nearly as follows:—

I rise, in compliance with this call, to second the motion of my friend Mr Whewell; and I do so with the sincerest pleasure, though I should not have presumed, if uninvited, to come forward on this occasion. How indeed, can I fail to feel deep pleasure in seconding a motion, which directs your attention and your thanks to a triad of men such as these—a triad containing one, a countryman and friend of my own, who, along with all that mathematical attainment, and all that scientific diligence, of which Mr Whewell has spoken, combines a flow of eloquence to which we all during this week have often been delighted listeners,—a triad containing also another Vice-President, a native of Scotland, but whose name and fame are not confined to Scotland, and who is known over the whole of Europe as the person who, by his experimental researches and sagacity, has done more perhaps than any living man for the science of physical optics; for that wonderful science, which, illustrating each by each, the more beautiful phenomena of light, and the subtlest properties of matter, enable us almost to feel the minute vibrations, the ceaseless heavings and tremblings of that mighty ocean of ether, which bathes the farthest stars, yet winds its way through every labyrinth and pore of every body on this earth of ours,—a triad, finally, containing as its head and organ that scientific soldier, of more than European reputation, who, while entrusted with the sword of his sovereign, and with the glory of his country in a far distant land, had founded an observatory in another sphere before he came to preside in this. It would be difficult to narrate the benefit which has been conferred on science by our President in the erection of that observatory. Had it not been for that observatory, the comet of Encke, at one of its late returns, would have eluded human scrutiny; since, though it was then visible in the southern, it was invisible in the northern hemisphere. Had it not been for that observatory, we should want several of our most important elements for determining the amount of astronomical refraction; that property of our earth's atmosphere, which, changing variously the course of light, bends the rays of Sirius here towards our pole, but bends them there towards the other. I remember well, though I was then but a young astronomer, the anxiety with which my dear and illustrious predecessor in the astronomical chair of Dublin awaited the arrival of those Paramatta observations; the delight with which he received them, and eagerness with which he proceeded to combine them with his own, and so to form that union of northern and southern observations, which will hand down together to the remotest ages of astronomical history, the names of Brinkley and of Brisbane.

There is, however, another view of Professor Whewell's motion, which my feelings will not permit me to pass by, though it must have occurred to all of you,—that view in which our Edinburgh President may be regarded as one of the representatives of the hospitality of Edinburgh. This hospitality has indeed been often alluded to already, but it will bear to be touched upon again; especially if—endeavouring to rise above that sense of personal obligation by which we all, all visitors of Edinburgh, would feel ourselves almost oppressed, if that splendid hospitality had not been as delicate as it was splendid—we contemplate it for a moment in a higher view, as a symbol of national union. This week the bond of scientific brotherhood has shewn to the world a new and striking picture—has brought a new thing to pass, in the history of this Association, nay more, in the history of this empire. I must not touch so far upon forbidden ground as to bring to your remembrance a time which was not like this,—a time when Englishmen and Scotchmen met, if they met at all, for other purposes, and in another spirit,—a time when walls like these would have rung with other shouts, with cries of national defiance, with voices menacing a dire and doubtful struggle. That time indeed has long passed, never to return. England and Scotland are not now two countries, distinct, independent, and hostile; they have been long one glorious nation, at unity within itself, impregnable to the world beside. Yet, is it not a new and touching sight, this festival of love, this solemn offering of concord, this crowding of the invited might of England into the metropolis of Scotland, this interchange not of peaceful courtesy alone, but of zealous friendship, this active and ardent cooperation in one high common cause? We, too, the representatives of Ireland, have joyfully responded to the call, and gladly gathered in this beautiful and ancient capital, and with ungrudging hearts have seen it take precedency of our own in the great work of hospitality to science, though in that work we long to bear a part. And when we find our birthplace, and its institutions, and its efforts for science, so held in honour and regard, as your resolution to visit it has testified, we feel a joyful and exciting hope that the reception which you will let us give to you in Dublin, even more (if that be possible) than the reception which you have given to us in Edinburgh, shall symbolise and mark a more embracing unity, a close and triple bond, and tend to make us *all* in heart and mind one people.

Professor Sedgwick, in proposing the thanks of the Association to M. Arago and the other distinguished strangers who had visited them, made some pertinent and eloquent remarks upon the advantages of science in smoothing the prejudices of different nations, and linking together learned men of all countries, and paid a high compliment to the merits of M. Arago.

The Lord Chancellor rose, to second the motion. After apologising for not sooner appearing at the meetings of the Association, which he said was attributable to accident, he remarked that he understood he owed the honour of seconding the motion of his reverend and learned friend to the circumstance—one of the proudest in his life—that he was a member of the National Institute of France. It had been often remarked, that war was a game, at which, if the people were wise, governments would not often play; and he might add, that in encouraging and fostering the exertions of men of science, who were of no party, and over whom the angry tempests of war passed innocuous, a government was taking the best means to facilitate that which ought ever to be their chief aim—peace on earth, and good will among men. He might remark also, that as among individuals, the older they grew, they became the more sensible that life was too short to be spent in personal quarrels; so he was happy to say, that the world was now too old, and too experienced, for neighbouring states to engage in war with little or no ground of quarrel. A great part of this softening influence was to be attributed to science, which formed a bond of brotherhood between learned men of all countries. It was, therefore, on scientific principles, and on the principles of an enlightened philanthropy, that he cordially seconded the motion of his reverend friend.

M. Arago returned thanks in a very energetic speech.

On the motion of the Rev. Dr Robinson, seconded by Sir Charles Lemon, thanks were voted to the Rev. Wm. Vernon Harcourt for his continued and unremitted exertions as General Secretary.

The President then addressed the meeting as follows:—Gentlemen, as the humble organ of this great intellectual body, I rise to return thanks for the reception the Association has received at Edinburgh. I have had the good fortune to attend the whole of the meetings of the Association at York, Oxford, Cambridge, and I am proud to think that Edinburgh has not fallen short, but has far exceeded the most sanguine expectations of any member of the Association, which is most gratifying to myself as a Scotchman; and the pleasure is enhanced by the honour I have this day received, namely, the Freedom of the City which I now hold in my hands. I therefore, congratulate the Association on its increasing prosperity; but how can it be otherwise, when so many distinguished individuals are found in our society, and when even the Lord Chancellor attends our meeting. The distinguished foreigners who have assisted in our labours, have all expressed their desire to co-operate with the Association in its different objects, and my friend M. Arago, who is a most distinguished deputy of France,—a Philosopher and Astronomer unrivalled on the Continent of Europe, whose name is a host in itself, and whom I have had the happiness of knowing these nineteen years, has desired me to convey his own willingness and that of the Institute of France, to co-operate in our labours, and what may not be accomplished by such combination of talent? I trust, therefore, that henceforth these great nations, France and England, will never become rivals but in emulating which of the two shall contribute most to the comfort and happiness of the human race. To the whole of the public authorities, our grateful acknowledgments are offered, and I speak with confidence when I say none of us will ever forget the reception that Edinburgh has given us. I have now to perform the only painful duty which has been imposed on me during the week, namely, to adjourn the society, which is hereby adjourned to the 10th August 1835, at Dublin.

The meeting then separated.

The Freedom of the City of Edinburgh conferred on M. Arago, Mr Brown, Dr Dalton, Professor Moll, and Sir Thomas Makdougal Brisbane, Bart.

An extraordinary meeting of the Town Council was held on Saturday, September 13th, the Lord Provost in the Chair. The Magistrates and Councillors having put on their robes of office, Sir Thomas Brisbane, alongst with M. Arago, Professor Moll, Dr Dalton, and Mr Brown, were introduced to the Council by Bailie Thomson. They were attended by Professor Jameson, Sir John Campbell, member for the city, and others.

The Lord Provost addressed these gentlemen, expressing the high sense entertained by his Lordship and the Council of the British Association, its importance, and its objects. It could not fail to be gratifying to observe how individuals, eminent for their learning, congregated together for the purpose of promoting scientific investigations. In these "piping times of peace," these men were now enabled to assemble from all quarters, not of this country merely, but also of the continent of Europe, and so to combine their efforts as to bring out results at once beneficial to the present race, and to the posterity which was to follow, the good accomplished not being confined to the passing day, but extending to future times. Viewing the proceedings of the British Association in this light, the Council had felt anxious to do all in their power to mark their approbation, by conferring the only honour which it was in their power to bestow, upon some of the distinguished members of that body, feeling satisfied, that, in so doing, they were conferring honour on themselves, while they attempted to do so to them. In selecting the individuals, they had certainly had much difficulty, from the great number of men of the first-rate talents and acquirements who had honoured their city by their presence. His Lordship trusted, however, that the selection which had been made, of the very distinguished President, and the four accomplished Fellows of the Association, Messrs Arago, Moll, Dalton, and Brown, would give universal satisfaction. Men more eminent in their respective branches could not easily be found. He, therefore, would now proceed to place in their hands the extract of the Town Council, conferring upon them the freedom of the Town. The extract was then read by the clerk, and a diploma, as freeman of the city, presented to each by his Lordship. His Lordship presented the diploma to M. Arago as the chief of physical science on the continent of Europe—to Mr Brown, as placed by the universal consent of botanists at the head of botanical science—to Dr Dalton, as founder of the atomic theory—to Professor Moll, as celebrated for his discoveries and writings; and to Sir Thomas Makdougal Brisbane, famed as an astronomical observer and discoverer.

On its being presented to M. Arago, he craved leave to address the Council, which he did in French. Sir Thomas Brisbane begged leave to act as interpreter, and stated that M. Arago expressed the high sense he entertained of the very great honour now conferred upon him, of being enrolled as a citizen of Edinburgh—a city which had been distinguished by such eminent names as Stewart, Leslie, &c. He now felt as a citizen of Edinburgh, and should any of its sons ever visit Paris, it would be sufficient for them to ask for M. Arago, to receive the utmost attention he could bestow upon them.

The diplomas granting them the Freedom of the City, having been all delivered, Sir Thomas Brisbane addressed the Lord Provost, expressing how highly honoured he felt in being placed at the head of the British Association. They had been received here in the most handsome manner. He was not disposed to make any invidious comparisons with the former meetings of the Association; but he might be allowed to say, that the meeting at Edinburgh was fully equal to any which had preceded it. As a Scotsman, he felt an interest in any thing relating to Scotland or its capital; but from the present moment, and after the honour now conferred on him, he would feel that a special tie connected him with the city of Edinburgh, in the prosperity of which he would always take a deep and lively interest, and be at all times ready to promote it. Sir Thomas Brisbane then acknowledged the honour conferred upon the Members of the Association, and the gentlemen retired.

EDINBURGH OBSERVATORY.

To the Editor of the Edinburgh Philosophical Journal.

Sir,

Knowing that an account of the proceedings of the British Association while assembled at Edinburgh, is to appear in your Journal, and that some remarks made in one of the meetings by Dr Robinson on the Edinburgh Observatory will probably have a place in that account, I beg leave to say a few words in reply to the observations of the learned Astronomer of Armagh; and I do this, not from a wish to enter into controversy, but in compliance with the recommendation of some friends who have for many years taken a warm interest in the establishment of the Edinburgh Observatory.

The whole of Dr Robinson's objections apply to the position in which the Observatory is placed. Now, it is well known to every person in Edinburgh, that it was utterly impossible to raise funds by *private subscription* for erecting an observatory in any other position than that on which it now stands. The people in Edinburgh, with a wish to improve science, have also a desire to adorn their city with elegant structures. Those who erected the Observatory judiciously availed themselves of this twofold stimulus, and directed it to a purpose creditable to Scotland, and beneficial to the whole world. Dr Robinson's objections express merely his own opinion. The Observatory has not yet been actively employed: it is only a few weeks since an astronomer was appointed to it; and its principal instrument has just been erected. Within the last twelve years it has been visited by many astronomers, British and foreign. Of the former, I may mention the late Mr Woodhouse and Professor Airy of Cambridge; Dr Brinkley (now Bishop of Cloyne), when Professor of Astronomy in Dublin; Sir Thomas Brisbane, and various members of the Astronomical Society of London. Of foreign astronomers, I recollect M. Gautier of Geneva, M. Tralles of Berlin, M. Quetelet of Brussels. None of these raised any objections such as were made by Dr Robinson. I believe I may say with confidence, that M. Arago was satisfied that the Observatory will fulfil the purpose for which it was erected; and that Professor Moll did not in the least agree in the opinions brought forward in the meeting of the British Association.

In this state of the case, it seems prudent to suspend farther discussion on the subject until the Observatory has had a fair trial. The proposition to convert an elegant structure, that has cost L. 5000, and that has been erected on a rock highly magnetic, into a Magnetic Observatory, will, I believe, find no supporters, and, when duly considered, not even the sanction of the learned proposer.

On the whole, I am confident, that, had Dr Robinson taken the trouble to make himself acquainted with the difficulties that have been surmounted in the erection of the Edinburgh Observatory, he would have told the meeting, that the men of Edinburgh had done for the improvement of astronomy what had been attempted before in Britain, but never accomplished—witness the Glasgow Observatory, of which the instruments have been long ago sold by public roup or private bargain, little to the credit of that opulent city.

William Wallace.
Professor of Mathematics.

Edinburgh, 24*th Sept.* 1834.

Proceedings of the Society for the Encouragement of the Useful Arts in Scotland.

The following communications were laid before the Society during the months of March, April, and May 1834.

March 12.—1. A Map of the Levels of London and its Environs, was exhibited, as a specimen of the mode of executing a proposed map of the levels of Edinburgh and environs; which will be done by subscription, if sufficient encouragement be given. By Mr William Moffat, 8 Middle Row, Knightsbridge, London.

2. Description of a Portable Warm-Air Stove, heated with gas; adapted at same time for other domestic purposes. Invented by Mr Robert Ritchie, ironmonger, 241 High Street, and 86 George Street, Edinburgh, Memb. Soc. Arts.

3. A Spring Slider for the Bottom of Doors, which, when the door shuts, slides down, and prevents a draught of air into the room or house. Communicated by C. G. Stuart Menteith, Esq. of Closeburn, Memb. Soc. Arts.

4. Supplement to his Essay on Improvements on the Syphon. By Mr Jonathan Davidson, ironmonger, High Street, Edinburgh.

A working model will be exhibited.

March 26.—1. On the Source of the Advantages of a Long Screw-Driver. By Mr Edward Sang, Teacher of Mathematics, Edinburgh, Memb. Soc. Arts.

2. A Grand Orrery, representing the Solar System, and calculated to convey an extensive and accurate knowledge of the Movements of Celestial Bodies. Invented and constructed entirely by Mr John Fulton, Fenwick, Ayrshire.

3. Suggestions regarding the substitution of Steam in place of the Breath in Blowing Glass; with a Sketch of the Apparatus. By T. S. Largs.

4. Donation.—The first Number of the Architectural Magazine, and Journal of Improvement in Architecture, Building, and Furnishing, and in the various Arts and Trades connected therewith. Conducted by J. C. Loudon, Esq. Hon. Memb. Soc. Arts. From the Conductor.

Samuel Eaden, Esq. Red Hill, Sheffield, was admitted an Ordinary Member.

April 9.—1. Suggestions in regard to the Adaptation of the newly-invented American Steam-Boat to Canal Navigation. By Mungo Ponton, Esq. W.S., F.R.S.E. Memb. Soc. Arts.

2. A new Pivot Castor for Furniture, possessing the advantage of retaining the Oil for an indefinite length of time. By John Robison, Esq. F.R.S.E., Vice-Pres. Soc. Arts.

3. A Pocket or Portable Cooking Apparatus, used on the Continent, was exhibited in operation. By Mr Robert Ritchie, ironmonger, &c. to their Majesties, 241 High Street, and 86 George Street, Memb. Soc. Arts.

4. Donation.—Presentation copy of the Literary Gazette and Journal of Belles Lettres, Arts and Sciences, &c. No. 888. From the Publishers.

April 23.—1. Description and Drawing of a Machine for Compressing Air by means of the Rise and Fall of the Tide, communicable to any distance. By Mr Thomas Aitken, Pittencrieff Street, Dunfermline.

2. Model and Description of a Machine for facilitating the Cutting of Curved Wood. By Mr William Kemp, Galashiels.

3. Description and Drawings of a Safety Frame for the Boilers and Vats of Dyers; and of a Safety Cover for the Boilers of Bleachers and Brewers, &c. By Mr Thomas Johnston, 137 George Street, Glasgow.

4. Letter from Mr James King, Sydney, N. S. Wales, dated 19th July 1833, respecting the merits of a discovery of the superior fitness of Sand, near Sydney, for the Manufacture of Flint and Plate Glass. With a printed copy of a letter from him to Lord Viscount Goderich, late Secretary of State for the Colonies, dated 1st July 1833; and relative Testimonials, printed and written.

5. Eight Specimens of various Prepared Earths; in particular of one called Ethereal Blue, which possesses all the properties of the Ashes of Ultramarine, has many advantages over it, and can be manufactured and sold at about one-fifth of the expense. Prepared by Mr Murdo Paterson, dyer, Inverness.

The following candidates were admitted Ordinary Members, viz.

John Hamilton, Esq. W. S., 1 Scotland Street.
Alexander Campbell, Esq., George Square, Edinburgh.

May 21.—1. Mr Ford of Bailey and Co. reported the result of their Experiments in making Glass with the Sydney Sand; and Specimens were exhibited.

2. Dr D. B. Reid reported the result of his Experiments in analyzing the Sydney Sand, and the Lynn Sand.

3. Mr Watson Gordon reported upon Mr Murdo Paterson's Prepared Earths, including his Ethereal Blue, &c.; and exhibited Specimens prepared with Oil, with their relative tints when mixed with white.

4. Donation.—Specimens of Lithographic Printing. By Mr Samuel Leith, Lithographer, Banff, Assoc. Soc. of Arts.

5. Donation.—Nos. 2 and 3 of the Architectural Magazine. From the Conductor, J. C. Loudon, Esq. Hon. Member Soc. Arts.

The following candidates were admitted Ordinary Members, viz.

Robert Horsburgh, Accountant, 15 London Street, Edinburgh.
Captain Alexander Milne, R. N., Inveresk.

An extraordinary meeting of the Society for the Encouragement of the Useful Arts was held in the Royal Institution, on Friday the 5th September 1834, Sir David Milne, K.C.B., Vice-President, in the Chair.

The following communications were laid before the Society:—

1. Essay on the Useful Arts—preliminary to the Series of Annual Reports regarding new Inventions and Improvements in the Arts throughout Europe—ordered by the Society. By Edward Sang, Esq. Teacher of Mathematics, and Lecturer on Natural Philosophy, Edinburgh, Memb. Soc. Arts.

2. Drawing and Description of Improved Sextant Telescopes, for taking Altitudes when the Horizon is invisible. By Matthew Adam, Esq. Rector of the Inverness Academy, Assoc. Soc. Arts.

The Instruments were exhibited.

3. Specimens of Glass manufactured by Messrs Bailey and Co. Edinburgh, from Sydney Sand and from Lynn Sand. Communicated by Dr D. B. Reid, Memb. Soc. Arts.

4. Donation.—On Differences and Differentials of Functions of Zero. By William R. Hamilton, A.B., Royal Astronomer, Dublin, Memb. Soc. Arts. for Scotland. From the Author.

5. Donation.—Specimens of Lithography. Presented to the Society by Mr Samuel Leith, lithographer, Banff, Assoc. Soc. of Arts.

6. Donation.—Specimens of Lithography. By ditto.

7. Donation.—Nos. 4 and 5 of the Architectural Magazine. From the Conductor, J. C. Loudon, Esq. Hon. Member Soc. Arts.

8. The Committee on Mr Smith's Instrument for cutting Coats was continued—Mr Sang, Convener.

9. The Report of the Committee on Mr Fulton's Orrery was read and approved of.

10. The Report of the Committee appointed to prepare a List of Prizes to be offered by the Society for Session 1834-35 was read.

11. The following Report of the Prize Committee, awarding the Prizes for Session 1832-33, was read, and the Prizes were delivered to the successful Candidates, by the Vice-President, with appropriate addresses, viz. :—

Report of the Committee appointed by the Society of Arts for Scotland, to award Prizes for Communications read and exhibited during Session 1832-33.

Your Committee having maturely considered the various communications, together with the reports of the respective Committees thereon, have, in terms of the remit made to them, awarded prizes to the following individuals, viz. :—

1. To C. G. S. Menteath of Closeburn, Esq. Memb. Soc. Arts, for his Model and Description of the lower part of a Limekiln with Double Grates and Doors ; and for his Model and Description of a Rail-road or Wheel-tracks for all sorts of Carriages :—both read and exhibited 6th February 1833—The Society's First Honorary Medal.

2. To Mr James Ballingall, Manager of the Kirkcaldy and London Shipping Company, and Surveyor of Shipping for the port of Kirkcaldy—for his Plan for Improving the Mercantile Navy of Great Britain and Ireland :—read 6th February 1833—The Society's Second Honorary Medal.

3. To Mr James Catleugh, Millwright, Haddington—for his Description and Drawing of a Machine for Bruising Corn and Malt :—read 23d January 1833—The Society's Third Honorary Medal.

4. To Mr Alexander Grant, Surgeon and Druggist, 11 Broughton Street, Edinburgh—for his Model, Drawing, and Description of a New Chimney-top or Can :—read and exhibited 26th February 1833—The Society's Fourth Honorary Medal.

5. To Mr James Macdonald, tailor, 46 West Register Street, Edinburgh—for his Drawing and Description of an Instrument for taking the Dimensions of the Human Body, with precision, for the purpose of fitting it, to absolute nicety, with Clothes :—read and exhibited 20th March 1833 ; and, since that date, made by Mr Macdonald of full size, with sundry improvements, under the name of the Andrometer—calculated to be of the greatest service in the army, &c. &c.—The Society's Gold Medal, value Ten Sovereigns.

6. To William Austin and Co., formerly of 17 Waterloo Place, Edinburgh—for their Description and Sketch of an Improved Family Mangle—The Society's Silver Medal, value Five Sovereigns.

The Committee beg leave to report, that upwards of a dozen of papers have been lodged by candidates for the prize of L. 20, offered for the best Alphabet and mode of Printing for the use of the Blind. Several of these papers are voluminous, and require much time and attention for their consideration. In consequence of which, the Committee appointed to report thereon have not yet been able to make up their report, and they are at present in correspondence with gentlemen in England well qualified to judge of the merits of these papers, with the view of enabling the Committee to fix upon the Alphabet and mode of Printing which may appear to be that which shall promise to be most extensively useful, and most generally desirable for the whole of Great Britain and Ireland.

In the mean time, the decision of the prize must lie over till that report is lodged, which will probably be early next session.

The Committee, in conclusion, are sorry to be obliged to report, that there has been no competition for the following prizes, viz.:—

1. The Keith Gold Medal of Twenty-five sovereigns.
2. The Society's second Gold Medal of Ten sovereigns.
3. The Lithographic Prize of Twelve sovereigns.
4. Ditto, ditto, Eight sovereigns.
6. Hats of fine quality, Honorary Medal.
7. Ditto of second quality, ditto ditto.

All which is humbly reported by

JOHN DUNN, *pro Convener.*

Royal Institution, Edinburgh,
7th June 1834.

The following distinguished men of science were proposed by the Council for election as Honorary Members, and were unanimously elected, viz.:—

1. M. Arago, Sec. Perp. de l'Institut de France.
2. M. Payen, Membre de la Societé d'Encouragement pour l'Industrie Nationale.
3. M. Francoeur, Membre de la Societé d'Encouragement pour l'Industrie Nationale.

The Society then adjourned till next Session.

List of Prizes offered by the Society for the Session 1834–35.

THE Society for the Encouragement of the Useful Arts in Scotland, offer the following Prizes for the Session 1834–35:

1. For the most important Discovery in Mechanics;—*The Keith Gold Medal, value Twenty Sovereigns.*

2. For the best set of Experiments on any branch of Practical Mechanics;—*The Society's Gold Medal, value Ten Sovereigns.*

3. For the best Specimens of Busts, and other fine Ornamental Castings in Iron;—*The Society's Gold Medal, value Eight Sovereigns.*

4. For the best Specimen of Lithographic Drawing and Printing, by Lithographic Artists resident in Scotland, of subjects in *Civil and Naval Architecture, Landscape, Machinery, and Maps,* from Ink Drawings; the size of each to be not less than 14 inches by 10. Three impressions of *each* of these subjects to be sent;—*The Society's Gold Medal, value Twelve Sovereigns.*

5. For the best ditto ditto, of subjects in *Portrait, Historical and Landscape,* from Chalk Drawings; the size to be not less than 9 inches by 6. Three impressions of *each* of these subjects to be sent; —*The Society's Silver Medal, value Eight Sovereigns.*

N. B.—The two Prizes last mentioned are from a Fund furnished by the Association for the Improvement of Lithography in Scotland. *Specimens intended to compete for either of these two Prizes must be lodged on or before the* 1*st of March* 1835. The successful Candidates shall be bound to furnish, if required, 50 impressions of each subject which shall be found entitled to either of the above Prizes,—for which they shall be paid an extra sum, to cover the outlay for paper and printing. The Society of Arts retain to themselves the power of withholding the whole or any part of the above Prizes till a future time, if there be not more than *three* competitors, or if the Specimens produced do not appear to be of sufficient merit.

6. For the best Paper on the treatment of Steel in the forming and hardening Tools for different purposes;—*The Society's Silver Medal, value Ten Sovereigns.*

7. For other Inventions, Discoveries, or Improvements in the *Mechanical* or *Chemical* Arts; or by which the *Natural Productions of Scotland* could be made more available to the Useful Arts than at present,—the Society will be ready to expend a further sum, in Premiums and Honorary Medals, of *Ten Sovereigns.*

General Observations.—The attention of Candidates is particularly directed to the following subjects, as a specimen of what the Society would desire to be brought before them; but Candidates are by no means limited to these subjects, viz.:—Best construction of Screw-plates, Taps and Dies, &c.—Means by which the expense of Diagrams, &c. for Books of Science, &c. may be lessened.—Economizing Fuel, Gas, &c.—Observations on *correct* representations of *Natural Objects*, for the ornamenting of Ceilings and Walls of Rooms, in the Printing of Cloth, Painting of China and Stone-ware, &c.—Improvements in Balance or Pendulum Time-keepers.—On the best Composition for Rollers employed in applying Ink to the Types in Letter-press Printing, especially with a view to preserve their adhesive and elastic properties in as uniform a condition as possible during damp and other variable states of the atmosphere.—Steam-boats for Canals, somewhat on the principle of the American Steam-Raft, and capable of sailing at the rate of 15 miles an hour, &c.

The Society shall be at liberty to publish in their Transactions copies or abstracts of all Papers submitted to them.

In the event of any communication not being considered of sufficient merit to entitle it to the whole Prize for which it competes, the Society reserve the power of lessening the Prize.

The Society particularly request, that all communications intended to compete for the above Prizes (except the 4th and 5th Prizes) may be forwarded to the Secretary as soon after the Society meets in November 1834 as possible, in order to insure their being read and reported on during the Session. At same time, they will be received any time from July 1834 to 1st April 1835.

Communications, Inventions, and Models, to be forwarded to James Tod, Esq. W. S. 21 Dublin Street, Edinburgh, Secretary to the Society; and it is requested, that, in all cases, *full descriptions* of the Invention may be sent; and, where the nature of the communication requires it, that there be also sent relative *Drawings, Sketches,* or *Models,* so as to enable the Society fully to judge of the merits of the communication.

Royal Institution, Edinburgh,
21st July 1834.

NEW PUBLICATIONS.

1. *A Treatise on Primary Geology.* By HENRY S. BOASE, M. D., Secretary to the Royal Geological Society of Cornwall, &c. &c. 8vo. Pp. 399. Longman and Co. 1834.

This interesting work, which excited so much attention in the Geological Section of the British Association, lately held in Edinburgh, ought to be in the hands of every geologist. Although some of the positions of the author have been contested, we are of opinion that much of the reasoning, and many of the curious and important statements of Dr Boase, remain unaffected by any thing that was said in the British Association at Edinburgh, or has been written on the subject of primitive geology.

2. *Elements of Practical Agriculture, comprehending the Cultivation of Plants, the Husbandry of the Domestic Animals, and the Economy of the Farm.* By DAVID LOW, Esq., F. R. S. E., Professor of Agriculture in the University of Edinburgh. 8vo. Pp. 695. Bell and Bradfute, Edinburgh. 1834.

No work on agriculture has appeared in our time which will bear a comparison with this excellent, and we would say classical, work of Professor Low. It will become the manual of practical agriculture for the British empire; and the judicious practical rules and sound views of our author, will unquestionably prove beneficial to the agriculturists of other countries.

3. *Guide to the Highlands and Islands of Scotland, including Orkney and Shetland, descriptive of their Scenery, Statistics, Antiquities, and Natural History; with numerous Historical Notices.* By Messrs GEORGE and PETER ANDERSON, of Inverness. With a Map engraved by Arrowsmith. Small 8vo, pp. 760. Murray, London. 1834.

We know the Highlands and Islands of Scotland well; we have walked through them repeatedly, not for a week or two, but for months at a time, and for years in succession, and are therefore prepared to give our opinion on any work illustrative of that interesting portion of Great Britain. We now do so, and hesitate not to say that the Guide of the Messrs Andersons contains a greater variety of accurate and well arranged information, illustrative of Highland scenery, history, adventure, natural history, routes, and all the varieties of information expected by the traveller in a Guide book, than any similar work hitherto published. In short, we consider it as our best Guide to the Highlands of Scotland.

4. *Recherches sur les Poissons Fossiles.* Par L. Agassiz. Neuchatel. 1834. 4to, with Coloured Plates.

In this work it is intended to give not only a description of all the fossil species hitherto discovered, but also to combine with it a general outline of the characters of the orders, families, and genera of all living fishes. At the same time, the first volume will contain a full anatomical description of the scales, the skeleton, and the teeth, and a comparison will be made with the other classes of the animal kingdom.

In the geological account of fossil fishes, the most remarkable organic remains of other classes, which are found associated with them, will be mentioned, in order to point out the character of the various geological periods, and to indicate the changes which have occurred in the order of succession of animals.

It is thus evident, that the work is intimately connected with zoology as well as with anatomy and geology. But for farther information on this subject, we must refer our readers to the preface to the first part.

The Geological Society of London, impressed with the importance of M. Agassiz' researches, accorded the highest mark of its approbation and encouragement, by decerning to him the Wollaston medal in 1834.

We are also happy to announce, that the British Association, during its late meeting in this city, was so fully aware of the value of the researches of M. Agassiz on fossil ichthyology, and of the important results to be obtained from them for geology, that it voted a sum of 100 guineas for the encouragement of this branch of zoological science in Great Britain.

Two numbers have already been published, of which we shall soon give a detailed account.

New Work on Geology.

We learn with pleasure that Professor Phillips has now in the press a Guide to Geology, the object of which is to give the elementary facts and generalizations, and to furnish correct information as to the modern and actual state of the science.

List of Patents granted in Scotland from 26th March to 19th September 1834.

1834.

Mar. 26. To Samuel Hall of Basford in the county of Nottingham, cotton manufacturer, for an invention of "certain improvements in steam engines."

April 12. To Samuel Slocum of the New Road, St Pancras, in the county of Middlesex, engineer, for an invention of "improvements in machinery for making pins."

To Samuel Slocum of the New Road, St Pancras, in the county of Middlesex, engineer, for an invention of "a certain improvement or improvements in machinery for making nails."

To James Jamieson Cordes, of Idol Lane, in the city of London, merchant, in consequence of a communication made to him by a late resident of the United States of America, now deceased, for an invention of "a certain improvement or improvements in machinery for making rivets or screw blanks or bolts."

To James Jamieson Cordes, of Idol Lane, in the city of London, merchant, in consequence of a communication made to him by a late resident of the United States of America, now deceased, of "a certain improvement or improvements in machinery for making nails."

16. To Charles Attwood of Whickham near Gateshead, in the county of Durham, manufacturer of soda, for an invention of "a certain pigment or certain pigments, by a certain process or certain processes not previously used for such purposes."

24. To John Read of Regent Street, in the county of Middlesex, merchant, and John Barton of Providence Row, Finsbury, in the same county, engineer, for an invention of "certain improvements in machinery or apparatus for raising and forcing fluids."

30. To Hooton Deverill of Manchester, in the county of Lancaster, for an invention of "a method of engraving and etching on cylindrical surfaces for printing and other purposes."

To John Christophers of New Broad Street, in the city of London, merchant, for "an invention of an improvement or improvements on anchors."

May 7. To Miles Berry of 66. Chancery Lane, in the parish of Saint Andrew, Holborn, in the county of Middlesex, civil engineer and patent agent, in consequence of a communication from a foreigner residing abroad, of an invention "for certain improvements in the construction of weighing machines."

16. To Thomas John Fuller of the Commercial Road, in the county of Middlesex, civil engineer, for an invention of "an improvement in the shape or form of nails, spikes, and bolts."

May 16. To Thomas John Fuller of the Commercial Road, in the county of Middlesex, civil engineer, for an invention of "an improvement or impovements in machinery or apparatus for making or manufacturing of nails."

20. To Janet Taylor of East Street, Red Lion Square, in the county of Middlesex, for an invention of "improvements in instruments for measuring angles and distances applicable to nautical and other purposes."

To George Washington Wildes of Coleman Street, in the city of London, merchant, in consequence of a communication made to him by a certain foreigner resident abroad, of an invention "for certain improvements in machinery for cutting marble and other stones, and cutting or forming mouldings or groovings thereon."

To John Paterson Reid, of the city of Glasgow, merchant and power-loom manufacturer, and Thomas Johnson, of the said city of Glasgow, mechanic, in the employment of John and Archibald Reid, of the said city of Glasgow, power-loom manufacturers, for an invention of "certain improvements applicable to certain looms for weaving different sorts of cloth."

26. To Benjamin Dobson of Bolton le Moors, in the county of Lancaster, machinist, and John Sutcliff and Richard Threlfall, both of the same place, mechanics, for an invention of "certain improvements in machinery for roving and spinning cotton and other fibrous materials."

June 14. To Henry Pinkus, lately of Pennsylvania, in the United States of North America, but now of Wigmore Street, Cavendish Square, gentleman, for an invention of "an improved method of, or apparatus for, communicating and transmitting or extending motive power, by means whereof carriages or waggons may be propelled on railways or common roads, and vessels may be propelled on canals."

23. To William Morgan of the Kent Road, in the county of Surry, Esq. for an invention of "improvements in certain kinds of steam-engines."

To Philip Augustus de Chapeaurouge of Fenchurch Street, in the city of London, gentleman, in consequence of a communication made to him by a certain foreigner residing abroad, for "an invention for producing motive power, which he denominates a self-acting motive power, and called in France by the inventor 'volant moteur perpetuel.'"

27. To Henry Hardingham Legget of Fulham, in the county of Middlesex, gentleman, for an invention of "certain improvements in the art of printing in colours."

To Matthew Bush of Dalmonarch Printfield, near Bonhill, by Dumbarton, North Britain, calico-printer, for an invention of "certain improvements in machinery or apparatus for dyeing and printing calicoes and other fabrics."

To Thomas Alcock, of the parish of Claines, in the county of Worcester, lace-manufacturer, for an invention of "certain improvements in machinery for making lace or net, commonly called bobbin-net lace, part of which improvements will enable such machinery to produce ornamental bobbin-net lace."

July 11. To Thomas Sharp, merchant, and Richard Roberts, engineer, both of Manchester, in the county Palatine of Lancaster, in consequence of a communication from a foreigner resident abroad, for an invention of "certain improvements in machinery for grinding corn and other materials."

17. To Charles Wilson of Kelso, in the county of Roxburgh, in that part of the united kingdom of Great Britain and Ireland called Scotland, for an invention of certain improvements applicable "to the machinery used in the preparation for spinning wool and other fibrous substances."

To William Septimus Losh of Walker, in the county of Northumberland, gentleman, for "an improved method of bleaching certain animal fats, and certain animal, vegetable, and fish oils."

To Joseph Shee of Laurence Pountney Place, in the county of London, gentleman, for an invention of "certain improvements in distillation."

To James Hamilton of Threadneedle Street, in the city of London, civil-engineer, for an invention of "certain improvements in machinery for sawing, boring, and manufacturing wood, applicable to various purposes."

21. To John Aston of Birmingham, in the county of Warwick, button-maker, for an invention of "an improvement in the manufacture or construction of buttons."

To John Gold of Birmingham, in the county of Warwick, glass-cutter, for an invention of "certain improvements in cutting, grinding, smoothing, polishing, or otherwise preparing glass-decanters, and certain other articles."

22. To Peter Wright, of the city of Edinburgh, manufacturer, for an invention of "an improved method of spinning, twisting, and twining cotton, flax, silk, wool, or any other suitable substance."

25. To Isaac Jacks junior of Bennet's Hill, in the city of London, gentleman, for an invention of "an apparatus or machine for putting or drawing on or off boots."

30. To Luke Hebert of the Hampstead road, in the county of Middlesex, civil-engineer, for an invention of "certain improvements in machines or apparatus for and in the process of manufacturing bread and biscuits from grain."

Aug. 1. To Richard Simpson, late of Rouen, in the kingdom of France, but now residing in Southampton Row, Bloomsbury, in the county of Middlesex, gentleman, for an invention communicated to him by a foreigner then resident in France, of "certain improvements in machinery for slubbing and roving wool and cotton.

5. To William Higgins of Salford, in the county of Lancaster, machine maker, in consequence of communications made to him by a

foreigner residing abroad, for "certain improvements in machinery used for making twisted rovings and yarn of cotton, flax, silk, wool, and other fibrous substances."

Aug. 5. To Henry Ewbank of Idol Lane, in the city of London, merchant, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention for "dressing rough rice or paddy, and certain other grain, by rubbing off its skin or pellicle, and redressing or cleansing rice."

6. To Daniel Ledsam and William Jones, both of Birmingham, in the county of Warwick, screw-manufacturers, for an invention of "certain improvements in machinery for making pins, needles, rivets, wood-screws, and nails."

18. To John Rapson of Penryn, in the county of Cornwall, engineer, for an invention of "an improved apparatus for facilitating the steering of vessels of certain descriptions."

26. To William Hale of Colchester, in the county of Essex, engineer, for an invention of "certain improvements in or on wind-mills, which improvements are applicable to other purposes."

29. To Joseph Whitworth of Manchester, in the county palatine of Lancaster, machinist, for an invention of "certain improvements in machinery or apparatus for cutting screws."

Sept. 12. To John George Bodmer of Bolton le Moors, in the county palatine of Lancaster, civil-engineer, for an invention of "certain improvements in the construction of grates, stoves, and furnaces, applicable to steam-engines and many useful purposes."

To John George Bodmer of Bolton le Moors, in the county palatine of Lancaster, civil-engineer, for an invention of "certain improvements in steam-engines and boilers applicable both to fixed and locomotive engines."

19. To James Berrie and David Anderson, both of the city of Glasgow, in Scotland, manufacturers, for an invention of "a machine or machines for making a new or improved description of heddles or healds to be used in weaving."

INDEX.

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4