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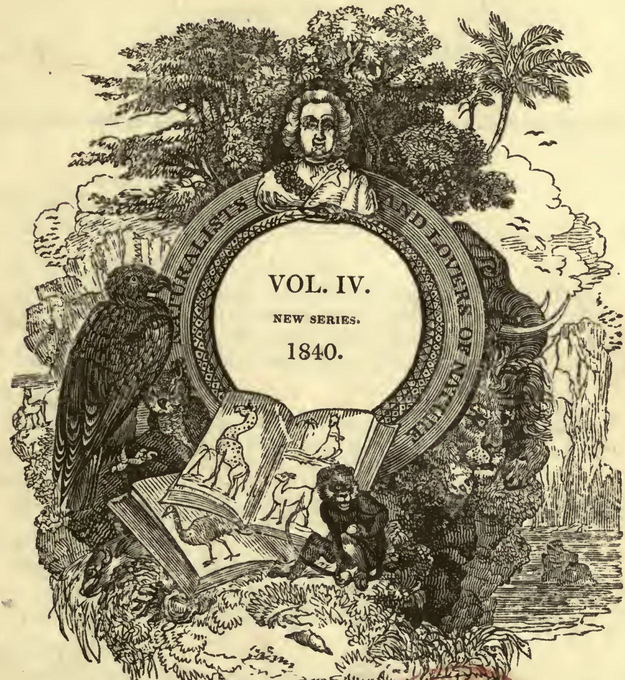
MAGAZINE OF NATURAL HISTORY.



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THE MAGAZINE

OF

NATURAL HISTORY.

JANUARY, 1840.

ART. I.—*View of the Fauna of Brazil, previous to the last Geological Revolution.*[1] By DR. LUND.[2]

DR. LUND commences his account by explaining the circumstances under which the fossil remains he is about to describe are found. "They are all found," he says, "in the caves which occur in the calcareous rocks that traverse, in various directions the interior Highlands of Brazil. A mountain chain composed of this rock diverges from the principal chain of the central plateau (Serra do Espinhaço) in the neighbourhood of the capital of the Province Minas, and extends towards the north west, dividing the waters of the rivers Rio das Velhas and Paraopeba. It is this chain which has hitherto formed the richest field of my researches; and indeed it is to the caves on its eastern declivity that I am indebted for all the relics of the inhabitants of a former world which I yet possess. Its western side presents fewer caves, and I have not been so fortunate as to find any trace of animal remains in them, any more than in the numerous caverns contained in the other small limestone chains connected with the above principal range.

The rock of which these chains is composed is a dark grey, fine-grained, crystalline, transition limestone, disposed in horizontal strata, which not unfrequently exhibit a very gentle

[1] This is a nearly literal translation of Dr. Lund's own title, but it is by no means descriptive of this first paper, at least, which is merely introductory, and might be better entitled,—"An Account of the Limestone Caves in the Interior Highlands of Brazil; with a description of the *Mammalia* now occurring in that district." His second paper is entitled—"A Survey or Sketch of the extinct species of *Mammalia* which inhabited the Highlands of Tropical Brazil, previously to the last Geological Revolution."—*Translator.*

[2] In a communication addressed to the Society of Sciences at Copenhagen, and printed there in 1838. Translated from the Danish, and communicated to the Mag. Nat. Hist., by the Rev. W. Bilton.

dip to the east. It rests upon a vast mass of talc slate, which dips at an angle of 90° to the east, and forms the outermost member in connection with the higher central range: and farther from this principal chain, it alternates with beds of siliceous and clay slates. It often contains veins of quartz, but I have never yet found in it any trace either of metals or of organic remains. These limestone hills form smooth, rounded ranges, which, however, have often a wild and picturesque character, from the extraordinary disruption of their masses, and the projection of their bare and steep walls of rock. They are also clothed with a peculiar vegetation; and among the isolated outliers of this same formation, are numerous lakes. Another physical phenomenon peculiar to the limestone district, is the sudden disappearance of rivers (Sumidouro), which again emerge at greater or less distances.—This phenomenon arises from the number of fissures by which the limestone is traversed, both above and below the surface. Of the form of these fissures no universal description can be given. Sometimes they appear as perpendicular clefts, running in one direction; sometimes they are intersected by a number of transverse fissures; sometimes they run in zigzags; but often expand into passages, chambers, halls, and other similar forms, in which breadth predominates. It is more particularly this latter form of which I think it necessary to give a more detailed description.

The first thing that strikes the observer in these caves, is the rounded form presented by them. The roof is arched, and passes into the side walls by a regular curve. The floor can seldom be seen; but when visible, it exhibits the same mode of junction with the sides; and it is impossible to avoid remarking, that all the angles which project at the different divisions or bends of the passages, are in like manner rounded off. If we examine the walls and roof more carefully, we perceive that the surface of the rock is full of protuberances and hollows, blending into each other with the gentlest curve. But especially do we observe numerous round holes, of all sizes, both in the sides and roof, which penetrate more or less deeply into the rock, and are closed at the bottom. Frequently, where the walls project beyond the regular line, these holes penetrate from one side to the other, and thus form passages, usually narrow. but sometimes large enough to admit of our creeping through them, and presenting, on a smaller scale, the same phenomena already described in speaking of the principal passages.

The surface of the stone is moreover smooth, often to such a degree as to reflect light: which circumstance, combined with the rounded forms assumed, frequently gives the rocky masses the appearance of bronze statues.

The number of caves already visited by me in Brazil, amounts to eighty-eight, to the whole of which the characters here described are more or less applicable; and these characters are such as, in my opinion, leave no doubt as to the mode of formation of the caves. In traversing them, one appears to be walking by the rock-bound shore of ocean, and to see its naked cliffs as they are hollowed and smoothed by the waves. And doubtless, such is their origin; doubtless, we must assign the formation of these caverns to those periods when either this whole tract, that is now dry land, was covered with vast lakes; or when it yet rested in the bosom of the sea. At any rate, it is certain that the filtration of water through the limestone, is quite insufficient to explain the phenomena we have been describing, and particularly the deep yet blind holes gnawed into the roof.

All these caves are more or less filled with soil, which I will take the liberty of describing, by giving a short account of the newest formation that covers the surface in this part of Brazil: it is exactly the same as that which is found inside the caverns.

The plains, the valleys, and the lower hills are, without exception, overspread by a vast layer of loose soil, from which the higher ranges alone are free. This stratum, with a certain degree of uniformity combines no inconsiderable variety, which is partly shown in its subordinate beds, and partly in the occasional appearance of these beds, without any such sequence, by the side of each other. The most common form assumed by these formations, is that of a stratum of coarse, red clay, from ten to thirty or more feet in thickness, in which there is no trace of stratification or other divisions. Occasionally we may follow this clay-bed over considerable tracts, without observing any remarkable extraneous substance; but it usually contains, in a greater or less quantity, rolled stones of quartz, from the size of a pigeon's egg to that of a man's head. These boulders are sometimes strewed about without order; sometimes they form more or less regular beds. Intermixed with these rolled quartz stones are found, but in far less quantity, similarly rolled fragments of the other kinds of rock belonging to this district; and it is out of this same soil that the natives obtain gold and diamonds by washing.—Quartz is also found in this bed of clay, under another form still more common in the Province of Minas, namely, that of sharp angular fragments, grouped together so as to form regular beds, of from six to eighteen inches thick, and frequently of considerable extent. This siliceous stratum has no uniform depth in the clay, but often approaches the surface, and then forms the 'Gruns Campos' so characteristic of that country, and so unfriendly to agriculture, though clothed with so peculiar a vegetation. Amongst this quartz occur, although sparingly, fragments of other rocks, particularly of ironstone.[1] The colour of the clay occasionally passes from red into dull ochre-yellow: and this change in colour is usually accompanied by a greater degree of fineness and uniformity of composition, with some slight trace of cleavage, or separation into cubical masses. I have hitherto had no opportunity of observing these beds of clay alternating with beds of sand; whereas this last formation appears on the surface over an extent of many degrees in length, in the broad vale watered by the St. Francisco river.

But the most peculiar character under which these newest formations present themselves in Brazil, is that called in this country "Tapanhoacanga," or negro-head. This formation is seen, like a stream of lava, spread over the plains, valleys, and hills, nay, even on the highest mountain-ridges of Brazil. It is most common in those districts where there are ironstone rocks of the older formations. It consists of a ferruginous conglomerate, composed of rolled and angular fragments of quartz, and other rocks of the country; but chiefly of the iron formation itself, such as ferriferous mica slate &c., united by a ferruginous cement of red, yellow, or black colour. Occasionally, the fragments disappear, and the cementing substance alone remains. It forms a mass almost as hard as stone, with a smooth and often shining surface; inside, it is full of vescicular holes, which give it the appearance of a slag. It very often contains stone marrow, and in general the same minerals that form the constituent portions of the common red

[1] The origin of this quartz-bed was long a puzzle to me, until more careful examinations of the principal mountain range enlightened me on the subject. One of the most important members in the composition of that range is a vast mass of talc slate, with numerous veins and subordinate beds of quartz. This talc slate, in most places, and to considerable depths, is in such a state of decomposition, as to crumble between the fingers, and the quartz separates with the touch into larger or smaller angular fragments. If we examine the beds of diluvium at the foot of this mountain chain, we there see the red homogeneous clay replaced by a mass of nothing but fragments of talc slate, lying in all directions, but containing also the same siliceous layers disposed with more regularity, so as often to look like connected quartz beds. That this talc slate is in a secondary position, is evident from its irregular stratification; and if we follow its development from the foot of the mountain, it is easy to convince ourselves of its gradual passage and change into the common red clay, with its usual siliceous bed.

clay, particularly gold, which is often in sufficient quantity to be worked. The same conglomerate is likewise found in the caves, and contains the same animal remains as the red clay; so that there can be no doubt of the contemporaneousness of their formation. These most recent formations are found in the caves, under all the conditions I have thus attempted to describe, and in about the same relative proportions as to quantity; the red clay soil being by far the most common, either in a pure state, or mixed with siliceous boulders or gravel. But all these kinds of soil have undergone changes since their deposition in the caves, of which changes I will now proceed to give an account.

The first change arises from the infiltration of water charged with lime. The lime derived from the evaporation of the water, and its contact with the surrounding hard bodies, unites the loose particles of the soil, and changes the whole into a mass as hard as stone. Indeed, the looser the soil was previously to this infiltration, the harder does it afterwards become, from the greater quantity of calcareous particles which it allows to penetrate. For this reason the fine yellow clay never becomes so hard as the coarse red, the loose composition of which even permits the calcareous incrustation to collect in connected masses, which are sometimes solid, at other times contain holes lined with beautiful small crystals of calcareous spar. Lastly, the beds of boulders and gravel, which outside the caves are loose and unconnected, are metamorphosed into perfect pudding-stone inside. Another character which distinguishes the soil within the caves from that without, is the much greater quantity of fragments of limestone contained in it. These fragments have their angles partly sharp, and partly broken off and rounded; and are from the most trifling size up to that of immense blocks. In those caves the opening of which is to the north, and which are closed at the end, these fragments are found in extraordinary abundance, especially in the inner part, towards the bottom: while in those which open to the south, or have two opposite entrances, the fragments are either entirely wanting, or occur in trifling quantities. This result is based upon the examination of so many caverns, that it cannot be looked upon as an accidental circumstance: and the natural explanation appears to be, that the inundation which deposited the soil in these caves, must have moved from north to south, and with sufficient force to carry along with it the blocks of all sizes which we see scattered inside and outside of the caves.

But the most important substance which has enriched the soil since its deposition, is, without doubt, saltpetre, which is found in sufficient quantity to become an article of industry and export to the neighbouring population. The origin of this salt was for a long time obscure to me; but at length numerous observations and experiments have convinced me that it is neither formed in the soil where it lies, nor contained in the limestone in which the caves occur, but that it owes its origin to the bed of mould which lies above the caves.— The rain water that filters through this mould becomes charged with decomposed vegetable particles, and by contact with the limestone in its passage through the fissures, the salt is produced.

Most frequently this bed of soil is covered with a coating of stalagmite. In my former communication, describing the cave of Maquiné, I have drawn attention to the difference in the manner and period of formation of this stalagmitic crust, and of those columnar and leaf-shaped productions, which, either as stalactites, hang from the wall or clothe the roof in a thousand fantastic and indescribably beautiful forms, or, as stalagmites, rise like enormous pillars or cones from the floor. By far the greatest quantity of this stalactitic mass belongs to ages antecedent to the introduction of the soil, which is seen resting on it as a base, just as the soil outside rests on the base of the older rocks. A much smaller portion has formed since, and indeed it continues daily to increase, even under the eye of the observer. On the other hand, the stalagmitic coating which covers the surface of the soil, must have been a later production, to which many causes have contributed; these causes it is important to distinguish, in order that we may be enabled to decide upon the age of the organic remains found under the stalagmite. In the communication already referred to, I have proved that the formation of the columnar and leaf-shaped stalactite is effected by a very slow process. If the dripping of the water proceeds too rapidly, its dissipating power prevents its setting, and the stalagmitic mass below is hollowed out, instead of being increased. The calcareous water which runs down the sides of the stalagmitic blocks, collects in the first hollow it finds near their base. There, and on its passage thither, it gradually deposits its lime, and thus there is gradually spread round the stalagmite mass, and in the depressions nearest to it, a calcareous coat, which the water continually enlarges, until by the filling up of the first hollow it is compelled to flow onward to the next, and so on. The splash of each drop that falls upon the stalagmitic mass also contributes to form a similar incrustation around it; as may easily be seen from the innumerable small inequalities of the surface, which, with the faces of their crystals, often give an appearance of crystallized sugar to the substances covered by them.

Whoever visits these caverns to admire the beauty of their fantastic draperies, will dwell with most pleasure on the formations of this class: while for the zoologist they possess less interest, since the substances they enclose are most commonly of very recent origin. Under the stalagmitic mass, and forming, as it were, the nucleus of these "confetti," I have often found recent bones of existing animals, shells, nay, even a piece of charcoal, probably left by savages. Not unfrequently do we detect nature in the very act of forming these incrustations, where, in a heap of bones lying on the floor, some are already entirely enveloped in stalagmite, others stick half out of it, while others again lie perfectly untouched, and awaiting the incrustation that will veil them from our view, and preserve them perhaps for the investigations of a future generation. Forasmuch as this formation depends on agencies which are in daily operation on the surface of the earth, that it to say, on the infiltration of rain water through the fissures and pores of limestone, there can be no reason for supposing that it should not also have been going on at a period before the introduction of the soil into the caves: and experience has convinced me that this is really the case. I have frequently had occasion to observe, under the stratum of soil, a similar stalagmitic incrustation, with those beautiful basin-shaped crystallizations known here by the name of "Pias," or baptismal fonts.

Besides these originating causes, there is scarcely a cave in which we may not see, at least in time of rain, the water drop from the roof, and form basins of a larger or smaller size.—At the bottom and round the edge of these basins, the same phenomena already described occur, and occasion incrustations and depressions in the floor. These two modes of formation of the stalagmitic flooring of many caves are indubitable; and where passages are narrow, and the quantity of stalactite on the roof and walls considerable, they are sufficient to account for the phenomenon: but at the same time they are evidently insufficient, in many respects, to serve as an universal explanation, as for instance, in the case of those wide and spacious halls into which the caverns often expand, where a coating of stalagmite covers the stratum of soil, like ice on the surface of the lake, and yet where no dripping from the roof betrays the actual presence of incrusting water,—no stalagmitic crust on the walls or roof attests its agency in time past.

In the communication already cited, I have drawn attention to the proofs derived from the condition and preservation of the bones found in, and immediately under, this calcareous tegument, which compel us to conclude that its formation dates from the time immediately subsequent to the introduction of the soil; and that it owes its origin to the extraordinary condition into which the surface of our globe passed, as a consequence of the equally extraordinary catastrophe that occasioned the destruction of the whole race of animals previously existing, and the introduction of their remains into the caves. This stalagmitic crust, which, with so few exceptions, covers the surface of the soil in all the Brazilian caverns, is never found beneath it; which circumstance contrasts so strongly with the abundance of the stalagmitic blocks produced during the remote period that preceded the introduction of the soil, as to be sufficient, in connection with the adduced proofs, to confirm their distinct origin.

I fear that I must have wearied the Society by dwelling so long on this point; I trust however that its practical importance will plead my excuse. Future visitors to these caverns may perhaps find here a hint to guide them in their search after the treasures contained in them, and in the determination of the age of those treasures. The same remark will hold good with respect to the observations I shall next proceed to offer on the conditions under which the bones are found in the caverns.

(To be continued).

ART. II.—*On the Genus* Argonauta. By M. RANG.

(Continued from Vol. iii. *page* 532.*)*

THUS the intention of the membraniferous arms with which certain species of poulps are provided is now known. These organs envelope the argonaut-shell as the lobes of the mantle in some other kinds of *mollusca* envelope their shells. But for what purpose are they so disposed? Some naturalists have thought, and there are those among the number whose learning and talent are incontestible, that the poulp secreted the shell by means of its airholes; would then an opinion be more unreasonable which should attribute this secretion to the membranes themselves? The slender form, the fragile and diaphanous nature of the shell, its sides, which so well indicate the different applications of the anterior margin of the membrane, its tubercles along the keel, constant in all the species; the coloration of the bases of the arms, corresponding so well with the colouring of the keel towards the spire; —are not these characters, which, more thoroughly investigated than they have yet been, would tend to support the fact of the membranous arms being the organs of secretion? We shall be told that it is not by the aid of their mantle that the *Mollusca* secrete their shells, but by the collar which unites them to the opening; and without doubt this is an opinion very justly adopted, and we have proved by more than one circumstance, and even in the preceding note, that we have for a long time ranged ourselves on the side of this opinion; but the argument does not, for that reason, appear to us the less feeble, for admitting it to be proved that it is by the collar that the *Mollusca* secrete their shells, it is also proved that there are exceptions to this rule. The mollusc of the *Nautilus*, for example, the shell of which is very solid and strong, and must have required two or three kinds of secretion, has no collar, as a skilful English naturalist teaches us by the anatomical examination which he has lately made, and in which we find no mention of these organs.—Now if the mollusc of the nautilus, without the aid of a collar, has constructed a shell so strong, so heavy, and so eminently calcareous, surely we may believe that the mollusc of the argonaut, likewise a cephalopod, is equally capable of constructing a shell without such aid. Such a supposition, according to our view, is so much the more admissible, because the argonaut, by nature delicate, flexible, and submembranous, would seem to favour such a theory much more readily than the nautilus. Would it then be very strange that the lobes of the large arms should possess the property of secreting this slender shell, and that it is but a mere membranous pellicle in its early stage? Do not the lobes of the mantle of the *Mollusca* which form the cowries and the olives, secrete calcareous layers, which change, in such a remarkable manner, the original aspect of these shells, and ultimately add largely to their thickness?

M. de Blainville, who rejects with all the weight of his authority this line of argument, has, from the very first, sought to re-consign the use of these membraniferous arms, with which we have made him acquainted, to the wants of the poulp of the argonaut, and, having so done, has pressed it into the service of parasitism. Indeed this philosopher points out to us, that since the poulp, as is now perfectly known, does not adhere to its shell by a muscle, or even a collar, it is therefore very necessary that it should have some organ to hold it by; and, if we do not deceive ourselves, there lay perhaps one of the difficulties which most clogged this naturalist in developing his whole ideas upon the parasitism of the mollusc in question, for it was impossible, with the knowledge which he has of the organization of this animal, that he should believe, like some naturalists, that the poulp made use of its suckers as a means by which to adhere to its shell. M. de Blainville sees then, in this abnormal organization of the large arms of the poulp, an arrangement necessary for its maintaining its position in the shell that it inhabits, and, without which, it would be every moment exposed to the loss of it. This is a fact incontestably demonstrated, and which cannot fail to be adopted indifferently by the partisans of non-parasitism, and those of parasitism.

Should it be objected, (for it is necessary as much as possible to anticipate objections), that the poulp can have no need to cling so strongly to its shell, because the effort that it makes to expel the water from its branchial cavity, when swimming, far from tending to separate the two, only on the contrary brings them closer together;—it would be easy for us to reply, that the movement does not consist merely of removal: and, that without speaking of shocks, of the agitation of the waves, &c., it is very natural to suppose that when the mollusc crawls along, carrying its shell with the opening turned downwards, the shell could not fail to escape, and mount to the surface of the water, on account of the air which it indubitably contains, if the poulp did not retain it by some means as constant and as powerful as those which it possesses.

The position of the large arms with their membranes over the shell, and the service which they render to the poulp, being once made known and adopted, let us see what are the inferences which may yet be drawn from this fact to throw light upon the question, and simplify it from the chaos of arguments presented on all sides, and generally derived from facts wrongly interpreted, or from pure imagination.

Those naturalists who have turned their attention to the argonaut, have been very little agreed as to the relative position of the poulp to its shell; and from this there has resulted—first, the inconvenience of not being able sooner to explain the use of the membraniferous arms;—and, secondly, a supply of weapons to the partisans of parasitism; for these latter have skilfully seized upon this disagreement to draw from it this certainly rather rational argument, that, since the mollusc adheres sometimes in one manner, and sometimes in another, it is a proof that the shell has not been made for it, and does not belong to it: this part of the question being one of the most important, we shall pause here a little.

Poli, who, from what he says, must have thoroughly inspected this animal in a living state, places its great arms in front, that is to say, at the anterior edge of the opening.—Ferussac, who has reproduced the beautiful, but too fabulous sketch of Poli, has, like that naturalist, placed it the wrong way, at the same time sketching other figures in a proper manner, which, on the part of one of the most ardent partisans of non-parasitism was a serious fault; but, at least, proved his candour in the discussion. However, in 1836, upon some information which we gave him from Algiers, and, we believe also, in consequence of some observations of M. Delle Chiaje, or Verany, he resolved to make a new copy of the plate borrowed from Poli, in order to turn the animal the opposite way, which is in fact the true one. It has been wrongly thought that he made this change only that he might not leave such weapons in the hands of his adversaries;—on the contrary, it was the result of conviction in his mind.

It is very clear, as to the rest, that Ferussac had adopted the idea that the palmated arms were to be found on the side of the spiral line of the shell; since, from 1825, as may be seen by the memoir which he read to the Academy, he supposed that the palmated portions of the great *tentacula* folded themselves into a globular mass in the spiral cavity of the shell, which he would not have been able to point out if he had thought that these arms were in the anterior part.

M. Delle Chiaje, whose observations will always be of great weight in all questions of malacological organization, has not been happy in this circumstance. In fact, he also reverses the animal in such a manner as to put its membraniferous arms at the anterior part of the shell; and manages, as he can, to explain how the animal holds its shell by the aid of suckers, which is difficult enough to conceive, since he says at the same time, that the arms are spread out upon the surface of the water. After these come Mr. Broderip, who affirms, that in a specimen which he had in his possession, the palmated arms were on the side of the back of the shell; and Mr. James Sowerby, who nevertheless acknowledges that in Cranch's specimen it was the contrary.

M. de Blainville has also had well-preserved specimens in his hands, and he has seen them turned in the manner we have described; nevertheless, he draws from the divergence of opinions, another argument in favour of parasitism. This argument ought now to fail him, and indeed the inference which he himself draws from it decides this question; for it is very certain, that since the function of the membranes of the large arms consists in seizing the shell, by enveloping it from the re-entering part of the keel, to its further extremity, the animal must be constantly turned, so that this arrangement can take place, that is to say, it must have its dorsal part towards the spire.

The partisans of parasitism place great stress upon an anecdote, which in fact would be very fit to decide the question, if it constituted an accurately made and precise observation, or even one worthy of confidence. We refer to the mollusc of which Rafinesque has made the genus *Ocythöe*. We know not if we are right, but it appears to us, that naturalists, who have in some instances very just pretensions to having based their opinion upon scientific principles, deceive themselves in this instance, and take hold of a fact of no value, as we are about to show. The history of the genus *Ocythöe* is as follows.—

A traveller, studying natural history in the Sicilian seas, found among other curious things, a cephalopod, of which this is the description, quoted, and no doubt verbatim, by M. de Blainville. "Tentacular appendages to the number of eight; the two upper ones winged within; with interior suckers; pedunculated; joined by the lateral wing; without any membrane at their base." If, as we cannot doubt, such is the description furnished by this naturalist, truly those who back themselves upon this clause to sustain their opinion are very fortunate, for never besides, according to what we know of the other poulps he has described, has this observer taken such great pains in describing a mollusc; he does not speak of any shell; so that fact has been seized upon to show that the mollusc is the poulp of the argonaut, walking freely in the open sea, and without its testaceous covering, as if the author usually took the pains to describe all that he sees in a mollusc—thus the parasitism is demonstrated!

To all this may we not make the following objections?—

1st.—It is not proved from this that the mollusc was without a shell, though Rafinesque says nothing of one.

2nd.—It is not further proved, that in case the animal was, as we are willing to believe, without a shell, the shell had not been lost a few minutes before the capture of the animal: it is necessary, in order to reap advantage from this anecdote, that we should be well acquainted with all its details.

3rd.—The astonishing descriptions of five or six poulps met with by the same traveller, and that taken from his *Ocythöe*, which are the only things that we have the honour of knowing about him, are not calculated to give to ourselves or others any confidence in the precision of his observations.

4th.—We have not felt ourselves obliged to believe that this cephalopod was an argonaut-poulp, more especially because his description states that the arms had no membrane at their base, and, as we have seen, although contrary to observations made upon specimens preserved in alcohol, these molluscs possess, if not very large, yet, at least, very visible ones.

5th.—If we wished to describe one of our "*Poulpes à grandes membranes*," of which Ferussac makes his *Vélifères*, and a species of which we shall introduce at the end of this memoir, we should choose very nearly the same expressions as M. Rafinesque, so much does his poulp resemble those of this division.

6th.—An expression made use of by M. de Blainville himself, shows of itself, all the uncertainty which prevails concerning this mollusc. "There have been found," says this naturalist, "in the seas of Sicily, poulps, whose pair of upper *tentacula* is spread out in width, probably as in the parasitic poulps, since they appeared to differ sufficiently from known species to form a distinct genus, under the name of *Ocythöe*." We shall just observe, that the veliferous poulps are common in the Mediterranean, and particularly in the seas of Sicily and Italy; and, that at the epoch when this traveller made his discovery, and even at that when M. de Blainville published his article, 'Poulpe du Dictionnaire,' no other species was known.

It would be the same with the argument which it has been attempted to draw from the two poulps *Ocythöe* that Ranzani had in his possession: they were in alcohol, and one of them moreover carried the fragments of the shell.

The partisans of parasitism bring forward yet another argument, to which we believe it easy to reply, so as to make it valueless; they say, that it is not always the same species of poulp that we find in the same species of shell. Their adversaries seek to demonstrate its non-parasitic nature, by sustaining, that it is always precisely the same. Which are we to believe? As for ourselves, our opinion upon this subject was formed long ago; and we endeavoured to prove it in the 'Bulletin Universel des Sciences,' by citing an occasion when we had been able to examine a great number of these animals, some occupying the *Argonauta Argo* and others the rice-grained argonaut. We then easily convinced ourselves that the same species always inhabited the same shell; for we never found in one those that we discovered in the other. But we will not argue from this fact; for, following the example of M. de Blainville, we think that it is not well in any case, to support ourselves upon an observation capable of being set aside as being but a mere anecdote; we will endeavour to proceed by means of reasoning.

M. de Blainville thinks, that it was an ordinary poulp that was seen by Aristotle in the shell of the argonaut; and he founds his opinion upon what is said by that great naturalist of the arms being united by a membrane, slender as a spider's web, in the same manner as the toes of ducks. Our own observation of the palmatures upon the arms of the poulps of argonauts, of which we have already spoken, overturns this argument, since it shows that the character observed by Aristotle, applies as well to the one as the other.

Mutien, Pliny, Born, and Bosc, have all spoken of a *Seiche* which inhabits the argonaut; that doubtless is true, but it is not less certain that these naturalists understood by a *Seiche*, a poulp; as is shown by the *Sepia octopus* of Linnæus, the *Sepia rugosa* of Bosc, &c., &c. How otherwise are we to comprehend that a *Seiche*, which is always an elongated animal, and not at all proteiform as poulps are in general, which besides, encloses in its body a large, straight, and solid shell, could ensconce itself in the cavity of an argonaut, and consequently cause the first shell to accommodate itself to the form of the other. And further, to admit the possibility of so extraordinary a circumstance, we must suppose, (the narrowness of the opening in some argonauts considered), that the *Seiche* would place itself sideways,—that is, for example, the ventral part to the right, and the dorsal part to the left; which would be contrary to what we have just pointed out as existing in the cephalopod of the argonaut, where the dorsal part is always behind, and the ventral part always before, without a possibility of its ever being otherwise.

M. de Blainville cites M. de Roissy, as having assured him that he had seen in the hands of M. Ferussac, in two different species of argonauts, the *A. lisse*, and the rice-grained argonaut, two poulps, evidently of the same species. Here is, certainly, a very strong objection, and one which appears to carry much weight; for M. de Roissy, as all naturalists know, is an observer as skilful as conscientious, and, for our own part, we often allow ourselves to be influenced by his judgment, because we have learned to know its worth; but, wishing to have on this subject very accurate details, we interrogated this naturalist, and, we confess, that the objection lost a great deal of its merit in our eyes, when we had learned from his own mouth that he had not seen the two poulps in question in the hands of M. Ferussac, but merely drawings of them! and that also these allowed some slight differences to be perceived, particularly in the colouring.

Every one will agree with us, that, if this fact does not entirely lose its importance by this explanation, it is at least allowable to adjourn all conclusions with respect to it, and set it on one side. As to the rest, what inference of any importance can we draw thence, when we have shown that the premises are incorrect? In fact, we can affirm positively, that the rice-grained argonaut has never been found in the Mediterranean, but chiefly upon the coasts of Brazil, at the Cape of Good Hope, and in the Indian ocean.

What we have said of the position and use of the membraniferous arms of the poulp of the argonaut, will suffice, according to our view, to demonstrate that the same species of poulp cannot inhabit indifferently either species of shell. If it were otherwise, it would be in fact difficult to conceive how the upper arms and the membranes should be found to correspond in form and proportion with the lateral faces of the shells, which vary much according to their species. Thus we should be troubled to comprehend, how the poulp could maintain itself one day in the rice-grained argonaut, and another in the *Argonauta Argo;* for, if its arms and membranes are just large enough to grasp the extent of the face of the former, they certainly would not be so for those of the latter. We declare further, that we have never found in the *Argonauta Argo* any but the species which we have sketched at the end of this memoir; and we are obliged to add, that the intensely blue colouring which we have never failed to meet with upon the large arms, gives us little confidence in the rather romantic pictures hitherto furnished.

The discovery of the use of the palmated arms overturns some other hypotheses also, from which one or the other party drew more or less force; and by this means it simplifies the question.

Among such, is the assertion advanced by one naturalist, that the two large arms of the poulp arrange themselves in the interior of the shell, in such a manner that they correspond exactly to the two tuberculated edges of the keel, and that then the suckers form the tubercles;—and also Ferussac's way of viewing it, who thought that the palmated part of the great *tentacula* rolled itself into a little globular mass, in the spiral cavity.

Such is also this other opinion of M. Delle Chiaje, who thinks that it is by means of suckers that the animal transudes the calcareous matter, destined for the progressive increase of the shell; and he finds proof of it in the supposed fact that the animal adheres to its shell only by these organs: an assertion also of this same naturalist, that the animal sees through his shell both his enemies and his prey; a circumstance, which we confess appears to us difficult to credit, on account of the covering over of this shell by the membrane of the great arms, which must considerably diminish its already small degree of transparency; and, finally, a description in which the same naturalist proclaims the fact, that when the poulp of the argonaut wishes to change its place, it overturns its shell, spreads out at the surface of the water its two membraniferous arms, as well as its pointed *tentacula*, so that there remains only its body in the shell, adhering to it only by means of some of the suckers nearest the base. It is now evident, that the palmated arms answer a purpose quite different to that of floaters.

(To be continued.)

Art. III.—*Notes on Irish Natural History, more especially Ferns.* By Edward Newman, Esq., F.L.S., &c.

(Continued from Vol. 3, page 577.)

Ascending the rising ground to the south of Ballinahinch, I found the view amply repay me for the trouble. Immediately beyond the palace — the *Martin* is a king in Cunnemara, and his house a palace—rose that strange assemblage of hills called the Twelve Pins. In my endeavours to count these Pins, I was quite unsuccessful; and the number appears to be optional on the part of the counter. Immediately around the palace,—a modern and by no means an elegant building, —water and wood are very prettily interspersed. It would add some fraction of interest, could I name the bold headlands that jutted out into the more level bog,—the lakes that even there, mountain-locked though they were, reflected a bright blue sky, and fleeting clouds of surpassing whiteness,—and those lovely islands, covered with the richest, thickest, wood. How is it that throughout Cunnemara the lake-islands alone bear trees; and that here they abound, often to crowding? I here observed the hen-harrier (*Cercus cyaneus*), hunting over the bog, as owls fly along our fallows in quest of mice. The face was turned downwards, often however being moved in every direction. I fancied that these marauders were looking after the young curlews, which I am sure must be abundant, for the old ones would run before me as I marched over the hills, and waken the echoes of the Twelve Pins with their piercing whistle. The curlews were evidently at enmity with the hawks, for sometimes two or three would follow a hawk, and attempt to stun him with their whistle; when they approached too near, the hawk would turn up his round face as if to take a survey, and then look down again, as if quite satisfied he had nothing to fear, and with noiseless wing beat the bog as regularly as a well-trained pointer. During the day I saw many of these hawks, but not one that I supposed to be the female of the same species. In the afternoon the clouds came down so low as to hide all the hills by whose forms I had endeavoured to steer, and I was not sorry to see something like a road winding round the shore of a beautifully wooded lake. This lake, I afterwards learned, was Garromin, and the beautiful woods the property of a Mr. Mahon, the residence being called Glendalough.—Just beyond the lake is a cottage called the Recess, and a few cultivated fields, one of which was completely purple with the blossoms of *Vicia Cracca*. The grass was intended for mowing, and was not yet cut; I think it would have averaged three feet in height, and was very close together; the crop must have been enormous. There was a patch of oats that must have been nearly five feet high. From what I learned at Roundstone, I believe the land might be taken at sixpence or eightpence an acre unbroken, and where partially reclaimed, for 2s. or 2s. 6d. The plan is, "to set a plot of ground to a man," that is, to let him have a tract marked out from point to point, but not measured, and for this he is to pay an annual sum. I saw, above Roundstone, plots of about forty English acres, as nearly as I could guess, set at £2 a-year. But between Urrisbeg and the sea, is a wonderfully rich and populous tract, which is let in small pieces, and at a higher rate. I was told that in the immediate vicinity of Roundstone, under Urrisbeg, is a population of nine or ten thousand people; and I was quite willing to credit it, for I could have counted near upon a thousand cabins, and ten inmates to a cabin is by no means an extraordinary number.—But this is going back. From "the Recess" I walked steadily along the road, in hopes of finding a place to stop at for the night, but the bog seemed to stretch out before me, and I began to anathematize the Irish miles. I felt a twinging in the shin-bone, which had annoyed me for seven or eight days, get so much worse that I could scarcely step with that leg without crying out. I had walked ten hours a-day, which would be thirty English miles, ever since I broke my shin in crossing Achill Sound, and it had day by day grown more painful. I began to think the bone was splintered, and that I might perchance meet with an effectual stop, if I persisted in bearing up against it: so I made a halt, took off my knapsack, and throwing it on a bed of *Menziesia*, sat myself down and examined my wound. I did not like its appearance, and waited for chances.

I sat for an hour, or rather perhaps lay, for I went to sleep; and a party of police, who had been spending a night in Clifden, to superintend "a pattern" which had been going on there, happened to come by on their way to their quarters. They had a queer vehicle; it was like a London brewer's dray in nakedness, but with this difference, that the wheels were under the bed of the vehicle, so that they could sit over the wheels and dangle their legs down, as in an outside car. Of course they gave me a lift, and deposited me safely at Flynn's half-way-house,—a wonderful place, and the residence of a whole colony of Flynns. I shall never forget the kindness of the people at this place: they made me an excellent fire in my bed-room, brought in an enormous dinner,—a fowl killed on purpose, a pile of potatoes, eggs and bacon, beyond all possibility of consumption, bread and butter, and whiskey *à discretion*. The next morning I could not walk; so I got a lift by Bianconi's car. Leaving Flynn's there is a most sweet lake—Lake Shindella—to the right, with such beautifully-wooded islands, it was like a land of enchantment. After Shindella was passed, there came a line of lakes along the road to the right, and finally these issued in a river, and this river still accompanied the road, but was so overshadowed by a fringe of huge *Osmunda regalis*, that the stream was often lost to sight, though its course was abundantly marked by this luxuriant fern.

The road towards Galway is cut beside the river Feogh, a picturesque although small stream. It runs rapidly over limestone slabs, and similar slabs often overhang its waters, particularly where it enters the village of Oughterard. Here, for many hundred yards, a beautiful skreen of the greenest ivy is suspended from the bank above, and actually dips its extremities in the rushing river, forming caves and grottoes that naiads and water-nymphs might be proud to occupy. The stream falls over a succession of ledges, and just after passing Oughterard, it flows under one huge slab of limestone, and is soon lost in the waters of Lough Corrib. The slab or mass of stone forms a natural bridge, over which passes the road to Galway. Here the country becomes more cultivated, and planting to a very great extent is going on. The face of the country is very curious, displaying the most wonderful forms of craggy limestone that I have ever seen, and crowded with such ferns as delight in rocks. *Ceterach officinarum* and *Asplenium Ruta-muraria* and *Trichomanes* were in the greatest possible luxuriance. And here I should remark that I have never seen *Ceterach* so fine as in Ireland. Many of the larger ferns were also in great profusion :—*Lastræa Filix-mas*, *Las. dilatata*, *Athyrium Filix-fœmina*, and all the forms of *Polystichum aculeatum*, and *Osmunda regalis*, as usual.— Most of these ferns, but more particularly *Ceterach*, occurred on all the walls till I reached Galway.

The walls and houses in Galway are half covered with *Parietaria officinalis ;* and being generally very old, and the *Parietaria* unusually fine, we might venture to call the old age of Galway "a green and vigorous old age." *Ceterach* is abundant all round and even *in* the town. Mine host having detected me in the act of stowing away a few fronds I had just been gathering, informed me of a botanist resident in the town, and assured me he would be glad to see me; so in a few minutes I found myself in the presence of one of the most ardent and right-spirited naturalists with whom it has ever been my good luck to foregather. He gave me authentic specimens of *Erica Mackaiana*, and also *Adiantum Capillus-Veneris* from the foot of Urrisbeg.

Having now finished Cunnemara, I will just run over an enumeration of its ferns. *Adiantum Capillus-Veneris*, rare; *Lomaria spicant*, abundant; *Pteris aquilina*, very sparingly scattered; *Polypodium vulgare*, very local; *Cystopteris fragilis*, local; *Polystichum aculeatum*, abundant in some places and in all varieties; of *Lastræa Oreopteris* I saw one plant only; *Las. Filix-mas*, rare; *Las. dilatata*, everywhere; var. *dumetorum*, abundant; *Athyrium Filix-fœmina*, abundant; *Asplenium Adiantum-nigrum*, common on rocks; *Asp. Ruta muraria*, on ruins; *Asp. marinum*, on cliffs by the sea; *Asp. Trichomanes*, on rocks and buildings, not uncommon; *Ceterach officinarum*, abundant on walls; *Scolopendrium vulgare* not common; *Hymenophyllum Tunbridgense* and *Wilsoni*, on wet rocks, and intermixed; *Osmunda regalis*, most abundant, sometimes covering small islands in the lakes. In passing through the country as I did, and omitting altogether the Mam Turk range and the Twelve Pins, I must of course have missed many of the finest localities; yet is this list a goodly one.

I must now make a comment or two on those ferns which I either did not see at all, or saw but seldom. *Polypodium Dryopteris* and *Phegopteris*; these species, in Scotland and Wales, abound in all districts similar to those which I hunted most diligently in Cunnemara, yet I never detected a single frond of either of them in the latter district. *Lastræa Oreopteris;* although I traversed large districts such as this fern usually delights in, yet I found it not. I suppose the climate is too warm for these three ferns, and that they may possibly occur at a greater elevation: but I should say that *Polypodium Dryopteris* is exceedingly rare in Ireland, and with the exception of two fronds gathered by Mr. Moore on Knocklayd, Co. Antrim, I have neither seen nor heard of a single specimen. *Osmunda* seems to have completely taken the place of *Pteris*, and *Filix-fœmina* of *Filix-mas*.

It was with regret that I left Galway without having visited the South Isles of Arran, but the lame leg was in the way, and moreover I was told it would be difficult to get away from them with the violent wind that was blowing off shore; and much as I wished to see them, especially Arranmore, where *Adiantum Capillus-Veneris* is found in such profusion, I was compelled to give it up, and to patronise Bianconi.—And really, after all, it is a great comfort that you never can find yourself in any considerable town, without finding also one of Bianconi's cars ready to transport you elsewhere as soon as you please. As I was jolting along the bank of Galway Bay, the clouds were driven headlong to seaward, and the sun burst forth with bright but watery splendour. I gazed on the Isles of Arran, as they rose clearly from the sea against the blue horizon, and even then I was half tempted to turn back, but having passed Oranmore, the sea was lost, and I turned my thoughts inland. The country now assumed a very different aspect; it appeared bleak, but was generally cultivated, and lets at fifteen or sixteen shillings per Irish acre. Close to the town of Galway the little paddocks for cattle let at £4. and £5. per Irish acre. Loughrea is a miserable place; whole streets of houses are without roofs, and I felt tempted to enquire whether the plague or cholera had stripped them of their inhabitants. Passing the little village of Aughrim, where the decisive battle was fought between James and William, I reached the important and thriving town of Ballinasloe. There I got on board a boat just starting by the canal for Shannon Harbour; it was drawn by three horses, a boy was on the first, and another on the third, and they set off at full gallop, the whips cracking, and the boys vociferating in the choicest Irish.

At Shannon Harbour I stayed a night, and then went on board a steam-boat going down to Limerick; it is a tedious passage. The steam-boat which took us to Portumna was a funny concern; the funnel, boiler, pistons, &c., amidships, and the paddle-wheels aft, and no wider than the deck. The locks and artificial cuts seemed endless; it is called navigating the Shannon, but you really navigate little canals, twelve or fifteen feet wide, for which the Shannon supplies the water. However, when we arrived at Portumna, a steamer of the usual build took us through Lough Derg, the scenery of which is certainly interesting, but can scarcely be called fine. This steamer took us to Killaloe, where a boat awaited us, which, with trotting or galloping horses, conveyed us through a perfect labyrinth of locks to Limerick. To compare the navigation of the Shannon with that of the Thames, is sheer nonsense. The Thames is a vast and deep tide-river, at all times navigable, but at high water capable of floating the largest ships ever built: the Shannon is wide, but so shallow in some places, and so rapid in others, that it never can be rendered of any mercantile importance. Limerick has in it a great deal to occupy the time and attention. It is divided into the new and old towns: the new town is very respectable in its way, a sort of Pavement-Moorfields-looking place, and a long straight street, and the houses much of a sameness; but the old town, on the Clare side of the Shannon, took my fancy amazingly. I ascended the tower of its antiquated and mis-shapen Cathedral, and gathered *Scolopendrium*, and *Ceterach*, and *Ruta-muraria*, from its summit, and looked over that ancient town, which is known by the opprobrious epithet of "*English.*"

I visited Castle Connell, a poor little village six Irish miles from Limerick, much frequented on account of the appearance of the Shannon, which is here very shallow, and runs over a bed of stones. I crossed to the Clare side in a little boat, and the boatmen were very impressive in their conversation touching the danger of the passage (which they perform twenty times a-day), and told me the falls were considered the finest in Europe, and that Mr. English (Inglis) had been there. On the Clare side are the grounds of Sir Hugh Massey, and the view of the river from the "hanging gardens" as they are termed, is very pretty: there is a constant ripple for half a mile. Having heard so much of Mr. Inglis at this place, I looked into his book, and find, after a page of grandiloquence, the following *wind up.*—"None of the Welsh waterfalls, nor the Geisbach in Switzerland, can compare for a moment, in grandeur and effect, with the rapids of the Shannon." On the walls of the hanging gardens I saw abundance of *Ceterach*, *Asplenium Ruta-muraria*, *Adiantum-nigrum*, and *Trichomanes*, *Scolopendrium vulgare*, *Lomaria spicant*, and *Polypodium vulgare.*

Having had my leg mended by a regular practitioner at Limerick, I determined to rest it another day, and so got on board a steamer bound for Kilrush. It was a glorious day, and the steamer ploughed the sea in gallant style. I call it sea, for though in courtesy called 'the Shannon,' it is in fact all sea below Limerick. There is much to amuse the tourist in this picturesque estuary, but I panted for the mountains, and was tired of steam-boats. On approaching Kilrush, we obtain a view of Scattery Island, with its numerous ruins and lofty round tower. I did not go to the island, but was told that this round tower is perfectly solid — a compact mass of stonework from the base to the summit. If so, it differs abundantly from all other round towers, for they invariably have an internal cavity, apparently to allow of ascent within.

The island is sacred to St. Senanus, who flourished here long before St. Patrick came into fame. The crabbed old saint indignantly refused to permit any woman to set foot in his territory. I believe it was Cannera, a saintess, conveyed thither on a raft by an angel, whose disappointment Moore deplores to the tune of '*The Brown Thorn*,' taking care, rather wickedly, to add, how

——— "legends hint that had the maid
"Till morning's light delayed,
"And giv'n the Saint one rosy smile,
"She ne'er had left his lonely isle."

Oh rare Tom Moore! I heartily wished I could conjure up old Senanus from his long rest, not to enquire about Cannera, but simply to ask what the Scientific Associations, and sapient literati of his day, said about the round towers. It would be amusing to know at what conclusions they arrived, and to whom was then assigned the premium on round-tower-speculation. It is very obvious that in the earliest days of Christianity, when it is to be presumed the Irish were possessed of more zeal than architectural skill, these towers were frequently built into their churches, and performed the office of heaven-pointing spires: but of so superior a structure were these spires, that even now, when the early churches have mended the roads, or are reduced to a confused and scattered heap of ruins, the towers stand triumphant and alone in their glory, sneering sarcastically at the feeble efforts of time.

When I landed at Kilrush, I found all the steam-boat people were going to Kilkee, and I did the same, without any definite object. There were from thirty to fifty cars on the quay where we landed, and twelve of these were soon freighted with live lumber for Kilkee. We were a formidable body altogether; it must have been a gallant sight to a traveller moving in an opposite direction! Whips cracking, carmen shouting, and the company laughing, talking, and smoking, and on terms of the most easy familiarity with each other.—On arriving at Kilkee there was a regular car-race to the best inn, and when that was glutted, to the second-best, and then to the third.

When I arose the next morning I was located at a fashionable bathing-place. I found it extremely difficult to make the Irish believe that I was such a fool as to wander over their island in search of plants or insects, or to see the country. At Kilkee the folks were of a very respectable class, and evidently felt hurt at my explanations; they thought I was "smoking" them, so I pleaded my leg as an excuse for coming to Kilkee, and this seemed perfectly rational; and when I left the place about thirty-six hours after my arrival, they kindly hoped I had "found the benefit." Up to this period I think I had been asked a hundred times my name, occupation in life, country, exact place of abode, the place I had last come from, the place I was next going to, the object of my journey, what I had in my knapsack, and at whose expense I travelled.

(To be continued).

Art. IV.—*On the London Clay formation at Bracklesham Bay, Sussex.*—By James S. Bowerbank, Esq., F.G.S., &c.

There are few localities where the London Clay can be examined, of which so little is known, and which at the same time is so worthy of a careful investigation, as that portion presented to our view by the action of the sea at Bracklesham Bay and its neighbourhood, on the coast of Sussex. The deposit here differs so much, both in its mineral character and fossil contents, from the same formation in other parts of England, and exhibits so close an approximation in both these respects to the corresponding beds in France—those of the *Calcaire Grossier*—as to render it a matter of surprise that it has not attracted a greater share of the attention of English geologists.

The low clay cliffs extending from Selsea Bill to the mouth of Chichester Harbour, seldom exceed ten or twelve feet in height, and for by far the greater part of the space intervening between these points, do not rise higher than six or seven feet. This section presents the usual characteristic appearance of the London clay,—dark brown or blueish clay, with rarely any fossil remains. The base of this low cliff is usually covered up with shingle, which extends towards the low water mark for about fifteen or twenty yards, and there it terminates. The remaining space intervening between the foot of the shingle bed and low water mark, is in many places at least 80 or 100 yards in length, and presents a surface, under ordinary circumstances, of a clean, dark greyish-green sand, with scarcely a single pebble to be seen: but after some tides it is literally strewed with thousands of the detached valves of *Venericardia planicosta*, and of other shells, while at other times scarcely a shell can be found. The part of the bay most interesting to the geologist, is that immediately in the neighbourhood of Bracklesham Barn, especially at about a furlong to the east of that spot, where there is a small break or chine in the low clay cliff. At this place, and at a few paces east and west of it, beneath about six or seven feet of clay, there is a stratum of light green marly sand, abounding in remains of *Venericardia planicosta* and other shells, but which is frequently entirely hidden by thrown-up shingle, and it is very rarely that more than a few feet in length of this bed can be seen. It is from this bed, or from one exceedingly like it, somewhat lower in the series, that perhaps most of the interesting shells of this district are to be procured. If we proceed from this little break or chine westward, for about forty paces parallel to the coast, and then in the direction of a line at a right angle to the cliff, and at the time of low water, we shall find, near the low-water-mark, the bed we have described as abounding in fossils, exposed by the action of the sea in the most favourable manner. At this spot *Venericardia planicosta* is found literally by thousands, with the valves united, the shells resting upon their edges, and packed close to each other, exactly in the manner that we might expect to have found them, supposing them to have been recent shells with the animals yet inhabiting them. Comparatively very few are gaping, and their condition and position strikingly impress upon the mind the idea that when alive, they must have inhabited the spot from which they are now disinterred; especially as there are numerous small and fragile species of other well-known London-clay shells, which could not have remained whole had they been subjected to much attrition amid the larger shells surrounding them. On the sands in the vicinity of this spot I found large masses of *Nummularia lævigata* cemented together, and numerous detached specimens of the same shell.

At the eastern extremity of this bed, which, at the time of my visit, was opened for about fifty yards, I found *Sanguinolaria Hollowasii*, a rare and fragile, but very beautiful shell, in a fine state of preservatiou. At about twenty or thirty yards westward of the western end of this interesting patch of shells there are large blocks of this bed, which, being of a firmer texture than the surrounding parts of the deposit, have suffered less from the action of the water, and project about twelve or eighteen inches above the surrounding sand, and, by presenting an obstruction to the ebbing tide, they usually induce the formation of a small pool amidst which they stand. At the south-eastern side of this pool, on one occasion I found the stratum, which is usually covered by the sand, completely exposed. At this spot there was scarcely a specimen of *Venericardia planicosta* to be seen, but instead of this shell, *Turritella conoidea* and *edita* were embedded in a dark green marly sand; and among them, together with *Fusus longævus*, and other well-known London-clay shells, I found *Venericardia acuticostata* and *mitis*, and a splendid specimen of *Conus deperditus*, fully equal in size to the one figured by Deshayes. Westward of this point I did not meet with anything particularly interesting.

Proceeding eastward from this locality, I found, at about midway between high and low water mark, *Cerithium Cornucopiæ*, a *Corbula*, whieh I believe to be *Corb. gallica*, *Cytherea trigonula* and *sulcatarea*, and a new species which I cannot find in Deshayes' work; and also *Arca duplicata* and a new species of *Crassatella*.

About midway between Bracklesham barn and the Thorney coast-guard station a series of patches of a deposit of chalk-flints was exposed: the first of these was nearly at low water mark, and the remainder of them ran, at short distances from each other, in a diagonal line towards the coast, nearly in the direction of a straight line drawn from their western extremity to the Thorney station houses. Apparently, this stratum of flints has not, at any time, exceeded eight inches or a foot in thickness, they are indeed so thinly scattered as rarely to occur piled upon each other: very few of them have suffered from attrition, and the greater part retain their original form and whitened surface. They are firmly embedded in the same light green marly sand, which I before described as occurring at the bottom of the London clay, in the neighbourhood of the little chine near Bracklesham barn. Amongst the flints there are numerous remains of the roots of trees, in the state of soft bog-wood; which indicate that this portion of the strata has been very thinly covered by the superimposed clay.

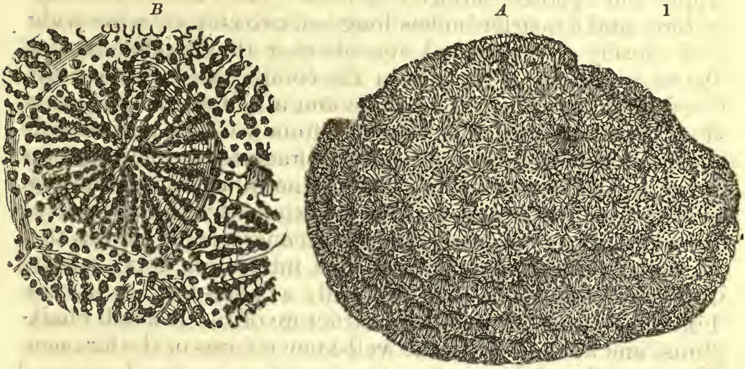
Upon one of the bouldered flints, firmly embedded in the marly sand, I found the most interesting of the valuable series of fossils which I had the good fortune to obtain during this excursion, namely, a fine specimen of *Astrea*, built upon the upper and exposed surface of a flint. The base of the coral is three and a quarter inches long and two and a quarter wide, and closely embraces and spreads over the rounded edge of the smooth stone. No part of the coral appears to have suffered from attrition: it is three and a half inches in height from the base to its upper surface, from which a considerable portion has been broken off, and the fractured surface presents every appearance of having suffered no other injury than that arising from the action of the water since it was exposed. I have carefully examined, with a high microscopic power, thin sections of the stone on which this interesting coral is built, and can safely assert that it is truly a chalk-flint, as it exhibits the characteristic organic structure of the Kentish chalk-flints, and abounds with the well-known forms of the foramenferous shells of the chalk. On the following day I obtained from one of the coast-guard, a second but smaller specimen of the same coral, which had been picked up close to the spot where mine was procured. This has been drawn by Mr. J. De C. Sowerby, and engraved to accompany the present paper.

Astrea is completely a tropical genus; but when we consider the many other tropical forms occurring in the same formation, such as those abounding among the fruits, the remains of saurians and fresh-water turtles, and also that *Astrea* has been found in the lower beds of the *calcaire grossier*, we shall not be surprised at its occurrence in beds which, according to the description of Mr. Webster, are so closely allied to those of the *calcaire grossier* of Liancourt, both as regards their mineral character and their fossil contents.

Near the Thorny coast-guard station *Cerithium Cornucopiæ and giganteum*, *Turritella sulcifera* (*Melania sulcata* of Sowerby), *Tur. terebellata* and *multisulcata*, are found, although I could not ascertain the beds from which they come, but their position is probably lower in the series than those which occur to the westward of Brackelsham barn. Sowerby, in the description of *Melania sulcata* in the 'Mineral Conchology,' states that at Stubbington, where the specimen figured was found, "the cliff is twenty or thirty feet high, composed of sand and gravel, more or less mixed with blue mud, and frequently irregular patches of sand. At the base of this is a stratum, not more than two feet thick, of blue clay or mud, in which the shells are found."

I propose naming the coral (fig. 1), *Astrea Websteri*, after the veteran geologist who has thrown so much light upon the formation in which it was found.

B A 1



A. Astrea Websteri from Brackelsham Bay. *B.* A portion of the same magnified.

Note by Mr. James De C. Sowerby, *upon the* Astrea *from Bracklesham Bay.*

Five or six species of *Astrea* nearly resembling this are found at Hauteville, and other places in La Manche, where *Cerithium Cornucopiæ* abounds. The existence of this *Astrea* at Brackelsham Bay is therefore another link between the London clay of Hampshire, and the tertiary beds of France, and would indicate a temperature progressively higher in that direction, when the beings, the remains of which we now find, were living. May we not hope that an assemblage of such indications may hereafter show if any, and what, changes have taken place in the position of the equator since the deposition of these strata?

Camden Town,
Nov. 23*rd*, 1839.

ART. V.—*On the Flora of Snow Formations, in reference to the theory of Spontaneous Generation.* By W. WEISSENBORN, Ph. D.

ALTHOUGH the recent discoveries of Professor Ehrenberg appear little favourable to the *casual* production of organic beings at the present period, yet they do not in the least affect the theory, that their original existence is owing to a *purely dynamic process.* The importance of this subject will perhaps excuse me if I try, in this place, to lessen the weight of the above conclusion,[1] by some reflections (founded on facts to which I have not alluded in my former articles on this question) on certain spontaneous generations which are undoubtedly going on in our time, and which, in calling the attention of the reader to the traces of a nascent future creation, may serve to throw some light on the conditions of the former and present ones, as well as to banish the *uncouth* idea of a Deus ex machinâ.

Although it would appear from the calculations of M. Fourier, as applied by M. Arago (Annu. du Bur. de Long. 1834), that the general temperature of the globe has not changed by $\frac{1}{10}$ of a degree centig. within the last 2000 years, yet, according to the theory of cosmogony now universally admitted, the body which shall next add a new crust to the solid part of our planet must be water, in the various modifications of structure which it presents under the forms of ice and snow. What proportion of the existing quantity of it will be required and consumed in completely oxidizing and cicatrizing the actual mineral crust, it is impossible to determine; but leaving the vapoury part of it out of the question, and supposing the mean depth of the ocean to be only four miles (the calculations of Laplace make it from four to five), and its extent about three fourths of the surface of the globe; then supposing the mean density of the solid and lasting products of water, from the hardest ice to the lightest snow, to be half that of water, the thickness of the strata that will be added to the globe by the solidification of the water existing on the surface of our planet will be six miles. Were the bed of the ocean to remain in its present state, the present mineral crust of the globe would be covered only three miles high, reasoning from the present level of the sea; but as that bed is constantly filling up, the distribution of the crust of ice and snow over the whole

[1] The conclusion alluded to will be found in Vol. 3, page 508, in an analytical notice of Ehrenberg's work.—ED.

surface of the globe, will be considerably more equable than might be otherwise anticipated.

Now we must suppose that long before the whole of this vast geological formation shall be added to the surface of the globe, the whole of the living creation strictly belonging to the present crust must have become extinct, and their remains imbedded and partly preserved in snow and ice, as their existence is incompatible with a ground composed (chiefly) of snow and ice, as well as with a perfectly dry atmosphere.

Let us not, however, conclude, that this new surface will present a dreary aspect, or be void of vegetable and animal life. During the gradual transition from the present state of things to the succeeding one, it cannot be doubted that many organisms will arise which will link the succeeding creation to the former; and though in the present state of the surface the geographical distribution of the species of the same families appears to prove that the complication and perfection of structure depends greatly on the quantity of free caloric present, yet we have no right to conclude that under circumstances entirely changed, the comparative absence of that element from the ground must render the new creation comparatively scanty and imperfect. Nay, there are many reasons which would seem to support an opposite conclusion.

For proofs of this new creation we have to look to the poles and the tops of the alpine mountains, where the geological formation of ice and snow has already fairly begun. The Flora of these regions is, as yet, very poor; but we have to consider that it is in an incipient state. On the Alps grow two species, the red snow (*Protococcus* or *Palmella nivalis*), and a very curious production which M. Hugi found only on the glacier of the Unteraar, but which is said also to occur on that of Chamouni, a description of which I shall give below. To the snow-flora of the Poles, consisting likewise of the red snow, the expedition of the Recherche to Spitzbergen has lately added (as stated in a letter from Dr. Robert to Baron Struve, the Russian minister in Hamburgh) a second species of red snow, and a delicate *green* flabelliform plant, two inches in height. Now we have only to notice the peculiar circumstances under which these plants are found, in order to be convinced that they are the specific and spontaneous productions of a soil that is neither "land" nor "seas," and to render it probable that from every new and well-established stratum there will spring a new creation at any time.

As the polar regions are comparatively unknown, and have never been visited by man within about ten degrees of latitude from the north pole, these phenomena have only been well studied on high mountains, especially the Alps of Switzerland, where besides, every modification of lasting ice and snow is comprised within a narrow range that may be surveyed with comparative facility.

From the foot of the glaciers to the highest tops of the mountains, the ice and snow present a constant change of stratification and structure. Below, the strata average eight feet in thickness, and the ice-crystals are often two inches in diameter. On the summit of the glaciers the thickness of the strata is at most two feet, and sometimes only six inches, and the crystals having become gradually smaller on the limit of the glacier, pass into that sort of granulated snow which in Switzerland is called *Firn*. The appearance of this firn marks the limit of a very important change in the meteorological conditions. Whilst the height at which the glaciers begin, as well as the line of perpetual snow, varies in Switzerland from 6,000 to 10,000 feet, according to the exposure, the firn-line, at about 8,000 feet, appears to be comparatively independent of temperature. At the height of from 10,000 to 12,000 feet above the sea, M. Hugi sometimes observed a heat of from +15° to +20° R. (the thermometer being probably exposed to the direct rays of the sun), but no melting of the snow, as is remarked below the firn-line at much lower temperatures; (see Hugi's Naturhistorische Alpenreise). At such high temperatures M. Hugi saw the fresh-falling snow losing its needles or rays, and converting itself into grains, so as to constitute *firn*, whilst the old firn became loosened to the depth of several feet, so as to resemble a heap of hemp-seed.

The *Palmella nivalis* is found exclusively on the firn, beginning at the firn-line and ending about 1,000 feet above it. It is never seen either on a glacier or common snow; its favourite habitat being sunny slopes where the snow is quickly changed into firn. In August it is already blackish and decaying at the firn-line, in full growth at 8,200 feet, and just springing at 9,000 feet. It is interesting to find that not only is the existence of the plant strictly dependent on that of firn, but that the form of the former bears an evident relation to the structure of the latter. The little plant, in its nascent state, has the form of the letter Y, the simple radicle descending between two grains of the second layer, and the two little branches embracing one grain of the surface-layer of the firn. In its incipient state it tinges the firn with a delicate rose colour, which is not observable when the eye is brought to the same level as the surface of the firn; but when fully developed, the plant appears above the surface of the firn, which is then beautifully crimson-coloured. The *Palmella* afterwards becomes dingy, and at last black, decaying into mould which sinks into the firn.

The second plant of the snow-formation, which M. Hugi discovered, is never found on the firn or common snow, but grows out of the *solid ice of the glacier* of the Unteraar. The circumstances under which it is produced, and the plant itself, are described by M. Hugi as follows.

It is well known that all snow melts away from that glacier (as well as others) every year; and a certain portion of the surface of the glacier of the Unteraar is afterwards seen studded with innumerable holes, from one to six inches wide and from three to twenty inches deep, the bottom of each being filled with black mould. In the neighbourhood of still existing snow-patches near the north-easterly side of the glacier, M. Hugi found these holes as yet very shallow, and a substance, more like jelly than mould, was still adhering to the surface. Soon after he also observed on the very borders of the snow-patches, while yet at some distance, spots of a bright yellow colour, which he found to be substances nearly the size of the hand, and an inch thick, very delicate and spongy, the under surface of which was strongly attached to the glacier, but they were unfortunately already in a state of decomposition. If he removed them, they melted into a colourless water, leaving his hands stained with an ochraceous substance. It was only in one spot that he found a well-preserved specimen of this plant. He cut out the part of the glacier on which it was growing; the ice was perfectly pure and transparent, the plant was about the size of a hand, and half an inch thick, and presented ill-defined hemispherical protuberances and almost the appearance of a *Tremella*, but had so little cohesion that every part, when touched, crumbled, or rather melted, away. The whole appeared like a beautifully bright yellow excrescence of the glacier, studded with bubbles, and melting into a water of the same yellow colour; whereas in the older and decayed plants, the colouring principle had already been precipitated. This production was sunk between the crystals of the glacier, into which it had struck innumerable capillary radicles. The line of separation between the ice and the growth could nowhere be distinctly made out, nor could any peculiar organization be discovered in the latter, even with the assistance of a lens.

We see, therefore, that the power of vegetation is inherent even to snow and ice, and that by creating organized beings, which decay, they lay the foundation for the existence of beings of a higher order.

As to the production of *animal* life on the snow-formation, we cannot expect to find living proofs of it in the present incipient stage of the formation itself. In the mean time the existence of creatures, as the *Podura nivalis*, which are matured by the influence of snow, and can only enjoy their lives on that substance, will justify the conclusion that a continuous surface, formed chiefly of ice and snow, does not exclude animal life.

But if we must admit the Flora of the snow-formation to exist by dint of spontaneous generation, it is but rational to conclude that the higher creatures, to whose purposes that Flora will, without doubt, be at some time subservient, will not be created by direct supernatural intervention; and although it may never be given to man to point out clearly *how* the natural powers, through which the Almighty manifests Himself to him, have operated or shall operate in creating animals, yet any unprejudiced mind may clearly discern that by cutting the knot in the customary manner, *we can never hope to arrive at anything like a fair solution of the question.*

Weimar, 1839.

ART. VI.—*Remarks on some species of* Asterias *found in Cornwall.* By JONATHAN COUCH, Esq., F.L.S., &c.

I HAVE the pleasure of forwarding for insertion in the Magazine of Natural History, a notice of some of the less known species of British star-fish, of the first of which Dr. Fleming complains of the want of a figure and description. These might have been supplied before now, if naturalists, living in some of our larger ports, where the trawl-fishery is chiefly followed, had examined the various matters torn from the bottom by that mode of fishing. I have no opportunities of this sort, and am chiefly indebted to accident for the possession of the specimen here described.

PRICKLY STAR-FISH. *Asterias spinosa.* Flem. Br. An. p. 487.

The diameter of this specimen, across the disk and rays, was thirty-three inches; but the five rays were not of equal length, the longest being fourteen inches. Across the disk, in one direction, the diameter was three and a half inches, in another, three inches; the disk depressed, although this may be only casual. The rays, at their origin, were two inches wide, tapering, depressed, flaccid; their spines stout, and each surrounded by a tuft of fine suckers. Two of the rays have a double row of spines along the middle, divided by a slight depression; on the other rays only one row of spines, which are irregularly scattered. There is also a marginal row pointing obliquely downwards and forwards. The colour above, reddish brown; tufts round the spines yellow.—Interior of the stomach pale green, and surrounded by eleven teeth. This species bears a greater resemblance to *Asterias glacialis* than to any other known to me; but besides its superior size, it differs in having the rays less tapering and more flaccid. The proportion of the rays to the disk is also different; for whilst in *Ast. glacialis* they are as two to one, in the species under consideration, on the under surface, where they are best defined, the breadth of the disk is to the length of the ray as two and a half to twelve. The individual spines are, indeed, not much unlike those of *Ast. glacialis,* but their distribution, and consequently the figure of the body, are different. The spines on the disk are smaller than those on the rays. The weight of the body cannot be sustained, or even turned over, by lifting it by the rays, without separating them. The specimen came from deep water.

It must be allowed that on comparing this description with the figure of a portion of a ray of *Asterias spinosa* in Borlase's Natural History of Cornwall, plate 25, fig. 18, the resemblance is not exact; and I have no opportunity of referring to other original authority. If, therefore, any learned naturalist shall dispute the appropriation of the name, I give it up, on the condition that a more correct reference be given.

The difficulty of doing this, however, will be somewhat appreciated by referring to the present state of our knowledge of some other species, which is far from satisfactory. Thus, in the Mag. Nat. Hist., o. s., vol. ix, page 145, the references are made on the supposition that two species have been confounded; one of which is studded with produced spines, while the other, strictly speaking, has none,—the moveable, leg-like crutches not being regarded as such. It is true, the species above described cannot have been known to Dr. Johnston; for he represents his *Ast. rubens*, of the length of twenty inches, as being superior in size to any other British species.—But the Doctor, who probably is as competent to settle the synonymes of the British *Asteriadæ,* as any naturalist in the kingdom, is in error when he judges it to be the same as that which he has represented at page 145 of the same volume; and which also is different from that which I understand to be signified by the name of *Ast. rubens.* The latter, as shown in Pennant's Brit. Zool., vol. iv., plate 30, fig. 58, ed. 1757, closely resembles a species familiarly known in Cornwall, which does not commonly exceed the size given in the plate, although a few may be found of twice that magnitude. I must therefore refer to Pennant's *Ast. hispida* as different from the *Ast. spinosa* described above, as well as from the species given by Dr. Johnston under the name of *Ast. rubens*, the figure of which, though slight, is characteristic of one, a description of which I subjoin, as it will supply a few particulars not contained in Dr. Johnston's account.

The diameter of the specimen was eighteen inches, to the extremity of the opposite rays; of the disk, two inches and three quarters; below, the proportion of the diameter of the disk to the length of a ray, as one to two and three quarters; the disk flat; rays seven, thin and tapering; breadth of the ray where widest, one and three-tenths of an inch. Skin coriaceous; on the disk minute spines, several from one base; those on the rays somewhat larger, but less thickly set; along the margin of the rays a double row, larger and more elevated than the others. Leg-spines three lines long; suckers beneath, in two rows, stout. Rays exceedingly frangible; two that were broken off, flaccid, especially at the points; one remaining uninjured, rigid at the tip. Colour reddish orange. In its stomach a purple *Spatangus*, crushed together.

I the more despair of finding a proper synonym for this species, that Dr. Johnston has failed in it; but, as some designation is indispensable, I have named it *Ast. pectinata*, from the minute points which crown the *ossicula*, and which become distinctly visible only when dry.

I shall conclude these remarks by the description of a monstrosity in the common species,—*Ast. glacialis*, the clam or cramp. It is of the ordinary size, and possesses eight rays; but to distinguish it from the simple duplication of parts, it possesses three of those circular dorsal organs, the use of which is uncertain, but of which a common specimen possesses only one. These three occupy triangularly, one half of the disk, and seem connected with four of the rays, the other four lying distinct from them.

This species is in great abundance in spring, being found in multitudes in the fishermens' crab-pots, the baits of which they readily find. As the season becomes warmer they disappear, and in summer comparatively few are to be seen.

Polperro, Cornwall.
December, 1839.

ART. VII.—*On the Monkeys known to the Chinese, from the Native Authorities.* By SAMUEL BIRCH, Esq., Assistant in the Department of Antiquities, British Museum; Assistant Secretary for the English Section of the Archæological Institute of Rome.

(*Continued from page* 592, *vol.* 3).

LEAVING the animals approximating nearest to man in Chinese Natural History, a secondary kind of baboons or apes appears in the Encyclopedia. Of these the Pei, the Hwatso, the Pih yuen, and others, are not very discernable with regard to species, although their general appearance is sufficiently accurate to identify the genera to which they appertain. The Pih yuen is apparently a *Hylobates* or *Machacus ursinus*, and its name implies "a white monkey." The 'San tsae' observes, —" There are in the Tsang ting hills many Pih yuen; their outward appearance is similar to a Me how. They have large and uneven arms and legs, taking long steps, and are excellent climbers of trees; their note is mournful."—San. Zool. iv. 41. The Pe or Pei is perhaps the Siamang; it is figured erect, is described as "like a wild boar, with white stripes, long neck, and tall legs. It stands erect like man, is fierce, stupid, and excessively powerful. It tears up trees and delights in injuring mankind."—San &c. Zo. iv. 19. This animal is not described or drawn in the Japanese Encyclopedia. The Mashe (horse-hog) is fabulous from its description, viz. " that in the Fow yu hills are quadrupeds whose exterior form is like that of an ape with four ears, tiger's hair, and a cow's tail. Their cry is a loud bark. They are designated *Ma she* and eat men. When seen it is a sign of plenty of water." This, if real, is the *Machacus leoninus*. Likewise the animal in Zool. iv. p. 30, where the description states that "in the Yuho kingdom are beasts whose body is of a black colour. Fire issues from their mouths. Their appearance is like a Mehow. They walk and sit like men."—Zool. iv. 30. This is either a *Hylobates* or *Simia Lar*.

The Pih heaou (white bawler) is an edible animal. The San tsae &c. fixes it to the Lun tseen hills. "In the Lun tseen hills are beasts like apes, with long arms. They are fit for killing, and called Pih heaou."—San tsae, &c. Zool. iv. 34. The *Papio Maimon* is figured under the name of Tao teih or glutton, and a most ludicrous mistake has been made by the describers; for since the eyes of the *Papio*, especially of the adult animal, are excessively small, they have accordingly been figured and described in the nipples of the animal!— "The gluttons" says the text "have a goat's body, with human face, eyes under their breast, tigers' teeth, and human nails. Their cry is like the squalling of children; they eat men as well as other things. They are found in the Kewyu hills.—The 'Book of Hills and Streams' calls them Keuhaou."—Zo. iv. 39.

The Tung (*Simia callitrix*) is another type that can be identified. They are described as "belonging to the Yuen yew species, being nimble in their movements, and excellent climbers of trees: both great and small kinds have the long tails of the Yuen, but of a golden colour, and are commonly called Kin-tseen Jung (golden thread Jung). They are bred in the Laeshin hills. Men shoot and kill them with poisoned arrows. Their tails are made into bed-clothes, saddle-housings, and rugs to lie upon. The Jung are vastly fond of their tails, but when struck with the poison gnaw them off through pain, to get rid of their calamity." The Hwatso is a fabulous animal, and consequently an object of superstition. "In the Yaoukwang hills are animals whose exterior appearance is like a Mehow, with human face and hogs' bristles. During the winter they dwell in caves. They are called Hwatso: their cry is like cut water, and when seen they are ominous of a conscription." The "cut water" probably refers to the noise of a mill. The expression "yaon yih" in the text, appears to imply the power that the Chinese, in common with other despotic Asiatic governments, have of forcing people to work for them. Yih is literally "police runners to send out" &c.—Zool. iv.

"The Yew are like the Mehow, and of a deep yellow and black colour; their tails are several cubits long, like an otter's, but have no tufts. When they scent the dew ascending to form rain, they then suspend themselves from a tree by means of their tails, to fill their nostrils with it, or else by both feet. In Keangtung they call them carriers, (Wuhkëen)."—Zool. iv. 38.

The Gaou are said to inhabit the Lunseen hills, to be like an ape, with long arms, to be good for killing, and called Gaou.—Zool. iv. 34. "The Yuen's arms, when cut through at the thick part, can be made into flutes rounder than reeds; they are of the monkey tribe, having long legs, and are good whistlers, given to dragging things about, whence their name is derived from the character yuen, to drag or lead.—Zool. iv. 36.

"The Jen are like the common monkey (how), with green body and dark jaws, they have black whiskers: their paws are also black. They are naturally very fond of their whiskers, and dote on their species, living and dying together; on which account if one can be got at a hundred may be killed. Men shoot them with poisoned arrows; the shot animals' companions draw out the arrow in order to wound themselves, and die with one another." They are also called Kwojen; vide Morrison, (Dict. Chin. and Engl., part ii. vol. i. p. 321. 4to. Macao).

The How monkey (*Simia*) is one name for five sorts, viz., How, Nao, Keo, Yu, and Muh. The female monkey is called [1]Moo how not pin. The name of the Muh (washers) is derived from their habits: "they are naturally addicted to running about, fond of stealing things and utensils, and of imitating men, and as soon as they have brought forth their young, in imitation of mankind, they plunge them into the mountain streams."—Zool. iv. The how is the proper monkey, but no plates being given of the others, it is difficult to guess which are indicated.

This closes the account of the *Simiæ* in the 'San tsae too hwuy:' many of the animals are fabulous, some few perhaps new. The following scattered notices have been collected from other sources, to throw as much light on the subject as limited time and materials will allow. Although Zoology as a science, may not receive much additional information from Chinese works, yet animals of new species may occasionally be found, as was the case of the tapir first described from the Chinese by Mr. Abel Remusat, the existence of which has since been satisfactorily proved. The Japanese Encyclopedia before quoted, only presents its readers with two sorts of monkeys—the Yuen or Yuen-how, and the How, also named Hoosun. Fan ming mo sze cha,—"The name of Fan provokes their irritability." "The keo are like monkeys, and fond of seizing in their paws men's property." Fan is the name of an Indian bonze. (See Heuen &c. part xii. p. 5).

The Urhya, of which a very splendid copy in 4to., an edition of the sixth year of the emperor Keaking, exists in the library of the British Museum, not only contains several drawings with explanations, but also an account of the Yu, or 'domesticated class,' with a commentary. The plates have the Fuh-fuh holding a sword, and said to be "like a man, with straggling hair, and to run after men to eat them."—Urh-ya, part Heahow, p. 27.

The Mung yung nao chwang; "the Mung yung have the appearance of the Nao." Mung yung means dull face.

The "Nao yuen that are good climbers;" a species of *Hylobates.*

[1] Moo, mother, instead of the common feminine adjunct or prefix for female animals.

The "Keo foo, remarkable for their steady gaze;" and in their description in a consecutive page it is stated, that "the Keo are like a large Mehow, that their general colour is of a blueish black, and that when they meet men, they are fond of looking favourably upon them." In commentary upon the Keu they are described as "located in the Keen ping hills, being about the size of a dog, and like a Mehow, having much whisker and hair on the top of their head, which they are fond of brushing, and that when they meet men they pick up stones and throw at them." In the Dictionary of P. Basil the Keo are described as a species of monkey like men, which ravish women. In comment upon the Nao it is stated that "their external appearance is like a luy, but smaller; that they are of a brownish black colour, can be domesticated, are more agile than cats in catching mice, and that Kewshih says that the Nao only come from the south, and are of the Mehow baboon species."

This finishes the monkeys in the works above cited. In the Chinese Dictionary of P. Basil, published by De Guignes, folio, Paris, 1807, the following apes are mentioned.

The "Kea,[1] sort of ape like a man." The Tsoo. The Hoosun, "animal like a monkey." The Nao, "sort of monkey fond of climbing up trees," (p. 408). The Yaou or Yew. The How tsan, (p 408)

ART. VIII.—*On the Siliceous Casts of the* Echinites *from the Chalk.* By EDWARD CHARLESWORTH, F.G.S., &c.

SOME curious facts have, at various times, fallen under my notice, explanatory of the anomalous appearances often presented by the siliceous casts of the *Echini* from the chalk, more especially the genera *Ananchytes* and *Conulus;* and bearing also upon the history of the occasional silicification of the shell itself. Were it not for the costly nature of the engravings necessary to illustrate the subject, I should have been tempted ere this, in some shape or other, to have placed my observations on record; and, without losing sight of this intention, I now only propose briefly to state some of the inferences I have arrived at. In doing this, however, I by no means would have it thought that I reserve to myself the exclusive right of entering, on a future occasion, upon the details

[1] Pronounced also Ko, same meaning as Keo; Kang he tsze &c.

connected with the results now indicated. On the contrary, I should be much gratified if the hints contained in this short notice, were to form the basis of a thorough examination of the whole matter, by any one who may have the time at command, and the necessary materials within reach, for following up the enquiry.

Circumstances attending the disappearance of the Shell from the investing siliceous Matrix.—Upon breaking up the masses of common flint which have been taken from the chalk, where that substance is quarried for economic purposes, the contained shells of the *Echinites*, or the calcareous spar representing the shell, will be found entire, and agreeing with the shells which occur in the chalk itself. This, however, is not the case with the chalk-flints that, at some remote period, have been removed from their original site, and subjected to diluvial action. In these latter, the shells of the *Echini* have disappeared, the removal being either total or partial, according to the alteration in character which the interior of the flint exhibits. In the ordinary flint-gravel, as for example that at Mousehold Heath, near Norwich, the original aspect of the flint is exchanged for a grey or a brownish-yellow colour, and then the calcareous matter of the included fossils is entirely gone, and the space which it occupied left quite free. But in other places, beds of flint may be found overlying the chalk, in which the change in the original condition of the silex is but slight, and the shells of the *Echini* and other fossils are then only partially removed.

Proposed separation of the flint Casts into true and false.—The *internal* siliceous moulds of the *Echini* may be separated by readily-appreciable characters, into what I propose to designate as the *true* and the *false* casts. The false casts are much more abundant than the true, and are distinguished by having upon that portion of their surface which corresponds to the internal face of the ambulacral or perforated plates of the shell, a series of circular and regularly concave pits. Each one of these pits corresponds to an ambulacral perforation; but it very frequently happens that the areas occupied by these hollows respectively encroach upon one another, and the whole then become merged into so many deep sulci, extending from the apex of the cast to the base, and indicating the course of the *ambulacra*. Another condition, much less frequent than the last, but exclusively confined to the false casts, is an abruptly truncate summit, with a surface clearly showing that the deficiency cannot be explained by an accidental fracture, but rather suggesting the idea of the siliceous matter having entered at the mouth and vent, (the shell being in its natural position,) without rising high enough to fill the entire cavity.

The *true* casts principally differ from the *false*, in presenting us with a faithful fac-simile of the internal surface of the ambulacral plates; and unless worn smooth by bouldering, or other causes, the course of the *ambulacra* is indicated by rows of short, cylindrical, siliceous processes, which are, in fact, nothing but the casts of the ambulacral pores; and the length of these processes is consequently just that of the thickness of the original shell. These processes upon the true casts, correspond to the pits upon the surface of the false casts.

Now, the presence of the siliceous processes, marking the course of the *ambulacra*, is a circumstance that we should naturally expect, the anomaly consists in this condition being so generally reversed,—a *hollow* taking the place of a *projection*. Every collector of fossils has probably noticed the difference in the two conditions, but I am not aware that any attempt has yet been made to explain how the difference originates. The secret of the matter is simply this:—In the case of the true casts, the silex has not only *filled* the cavity of the shell, but it has also completely *enveloped* the shell externally, the *Echinus* forming as it were, the nucleus of the flint nodule. In the false casts the siliceous matter has filled the cavity of the shell, but not surrounded it with a mass of the same substance. Now in both these instances, the cavity of the *Echinus* is entirely filled with silex, and the difficulty therefore is still unaccounted for; but if we take an *Echinus* filled with, but not surrounded by, flinty matter, and by an artificial process carefully remove the shell, it will be found, that a portion of the contained silex (forming the cast) is in a state of disintegration. Now it seems, that this process of disintegration always commences on those portions of the casts which are in the immediate neighbourhood of the natural openings of the shell, and that it goes forward to a greater or less extent, without the degree being regulated by any apparent law[1]; but where the *Echinus* forms the nucleus of a mass of flint, the disintegrating process takes place (if at all) on the surface of the entire stone, and does not reach the flint within the *Echinus;* consequently, the casts formed in this way, present us with an

[1] I have used the term "disintegrated" silex, in the absence of a more appropriate designation. That this condition of the silex is an *altered* one and not the state in which it was originally deposited, I have in my possession tolerably conclusive evidence.

exact mould of the cavity of the shell. The *Echinites* simply filled with flint, after being torn from their bed of chalk by the operation of diluvial currents, are worn away by bouldering, and the internal cast becomes exposed ;—the disintegrated silex, (no longer protected by the shelly covering), then separates from the rest of the mass, leaving the hollows or deficiencies that I have described. I do not pretend to explain how the disintegration of the flint originates, or to define the change in its mineral condition, as that enquiry forms a distinct subject of investigation.

Partial silicification of the Shell itself.—The above remarks have reference solely to the flint which fills the cavity of the shell; but the shell itself is frequently more or less silicified, in this respect following a general law which appears to me altogether inexplicable, namely, the shells which have undergone the greatest amount of silicification, are those which contain false casts; it is comparatively rare for the shells containing true casts, to exhibit any traces of this process.

The silicifying process appears to commence at a short distance from the natural openings of the shell, and a beautifully defined siliceous ring having been deposited around each of the ambulacral perforations, and also around the mouth and vent, the silicification frequently does not extend farther. At other times the silex proceeds to invest the secreting membrane of the shell, where this membrane dips between the sutures of the separate plates; and instances sometimes occur where one third or more of the calcareous matter is replaced by silex.

Cavity of the Shell not always perfectly filled.—It sometimes happens that the silex does not fill the entire cavity of the *Echinus;* but I believe that when this is the case, it invariably lines the whole internal surface of the shell, leaving a central hollow, the interior of which presents a chalcedonic or crystallized surface. The nature of this surface therefore readily shows, whether the deficient part of the mould arises from the cavity never having been filled, or from the subsequent process of disintegration. The occasional imperfect filling of the cavity of the *Echini,* is a condition common to both the true and the false casts.

I have never yet met with an *Echinus* enveloped externally with flint, having chalk in its interior, a circumstance sufficiently curious to deserve notice.

Distinct Crystals of Calcareous Spar on the internal surface of the Shell.—It is by no means a circumstance of rare occurrence to find crystals of calcareous spar lining a portion, or even the whole of the internal surface of the chalk *Echini*. These crystals are of a pyramidal figure, and each plate of the shell of the *Echinus* is occupied by the base of a single crystal, the size of the crystals being regulated by the size of the plates, an arrangement depending, I suppose, upon some law of crystallization with which I am not familiar. Now it would seem that these crystals existed before the introduction of the flint into the cavities of the *Echinites*, as I have had specimens in my possession in which the crystals are present, while the rest of the hollow is occupied by silex.[1] A cast formed under these circumstances has a most remarkable appearance; for instead of its giving you a mould of the cavity of the shell, it is a mould of the cavity formed by the crystals which line the shell; and what renders the appearance still more puzzling is this, that wherever the silex is in immediate contact with the crystals, the silex itself assumes a semi-crystalline structure.

If the various points connected with this subject were thoroughly investigated, I think it probable that the result of the enquiry might develope some important suggestions with reference to some of the conditions under which the chalk-flints were deposited or aggregated. Considered zoologically, the enquiry is certainly not without interest; for our knowledge of some extinct organisms is drawn from natural moulds; and as I have shown that these moulds are sometimes modified by subsequent causes, all facts tending to elucidate the nature and possible extent of those modifications, must be looked upon as a means of guarding us against inferences of a fallacious nature.

THE

MAGAZINE OF NATURAL HISTORY.

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A Paper has been published in the last number of Guy's Hospital Medical Reports,[2] detailing some facts connected with the development of

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[1] The most characteristic specimen of this kind that I have ever examined, is in the rich collection of Mr. Fitch, of Norwich.

[2] Guy's Hospital Reports, No. 9, October, 1839: edited by G. H. Barlow, M.A., &c., and J. P. Babington, M.A., &c.

the bird *in ovo*, which are well worthy the attention of those of our readers who may pursue the subject of Animal Physiology. It appears that the anatomical modeller to the Hospital,—Mr. Joseph Town,—whose high talent in his profession has so largely contributed to the celebrity of the Museum in that establishment, received instructions from the treasurer,—Mr. Harrison,—to prepare a series of models illustrative of the changes which occur in the egg during the period of incubation. Before however commencing his task, he consulted the works of Sir Everard Home and other writers, that he might render himself familiar with the opinions of previous observers, and see how far his own observations would tally with the views entertained in reference to this subject, by physiologists of the present day. The result was, that in the very outset of his undertaking, his attention was drawn to a circumstance which seemed to oppose the generally-received theory of the decarbonization of the blood in the embryo, by its contact with the oxygen of the atmosphere; and this induced him to institute a series of experiments, which convey the startling announcement, that the natural development of the contained embryo goes forward and is perfected without the transmission of atmospheric air to arterialize the venous circulation.

The lining membrane of the shell, through which the air was supposed to pass before reaching the chorion, was remarked by Mr. Town to increase in density, and become apparently less permeable to air, in a ratio corresponding to the extent of time during which the process of incubation had gone forward, a condition directly opposed to that which might have been expected to obtain, assuming the correctness of the above-mentioned theory of decarbonization. It then occurred to Mr. Town, that in the cases in which it had been found that incubation did not go forward when a barrier was put to the supposed egress of atmospheric air, that a source of fallacy might have been present, in the employment of a substance to protect the shell, which, from its noxious qualities had been fatal to the existence of the contained embryo. Having determined to satisfy himself on this point, Mr. Town informs us that he repeated the experiment in question in the following manner.—

"Having selected a number of fresh eggs, as nearly as I could of the same size and form, I varnished them over, many times, with albumen, which had been allowed to stand for some time in an open vessel, until, by evaporation, it had acquired considerable consistence: this was repeated until the shells appeared completely lackered. I then, with a pencil, marked one of them into equal sections, like the divisions in an orange, and cut a piece of card to correspond exactly with one of these divisions; then a great number of papers similar to this card; and macerated them for two days in albumen, until they were thoroughly saturated, and so soft and pulpy that I could readily apply them to the egg, and bring the edges so well together, that the joining was scarcely observable. After having covered the eggs in this way, and allowed them to dry, I repeated the papering and varnishing four times; taking care to bring the middle of each section opposite the joining in the previous coating. They were now covered with four thicknesses of paper, saturated as described; besides very numerous coatings of albumen, used as a varnish, first on the shell, and subsequently between each layer of paper; the whole forming a covering so thick and horny, that I felt convinced it was entirely impermeable."

The eggs thus protected were submitted to incubation on the 11th of April, and such of them as were examined during different periods of the process exhibited the development of the embryo without any deviation from its normal condition, the chick arriving at maturity in the same time as when placed under ordinary circumstances. To render the experiment doubly sure, it was afterwards repeated, and attended with a similar result, though in addition to the covering already described, there were added several coatings of oil-paint, purposely prepared with the most noxious materials, as a test of the air-proof nature of the protection used in the first instance.

Another observation of importance recorded by Mr. Town, is that upon his removing a large portion of the shell without injury to the chorion, while circulation was going forward, no visible effect was produced on the blood by the admission of atmospheric air.

" The blood still continued to leave the chick of a livid-red or venous colour, pass to the chorion, and, after having circulated through that membrane, was returned to the chick of a bright scarlet; and this difference remained perfectly apparent so long as the circulation continued; and then, but not until then, the atmosphere appeared to act upon the blood; and both arteries and veins became alike bright scarlet, as if this change were effected by circulating through the chorion, and depended on some principle of vitality."

It appears to us that Mr. Town's first experiment might be advantageously varied by placing the egg in a condition under which the possible access of atmospheric air would be even still more effectually guarded against. It might, for instance, protected in the way he has described,—be immersed in mercury,—and then subjected, in an hermetically sealed vessel, to the temperature under which artificial incubation is known to be produced. The development of the embryo under these circumstances, even though the experiments already performed may be thought to have proved enough, would still be a result sufficiently interesting to repay the trouble of the attempt.[1]

Mr. Town's paper is illustrated by a number of coloured and beautifully executed lithographic drawings, and his observations contain several other matters of great interest, but into the details of which we do not enter; our object having been rather to point out the channel through which he has made public his experiments and deductions, than to discuss the physiological considerations they involve.

The first step has just been taken to establish a Society for the promotion of Natural History by means of microscopical observations, and a meeting, with that object in view, was held a few days back at the Horticultural Society's rooms, in Regent Street. Our own feeling is strongly opposed to the multiplication of scientific bodies, upon the principle that *one* association of the kind, well supported, can do more to promote the interests of science, than can be effected by the exertions of half a dozen, when each is restricted to a particular department of research, and, from that very restriction, probably cramped in its available resources for prosecuting the contemplated purpose of its formation. In the present instance, however, the proposed institution cannot justly be regarded as any off-shoot from a parent stock. The nature of the enquiries it contemplates pursuing, is as independent and distinct as the field which lies before it is boundless; and if only a reasonable share of support be proffered it by the cultivators of science, its establishment must eventually give rise to the happiest results.

The new year opens with a rich promise of additions to our scientific literature. Messrs. Whitehead and Co. announce for publication an illustrated work upon the history of the entire class *Mammalia*, in which all the known species will be described and figured. Having some knowledge of the great capital embarked in this undertaking, and entertaining a high opinion of the zoological acquirements of the author—Mr. Martin, we anticipate in this work a contribution to Natural History of no ordinary importance. Mr. Bowerbank is ready with the first part of his history of the Sheppey fossil fruits,—a work which will put the scientific world in possession of the contents of his unique collection, and the result of many years most diligent research into the history of this little-known class of organic remains. A wide and comparatively untrod-

[1] It would of course be necessary that the egg should have a column of mercury above it, equal to the ordinary weight of the atmosphere.

den field of philosophical investigation has lately been engaging the attention of Professor Owen, the microscopic structure of the teeth throughout the *Vertebrata* generally, but more particularly as developed in some of the extinct genera among the fishes and *Reptilia*. Mr. Baillière announces for publication in the month of February, some portion of Mr. Owen's observations.

British Natural History, in the hands of Mr. Van Voorst, seems to flourish amazingly. Mr. Newman on the British Ferns, and Mr. Bell on the British *Crustacea*, are both about to issue from No. 1, Paternoster Row, and likewise another work which we announce with no small share of gratification;—a History of our indigenous species in the families *Asteriadæ* and *Echinidæ*, by Mr. Forbes. Mr. Lowe commits a work, in active preparation, on the Fishes of Madeira, to the same able superintendance; and a volume, styled "The Canadian Naturalist" makes its appearance from the same quarter. Our own publishers, Messrs. Longman and Co., promise an illustrated history of the various breeds of our domesticated animals, by the celebrated agricultural professor, Mr. Low, and a revised edition of Turton's Land and Fresh-water Shells, by Mr. Gray, of the British Museum.

2

SHORT COMMUNICATIONS.

New Species of Siphonia *from the Yorkshire Chalk.*—In the Mag. Nat. Hist. for 1839 (page 10), it was observed that the numerous specimens of *Spongiæ* and *Siphoniæ* from the chalk in the neighbourhood of Bridlington, exhibit such varieties of form, that it is difficult, and in some cases almost impossible, to distinguish the species.— The *Siphonia* of which the annexed figure (fig. 2) is a reduced sketch, is, however, marked by such distinctive characters, that I have ventured to consider it as a new species, under the name of *Siphonia fusiformis*. At the time the account above referred to was written, I was not aware of the existence of this species; the specimen from which the drawing was taken had been received some time since from Mr. Wilson, the lapidary, of Bridlington Quay, and had inadvertently been laid aside amongst a number of duplicates.

SIPHONIA *fusiformis.*—Irregularly fusiform, very much elongated, the summit composed of a cluster of naked tubes.

This species may at once be distinguished by the form of the crown, and by its great length, which is equal to about ten times its greatest breadth. The stem, near the root, contains as usual a single canal, which, at the distance of four inches from the bottom, is divided into four or five; the number is encreased on approaching the summit, which is pierced by about twelve apertures (fig. 3). These canals are neither

so crowded as those of *Siphonia clava*, nor so large and distant from each other as those of *Siph. anguilla*. The appearance of the crown is that of a cluster of thick, solid tubes, but this character is lost at a short distance from the extreme point. The annexed sketch, which is drawn of the natural size, will give an idea of the general appearance of the summit. The length of the whole specimen is more than fourteen inches.—*John Edward Lee.—Hull, Sept.* 18, 1839.

Little Bustard Shot in Devonshire.—On Friday, the 15th of November, a specimen of that very rare bird, the little bustard, (*Tetrax campestris*), was killed at Bigbury, in the south of Devon, which came into my possession the next day; this is I believe the second occurrence of this bird in that county, and it is rather singular, that in the other instance the bird was bought in Plymouth market in 1804, by my brother, Wm. Prideaux, and presented to the late Col. Montagu, and is now in the British Museum; it was killed in the north of Devon.—*Charles Prideaux.—Hatch Arundel, near Kingsbridge, Devon.—Nov. 22nd*, 1839.

Habits of the different Species of Sterna and Larus. The sandy island of Mareat is quite covered with salt plants, between which thousands of sea-birds had built their nests, in different groups, according to the different species. I remarked five species that had collected there for the purpose of hatching their young: *Sterna affinis, St. nigra, St. tenuirostris, Larus leucophthalmus*, and *Lar. flavipes.* Each species had occupied a division by itself, in which the several nests were hardly a foot distant from each other. In each nest of four of the groups there was only one egg, in a much-advanced stage of incubation; in the nests of *Sterna nigra* only were there two eggs. The sailors collected a great number of eggs, every one of which they were obliged to throw away. It was heart-rending to hear the cries of the disturbed birds, which were so eager to hatch that those which had lost their own egg, occupied the first nest they could find with an egg in it.—*Rüppell: Travels in Abyssinia.*

Society for Microscopical Investigation.—A meeting was held at the house of E. J. Quekett, Esq., Wellclose Square, Sept. 3rd., 1839, to take into consideration the propriety of forming a society for the promotion of microscopical investigation, and for the introduction and improvement of the microscope, as a scientific instrument;—The following gentlemen were present:—Rev. J. T. Bean, Rev. J. B. Reade, Dr. F. Farre, Messrs. Francis, Greening, Jackson, Lister, G. Loddiges, C. Loddiges, E. J. Quekett, Rippingham, Ross, R. H. Solly, C. Varly, N. B. Ward, and A. White. It was "Resolved, that such a society should be formed; that a provisional committee be appointed to carry the resolution into effect; and that the said committee do consist of the undermentioned gentlemen;—Messrs. Bowerbank, Lister, Loddiges, Quekett, Reade, Solly, and Ward." The provisional committee, in accordance with the above resolution, having prepared an outline of a constitution for the society, a meeting was held at the Horticultural Society's rooms, Regent-street, on Friday evening, the 20th instant. The meeting was numerously attended. Professor Owen, F.R.S., &c., took the chair, and was elected president; after which, the treasurer, N. B. Ward, Esq., the secretary, Dr. A. Farre, and the council were appointed. The constitution prepared by the provisional committee was unanimously adopted by the meeting, and the president announced, that the future meetings of the society would be held in the Horticultural Society's rooms. The society will be designated the "Microscopical Society." Its objects are, to promote improvements in the optical and mechanical construction of microscopes; the reading and discussion of papers, upon new and interesting subjects of microscopical enquiry; the formation of a collection of rare and valuable microscopical objects; and of a library of reference. At the close of the business of the evening, upwards of fifty gentlemen joined the society. The terms upon which members are admitted, are one guinea entrance, and a yearly subscription to the same amount.

THE MAGAZINE

OF

NATURAL HISTORY.

FEBRUARY, 1840.

Art. I.—*View of the Fauna of Brazil, anterior to the last Geological Revolution.* By Dr. Lund.

(Continued from Page 8).

The greater proportion of these caves have their entrances so disposed, that rain-water penetrates into them, either in the shape of casual or periodical floods, or else in that of constant streams. This water has often no other outlet than the fissures in the floor of the cave; but it not unfrequently pursues its course quite through, and escapes by another aperture. The rain-water necessarily brings with it soil and organic remains, which it is highly necessary to distinguish from the older deposits of the same kind; and fortunately this is no very difficult point. The dry bed of former floods is often so distinguishable that it cannot easily escape notice. A channel, often so deep in the soil as to expose the rock, and strewed with sand and boulders, admits of no doubt; and in it besides are often found shells of recent *Mollusca*, branches, roots, and leaves of trees, &c.

When this proof is wanting, the condition and contents of the soil itself will serve to characterize it. Should it be loose and light, of a grey or black colour;—or if it contain the slightest trace of vegetable remains, uncarbonized;—then there can be no doubt of its recent origin, even in the rare contingency of our not being able to discover the passage by which it has entered, which is usually easy enough to perceive. Where these distinguishing marks are all absent, the determination becomes more difficult. In some caverns I have seen alluvial deposits in which no trace of vegetable remains could be detected, and which notwithstanding, if they have not been introduced at late periods by water, have at least been exposed to its influence. The characters most to be depended on in the latter case, are a very pale dull colour, approaching to ashen-grey; a greater intermixture of sand than usual on the surface, with a diminution in the quantity downwards; the presence of rolled stones lying loose on the surface; the absence of saltpetre, and of the stalagmitic coating:—these signs are sufficient to prove that rain-water has gained admission into the caverns, and operated on the soil therein, though it may not have originally deposited the latter. Caverns of this description demand the most careful examination; for not only might recent bones be covered with soil deposited by the water in its passage, but also, really ancient fossils, which had previously lain in the soil, might be washed out by the same agency, and deposited in places where their origin and age might easily be mistaken. I have seen examples of both these cases; but I must confess that the great majority of caverns here, present no such difficulties, but in general have a single opening in the perpendicular, naked face of a rock, high above the surrounding soil, and most frequently protected by a projecting roof of limestone. A layer of reddish stalagmite is spread, like a carpet, over the soil of the cavern, and serves to mark the boundary between the past and the present. None of nature's devastating forces have here had place; all lies undisturbed, and in the same condition as when deposited by that mighty catastrophe which closed the curtain over a former world and its inhabitants.—Such is the theatre to which I wish to introduce the reader; for what this mantle covers—what this soil contains, belongs without exception to that extinct world.

The nature and condition of the fossils themselves often afford still better means of determining their age. In by far the greater number of instances they present the following appearances. The bones are entire and uninjured, with their smallest processes and their finest points and edges well preserved. Their exterior is of a beautiful reddish ochre-yellow, and their fractured interior of the purest white. They are much lighter than recent bones, and so extremely brittle as to crumble to pieces if carelessly handled: they adhere closely to the tongue: if exposed to the action of fire, they turn black, and give out, although in a slight degree, a burnt and fetid odour. A portion of the soil in which they have lain always adheres to these bones, either in the form of a fine dust or coating, or as filling up their cavities. When the enveloping soil has been saturated with lime-water, it adheres so closely to the bone that it is impossible to separate the two. More rarely the bones, without losing their uninjured surface, or the pure white colour and osseous structure which they display when broken, have their cells lined, or sometimes quite filled, with a stony substance, and their weight is consequently so much increased that they appear to be of the same specific gravity as limestone. There is yet a third condition, in which these bones depart still farther from their original character than in the two above named; that is, where the organic structure has entirely disappeared, and calcareous spar is substituted for the osseous substance. This alteration, I have satisfied myself, is owing to the bones having lain long under water.

It will next be my object to point out the mechanical changes which these fossils have undergone, and which may be treated of under three classes. First; splits and fractures in all directions, but for the most part longitudinal, and not unfrequently accompanied by a more or less evident compression of the bone. In these cases the interior surface of the medullary cavity and of the cellular structure, as well as the sides of the fracture, are of the same reddish yellow colour as the exterior: and if the soil be impregnated with calcareous particles, then are these internal surfaces overlaid with a thin coating of very fine crystals of calcareous spar; but they are never filled with earth. Besides, the outer surface being perfectly uninjured, it is clear that these bones have been buried in the soil in a more or less fresh condition, and that it was only from their increasing brittleness, that in the course of time they have begun to yield to the continual superincumbent pressure. To this class also belong those injuries of which I have spoken in describing the cave of Maquiné. The bones, in that case, were not only split in all directions, but often quite crushed; yet in such a manner that the fragments lay by the side of each other in their natural position. In the same paper I have shown how this fact, as well as some others met with in that cavern, can only be explained by the supposition of effects produced on the animals, when still clothed with flesh and skin, by vast masses of rock.

The second class of mechanical changes which these bones have undergone, has been effected by the teeth of predatory animals. And again, these changes depend partly on the resistance the bones were calculated to offer,—partly on the character of the animal that attacked them. Such beasts of prey as derive a considerable quantity of nutriment from the bones themselves, and for that purpose are provided with crushing teeth, like the huge hyænas of the Old World, the remains of which are found in European caves, were wanting in this part of the globe, and were represented by others, which have left the records of their existence imprinted in a much less destructive form on the relics of their prey. Of this class I need only remark, that the fragments are scattered promiscuously together, and are enveloped in, and filled with, earth. Moreover, they are often gnawed by the teeth of small animals.

The third kind of mechanical change which these bones exhibit, is an abrasion of their points and angles, which, in a former communication, I conceive that I have proved to be attributable to the action of the water which formerly had access to the caves wherein they are found.

In all the cases of which I have hitherto spoken, the bones have been protected from the action of the atmosphere, either by their stalagmitic covering, or by lying in water. But many have not been so favourably circumstanced. Many have I seen which, from their peculiarly raised position in the middle of a basin in the floor, have escaped being buried in the soil; and these present the most remarkable examples of the destructive power of time. Their most exposed parts are mouldered away, and changed into a yellowish dust, which, by covering the inferior portions, has protected these from decomposition. In those caverns where water has had access in later periods, these fossil bones have occasionally been torn out of their beds, and by the joint operation of water and air, their decomposition has been so accelerated that they resemble in appearance half-decomposed recent bones. This has come under my notice at least once; and had it not been for the size of the bones, which indicated a much larger animal than any mammal now living in this quarter of the globe, I confess I might have mistaken its age. Such, however, if I may judge from my own experience, is seldom the case; at any rate I can assure the Society that the above is the only instance open to any doubt, among the fossils forming the subject of this communication; all the others were found under circumstances that left not the slightest question as to their origin.

So much for the circumstances under which these fossils are found and the changes they have undergone. But before I proceed to a more accurate description of the animals to which these remains belong, it will perhaps be advisable to preface it with a few remarks on those existing species which frequented or have left their traces in the spots that have afforded us so astonishing a catalogue of the creatures of a former world.

The first place on this list is due to the family of the bats. There are few caves which do not harbour at least some individuals of this family: but to one who has not been an eye-witness of the fact, the multitudes in which they are found in certain caverns, must seem perfectly incredible. There are caverns, commonly called "Lappas dos Morcegos," that is, bat-caverns, which are rendered almost impassable by these creatures. Their liquid excrement covers considerable surfaces of the walls and floor, rendering the latter so slippery that it is hardly safe to cross it where at all inclined; and besides, the strong ammoniacal odour exhaled is enough to stifle the intruder whom science has lured into these murky labyrinths. I have seen considerable spaces of the roof so thickly covered with bats, that they appeared matted together; and when disturbed, the universal flutter agitates the air so much as to extinguish the lights. Nor is it only in the living state that they are found, but dead or dying they are also seen hanging from the roof by their hind legs; while their remains are strewed over the floor in every stage of decomposition.

In these caves I have discovered some species of the genera *Phyllostoma*, *Molossus*, *Glossophaga*, *Vespertilio*, and others; but the most abundant by far is a new genus, which, from its peculiar dental system, is not only far removed from the other genera of this family, but even stands alone in the order *Mammalia*. Of this very remarkable creature I hope soon to forward a description and drawings.

As the bats hold the first place among the living inhabitants of these caves, so is it the family of rodents which have left the greatest number of recent bones there. And as the bones of the *Rodentia* form no inconsiderable portion of the fossil remains;—and the species of this family at present existing in these parts being besides very imperfectly known;—it will be advisable to offer a short sketch of them: premising that in their enumeration, as well as in that of all others that may follow, I strictly confine myself to those which, either from my own observation, or from the reports of trustworthy witnesses, I know to be inhabitants of the district in which these caverns are situated.[1]

[1] Brazil embraces, within its extensive boundaries, great varieties of climate and other physical conditions. In the southern provinces, most of the tropical forms, both of animals and vegetables, disappear, and are replaced by new ones. There is scarcely less difference observable in comparing the interior highlands with the narrow tract which extends along the coast, and is bounded by a high, wooded, wall of rock. For this reason I have not been able to confine myself to mere political divisions. And besides, as it is an indubitable truth that the extinct animals have in general lived in the spots where their remains are now found, a geographical comparison of existing and extinct animals must necessarily be confined to the district where the latter occur.

I naturally place at the head of the rodents the largest yet known—the *Kapivar*. It is spread over all the warm parts of eastern South America, and its amphibious habits partially protect it from the fate to which it is doomed in consequence of its depredations on the corn-fields. There is but one known species of this genus,—the *Hydrochœrus Capibara*.

The next in size and abundance is the Paca,—*Cœlogenys Paca*. This is much prized for its flesh, and its numbers have in consequence sensibly decreased in many places. Its colour varies through all shades, from a light yellow-brown to a black-brown. I confess that I cannot, from my own experience, affirm a distinction between *Cœl. fulvus* and *Cœl. fuscus;* and the Brazilians, who have a remarkably good eye for permanent marks of distinction, and who often perceive specific differences where a naturalist can see only varieties, are unanimous in recognizing but one species of Paca. The same is the case with the Gutia (*Dasyprocta Aguti*), and the Pereá (*Cavia Aperea*),—the only species of their respective genera in those parts of Brazil which I have visited. Of the genus *Lepus* there is but one species (*Lep. Tapeti*), of an intermediate character between the rabbit and the hare, though in its habits it agrees best with the latter. There is likewise but a single species of squirrel (*Sciurus æstuans*): and finally the list closes with an animal (*Sphiggurus spinosa*, F. Cuv.), which, from its sluggishness and awkwardness, would ere this have been nearly extinct, had it not been provided by nature with a formidable cuirass, by which it is protected from all its enemies.

Such is the catalogue of the rodents hitherto known and described as inhabiting this district; but the number which has escaped the observation of naturalists is still greater, and as they fill a distinguished part in the history of the inhabitants of these caverns, it is important that the reader should be made acquainted with them.

I begin with the genus *Echimys*, or spiny rat, of which there are four species in these parts, all different from those of Paraguay and Guiana. The largest of them is about the size of the Pereá, while the others are not larger than our common house-rat. They are nocturnal animals, passing the day in subterranean holes in the woods, and feeding not only on frogs, but on insects, the wings and *elytra* of which are seen strewn outside their holes. They present several peculiarities in their internal structure; but on the whole approach nearest to the porcupine (*Hystrix*), which they serve to connect, in many points, with the cavies, and especially with the genus *Dasyprocta;* while in occasional features they remind us of the true rats. The commonest species, of which we shall have occasion to speak more at large hereafter, is distinguished from the others by a groove or furrow on the front of the incisors, for which reason I propose to name it *Echimys sulcidens*.

Of the genus *Mus* I am acquainted with five species, which all appear different to those described by Azara and Rengger from Paraguay. Two of these infest houses, the three others keep out in fields and woods. One of the former appears to me identical with our own house-mouse (*Mus musculus*), but the other is quite different from our two kinds of house-rat, *Mus decumanus* and *Mus rattus*. I call it for the present, *Mus setosus*, on account of the long black bristles which are scattered here and there over its skin. This species was introduced into these parts about the commencement of the present century; and it has driven out of the houses a smaller species, of a pretty chocolate-brown on the back, chesnut-brown on the sides, and white on the belly, with a fine short fur, and a short silky tail: the latter species has become rare, and is now only seen about cultivated fields.—A fourth and still smaller species frequents gardens: it is of the same size as our house-mouse, has a large head, with large hairy ears, and a very short tail. I have named it *Mus lasiotis*. But the most remarkable species of this genus lives only in the woods. I have hitherto been unable to procure a single specimen in fresh condition; but I have found it abundant in the stomachs of the larger diurnal and nocturnal birds of prey. Its tail is provided with stiff bristles; on which account I have given it the temporary name of *Mus lasiurus*.

After this cursory sketch of the rodents inhabiting this district,[1] I shall proceed to consider them with reference to the remains which they have left in the caves, in order to explain thereby some points connected with the history of the fossil bones in the same places.

I commence with the common Brazilian wood-rat, *Mus lasiurus*. I know of very few caves in which remains of this animal are not found; and in many they occur in such prodigious quantities, as to excite the utmost astonishment. To convey some idea of this, I will give a brief description of the first cavern of this kind that I had an opportunity of examining near Caxoero do Campo. This cavern is one hundred and twenty feet long, from six to nine feet wide, and from thirty

[1] The Moco (*Kerodon rupestre*) is not found within the district in which the caves are situated, although the southern boundary of its habitat is but a few degrees from that locality.

to forty feet in height. Its floor, for a distance of twenty feet from the entrance, was covered with a bed of earth, perfectly identical with the soil outside, and which had evidently been washed in by rain water. Farther in, this bed of earth disappeared, and was replaced by a layer of very loose brownish or black mould, about a foot thick, and completely full of small bones, more abundant in some places than in others.—I filled a box, containing about half a cubic foot, with this mould; and on my return home counted in it about 2,000 separate *rami* of the under jaw of *Mus lasiurus*, and about 400 of *Didelphis murinus*, besides a small number of the jaws of other animals, of which I shall presently speak more particularly. These bones were for the most part broken: only the smaller, such as those of the feet, the *vertebræ*, and the strongest long bones, being entire. All the skulls, without exception, were fractured, so that a portion of each, particularly the *ossa interparietalia*, was usually wanting: the weaker ascending *ramus* of the under jaw was also generally absent. The bones were in different states of preservation, according to the position occupied by them in the bed of earth; those lying deepest being brown, brittle, and adhesive to the tongue, properties which diminished upwards, so that the uppermost of all were very fresh. Upon the surface of the earth lay scattered the *elytra* and legs of beetles.

The extraordinary collection of bones at this place, and their fractured condition, led me involuntarily to the conclusion, that they must have been introduced by some predatory animal; and subsequent examination has taught me to recognize this animal in the common Brazilian owl, *Strix perlata*. This owl is met with in abundance in the caverns, where also I have had frequent opportunities of examining its nest, under which I have invariably found a heap of ball-shaped bodies, from an inch and a half to two inches in diameter.—These balls consist of tangled hair, intermixed with the above-mentioned bones; and are the well-known balls cast up by all predatory birds after digesting the flesh of their prey.—They fall asunder in the course of time, when the less durable portions, such as the hair &c., moulder away, and form the loose soil before spoken of, in which the bones lie scattered about. I have had frequent opportunities of tracing the formation of these heaps from their first commencement: but to remove all possibility of doubt, I kept several species of owls in my own house, and supplied them with small mammals and birds; and under my own eye they cast up the very same sort of balls filled with bones, which exhibited exactly the same marks of injury as those in the caves.

With regard to the species to which the bones in the balls have belonged, I have found the following kinds, and in about the following proportions. Out of 1000 individuals, *Mus lasiurus* constitutes 800; *Didelphis murinus*, 100; *Echimys sulcidens*, 50; small birds, 20; bats, 10; a smaller species of *Mus*, which I consider to be *Mus lasiotis*, 10; and the remaining 10 are composed of the three other species of *Echimys*, together with some young individuals of rabbit and Pereá in about equal proportions. Unconnected with this heap of bones are frequently found the remains of the two larger species of this family,—the Paca and the Gutiá. The numerous foot-prints of the first of these animals, which may be observed in almost all the caves, prove that it is a constant visitor at least; and indeed, in some caverns, where their path lay over narrow passes, I have seen the limestone quite polished by their feet.

Of the three other species of this family, the Kapivar, the *Sphiggurus* and the squirrel, I have as yet discovered neither traces nor remains.

(To be continued).

ART. II.—*On the Genus* Argonauta. By M. RANG.

(Continued from page 16).

LET us now turn to the consideration of a fact of more importance, and which, beyond contradiction, furnishes one of the strongest arguments apparently at least in favour of parasitism.

M. de Blainville very ingeniously makes use of our discovery to corroborate the opinion that he advocates; and it is with that clearness which runs throughout his demonstrations, that he here developes his views, which are undoubtedly very likely to carry us along with him, but which, nevertheless, rest upon an observation, respecting which we are somewhat at variance, so that we do not find in this new argument all the force which at the first glance it appears to possess. The Professor, admitting our assertion that the poulp of the argonaut crawls with its tube above, that is to say, according to his idea, with the ventral part uppermost, remarks that in this respect the poulp completely differs from the ordinary *Octopi* which he has observed upon the shores of Provence, and particularly from the *Oct. moschatus.* These *Octopi*, he says, crawl by dragging themselves along the ground, but always with the tube below, and the dorsal region above; and he consequently infers that our argonautic *Octopus* is in an anomalous position, while the *Octopi*, properly so called, are in a normal one: from which M. de Blainville deduces a new proof of the parasitism of the animal.

Without venturing to discuss the validity of this argument, we will merely observe that if we do not admit it, it is in a great measure because we are not agreed as to its elements.

We have often seen *Octopi* out of the water, in the act of progression; and a drawing made on the coasts of Provence by M. de Blainville himself, and which he very kindly showed us, appeared in some measure to confirm what we on our part had observed. The species we have seen is precisely the same as that which engaged the attention of this naturalist; we have found it in the same position as he did, but we are far from affirming that it assumes no other, for we have often observed the contrary.

The *Octopus moschatus* is undoubtedly, of all the species, the one which most readily accommodates itself to this experiment, not only because it is the most common in the nets of the Mediterranean fishermen, but also because, when out of the water, it exhibits surprising strength and agility. We have studied it in the road of Algiers, at the instant when the fishermen hoisted up their nets, almost always full, upon the deck of their boats. Escaping through the meshes, these animals would run about, endeavouring to regain the sea; and nothing in fact could be more curious than the motions used to attain that end. They did not crawl in the manner of gasteropods, but holding themselves bent double, so that only their head and the extremity of their sac rested upon the deck, they seemed to gallop at a great rate, if we may so express ourselves, enlarging their back or stomach according to their position; while their arms, which they carried before them, or by their sides, had an undulatory or serpentine motion, and, fixing themselves alternately by their suckers, assisted the *Octopus* to draw itself along, while raising itself on its extremities. What we inferred from this was, that when out of the water, these invertebrate animals move themselves as they can, by the power of the arms furnished with suckers, according to the position in which they find themselves placed, and according to the vitality remaining in them. This condition is really an accidental one for them, since by the nature of their organization, the power of living habitually out of the water has not been granted to them, and consequently they have not been provided with any particular organ for terrestrial progression. But their normal state is when they are in the water; it is there only that they enjoy the faculties for action which have been given them; and in the water we affirm that they never crawl or progress in the manner above described, any more than that they swim in turning upon themselves, as has been advanced.

We do not however altogether disallow the force of the argument advanced by M. de Blainville, and this is the way in which we understand it. If we suppose that a poulp, from the nature of its exigences, and from its peculiarly pelagian destiny, is compelled to have recourse to a shell in which to lodge itself, and pass the whole or a part of its life, we must certainly admit, in some of its organs, a particular pre-arrangement or modification. It would, for example, be necessary that nature should have provided it with organs specially destined to maintain its position within this foreign covering;—and such organs we find in the membraniferous arms of the poulp we are now considering. Thus, when we meet with an animal surrounded by these peculiarities,—a mollusc in this anomalous state,—we may be justified in considering it to be a parasite, but can we affirm with certainty that it is so? It is after all but a presumption; and to consider the parasitism demonstrated, would perhaps be hardly compatible with sound logic.

We cannot conclude this portion of our memoir without remarking that Ferussac, a short time before his death, as we have just learned, entertained the idea that the membraniferous arms of the poulp of the argonaut were disposed by the poulp upon the lateral faces of the shell; this, at least, is what we found expressed in a letter, written by him to M. Prêtre, requesting from him a new plate for his great work on the cryptodibranchiate cephalopods, and which letter that skilful artist very willingly communicated to us. The passage is as follows.—"In the second phial is a specimen in its shell (of which also a view must be taken), and having the large membrane very much spread out with great care over the shell, in the same position as its arm." Unfortunately, neither the animals nor the beautiful drawing made by M. Prêtre are to be found; and the sentence we have just quoted is all that we possess by Ferussac relating to a subject which it would have been very interesting to see treated of by him.

Locomotive Faculty of the Argonaut in deep water.—When in deep water the poulp of the argonaut swims in the same manner as other cryptodibranchial cephalopods,—by the rejection of the water introduced into the sac by means of a tube situated opposite to the anus. Such is the third observation we have made ; it evidently tends to restore this poulp to the normal state of the other cephalopods, from which it has been so strangely separated;—it destroys the fabulous notions of the navigation of the argonaut;—it explains why it is necessary that this poulp should have two palmated arms to retain its shell;—and finally, it overturns the argument drawn from the divergence of opinions as to the manner in which the poulp swims on the surface of the water, and on which was founded the statement that the parasitic inhabitant of the argonaut was not always a poulp with palmated arms, or else that it did not always place itself in the same relation to the shell.

Locomotive Faculty of the Poulp of the Argonaut at the bottom of the Sea.—The observation we have just made, and the description we gave at the commencement of this memoir of the manner in which the poulp of the argonaut crawls along the bottom of the sea, constitute a fact which is quite new, and which seems not to have been previously observed. It is nevertheless but just to state that it had already been pointed out; for Rumphius long ago said that this mollusc walked at the bottom of the sea by the aid of its arms, and with the keel of its shell uppermost. We then merely give a more detailed confirmation of his observation. It naturally follows from what we have said on this subject, that these poulps do not always carry themselves with their ventral part beneath, but frequently also with it above.

This observation weakens still more the opinion of those naturalists who suppose that the palmated arms are turned to the side of the anterior part of the shell, and of those who think that the mollusc places itself indifferently either one way or the other; and draw from thence an argument for its non-parasitism. And finally, it also restores the poulp to a more normal state than that which had been assigned to it.

Will not this peculiar mode of reptation at the bottom of the sea explain why the poulp in question, supposing it to be the real constructor of the shell, should preserve a space at the end, instead of filling the forsaken part with a solid deposit, like the *Magilus*, or forming partitions there, like the *Nautilus?* May it not be for the purpose of preserving a reservoir of air, in order to facilitate its rapid and vertical ascent to the surface of the water? Rumphius, who was a close observer, as we have just shown, seems to confirm this idea, when, in referring to this mollusc, he remarks that it also re-ascends in a reversed position; that is, with its head below and the keel of its shell above. In fact, is it not evidently for the purpose of retaining the air compressed by it into the bottom of the shell, that the poulp thus holds itself upside down during its ascension? If, on the contrary, it ascended with the keel downwards, this air could not fail to escape, and it would then be necessary for the animal to use its organs of "refoulement" to remedy this loss. This remark will perhaps appear strange to some persons; but it is certain that many of the *Mollusca* and *Acalepha* ascend on this principle: and we have many times seen them leave at the surface of the sea, the bubble of air which they had undoubtly obtained at the bottom by means of some peculiar faculty.

Examination of the arguments which have been presented in favour of one or the other opinion.—M. de Blainville, in his interesting letter, has advanced a series of arguments in favour of parasitism, to the greater part of which our preceding observations apply. But there are some still remaining, to which we have objections to oppose; such as, for example, his fourth argument, which is drawn from the absence of organic attachment between the shell and the animal, a circumstance tending to indicate that the two are foreign to each other.

We quite agree with those naturalists who recognise this want of attachment; in fact there is no other connection between the shell and the animal, than that of contact, and this argument has always been regarded as one of the most valid: we can, however, meet it by another, namely, that the true constructor of the shell, supposing it not to be the poulp, did not adhere to it either; since, contrary to what we observe in other shells, there are upon the argonaut no traces of attachment, or, in fact, of any muscular impression. This remark, moreover, is not our own; it was made to us by Cuvier ten years ago, in a conversation with him on this subject.

To this observation it has sometimes been objected, that the argonaut is evidently an internal shell. We confess that we should have some difficulty in picturing to ourselves an internal shell of this description,—so "enroulée", so symmetrical and diaphanous, and possessing ribs and tubercles, and, in short, so little analogous to all internal shells, whatever may be the order of *Mollusca* to which they belong.

An attempt has also been made to approximate this shell to the genus *Atlanta*, which attempt rests upon some recital of an inhabitant of the island of Amboina, of which however we have nothing but a completely anecdotal report. But in the genus *Atlanta* the animal is united to the shell by a very evident attachment, and upon the whole there exists no relation between the argonauts to the *Atlanta* on the one hand, or to the *Carinaria* on the other; for these two nucleobranchiate genera constantly possess a simple and central keel, which the argonauts do not. And again, as we have long since shown, the *Atlanta* and *Carinaria* are not all symmetrical in form, while, on the contrary, all the argonauts are so.

In order to prove that the argonaut is inhabited by another mollusc, besides the membraniferous-armed poulp, a beautiful specimen of this shell, in the possession of M. de Roissy has been quoted, in which, by means of an accidental fracture, a torn fragment fixed to the internal wall may distinctly be perceived. We have not seen this shell, but from what M. de Roissy himself has told us of it, we do not think that an argument of any great weight can be drawn from this circumstance. Are not parasites, such as the *Ascidiæ*, *Anatiferæ*, and *Actiniæ* for instance, often found (as we ourselves have seen) fixed to forsaken shells? And might they not leave fragments of their base attached? The argonaut of M. de Roissy perhaps offers an example of this kind; there is nothing to prove the contrary.

The fifth argument of M. de Blainville tends to demonstrate that the form of the animal has no true correspondence with that of the shell. We shall not return to this subject; for to establish this correspondence is exactly what we endeavoured to do towards the commencement of this memoir, and we find it still greater, now that we know the use of the membraniferous arms.

In his ninth argument M. de Blainville expresses himself thus.—"The animal may be drawn out of its shell apparently without feeling any inconvenience, and without suspending its movements; as Cranch has proved positively by experiment." Nothing is so embarrassing as to have to refute an argument, based upon what a person highly worthy of credit says he has seen. We have over and over again observed exactly the contrary. One may combat an opinion; but in conscience one cannot tell an observer who professes to have seen a thing,—"You have not seen it!" although one may feel certain that it never could have been so. We shall therefore omit the consideration of Cranch and his poulp, and merely recall what we said in detailing our own observations upon the one that was on the point of expiring, and which, weakened, and scarcely retaining any life, had contracted its membraniferous arms, and being no longer able to hold its shell, was accidentally separated from it. We made the same observation many years ago, upon the particular species spoken of by Cranch, but with less of detail, because we did not then know the use of the large arms; and also at a later period, at the Cape of Good Hope; and lastly, we have studied the facts we relate in this memoir, at Algiers. We declare that we have never seen the poulp voluntarily quit its shell; and that it was only when deprived by weakness of the power of adhering to it by means of the organs which nature had given it for this purpose, that it was separated from it by a fortuitous circumstance, and one that was quite independent of its will. And if, in this case, the poulp resumed an appearance of activity, it was only to expend all its remaining strength in one effort, and expire almost immediately. We shall say nothing further on this subject; for reasoning, in this case, can do no more, and subsequent experience only can show the weight of these arguments. On this ground we earnestly entreat those who may have opportunities of studying the poulp of the argonaut in its shell, to multiply their experiments on this fact as much as possible, and carefully to record all they witness.

If we have found ourselves under the necessity of combating many arguments in favour of parasitism, we have also had occasion to attack some of those put forward by the partisans of non-parasitism. We have already extinguished several of these, in opposing to them the use and position of the large arms; as, for example, we showed that these arms were not directed towards the interior of the shell, on each side of the keel, to form the tubercles.

We have also done away with the arguments founded on a pretended observation, that the animal, when drawn out of the shell, exhibits upon its mantle the entire form of that shell, and the impression of the furrows and tubercles with which it is ornamented. But there is a more important fact which ought to detain us a moment, since it has been for some time advanced with great success, and yet it must now fall to the ground. This will doubtless be the case with many other arguments—fruits of an active imagination—to which too much attention has hitherto been paid, but which perhaps only await a simple observation, conscientiously made upon the animal when full of life and at liberty, to be completely nullified. The nature of this fact we will now explain. The partisans of non-parasitism thought that the best method of solving the problem, was, to assure themselves whether the rudiments of the shell of the membraniferous-armed poulp were to be found in the *ova* of the animal. This investigation might be decisive. Many naturalists, relying solely upon it, soon exclaimed, "the question is decided, for the shell is there!" It was a truly eminent anatomist, whose reputation extends throughout Europe, who first uttered the cry of victory, which was immediately enregistered in a host of publications, all relating the marvels of this observation, and all bringing forward the determination of the skilful Italian, as beyond appeal. Nevertheless, many other naturalists of acknowledged talent, and we must quote M. de Blainville as among the first, would not allow themselves to be convinced by hearsay, but wished to verify the fact; but neither M. de Blainville, nor Sir Everard Home, nor M. Bauer, saw what had been announced. Still further, Madame Power, a disciple of the celebrated Poli, of whom we have already spoken in reference to her striking observations upon the poulp of the argonaut, and who is one of the most enlightened defenders of non-parasitism, now gives a formal contradiction to her master, declaring that there is no appearance of a shell in the *ova* of the poulp; and she then concocts a very ingenious little system, which can however do no prejudice to the cause of non-parasitism, and shows how the shell may be formed after the birth of the mollusc.

As for ourselves, we also have been very curious to verify Poli's observations. We have tried at different times, sometimes upon eggs which were preserved in alcohol, sometimes upon those we had taken quite fresh from the sea, and which had, without doubt, arrived at different degrees of maturity; and after all we never found anything but the nucleus which may be observed in eggs in general.

Thus the strongest argument brought forward by the partisans of non-parasitism, must evidently give way before these reiterated observations.

(*To be continued*).

ART. III.—*Notes on Irish Natural History, more especially Ferns.* By EDWARD NEWMAN, Esq., F.L.S., &c.

(*Continued from page* 23.)

KILKEE stands in a little semicircular bay, which, having a bar of rock across its mouth, affords no shelter for shipping; indeed, nothing can be more forbidding to the seaman than the west coast of Clare. From the Hag's Head or Mohir on the north, to Louphead on the south, it consists of black, precipitous, slate cliffs, against which the restless Atlantic frets itself into a perpetual foam. The whole line of coast, in extent about forty miles, is called Malbay; and throughout the entire distance there is not a single place of safety for shipping, and scarcely a creek to afford a doubtful and temporary shelter in extreme distress. The cliff is worn by the ocean into forms more wonderfully grotesque, and oft-times more strikingly picturesque, than the most vivid imagination could devise. Here a peninsula of rock stands boldly out to sea, and the isthmus which connects it with the land has for centuries lost its basement, and only exists as a bridge, hanging as it were by magic over the dark waters, which meet and strive, with thundering din, hundreds of feet below its aërial span;—there, an enormous cleft in the face of the cliff, riven as by an earthquake, forms a wedge-shaped chasm, into which the vast waves gambol one after another like huge leviathans, playing a thousand antics, and sending the "playful spray" aloft, to be borne on the wings of the wind. Then comes some vast cavern, with vaulted roof and gigantic columns, divided maybe into a hundred minor caves, in some of which a ship might float with every stick standing; and from these caves there issues such a reverberation of the roar of waters, that thunder might mutter its loudest and remain unheard.

I took the southern cliff from Kilkee, and skirted along its extreme margin as best I could. Where it was broken and uneven, and I could accomplish it with safety, I descended the cliff as far as practicable; I often found the crevices filled with *Asplenium marinum*, *Aster Tripolium*, *Silene maritima*, *Arenaria marina*, &c. &c. Nothing could exceed the audacity of the gulls while I was clambering about these cliffs: some of them came so near me that I could have touched them with a whip, and their screaming was fearful. There were also flying around me curlews, hawks, and choughs;—the curlews whistling, the hawks screeching, and the choughs chattering; but the gulls were the most noisy and numerous of all. On the top of the cliff is a short close herbage, of that kind which in England we call good sheep-walk; and on this, and the turf walls which separated it, were rooks, carrion crows, hooded crows, ravens, magpies, and choughs innumerable; the latter bird predominating in number over all the others of the crow family. Further along the coast I saw a settlement of sea-birds; puffins, guillemots, rasor-bills and corvorants, intermixed with the eternal sea-gulls: and I saw three large birds which I supposed to be the great northern diver; they sat up like penguins, on a rock that just peeped out of the water, and was now and then covered by the swell; this sometimes carried off one or two of the divers, but they almost instantly returned and resumed their station. The distance from the top of the cliff to the water will, I think, afford an excuse for my inability to name the species.

These cliffs afford perfect security for eagles and hawks, and I have little doubt that very large numbers are annually bred here, as well as gulls, corvorants, and other aquatic birds. With regard to the gulls, I could make out but little as to species; there were two of very different sizes, but nearly alike in colour, which I supposed to be the greater and lesser black-backed; and occasionally a little covey of four or five individuals of *Lestris* would make their appearance, but these always seemed passing on, as though bent on other business, while the whiter gulls appeared to have no other amusement than screaming round my head; I never was so insulted; they swept round and round in semicircles, fanning me with their wings every time they approached: I longed for a gun, just to have given them an admonisher. Proceeding farther along the cliff, I found a man and boy fishing with lines 400 feet in length. The hook was baited with the inside of a crab, and a stone was tied near the extremity of the line, and being thrown into the sea carried the line with it, which otherwise could not have been persuaded to make the descent. I waited some time, but to no purpose, in the hope of seeing a fish hauled up; and I was equally unsuccessful in learning what kind of fish were taken in this way, for as neither party understood a word that the other said, it might be called on my part, "the pursuit of knowledge under difficulties." Shortly afterwards I saw a single rock dove (*Columba Livia*) fly to the cliff, and apparently enter a hole; it was closely pursued by a kestrel, which continued sailing backwards and forwards along the cliff, until I left the spot. I ascended a hill to examine what appeared a most singular ruin: on reaching this I found it was only a Napoleon-tower, with a small portion rearing its head high over the shapeless mass of ruins which surrounded it. The view from this tower is magnificent; you can see nearly two hundred miles of coast, so ruggedly rocky, so curiously indented, and so intermixed with sea, that it requires a tolerable degree of map-knowledge to understand the objects you are beholding.

The high promontory on which this tower stands, terminates in Cape Lean, or, as it is usually termed, Loop-head. After enjoying the splendid panorama to my heart's content, I turned southward, and soon falling in with a road or track, returned to Kilkee. The ferns of this promontory are *Lastræa Filix-mas*, rare; *Las. dilatata*, abundant; *Athyrium Filix-fœmina*, abundant; *Pteris aquilina*, sparingly; *Osmunda regalis*, abundant; *Lomaria spicant*, not frequent; *Asplenium marinum*, abundant. The population is very great in the neighbourhood of Kilkee and Kilrush; the cabins are thickly sprinkled over the whole surface of the country, except along the high cliffs by the sea. There was a great deal of oats, wheat, rye, and potatoes, in small patches attached to the cabins; the wheat and rye in ear, the oats backward, and potatoes not looking vigorous, the ground being very conspicuous between the rows.

Having crossed the estuary of the Shannon, I landed at Tarbert, and proceeded by car to Listhowel over a flat, uninteresting, and unprofitable bog. Ballinruddery, the seat of the Knight of Kerry, is to the left of the road, and the fine wood of his demesne is the most pleasing object throughout the journey, and is again seen to great advantage from the bridge over the Feale, on leaving Listhowel for Tralee. The bog appears to present no impediment to cultivation, and why it is left in its present state is unaccountable.

Leaving Listhowel the country continues for the most part flat, and generally cultivated, but in a wretched manner. Where the bog remained in its native state, it seemed to be rather left for the sake of cutting turfs for burning, than from any impediment it offered to the cultivator. The face of the country abounds in ruins, some of them very fine; and a round tower of considerable height is visible to the right of the road for nearly two hours. The road passes over the shoulder of the Stack Hills, and then the view of the town and bay of Tralee, with the fine hills beyond, bursts on the traveller with great beauty. The principal ferns were *Lastræa dilatata*, *Osmunda regalis*, and near Tralee *Scolopendrium vulgare* and *Polystichum aculeatum;* and choughs and hooded crows the commonest birds.

Leaving Tralee I ascended the hill south of the town, and was delighted to find a lovely little plant which I had never before seen growing,—*Sibthorpia europæa.* On the hilly land in Ireland you find deep and narrow channels cut by torrents of rain water after heavy showers, but, except immediately after rain, perfectly dry. Spread over the banks, and pendant in graceful festoons from the grassy margins of these channels, I found the *Sibthorpia* in the greatest profusion.—With a degree of greediness equal to that of the sailor who loaded his boat with plunder from a wreck, till the boat and the thief went down together, I cut up masses of soil covered with this pretty plant, till I found myself quite unable to carry them, and was compelled to relinquish them one after another, and retained but a solitary specimen. I here found also, in the greatest abundance, the *dumetorum* variety of *Lastræa dilatata*, and furnished myself with a good supply of roots: my first package of this plant, forwarded from Newport and consigned to Mr. Ward, had never reached him, and I was particularly desirous of sending a second supply. On this high ground few other ferns made their appearance; some feeble plants of *Osmunda*, and an occasional *Filix-fœmina* or *Filix-mas*, with plenty of *Lomaria spicant*, were all that I observed. The heaths, as everywhere else in Ireland, were beautiful: the bells of *Erica Tetralix* were larger, and of a brighter and more varied colour than I have ever seen them in England.

How my heart leaped within me when I peeped over these Tralee hills! When, after an ascent of five hours, I saw the Reeks, Tomies, Carran Tual, Mangerton, Glena, and Turk,—names familiar as household words,—and, while their dark, empurpled, cloudless summits were relieved against a sky of the purest blue, watched the snow-white clouds drifting amongst them, passing in front of one huge peak and behind another! I instantly singled out Carran Tual as the highest land I had seen in Ireland, but I knew nothing of the others, and contented myself with admiring the beauty of the group, without making any attempt to ascertain the names of individuals.

On leaving Tralee I had determined on staying that night at Cloghereen, and had entrusted my knapsack to the care of two gentlemen who were going there in the afternoon by the mail-car; had it not been for this, I believe I should have made for "the Reeks" at once, bent my course southward to Kenmare, and missed the lakes of Killarney, the mob of guides, and *Trichomanes speciosum*. It was a long struggle, but the knapsack and *Trichomanes* gained the day, and I began to descend the hill towards the most celebrated of all touring localities, with anything but anticipations of pleasure, for I really hate touring-places, and touring people, and touring guides: however, I resolved to face it, and so forthwith put myself in order to see the lions of Killarney. Having made up my mind I began to descend from the heights, and it was really a fine walk; the beautiful mass of Kerry hills shut out the horizon, and that dark, grove-like, mountain-locked basin at their feet, contained the lakes which were weekly visited by hundreds of felicity-hunters. Throughout the walk I found abundance of the *dumetorum* variety of *Lastræa dilatata*, and many other forms of the same plant, but I could not quite satisfy myself that they merged in one another. *Osmunda regalis*, not abundant; *Pteris*, still less common; *Asplenium Adiantum-nigrum*, *Aspl. Ruta-muraria*, and *Aspl. Trichomanes*, on walls and ruins; and, approaching Killarney, *Polystichum aculeatum*, *Lastræa Filix* *mas*, and *Athyrium Filix-fœmina*, in the greatest abundance.

When you approach Killarney you find yourself shut in between enormous stone walls, which totally preclude the possibility of seeing anything of the scenery. These walls occur on all the roads in the neighbourhood, and make them anything but picturesque; and Killarney itself is a large, dull and dirty town, and one which a traveller would never suspect was situated amidst the most celebrated scenery in the British dominions.

Cloghereen, an hour's walk beyond Killarney, is merely a dozen houses, the best of which is an inn. Almost opposite to the inn is an entrance to the demesne of Muckruss, and the old abbey is within ten minutes' walk. The abbey is a very beautiful object; it is in excellent preservation, the roof alone being wanting, and the walls are covered with a drapery of ferns, among which *Scolopendrium vulgare* is the most abundant and conspicuous; it is surrounded by fine forest trees of the most beautiful growth: here also was the *Arbutus Unedo* growing in a state of nature, mingled with holly and yew, and forming the most beautiful natural shrubbery I have ever beheld. The rocky ground below was carpeted with mosses, intermixed with the most luxuriant tufts of *Scolopendrium vulgare*, which here assumes a character I had never before seen; ten or twenty very long fronds emanate from a common centre, and each is bent in a graceful semicircle. Here also *Polypodium vulgare* grows to an immense size, and runs into those luxuriant excesses in which the *pinnæ* become again divided, and its normal form is altogether lost: and here *Hypericum calycinum* grows with all the vigour of a native plant, and, if introduced, as some botanists assert, it has made its footing so secure that I much doubt the ability of man to eradicate it. Passing through this little paradise you stand on the bank of Lough Lane, the largest and most beautiful of the lakes of Killarney. A boat, with rowers, steersman, and bugleman was in waiting, and in a few minutes I was floating over its placid waters, the wooded heights of Glena and the purple summit of Tomies rising immediately before me.

Lough Lane covers an area of 5,000 English acres, and contains twenty-four named islands; the largest of these,—Ross Island,—contains 150 English acres, and is laid out in a tasty manner by its proprietor, Lord Kenmare, who allows all visitors to land, and wander about its beautiful shrubberies just as they please. Ross Castle, on this island, is a fine old building covered with ivy, and the visitor is expected to ascend to its summit, from whence the view is very beautiful.

The mixture of the foliage of holly, arbutus, and yew, with the rugged moss- or lichen-stained rocks, is different from what one sees elsewhere. Embarking again, the boat coasted along Ross Island, the bugleman extracting some fine echoes from the ivied walls of the old castle, the wild ducks continually flying off the water before us, and circling high above us in the air, and dozens of corvorants, squatted on their tails, watching our movements from the naked little rocks which just peep above the surface of the water. We passed between the islands of Ross and Innisfallen, and either my vision had been long unaccustomed to trees, or those on Innisfallen, particularly the ash and holly, were unusually majestic in size and beautiful in figure. Indeed it looked like a fairy island raised by magic out of the placid water; and I cannot much wonder at the strange legends of O'Donohue and his milk-white steed, which are so implicitly believed, that an Irish maiden fell in love with the imaginary chieftain, and actually plunged into the crystal waters of Lough Lane, in hopes that after death, her spirit might meet with his; this tale suggested one of Moore's sweetest melodies.—

"Of all the fair months that round the sun, &c."

But the wooded heights of Tomies are paying back the notes of Gandsey's bugle ; so—

"Sweet Innisfallen, fare thee well!
And long may light around thee smile
As soft as on that ev'ning fell
When first I saw thy fairy isle!

Thou wert too lovely then for one
Who had to turn to paths of care;
Who had through vulgar crowds to run
And leave thee bright and silent there."

Landing again where the huge buttresses of Tomies mountain, densely covered with birch, oak, arbutus, holly, and yew, come down to the edge of the lake, I made my way to O'Sullivan's cascade, in hopes of finding on its shady and moss-covered rocks the rare *Trichomanes*, but I was disappointed. Abundance of *Hymenophyllum*, intermixed with luxuriant mosses, covered every stone; and the most graceful form of *Lastrœa dilatata* waved its feathery fronds from every crevice where it could find a footing, and every plant had its pinnules crisped and concave, giving it that appearance which Mr. Babington has elsewhere noticed. Here I will take leave of the lake for a short time, and ascend the mountains.

The mountains of this district appear to be divided into three principal ranges, and certainly possess, beyond all comparison, the most elevated points in the island. The first of these ranges is considerably to the west of Lough Lane; it extends almost to Cahirsiveen in Dingle Bay, and is called Macgillicuddy's Reeks; Carran Tual, the highest point of these, being 3,400 feet above the level of the sea. The second range is also to the west of the lakes, and immediately between them and the Reeks, being separated from the latter by an opening called the Gap of Dunloe; this range is called the Tomies, and includes the Purple Mountain and Glena.—The third range is to the south-east of the lakes, and is usually denominated the Turk Range; its highest points are Mangerton and Turk. On all these mountains that rare and beautiful animal, the red deer, exists in his native freedom; and is said to feed on a species of lichen which is here very abundant. I was told that on Turk herds of several hundreds were occasionally seen. It is thought strange that this fine animal will not breed in parks and enclosures; but when we recollect that in a state of nature he only frequents the summits of the barest mountains, and possesses unbounded freedom;—that restraint to him is like the chain to the eagle, an indignity his proud heart cannot brook;—that in confinement he becomes fretful, impatient, and savage;—we can scarcely wonder that he fails to propagate his kind, and lives always hating and generally hated by the creatures with whom he may chance to come in contact. Very great pains have been taken in Kerry to preserve the red deer, but it is found that their numbers are fast decreasing. Lord Bantry has paid great attention to them at his seat on Bantry Bay, where everything has been done to secure them from molestation; but for some years not a single fawn has been observed. I saw a noble stag on his lordship's estate, a few days after leaving Killarney; it was at some distance, and I at first took it for a bay horse quietly browzing on the mountain-side. The horns and heads of red deer which adorn Lord Bantry's residence, would furnish half the museums in the kingdom. A head, placed over the entrance, is considerably larger than that of a donkey; which would imply the possession of a body nearly twice as large as that of the quadruped from Jerusalem. Several of the noblemen and gentlemen round Killarney have attempted to rear the red deer in their enclosures, by taking them when very young; but it is next to impossible to effect the capture of a fawn, without doing it some mortal injury: a heavy blow with a stick or a stone will so injure one of the delicate little creatures, that it frequently dies in consequence of the wound. Still, however, several instances have occurred, in which fawns have been secured without injury, and if they survive the period of sucking, they are sure to do well, although never sufficiently reconciled to captivity to encrease their kind.

It was through the Gap of Dunloe that I now took my course, having heard its sublimity mightily extolled, but I cannot say I think it would pay for the trouble of exploring, unless to those who are unacquainted with the wilder parts of Wales and Scotland; for the pass is thronged with guides, and it is a sheer impossibility to address a single observation to a companion, without receiving a reply from at least half a dozen of these familiar Milesians. At the top of the Gap I rested awhile in the cottage of the celebrated Kate Kearney, and from her own hand I received the cup of poteen and goat's milk,—a very comforting mixture. After reaching the summit of the gap I turned to the left, and seeing below me the cottage at the end of the upper lake, I abandoned the path, and pursued my way towards the lake in a tolerably direct line, over the trackless waste. All over the side of the mountain *Pinguicula grandiflora* occurs in great abundance: its flowers were generally gone, but some were even yet remaining, and struck me as very beautiful. I believe botanists do not agree as to the value of this species, but to me it appeared distinct: I should however mention that not having the commoner species (*Ping. vulgaris*) at hand, I had no opportunity of comparing them.

At the extremity of the upper lake a boat was in waiting, and I once more embarked. The upper lake looks small compared with Lough Lane, but is said to cover an area of 1000 English acres: the little rocky islets rising from it are very beautiful. Leaving this lake the rowers entered a rapid river connecting it with Loughs Lane and Turk, and having reached a fine bold cliff called "the Eagle's Nest," pulled to the opposite shore, and we landed in order to hear to greater advantage the extraordinary echo for which this spot has long been famous. A tune played by the bugleman standing opposite the cliff, has the effect of a duet. Whether it be possible to produce a duet by merely causing a repetition of a *first*, I must leave the scientific to decide, but so it appeared to me. Indeed the echoes in many parts of these lakes seem perfectly magical. But I am forgetting the ferns. After leaving the Eagle's Nest, *Osmunda regalis* completely fringes the banks of the river between the lakes, and forms a prominent feature in this most lovely scenery. So altered is the usual character of this fern, that its long fronds arch gracefully over, and dip their masses of seed in the crystal water;

while the saucy coots, from beneath the canopy it affords them, gaze fearlessly on the visitors who are continually passing by. One of the boatmen employed by Sir Walter Scott, on the occasion of his visit to Killarney, told me that Sir Walter uttered scarcely a syllable in praise of the scenery, until he came to this spot; and here he stopped the rowers, and exclaimed, "*This* is worth coming to see!" The boatman evidently thought very meanly of Sir Walter's opinion, whom he considered in duty bound to be in raptures with the lakes and mountains. I do not wonder at the great man's taste: to me it was the most wonderfully beautiful spot I had ever beheld, and this beauty is mainly owing to the immense size and number of these pendant fronds. I now approached Dinas Island, on which, I believe, stand some of the largest arbutus trees in the world. The stem of one of them is seven feet in circumference, and its height is equal to that of an ash tree of the same girth which stands near it. There are several others closely approaching this in size. The arbutus, in a state of nature, possesses but a distant resemblance to the trim, formal, bush-like figure which it assumes in cultivation. The branches are very long, gnarled, crooked, and naked to the extremity, where they are crowned with bright green leaves. They seem particularly fond of the fissures of rock, and, like the holly and yew, with which they are ever intermixed, flourish most where there is least appearance of soil to support them.

In this river-scenery the silvery stem and feathery foliage of the birch, and the picturesque figure of the oak and ash, are not to be overlooked; nor should the white water-lilies, floating on the stream, nor the multitudes of mosses, nor the rich bells of *Erica cinerea*, be passed by without a notice.—Passing to the left of Dinas Island, the rowers pulled into Turk Lake, a beautiful sheet of water occupying an area of rather more than 1000 acres, with scarcely an island to interrupt the uniformity of its surface. On the right, the woods of Turk Mountain come down to the water's edge. This fine lake is separated from Lough Lane by a narrow strip or neck of land, and through this is a small opening, over which is thrown a bridge, called "Brickeen Bridge." We passed under the bridge into Lough Lane, and steering to the right, the rowers rested on their oars in the cove of Glena. Here is a cottage belonging to Lady Kenmare, and its tasty architecture, its beautiful flowers, its green lawns, its sweet accompaniments of wood, rock, and water,—render it a spot of uncommon loveliness. From this little cove the boatmen pulled across to Muckruss, passing O'Donohue's horse. This curious rock has so precisely the appearance of a huge horse standing on the surface of the water, with his head down, as though grazing or drinking, that it seems almost impossible to get rid of the illusion; as we approached it, however, it proved very rock-like, and seven corvorants flew off, and skimmed in a line along the surface of the water to some little rocks we had just before passed. After leaving the horse I soon landed, and returned for the night to Cloghereen.

I next paid my respects to Turk waterfall. Owing to an excess of rain the fall was really very striking,—far more so than I had expected to find it: it was a continuous sheet of foam. I first found *Trichomanes speciosum* to the left of the seat whence visitors take the first view of the fall. About fifteen yards higher up the stream, the rocky bank projects into the river; the projection is only to be approached by leaping from stone to stone, along the bed of the torrent, which, in times of flood, as happened to be the case when I paid it this visit, is rather an exciting and ticklish operation. You are so close to the fall as to be covered by the spray, and the roar is almost deafening. Having reached the projection, the botanist must ascend it by means of the roots and branches,—a feat very readily performed, and there is a little platform at the top, where he can stand very comfortably; and while so standing, he will find the rocky bank, just on a level with his eye, completely clothed with *Trichomanes*, the dark green fronds hanging heavily down, dripping with wet, and, if the sun happen to shine, begemmed with sparkling drops. It is a beautiful sight, and well worth the wet feet, which, when the flood is on, form a necessary accompaniment of the expedition. The scenery around is well worthy the rare fern which it cherishes in its bosom. The *rhizoma* of this fern is black, velvety, tough, and remarkably long: it formed a kind of network on the perpendicular surface of the rock, in which its roots had no kind of hold: this was the character of the plant when most luxuriant, but I found other and much smaller plants, which possessed more root and less *rhizoma*, and the roots were fixed in a thin layer of moist earth, among a profusion of moss and *Hymenophyllum*.

At this waterfall, and again in various localities along the road winding towards Kenmare, I found both species of *Hymenophyllum* growing together in the greatest luxuriance and profusion, sometimes on rocks, and sometimes clothing the stems of oak trees to the height of three or four feet from the ground.

In walking under Turk Mountain on my way to Kenmare, I found *Asplenium marinum* in considerable abundance, to the left of the road, on a rock which appeared to have been blasted but a few years back. It was of small size and somewhat unusual form, and I consider the habitat worthy of notice, as being so completely inland. The plant grows at a considerable height on the cliff, and, except to a practised eye, would have the appearance of *Ceterach officinarum*.—The site of this fern is near a tunnel of rock, through which the road passes. The walk here is very fine; the arbutus trees are most ornamental and of large size: I measured the trunk of one that was lying by the road-side, and found it four feet nine inches in circumference. *Robertsonia umbrosa*, or the plant which I have taken for it, is very abundant both here and at the Gap of Dunloe.

The ferns which I met with at Killarney are these. *Lomaria spicant*, *Pteris aquilina*, *Polypodium vulgare*, *Polystichum aculeatum*, *Lastræa Oreopteris*, *Las. Filix-mas*, *Las. dilatata*, *Athyrium Filix-fœmina*, *Asplenium Adiantum-nigrum*, *Asp. Ruta-muraria*, *Asp. marinum*, *Asp. Trichomanes*, *Scolopendrium vulgare*, *Ceterach officinarum*, *Trichomanes speciosum*, *Hymenophyllum Tunbridgense*, *Hym. Wilsoni*, and *Osmunda regalis*: and to these Mr. Mackay has added *Lastræa Thelypteris*. *Polypodium Dryopteris* and *Phegopteris*, ferns which delight in regions like these, I was unable to discover.

(To be continued).

ART. III.—*A Systematic Catalogue of the Fossil Plants of Britain.* By JOHN MORRIS, Esq.

(Continued from Vol. III, page 548).

ADIANTITES, Göpp.

Frond stipitate, semi-orbicular, fan-shaped, entire, deeply lobed or pinnate, *pinnæ* nearly orbicular or oblong, usually unequal and cordate at the base; veins very numerous, forked, arising from the base; midrib scarcely apparent.

* Frond simple, stipitate.

Adian. digitatus, Göpp. page 217; *Sphenopteris latifolia*, Phillips, tab. 7, fig. 18. *Cyclopteris digitata*, Brong. Hist. i. page 219, tab. 61, fig. 2, 3; Sternb. part v. and vi. page 66. *Upper Oolite shale*, Scarborough; Burniston Bay.

—— *Huttoni*, Göpp. page 217. *Cyclopteris digitata*, Lind. and Hutt. page 179, tab. 64. *Cycl. Huttoni*, Sternb. part v. and vi. page 66. *Oolite shale*, Scarborough.

—— *Phillipsii*, Nob. *Sphenopteris Phillipsii*, Mant. Geol. South East Eng. page 239, fig. 2. *Hastings sands*, Heathfield.

* * Frond pinnate.

—— *Cyclopteris*, Göpp. page 218, tab. 34, fig. 8 *a*. *Cyclopteris orbicularis*, Brong. Prod. page 52; Hist. i. page 220, tab. 61, fig. 1, 2; Parkinson, i. tab. 5, fig. 5. *Cycl. Germari*, Sternb. part v. and vi. page 68. *Coal measures*, England; Belgium; Bohemia; Silesia.

—— *Germari*, Göpp. page 218. *Coal measures*, Wettin, Germany.

—— *flabellatus*, Göpp. page 219. *Cyclopteris flabellata*, Brong. Prod. page 52, Hist. i. page 218, tab. 61, fig. 4—6; Sternb. loc. cit. page 167. *Transition slate*, Berghaupten, Germany.

—— *Bockschii*, Göpp. tab. 36, fig. 6. *Transition slate*, Hausdorf, Glatz.

—— *reniformis*, Göpp. page 220. *Cyclopteris reniformis*, Brong. Hist. i. page 216, tab. 61, fig. 1, excluding synonymes; Sternb. part v. and vi, page 67. *Coal measures*, Frejus, France.

—— *trichomanoides*, Göpp. page 220. *Cyclopteris trichomanoides*, Brong. Hist. i. page 217, tab. 61, fig. 4; Sternb. loc. cit. page 67. *Coal measures*, St. Etienne, France.

—— *obliquus*, Göpp. page 221. *Cyclopteris obliqua*, Brong. Prod. page 51; Hist. i. page 221, tab. 61, fig. 3; Lindl. and Hutton, tab. 90, fig. A, B. *Cycl. auriculata*, Brong. Prod. page 51. *Coal measures*, Yorkshire; Pontnewydd, South Wales; Charlottenbrunn, Silesia.

—— *giganteus*, Göpp. page 221, tab. 7. *Coal measures*, Waldenburg.

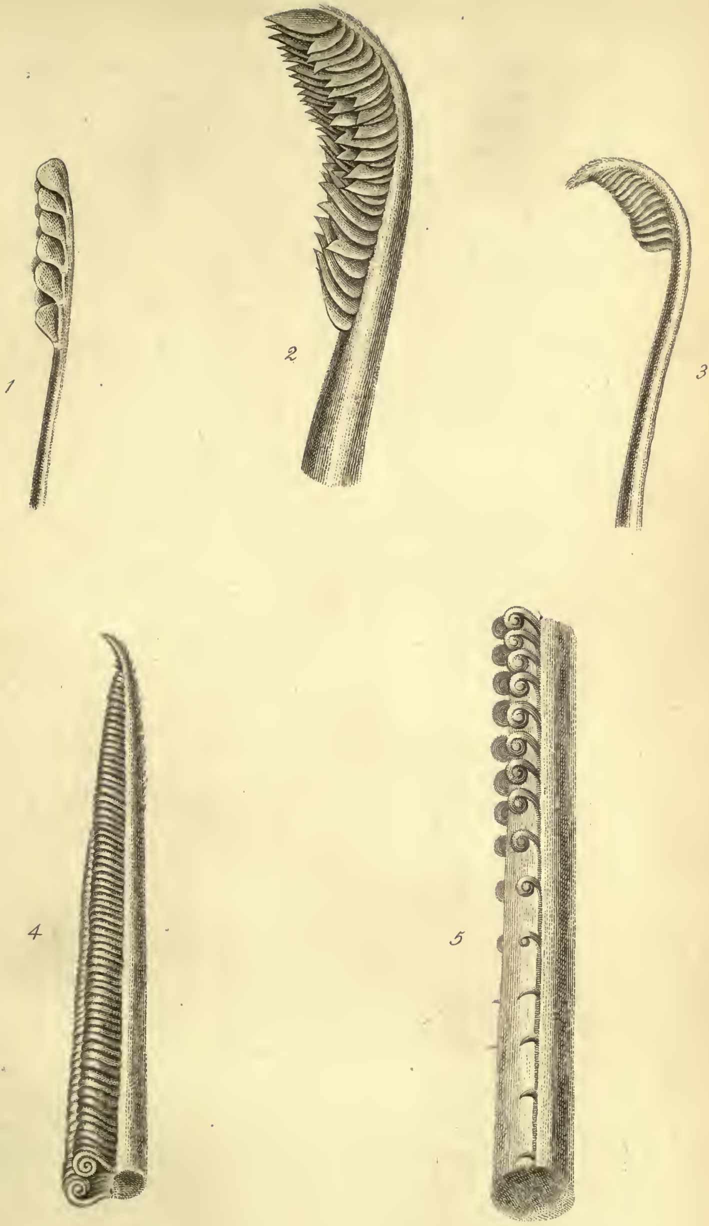
—— *oblatus*, Nob. *Cyclopteris oblata*, Lindl. and Hutt. iii tab. 217. *Coal measures*, Little Hever, Bolton-le-Moor.

—— *umbilicatus*, Göpp. page 221. *Cyclopteris dilatata*, Lindl. and Hutton, ii. tab. 91, fig. B. *Coal measures*, Felling, England.

—— *heterophyllus*, Göpp. page 222, tab. 35, fig. 1, 2. *Coal measures*, Schlegel, Glatz.

—— *irregularis*, Göpp. page 385. *Otopteris cuneata*, Lind. and Hutt. ii. tab. 155, page 203. *Oolite shale*, Gristhorpe Bay, Yorkshire.

—— *Murchisoni*, Göpp. page 386. *Otopteris dubia*, Lindl.



Vernation of Cycadeæ.

Swaine sc.

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and Hutt. ii. tab. 150, page 191. *Coal measures*, Knowlsbury, England.[1]

—— *Beani*, Göpp. page 223. *Cyclopteris Beani*, Lind. and Hutt. i. page 127, tab. 44; Sternb. part v. and vi. page 67. *Oolite shale*, Gristhorpe Bay.

—— *otopterides*, Göpp. tab. 35, fig. 7. *Coal measures*, Zalenge, Silesia.

—— *semiflabelliformis*, *Cyclopteris*, Geol. Trans .2nd Series, vol. v. tab. 38, fig. 7. *Coal measures*, Coalbrook Dale.

*** Frond bipinnate.

—— *auriculatus*, Göpp. page 224. *Neuropteris auriculata* Brong. Hist. i. page 236, tab. 66. *Cyclopteris auriculata*, Sternb. part v. and vi. page 66, tab. 22, fig. 6. *Coal measures*, St. Etienne, France; Bohemia; Waldenburgh.

—— *Villiersii*, Göpp. page 225. *Cyclopteris Villiersii*, Stern. part v. and vi. page 66. *Neuropteris Villiersii*, Brong. Prod. page 53; Hist. i. page 233, tab. 64, fig. 1. *Coal measures*, Alais, France.

—— *trilobus*, Göpp. page 225. *Cyclopteris dilatata*, Stern. part v. and vi. page 66. *Sphenopteris dilatata*, Lind. and Hutt. i. tab. 47. *Coal measures*, Bensham.

—— *pachyrhachis*, Göpp. page 387. *Sphenopteris crassa*, Lindl. and Hutt. iii. page 21, tab. 160. *Coal measures*, Burdiehouse, Edinburgh.

**** Frond tripinnate.

—— *cuneatus*, Göpp. page 226. *Sphenopteris nervosa*, Brong. Prod. page 50; Hist. i. page 174, tab. 56, fig. 26 and 29; Sternb. part v. and vi. page 56. *Coal measures*.

—— *concinnus*, Göpp. page 226. *Sphenopteris adiantoides*, Lindl. and Hutt. ii. page 91, tab. 115. *Coal measures*, Jarrow.

—— *oblongifolius*, Göpp. page 227, tab. 21, fig. 4, 5. *Coal measures*, Charlottenbrunn.

—— *microphyllus*, Göpp. page 228. *Sphenopteris obovata*, Lindl. and Hutt. ii. tab. 109. *Coal measures*, Newcastle.

SPHENOPTERIDES, Göpp.

Frond bi- or tri-pinnate; *pinnulæ* entire, for the most part lobed, cuneate at the base; or bi- or tri-pinnatifid, the lobes toothed or lobed, veins furcate: midrib distinct and rather flexuose; veins obliquely ascending, one to each lobe, simple or dichotomous, and forked at the apex. Fructification marginal.

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[1] Probably not a fern, as the leaves appear to be whorled around the stem; it more nearly resembles a small branch of *Dammara australis*.

Cheilanthites, Göpp.

Frond and *pinnulæ* as in the sections. Veins dichotomous, simple or forked (rarely tri-forked) in each lobe; margin of the lobes thickened.

* *Davallitæ.*

Frond bi- or tri-pinnate, *pinnulæ* or segments of the *pinnulæ* cuneate, nerves obliquely ascending, solitary or in pairs.

Cheil. Mantelli, Göpp. page 231. *Sphenopteris Mantelli*, Brong. Prod. page 50; Hist. i. page 170, tab. 45, fig. 3—7; Sternb. part v. and vi. page 56. *Hymenopteris psilotoides*, Mant. Tilgate Foss. plate 1, fig. 3, *a*, *b*, plate 3, fig. 6, 7, plate 20, fig. 1, 2; Geol. Trans. 2nd Series, i. page 424; Sternb. part iv. page 22. *Hastings sand*, Tilgate Forest.

—— *linearis*, Göpp. page 232, tab. 15, fig. 1. *Sphenopteris linearis*, Sternb. part iv. tab. 42, fig. 4; Brong. Hist. i. page 175, tab. 54, fig. 1; Lindl. and Hutt. tab. 230. *Coal measures*, Newcastle, &c.; Swina, Bohemia.

—— *denticulatus*, Göpp. page 232; *Sphenopteris denticula*, Brong. Hist. i. page 188, tab. 56, fig. 1; Sternb. part v. and vi. page 61. *Oolite shale*, Scarborough.

—— *acutilobus*, Göpp. page 233. *Sphenopteris acutiloba*, Sternb. part v. and vi. tab. 20, fig. 6. *Coal measures*, Bohemia.

—— *laxus*, Göpp. page 234. *Sphenopteris laxa*, Sternb. part iii. tab. 31, fig. 3, part v. and vi. page 58; Brong. Hist. i. page 213. *Coal measures*, Durham.

—— *elegans*, Göpp. page 234, tab. 10, fig. 1, tab. 11, fig. 1, 2. *Sphenopteris elegans*, Brong. Prod. page 50; Hist. i. page 172, tab. 53, fig. 1, 2; Sternb. part v. and vi. page 56. *Acrostichum silesiacum*, Sternb. part ii. page 29, tab. 23, fig. 2. *Filicites adiantoides*, Schloth. Flore der Vorw. tab. 10, fig. 18; Petrefact. tab. 21, fig. 2. *Coal measures*, Waldenburgh and Charlottenbrunn: Radnitz, Bohemia.

—— *divaricatus*, Göpp. page 238, tab. 12, fig. 1, 2. *Sphenopteris elegans*, Sternb. part v. and vi. tab. 20, fig. 3, 4; Bronn, page 30, tab. 7, fig. 5. *Coal measures*, Waldenburg.

—— *microlobus*. Göpp. page 238, tab. 13, fig. 1—3. Scheuch. tab. 1, fig. 7. *Coal measures*, Waldenburg.

—— *tridactylites*, Göpp. page 240. *Sphenopteris tridactylites*, Brong. Hist. i. page 181, tab. 50; Sternb. part v. and vi. page 59. *Coal measures*, Montrelais; Waldenburg.

—— *tenellus*, Göpp. page 240. *Sphenopteris tenella*, Brong. Hist. i. page 186, tab. 49, fig. 1; Sternb. part v. and vi. page 60. *Sphen. multifida?* Lindl. and Hutt. ii. tab. 123, page 113. *Coal measures*, Oldham; Yorkshire.

—— *cysteoides*, Nob. *Sphenopteris cysteoides*, Lindl. and Hutt. iii. tab. 176. *Oolite*, Stonesfield.

—— *meifolius*, Göpp. page 241. *Sphenopteris meifolia*, Sternb. part v. and vi. page 56, tab. 20, fig. 5; *β*, *trifidus*, Göpp. tab. 15; fig. 3, 4. *Coal measures*, Radnitz; Waldenburg.

—— *tenuifolius*, Göpp. page 241. *Sphenopteris tenuifolia* Brong. Hist. i. page 190, tab. 148, fig. 1; Sternb. part v. and vi. page 61. *Coal measures*, St. George's-Chatellaison. (*Sphen. arguta?* Lindl. and Hutt. iii. tab. 168.—*Oolite shale*, Yorkshire).

—— *grypophyllus*, Göpp. page 242, tab. 36, fig. 1, 2. *Coal measures*, Charlottenbrunn.

** *Cheilanthitæ veri.*

Frond bi- or tri-pinnate, *pinnulæ* entire, usually pinnatifid or lobed, the nerves pinnate, the secondary ones usually in pairs, rarely solitary, forked at the base.

—— *distans*, Göpp. page 243, tab. 9, fig. 1, 2. *Sphenopteris distans*, Sternb. part v. and vi. page 62; Brong. Prod. page 51; Hist. i. page 198, tab. 54, fig. 3. *Coal measures*, Ilmenau; Waldenburg.

—— *Höninghausi*, Göpp. page 244. *Sphenopteris asplenioides*, Sternb. part iv. page 16, part v. and vi. page 62.—*Sphen. Höninghausi*, Brong. Hist. i. page 199, tab. 52.—*Coal measures*, Newcastle; Werden, Radnitz.

—— *rigidus*, Göpp. page 245. *Sphenopteris rigida*, Brong. Hist. i. page 201, tab. 53, fig. 4; Sternb. part v. and vi. page 63. *Coal measures*, Waldenburg.

—— *trifoliolatus*, Göpp. page 245. *Sphenopteris trifoliolata*, Brong. Hist. i. page 202, tab. 53, fig. 3. *Filicites trifoliolatus*, Artis, page 11, tab. 6; Parkinson, i. tab. 5, fig. 2.—*Coal measures*, Elsecar, Yorkshire; Valenciennes; Waldenburg.

—— *polyphyllus*, Göpp. page 388. *Sphenopteris polyphylla*, Lindl. and Hutt. ii. tab. 147, page 185. *Coal measures*, Titterstone Clee, Knowlsbury, Shropshire.

—— *Hibberti*, Nob. *Sphenopteris Hibberti*, Lind. and Hut. iii. tab. 177. *Coal measures*, Kirkton, Linlithgow.

—— *obtusilobus*, Göpp. page 246. *Sphenopteris obtusiloba*, Brong. Hist. i. page 204, tab. 53, fig. 2; Sternb. part v. and vi. page 63. *Coal measures*, Waldenburg.

—— *irregularis*, Göpp. page 247. *Sphenopteris irregularis*, Sternb. part v. and vi. tab. 17, fig. 4. *Coal measures*, Radnitz.

—— *botryoides*, Göpp. page 247. *Sphenopteris botryoides*, Sternb. part v. and vi. page 63. *Pecopteris venusta*, Sternb. part iv. page 19; part ii. tab. 26, fig. 1. *Coal measures*, Swina, Bohemia.

—— *repandus*, Göpp. page 248, tab. 15, fig. 2. *Pecopteris repanda*, Lind. and Hutt. ii. tab. 84. *Coal meas.* Jarrow.

—— *Conwayi*, Göpp. page 389. *Sphenopteris Conwayi*, Lind. and Hutt. ii. tab. 146. *Coal measures*, Pontnewydd, Wales.

—— *debilis*, Göpp. page 389. *Pecopteris debilis*, Sternb. part. ii. page 30, tab. 26, fig. 3; part iv. page 18. *Coal measures*, Radnitz and Schatzlar.

—— *undulatus*, Göpp. page 248. *Neuropteris undulata*, Lindl. and Hutt. ii. page 83. *Oolite shale*, Scarborough.

—— *crenatus*, Göpp. page 248. *Sphenopteris crenata*, Lind. and Hutt. i. tab. 39; ii. tab. 100, 101; Sternb. part v. and vi. page 61. *Coal measures*, Whitehaven, Bensham.

**** Dicksonitæ.*

Frond bi- or tri-pinnate, *pinnulæ* sessile, pinnatifid, veins obliquely ascending, dichotomous and forked in each lobe.

—— *Gravenhorstii*, Göpp. page 249. *Sphenopteris fragilis*, Brong. Prod. page 51. *Sph. Gravenhorstii*, Brong. Prod. page 51; Hist. i. page 191, tab. 55, fig. 3; Sternb. part v. and vi. page 61. *Filicites fragilis*, Schloth. Petref. page 408, tab. 10, fig. 17. *Coal measures*, Waldenburg. Var. β, Isle of Anglesea.

—— *Schlotheimii*, Göpp. page 250, tab. 15, fig. 5. *Sphenopteris Schlotheimii*, Sternb. part iv. page 15; part v. and vi. page 62; Brong. Hist. i. page 193, tab. 50. *Filicites adiantoides*, Schloth. Pet. page 408, tab. 10, fig. 18. *Coal measures*, Breitenbach and Saarbruck; Waldenburg.

—— *Dubuissonis*, Göpp, page 250. *Sphenopteris Dubuissonis*, Brong, Prod. page 51; Hist. i. page 195, tab. 54, fig. 4: Sternb. part v. and vi. page 62. *Coal measures*, Montrelais.

—— *gracilis*, Göpp. page 251. *Sphenopteris gracilis*, Brong. Prod. page 51; Hist. i. page 197, tab. 54, fig, 2. *Sphen. fragilis*, Sternb. part v. and vi. page 62. *Coal measures*, Newcastle.

Doubtful species.

—— *excelsa ?* Nob. *Sphenopteris excelsa*, Lind. and Hutt. iii. tab. 212. *Coal measures*, Newcastle.

—— *cuneolata*, Nob. *Sphenopteris cuneolata*, Lindl. and Hutt. iii. tab. 214. *Coal measures*, Newcastle.

(To be continued).

Art. V—*Notice of some additional species of the genus* Equus, *to those currently admitted by Zoologists.* By Edward Blyth, Esq.

It is to be lamented that since the establishment of periodicals exclusively devoted to particular departments of scientific enquiry, some *systematic* record has not been regularly published, of the scattered items of information which incidentally but continually appear in the narratives of travellers, and in other works of a general character. Valuable hints are frequently lost, or perhaps only met with when their utility in promoting investigation shall have been superseded, by the fortuitous re-discovery of the facts long previously indicated, and which might have been sought for and re-ascertained much earlier, had some such record been adopted.—Quotations of short passages, and references to such as are less conveniently transferrible, would unquestionably, in proportion as they keep pace with the progress of publication, and are extracted from works unlikely to pass into the hands of the class which they most concern, exert a highly beneficial influence on the progress of knowledge, and would obviate the tiresome necessity now imposed upon those who happen to be engaged on any particular science, of wading through volume after volume in pursuit of casual observations.

This idea has just forcibly occurred to me, on looking over my memoranda for some details on the species of *Equus*, which are probably much more numerous than is currently supposed, as the following extracts will show. Six species only are admitted by Cuvier, or four besides the horse and ass, which latter I need not here treat of. Three of them are well-known inhabitants of South Africa, remarkable for their striped skins; viz.—

1. The Quagga (*Equus quagga*, Gmelin), so named from its barking voice, and at one time supposed to be the female zebra. It is termed "*Wild Horse*" in the Cape colony, and is indeed the most horse-like, in figure and action, of all the species with callosities on the fore-limbs only, though still essentially asinine in its details. The head and ears are remarkably like those of a horse; and it has stripes on the neck, hind-head, and fore-part of the body only, becoming obsolete behind. This animal has not been observed northward of the Gariep river, and associates very much with the common or white-tailed gnoo. Some years ago a pair of them were frequently seen drawing a curricle about the parks of London.

2. The Douw[1] (*Equus Burchellii* of some, or better, *Eq. cristatus*; *Eq. zebra* of Burchell; erroneously referred by Cuvier to *Eq. montana* of Burchell). Colour of a pale ass, marked with broad blackish stripes, and generally narrower and fainter intermediate ones on the neck and body, but none on the limbs of the adult; the mane forming a high crest between the ears; the face more completely striped than in the true zebra, the stripes terminating in a blackish muzzle.—This handsome species was first indicated as "a curious cream-coloured quaccha" in the Journal of the expedition into the interior of South Africa which was accompanied by the artist Daniell, and which is appended to Sir J. Barrow's 'Voyage to Cochin China' (page 410); it being afterwards stated (page 415) that "the whole body is covered with dark brown stripes," which differentiate it from the "Isabelline Zebra" of Le Vaillant.[2] It chiefly inhabits northward of the Gariep, and associates with the brindled gnoo (*Catoblepas taurinus* and *gorgon*).

3. The Zebra (*Equus Zebra*, Lin.; *Eq. montana*, Burchell). The confusion of the names of these animals by Dr. Burchell, was long since pointed out by Mr. J. E. Gray, in the 'Zoological Journal,' vol. i. page 241; the present one being the mountain species, and distinguished by its admirably regular stripes extending quite down to the hoofs. It is diffused from the Cape colony to Guinea, Congo, and even Abyssinia, according to Ludolff; while Bruce also mentions that "the zebra is found nowhere in Abyssinia, except in the south-west extremity of Kuora amid the Shangalla and Galla, in Narea and Caff, and in the mountains of Dyre and Tegla, and thence to the southward."—(Travels, vol. iv. p. 522). It is the *Wilde-Paarde* of the Cape colonists: and two or three individuals which have been broken in by the celebrated equestrian, Ducrow, entirely lost their spirit and vivacity in consequence, assuming the humbled bearing of the common donkey.

Bruce also states, in the same place,—"Wild asses I have frequently seen alive, but never dead: in neck, head, face and tail, very like ours, *only their skins are streaked*, not spotted [?]." I do not remember that Rüppell anywhere mentions

[1] Pronounced like the first syllable of *dower*.

[2] In the same narrative (page 400) occurs a distinct notice of the recently established *Rhinoceros Ketloa* of Dr. Andrew Smith, which is described by the name of *Jeckloa*. "It measured from the head to the root of the tail, 10 feet 7 inches, and its height exceeded 5 feet 6 inches. But its size was less a subject of remark than the peculiarity of its horns, which were pretty nearly of the same length."

the occurrence of wild *Equi* in that country; but it is not unlikely that a fourth species is here indicated by Bruce, which remains to be established by future observers.

The next quotation is from Bell's 'Travels in Tartary,' vol. i. chap. iii. p. 224; "Journey from Tomsky to Elimsky, in the country of the Tzulimm Tartars."—"Here are also wild asses. I have seen many of their skins. They have, in all respects, the head, tail, and hoofs of an ordinary ass; but their hair is waved, white and brown, *like that of a tiger*." This comparison would lead us to believe that they are striped; and it should be remembered that the common ass, aboriginally an Asiatic species, has frequently distinct stripes upon the legs when very young. Bell adds—"There is, besides, a number of wild horses, of a chestnut colour, which cannot be tamed, though they are caught when foals. These horses differ in nothing from the common kind in shape, but are the most watchful creatures alive. One of them waits always on the heights, to give warning to the rest; and, upon the least approach to danger, runs to the herd, making all the noise he can; upon which all of them scud away, like so many deer. The stallion drives up the rear, neighing, biting, and kicking those which do not run fast enough. Notwithstanding their wonderful sagacity, however, these animals are often surprised by the Kalmucks; who ride in among them, well mounted on swift horses, and kill them with broad lances.—Their flesh they esteem excellent food, and use their skins to sleep upon instead of couches."

The preceding paragraph to that just cited, in the same work, is especially interesting, as containing a distinct notice of the aurochs, (*Bos caucasica*?) which I am surprised has been overlooked by naturalists who have sought for traces of the European bison in Asia: that the Caucasian animal, however, which this would probably be, is a distinct species, I think (with Dr. Weissenborn), extremely probable.[1] "On the hills, and in the woods near this place, are many sorts of wild beasts; particularly the urus, or uhr-ox, one of the fiercest animals the world produces. Their force is such, that neither the wolf, bear, nor tiger, dares to engage with them." This of course cannot be the gaour, or *Asseel Gayal*, of India; of which latter, I may remark, there is a skull in the Museum of the United Service Club, said to be from the south of

[1] In the Catalogue of the Museum of the Royal College of Surgeons, London, a bison's skull (that of a young female, and the only portion of this animal that I know of in London), is marked—"Habitat *the forests of southern Russia in Asia*, the Caucasian and Carpathian mountains, &c."

China. Bell continues,—"In the same woods is found another species of oxen, called *Bubul* by the Tartars [obviously the yack]. It is not so large as the urus; its body and limbs are very handsome; it has a high shoulder and flowing tail, with long hair growing from the rump to its extremity, like that of a horse. Those which I saw were tame, and as tractable as other cattle." But to return to the *Equi*.

The Djigguitai (*Equus hemionus*, Pallas), a well-known Asiatic species, with a broad, dark, mesial stripe down the back, widening upon the croup; and which undoubtedly is extensively diffused, though at present much confounded with others. Such is

The Khur, or wild ass of Persia, so celebrated for its fleetness; and observed near Mount Taurus by Mr. Ainsworth ('Travels in Assyria, Babylonia, and Chaldea,' page 41).—The most detailed notice occurs in Sir R. Ker Porter's Travels (vol. i. page 459), where there is also a coloured figure of the animal. "The sun was just rising," observes Sir Robert, "over the summits of the eastern mountains, when my greyhound suddenly darted off in pursuit of an animal, which my Persians said, from the glimpse they had of it, was an antelope. I instantly put spurs to my horse, and, with my attendants, gave chace. After an unrelaxed gallop of full three miles, we came up with the dog, who was then within a short stretch of the creature he pursued; and to my surprise, and, at first, vexation, I saw it to be an ass. Upon a moment's reflection, however, judging from its fleetness that it must be a wild one, a creature little known in Europe, but which the Persians prize above all other animals as an object of chase, I determined to approach as near to it as the very swift Arab I was on would carry me. But the single instant of checking my horse to consider, had given our game such a head of us, that notwithstanding all our speed we could not recover our ground on him. I, however, happened to be considerably before my companions, when, at a certain distance, the animal in its turn made a pause, and allowed me to approach within pistol-shot of him. He then darted off again with the quickness of thought; capering, kicking, and sporting in his flight, as if he were not blown in the least, and the chase were his pastime.

"He appeared to me to be about ten or twelve hands high; the skin smooth, like a deer's, and of a reddish colour; the belly and hinder parts partaking of a silvery grey; his neck was finer than that of a common ass, being longer, and bending like a stag's; and his legs beautifully slender: the head and ears seemed large in proportion to the gracefulness of these forms, and by them I first recognised that the object of my chace was of the ass tribe. The mane was short and *black*, as was also a tuft which terminated his tail. *No line whatever ran along his back, or crossed his shoulders*, as are seen in the tame species with us. When my followers of the country came up, they regretted that I had not shot the creature when he was within my aim; telling me that his flesh is one of the greatest delicacies in Persia. The prodigious swiftness and peculiar manner with which he fled across the plain, coincided exactly with the description that Xenophon gives of the same animal in Arabia, (vide Anabasis, book i.). But above all, it reminded me of the striking portrait drawn by the author of the book of Job.

"I was informed by the mehmendar, who had been in the desert, when making a pilgrimage to the shrine of Ali, that the wild ass of Irak Arabi differs in nothing from the one I had just seen. He had observed them often, for a short time, in the possession of the Arabs, who told him the creature was perfectly untameable. A few days after this discussion, we saw another of these animals; and pursuing it determinedly, had the good fortune, after a hard chace, to kill it and bring it to my quarters. From it I completed my sketch."

Allied to the khur, would seem to be the "Isabelline Zebra" of Le Vaillant, observed by that author in large herds in southern Africa, but met with by no subsequent traveller.—"It was only under the twenty-fifth parallel," he states, "that I found a kind of wild ass, of an isabelline or pale yellow colour. This animal is, by the Greater Namaquas, styled the *White Zebra;* but it is certainly a wild ass, for, instead of having a striped skin like the zebra, it is of one colour, which has a yellow tinge. No animal in all Africa, perhaps, is so suspicious and so shy as this kind of ass. It appears everywhere in large herds; but I could never get near enough to fire at any of them. I have, however, in my possession, a skin, which was employed to cover the hut of a savage."[1] It is remarkable that there is here, also, no mention of a median dorsal stripe.

Col. Hamilton Smith, in his notice of the isabelline antelope (*Redunca isabellina*), suggests that—"It may be asked here if the female of this animal can have been mistaken by M. Vaillant for a kind of wild *Equus*, which he designates as an isabella-coloured zebra. As the gallop of the preceding species (*Red. fulvo-rufula*, H. Smith), is said to resemble the action of a horse, the mistake may have occurred when the

[1] 'New Journey,' English Translation, iii. page 34.

creature was seen at a distance."[1] To this it may be replied, that the very inferior size of the isabelline antelope, the non-gregarious habits of its group—at least to any extent, and above all, the matured judgment of so experienced an observer as Le Vaillant (who even possessed a mutilated skin, that would have enabled him to correct a hastily-formed opinion, for the shaggy coat of a *Redunca* is widely different from that of an *Equus*), alike concur to negative the conjecture.

The last animal I have to mention is a wild *Equus* of the Eastern Himmalayas. In Moorcroft's 'Travels in the Himmalayan Provinces' (Residence at Ladakh, vol. i. page 311), we read, —"In the eastern parts of this country is a non-descript wild variety of horse, which I may call *Equus kiang*. It is, perhaps, more of an ass than a horse, but its ears are shorter [they are long in *Eq. hemionus*], and it is *certainly not the Gurkhor*, or wild ass of Sindh. Its activity and strength render its capture difficult." He afterwards narrates (page 443),—"We saw many large herds of the kiang, and I made various attempts to bring one down, but with invariably ill success. Some were wounded, but not sufficiently to check their speed, and they quickly bounded up the rocks, where it was impossible to follow. They would afford excellent sport to four or five men well mounted, but a single individual has no chance. The kiang allows his pursuer to approach no nearer than five or six hundred yards; he then trots off, turns, looks, and waits until you are almost within distance, when he is off again. If fired at, he is frightened, and scampers off altogether. The Chan-than people sometimes catch them by snares, sometimes shoot them. From all I have seen of the animal, I should pronounce him to be neither a horse nor an ass. His shape is as much like that of the one as of the other; but his cry is more like braying than neighing. [That of the Djigguitai is a curious compound of both]. The prevailing colour is a light reddish chestnut; but the nose, the under part of the jaw and neck, the belly and legs, are white; the mane is dun and erect; the ears are moderately long; the tail bare, and reaching a little below the hocks; the height is about fourteen hands. The form, from the fore to the hind leg, and feet, to a level with the back, is more equal than that of an ass. He is, perhaps, more allied to the quagga, but is without stripes, except a reported one *along each side of the back* to the tail. *These* were distinctly seen in a foal, but were not distinguished in the adults."

[1] Griffith's English Edition of the 'Règne Animal,' vol. iv. p. 241.

Here, then, we have notices, more or less distinct, of perhaps five species additional to the six which are well known (if, indeed, the wild ass can be considered as well known).— First, in the African continent, besides the three striped species of the Cape, the striped wild ass of Bruce, as distinguished from his zebra; and, in Asia, the wild ass of Bell, with "hair waved, white and brown, *like that of a tiger*." Then, there is the "Isabelline Zebra" of Le Vaillant, of a uniform sort of cream colour, with a yellowish tinge; the Khur of Persia and Arabia, also stripeless, but otherwise very like the Djigguitai, and which of course is the "wild mule" of the ancients; and finally, the Kiang of Thibet, also, it would seem, much resembling the *Equus hemionus*, with an obscure (?) stripe in the adult, more distinct in the foal, running along each side of the back to the tail, and which is moreover like the true zebra, a mountain animal, that "bounds up the rocks," so as to defy pursuit. The re-publication of these notices may possibly lead to the desired investigation.

In conclusion, I would suggest that it would be quite as well if geologists, who continually have occasion to speak of fossil remains of this genus, were always to mention them under the latin name of *Equus*, instead of the English *Horse*, inasmuch as it is very generally supposed, in consequence of the latter term being employed, that the *Eq. caballus* is the animal so commonly met with in the European superficial strata, which there is no reason to suppose is veritably the case.[1]

North Brixton, January 1st, 1840.

ART. VI.—*Remarks on the teeth of Reptiles, from the Tilgate Grit of Battle and St. Leonard's.* By JOHN EDWARD LEE, Esq.

THE following remarks on the teeth of reptiles from the Tilgate grit and clay of Battle and St. Leonard's, may perhaps not be unacceptable to you for insertion in the Magazine.

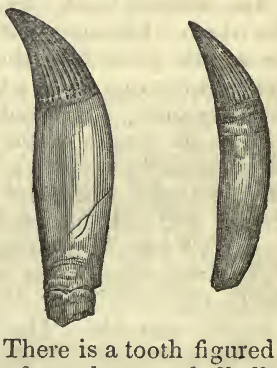
The fall in the cliff near the church at St. Leonard's, which took place last winter, afforded an opportunrty of examining more particularly the stratum which contains the patches of

[1] We cannot help strongly seconding this judicious recommendation of Mr. Blyth's. The impropriety has most probably originated in the circumstance of the close agreement displayed in the characters of the teeth throughout the species of the genus *Equus*.—Ed.

coarse grit. Several teeth and fragments of bones were found in this irregular layer, but the larger and more perfect bones were obtained from the clay bed immediately above the grit. This was also the case at Telham, near Battle; the patches of "sea-sand," as it is called by the workmen, are only very occasionally met with, but when found, are in general rich in organic remains; they are covered with a thick bed of clay, in which large *vertebræ*, and bones of the crocodile, and probably of the iguanodon, are occasionally found; but the richest locality is in the immediate neighbourhood of Battle. A very small quarry, which was opened north of the town early last spring, has afforded rather an extensive series of teeth, and bones of reptiles, fishes, and turtles. These are the principal localities in the neighbourhood of Hastings, from which I have obtained any considerable number of Wealden fossils.

I.—*Crocodile.* Good specimens of the teeth of this animal are comparatively rare at St. Leonard's; but from Telham and Battle they may be obtained nearly equal to the largest of those from Tilgate forest. At Battle especially, they are particularly abundant: in some parts of the bed hardly a stone can be broken without exhibiting portions, at least, of these teeth; and the difference in form which may be observed in them is very great indeed. Besides those noticed by Dr. Mantell, as probably belonging to two or more species, a variety is occasionally found much shorter and thicker than those from Tilgate, though in other respects the characters are nearly the same: in some cases the length of the perfect tooth does not much exceed once and a half its greatest breadth.—But the variety to which I particularly wish to draw your attention, is one which appears to be extremely rare; for I had only the fortune to obtain two, during the past winter: they differ considerably from each other, but still have several characters in common, and probably belong to the same species. There may perhaps be some doubt as to whether they ought to be referred at all to the crocodile, as several of their characters point out a connection with the teeth of some other saurians; respecting this, however, I leave it for others to decide.

4 5



There is a tooth figured in the 'Ossements Fossiles' of Cuvier, from the muschelkalk of Luneville, which, in the length of the fang and partial curvature of the upper part of the tooth, bears a very slight resemblance to the larger specimen; still it appears a distinct species.

Fig. 4, which is drawn of the natural size, represents the larger tooth. The upper portion, which is considerably curved, bears all the characters of the common teeth of the crocodile from the wealden formation, being deeply striated, and with an elevated ridge on each side. The *striæ*, as usual, all commence at an equal distance from the point; a little below the line of their commencement, the colour, which, with the exception of a light annular streak, had been very dark brown, changes, in the rest of the tooth, to a dull white or lead colour. This light-coloured portion or fang, is double the length of the striated or upper part of the tooth: the middle portion is, in some measure, inflated, and again contracts towards the bottom. It should, however, be mentioned, that the original specimen is embedded in very hard grit, which covers some parts of the inner curve, so that though from the appearance of what is exposed, there is little doubt that the drawing is correct, yet the exact bend of the inner outline is in a small degree assumed: the outer side, however, is clearly shown, and exhibits a very regular curve almost to the bottom. The middle part of the tooth is marked with obscure longitudinal *striæ*, and is rather flattened at the sides, in addition to which, on the upper side, as it lies exposed in the stone, there is a slight longitudinal depression. The lower part of the tooth is marked by several annular furrows, one of which, near the bottom, is very deep and well defined; the other three, of which one is below, and two above, are indistinct, and rather obscure.

The smaller tooth (fig. 5) is more slender than that just described. The outer curve is still more regular, and though the middle portion of the tooth is slightly thicker than the rest, yet the inner curve follows very closely the character of the outer one. The striated portion is rather more than one third of the length of the whole tooth. There is no annular furrow near the bottom, as in the last specimen, but two very indistinct ones just below the commencement of the striated portion, and a slight annular depression, rather than a furrow, about half way up the fang.

Both these specimens were found in the small quarry near Battle.

II.—*Iguanodon.* The teeth of this animal, as found at St. Leonard's, are usually only the stumps, worn down by attrition, and in most cases hollowed to mere shells by the absorbent action of new teeth. On the contrary, many of those which are found at Battle appear to have come fresh from the jaw; the serrated edge, in some specimens, is beautifully perfect, and many of them are nearly equal in size to the largest of those from Tilgate Forest. Good specimens, certainly, are not common. At the same time I obtained from the quarrymen, and by my own exertions, a series in every stage from the perfect tooth to the worn-out stump.

III.—*Megalosaurus*. The teeth of this animal found at St. Leonard's and Battle, cannot by any means vie in magnitude with those from Stonesfield: in general they are only about half the size, and some of them are still smaller. There is a variety occasionally met with, which is shorter, more curved, and rather thicker in proportion than those of the usual form; it might be a subject of enquiry whether this belongs to a distinct species, or be only from a different part of the same animal. At St. Leonard's it is of rare occurrence to meet with even a fragment of the tooth of a *Megalosaurus;* but at the small quarry near Battle, before alluded to, they are occasionally found, though not in any great abundance.

IV.—*Phytosaurus cylindricodon*. The teeth which are figured by Dr. Mantell, as belonging to this animal, appear to be extremely rare throughout the wealden: I met with only one at St. Leonard's during the last winter, and from the Battle quarry, four or five perfect and imperfect specimens were all that could be obtained. They appear to differ slightly from those figured by Dr. Mantell; the upper part is rather more flattened, and bent inwards, and the whole surface is deeply and irregularly wrinkled. All the specimens, like those from Tilgate Forest, appear to have been broken short off.

Ventnor, October 11*th* 1839.

Art. VII.—*Remarks on the Skeletons of the common tame Goose, the Chinese Goose, and the Hybrid between the two.* By T. C. Eyton, Esq., F.L.S.

The circumstance of hybrids produced between the tame goose and the Chinese goose being again productive, was the inducement which led me to make the following examination of their respective skeletons, in order to ascertain the degree of similarity existing between them. Before however stating the results, it may perhaps be as well to mention the mode I followed in order render the hybrids productive, which was simply the placing together a male and female, of different hatches; though the birds were both young, they reared eight young ones.

The mode adopted by me in counting the *vertebræ* is the same as that mentioned in my Monograph on the *Anatidæ*,—namely, to consider as cervical all the *vertebræ* which are anterior to the ribs, and have no attachment to them; the dorsal are those anterior to the peloinal bones, having ribs attached to them; the sacral are those anchylosed together, and immediately succeeding the dorsal, to some of these are attached either true or false ribs.

The following table will show the numbering of the vertebral column, as far as I have been able to ascertain it, in the birds above mentioned, and also in a variety of the Chinese goose.

	Cervical.	Dorsal.	Sacral.		Caudal.
Tame goose	17	6	16	2	6
Chinese ditto	17	6	17	2	6
White variety of ditto	17	6	16	3	6
Hybrid		6	16	2	6

The second number under the head of sacral *vertebræ*, denotes those to which ribs are attached; so that the total number in the tame goose for instance, is eighteen.

It will be seen from the foregoing table, that some difference exists in the numbers of the dorsal and sacral *vertebræ* of the Chinese goose, as given here and in the work above mentioned, which is caused by one being taken from the dorsal and added to the sacral. This change I have been induced to make, on the examination of a section of the *pelvis;* the skeleton referred to in the Monograph, is that now spoken of as the white variety. I greatly regret, that in consequence of some of the cervical *vertebræ* of the hybrid having been lost, I have been unable to give their number with certainty, but it is most probably seventeen, both the parent birds possessing that number. The caudal *vertebræ* of the Chinese goose were also damaged.

The *sternum* of the hybrid bird presents some curious features; but previously to referring to them, it will be necessary to point out the differences between that part in the common goose and the Chinese, which are as follows. The tame goose has the *sternum* broader and longer, with the keel deeper, and is altogether larger than that of the Chinese; it is also characterized by a wave or indentation[1] in its plane, at about

[1] This feature increases with age. I possess the *sternum* of a tame goose above twenty years old when killed, which shows it much more distinctly than that of a young one. The fissures also on the posterior margin are closed up, and form *foramina.*

one third of its length from the posterior extremity, possessed only in a slight degree by the Chinese.

The knob situated between the junctions of the coracoid bones is also larger in the common goose than in the Chinese.

The *sternum* of the hybrid bird is as long as that of the tame goose, but not broader than that of the Chinese; it is consequently more elongated than either. The indentation or wave in its plane is about the same as that in the Chinese, as is also the size of the knob situated between the coracoids.

The bones of the *pelvis* in all three are very similar in form, but that of the tame goose is the largest and most massive. The hybrid differs from either of its parents, in having the notch situated on each side of the posterior margin of the *ilium*, merely represented by two slight indentations.

The *cranium* of the Chinese differs from that of the common goose, in having two tubercles at the base of the bill, that part being, on the contrary, depressed in the tame goose. The hybrid takes an intermediate form, being only slightly elevated.

The remaining bones do not differ in form in either of the three birds mentioned; in the tame goose, however, they are larger than in the Chinese, while those of the hybrid are intermediate between the two.

From the above brief notes the following deductions may be drawn.—That the hybrid possesses characters nearly intermediate between its parents, but in one particular varies in form from either: and that the most material difference between the parents consists in size, and in the numbering of the sacral *vertebræ;* in the latter particular the hybrid agrees with the female parent.

REVIEWS.

ART. I.—*A Manual of British Coleoptera, or Beetles; containing a brief description of all the Species of Beetles hitherto ascertained to inhabit Great Britain and Ireland: together with a notice of their chief localities, times and places of appearance, &c.* By JAMES FRANCIS STEPHENS, F.L.S., &c. London: Longman and Co. 1839. 8vo. 443 pp.

To a person unversed in zoological literature, the various points of view in which the majority of the subjects of zoological science may be treated, must appear quite useless.—Such an one would naturally be led to suppose, that when you have once learned the name of an obscure animal, no farther knowledge is requisite, or to be obtained. He would either not think at all upon, or would ridicule, the researches of the anatomical zoologist who, like Swammerdam or Lyonnet, would spend years over the investigation of a single species; whilst others, whose observations are directed to the study of the habits of the different species, or their relations in the great chain of the creation, would be equally thought to be following up an amusement having no solid object in view. We are not, fortunately, now-a-days called upon to write long essays to prove that all or any of these different views of studying nature, are possessed of respective advantages; neither do we feel it necessary to enter into any enquiry whether the researches of nomenclaturist, comparative anatomist, natural-historian, or natural-classificationist, be entitled to the greatest weight. All are leading us on to the same end, — the knowledge of nature, — by different routes; and therefore, a work like the one whose title stands at the head of this article, is in its way as valuable, in our opinion, as that reviewed in the following one, although there is not a single *readable* passage in the work of Mr. Stephens. We are exceedingly glad to find that the author has followed the plan of some of the best botanical writers, in giving a short abstract of his great work, which, from its price, is necessarily in a limited number of hands. The volumes of the "Illustrations of British Entomology" which comprise the beetles, cost nearly eight pounds; whilst here, for little more than one sixteenth of that sum, the specific characters, sizes, habitations, and periods of appearance, of nearly 3500 species of beetles are given to the reader: each page averaging about nine species. Short characters of the order, and its primary and secondary sections, are first given, which are succeeded by the families of the stirpes, and the genera in the respective families. A good plan has been adopted in respect to the latter; all the genera in a family being characterized in the same page, instead of their characters being given at the head of the respective genera: by this means the student has under his eye, at one glance, the whole of the genera in the family, and is able far more readily to determine the genus of an insect, than if they had been scattered over many pages. The following description of the first genus and species will show the manner in which the characters are given, and which will be seen to be sufficient for all practicable uses.

Genus 1. CICINDELA. Anter. tarsi of the ♂ (male) with three dilated joints; of the ♀ (female) simple: elytra oblong ovate depressed (with discolorous markings): tho. somewhat quadrate; rarely a little cylindrical.

1. *sylvatica*, L. vol. ii. p. 658. Sam. pl. 3, fig. 8. Ste. M. i. 7. Above brown with purplish tints: elytra with a crescent on the shoulder, a nar-

row flexuous reflexed stripe in the middle and a spot near the tip, whitish: upper lip black. (Length 7-8 lines). Sandy heaths: Cobham, Weybridge, Godalming, &c., Surrey; Christ Church, &c., Hants; Martlesham, Aldborough, &c., Suffolk: 4-7 [April to July.]

The following enumeration of the extent of the collection of Mr. Stephens, will, we should think, be sufficient to prove his ample qualifications for the work in question.

	Species about.	No. of Specimens.	Drawers.
Coleoptera	3320	26109	comprised in 28
Dermaptera	7	48	2
Orthoptera	59	326	
Neuroptera	223	1066	7
Trichoptera	177	620	28
Hymenoptera	2960	21122	3
Strepsiptera	2	2	
Lepidoptera	1825	14477	56
Diptera	2450	11546	20
Homaloptera	10	46	
Aphaniptera	14	26	1
Aptera	154	360	
Hemiptera	346	2720	4
Homoptera	351	1984	2
	11898	80452	151
Ametabola	94	260	1
Crustacea	457	1470	21
Arachnoida			
Acari			
Myriapoda			
Metamorphoses		About 6000	20
	12449	88182	193

We give this enumeration as a scientific curiosity, showing, as it not only does, the vast extent of the author's collection, but also the various proportions of the different orders; the great number of specimens over that of species is likewise of importance, as exhibiting the materials the writer must have for the discrimination of varieties from species, We likewise observe with pleasure that the publication of the 'Illustrations is only *suspended*, and not, as we began to consider, *defunct:* we therefore look forward to its continuance with great pleasure, and will merely observe that we consider it to be a duty which Mr. Stephens owes to his subscribers, to terminate the work; whilst they, on their part, may rest assured, that as the 'Illustrations' are printed "*for the author*," his interest is sufficiently great to lead him to complete the work as quickly as possible, whereby it will become of so much greater value, in a mercantile point of view, to himself.

The present volume is complete in itself, but it is intended to publish three or four others, containing the remaining orders. Thus for a trifling expense, at a few periods, a complete abstract of a twenty-guinea work will be in the hands of the reader.

Art. II.—*A Treatise ou the Insects injurious to Gardeners, Foresters, and Farmers.* By Vincent Kollar, Curator of the Royal Cabinet of Natural History at Vienna, and Member of many learned Societies. Translated from the German, and illustrated by Engravings. By J. and M. Loudon: with Notes, by J. O. Westwood, F.L.S., &c. London: W. Smith. 1840. Sm. 8vo. pp. 377.

This work is of a totally different character from that of Mr. Stephens above described. Impressed with the conviction that the most likely means of attaining a knowledge of the most effectual remedies against the attacks of the various species of obnoxious insects, are to be sought for in a knowledge of their respective habits, M. Kollar and his associates, Baron Kreigelstein and Canon Schmidberger, have devoted their attention to the economy of the different species, and the result has been a volume on the history and "Private Lives" of about one hundred and twenty-five of the insects most obnoxious to vegetation. And we have now to thank the Misses Loudon for their careful translation of the work from the German, and for the numerous wood-cuts with which they have illustrated the text, whereby a better notion will be obtained of the species.

To enable the farmer and arboriculturist,—who, of course, are not always adepts in scientific classification,—to find more easily the insects particularly interesting to each, the families and species are not given in any systematic arrangement, but according to the branch of culture to which they prove most injurious. The insects troublesome to sheep and cattle are also given, and the work contains a popularly written sketch of the principles of entomological science. The translation being more especially intended for the agriculturist and gardener, those chapters in the original work which treated upon body insects and house insects are omitted in the translation.

The following is a sketch of the sections into which the translation is divided.

1. Insects which do not live upon the body, but are troublesome from their attacks upon man.
   Insects which live on domestic animals.
   Insects not parasitical, but which sometimes attack domestic animals.
   Insects which injure bees.

2. Insects which injure grain in a growing state, and in the granary.
   Insects injurious to meadows [and pastures].
   Insects injurious to culinary vegetables.
3. Insects which injure the vine.
   Insects which particularly injure greenhouse and hothouse plants.
   Insects injurious to fruit-trees.
   Insects which are destructive to woods and forests.

From the very neat and cheap manner in which the volume is got up, we trust it will become a favourite, not only with the entomologist, but with every lover of agriculture, arboriculture, and horticulture.

ART. III.—*British Entomology: or Illustrations and Descriptions of the Genera of Insects found in Great Britain and Ireland.* By JOHN CURTIS, F.L.S., &c. 16 vol. 8vo., or 192 monthly parts. London: printed for the Author. Pigot and Co.

WE congratulate the author upon the completion of this, one of the most elegant periodical works which has ever issued from the British press. We may likewise congratulate our readers on possessing so national, as well as so valuable, a production. In the beauty of its figures, and the superior style of finish of the colouring, it stands superior to any other entomological work yet published; whilst the anatomical outlines will render it a work of constant reference to those who do not content themselves with the mere names of species.

Much however still remains to be done before we possess a complete iconographical illustration of the genera of British insects. In Mr. Curtis's work very many groups of trivial value have been figured, whilst others, types of well-marked groups of considerable extent, have been omitted; indeed we would strenuously recommend the author to add another volume, in which these omitted groups might be illustrated.—*Scydmænus*, *Hispa*, *Sialis*, *Xiphydria*, *Attacus*, *Sphex*, *Lyrops*, *Stigmus*, *Limacodes*, *Cilix*, *Scatopse*, *Anthomyia*, *Calobata*, and many others (as well as all the Ametabolous genera, which Mr. Curtis considers as insects), ought to have been illustrated; and even in many of the genera figured, we find details omitted upon which the genera are chiefly founded: a deficiency evidently originating in the higher value supposed by the author to be possessed by the *trophi*, which he has accordingly always represented in detail, although, in many great groups, they clearly afford but secondary characters. The nervures of the wings of the *Lepidoptera*, and their transformations, ought never to have been neglected;

whilst the structure of the thoracic segments, and of the abdominal appendages, has not been investigated with that precision which was requisite in a work of such high pretension. In these respects Burmeister's 'Genera Insectorum' (which has now reached the fourth number), and the Illustrations of the Crochard edition of the 'Règne Animal,' come much nearer to our notions of generic illustration.

With all these deficiencies, however, the work will stand as a lasting memorial of the zeal and untiring energy of its author, during the best period of his life.

Mr. Curtis has announced an Atlas to illustrate all entomological works, consisting of figures and generic characters of the groups not figured in the 'British Entomology,' each plate containing four or five genera; to consist of 200 plates, price 6d. each, uncoloured. Thus it will be seen that there remain nearly 1000 genera over and above those delineated in the Illustrations. We wish Mr. Curtis health to complete his announced undertaking, which appears to us capable of being rendered even more useful than his former work.

Art. IV.—*Histoire Naturelle des Animaux Articulés.* Par Laporte de Castelnau, Lucas, et Blanchard. 4 forts vol. in 8vo. Paris: Dumenil.

This very cheap illustrated work, noticed by us vol. i. p. 499, has reached its 96th number, to the middle of the *Hymenoptera*, and is promised to be completed in 24 more parts. M. Lucas has completed the apterous insects of Linnæus, and M. Blanchard is the writer of the winged insects; Laporte describing the *Coleoptera*.

Art. V.—*Revue Zoologique.* Par la Société Cuvieriénne: publié sous la direction de M. F. E. Guerin Meneville. 8vo. Paris.

This periodical contains numerous original memoirs, comprising the descriptions of various tribes, genera, or detached species, contributed by the Marquises Spinola and De Bresne, MM. Lesson, Kiener, Gory, Chevrolat, Guérin, and many other writers; analyses of new works; and notices of the proceedings of the Parisian Societies. The last number we have received (for November, 1839) contains a notice of the proceedings of the scientific meeting held last year at Pisa, at which we perceive M. V. Audouin and Prince C. L. Buonaparte were present.

Art. VI.—*Genera et Species Staphylinorum.* Auctore G. F. Erichson. Pars prior. Berolini: 1839. Large 8vo. 400 pp. 3 pl.

It is intended that this work should be a complete Monograph of the family *Staphylinidæ*, or the genus *Staphylinus*, Linn.; and from the talents of the author, and the valuable materials at his command contained in the rich collection of Berlin, it promises to be as perfect a work as can be produced upon the subject. The plates are in outline, and represent the structural peculiarities of the genera.

Six hundred and forty-eight species are described in this first part: but we are sorry to perceive that the author is not acquainted with the great work of Stephens.

Art. VII.—*An Introduction to the Modern Classification of Insects.* By J. O. Westwood, F.L.S., &c. Longman. 8vo.

Mr. Westwood's Introduction has reached the fifteenth part, the *Mandibulata* and *Lepidoptera* being nearly completed: the Linnæan *Diptera* and *Hemiptera* still remain to be described.

Art. VIII.—*A History of the British Ferns.* By Edward Newman, F.L.S. London: Van Voorst. 1840. 8vo. pp. 104.

It affords us much satisfaction to find Mr. Van Voorst extending to the botanical kingdom the circle of scientific treatises, in which, as publisher, he has been eminently successful. A History of our Forest Trees is in progress by Mr. Selby; while the work before us, treating on a more lowly tribe,—our indigenous Ferns, may vie, though not in bulk, yet in intrinsic merit, with the late-published 'Histories' in British Zoology, that have with justice earned so high a reputation.

Mr. Newman sets outs with a remark, the purport of which, if more generally attended to than it has been, would prevent so much confusion and discrepancy existing between the accounts of different systematic writers on the protean tribes of which this volume treats.

"I think no botanist, who allows his memory to turn to the varieties he has observed of *Lastræa dilatata* and *Polystichum aculeatum*, will for a moment deny this; and yet what botanist has ever presumed to treat of the cutting of the frond in Ferns as of any other than the highest importance? I entertain a different opinion. I think that mere cutting of frond is of no more value than colour in fowls or cows, and therefore should not be used as the leading character of a species; to distinguish which, I would look for less fickle characters in the figure, position, and covering of the masses of seed, in the habit of the *rhizoma*, and in the general outline of the frond."

The author dilates, at some length, on Mr. Ward's plan of cultivating ferns in closed vessels, so as to exclude the very free access of air. But, beautiful as are the results arrived at by that gentleman, we cannot help thinking that Mr. Newman's enthusiastic commendations partake rather of what our Gallic neighbours call *couleur de rose*. Certainly, however, it is by no means so generally known as it ought to be, how very successfully ferns can be cultivated in closed glass jars, and that thus our drawing-rooms may readily become decorated with these most exquisite of Flora's productions.

"This end is obtained by the use of glass, the light so essential to vegetation being thus freely admitted. The most ready way to try the experiment is, to procure a glass vessel, for instance, one of those jars used by druggists and confectioners; introduce some soft sandstone, or some light soil, filling one-sixth of the jar with it, and taking care that the earth be very moist, yet allowing no water to settle at the bottom of the jar; plant a fern in the earth, and then cover the jar with its glass lid, first supplying a slip of wash-leather round the rim of the jar, which will pretty nearly cut off the communication between the internal and external air; no farther attention will be required: the fern will live, thrive, and probably seed, the seed also vegetating, and at last the jar will become too small for its contents; no watering is needed, the moisture in the earth will exhale, condense on the glass, trickle down its sides, and so return to the earth whence it arose."

With regard to the theoretical action of Mr. Ward's closed fern-cases, the author seems to doubt whether they have not some influence distinct from that of merely excluding soot, and thus supplying their vegetable inhabitants with what may aptly be called *filtered air*. The experiment cited by Mr. Newman, and on which he has based his opinion, that some peculiar action is exerted in addition to that alluded to, is by no means conclusive; as it is obvious that the plantules confined in the moist atmosphere of the phial, would, *cæteris paribus*, have a better chance of existing, than those exposed to spontaneous evaporation in the atmosphere,—an action, against which their feeble vitality could oppose but little resistance.

"On a hot day in the summer of 1837, I brought home in a tin box about a dozen seedlings of *Lastræa dilatata*, which I had picked out of moss; each had a single frond of very small size, and extremely minute, white, and delicate roots. Having a wide-mouthed phial at hand, I put in it a small quantity of very wet earth; and then passing a pin through the single frond of one of the seedlings, and pinning it to a cork previously covered with wet wash-leather, I fixed the cork firmly in the phial, and left the fern hanging at the head of the pin with its roots downwards. Some hours afterwards I looked at my little fern, and found it exhibited no symptoms of withering; whereas the other seedlings, left carelessly on the ground beside the phial, were completely dead, and crumbled to powder between the finger and thumb. I hung up the phial by a string to a nail in the garden wall, and here it was hanging twelve months afterwards. The cork was fastened exactly as I left it, but the phial was filled with something green, which, on taking it out, proved to be a plant of the common chickweed, but to my great joy the little fern still hung from the pin; its roots were longer, it had made two fronds, and the original frond had withered, but was still strong enough to support the fern."

The value of characters drawn from the venation of ferns is very properly alluded to as affording a means of distinguishing closely-allied genera: an observation moreover deserving consideration, from its being generally very distinct in the fossil species of this tribe, where the fructification ceases to afford a sufficient or tangible guide.

"Most authors have admitted the importance, for purposes of nomenclature, of those characters which are spoken of by Smith as derived from the fructification; but, until lately, other characters of equal value, drawn from the situation of the veins, have been entirely neglected; this is now no longer the case, and I am inclined to believe, that henceforward, in the veins of a new fern will be sought the characters which shall decide its genus."

The illustrations display considerable taste, and what is of far greater moment, correctness. The descriptive part of the volume is accurately and clearly written, and the list of habitats is tolerably copious. The reader will at first be surprised by the appearance of several old friends under new faces, from some alterations of nomenclature introduced by Mr. Newman, none of which, however, appear to have been made hastily or unnecessarily; and in no instance has the author *coined* a new name.

Under *Lastræa dilatata* (*Nephrodium dilatatum*) mention is made of the two remarkable varieties of this fine fern, characterised by the nearly flat, and the convex recurved fronds. There is one curious variety not, however, referred to, which grows under some hedges skirting Hampstead Heath, in which the frond is often three feet high, and the spores are of a brilliant jet black, the *indusia* being white: the whole plant, whilst drying, evolving an aromatic odour like woodroffe (*Asperula odorata*).

The closely-allied species of *Aspidium*, viz., *lobatum*, *angulare*, and *aculeatum*, are very correctly treated of as varieties of one and the same plant, arising from accidental circumstances connected with soil, situation, &c.; and Mr. Newman is inclined to place *Asp. Lonchitis* in this group, as a variety also. But although he has adduced many plausible reasons in support of his opinion, we believe that few botanists will feel inclined to class the sharp and rigid *Asp. Lonchitis* of the Scotch and Irish mountains, with the remarkable variety of *Asp. lobatum* termed *Lonchitioides,* which is not unfrequently found in the southern counties. We have met with *Lonchitis* growing in abundance by the side of the source of the White-water, terminating the Glen of Dole in the Clova mountains, and scattered in patches over the whole glen, as far as the terrific cascade of Bach-na-gairn; but we never saw the slightest variation fiom its normal type: it preserved its rigid and even spinous fronds under every variety of aspect in the glen of Dole. The most perfect specimens of *Aspidium lobatum* var. *lonchitioides* we have met with, were found in *fructification,* in hedges by the side of Buckhurst Park, at Withyham, Sussex, close to a farm called Florence; and these, although observed annually during eight years, never acquired anything approaching to the rigidity of the alpine plant. This question remains, however, *sub judice,* and is an interesting one for the practical botanist to determine.

To every lover of British Botany we cordially recommend Mr. Newman's volume, and we assure him that to *us* it is not the less welcome as being the production of a professed *entomologist*.

SHORT COMMUNICATIONS.

Singular *mode of extrication of the Imago from the Pupa-case in a species of Crane-fly.*—My attention was drawn, during the autumn, to some dipterous *larvæ* of a dirty blackish colour, among the roots of some moss which grew upon the plastering of the back wall of the house. From one piece, about the size of a small marble, which I pulled off, I shook out no less than seven of them; some three quarters of an inch in length: when stretched out to their full extent, as they were creeping about, their length was nearly an inch. I had often noticed them previously, in their peregrinations on the glass of the window, a thing not very usual with the *larvæ* of dipterous insects. I tried to rear several during their *pupa* state, but could not succeed. One fine morning, about the middle of October, I observed what appeared to be the body of a small crane-fly, projecting from the moss; upon examining it more closely, I found it was an *imago* disengaging itself from the *pupa-case*, (there was no appearance of a *larva-case*): the head and thorax were very small, but the abdomen was stretched out, and to all appearance quite rigid, being about half an inch in length, and one sixteenth in diameter. The wings were expanded, and spread out at right angles to the body; the apex of the abdomen was still enclosed within the pupa-case; the legs were not yet extricated from their *exuviæ*. The colour of the abdomen was whitish, but, from its extreme tenuity, owing to its excessive enlargement, it was perfectly transparent, showing the intestinal canal as a very dark, fine thread within it. After watching it for some time, and not observing any motion, I thought it had died, as the previous night had been very cold; but while I went into the house for a knife to detach the moss from the wall, it disengaged itself, and when I returned was creeping about and fluttering its wings, the abdomen remaining distended as before. I pricked it with a fine pin, when it instantly shrunk to about a line and a half in length, and the thickness of a common sewing-thread; losing its transparency, but not its colour, remaining of an opake white for about half an hour, when it began to assume its natural hues. On examining the wall I found numberless *exuviæ* in positions exactly similar to the one above described, and afterwards observed several imagoes flying about with the abdomen of the size and colour mentioned, not having yet assumed its natural dimensions and appearance. In one the abdomen was considerably reduced in length, but not in its lateral dimensions; the cause of so unusual a condition I suppose to be owing to the length of the legs, which, when stretched out to their full extent, measured about nine lines in length. Most other insects make use of their legs in extricating themselves from their *exuviæ;* but in this, owing to their length so greatly exceeding that of the body, it is evident there must be some other method of extrication, which is effected by the elongation of the abdomen. There were numberless small patches of moss on the wall, many of them not above a line in extent; but I could not find one which was not infested by these *larvæ*, and I often saw them, after having devoured the roots of the moss where they had been hatched, creeping about in search of another habitation. By the end of October they had destroyed every vestige of the moss, not one living plant of it being found anywhere on the wall, by the most diligent search.—*James Bladon.—Pontypool.—January* 11*th*, 1840.

Habits of the Toad, its change of Skin, &c.—Being advised about two years ago to keep a toad in my greenhouse for the purpose of destroying ants, I procured one, and find that this animal eagerly devours not only ants, but earwigs, caterpillars, small beetles, centipedes, slugs, and I believe all small insects that may chance to fall in its way, but studiously refusing to touch them if dead. I likewise am able to affirm the shedding of the skin, a fact which I have not seen noticed by naturalists, probably arising from the circumstance of the toad hiding itself at the time of the operation, and swallowing the skin afterwards.

In the spring of 1839, the one in my possesson seemed in a sickly state, looking thin, and refusing food: one morning I found him under an inverted flower-pot, (in which I had cut a hole), evidently in great agony, (surrounded by a pool of fluid excretion, with which the surface of his body appeared saturated), and apparently choaking. I took it into my hand, and found something hanging from both corners of its mouth. I took hold of one portion with my fingers, and drew it easily out, which I threw down, but on the removal of the other piece, I was induced to throw it into water, when it expanded, and I found it to be half the skin of the animal, even to the very tips of its toes. The toad was of a much lighter colour after, than before the operation, and I find that invariably its colour changes from dark to light, whenever it emerges from its hiding-place.—*John Bright.*—*Brixton Hill, Surrey.—January* 10*th*, 1840.

[We believe that zoologists are indebted to Prof. T. Bell, for the first notice of the above curious circumstances which accompany the change of skin in the toad, and which are fully described in the 'History of British Reptiles,' p. 109.—*Ed.*]

Information for Collectors in Natural History.—When in Naples last year, I made the acquaintance of a very zealous naturalist and collector, who is willing to send to this country collections of the productions, in various branches of the Natural History of Naples and Sicily, but more particularly of the shells, recent and fossil, and insects, either in exchange for the shells, recent and fossil, of Britain, or for payment in money. I can testify to his ability and liberality, and strongly recommend him to the notice of those who are desirous of collecting the productions of that interesting portion of Europe.

I may add that Mr. Morell is a native of Switzerland, and was, for many years, a correspondent of the well-known naturalist, Dr. Leach.

His address is,

Monsieur JAQUES MORELL,
Chez Messieurs Cotterell, Iggulden. & Co.,
Banquiers,
A Naples.

W. C. Trevelyan.—*Edinburgh*, 5*th December*, 1840.

Hint to Ornithologists.—Having been lately at a meeting of scientific friends, where the oft-repeated experiment of puncturing the small end of an egg and placing it in an exhausted receiver, was performed, to show the existence of the air globule at the other end, and to illustrate its elasticity, it suggested to me a neat mode of preparing eggs for collections, without the necessity of making a hole at both ends, for the purpose of blowing them in the ordinary manner.—The amount of air originally in the egg is not sufficient,—however well the receiver be exhausted,—to expel the whole of the contents; but if it be exhausted until a portion of the albumen fall from the egg, on the re-admission of air, a fresh portion will be drawn in. On again removing atmospheric pressure, a much larger quantity of the contents will escape, nd, by a third or fourth repetition, the whole of the contents of the egg will be evacuated. By reversing the experiment, a little water may be drawn in to wash the inside of the shell, and this may be again removed by the former process. By these means eggs may be prepared for museums without any further disfigurement than a needle puncture at one end, and that so small as to be scarcely perceptible.—*T. Bell Salter.*—*Ryde, Isle of Wight.*—*January* 21*st*, 1840.

Alyssum calycinum near London.—Whilst engaged in looking through the parcels of British plants received this year, for the annual distribution of the Botanical Society of the London, I was not a little surprised to find in the parcel received from Mr. Isaac Brown, of Hitchin, Herts, nearly one hundred specimens of this rare British plant, found by him in May, 1839, near Hitchin Common, Herts. I am thus anxious to make known this circumstance to metropolitan botanists, as I believe it has never been observed within so short a distance of London before, and must be looked upon as one of our rarest British plants.—*Daniel Cooper, Surgeon, Curator B. S. L., &c*—16, *Great James Street, Bedford Row.*—*January* 3*rd*, 1840.

THE MAGAZINE

OF

NATURAL HISTORY.

MARCH, 1840.

Art. I.—*View of the Fauna of Brazil, anterior to the last Geological Revolution.* By Dr. Lund.[1]

(*Continued from page 57*).

Next to the bats and rodents the most important family with reference to the abundant relics that one species has left in the caves, is that of the Marsupials: and of these the *Didelphis murinus* has been already alluded to, in specifying the contents of the heaps of bones introduced by the owls.

Of the family of *Pachydermata*, there is one genus in Brazil (*Dicotyles*), the species of which both habitually frequent caves, and also use them as places of refuge from their enemies. I have accordingly remarked their traces in many caverns, and have even followed them far into their dark passages; whereas I have only very rarely met with their bones there. I have in vain sought for either remains or foot-prints of the tapir, whence I conclude that it does not take refuge in caves.

Of Ruminants there is also only one genus in Brazil, that of *Cervus*, which contains five species, whereof one, of the size of the musk, is undescribed. This animal does not penetrate into the caverns; but when their entrances form a spacious chamber, its footsteps may be frequently seen there. Only in Lappa Nova de Maquiné have I discovered the remains of a single individual of that genus (*Cervus rufus*), and in my account of that cave I have already endeavoured to explain the circumstances of its discovery. I will only add here that subsequent experience has confirmed me in the opinion I there expressed. I have since observed in many caves that lay near to the habitations of man, the uninjured skeletons of several domestic animals, especially oxen and goats, the exa-

[1] Translated from the Danish, and communicated by the Rev. W. Bilton, F. E. S.

mination of which clearly explained to me the mode of their introduction; namely, that these creatures, whose footsteps I had often traced in the mouth of the caverns, had undoubtedly visited them for the purpose of licking the saltpetre, of which the crystals cover the surface of the floor; and that they had lost themselves in the labyrinthine passages.

Of predatory animals there is no existing species that makes caves its habitual dwelling-place. I certainly have remarked an abundance of the foot-prints of the Couguar (*Felis concolor*), not only at the entrance, but a long way in the interior of caverns; but I imagine it only enters them when following the track of the wild hog or the paca. That it does not use the caves as fixed dens, I presume from the fact that I have never found the animal itself, nor the remains of its meals there, while I have often had opportunities of seeing them in the woods, even in the immediate vicinity of caves.

The two remaining orders, the *Edentata* and *Quadrumana*, do not enter into the question here, forasmuch as I have not hitherto found the slightest trace of them in the Brazilian caverns. However, the imperfect state of our knowledge respecting the first of these orders, together with the important part they have played here in a former age, compel me to take a cursory survey of the species of this class which now inhabit this part of the New World. It is divided into the three natural families of the armadilloes, the ant-bears, and the sloths. I shall commence with the most numerous, and the most difficult, the armadilloes.

The unfortunate idea of founding the determination and nomenclature of the species of this family on the different number of the bands, has introduced such confusion into their natural history, that most later authors have recognized the necessity of beginning *de novo* the systematic arrangement of these animals. In consequence of these attempts some of the previous confusion has been removed; but at the same time new errors, in my opinion, threaten to creep in, as long as naturalists will continue to determine these very variable creatures, from individuals kept in collections. In this district of Brazil there are found four species of this family; of which by far the most abundant is the *Dasypus longicaudus*, Pr. Max. On account of its universal occurrence it is called by the Brazilians, the "Tatu-verdadeiro," *i. e.* the true or proper Tatu; which indeed is nothing more than a translation of its Indian name, *Tatu-eté*, which has the same signification.—Marcgraaf has given a very good description of this species, and under its right names, Tatu-eté, Tatu-verdadeiro (p.231), but by a blunder of the binder, the wood engraving representing this species (which by the way is not original, but borrowed from Clusius, and very poor), is attached to the description of Tatu-peba (*Dasypus gilvipes*, Ill.). This blunder has caused Cuvier to refer Marcgraaf's Tatu-peba to the one we are considering; an error which has even been introduced into the system by subsequent French zoologists, by the elevation of the Indian name into the systematic specific appellation of the species; so that by Desmarest it is styled *Dasypus peba;* by F. Cuvier, *Tatusia peba!* Moreover, it is identical with the *Dasypus octocinctus* of Linnæus; with Buffon's *Cachicame*, *Tatu-eté*, and *Tatou à longue queue;* as also with Azara's *Tatu negro*. Much less common are the two next species, *Dasypus gilvipes*, Ill. and *Dasypus gymnurus*, Ill. The first, as I have said, is called *Tatu-peba* by the Brazilians, and is very well described by Marcgraaf, pa. 231. It is the *Dasypus sexcinctus* and *Das. octodecemcinctus* of Linnæus; Buffon's *Encoubert* and *Cirquinson;* and Azara's *Tatu-poyu*. Illiger's name is a translation of the Indian one, which signifies "the Tatu with the yellow foot;" and should be preferred to the later titles proposed by Desmarest, of *Dasypus Encoubert*, or *Das. setosus* of the Prince of Neuwied. The other, *Das. gymnurus*, Ill., is called by the Brazilians "Tatu de rabo molle;" that is, the Tatu with the soft tail. It is the *Dasypus unicinctus* of Linnæus; Buffon's *Cabassou propre;* and Azara's *Tatu-ay*. This Indian title has been less happily advanced to the systematic specific name, in the *Das. Tatu-ay* of Desmarest; and *Tatusia Tatu-ay* of F. Cuvier.[1] The largest existing species of this family, *Dasypus Gigas*, Cuv. (called by the Brazilians "Tatu canastra"), is much rarer than the three preceding, and has indeed been seen by few persons. I only know it from the description of the Brazilians; which is also the case with a fifth species, called "Tatu-bola" by the Brazilians, (*Dasypus tricinctus*, Linn.; *Tatu-apara*, Marc.; *Tatu-matocco*, Azara); which does not occur in this district, but serves to complete the list of the species of this family existing in the whole of Brazil.[2]

[1] Cuvier, in his 'Ossem. Foss.' and 'Règne Anim.' ascribes to his cabassou, a tail provided with *scattered* scales; and applies to it the synonyms of Linnæus, Buffon, and Azara. At the same time he describes, as a distinct species, one with a *naked* tail, only armed with scales underneath, near the end. How far such a specific distinction has any real foundation in nature, I dare not determine: I can only affirm that the species which occurs here, has a tail entirely naked above, and more or less scaled underneath for the whole of the *hinder* half: and as Azara gives a similar description of the Paraguay species, his Tatu-ay, as well as the Tatu de rabo molle, may be classed among the last, instead of the first two of these species.

[2] Cuvier indeed speaks of a sixth species brought home by A. de St. Hi-

The second family, the Ant-bears, contains two species occurring in this district; namely, *Myrmecophaga jubata*, Linn., and *Myr. tamandua*, Cuv., the latter of which is very common, while the first is but seldom met with.

The third family, Sloths, is not found here. It contains three known species, which are confined to the continuous belt of forest that covers the coast-line of Brazil, and the banks of its larger rivers; in the interior highlands they are never seen.

Thus we see that seven species of the order *Edentata* at present inhabit this district; one of which, namely, *Dasypus longicaudus*, is extraordinarily abundant: three, namely, *Das. gymnurus*, *Das. gilvipes*, and *Myrmecophaga Tamandua*, are tolerably common: one, namely, *Myrmecophaga jubata*, is very rare: and finally, the seventh species, namely, *Dasypus Gigas*, is so scarce that it has been seen but by very few. A much more extensive variety of species in this remarkable order of animals inhabited Brazil, in that remote period of which I shall now proceed to treat; and they have left remains which, as records of the extraordinary conformation, comparatively clumsy make, and gigantic size of the animals when living, must excite our wonder and admiration.

PART II.

HAVING in my previous communication detailed at length the circumstances under which the fossil bones occur in the Brazilian caves, I shall now pass at once to a particular description of the species of *Mammalia* which they include.

First Order, *BRUTA*.

[In my former paper I adopted Cuvier's name for this order, *Edentata*, or teethless animals: but with all my reverence for the great man from whom this nomenclature proceeds, and in spite of its almost universal adoption, I cannot any longer conceal the serious objections which the use of so improper an appellation has ever excited in me; since out of the great number of species contained in this order, two only are without teeth. Neither can we admit Oken's change of the name

laire, under the name of Tatu verdade, but we have already seen that this is the name of *Dasypus longicaudus;* and as this animal varies exceedingly, it may be easily considered as only a variety of this species. Azara's Tatu-mulita, Tatu-velu, and Tatu-piehy, do not belong to the class of tropical animals, as they are only found to the south of the tropics, never in Brazil.

to that of *Zahnarm*, or "ill-provided with teeth;" because we not only find in this very Order, the greatest number of teeth yet known in mammals (*Dasypus Gigas* having ninety-six), but also every variety of teeth; namely, molars in all, except *Myrmecophaga* and *Manis;* canines in *Choloepus* and *Bradypus;* incisors in *Euphractus* and *Chlamydotherium.* If we are determined to give this order a name, taken from the dental system, it cannot possibly be derived from any other peculiarity than the simple structure of the teeth; and we might in that case call this order, *Simplicidentata.* But I much prefer Linnæus' name, *Bruta*, which he has characterized by the absence of incisors; notwithstanding that he was compelled to admit some forms not strictly belonging to the order, from the indeterminateness of this character. In addition to the authority and antiquity of this appellation, it has the further recommendation of signifying the imperfect organization of the animals. I translate it by the term Sloths, or slow animals; and I think it scarcely possible to find in their structure or habits any point more characteristic of the whole order, than that signified by this title. For the first family of this order which we have to consider, I have hitherto followed Illiger's name, *Vermilinguia;* which, with him, likewise includes the genus *Orycteropus.* But further considerations, connected with my discoveries in the fossil Fauna, having convinced me of the correctness of Cuvier's views, in placing this animal among the armadilloes, Illiger's term becomes inapplicable, particularly as another genus (*Echidna*), also possessing the same description of tongue, must be excluded from this family. However, forasmuch as the genera belonging to this family are the only mammals entirely without teeth, or any substitute for them, I think it impossible to employ for them any name more applicable than that of *Edentata*, or teethless animals. For further details on this subject, I beg to refer to a paper in the 'Monthly Journal of Literature' for 1832, &c.]

First Family, *Edentata*, Teethless.

This family consists at present of two principal genera, *Manis* and *Myrmecophaga:* the former of which is confined to the Old, the latter to the New, World. Among the fossil remains here, I have discovered traces of the last-named genus, which are however too imperfect to enable us to determine more accurately its relation to existing species. The fragments indicate an animal of the size of an ox; wherefore I propose for it the temporary name, *Myrmecophaga gigantea.*

It is well known that the remains of a gigantic species of *Manis* have been discovered in the temperate regions of the Old World: and we thus see that this family of animals existed in former ages under the same geographical divisions as now, but under gigantic forms.

Second Family, *Effodientia.*

Insignificant as are the traces of the last family hitherto discovered by me, proportionably numerous are the remains that I possess of the next, the armadilloes. This family is also now similarly abundant in species, arranged in several groups, which are so distinguished from each other, not merely in their dental system, but also in their entire internal structure, that I cannot but agree with the views of those later zoologists, who have raised these long-acknowledged groups to the rank of genera; which have at least a far better foundation than a large proportion of the genera established among the *Feræ*, *Rodentia*, and *Quadrumana*, but especially in the *Ruminantia*. This generic division is still more indispensible for many of the extinct species of this family, which, in very essential points differ from those now existing; while others, which I shall first examine, show remarkable coincidences with them.

The only species of the genus *Dasypus* (Wagl.) that occurs here, is the one described by Cuvier (Recherches, vol. i. pa. 118) as brought over from Brazil by M. A. de St. Hilaire, under the name of Tatu-verdadeiro, and which he declares to be a new species,[1] but for which I propose the name *Das.*

[1] Should this opinion of Cuvier prove correct, as I confess later examinations lead me to believe it will, then the name of *Dasypus longicaudus*, which the Prince of Neuwied, without any further description, has given to the species of this genus observed by him, certainly must not be applied to this, which is precisely distinguished from *Dasypus novemcinctus* by its shorter tail. On the contrary, I should consider it to be the *Das. octocinctus* of Linnæus, for after examining a great number of individuals, of all ages, the normal number of bands in this species seems constantly to be eight. I also consider it from the description, and especially from the number of teeth given, to be identical with the species mentioned by Azara and Rengger, as belonging to Paraguay, (Tatu-hu or Tatu-noire). If these suppositions be confirmed, of which I entertain little doubt, then the three known species of this genus will form a very remarkable transition, in respect both of their geographical distribution, and of their size, number of bands, length of tail, &c. For the largest species (*Das. novemcinctus*, Lin. *Das. longicaudus*, Pr. Max.), with seven molars, nine bands, a tail of the same length as the body, inhabits Cayenne and the northernmost parts of Brazil; the middle-sized species (*Das. octocinctus*, Lin. *Das. uroceras*, M.), with eight molars, eight bands, and a tail somewhat shorter than the body,

uroceras, on account of the horny kind of sheath which encloses the end of its tail. Among the fossil remains of this family, I find traces of a species of an existing genus which, in size, corresponds exactly with the living species; but as all the species of this genus resemble each other very closely in their internal conformation, I dare not yet quite decide upon the relation between the fossil and the existing species.

Of the genera *Tolypeutes* (Illiger) and *Priodon* (F. Cuv.) I have hitherto discovered no traces among my fossils. At the same time I find several bones of species of Tatu, of which some resemble the corresponding bones of the genus *Euphractus* of Wagler, (*Dasypus*, Fr. Cuv., *Les Encouberts*, G. Cuvier); while others agree more with those of the genus *Xenurus*, Wagler, (*Les Cabassous propres* Cuv.); and others again most resemble those of the proper *Dasypus* genus.—Without doubt these bones belong to several different species, all of considerable size; but as I am not yet able to speak of them with certainty, I will leave them for the present, in order to pass on to others, of which I possess either more characteristic or more perfect remains.

Of the first of the two genera I am about to describe, I possess indeed only portions of the dental system; but they are so characteristic as to be abundantly sufficient, in and by themselves, to indicate the extinct generic forms. The first genus I for the present call *Euryodon*. And before proceeding to describe it I would remark, once for all, that the generic names which I have found myself obliged to apply to animals of whose organization I possess but few remains, are only to be considered as provisional. For although the little that I do possess of them is amply sufficient to determine their independence as a genus, it is yet very possible that the most essential or the most distinctive characters may exist precisely in those parts of the skeleton which I have not yet found. However, it was absolutely necessary to give these new generic forms at least provisional names, for the sake of brevity and clearness in the following general descriptions. The teeth of *Euryodon* are distinguished from those of all existing armadilloes in this respect, that they are compressed from the front, backwards, while the others are more or less laterally compressed; for the rest, the grinding surface, as in

frequents central Brazil and Paraguay; while the least of all (*Das. septemcinctus*, Linn., *Das. hybridus*, Desm.), with seven molars, seven belts, and a tail much shorter than the body, occurs only in the extra-tropical part of South America.

the existing species, consists of two faces, which meet at an acute angle in a transverse ridge.

If the bones which I have found associated with these teeth belong to the same animal, they seem to attest a size approaching that of *Priodon giganteus*.

The second of these genera I call *Heterodon*, because its teeth exhibit a much greater want of conformity amongst each other, both as to their shape and size, than is the case in the existing armadilloes. Both the anterior and posterior molars are small and conical; while the penultimate and antepenultimate are much larger, the section of the former being oval and of the latter heart-shaped. The single species of this genus possessed by me, is of the same size as the commonest recent armadillo, *Dasypus uroceras*.

(To be continued).

Art. II.—*Notes on Irish Natural History, more especially Ferns.* By Edward Newman, Esq., F.L.S., &c.

(Continued from page 75).

In the ascent from the lakes of Killarney towards the police-station on the Kenmare road, I turned a little out of my way to see the fall of Derrycunhuey. It is very different in character from O'Sullivan's or Turk, being of greater breadth and volume but of less height, and having in its channel vast masses of lichen-stained rock. I never saw such a profusion of *Hymenophyllum* as grows on the stones around this waterfall: the two species were completely intermixed. I spent an hour searching every dark hole for *Trichomanes*, but without success. From the police-station the view over the lakes is very celebrated; but I think they lose a great part of their beauty when seen from so great a height and distance. They become diminutive, and you see their sinuous outline and numerous islands as laid down in the maps, but the sweet variety of colour, and fantastic figure of rocks, which, when viewed more nearly, form so prominent a part of their attractions, are here entirely lost. I lingered to take a last farewell of those lovely lakes, and to smile on the legendary lore so highly prized by the dwellers amid this wild scenery: no mountain-summit, no shady cove, no rocky islet, but has its fairy tale. The lakes themselves are clearly traced to fairy origin, as the legend of Norah and Coolin abundantly sets forth. The wild country around the lakes admits of little farming; but the land north of Killarney is good, and lets well, — 10s. and 12s. per Irish acre. It principally belongs to Lords Headley and Kenmare, both highly respected by their tenantry. One of those touching testimonials, so characteristic of Ireland, was about to be offered to Lord Headley shortly after I left: — his tenants intended inviting him to a public dinner. The Irish landlords appear to be revered throughout Ireland, in a manner of which, in England, we can form no notion; a few, a very few, exceptions to this may be met with, in which political landlords have uniformly turned out the native residents to make room for tenantry more subservient to their views.

The road to Kenmare is over a bleak and wild mountainous country, but little cultivated. The outline of Macgillicuddy's Reeks, stretching far away to the right, I thought particularly fine. I found every possible form of *Polystichum aculeatum* growing among the rocks; also *Lastræa Filix-mas*, *Las. dilatata*, and *Athyrium Filix-fœmina.* Descending from the high ground towards Kenmare, I was struck with the great abundance and beauty of *Bartsia viscosa* and *Pinguicula grandiflora;* and, on the high ground, with the size and luxuriance of a species of *Euphorbia*, perhaps *Eu. Characias.* The land towards Kenmare belongs, I believe, almost exclusively to the Marquis of Lansdowne. The rent is various: some little farms, to which farm-houses are attached, fetch as much as 5s. or 6s. per Irish acre, and I heard of one farm as high as 9s., but it must be recollected that this price includes the house, which, on the Lansdowne property, is almost invariably good.

It is a fine wild walk over the hill from Kenmare to Glengarriff: the road has lately been cut at great expense and labour; it passes through tunnels of solid rock, the last of which, on the very crest of the hill, is of great length and very dark, notwithstanding a light-hole in the centre from above. On emerging from the last tunnel, the view southward begins to open; it is very extensive, and the outlines are rugged and extremely picturesque. The view varies with every step.— Bantry Bay, its coves, islands, and sinuous shores, and the wild hills rising above them, are spread map-like before you. It was from this descent that I saw that most noble of our native quadrupeds, the stag, browsing at a distance on the mountain-side. As I approached Glengarriff there seemed no end to the variety of form and colour in which rocks and woods were combined. Holly, arbutus, yew, birch and oak are the most abundant trees, and they seem to vie with each other in hardihood: they may be continually seen rooted in the scarcely perceptible clefts of a perpendicular rock, and throwing their fantastic branches over a river brawling at its base. The rock itself is so beautiful, and the streams are fringed with such magnificent *Osmunda*, that even Killarney would gain but little by a rigid comparison with Glengarriff. The rabbits and hares which swarm in this neighbourhood afford ample entertainment for foxes; and I am led to believe that these animals are more numerous here than in any other part of the kingdom. Hunting, in a country where the foxes could go to earth at least once in every hundred yards, is out of the question; and it is consequently held fair to trap and slaughter them by wholesale. Otters abound, and are very destructive to the fish. The golden eagle breeds in Hungry Hill, the Eagle's Nest, and several other hills on the west side of Bantry Bay: hawks are most abundant; ravens, crows, rooks, jackdaws, and magpies, occur in profusion. Hooded crows and choughs are known here, but are not, as in some localities I have mentioned, the most numerous birds of the crow tribe. I did not stay sufficiently long at Glengarriff to make anything like a list of the ferns; nor did I notice any species that I have not already mentioned as common throughout my journey.

At Lord Bantry's lodge I was extremely amused with the manifold trophies of defunct stags which are exhibited within and without the dwelling, and also with the singularly wild and picturesque character of his demesne. After wandering about for some time I returned to the shore, and throwing my knapsack into a boat, I took the helm, and in a few minutes four noble-looking fellows were rowing me down Bantry Bay. I steered first along the western coast, to see the eastern to greater advantage, but the hills are rounded and insignificant; I then crossed to Whiddy Island, a fertile and extremely well cultivated tract, three miles in length, and containing five hundred inhabitants: from this a fine view is obtained of the Caha Hills on the west coast. The sea was, for an Irish sea, extremely calm; the air perfectly still, and the sky cloudless: there was a rich saffron tint in the air, which seemed to invest every object with a kind of golden mantle: it appeared more like what I fancy an Asiatic than an Irish atmosphere. There was that quiet and balmy feel in the air which we call an indication of settled weather: I could scarcely believe that I was in a land where

> "Suns with doubtful gleam
> Weep while they rise."

After landing at Bantry I strolled over Lord Bearhaven's park and garden, and here I first found *Polypodium vulgare* in its very divided form, as discovered by Mr. Mackay in County Wicklow: it grows abundantly on the park-wall, just out of the town. The growth of some exotics in the garden is of extraordinary luxuriance: on the lawn before the house are some ten or twelve hydrangeas, the smallest of which is twenty long steps in circumference, and the one which I supposed the largest, thirty-three steps, or at least a hundred feet. Several species of *Erica*, and amongst them *Erica Mediterranea*, also several Fuschias, were of equally luxuriant proportions, and the laurel contended in vigour with the native arbutus.

I walked up the hill at the back of the house, through a herd of fallow deer, which seemed infected by the luxurious and enervating calm of the evening, and would hardly rise at my approach. The summit of this eminenee was covered with rooks and jackdaws; I think there must have been millions, they really blackened the sky when they rose, which they did with reluctance and soon settled again, some on the ground, and some on little clumps of young firs, which bent down with the unusual weight. This eminence commands a glorious view over the Bay, its islands, Glengarriff, the Caha Hills, &c., and the extraordinary fall of Adrigoil in Hungry Hill was just visible, like a thread of silvery light. This cascade appears little known; few, if any of our tourists have visited it, and I had never by any chance heard even its name until now, in its immediate neighbourhood: it is said to be 850 feet in height, which I think will exceed any other in the kingdom. I lingered till the last ray of the setting sun had vanished from the Caha Hills and the distant Reeks,—till the golden flame-tint had left the north-west, and was succeeded by a green indescribably beautiful; while the zenith, and indeed nearly all the other parts of the sky, were of that exquisite colour which I suppose the Latin poets mean by the word *purpureus*, as in

> "Largior hic campos æther et lumine vestit
> Purpureo;"

and many other passages. This colour rapidly faded, and as night was fast approaching I made the best of my way to Bantry.

There are few who happened to be in Ireland on the 30th of July, 1839, that will forget it. The flood-gates of heaven seemed opened, and earth appeared about to be visited by a second deluge: men, women, children, cows, sheep, goats, pigs, were swept from the face of the earth: nearly a hundred bridges were totally demolished, and watercourses,—foaming rivers,—flowed where none were known before. It began raining about midnight, little more than two hours after the glorious sunset I have described, and the water seemed to descend in streams rather than drops during the whole of the ensuing day.

From Bantry I went to Skibbereen: the stony and hilly country possessed but little that was interesting, and though farming was attempted, it was the most wretched attempt I had yet seen. *Osmunda*, *Athyrium Filix-fœmina*, and *Lastræa dilatata*, were abundant, but generally of stunted growth. Between Skibbereen and Rosscarberry are some small but picturesque lakes, celebrated for their trout-fishing; the gillaroo trout being taken in great abundance. Mr. Pennant treats of this as a variety of the common trout; but almost every fisherman in Ireland thinks differently, and the Rev. Mr. Maxwell also appears to treat it as distinct.[1] I cannot say much in favour of Mr. Maxwell's zoological attainments; his mistaking the eagle for the osprey, and gravely quoting Bewick's description of the osprey as an illustration of the eagle of Achill, is a remarkable instance of his ignorance in the ornithological branch of the subject; but with fish he is evidently more at home. The gillaroo trout differs principally from the common trout in the extreme hardness and gizzard-like structure of its stomach, a character that especially adapts it to the comminution of the testaceous *Mollusca* on which it feeds. It is usually twice the size of the common trout. It has been said that the gillaroo has only been found west of the Shannon, but this I am scarcely inclined to believe: I have indisputable authority for recording it as a native of the loughs near Skibbereen, which, though not to the east of the course of the Shannon, can nevertheless scarcely be said to be west of that river. The fish from Lough Neagh figured by Mr. Yarrell[2] has little resemblance in form to the gillaroo of the west, a fish which is more correctly represented by the rough cut in the 'Dublin Penny Journal.'[3]

Sir Humphrey Davy says that "the gillaroo trout differs in appearance very little from the common trout, except that they have more red spots, and a yellow or golden-coloured belly and fins, and are generally a broader and thicker fish; but internally they have a different organization, possessing a large, thick, muscular stomach, which has been improperly

[1] Wild Sports of the West, *passim*. [2] British Fishes, ii. 57.
[3] Dublin Penny Journal, i. 80.

compared to a fowl's, and which generally contains a quantity of small shell-fish of three or four kinds; and though in those I caught the stomachs were full of these shell-fish, yet they rose greedily to the fly. The common trouts of these lakes have stomachs like other trouts, which never, as far as my experience has gone, contain shell-fish; but of the gillaroo trout I have caught some, not larger than my finger, which have had as perfect a hard stomach as the larger ones, with the coats as thick in proportion, and the same shells within; so that this animal is at least *now* a distinct species, and is a sort of link between the trout and char, which has a stomach of the same kind with the gillaroo, but not quite so thick, and which feeds at the bottom in the same way. I have often looked in the lakes abroad for gillaroo trout, but never found one."[1]

From Rosscarberry to Cloghnakilty the country is pretty generally cultivated, and a good deal of wheat was in ear.—Near the town of Cloghnakilty I obtained a fine view down the bay of the same name. From thence I proceeded to Bandon. Near Innisshannon the country is picturesque, and the banks of the river Bandon are finely wooded. On approaching Cork I found the immediate neighbourhood well cultivated, and intersected with hedge-rows as in England. There are a great number of trees in the hedge-rows, and these were shrouded up the sides, a horrible practice, long sanctioned by fashion throughout England, but seldom observable in the sister island, where trees are too scarce and valuable to be treated in this ignominious and injurious manner.

I believe a tourist might write a good chapter on the town of Cork, its magnificent jail, its innumerable cars, its splendid horses, and its rapid river; but I will not treat on these topics farther than to say that it is a good-looking town, and contains 100,000 inhabitants. The river or creek below Cork is very pretty, the hilly banks being loaded with luxuriant shrubberies and tasty-looking houses. The town of Cove is prettily situated on an island called Great Island, and is about ten miles nearer the sea. Its houses are covered with a Mackintosh of slate to shoot off the rain, which is here nearly incessant. The view of Cork Harbour from the upper part of Cove town is very fine.

At low water, a considerable space of mud being left uncovered, I had an opportunity of observing the extreme boldness of the curlews, crows, and sea-gulls, which come in

[1] Salmonia.

great numbers close to the town of Cork, apparently attracted by some favourite food left by the falling tide. The ferns I observed in the immediate neighbourhood of Cork, were *Lomaria spicant*, rare; *Pteris aquilina*, rare; *Polypodium vulgare*, very common, and numerous palmated varieties; *Polystichum aculeatum*, *Lastræa Filix-mas* and *Las. dilatata*, *Athyrium Filix-fœmina*, *Asplenium Adiantum-nigrum*, *Aspl. Ruta-muraria*, *Aspl. Trichomanes*, *Scolopendrium vulgare*, *Ceterach officinarum*, and *Osmunda regalis;* and it should be remarked that these were observable merely in passing along the main thoroughfares, all of them except the two first, growing on the walls of the gardens and shrubberies.

From Cork I proceeded northwards, through Watergrass Hill and Rathcormuck, to Fermoy. The river Blackwater having, during the late inundation, swept away entire fields of hay, a vast quantity had been arrested in its progress by the bridge at Fermoy, and was now collected in large cocks in the town, presenting a curious spectacle. *Ceterach officinarum*, *Asplenium Adiantum-nigrum*, *Aspl. Ruta-muraria*, and *Aspl. Trichomanes*, and *Scolopendrium vulgare*, half-covered the walls between Cork and Fermoy. The country was apparently well farmed, but otherwise very uninteresting. From Fermoy to Mitchelstown the country is not so well farmed, still there is little, if any, out of cultivation. I travelled this distance, and also from Mitchelstown to Sheely's Inn, half way between that place and Cahir, by Bianconi, but I found it a loss rather than a gain of time, for several bridges had been totally destroyed, and temporary crossings of the most ticklish description served as their substitutes, often causing very tedious delays. The ferns which I observed by the way, in addition to the five lately mentioned, were *Lomaria spicant*, *Pteris aquilina*, both rare, and requiring a sharp look-out; *Athyrium Filix-fœmina*, *Lastræa dilatata*, and *Osmunda regalis*, in the usual abundance. Sheely's Inn is in the county of Tipperary, and midway between Mitchelstown and Cahir.

Long before I set foot in Ireland I had determined to visit the caves at this spot. They are usually known as the Kingston Caves, because situated on the property of Lord Kingston. Crossing the road from Sheely's Inn, you proceed up a narrow lane for about a mile, two low roundish hills rising before you, which are partially covered with a growth of shrubby wood. In each of these hills is a cave: that to the right is the cave of Sheheewrinky, and is also called the Old Cave, having been known for a great number of years; and that to the left the Cave of Coolnagarranroe, being situated in the town-land of that name, and is called the New Cave, having been unknown till the year 1833, when it was explored by a son of mine host, in company with a man named Condon, who first discovered its entrance while at work in the quarry. It is now only to be seen by applying to Gorman, the man who rents the ground, and who, having had the entrance secured by a door which is kept carefully locked, excludes all but those who pay handsomely for the entertainment. Throughout Ireland you either get served for nothing, or pay ten times as much as in England: there is no moderation. The Cave of Coolnagarranroe is now exclusively visited. The little hills containing these caves are described by a geologist[1] as lying "in the valley which separates the Galtee and Knockmildown chains of mountains, the former constituting its northern, the latter its southern boundary. The prevailing rock at this extremity of the Galtees is conglomerate, which occasionally passes into sandstone; while that which composes the opposite chain of hills possesses a structure intermediate between that of sandstone and schist, and includes few if any rounded or water-worn pebbles. The material of the interposed valley is compact grey limestone, and this rock forms two small rounded hills, &c." Although nothing of a geologist, I presume I may venture to assert that the interior of the cave presents nothing to the eye but the grey limestone before alluded to by Dr. Apjohn, carbonate of lime in the infinitely varied conformations of spar, a soft red clay which appeared partially to cover the floor of the cave, and to fill many of its crevices, and a fine light-coloured sand which is present in one chamber only.

Immediately on the door being opened I followed Gorman down a steep passage of some two or three dozen yards in length; after me came Gorman's son, and a "boy" who volunteered the office of assistant-assistant-guide, brought up the rear. Each of the four held a candle, and each proceeded in the position most agreeable to himself; the height of the passage being somewhat less than four feet, and our own heights varying between five feet and a half and six feet and a half. This passage was pleasantly varied about the middle by a perpendicular fall of five or six yards, and here a ladder was fixed. At its termination the footing becomes tolerably horizontal, and the aperture wider and higher, and this continues for about a hundred yards, when I was ushered into the "House of Commons," a splendid chamber, which

[1] Dr. Apjohn.

I should suppose fifty yards in length, twenty to twenty-five in breadth, and twelve or fourteen in height. Young Sheely and Gorham gave me the precise measurement, but not only did they not agree, but their statements considerably exceeded my estimate of the dimensions; therefore I prefer my own, which is deduced from steps, when a question of length and breadth, and from guess, when one of height. The roof is adorned with an infinity of icicle-like stalactites, and the fissures in the limestone are encrusted with glittering spar: the floor is almost without a trace of stalagmitic deposition, but in several places I observed the huge blocks of limestone of which it is composed deeply pitted with the dropping of water from the roof.

I was now led through a passage of perhaps ten or fifteen yards in length, and of considerable breadth and height, into the "House of Lords." My childhood's dreams of the Grotto of Antiparos were here completely realized. I felt that to see this alone I would gladly have crossed the channel. I know not how to describe it. Suppose a room a hundred yards long, thirty yards wide, and ten yards high, — these are not ascertained dimensions, — suppose the roof beautifully arched as in Gothic halls, and that arched roof hung all over with icicles, and suppose some dozen or so of these icicles of vast size, hanging down till they reach incrusted masses of ice rising from the floor, and so become graceful pillars supporting that vaulted roof; and then you may form some idea of the extreme beauty of this fairy chamber. One huge pillar is called the "Tower of Babel;" and a mass of spar, where the water containing the carbonate appears to have fallen on a projecting rock, and so been compelled to trickle in various directions, thus forming a multitudinous mass of conglomerated stalactites, is called the "Turkish Tent." A third vast aggregate of spar is called "the Beehive," and a fourth "the Organ." The similarity of these beautiful masses of spar to the objects whose names they bear, is very obscure; in the instance of the organ, however, the pendant stalactites do really in some degree resemble the pipes of an organ, and Gorman and his associates followed each other in playing a voluntary on these pipes, by flourishing their "sprigs" along them, and thereby producing a sound by no means unpleasant. Between these are many large accumulations of spar, all carefully named, but alas! I have forgotten the nomenclature; and almost every part of the floor is covered by stalagmitic incrustations, which rise opposite to the stalactites pendant from the roof. In a few instances, however, I observed that the continued dripping from the roof, instead of causing a stalagmitic deposition, had worn small circular holes in the limestone, as in the "House of Commons."

Leaving this splendid chamber Gorman led me into what he called the "Long Cave:" the footing here was very bad; little was obtained to pay for the trouble, for whichever way we turned we soon came to a part which Gorman told me was unexplored, and into which he did not choose to take me. I think there were seven or eight branches of this "Long Cave," which he said had never been visited, but the parts through which I wandered must have measured nearly an English mile. The floor sometimes consisted of the naked limestone, sometimes of clay, and sometimes spar, the latter being rugose and angular, and not possessing a smooth surface like that of the stalagmitic deposition to which I have before alluded. The roof occasionally exhibited festoons of spar, resembling wreaths of flowers, and struck me as very beautiful; but still I was disappointed with this "Long Cave," because I fancied that I left so much unseen: and when Gorman repeatedly told me that that was as far as we could go, although I saw a very obvious opening still farther, I could not help repeating the indignant and emphatic enquiry of the renowned Miss Squeers,—"Is *this* the *h*end?"

There is a second opening from the "House of Lords" leading, as Gorman assured me, to a river, but this river I could not find: there was, indeed, a puddle, and nothing more: I have since turned with some interest to see what Dr. Apjohn makes of the river, but I find he received it on trust, "such phenomenon was not *at the time* to be observed." I thence returned to the "House of Lords," entering that wondrous place for the third and last time, and thence to the "House of Commons" for the second time. Three passages emanate from this chamber, besides that by which I entered it, and the one leading to the "House of Lords." One of these possesses little interest, except that its extent is unascertained: the two others are but different ways into another chamber, which is divided into two compartments by a rock running along the middle: this chamber has some enormous stalactitic pillars, and one little recess called the "Queen's Bedchamber," which is exquisitely beautiful, every part of it being covered with an incrustation of the most sparkling spar, in great variety of form.

From the "Bedchamber Cave" (I believe this is the right name) I was led towards another passage, near the entrance of which stand some enormous masses of stalagmite, and one larger than the rest is called "Lot's Wife."[1] This passage leads into the "Kingston Gallery," a straight and narrow chamber or gallery sixty yards long. The roof and walls, throughout its entire length, are clothed with spar, which I remarked here assumes a variety of colours, sometimes bright red, sometimes barley-sugar-coloured. The beautiful roof possesses more architectural symmetry than that of any of the other chambers, and the pendant festoons of spar, resembling wreaths of flowers and flowing drapery, are most gorgeous. At two thirds of its length this gallery was originally completely divided by a transparent curtain of spar, but through the centre of this an aperture has been made, and the entire chamber has been thus rendered visible at one view, partially interrupted however by six stalactitic pillars. The "Kingston Gallery" leads into a chamber which is nearly square, and without much ornamental spar; and beyond this is another passage or gallery, which runs for more than a hundred yards in a continuous line with the "Kingston Gallery," and of which the termination has not yet been found, so that it is compulsory to return to the square chamber, from which is a passage parallel with the "Kingston Gallery," and of somewhat similar width and precisely similar length. The bottom of this passage is strewed with sand, and it is consequently called the "Sand Cave;" it contains little or nothing to attract admiration after the eyes have been feasting on the gorgeous beauties of the "Kingston Gallery."

The two galleries open side by side, and within a few yards of "Lot's Wife" already noticed, and immediately adjacent to another chamber called the "Garret Cave," which appeared to me more extensive than either of the others. I fancy it is considerably more than a hundred yards in length, and it varies greatly in breadth. The floor is composed of stalagmitic masses and incrustations, and blocks of limestone rudely tossed together, the travelling over which is not very convenient; it rises towards the farther extremity, thus reducing the height in that part. The walls are mostly sheeted with spar, and the stalactites, like glittering icicles, and often of very small size, hang by thousands from the roof; some however are of noble size, and having united with the stalagmite, form graceful pillars seemingly created purposely for the support of the roof.

[1] This has been described as *stalactitic*, but I must allow my original note to stand, as it was made on the spot; and though I will not lay claim to infallibility, especially in a science in which I am a confessed ignoramus, yet my impression was and is, that it was the deposition from dropping, not from trickling; that it had grown upwards, not downwards.

It will I think be seen from this description, imperfect as I acknowledge it to be, that the cave of Coolnagarranroe is not, as generally supposed, a single opening of ascertained size and figure, but more resembles in its ramifications a vast mine, in which large excavations have occasionally been made, and of which no less than thirteen or fourteen obvious openings yet remain to be explored, offering a fine field, not merely for adventure but for theory, for who shall say what wonders are yet undiscovered. I was perhaps rather lazy after slipping about for seven or eight hours on the soft and slimy clay, and little inclined to prosecute further researches at the time, but I have often since regretted that I did not stimulate Gorman to further exertions while I was present, for he receives such a revenue from the fleecing of his present visitors, that he will never take the trouble by himself to make more discoveries. It is strange that those who manage the estate on behalf of the proprietor do not, in some way, restrain the impositions of this greedy man, for the cave being no part of his taking, he can only by courtesy be allowed the pecuniary advantage of showing it.

I have not in this rapid sketch noticed a tenth part of the curious forms of spar to which my attention was called; among these the "Churn," the "Angel's Head," the "Avalanche," the "Lord Chancellor's Wig," and "Aladdin's Lamp," struck me as remarkably beautiful; the last is a pendant and somewhat cylindrical sheet of spar, so thin that the light of a candle is scarcely decreased by being placed inside it. Perhaps however the most magnificent mass of spar, either here, or known in the world, is that called the "Queen's Mantle." Its appearance is somewhat similar to that which might be produced by throwing a dozen shawls carelessly over a pole suspended horizontally, their folds being allowed to arrange themselves at random, as they hung from the pole in a confused mass; the height of this mass is twenty or twenty-five feet, and the substance so thin as to show very clearly the flame of a candle held behind it: its extremities do not quite reach the floor. Owing to the frequent disturbance from handling which this and all the most remarkable objects are constantly undergoing from visitors, most of whom purchase the right of chipping off and carrying away what they please, it is much to be feared that the cave will eventually be robbed of a great portion of its present beauty.

I did not leave the cave without examining the clay and sand deposited in cavities uuder the stalagmitic incrustations of the floor, in the hope of finding the bones of extinct animals; but it was to no purpose, and I learned from Gorman that similar searches had frequently been made, but always without success. This leads me to believe in the correctness of Dr. Buckland's theory, that the cave at Kirkdale was the actual residence of the hyænas whose bones it contained, and that these animals were instrumental in bringing thither those other animals, of which the bones are mingled with their own.

On my return to Sheely's Inn the rain was so heavy that I determined to stay there for the night, an example which I can scarcely recommend any of my compatriots to follow, except those who, like myself, are willing to seek information at the expense of comfort. The landlord favoured me with his company, as did his son who first explored the cave, and of course *the cave* was the principal topic of conversation. I found Sheeley a remarkably intelligent man; he possessed a knowledge of the politics of the day which quite put me to the blush, for I had not read a newspaper, to the best of my belief, for full six weeks previously to my visit to him. On the subject of the cave I learned some interesting particulars, more especially in relation to the frequent visits paid them by Lord Kingsborough. He showed me a variety of spars, and explained them very scientifically, expressing his regret that the spar should be wantonly broken and carried away by visitors. Some of my countrymen, in their zeal for science, had thrown large stones where the stalactites were most thickly clustered, and thus produced showers of spar, a phenomenon readily obtained, for the slender stalactites break almost as readily as icicles.

Sheeley told me that the first time Gorman entered the cave with two or three companions, their candles accidentally went out and left them in utter darkness. They had advanced so far that their cries were inaudible above ground, and they sat down, expecting nothing better than death by starvation. Gorman having been missed from his home, his son luckily thought of the cave, and descending with a light restored his father to the upper world.

(To be continued).

ART. III.—*Sketch of the Flora of the neighbourhood of Ipswich: including the Phænogamic Plants, the Filices, and Equisetaceæ.* By WILLIAM BARNARD CLARKE, M.D., F.B.S.Ed.

IPSWICH, the county-town of Suffolk, is situated about seventy miles from London, on the road through Romford, Chelmsford, and Colchester, at the confluence of the rivers Orwell and Gipping. The Orwell commences at the harbour, between Landguard Fort on the Suffolk side, and Harwich on the Essex side, as also does the Manningtree river the Stour. Leaving the Stour, the Orwell takes a gradually winding course for about twelve miles, when it passes the gentle declivity upon which the town of Ipswich is situated. Nearly half a mile beyond this it receives the waters of the Gipping, at the lock which is considered the boundary between the two: beyond which the Orwell is continued under the latter name, as far as the town of Stowmarket. The margins of these rivers afford a variety of plants; several maritime species are met with in the salt marshes on the banks of the Orwell, whilst the rich borders of the Gipping present the botanist with a greater number of species. The geological character of the country is various: chalk and clay extend over a considerable portion of the district, and the strata of sand and comminuted shells (provincially called "crag") cover most of the south-eastern part, whilst another portion consists of gravel and siliceous sand. There is also in the neighbourhood a considerable extent of heath on the eastern side. The elevation of the higher parts of the district is from about sixty to one hundred feet; and the country is exceedingly rich and well cultivated, beautifully undulating, and contains much wood. The plants growing in the neighbourhood are the following, which I shall group together according to the natural system of Lindley. There are about 471 phænogamic plants, 12 *Filices*, and 4 *Equisetaceæ*.

DICOTYLEDONES.

RANUNCULACEÆ.

CLEMATIS *Vitalba*. Common in hedge-rows.
THALICTRUM *flavum*. In various parts of the borders of the Gipping.
ANEMONE *nemorosa*. Common in the woods.
MYOSURUS *minimus*. Rare in marshes near the Gipping.
RANUNCULUS *Flammula*. Boggy parts of Norton Heath, not uncommon.
———— *auricomus*. Woods; rather local.
———— *sceleratus*.
———— *bulbosus*.
———— *repens*.
———— *acris*. } Marshes, not uncommon.
———— *hederaceus*. Watery places, local.
———— *aquatilis*. Ditches in several localities, common.
FICARIA *verna*. (*Ranunculus Ficaria*). Moist banks: side of the Gipping, common.
CALTHA *palustris*. Marshes, very common.

NYMPHÆACEÆ.

NYMPHÆA *alba.* Ponds at Holbrook.
NUPHAR *lutea.* River Gipping, very common.

PAPAVERACEÆ.

PAPAVER *Argemone.* Fields near Freston, common.
——— *dubium.* Cornfields.
——— *Rhœas.* Cornfields, common.
GLAUCIUM *luteum.* Sea-coast at Walton and Felixtow, ten or twelve miles from Ipswich, extremely common.
CHELIDONIUM *majus.* Roadsides in several places, but not common.

FUMARIACEÆ.

FUMARIA *officinalis.*
——— *parviflora.*
——— *capreolata.*
} Field-sides in several places.

CRUCIFERÆ.

CAKILE *maritima.* Sea-shore at Walton and Felixtow.
THLASPI *arvense.* Road- and field-sides, apart from gardens.
CAPSELLA *Bursa-pastoris.* Common everywhere.
TEESDALIA *nudicaulis.* Gravelly fields, local.
LEPIDIUM *latifolium.* Banks of the Orwell, common.
——— *ruderale.* Banks of the Orwell, local.
COCHLEARIA *officinalis.* Marshes near the Orwell.
DRABA *verna.* Borders of fields &c., common.
CARDAMINE *hirsuta.* Road-sides, local.
——— *pratensis.* Meadows, common.
——— *amara.* Side of Gipping, not common.
ARABIS *Thaliana.* Field-sides.
BARBAREA *vulgaris.* Moist meadow-banks.
NASTURTIUM *officinale.* Ditches, common.
ERYSIMUM *Alliaria.* Road-sides.
——— *cheiranthoides.* Road-sides.
BRASSICA *Rapa.* Field-sides.
SINAPIS *arvensis.* Fields, common.
RAPHANUS *Raphanistrum.* Field sides, common.

RESEDACEÆ.

RESEDA *lutea.* Field- and road-sides on a chalky soil, local.
——— *Luteola.* Field- and road-sides on a chalky soil, common.

VIOLACEÆ.

VIOLA *odorata.*
——— *canina.*
} Woods and groves, common.
——— *tricolor.* Fields and waste ground, common.

POLYGALACEÆ.

POLYGALA *vulgaris.* Heaths and woods, common.

SILENACEÆ.

SAPONARIA *officinalis.* Road-sides, local, apparently from gardens.
SILENE *inflata.* Road- and field-sides.
——— *maritima.* Fields near the coast, Walton.

SILENE *noctiflora*. Fields, very local.
AGROSTEMMA *Githago*. Corn-fields, common.
LYCHNIS *dioica*. Road-sides &c., common.
——— *Flos-cuculi*. Meadows and pastures, common.

ALSINACEÆ.

STELLARIA *holostea*. Field- and road-sides, common.
——— *graminea*. Road-sides and woods, rather local.
——— *uliginosa*. Moist woods, not uncommon.
——— *media*. Everywhere.
ARENARIA *peploides*. Sides of the Orwell in several places.
——— *trinervis*. Shady lanes, local.
——— *serpyllifolia*. Rubbish, old walls, &c., common.
CERASTIUM *vulgatum*. } Fields and road-sides.
——— *viscosum*. }
——— *aquaticum*. Moist hedges, local.

TAMARICACEÆ.

TAMARIX *gallica*. Walton and Felixtow, ten or twelve miles from Ipswich, apparently introduced.

ILLECEBRACEÆ.

SPERGULA *arvensis*. Fields and uncultivated ground, common.

LINACEÆ.

LINUM *usitatissimum*. Fields, local.
——— *catharticum*. Chalky fields, common.

MALVACEÆ.

ALTHÆA *officinalis*. Sides of ditches near Walton, common.
MALVA *sylvestris*. Field- and road-sides, common.
——— *moschata*. Road-sides, local.

TILIACEÆ.

TILIA *europæa*. Plantations, &c.

HYPERICACEÆ.

HYPERICUM *quadrangulum*. Side of the Gipping &c., common.
——— *perforatum*. Road-sides, not uncommon.
——— *humifusum*, Road-sides, local.

ACERACEÆ.

ACER *campestre*. Hedge-rows, common.
——— *Pseudo-platanus*. Woods, &c.

GERANIACEÆ.

GERANIUM *sylvaticum*. Road-sides, local.
——— *robertianum*. Road-sides, common.
——— *lucidum*. Woods, local.
——— *molle*. Road-sides, common.
——— *pusillum*. } Road-sides, fields, &c., common.
——— *dissectum*. }
ERODIUM *cicutarium*. Fields and barren places, common.

OXALIDACEÆ.

OXALIS *Acetosella.* Woods, local.

CELASTRACEÆ.

EUONYMUS *europæus.* Woods and hedge-rows.

AQUIFOLIACEÆ.

ILEX *Aquifolium.* Woods and hedge-rows, common.

LEGUMINOSÆ OR FABACEÆ.

ULEX *europæus.* Heaths, common.
ONONIS *arvensis.* Fields at Walton, common.
ANTHYLLIS *vulneraria.* Chalky meadows.
TRIFOLIUM *repens.* } Fields, common.
——— *pratense.* }
——— *arvense.* }
——— *fragiferum.* Plentiful in several pastures.
——— *procumbens.* Road-sides, common.
LOTUS *corniculatus.* Pastures, common.
ORNITHOPUS *perpusillus.* Heaths, local.
VICIA *sylvatica.* Hedges.
—— *sativa.* Field-sides.
—— *sepium.* Field-sides and hedges.
ERVUM *tetraspermum.* Road-sides, local.
——— *hirsutum.* Road-sides, local.
LATHYRUS *Nissolia.* Fields, very local.
——— *pratensis.* Field-sides, common.
——— *palustris.* Moist meadows and woods, not uncommon.

ROSACEÆ.

SPIRÆA *Ulmaria.* Sides of the Gipping, &c., common.
——— *Filipendula.* Woods, local.
GEUM *urbanum.* Woods and field-sides, common.
RUBUS *cæsius.* Hedges.
———*fruticosus.* Hedges, common.
FRAGARIA *vesca.* Woods, common.
TORMENTILLA *officinalis.* Heaths, common.
POTENTILLA *anserina.* Fields; sides of the Gipping, common.
——— *argentea.* Road-sides.
——— *verna.* Sandy fields, rather local.
AGRIMONIA *Eupatoria.* Sides of the River Gipping, common.
ROSA *canina.* Road- and field-sides and hedges, common.
—— *Eglanteria.* Hedges, probably introduced.

AMYGDALEÆ.

PRUNUS *Cerasus.* Hedge-rows, occasionally.
——— *spinosa.* Hedges, common.

POMEÆ.

CRATÆGUS *Oxyacantha.* Hedges, common.
PYRUS *Malus.* Woods at Freston.

SANGUISORBEÆ.

ALCHEMILLA *arvensis.* Sandy fields, very common.

ONAGRACEÆ.

ELIPOBIUM *hirsutum*. Sides of the Gipping, ditches, &c., common.
——— *parviflorum*. Moist woods and sides of ditches.
——— *montanum*. Freston woods, common.
——— *tetragonum*. Moist places occasionally.
——— *palustre*. Sides of ditches, frequently.

CIRCÆEÆ.

CIRCÆA *lutetiana*. } Freston Woods, common.
——— *alpina*. }

HALORAGEÆ.

MYRIOPHYLLUM *spicatum*. Marsh-ditches, frequently.
HIPPURIS *vulgaris*. Gipping river, common.

CALLITRICHACEÆ.

CALLITRICHE *verna*. Ditches, common.
——— *autumnalis*. Ditches, frequently.

LYTHRACEÆ.

LYTHRUM *Salicaria*. Sides of the Gipping, &c. common.

CUCURBITACEÆ.

BRYONIA *dioica*. Hedges, very common.

SCLERANTHACEÆ.

SCLERANTHUS *annuus*. Waste sandy places.

CRASSULACEÆ.

SEDUM *Telephium*. Woods occasionally.
——— *acre*. Walls and house-tops, common.
——— *reflexum*. Old walls, local.
SEMPERVIVUM *tectorum*. House-tops, occasionally.

SAXIFRAGACEÆ.

SAXIFRAGA *granulata*. Meadows, very common.
——— *tridactylites*. House-tops, local.
CHRYSOSPLENIUM *oppositifolium*. Boggy parts of woods, frequently.

UMBELLIFERÆ OR APIACEÆ.

HYDROCOTYLE *vulgaris*. Boggy parts of heaths, common.
SANICULA *europæa*. Woods by the side of the Orwell, frequently.
APIUM *graveolens*. Ditches, common.
SISON *Amomum*. Base of moist hedges, and wood-sides, frequently.
ÆGOPODIUM *Podagraria*. Lanes, local.
BUNIUM *flexuosum*. Woods and meadows, common.
PIMPINELLA *Saxifraga*. Chalky meadows, frequently.
SIUM *angustifolium*. Ditches, common.
HELOSCIADIUM *nodiflorum*. Ditches, common.
ŒNANTHE *fistulosa*. } Watery places, rare.
——— *crocata*. }
ÆTHUSA *Cynapium*. A troublesome weed in cultivated ground, &c.
FŒNICULUM *vulgare*. Road-sides, occasionally.
ANGELICA *sylvestris*. Sides of the Gipping, common.

PASTINACA *sativa*. Chalky districts, common.
HERACLEUM *Sphondylium*. Road- and field-sides, common.
DAUCUS *Carota*. Road-sides, frequently.
CAUCALIS *daucoides*. Fields, common.
SCANDIX *Pecten-Veneris*. Field-sides, common.
ANTHRISCUS *vulgaris*. } Field- and road-sides, frequently.
——— *sylvestris*. }
TORILIS *Anthriscus*. Meadow- and field-sides, occasionally.
——— *nodosa*. Road-sides, rare.
CHÆROPHYLLUM *temulentum*. Field- and road-sides, frequently.
CONIUM *maculatum*. Road- and field-sides, common.
SMYRNIUM *Olusatrum*. Road-sides, local.
ERYNGIUM *maritimum*. Walton shore, common.

ARALIACEÆ.

HEDERA *Helix*. Woods, common.
ADOXA *moschatellina*. Moist woods, frequently.

CORNACEÆ.

CORNUS *sanguinea*. Woods, frequently.

CAPRIFOLIACEÆ.

SAMBUCUS *nigra*. Hedge-rows, common.
LONICERA *Periclymenum*. Hedges, frequent.
VIBURNUM *Opulus*. Hedge-rows, local.

LORANTHACEÆ.

VISCUM *album*. Orchards, occasionally.

RUBIACEÆ OR STELLATÆ.

SHERARDIA *arvensis*. Fields, common.
GALIUM *cruciatum*. Road-sides, occasionally.
——— *palustre*. Moist meadows.
——— *saxatile*. Road-sides, frequently.
——— *verum*. Fields and road-sides, common.
——— *Aparine*. Road-sides, very common.

VALERIANACEÆ.

FEDIA *olitoria*. Road-sides in several places.
VALERIANA *dioica*. } Sides of ditches, occasionally.
——— *officinalis*. }
——— *pyrenaica*. Woods at Freston, three miles from Ipswich.

DIPSACEÆ.

DIPSACUS *sylvestris*. Sides of the Gipping and Orwell, common.
SCABIOSA *succisa*. Pastures, occasionally.
KNAUTIA *arvensis*. Common by road-sides, pastures, &c.

(*To be continued.*)

BRITISH MUSEUM
Z D
NATURAL HISTORY

Argynnis Aphrodite.

Swain

Art. IV.—*Notice of the Capture of* Argynnis Aphrodite *in Warwickshire.* By The Rev. W. T. Bree, M.A.

I have the pleasure of announcing to the entomological readers of the 'Magazine of Natural History,' the capture of an insect in this county which I believe to be hitherto entirely unheard of as a British species,—the *Argynnis Aphrodite.* A single example of this fine insect was taken by James Walhouse, Esq., of Leamington, in Ufton Wood, a few miles from that town, in the summer of 1833, and was kindly presented to my son, in whose possession it now is, by Moreton J. Walhouse, Esq., the brother of the captor.

In thus announcing this interesting addition to our native Fauna, I am prepared to expect that entomologists may be a little sceptical on the subject, if they do not altogether disbelieve the fact. We know but too well that dealers will, without scruple, play all sorts of tricks—*frauds*, I ought to say,—by attempting to pass off foreign articles for native ones, whenever it may suit their purpose. We know too, that even honest collectors are not absolutely exempt from occasional mistakes, and that, accordingly, a stray exotic does now and then creep into the British cabinet quite surreptitiously. Again we are told, and I believe told truly, that insects are not unfrequently imported, either in the egg, *larva*, or perfect state, with timber or other suitable merchandise. And lastly, we hear of Lepidopterous insects in the winged state, being blown over from the continent to our shores, if they have not undertaken a voluntary voyage thither. Bearing these circumstances in mind, and wishing as far as possible to anticipate objections, I deemed it right to obtain, and trust I shall be excused for stating, all the particulars I could learn relative to the subject of the present article. Let us sift the evidence, then, and see how the above objections bear upon the case before us.

And first for fraud: the specimen of *Argynnis Aphrodite* now before me, let it be remembered, has never been in the hands of a dealer, nor in the possession of any other person except Mr. Walhouse and his brother, from whom, as already said, my son received it. These gentlemen are men of the highest respectability, quite above all suspicion of intentional deception. I may add, too, that at the time the insect was taken, Mr. Walhouse was only just beginning to pay attention to Entomology. The immediate object of his visit to Ufton Wood was for the purpose of taking *Argynnis Paphia;* and so little acquainted was he at that time with our British *Papiliones*, that in the first instance he even doubted whether this specimen of *Arg. Aphrodite* were anything more than the usual sexual distinction of *Arg. Paphia.* I mention this circumstance in order to show that Mr. Walhouse was not at first *aware* of the prize he had taken, and therefore can hardly be suspected of having been actuated by the false feeling, which might induce a dishonest person to pretend to have been the discoverer of a new British species.

But acquitting these gentlemen (as we do entirely) of anything like *wilful* misinformation, may we not suppose that they have fallen into a mistake, and have inadvertently allowed a foreign specimen to gain admission among their British ones? This is a fair question, and deserves consideration.—Mr. James Walhouse is now in India, and cannot conveniently be examined in the matter. On his leaving this country his collection of insects remained in the possession of his brother, Mr. Moreton Walhouse. Now I have narrowly cross-examined this gentleman as to the possibility of a foreign specimen having found its way into their collection of native insects; and he assures me, in reply, that they possessed no foreign insects whatever, till long after the time when *Arg. Aphrodite* was taken. And, what is more to the purpose, Mr. Moreton Walhouse informs me, that although he was not in company with his brother at the capture of *Arg. Aphrodite*, he yet himself saw the specimen as soon as it was brought home, while the wings were yet limber, and before the specimen was set. Both gentlemen also were immediately aware of the great dissimilarity of the insect to any other with which they were acquainted, though they knew not what to make of it. Under these circumstances, therefore, I cannot withhold my own belief of the fact, that the individual specimen of *Arg. Aphrodite* now before me, was actually taken at Ufton Wood, as above stated.

But next comes the question of *importation;* in answer to which it is sufficient to state that Ufton Wood is situated in a thinly-populated part of the country, remote from any port or large mercantile town, a spot, therefore, extremely unlikely to have been the depository of an insect accidentally transmitted from abroad among articles of foreign produce.

Lastly, as *Arg. Aphrodite* is a native of North America, (and not, I believe, of the European continent), the notion that the specimen had, either by accident or design, made its way across the Atlantic, and settled down, in a state of good preservation, as nearly as may be in the centre of our own island, is too improbable to be seriously entertained for a moment.

I regret that Mr. Walhouse omitted to record the precise date of the capture of *Arg. Aphrodite;* but as it occurred during the season when *Arg. Paphia* was on the wing, it must, most probably, have been in July or August. We may conclude also that the period of flight, with both insects, is the same.

The accompanying figures (Sup. Ill. Pl. x.) supersede the necessity of entering into a minute description of the insect. It is larger[1] than *Argynnis Paphia*, and of the same rich fulvous colour, checkered and spotted with black, on the upper surface. The black spots and markings on the second pair of wings are niether so large nor so strongly developed as in the corresponding wings of that species, and of *Aglaia* and *Adippe;* to which latter species our insect more nearly approaches on the under surface, having the second pair of wings adorned with numerous silver spots on a buff-coloured ground, which is dark towards the base of the wings, and becomes lighter towards the lower extremities, with a marginal row of semicircular silver spots. In the grouping of our British species I should feel disposed to place *Argynnis Aphrodite* between *Arg. Paphia* and *Adippe,* possessing, as it does, some characters in common with each, while it is yet abundantly distinct from either.

ART. V.—*Notes on* Telephori, By PETER RYLANDS, Esq.

THE object of the present notice is to clear up the synonymy of *Telephorus ater,* and to correct some errors into which Mr. Stephens has fallen respecting that and allied species.

As a text to the remarks which I have to submit on this subject, the following descriptions of *Tel. ater* and *flavilabris* from Mr. Stephens's 'Illustrations' may be given.

> "*ater.* *Ca. ater,* Linné. *Te. ater,* Steph. Catal. 130, No. 1322.
>
> "Elongate: head black: mouth testaceous: *thorax* fuscous black with *the margin testaceous: scutellum* and *elytra* also of the same hue, the latter clothed with a griseous pubescence: *abdomen* black, with the apex broadly flavescent or pale testaceous: *femora* black: *tibiæ* entirely of the latter colour: *tarsi* fuscescent: *antennæ* with the three basal joints flavescent, the rest black.—Long. corp. 3-3½."
>
> "*flavilabris.* *Ca. flavilabris,* Fallen. *Tel. flavilabris,* Steph. Catal.

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[1] I am informed that the specimens of *Argynnis Aphrodite* in the British Museum, are generally larger than our individual.

"Oblong, black: with the mouth, the three basal joints of the *antennæ*, the *margins of the thorax*, the disc of which is very glossy, the *tibiæ*, and apex of the abdomen, *pale testaceous: palpi* fuscous. Long. corp. 3-3½.

"This *closely resembles* the preceding species, *but it is of a deeper black*, &c."—'Mandibulata,' iii. 295.

It will be evident on examination that the two descriptions now quoted do not present sufficiently distinct characteristics to justify the adoption of both as species. Either Mr. Stephens's *ater* must be a variety of his *flavilabris*, or *vice versâ*. Both are acknowledged to be very "variable," and yet the only mark of distinction given as *permanent*, is that *flavilabris* differs from the other by being of a "deeper black"! This surely cannot be considered sufficient for establishing a species; and a fact which has been stated to me by my esteemed friend, J. C. Dale, Esq., F.L.S., seems to prove that even this slight peculiarity cannot be relied upon,—his specimens (named by Mr. Stephens himself), Mr. Dale informs me "are *not* so dark."

The above remarks I think lead to the conclusion that Mr. Stephens's *ater* and *flavilabris* are *one and the same species.* I believe, however, that I shall be able to show that the TRUE *ater* (of Linné) and *flavilabris* (of Fallen) are distinct.

My attention was directed to this point by capturing a specimen of *Telephorus* at Egremont, near Liverpool, which did not agree with any of Mr. Stephens's descriptions, and which, for some time, appeared to me to be an entirely new species. Referring however to Paykull, I found that his description of *Tel. ater* of Linné did not agree with the description of *ater* given by Mr. Stephens, but that it did agree with my specimen. It appears evident, therefore, that previous to the occurrence just mentioned, the true *ater* has not been recorded as taken in Britain, although a spurious *ater* has been for some years entered in our Fauna. In this opinion I am supported by the fact that in the last edition of his 'Guide' Mr. Curtis places an ‡ before *Tel. ater*, Lin., denoting that he has only foreign specimens of it. And I think that entomologists, when they compare Mr. Stephens's descriptions with the following one of my specimen, and with Paykull's of the Linnæan *ater*, will be convinced of the correctness of the above remarks.

Description of a specimen of *Telephorus* captured near Liverpool in 1838; and presumed to be *Tel. ater* of Linné.

Elongate: head, *thorax*, and elytra *entirely black*, the latter with a griseous pubescence: antennæ with the basal joint testaceous: femora black with the apex testaceous; tibiæ of the latter hue: tarsi fuscescent. Long. corp. 3½ lin.

Description of *Tel. ater* from Paykull.

"*Caput* nigrum, antennæ elytris ferè longiores basi rufescentes. *Thorax niger*, convexus, marginatus, anticè rotundatus posticè subtransversus, vix latitudine longitudinis. *Scutellum* subtriangulare, nigrum, apice obtuso. *Elytra* nigra, punctulata, thorace paulo latiora et latitudine plus triplo longiora. *Alæ* fusco-hyalinæ. *Pectus* et *abdomen* nigra. *Tibiæ* sæpius fusco-pallidæ rarius ferruginæ. *Tarsi* jam nigri jam fusco-pallidi."—'Fauna Suec.' Insecta. tom. i. 266-7.

The point in which *Telephorus ater* chiefly differs from *Tel. flavilabris*, and the specimens erroneously designated *Tel. ater* by Stephens, consists in the *thorax* being *entirely black*. I communicated the above descriptions to Mr. Dale, and requested him to inform me whether any of his specimens of *Telephori* agreed with them. His reply is in the negative, and furnishes additional evidence that the Linnæan *ater* has been mistaken by Mr. Stephens, who has rather carelessly (it must be confessed) placed under that designation specimens having no distinctive characters, and evidently only varieties of *flavilabris*.

The genus *Telephorus* still requires much attention in order to elucidate its contents. *Tel. fulvicollis*, *iridis*, *affinis* (var. of *fulvicollis?*), *cantianus* (var. of *pellucidus?*), and others, require a strict investigation, in order that the *species* may be correctly characterised, and the mere *varieties* arranged as such. In the forthcoming 'Synopsis' of Mr. Curtis it is to be hoped that much of this confusion will be obviated, and many errors in this and other groups corrected.

The synonyms of the two species confounded by Mr. Stephens may be given as follows.—

1. *Telephorus ater.*

Cantharis atra, Linn. 'Syst. Nat.' ii. p. 649.
„ „ Fabr. 'Ent. Syst.' i. p. 215.
„ „ Paykull, 'Fauna. Suec. Ins.' i. 266.

2. *Telephorus flavilabris.*

Cantharis flavilabris, Fallen. 'Mon. Canthar.' i. 12.
„ „ Gyllen. 'Ins. Suec.' i. 337.
„ *picea*, Kirby, MSS. Steph. 'Catal.'
Telephorus flavilabris, Stephens, 'Illustr. Mand.' iii. 294. 'Ent. Edin.' 211. Vars. *β*, *nigrina*, Kirby; *γ*, *dissimilis*, Marsham; *δ*, *ater*, Steph

Warrington, Jan. 27th 1840.

ART. VI. — *Notice of the Occurrence of a living specimen of the* Testudo Caretta *on the Coast of Devonshire. In a Letter addressed to* W. WILSON SAUNDERS, Esq., *by* Mr. W. WILCOX.

East Hill, Wandsworth,
19th Feb. 1840.

SIR,

I send you an extract from a letter I have just received from my friend, Mr. William Wilcox, of Biddeford, North Devon, which I think will interest some of your readers, as it announces the arrival on our coasts of a stranger, which had never visited us before.

"I should have sent you the following particulars long before this, but I have had no opportunity of examining the subject of your enquiries until now, — he having been on his travels since the date of my last to you. Indeed, the old saying of "it's an ill wind" &c. has been decidedly correct in the present case, for I believe the fortunate discoverers of this amphibious stranger have derived a very considerable advantage from showing him here, and in the adjacent towns, at the small charge of 2d. each person; thus deriving a profit from the extraordinary gales of 1840.

"I forget the precise date on what he was found, but know that it was two days prior to the date of my last, if you still have it to refer to.[1] It was lying on the beach at the mouth of the river Tor, about half a mile from the village of Inslow. When I saw it, it was in a very torpid state, and about ten days after it was discovered, it died. It has not been weighed, but has been handled by myself, and by several others more conversant with such matters, and pronounced to be about 200 lbs.; and it is fair to suppose, that before its arrival into the cold waters of our climate, it must have weighed considerably more. Its extreme length after death, when the neck hung out from under the shell, was 4 feet 5 inches, and its breadth, 2 feet 9 inches; but this measurement was, in both instances, made by running a line over the convexity of the shell, which of course adds something to the actual length and breadth. Having no better book to refer to than the abridgement of Cuvier which came out in numbers a few years ago, I speak with some diffidence when I call this animal the *Testudo Caretta;* but as the other species appears to have fifteen scales, there can be little doubt about it, if the said

[1] Mr. Wilcox's letter is dated 30th January, and the turtle must therefore have been discovered on the 28th January.

book is right. There are also other peculiarities, which serve to confirm me in this opinion. There is a ridge, although a very inconsiderable one, running along the centre of the middle scales; and the toes, two on each fin, are strongly developed, the larger one on the anterior fin being an inch and a quarter in length. Should you, on reference to better works on the subject than I can procure at this distance from London, think with me that this is the *Caretta*, it will next become a question whether he visited these shores through the agency of man or the elements; and on this part of the subject I must say that I am strongly inclined towards the latter opinion; for certainly I never in the course of my experience, remember weather more favourable to such a result. For three weeks prior to the day he was found, there had been one uninterrupted gale from the West-South-West, excepting only once or twice for an hour or two, when the wind shifted a few points nearer to the North. Now supposing him to have been an inhabitant of the seas to the northward of the Azores, and on this subject I speak with deference to your greater knowledge, such a gale as I have described would have brought him here, lying, as I presume he did, on the top of the waves, at the rate of several miles an hour. And on the other hand, there is no great likelihood of his having been washed from on board any ship, inasmuch as both his flesh and shell are nearly valueless, and therefore it is improbable that any one should have taken the trouble of bringing him to this country."

Mr. Wilcox having favoured me with a sketch of the turtle forming the subject of the above extract, with dimensions of the various parts, taken apparently with great accuracy, I will add the following particulars from his data, which I think will serve to illustrate the subject more fully.

Total length of the dorsal and marginal plates.........	2 feet	11	inches.
Total width of ditto...	2 „	9	„

These measurements are taken over the convex surface.

The dorsal plates, fifteen in number, commencing from the fore-part, measure as follow.—

First lateral plate......	6½ inches broad,	3½	inches long.	These measurements give the greatest length and breadth of each plate.
Second ditto	12 ditto,	5½	ditto.	
Third ditto	12½ ditto,	6	ditto.	
Fourth ditto	11 ditto,	6	ditto.	
Fifth ditto............	7 ditto,	7	ditto.	
First central plate ...	5½ ditto,	4	ditto.	
Second ditto	6 ditto,	6½	ditto.	
Third ditto	6 ditto,	6	ditto.	
Fourth ditto	6 ditto,	6	ditto.	
Fifth ditto	6 ditto,	6	ditto.	

Head from fore-part to the posterior margin of the scales,	$10\frac{1}{4}$ inches.
Ditto, width across the eyes,	$6\frac{1}{2}$ inches.
Fore-fin, length of scaly portion,	1 foot 5 inches.
Ditto, greatest width,	6 inches.
Hind fin, length,	8 inches.
Ditto breadth,	6 inches.

There can be but little doubt of the animal under consideration being the *Testudo Caretta* of Linnæus, the *Chelonia Caretta* of modern authors. It agrees with the figures and descriptions of this creature in my possession, except that the middle row of dorsal plates have not a very elevated ridge along their centre. The *fifteen* dorsal plates and great length of the fore-fin or paddle, with the well-developed toes, distinguish this species from the common green turtle so much in repute among epicures.

I remain,
Sir,
Your obedient Servant,
W. Wilson Saunders.

To the Editor Mag.Nat. Hist.

Art. VII.—*Notice relating to the recent Landslip on the Devonshire Coast.* By John Young, Esq., *in a letter to the Editor.*

My visit to the landslip at Culverhole Point, near Axmouth, was of so hasty a nature as scarcely to warrant my attempting any description of the phenomena which came under my notice. I have moreover, for the last twenty years, given my attention to Horticulture rather than to Geology, and fear therefore, that I shall fail in furnishing such an account as would interest those of your readers who are acquainted with the latter science, and that my unlearned description will be hardly worthy a place in your journal.

My ignorance of the original position of the ground where the subsidence has taken place, is one disadvantage under which I labour. It appears, however, that numerous catastrophes of a similar kind had taken place on this part of the coast at remote periods, forming what is called the *Undercliff*, and let it be remarked, that it is in connection with a part of *this Undercliff*, about a mile to the east of Culverhole Point, that the sinking of the ground on the 24th December last commenced, taking a direction towards the north-west, continuing in this course about half a mile, then gradually bending to the south, till it meets the shore at or near Culverhole Point, its western extremity. The level of the ground, in which this ravine has been formed varies considerably; its deepest part showing cliffs of 200 feet perpendicular height, whilst at the extremities adjoining the Undercliff, the level is not greatly changed, and the line of subsidence marked principally by numerous small fissures in the ground, by the fall of trees, &c. The present appearance of the ravine is highly picturesque, for notwithstanding the depth of the chasm, and the almost entire giving way of the sub-strata, there are numerous columnar, tower-shaped, and pyramidal masses of the chalky limestone, standing in the deepest part, apparently unmoved, and which, particularly when viewed longitudinally, form a striking group, whilst around and beneath them the furrows of the corn-fields can be traced nearly in the state the plough had left them. The character of this late subsidence differs materially from that of those which have preceded it,—the latter having taken place in that part of the land immediately adjacent to the sea beach,—the one now under consideration having left the outer portion of the cliff almost entirely unmoved; and this circumstance viewed, in connection with the reef which has been upraised in the sea, makes the unaltered position of the cliff the more remarkable: it was this part of the scene which attracted much of my attention, particularly the reef, (for I know of no better name for it), which was formed simultaneously with the sinking of the land; consequently, there must have been a connection between the causes which produced these two phenomena,—the lower or west extremity of the reef, commencing at or near Culverhole Point, forming a segment of a circle, (a little irregular in its shape), runs into the shore, at the eastern extremity of the subsidence;—the reef is composed of the original bed of the sea upheaved, without disturbing, to any considerable extent, the masses of stone with which it is covered; and without injuring the beautiful sea-weeds which appear to have luxuriated in the habitat which they had chosen, for they had covered the stones like a carpet; and so gently must this extraordinary upheaving have taken place, that scarcely a fibre of these beautiful marine plants exhibits traces of injury. The main bulk of the reef is composed of what appears to be indurated sand, or possibly a bed of marl connected with the lias formation; the seaward face of it stands about 25 feet above high-water-mark, at its highest point at the east end, whence the whole gradually sinks as it runs westward; and when it reaches the western extremity of the subsidence, it is scarcely distinguishable at high water: it is said that previously to 24th December last, vessels sailed over the whole of this ground.

I would gladly enter into the subject of the probable causes which have been instrumental in producing the changes I have described, but as my acquaintance with the Geology of the neighbourhood is very limited, I must not venture upon any thing further than the above hasty sketch of this interesting spot: I must not, however, omit to refer to the fact, that the chalky limestone, which, I believe, forms the upper stratum of the rock of this part of the county, overlies a bed of loose sand, the action upon which, of the superabundant rains of 1839, added to the same agency carried on through past centuries, conspired in producing the phenomena I have been endeavouring to describe. If the anserine tribe had the power of speech, they could give valuable evidence, *as it is said*, that some of that family made a subterranean voyage from a neighbouring farm into the sea.—I must, *en passant*, tell you of another report; it is said, that a few years back, some persons in digging a well, at or near Axminster, about 5 or 6 miles to the northward of the seacoast, came to a bed of sand through which a rapidly flowing stream was passing towards the ocean, and that they had the curiosity to throw into it a few bushels of charcoal, which made its appearance in the sea, about three miles eastward of the mouth of the Axe.

I hope Professor Buckland, (who, I understand has visited the spot), or some other eminent Geologist, will give us his opinion on the causes which have led to these phenomena; and which, it appears to me, will tend to throw much light on the formation of many narrow and deep valleys, in certain districts of this country.

Elm Cottage, Taunton,
Feb. 21*st.* 1840.

Art. VIII. — *Observations upon the relationships existing amongst Natural Objects, resulting from more or less perfect resemblance, usually termed Affinity and Analogy.* By J. O. Westwood, Esq. F.L.S.

In the fourteenth volume of the 'Transactions of the Linnea Society,' is contained an interesting paper by the Rev. W. Kirby, with the title, "A Description of some Insects which exemplify Mr. William S. Macleay's Doctrine of Affinity and Analogy;" wherein the reverend author points out the confufusion which has occasionally arisen, in attempts made to distribute the objects of nature according to their natural relations, in consequence of the authors of such attempts having no clear perception of the distinctions which exist between these two kinds of relations, and therefore confounding them together, or even occasionally giving the higher rank to relations of analogy instead of affinity.

Since the publication of this memoir (which was read in 1822), and of the 'Horæ Entomologicæ' (one of the theories contained in which it was intended to illustrate, and which was published in the preceding year), much has been written upon thes ubject of affinity and analogy in Natural History by men who have brought a great share of practical knowledge, as well as philosophical research, to bear on the subject.

Still however it is unquestionable that great misconception has existed and still exists upon this subject, owing, it is true, to many causes which, it is to be hoped, are gradually passing away. The novelty of the subject, that is, as regards the practical distinction between these kinds of relationships, and their employment as respective elements in the natural distribution of animals; the difficulty of general access to the chief works wherein the nature of the distinctions between affinity and analogy are traced; and, above all, the dislike of uprooting long-established opinions, and the substitution of others which required from their supporters a far more extensive acquaintance with the objects of nature than was required in the old works on classification; — have tended, in a great measure, to prevent a general recognition of the doctrine of affinity and analogy. Thus some writers have maintained the impossibility of the existence of any *relation* between animals which are not specifically related, contending that each is, *per se*, totally independent of the rest: others again, looking rather more widely at nature, perceive that as the plant gives support to the insect, and the insect to the bird or the bat, so there is a relation between these objects, which kind of relation, and others of a similar nature, have by these writers been termed analogies: others again, admitting the necessity of the existence of some kind of relationship founded upon similarity of structure, in order to establish thereon a distribution whereby species are endeavoured to be arranged according to their greater or less similarity of organization, have admitted only one kind of relationship or resemblance, as they have termed it, regarding affinity and analogy as only synonymous with resemblances.

The first and second classes of these writers may be dismissed from our notice in a very few words. If we admit that there be a system of nature wherein animals are arranged and distributed, we must admit the existence of relations founded upon structural similarities or differences; and these relations, according to their degree, constitute what have been termed affinity and analogy, the distinctions of which are overlooked by the third class of writers, who however acknowledge structural relations or resemblances.

It appears to me, however, that notwithstanding all that has been said by the supporters of the doctrine of affinity and analogy as distinct relations, not a small share of the misconception which has prevailed upon the subject with professed distributionists, has resulted from the silence of the former concerning what appears to me to be a fundamental principal in the theory, which may be stated as follows.

Relations of affinity and analogy have their origin in more or less perfect resemblances of structure or habits, and are of comparative and relative value; and hence that distinct relations, both of affinity and analogy, exist between the same groups.

Much has been admirably said respecting the immediate and the remote relations of objects, and the differences between their symbolical relationship and their natural affinities, whilst at the same time many excellent examples have been given in illustration of their distinctions. Amongst the latter, none have been more striking than that employed by Mr. Swainson in his 'Treatise upon the Natural System of Animals,' illustrating the relation of affinity existing between the swallow and the goat-sucker, and the relation of analogy between these and the bat.

In applying this example as a practical illustration of the principle above laid down, it will scarcely be deemed necessary, in the first place, to enter into any argument to prove that relations of affinity, as well as of analogy, have their origin in more or less complete resemblances of structure or habits: indeed, one of the most strenuous advocates of the doctrine in question, has observed that "every created being has different degrees of relationship or of *resemblance* to others," constituting its relations of affinity and analogy. It is in consequence of the more complete resemblance *in the numerical majority of the essential characters* of the two beings or groups of beings contrasted together, that the relationship becomes one of affinity; while from a resemblance in the *numerical minority* of such essential characters, the relationship is deemed an analogy.

In proceeding with our examination of the principle above stated, it will be necessary that in addition to the swallow, the goat-sucker, and the bat, we extend our views to the martin, the swift, and the dragon-fly; all which are distinguished by their large wings when extended in action, their rapid motions, large mouths, and insectivorous hawking flight.

The swallow and the martin are two birds belonging to the same genus (*Hirundo*), and resembling each other in the greatest possible number of their essential characters; they are therefore allied together by an affinity of the first or highest degree. Between these birds and the swift, a bird belonging to a different genus but to the same family, we find an affinity of a lower, or second degree. A third or still lower, is found between the goat-sucker, belonging to a different tribe, and the three preceding birds. Again, as vertebrated animals, the bat and the four birds above mentioned exhibit an affinity when compared with animals belonging to the four invertebrated sub-kingdoms, whilst as members of the animal kingdom, and compared with members of the vegetable kingdom, the dragon-fly (an invertebrated animal) and the five vertebrated animals above alluded to, are related together by affinity.

It may however, and probably will be, said, that in the case of the two last-mentioned animals (the bat belonging to the *Vertebrata*, and the dragon-fly to the *Annulosa*), I am confounding analogy with affinity. I however maintain the contrary to be the case, asserting that these relations are strictly comparative and relative; that if the relation between the swallow and the martin, as species respectively possessing the characters of the next higher group (that is, genus), when compared with the swift or any other bird belonging to the other groups of the next higher rank be deemed an affinity; or if the relation between the swallow and the goat-sucker, founded upon a comparison of their joint characters as belonging to the higher group (*Aves*), when compared with the animals composing the other vertebrated orders (the bat for example) be deemed an affinity; the relation between the bat and the goat-sucker, as vertebrated animals, when compared with all invertebrated animals (the dragon-fly for instance) must also be an affinity. In like manner the dragon-fly and the bat, as *animals*, must possess a common relation of affinity when compared with *vegetables* (the catch-fly plant for instance). And even between animals and vegetables as compared with inorganic matter, there must be a relation of affinity founded upon the circumstance of their organized structure.

To show then the existence of distinct relations of analogy amongst these six animals of which we have thus traced the affinities, it will be necessary to reverse the series as well as the mode of comparison; for if a relation of affinity of the first degree be found to exist between the species of a genus or sub-genus, the first degree of analogy will be found in the objects most widely apart, that is in those belonging to different sub-kingdoms. Here then we find the bat and the dragon-fly, belonging to different sub-kingdoms of the animal kingdom (or next higher group), and we have already seen that as compared together with reference to the objects of the other next higher group (that is the vegetable kingdom), an affinity existed between them; but if, on the other hand we regard the relations of the bat with any other animal belonging to the *same* sub-kingdom as itself (*Vertebrata*, the goat-sucker for instance), we find the relation between the bat and the dragon-fly to be so comparatively remote, that we term it a relation of analogy. In like manner, if we compare the goat-sucker with any other animal belonging to the same vertebrated class as itself (birds, the swallow for instance), we find the relation between either of these animals and any other animals belonging to the other divisions (*Reptilia*, *Pisces*, *Mammalia*, the bat for example) composing the next higher group (*Vertebrata*) to be one of analogy, as indeed Mr. Swainson has truly regarded it; but by pursuing this train of reasoning, it will be equally evident that the relation between the goat-sucker and the swallow, in comparison with that between the latter and the martin, is merely one of analogy and not of affinity.

In like manner we say that all the insects belonging to an order (the *Coleoptera* for example), are related to each other by affinity, as compared with the other orders of insects; but when we come to analyze the order *Coleoptera*, we find innumerable analogies existing amongst those very insects which we have just previously admitted to possess a general affinity with each other.

It has long appeared to me to be most probable, that the fact of two animals or groups of animals thus exhibiting relations both of affinity and analogy, was the chief cause which induced many persons to regard both these relations as of equivalent value, and as synonymous with resemblances.

THE

MAGAZINE OF NATURAL HISTORY.

MARCH, 1840.

THE penny-postage rate, thanks to Mr. Rowland Hill, has now become the order of the day. Of its benefits, we can speak feelingly, for the long array of contributions whose titles grace each volume of this journal, has involved no insignificant patronage of Her Majesty's establishment in St. Martin's le Grand. Not that we have been treated otherwise than most liberally by our correspondents in the matter of postage, for the cases have been rare indeed that a letter has come to hand unfranked by the sender. It has been in the *outward* bound despatches where we have felt so crippled by the impost. *Now* if we wish to communicate with our contributors, in the most distant part of the kingdom, we can do it as often as we please, without a calculation as to what it may cost us to attain our object. We have no longer to send our *ounce* despatches to some M.P., with a respectful hint that by franking them to their respective destinations, he will be serving *us*, and serving *science* at the same time. Natural History has indeed received a boon, and we should be ungrateful, did we not offer some acknowledgment to the talent and boldness, displayed in the first bringing forward a measure, which the voice of the country has since so triumphantly carried into effect.

The long looked-for and long despaired-of 'Zoology of Beechey's voyage' is at last before us. Its merits as a scientific work will be duly noticed in another place; on this occasion we shall merely say a word touching a passage, which greatly helps to fill out the preface. Sir William Beechey, after doing justice to the labours of several distinguished men of science, by acknowledging the obligation he is under to them for their co-operation in getting up the descriptive portion of the volume, thus proceeds.—

"I wish I could with sincerity have included with the above-mentioned names that of Mr. J. E. Gray, who undertook to describe the shells, "but the publication has suffered so much by delay in consequence of his "having been connected with it, that it is a matter of the greatest regret "to me that I ever acceded to his offer to engage in it. This delay has "from various causes, been extended over a period of eight years, and I "cannot with justice or propriety conceal from the government, the collectors, and especially from the contributors to the work, whose MSS. "have been so long printed, that it has been occasioned entirely by Mr.

"Gray's failing to furnish his part, in spite of every intercession from my-"self and others: promising his MS. from time to time, and thereby keep-"ing the department in his own hands, yet always disappointing the print-"er, until at length, from other causes, the publisher (Mr. Richter) fell "into difficulties, and all the plates and letter-press were sold by the as-"signees, and lost to the government.

"The plates and sheets thus dispersed were however with difficulty and "at considerable expense brought together, by the spirited conduct of the "present publisher, Mr. H. G. Bohn; who, anxious that the work should "if possible be completed, again applied to Mr. Gray, but much against "my wishes. That gentleman however repeated his offer of assistance, but "as before it served only to delay the work another year. At length Mr. "G. B. Sowerby was engaged to complete the Conchology, and to revise "the unprinted portion of Mr. Gray's MSS., and thus after an unpre-"cedented and vexatious delay, and with a considerable additional ex-"pense, I am now only able to submit the work to the public."

Now if naturalists, either knowingly or through inadvertence, so place themselves, as to stand committed, they must not expect to enjoy a monopoly of exemption from the natural consequences; but must take their chance with the race of ordinary mortals. It is for the interest of science, equally with that of society, that misdemeanours should not pass altogether unregarded, however high may be the names with which they stand connected. We do not, therefore, impute blame to Sir William Beechey, on the score of what he has *said* in assigning a cause for the long period during which the work has been forthcoming, although every one must regret that he should have felt himself under the necessity of passing such severe comments upon one of the officers attached to the British Museum; but we have an objection to offer to the *shape* in which these strictures are presented to our notice. The statement is purely an *ex parte* one, for it comes before us, through a channel which affords the party criminated no opportunity of reply. Mr. Gray appears as the avowed author of one portion of the volume, and as such, we conceive that he was, beyond all dispute, entitled to a copy of the indictment prior to its publication, with the liberty of defending himself, if he thought proper. For aught we know, this step may have been taken, but we assume that it was not, as no intimation of such course having been followed, is affixed to the article in question.

On our first and hasty look through the work, to gain a mere insight into the nature of its contents, our attention was caught by a passage occurring in Mr Gray's introductory remarks to the Malacological department, one which still comes out under his responsibility. It was written, we presume, some considerable time since, and is as follows:—

"In the following memoranda I have given such observations as I have "been enabled to make on the animals of the various genera, brought home "either by Captain Beechey's expedition, or by several other voyagers, as "Captain Lord Byron, Mr. Fryer, and the Rev. Mr. Hennah, who about "the same time touched at several parts of the world, which were also vis- "ited by this expedition. I have been induced to follow this course, rather "than to give only a description of the new species discovered by the offi- "cers of the expedition, (as it was my intention to have done when first "the specimens were put into my hands), because Mr. Sowerby and Mr. "Broderip, almost immediately on the return of the expedition, described "many of the new and interesting species discovered during the voyage, "specimens of most of which were given to the Zoological Society by "Lieutenant (now Captain) Belcher, one of the officers of Captain "Beechey's ship. In my MSS. I had given names to most of the species, "but I have since substituted those used by the above-mentioned gentle- "men, that science might not be burthened by the many useless names "which an opposite course would have produced.

"I have only given a *zoological* description of the animals; as accord- "ing to the rules of the British Museum, we are very properly forbidden "to dissect the animals under our charge, which might thereby be render- "ed useless for subsequent observers."

Any one at all conversant with Zoological etiquette, upon reading the above extract, will not fail to detect a flaw on the side of Sir William Beechey. We can readily understand the annoyance it must have occasioned Mr. Gray, to find himself anticipated in the describing the new *Testacea*, brought home in the 'Blossom,' when he had commenced the drawing up those descriptions himself. It may be argued that Sir William Beechey could not be answerable for an officer of the ship's company, giving to the Zoological, or any other Society, specimens which might be duplicates of those in his [Sir Wm. Beechey's] possession; but it should be borne in mind that the expedition was a Government affair, and a sum of money being granted to put the public in possession of the scientific results of that expedition, and the Malacological portion of the Natural History department, being consigned to an officer in the British Museum, an understanding surely ought to have been entered into, by which the result just mentioned might have been guarded against. Precisely the same thing might have happened with the novelties in the other branches of Natural History, for there are plenty of channels open to the speedy publication of the characters and proposed names of new species; and the respective authors of each separate section, might thus not only have been anticipated, but the value of the work itself greatly diminished. We know nothing of the history of this matter, beyond that which we can gather from the statements now quoted, and the second of these is apparently written without reference to the first; the possible relation which the one may bear to the other, being purely a matter of assumption on our part. We do not notice this circumstance, for the purpose of showing up the errors of Mr. Gray, or those of Sir Wm. Beechey, and striking a balance between them, but because we have seen too much of *ex parte* statements, and have too often been subject to them ourselves, not to deal out justice with an even hand, when the opportunity lies in our power, and a legitimate occasion comes before us.

We have the satisfaction of stating that the one hundred and sixth number of the 'Mineral Conchology of Great Britain,' has *really* made its appearance, for we have a copy now lying on our table. The author has therefore redeemed the pledge to that effect, which he put forth a short time since in the pages of the 'Magazine of Natural History.' The most gratifying circumstance that could possibly have come before us, in connection with this publication, is that of the Council of the Geological Society having just awarded to Mr. Sowerby, the proceeds of the Wollaston Donation Fund, as an aid towards the immediate continuation of a work, which bears so important a relation to our knowledge of the British fossiliferous deposits. The materials now in hand, for carrying forward the 'Mineral Conchology' are so extensive, and promise to accumulate so rapidly, that we trust there will be no impediment in the way of its regular appearance, but that Mr. Sowerby may receive from the hands of English naturalists, a share of support which shall show their due appreciation of the value of his labors; and that, without infringing on the boundary of justice towards himself or his family, he may be enabled to go on, so long as he shall have health and strength to engage in the undertaking.

It is generally understood that Mr. Children has resigned his post at the British Museum, and that his valuable library and entomological collection is shortly to come to the hammer. Whoever may be appointed to fill the vacancy thus occasioned, will we trust have, like his predecessor, other attainments to grace the station than only those arising from a profound acquaintance with zoological science.

SHORT COMMUNICATIONS.

Habits of the Water-Shrew, (*Sorex fodiens*), *as observed near Ipswich.*—Whilst walking by the side of the river Gipping, in May, 1838, between Ipswich and the village of Sproughton, my attention was arrested by several water-shrews actively engaged in a dyke that runs parallel to the river.—These little creatures were in such rapid motion on the water, that its surface was thrown into a state of quick undulation, though the dyke was at least four feet wide. At times they would be upon the surface moving at a rapid rate between the blades of the aquatic plants, consisting principally of *Sparganium ramosum* and *simplex*, that grew from the bottom,—then they would dive, and for a while remain beneath, but always on returning to the top, displaying the greatest rapidity in their movements. Whilst above water they were constantly repeating their faint, though shrill, tremulous squeak, which appeared as though expressive of pleasurable sensations.

On visiting the spot the following evening, and secreting myself, I had an opportunity of remarking the movements of these little animals on land.

I found beneath a slightly hanging bank, and close by the water side, a long gallery, which, though in a great measure naturally formed, yet much had been done by the shrews to render it a convenient viaduct between one hunting-place and another: the grasses and other plants had been removed, as well as, here and there, small portions of earth, in order to render this passage, in their movements from end to end, as commodious as possible. I observed the shrews continually passing backwards and forwards through this passage, which enabled them to travel with facility from one part of the ditch to another, and which was principally a little above the water level, but at intervals there were depressions at which the water passed on to, or over its floor This passage was evidently the common property of many shrews, as several were continually running backwards and forwards, along its whole extent, and ultimately taking to the water, swimming up or down the ditch, diving, and performing various evolutions in search of their insect prey. They swim upon or under the surface of the water with equal rapidity, and when beneath, the hair upon their bodies so completely repels the water, that the air entangled amongst the hair gives to the bodies of the little animals the brilliancy of silver, as they pursue their course; on emerging from the water the coat appears perfectly dry, but this is further ensured by the little creature giving itself a sudden shake on arriving at its landing place. I remarked that in travelling along the above-mentioned gallery, the tremulous shriek is always heard when two shrews happen to pass each other; and the same thing occurs, although not so invariably, in their movements in the water.—When a shrew secured an insect it quitted the water, and ascended a convenient stone, or projecting root of a tree, clod of earth, or some other similar body, where at its leisure it devoured its prize, steadying the insect with its fore paws, whilst it nibbled, apparently with the greatest enjoyment, one or other of its extremities.

These animals are generally infested with an immense number of *Acari*, which, after the death of the shrew, leave the surface of the skin and come to the extremity of the hairs, where to the naked eye they appear like white dust. I once traced a pair of shrews into a small hole in a bank by the side of the ditch where I had been in the habit of observing them; and in order to try and secure them I carefully removed the earth, when I found that although the entrance was scarcely larger than just to allow of two shrews passing together, yet it led into a very capacious vestibule, with galleries leading one into another, and so extensive that there was no possibility of ascertaining their full extent without removing the greater portion of the bank.—*W. Barnard Clarke, M.D.* —*Edinburgh, Jan.* 20, 1840.

Calosoma sycophanta.—At the meeting of the Entomological Club on Thursday the 20th February, Mr. J. F. Christy exhibited and presented a fine specimen of this beautiful insect. It was found by Mr. Rutter, on the grass-plot opposite Arundel Terrace, Kemp Town, Brighton: when taken it was not only alive, but very active. Two other specimens were taken by a little girl at Brighton during the summer of 1838, and were purchased by Mr. Hoyer. A single individual was found by Mr. Bennett on the 20th of June 1839, at Ramsgate, on the chalk cliff; it was kept alive more than a month, and ate caterpillars greedily. Three specimens were found during the past summer at Hastings, by Mr. Hanson or some of his family: one was living and active, the others dead, and lying on the sea-shore. We have thus abundant evidence that this fine insect is really British, and, as far as my observations have extended, it appears to be confined to the south-eastern and southern shores of England; its range extending along the coast from Norfolk to Cornwall. I think it right to add that numerous French and German specimens are being hawked about by dealers at a very low price, and are warranted *genuine Britons, and taken near Norwich.*—*Edwd. Newman.*

SCIENTIFIC INTELLIGENCE.

THE Microscopical Society of London held their first meeting on Wednesday, January 29th, at the Horticultural Society's Rooms, No. 21, Regent St. The meeting was attended by upwards of a hundred members and visitors.

The President, Prof. OWEN, announced that since the provisional meeting on the 20th of December, for the purpose of forming the Society, the number of members had encreased to one hundred and ten; and a further addition of twenty-nine names was announced in the course of the evening, making a total of one hundred and thirty-nine original members of the Society. (It having been determined that those who joined the Society on or before the first night of meeting, should be considered *original* members).

Mr. Owen communicated a paper 'On the application of microscopic examinations of the structure of teeth to the determination of fossil remains.' After alluding to the essential service rendered by the microscope to the chemist, mineralogist, and vegetable physiologist, he proceeded to offer a few examples of the utility of the microscope to the geologist, when applied to the investigation of the structure of fossilized teeth.

The first example adduced was that of the *Saurocephalus,* an extinct fossil animal which had been referred to the class of Reptiles.—After pointing out the distinctive characters of the microscopic texture of the teeth in reptiles and fishes, it was shown that the *Saurocephalus,* according to this test, unquestionably belonged to the latter class, and that it most closely resembled the *Sphyræna,* among recent fishes, in its dental structure.

The second instance was the *Basilosaurus* of Dr. Harlan, which had been referred to the class *Reptilia,* and the double-fanged structure of its teeth had, on the strength of its supposed saurian affinities, been adduced to weaken the arguments in favour of the mammiferous nature of certain fossils from the Stonesfield oolite. Mr. Owen, after describing the microscopic characters of the teeth of the *Basilosaurus,* showed that it deviated from the saurian structure in this respect, as widely as the *Saurocephalus,* but that the modification of its dental structure resembled most closely that of the cachalot and herbivorous *Cetacea.*

Lastly, Mr. Owen alluded to the difference in the views entertained by Cuvier and M. de Blainville as to the affinities of the *Megatherium,* which was referred by the one to the family of the sloths, and by the other to that of the armadilloes: after explaining the well-marked differences in the microscopic characters of the dental structure in these two families of the so-called *Edentata,* Mr. Owen proceeded to describe the structure of the teeth of the *Megatherium,* and to show that in its close resemblance to the dental structure of the sloths, it confirmed the views of the great founder of the science of fossil remains.

Mr. Jackson then read a short paper, drawing the attention of the Society to a mode of mounting the compound microscope, which differs in some particulars from the methods generally adopted. The principal object to be kept in view in the construction of this instrument is the prevention of those accidental vibrations which so much interfere with microscopical examinations, especially in the neighbourhood of crowded thoroughfares. This object is effected by connecting together the body and stage of the instrument, in such a manner that whatever vibrations are communicated to the one shall be equally communicated to the other. In Mr. Jackson's instrument this principle has been carried farther than had hitherto been effected; and it also affords improved facilities for minute adjustment, and the accurate admeasurement of microscopic objects.

The second meeting of this Society was held on Wednesday, the 19th of February; R. H. Solly, Esq., in the chair.

A paper was read by Mr. Quekett, 'On the development of the vascular tissue of plants;' in which it was shown that the membranous tube of vessels originated from a cytoblast, in a manner similar to that described by Schleiden in the formation of cells. Before the fibre is deposited, the contents, which are gelatinous, are crowded with numerous most minute granules, which possess the motion known in "active molecules:" and after a short time, when they have become a little enlarged, they adhere to the inner surface of the tube containing them, in a different manner for each vessel; so that the several varieties of vascular tissue are not degenerations of any other kind, but are each constructed originally on the plan they are always observed to present to the eye.

It had been conjectured by Schleiden that a current existed between the gelatinous contents of the cell and its walls, which current preceded the formation of a fibre, and gave the direction it afterwards took; this was refuted by showing that the granules become separately attached to the inside of the vessel, a short distance from each other, beginning first at one end and proceeding to the opposite one; the fibre elongating like a root by the materials of growth being always added to the point. The granules so attached become nourished by the contents of the vessel, and the spaces between them are in a short time obliterated by the fibre acquiring a defined border, which completes its development.

This act is the one observed in the formation of all vessels; but the arrangement of the granules differs, so as to constitute the several varieties. In the *annular* vessel the granules attach themselves horizontally, forming rings; — in the *spiral* they become inclined, and by continuing this direction around the interior of the membranous tube the peculiar character of this vessel is obtained: — in the *reticulated*, for each division or branch of the fibre, a granule becomes enlarged in the line, and forms the starting-place for the fresh direction of the fibre. In the *dotted* and *scalariform* vessels, the fibres become so reticulated as to leave portions of the outer membrane of the vessel without any deposit within; and this spot so left constitutes the *dot* or *linear marking* seen on these vessels.

This dot is *plain* in all such kinds of vessels, excepting those found in woody exogens, where it possesses (from a slight difference of structure), a central mark analogous to that on the woody tissue of coniferous plants, with which Mr. Quekett considered it identical, only of a smaller size.

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Art. I.—*View of the Fauna of Brazil, anterior to the last Geological Revolution.* By Dr. Lund.

(Continued from page 112.*)*

I now pass on to the most abundant of the extinct genera of the *Armadillo* tribe, whose numerous remains, in several cases, have enabled me to determine with accuracy its place in the family, as well as its relations to existing species. This animal constitutes, in many points, a perfectly *connecting link* between the genera *Dasypus* and *Euphractus* ; but in other points it differs from all living genera of this family. Like *Dasypus*, it has only four toes on the fore-foot; and the construction of its fore and hind feet is very much the same in all the principal characters: except that the bones are much shorter, and the claws broader. On the other hand, the remaining bones of its extremities, as also those of its body, correspond in the most striking manner with those of *Euphractus* ; which it moreover resembles in the structure of its corslet.

In connection with these remarkable correspondences to existing genera, one cannot help being greatly astonished at the entirely dissimilar structure of the dental system, presented by the fossil species, and which compels us to ascribe to this animal totally different habits and food, from those which we know to belong to the living species. With respect to the habits of these latter great contradictions occur, not only in the works on natural history, but even in the accounts I have collected from the natives. The countrymen destroy them as being injurious to their maize, gourds, &c., while those which I kept in my house[1] invariably showed an aversion to all such vegetable productions; but on the other hand, exhibited an extraordinary predilection for putrid flesh, as well as a remarkable skill in managing it. When the morsel is too large to be swallowed whole, they take it between the front teeth, and then work it with the claws of the fore-feet with such incredible rapidity, that in a moment it is riven asunder, and thus swallowed piecemeal. I have always found in the stomachs of those that I have examined, numerous remains of insects, particularly beetles and *Scolopendras;* together with a fine pulp, the nature of which I have not been able to determine. Hence we see that the modern armadillos are insectivorous and carnivorous: and in truth, the masticating surface of their teeth seems much better adapted to cut their food than to grind it. In the fossil species the upper jaw has eight teeth on each side, and the lower nine; of these, the two front in the upper jaw, and the three front in the lower, are incisors. The latter are shaped like small cylinders of a more or less reniform section; while the molars are very large, and compressed longitudinally, so that their section resembles an elongated kidney. Their lateral surface is marked with several canaliculated impressions, and their grinding surface presents two projections, the effect of the indentation of the teeth of the opposed jaw. In other respects it is flat, or even a little hollowed in the middle, as in the sloths; so that in all the principal points, these teeth are constructed on the same plan as those of a *Megalonyx*, and are evidently suited to grind, and not to cut: hence we may conclude, with a high degree of probability, that vegetable substances were their appointed food. I propose the name of *Chlamydotherium* for this extinct genus; and to the species which is as yet the best known, I would venture to add the name of the first modern naturalist, by calling it *Chlam. Humboldtii.* Its length, from the point of the snout to the root of the tail, is six feet; its size, therefore, is double that of the largest existing armadillo, or about equal to that of the tapir.

I have found, also, the remains of another species, though much less frequently than the above, which I have named *Chlam. giganteum*, on account of its vast size, in which it was certainly not inferior to the *Rhinoceros*; and it surpasses all I have yet discovered of the entire order *Bruta*.

[1] This was the case even with the genus *Dasypus*, Wagl., to which the opposite of such habits is usually attributed.

The next genus, with only one species of which I am acquainted, carries us still farther towards the confines of the armadillo family; and surprises us with characters hitherto considered peculiar to the sloths. This astonishing monster combines, with the size of an ox, a coat of mail, most like that of *Tolypeutes*, but of astounding thickness, and extremities fashioned nearly after the form of the armadillo, with short thick feet, and with immensely broad and short claws, which must have given its foot almost the appearance of that of an elephant or hippopotamus. It has the sloth's head, with the same characteristic structure of the *zygoma*. Its teeth are shaped like the molars of the *Capivar*, but have a different structure, inasmuch as they are simple, and not composed of *laminæ*. In the details of its internal structure, it presents, besides, several peculiarities not observed in any other animal. I call this remarkable animal[1] *Hoplophorus Euphractus*.

Finally, I conclude my list of this family with a genus, which, from the little I yet know of it, seems to offer so complete a passage into the next family, that it will require more perfect specimens to decide to which of them it belongs. The general characters of its feet are those of the armadillo; but so shortened and *thickened*, and with such massive proportions, that I cannot resist indicating this genus, for the present, by the name of *Pachytherium*. It seems to have been of the same size as the preceding, or rather larger; but I have not hitherto found any trace of its having had a coat of armour.

Armadilloes are now confined to this portion of the globe; and we see from the above short sketch, that they also inhabited this district in the previous geological period. No trace of these animals in the fossil state having hitherto been discovered in the old world, we may conclude with certainty, that the geographical distribution of this genus was the same then as now. We shall have further opportunities of establishing this fact in the course of our enquiries. But not only did this group of animals exist here in former times; it was also richer in subordinate forms and species than it now is. If we examine more closely the generic forms that formerly composed this family, we find that one of them (*Dasypus*, Wagl.), still exists, but that the remaining five are extinct. And although there are good grounds for suspecting that a more accurate acquaintance with the fossils of this district will bring to light more existing generic forms of this family,

[1] Named by Professor Owen, *Glyptodon*.

still we may assert generally, that notwithstanding the continuance of this group of mammals in this region from that ancient period, betokens a similarity with the existing fauna, as far as regards fundamental types, still there is a visible difference in the details of its composition, and in its subordinate generic forms. With regard to the species of this family, the greater number of them in those former ages exhibited gigantic forms; while those now existing are all small animals, scarcely one reaching the middle size. We may therefore conclude that this family, as now existing, when compared with what it formerly was, is developed on a reduced scale, as well with reference to the number of genera and species, as to the size of individuals.

Third Family, *Tardigrades*, Sloths.

Two genera of this family, both now extinct, formerly inhabited this district. The first of these I have spoken of in my account of the Cave of Maquiné, under the name of *Megatherium;* but subsequent investigations have satisfied me that it should be separated from that genus. It has four molars in the upper, and three in the lower jaw. The teeth are somewhat compressed cylinders, with an even grinding surface; which, however, by trituration becomes hollowed in the middle, so as to be surrounded by a continually increasing margin, which, by the action of the teeth of the opposed jaw, usually has some irregular indentations. These teeth consist of an outer crust, composed of osseous *laminæ,* partly superimposed obliquely, and disposed one over the other, without any immediate contact, like the plates in a Voltaic pile. The teeth are fixed obliquely in the jaw; and the last molar of the upper jaw is distinguished from the rest by being much smaller, and of a different shape. For this animal, which, in the structure of its teeth, approaches nearer to the three-toed sloth than to the *Megatherium,* I propose, for the present, the generic name *Cœlodon.* I only know one species, about the size of the tapir; and which, from the spot where it was found, I call *Cœl. Maquinense.*

The second genus of this family, which I now proceed to describe, has been much longer known. It was discovered and first described by President Jefferson, who took it for a predatory animal; but Cuvier was the first to determine its proper place among the sloths, under the name of *Megalonyx.* But its connection with the other animals of that order was so obscure, from the imperfect state of the fragments, that Pander and Dalton, who have described and figured the skeleton of the great *Megatherium* on the spot, considered the animal described by Jefferson as specifically identical.

The numerous remains which several species of this genus have left in the caves of Brazil, enable me not only to elucidate this point in science, but also to throw some light on the habits of this very remarkable animal, so different from any now living.

The *Megalonyx* forms a genus perfectly distinct from the *Megatherium.* It resembles, nay exceeds, the latter, in the thick clumsy shape of its body. The construction of the fore and hind feet is the same; but in the formation of its head, and particularly in the number, shape, and position of its teeth, it differs greatly. The *Megalonyx* has five molars in the upper, and four in the lower jaw. The teeth are flattened cylinders, somewhat curved, both in a longitudinal and transverse direction, and inserted obliquely in the jaws: they present a rather excavated grinding surface, the margin of which is *notched* at one end. The posterior molar of the under jaw has a somewhat *consolidated* aspect, as if it were formed by the union of two. Like the *Megatherium*, it had a long and strong tail, composed of many *vertebræ*, and extraordinarily powerful, especially at its root. Its ribs are grooved longitudinally along its external surface; a character only observed in some species of *Dasypus.* However, as it is not my object to give here a detailed description of this animal, I will confine myself to the general observation, that most of the points in which *Megalonyx* differs from *Megatherium*, present so many approximations to the modern sloth; one of which is of too great importance to be passed over without examination. It is well known that in the three-toed sloth, the foot articulates with the *tibia*, not, as in all other *Mammalia*, in a plane at right angles to the leg, but on a plane continuous with it; so that if this creature wished to stand upright upon its four legs, it would have to rest upon the outer edge of the soles of its feet; while, in order to rest upon the flat of the sole, it must lie on its belly, with its feet stretched out straight from its body. This peculiarity, of which we are as yet acquainted with only this one living example, is repeated in the *Megalonyx*, although the mechanism whereby this distortion, if we may so call it, is effected, differs much in the two animals. In the sloth it is produced by the singular mode in which the *tibia* and *fibula* articulate with the *astralagus;* while in the *Megalonyx* this joint occurs in the manner usual among *Mammalia;* and the irregularity of the plane of the foot depends on the articulation of the first row of the metatarsal bones with the *astralagus* and *calca-*

neum; which last, therefore, entirely differ from those of all other mammals, in the form and position of their anterior articular surfaces.

Inasmuch as the mechanism of the sloth's movements, so far as I am aware, is not very well known, I may take the liberty of recording the observations I made on the three-toed sloth, *Bradypus torquatus*, which I kept in my house for a considerable time. This animal climbs with remarkable sureness and aptitude, although, as is well known, with a degree of slowness which, however, may be called rapidity in comparison with its terrestrial movements. The manner in which it moves is this :—Lying on its belly, with all its four extremities stretched out from the body; it first presses one of its hind feet with all its might against the ground, whereby the corresponding side of the body is a little raised. The fore-leg on the same side thus becomes sufficiently free for the animal to advance it a trifle forward. It then hooks its powerful claws fast in the earth, and so drags its body a little onwards. The same manœuvre is next repeated on the opposite side; and thus the poor creature progresses in the slowest and most laborious manner possible. But this mode of progression requires certain conditions of the surface; for if it is not soft enough to admit the insertion of the claws, or if there are no inequalities for them to hold by, the sloth is completely deprived of the power of changing its position. For instance, when I laid it on a table of polished mahogany, it could not advance the least, notwithstanding all its exertions. But in proportion as the sloth's organization unfits it for terrestrial progression, is it wonderfully adapted to climbing trees. With its long arms it reaches high up, and clings fast to the branches with its strong crooked claws. The *inverted* position of the soles of its hind feet gives it a power of *grasping* the trunk of the tree, which no other mammal possesses. So that truly, when we see it climbing a tree, we can scarcely believe it to be the same animal that lies so helpless on the ground. Hence we see, that the sloth's organization is entirely adapted for living in trees. Compared with the slowness of its motions, it is the best climber among mammals, while it is the worst walker; or rather, it is the only mammal that can neither walk nor stand. These peculiarities depend on three principal points in its organization :—1st, the great length of its anterior extremities, in comparison with the posterior; 2dly, its powerful, crooked claws; and 3dly, the *irregular position* of its hind feet.

Let us now see how far the results to which we have been led by the consideration of the living sloth's structure and habits may serve to elucidate the habits of that extraordinary tenant of a former world, now under our consideration. Now, the *Megalonyx*, like the sloth, is provided with powerful clavicles; like it also, its anterior extremities are longer than the hind; its toes are armed with immense claws; and lastly, the sole of its hind foot is turned inwards instead of downwards.

The first of these characters, or the existence of perfect clavicles, proves that this animal used its fore-extremities for more purposes than for walking; which position I take to be incontrovertible, as it is founded on a rule that has no exception among mammals. The purposes for which mammals, provided with clavicles, employ their anterior extremities, are the following:—1st, for flying, as in the bats, with which we have nothing here to do; 2dly, for the apprehension of food, and the bringing it to the mouth, either with one hand, as in the apes, or with two, like most rodents, some marsupials, &c. Now, the first of these purposes requires a peculiar disposition of the fingers, and a certain freedom of motion in them; which conditions are both wanting in the *Megalonyx*, notwithstanding this animal, as well as the *Megatherium*, has in reality been classed by Wagler among apes. The second purpose is confined to animals that have short *fore-limbs*, and whose dental system is adapted to gnaw substances which they hold in their paws; which conditions being also inapplicable to the *Megalonyx*, there can be no occasion to dwell longer on them; 3dly, for tearing asunder their prey, as in the feline tribes. This purpose requires only an imperfect development of the clavicles, but at the same time, a peculiar arrangement in the shape and attachment of the claws; which, again, is not the case in the *Megalonyx*. Besides, the dental system proves it to have been graminivorous; although authors have not been wanting (as Jefferson and Faujas) who have placed it among the *Carnivora*.

There remain, therefore, only two functions that are exercised by the animals provided with bones for the attachment of claws; viz. digging and climbing. And it is the more necessary to confine our attention to these two points, inasmuch as we see that all animals belonging to the same order as *Megalonyx*, exercise one or the other of these functions. Their immediate instruments for these purposes are claws, which consequently are powerfully developed in all; but in none in so high degree as in the extinct genera *Megalonyx* and *Megatherium:* in these they have reached the highest degree of development we yet know in the animal kingdom. It would be contrary to all experience in natural history, to suppose that an organ should lose its function and significance precisely at the point where it is most perfectly developed. We cannot doubt, therefore, that the powerful claws with which we find *Megalonyx* and *Megatherium* armed, have had their use. We may even conclude with certainty, that the habits of the animals were closely connected with these organs, and that their very existence depended on them. Now, as we only know of two uses for strong claws in *Mammalia*, digging and climbing; and as these two purposes require different anatomical arrangements; it will not be very difficult to decide for which of them the powerful organs in the extraordinary creatures we are considering were intended. We find among the animals the most perfectly organized with respect to burrowing, such as *Talpa*, *Spalax*, *Condylurus*, &c., that the claws are strong, long, broad, and nearly of equal size, that all the digits are provided with similar claws, and are extended in almost the same plane as the hand, which is of considerable breadth. Next to moles, the best diggers or burrowers are found in the order to which *Megalonyx* belongs, especially in the family *Dasypus*; but the different species of that genus are not all equally well provided in this respect. The best diggers are the *Cabassous* (*Xenurus*, *Priodon*), among which we again recognize the same characters as in the moles; a broad hand, all the digits provided with claws, very broad, and nearly equal. In the *Euphractus* the hand is somewhat smaller, as are also the claws, although their number remains undiminished; consequently, the species of this family cannot compete with the former as burrowers. In the proper *Dasypus*, the number of digits provided with claws is reduced to four; and they are so inferior to the first described, in the faculty of digging, as to avail themselves, for the most part, of the burrows the others have excavated.

Let us next examine the plan of construction of the hand in those animals that use their claws as hooks to climb with. We find the most perfect form of this kind in the sloth. Its claws are extraordinarily long, curved and compressed: they are so articulated as to be incapable of extension, whence, during the animal's repose, they are bent under the fore foot; and at the utmost, can only be extended so as to form a right angle with it. Again, not more than three digits in some, and two in others, are furnished with equal claws; and the hand is small. We thus see, that hands adapted for climbing and burrowing, are constructed on two almost opposite plans; let us then examine to which of them the *Megalonyx* bears the most resemblance.

The claws in the *Megalonyx* (and still more in the *Megatherium*), were extraordinarily long; according to all appearance, longer even than in the sloth, which has the longest of all existing mammals. They are neither flat-shaped, as in the burrowers, nor are they compressed, as in the sloth; on the contrary, their dimensions as to height and breadth are nearly equal. They are curved longitudinally, as in the sloth; and have the same peculiarity in their articular surfaces, that they cannot be extended in the same plane as the hand. Their number, also, as in the sloth, is reduced to three; a reduction we do not else find in any burrower. It is therefore evident, from this comparison, that the hand of *Megalonyx* was constructed rather on the plan of the sloth than of the burrowers; and that all its provisions were ill adapted for digging.

(To be continued.)

ART. II.—*Observations on the Young of the Salmon, more particularly on the Samlet, or small Fish found in the Wye and other Rivers, in the autumn months, called, in Herefordshire, "Lasprings, or Gravel-Lasprings."* By THOMAS JENKINS, Esq.[1]

VARIOUS opinions are entertained respecting the above-named fish, but up to the present time, as far as I am aware, their specific identity has not been clearly ascertained. I here particularly allude to those *seen in the autumn*; all observers agreeing that those of the spring, also called 'lasprings' in this locality, are the produce of the salmon. An opinion prevails in this neighbourhood that the samlets are peculiar to the Wye, and one or two other rivers; so far from this being the case, I have myself taken them in *nearly thirty* different rivers in England and Wales, where they are known under the several local names of lasprings, gravel-lasprings, salmon-pink, salmon-smelts, samlets, par, scarlings, seals, smoults, gravelings, fingerlings, and small trout.

The samlets are generally thought to constitute a species of themselves, not growing larger than we see them here, where they attain the average length of four inches; some, however, entertain the opinion that they are hybrids, the produce of the salmon with the sea-trout, or with the common trout; of which latter opinion was the late Sir Humphrey

[1] Read at the Soirée of the Herefordshire Natural History Society, 19th February, 1840. Communicated by the author to the Mag. Nat. Hist.

Davy: few believe them to be of the same species as their namesakes of the spring, that is, the salmon-fry; but with these latter, however, I think I shall be able to identify them. I have taken samlets at the foot of several considerable waterfalls in Wales, whereas above those places, being beyond the ascending power of the salmon, great as it is known to be, small trout of the same size may be taken, but not *one* samlet: were the former a distinct species, inhabiting those rivers the whole year, I presume they would, like the trout, be found above as well as below the falls; and I will venture to assert, from my experience, that the autumn samlet, called by whatever name it may, will be met with in *all* rivers frequented by salmon, and *in no others*; that they will be found as far up those rivers as the salmon go, and no farther, which is strong presumptive evidence of the one being the produce of the other.

With regard to the milt, or soft roe, contained in samlets in the autumn months, this appears to be the mere germs of spawn not come to maturity; and the same appearance is to be seen in small salmon of half-a-pound and upwards, at that season of the year. Indeed, the absence of the ova, or perfected spawn, at any time of the year (and I have examined samlets in almost every month), clearly shows that they are not come to their full growth. But what I rely upon more than anything else, is, that the anatomical structure will be found to correspond exactly with the salmon, and the salmon-fry. In addition to this, the bones of the samlets are soft and tender, unlike those of a fish come to maturity, as may be seen on examining the bones of any small fish of full growth, a minnow, for instance. It is true, that salmon generally spawn at one particular time of the year, namely, December and January; and it does, at first, seem rather strange, why the produce should appear at different times of the year;—but the trout furnishes us with a similar instance; this fish is known to spawn about the same time as the salmon, yet very small trout will be met with at all times of the year, without surprise to the angler: I have myself seen them less than minnows, in September, and also the same size in April, though it is clear that the young trout of September, and those of April, could not have been spawned at the same time, and I call attention to this, in reference to the spring and autumn samlet. Another circumstance which I will here mention, is, that so late as December I have taken with a fly, small salmon of half-a-pound each (and I trust I am sufficiently acquainted with these fish, to say that they belonged to no other species), which, according to the rate they are known to increase in size, must have been samlets in the previous month, and about June must have been spawned; thus accounting for the samlet, or salmon-fry, so late as November. Indeed, I can say, that I have taken them in almost every month of the year; they are by far in the greatest abundance in April and May, no doubt the produce of the general spawning time of December and January.

I come now to the opinion, sanctioned as it is by a great chemist and philosopher, though, I believe, no naturalist, the late Sir Humphrey Davy, that the samlets are a breed between a salmon and sea-trout, or the common trout. With due deference to so high an authority, I must beg leave to say, that such an opinion is quite untenable; for it is to be observed that both the sea-trout and salmon-trout are rarely to be met with in any of the rivers of North Wales, whereas in most of the salmon rivers in that part of the principality, I can state from experience, that the autumn-samlet will be found in abundance. Then, as to the common trout, in many of the Welsh salmon-rivers, it is rare to meet with one so large as two pounds in weight; the samlet is, therefore, not likely to be the offspring of two fish so different in size as the salmon and trout; and I may add, that these little fish have not even the analogy of bearing that proportion in size to the originals, which such deviations from nature in the animal creation are invariably known to possess.

Since committing the above observations to paper, I have seen Mr. Yarrell's work on British Fishes, wherein an opinion is expressed by the author, that the samlet, or par, as it is there called, is a distinct species, and in this opinion he is joined by Sir William Jardine and Dr. Heysham, of Carlisle: the last-named gentleman also stating that the samlets spawn in December and January, going down to the sea in the spring, and returning in the autumn; but how he comes to this conclusion does not appear. Before these fish are exposed to indiscriminate destruction, on the above authority, and not preserved as the young of the salmon ought to be, I must be allowed, on behalf of the salmon species in general, though not a specially retained advocate, to make a few further observations in their behalf: and, differing so widely as I do from three such distinguished and experienced naturalists, it will be necessary for me to go into more minute details in support of my own conclusions, and to which I will beg to claim further attention.

I have first to remark, that in an examination of nearly

400 individuals, made at different times, by Dr. Heysham, that gentleman does not say that the ova were perfectly formed in any one out of that number; that he was able to distinguish the male from the female spawn may be possible. There are, however, two, mentioned by Mr. Yarrell as having contained ova, but these, it is to be remarked, in March, at a time the salmon-fry are known to be in the rivers; so that although they are set down as spawning in December and January, yet the only two ever said to contain full-sized roe, were taken nearly three months afterwards, and had not then spawned. The size of these fish, I observe, is not stated, but mention is made of one seven inches in length, with the roe said to be in a forward state. Now this is nearly double the usual size of the samlet, and although I have taken this fish in twenty-eight different rivers, I have never met with one of that size which had not been to the salt water, and shown itself as a complete young salmon; and of that size they will only be found in or near the estuaries. The question therefore arises, what fish these were? I would ask, were the number of *vertebræ* examined? Might they not possibly have been young trout, to which the samlet bears an external resemblance? I am not aware at how small a size a salmon might contain mature ova, but I will just observe, that I have seen a trout of not more than three ounces, containing full-sized spawn, although that fish is known to attain a size occasionally of fifteen or twenty pounds. It is admitted that samlets have never been seen spawning in the rivulets and shallow streams, like trout, and I can confidently say, there is no evidence of their spawning at all, whilst their dimensions are such as are given to this *assumed* species. I have myself examined some hundreds of samlets, at various times of the year, and particularly about the end of September, at which time I have generally found the greatest accumulation of roe, or rather milt, for it has that appearance, and it appeared to me, that if they spawned at all, they spawned about that time, but I have never been able to discover any appearance of ova; and in October and November, so near the period at which they are said to spawn, when a more forward appearance would be expected, I have especially remarked *that they have not been so full.* As the samlets are said to spawn in December and January, this is a time of year unfavourable to angling, and few examinations can then be made, either to prove or disprove the fact. I have, however, taken two on the 12th of January, and if the samlet contained spawn at that time, the probability is, that one of these, at least, would have been a

spawner, but that was not the case; they both contained the soft roe, which, on their being handled, came from them of a thick cream-like appearance. These two fish, which ought then to have attained their full growth, they had been distinct species, were under the usual size. It becomes a question, whether or not this accumulation does not disappear spontaneously, as just mentioned, at that season of the year, and that the same accumulation may take place at the usual season in the following year, and become perfected spawn when the fish attains a size adapted for maturing it. Whether this be the case or not, it must be admitted that such an accumulation of milt as is to be seen in these fish in the autumn, is extraordinary, and is the *only* circumstance, as far as it goes, that has come under my observation, in favour of the opinion that the samlet is a distinct species.

That these fish go down to the sea, I have no doubt; it is the natural instinct of the young of the salmon to do so; but that they return again the same size as they go down (as is asserted by Dr. Heysham), I must utterly deny to be practicable in the situations where I have found them, and which I think will appear from what I shall have to say on this point. They are to be taken as far up the river Wye as Llangerig, eight miles above Rhayader; and, independently of the distance being nearly 200 miles by water, they would have a cataract to surmount, above the bridge at Rhayader, of about three feet, with numerous other falls and rapids; I believe I may assert, that there is no instance in the history of fishes of so small a size, taking yearly, such a voyage in the fresh water, to say nothing of its impracticability.

Near the celebrated pass of Pont Aberglaslyn, in Carnarvonshire, the river becomes a roaring cataract during the course of half a mile, falling over ledges of rock, varying from one to four feet; and at the mill at Beddgelert, a mile higher up the same river, is a weir of two to three feet, where I have witnessed a fish, nearly a pound weight, fail in its attempt to ascend. The ascent, therefore, is not likely to be accomplished by a fish weighing scarcely two ounces, independently of the cataracts mentioned below; yet, I have taken samlets in the river, above the weir, in the lake of Llyn-y-Dinas; and in the same river, as far up the vale of Gwynant as the foot of Snowdon. They are also to be taken in the Ogwen, in the vale of Nant-Frangon, as far up that river as Mr. Pennant's slate quarries, which is as high as the salmon go, and no further, being there stopped by a fall of about sixteen feet; to go thus far, they have to ascend innumerable rocks and falls, such as could only be surmounted by the salmon, or other large fish. Near Bangor Iscoed, on the Dee, is a weir of such a height as to prevent all salmon under three or four pounds from ascending ; at Llangollen, below the bridge, is a weir of about six feet, and two miles above, is another weir, of about three feet, with several natural falls and rapids on the same river : yet I have taken samlets in September, some miles above the highest of these weirs, and also in the Alwen, a tributary of the Dee, near Corwen. That these little fish can surmount the difficulties here enumerated, must be considered impossible, and their existence can only be accounted for in those situations, by admitting them to be the young of the salmon.

It is acknowledged that the fish in dispute are peculiar to our salmon-rivers, as I have before remarked, and not one single river in theUnited Kingdom is pointed out as containing the one, that does not also contain the other. It is, however, said, that samlets are to be found in some streams in the Western Isles, in which salmon are not. The Western Isles, I must say, are rather remote from observation, and that mistakes may occur in assertions of this kind, from our not being sufficiently informed, the following circumstance will show. Being fishing in the month of September, in the Ceiriog, four miles south of Llangollen, amongst the few trout I had taken, was a fish which I immediately recognized as the autumn-samlet, and which I considered as a proof that this river was frequented by salmon ; although I had previously been informed such was not the case, the salmon being stopped by a weir somewhere about Chirk ; however, on fishing a few days later, lower down the same river, I learnt that a salmon had been taken there the day before, although it was acknowledged to be a rare circumstance. Now, a person relying on the first information, might have set down that river as not containing salmon. I have taken the *autumn*-samlet in twenty-eight different rivers, and have never met with *one* salmon-river without them ; nor have I ever found them in any river unfrequented by salmon ; and if the branches of any considerable river be examined, there will be found many small rivers adapted to fishes of this small size, yet only in those branches containing salmon, will these fish be found. I must observe, on the assertion that samlets, having been spawned in the winter (as is said by Dr. Heysham), do not come to their full growth till late in the autumn, thus taking nine months to attain a size of barely two ounces ; I believe there is no instance of fishes of so small a size taking so long a time in coming to maturity, and such a circumstance is contrary to the order of nature ; for the salmon, as is well known, will attain a size of five or six pounds, in the same time.

One principal reason why naturalists are indisposed to consider the samlets as the young of the salmon, is, that the latter are supposed to spawn during the winter months *only*, and I observe considerable reliance is placed on this circumstance (erroneous though it be), in classing these fish as a distinct species. That salmon spawn at other times of the year, further and more accurate observations will prove, and to them I have already alluded in the present communication. In addition to what I have here brought forward, it is satisfactory to me to have had my views confirmed in this respect, having, since the first part of this paper was written, seen an extract from an old author and accurate observer (Francks), stating that, "being angling one hot sun-shiny day, he took umbrage under a tree near the river, from the heat of the sun, and there observed two salmon, male and female, in the act of depositing their spawn," which he describes very particularly, and which description coincides with later and more accurate observations of the present day; therefore, it must be admitted, that this "hot sun-shiny day" could not have been in the winter months. And in Mr. Yarrell's own work, I observe it is stated, that in Sweden salmon do spawn in the summer; is it therefore unlikely that they may occasionally spawn in the summer in this country, and in other months?

In further support of this I will mention, that in the estuaries, or within a short distance in the fresh water, the same net will occasionally draw out salmon of every size, varying from one to four pounds, and upwards; and if the mesh be sufficiently fine, down to the smallest samlet; which alone proves, from their different sizes, that they must have been produced at various times of the year. I have myself seen salmon of every size, varying from half-a-pound up to ten pounds, in each month, from May to December; yet Mr. Yarrell says there is no instance of one in the autumn under sixteen or eighteen inches in length; those who have been fishing in the fresh water within a short distance of the tide-way of rivers, will bear testimony to what I say, that they are numerous at that time, and it needs but little enquiry and observation to ascertain this, and even so late as December, as I have already noticed in the previous paper. I will just remark, that those between half-a-pound and three pounds in weight, are called, in different places, salmon-peal, morts, salmon-morts, grilse, sewin, and various other names, except the right one, which only tends to mystify the subject; and some will even pretend to say they are a distinct species, but any person acquainted with the salmon in their various stages, needs no information as to what species they belong.

It is a common observation amongst anglers, in the months of June and July, that there are, at that time, few or no samlets in the rivers (having, as is supposed, all gone down to the sea), and this opinion is formed, because few are then to be taken with the rod and line. This may be accounted for as follows :—the previous shoals, the produce of the general spawning time, having migrated to the sea, leave behind a superabundance of food for the lesser number that remain, being the produce of a later period of spawning, so that the invitation, with hook and line attached, is not taken so readily, and the angler can find but little sport, although several may even then be occasionally captured. That they are then tolerably numerous in the rivers, is proved by the success which I have known to attend the discreditable practice of netting with illegal nets at that time. In August and September, when there is not that abundance of insect food as in the summer months, the hook and line becomes again tolerably successful, and from this circumstance it is said, though erroneously, that there are a larger quantity of the samlets in the rivers, and a name is given them as a distinct species.

If one circumstance more than another tends to prove the autumn-samlet to be the young of the salmon, the following may be mentioned. In the estuaries of rivers, in the month of December, and doubtless, in other months (but I speak only from my own observations), these fish may be seen, varying in size, from two ounces to a quarter-of-a-pound, changing their red spots and trout-like appearance for the darker spots and silvery appearance of the salmon, those of the larger size having completely acquired their salmon-like appearance, and which external change, it is well known, the salmon-fry undergo in the salt-water. That this alteration in their appearance is caused by coming in contact with a different element, joined with a different description of food, I think is very probable. The exact time I speak of was the 29th of December. Now, I think it must be admitted, that seeing these fish in their various stages in the salt water, at a time when they are said to be but of one size, and spawning in the fresh water, must make an impression on the mind of the observer that they are not a distinct species, but the young of some other fish, and that fish the salmon. I know of no better situations for coming to a right conclusion than places of this kind, where they may be seen in all their various stages, and I would beg to call upon those who reside near the tide-way of rivers, to turn their attention to the subject, so that the observations of one person being confirmed by others, the public mind will become convinced, and these fish preserved, as the young of salmon, from indiscriminate destruction.

I trust I have brought forward sufficient circumstantial evidence, to establish the correctness of my belief, that the samlets are the young of the salmon; but as we cannot be too cautious in cases of this kind, it is my intention, should my health permit, to visit various parts of Scotland, in the ensuing summer, where, in the numerous rivers of that country, I may possibly pursue the subject further; in that event, I shall be happy to communicate the result of my observations. In the meantime, I would beg to name it as worthy the attention of others, perhaps more capable of judging than myself, that, although Natural History is receiving a large share of attention in all its branches, yet the history of the salmon, in its various stages, appears to me to be very imperfectly understood, though one of the most generally distributed and valuable of our fishes.

Hereford, January 29, 1840.

Art. III.—*Remarks on the Species of* Cicindela *and* Elaphrus, *mentioned in Olivier.* By The Rev. F. W. Hope, F.R.S. F.L.S.

Cicindela, Linnæus.

Cicindelidæ, Leach. *Cicindeloidea*, Hope.

Olivier's Species.	Country.	Genera of Authors.
1 *maxillosa*	Cape of Good Hope	*Manticora*, Fabricius.
2 *aptera*	East Indies	*Tricondyla*, Latreille.
3 *longicollis*	Siam	*Collyris*, Fabricius.
4 *megacephalus*	Senegal	*Megacephala*, Latreille.
5 *grossa*	Coromandel	*Apteroessa*, Hope.
6 *Chinensis*	China	*Calochroa*, Hope.
7 *cincta*	Sierra Leone	*Calochroa*, Hope.
8 *bicolor*	East Indies	*Calochroa*, Hope.
9 *campestris*	England	*Cicindela*, Linnæus.
10 *hybrida*	England	*Cicindela*, Linnæus.
11 *nemoralis*	France.	
12 *purpurea*	North America.	
13 *sylvatica*	England.	
14 *tristis*	North America	*Oxycheila*, De Jean.
15 *interrupta*	Sierra Leone	*Calochroa*, Hope.

Olivier's Species.	Country.	Genera of Authors.
16 *lunulata*	Cape of Good Hope.	
17 *lurida*	Cape of Good Hope.	
18 *flexuosa*	Spain	
19 *Capensis*	Cape of Good Hope.	
20 *catena*	East Indies.	
21 *Germanica*	England	*Cylindera*, Westwood.
22 *tuberculata*	New Zealand	*Cicindela*, Linnæus.
23 *unipunctata*	South America	*Cicindela*, Linnæus.
24 *Cajennensis*	Cayenne	*Diplocheila*, Brullé.
25 *sexpunctata*	Malabar	*Calochroa*, Hope.
26 *quadrilineata*	East Indies	*Cicindela*, Linnæus.
27 *biramosa*	East Indies	*Cicindela*, Linnæus.
28 *sexguttata*	Carolina.	
29 *punctulata*	North America.	
30 *octoguttata*	Sierra Leone	*Cicindela*, Linnæus.
31 *trifasciata*	Guadeloupe.	
32 *Carolina*	Carolina	*Tetracha*, Westwood.
33 *Virginica*	Virginia	*Tetracha*, Westwood.
34 *maura*	Algiers	*Cicindela*, Linnæus.
35 *minuta*	East Indies	*Cicindela*, Linnæus.
36 *emarginata*	Paris	*Drypta*, Fabricius.

Elaphrus, Fabricius.

1 *riparius*	England	*Elaphrus*, Fabricius.
2 *paludosus*	Paris	*Elaphrus*, Fabricius.
3 *caraboides*	Austria.	
4 *littoralis*	Paris	*Bembidium*, Illiger.
5 *aquaticus*	England	*Nothiophilus*, Dumeril.
6 *semipunctatus*	England	*Nothiophilus*, Dumeril.
7 *flavipes*	England	*Bembidium*, Illiger.

Remarks and Annotations on the Species of *Cicindela* and *Elaphrus* mentioned in the above Tables.

Sp. 2. *aptera*. This insect, according to M. Brullé, is a *Tricondyla* of Latreille, and *Colliuris major*, Lat. appears to be the same insect as *Collyris aptera*, Fab. In a letter lately received from Westermann of Copenhagen this opinion is incorrect, as he writes *Col. major* Latr. is quite distinct from *Col. aptera* Fab. He remarks it is certainly not apterous, but is a true winged *Colliuris*. It is nearly as large as *longicollis*, black, and quite different from all the blue species of *Colliuris:* vide Westermann in litt.

Sp. 3. *grossa*. Now an *Apteroessa*, Mihi: for an account of its characters vide 'Manual,' part ii. page 159, fig. 1.

Sp. 9. *campestris*. The true type of *Cicindela:* the green varieties of *Cic. purpurea*, Olivier, according to Mr. Kirby, seem to be the American representatives of the European *campestris*. In Africa, at the Cape of Good Hope, we meet with *Cic. rotundicollis*, which may be regarded as representing it on that continent. In a collection of insects also made by Mr. Strickland in Asia Minor, there appears to be two undescribed species which closely resemble *Cic. campestris*, Lin.

Sp. 11. *hybrida.* For various observations respecting this species, the reader is referred to Mr. Stephens's 'Illustrations of British Entomology,' vide vol. i. page 8, &c. *Cicindela hirticollis*, Say, appears in the New World to represent the European *hybrida.*

Sp. 12. *purpurea.* This insect is subject to vary considerably; by inexperienced entomologists some varieties are regarded as distinct species. I suggest the adoption of the Fabrician name of *marginalis* instead of the above, on the ground of priority.

Sp. 15. *interrupta.* This species belongs to my genus *Calochroa;* it closely resembles some of the dark varieties of *C. Chinensis.*

Sp. 16. *lunulata.* From Dr. Gistl's description I suspect that the insect which he has named *Cic. Hopei*, is only a variety of the Fabrician *lunulata.*

Sp. 20. *catena.* Olivier gives the East Indies and the Cape of Good Hope as the localities of this species; in the latter continent I am inclined to think it never occurs. The specimens purchased at Cape Town, from Verreaux and other naturalists, are probably obtained from merchantmen trading with India. A few years ago I purchased a collection *labelled as "Insects of the Cape."* At first sight I was aware that they were peculiar to the East Indies, although I could not state the exact locality; on removing the paper I discovered a memorandum that they were collected at Singapore, and afterwards sold to a dealer at the Cape: the locality turned out correct.

Sp. 22. *tuberculata.* This insect appears to be exceedingly rare: it is rarely to be found in modern collections. The Banksian cabinet contains almost the only specimen which has fallen under my notice.

Sp. 23. *unipunctata.* Olivier gives South America as the locality for this species; I am inclined to think it peculiar to North America.

Sp. 25. *sexpunctata.* This species enjoys a very wide range; it occurs at Bombay, Ceylon, Madras, Calcutta, Singapore and Assam: it is subject to considerable variation of markings and colour, some of its varieties have been considered as distinct. One, which in General Hardwicke's collection was named by me *Cic. flavomaculata*, is only a variety. Olivier's figure is execrable.

Sp. 26. *quadrilineata.* This species is exceedingly abundant. I have seen Indian basket-work ornamented with the *elytra* of this insect; the effect was good. From the account I received from my informant, the Malays and some of the races which inhabit Singapore, adorn their handy-works with the wings of the above insect.

Sp. 30. *octoguttata.* I have thought proper to change Olivier's locality for this insect; he records it as a species from South America. Fabricius mentions North America; Palisot Beauvois the Island of St. Domingo; Schönherr, in his 'Synonymia Insectorum,' gives Sierra Leone as its native country; and with the latter authority I am inclined to side.

Sp. 31. *trifasciata.* This insect must not be confounded with our European species, which is evidently distinct. The *trifasciata* of the New World enjoys a very extended range, occurring in North and South America as well as in several of the West Indian isles.

Sp. 35. *minuta.* This species has not fallen under my notice; I give it as a *Cicindela* on the authority of French entomologists.

Sp. 36. *emarginata.* Now a *Drypta* according to Fabricius. Olivier considered *Drypta* as a *Cicindela;* according to modern views the *Dryptidæ* constitute a particular family, consisting of several genera. As far as is at present known respecting *Drypta,* it belongs to the Old World. The European species are comparatively rare inland; in Sicily and Italy it is abundant under the *rejectamenta maris;* in England I believe it has only been found on the coast of Hastings and Devonshire. In Scotland, some years back, I captured it at Leith in a similar situation. Although it does not appear to be known in the New World, I think it not improbable that it will eventually be found there, occurring perhaps in North as well as South America. The East Indies afford several species; those from tropical Africa are worthy of notice, some in my collection are from the banks of the Gambia, and others from Sierra Leone.

Elaphrus, Fabricius.

The genus *Elaphrus* was by Linnæus regarded as a *Cicindela*, Geoffroy properly considered it as belonging to *Carabus* rather than to the former genus; he however injudiciously applied to the species the name of *Buprestis.* Fabricius first separated them from *Carabus*, and they now form a family by themselves, according to the views of Messrs. Stephens and

Kirby. My friend the Comte de Castelneau arranges with them the *Lebiadæ*. Preferring the English authorities, in my Manual I have adopted their views. As a group it appears (as far as is known at present) to frequent northern climes, no instance having occurred of its appearance in southern regions.

Sp. 1. *riparius* of Linnæus and Olivier appears to be the selfsame species, the *riparius* of Schrank however is *El. uliginosus* of Fabricius.

Sp. 2. *paludosus*. This is probably only a variety of the preceding species.

Sp. 3. *caraboides*. This insect is apparently unknown in the Parisian collections at present. Schönherr evidently regards it as a distinct species. It is singular that the Baron De Jean does not mention it in his last Catalogue.

Sp. 4. *littoralis*. This species cannot be considered as an *Elaphrus*. The Baron De Jean, in his Catalogue of 1833, applies the name of *littoralis*, Megerle, to another species of *Elaphrus* from Hungary; it would be better to substitute that of *Megerlei* or *Dejeanii* for the last species, instead of the name already used by Olivier.

Sp. 5. *aquaticus*. Now a *Nothiophilus* of Dumeril: for an account of our British species I refer to Mr. Waterhouse's Monograph in the first volume of the 'Entomological Magazine,' in which eighteen species are described.

Art. IV.—*Notes on Irish Natural History, more especially Ferns.* By Edward Newman, Esq., F.L.S., &c.

(*Continued from page* 124).

It was a brilliant morning when I took my leave of Sheely, and a last lingering look towards those beautiful caverns, which, once to have seen, is worth more than the fairest picture of imagination. The road towards Cahir is wide and straight; it possesses little to interest the traveller, except the joyous faces of the happy people, whom I met by crowds on their way to the market at Mitchelstown. The poorer Irish appear to me the most easily contented, and the most philosophically and truly happy of any peasantry I have ever seen; faithful, generous, warm-hearted, fearless, and reckless: they smile in peace over a handful of bad potatoes, and devoutly thank the Providence who provides it. Oh!

what have politicians to answer for who try to teach these people that they are unhappy! I verily believe that there is not a peasant in Tipperary who has not a lighter heart and a clearer conscience than half the legislators of St. Stephen's. To use the words of Lady Chatterton, "I have come to the wise determination of allowing people to be happy in their own way; the more we see of the world the more convinced must we be, how totally independent of every outward cause and circumstance is happiness: that it springs entirely from the mind the Irish are living and laughing proofs[1]."

The fine range of Galtees forms the horizon on the left, the Knockmildown chain on the right, and as I approached Cahir, a rich and highly cultivated valley opened before me, lying along the base of the Knockmildowns, and stretching towards Clonmel. In England you meet with few more cultivated scenes than this; it is watered by the Suir, and is exceedingly productive. A considerable part of this fine tract belongs, as I was told, to Lord Glengall, and is let to the actual occupier, at the high rent of 35*s.* to 40*s.* per Irish acre: I say *actual occupier*, because this enormous rent does not go into the pocket of the freeholder; the rent received by his lordship being much less.

Cahir is an interesting little town, situated on the Suir: here I observed a well-fruited orchard, and several very good gardens, in which I was particularly struck with the beauty and luxuriance of some of the plants, more particularly hydrangeas, fuchsias, myrtles and laurels; the walls, especially of the castle, Lord Glengall's residence, are covered with *Polypodium vulgare*, occasionally mixed with *Asplenium Ruta-muraria*, *Asp. Trichomanes* and *Asp. Adiantum-nigrum*. The castle is kept in good repair, but is not a remarkably ornamental building.

I proceeded by Bianconi to Clonmel, through a rich flattish country, finely wooded, where there were little hills; the land is highly cultivated and very productive. Clonmel is pleasantly situated on the Suir, and is surrounded by gentlemen's seats, in beautifully wooded demesnes. The country continues rich and well cultivated to Carrick-on-Suir; the hills continue to be luxuriantly wooded, and the river, which runs near the road, is very picturesque; the stone walls are but few, and English-looking hedges tolerably abundant. The crops of wheat, oats, and barley, promised well. Carrick is an old town, with abundance of spirit stores, whence issued a swarm of beggars. Four miles from

[1] 'Rambles in the South of Ireland,' vol. i. p. 12.

Carrick is Pilltown, a most delightful village: nearly all the cottages are of the neatest possible description, and half hidden by wreaths of clematis, roses, myrtles, and fuchsias. The timber is also very fine, the gardens brilliant with flowers, and the crops of corn in the neighbourhood clean, even, and most abundant. I have seen no village in England superior in neatness or beauty to Pilltown. I saw a field of oats cut, and in sheaves, not a usual mode of husbandry in England: the wheat was apparently ready for the sickle.

From Pilltown to Waterford the country is highly cultivated, very English looking, and very beautiful; the hills are wooded, and the approach to the city of Waterford, over the wooden bridge, is very picturesque. Between Clonmel and Waterford I observed the following ferns:—*Lomaria spicant*, *Pteris aquilina*, *Polypodium vulgare*, *Polystichum aculeatum*, *Lastræa Filix-mas*, *Las. dilatata*, *Athyrium Filix-fœmina*, *Asplenium Adiantum-nigrum*, *Asp. Trichomanes*, *Asp. Ruta-muraria*, *Scolopendrium vulgare*, *Ceterach officinarum*, and *Osmunda regalis*: the last-named species had been gradually getting more rare after leaving the boggy districts, and was now but seldom seen.

Waterford is a one-sided place, having its principal street (called the quay) situated at the very outside of the town; this quay is fine, nearly a mile in length, and crowded with shipping; the river Suir is here really noble, and capable, at high water, of floating ships of any burthen. Almost immediately below the town, the Suir is joined by the Barrow, a still more noble and beautiful stream, and in my estimation, one of the finest rivers in the kingdom. From Waterford I re-crossed the long wooden bridge, and proceeding towards Ross found the country poor, and although generally cultivated, producing but meagre crops, and those principally potatoes. There was nothing in Ireland struck me as more requiring reform than the mode of cultivating potatoes. In London, every one expects to see a good potato on his table, as a matter of course. I never recollect seeing at a Londoner's table, a dish either of half-boiled potatoes, or watery potatoes, or waxy potatoes, or potatoes full of bruises and sore places: such things can be obtained in London, of a certainty, but never at the table of a Londoner. (I will just observe, *par parenthèse*, that every vegetable is better in London than in any part of the United Kingdom.) Now, in Ireland, there appears to be no care taken, when setting a crop, that all the seed be of one variety. The earliest and latest sorts very commonly occur in the same row; and I have often seen on the same dish, some potatoes green and watery, some waxy, and some beautifully floury. The season for each would have come round in its turn, but the Irishman knows but one season for setting potatoes, and one for getting them in. I remarked that the only criterion by which an Irishman judges of the value of his potato crop, is the vigour of the haulm, and *this* every horticulturist in England would consider most fallacious; for it is well known that the portions above and below ground often increase or decrease, in an inverse ratio. A second fault is the carelessness with which they are harvested; throughout the West and South of Ireland, at the inns, you will constantly see halves and fractional parts of potatoes, which almost every one rejects, and which are, therefore, wasted. At Ross I made many enquiries about the price of potatoes, seeing that root was so staple an article of agriculture. I found the *then* price was $3\frac{1}{2}$*d.* to $4\frac{1}{2}$*d.* per stone of 14lbs., that they had been as low as $2\frac{1}{2}$*d.*, and this only a short time previously; but owing to the failure in the west, a very considerable export of potatoes from Ross to Clifden, Galway, Tralee, &c., had taken place; and this diminution of supply had raised the price: at Tralee, the price of potatoes was $6\frac{1}{2}$*d.*, at Galway, 7*d.*, and at Clifden $7\frac{1}{2}$*d.* per stone, when I was at those towns; this is an unusually high price, and does not correspond with the price of labour, 6*d.* or at most 8*d.* per day. I hope the readers of the 'Magazine of Natural History' will not grumble at this dissertation on potatoes: if they do, I can make no apology for its introduction; for I conceive a true lover of his race, as a naturalist ought to be, cannot consider the welfare of the Irish peasant a subject beneath his notice; and I believe the introduction of a dozen good productive varieties of the potato would be a blessing to Ireland, far less equivocal, that the political nostrums so frequently proposed.

At Clonroche, on the road between Ross and Enniscorthy, I was struck by the preparations making by the tenants of Lord Carew, to give him a public dinner. A tent of enormous size had been erected, and the expense defrayed by rather more than three hundred tenants: I enquired whether this was a return for any particular popular act on the part of his lordship, but found that it was simply a testimony of respect and esteem.

In the journey from Waterford to Enniscorthy I saw none but the commoner ferns, *Polystichum aculeatum* becoming more abundant, and *Osmunda regalis* more rare. Enniscorthy is an old and large town, built on the side of a hill, and by every approach you enter it through a long line of very humble, and not particularly clean cottages; all the good buildings being in the centre of the town, and reminding one of Palmyra and the cities of the East, as seen by us through the optics of Silk Buckingham, whom, if my memory serve me aright, I have heard eulogizing the mode of confining the palaces, &c., to the centre of bustle, dust, and smoke. The castle in the middle of the town, now occupied by the Protestant curate, is a striking building. I mounted the celebrated Vinegar Hill, an eminence close to the town, whence an agreeable view of the surrounding country is obtained: the hills on every side rise with gentle slopes, and are prettily wooded. Vinegar Hill appears to be composed of granite; the summit is bare, with the exception of large patches of *Sedum Anglicum*. The country around Enniscorthy is generally hilly, the land apparently poor, but invariably cultivated, although the crops were very indifferent. Stone walls are comparatively rare, and are replaced by furze-hedges; and I not unfrequently observed furze had been sown in the interstices of stone walls. These furze-hedges are in many places allowed to become wild and straggling; and not only is their appearance in this state very untidy, but their utility, as fences, very questionable. Throughout the South of Ireland furze is grown in some abundance, as food for cattle: it is cut very frequently, and always while the shoots are young and tender; and is bruised previously to being given them. The river Slaney, which runs through the town, is a noble and navigable stream.

Leaving Enniscorthy I passed through Ferns, Gorey, Arklow, and Rathdrum, to Wicklow: there was little for the naturalist to observe, except the superabundance of furze hedges; and now, travelling by coach, I was compelled to take such roads as coaches could travel, and thus I missed the vale of Ovoca, and must trust to hearsay, that

> "There is not in the wide world a valley so sweet,
> As that vale in whose bosom the bright waters meet:"

for the wooden bridge at Ovoca, and divers minor bridges, had been swept away by the flood of the 30th of July, and there was no longer any coach-road through "the sweet vale of Ovoca." The road from Wicklow to Newtown-Mount-Kennedy is pretty; from Newtown-Mount-Kennedy to Bray it is fine. This latter passes through the glen of the Downs; steep and beautiful hills are piled up almost perpendicularly on each side of you; and the glen, which pretty much consists of the demesne of Mrs. Latouche, is completely wooded with majestic evergreens; such *Arbutus*, *Quercus sempervi-*

rens, and laurel, are not to be seen in England;—the laurels are magnificent.

From Bray to Luggelaw on Lough Tay, is a wild walk through some fine Wicklow scenery. Lough Tay is a sweet place; there is such a bold bluff, such a tumultuous multitude of rocks stretching out into the lake, that I gazed on the scene with nearly as much delight as on any that the island had before afforded. Lough Dan, a few miles further, is beautiful, exquisitely beautiful, but its beauty is less bold, its grandeur is not sublime. I found on the banks of Lough Dan, *Lomaria spicant*, *Pteris aquilina*, *Polypodium vulgare*, *Polystichum aculeatum*, *Lastræa Filix-mas*, *Las. dilatata*, *Las. Oreopteris*, *Asplenium Adiantum-nigrum*, *Aspl. Ruta-muraria*, *Aspl. Trichomanes*, *Scolopendrium vulgare*, *Osmunda regalis*, *Isoetes lacustris*, and *Littorella lacustris*.

Near the little village of Roundwood I saw, in a bog, a profusion of *Pinguicula Lusitanica*, *Malaxis paludosa*, and *Narthecium ossifragum;* the latter plant, and *Anagallis tenella*, are most abundant on nearly all the Irish bogs. From Roundwood, I bent my course to the Seven Churches of Glendalough, a place which, if one might enjoy it alone, is well worthy of an hour's contemplation; but alas! it is so infested with guides, that one is driven half wild with the clamour. I here had two companions, and I think about forty guides followed us with unremitting assiduity; at last we escaped them and looked back on the stately round tower of Glendalough, fringed at half its height with a belt of *Parietaria:* on the walls of the churches and ruins I observed the three more common species of *Asplenium*, *Scolopendrium vulgare*, and *Ceterach officinarum*, and one roof was half covered with *Mentha rotundifolia*. In the lake we found *Lobelia Dortmanna*, *Littorella lacustris*, and *Isoetes lacustris;* then, turning away from its margin, we made for the waterfall; and here, as I was scrambling over the slippery rocks in search of *Hymenophyllum*, I lost my footing, and fell plump into the stream; but luckily, making the descent legs first, I kept myself upright, and affectionately embracing a projecting rock, I maintained my position in spite of the power of the river, and joined heartily in the laugh of one of my companions. This district is full of ferns and mosses; *Las. Oreopteris*, generally so rare in Ireland, here occurs in boundless profusion; and the varieties of *Las. dilatata*, whether flat, convex, or concave, seem absolutely endless: from every crevice in the rocky hills which surround that picturesque lake, this fern may be seen waving its bright green fronds. Here and there, as we proceeded, a huge mass of

Polypodium vulgare, perhaps the growth of centuries, was sending forth its thousand fronds; but everywhere, heath, and fern, and moss, and rock, and trickling streams of water, were so mingled with the forestry moored in the crevices, that it was a scene goodly to look on and delightful to scramble over. Afterwards the hill became bare, and a little track of steps worn or cut in the rocks, showed that human beings went further down towards the water. It was very steep, but we descended in single file; presently our leader disappeared; he had entered that strange cavity in the rock called St. Kevin's bed; I followed, and our companion followed me. It is a strange excavation, and its romantic situation, the difficulty of access to it, and the little probability of a visit, point it out as a likely residence for such an ascetic as St. Kevin. We read the autographs of Scott, Moore, and other wise men who had ventured into this strange place, and had written their names against the wall; and there we sat, huddled together, gazing out upon

> "That lake whose gloomy shore,
> Skylark never warbled o'er:"

and so end my Notes on the Natural History of Ireland.

ART. V.—*A Systematic Catalogue of the Fossil Plants of Britain.* By JOHN MORRIS Esq.

(*Continued from page* 80.)

HYMENOPHYLLITES,[1] Göpp.

Fronds membranous, bi- or tri-pinnate; *pinnulæ* pinnatifid and dilated at the base, adhering to the rachis, which is generally winged. Veins simple, direct, one to each *lacinia*, rarely dichotomous. *Sori* roundish, marginal.

* *Rachis terete.*

Hymen. quercifolius, Göpp. page 252, tab. 14, fig. 1, 2. *Coal measures*, Silesia.

—— *Humboldtii*, Göpp. page 254, tab. 31, fig. 1, 2. *Coal measures*, Waldenburg.

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[1] In the last part of Sternberg's 'Flora der Vorwelt,' Presl has referred to a new genus, *Rhodea*, some species of Göppert's *Hymenophyllites* and *Trichomanites;* the genus is characterized as follows.—

RHODEA, Presl. Sternb. Flor. der Vorw. part vii. and viii. page 109.—Frond bi-tripinnate, slender, *pinnulæ* dichotomously pinnatifid, parted or linear, running down a filiform rachis. Veins pinnately branched.

** *Rachis winged.*

—— *crenulatus*, Göpp. page 255. *Sphenopteris crenulata*, Brong. Hist. i. page 187, tab. 56, fig. 3; Sternb. part v. and vi. page 60. *Oolite shale*, Yorkshire.

—— *Grandini*, Göpp. page 255, tab. 15, fig. 12. *Sphenopteris alata*, Brong. Hist. i. page 180, tab. 48, fig. 4; Sternb. part v. and vi. page 59. *Coal measures*, Geislautern.

—— *Phillipsii*, Göpp. page 256. *Sphenopteris hymenophylloides*, Brong. Prod. page 50; Hist. i. page 189, tab. 56, fig. 4; Sternb. part v. and vi. page 60. *Sphenopt. stipata*, Phillips, tab. 10, fig. 8. *Oolite shale*, Gristhorpe Bay.

—— *obtusilobus*, Göpp. page 257. *Sphenopteris trichomanoides*, Brong. Hist. i. page 182, tab. 48, fig. 3; Sternb. part v. and vi. page 59. *Rhodea*, Presl. *Coal measures*, Valenciennes.

—— *Gersdorfii*, Göpp. 257, tab. 37, fig. 1, 2. *Rhodea*, Presl. *Transition slate*, Landshut, Silesia.

—— *Brongniartii*, Göpp. page 258. *Sphenopteris Brongniartii*, Sternb. part v. and vi. page 57. *Sphen. stricta*, Brong. Prod. page 50; Hist. i. page 208, tab. 48, fig. 2. *Coal measures*, Glascow.

—— *Williamsonis*, Göpp. page 259. *Sphenopteris digitata*, Phillips, tab. 8, fig. 6, 7. *Sphen. Williamsonis*, Brongn. Hist. i. page 177, tab. 49, fig. 6—8; Sternb. part v. and vi. page 58; Lindl. and Hutt. ii. page 139, tab. 31. *Rhodea*, Presl. *Oolite shale*, Gristhorpe Bay.

—— *furcatus*, Göpp. page 259. *Sphenopteris furcata*, Brong. Hist. i. page 179, tab. 49, fig. 4, 5; Sternb. part v. and vi. page 58; Lindl. and Hutt. iii. tab. 181? *Rhodea*, Presl. *Coal measures*, Northumberland; Wardie; Belgium; Saarbruck; Waldenburg.

—— *dissectus*, Göpp. page 260. *Sphenopteris dissecta*, Brong. Hist. i. page 183, tab. 49, fig. 2, 3; Sternb. part v. and vi. page 59. *Rhodea*, Presl. *Coal measures*, St. George's-Chatellaison; Montrelais; St. Hippolyte, Vosges; Berghaupten; Waldenburg.

—— *Zobelii*, Göpp. page 260, tab. 36, fig. 3, 4. *Rhodea*, Presl. *Coal measures*, Waldenburg.

—— *macrophyllus*, Göpp. page 262. *Sphenopteris macrophylla*, Brong. Prod. page 50; Hist. i. page 212, tab. 58, fig. 3; Sternb. part v. and vi. page 65. *Rhodea*, Presl.—*Stonesfield slate*.

TRICHOMANITES, Göpp.

Fronds thin, bi- or tri-pinnate, filiform, rachis terete: *pinnulæ* dichotomously divided. Veins divergent, simple.

Trichoman. myriophyllus, Göpp. page 263. *Sphenopteris myriophyllum,* Brong. Prod. page 51; Hist. i. page 184, tab. 55, fig. 2; Sternb. part v. and vi. page 59. *Grès bigarré,* Sulz-les-bains.

—— *Kaulfussii,* Göpp. page 264. *Coal measures,* St. Ingbert, Germany.

—— *bifidus,* Göpp. page 264. *Sphenopteris bifida,* Lindl. and Hutt. i. page 147, tab. 53; Sternb. part v. and vi. page 60; Edinb. Trans. 13, tab. 6, fig. 1, 2. *Coal measures,* Edinburgh.

—— *Beinertii,* Göpp. page 265, tab. 32, fig. 1. *Hymenophyllites,* Presl. *Coal measures,* Charlottebrunn.

—— *adnascens,* Göpp. page 266. *Schizopteris adnascens,* Lindl. and Hutt. ii. tab. 100, 101. *Coal measures,* Whitehaven.

—— *delicatulus,* Göpp. page 267. *Schizopteris delicatula,* Brong. Hist. i. page 183, tab. 58, fig. 4. *Rhodea,* Presl. *Coal measures,* Saarbruck.

—— *Sillimani,* Nob. *Sphenopteris Sillimani,* Mant. Geol. South East Engl. page 239. *Hastings sands,* Heathfield.

Steffensia, Göpp.

Fronds tripinnate; *pinnulæ* ovate; veins divergent, direct, soriferous towards the margin, the *sori* roundish.

Steff. davalloides, Göpp. page 269, tab. 11, fig. 3, 4. *Coal measures,* Waldenburg.

PECOPTERIDES, Göpp.

Fronds simple, pinnate, bi- or tri-pinnate, or bi- or tri-pinnatifid; *pinnulæ* equal or dilated at the base (rarely contracted), adnate to the *rachis,* or united to each other, midrib prominent, extending to the apex; veins variable; in the narrow *pinnulæ* dichotomous, horizontal, and more or less straight; in the broader *pinnulæ* they are dichotomous, oblique, having the branches bi- or tri-furcate and anastomosing.

Beinertia, Göpp.

Fronds pinnate; veins pinnate, branches prominent, obliquely ascending, dichotomously forked and parallel towards the margin. Fructification resembling *Gymnogramma.*

Bein. gymnogrammoides, Göpp. page 273, tab. 16, fig. 4, 5. *Coal measures,* Charlottebrunn.

DIPLAZITES, Göpp.

Fronds pinnate. Veins bipinnate, secondary veins arched, alternate.—*Sori* linear.

Dipla. emarginatus, Göpp. page 274, tab. 16, fig. 1, 2. *Coal measures.*

—— *longifolius*, Göpp. page 275. *Pecopteris longifolia*, Brong. Prod. page 56; Hist. i. page 273, tab. 83, fig. 2.—*Coal measures*, Saarbruck.

SCOLOPENDRITES, Göpp.

Fronds simple, linear, midrib thick, veins simple. *Indusium* large, oblong, free and dehiscent.

Scol. Jussieui, Göpp. page 276. *Reussia*, Presl; Sternb. part vii. and viii. page 125. *Filicites scolopendrioides*, Brong. Ann. des Sci. Nat. vol. xv. page 443, tab. 18, fig. 2; Brong. Hist. i. page 388, tab. 137, fig. 2, 3. *Grès bigarré*, Sulz-les-bains.

ASPLENITES, Göpp.

Fronds pinnate, bi- or tri-pinnate, *pinnæ* equal or unequal, oblique and somewhat rhomboid; veins obliquely ascending, simple or dichotomous.—*Sori* linear or ovate-linear.

Aspl. heterophyllus, Göpp. page 278, tab. 18, fig. 1. *Coal measures*, Charlottebrunn.

—— *crispatus*, Göpp. page 279, tab. 18, fig. 2, 3. *Coal measures*, Charlottebrunn.

—— *nodosus*, Göpp. page 280, tab. 14, fig. 1—3. *Coal measures*, Landshut, Silesia.

—— *ophiodermaticus*, Göpp. page 280, tab. 17, fig. 1, 2.—*Coal measures*, Waldenburg.

—— *trachyrrachis*. Göpp. page 281, tab. 17, fig. 3, 4. *Coal measures*, Waldenburg.

—— *divaricatus*, Göpp. page 282, tab. 20, fig. 1, 2. *Coal measures*, Waldenburg.

—— *Palmetta*, Göpp. page 283, tab. 15, fig. 6. *Sphenopteris Palmetta*, Brong. Prod. page 51; Hist. i. page 211, tab. 55, fig. 1; Sternb. part v. and vi. page 64. *New red sandstone, Grès bigarré*, Sulz-les-bains.

—— *Virlettii*, Göpp. page 284. *Sphenopteris Virlettii*, Brong. Prod. page 51; Hist. i. page 209, tab. 58, fig. 1, 2; Sternb. part v. and vi. page 64. *Coal measures*, St. George's-Chatellaison.

ACROSTICHITES, Göpp.

Fronds simple or pinnate; veins oblique, dichotomously forked, digitate, or anastomosing.

* Frond bipinnate.

Acros. Williamsonis, Göpp. page 285. *Pecopteris Williamsonis*, Brong. Prod. page 53; Hist. i. page 324, tab. 110, fig. 1, 2; Lindl. and Hutt. i. page 125, tab. 126. *Oolite shale*, Scarborough.

** Frond digitate.

—— *Phillipsii*, Göpp. page 286. *Glossopteris Phillipsii*, Lindl. and Hutt. i. page 167, tab. 63; Sternb. part v. and vi. page 69. *Sagenopteris*, Presl. *Pecopteris paucifolia*, Phillips, tab. 8, fig. 8, *h*. *Oolite shale*, Gristhorpe Bay.
—— *inæquilaterus*, Sternb.; Göpp. page 287. *Keuper*, Schrullendorf, Franconia; Sinsheim, Baden.

WOODWARDITES, Göpp.

Fronds pinnatifid; veins areolately reticulated, dichotomous towards the margin; the areoles irregular, smaller near the midrib.

Woodw. obtusilobus, Göpp. page 289, tab. 21, fig. 1. *Sagenopteris*, Presl. *Coal measures*, Waldenburg.
—— *acutilobus*, Göpp. page 289, tab. 21, fig. 2. *Lonchopteris Göppertiana*, Presl. *Coal measures*, Waldenburg.
—— *Brownii*, Nob.[1] *Hemitelites*, Göpp. page 334, tab. 38, fig. 1. *Phlebopteris contigua*, Lind. and Hutt. ii. tab. 144, page 177. *Oolite shale*, Gristhorpe Bay.

CLATHROPTERIS, Brong.

Fronds pinnate. Midrib thick and excurrent; veins simple, straight, parallel, united by transverse venules, forming quadrangular areas: (as in *Meniscium*, *Drynaria*, and some species of *Acrostichum*. *Goniophlebium* of Smith.

Clath. meniscioides, Göpp. page 290, tab. 15, fig. 7. *Filicites meniscioides*, Brong. Ann. des Sci. Nat. 1825, vol. iv. page 218; Bronn, Leth. Geogn. ii. tab. 13, fig. 2, page 149. *Lias*, Hör; Neue-welt near Basle.

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[1] Judging from the anastomosed veins at the base parallel to the midrib, this species may be regarded as belonging to *Woodwardites* rather than to *Hemitelites*, to which Göppert has referred it.

(*To be continued*).

ART. VI.—*On a new Genus of Fishes from India.* By WILLIAM THOMPSON, Esq., Vice Pres. Natural History Society of Belfast.

IN the course of last spring my friend Dr. Cantor, favourably known to naturalists by his zoological investigations in India, communicated a description and drawing of a new genus of fishes for publication in this Magazine. From the drawing a wood-cut was executed; but in the mean time the description was unfortunately mislaid. When I last saw Dr. Cantor in London, in the month of June, he was soon to re-embark for India; and having some time before given me a specimen[1] of the fish in question, he urged me to draw up an account of it. Considering it better that the discoverer should also be the describer, I delayed, still in the hope that the missing MS. might be discovered, but I am now informed that every search has in vain been made for it.

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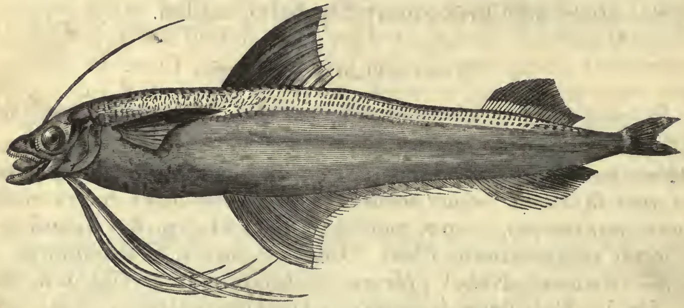

Bregmaceros McClellandii, Cantor, MS.

To the ichthyologist this fish must, in every respect, be highly interesting. It is from the brackish water of the Gangetic Delta, and ranks under the family *Gadidæ,* which chiefly inhabit the waters of the temperate and colder regions of the globe. In generic form it is quite anomalous, the filament springing from the upper part of the head—whence the name *Bregmaceros*—giving to the species an unique appearance, whilst the greatly elongated ventrals at once bring to mind the genus *Phycis* (this being the only generic resemblance); but instead of the ventrals consisting each of a single ray, as in *Phycis,* we find these organs as numerous as in any genus

[1] Only two were obtained; the other was, I believe, sent to the Radcliffe Library, Oxford, along with the specimens and drawings illustrative of Dr. Cantor's 'Spicilegium Serpentium Indicorum,' (published in the Zoological Proceedings, 1839).

throughout the family, unless *Brotula* (the number of whose ventral rays I have not seen mentioned) should prove to be an exception. It may be thus characterised.

Genus, *Bregmaceros.* Cantor, MS.

Body elongated: two dorsals; one anal; ventrals very long, consisting of several rays: chin without barbule: a filament projecting from the top of the head.

Species, *Bregmaceros McClellandi.* Cantor, MS.[1]

Head small; frontal filament equal to one fourth the length of body; ventrals equal to half its length; first dorsal high, subtriangular; second very long, low and equal anteriorly, then moderately high; anal resembling three fins joined, the first portion high and subtriangular, the second low and equal, the third of moderate height; dorsal and anal corresponding to each other throughout; caudal forked.

Description.—Length 3 inches, depth $\frac{1}{2}$ an inch, thickness $\frac{1}{4}$ of an inch. Dorsal *profile* somewhat arched to first D. fin, thence decreasing very gradually to the tail; ventral convex to the vent, thence corresponding with the dorsal outline. *Head* small, occupying one sixth of the entire length; snout truncated; lower jaw barely exceeding the upper; *teeth* numerous along the margin of both jaws, those in the upper very small and uniform, those in the lower varying in size, and some considerably larger than those in the upper, all pointed and hooked inwards; similar teeth in front of *vomer: tongue* large and fleshy: *eye* $1\frac{1}{3}$ line in diameter, placed at this distance from the snout; a similar space intervening between the eyes; that between them and posterior line of opercle equal to twice their diameter; protected in front by a slightly-elevated bony process, within which, and near to the upper margin of the eyes, are situated the *nostrils*, which are simple apertures; *operculum* rounded; rays of branchiostegous membrane—?[2] filamentous or unarticulated *appendage* originating 4 lines from base of snout, 9 lines in length, very delicate; from its base to first D. is a deep channel, with slightly-elevated *Trigla*-like scales on either side, but smooth-margined. *Scales* of moderate size, somewhat rounded at their free margins, delicately sculptured,[3] twelve in an oblique row from vent to dorsal profile; lateral line unmarked by colour and hardly

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[1] The name was the only MS. left with me on the subject. The species was named by Dr. Cantor in honour of his friend, J. McClelland, Esq., whose researches in the Zoology and Geology of India are well known.

[2] Seven are given as a family character: in this instance I could not be satisfied of the number, four only being reckoned with certainty. Repetitions of some of the family characters appear in the description, as I did not consider them misplaced in a genus entirely new.

[3] See figure 7, which is a magnified view of a scale from the lateral line.

distinguishable, taking the form of dorsal profile, at first nearer to the dorsal than ventral outline, then midway between them: *vent* 11 lines from extremity of lower jaw.

FINS.— *Ventrals* originate $3\frac{1}{2}$ lines from last-named point, have six or seven rays, the three or four inner ones of ordinary size and bifurcated, the three outer unbranched, greatly prolonged, and of singular structure, the first 13, the second and third 18 lines in length; they are beautifully and conspicuously articulated, the articulations increasing in length towards the filamentous termination; these rays are very broad towards the base, and are winged or margined on either side by a beautifully transparent process, in form calling to mind the dorsal *lamina* of the cephalopodous genus *Loligo: pectorals* elongate triangular, placed high, originating $5\frac{1}{2}$ lines from snout, $5\frac{1}{2}$ lines long, first ray shorter than the succeeding nineteen, the shorter bifurcated, the longer unbranched, protected by scales at the base: *first dorsal* broadly triangular, originating about 12 lines from snout, base occupying 7 lines, longest ray equal in length to the greatest depth of body, 19 nineteen unbranched rays; *second dorsal* originating close behind the first, extending for 15 lines, to the base of caudal, 9 low and of uniform height at first, then becoming of moderate 36? elevation, thirty-six ? unbranched rays; *anal* originating $11\frac{1}{2}$ lines from point of lower jaw and extending to base of caudal, occupying 21 lines, about the first third broadly triangular and very prominent, its longest ray 7 lines, next third low, short and equal, last third rather more than moderately developed, longest ray of this portion 3 lines, 60 sixty unbranched rays in all, protected by scales at the base; *caudal* small, forked, 4 lines long, occupying one ninth of the entire length, rays $12\frac{7}{7}$, the longest doubly bifurcated.

COLOUR (in spirits) of a yellowish sandy hue along the back and upper portion of sides, varied with minute black spots, rather more than the lower half silvery, head of this colour; upper portion of pectorals and of both dorsals black, remainder hyaline; caudal fin black; ventrals and anal hyaline, except a slight dusky tinge towards the extremity of the latter fin.

This genus is so anomalous as to render quite unnecessary a comparison between it and any other of the *Gadidæ*.

Belfast, Feb. 1840.

[In a note accompanying Mr. Thompson's communication on this highly interesting genus, he observes,—" To render the communication the more perfect, you should, if possible, have the following alterations made in the engraving:—two shorter rays should be added to the first D. fin, and the second D. entirely altered; this and the anal should be made to touch upon the base of C. fin; the whole body should be scaled over; the frontal spine too should originate in a line with the *posterior* margin of the eye. These alterations made, the figure would be about perfect."

The engraving being on wood, unfortunately does not allow of the introduction of the proposed corrections; but the characters of the genus are so strongly marked, that no material ambiguity can result from the error on the part of the artist.—Ed.]

ART. VII.—*On the occurrence of Mammalian Remains in the Lower Eocene deposits of Epernay, Marne.* By JOS. PRESTWICH, Jun., Esq., F.G.S. &c.

IN a paper read before the Geological Society of Paris, December, 1837,[1] I gave a short account of some peculiar organic remains, which I had found in a coarse arenaceous stratum of the plastic clay series, forming the summit of Mont Bernon, near Epernay.

As I have since added to the list of fossils which I then was able to enumerate, you may, perhaps, think the substance of the communication, along with a reference to a memoir upon a nearly equivalent deposit in the neighbourhood of Paris, by M. Chas. D'Orbigny, of sufficient interest for insertion in your journal.

Epernay is situated upon the eastern margin of the basin of Paris. The tertiary strata merely cap the hills, the bases of which, with the valleys, consist of chalk. The small streams flowing off from the high table land, frequently expose, in their course down the steep declivities, excellent sections of the several deposits.

Numerous small sections are also made in excavating the bitumino-carbonaceous clays (*cendres*), common in the plastic clay of this country, and used as manure for the vines. Several pits of this nature are worked on Mont Bernon, affording good opportunities of studying its structure; but the superposition of the beds is in some places rendered rather obscure by numerous small faults, which range about 10° E. of S., and 10° W. of N. Connecting, however, the various sections, the following is, as well as I could ascertain, the order of superposition, commencing from the summit.

[1] Bulletin Soc. Geol. de France, vol. ix. p. 84.

		Ft.	In.
1. Fresh-water limestone; colour, chalk white	Superior to the *Calcaire grossier*.[1]	1	8
2. Green Marl		3	11
3. Light yellow and brown clay		1	0
4. Fine yellow sand, without organic remains		6	10
passing into—			
5. Coarse quartzose sand, sometimes mixed with ferruginous clay, containing remains of *Mammalia*, *reptiles*, *fishes*, and *shells*		4	7
6. Soft Lignite, brown and black		1	4
7. Grey sand, with clay and veins of earthy, friable carbonate of lime, passing downwards to a ferruginous, and lastly to a light grey sand		6	3
8. Brown and ferruginous plastic clay		1	10
9. Light grey sand, passing downwards to a very ferruginous sand		4	7
10. Dark grey plastic clay, intermixed with ferruginous sand		1	0
11. Very soft and impure lignite		"	4
12. Brown and ferruginous plastic clay		"	8
13. Dirty yellow argillaceous sand, full of shells, almost all of the genus *Melania*		1	4
14. Thinly laminated grey and brown plastic clay, with well-preserved specimens of *Melania*, *Cyrena*, *Melanopsis*, *Neritina*, and traces of vegetables. It contains, in the lower part, a thin bed of pyritical sandstone		1	4
15. This bed consists almost entirely of broken shells (principally *Melania*), mixed with a little whitish sand		2	2
16. Fine whitish sand, with small patches of *Melania*, *Cyrena*, *Melanopsis*, &c.; (the *Planorbis hemistoma*, Sow., is found also in this bed)		3	7
17. Yellow argillaceous sand, with many well preserved *Cyrenæ*		1	8
18. Dark grey plastic clay		1	0
19. The same with shells, principally *Melaniæ* and small *Ostreæ*.		1	0
20. Irregular lignite		"	4
21. Very friable yellow marl, with traces of leaves and shells, and numerous seeds of the *Chara*		1	2
22. Lignite		"	4
23. Dark grey plastic clay, with some shells		6	0
24. Irregular lignite		"	4
25. Similar to 21		1	2
26. Lignite		"	4
27. Similar to 23		2	4
28. Tough clay continuing apparently to the chalk, which appears about 10 or 15 feet below 27; the contact between the two is not, however, exposed.			

These strata present rapid variations in thickness and lithological structure. In almost all the beds of lignite and clay, selenite, carbonate of lime, Websterite, oxide and sulphuret of iron, frequently occur. The clays are all more or less carbonaceous.

The foregoing section exhibits the usual numerous alternations of the thin and very irregular strata, characteristic of

[1] The *Calcaire grossier*, which is well developed, and abounds in fossils at a distance of about five miles westward of Epernay, is here entirely wanting. There are some fine sections of it at Damery, Arty, Fluery, Boursault, and neighbourhood.

this fluviatile deposit, which is so frequently interpolated in the lower eocene of England and France; but the remarkable fact connected with this locality, is the occurrence of the remains of several terrestrial *Mammalia*, associated with those of reptiles, fishes, and shells. They are confined to the lower part of the stratum of coarse sand, No. 5[1] of the foregoing section, and are very scarce; whilst remains of *Testacea* and bones of the *Trionyx* and *Emys* are far from uncommon. The sand of that part of the bed in which they are imbedded is coarser than other parts of the stratum, and contains occasional pebbles of quartz. It varies rapidly in thickness, from 10 to 20 feet, and the organic remains are limited to a very small vertical range.[2] For the determination of the bones of the following list, I am indebted to M. Laurillard, of Paris.

ORGANIC REMAINS OF STRATUM, No. 5.

MOLLUSCA.

Melania inquinata, De Fer.
Melanopsis buccinoidea, De Fer.
Paludina.

CONCHIFERA.

Anomia.
Cyrena ? antiqua.
Teredina (personata ?) Lam.
Teredina. Two new species.
Anodonta.

FISHES.

Several scales, and bones.

REPTILES.

Crocodile ?[3] Several teeth.
Mososaurus ? Part of a rib, and some teeth.
Numerous bones of the *Trionyx* and *Emys*.
Serpent. A *vertebra*. (See woodcut page 190.)
Lizard ? Small species, a jaw-bone.

[1] The organic *reliquiæ* of the underlying strata, assimilate to those usually found in the plastic clay, the detail of them would therefore present nothing new.

[2] In its horizontal range, I have since traced this stratum along the flanks of all the surrounding hills, especially at Cuys and Chavots, where it abounds in large and well preserved *Uniones*, one species of which closely resembles the *Anodonta antiqua*, figured by Charles D'Orbigny.

[3] M. Drouet has found bones of the crocodile in some of the underlying beds also.

MAMMALIA.

Anthracotherium? Small species, a molar tooth.
Lophiodon. Perhaps two species; four inferior molar teeth, one inferior canine tooth. One *femur*, one *vertebra* (*atlas*) not determinable, perhaps of a rodent or carnivorous animal.

As I believe the occurrence of ophidian remains is rather remarkable in the eocene series, the figure of the only one specimen which I have found is annexed.[1]

8

This deposit presents a striking analogy with that described by M. Chas. D'Orbigny, at Meudon. In both places, the strata containing these terrestrial *reliquiæ*, have evidently been produced by a river action more violent than that which has accumulated the accompanying strata. At the same time their superposition is not precisely the same, as may be seen by the following section given by M. Chas. D'Orbigny, of the Hill of Meudon.

	Ft.	In
1. *Calcaire grossier*	46	2
2. Plastic clay, mottled red, grey, &c.	6 to 26	6 to 2
3. White marl, with a few calcareous nodules	1	2
4. Lignite, with large *Paludinæ* and *Anodontæ*	1	4
5. Finely laminated clay, with crystals of selenite, and layers of ferruginous sandstones	,,	8
6. Conglomerate of plastic clay, with pebbles of chalk and pisolitic limestone, sometimes 10 inches in diameter, enclosing remains of *Mammalia*, fishes, with marine and fluviatile shells	1	6
7. Yellowish, slightly coherent, *calcaire grossier*, with numerous fossils, amongst which occurs the *Cerithium giganteum*	4	7
8. Laminated marl, with pectens	,,	2
9. *Calcaire grossier*, with numerous fossils, chalk, hard and yellowish, with irregular layers of flints, and numerous fossils.	4	11

[1] Mr. Owen has lately described to the Geological Society the remains of a mammal, somewhat allied to the *Chæropotamus* and *Peccari*, found by Mr. Richardson in the lower part of the London clay, at Herne Bay, which would thus bring it to about the same age as the Epernay specimen. He likewise gave an account of a series of *vertebræ* from the London clay of the Isle of Sheppey, in the fine collection of Mr. Bowerbank, which he considered to belong to a large serpent, and has named it *Palæophis tolypeutes*.

White chalk, in which were found, amongst other more ordinary fossils, an indeterminable species of *Cerithium*, fragments of fishes, part of the jaw with teeth and other bones of a large saurian, analogous to that of Maestricht, and remains of a turtle, about 16 inches long.

The yellow chalk contains the following fossils :—

CONCHIFERA.

CARDIUM *Hillanum*.
ARCA.
LIMA.
PECTEN *quinque-costatus*, Sow.
PECTUNCULUS.
NUCULA.
CATILLUS *Cuvieri*, A. Brong.
INOCERAMUS *Lamarckii*.
TEREBRATULA *carnea*, Sow.
——— *octoplicata*, Sow.
——— *plicatilis*, Sow.

MOLLUSCA.

TROCHUS *Basteroti*, A. Brong.
TURRITELLA.
PLEUROTOMARIA, or *Solarium*.
BELEMNITES *mucronatus*, Schlot.

"MM. Elie de Beaumont and D'Archiac were the first to announce to the Geological Society of France, the existence of a marine calcareous stratum between the plastic clay and the chalk of Meudon, to which a careful examination now enables me [C. D'Orbigny], to add several new details." (See section, p. 190).

This *calcaire grossier* is whitish or yellowish, and generally slightly coherent, frequently encloses numerous fragments of *Polyparia* and *Radiaria*, and is characterised in many places by the presence of numerous pisolitic grains.

This series contains the following fossils, some of which had been previously determined by M. D'Archiac.

ZOOPHYTA.

ORBITOLITES *plana*. Characteristic of the central division of the *calcaire grossier*.
TURBINOLIA *elliptica*, A. Br.
FLUSTRA.
ESCHARA.

RADIARIA.

SPATANGUS. Of the same species as the one found on the *calcaire grossier* of Grignon.
CIDARIS (Spines of the)
ASTERIA (Articulations of)

ANNULATA.

DENTALIUM.
SERPULA.

CONCHIFERA.

CRASSATELLA *tumida*, variety B., *Lamk*.
CORBIS *lamellosa*, Lamk.
LUCINA *grata*, Def.
LUCINA *contorta*, Def.
CYTHERÆA *obliqua*, Desh.
VENUS *obliqua*, Lamk., and another indeterminable species.
CORBULA *gallica*, Lamk.
VENERICARDIA.

CARDIUM *porulosum*, Lamk.
——— *granulosum*, Lamk.
——— *rugosum*, Lamk.
——— *obliquum*, Lamk.
CUCULLÆA *crassatina*, Lamk.
ARCA *biangula*, Lamk.
—— *rudis*, Desh.
—— *barbatula ?* Lamk.
ARCA *filigrana*, Desh.
CHAMA.
MODIOLA *cordata*, Lamk.
LIMA *inflata*.
LIMA. New species, resembling the *Lima spatulata*.
SOLEN.

MOLLUSCA.

HIPPONIX *cornu-copiæ*, Def.
CALYPTRÆA *trochiformis ?* Lamk.
NATICA *patula*, Desh.
NERITA *angistoma*.
DELPHINULA or TURBO.
SOLARIUM *patulum*, Lamk.
TROCHUS *subcarinatus ?*
TURRITELLA *imbricataria*, Lamk., Variety C.
TURRITELLA (indeterminable.)
CERITHIUM *giganteum*, Lamk.
CERITHIUM *semi-costatum*, Desh.
FUSUS.
OLIVA *Branderis ?*
CYPRÆA.
PLEUROTOMARIA *concava ?*
NAUTILUS. An indeterminable species, found by M. Raulin.
MILLIOLITES. Very numerous.

PISCES.

Teeth of the *Squalus*.

"It will be perceived that the fossils of this deposit are tertiary, and that not one is common to the underlying chalk, with which it appears some French geologists had classified it." M. D'Orbigny, who designates this deposit under the name of *Calcaire pisolitique tertiaire*, then mentions several localities in which it occurs, in the vicinity of Paris, as at Bougival, Port Marley, Vigny, and Auteuil. He also considers the shelly beds of *calcaire grossier* of Laversine, near Beauvais, as the equivalent of the same stratum.

A pit recently excavated at Montalets, Bas Meudon, led M. Chas. D'Orbigny to discover a series of new and interesting strata immediately overlying the above-mentioned *calcaire pisolitique* (see section, p. 190), and from the chalky conglomerate No. 6, he obtained the following *reliquiæ* :—

FRESH-WATER SHELLS.

ANODONTA *Cordierii*, Ch. D'Orb.
——— *antiqua*, Ch. D'Orb.
CYCLAS.
PALUDINA *lenta*.
PLANORBIS.

Several bones of fishes,—species not determinable.

REPTILES.

Crocodile	Several teeth, and fragment of a jaw.
Tortoises	Several bones of *Trionyx*. do. do. *Emys*.
Mososaurus	Three teeth and part of the *humerus* of a great saurian, approaching the *Mososaurus*, or monitor of the Maestricht chalk.

MAMMALIA.

Anthracotherium. (large species)	Two inferior posterior molar teeth.
	Two inferior anterior molar teeth.
	One superior anterior molar tooth.
	Canine tooth.
	Five incisors.
Anthracotherium (very small species)	Superior molar tooth.
	Incisor.
Lophiodon	Inferior molar tooth.
	Inferior canine tooth.
	Part of a rib.
Otter	Inferior molar tooth.
Fox	One incisor.
	Posterior molar tooth.
Civet-cat ?	Superior anterior molar tooth.
	Bone of the *metacarpus* and *humerus* of an indeterminable carnivorous genus.
Squirrel ?.........	Superior incisor.
	Incisor of an indeterminable rodent.

" Above the conglomerate is a series of beds of plastic clay, slightly calcareous, and frequently arenaceous, with crystals of selenite, gypsum, and numerous traces of vegetables which sometimes form an irregular seam of lignite." "Associated with the terrestial debris are fresh-water *Testacea*, of the genera *Anodonta* and *Paludina*, often cast in iron pyrites, and much compressed." Stratum No. 2, is considered to represent the ordinary deposit of *argile plastique* of Paris, over which lies the thick deposit of *calcaire grossier* proper.

In conclusion, M. D'Orbigny considers,

1stly, " That the genus *Hamites* existed until the end of the cretaceous epoch, and that the chalk of the Paris Basin contains turtles of a large size.

2dly, " That the plastic clay of the neighbourhood of Paris is indisputably separated from the chalk by a distinct group, which may be named *Calcaire pisolitique tertiaire*, and which, from the nature of its organic remains, belongs evidently to the palæotherian and tertiary, and not to the cretaceous period.

3dly, " That there evidently existed, during the accumulation of the lower part of the plastic clay, several genera of *Mammalia*, differing considerably from those discovered in the upper series of the Paris Basin."

Whilst, however, this fluviatile deposit is, in the neighbourhood of Paris, distinctly proved to be interpolated between two well characterized portions of the *calcaire grossier*, it can, in Champagne, only be considered as synchronous with the lower part of the eocene series. At Damery, the plastic clay with its characteristic fossils, may be seen underlying the *calcaire grossier*, but its position with regard to the saccharine fossiliferous limestone of Mont Aimé and Vertu[1] (imagined to be the equivalent of the *calcaire pisolitique*), is yet uncertain, although the latter reposes, in some places, immediately upon the chalk, and in others, is only separated from it by a few feet of clay, containing remains exclusively marine. At neither ofthese localities, however, where this limestone varies in thickness from 60 to 100 feet, is it overlaid by strata sufficiently well characterized to establish its superposition; neither do the organic *reliquiæ* afford us much information, for the species of none of the *Testacea* have yet been recognized, in consequence of their almost invariably occurring in the state of casts : as a group, however, this deposit appears to exhibit a close analogy to those of the *calcaire pisolitique* of the neighbourhood of Paris. Still I obtained from this locality, a few very perfect specimens of the teeth and bones of reptiles and saurians; also some palates and teeth of *Squalus* and other fishes.

Art. VIII.—*Descriptions of a few Longicorns, MS. names of which are published in the Sale-Catalogue of Mr. Children's Insects.* By Edward Newman Esq., F.L.S.

Genus. Niræus, *Newman.*

Facies ferè *Aromiæ :* mandibulæ latæ, ferè triangulares, apice curvato, subacuto, faciei interiori complanatâ, marginibus anticâ et posticâ dentem magnum haùd acutum ferentibus; palpi apice cýlindracei, maxipalpi brevissimi: antennæ corporis dimidio vix longiores; articulus 1us incrassatus, obconicus, 2us brevissimus, 3us elongatus, cæteri pedetentìm breviores, obliquè truncati: prothoracis latera medio 1-tuberculata: scutellum parvum, triangulare: elytra apice rotundata: femora vix tumida, tibiis compressis.

Sp. *Nir. tricolor.* Antennæ nigræ, medio albidæ : mandibulæ et oculi nigra; capitis cætera rubra: prothorax rufus: elytra basi rufa, maculâ scutellari nigrâ, apice latè nigro. (Corp. long. 1·4 unc. lat. ·4 unc.)

Inhabits.—Tenasserim coast.

Genus. Mallodon, *Serville.*

Sp. *Mall. spinosum.* (Corp. long. 1·5 unc. lat. ·6 unc.)

Mandibles short, incurved, the internal margin toothed: head very coarsely and rugosely punctured, the punctures

[1] Ten miles southward of Epernay.

united: *prothorax* broader than long, the posterior margin convex, the lateral margins armed with eight or ten acute but short spines, within the margin are two elevated lines, of which the exterior is the shorter, and the space around them is deeply and rugosely punctured, the disk is glabrous, with a few impressed punctures about the centre. *Elytra* rather wider than the *prothorax*, the margins are rather convex and recurved: near each margin, and parallel with it, is a longitudinal impression on both sides; at each anal angle is an acute, distinct, but short spine.

Inhabits.—Brought from Velasco in Mexico.

Genus. MEGADERUS, *Dejean.*

Sp. *Mega. corallifer.* Niger: elytra rufa, apicibus maculâque discoidali nigris: pedes nigri, tibiis coccineis. (Corp. long. 1·2 unc. lat. ·5 unc.)

An injured specimen. Black: the *elytra* are red, the apex and a large discoidal spot being black: the *tibiæ* are bright red, resembling coral, the rest of the legs black.

Inhabits.—Mexico.

Genus. PHÆDINUS.

Sp. *Phæd. mæstus.* Ater, nitidus: prothorax utrinquè maculâ posticâ sanguineâ ornatus. (Corp. long. ·9 unc. lat. ·3 unc.)

An injured specimen. Black, with the exception of two lateral posterior red spots on the *prothorax*, which is thickly and rugosely punctured, and has three dorsal tubercles.

Inhabits.—Brazil.

Genus. RACHIDION, *Serville.*

Sp. *Rach. obesum.* Nigrum: prothoracis dorsum elytrisque, margine excepto, nigris; facies tomentosa: corpus subtùs nigrum, lanugine micanti vestitum. (Corp. long. ·85 unc. lat. ·35 unc.)

Head and *antennæ* black; face thickly tomentose: *prothorax* above deep red, thickly covered with confluent punctures, the posterior angles deeply excavated, having an acute point at each posterior angle, beneath black and clothed with a shining pubescence: *scutellum* black, very elongate, narrow, thickly punctured at the sides and longitudinally compressed in the middle: *elytra* dull red, with all the margin, more particularly the humeral portion, black, regularly and deeply punctured: the under side of the body is black, and nearly covered with a shining but short pilosity; the legs are black.

Inhabits Brazil, and may possibly be the female of *Rachidion nigritum*, there being many decided similarities.

Genus. Ommidion, *Newman.*

Caput exsertum inclinatum nec pronum neque porrectum: antennæ filiformes corpore vix longiores; articulus 1us elongatus, 2us brevis, 3us, 4us, et cæteri æquales: oculi minuti lineares vel subreniformes, antennis distantes: prothorax anticè constrictus, lateribus medio tuberculatis, vix spinosis, dorso planus: elytra apice rotundata, nullo modo spinosa: femora pedetentìm tumida.

Sp. *Omm. modestum.* Testaceum: obscurè lanuginosum: prothorax rugosus: scutellum tomentosum: elytra basi puncta. (Corp. long. ·7 unc. lat. ·2 unc.)

Inhabits.—Brazil.

Genus. Eburia, *Serville.*

Sp. *Ebur. Virgo.* Testacea: prothorax spinis nigris 2 dorsalibus 2 lateribus armatus: elytra maculis 6 eburneis, quarum 1 basali 2 discoidalibus conjunctis, ponè medium sitis, exteriore interiorem superante anticè posticèque: meso- et meta-femorum apicibus nigris. (Corp. long. ·85 unc. lat. ·2 unc.)

Pale testaceous, the *antennæ* slightly hirsute and the eyes black: the *prothorax* has two black spines situated rather nearer its anterior margin than the middle of the disk, and behind each of these spines is a black tubercle, at about an equal distance from the posterior margin; on each of its lateral margins near the middle is another black spine, and on each side before this is another small black tubercle: each *elytron* has three ivory spots, one somewhat oval touches the base, and is placed rather obliquely, the lower extremity being outermost, and bordered with black, the other two being rather below the middle of the *elytron*, commencing nearly on the same level, closely united at the upper extremity, then separating, and the exterior prolonged much below the interior, each has a black mark at either extremity, the apical ones are elongated; the apical spine is black, and emits a brown ill-defined line, which bends a little towards the exterior margin of the *elytron:* the apices of the *meso-* and *meta-femora*, as well as the spines, are black.

Inhabits.—Brazil.

Sp. *Ebur. Puella.* Rufo-testacea: prothorax lineis 2 nigris longitudinalibus anticè in tubere productis: elytra maculis 6 eburneis, quarum 1 basali ovali, 2 discoidalibus anticè ferè conniventibus posticè paullò divergentibus signata: meso- et meta-femorum genubus spinisque nigris. (Corp. long. ·75 unc. lat. ·175 unc.)

Art. IX.—*Notice of a Fluvio-marine Deposit containing Mammalian Remains, occurring in the Parish of little Clacton, on the Essex coast.* By John Brown, Esq., F.G.S.

Several lacustrine formations have from time to time been recorded in this Magazine, as occurring in this or the adjoining counties, but their general character has been that of purely fresh-water deposits.

The one I now wish to bring under notice, is on the eastern coast of Essex, ten miles south of Walton, and has abundance of marine shells, mixed with those of fresh-water. The beds of shells, alternate with thick deposits of peaty matter in the cliffs, to the depth of 18 or 20 feet, as shown in the accompanying section, which was taken on the spot; and at the same time, the shells, fossil wood, seeds of *Chara*, and *Cypris*, were collected, which I now forward you with this paper. The hollow or basin occupied by this deposit, measures about 600 yards in a north and south direction; and at low water, it can be traced for about 80 yards eastward from the face of the cliffs; and it doubtless extends much farther under the sea, as the fresh-water shells, and bones of the fossil *Mammalia*, are seen lying in their lacustrine beds, close up to low-water mark; and we may feel assured, that if this coast continues to be visited with the same destructive elements to which it has been exposed for the last forty years, unless these lacustrine beds are continued for a considerable distance under the surface of the land to the westward from the present line of cliffs, there will be nothing to indicate to future geologists, that a fresh-water lake ever existed here; but that this fluvio-marine deposit, which now presents so many features of interest from the great number and the character of its organic remains, will, like the beds of crag once seen at Harwich, be swept away by the sea, before many centuries have passed over.

Geological writings are the only evidence which will transmit to posterity that the shelly beds of the crag once existed on the cliffs at Harwich; and this consideration makes it desirable that as great an amount of facts as can now be collected, should be published in scientific works open to such observations.

The finding of fossil fresh-water shells in certain places, is not always indicative of a lake having existed in such spots; the shells may have been drifted down a river into an estuary, and by that means become mingled with those of the sea. But in the instance under review, I can adduce not only the fresh-water and land shells, as well as nuts, seeds, and whole trees, but the successive accumulations of peaty matter, and their various thicknesses, which are seen alternating with layers of marine and fresh-water shells, mingled together in the same bed, as shown in the section, (fig. 9).

Moreover, this thick mass of peat affords us sufficient evidence that the marine shells were introduced to the different beds by various irruptions of the sea into this fresh-water lake, while the beds of peat were there forming. And it is worth remarking, at the same time, that there does not appear to be any admixture of marine fossil shells in the lower stratum (No. 7.) of the section, in which such a great number of bones of *Mammalia* have been found associated with *Paludina*, *Valvata*, *Lymnea*, *Planorbis*, *Ancylus*, *Helix*, *Vertigo*, *Clausilia*, *Unio*, *Cyclas*, *Cypris*, and seeds of *Chara;* the last being very numerous.

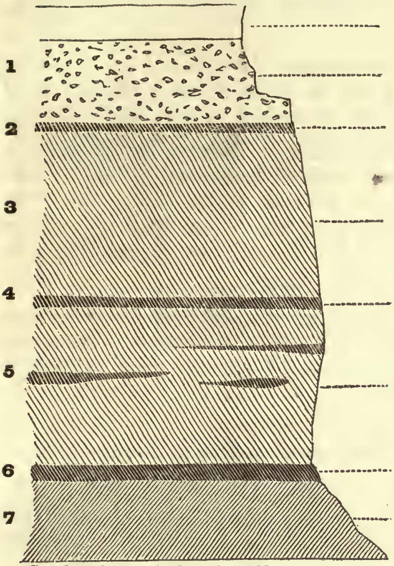
The lowest stratum appears to have been formed under different conditions to those which prevailed when the beds of mixed shells above it were deposited.

The fluviatile shells of the upper beds were doubtless derived from this lowest stratum, as the fluviatile species of the upper beds occur in great abundance in the lowest, namely, No. 1 of the section. But at the same time there are molluscous genera in the lowest stratum, which have not been met with in the newer beds: for instance, the *Unio*, which occurs in great plenty in the lowest bed, has not been found in the upper beds.

If this is the same species of *Unio* as that which is found fossil at Grays, and is figured at page 548, vol. ii. n. s. of this Magazine, according to the description there given, it does not appear either to be common as a fossil, or to be known as a recent British species.

Geologists have termed these fossil mammalian remains, *diluvial*, from the circumstance of their being very frequently found in gravel; but oftentimes their high state of preservation is opposed to the idea of their being drifted from any great distance, in company with the rough and hard materials of which gravel is generally composed. And from what I have observed of the bones of the larger *Mammalia* which were deposited in the lower bed of this formation, the good condition of these and of all the fossil bones that have hitherto been found along this coast, leads me to infer that their history is more closely connected with the fresh-water beds, than with the gravel.

I have had opportunities of observing the relation between fossil *Mammalia*, and the lacustrine beds in which they were found, at other places as well as on this coast, viz. at Stutton, Section of a Fresh-water Formation near Walton, on the Essex Coast. 9



in Suffolk, and at Copford, in Essex. In all these places the bones have been found in the midst of the fresh-water strata, and cut off from the upper and superincumbent strata of sand and gravel, by intervening beds of shells, peat, and sand.

Of the bones of fossil *Mammalia*, which have been found at Walton in such quantities, some of them have been detected with fluviatile shells adhering to them, although there is not that decided lacustrine character in the strata at Walton which we find in other localities on this line of cliffs.

According to the description given in this Magazine, for 1838, of the fossil *Mammalia* found in the deposit at Grays, there is strong evidence for presuming that a closer relation existed between those remains and the fresh-water beds of that locality, than between the remains and the sand and gravel which are superimposed to them. We hear of no fossil bones being found in the latter; but at the same time, I will readily admit that this class of fossils has been frequently found in gravel, without exhibiting any decided marks of a fresh-water derivation, and it is not always very easy to account for the anomaly of finding these remains so perfect in condition, considering that they had been found in beds of rolled flints and other hard substances, which have, at the same time, borne marks of great friction, consequent upon the action of transport by currents of water to which they must have been subjected.

The perfect state of the mammoth's jaw, of which we have such a good representation in p. 348, volume iii. n. s. of this Magazine, would lead us to infer that it has not been subject to the violent removal which attends the drifting of gravel; but that rather, as the accompanying remarks state, has been disengaged from its former deposit, by the sea-wasting of the land, in the same manner as that element is now laying bare the same kind of fossil bones embedded in fluviatile strata along our eastern coast.

ELEPHANT.—Amongst many fragments of tusks, one specimen 4 feet long, in good preservation, and another, 7 inches in diameter. Numerous grinders, several very perfect, and of full size. A large mass of the lower jaw, including the *symphysis*.
Vertebræ, 8 inches diameter, perfect.
Large *femur*, *scapula*, and corresponding *condyle*.

RHINOCEROS.—*Os frontis;* three distinct lower jaws with molar teeth remaining.
Vertebræ and detached teeth. A *radius*.

DEER.—Horns with both round and flat antlers; also branched and broadly palmated: teeth, *vertebræ* and bones of the legs, and various other parts of the skeleton.

BOS URUS.—*Vertebræ* and other parts of the skeleton.
Horns. See 'Mag. Nat. Hist.,' Vol. i. p. 160.
A horn broken off at the smaller end, to 4 inches diameter; it is still 3 feet long, and measures 20 inches in circumference at the larger end. This specimen was at least 4 feet 4 inches long when whole, and when sheathed in its original covering must have measured the enormous length of five feet on the exterior curve.

Incisor tooth of water-rat, figured in '*Reliquiæ Diluvianæ*, pl. 11. fig. 2.

HORSE.—Tooth. *Vertebræ*.

Stanway, near Colchester.

[With the above communication the author forwarded to us a box, containing specimens of the fossils to which he has referred, along with portions of the matrix in which they occur. From the contents of this box, and also from a hasty visit subsequently paid to the place itself, we anticipate the most interesting results from this discovery of Mr. Brown's. Whilst the great mass of the layers No. 5 and 6, is composed of marine shells, (we state this solely from the contents of the box, and not from our own examination of the beds themselves, since our visit only occupied about two minutes), mixed up with these were hundreds of specimens belonging to land or lacustrine genera. A list of these, with the names of such species as can be identified, will be supplied on a future occasion.

A short abstract of a paper on the mammaliferous strata of this part of England, which was read by the Editor in 1836, at the Bristol Meeting of the British Association, is introduced at p. 42, vol. ii. N. S. of this Journal. The first bed there noticed, in a table given of these strata, is thus referred to :—" Superficial gravel, containing bones of land animals, probably *washed out of stratified deposits*." As we know that Mr. Brown has lately been paying great attention to the gravel and fresh-water beds of

Essex, it is satisfactory to find his own view upon this important subject, so strongly supporting the one above quoted. Mr. Brown's collection of mammalian remains, promises, ere long, to be on a par with that of Miss Gurney, of Northrepps, Cromer, or that of Mr. Gibson, of Mile End; and it should be visited by every naturalist interested in the fossil productions of this island. We have in preparation some remarks upon the Geological features of the line of coast extending from Southend to Harwich, in the course of which we shall bring forward all the facts we can, bearing upon the relation either actual or hypothetical, which the marine and fresh-water deposits of this district have to each other.—Ed.]

SCIENTIFIC INTELLIGENCE, CORRESPONDENCE, &c. [1]

Admiralty Office, Somerset House,
March 21, 1840.

Sir,

As you have published in a former volume of your Magazine, a letter addressed to me by my brother, Mr. George Thompson, of Cape Town, accompanying a meteorolite, of the fall of which he was an eye-witness, the following further particulars relating to this occurrence, may be thought by you of sufficient interest to lay before the public. They are taken from a letter sent by Mr. Maclear, of the Royal Observatory, Cape Town, to the 'South African Commercial Advertiser,' of December 11, 1839.

I remain, Sir,
Your's &c.,
ROBERT THOMPSON.

Editor 'Mag. Nat. Hist.'

"The first account of the Meteor was brought to Cape Town by the Hon. Judge Menzies and Mr. George Thompson, who were travelling together from the Frontier. I called upon these gentlemen, and obtained afterwards a written statement from them, by which it appears the Judge's cavalcade was out-spanned on the Blood River on the morning of the 13th of October; (this River falls into the Gouritz River). The spot is sixteen hours, or about 90 miles, at their rate of travelling, eastward of the Cold Bokkeveld. Mr. Thompson states, "At about nine o'clock on the morning of the 13th of October, the meteor appeared, to the best of my judgment, to approach from the west, with great velocity, and precisely similar to a Congreve rocket

[1] Under the head of Scientific Intelligence, Correspondence, &c., we propose devoting in future numbers, a chapter to Proceedings of Societies, Extracts, and communications of a miscellaneous nature.

of large dimensions,—the phenomenon expanded nearly over-head, and apparently not more than 300 or 400 feet high, dispersing in large globes, the size of 42lb. shot, of quicksilvery appearance; then fell for a few seconds toward the earth, and vanished." Neither Mr. Menzies nor Mr. Thompson heard any noise. On reaching the Bokkeveld, they ascertained that the meteor had fallen there about the time they witnessed the phenomenon above described.

Extract of a letter from the Rev. Mr. Zahn, of Tulbagh, addressed to F. Watermeyer, Esq., and dated 6th November, 1838:—

"The object of these lines is to fulfil my promise of sending to you herewith one of the stones, which fell simultaneously during the atmospheric tremor in the Cold Bokkeveld, on the 13th of October. This stone was found between the estates of Jacobus Jooste and Pet. du Toit. Several have fallen on the place of Rudolph van Heerden, where one fell on the hard road, and was smashed to pieces. Another on a ploughed field, sunk a few inches into the ground; and a third, falling on a moist place near the water, lodged itself to the depth of several feet. Some people say they observed smoke whilst the stones fell; and also, when they were picked up a smell was observable, between sulphur and gunpowder.

"The stone which you receive lay an hour distance from the place where others were found, in the same direction in which the agitation was perceptible, viz., from N. W. to S. E., more stones were found. Some people saw in the same direction also, a dark blue streak, which lost itself in a south-easterly direction.

"I have another, somewhat larger stone in the Bokkeveld, which was too heavy for me to carry on horseback. If the latter can be of service to you, I shall not fail to send it. The present stone was found in two pieces, as it is at present."

This specimen sent by Mr. Zahn, Mr. Watermeyer was so kind as to place in my hands for Sir John Herschel, and is the same which was analysed by Mr. Faraday. It was found by P. du Toit, between his habitation and Jacob Jooste's. It weighed 27 ounces, troy. The other was afterwards forwarded by me to Sir J. Herschel, by permission of Mr. Watermeyer. It weighed about 4 lbs. 2 oz., avoirdupois, and is the same, I believe, that fell in the moist ground, close to R. van Heerden's house. I suspect its destination will be the British Museum.

When Dr. Truter, the civil commissioner of Worcester visited Cape Town, in November, 1838, I called upon him. He told me he was sitting in his office on the before-mentioned morning, when the windows suddenly shook in such a way, that he apprehended the shock of an earthquake. He examined his barometer, and found the mercury depressed to the lowest point of its range throughout the year. He likewise mentioned the leading occurrences related in Mr. Zahn's letter. Dr. Truter afterwards was so kind as to send me a map of the Bokkeveld, whereon the path of the Meteor was laid down, together with the affidavit of Kieviet, reported in your Journal of the 27th, also several specimens of the Meteorolite, including the one recognised by Kieviet. These have been forwarded to the Admiralty, together with two interesting letters from the Doctor; copies of which, I am sorry to say, I did not retain.

I will now proceed to describe the result of my visit to the Bokkeveld, accompanied by Mr. Watermeyer, Lieut. Jacob, of the Indian Service, and Mr. Bailey, of the H.E.I.C. Civil Service.

It may be proper to state that the Cold Bokkeveld is an irregular valley or basin, bounded by high rugged mountains, which is the character of the basins enclosing the towns of Worcester and Tulbagh. Within the valley of the Bokkeveld the ground undulates, and in some parts is considerably elevated, so as to partially screen the farm-houses from each other. The Schurfde Berg forms the west boundary. About a mile from its base, towards the north, is the farm-house of Rudolph van Heerden. The house of Barend Jooste is about 6 miles from the latter in a north-easterly direction. The house of Jacob Jooste is some miles further east, and the habitation of Pieter du Toit seems to be about 15 miles S. E. of van Heerden's, so that Jooste's is north of a right line joining the latter. It is unnecessary to give separately the minute details of the examination of these people with reference to the general character of the phenomenon, for they all agree in their description of it. Their attention was first excited by a violent explosion, followed by a rumbling noise, like that from heavy waggons passing over stony ground. On looking up they saw a blue stream of smoke, as if from fired gunpowder, passing over from S. W. to N. E., viz., from the Schurfde Berg range, at a point a little north of Van Heerden's, towards Pieter du Toit's At the instant, the son of Van Heerden was standing between his house and the Schurfde Berg, where he saw something fall, which he picked up. The sky was cloudless, and no wind. His mother ran out of the house, and observed another plunge into the swamp N. E. of the house, where it sunk to some depth, from whence it was afterwards removed. These positions are separated by about a mile, and were shown to us by Mrs. Van Heerden. 2dly. Barend Jooste, with two servants, was near the mountain south of his house at the moment of explosion. He saw something descend to the ground, and where it struck, the grass smoked. The meridian of this spot was shown to us by B. Jooste in person, and the specimen is that sworn to by Kieviet in his affidavit. 3dly. A servant of Pieter du Toit's was standing near his master's house. He saw something fall to the ground about a mile below the garden, in the brushwood, which he ran towards and brought to his master. The spot was shown to us by the servant. I did not enquire to whom the specimen was given. Each of these persons assert, that on approaching the meteorolites, they were so hot that they could not be taken up in the hand; also, that the sky was cloudless and calm.

I have thus enumerated *all that was seen falling at the moment of explosion.* But the curiosity of the people being excited, further search was made. A mass was discovered on the road, N. E. of Van Heerden's house, in fragments, broken apparently by striking the hard ground. Barend Jooste found a lump which separated into fragments on taking it up, owing, he thinks, to moisture before it was found. He had parted with some of it; the remainder, weighing four pounds less by half an ounce, he gave to us. Pieter du Toit found a quantity in fragments on the road near his house; and farther on towards Jacob Jooste's another, (the specimen first sent to Sir J. Herschel), and his son found a third in the brushwood, N. E. of the house. The points at Du Toit's, where these several specimens were discovered, are about a mile from each other. If a zone of one mile in breadth and 16 miles long, is conceived to extend from the Schurfde Berg, near Van Heerden's, to Pieter du Toit's, I believe all the points where the meteorolites fell will be found to lie within it. A small portion of this ground is cultivated—the remainder is covered with brushwood like that over waste land.

It appears that six persons only chanced to be in this tract at the time—two of them within a mile of each other—three close together, but about six miles from the latter—and one at Du Toit's, eight miles farther on. Beyond Du Toit's the line of direction is over rugged mountains for a considerable distance, and, I believe, uninhabited.—Hence I conceive I am warranted in supposing that but a small proportion of the original mass has been found; but enough for the purpose of analysis and future comparison; altogether about 20 lbs. avoirdupois, according to the following list:

		lb.	oz.
No. 1.	Sent to Sir John Herschel	1	13½
No. 2.	Ditto, ditto,	4	2
No. 3.	To Capt. Beaufort of the Admiralty	3	14½
No. 4.	In fragments, found by Barend Jooste, most of it in my possession	3	15½
No. 5.	Given to me by Doctor Versveld, of Stellenbosch, the property of this Observatory	„	15½
		14	13
	Estimated amount of the portions in the hands of private gentlemen, most of which I have seen	6	0
	Total	20	13

Nos. 1, 2, and 3, are covered with the fused crust all round, indicating that they separated from the original mass in a state of fusion. No. 3 is nearly so, but is cracked near the centre, and a small portion appears to have been separated from it.

The following analysis of No. 1, by Faraday, was sent to me by Sir J. Herschel:—

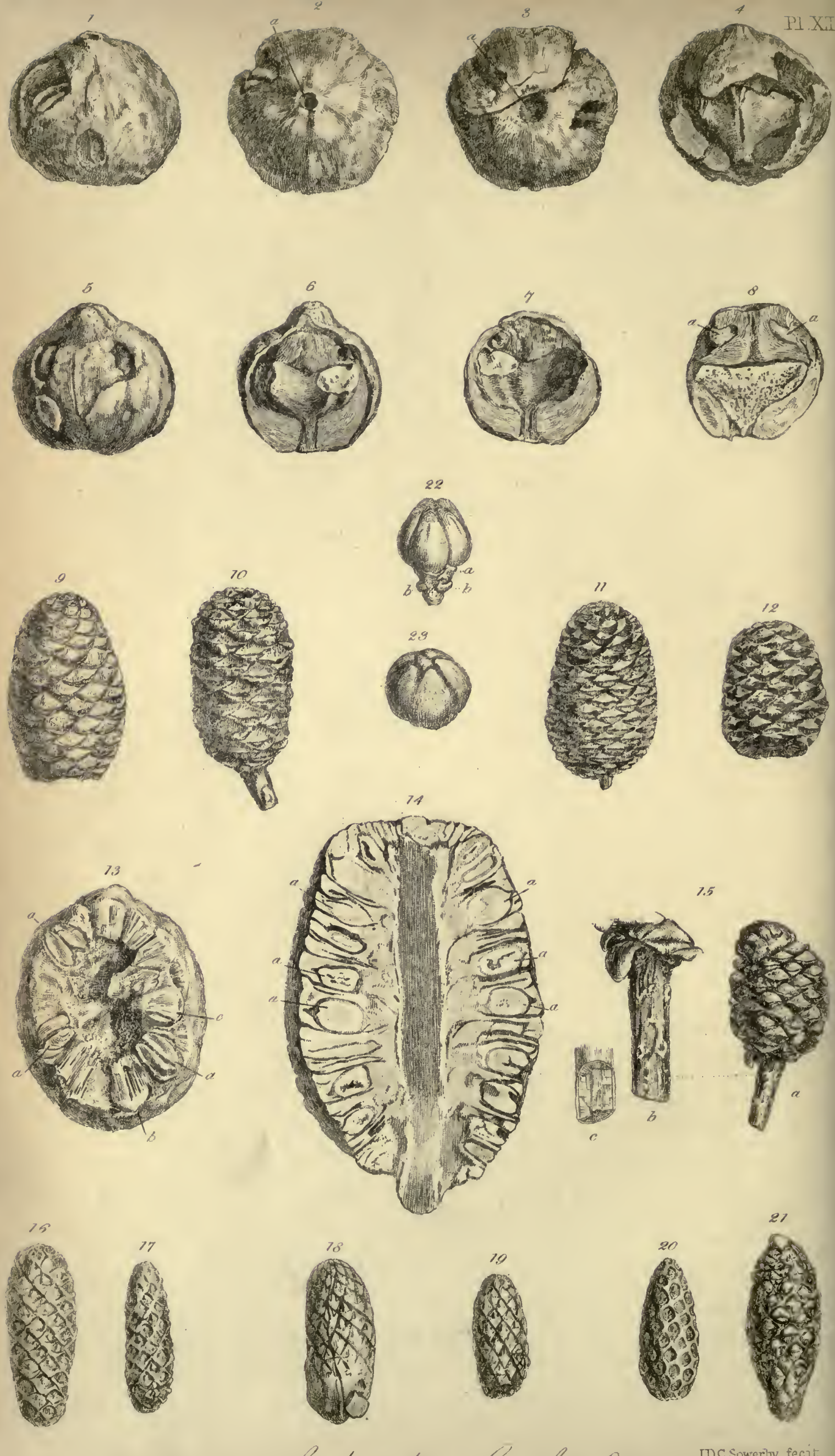
Water	6·50
Sulphur	4·24
Silex	28·90
Protox. of Iron	33·22
Magnesia	19·20
Alumina	5·22
Lime	1·64
Ox. of Nickel	0·82
——of Chrom	0·70
——of Cobalt	trace
Soda	trace
	100·44

The violence of the explosion of this meteor may be surmised from the fact, that it was heard at the distance of 50 miles from the Bokkeveld. At Worcester two reports were heard in succession, but I apprehend the second was the echo of the first, since no person in the Bokkeveld heard two explosions, and the lay of the mountains was likely to produce several echoes. The optical deception of the ball appearing to separate nearly over-head where Mr. Menzies and Mr. Thompson stood, is a proof that it was much elevated at the instant.

I have only to add, that as the material specimens of meteors do not possess any intrinsic value, beyond the extension of natural knowledge, they should be forwarded to natural depositories or scientific institutions, and not retained as mere objects of curiosity, or in the less informed circles of delusion. I saw last week a fine specimen in the hands of a farmer in the country, which was picked up by a Hottentot (belonging to his grandfather) near the Great River, who saw it fall. It must have been in their possession about 60 years. This man had refused 50 dollars for it, as a captain of a ship said it would secure the possessor against the effects of a thunder-storm!"

BRITISH MUSEUM
Z D
NATURAL HISTORY

Pl. XI



London-clay. Coniferæ &c.

J.D.C. Sowerby fecit.

MR. EDITOR,

I HAVE been so frequently applied to by geologists, as to the best mode of procuring the fossils of the London clay from the Isle of Sheppey, that I am induced to send you a few hints as to the mode of collecting in that locality. Although one of the most accessible, it is probably the least known of any of the rich geological fields that are within a short distance of the metropolis. As a trip to this interesting spot can be accomplished by an absence from London of only three days, and yet the collector be amply laden with fossils on his return, I will endeavour to put your readers in possession of the best mode of conducting such an excursion. The best conveyance is by the Southend and Sheerness steam-packets, which leave London Bridge on Tuesdays, Thursdays, and Saturdays, at 11 o'clock in the morning, and reach Sheerness about 4 or 5 o'clock in the afternoon. The town is divided into two parts,—the one contained within the limits of the garrison being designated the Blue-town, while that beyond the fortifications to the north-east is designated the Mile-town; —and it is to this portion that I should recommend the visitor to proceed, and to take up his quarters either at the Royal Hotel, or at the Wellington: the latter is an exceedingly snug and comfortable house, and is the one which I have resorted to for many years. After having established yourself in your inn, request the Boots to desire the attendance of Mr. Hays, (better known perhaps by the name of Paddy Hays), from whom you may purchase, at a very reasonable rate, some good fossils, such as crabs, lobsters, heads and portions of fishes, and numerous species of fossil fruits. Our traveller will then have accomplished all that can be done towards the acquisition of fossils until the following morning; there not being, I believe, any other collector in the town from whom purchases can be made.

On the following morning I should recommend an early breakfast, as a considerable extent of ground is to be traversed. It is advisable to go provided with five or six sheets of soft paper, to wrap fragile specimens in, and a few cotton or linen bags, of about four or five inches in diameter, to separate the large from the small fossils; the whole to be carried in a good sized blue bag or haversack, no chisel or hammer being necessary on this occasion. If our geologist has a desire to view the great section of the London clay, afforded by the cliffs of the north shore of Sheppey, and is content with comparatively the few fossils which he may be able to procure by his own exertions, he may proceed in the following manner.—Leaving Sheerness by the new town, he will pass along the sea wall, towards Minster, until he reaches Scaps-gate, where the cliffs begin to rise from the low lands of the western end of Sheppey. A few cottages are scattered round this point, some of the inhabitants of which work upon the beach, either collecting cement stone or *pyrites*, the latter being better known by the name of copperas. To these, application should be made to know if they have any "curiosities," and very frequently excellent specimens, and at a small price, will be thus procured. From this point the route will then be beneath the cliffs upon the shingle, amidst which, dark patches, ten or fifteen yards in length, will be observed, composed of nodules of *pyrites*, intermixed with pyritized fragments of branches of trees, in great abundance. It is at such spots that the numerous and beautiful specimens of fossil fruits are found; but, to ensure success, the collector must be content to go upon his knees, and carefully search among the fragments. The whole of the beach, from about the parallel of Minster church to Warden Point, abounds with these patches of *pyrites*, and I have by this means obtained in the course of a morning upwards of one hundred fine fruits of various sizes. Care must be taken in such an investigation of the coast that it be undertaken during the falling of the tide, or unpleasant consequences may arise from being shut in between the shoots of mud which are projected into the sea at many points of the coast.

If the principal object be the attainment of the greatest quantity of fossil organic remains, a different course should be pursued. The collector should then, after having made his purchases at Scaps-gate, direct his steps towards Minster church, passing which, he will proceed on the road towards Warden. About three quarters of a mile beyond the church, he will find a lane on his left hand, leading towards the Royal Oak, in which lives a woman named Mummery, and several others, who work upon the beach, and from whom fossils are frequently to be procured. These people will direct the traveller to the cottage of a family named Crockford, where there is usually a good assortment of fossils, and will direct our fossil-hunter to many other parties who also work upon the beach, and who reside between this point and Hensbrook, to which our traveller must now direct his steps. At Hensbrook enquiry should be made for a man named Pead, who has usually a considerable number of good specimens in his possession. From this point, Hensbrook, the collector must proceed along the top of the cliff towards Warden, calling at the various cottages in his way, until he arrives at Warden Point, at which place he must enquire for Mud Row, many of the inhabitants of which work upon the beach, and from whom a considerable addition to the specimens already collected may be purchased. Beyond this point nothing will be obtained, and the best way to return to Sheerness is by the road which runs through the most level portion of the country; the path along the north cliff undulates very considerably more than the road.

The course of proceeding thus sketched applies to the supposition that the time is limited to three days, but if a greater extent of time can be spared, I should recommend the tourist not to leave Sheerness without viewing the dock-yard; and the return to London may be made by the way of Chatham and Gravesend, affording the gratification of a view of the dock-yard and lines at Chatham, and of the fine old cathedral and castle at Rochester; and, at the same time, enabling him to arrive in London on the evening of the same day that he quits Sheerness.

I remain, Mr. Editor,
Yours, &c. &c.
J. S. BOWERBANK.

19, *Critchell's Place, Hoxton,*
March 24*th*, 1840.

THE MAGAZINE

OF

NATURAL HISTORY.

MAY, 1840.

Art. I.—*View of the Fauna of Brazil, anterior to the last Geological Revolution.* By Dr. Lund.

(*Continued from page* 161.)

In the order *Edentata*, there is a family which may be said to form a connecting link between the burrowing and the climbing mammals, and to which the *Megalonyx* has some points of resemblance: I mean the ant-bears. Let us see how far an examination of the construction of the hand, in connection with their known habits, is calculated to throw light on the subject before us.

In the ant-bears (*Myrmecophaga*), the claws are curved and laterally compressed, as in the sloths, and in both we find these organs exhibiting the same kind of articulation; but in the two larger species they are much shorter, though still of considerable strength. The hand is not very broad; while the number of claws is only four in the larger species, and is even reduced to two in the smallest. It is clear that such a construction is not well adapted for burrowing: and accordingly I have satisfied myself that these animals never do burrow at all. They use their claws to tear open the strongly-built nest-hills of the *Termites* (white ants); but they are not able to dig burrows under ground. In the two-fingered ant-bear (*M. didactyla*) the provisions are exactly the same as in the sloth; accordingly, also, it lives only in trees, where it subsists on the *Termites* that build there. Thus, if we compare the *Megalonyx* with that family of animals to which it indubitably bears the most resemblance, next to the sloth, the comparison is equally unfavourable to the idea of its having been a burrower. The next point to be considered is the construction of the *arm*, which is invariably short among burrowing animals, as, indeed, theory would have led us to expect. Now, in both the *Megalonyx* and *Megatherium* the fore-limbs are long, even longer than the hind ones, which is only the case with some apes, and the sloths; these two being, above all other mammals, the best climbers. Next, in the conformation of the hind feet, we find strong marks of distinction between burrowers and climbers; for instance, in moles the hind feet are remarkably weak in comparison with the fore-feet, and there is nothing extraordinary in their structure. So also in those powerful burrowers, *Dasypus gigas*, and *Das. gymnurus*, we observe in these parts the same conditions obtaining; in none of that family is there anything uncommon as to their construction.

Now, it is easy to see that the hind feet could be but of very little use to animals intended to burrow: but very different is the case with those intended for climbing. Not only must the hinder extremities afford firm support to the body, while the creature is climbing up, but often must they be entrusted with its entire weight; for instance, when it is seeking for points of attachment for its fore-claws. Exactly, therefore, as in the burrowers the main strength is placed in the fore, so in climbing animals is it in the hinder extremities. For this reason, we often see the hand in apes without an opposable thumb, or with only a rudimentary thumb, or with none at all; while the thumb is always *completely developed*, and perfectly opposed, in the *hind feet*. So, also, in other families of climbers, we find the thumb constant on the hind, while it is often absent from the fore feet; and for the same reason, the sloth is furnished with the same powerful claws on the hind feet that we have already seen it to possess on the fore feet. But both *Megalonyx* and *Megatherium* far exceed all these animals in the extraordinarily powerful development of their hind feet. The middle toe is disproportionately strong, and is provided with so immense a claw, that we can show nothing else like it in the whole animal kingdom; which proves that these creatures must have used their hind feet for some especial purpose. Their hind foot being furnished with a single claw, shows that it was not intended for digging: and the only analogy we can discover to it, is the single strong claw that bats have on their anterior extremities, which serves them to hang by. But much the most important character in the foot of the *Megalonyx* is its *oblique position* with relation to the leg, in consequence of which its sole turns inwards instead of downwards. We have already seen that this peculiar conformation in the existing creation is found in the sloth, and I have pointed out the effect produced on that animal's habits, by this organization. An approximation to this conformation is also observed in apes, which, when they walk, place the foot somewhat obliquely on the outer edge, and it is well known that this peculiarity incommodes them in terrestrial progression, as much as it fits them for climbing. But what in the apes is only, as it were, partially *indicated*, is fully developed in the sloth; which animal, in consequence of this organization, has been shown to be confined throughout its existence to an arboreal life. Thus in every one of the points of comparison we have instituted between burrowers and climbers, we have seen that *Megalonyx* constantly differs from the former, and resembles the latter: but the point to which I last alluded, I consider to be quite decisive. There is one other character in its organization, which is not quite without weight in reference to our present enquiry, I mean its unusually powerful tail. Now, it is certainly true, that many animals which are not climbers have a powerful tail, as for instance, armadilloes, &c., while others that climb well have none, as sloths, and some apes; but when we find a remarkably powerful tail attached to an animal, that, according to all probability, was a climber, we are led to infer that this organ must have served for that purpose, in other words, that *Megalonyx* was furnished with a prehensile tail.

How far the *Megatherium* is to be considered in the same light as *Megalonyx*, cannot be decided without an accurate and scientific examination of the skeleton at Madrid. Pander and Dalton do not mention any *distortion* of the hind foot, neither does their figure exhibit any. It is, nevertheless, quite possible that such may exist, but that it is disguised by the faulty manner in which the skeleton is *put up*. It strikes me as very unlikely, that two animals which agree so closely in all other striking particulars of their organization, should differ so much in *one* of the most important. The *Megatherium* has been proved by later discoveries to possess the same powerful tail as the *Megalonyx;* and as it besides corresponds with the latter entirely in the conformation of its extremities, the same difficulties present themselves against the supposition of its having been a burrower. But if the *Megatherium* was really a climber, it must have had still more occasion (on account of its greater size), for that peculiar arrangement of the hind feet, which we have described in the *Megalonyx*. I am aware that most people, from the immense bulk and clumsy make of these animals, will object to the view I have ventured to give of their habits. I confess the weight of this objection, which no one can feel more than I do. Indeed, it had the effect of long preventing me from coming to what appeared so improbable a conclusion, and impelled me to a detailed and wearisome examination of all the relations and circumstances that could bear upon the subject, to discover, if possible, some other solution of the phenomena which the osteology of the *Megalonyx* presents. This is not the place to detail all my investigations; but at least I may say this, that the more points of view in which I considered the subject, the more irresistibly was I led to the conclusion I have ventured to express; although no one confesses more readily than I do, how much, at the first glance, it appears to be at variance with nature.

In truth, what ideas must we form of a scale of creation, where, instead of our squirrels, creatures of the size and bulk of the *Rhinoceros* and *Hippopotamus* climbed up trees! It is very certain that the forests in which these huge monsters gambolled, could not be such as now clothe the Brazilian mountains; but it will be remembered, that in the former communication which I had the honour of submitting to the Society, I endeavoured to show, that the trees we now see in this region are but the dwarfish descendants of those loftier and nobler forests which originally covered these Highlands; and we may surely be permitted to suppose that the vegetation of that primæval age was on a no less gigantic scale than the animal creation.

In the present order of existing nature, all the mammals that are appointed to live in trees belong to the smaller kinds; which seems so essential a condition, that in the families and genera containing climbers, the development of this faculty diminishes in a ratio corresponding to the increase in size of the species. Thus, in the genus *Felis*, the smaller species live for the most part in trees; those of an intermediate size, hunt their prey on the ground, but climb with more or less activity; while the largest species of all are entirely deprived of that power. Again, in the family of *apes*, the existence of the smaller kinds is indissolubly linked with arboreal habits; while the larger frequently descend, and pass a considerable portion of their lives on the ground. So, also, in the ant-bears, the smallest species of all lives entirely in trees; those of middle size feed principally on the ground, but also ascend trees; while the very largest have the ground assigned them for their perpetual abode. It therefore very reasonably excites our astonishment, to find that in a former period of creation, such enormous monsters should have had trees allotted them for their *habitat*.

But, on the other hand, we must not forget that this same rule which, in the existing races, is observed with respect to those that climb, holds equally good with reference to those that burrow; which latter faculty is also restricted to the smaller species. Thus, in the genus *Canis*, foxes and jackals burrow, but not wolves: in the genus *Lepus*, rabbits burrow, but not hares: among the rodents (excluding hares and climbers), all species dig, except the largest of all, the *Capivar*. Must it not, therefore, equally excite our astonishment, to find among the relics of that extinct creation, species of burrowing animals that do not seem to have yielded in size or bulk to the huge climbers of that same period, such as *Chlamydotherium Humboldtii*, and *giganteum*, *Hoplophorus Euphractus*, &c.? And should we have brought the phenomena of the economy of that former world more into harmony with the present, by assigning to those gigantic sloths the faculty of *burrowing*, instead of climbing? If we examine this creature's habits, we shall find that they, like everything else, speak equally in favour of the latter conclusion. We are acquainted with no single existing animal which feeds on nothing but grass and leaves, that digs or burrows. And for what purpose should these monsters have burrowed? To protect themselves from their enemies? Without alluding to the length of time so bulky and helpless a creature must have required to excavate a hole sufficient for its huge carcass, of what use, I will ask, could such a *den* be for a refuge to an animal, whose food would, of necessity, often call it far away; and which, in case of danger, was so unsuited to get safely back to it, from its conformation being so ill-adapted for running, nay, as we have seen, even for walking or standing!

Should it be contended that *Megalonyx* had sufficient means of defence in its formidable claws; I ask, for what purpose then did it burrow? Certainly not to obtain food, forasmuch as all animals that seek their food underground (which can only consist of insects, frogs, and roots) are necessarily small, and provided with hands, without which conditions, the impossibility of their existence in this manner is abundantly evident. Now, what was it that formed the food of these animals? The *fæces* of one of these monsters which I have had the opportunity of examining, presented very finely-comminuted remains of plants. May we not then conclude, that they grazed like cattle and some *Pachydermata?* I answer, we find no incisors in the fore-part of the mouth, wherewith they could bite grass. Hence we are forced to conclude that they fed on leaves of trees, like their existing representatives, which they exactly resemble in their dental system; so that, as the main result of this enquiry, I think I may lay down this proposition; that, from whatever point we consider this gigantic animal's habits, we are compelled to conclude that they agreed in all respects most perfectly with those of the living type of the family, the sloth.

In conclusion, I will only remark, that unless we attribute to the *Megalonyx* the power of climbing, it will be difficult to understand how it could possibly have preserved its existence in a country that swarmed so with beasts of prey, as we shall, in the sequel, see was the case in Brazil in ancient geological periods. Let it not be imagined that their enormous bulk, or formidable claws, were sufficient for their protection. I have satisfied myself by numerous personal observations, how very easily the sloths of our day fall a prey to predatory animals that are greatly inferior to them in size and strength; and I have frequently had the most astonishing proofs of the fearlessness and powers of the predatory beasts that now exist there. It would be out of place here to describe the scenes of battle and slaughter that have passed in my own house, and under my own eyes, partly with the view of throwing light upon this very subject. But this at least I can affirm, from what I have myself seen, that if the *Megatherium* and *Megalonyx*, with their helpless powers of motion, had been confined to living on the ground, they would soon have been exterminated; and we never should have found their remains associated with those of the huge antediluvian tiger, deposited in their resting-place during those latter days that preceded the mighty catastrophe, which closed the curtain between that former, and our present world.

There are three species of this genus (all different from the N. American species, *Megal. Jeffersonii*) whose remains are found in the diluvian soil of this district. The most common of them, *M. Cuvieri*, is about the size of an ox; but in consequence of the massive build that distinguishes all this race, most of its bones, when compared with those of the ox, appear to be two or three times larger in circumference and bulk.

The two other species are much less abundant. One of them, *Megal. Bucklandi*, is the size of the tapir; while the other, *Meg. minutus*, can scarcely compete with the hog in that respect. Whether the animals of this and the preceding genus had any defensive armour, is a question that I have not hitherto been able completely to solve. Associated with one individual of *Coelodon Maquinense* I found a mass of granular concretions, which I cannot describe otherwise than as calcareous *divisions* in the skin: but among the very abundant remains of *Megalonyx Cuvieri*, I have never found any trace of armour, with the exception of a portion of an extraordinarily thick plate, whose surface presented what appeared ornaments in relief, so as to give it the look of a fragment of some architectural decoration; but its fracture sufficiently proved its organic origin.

Should this creature have really been furnished with a coat of mail, it cannot be denied that this must have as much incapacitated it for climbing up trees, as all the other conditions we have examined indisputably disqualified it for terrestrial motions.

The family of sloths is at present confined to South America; therefore, in the fact of their existence in the same quarter during a former age of the world, we have another corroboration of the result to which the history of the two previous families led us, as to the correspondence of fundamental types between the extinct and existing animals in that portion of the globe. In like manner, also, we find the other results obtained from the same former examination recurring here, namely the much greater *abundance* of generic and specific forms belonging to the ancient animal creation, and the vastly superior size which its species attained. This disproportion is even still greater if we only confine our comparison to the very district in which the fossil remains are found; for there are now none of the family I last described within its limits: although I would not venture to assert, that they also did not exist here previously to the disappearance of the forests that originally clothed this country.

(To be continued.)

Art. II.—*Remarks on the* Lepidoptera *of North America, with occasional descriptions of New Species; being the result of nineteen months' travel in the United States.* By Edward Doubleday, Esq.

> "Por aquesta razon de ti escuchado,
> Aunque me falten otras, ser merezco,
> Lo que puede te doy, y lo que he dado
> Con recebillo tu, yo me enriquezco.
>
> Garcilasso de la Vega, Ecloga 3.

I shall preface the observations I am about to make on the *Lepidoptera* of the United States, by a few remarks on the causes which led me, during my journey in those countries, to direct my attention to this order in preference to any other; and also by some short notices of the places at which I chiefly collected; these being intended to afford to my readers (should I be fortunate enough to get any), the means of drawing more profit from what will follow, especially in regard to the influence of climate and locality, as well upon individuals of any given species as upon the geographical distribution of genera and species.

The first place which I chose for a hunting ground, was the vicinity of the beautiful falls of the West Canada Creek, known as Trenton Falls. I had not long collected in this spot, when a fact which surprised me forced itself on my attention. This was the paucity of insects in general. There was, it is true, a great number of species to be met with, but they wanted a deal of looking for, and were almost invariably few in individuals. It is true, that as summer advanced, some one or two species of *Coleoptera* came forth in countless thousands, especially *Philochlænia elongata*, Dej., which, in the early part of June, stripped the young beeches of their leaves. *Cicindela sex-guttata*, F., and *vulgaris*, Say, *Orthosoma cylindricum*, and some few other *Coleoptera*, were also tolerably common. But in general, insects, though pretty numerous as to species, were few as to individuals of any species, but this was not so entirely the case in some orders as others.

The great exception was the order on which I am about to make my remarks, to wit, the *Lepidoptera*. As soon as the young leaves had fairly burst forth, a host of beautiful *Geometridæ* began to appear in the woods, and were speedily followed by a second host, composed of *Noctuidæ* and *Bombyces*. The situation of the inn in which I had fixed my abode, was peculiarly favourable for attracting *Lepidoptera* by illuminating the windows; and by following up this plan regularly, I found myself every week adding immensely to my stock of moths. Mr. Moore, the worthy host and owner of the inn, a man of sound sense, good heart and great information, and of an intellectual turn of mind,[1] soon became as earnest as myself in the work of collecting. To him and his excellent wife, a pattern of what a wife and mother ought to be, I am indebted for many species I did not take myself, as I shall hereafter mention.

There was enough to make me prefer the *Lepidoptera* above all other insects. I knew that not one fourth of the

[1] Vide Fanny Kemble.

Lepidoptera of the United States had been described; that our English collections were very sparingly furnished with them, especially with the nocturnal ones, and I found them far more numerous and easy of capture than any of the other orders. The bias thus given, continued throughout my journey of more than 8,000 miles, though I by no means allowed it to cause me to neglect other orders.

The first place, as I have said, which I chose for a hunting ground, was the vicinity of Trenton Falls. It is not my intention to tell of the waterfalls, nor the deep ravine, whose rocky sides are overhung with gigantic trees, or adorned with flowers innumerable, nor to describe the beautiful banks of the West Canada Creek, as it flows onward to join the Mohawk, nor of the wood-clothed hills, from whose summit the traveller may survey a country as fair as God has ever created for man to dwell in; nor may I here tell of the kindness that I, a solitary wanderer, far from my home, met with in this place, nor of the delightful society to be found during the summer amongst the visitors to the Falls. I could let my pen run wild on these subjects, but I will confine myself to what more properly belongs to natural history, and that part thereof on which I am now treating, and merely state what was the soil, what the vegetation, what the local peculiarities, of the spot in which I first collected in America.

The town [1] of Trenton (formerly Olden Barnevelt), is situated about fourteen miles north of Utica, in latitude 43° 20′ N., longitude 75° 10′ W., or thereabouts. The town itself is of considerable extent, containing about 16,000 acres under cultivation, and a population of between 3,000 and 4,000. A large portion of the land is still wooded, and the gradual improvement of this gives employ to a good many sawmills; fourteen, I believe, are in being within the town, though some have evidently declined working for some time. The common mode of clearing is, first to cut down the larger trees, and then stub up the brushwood, leaving the stumps of the trees to decay out of the ground. If the bushes are not cut up, as they often are not, until the year following the cutting down of the trees, the ground becomes covered with brambles, raspberries, &c., with a variety of flowers, though these are not so numerous in such places as might be expected.

Mr. Moore's house is situated on the verge of the town, close to the West Canada Creek, about two miles from the village of Trenton. Its elevation above the sea is full 1,200

[1] The word town, in New York, means nearly the same as parish in England.

feet, and the average annual temperature $47\frac{1}{2}°$. In the severe winter of 1836, the thermometer descended more than 30° below zero; the highest I ever observed it in the summer, was but little above 80°. This, however, gives a wonderful range, compared with England, though by no means uncommon in the northern parts of the United States.[1] No doubt this intense severity of the winters affects much the Entomology of this part of America.

At the back of the house was an extensive range of woods skirting the banks of the Creek on the western side, up to the village of Prospect, situated close to where the Creek enters the ravine. In front the country is more open, but beautifully diversified by large patches of wood, for every farm has its wood. Here and there are patches of boggy soil, in which grow thousands of *Cypripedium spectabile*, and a variety of ferns, of which the most interesting, perhaps, are *Onoclea sensibilis*, and *Struthiopteris Pennsylvanica*.

In general, the soil is a rather shallow stratum of black vegetable mould, on a limestone bottom. This is very fertile, the Indian corn often producing from sixty to eighty bushels per acre, where the soil is not a foot deep. In some places there is a great deal of sand, generally forming small hills. I am no geologist, so will not venture to say any very great deal about the limestone; however, I know this, that it contains a vast quantity of *Encrinites, Orthoceratites, Trilobites*, and other *ites*, of which I know nothing. Moore has a vast many beautiful specimens of fossils, amongst which the most interesting to me were the very perfect individuals of *Isoteles gigas*, and some fragments of the same, showing that it must have sometimes much exceeded a foot in length. From this account of the contents of this limestone, I leave others to judge whether it be *transition*, or any other *ition* limestone.[2]

The woods here are distinguished by the want of all the oaks, and all the *Coniferæ*, except the white cedar (*Thuja occidentalis*), the hemlock fir (*Pinus Canadensis*), and the yew, or, as it is commonly there called the running hemlock (*Taxus Canadensis*.) I do, it is true, remember, that just by the bridge where the road from Little Falls crosses the creek, there was a solitary oak, and a solitary pine, but they

[1] In that most valuable publication, the 'American Almanack,is a table for Dover, N. H., in latitude 43° 13′, which shows a mean annual range of 113°, and one year the range amounted to $127\frac{1}{2}°$.

[2] Let not my readers think that I am disposed to undervalue the science of Geology. I regret much my want of knowledge of it, but *non omnes possumus omnia.*

evidently did not much like the neighbourhood they had got into.

Having said this in regard to the woods, I need not, were my readers Americans, say any more about them, but as I suppose that amongst my readers may be some one almost as ignorant on this sylvan subject as our legislators are on all that relates to the geography, the laws, and resources of the United States, I shall have to tell what trees are there. There are plenty of hemlocks, spreading their long dark branches; there are beeches, with leaves of the lightest green; there are birches too, and maples of various species, and here and there the shad tree (*Amelanchier botryapium*) and the bird cherry (*Cerasus Virginianus*) displayed their snow-white blossoms, and the ravine is bordered by large clusters of giant cedars, often of the most grotesque forms. The elms here attain an enormous size, though I have rarely seen them so beautiful in form as in the more Eastern States. I measured one fallen elm here which was ninety feet before it branched. The sycamore, or button wood, of the Americans (*Platanus occidentalis*) also occurs here, and the basswood (*Tilia Americana*), is abundant. The undergrowth consists of two or three species of *Viburnum, Acer, Lonicera, Rubus, Sambucus rubens*, and young growth of the various hard-wooded trees. On the skirts of the woods, and by the road-sides, the red and purple-berried sumachs (*Rhus glabra* and *Rhus typhina*) abounded, and the beautiful flowering raspberry (*Rubus odoratus*) hangs from the sides of the rocks, as if trying to bathe its rosy blossoms, or delicious fruit in the amber waves, or forms large thickets on the sunny sides of the wood.

The spring, the short spring of New York, calls up a host of flowers here. First, the little blue *Hepatica* peeps from the clefts of the rocks,[1] and is soon followed by the fragrant *Dielytra cucullaria*, and the spring beauty (*Claytonia Virginica*.) Then follow *Violæ*, blue, yellow, and white; *Trillia, Uvulariæ, Convallariæ* of various species; *Aquilegia Canadensis*, the gold thread (*Coptis trifolia*), three species of *Actæa, Tiarella cordifolia, Clintonia borealis, Streptopus roseus*, and a variety of other flowers too numerous to mention.

The march of summer is announced by the flowering of the *Compositæ*, which continue until late in autumn. In summer, too, various species of *Asclepias, Lobelia, Desmo-*

[1] I have often seen this flower blue, pale blue, and white, in America, never pink. *It is said* to be found with pink flowers in the mountains of Carolina.

dium, &c., are to be found in flower, and then the ravine is in its beauty. I could here run riot in telling of its summer glories; of the rocks clothed with flowers and ferns; of the dark branches of the cedars and hemlocks; of the blue birds, the baltimores, the scarlet tanagers, that make it their abode; but I must not do so. I only just hint to my readers that such things are, and that there are in that ravine beauties, which they

> "Caged in the space of Europe's pigmy span
> Can scarcely dream of;—which their eyes must see,
> *To know how beautiful this world can be.*"

So much for the spot where some of the happiest hours of my life were spent, and where I first learned "to honour the Americans as a nation, and to love many of them as personal friends," feelings which grew upon me more and more the longer I stayed amongst them.

The period I spent in this place was from the middle of May to the middle of August, except that I took a short excursion, of about three weeks, to Niagara, the borders of Pennsylvania, and the central part of New York, during which journey, of about 600 miles, I travelled over some interesting country, never before visited, I believe, by an English traveller. That my time was not idled away during the many weeks I spent in that spot, I hope soon to show, by the frequent mention I shall make of it when I come to the moths.

In August I left the house I had so long made my home, and proceeded west. On this journey I did not stay long in any one place, in fact, a few days hunting near Cincinnati, sometimes in company with that excellent botanist and right good-hearted man, Thomas G. Lea, was all I did until I reached Albion, in Edward's County, Illinois. At that place Mr. Foster (who had rejoined me at Cincinnati), and myself spent several days. It was at the house of a near relation of more than one English entomologist, that we were most kindly entertained. The situation was truly beautiful, and the fine October weather added to the pleasure derived from seeing Nature where man had molested her so little. The house stood on the edge of a small prairie, now one waving mass of asters, of all shades of blue and white, and sometimes purple, *Solidagines*, *Rudbeckiæ*, and an infinity of composite flowers. This prairie, probably, is not above four miles long, and is skirted by the most noble timber, with here and there scattered clumps of trees. What nobleman in England has a park to be compared to it!

> "These are the gardens of the desert, these
> The unshorn fields, boundless and beautiful,

For which the speech of England has no name.
Man hath no part in all this glorious work,
The hand that built the firmament hath heaved
And smoothed these verdant swells, and sown their slopes
With herbage, planted them with island groves
And hedged them round with forests."

No one who has not seen them, can have an idea of the prairies, and I am quite sure I can find no means of giving anything like an idea of them, so I shall attempt it no more.

Here the autumnal butterflies were still numerous, and there were a great many moths attracted by the lamps, during the warmer evenings.

Leaving our Illinois friends, we proceeded to St. Louis, the spot where first the French settled on the banks of the "Father of Waters," and thence to Alton, on the same river, about eighteen miles north of St. Louis. We had but two or three days to collect here, but I was rewarded by finding one butterfly, never before found, I believe, in the United States.

We now travelled onwards to Chicago, and thence to Green Bay and Mackinaw, and back to New York, a journey highly interesting, but which, from the season, and other causes, produced us but little in the way of Entomology. From New York we started for the south in November, merely staying a few days in the various cities we passed through, until at length we came to an anchor for about six months at St. John's Bluff, East Florida, which place I must now try to give an idea of.

(To be continued.)

Art. III.—*Observations upon the Affinities and Analogies of Organized Beings.* By Hugh E. Strickland, Esq., F.G.S.

I have read with much interest the paper by Mr. Westwood, at page 141, on affinity and analogy. The writings of this gentleman are distinguished no less for scientific accuracy than for a spirit of sound philosophy, untainted by those visionary and theoretical views entertained by some of our modern zoologists. Instead of assuming an *à priori* system of his own, and then twisting facts into a partial coincidence with that system, he is content to take Nature as he finds her, and not the less to admire her luxuriant variety because she refuses to marshal her irregular troops into straight lines, circles, or pentagons. This healthy tone of mind imparts a high value to all that proceeds from Mr. Westwood's pen, and it is, therefore, with much diffidence that I venture to make a few remarks on the short essay above referred to.

There is no branch of the philosophy of Zoology so obscure as the subject of affinity and analogy; and although many naturalists can correctly apply these two kinds of relations to particular cases, yet few can give any clear explanation of the rules which influence their practice. Mr. Westwood's remarks go deeper into the subject than those of most of his predecessors, yet it seems to me that he has not quite set the question in its true light. Before referring to his observations, I will endeavour to explain my own views on this difficult subject.

Relations of affinity and analogy are in my opinion perfectly distinct from each other in every point of view. In order to arrive at their definitions, we must first prove the existence of a real *natural system*, a subject which involves an enquiry into the designs of creative power, one of the most awful themes which the human intellect can attempt. The most obvious and undeniable examples of design in the organised creation are seen in the adaptation of each species to the circumstances in which it lives. Now, if this were the *sole* mark of design, if each species constituted a being *per se*, adapted to its peculiar condition of existence, but not allied in physiological structure to its fellow species, there would then be no *natural system;*—man might indeed classify such objects according to their accidental or fancied resemblances, but there would be none of those essential peculiarities of structure which we find to pervade vast groups of beings whose external forms are often widely dissimilar. The existence then of a comparatively few grand types of structure, or "centres of creation," from the different modifications of which the innumerable species now existing derive their characters, may be taken as a proof that species were created not absolutely, but relatively, — not merely with reference to their destined mode of life, but also with reference to other species whose destination was similar, though not identical with their own. If these views be correct, it results that the *resemblances* of *different* species in essential points of structure, furnish evidences of design, less obvious, perhaps, but not less certain, than the *adaptation* of any one species to its external condition of existence; and the "natural system" thus acquires an air of truth not inferior to the ocular demonstrations of anatomy. The reality of the natural system is not affected by the difficulty experienced by man in detecting it; for it is no more to be expected that systematists should have already unravelled all the resemblances between species contemplated by the Creator, than that anatomists should have arrived at the final cause of every organ of the human body. The variety of classifications adopted by different naturalists, shows that we are still far from the true system of Nature, yet I think there can be no doubt that naturalists have already sketched out its principal features with considerable accuracy. Who, for instance, can doubt that such groups as *Vertebrata, Insecta, Mammalia, Pisces, Coleoptera*, &c., are not merely human generalizations, but real apartments in the edifice of the Divine Architect? It is not, however, sufficient, that man should detect these natural groups,—he must also give a definition of their characters,—not of the *superficial* and *arbitrary* ones, but of the *essential* and *important*, and this is often the most difficult part of his task. Although these *essential characters* form the groundwork of the natural system, yet no rule can apparently be laid down for their determination in particular cases. All that man can do is to use his best judgment in selecting such characters for a group, as seem to him the most important in their influence on the vital functions of the beings which compose it. They must, in great measure, be left to the determination of what Linnæus called a "latent instinct" which Professor Whewell defines to be "an unformed and undeveloped apprehension of physiological functions.[1]"

When by these considerations we have arrived at the notion of a natural system, composed of natural groups arranged in a determinate order, we may proceed to define *affinity* as *the relation which subsists between two or more members of a natural group*, or in other words, *an agreement in essential characters*. After the *essential characters* of such a group have been discovered and defined, then all the objects which possess those essential characters are said to have an affinity for one another. Hence we see why the idea of a *natural system* is necessary to the definition of *affinity*, for in an *artificial system* the characters of the groups are not *essential*, but *arbitrary*, and the relation between the members of such a group would be, not *affinity*, but mere *resemblance* or *analogy*. Thus, if an author were to establish the characters of the class *Pisces*, not on the essential characters derived from the circulatory system, but on the arbitrary one of being adapted for swimming, he would then include the *Ce-*

[1] History of the Inductive Sciences, vol. iii. p. 312.

tacea and the *Phocidæ* among his fish. Now, on comparing a porpoise with a cod, no one could deny that they both were fish according to the assumed definition, yet no naturalist would assert the resemblance between them to be one of affinity. It is evident then, that the word affinity derives its meaning from a belief, acknowledged or tacit, in a *natural system*, and I do not see how a person who denies the latter, can attach any meaning to the former, as distinguished from *analogy*.

From the above definition of affinity, it follows that *the degree of affinity is inverse to the rank of the group*, in other words, that the members of the lowest group have the highest or nearest affinity, and *vice versâ*. The nearest of all affinities is that which subsists between species of the same genus, and the most remote is that between *animals* and *vegetables*, as members of the next highest group, viz. *organized bodies*. The affinity between two very distantly allied species, is merely that between the highest separate groups to which they belong. Thus, the affinity between a bat and a goatsucker (to take Mr. Westwood's illustration), is merely that which subsists between mammals and birds, as members of the group *Vertebrata*, and is seen quite as perfect in the whale and the humming-bird, or any other examples of the two classes. By parity of reasoning, the affinity of a goatsucker to a dragon-fly is merely that which subsists between the subkingdoms *Vertebrata* and *Annulosa*, as members of the natural group *Animals*, and is, therefore, quite as strongly exhibited in the case of a shark and a butterfly, or an elephant and a mite, &c. We thus perceive the distinction between affinity and analogy to consist, not in *degree*, but in *kind*, for there is undoubtedly a very strong *analogy* between a goatsucker and a dragon-fly, though the *affinity*, as above shown, is very remote. Analogy, in short, is nothing more than *an agreement in non-essential characters*, or a resemblance which does not constitute affinity. Hence, analogy is necessarily a very partial resemblance, existing, as Mr. Westwood remarks, in the "numerical minority" of characters, and often confined to one organ alone. Analogy originates, not in the *intentional relation* of one species to another at their first creation, but in the other instance of creative design above referred to, viz. the *adaptation* of organic beings to their destined conditions of existence. To perform any given mechanical action, there is one, and in general, only one, arrangement of mechanical structure which is better adapted to that end than all others, and hence, when any two beings, whose affinities are remote, are destined to perform a similar function, we find that they are provided with more or less similar instruments for that purpose. The *resemblance*, in such a case, goes no further than the fulfilment of the required object, and may, therefore, be regarded as *unintentional*, or, in common parlance, accidental. For instance, there can be no question, that a lengthened form, destitute of sharp angles, and anteriorly pointed, is the best adapted for passing through the water; and accordingly, we find it to prevail, not only in fish, but in *Cetacea*, aquatic birds, *Dyticidæ*, *Notonectidæ*, cuttlefish, &c., and man imitates it in his naval constructions. Yet we have no evidence that such *resemblance* is *intentional*, or in other words, that whales and *Dyticidæ* were created *for the sake* of resembling fish, but we merely suppose that in each case, the boat-shaped structure was given to adapt the animal to an aquatic life. The examples of these analogies are innumerable, and appear to me to be owing to the fact, that the real variations of circumstances which this planet affords are very few, compared with the number of organized beings destined to inhabit it, so that the performance of the same function continually recurs in different groups of the natural system, and requires, in each case, a corresponding or analogous organization. Thus, e. g. there are not more than *four* principal varieties of *locality*, viz. the air, the ground, shallow water, and deep water. These four variations of *habitat* have determined the structure of the four orders of birds, *Insessores*, *Rasores*, *Grallatores*, and *Natatores*. Again, the twofold division of *food* into *animal* and *vegetable*, has caused the group *Raptores* to be divided off from the *Insessores*, and we thus get the five groups under which the class, birds, is commonly arranged. Now, as every other species of animal must inhabit one of the above four localities, and must feed on one of the above two kinds of food, it follows that the organs of locomotion and of nutrition, are susceptible of comparatively very few grand differences of structure, and that the inhabitants of the same element, or the eaters of the same food, must present numerous points of resemblance, quite independent of their *natural* or *essential affinities*. This it is which has given to distantly allied groups an *appearance* of regularity in their analogies, whence has arisen the "theory of representation," respecting which I will take occasion to say a few words.

The theory of representation announces, that "the contents of every circular group are symbolically or analogically represented by the contents of every other circle in the animal kingdom.[1]" This has always appeared to me one of the most unsound and unphilosophical of the doctrines maintained by the advocates of the circular system. It seems derogatory to Creative Power to suppose that the principle of *representation* had any place in the scheme of creation, or that certain organs were given to species, not with a view to the discharge of certain destined functions, but for the apparently useless object of *imitating* or *representing* other species in a distant part of the system. The advocates of this theory would have us believe that the long tail of the horse was given it, not for the purpose of brushing off flies, but in order to represent the long "tail" [train] of the peacock,[2] and that both pigs and humming-birds have small eyes, *because* they are the tenuirostral types of their respective "circles."[3] Without wasting words upon the serious discussion of such puerilities, I will merely repeat my deliberate conviction, that relations of analogy are not to be regarded as affording any evidence of προαίρεσις, or *intention*, in the scheme of creation, but are mere coincidences of structure, incidental to the grand design of adapting a large number of organized beings to perform a comparatively limited number of functions.

It will be seen that the above view of affinity and analogy differs considerably from that of Mr. Westwood, in p. 143 of this Magazine. Mr. W. seems to regard affinity and analogy as the same relation under different points of view, and as depending upon the numerical majority or minority of the points of agreement between the objects compared. Mr. Westwood's views may be explained by the following tabular arrangement, showing the number of points of agreement between four analogous genera.

Goatsucker.	Bat.	Dragon-fly.	Dionæa.
Organized,	Organized.	Organized.	Organized.
Animal.	Animal.	Animal.	
Vertebrate.	Vertebrate.		
Fly-catching.	Fly-catching.	Fly-catching.	Fly-catching.
4	4	3	2

According to Mr. Westwood, the dragon-fly would be said to have an *affinity* to the bat or goatsucker, and an *analogy* to the *Dionæa*, because it agrees with the former creatures in three points, and with the latter in only two. Again, the bat has an *affinity* to the goatsucker, from agreeing with it in

[1] Swainson, 'Geog. and Classif. of Animals,' p. 230.
[2] Swainson, 'Classif. of Birds,' vol. ii. p. 159.
[3] Ib. vol. i. p. 43.

four points, and an *analogy* to the dragon-fly and *Dionæa*, from agreeing with them in only three and two points respectively. So that an *affinity* subsists between the bat and dragon-fly, when compared with the *Dionæa*, and an *analogy*, when compared with the goatsucker. This seems to me to be a correct statement of Mr. Westwood's views, if I rightly understand them, and they certainly merit the praise of ingenuity. It seems to me, however, that they contain a fallacy, owing to Mr. W. not having attended to the distinction between *essential* and *non-essential* characters. Thus, the words *organized*, *animal*, and *vertebrate*, in the above table, refer to characters of the highest importance to the vital functions of the creature, and consequently, to its place in the natural system, whereas the word *fly-catching* merely relates to a point of detail in the habits of the creature, of very secondary value, compared to the former characters.[1] I should say then, that these four creatures have *affinities* for one another, in consequence of their agreeing in the essential characters above stated, and that the degree of their affinities is proportionate to the number of the essential points in which they respectively agree, but that their *analogies* are derived solely from the one non-essential point of *fly-catching*, which applies to them all in an equal or nearly equal degree. In short, however strong may be the *analogy* which the goatsucker bears to the dragon-fly, I do not consider that it has any more *affinity* to the latter, than it has to a beetle, a lobster, or any other of the *Annulosa*.

Since writing the above, I have referred to the very valuable remarks by Mr. Blyth on affinity and analogy, in 'Mag. Nat. Hist.,' vol. ix. p. 399, &c., to which I had not sufficiently attended at the time of their publication. His views appear to me to be more nearly correct than any others which I have seen in print. The chief point in which they differ from mine, is in the introduction of a third term, *approximation*, as distinct both from affinity and analogy. Mr. Blyth considers it to be a strong resemblance between certain members of groups really distinct, and he illustrates it by the similitude of *Anthus* to *Alauda*, of *Ornithorhynchus* to birds, of *Myxine* to *Mollusca*, &c. Now, it seems to me, that this *approximation* resolves itself into affinity or analogy, accordingly as we admit one or other of these two propositions, either that natural groups are quite distinct from each other

[1] I only mean that the character of *fly-catching* is unimportant in comparing groups of such high rank, but of course it becomes an essential character when applied to smaller groups, such as *families* or *genera*.

in every part of their contents, or that they touch or show a tendency to touch each other at some particular point. Thus, if we suppose *all birds* to be equally distinct in essential structure from *all mammals, all Vertebrata* from *all Mollusca*, it is plain that the *approximation* between *Ornithorhynchus* and birds, and between *Myxine* and *Mollusca*, resolves itself into mere *analogy*. But if birds have a *tendency* to unite with *mammals* by means of *Ornithorhynchus*, and *Vertebrata* with *Mollusca* by means of *Myxine*, then this *approximation* must be regarded as an affinity. So that in either case, *approximation* is not to be considered as a distinct principle, but only as an undetermined analogy or affinity.

With regard to the above enquiry, I am inclined to believe that the larger natural groups are not only widely separated, but have no real tendency to unite,—that no mammal, for instance, is in essence any nearer a bird,—no vertebrate any nearer a mollusc than another. Be this, however, as it may, we cannot assert the same complete separation of natural types, when we look to the smaller groups. There can be no doubt that the lower groups, such as families and genera, do, in numerous instances, come into contact, or *pass into* one another, and in other cases, where the contact is not complete, yet a *tendency* towards it is very evident, and in such cases, the *approximation* becomes one of real affinity. Such is most probably the case with *Anthus* and *Alauda*, quoted by Mr. Blyth as examples of approximation.

Cracombe House, Evesham, Worcester.
April 10, 1840.

ART. IV.—*Notice of a few rare Plants, collected principally during the Autumn of* 1839, *in Jersey*. By JOSEPH DICKSON, Esq.

THE subject which I now bring before your notice, appears to me to be one of considerable interest, for two reasons :— First, as relating to a field, which those who are really fond of Botany, and who wish to make their herbariums as complete as possible, will not fail to visit; many plants being found in this island, which, if not entirely wanting, are for the most part extremely rare in the British Flora. The second reason is, that no slight degree of controversy exists, as to whether the plants of this and the neighbouring islands should be included in the English or French Flora.

As far as I am able to institute a comparison (having visited Brittany and Normandy), the Botany of Jersey (more particularly the N. E. parts), much more resembles that of the above provinces of France, than of the southern counties of England.

Mr. Babington, of Cambridge, having given a very able sketch of the history of Guernsey and Jersey in a little work lately published, I refrain from giving any description of them, fearing to trespass too far on the part of your valuable journal allotted to this paper. I cannot, however, help noticing, that there is one locality in Jersey which Mr. B. appears to me not to have visited; I refer to a place called "Les Veaux," a beautiful spot towards the S. E. part of the island, consisting alternately of wood and plain, mountain and valley, and where the plants that are found are of the richest, rarest, and most luxuriant kinds.

It may not be out of place here to correct an erroneous statement I have seen in some works on Botany, namely, that *Centaurea Isnardi* is found in *pastures in the Isle of Jersey.* From four years constant study of the Botany of this island, I think I may safely affirm that no such plant is found there; in this view I am supported by the testimony of several other botanists: it is, however, found in one spot in Guernsey, on the authority of Mr. Babington. I will now proceed to the immediate object of the present paper, by giving a list of the plants I have found in the above-mentioned island.

Those marked thus * I have myself discovered.

RANUNCULACEÆ.

*ANEMONE *nemorosa.*
RANUNCULUS *aquatilis.* (Two var. a large and a small one.
———— *Lingua.*
———— *ophioglossifolius.*
DELPHINIUM *Consolida.*

BERBERIDEÆ.

BERBERIS *vulgaris.*

PAPAVERACEÆ.

*PAPAVER *maritimum.*
GLAUCIUM *luteum.*

FUMARIACEÆ.

FUMARIA *officinalis.* Rather rare.
———— *capreolata.*

CISTINEÆ.

HELIANTHEMUM *guttatum.*

FRANKENIACEÆ.

FRANKENIA *lævis.*

LINEÆ.

*LINUM *usitatissimum.*

POMACEÆ.

MESPILUS *germanica.*

CRUCIFERÆ.

MATTHIOLA *sinuata.*
CHEIRANTHUS *Cheiri.*
*ARABIS *hirsuta.*
CARDAMINE *pratensis.* Two var.

CARDAMINE *hirsuta.*
GLYCE *maritima.*
EROPHILA *vulgaris.*
COCHLEARIA *Armoracia.*
——— *officinalis.*
——— *anglica.*
——— *danica.*
THLASPI *arvense.*
TEESDALIA *Iberis.*
CAKILE *maritima.*
ALLIARIA *officinalis.* (Rare).
CORONOPUS *didyma.*
*LEPIDIUM *Draba.*
BRASSICA *oleracea.*
SINAPIS *cheiranthus.*
DIPLOTAXIS *muralis.*
*CRAMBE *maritima.*
RAPHANUS *maritimus.*

CARYOPHYLLEÆ.

DIANTHUS *prolifer.*
*SILENE *quinquevulnera.*
——— *conica.*
——— *nutans.*
——— *noctiflora.*
SAGINA *maritima.*
STELLARIA *nemorum.*
HOLOSTEUM *umbellatum.*
ARENARIA *tenuifolia.*
*——— *media.*

MALVACEÆ.

*MALVA *pusilla.*
LAVATERA *arborea.*

HYPERICINEÆ.

ANDROSÆMUM *officinale.*
HYPERICUM *elodes.*

GERANIACEÆ.

GERANIUM *rotundifolium.*
——— *columbinum.*
*——— *nodosum.*
ERODIUM *maritimum.*

LEGUMINOSÆ.

*ONONIS *reclinata.*
*——— *spinosa.*
MEDICAGO *denticulata.*
——— *minima.*
——— *maculata.*
TRIFOLIUM *glomeratum.*
*TRIFOLIUM *arvense.* A very woolly variety.
LOTUS *angustissimus.*
*VICIA *lutea.*

ONAGRARIÆ.

EPILOBIUM *angustifolium.*
*ŒNOTHERA *biennis.*
ISNARDIA *palustris.*

CALLITRICHINEÆ.

CALLITRICHE *pedunculata.*

TAMARISCINEÆ.

TAMARIX *gallica.*

CUCURBITACEÆ.

BRYONIA *dioica.*

ILLECEBREÆ.

ILLECEBRUM *verticillatum.*
POLYCARPON *tetraphyllum.*

CRASSULACEÆ.

UMBILICUS *pendulinus.*
SEMPERVIVUM *tectorum.*

SAXIFRAGEÆ.

*PARNASSIA *palustris.*

UMBELLIFERÆ.

DAUCUS *Carota.*
——— *maritima.*
ERYNGIUM *maritimum.*
——— *campestre.*
PEUCEDANUM *officinale.*
*ŒNANTHE *pimpinelloides.*

STELLATÆ.

*GALIUM *saxatile.*

CAPRIFOLIACEÆ.

*LONICERA *Xylosteum.*

LORANTHEÆ

*VISCUM *album.*

COMPOSITÆ.

*Chrysocoma *Linosyris.*
Solidago *Virgaurea.*
*Achillea *Ptarmica.*
*Lactuca *muralis.*

BORAGINEÆ.

Echium *violaceum.*
*Anchusa *officinalis.*

CONVOLVULACEÆ.

Calystegia *Soldanella.*
Cuscuta *Epithymum.*

CAMPANULACEÆ.

*Campanula *rotundifolia.*
——— *hederacea.*

PLANTAGINEÆ.

Plantago *major.* Small var.
——— *maritima.*

PLUMBAGINACEÆ.

Statice *Limonium.*
——— *spathulata.*
*——— *reticulata.*
Armeria *maritima.*

OLEACEÆ.

Ligustrum *vulgare.*

ERICEÆ.

Calluna *vulgaris.*
Erica *Tetralix.*
*——— *vagans.*

PYROLEÆ.

*Pyrola *rotundifolia.*

MELAMPYRACEÆ.

*Melampyrum *pratense.*

GENTIANEÆ.

Exacum *filiforme.*
Erythræa *littoralis.*
——— *latifolia.*
——— *pulchella.*
*Gentiana *campestris.*
Menyanthes *trifoliata.*

SOLANEÆ.

Datura *Stramonium.*
Hyoscyamus *niger.*
Verbascum *Thapsus.*
——— *nigrum.*
*——— *lychnitis.*
Solanum *nigrum.*
Atropa *Belladonna.*

PRIMULACEÆ.

Glaux *maritima.*
Samolus *Valerandi.*
Primula *veris.*
Anagallis *arvensis.* Flesh-coloured flowers.
——— *cærulea.*

LENTIBULARIÆ.

Utricularia *vulgaris.*
Pinguicula *vulgaris.*
——— *lusitanica.*

SCROPHULARINEÆ.

Bartsia *viscosa.*
——— *Odontites.*
*Digitalis *purpurea.* A white-flowered variety.

OROBANCHEÆ.

Orobanche *major.*
——— *minor.*
——— *cærulea.*

VERBENACEÆ.

Verbena *officinalis.*

LABIATÆ.

*Salvia *pratensis.*
*Ajuga *Chamæpitys.*
*Lycopus *europæus.*
*Mentha *pratensis.*
Thymus *Serpyllum.*
Origanum *vulgare.*
Melissa *Acinos.*
*Melittis *Melissophyllum.*
*Galeopsis *villosa.*
Lamium *album.*

LAMIUM *incisum.*
*——— *Galeobdolon.*
MARRUBIUM *apulum.* A woolly variety of *vulgare.*
SCUTELLARIA *galericulata.*

ELEAGNACEÆ.

*HIPPOPHAE *rhamnoides.*

POLYGONEÆ.

RUMEX *maritimus.*
——— *Nemolapathum.*
POLYGONUM *Persicaria* (white-flws.)
——— *Bistorta.*
——— *Fagopyrum.*
——— *Convolvulus.*

CHENOPODEÆ.

SALSOLA *Kali.*
*CHENOPODIUM *botryodes.*
*——— *maritimum.*
*——— *fruticosum.*

CUPULIFERÆ.

CASTANEA *vesca.*

URTICEÆ.

PARIETARIA *officinalis.*
*URTICA *pilulifera.*
HUMULUS *Lupulus.*

FLUVIALES.

ZOSTERA *marina.*
RUPPIA *maritima.*

IRIDEÆ.

*TRICHONEMA *Bulbocodium.*
IRIS *fœtidissima.*

ORCHIDEÆ.

SPIRANTHES *autumnalis.*
LISTERA *ovata.*
EPIPACTIS *palustris.*
ORCHIS *mascula.*
——— *maculata.*
——— *latifolia.*

AMARYLLIDEÆ.

*GALANTHUS *nivalis.*
*NARCISSUS *poeticus.*
*——— *Pseudo-narcissus.*

LILIACEÆ.

TULIPA *sylvestris.*

ASPHODELEÆ.

ASPARAGUS *officinalis.*
SCILLA *autumnalis.*
HYACINTHUS *non-scriptus.*

JUNCEÆ

JUNCUS *capitatus.*
*——— *maritimus.*

DIOSCOREÆ.

TAMUS *communis.*

ART. V.—*On the Fossil Shells of the Crag.* By S. V. WOOD, Esq., F.G.S..

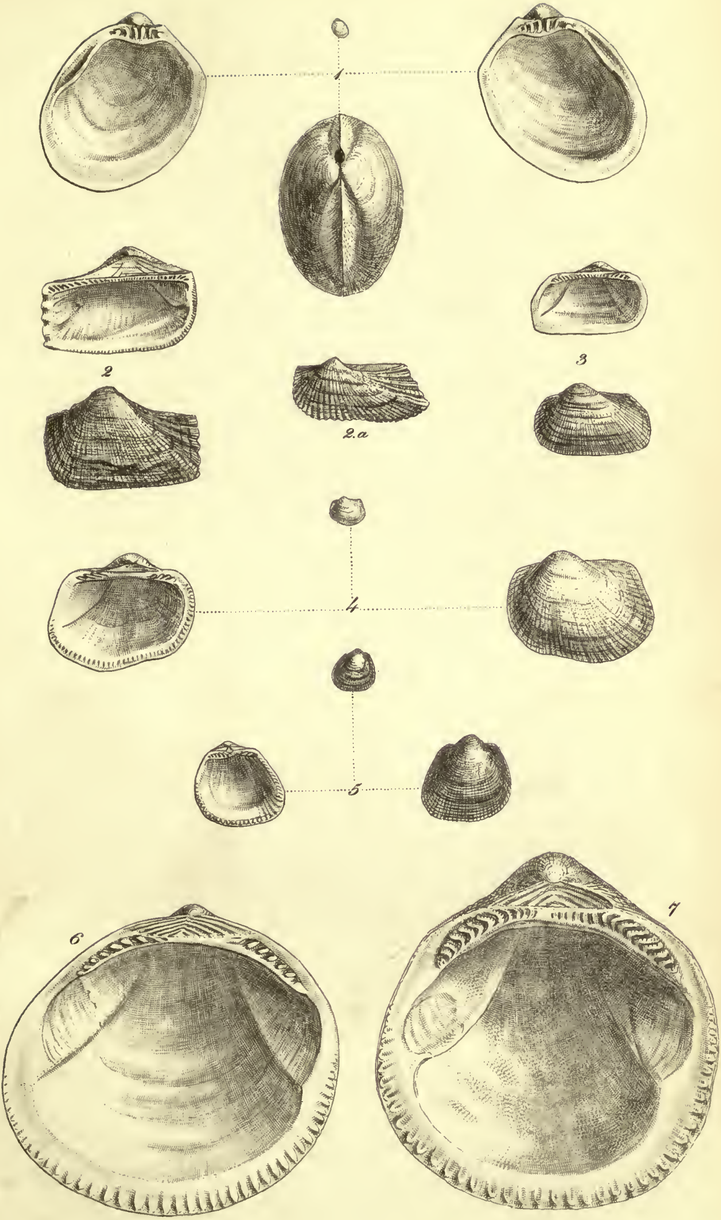
PLEURODON, (new genus.)

Shell bivalve, inequilateral; hinge-line curved; cardinal teeth several; one large lateral tooth; ligament external.

Pleurodon ovalis, Nob. Suppl. Pl. No. xiii. fig. 1.

Nucula miliaris(?), Desh. Coq. Foss. des Env. de Par. Pl. 36, fig. 7—9.

Shell ovato-deltoidal, gibbous, smooth, margin entire, 5—6 converging cardinal teeth. Largest diameter $\frac{3}{32}$ of an inch.



Crag Arcacea.

J.D.C.Sowerby fecit

BRITISH MUSEUM
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NATURAL HISTORY

Coralline crag, Ramsholt and Sutton.

I propose to give the generic name *Pleurodon*, to a small coralline-crag shell, which it seems to me cannot properly be referred to any genus hitherto established. The *Nucula miliaris* of M. Deshayes, if not specifically identical with this shell, must be closely allied to it; and in placing the French fossil in the genus *Nucula*, M. Deshayes has expressed a doubt as to the correctness of his determination, in consequence of the existence of the lateral tooth. The affinities of *Pleurodon*, as indicated by the shell alone, probably justify its being placed with the *Arcaceæ;* though its precise position in that family cannot be determined in the absence of a knowledge of the animal.

At Ramsholt the valves are generally found united, the large prominent teeth retaining them in apposition. I have given as a synonyme the above name of Deshayes, presuming his *Nucula miliaris* may be the same shell, but I am unable to discover the internal pit for the ligament he speaks of, and which his figure so conspicuously displays. The crag specimens have a depression placed externally to the teeth on the shorter side, where I imagine the ligament to have been situated. Deshaye's figure shows seven teeth, whilst the shells from the crag never display more than six, and seldom more than five.

Arca Noæ, Auct. Pl. xiii. fig. 2 and 2 *a*.

Red crag Sutton, fig. 2. Coralline crag, Sutton, fig. 2 *a*.

I feel unwilling to give a new name to this shell, since a very scrupulous examination inclines me to the belief of its not being specifically different from a well-known British species, although it was a matter of some difficulty to satisfy myself on this point. My crag specimens vary much, as do also the recent shells: a young one of the latter in my possession, corresponds with the fossil in all respects except in the direction of the teeth, which, in the recent, are vertical, while in the fossil, those placed towards the extremity of the hinge-line are much inclined: I believe, however, that in the recent shell this character may be occasionally observed.—The longitudinal rays, in some specimens, are regular and single, in others they are alternately large and small. The lines of growth are very distinct; the shell is thickened by the lateral muscular impressions; the chevron-formed lines in the ligamental area are about four or five. Small specimens from the coralline crag are sufficiently abundant; those from the red crag occur but rarely.

The two figures referred to I consider to be of the same species; but must leave it for further observation to confirm or refute this opinion.

Arca lactanea, Nob. Pl. xiii. fig. 3.

Shell transverse, decussated, anterior side subangulated, beaks approximate, margin entire. Longitudinal diameter, $\frac{3}{8}$; transverse diameter $\frac{5}{8}$ of an inch.

Red crag, Walton, Essex. Coralline crag, Sutton.

This shell differs from *Arca lactea* in being rather more inequilateral, and in being less angular at its anterior margin: the ligamental area is also so much smaller as, in my opinion, to render its separation from that species necessary. The rays are numerous, and decussated by lines of growth; the teeth in the centre of the hinge-line are vertical, but they become gradually inclined towards its extremities. Specimens from the red crag are often much distorted, like the recent shell, so as to be nearly equilateral and scarcely transverse; but the anterior side always preserves a more rounded form than any of the recent specimens that I have seen. There is a small *sinus* in the basal margin. One antique specimen from the red crag measures an inch in its transverse diameter.

Arca raridentata, Nob. Pl. xiii. fig. 4.

Shell subrhomboidal, tumid, externally rayed, teeth much inclined, margin crenulated. Longitudinal diam. $\frac{1}{10}$; transverse diam. $\frac{3}{20}$ of an inch.

Coralline crag, Sutton.

An abundant species. The teeth in this shell are similar in their arrangement to those of *Cucullæa*, but the valves of that genus are described as being closed and free, while in this there is a *sinus* in the margin, and probably it was an attached shell. The teeth are generally three in number on the shorter side, making an angle of 45° with the hinge-line, and three on the longer side, which are nearly horizontal.—The shell is longitudinally striated, and the *striæ* are cut by elevated lines of growth, giving to the exterior a cancellated appearance; the anterior side is rounded; posterior subangulated; muscular impressions indistinct.

Pectunculus pilosus, Auct. Pl. xiii. fig. 7.

Pectunculus glycimeris, Turton. *Pectunculus polyodonta*, Goldf.
" *pulvinatus*, Brongn. " *variabilis*, Min. Con.
" *obovatus*, Lam.

Red crag, *passim*. Coralline crag, Ramsholt and Sudbourne.

An abundant shell in both the red and coralline deposits, and very variable. I have included the whole of the above synonymes, considering them all to refer to the same species. Among a hundred specimens of the crag fossil, some may be found thick, tumid, and longitudinal; others thin, lenticular, and transverse. After a careful examination of an immense series of specimens, I cannot detect any character sufficiently permanent to warrant a separation of the crag from the recent shell. The number of teeth in the younger shells is upwards of twenty, but the central ones become obliterated. The large obtuse angle on one side is rather more strongly marked in the recent shell, but it is visible in most of my specimens. At most it would only constitute a variety. One antiquated specimen in my possession has only three teeth remaining. The chevron-formed lines in the ligamental area (from five to eight), are deeply impressed in some specimens but obsolete in others, even when the valves are found united, so that the above condition could not be a result of subsequent attrition. My largest specimen measures a little more than three inches across, but a magnificent specimen of the recent shell from the Mediterranean, in the possession of Mr. G. B. Sowerby, has attained the diameter of nearly six inches.

A number of specimens of the same species from Sicily, obligingly sent me by Madame Power, exhibit the same variations in character, although a slight obliquity prevails among the generality of them.

I have given a figure of this shell, for the purpose of showing the difference between it and another crag *Pectunculus*, which I regard as new. In many instances however the former shell contrasts much less strongly with the new species, than in the one which I have had drawn for illustration.

Pectunculus subobliquus, Nob. Pl. xiii. fig. 6.

Shell lenticular, sub-inequilateral, slightly oblique, longitudinally striated with transverse lines of growth, margin crenulated. Diameter 2 inches.

Red crag, Walton-Naze.

I consider this a distinct species, from its obliquity and general uniformity of character: when compared with the last species, it is thinner, the hinge never so broad, and the *umbo* is but slightly prominent; externally it is covered with numerous fine *striæ*; the chevron-formed lines are eight or nine in number, and deeply impressed.

At the above locality these shells are very abundant and in good preservation, the valves being frequently united, an uncommon occurrence with the bivalves found in any other locality of the red crag. If this really be a distinct species (and I have given my reason for thinking it such), the duration of its existence was very short, the creation and extinction appearing to be limited to the period of the red crag.

Pectunculus pygmæus. Pl. xiii. fig. 5.

Pectunculus pygmæus, Philippi, 'Moll. Sicil.' page 63, tab. 5, fig. 5.

Shell rhomboidal, inequilateral, oblique, ventricose, ligamental area with a central deltoidal pit; hinge teeth about ten, margin crenulated. Longitudinal diameter $\frac{1}{4}$; transverse diameter $\frac{1}{5}$ of an inch.

Coralline crag, Sutton.

Abundant. This shell appears to agree with the description and figure above quoted. The four teeth on the anterior side are nearly horizontal, while four of those on the other are vertical and very prominent, the two most lateral, angulated. The arrangement of the teeth in the figure of the Sicilian shell is indistinct, and the description merely says,—"dentes circa decem." The shell is striated and sulcated, by which the exterior has a cancellated appearance; the longitudinal *striæ* are visible internally; a few on the anterior angle are more conspicuous; the margin is crenulated all round. This species, and a few others, have been put into a separate genus (*Limopsis*), in consequence of the central deltoidal pit in which the ligament appears to have been placed, instead of being spread over the whole area; between the hinge line and *umbo* a small opening is visible in those specimens where the valves are found united. *Pectunculus nanus* of Deshayes differs in the arrangement of the teeth, and in having the longitudinal *striæ* more strongly marked: his figure does not show the deltoidal pit.

(To be continued.)

ART. VI.—*Description of a Specimen of the* Orthagoriscus mola *(Sun Fish), caught off the Irish Coast in June,* 1839, *and preserved in the Museum of the Natural History Society of Dublin.* By O'BRYEN BELLINGHAM, M.D., &c., &c.[1]

THE *Orthagoriscus mola*, (the *Tetraodon mola* and *Diodon mola* of former writers), the short sun-fish of English authors, so very rarely occurs upon our coasts, and the descriptions and figures, more particularly the latter, given by naturalists, vary so much, that I have drawn up a short account of the very fine specimen before me, and at the dissection of which I assisted.

It was caught off the coast of Acklow, by some fishermen belonging to Kingstown, on the 29th of last June, by means of a noose thrown over it, while apparently asleep upon the surface of the water; from them it was purchased by T. Dixon, Esq., a member of the council of this Society, and by him has been most liberally presented to our Museum. It has been, I need hardly observe, admirably preserved by Mr. Glennon.

The fishermen stated, that when seized, it made several ineffectual attempts to escape, and splashed the water in a furious manner, throwing up great quantities with its tail.

The majority of the specimens of this fish which have been taken off the British coast, have been observed floating upon the surface of the water. Mr. Couch says that in calm weather it often mounts to the surface, and lies, probably asleep, with its head partly above water, presenting the broad surface of one side to view, and floating with the tide. It seems, says Dr. Neill (speaking of one brought to him), "to be a stupid dull fish ; it made little or no attempt to escape, but allowed one of the sailors to put his hands under it, and lift it fairly into the boat :" Mr. Couch, however, says that he has known it to make powerful efforts to escape, just as the fisher men describe this one to have done.

The *Orthagoriscus mola* belongs to the order *Gymnodontes*, the characters of which are "no true teeth, but the jaws are covered with a lamellated substance resembling ivory, either entire or divided in the middle by a suture: opercle small; branchiostegous rays five upon each side, deeply concealed," and to the genus *Orthagoriscus*, in which "the jaws are undivided, body very much compressed, short, truncated behind,

[1] Read at a meeting of the Society, Friday, April 3, 1840. Communicated by the author to the 'Mag. Nat. Hist.'

rough, but without spines, not capable of inflation; dorsal and anal fins high and pointed, uniting with the caudal."

Two species of *Orthagoriscus* are described to inhabit the British seas, *Or. mola* and *Or. oblonga.* Lacepède, however, asserts, that there is no specific distinction between them, and Gmelin makes the *Or. oblonga*, a mere variety of the *Orthagoriscus mola.*

The colour of the specimen, the subject of the present communication, when recent, was dusky grey upon the back, speckled with silvery white upon the sides; belly silvery.

The cuticle was very rough, with innumerable minute prickles; these were largest and most prominent about the lips and upon the head.

It weighed one hundred and ninety-six pounds.

It measured 4 feet in length; its greatest depth was 2 feet 4 inches, and its greatest thickness near the head, 14 inches.

The dorsal and abdominal edges were sharp and keel-shaped; a transverse depressed line appeared to mark the distinction between the body and the caudal portion.

The dorsal and anal fins are opposite each other; each is 19½ inches long, and 9 inches broad, at the base.

The pectoral fin on right side is larger than on left, measuring 7 inches in longest diameter, which is diagonally from above downwards and backwards.

From the centre of the pupil to the angle of the mouth, 6½ inches, and from centre of pupil to anterior edge of branchial aperture, 6½ inches.

The vent is 4 inches anterior to anal fin, and an inch behind the vent is the external opening of the urinary organs.

Number of Fin-Rays.

Pectoral, 13; Ventral, 16; Dorsal, 18.

These measurements were all made and noted down before the animal had been skinned. The dissection was performed by Mr. Glennon, superintended and assisted by T. Dixon, Esq., Dr. Gilgeons, and myself. The mouth is small, consists of two lips, and two solid, undivided, bony jaws, the upper having a groove to receive the under; their edges are jagged and sharp. The gape of the mouth is 2 inches, its width 3¾ inches.

Above the mouth is a moveable proboscis which projects an inch beyond the upper lip; its point is protected by a bony scale convex anteriorly, and resembling the common limpet-shell.

There is another bony scale 6 inches below the inferior *maxilla*, which is 2 inches long and 1 inch broad.

The aperture of the eye is somewhat oval, $2\frac{1}{2}$ by 2 inches in the long diameter from before backwards. The eyes have a deep blue colour, the iris silvery white, $\frac{1}{8}$ of an inch wide; when the eyeball was pressed inwards, a white membrane, resembling the *membrana nictitans*, appeared, and could be made completely to cover the eye.

The branchial aperture is situated an inch anteriorly to the pectoral fin, its length is 3 inches, its breadth 2 inches, the long diameter being from above downwards; it is closed by a loose fleshy valve.

The dorsal and anal fins are very long and pointed, their anterior margin straight and thick, their posterior thin and rounded, and continuous with the caudal portion.

The pectoral fins are much smaller, have a rounded figure, narrower at the base, they are attached horizontally; the pectoral of right side is a little larger than that of left.

The caudal portion of the animal consists of nine semicircular lobes, the convexity directed backwards, the central lobe the largest, measuring $4\frac{1}{2}$ inches from above downwards; between each pair of these lobes is a deposit of bony matter, firmly connected to the integuments; these are eight in number; they are nearly an inch in length; their concave edge looking backwards.

The palate and tongue were provided with a few sharp curved teeth.

The alimentary canal measured 11 feet 6 inches in length; its coats were strong and thick; there was no apparent distinction between stomach and intestine: the alimentary canal, near the *œsophagus* being little wider than at its termination. As there was no mark by which to distinguish the junction of the stomach with the intestine, it could not be said whether the biliary duct opened into the stomach or duodenum.

The gall bladder was very large, as also its duct. The liver was of a considerable size. The urinary bladder was large and thick; its interior surface plicate.

The stomach and intestines contained some fragments of corallines, with much viscid mucus.

The alimentary canal contained, in addition, several species of *Entozoa*, which are new, I believe, to the British Fauna. In the stomach and intestines I found great numbers of the *Bothriocephalus microcephalus*, and of the *Distoma nigroflavum*, besides a species of *Distoma* which does not appear to have been described. In the liver and *peritonæum* were several of the *Anthocephalus elongatus*, and upon the gills many of the *Distoma contortum*. Attached to the gills, were also a few specimens of a very remarkable crustaceous parasite, which have been preserved by Mr. Dixon, and will, probably, prove to be new to the Fauna of this country.

This short account of the internal anatomy of this rare fish, may, perhaps, appear to be very superficial: my apology must be that the dissection was conducted under very unfavourable circumstances; it was made in the open air, and upon the ground in the commissioners' yard at Kingstown, in a position where we were liable to continual interruptions. Moreover, my time being limited, the examination was necessarily more hurried than I could have wished; and I had no means of preserving any of the parts for a more extended and careful examination.

The figures given in different works intended to represent this species, are, many of them, very unlike the present specimen. In Bloch's figure, which appears to have been copied into Shaw's work on Zoology; in Donovan's and in Pennant's figures, and in that given in the best and most recent work upon British fishes, by Yarrell, the caudal portion of the animal is represented to have all the appearance of a fin, the rays being distinct.

The figure which Dr. Jacob has given in the last number of the 'Dublin Philosophical Journal,' of a specimen which he dissected, represents this part pretty accurately, but in other particulars it differs materially from the specimen before me. In his figure, the shape of the pectoral fin is materially different; the number of fin-rays was also not the same, and the animal was shorter in proportion to its depth. No writer appears to have noticed the bony deposits upon the proboscis and below the inferior *maxilla*, nor those between the lobes of the caudal portion of the animal.

In the 'Historia Piscium Francisci Willughbei,' published at Oxford in 1686, is contained a description of the *Orthagoriscus mola*, which is accurate in many particulars; the figure is also very good for the time. The specimen from which he took his description was upwards of 2 feet in length. "The body short and deep, posteriorly provided with a circular *pinna*, or tail; the skin not scaly, but hard, thick, and rough. Colour on back blackish, belly silvery; mouth very small for a fish of its size. *Maxillæ* hard, sharp and rough; eyes small; where the body passed into the caudal portion, the skin was marked with a transverse band, or depression."

"Liver large, thick, and whitish; gall-bladder very large; its duct opened into the stomach, not far from its superior orifice; spleen small and dark-coloured; urinary bladder very large, the *urethra* not terminating in the *anus*, but opening externally by an orifice peculiar to itself, a little behind the *anus*."

I have only seen two other specimens of the *Orthagoriscus mola;* one is preserved in the Museum of the Royal Dublin Society; it is much smaller, and its depth is greater in proportion to its length than in the example before us: the other, Mr. Robert Ball has very kindly given me permission to exhibit to the Society this evening; it is evidently a younger fish; it agrees, however, in every particular except in size, with the specimen before us.

In a communication which I have very recently had with Mr. Ball, he says "between the years 1818 and 1825, I saw five of these animals off the coast of Youghal; three were dead, and two of them are in my possession, the specimen I send is the larger." "Considerable difference exists in the thickness and other proportions of the caudal portion of the animal; in the young it is very thin; the bony tubercles are also probably much dependent in number and size upon age. A specimen which I saw in Professor Harrison's Museum, only a few inches long, was much more like the figure in Yarrell's work. There seems to be a regular gradation from his, through mine, up to the specimen contained in the Museum of the Natural History Society."

In conclusion then, it would appear that the specimens of *Orthagoriscus mola* which have been figured and described by British writers, were young fish, whereas the subject of my present communication is evidently a nearly full-grown animal; and this will sufficiently explain the reason why their descriptions and figures do not tally in every respect with the specimen upon the table. It does not appear to me to be at all necessary (as a member of our Society seems to think), that we should look out for some new genus to place it in. Its characters agree in every particular with those of the genus *Orthagoriscus*, as laid down in one of the latest and best works upon British Animals, 'Jenyns' Manual;' and that it is the *Orthagoriscus mola*, I have the additional authorities of Mr. William Thompson, Mr. Robert Ball, and Mr. Wilde, all of whom have carefully examined it, and whose opinion upon such a point, no person in Dublin is competent to call in question. But if additional evidence be wanting, I may mention, that the four species of *Entozoa* which occurred in it, are identical with those found in the *Orthagoriscus mola* by Cuvier, Rudolphi, and Bremser; and three of those species have never been found to inhabit any other fish.

Subjoined is a short description of these *Entozoa*, specimens of which I have the pleasure of presenting to the Natural History Society.

Distoma contortum.

On the gills of the sun-fish were a great number of this species.

They are cylindrical; the longest 10 or 11 lines in length; colour white anteriorly, yellowish-red posteriorly: they almost all assumed a curved form (as Rudolphi has described, and from which he has given the name).

The anterior pore is small and circular, the ventral larger, elliptical, or circular; in some seated upon a peduncle nearly the length of the neck. The neck measured nearly 3 lines in largest specimen, it is convex upon dorsal, concave on ventral surface; it, as well as the head, is armed, particularly upon the convex surface, with numerous minute spines; these are not continued posteriorly beyond the ventral pore. The body is cylindrical, its greatest diameter being behind the ventral pore, and diminishing gradually towards the caudal extremity. The ova are very small, and exceedingly numerous; they have a yellow colour, and appear to be the cause of that colour in the posterior part of the body.

This species has only been found upon the gills of the *Orthagoriscus mola.*

Distoma nigroflavum.

The intestines of the sun-fish contained several *Distoma* which have most of the characters of *Distoma nigroflavum*, but I could not see the *aculei* which Rudolphi describes upon the head and neck.

The longest specimen was an inch and a half, others about three quarters of an inch; their colour in some parts black, in others yellowish; the head white.

The ventral pore is pedunculated; the peduncle in a few being as long as the neck, from which circumstance they bear a close resemblance to the *Distoma furcatum*, and might even pass for this species, as Rudolphi has remarked. The body of the animal is cylindrical and slender, nearly of the same diameter throughout; the skin is flaccid.

This species Rudolphi has only found in the alimentary canal of the *Orthagoriscus mola.*

Bothriocephalus microcephalus.

The intestines of the sun-fish contained an immense number of this species; the longest was upwards of 3 feet in length: colour white: the widest part of the body was a little more than 3 lines. Several of them lived in water for twenty-four hours.

The head is small, triangular, or sagittate, terminating anteriorly in a little papillary eminence. The *bothrii*, or depressions upon the head, are two in number, of an oval shape, wider posteriorly; there is no neck; the anterior articulations are funnel-shaped; they gradually become longer, then shorter, and in some individuals the most posterior articulations are merely transverse *rugæ*. The articulation which terminates the body is smaller than the others.

Rudolphi describes this species as being found only in the *Orthagoriscus mola.*

Anthocephalus elongatus.

This species was discovered by Cuvier, in the liver of the *Orthagoriscus mola:* he created a new genus for it, which he called *Floriceps;* but as it agrees in every respect with the genus *Anthocephalus* established by Rudolphi, it is now placed there.

The whole surface of the liver was marked with the cysts of this species, the longest which I was able to extract was 7 inches, and it was not complete.

The body of the animal is soft, flattened in parts, in others nearly cylindrical: it terminates posteriorly in a caudal vesicle; the head is provided with two ear-shaped depressions, and four retractile armed *tentaculæ*.

Rudolphi has found this species in the liver and mesentery of the *Orthagoriscus mola,* in the mesentery of the *Centronotus glaucus,* and also in the *Sciæna aquila.*

Art. VII.—*A few Notes on British Ferns.* By Edward Newman, Esq., F.L.S., &c.

Sir,

The publication of my 'History of British Ferns' has been the means of bringing me a very valuable and somewhat voluminous correspondence, touching botany generally, and ferns more particularly. A great portion of the information which I have thus obtained, valuable as it now is, would have been still more so had I possessed it previously to the appearance of the monograph; for a detached Appendix is nearly sure to be lost, and if I refrain altogether from publishing, in the chimerical hope of a second edition, the loss seems equally certain; I therefore gladly avail myself of your kind permission, to insert in the 'Magazine of Natural History' those memoranda which appear more particularly interesting.

I am, Sir,
Your's &c.,
EDWARD NEWMAN.

45, *Wellclose Square*,
April 13, 1840.

Note 1. *Pteris aquilina* of Authors.

I am indebted to Mr. Bladon, of Pont-y-pool, for the following note.

"In many of the open mountainous parts of Wales, where it grows abundantly, the Brakes is cut down in the summer, and after being well dried, is burned by the cottagers in large heaps, for the sake of the alkali contained in the ashes: when sufficiently burned, enough salt-water is sprinkled on the ashes to make them adhere together, when they are rolled into round balls, about two inches, or two-and-a-half in diameter. These balls are thoroughly dried, and carried about the neighbournood where they are made, for sale in the markets; and they are also frequently kept by shopkeepers, to supply their customers. The price of these balls varies in different seasons, from 3*d*. to 8*d*. per dozen. They are very much prized by some housewives, for their utility in the wash-house, in economizing the use of soap. When about to be used they are put into the fire, and when heated to a red heat, are taken out and thrown into a tub of water; the water, in the course of an hour or so, becomes a strong ley, and is then fit for use."

Note 2. *Lastræa rigida*. Presl.

I am indebted to Mr. J. Tatham, Jun., of Settle, in Yorkshire, for an abundant supply of *Lastræa rigida*, the fronds in a dried, the roots in a growing state. The frond appears to be early deciduous, no trace of it remaining on the roots which I have received. It grows abundantly in the fissures of limestone rocks, at an elevation 1550 feet above the sea, 1050 above the town of Settle: in company with it are found *Asplenium viride* and *Polystichum Lonchitis*, the latter sparingly. Mr. Tatham observes, "the representation of *Pol. Lonchitis* at page 44 (of the 'British Ferns') is excellent; the masses of *thecæ* with us are generally confined to about a third of the frond, but I have some that are half covered." I shall, perhaps, be pardoned, for citing this laudatory passage; my object is to show that Mr. Tatham considers the Irish plant figured at p. 44, as identical with the Settle plant.

Note 3. *Asplenium Trichomanes* of Authors.

This fern is, generally speaking, constant in its form, and rather remarkable for its uniformity of appearance. I have, however, received a beautiful and very marked variety from Mr. Samuel Gibson, of Hebden Bridge, near Halifax, in Yorkshire: the *pinnæ*, instead of being nearly entire, as is usually the case, are deeply pinnatifid, as represented in the accompanying figure, and the *pinnulæ* or lobes are irregularly den-

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Variety of *Asplenium Trichomanes.*

tate. The specimens sent by Mr. Gibson are perfectly without fructification, but I do not know whether this is to be considered a character of the variety, or incidental only to the fronds I have received. The lower figure is a *fac-simile* representation of one frond, as regards form and size, the upper figure represents a portion of a frond, in which the divisions are still more irregular.

This beautiful variety appears to have been known to our earliest botanists, two previous figures of it existing in their works; neither of them, however, represents the fronds quite so deeply divided as in the present instance. One figure is in Plukenet's 'Phytographia,' tab. 73, fig. 6, the plant being described in that author's 'Almagestum Botanicum,' p. 9, as "*Adiantum maritimum, segmentis rotundioribus:*" it is stated on the authority of Sherard, to have been found in Jersey. The second figure is in Tournefort's 'Institutiones Rei Herbariæ,' tab. 315, fig. 6. It is also noticed in Dillenius' edition of Ray's 'Synopsis,' and by Smith, in the 'English Flora,' where it constitutes the variety β of *Asplenium Trichomanes*. The variety γ of Smith, to which that author quotes Sir Robert Sibbald's description ("*Tri. aliud, foliis mucronatis profundè incisis*," 'Scotia Illustrata' sec. 2. p. 52) appears to have little resemblance to the variety, or even species in question, if I may venture to judge from the figure (Sibb., tab. iii. fig. 4) to which Smith refers; but as to the correctness of the reference, I am unable to speak, for Sibbald himself has, in no way that I can discover, connected the text and figure.

Mr. Gibson's plant was gathered at Kant Clough, four miles from Burnley, in Lancashire: it was originally discovered there in 1832, and some plants taken up at that date and planted in a garden at Halifax, have been found to retain their remarkable character in cultivation.

CORRESPONDENCE, INTELLIGENCE, &c.

Missionary Settlement,
Achill Island, April 8, 1840.

Sir,

I am indebted to the kindness of some anonymous correspondent, for the following extract from an article entitled 'Notes on Irish Natural History,' which appeared in the 36th number of your Magazine; and as it contains what seems to me very uncandid misrepresentations of the character of the establishment which is entrusted to my superintendence, I trust your sense of justice will induce you to publish this letter, supplying the *facts*, which the writer of the article to which I have alluded has thought fit to suppress.

The objectionable passage to which my attention has been directed, is as follows:—

"The natives of Achill are charged with being thieves and murderers; and if I were to place full reliance on all I heard at the Settlement, they would appear to be so. Mr. Long, however, with every thing constantly exposed (walls and hedges being here unknown), and living amongst a population from whom he has no power to defend himself, has never lost even a potato. I allude not to this politically, but bearing in mind solely the natural history of the island, and its capability of improvement, I pronounce, without hesitation, that if goodness of soil, lowness of rent, cheapness of labour, and safety of property be recommendations, then that no spot I have ever seen, is more likely to reward the emigrant than the island of Achill. Would that some unpolitical and unsectarian philanthropists, men who took a human view of the human wants and human failings of these poor islanders, would settle among them, and place in their hands the plough and the spade, teach the children to read and write, the boys to make shoes and coats, to fish, and to dig, and rake, and sow, and reap, and build houses; and the girls to knit and spin, and make gowns; use them like brothers and sisters and children, then might this island become a centre of happiness and prosperity."

I am sure, Sir, that candour will constrain you to admit, that any person forming a judgment of our proceedings from the statements contained in the above extract, would conclude that we took no care whatever to promote the temporal interests of our poor neighbours, or the general improvement of the island.

This is a most unfair and ungenerous misrepresentation. We are at considerable cost and much labour, "teaching the children to read and write," "the boys to make shoes and coats, to build houses;" "the girls to knit and make gowns, &c.," and the men to adopt an improved system of agriculture. But, Sir, as I wish to give you proof, and not assertion, let me call your attention to the following extracts from the authorized report of the evidence given before the Committee of the House of Lords, which sat on the Irish Education System, in 1837. The first witness, from whose evidence I make the following brief extract, is J. Dombrain, Esq., Inspector-General of Coast-guard in Ireland.

"Were you ever in the Island of Achill officially?

Several times.

In the course of the last two years?

I was there last summer and the year before.

Did you visit the Protestant settlement there?

I did.

In what state did you find it; was it in a state different from the rest of the island, or like the rest of the island?

Very different from the rest of the island.

Was it in a state inferior or superior?

Very superior.

In what respect?

Generally superior in respect of industry and cultivation; it had superior buildings, and everything that would stamp an appearance of civilization in a very remote district.

As far, therefore, as appeared to you, it would be a great blessing to the island, if the example so set could be followed through that island?

Decidedly."

Such, Sir, was the sworn testimony of Mr. Dombrain.—Not less decisive is the evidence of another witness, Francis Reynolds, Esq., chief officer of coast-guard, who was examined before the same Committee.

"How long have you been in Achill?

I have been in Achill this last time six years.

Had you ever been there before?

I had, for three years.

What interval was there between the two times of residence?

About three years.

You were, therefore, there when Mr. Nangle came in 1834?

I was.

Did you see any improvement produced in the island?

I did; I saw great improvement in the cultivation of the land, and also in having schools for the instruction of the children.

Did the people testify any feeling toward Mr. Nangle at that time?

They always appeared to me at the time to be very fond of Mr. Nangle, and to like him very much.

When were the first indications of a different feeling towards Mr. Nangle?

When the priest gave orders to shout after the protestants whenever they saw them."

This evidence needs no comment. * * * *

The allusion to Mr. Long, in the article which has called forth these remarks, induces me to believe that it was written by an English tourist, who paid a hasty visit to this island last summer. I think his name was Newman. I accompanied that gentleman and the Rev. William Prior Moore, Head Master of the College of Cavan, who was then at the settlement, to Mr. Long's, and I shall now detail the *whole* of the conversation, from which the writer in your Magazine has selected so much as suited his purpose.

As we walked across the mountain, I remarked, that a system of petty thieving prevailed generally in Ireland, to an extent which operated as a check on the expenditure of capital for the improvement of the country; and I attributed the prevalence of this evil to the Romish doctrine, which teaches that a sum, amounting to some shillings, may be stolen from a gentleman without the guilt of mortal sin.

When we arrived at Mr. Long's, the English tourist asked him whether he had suffered much from petty depredation. He replied in the negative. I remarked that that could readily be accounted for, as he lived in the centre of a large tract of land, where there was not a single human habitation within a mile of him. To this Mr. Long assented, at the same time remarking, that he was further secured against depredation, by there being no thoroughfare through the valley in which his house was situated; but, added Mr. Long, notwithstanding these peculiar securities, *I have a man summoned at this moment, for cutting my grass.* Here the conversation ended: Mrs. Long then came into the room, and without knowing the subject of our previous conversation, she began to speak about the coast-guards in the next village, saying that they complained sadly that they could not have a garden, *for such a system of thieving prevailed in the village, that their neighbours would not leave them so much as a cabbage.* I immediately told our English tourist to note that. It seems, however, that my admonition was unheeded; this *unsectarian* and *unpolitical* gentleman, in the plenitude of his liberality, was determine to shut his eyes and close his ears against every *fact* which did not square with his preconceived theory.

Permit me further to remark, that if the writer of the article in your Magazine means to say that I described the Achill islanders as sinners above the rest of their countrymen, he grievously misrepresents my sentiments. I believe that all men, whether they be Papists or nominal Protestants, if unconverted and unregenerated, are alike sinners before God. Without at all infringing on this truth, I admit that some men are naturally more gentle and tractable as regards their intercourse with their fellow-men, than others; and I have always given the Achill people credit for a large share of this animal amiability; indeed, I never experienced anything but kindness from them, until their passions were kindled by the inflammatory harangues of their priests; since then we have lived in an element of violence and outrage, many of us have suffered personal violence, and one has been murdered. I allude to Mr. Reynolds, whose testimony I have quoted in a former part of this letter. We, however, do not repine. I trust we can say of our persecutors, in the forgiving spirit of our blessed Master, "Father, forgive them, for they know not what they do."

The establishment entrusted to my superintendence being dependant for support upon the voluntary contributions of the christian public, I trust that a sense of justice will constrain you to publish this reply to the misrepresentations put forth by the writer in your Magazine, which, if uncontradicted, might tend materially to diminish its claims upon their benevolent assistance.

I am, Sir,

Your faithful Servant,

EDWARD NANGLE.

To the Editor of the 'Mag. Nat. Hist.'

[When our correspondent, Mr. Newman, in his "Notes on Irish Natural History" incidentally alluded to the 'Settlement' at Achill, we knew nothing of the nature of that establishment, and consequently we did not foresee that the wish expressed in reference to "unpolitical philanthropists, &c." involved anything like a reflection upon the institution carried on under Mr. Nangle's superintendence. The reasons assigned by the writer have induced us to give insertion to the present letter, but we cannot find room for any further discussion upon the morality of the people of Achill.

Why an anonymous correspondent should have taken the trouble to send Mr. Nangle an extract from our pages, but have left it to his ingenuity to guess who might be the writer of that extract, is a matter we don't quite understand. We have shortened the communication, but in so doing have omitted nothing of importance to the legitimate object of the writer.—Ed.]

Dublin Natural History Society.—Extract of a letter from the Secretary to the Editor, dated April 20, 1840.—"It has often occurred to me, that the Natural History of Ireland had been wofully neglected, there being no public collection of it in Dublin, nor any place where young men interested in its investigation could meet. The Dublin Society (I presume), regarded this as a matter of less importance than the pursuit of other branches of science; and besides, the expense of becoming a member of that body was very considerable. Under these circumstances, a new Society appeared to me to be the only way of supplying the deficiency, and in March, 1838, with the aid of my friends, the Rev. B. J. Clarke and R. R. Williams, Esq., the Dublin Natural History Society was ushered into the world. You may easily imagine the difficulties we had to contend with, through evil report, and through good report, we have carried on the Society step by step, until it has attained a degree of prosperity we never thought of arriving at, and I am sure, little expected by those who were opposed to us at first. Its objects are, the forwarding of the Natural History of Ireland exclusively, as we consider, that while engaged in this, the Society will be of real importance; while, were we to merge into the more ample field of foreign productions, we should be insignificant. The above object we have endeavoured to bring about, by making a standard collection of the natural productions of the island, and by having evening meetings, where papers relative to it may be discussed. Our collection, by the kindness of the members, is already far advanced: the conchological part, the most so of any: Mr. Warren having presented 140 different species of the marine shells, and the Rev. B. J. Clarke, nearly all the land and fresh-water shells of Ireland. The sun-fish, too, is a great source of attraction, from its curious appearance. The ornithological part of the museum is rapidly increasing, and altogether, our collection is in a very fair way. At any rate, when a stranger comes to Dublin now, he may be shown something of the natural productions of Ireland, a thing formerly out of his power to see, except in private collections. Among the papers that have been read, there has been much interesting matter; a paper on the sun-fish, by my friend Dr. Bellingham, I believe is sent to your Magazine for publication. Our finances (the most important point), are flourishing, for, though our subscription is only £1. we had, at the last quarter, a balance of £80 in the Treasurer's hands. The number of our members is 220. Altogether, from the *nationality* of our objects,—from the papers that are read often being interspersed with popular lectures,—from the good feeling that exists among our members,—I firmly and conscientiously believe, no society in Dublin has so much hold on public opinion; and supported as we are by members of most of the other scientific bodies of the metropolis, sanctioned by the University, whose respected head is one of our members, and who has borne most ample testimony to our efficiency and utility, and encouraged by all who wish well to our national institutions. I cannot but look forward to a continued course of prosperity."—*Henry H. Dombrain.*

Mr. Denny's forthcoming work on the Anoplura.—You are aware, doubtless, of the work I have in hand: to procure the insects for illustrating which, has cost me above 12 years: and though I have been assisted by friends in various parts of the kingdom, with specimens of the lice of quadrupeds and birds, still there are several which I have never been able to procure. When I first began seriously to think of publishing, I considered that about 100 species, (which is 20 more than Mr. Stephens' gives in his Systematic Catalogue), would be as many as I should get. I have now, however, made drawings of 173 species, which occupy 22 plates; besides which, I have several still by me to figure, which have occurred since, so that with these, and some others I am in search of, I shall not be surprised if I reach 200 species: whether, however, I am in the end enabled to publish, is a mystery, as my expenses for each copy will be considerably above the price fixed, £1. 1s. Standish, who coloured Curtis's Entomology, estimates the colouring of the plates alone, that is, the 173 species, to amount to 16s. 6d. per copy; and I have many other items to add.—Do you think that out of your numerous correspondents you could induce any of them to send me the lice I am most in want of, (that is supposing such fell in their way)? Whether they are from British examples of the animals, birds, &c., or from continental ones, is of no moment, as the same species will have the same kinds of parasite all over Europe. The specimens might be sent in a letter very easily. What I want most are the lice of the hare, otter, hedgehog, squirrel, pine marten, mole, shrew, dormouse, fox, seal, rat, mouse, bat, red deer, roebuck,—bustard, black stork, crane, roller, golden oriole, Cornish chough, bee-eater, little bittern, quail, pratincole, ger falcon, merlin, eagle owl, little owl, scops owl, northern diver, great auk, little auk, avocett, stilt plover, little bustard:—most of these are so rare that there is little hopes of obtaining them, still specimens might occur, upon which, if any friend would search, he might be able to do me an essential service, as after so long a time spent upon the subject, I am extremely anxious to render my work (whether published by myself or a bookseller), as perfect as possible. I have received very great assistance from Mr. Selby, Sir Wm. Jardine, Rev. L. Jenyns, Mr. Thompson of Belfast, Mr. Tweedy of Truro,—but still you will see that it is exceedingly difficult to get the materials for such a monograph as I meditate. Many of the above lice might be found, I am sure, by searching dried skins of the animals; as I have detected several upon birds in our museum, which had remained under the feathers for many years. Professor Burmeister of Halle informed me a few days since in a letter, that he is also engaged upon a thick 4to vol. on the *Epizoica.—Henry Denny.—Philosophical Hall, Leeds, Jan. 27th* 1840.

SHORT COMMUNICATIONS.

Descriptions of eight new Cryptocephali, *captured near St. John's Bluff, E. Florida, by Edward Doubleday, Esq.*

Sp. 1. *Cryp. bivius.* Antennæ fuscæ, basi ferrugineæ: caput ferrugineum, maculâ suboculari albidâ: prothorax albidus, lineis 4 longitudinalibus, puncto laterali, marginibusque nigerrimis: elytra dorso ferruginea, lateribus albida, maculis 10 margineque postico nigris: abdomen pedesque pallidè ferruginea. (Corp. long. ·2 unc. lat. ·125 unc.)

Sp, 2. *Cryp. larvatus.* Antennæ fuscæ, basi testaceæ: caput nigrum, fasciâ sinuatâ medio interruptâ verticali, alterâ sub oculis, labroque flavidis: prothorax flavidus, maculis 2 punctiformibus nigris: elytra flavida uterque maculis 3, quarum 2 basalibus, 1 discoidali, nigris signatus; suturâ margineque laterali ferè ad basin quoque nigris: abdomen nigrum, lateribus, maculâ mesosterni utrinquè, podiceque flavidis: pedes flavidi. (Corp. long. ·25 unc. lat. ·15 unc.)

Sp. 3. *Cryp. lautus.* Antennæ fuscæ, basi pallidæ: caput, prothorax, abdomen et pedes ferruginea: elytra nigra, maculis 16 niveis; striata, striis profundè punctis. (Corp. long. ·175 unc. lat. ·1 unc.)

Sp. 4. *Cryp. limbatus.* Antennæ piceæ: caput profundè punctum, nigrum, maculâ anticâ oculorumque marginibus albidis: prothorax profundè punctus, niger, margine laterali albido: elytra rugosè et irregulariter puncta, miniata, maculâ dorsali communi nigrâ: abdomen et pedes nigra. (Corp. long. ·17 unc. lat. ·09 unc.)

Sp. 5. *Cryp. mammifer.* Antennæ nigræ, basi piceæ: caput nigrum, maculâ suboculari, genæ et clypeus albida: prothorax glaberrimus, aterrimus, lineâ submarginali anticâ et laterali, maculis quoque 2 dorsalibus albidis: elytra striata, striis rugosè et profundè punctis, aterrima, maculâ laterali alterâque apicali croceis: cætera nigra, prosterno tuberibus 3 mammiferibus albidis ornato. (Corp. long. ·2 unc. lat. ·125 unc.)

Sp. 6. *Cryp. geminatus.* Antennæ fuscæ: caput ferrugineum, oculorum marginibus albidis: prothorax ferrugineus, marginibus nigris, lineâque submarginali albidâ: elytra nigra, utriusque lineis 4 longitudinalibus albidis; 1ma dorsalis distincta, 2da obscura, interrupta, 3a 4aque submarginales conjunctæ: abdomen ferrugineum: femora ferruginea; tibiæ et tarsi nigra. (Corp. long. ·15 unc. lat. ·1 unc.)

Sp. 7. *Cryp. lixus.* Antennæ testaceæ, apice fuscescentes: cætera testacea, vittis vix distinctis, prothoracis elytrorumque communibus 4 luteolis: elytra striata, striis profundè punctis. (Corp. long. ·14 unc. lat. ·08 unc.)

Sp. 8. *Cryp. luteolus.* Antennæ luteolæ, apice fuscescentes; oculi nigri: elytrorum maculâ minutâ humerali, alterâ subapicali, strigâque anticâ, discoidali, brevi, nigris; cætera luteola: elytra glaberrima, 8-striata, striis profundè punctis. (Corp. long. ·14 unc. lat. ·08 unc.)]

Edward Newman, April 14, 1840.

Hoopoe taken at Fishguard, Pembroke.—It may be matter of interest to your ornithological correspondents to know that a *Hoopoe* was shot by Mr. James Bowen, at Llwyngwair, near Newport in Pembrokeshire, on the 6th of this month, and is now in the possession of Dr. Rowland, 57, Wimpole Street, London. It came into the drawing-room at Llwyngwair, appeared tame, and was observed to take its food in the peculiar manner mentioned in your Maguzine, vol. ii. p. 596. The last shot in this neighbourhood on record, is that mentioned in Fenton's History of Pembrokeshire, page 17.--*Henry McLauchlan, F.G.S.--Fishguard, March 24th*, 1840.

THE MAGAZINE

OF

NATURAL HISTORY.

JUNE, 1840.

Art. I.—*View of the Fauna of Brazil, anterior to the last Geological Revolution.* By Dr. Lund.

(*Continued from page* 213.)

Second Order, *ACLEIDOTA.*

First Family, *Ruminantia.*

Without doubt, one of the most remarkable phenomena connected with the geographical distribution of the class of *Mammalia*, is to be found in the very unequal apportionment of the genera of ruminants in the warmer regions of the two great continents. While Africa and Asia possess all the several genera belonging to this family, the new world possesses only one of its members, *i. e.* the genus *Cervus*, together with the single sub-group of *Camelus*. The latter being entirely confined to the mountain chain of the western coast, there remains only the single genus *Cervus* for the whole vast extent of the South American plains, with their subordinate ranges of hills. This poverty of genera in the family *Ruminantia*, did not characterize the ancient *fauna* of this region; for besides the genus *Cervus*, I have discovered two others belonging to that period. The fossil remains that I possess of the genus *Cervus*, may be referred to two species, of which one is about the size of *Cervus simplicicornis*, Ill., the other, nearer to *Cerv. campestris*. The genus *Antilope* has left remains of one species, *Ant. Maquinensis*, as large as a buck, with short bow-shaped horns, curved backwards. It was a gregarious animal, like its congener, which is now confined to the old world. For a further description of this animal, and a drawing of its *cranium*, I must refer to my paper on the cave of Maquiné.

Of the genus *Camelus*, I possess the remains of two species; one exceeding a horse in size, the other a little less. To which of the two sub-groups of this genus the fossils belong, that is, whether to the modern inhabitant of the warm regions of the old world, *Camelus*, Ill., or to that now found in the chain of the Andes, *Auchenia*, Ill., my insufficient means of comparison will not allow me to decide.

I conclude this family with a small group that does not exactly coincide with any one of its existing genera. This genus, which, in the lightness of its form, rivals the most agile of the antelope tribe, departs far from that group in the details of its structure, as also from the goat, sheep, ox, and camel. In some isolated points, it approaches the genus *Cervus*, while in others, it differs from it more than do any ruminants from each other. I propose the name of *Leptotherium* for this extinct genus, of which I possess the remains of two species, one less than the roe, the other bigger than the *stag* ["*crown-hart*?"]. If, now, we take a comparative view of the genera and species of this family, in the former and present periods, we must remark, in the first place, the much greater abundance of generic forms that distinguished the ancient creation, inasmuch as their numbers are in the proportion of four to one. Of these four, *Leptotherium* is entirely extinct: with regard to the genus *Camelus*, I have already observed, that the insufficiency of my means of comparison prevents me from deciding whether the fossil species belong to those of the old world, or to the sub-group of this continent. But, if I might be allowed to venture a speculation on this subject, I should declare for the latter opinion, on the strength of a rule which we shall, in the sequel, see confirmed by many examples, namely, that the animal species of the ancient world exceeded in size the corresponding species of the existing races: and we have seen that the fossil species of *Camelus* are considerably inferior to those of the old continent, while they somewhat surpass those of the new world.

In regard to the genus *Antilope*, it is well known, that in our times, the warmer portions of Africa and Asia constitute its dwelling-place. At the same time I must observe, that one of the latest travellers in Chili, M. Gay, believes that he has recognized a species of antelope, in the descriptions given him by the natives, of an animal that inhabits the inaccessible regions of the Andes. Should this supposition prove correct (at which we should not be surprised, when we remember that other animals, more likely to attract man's attention, such as the tapir and the bear, had escaped the observation of naturalists, in those very same mountains, until a very recent period), we are then brought back to this remarkable result, that two genera, which are at present restricted to the highest chain of the South American coast, were in the age of our fossil remains extended over its plains.

The discovery of an animal form, now usually considered as peculiar to the old world, among the purely American forms such as the extinct fauna of this quarter has hitherto produced, being a fact of great importance, I thought it right to allude to this novel information, while I reserve myself from giving any positive judgment, until farther enquiries shall have enabled M. Gay to verify or disprove this report.

But not only the number of genera, but also the total amount of species, was greater in those days, than now. There are now five species (all as before mentioned belonging to the genus *Cervus*) that inhabit this district: while I already reckon seven species of the four fossil genera. The great number of species which the genus *Cervus* now contains within this region, inclines us to suspect that our knowledge of the fossil species is very far from being complete. The circumstance of these animals living solitary, or at most in small herds, together with their rapidity of flight, secures them from the attacks of predatory beasts, much more than animals that either live in large societies, like the *cloven-footed*, or which are bad runners, as the *Tardigrada*; and this accounts for our finding their remains so seldom in the caves of wild beasts, in comparison with those of the other classes. Therefore, as the main result of our enquiry respecting this family, we see that of the four genera of which it was composed in that former period, only one still continues to exist in this same district; two must be sought for in the higher chain of the Andes (or perhaps only in the warmer zones of the Old World), and finally, one has entirely disappeared from the surface of the earth.

Second Family, *Pachydermata*.

This family at present contains only two genera belonging to Brazil; the tapir, with one species, and the peccari (*Dicotyles*) with two. I am in possession of fossil bones, which evidently belong to the first genus; but they are too imperfect to determine their relation to the recent animal.

Among the immense quantity of remains referrible to the latter genus, I can make out at the least four species, very distinct from each other, and equally so from the two recent species, one of the fossil nearly doubling in size either of the recent.

But it seems that a *third* genus of this family formerly inhabited this district. I have found the dorsal *vertebræ* of a large animal, which, both in form and size, agree with those of the elephant. The peculiar shape of these bones renders it impossible to confound them with those of any other large animal belonging to either the extinct *Tardigrada*, or to the existing *Pachydermata*. At the same time, as I am aware that the bones of the *Mastodon* have been discovered in Brazil, and as I have not the means of ascertaining how nearly the *vertebræ* of this animal resemble those of the elephant, I am willing for the present to suppose the fossils I have found may belong to the *Mastodon;* partly because I do not wish to insert any doubtful animal in this catalogue ; and partly because I feel it would be wrong to build so important an hypothesis as the former existence of the elephant in South America, upon any but the surest foundation. I should, perhaps, remark, that the bones I have hitherto obtained of this animal, differ in their external appearance from all the other fossil bones I possess : and rather resemble the partly decomposed bones of the recent period. They are also the only bones that have not been dug out of the diluvial soil of the caves, under my own eyes; but were found lying loose upon it. It will be remembered that I have endeavoured to account for this circumstance in a previous part of my communication ; to which I therefore refer.

We thus see that the family *Pachydermata* was richer formerly than now, both in genera and species, and also that it appeared under more colossal forms.

Third Family, *Feræ*.

At the head of this family naturally stands the genus *Felis*, which, notwithstanding the number of its species, yet constitutes one of the most natural and best defined genera in the whole class of *Mammalia*. The hunting leopard (*Felis jubata*, Linn., *Cynailurus*, Wagl.), which differs from the rest of the *Feles* in many essential characters, has very properly been formed into a separate genus. Its claws are not retractile ; it is gregarious, and of a mild disposition, so much so, that it is frequently tamed, and employed in the chase. But as a remarkable contrast to this, its dental system is upon a more murderous plan than that of the true

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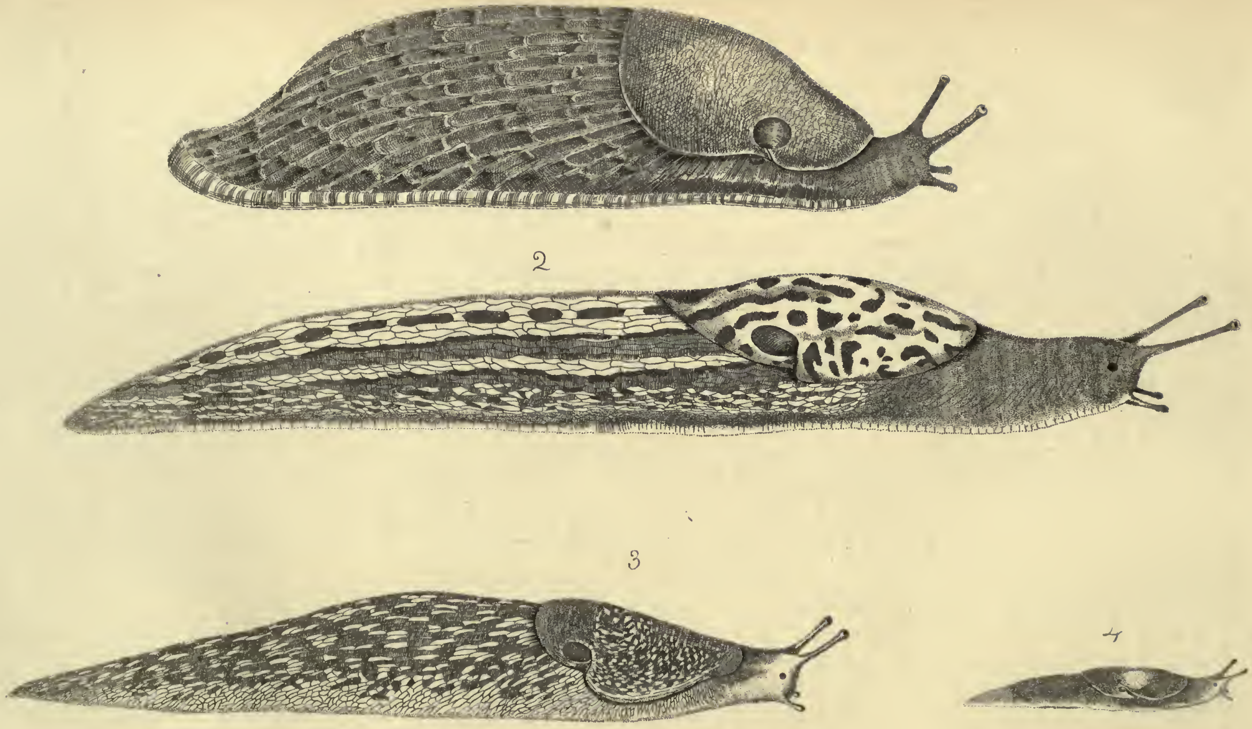

British Limaces.

Feles, not possessing *the flat projection on the large tearing* molar of the upper jaw, which is found in all the other predaceous genera, and the development of which is in inverse proportion to the animal's carnivorous propensities. I have recognized this form of dentition in a small animal of the extinct Fauna of this region, which does not exceed the domestic cat in size; and which, for that reason, I call *Cynailurus minuta.* Besides this, I have discovered the remains of two species of the normal feline form; one as large as the long-tailed tiger-cat (*Felis macroura,* Pr. Max.), the second, larger than the jaguar, (*Felis Onça,* Lin.), and comparable to the largest species of the old world, the tiger and the lion. The genus *Canis,* at present comprises two species in this district; of which one (*Canis Azaræ*) resembles our fox in size, form, and habits; while the other (the Guara, *Canis jubatus*), differs more from its congeners. Its extraordinarily high legs, and halting gait, exhibit a resemblance to the *Hyæna,* which, however, its timid disposition does not bear out. In like manner, I have found traces of two species of this genus, among my fossil remains. One of them (*Canis protalopex*), evidently belongs to the subgroup of *Vulpes,* although the fragments I have as yet obtained are not sufficient to determine its exact relation to the living species. The other (*Canis troglodytes*), belongs to the more carnivorous division of wolves and jackals. The far greater lightness of its extremities distinguishes it well from the *Guara,* and brings it nearer to the European wolf, from which, however, it differs in its longer and weaker neck, a character that again approximates it to the *Guara.* It was equal to either of them in size, and committed fearful havoc among the antediluvian inhabitants of these regions, whose remains are found amassed together in the caves that served for its den. Scarcely less frightful were the ravages committed by another predatory animal, not much bigger than our fox; which, in general character, much resembles the jackal division of the genus *Canis,* but differs from all of that class in the more powerful development of its dental system, and the absence of the hindmost tuberculated molar in the *under* jaw. Its principal food consisted of the various species of Paca, to which latter genus belongs that enormous quantity of bones I have described in my account of the cavern of Cerca Grande. For this remarkable animal (which may be considered as a sub-genus of *Canis,* in like manner as *Cynailurus* may be regarded a sub-genus of *Felis*), I propose the name of *Speothos,* or jackal of the caves; and for the species here described, which is the only one I hitherto know, I offer the specific name *pacivorus*, from the animal that formed its chief prey.

There is a small group of *Feræ*, peculiar to South America, called by the Brazilians *Eirara*, which forms a passage between the *Digitigrada* and *Plantigrada*, and serves to unite the genera *Gulo* and *Mustela*.[1] There are two species known of this sub-genus, which is hitherto without any systematic name; and whose species have therefore been classed by some with *Gulo*, by others with *Mustela*:—*Viverra vittata*, and *Mustela barbata* of Linnæus; which latter, however, in consequence of an original error in the printing, is now generally called *barbara*. Another genus, *Nasua*, at present confined to this continent, existed also here in the former period. I possess the remains of one species, but too imperfect to decide on its relation to existing species.

But the most remarkable animals that frequented this district in ancient times, are the two I next proceed to describe, the bear and the hyæna; both of which are now lost to its Fauna. The fossil Brazilian bear (*Ursus Brasiliensis*), is far inferior to the gigantic species whose remains occur in the European caves; and must even be classed among the smaller kinds of the existing race, though it is distinguished by its massive build. On the contrary, the fossil Brazilian hyæna (*Hyæna neogæa*), will rank with the largest recent species; although even in this respect, it must yield to those monsters of this same genus, whose relics have been found so abundantly in the caves of the old world. If we now compare the genera of this family, with reference to the two epochs under our view, we find that of the five which now inhabit this district, viz. *Felis*, *Canis*, *Lutra*, *Nasua*, and *Eirara* (for I am compelled to use this for want of any proper systematic name), four have been already discovered to belong to the more ancient period. *Lutra* is, therefore, the only one of which I have hitherto found no remains. But this may easily be explained, without concluding that the genus was entirely wanting to the antediluvian Fauna of Brazil. The otter neither frequents caves itself, nor is it likely to become the prey of beasts that do so: it is, therefore, not to be wondered at that we do not at once discover this form, at the very first glance we cast over these fossils. We may, therefore, consider ourselves justified in assuming that the former *system* of animal creation in these parts, contained all the genera of the *Feræ* we find occurring there at the present day. But we have seen that in addition to these, the previous *system* possessed four other forms that do not now

[1] *Galictis* of Prof. T. Bell. See 'Zool. Trans. of Lond.' v. ii. p. 201.—Ed.

exist; of which two (*Cynailurus* and *Speothos*) may be considered sub-genera of *Felis* and *Canis*, while the other two, bear and hyæna, constitute proper genera. Thus the proportion of genera between the former and the present period, is as eight to five. Again, of the five existing genera, three, namely, *Felis*, *Canis*, and *Lutra*, are common to both continents; but *Nasua* and *Eirara* are peculiar to the new world. The presence, therefore, of these two last genera among the fossil Fauna, supplies one more link in support of the proposition I have already laid down, that the animal world was framed upon the same plan in the former epoch, as now.

Of the four fossil genera not now found in America, *Cynailurus* and *Speothos* have each a living representative in the warm regions of the old world. Already in our examination of *Ruminantia*, we are made acquainted with a genus belonging to the extinct creation of this continent, the antelope, which at least seemed to present a similar connection; but at the same time, as there remained some room for doubt, I would not ground so important a result on an indefinite fact. So also with reference to the examples I have just produced, doubts may be entertained as to these geographical relations; partly, because the equivalent forms are only to be considered as sub-genera; and partly, because it may be supposed, that from the great external resemblance which the first (*Cynailurus*), undoubtedly bore to the true genus *Felis*, we may possibly hereafter discover its peculiar dental system in some one or other of the less known existing species of *Felis* on this continent. It is, therefore, better to abstain from any positive decision on the subject at present; the rather, as the history of the next genus I am about to describe, shows the necessity of caution in grounding results on negative evidence. The bear was believed, until within a few years, in spite of Molina's account, to be peculiar to the old world and North America: later discoveries, however, have proved that at least one, if not several species, inhabit the mountains of Peru and Chili. The latter are distinguished from the others of their race by their inferior size: and as we have seen that the fossil Brazilian bear exhibits the same proportions, I consider this latter as nearer related to that of the Andes, than to that, either of the old world or of North America. Thus in this genus, we have a confirmation of the supposition we hazarded respecting the antelope, which affords another example of that very remarkable geographical relation, that the Andes now possess the representatives of several animal forms, which, in ancient times extended over the elevated plains of South America. The constant repetition of this singular fact might almost tempt one to ask, whether it be not possible that the last genus that remains to be described, the hyæna, may not be similarly circumstanced? I must, however, observe, that however well founded such a supposition may appear, when it regards an animal like the antelope, whose timidity and shyness withdraw it from the neighbourhood of man, and whose speed and activity enable it to distance all pursuit, it loses its force when it regards an animal whose habits incline it rather to seek man's proximity; and whose voracity and fearlessness at the same time render it so bad a neighbour, that its existence can scarcely remain unobserved. The existence, therefore, of the hyæna, in that ancient period to which our fossils belong, is a phenomenon that fairly authorizes the conclusion, "that the last extinct, and so remarkably rich a Fauna, which inhabited the elevated plains of tropical South America, among its great abundance of peculiar American forms, contained also some which are now confined to the old world."

If we compare the relative numbers of species of this family in the two periods, it is true that the list of fossil species will not be so numerous as that of the recent; but we ought not, on that account, to conclude that the family of *Feræ* was poorer in species formerly than now, as will appear evident from the following considerations, and which are equally applicable to the next family. All the remains which I have as yet obtained of the extinct Fauna of this continent, have been disinterred from caves, and belong partly to the predatory beasts which lived there, and partly to the animals which served them for food, and which were dragged in by them either entire or piecemeal. We could not, therefore, expect to find in these receptacles of the bones of their prey, the remains of such animals as either their nature or habits withdrew more or less from the attacks of the larger beasts. All the above described animals are those which form the regular food of the larger predaceous classes; whence we find their bones very abundant in these dens. But it is a very different case with the smaller *Feræ*. The sharpness of their senses keeps them away from danger; and the various retreats they all possess, such as the tops of trees, or holes underground, or, as in the otter's case, the water, protect them from almost any pursuit of the larger beasts, which at once accounts for the extreme rarity of their bones in these situations. If we give due weight to these considerations, and at the same time remember the great proportional number of species we have already found at the very first glimpse we have cast over this antediluvian Fauna (there being only three wanting to equal the amount of existing species), I think most surely, that every one will rather be led to the opposite conclusion, that the number of species was greater in that former period than in the present. With respect to the number of genera, we have already proved this position to a certainty, and we may, therefore, conclude that this family presents the same condition as all those previously described, namely, that it was richer both in genera and species, in former geological periods, than now.

(To be continued.)

Art. II.—*On the Action of Light upon the Colour of the* River Sponge.[1] By John Hogg, M.A., F.R.S., L.S., C.P.S., &c., Late Fellow of St Peter's College, Cambridge.

Having for several years past observed many variations in the colour of different specimens of the river sponge (*Spongilla fluviatilis)*, (1[2]) although growing in, or inhabiting the same rivulet, and during the same season of the year, I was at first led to attribute the *difference of age* as the most probable *cause* of those variations. But a circumstance occurred to me in the summer of 1837, which has indisputably afforded the *true cause* of this variation in colour; and, as I consider it to be of great interest, I do not hesitate to publish a notice of that circumstance, and of the experiments instituted by me, as well towards its elucidation as towards its full and direct confirmation.

The circumstance alluded to is this: during August 1837, whilst searching for some good specimens of the *Spongilla fluviatilis*, which is not unfrequent in a rivulet of beautifully clear water at Norton, in the county of Durham, for the purpose of making observations on its supposed *animal* nature, I dragged from the bottom of the stream a flat piece of tile, having a patch of sponge growing upon its upper surface, of a bright green colour, and also another patch of the same species growing to its under surface, but which was of a pale brown, or sand colour, and not in the least tinged with green.

[1] Read before the Royal Society, June 21st. 1838, and communicated to the 'Mag. Nat. Hist.' by the author. This paper is noticed at p. 457, vol. 13, of the 'Philosophical Magazine,' also in the 'Bibliothèque Universelle' de Genève, p. 207, tom. 19, where it is entitled "De l'action de la lumière sur la couleur de l'éponge de rivière." See also vol. 4, p. 72, Proceedings of the Royal Society.

[2] The numerals refer to notes at the end of the paper.

Supposing then that the situations in which the two patches of sponge were respectively placed, (one being exposed to the light, and the other deprived of it), formed the real cause of the difference of colour in them, I took home the tile in a vessel of water, and commenced the following experiments, in order that I might, if possible, prove the correctness or falsity of my conjecture.

On the 18th August, 1837, the day in which I obtained the fragment of the tile, I lost no time in returning home with it, carrying it in a vessel filled with water, very gently, and with great caution, lest the living specimens of the sponge should be in any way shaken, or broken, or otherwise injured. I then placed the tile in a basin of fresh water, but exactly reversed the position of the two patches of sponge, that is to say, I put the underside of the tile with the *pale brown* piece of sponge growing to it uppermost, and exposed it to the light in a window, where I allowed the whole rays of the sun to enter and shine upon it; and the original upperside of the tile, containing the *green* patch of sponge, of course, then occupied the underside, and was almost entirely deprived of light. I changed the water twice or thrice a day, adding it fresh from the pump, and not rain water, or water taken from a pond, or in any way intermixed (as far as I could possibly ascertain), with any fresh vegetable or colouring matter. After a couple of days, I thought the brown piece of sponge began to assume a greenish tint, and the green piece under the stone to lose, though in a less degree, somewhat of its deep colour; these appearances, after a few days more, became distinctly manifest, and by the 29th of August, the brown or sand-coloured patch of sponge had changed to a bright grass-green, and the green patch on the under surface of the tile had diminished in its green hue, and had approached to a light grey. At length, after the expiration of *twenty* days, the upper sponge had much increased in depth of colour, and the lower one had lost a good deal of its green, (2) and had changed to a light brownish green, resembling, indeed, the colour of the patch when originally growing to the underside of the tile, as I had first observed it upon taking it out of the rivulet. Hence, I conceived it to be evident, that the action of *light* directly *caused* the green colour to be secreted in this sponge; and for the sake of corroborating this opinion, I subsequently made these additional experiments.

A short time afterwards, I fished up by means of a wire-gauze net fixed to the end of a rod, a large mass of the river sponge from the same rivulet; this specimen was growing upon a stone ; it was somewhat lobed, but of a pale buff, or yellowish-brown, and not unlike, in colour, the common *officinal sponge* when dry. It was entirely destitute of any green tint, and was taken by me from the bottom of the rivulet under the low and narrow arch of a small bridge, where, in truth, the rays of the sun could, even on a summer's day seldom or never penetrate. Having carried home this mass in a vessel of water, I immediately placed it in a basin of water, adding fresh daily, as I had done in the former experiment, and put the basin in a window facing the west, so that the sun might shine through the glass fully upon it : after a short period, I saw the extremities or tips of the lobes of the sponge begin to be slightly coloured, and in a few days I had the satisfaction of observing the lobes to have received a distinct green, which gradually commenced spreading over the lower portions of the mass that were further apart from the direct and nearer influence of the light. It was consequently apparent, that the *want* of *green* colour in this mass proceeded from the *absence* of *light*, at least, of a *sufficient* quantity of light, in the original spot, underneath the low and dark arch, where the specimen was found growing.(3) Another experiment will also establish the fact, that *light* is requisite to *increase* the *green* colour *already received* by this substance ; namely, I procured a piece of *greenish* or very *light-green* sponge (4) attached to a small stone, which I immersed in a dish of water, and similarly placed it, as in the former experiments, before the *brightest* light in the same window : the colour continued to increase in *intensity*, until, at the end of three weeks, it had become of a beautiful and *dark emerald* green. Thus, then, from these observations, I think no doubt can be entertained, that *light* is absolutely *necessary*, not only to *produce* the *green* colouring matter in this kind of sponge, but likewise to *increase* the *intensity* of the green itself, in various degrees, in proportion as it is *more or less powerfully* (5) acted upon by the addition of the sun's rays.

Therefore the opinion advanced sometime since by Lamouroux, can no longer be considered tenable ; "leur *couleur*," says that eminent zoophytist, "est un *vert* plus ou moins foncé, qui semble *varier suivant la nature du corps auquel elles adhèrent*." ('Hist. des Polyp. Corall. Flex.,' p. 5).

Now, the influence of light upon the colouring matter of this *Spongilla*, proved by the experiments shortly here detailed, will strike every botanist as being precisely analogous to the same cause, which effects the like phenomena, under similar circumstances, on the stems and leaves of *plants ;* and therefore, may have much weight in determining the *true* *nature* of the river sponge. Every one knows that when it is necessary to blanch vegetables, they are carefully concealed from the light; and when plants are grown in a dark place, they are of a delicate white, or yellowish-white, and perfectly *devoid* (6) of their natural green colour; but which, on their being exposed to the light, soon becomes conspicuous, and after a short time the plants obtain their usual green. (7) So also, with most flowers and fruits, their colours are more or less bright and vivid, according as they are exposed to or concealed from the sun; and I have sometimes noticed, that purple grapes (the *Black Hamburgh*, for instance), on being gathered and placed for a few days out of the light, in a dark closet, apparently lose somewhat of their deep and rich colour. I would, therefore, suggest that this remarkable and *peculiar* (8) property of light, which *causes*, as well as *increases*, the colours upon the several parts of *plants*, might be appropriately termed, the *chromatic action of light*. Whilst, on the contrary, from this action upon the colours of *animals*, no such effect is known to occur; for with them a very powerful light, such alone as is communicated by the most luminous rays of the sun, has merely the effect of *darkening*, or, as it is more commonly called, *tanning* the skin, and *not* of *causing* any distinctly bright or *new* colours to appear. (9) Neither does any similarity herein exist (as far as I am aware) with regard to the lower animals, that more nearly approximate to plants, as the *Animal-flowers*, or *Actiniæ*; because, from much acquaintance with some of these singular creatures, both from having observed them in their native localities among the rocks of our sea shores, and from having often kept them alive in glasses, for the sake of attentively examining their structure and habits, I have never noticed any difference between the colours of those which inhabit deep holes *underneath* the rocks (10) about low-water mark, that are thickly covered with the dark sea-weeds (*Fucus serratus*, and *F. vesiculosus* chiefly), and so concealed from the light, and of others which adhere *upon the surface* of the rocks, and are well exposed to the solar rays. Nor have I been able to ascertain that the green colour in the common *green fresh-water Polype* (*Hydra viridis*, Lin.) is ever found to vary, or to become more or less manifest, by *admission* to, or *exclusion* from, the *light*. (11) But it would be superfluous to add any further examples of the same kind.

Some naturalists, even at the present day, are doubtful respecting the real nature of the *Spongilla*, as well as of the other *Spongiadæ* (if animals), or *Spongiaceæ* (if plants), although the majority appear to decide in favour of their *animality;* among those in the minority, I will mention a very able botanist and zoologist, Dr. George Johnston, who, in his essay on *British Zoophytes*, published in the "Magazine of Zoology and Botany," states his view of these productions in the following words:—"I cannot go the length of Ellis, in considering it proved that *sponges* belong to the same class (*Zoophytes*); Ellis, we have seen, knew that *no polypes* were to be found in sponges."...."Now, this fact, in my opinion, determines the point, for if they are *not* the productions of *Polypes*, the zoologist who retains them in his province, must contend that they are *individually animals*, an opinion to which I cannot assent, seeing that they have *no animal* structure, or individual organs, and exhibit *no* one function usually supposed to be characteristic of that kingdom. Like vegetables, they are permanently fixed; like vegetables, they are non-irritable; their movements, like those of vegetables, are extrinsical and involuntary; like cryptogamous vegetables, or *Algæ*, they usually grow and ramify in forms determined by local circumstances; and if they present some peculiarities in the mode of the imbibition of their food, and in their secretions, yet even in these they evince a nearer affinity to plants, than to any animal whatever."(12) This author, therefore, restores the *sponges* "to the *Vegetable* kingdom, to which the earlier naturalists (13) believed they had a *rightful* claim." (14)

I will not here venture to give any express opinion, whether the *Spongilla* be a *vegetable* or an *animal* production, but will merely notify, that after many repeated and careful observations I have hitherto entirely failed in perceiving any individual and decided marks of its animality; though, on the contrary, the facts of the *want* and *intensity* of the *green* colour, (15) as in plants, being caused by the *absence* and *presence* of *light*, may afford *strong evidence* that might be very fairly advanced more in favour of its being nearer allied to the *Algæ* or *Fungi*, and so belonging to the *vegetable* kingdom. (16) Future observations will alone set at rest this dubious question in Natural History.

I am, however, most desirous that those naturalists who reside near the sea-coasts, would ascertain whether *light* does not cause *similar* effects on the *colours* (17) of the different *marine sponges*, which, indeed, seems to me extremely probable from the following statement:—"Je ne peux rien dire de certain," observes M. Lamouroux, "sur la *couleur* (*des éponges*), qui paraît *très fugace* et *très-variée;* d'après les auteurs qui les ont observées *vivantes*, les *nuances* qu'elles présentent seraient *nombreuses* et *brillantes*;" (18) and thus would they afford other and not unimportant proofs, towards a certain knowledge of these remarkable substances, and of the true position which they occupy amongst the manifold works of Nature.

NOTES.

(1) This species is the *Spongilla friabilis* (Lamarck); *Ephydatia fluviatilis* (Lamouroux); *Halichondria fluviatilis* (Fleming); and *Spongia fluviatilis* (Linnæus).

(2) Having, in the summer of 1838, finished another experiment on this head, I found, by placing some living specimens of the *Spongilla* of different shades of green,—one of which was a very deep green,—in a pan of water confined to the *dark*, that the green colour gradually decreased, and became much paler. I must also mention, that the colour *underneath* all those specimens whose upper surfaces were submitted to the full action of the light, continued to the last perfectly *greenless*, by reason of the *deprivation* of light.

(3) I have lately repeated the same experiment with success. On March 20th, 1838, I obtained some small greenless specimens of the *Spongilla* from under the same arch, and after the expiration of sixteen days, they had received a clear, though rather pale green colour. This change took place in the light of a window, at a season when the sun shone but little, and when its illuminating rays had only a small degree of power. I ought to state, that all the masses of river sponge, on which the above observations were made, were free from any moss, or other vegetable, by means of which the *green colouring* matter might, perhaps, be supposed to have been communicated to those specimens.

(4) I will take this opportunity of making known, that I discovered in the summer of 1837 (August 24), among some of the before-mentioned specimens of this *Spongilla*, the little fresh-water *Zoophyte* called *Tubularia repens* by Gmelin, *Naïsa repens*, by Lamouroux, and *Plumatella repens*, by Lamarck. I am not aware that it has ever before been found in England, though Dr. Fleming gives the "Lochmill-loch, Fife," in Scotland, as a locality for it. The elegant crystalline *Polypes* lived in great activity for several days. In March, 1838, I again found a mass of these *Polyparies*: at first, no *Polypes* were visible, and I, of course, fancied that they were dead; after three or four days, when the mass had been placed in water in the window of a room in which there was a fire during half the day only, two or three *Polypes*

appeared, protruding themselves from the extremities of the tubes; but at the end of a fortnight, or rather longer, to my surprise, the *Polypes* became extremely numerous, and exhibited themselves in full life; thus showing that in the winter, or cold weather, these little animals are torpid or inactive, and keep entirely within their tubular dwellings; but, on the return of spring, when the temperatures of the air and water are again sufficiently *warm*, they revive and become lively.

(5) The green in this *Spongilla* being *increased* by the more *powerful rays* of *light*, as shown in the last-mentioned experiment, affords a case extremely analogous to the results of certain experiments made by Professor Daubeny on plants, and which are stated by him in these words:—"from a few experiments I have made on the secretion of *green matter* in the leaves, I should be led to infer, in contradiction to the results of Senebier, that the most luminous rays were most influential; the orange glass, whose chemical influence was as four, whilst its illuminating power was as six, quickly imparting to the primordial leaves of beans which had just appeared above ground, a bright green hue, whereas, under the ammonio-sulphate, whose illuminating power was as two, whilst its chemical influence was as five, they continued of a pale yellow, scarcely indeed of a shade darker than in another case where light was completely excluded. I have made some experiments, with similar results, on the *colours of flowers*, the intensity or depth of which appeared, also, to depend on the brightness of the kind of light that had been allowed admission to them." Again, the Professor observes, —"Upon the whole, then, I am inclined to infer, from the general tenor of the experiments I have hitherto made, that both the exhalation and the absorption of moisture by plants, so far as they depend upon the influence of *light*, are affected in the *greatest* degree by the *most luminous* rays, and that *all the functions* of the *vegetable economy*, which are owing to the *presence* of this *agent*, follow, in that respect, the *same law*."—(See Professor Daubeny's paper, "on the Action of Light upon Plants, &c." p. 158 and p. 163 in the Philosophical Transactions for 1835).

(6) Captain Sir Edward Parry, in his North Polar voyages, used to raise quantities of mustard and cress in his cabin, in small shallow boxes filled with mould, and placed along the stove-pipe, for the purpose of giving a salad to the scorbutic patients, "the mustard and cress," he tells us, "thus raised, were necessarily *colourless*, from the *privation* of *light*; but, as far as we could judge, they possessed the same pungent aromatic taste, as if grown under ordinary circumstances."—(p. 133, Parry's Journal of his *first* Voyage, 1821.)

(7) I need not detail here the experiments by which I became convinced of the truth of the before-mentioned curious results. *One* case, indeed, did not occur to me to try; but which is thus related by Dr. Daubeny:—" Professor De Candolle found that the *leaves* of plants placed in a cellar became *green* on exposure to a strong *light from lamps*."—(See 'Phil. Trans.,' 1835, p. 161.) Mr. N. B. Ward, F.L.S., has also kindly informed me, that from very recent experiments, he has ascertained that *crocuses*, grown in a dark situation, and submitted for about six hours every evening to the full influence of *gas light*, secreted in their *leaves* their usually bright *green colour*, and that the *flower* of one specimen received a pale *blue*. *Another* case I have commenced, but not as yet satisfactorily advanced; *namely*, how far the *greenness* in the leaves and other parts of vegetables at first grown in the light, is *fugacious*, and liable to lose its *depth* of *colour* on being afterwards excluded from the light, and confined to *total darkness*.

(8) There can be no foundation, I think, for asserting that *heat*, *independent* of light, possesses the like *chromatic action:* because plants, when forced by *artificial heat*, but deprived of light, are not invested with their green colour; and fruit, howsoever ripe it may be made by the high temperature of a stove or hothouse, if concealed from the light by many leaves, or in any other way, remains quite pale, and never receives its proper and full tints. The same thing likewise takes place with fruit, from which the light is excluded, when ripened by the *heat* of the *sun*.

(9) With many animals which have variable fur or plumage, *heat* and *cold*, but *not* light, are the principal *causes* of their *variations* in colour. In support of these facts, consult my paper on the changes in the colour of the fur in the *ermine*, at p. 718, vol. 5, 'Magazine of Natural History;' and for Capt. Ross's very conclusive experiment on this subject, refer to Professor Bell's 'British Quadrupeds,' p. 153.

(10) Notwithstanding that the *Actiniæ* are endowed with the power of locomotion, I have watched some individuals continue fixed in the same crevices of rocks for many successive days, indeed, for a sufficient time to render any change in their colours quite conspicuous.

(11) M. Trembley tried to communicate to the brown *Polype* (*Hydra fusca*), a *green* tint. Not having any water insect of that colour, he had recourse to the *green* variety of the rose louse (*Aphis rosæ ?*) Several *Polypes* devoured some of those green lice, and after they had digested them, they received a faint colouring of *green;* (*vide* Mémoires pour

l'Hist. des *Polypes d'eau douce*, p. 128). Now, the same author has described the true cause of the *changes* in the *colours* of the freshwater *Polypes*, thus, "Après avoir nourri pendant quelque tems des *Polypes*, j'eus lieu d'être persuadé, que cette variété de couleur venoit, non seulement du plus ou du moins de nourriture que je donnois à ces animaux, mais aussi de la diversité de la couleur même des alimens qu'ils prennoient." (p. 126). Also, "les grains, qui se trouvent en abondance dans la peau des *Polypes*, sont colorés : c'est de leur couleur que dépend celle des *Polypes* ; et la leur dépend de celle du suc nourricier que ces animaux tirent des alimens qu'ils prennent. Ces grains deviennent, par exemple, rouges ou noirs, quand les *Polypes* ont été nourris par un suc rouge ou noir. Ils ont des nuances, plus ou moins fortes, de ces différentes couleurs, à proportion de la force de la nuance de celle du suc nourricier, et à proportion de sa quantité. Enfin, ils perdent peu-à-peu leur couleur, si on ne l'entretient, en donnant de tems en tems des alimens de même couleur aux *Polypes*." (p. 131). And again, "Il est très-aisé de remarquer, que, lorsqu'un *Polype* a peu mangé, et surtout lorsqu'il a jeûné, il n'y a que quelques couches de grains, savoir, les plus près des parois de l'estomac, qui soient colorées : les autres sont blanches, et forment cette enveloppe transparente, garnie de grains non-colorés." (p. 132.) So, Dr. George Johnston writes of the *Nereis viridis* (Lin.), "when kept in a vessel of sea-water, *deprived of food*, the *green* colour becomes *less intense*." ('Annals Nat. Hist.,' vol. iv. p. 229).

(12) See 'Mag. of Zoology and Botany,' vol. i. p. 229.

(13) Linnæus at first classed the *Spongilla* in his 'Flora Lapponica' among the *Cryptogamia Lithophyta*; then in his 'Flora Suecica' (edit. 1755), and 'Species Plantarum,' (edit. 1753), with the *Cryptogamia Algæ*.

(14) See 'Magazine of Zoology and Botany,' p. 230.

(15) It may, perhaps, be objected, that the *Spongilla*, being either an animal, or else a group of animals, had imbibed for food certain *green vegetable matter*, and in accordance with the facts proved by M. Trembley (see his Memoir, p. 126 to p. 133), that the colours in the *Hydræ*, or freshwater *Polypes*, are caused by the substances they have swallowed and digested,—that this *green colour* was, consequently, derived from *such vegetable matter ;* but in the experiments before described, tt was impossible that this could have been the case ; because I carefully preserved the living *sand-coloured* specimens of the *river sponge* in a basin of pure and fresh *pump-water*, entirely apart from every vegetable substance, until they had received their full *greenness* from the solar light.

(16) From a series of experiments, and some of them the same as those detailed in the preceding pages, which I have again undertaken upon several masses of the *Spongilla* since this paper was communicated to the Royal Society, I am now obliged to confess, that I have no doubt whatever regarding the *vegetability* of this kind of *freshwater sponge.*

(17) Chemistry will, I am sure, present us with some very good tests whereby to decide the *vegetability* or *animality* of the *Spongiæ marinæ;* and amongst other chemical researches to be hereafter instituted, the application of *acids* and *alkalies* to the *colouring* matter, or to the *colours*, when expressed from many species which exhibit the brightest and most brilliant tints, will tend, in a satisfactory manner, towards the solution of that doubtful point.

(18) *Vide* 'Hist. des Polyp. Coral. Flex.,' p. 15. I find that even in the Mediterranean Sea, the *colours* of the different species of *Spongia*, when fresh and living, are exceedingly bright and numerous; among them the following may be enumerated:—pale yellow, yellowish-white, white, red brown, dark brown, red wine, rose-grey, reddish, violet, blue, sulphur, grey, yellow-grey, russet, pink or flesh-coloured, purple, chestnut, reddish-white, saffron-yellow, orange, coral red, &c. See p. 371—380, tom. 5, 'Hist. Nat. des Principales Productions de l'Europe Méridionale,' par A. Risso: Paris, 1826. And it may be interesting to add, that *green* is also a colour observable in some of the *sea* sponges; for example, in the *Spongia urceolus*, of Lamouroux.

Temple, London, May 9, 1840.

ART. III.—*Remarks on the* Lepidoptera *of North America, with occasional descriptions of New Species; being the result of nineteen months' travel in the United States.* By EDWARD DOUBLEDAY, Esq.

(Continued from page 219.*)*

FOR about six miles from its mouth, the St. John's presents on its shores little but snow-white sands, and dreary salt marshes. A few low huts, the houses of the pilots, a lighthouse, a hammock of dead trees,[1] a few starved-looking cows (you wonder what they live on), two or three miserable

[1] Killed by being overflowed by the sea, in the gale of August, 1837.

palmettoes, their fronds all broken with the winds, are all that relieve the monotony of the scene. And is this Florida? exclaims the traveller, who for the first time catches sight of her shores, his head filled with imaginings drawn from her name, and Bartram's strained description. Can this be Florida? Was it here that Ponce de Leon sought the fountain that, like the cauldron of Medea, was to bring back youth? Yes, this *is* East Florida, and the fountain of perpetual youth is to be found in its delicious atmosphere, which revives the invalid, even on these barren sands.

As you enter the river, however, you perceive two small islands in its channel, clothed with live-oaks, and fringed with lofty palmettoes, and beyond them, on the south side of the river, the steep wood-covered slope of St. John's Bluff, above which the river mostly runs between low wooded banks, with here and there other bluffs of small elevation. The highest, perhaps, of these bluffs, is St. John's, yet even that is not more than about eighty feet high. On the east it rises abruptly from the salt marshes, and is clothed with thick underwood, mingled with taller trees ; at the foot, is a small creek fringed on one side with bushes of a species of *Lycium*, and tall *Yuccæ*. The northern side is nearly perpendicular, for the tide washes it away at its base, and the sea-breeze, if, perchance, it blow stronger than common, scatters far and wide the sands of which it is composed. The vegetation on the summit, by slightly holding the earth together there, makes that part the last to fall : but during my stay, I was witness to the disappearance of more than one large tree from the undermining of its roots by the winds and waves. As is common in this part of the St. John's, beds of oyster-shells occur at various elevations ; they appear to belong to the same species as the delicious ones which are as abundant at the mouth of the river in a living, as these in a dead, I can hardly say fossil, state, for they look just like the scattered shells we see in the roads and fields around our own habitations. On the summit of the bluff, especially near to the river, are a great many Indian mounds, the burial-places of a race, in all probability extinct before the white man set foot here. As the face of the bluff crumbles away, it exposes fragments of pottery, and sometimes bones, arrow-heads, and stone battle-axes from these tombs. The pottery is always in small fragments, rarely more than a few inches wide. These appear to be portions of large round vases, perhaps two feet in diameter. They are sometimes plain, sometimes reticulated on the outside with raised lines, and have evidently been subjected to intense heat.

To the south there is a gentle slope to the open pine-barrens which extend in this direction beyond St. Augustine, and westward, the slope too is gradual, until the banks of the river are but little elevated above the water's edge. It was just where the level ground commences on this side of the bluff that we made our home, in a large house originally built for a boarding-house, but then only tenanted by a widow lady and two grandchildren, with the usual accompaniment of a variety of negroes of all ages, who, however, lived in the yard close by. We were fortunate in obtaining an introduction to Mrs. Smith (such was the name of our excellent landlady), for nowhere else in East Florida could we have found so comfortable a home. At this time, south of Mandarin, there was no safety, a foe

> "Curst with each evil that pollutes,
> Mankind where least above the brutes,
> Without e'en savage virtue blest,"

had deluged the homes of the planters with blood: and the tomahawk, the scalping knife and the firebrand had again converted the shores of the St. John's into a wilderness. Mournful truly was it to ascend the St. John's. The cotton-grounds and the cane-fields were overgrown with thorny briars, the cattle wandered wild round the ruins of the dwellings of those who once were their owners; here, a large black spot on the ground, a few scattered fruit-trees, and perhaps some flowers, not indigenous, told that once the white man had dwelt where now nature reasserted her dominion; there, the same story was more clearly told, by the ruins of some saw-mills, and the scattered fragments of the steam-engine rusting on the ground.

St. Augustine, and that town

> "of hectors,
> Thieves, supercargoes, sharpers and directors,"

Jacksonville, were the only other places where we should have found any accommodation, and neither of these could be preferred to St. John's Bluff. We had comfort, and what was more, perfect freedom, for we had scarce any neighbours, there being only two or three little houses near. A small room up stairs was soon fitted up as our laboratory; our boxes, setting-boards, jars, and all such apparatus were ranged against the walls, which were further adorned with an axe, a cutlass, a gun, and its accompaniments of shot-belt, powder-horn and game-bag, and our insect nets. I selected this room, because on one side it commanded a view of the open western slope of the bluff, on the other, of the wooded hill at the back of the house; and moreover had a door into a piazza, where I could sit and skin a bird, or fish, and watch the *Pelopæi* building their clay nests. At the back of the house was a hill, or rather an elevated ridge of land, continued from the southern side of the bluff, and bordering the shore for a considerable distance, separating the low tract next the river from the pine-barrens behind. This was entirely a hammock, that is to say, originally a wood of other trees than pines. In Florida, the term *hammock lands* is applied to lands that are or have been covered with hard-wooded trees, as oaks, sweet-gum, hickory, &c.; the term pine-barren, of course, belongs to the low barren pine-covered tracts, though these are often not so barren as their name or appearance would lead us to believe; swamps are generally distinguished as cypress-swamps, where the chief growth is *Cupressus disticha* and bay-galls; where the growth is chiefly *Lauri* and *Gordoniæ*, the latter are mostly near to the rivers. At the back of the house, then, we had a hammock, composed of live oaks (*Quercus virens*), willow oaks (*Quercus Phellos*), and other species, hickory (*Carya*[1]), chinquapin (*Castanea pumila*), sweet gum (*Liquidambar styraciflua*), beneath which was an undergrowth of *Olea Americana*, *Hopea tinctoria*, *Porcelia pygmæa*, *Ptelea trifoliata*, *Hamamelis Virginica*, and a variety of beautiful *Andromedæ* and *Vaccinia*, intermingled with sweet bays and other shrubs, and a few young or stunted plants of the tall palmetto. *Bignonia capreolata*, *Lonicera sempervirens*, and *Gelseminum nitidum* were common here; *Bignonia radicans* was more rare. Amongst the bushes *Cactus Opuntia* abounded in every sunny spot; its large golden flowers were the favourite resort of *Trichius piger?* and one or two *Lepturæ*. Flowers were not numerous here, though there were a few rather interesting species. I had omitted to mention the luxuriant vines of two or more species which overspread the bushes, one species having flowers as fragrant as the mignionette of our gardens; neither have I said a word of the various species of *Smilax* annoyingly common here.

There were various paths through this hammock, though many nearly grown up, but my cutlass soon opened these, and gave us a freer range. At the back of this hammock was a large pond, abounding in water lilies and other aquatic plants, especially a beautiful *Eriocaulon*. This pond abounded, too, in aquatic *Coleoptera*, especially in February, for after that month they became less numerous: dragon-

[1] Not having seen the fruit, I cannot say which species they were.

flies (*Americè*, "devil's darning-needles") were there in swarms in April and May. Frogs, too, there was no lack of, and noise enough they made in April, May and June. I used to think of a story I heard on ship-board. A fellow was boasting of the fertility of his lands on the Savannah river, they were so rich that they produced three hundred bushels to the acre. A bystander reminded him they were all a swamp; "True," says the boaster, "and they'll produce you three hundred bushels of frogs to the acre, and alligators enough to make a rail fence round them." As to frogs, this pond was quite as productive. Beyond this pond all was one long space of open pine-barren, for I know not how far south. Occasionally swamps are to be met with, and one long line of swamp is known as the Twelve-mile Swamp, a name, the origin of which I do not know; it can have no relation to its length, for it reaches to within six miles of the bluff, and in walking through the pine-barrens thirty miles further south between Picolata and Augustine, I had to cross it, and observed that it continued much further south. There are several large and small ponds in the pine-barrens, one, I should think covering above a hundred acres. The banks of this are full of holes of the large tortoise or *Gopher* (*Testudo Carolina*), looking like rabbit burrows. These pine-barrens are generally covered with dwarf palmettoes, *Chamærops serrulata*, and a low growth of *Quercus pumila*, and some other shrubs, amongst which *Ceratiola ericoides* is very conspicuous from its heath-like appearance; in fact, were it not that it is rarely to be seen without many of its two-seeded berries still adhering to the last year's shoots, it might easily be mistaken for a heath. There are a good many flowers scattered through these pine-barrens, the larger portion being *Compositæ;* but the custom of annually burning the grass destroys these as well as the insects, the seeds of the annuals being, in a great measure, burnt, and the growth of all others of course is injured.

Westward from our dwelling, but separated by a little creek and a narrow strip of marsh, was a large plantation known as the Ship-yard, from a part of it having been once used for that purpose. The soil and general appearance of the surface was the same as at the bluff, the former varying from all sand to a mixture of one part of vegetable mould with one of sand. Large tracts had been cleared here and neglected, and now were overrun with bushes of the Chickasaw plum, dew-berries (*Rubus trivialis*), whose pleasant fruit ripens in April, wild vines, various species of *Smilax* and *Cactus Opuntia*, or an allied species (for certainly here are more than one species of this genus in the southern states; Elliott thought three), and a variety of other plants. These clearings were surrounded by hammocks, similar to the one I have previously described, but with many other trees and shrubs interspersed, as *Magnolia grandiflora*, *Cupressus thyoides*, *Aralia spinosa*, *Euonymus Americanus*, *Itea Virginica*, and a beautiful *Andromeda* (*A. paniculata?* Walt.) The tall palmetto, too, was abundant here, both as tall trees of fifty or sixty feet high, or in its younger state, with its fronds springing immediately from the ground, the only state in which I have ever seen it when growing far from the water.

In front of the house, and extending up to the western side of the summit of the bluff, is a strip of open ground. This is mostly covered with grass, but is here and there, in spring and summer, one entire mass of *Passiflora carnea* and *Galactea Elliottii*, and in one spot there was a large patch of *Clitoria mariana*, intermingled with *Centrosema Virginiana:* scattered fig-trees, orange trees and pomegranates, with here and there a bush of *Melia Azedarach*, give signs of a former cultivation, more extended than it now is, probably when the British held East Florida. *Cactus Opuntia* abounded here, and *Erythrina herbacea* shot up its long spikes of scarlet flowers from the sides of the bushes of *Xanthoxylum tricarpum*.

Situated in about lat. 31° 35′, of course, the climate of the bluff must be mild. In January, we had the thermometer in the day-time, frequently up to between 60° and 70°; the violets, our own sweet violets, were in flower in the gardens, but beyond this there was little sign of vegetation making any progress, until the end of the month. On the first of February, we had the thermometer up to 69° at eight A.M., in the piazza on the north side of the house, open to the cool sea-breeze; the plums were getting into flower, and the young leaves of *Hamamelis virginica* were bigger than a dollar; some species of *Smilax* had young shoots above two feet long; *Pinguicula pumila*, *Viola lanceolata*, and a blue-flowered species, were in full bloom, and *Vaccinium stamineum*, and some of the *Andromedæ* were coming into flower. This was followed by thunder showers, and on the 4th there was frost enough to brown slightly the young shoots of the orange trees. The weather then again became more warm, and on the 15th, the thermometer in the piazza, at 8 A.M., was 69°, and at 2 P.M., 77°. Vegetation now began to progress rapidly. The scarlet grosbeaks and mocking birds were paired; numerous *Lepidoptera* and many *Coleoptera*, especially *Telephori*, came to my lamps, and spring seemed quite set in. But again vegetation experienced a check, for early in the morning of the 17th was a very slight frost, and once in the ensuing fortnight we had the thermometer as low as 41°, at 8 A.M. By the 1st of March the peaches were in full bloom, and then the bushes were hung with festoons of the fragrant golden flowers of *Gelseminum nitidum*. About this time, too, the black snakes and the alligators came forth from their winter quarters, and the river swarmed with brown pelicans (*Pelicanus fuscus*). Although during the next two weeks, the weather was sometimes rather cool, the thermometer once or twice being as low as 45°, and never above 70°, vegetation still progressed rapidly. Insects, as yet, were less numerous than I had anticipated, and the frequent rains limited my hunting excursions. The swamps, hitherto tolerably dry, were now quite impracticable, the ponds overflowed their banks, and the low grounds in the pine-barrens were all under water.

After the vernal equinox, the weather was beautiful during the remainder of our stay in Florida, that is, until the 15th of June, although, during May, vegetation suffered from drought. February and March had been wetter and colder than usual; April, May, and June, though not warmer than common, were much drier, and the cotton and Indian corn suffered considerably. Occasionally we had a cold day, but they were not frequent. Once in April I observed the thermometer as low as 51°, and once in May, 58°. The general temperature of April was from 60° to 70° at 8 A.M., and 75° to 84° at 2 P.M. In general the nights were warm; I observed the thermometer more than once at 74° between midnight and 3 A.M., my common bed-time, when there was no moon. The thermometer, generally, gradually sunk from sunset until sun-rise, then rose again until 2 P.M., unless the sea-breeze blew strong, for then, sometimes, it was cooler at that time than at eight or ten o'clock in the morning. Exposed on the sands to both the sun and the sea-breeze, the thermometer often rose to 124°. In May and June we often had the thermometer at 78° at 8 A.M., and 88° at 2 P.M. Nothing could be more luxurious than this weather, especially for an entomologist. The plan I generally followed was to rise about six, to spend an hour spreading the last night's captures, breakfast at seven, start at once for a walk till two, then spend an hour in ablutions for the sake of health, and rubbing myself with oil to kill the *bêtes-rouges*, which swarm in some parts of the hammocks; take a short siesta after dinner, and then a short walk, until sunset brought on darkness without the intervention of twilight; and then, after an evening meal, light my lamps to attract moths, and sit down to spread insects, or press plants, always till after midnight, sometimes until the rosy-fingered goddess showed herself in the east, when, from a couple to four hours of sleep, made me ready to begin a new day. Reader, if ever it should be thine to reside in a hot climate, never neglect two things; the one to take plenty of exercise, regardless of the heat of noon, but regardful of chill night dews; the other, to take great care of thy skin, for the moment perspiration is stopped fever begins. Ill health I never knew in East Florida, until lameness hindered me for some days from taking long walks; want of exercise then brought on lassitude and debility.

Coleoptera were now getting more numerous. Of the *Cicindelæ,* the earliest was *Cic. unicolor*, Dej., which, though found in almost all the pathways distant from the river, was still a rather rare insect. This was followed by *Cic. tortuosa*, Dej., *Cic. punctulata*, Fab., and *Cic. marginata*, Fab., all mostly frequenting the shores of the river, especially the mud left bare at low water. Further from the shore the beautiful *Cic. abdominalis,* Fab., made its appearance in June, and then, too, *Cic. dorsalis*, Say., (*Cic. signata*, Dej.), was very abundant at the mouth of the river. Under the fallen trees, or any scattered boards (stones there are none) you would find *Pasimachi*, *Galeritæ*, *Chlænii*, *Scarites subterraneus*, *Harpalus bicolor*, *Clivina crenata*, and other allied insects. Beneath the bark of the pine stumps, *Anchomenus decorus*, *Alaus myops*, and sometimes *Al. oculatus*, a good many *Heteromera* and not a few scorpions were to be found; and in the decaying stumps of the water-oak, many species of *Tenebrionidæ* were common, though less numerous than they had been in the winter months. On the brush-wood various *Telephori*, *Digraphæ*, *Dictyopteri*, *Elateridæ*, *Curculionidæ*, and *Chrysomelidæ* were now to be found. On the flowers of the dwarf oaks we took two or three species of *Hydnocera* and some beautiful *Cryptocepali;*[1] on those of the farkleberry (*Vaccinium arboreum*) several small *Lebiæ* (as *Leb. tricolor*, *vittata*, *viridis*, *pulchella*), the pretty *Trichius viridulus*, and the equally pretty but more rare *Trichius lunulatus*. *Cetoniæ* too appeared; two species in great abundance, *Cetonia brunnea*, Dej. (*Scarabæus Indus*, Lin.), and *Cetonia sepulchralis*, the latter in various flowers, the former flying over the pathways. The palmetto flowers were the favourite haunts of *Trichius Delta*. During our whole stay in East Florida I had adopted my old plan of illuminating our

[1] Vide 'Mag. Nat. Hist.' new series, vol. iv. p. 250.

windows, except on evenings when there was much moonlight, for then it was useless, and then I loved to sit in the piazza and look out over the broad calm river, to listen to the hoarse roar of the alligators, the loud drumming of the drum-fish, the croaking of the frogs, the loud plaint of the whip-poor-will, or the music of the mocking-bird. Beautiful, most beautiful were those calm clear nights, when the moon, almost vertical, hung like a silver globe *beneath* the dark blue sky, which, studded with a few bright stars, seemed to lie far beyond her. To me these evenings seemed to tell more clearly than the days that I was far from home; whatever sound we heard, whether it was the plaint of the whip-poor-will, or the alligator's roar, or whether it was the gay songs of the negroes as they paddled by in their canoes: whatever object the moon revealed to us, all was unlike to what we could see and hear in our native land, and over everything visible was poured forth a flood of light so beautiful,—but words cannot describe it, and I am digressing, and must "try back," as the Florida phrase is.

It was during the period I have now been speaking of, that my evening and nocturnal labours were most successful; in the early part of the time in *Lepidoptera*, later, in *Coleoptera*. The latter sometimes came in great numbers, and on those nights the *Lepidoptera* all stayed away. I might try to lure them from the woods, but they would not come when I called them. The *Coleoptera* which chiefly came, were one or two *Lebiæ*, *Omophron Lecontei*, *Panagæus fasciatus*, one or two *Anchomeni*, *Harpalus bicolor*, various *Selenophori*, *Melolonthæ hirticula*, *varians*, *frondicola*, Say, and other species. *Cyclocephala immaculata*, *Serica sericea*, and other of their allies, a few *Elateridæ*, *Enoplium marginatum*, Say, and one or two other species: various *Telephori*, *Euparius lugubris*, *lunatus* and *coronatus*, *Monohammus dentator*, *Cerasphorus garganicus*, *Elaphidion mucronatum* and *putator*, *Lamia Alpha*, *Acanthocinus obsoletus* and other longicorns, amongst which were some interesting new species. Occasionally, too, a host of *Cicindelæ* would pay me a visit, a circumstance I was at a loss to account for, until I found that they were all labouring under hydrophobia, brought on by the passing of a steam-boat, or the rise of the tide. *De Coleopteris satìs dictum est.*

Orthoptera and *Neuroptera*, too, were getting pretty numerous; for the former, however, the autumn is the best season. The curious genus *Mantispa* appeared in April and May, when *Man. brunnea* was not unfrequently to be found on the bushes of *Baccharis* and *Lycium*, near the shore. A few

Mantidæ came to my lamps. *Hymenoptera*, in June, began to abound. Previously, numerous bees had been out, but in June the space in front of the house swarmed with various species of *Scolia*, *Bembex*, *Mutilla*, *&c.*, but of these it will be spoken elsewhere. *Hemiptera* were not numerous, neither did I meet with so many of peculiar forms as I had hoped.

The *Diptera* I would gladly say nothing about, for I love East Florida dearly; I don't like to say anything to her discredit; but alas! I can't do otherwise than tell the truth, the whole truth, and nothing but the truth. There are, in East Florida, musquitoes enough to teach the whole world, as Sam Slick has it, the moral of feeling, and not only this world, but another or two besides, and if the 'squetoes would not do it, the sandflies would, and if the sandflies would not, the *Tabanidæ* would. I once ran a thorn of *Cactus Opuntia* through my boot into my ancle, and broke it off below the skin. I I could not extract the barbed point, and so was lame for a few days, and had to go about in low shoes. I fancied the musquitoes had been busy one day at my foot, above the shoe, so set to work to count the bites. There were marks of nearly five dozen bites on my ancles and instep, all swelled up as big as peas. When collecting in the vicinity of the swamps and ponds, hands, face, and neck came in for an equal share of bites: the very time occupied in pinning an insect enables a dozen to have a fair chance at you. Upon the principle laid down by Lucretius, in his second book concerning the nature of things,[1] it may not be uninteresting to the English entomologist to know that there is a vast variety in the sensations caused by the bites of various species of musquitoes. There is a red fellow like our *Culex rufus*, and another, like our *C. annulatus*, that bite pretty sharp, but don't cause much pain, itching or swelling afterwards; there is another fellow, with pearly white wings, and semi-transparent body and legs, that goes to work upon you so gently that you don't perceive it, but ends in getting such a meal at your expense, that he can hardly fly off with it; you know some twelve hours after where he got it from, and won't forget for a couple of days. Then there is a brown fellow who torments you both now and hereafter; I mean that his bite is very sharp, and that it leaves a good-sized swelling to plague you for three days; and if you rub it you

[1] Suave, mari magno turbantibus æquora ventis,
E terrâ magnum alterius spectare laborem:
Non quia vexari quemquam est jucunda voluptas,
Sed quibus ipse malis careas, quia cernere suave est.

make a sore that will teaze you for a long time. Your true musquitoes, when they alight on you, don't begin to bite at once, but sit down and feel your skin with their *beaks*, and lift up a leg and put it down again, with sundry other manœuvres, and then commence operations: but there are some species who come at you with the beak stretched out, as if charging with a bayonet, and fairly have it in your skin ere they alight; these are *gallynippers*. *Satìs de Culicibus dictum est.*

Next to the musquitoes in rank, as annoyances, come the *Tabanidæ*. First, there are two or three species of *Chrysops*, of which you may have a hundred round your head waiting for a chance to bleed you: then you have the true bred *Tabani*, some as big as the last joint of your thumb; there can be no need of leeches where *they* are. Last come the sand-flies, a most intolerable pest near the shore, and there only. I don't know the genus, they are little fellows, very like our *Ceratopogones*, but possibly are *Simulia*, though much smaller than our British species. Near the river they are in millions, and creep into your hair, whiskers, eye-brows, and if you have silk or cotton gloves, put their heads in between the threads, and bite pretty smart, though not so very bad, if they did not come at you by hundreds at a time. Such are the troubles of an insect-collector in East Florida; troubles, however, he soon gets used to, and ceases to care about.

Much more remains to be said of the *Diptera*, much has been passed over of interest in the other orders; but already I have made too long an introduction to my paper, and it is more than time that I began the real subject of it. Gentle reader, if so be that thou art gentle, as I hope thou art, pardon the tediousness of much that has been written. No doubt thou hast read in Gil Blas what made the Archbishop of Grenada's homilies more tedious than usual; a similar cause has made this introduction what it is. For the future, *Dios te libre, lector, de Prologos largos, y de malos Epitetos.*

On the 15th of June, we bade adieu to our excellent landlady and her household, not forgetting all the negroes (for your southern slaveholder, even, if leaving home for a long time, shakes hands with his domestic slaves), and left Florida a day or two afterwards. Our course northward lay through the beautiful sea islands on the coast of Georgia.

These islands produce the valuable sea-island cotton, and are well cultivated in their interior; their shores are in general beautifully fringed with woods, though here and there is a portion of salt marsh. But the voyage between them is truly delightful, not only for the beauty of the scenery, but the picture of happy labour it presents. It would be hard to find an equal in beauty to the zone of vegetation that surrounds them, or a more enlivening scene of labour. By the side of the lofty cone of the magnolia, displaying her snowy blossoms to the breezes which waft their fragrance afar, shoots up the tall stem of the palmetto, crowned with its vast fan-like fronds; the dark foliage of the cypress and pine are intermixed with the delicate green of the water-oak, or the liquidambar; the live oak spreads forth its crooked arms, all hung with long grey tresses of *Tillandsia*, over the thickets of *Bumeliæ*, *Hopea*, *Lauri*, *Andromedæ*, *Vaccinia*, with snow-white blossoms; *Myrica*, *Olea*, *Glycine*, and countless other shrubs, interwoven with scarlet-flowered honeysuckles, grape-vines with fragrant flowers, and the two *Bignoniæ*, their flexible branches ascending the tallest oaks, and hanging with flowery wreaths their rugged arms. From this mass of foliage and flowers the mocking bird pours forth his ever-varied lay; the scarlet grosbeak, his humbler but melodious notes; and the little ground-doves complain in mournful tones. High above soar the vultures, mere moving dark spots on the deep blue sky, and bright as silver glistens the white head of the bald eagle, as he wheels in wide circles keeping watch over the fish-hawk, seated on the dead branch of a pine. Here a sturgeon leaps, or a porpoise blows, there an alligator floats like a log on the surface of the water, or basks extended in the sun. Swift from some little cove darts forth a light boat, manned by some half-dozen negroes, with faces looking happy as a schoolboy's on a holiday; their oars keep time to their songs in praise of their boat and their master. From behind the bushes burst forth the sound of loud laughter, or gay voices, perhaps, echoing back the chorus of the sable crew of the boat. An opening through the leafy screen at the bottom of the cove whence the boat came, discloses the interior of the island, showing wide-spread cotton fields, the mansions of the planters, the little towns of negro-houses, half buried in trees, and the cheerful gangs of labourers (must I say slaves?) whose merry voices have been heard before. At the boat-landing, groups of little negro-children, perhaps, too, there are many white children mingled with them, are playing on the sands, or angling in the clear wave, and here and there an old superannuated negro is enjoying the sunshine, or aiding the young ones in their sport. The scene is one of beauty, life, and happiness. Such are the shores of Georgia. From Savannah we proceeded to Augusta, thence to the Warm Springs in North Carolina, and so northward and eastward to New York. After spending a few days near the clear blue waters of the Horicon, I proceeded to Boston in order to spend a few days near Dr. Harris. I knew that from an American I could expect nothing but candour and kindness, for it is the grand characteristic of the nation. But much as I had expected, what I met with far exceeded my expectations. It would take up too much space, were I to enter into a detail of all the claims that Dr. Harris has on my esteem and gratitude; but I cannot let an opportunity pass by without testifying to his unostentatious kindness and liberality. Cabinets, books, and manuscripts, were all thrown open to me. His collections, entomological books, &c., were in two rooms in the college buildings. A duplicate key given to me, enabled me to gain access at any hour. But enough. May the day never come when I shall cease with grateful heart to honor and esteem him. Long may he live the first trans-atlantic Entomologist!

(To be continued.)

Art. IV.—*Remarks on the Theory of Spontaneous Generation.* By Mr. J. B. Bladon.

In your January number, Dr. Weissenborn endeavours to argue from the Flora of the snow formation, the possibility of spontaneous generation; but as it often happens with controversies upon cause and effect, the same fact may be wrested to support both sides of the question. He assumes a spontaneous origin to the plants of the snow formations, without assigning any reason whatever for it. We know that animal life can exist when the body is not only exposed to cold far below the freezing point, but some of the cold-blooded *invertebrata* of the arctic regions, may be frozen, thawed, and re-frozen, several times successively, and yet without life becoming extinct. Surely he does not mean to intimate that the cold and barren state of those places is incompatible with animal or vegetable reproduction; but that it still possesses vigour sufficient to produce the vital principle of vegetable organisms. It is well known that there are vegetable organisms peculiar to water in its fluid state, without contact with any earthy substance whatever; and that also there are others peculiar to places where the atmosphere is fully charged with it in the state of vapour, only requiring a resting-place where they may be subject to its influence: then, what reason is there that we should deny organisms to it in its solid state? It is still composed essentially of the same substances as in its other states: I say essentially, because in its fluid, or other states, it is often contaminated by other extrinsic substances, which may form a *pabulum* for some peculiar organism, which can only exist when such contamination takes place. What reason then can be assigned that we should assert immediately a vegetable organism appears upon it when in the solid state, that this owes its existence to spontaneous generation? It is not positively shown that they were without seeds or germs, although it may be admitted that these were not observable; the reproduction of *fungi* is at present in such a state of obscurity, that it would be extremely difficult for any botanist to determine what are the germs in numberless species; were the *fungi* of the glacier *proved to be germless*,—that they did not possess the faculty of reproduction,—then there would be a resting-place or foundation for the doctrine.

By the Doctor's manner of reasoning, when we find an unknown organism in a situation where we expected to find none, or observe a well-known one in an unusual habitat, or at a considerable distance from its other known localities, we are directly to ascribe a spontaneous generation to it; surely this is a most unphilosophical mode of disposing of the question, as direct a cutting of the Gordian knot as the most dogmatical assertion of the contrary doctrine could possibly be. This is flying in the face of his own rule, which is the only safe foundation for us to proceed upon; let us, in all disputed cases, leave ourselves open to conviction, and search out for the truth with unbiassed minds, or at least with a determination to avoid preconceived opinions, and to take every fact into consideration, with its proper deductions, and not to strain it, and by a tortuous mode of reasoning, try to put a false construction upon it. There are very few, if any, facts taken in support of the doctrine of equivocal generation, but what may as equally (and perhaps as justly) be used to support the contrary opinion; for it is not the obvious appearance of the organisms, whether vegetable or animal, that is disputed, but the cause of their appearance. A known organism appears in some unusual place, from its previously known habitats, or an unknown one is observed in some locality never as yet minutely examined, or at least not made known that it has been examined; the advocates of spontaneous generation immediately say, that our doctrine is the right one, is plainly evident, because here an organism has appeared, which cannot be accounted for otherwise. Is assertion to take the place of positive facts, and is not this mere assertion? How can we *prove* that there were no germs of that type of organisms in that place where we now observe the organism in question? We find, when we begin to examine it, that it produces germs itself; then, by what parity of reasoning can we assert, that it has sprung from matter without any previous germ, when we find, in every succeeding instance, a germ is always given for a succeeding organism?

Philosophers are too much in the habit of considering themselves bound to assign a cause for every extraordinary proceeding in nature, as soon as observed, instead of candidly confessing their ignorance upon the subject, as though it would disgrace them to own that there were some subjects on which they possessed no more knowledge than the most unlearned. What ridiculous theories we should have been spared the pain of observing attached to respectable names, had they acted as above; but instead of proceeding upon experiments and facts, and adapting their superstructure to the knowledge so obtained, they boldly assume a position, propound a theory in accordance with it, and then strive to support it by placing the most favourable observations in direct support, and wresting the unfavourable ones from their legitimate bearings and deduction to support it. This has happened with the advocates of many doctrines, and in nearly every branch of natural science.

I can as easily believe that spontaneous generation can produce the largest, or the most complex organism, as that it can produce the smallest, or the most simple. Why should the operation of it be confined in general to the lower grades of organisms, both animal and vegetable, by the advocates of it, but solely because they know that they are unable to bring the slightest reason (much less fact) in support of their doctrine? If they apply it to the larger, the universal experience of all mankind is against them. It is an idea commonly held by many of the most ignorant and illiterate persons, that *Pediculi* are generated spontaneously owing to an ill habit of body; and I have known instances where the appearance of a single specimen of *Ped. vestiamenti* (*P. corporis humani*, Weiss.), would throw a whole family into the greatest inquietude, being regarded as the precursor of severe bodily illness, or trouble in worldly affairs. With regard to the statement of cleanliness preventing the operation of spontaneous generation in producing the various species of *Pediculi,* I can easily understand how cleanliness may remove them after generation; but I must confess my obtuseness in not being able to comprehend how cleanliness prevents spontaneous generation from producing them. I may safely appeal to the daily experience of hundreds, who, for long periods of time, have used no other modes of prevention than merely combing their hair with a common comb, without ever being infested by *Ped. capitis.* What prevents spontaneous generation from taking place in such cases? They are subject to the same influence as others; whatever secretions are produced, there they remain, subject to their power and influence, yet none are generated; but if, by some chance, they should come in sufficient contact with an infested person, so that they might receive some from him, they will then live and multiply as favourably as if the person had been subject to them for years.[1]

In respect to banishing the "uncouth idea of a *Deus ex machinâ*," so much insisted on by Dr. W., I think the arguments adduced by him are not very effective for that purpose; the utmost they do is merely to remove it a step further. If the vital principle is inseparably connected with matter, the varied purposes, actions, and operations of that principle, in all the varied forms of organic beings, must have been foreseen by the Almighty; and powers suitable to those purposes have been implanted either in the different combinations of matter, or have endowed this unknown dynamic power with those peculiar powers, by the direct operation or command of the Deity, which then brings him into as direct operation in creating this complex unknown power, as a means of working out his intentions with respect to the created world and its inhabiting beings, as the common opinion does, which is the more simplified manner of action; his omniscience is called into exercise equally as much, whichever opinion we adopt; he knows the forms best suited for each purpose or locality, and whether he, by his direct word, called the types of all organisms into existence, or created a power which should perform the creations of those forms according as he saw best and most fitting, his omnipotent or almighty power is equally called forth. But to proceed (as I intend to return to this part of the subject further on), if we consider life solely by its effects, it is certainly a dynamic power, exhibiting results accomplished by no other power whatever; but with regard to its operations upon matter alone, as exhibited to our general view, we have nothing to do in our present

[1] I have pursued this view of the subject much farther in the 'Entomological Magazine' for April, 1837, in controverting the opinions of Burmeister upon Phthiriasis, &c., as expressed in his 'Manual of Entomology,' §§ 202, 203.

enquiry: our object is simply to ascertain, as far as we possibly can, whence life originates as we see it in operation in a living organism as to its primary origin. I believe there are but very few persons who object to ascribing it to a Supreme Being, and as one of the works that bears the impress of an Almighty Creator, the most visibly to the material eye of man. Our question is, do living organisms always receive their vital or living principle from a typical predecessor, or do they ever receive it from a universal dynamic power acting upon matter, without typical predecessors?

Spontaneous generation, when divested of all circumlocution and long array of words, amounts simply to one of the two following theorems, if I may be permitted to adopt a mathematical expression.

1st. That life is an inseparable attendant upon matter.

2nd. That a principal attribute of matter is life; for firstly, matter has only to be exhibited under different modifications to the influence of the unknown dynamic power to produce all the different modifications of organisms (living or extinct), the less variation causing the difference of species, and consequently the greater ones, the genera, families, orders, and classes; or secondly, all organisms are created by spontaneous generation "from the reaction of different kinds of matter upon each other, in consequence of the inherent qualities and power with which they were invested through the omnipresence of the Creator." In the foregoing quotation, Dr. W. either has alluded to a different attribute of the Supreme Being from what he intended by the tenor of his essay, or otherwise, he controverts his own arguments respecting God as the acting Creator. I should suppose he meant to allude to the omniscience and omnipotence of God in investing matter with the qualities he speaks of; if he really alludes to the omnipresence of the Creator, at the arrangement of matter previously to the commencement of the life of every organism, this is bringing a direct interference of the Creator in every individual instance of the production of an organism, which is a more "uncouth idea" of the working Creator than the commonly received opinion of the Deity calling every typical form of organic being into existence at the creation, by his own word alone.

If we admit, for argument's sake, the possibility of a universally distributed dynamic power that has the power of giving life and individuality to organisms, we shall be driven to adopt the hypothesis of the Archeus, or *Spiritus mundi* of former ages, to direct and controul its operations. If we view it as a principle or power incident to matter, something analogous to the electro-galvanic fluid, how are we to account for its varied results in the different classes of organisms, so as to give such extremely different powers of vital chemistry (if I may be permitted the use of the expression), that it shall endow one race of organisms with the faculty of secreting a combination of matter sought for in vain in every other race of organic beings, or in any combination of unorganized matter found upon the earth, and past the art of man so to combine matter to produce the same results, even although he knows all its constituents and the proportion in which they are combined,—that two insects shall be hatched, feed, and come to maturity upon the same plant, yet each of them shall so secrete matter, that it shall be utterly in vain to find the least trace of the peculiar product of the one in the other, although they have been subject to the influence of the same matter externally and internally. There surely must be a something beyond the mere effect of matter operated upon by any single power attached to it; it is rather difficult to comprehend the idea of one and the same purely dynamic (consequently unintelligent) power, creating by its operations alone upwards of a million different species of organisms, endowing each with different powers from all the rest, giving each the everduring faculty of reproduction of its kind, so that its descendants may never deviate from its type.

If the powers of life are inherent in matter, and invested in or with it by the Almighty, how are we to account for the extinction of races of organic beings? If we allow that man can carry on a war of extermination against a species of beings, what prevents spontaneous generation in such cases? Are we to allow that one organism possesses the power of utterly eradicating another organism, notwithstanding the inherent powers of matter by which they are both produced; —that man is more mighty than his Maker;—that spontaneous generation can create a power more powerful than itself? What absurdities we are liable to be led into when we overstep the plain boundaries of observed truth! If we are so far distant from the great creative period of the present world, as to expect no new forms of organisms, what prevents it from reproducing the present or extinct forms? Are we to conclude that it lies dormant as to one race of organisms, and in full operation as regards another; or that it is so far exhausted, that it is not able to produce the higher organisms? If so, it must, in time, become utterly exhausted and extinct, unless, by the direct interference of the Creator, it is renovated or renewed; if it is not weakened, it must necessarily produce the very same forms of beings that it already has produced unless the combinations of matter vary, in which case the organisms produced must vary, which would, consequently, be new typical forms; or lastly, we must conclude that there must be combinations of matter, not susceptible of its influence, which would destroy the doctrines of the inherent qualities of matter.

A serious objection against the admission of the doctrine of spontaneous generation, whether as explained by Dr. Weissenborn, or by Burmeister, in his 'Manual of Entomology' (who only allows spontaneous generation to act upon the secretions rejected by the vital principle of an organism), is the individuality of the organisms produced by its operations. What power is there to regulate its actions upon the matter exposed to it? When a dynamic power of any kind acts upon matter, it acts upon the whole mass exposed, and not upon the most minute portions of it alone; therefore this presumed dynamic power, when in action, must animate the whole mass of matter exposed, whether great or small. If the mass in question is more than enough to furnish one germ or organism, what power regulates it, that it shall only create them of the size wanted, and that it shall not create a larger one? Is it capable of dividing the matter, of knowing how much is required for each organism? There must be some superintending power in attendance upon it, to prevent the confusion that must necessarily operate in producing germs of the same species of every conceivable size, from the most minute to the largest, according to the size of the varied collection of matter subjected to its influence; or are we to believe that matter always assembles of itself in one determinate quantity according as it is required by this unknown power? If, by its power, it creates such quantities of *Pediculi*, that a sick person's head is "*covered*" with them "*at once*," with adults, not with germs; if there was such a quantity of disposable matter at hand, it must have been distributed in quantities of various sizes. I should very much like to be informed, how, in such a case, the matter was regulated, so that there should be no overplus in one place nor any deficiency in another, if the currents of the power were all in a state of equal tension or density, so as to give an equal proportion of vital power to every atom of the matter exposed; and if so, what power divides the living matter into the proper quantities for each insect? Might we not rather suppose that it would create one organism of each separate quantity of matter? That would be most in accordance with every observed action of every known dynamic power.

(To be continued.)

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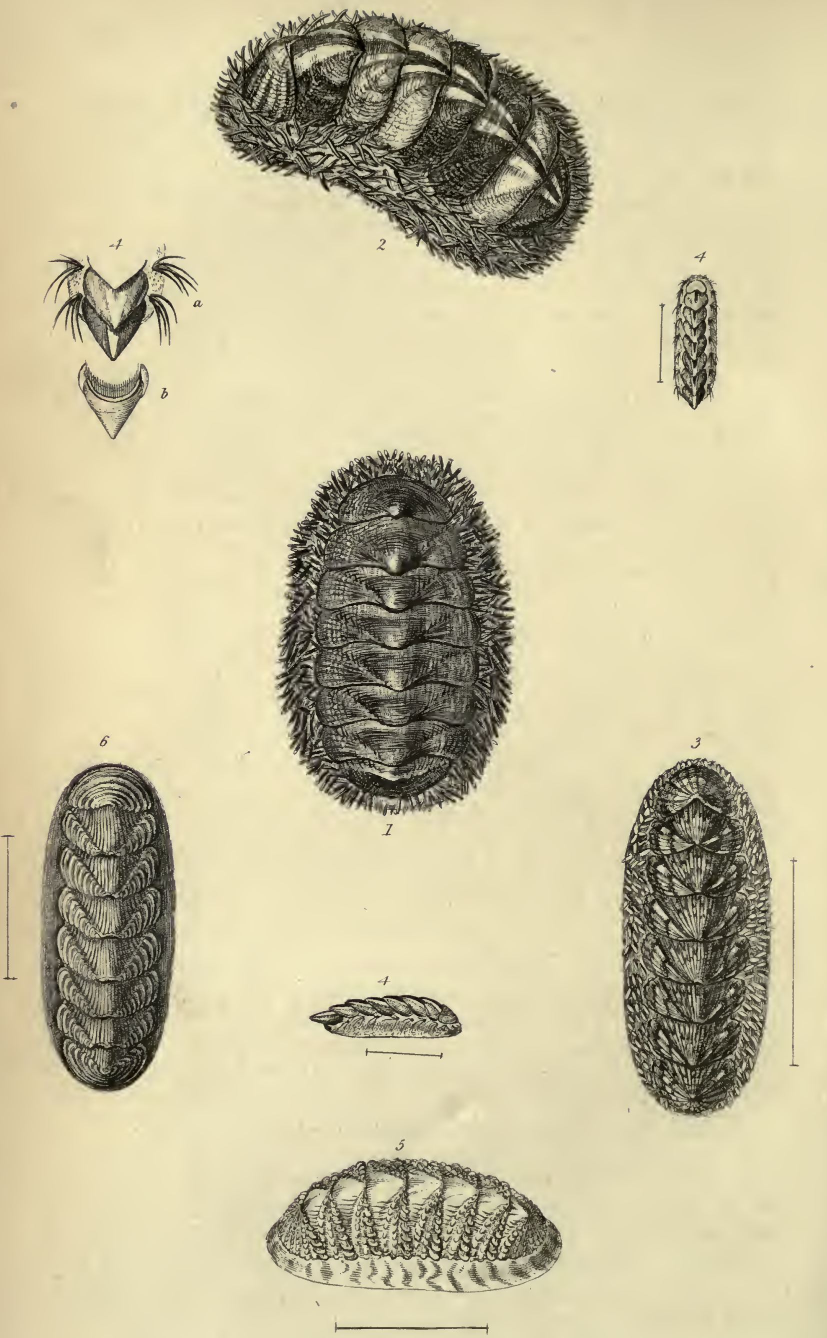

Chitones.

G. B. Sowerby Junr. fecit.

Art. V.—*Descriptions of some new Chitons.* By Mr. G. B. Sowerby, Jun.

Sir,

Having lately examined the principal collections of shells, for the purpose of making a revised list of the species of the genus *Chiton*, I have met with the following which I am unable to refer to described species. As this genus is one of great interest, I am desirous of making the new species known by their publication in the 'Magazine of Natural History,' and I have, therefore, drawn up the accompanying descriptions, with drawings of such of them as appear to me most worthy of illustration.[1]

Editor of the Mag. Nat. Hist.'

Your's &c.,
G. B. SOWERBY, Jun.

Chiton brevispinosus. (Supp. Pl. xvi. fig. 1).

Ch. testâ nigrâ, depressâ, ovali, anticè sub-attenuatâ; dorso rotundato, lævi; lateribus concentricè undulato-granulatis; areis lateralibus vix distinctis; margine spinis brevibus, nigris, numerosis instructo. Long. $1\frac{7}{10}$; lat. $1\frac{1}{10}$ poll.

Ins. Johanna, E. Africa. Mus. Cuming.

Differing from *Ch. spinosus*, in the number and length of the spines, as well as in the sculpture of the valves, this species forms a pretty addition to the thorny-margined section of *Chitons*. It is rather flat, oval, narrowed in front; the valves are rounded and smooth at the beaks, and granulated at the sides, in undulating, concentric ridges; an indistinct beaded rib met by *striæ* at acute angles, separates the lateral from the dorsal areas; the numerous short black spines studding the margin, are tipped with light yellow points, which afford a pretty relief to the general black colour of the shell.

Chiton spiniger. (Supp. Pl. xvi. fig. 2).

Ch. testâ depressâ, ovato-elongatâ omnino granulatâ; valvis reclinentibus, terminalibus rotundatis; margine lato, spinis subarcuatis numerosis instructo. Long. $2\frac{1}{10}$; lat. $1\frac{1}{2}$, poll.

Hab.————? Mus. Stainforth.

Another spiniferous species, rather narrow in proportion to its length; depressed and finely granulated; the valves are rounded at the lateral extremities, with their apices lean-

[1] The illustrations to this paper will be given in the next number of the Supplementary Plates.—Ed.

ing backward, and their lateral areas scarcely distinguished: numerous slightly curved spines, nearly $\frac{1}{2}$ an inch long, cover the margin; general colour like a faded leaf, with dark patches of reddish brown covering the lateral, and part of the central areas of some of the valves, and a dark dorsal band bordered by bright green and white.

Chiton alternatus. (Con. Illus.[1] fig. 141).

Ch. testâ elongatâ, subdepressâ, minutissimè granulatâ; costâ centrali levitèr carinatâ; areis lateralibus costâ laterali separatis; valvâ anticâ radiatim costatâ; posticâ parvâ; margine spinis minutis instructo, nigro alboque alternè fasciato. Long. $\frac{7}{12}$; lat. $\frac{4}{12}$, poll.

Japan. Mus. Cuming.

Narrow, rather flat, minutely granulated; the first valve is radiated with slightly raised ribs, the intermediate have each three faint ribs, one dorsal, the other two lateral; the last is very small, with a nearly terminal apex; the margin is covered with very minute spines, in alternate patches of black and white; general colour dark olive green.

Chiton pectinatus. (Supp. Pl. xvi. fig. 3).

Ch. testâ elongatâ, subcarinatâ; valvis reclinentibus, retusis, radiatim costatis; areis lateralibus exiguis valdè elevatis; margine spinis brevissimis obtusis instructo. Long. $1\frac{1}{2}$; lat. $\frac{4}{12}$, poll.

Cape of Good Hope. Mus. Stainforth.

This species differs from *Ch. limaciformis* in being strongly ribbed, and from *Ch. retusus*, in not being fasciculated. It is elongated and narrow, with strongly relieved lateral areas, as in both those species; it is strongly pectinated; the valves are obtuse, and lean towards the posterior extremity, the apex of the last valve being nearly terminal. The margin is narrow, and covered with short blunt spines. Colour brownish white, variegated with reddish spots.

Chiton Watsoni. (Con. Illus. fig. 81, 130).

Ch. testâ elongatâ, tenuissimè radiatim striatâ; dorso rotundato; valvis ad latera disjunctis; areis lateralibus paulò elevatis, angustis; margine setis numerosis nigris instructo. Long. $2\frac{1}{4}$; lat. $1\frac{1}{8}$, poll.

Cape of Good Hope. Mus. Watson.

Quoy and Gaimard have described this shell in the 'Voyage de l'Astrolabe,' under the the name of *Ch. castaneus*, which name being pre-occupied, I have taken the liberty of

[1] Conchological Illustrations, by G. B. Sowerby, Jun.

exchanging it for that of the gentleman who has brought from the Cape the only specimens existing in British cabinets.

Chiton ciliatus. (Con. Illus. fig. 79).

Ch. testâ depressâ, dorso subrotundato, valvis subreniformibus, ad latera disjunctis; areis lateralibus obliquè granoso-sulcatis; costâ granulosâ utrinque marginatâ; areis centralibus granoso-sulcatis, valvâ primâ radiatim costatâ; margine ciliato. Long. $\frac{7}{8}$; lat. $\frac{1}{2}$, poll.

Hab.———? Mus. G. B. Sowerby, Sen.

The valves are flattish, with the edges arched and slightly beaked, and not united at the lateral extremities; central areas longitudinally grooved: a slightly raised granular rib separates the lateral from the central areas; these are obliquely grooved so as to meet the grooves of the central areas at acute angles on the rib; margin brown, covered with light brownish hairs; the colours are variegated, green, yellow and black.

Chiton petholatus. (Con. Illus. fig. 64, 65).

Ch. testâ lævi, subdepressâ, anticè posticèque sub-attenuatâ; dorso rotundato; valvis subrostratis, primâ radiatim costatâ, ultimâ apice terminali; margine lato ciliato. Long. $1\frac{3}{4}$.; lat $1\frac{3}{8}$, poll.

New Holland. Mus. G. B. Sowerby, Sen.

The margin of this beautiful species is broad, flesh-coloured, with brown bands, and short stiff hairs; the shell is smooth, rather flat, tapering towards each extremity; the valves are slightly beaked, the first radiated, the last with a terminal apex. A very slight rib separates the lateral from the central areas. The ground colour is light green, varied with brown, and a broad central white line. A variety occurs with a brown dorsal band, and angular brown markings, named *Ch. porphyrius* in 'Conchological Illustrations.'

Chitonellus strigatus. (Con. Illus. fig. 63).

Ch. corpore limaciformi, velutino; valvis testaceis parvis, primâ semilunatâ, ultimâ ovali, intermediis hexagonis, fasciis tribus dorsalibus; margine fasciculis minimis prope valvas instructo. Long. $\frac{3}{4}$; lat. $\frac{3}{8}$, poll.

Hab.————? Mus. Stainforth.

The body is cylindrical, minutely studded with hairs, giving it a velvety appearance. As in other species of *Chitonellus*, the valves are small; the first is semilunar, the last oval, the intermediary hexagonal, with three diverging bands of dark brown on each. In the much dried specimen from which the figure and description are taken, the minute bunches of hairs are so rubbed that I did not at first discover them.

Chiton hastatus. (Supp. Pl. xvi. fig. 4).

Ch. testâ exiguâ, granulatâ; valvis reclinentibus, acutis: quinque primis perexiguis; tribus ultimis latioribus; margine crasso, rudi, fasciculis rubris minutis prope valvas instructo. Long. $\frac{3}{8}$; lat. $\frac{1}{8}$, poll.

Hab. ———? Mus. G. B. Sowerby, Sen.

An interesting little shell, forming a link between the genera *Chitonellus* and *Chiton.* The margin is thick and rough, with little bunches of red hairs; the valves lean backwards, and are strongly pointed; the first five considerably narrower than the last three; the last has a pointed terminal apex.

Chiton foveolatus. (Con. Illus. fig. 60).

Ch. testâ ovali depressâ, dorso rotundato, lævi: areis lateralibus et terminalibus radiatim sulcatis, elevatis; valvarum marginibus posticis denticulatis; areis centralibus ad latera foveolatis. Long. $1\frac{2}{10}$; lat. $\frac{7}{10}$, poll.

Hab. ———? Mus. Stainforth.

I am not acquainted with the margin of this species, but from analogy, am inclined to think that it is scaly. The shell is regularly oval, rather flat, smooth, rounded in the centre, the lateral and terminal areas rather elevated, radiately groôved; the posterior edges of the valves denticulated, the central areas marked at the sides with deep ruts in the direction of the lines of growth. General colour bay, inclining to grey at the edges, varied with light patches, and occasional bands of light brown.

Chiton Australis. (Con. Illus. fig. 46).

Ch. testâ ovali, carinatâ; dorso elevato; areis lateralibus distinctis, granoso-costatis; areis centralibus granoso-lineatis; margine squamoso. Long. $1\frac{8}{10}$; lat. $1\frac{1}{8}$, poll.

Australia. Mus. G. B. Sowerby, Sen.

Shell regularly oval, with the back rather elevated, angular, the central areas characterized by granular ridges, and the lateral areas by coarser moniliform ridges. The margin is scaly; the general colour olive green, tinged with rose at the apices of the valves.

Chiton virgulatus. (Con. Illus. fig. 132).

Ch. testâ ovali, subcarinatâ, depressâ; areis centralibus longitudinaliter striatis; areis lateralibus ramosè sulcatis; areis centralibus longitudinaliter striatis; margine squamoso. Long. $1\frac{7}{10}$; lat. 1, poll.

Hab. ———? Mus. G. B. Sowerby, Sen.

Differing from *Ch. Stokesii* in the fineness of the sculpture; it nevertheless resembles that species in general appearance. It is oval, keeled, depressed; the central areas finely striated in a longitudinal direction; the lateral areas and terminal valves radiated with fine ridges, branching like bunches of twigs; the scaly margin is green, banded with mingled black and green, and the general colour is olive-green, variegated near the centre with tortoise-shell brown.

Chiton patulus. (Con. Illus. fig. 134).

Ch. testâ latâ, subcarinatâ; valvis terminalibus et areis lateralibus radiatim rugulosis; areis centralibus longitudinaliter sulcatis; margine squamoso. Long. 3; lat. $2\frac{1}{8}$, poll.

Hab. ———? Mus. Cuming.

The sculpture of the lateral areas is much coarser than in the last species, and the shell is much broader in proportion to the length; it is subcarinated, with the central areas striated; the lateral areas bordered by two flat ridges, with two or three intermediate, coarsely granulated, irregular ridges running into each other at intervals. General colour tortoiseshell brown, with black and white dorsal bands. Margin scaly, dark bottle-green, clouded with black.

Chiton evanidus. (Con. Illus. fig. 139).

Ch. testâ subelongatâ, carinatâ, dorso elevato, lævi; areis centralibus ad latera tenuissimè striatis, areis lateralibus subelevatis, granuloso-striatis, concentricè subundulatis; margine squamoso. Long. $2\frac{1}{8}$; lat. $1\frac{1}{4}$, poll.

New Holland. Mus. Metcalf.

Rather long in proportion to its width, dorsal angle obtuse, valves straight, central areas smooth in the middle, faintly striated at the sides; lateral areas rather elevated, with radiating granular *striæ*. General colour faded leaf, tinged at the apices with rose, margin scaly.

Chiton tenuistriatus. (Con. Illus. fig. 135).

Ch. testâ ovali, carinatâ; dorso elevato; valvis omnino tenuissimè radiatim striatis; margine squamoso. Long. 1; lat. $\frac{6}{10}$, poll.

Hab. ———? Mus. Stainforth.

A neat little shell of a dark olive-green colour, oval, elevated, nearly smooth in appearance, but finely striated; margin lighter green than the rest of the shell; inside blueish-green.

Chiton Cymbiola. (Con. Illus. fig. 45).

Ch. testâ carinatâ, lævi; dorso elevato; areis lateralibus acutissimè elevatis; margine squamoso. Long. $1\frac{6}{10}$; lat. $1\frac{1}{10}$, poll.

Cape of Good Hope.

This species is smooth, strongly keeled, elevated; the lateral areas are much relieved and separated from the central areas by a sharp angle. The margin is scaly. Colour varies from green to purple, brown, and grey zigzag markings on a greenish or cream-coloured ground; the inside is green.

Chiton sculptus. (Supp. Pl. xvi. fig. 5).

Ch. testâ subelongatâ, depressâ, anticè sub-attenuatâ; dorso rotundato; areis centralibus lævibus, ad latera striatis; areis lateralibus, costis exfoliatis radiatim ornatis; margine lævi. Long. $\frac{8}{10}$; lat. $\frac{4}{10}$, poll.

Hab. ——— ? Mus. Stainforth.

A beautiful little shell, remarkable for the exfoliated sculpture of the lateral areas, on each of which there are three radiating series of strongly relieved *laminæ*. The central areas are nearly smooth, except at the sides, where they are slightly striated. The margin nearly smooth, light brown, irregularly striped with grey; and the general colour is creamy white.

Chiton versicolor. (Con. Illus. fig. 75, 122).

Ch. testâ oblongâ vix carinatâ, anticè subattenuatâ: areis centralibus ad latera striatis; areis lateralibus radiatim striatis; striis ad margines ramosis; margine minutissimè squamoso. Long. 1; lat. $\frac{1}{2}$, poll.

Hab. ——— ? Mus. Stainforth. Var. Alb. Dr. Stanger.

This species is oval, oblong, rather narrower in front, the central areas nearly smooth in the middle, and striated at the sides; lateral areas covered with slight radiating ridges, which branch off towards the edges. The colours are sufficiently variable to justify the name given above; several varieties in the collection of Mr. Stainforth being variegated with rose, green and grey; and one communicated by Dr. Stanger, nearly white, with grey spots.

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Chiton lyratus. (Con. Illus. fig. 126).

Ch. testâ ovali, lævi; dorso subangulato; areis lateralibus subelevatis, lævibus; areis centralibus ad latera longitudinaliter lyratis; margine lato, minutè squamoso. Long. $1\frac{1}{2}$; lat. $\frac{7}{12}$, poll.

Hab. ———? Mus. Cuming.

This shell is so thin as to be nearly transparent; it is smooth, rather angulated in the centre; the lateral areas smooth, slightly elevated; the central areas marked with faint longitudinal ribs. The margin is covered with regular minute scales. The general colour is drab, variegated with brownish concentric lines at the sides, and reddish-brown dorsal bands on some of the valves.

Chiton lentiginosus. (Con. Illus. fig. 120).

Ch. testâ ovali, carinatâ, lævi; dorso elevato, areis lateralibus inconspicuis; colori fulvescente brunneâ, maculis cæruleis, rotundatis, ornatâ; margine minutè squamoso. Long. $\frac{5}{8}$; lat $\frac{3}{8}$, poll.

Australia. Mus. Dr. Stanger.

Remarkable for the regular rounded blue freckles scattered over the yellowish-brown ground. The shell is oval, smooth, keeled, with the valves straight, and the lateral scarcely distinguished from the central areas. The margin is covered with minute scales.

Chiton concinnus. (Con. Illus. fig. 117, 118).

Ch. testâ ovali, latâ, subdepressâ, minutissimè granulatâ, subcarinatâ; valvis rectis; areis lateralibus inconspicuis; margine lato, minutè squamoso. Long. $\frac{1}{2}$; lat. $\frac{3}{8}$, poll.

Chonos. Mus. G. B. Sowerby, Sen.

A very neat, small, dark-coloured species; granulated, but so minutely as to appear smooth. It is distinctly keeled, yet depressed. The lateral areas are not very distinctly separated from the central; the margin is broad and scaly; the colour dark olive, in some specimens nearly black; inside green.

Chiton fimbriatus. (Con. Illus. fig. 137).

Ch. testâ ovali, depressâ, latâ; areis centralibus tenuissimè granoso-lineatis; areis lateralibus valvisque terminalibus tenuissimè cancellatis; margine minutissimè granulato. Long. $\frac{7}{10}$; lat. $\frac{1}{2}$, poll.

Peru. Mus. Cuming.

The sculpture of this very pretty little species resembles the finest lace-work. The shell is rather flat, and regularly oval; the central areas finely and granularly striated; on the lateral and terminal areas, the radiating ridges are so regularly intercepted by concentric lines, as to present a cancellated appearance. The margin is apparently smooth, but the lens discovers very minute sandy granulations ; its colour is brown, banded with darker patches. The general colour of the shell is creamy-white, variegated with red.

Chiton atratus. (Con. Illus. fig. 57, 58).

Ch. testâ ovali-oblongâ, lævi, subcarinatâ; valvis subrectis, subrostratis; areis lateralibus inconspicuis; margine lævi. Long. 1; lat. ½, poll.

Falkland Islands. Mus. Miller.

The smooth, narrow, slightly angulated species above described, is of a blackish brown colour; the lateral areas are not distinctly marked, except by rays of lighter colour in some specimens on them, and on the terminal valves. The margin is smooth.

May, 1840.

ART. VI.—*On the Fossil Shells of the Crag.* By S. V. WOOD, Esq., F.G.S.

(*Continued from page* 234.)

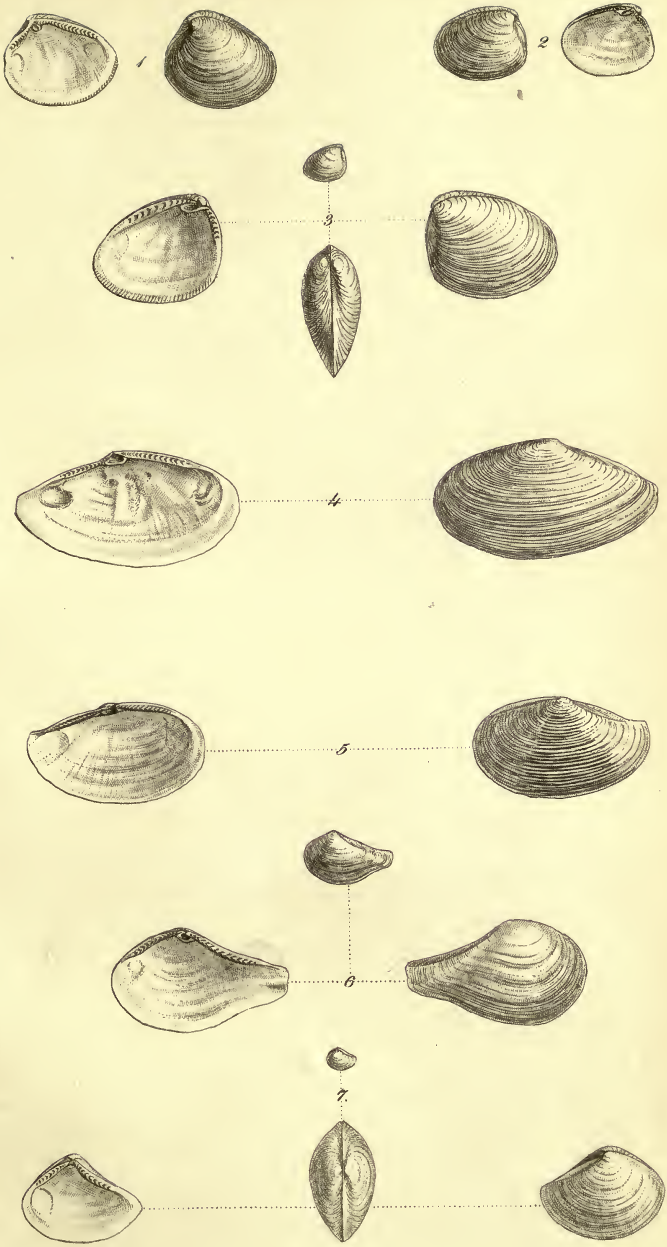
a. Inequilateral, ligamental pit at the angle; edge crenulated.

Nucula nucleus, Fleming. Pl. xiv. fig. 1.

Arca nucleus, Linn. Syst.

Red crag, Sutton. Coralline crag, Sutton.

This shell is not common in the red crag, although very abundant in the coralline deposit. It exhibits a well marked identity with the British variety of Lamarck's *Nucula margaritacea;* and though this species appears to have existed from the commencement of the tertiary period, the specimens from the Paris basin, as well as those from the London clay, are much larger than those of the crag; a condition perhaps depending upon the reduction in temperature which the climate of this part of the globe is supposed to have undergone, since the eocene strata were deposited.



Crag Arcacea

J.D.C. Sowerby fe.

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b. Ligamental pit at the angle, edge entire.

Nucula trigonula, Nob. Pl. xiv. fig. 3.

Shell ovato-deltoidal, tumid, smooth, *umbo* prominent, margin crenulated; lunule embedded, convex in the middle. Longitudinal diameter, $\frac{3}{16}$; transverse diameter, $\frac{1}{4}$ of an inch.

Coralline crag, Sutton.

Posterior side truncate and straight, the centre of the lunule only projecting beyond a line drawn from the *umbo* to the extremity of the ventral margin; anterior side angulated; from six to eight teeth on one side of the ligamental pit, and from twelve to fifteen on the other, with a sub-carinated dorsal projection; no vestige of longitudinal *striæ* externally, except when the outer coating is removed. This shell differs from *Nucula trigona*, 'Min. Con.' tab. 192, fig. 5, in being more tumid,—in the posterior side being more truncated, and in the *umbo* terminating in an acute angle. Fifty specimens of this fossil in my possession preserve such uniformity of character as to warrant its establishment as a distinct species.

Nucula tenera, Nob. Pl. xiv. fig. 2.

Shell subtriangular, smooth, margin entire. Longitudinal diameter, $\frac{3}{8}$; transverse diameter, $\frac{1}{2}$ an inch.

Mammaliferous crag, Southwold. Red crag, Bawdsey.

Bawdsey, a village on the Suffolk coast, is the only red crag locality from which I have obtained this shell: it closely resembles *Nucula nucleus* in shape, but it is destitute of longitudinal *striæ*, and has the margin entire. *Arca tenuis*, Montague, 'Test. Brit.' Suppl. page 56, tab. 29, fig. 1, approaches this fossil in most of its characters, differing only in having the dorsal margin more rounded, and in having but fifteen teeth, the crag species possessing upwards of twenty; six or seven on one side of the ligamental pit and fourteen or fifteen on the other. Many of the specimens from the crag appear as if they had been concentrically striated, but this arises from their altered state, two or three in my possession being quite smooth. This shell appears to connect the two species above referred to, having the contour of the former with the entire margin and delicacy of the latter.

Nucula Cobboldiæ, 'Min. Con.' tab. 180, fig. 2.

Mammaliferous crag, Bramerton. Red crag, Sutton.

The red crag specimens, judging from those which I have seen, are flatter than those from the mammaliferous deposit. The specimen mentioned by Sowerby as having been found at Roydon, Norfolk, is an error, there being no crag within many miles of that place. Roydon was at one time the residence of my late friend, the Rev. G. R. Leathes, a well-known collector of the crag fossils, and in this way the mistake probably had its origin. This shell, when found in the mammaliferous crag, has the valves occasionally in contact, and in a state of preservation which shows that they are regularly and concentrically striated. Specimens are now and then found much thickened internally, leaving two deep, sub-oval, muscular impressions: there is a very small *sinus* in the impression of the mantle on the shorter side, which, on that account, I suppose to be also the posterior side.

Nucula lævigata, 'Min. Con.' tab. 192, figs. 1, 2.

Red crag, Walton Naze. Coralline crag, Sutton.

This shell has been found plentifully at Walton, but I have only a few small specimens from the coralline crag. It is perfectly smooth externally, free from markings of any kind, and a more transverse shell than the preceding; my largest specimen measures nearly an inch and a half across its widest diameter. The figures of these two shells are so correctly given in the 'Mineral Conchology,' that any further representation is unnecessary.

c. Sub-equilateral, ligamental pit sub-central, edge entire.

Nucula oblonga, 'Min. Con.' tab. 180, fig. 1.

Red crag, Bawdsey.

This I presume to be a rare fossil, not having found more than half a dozen specimens, and those all at the above locality: mine are all adult shells, being much thickened internally. The figure above referred to is excellent, but no mention is made of the markings which ornament the exterior, consisting of slightly undulating lines running in an oblique direction, and at an angle of about 30° with an imaginary line drawn through its transverse diameter. A shell from the Arctic Ocean, now in the British Museum, figured and described in the 'Zoological Journal,' vol. iv. p. 359, pl. 9, f. 1, under the name of *Nuc. arctica*, appears identical with this species; it is, however, rather thinner and smaller, a difference which may depend upon climate or other incidental causes. There is a small *sinus* in the anterior margin of the crag shell (at least in my specimens), which I did not find in the recent

Nucula just referred to, yet I cannot help thinking that a few more specimens of each of these would establish the specific identity of the two. The muscular impressions in the crag fossil are rendered indistinct by its internal thickening.

Nucula oblongoides, Nobis, Pl. xiv. fig. 4.

Shell ovato-lanceolate, transverse, sub-inequilateral, posterior side acuminated, anterior rounded, lunula lanceolate, edge entire. Longitudinal diameter $\frac{5}{8}$; transverse diameter, $1\frac{1}{4}$ inch.

Mammaliferous crag, Bramerton. Red crag, Butley (Suffolk.)

This *Nucula* appears to be quite distinct from the preceding, for which it has nevertheless been mistaken. I have never seen the young of the *oblonga*, but the following characters induce me to regard it as distinct from that species. It is more equilateral; the difference between the anterior and posterior portions of the *oblonga* being at least in the proportion of two to one, while in this there is but a trifling difference. The teeth also are more equally divided than in *Nuc. oblonga*. It has a smooth exterior in the place of the diagonal lines which ornament that shell; there is also no *sinus* in the anterior margin, and the pallial scar reaches nearly as far as to a line drawn from the ligamental pit to the centre of the ventral margin; the lines of growth are the only exterior marks that I have been able to detect.

Nucula semistriata, Nobis, Pl. xiv. fig. 5.

Shell transversely ovate, sub-inequilateral, thin, posterior side acuminated and striated transversely, anterior side rounded and smooth, margin entire. Longitudinal diameter $\frac{1}{2}$; transverse diameter 1 inch.

Coralline crag, Sutton.

I have not seen this from any other locality than the one named, but it is by no means rare at that spot: specimens in size about half that of the above may be obtained in considerable numbers. One half of the shell is strongly striated except on the posterior slope, where the *striæ* are nearly obliterated; these *striæ*, or rather ridges, with spaces between them forming small furrows, are only on the posterior side, as if that half of the shell had been buried in the sand, while the other had been worn smooth by exposure. The *lunula* is large, lanceolate, and smooth, with a distinct corselet of a lanceolate form; the muscular impressions are indistinct; and the shell being thin, the transverse furrows are visible on the inside. The deltoid ligamental pit varies much in shape, and cannot be depended upon as a character, some specimens having a central elevation dividing the pit into two parts. *Nucula oblonga* has nearly twice the number of teeth on one side that it has on the other, whereas in this species they are nearly equal in number.

Nucula minuta. Pl. xiv. fig. 6.

Arca minuta, Mont. page 140.
„ *caudata*, Donovan, Brit. Shells, tab. 78.

Red crag, Sutton.

Of this shell I have found but one specimen, which, however, is in good preservation, and is probably identical with the British recent species. It is however rather more transverse, the posterior or acuminated side being a little more produced, thereby removing the *umbo* farther from the centre, but from the examination of only a single specimen I should not venture to regard these distinctions as specific. I found it myself in undisturbed red crag, three feet beneath the superincumbent sand.

Nucula pygmæa. Pl. xiv. fig. 7.

Nucula pygmæa, Goldfuss, Pet. Tab. 125, fig. 17.
„ *tenuis*, Philippi, page 65, tab. 5, fig. 9.
„ *corbuloides*, Smith, Wern. Mem., viii. t. 2, f. 10, 10*.[1]

Shell transversely ovate, gibbous, smooth, thick, sub-equilateral, one side slightly acuminated, the other rounded, umbo prominent, margin entire. Longitudinal diameter $\frac{1}{8}$; transverse diameter $\frac{5}{32}$ of an inch.

Coralline crag, Ramsholt and Sutton.

I have given the above as synonymes, presuming all to refer to the same species, although there are some slight differences which require notice. The crag shell appears to be smaller than any of those quoted, and among fifty specimens that I possess, not one is more than two-thirds the size of the *Nucula* given me by Mr. Smith, and which was obtained by him in the deposit exposed by the cutting for the Greenock railway. In the description by Philippi, the term "*tenuissima*" is used for his shell, a character the present does not merit. I would have adopted Mr. Smith's name, but that I

[1] Figured pl. 2, f. 10. 10*, in a pamphlet entitled "On the last Changes in the relative levels of the Land and Sea in the British Isles," by James Smith, Esq., of Jordan Hill, published in the Memoirs of the Wernerian Nat. Hist. Society, vol. 8.

consider the shell, from the figure and short description Goldfuss has given, to be the same. My shell is strong, tumid, and perfectly smooth; hinge-line broad, forming a large obtuse angle with the umbo: eight to ten strong teeth (some of which are prominent and angulated) on each side of a small ligamental pit; lateral muscular impression large, that of the mantle indistinct. The Scottish shell has the acuminated side larger than the crag one, and the figure in Goldfuss is more oval in shape, but the contour is, I think, insufficient to indicate a specific difference, my own specimens varying, in that respect, among themselves; the younger ones being less acuminated than those which I suppose are adult. The valves from Ramsholt are often found united together by their large prominent teeth, which, in arrangement, correspond with the Scottish shell; the interior of the Sicilian one is not represented.

ART. VII.—*Description of a new Marsupial Mammal, belonging to the genus* Phascogale. By. G. R. WATERHOUSE, Esq., Curator to the Museum of the Zoological Society, &c.

THE little quadruped I am about to describe, belongs to that section of Australian mammals (order *Marsupalia*), which M. Temminck[1] separates from the genus *Dasyurus* of Geoffroy S. Hilaire,[2] under the name *Phascogale;* the type of this genus being the *Didelphis penicillatus* of Shaw,[3] which, through the kindness of Professor Owen (who allowed me to examine the original specimen sent over by White, and now in the Museum of the College of Surgeons), I am enabled to state, is the *Tapoa Tafa*, or *Tapha*, of White.[4] The identification of *Phasc. penicillata* with the *Tapoa Tafa*, is of some little importance, since the *Dasyurus Tafa* of Geoffroy, which appears in most works as a distinct species, is founded upon White's animal.

In Temmincks' 'Monographies' a second species of *Phascogale* is described, that author having placed in this genus the *Dasyurus minimus* of Geoffroy; but as he had not the opportunity of examining the dentition of this animal, he felt doubtful whether it might not prove to be the young of a species, the adult state of which remained to be discovered.

[1] 'Monographies de Mammalogie,' tom. 1. p. 56.
[2] 'Annales du Muséum National d'Histoire Naturelle, tom. 3. p. 353.
[3] 'Gen. Zool.,' vol. 1., part 2, p. 502., tab. 113, fig. 1.
[4] Journal of a Voyage to New South Wales.

In the 'Proceedings of the Zoological Society,' for July, 1837, two other species of this genus are described by myself, under the names *Phasc. flavipes*, and *Phasc. murina;* of both these species I had an opportunity of examining the dentition, and as I found them to be adult animals closely allied to *Didelphis minimus*, it is almost certain that that animal is also adult, since it would appear that it was the smallness of its size, which led M. Temminck to believe it might be young. I may mention, that one of the species described by myself (the *Phasc. murina*) is smaller than *Did. minimus*, and that both the species agree in all essential particulars with the larger typical species, which latter differs from others of the genus hitherto discovered in having long hairs forming a kind of bush on the apical half of the tail.

I am now enabled to add a fifth species to the genus *Phascogale*, and of which I have drawn up the following description. The original is in the museum of Mr. Swainson, who has kindly permitted me to examine and describe it, and whose name I have made use of, to distinguish the species.

SWAINSON'S PHASCOGALE.

PHASCOGALE Swainsonii, Waterhouse.

This species is rather larger than either the *Phas. flavipes* or *Phas. minima:* its fur, instead of being, as in *Phas. flavipes*, of a yellow-grey tint, is of a dark and rich brown hue; the under parts of the body are deep grey, slightly grizzled with white, whereas in *Phas. flavipes*, the under parts are yellow and white: the most important differences, however, consist in the more attenuated and elongated form of the head, especially of the anterior portion; the teeth, nevertheless, form an uninterrupted series, hence each tooth (especially the false molars), has a proportionately greater antero-posterior extent. The distance from the forepart of the front incisors of the upper jaw to the hinder part of the third true molar in *Phasc. Swainsonii*, is 7½ lines, whilst in *Phasc. flavipes*, the same measurement gives 6¾ lines: the teeth are less powerful than in the animal last mentioned, and the incisors of the upper jaw form an uninterrupted series, whilst in *Phasc. flavipes*, there is an interspace on either side between the anterior pair of incisors and the lateral ones. In the elongated and slender form of the muzzle, and more delicate teeth, the present animal evinces an approach to the *Myrmecobius*; its fur is moderately soft, rather long and glossy, of a deep slate-colour next the skin; the hairs are most of them narrowly annulated towards the apex with rich brown, or yellow-brown; the longer hairs are black; on the under parts of the body, the hairs (which, like those of the upper parts, are of a deep slate-grey at the base), are slightly tipped with brownish-white, or ash-colour; the feet are covered with dark brown hairs above (not yellow as in *Phasc. flavipes*); the tail is also dark brown, and on the upper side inclining to black; the hairs on this part are all short and adpressed.

There are other differences between the present species and that with which we are comparing it, which may help to distinguish them, I allude to the colouring of the head: in *Phasc. flavipes*, the upper lip, lower part of the cheeks, chin, and throat, are white, and there is, moreover, a white spot beneath the eye, whereas, in *Phasc. Swainsonii*, no white is visible, indeed the head is almost of an uniform colour with the body, the hairs on the sides and upper parts are black, slightly grizzled with yellowish, and on the chin and throat they are grey, tinted with brownish, especially on the chin.

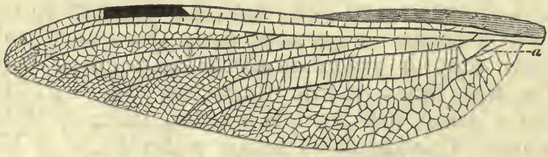
The specimen from which this description is drawn up, is apparently a female, and furnishes the following dimensions; length from nose to root of tail, 5″ 2‴; tail about 3″ 5‴; head, about 1″ 2‴; *tarsus* to end of claws, 10 lines: it is from Van Dieman's Land.

28, *Leicester Square*,
May 23*rd*. 1840.

ART. VIII.—*On the occurrence of a Fossil Dragon-fly in the Lias of Warwickshire.* By H. E. STRICKLAND, Esq., F.G.S., &c.

THE accompanying drawing (fig. 11), represents a very perfect wing of a Libelluline insect, lately found in the lias of Warwickshire. It is the property of Mr. J. Gibbs of Evesham, who has kindly lent it to me for examination and description. It exhibits a very distinct impression on the surface of a slab of blue lias limestone, the wing being of a pale brown colour, and the nervures a darker tint of the same. The opaque spot which exists at the anterior margin of the wing in most of the *Libellulidæ*, is here distinctly marked, being of a much darker brown than any other part of the wing.

This specimen appears to be the left anterior wing of the insect. On comparing it with recent species of *Libellulidæ*, it exhibits a close resemblance to them in the general arrangement of the nervures. It is well known that the insects of this family present certain generic peculiarities in the nervures at the base of the wings. In this respect the specimen before us exhibits characters most nearly allied to the genus *Æshna*, Fab., but approximating also to the structure of *Libellula*. These distinctions being more easily shown by drawings than description, the reader is referred to the figures, where the originals have all been carefully copied of



Fossil wing of *Æshna liassina*. Strickland.

the natural size. Fig. 11 shows the structure in the fossil specimen; fig. 12, in the recent *Æshna grandis*, and fig. 13,

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Wing of *Æshna grandis*.

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Wing of *Libellula depressa*.

in *Libellula depressa*. The dimensions of the fossil are about one-third greater than those of *Æshna grandis*, one of the largest of our British species, its length being 2 inches 10½ lines, and its greatest breadth 8½ lines.

It is proposed, for the sake of distinction, to denominate this fossil insect *Æshna liassina*.

The specimen before us furnishes, I believe, the first example of an insect of this family in so old a formation as the lias. It is well known that *Libellulæ* occur in the lithographic stone of Solenhofen,[1] which belongs to the upper part of the oolitic series, and is the lowest rock in which these insects have hitherto been found. The present specimen is, therefore, unquestionably of great geological interest, especially when we contrast its close affinity to existing forms, with the extraordinary saurian, piscine, and molluscous structures which were its contemporaries.

This specimen was found in the neighbourhood of Binton,

[1] See Delabeche, 'Geological Manual,' p. 345, &c.

near Bidford, in Warwickshire, where the beds of limestone near the base of the lias are largely quarried for flooring, &c. These beds of limestone, besides the usual Ammonites, &c. of the lias, occasionally contain specimens of *Ichthyosauri*, *Plesiosauri*, three or four species of fish, crustaceans, and two or three species of ferns. The latter circumstance indicates the proximity of land at the time of the deposition of the strata, a supposition which is further borne out by the fossil insect above described.

One of the species of fish found here appears to be a Cycloid, and furnishes an exception to the generalization of M. Agissiz, that no cycloidian fish occur below the chalk.

In the first series of this Magazine, vol. v. p. 549, is a figure of a fossil fish from Wilmcote (misspelt Wilments), near Binton. This specimen is now in the Warwick Museum, and has been figured by M. Agassiz, under the name of *Tetragonolepis angulifer*.

The rock in which these fossils are found, is a fine-grained blue limestone, dividing into thin slabs, resembling in texture the Solenhofen stone, and like it adapted to lithographic purposes.

Cracombe House, Evesham,
May 7th, 1840.

Art. IX.—*Notice of the existence of a distinct Tube within the hollows of the* Paramoudra. By Robt. Fitch, Esq., F.G.S.

After the interest which has been felt upon the subject of the *Paramoudras* or pot-stones of the chalk, through the observations of Professors Buckland, Ehrenberg, and other writers, I am surprised that one fact in the history of these most singular bodies should hitherto have escaped notice:—I refer to the existence of a central tube passing through the long axis of each chalk nucleus, and as I am led to imagine, originally forming a connecting link between the detached pieces of the entire column, as seen in the accompanying section (fig. 14). In this sketch I have represented the extremities of the two *Paramoudras* in contact, as I have frequently found this to be the case. Dr. Buckland attributes their being so to accident, remarking " sometimes the extremities of two specimens are found in contact, but this seems to be the result of accidental juxta-position, not of any ori-

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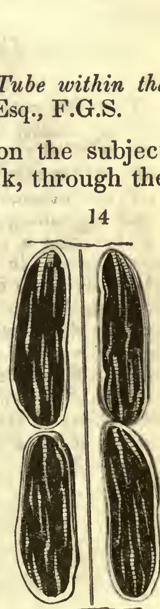

ginal connexion of the animal bodies." 'Geol. Trans.,' vol. iv.

The superior terminal *Paramoudra*, I have always found round and closed at the top, but upon breaking it, there will be found in every specimen, the tube passing through from the chalk into the substance of the flint, and coming out on one side two or three inches below the top.

Mr. Lyell, in his paper read at the meeting of the British Association, 1838, says that each *Paramoudra* contains *invariably* the *cylindrical nucleus of chalk* : this is not always the case, for during one of my visits to the pit at Horstead last October, I was breaking a specimen for examination, when, instead of finding chalk, I found it flint throughout, and the hollow tube quite silicified passing through the centre, as I have invariably found it passing through the chalk nucleus. I have since found another similar specimen at Whittingham. I have observed in the chalk nucleus, several *Ventriculites*, *Ananchytes*, *Plagiostoma spinosa*, *Terebratula octoplicata*, and indeed, most of the fossils *common* in our chalk ; and in almost all the specimens that I have broken, have found masses of *Pyrites* usually attached to the flint, but projecting into the chalk nucleus ; occasionally I have observed the belemnite passing through the flint, and entering the chalk nucleus.

Norwich, *Feb.* 10, 1840.

[Can this curious tube be in any way connected with the aggregation of flinty matter forming the paramoudral column? If the pot stones were originally sponges, or organic bodies of any kind, how comes it that when broken they display no structure, or at any rate, nothing which will distinguish them from the ordinary nodula or tabular chalk-flints?

After spending a morning in the chalk-pit at Horstead, near Norwich, and breaking a considerable number of the *Paramoudras*, we found the tube present in every instance; sometimes however, so nearly obliterated as to be only traceable by the discoloration of the chalk around its original site. The tube varies in diameter from the thickness of an ordinary-sized quill, to that of the finger. The wall of the tube is generally of a green colour, and about as thick as the rind of an apple; the cavity is filled with chalk. Mr. Bowerbank finds it to consist of siliceous particles. The mode in which the tube quits the chalk, and passes through the flint cap of the terminal *Paramoudra* is very remarkable. We believe the sole merit of this discovery to rest with our correspondent Mr. Robt. Fitch.]—Ed.

Art. X.—*A few further Observations on Affinity and Analogy.* By J. O. Westwood, Esq., F.LS.

Coinciding, as I fully do, in many of Mr. H. E. Strickland's observations on the distinction existing between the affinities and analogies of organized beings, allow me a few lines to correct the erroneous impression which his illustration (pp. 224 and 225) of my remarks, is likely to cause, and to remove the notion that they contain a fallacy. The object of my short article was to record my opinion,—

1. That relations of analogy and affinity are nothing else than instances of more or less perfect resemblance : and

2. That as these relations are absolutely comparative in their nature, it follows that both kind of relations may coexist at the same time between any two animals, according to the animals compared therewith. These opinions may be thus illustrated :—

1. The natural system depends not only upon the existence of a resemblance in essential peculiarities of structure, or an agreement in the numerical majority of characteristics, whereby the species of a genus, the genera of a family, the families of an order, or the orders of a class, are linked together, whence the origin of affinity, but also upon less perfect resemblances, whereby a species of one genus is compared with a species in some other genus, family, or order, or a genus in one family is compared with a genus in some other family, and so on, whence the origin of analogy. Both these relations are, however, necessarily dependent upon *resemblance*, and the *approximations* of which Mr. Blyth speaks, not only prove the occasional difficulty of drawing the line between the two kinds of relations, but also the truth of the Linnæan adage " Natura non facit saltus," a principle which Mr. Strickland would adopt in the lower groups, but reject in the higher.

2. The co-existence of these relations between any two objects, is illustrated by the goat-sucker, bat, dragon-fly, and *Dionæa* in the following manner. All these animals possess *inter se*, in their common character of fly-catchers, an analogical relation ; but when I compare the two former, as vertebrated animals, with the dragon-fly, as an invertebrated animal, I find a higher relation (that is an affinity) existing between the bat and the goat-sucker, than subsists between either of these and the dragon-fly. When, however, I compare these three *animals* with the *plant*, *Dionæa*, I find a relation (that is an affinity) established between the dragon-fly

and the two other animals, exactly as much higher than the relation between these animals and the plant, as the relation between the dragon-fly and the bat or the goat-sucker was, when these two last-named animals were compared with the dragon-fly. In other words, instead of saying, as Mr. Strickland makes me say, that "an affinity subsists between the bat and dragon-fly, when compared with the *Dionæa*, and an analogy when compared with the goat-sucker," I should say that an affinity exists between the bat and dragon-fly, when these two *animals* are compared with the *vegetable*, *Dionæa*, and that an analogy exists between two such animals, when the *vertebrated* bat is compared with the vertebrated goat-sucker. Again, from the comparative nature of these resemblances, it appears to me that the relation which exists between the goat-sucker and dragon-fly, when these two animals are compared with a *Dionæa*, is as close an affinity as that which exists between the goat-sucker and the swallow, when these two birds are compared with the bat. Independent of such comparison, the two birds are, of course, closer in affinity than one of the birds and the bat. Whilst, as Mr. Strickland justly remarks, the dragon-fly (independent of its fly-catching habit), has no more affinity to the goat-sucker than a beetle, a lobster, or any other annulose animal.

Argynnis Aphrodite, a British species.—I was very much gratified by the appearance of *Arg. Aphrodite* in the illustrations to the Magazine, as a few years ago, while out entomologizing, I saw, in a brake of thorns near a wood (Coed Gwynion), a short distance from the town, a beautiful *Argynnis.* I stood observing it some time; I was within four feet of it, so that I had a good opportunity of observation. It being a stranger to me, I searched Duncan's 'British Butterflies' for it, but in vain: *Arg. Paphia* was the nearest, but my stranger had a row of crescent-shaped marks towards the external edges of its wing; *Paphia* is represented with spots instead of crescents in the above work. From the other large *Argynnides* it differed in not having a black border on the outside of the crescents. At the time I concluded that it was *Arg. Paphia*, thinking that the figures of the markings might not be exactly delineated, as it oftentimes happens in cheap works, that they cannot take the time necessary for perfectly drawing and colouring the plates; but if the *Argynnides* in the above work are truly figured, it is a moral certainty in my own mind, that the butterfly I saw was *Arg. Aphrodite.*—*James Bladon.*—*Pontypool, May* 1840.

THE MAGAZINE

OF

NATURAL HISTORY.

JULY, 1840.

Art. I.—*View of the Fauna of Brazil, anterior to the last Geological Revolution.* By Dr. Lund.

(*Continued from page* 259.)

Third Order *MYOIDEA.*

Family of *Rodents.*

The most numerous genus of this family is the genus *Mus*, of which I am acquainted with six species, indigenous to this district, besides two which have been introduced, the one from Europe (*Mus. musculus*), the other, as I suspect, from Asia (*Mus setosus*, mihi.) The abundant remains of this genus that I have collected from the caverns, prove its existence in the ancient world. The species, difficult to distinguish by external characters, are still more so when we have nothing more than fragments of their skeletons. Among the remains hitherto obtained, I can distinguish two, or perhaps three extinct species, without being able to speak more decidedly as to their relation to those still existing.

Next to the genus *Mus*, with reference to the number of species, follows that of *Echimys* (*Loncheres*, Ill.), or spiny rats. This genus comprises here four species, as distinguishable from each other by their outward form, as they are closely allied in their internal structure. The largest species (*E. apereoides*, mihi), bears the most striking resemblance to the *Pereà* (*Anœma Aperea*) in size, colour, form, and whole appearance; excepting that it is provided with a long and very hairy tail. It has no trace of spines; and its hair is of ordinary quality. In several caverns I have found remains of a fossil species, which approaches it very nearly, but at the same time exhibits some specific distinctions, and moreover, rather exceeds it in size. Another species (*Ech. elegans*, mihi) is of the size of our large house-rat (*Mus decumanus*), to which it bears a perfect external resemblance, and is furnished with a very long, bare tail. It is armed with spines, and displays a purity of colour that makes it a very pretty little animal. I have discovered the fossil remains of a species, which, judging by the fragments I possess, agrees well enough with this. A third species (*Ech. sulcidens*) I am only acquainted with from the abundant remains it has left upon the surface of the soil in the caves, where they form no inconsiderable portion of the vast heaps of bones I have described in my preceding communication. I have also found traces of a species belonging to the fossil period, which seems to agree very closely with this recent animal; but I have again to lament that the fragments I as yet possess, are not sufficient to enable me to pronounce decidedly on its identity.

A fourth species (*Ech. laticeps*, mihi) is rare, and I have not met with any trace of a corresponding species among the fossil bones of these caverns.

Equally peculiar to South America as the preceding genus, to which it also bears a near affinity, is the genus *Synetheres*, of which there are only two known species, *Syn. prehensilis*, L., and *Syn. insidiosa*, Licht.;[1] the last of the size of a rabbit, the former twice as large. The antediluvian world also possessed this animal form, but, with the character peculiar to that ancient fauna, of a gigantic size; for the fragments that I have, betoken a creature very little inferior in bulk to the wild hog. The fossil species, moreover, presents a nearer resemblance to the smaller existing species (*Syn. insidiosa*) than to the larger (*Syn. prehensilis*), so that it would be classed by those zoologists who make a generic division between these two species, under the genus *Sphiggurus*.

Each of the remaining genera of this family, viz. *Sciurus*, *Lepus*, *Anœma*, *Dasyprocta*, *Cœlogenys*, and *Hydrochœrus*, contains at present but a single species.

Of the first genus I have hitherto found no trace whatever

[1] I cannot agree with the views of some of our modern zoologists who make a generic distinction between these two species (*Synetheres* and *Sphiggurus*, F. Cuv.), inasmuch as they sufficiently agree in their habits, external appearance, and even in their internal structure, especially in their dental system. The principal difference lies in the great development of the nasal and frontal bones, which the former possesses, in common with the *Hystrices* of the old world, but which is not seen in the latter.

in the diluvian soil; on the other hand, I have discovered bones of a small and evidently rodent animal, which does not agree with any of the genera at present existing in this country.

The caves contain abundant fossil remains of the genera *Lepus* and *Anœma;* and still more of a species belonging to the genus *Dasyprocta;* all of which resemble more or less closely the recent species of their respective genera. There is, however, a second species of the last-named genus which merits a particular notice, not merely from its much greater rarity, but from its extraordinary size, which at first misled me in the identification of its bones. The long bones of the hinder extremities of this species are, in fact, almost as large as those of the roebuck, for which reason I propose for it the name of *Dasy. capreolus*, in order to connect with its specific title an idea of a size so unusual in this family.

The same relation which we have observed in the genus *Cutia*, is repeated in the genus *Capivar*. I find two extinct species of this genus; the one identical with that now existing, the other, on the contrary, of astonishing size. I propose for this last, the name *Hydrochœrus sulcidens*, because its incisors, instead of being smooth, as in the living species, are furnished on their anterior surface with a number of longitudinal furrows, separated by parallel, rifle-like ridges. It approached the very considerable dimensions of five feet in length, so as to stand exactly midway between the existing species of this genus, and the giant of South America's recent fauna, the tapir.

I conclude my brief survey of this family with a genus that requires a more detailed examination than the former, on account of the important light it throws upon the ancient fauna, and its relation to the recent; I mean the genus *Paca.*[1] The remains of this genus, in a fossil state, are found in the soil of most of the Brazilian caves: I have endeavoured, in my description of that of Cerca Grande, to convey some idea of the astonishing extent to which they are there amassed. A cursory examination of these remains showed me no essential difference from the recent *Paca*. We have already, in our survey of the previous families, met with fossil remains that seemed to agree more or less accurately with existing animals, but whose complete identity we were prevented from determining by the imperfect state of the fragments. But it is particularly in the family now under review, that this difficulty so frequently occurs; the genera *Echimys*, *Anœma*, *Lepus*, and *Dasyprocta*, have furnished examples of this. What has thus occurred to myself in the determination of the

[1] Cœlogenys.

species of the extinct fauna of this continent, has also happened to those who have been occupied in similar researches in the old world; and the important question yet remains unanswered, whether in fact species identically the same, can be shown as belonging to these two periods. The genus now under consideration, seems likely to afford a solution to this question, on at least one point. On the one side, the complete identity with the living species which the first view of the fossil remains of this genus exhibited, seemed to authorize me to consider it in the same light as those already described as coinciding more or less with existing genera; while on the other hand, the extraordinary abundance and perfect state of the materials for comparison, which I possessed of this genus, far exceeding those at the disposal of any former zoologist, enabled me to decide the point, so far as it referred to the corresponding species of this district, with an accuracy which could not leave any room for doubt or uncertainty. Two subsequent visits to that remarkable cave, and long-continued excavations, had so multiplied my materials, that I was enabled to enter upon the investigation with more or less perfect remains of above a hundred individuals, of all ages.

The result of these examinations was equally surprising and conclusive. In spite of the close correspondence in every other part of the skeleton, a more exact consideration of the skulls, proved that the vast number of the fossil remains of this genus consisted of two species, both of which are very distinct from the living *Paca*. One of these I call *Cœlogenys laticeps*, from the circumstance of the zygomatic arches being posteriorly so far retired from the skull, that it acquires the appearance of being pressed flat. The other species I have already described under the title of *Cœlogenys rugiceps*, a name that well becomes it. In this species the glenoid cavities and adjoining *zygomata* are so extraordinarily developed, that these same anatomical relations in the existing species seem to be only a feeble indication of the almost monstrous development that gives the skull of this species so peculiar a character. Both these species appear to have been very common in those olden times; but much rarer are the remains of a third species, *Cœlogenys major*, which considerably surpasses both the former in size, and which, in this respect, is not inferior to the living *Capivar*.

These examinations of the genus *Paca*, which, from their completeness, may serve for the foundation of the subsequent comparisons, give the same relations for this genus, that we have already remarked in the genera *Cutia* and *Capivar;* namely that the ancient fauna possessed two subordinate forms of this genus, of which one shows a striking agreement with the existing species, while the other repeats it on a magnified scale. The first of these subordinate forms comprises two species in the genus *Paca;* whereas in the two last-named genera, we are only acquainted with a single species of each, most probably in consequence of the imperfect state of our materials. How far this subordinate form is to be considered as specifically identical with the existing animal, we have been able to decide with certainty in the genus *Paca;* and we are entitled, at least for the present, to extend this result not only to the other genera of this family, which are found similarly circumstanced, but still more to those out of it, in which the resemblance between extinct and living forms is much less obvious. With reference to the numerical relations of this family in the present and former periods, we see that only one of the existing genera is absent from the list of the fossil genera, namely that of squirrels. The contrary could scarcely have been expected. The squirrel is an animal whose habits and haunts entirely protect it from the pursuit of the larger predaceous beasts; and that it evidently also very seldom becomes the prey of birds, I conclude from the circumstance of my having never seen any trace of it in those heaps of bones that are formed in the caves from the remnants of the food of the *Strix perlata.* The absence hitherto of this genus, therefore, from the list of fossil species, does not warrant us in concluding that this form was in reality non-existent in those former times: on the contrary we may suppose, with a high degree of probability, that inasmuch as this family presented so perfect an agreement in those ages with what it now contains, neither was this form wanting.

In addition, there appears on the list of the extinct fauna a genus which is not now found here; whence it seems that the greater variety or richness of generic forms, which formerly characterized all the families we have yet considered, also extends to this. Of the nine genera whereof this family now consists, six are peculiar to this continent; the *Capivar*, *Paca*, *Cutia*, *Perea*, *Synetheres*, and *Echimys*. All these genera are again found in the extinct fauna of this district: and we thus obtain one of the most beautiful and conclusive arguments for a result we have already frequently insisted on, namely, that the extinct fauna of this continent was a true prototype of the existing races. Before I proceed to compare the number of species of this family for the two periods, I think it right to add a few remarks respecting the conditions in which the fossils are found. The remains of the larger kinds appear under the same circumstances as the former family, that is, scattered about in the soil of the caverns, and gnawed; in short, exhibiting unequivocal marks of having been dragged in by beasts of prey. Occasionally, also, I have found the bones of smaller kinds similarly circumstanced, and intermixed with the remains of the larger animals; so that I am led to conclude that these also have, in some instances, served for food to the *Carnivora*. But more frequently the remains of these lesser species are seen separate from those of the greater, and forming a kind of osseous conglomerate by themselves. I have in my former paper given a detailed account of the remarkable masses of bones which are collected in these caves, even in our own day, and which I have shown to be attributable to the *Strix perlata*, Licht. Now, if we suppose an irruption of water penetrating into these caverns, dispersing the heaps of bones, and enveloping the scattered fragments in its sedimentary soil, which, in the process of time, would be impregnated with calcareous particles from the dripping of the roof, and thus be converted into a perfectly hard mass, that would act as a cement to the bones; under such conditions we should have the very breccia of which I have spoken. Indeed, the resemblance between these osseous conglomerates, and the heaps of bones above described, is so striking, that at the first I was mistaken as to their respective age:[1] for the animals of whose remains they are composed, are in the main the same, being principally species of the genera *Mus*, *Echimys*, *Anœma*, or young individuals of *Lepus*. The total number of the species of this family that at present exist here is eighteen; whereas I have as yet discovered only sixteen belonging to the extinct fauna. The genus *Mus* constitutes a third part of the whole existing number; and it is precisely this genus that gives the list of recent species its preponderance over that of the fossil. But this present superiority of the genus *Mus*, with regard to the number of species, in all probability arises from our greater ignorance of the ancient fauna. Long before I was acquainted with some of the recent species of this genus, now existing here, I possessed hundreds of fragments of their skeletons; but among these

[1] Thus in my description of the cave of Maquiné, I have mentioned a similar breccia in its second chamber, which I then considered to be a recent formation; but later investigations have convinced me that the osseous remains it contains belong to a more ancient fauna.

I was unable to distinguish accurately more than two, or at the most three species, whereas an examination of the Natural History of the district soon convinced me of the existence of six species in these parts. If we then suppose the same number of species to have existed in the former epoch, which we know to exist now (a supposition evidently not too over-stretched for a period that is proved to have been so rich in animal forms), we have already a greater number of species for that period than for the present; and we are emboldened to extend to this family also, the result to which the consideration of all the other families has led us, namely, the superiority of the ancient fauna over the present, with reference to variety of species. This conclusion, which must be considered as established for the family taken as a whole, is also true for many of its genera, as for instance, *Cutia*, *Capivar*, and *Paca*. We have already observed the same fact in one family, in the instance of the genus *Dicotyles;* and we are thus conducted to the remarkable result, that not only are all the families we have hitherto examined to be considered as mere fragments of what they were in former times, but that also many of their genera are similarly circumstanced. A closer examination of these genera acquaints us also with two other facts, not less important, namely, first that they are all such as are now peculiar to the new world; and secondly, that such of their specific forms as have representatives in the existing fauna, are distinguished by a smaller bulk than those which have no such representatives now; so that we are induced to regard the existing fauna as a repetition of the extinct, on a diminished scale, with reference both to numbers and size.

Family of *Marsupials.*

Of this family there is only a single existing genus in this district: it is, however, tolerably abundant in species. These admit of two subdivisions according to their size; one comprising the larger species, which both in habits and magnitude may be compared to our martens and polecats; the other the smaller, that scarcely exceed our mice and rats. I am acquainted with two species in the first division, *Didelphis aurita*, Pr. Max., and *Did. albiventer*, mihi, and three in the latter, *Did. murina*, Lin., *Did. brachyura*, Pall., and *Did. pusilla*, Desm. I find the fossil remains of species belonging to both these divisions, which, for the present, I refer to only two species; at the same time applying to this genus the observations I made respecting the number of species in the genus *Mus* of that former period, from which I deduced the conclusion that we have no ground for believing this number to have been in reality less than it now is. The fossils of this family are found under the same conditions as those of the rodents; the bones of the smaller kinds entering into the composition of the fine osseous breccia, while those of the larger species are intermixed with the bones of those animals that have constituted the food of the beasts of prey.[1]

Family of *Bats.*

All the families we have hitherto considered, have either exhibited to us a greater abundance both of genera and species, in the former than in the present period, or have at least allowed room for the supposition that they were not inferior in this respect. It is otherwise with the family we now proceed to examine. Notwithstanding the most careful search, I have not yet been able to discover the least trace of any animal of this family in the sediment of the last great deluge in this district; and, as far as my information extends, the investigations of scientific men in the old world have been equally unsuccessful. We might thus seem authorized to conclude, that this family was really wanting in the preexistent fauna. I must, however, draw attention to several circumstances that show the necessity of caution, before we come to a positive decision on this point. Of all mammals, bats are the least exposed to the attacks of predatory beasts; and we therefore could hardly expect to meet with their remains among those of the animals that have served for their food. It would, on the other hand, appear probable that they should be the prey of owls, through whose instrumentality many of the other bones have been introduced into our caves. I have, however, shown, in my former communication, how very small a quotient (only 1 per cent.) their bones constitute in the composition of these heaps. If to these considerations we add the fact that this family has left traces of its existence in a still more ancient period of the world, in the gypsum of Montmartre, we surely see that we must not, without a very strong amount of proof, agree to so extraordinary a result, as such an alternating appearance, disappearance,

[1] I possess a molar tooth of a large animal, which differs in its form from the molars of all predaceous animals, and most nearly approaches the hindmost teeth of *Didelphis*. Until I am fortunate enough to obtain more fragments of this remarkable animal, I abstain from guessing at its proper place in the system. The tooth seems to belong to an animal of the size of a large wolf.

and re-appearance of an animal family would be. I therefore abstain for the present from offering any opinion whatever on this subject; although I must confess that this constant failure of all my efforts to find in the soil of the caves even a trace of any single individual of this family, has already long excited my wonder; the rather, because the family of bats is now, next to the rodents and *Feræ*, the most abundant in species within this district; and, as I have elsewhere shown, claims the first place on the list of those animals which, in the present day, make caverns their residence.

Fourth Order, *QUADRUMANA*.

Family of *Apes*, (Simiæ.)

If my attempts to discover any of the preceding family (Bats), have hitherto been fruitless, so have they been rewarded with most unexpected success, in the case of the family I next proceed to consider. I am at length enabled to solve the important question as to the existence of the highest class of mammals in those ancient times to which these fossils belong; a question which has as yet been unanswered, or which most philosophers have thought right to answer in the negative. It is certain this family was then in existence; and the first animal of the class recovered is of gigantic size, a character belonging to the organization of the period. It considerably exceeds the largest individuals of the orang-outang, or Chimpanzee, yet seen; from which also, as well as from the long-armed apes (*Hylobates*), it is generically distinct. As it equally differs from the apes now living here, I would place it for the present in a genus of its own, for which I propose the name *Protopithecus;* with the specific distinction *Prot. brasiliensis*, from the quarter where the first representative of this family saw the light of day. I cannot omit this opportunity of recording a tradition very general over a considerable extent of the interior highlands, especially in the northern and western portions of the province of St. Paul, and the Sertao of St. Francisco. According to this current report, the district here mentioned is even yet inhabited by a very large ape, to which the Indians (from whom the report comes), have given the name of *Caypore*, which signifies the dweller in the wood. The *Caypore* is said to be as big as a man, and covered over its entire body and a portion of its face with very long curly hair. Its colour is brown, with the exception of a white mark on the belly, immediately above the navel. It climbs up trees with facility, but most frequently keeps to the ground, where it walks upright like a man. While young it is a quiet, inoffensive creature, living upon fruits, and its teeth are shaped like the human; but as it increases in age, it becomes rapacious and bloodthirsty; it takes to chasing birds and small mammals; huge canine teeth project from its mouth, and it is dangerous even to man. Its skin is impenetrable to ball, everywhere except the white mark on its belly. The natives dread this animal, and avoid the spots it frequents, which are rendered evident by the *Caypore's* characteristic footmark; for, according to this same tradition, its foot is not formed like that of man, but ends in a heel, both before and behind, so that it is impossible to know in which direction the animal has gone. It is easy to recognize in many of the traits of this mysterious creature's natural history, the childish embellishments of a savage race. The meaning of an anterior heel is evidently this; that the forepart of the foot is not broader than the hind, and that the impressions of the toes are not distinguishable. As to the white spot on the belly, I must remark that all the long-haired apes, now found here, have the central part of the belly very thinly covered with hair, so that when the hair is of a dark colour, and the skin light, an effect is produced during the act of respiration, as if there were a white spot on the stomach. The impenetrability of its hide may seem fabulous; but I really am acquainted with a species of this family, the *Guigó* (*Mycetes crinicaudus*, mihi), which has this property. This undescribed animal (which constitutes a remarkable link between *Mycetes* and *Cebus*, inasmuch as it combines the vocal organs of the former with the perfectly hairy tail of the latter), is provided with a skin clothed with such long and felted hair, as to be proof against shot on its back and sides. It would seem to be well aware of its good buckler; for instead of seeking safety in flight, like other apes, on the approach of danger, it rolls itself up in a ball, as if to cover the part least protected with hair, and thus bids defiance to the hunter's shot.

I have introduced this tradition, less on account of its zoological interest, than for the striking coincidence it displays in many points with the stories related of the Pongo of Borneo. If no such animal exists in the district where this tradition is current, whence did it arise? Is it possible that the Indians have received it from their forefathers? And may this tradition then be considered as one more testimony to the Asiatic origin of the first inhabitants of America? I have given this tradition as it is told by the Indians of the province of St. Paul. In the Sertao of S. Francisco, it is coupled with additions which weaken its zoological interest, but give it another, as betraying the only trace I have met with in this district, of a belief in fairy beings. According to the natives of Sertao, the *Caypore* is lord over the wild hogs; and sometimes when one of these animals has been shot, the voice of the enraged *Caypore* is heard in the distance, and the hunter at once quits his prey to save himself by flight. The *Caypore* is said to have been seen in the centre of a herd of swine, riding on the biggest: and indeed has been sometimes described as a sort of Suscentaur, that is, an ape above, and a hog below.

[This paper is rendered somewhat obscure from the circumstance that the author mentions the same animal, in some instances, under two or three different names, and he occasionally uses as generic terms, the names by which certain species are known in their native country. The Capivar is the *Hydrochœrus Capybara* of most systematic works; the *Paca* is the *Cœlogenys subniger* of authors; *Cutia*, is generally used in the paper for the genus *Dasyprocta;* by *Perea*, is meant the *Cavia Aperea* or *Cavia Cobaya*. *Synetheres* and *Echimys* are systematic names, and have the advantage of being intelligible. The local names being spelt in all manner of ways, and often very numerous, are difficult to recollect.]

(To be continued).

ART. II.—*Sketch of the Flora of the neighbourhood of Ipswich: including the Phænogamic Plants, the Filices, and Equisetaceæ.* By WILLIAM BARNARD CLARKE, M.D., F.B.S.Ed.

(Continued from page 130.)

COMPOSITÆ.

TRAGOPOGON *pratense*. Fields; frequently seen.
HELMINTHIA *echioides*. Field-sides, at Walton and Felixtow, common.
SONCHUS *arvensis*. Fields, common.
——— *oleraceus*. Fields and waste places, common.
LEONTODON *Taraxacum*. Fields, pastures, &c., everywhere.
THRINCIA *hirta*. Banks by the side of the Orwell river, common.
APARGIA *hispida*. Banks by the side of the Orwell.
HIERACIUM *Pilosella*. Sandy fields, common.
LAPSANA *pusilla*. Sandy fields, local.
——— *communis*. Field-sides, common.
CICHORIUM *Intybus*. Field and road-sides, common.
ARCTIUM *Lappa*. Road-sides, common.
CARDUUS *marianus*. Road-sides, frequently.
CNICUS *lanceolatus*. Fields and road-sides frequently.
——— *palustris*. Pastures, &c., common.
——— *arvensis*. Fields and road-sides, common.

ONOPORDON *Acanthium.* Fields and road-sides, common.
CARLINA *vulgaris.* Chalky meadows, rather local.
BIDENS *tripartita.* Wet spongy places, rare.
——— *cernua.* Moist meadows, rare.
EUPATORIUM *cannabinum.* Sides of the Gipping, &c., common.
TANACETUM *vulgare.* Fields and road-sides, common.
ARTEMISIA *maritima.* Walton shore, side of Orwell river, common.
——— *gallica.* Road-sides near Walton, common.
——— *Absinthium.* Road-sides at Freston, Belstead, &c., common.
——— *vulgaris.* Road-sides, very common.
GNAPHALIUM *gallicum.* Fields and road-sides, common.
——— *germanicum.* Fields, very common.
——— *minimum.* Sandy fields, common.
CONYZA *squarrosa.* Chalky districts, frequently.
ERIGERON *acre.* Field-sides, occasionally.
TUSSILAGO *Farfara.* Sides of the Gipping river, frequently.
PETASITES *vulgaris.* Sides of the Gipping river, common.
SENECIO *vulgaris.* Fields and waste land, common.
——— *viscosus.* Shore at Walton, occasionally.
——— *sylvaticus.* Road-sides, frequently.
——— *tenuifolius.* Fields and road-sides, in chalky districts.
——— *Jacobæa.* Meadows, very common.
ASTER *Tripolium.* Marshy land by the side of the Orwell, common.
PULICARIA *dysenterica.* Sides of the Gipping, &c., common.
BELLIS *perennis.* Pastures, &c., everywhere.
PYRETHRUM *Parthenium.* Waste land, common.
CHRYSANTHEMUM *segetum.* Fields, frequently.
MATRICARIA *Chamomilla.* Field- and road-sides, common.
ANTHEMIS *arvensis.* Fields occasionally.
ACHILLÆA *Millefolium.* Fields and pastures, everywhere.
CENTAUREA *Cyanus.* Corn-fields, common.
——— *nigra.* Fields and road-sides, common.

CAMPANULACEÆ.

CAMPANULA *rotundifolia.* Fields and road-sides, common.
——— *Trachelium.* Fields and thickets, on chalky soil, common.

ERICACEÆ.

ERICA *Tetralix.*
——— *cinerca.*
CALLUNA *vulgaris.*
} Heaths, common.

OLEACEÆ.

LIGUSTRUM *vulgare.* Hedges, occasionally.
FRAXINUS *excelsior.* Woods, common.

APOCYNACEÆ.

VINCA *major.* Hedges at Freston, local.
——— *minor.* Hedges, occasionally.

GENTIANACEÆ.

GENTIANA *Amarella*. Chalky fields, local.
ERYTHRÆA *Centaurium*. Woods, local.
MENYANTHES *trifoliata*. Wet meadows, local.

CONVOLVULACEÆ.

CONVOLVULUS *Soldanella*. Sandy places at Walton.
——— *arvensis*. Banks, road-sides, common.
——— *sepium*. Hedges, common.

BORAGINACEÆ.

LITHOSPERMUM *arvense*. Fields, occasionally.
PULMONARIA *angustifolia*. Road-sides, local.
SYMPHYTUM *tuberosum*. Moist banks, side of the Gipping occasionally.
ECHIUM *vulgare*. Waste ground, frequently.
LYCOPSIS *arvensis*. Fields and road-sides, common.
ANCHUSA *sempervirens*. Road-sides, not common.
MYOSOTIS *palustris*. Side of the Gipping, common.
——— *arvensis*. Fields and hedge banks, common.
CYNOGLOSSUM *officinale*. Banks by the side of the Gipping, road-sides, &c.
BORAGO *officinalis*. Waste-ground, occasionally.

SOLANACEÆ.

VERBASCUM *Thapsus*. Field-sides, frequent.
——— *nigrum*. Chalky districts, common.
HYOSCYAMUS *niger*. Road-sides near Ipswich, rare; but very abundant near Walton and Felixtow.
DATURA *Stramonium*. Road-sides near gardens, hardly wild.
SOLANUM *nigrum*. Waste-ground, frequently.
——— *Dulcamara*. Sides of ditches, near the Gipping, &c., common.

SCROPHULARIACEÆ.

ANTIRRHINUM *majus*. Walls, local.
——— *Orontium*. Fields frequently.
LINARIA *Cymbalaria*. Walls, &c., near gardens.
——— *Elatine*. Corn-fields, frequently.
——— *vulgaris*. Hedges, common.
SCROPHULARIA *nodosa*. Woods and meadow-sides, frequently.
——— *aquatica*. Sides of the Gipping, common.
DIGITALIS *purpurea*. Bently woods, frequently.
BARTSIA *Odontites*. Chalky fields, in abundance.
EUPHRASIA *officinalis*. Heaths and chalky places, common.
RHINANTHUS *Crista-galli*. Sides of the Gipping, frequently.
MELAMPYRUM *pratense*. Woods, common.
PEDICULARIS *sylvatica*. Moist meadows, occasionally.
VERONICA *serpyllifolia*. Meadows, local.
——— *Anagallis*. Ditches, common.
——— *Beccabunga*. Side of the Gipping, common.
——— *officinalis*. Pastures, occasionally.
——— *montana*. Woods, occasionally.

VERONICA *Chamædris.* ⎫ Hedge banks, common.
——— *hederifolia.* ⎭
——— *agrestis.* Fields, occasionally.
——— *arvensis.* Fields, common.

LABIATÆ.

LYCOPUS *europæus.* ⎫ Sides of the Gipping, common.
MENTHA *piperita.* ⎭
TEUCRIUM *scorodonia.* Roadsides, woods, common.
AJUGA *reptans.* Moist woods, common.
BALLOTA *nigra.* Hedges, common.
GALEOBDOLON *luteum.* Wood-sides, rather local.
GALEOPSIS *Ladanum.* Fields, frequently.
——— *Tetrahit.* ⎫ Field-sides, occasionally.
——— *versicolor.* ⎭
LAMIUM *album.* Road-sides, &c., common.
——— *amplexicaule.* ⎫ Hedge-banks, common.
——— *purpureum.* ⎭
——— *incisum.* Hedge-banks, local.
BETONICA *officinalis.* Watery lanes, occasionally.
STACHYS *sylvatica.* Woods, common.
——— *palustris.* Sides of the Gipping, common.
NEPETA *Cataria.* Road-sides, near the village of Kersey, abundant.
GLECHOMA *hederacea.* Woods and hedge-banks, abundant.
CALAMINTHA *Nepeta.* Hedge-banks, local.
CLINOPODIUM *vulgare.* Woods, common.
PRUNELLA *vulgaris.* Sides of lanes, occasionally.
SCUTELLARIA *galericulata.* Side of the Gipping, frequently.
SALVIA *verbenaca.* Road-sides, occasionally.
VERBENA *officinalis.* Field- and road-sides, common.

OROBANCHACEÆ.

OROBANCHE *major.* Sandy hills, amongst the roots of broom, occasionally.
——— *minor.* Clover-fields, extremely common.

PRIMULACEÆ.

PRIMULA *veris.* Pastures, common.
——— *elatior.* Pastures, local.
——— *vulgaris.* Woods, common.
HOTTONIA *palustris.* Ditches, local.
GLAUX *maritima.* Salt marshes, near the Orwell.
LYSIMACHIA *Nummularia.* Sides of the Gipping, rather local.
——— *nemorum.* Woods, frequently.
ANAGALLIS *arvensis.* Uncultivated parts of fields, &c., common.
——— *tenella.* Boggy parts of Nacton Heath, scarce.
SAMOLUS *Valerandi.* Sides of ditches, local.

PLUMBAGINACEÆ.

STATICE *Armeria.* ⎫ Sides of the Orwell, frequently.
——— *Limonium.* ⎭

PLANTAGINACEÆ.

PLANTAGO *major*. Fields and road-sides, common.
——— *media*.
——— *lanceolata*. } Pastures and road-sides, very common.
——— *maritima*.
——— *Coronopus*. } Sides of the river Orwell, frequently.

CHENOPODIACEÆ.

SALSOLA *Kali*. Shore at Walton, frequently.
CHENOPODIUM *maritimum*. Sides of the Orwell, common.
——— *Bonus Henricus*.
——— *hybridum*. } Road-sides, occasionally.
——— *album*. Road-sides, &c., common.
——— *urbicum*. Fields, occasionally.
ATRIPLEX *laciniata*. Sides of the Orwell, frequently.
——— *littoralis*. Sides of the Orwell, common.
——— *patula*. Road-sides, &c., common.
——— *angustifolia*. Road- and field-sides, occasionally.
BETA *maritima*. Banks of the Orwell, common.
SALICORNIA *herbacea*. Shores of the Orwell, very common.

POLYGONACEÆ.

POLYGONUM *amphibium*. Sides of the waters of the Gipping.
——— *Persicaria*. Fields, &c., common.
——— *Hydropiper*. Watery lanes and moist marshes, frequently.
——— *aviculare*. Road-sides, fields, &c., very common.
——— *Convolvulus*.
——— *Fagopyrum*. } Fields, occsionally.
RUMEX *Hydrolapathum*. Sides of the Gipping, frequently.
——— *crispus*. Meadows, &c., common.
——— *Acetosa*. Meadows, common.
——— *Acetosella*. Hedge banks, &c., common.

EUPHORBIACEÆ.

EUPHORBIA *Peplus*. Fields, common.
——— *exigua*. Fields, on a chalky soil, common.
——— *paralias*. Walton shore, common.
——— *Helioscopia*. Cultivated ground, common
——— *amygdaloides*. Woods, common.
MERCURIALIS *annua*. Troublesome weed on cultivated soil.
——— *perennis*. Woods, common.

URTICACEÆ.

URTICA *urens*. Fields, road-sides, &c., common.
——— *dioica*. Fields, roadsides, &c.
PARIETARIA *officinalis*. Old walls, local.
HUMULUS *Lupulus*. Hedges, frequently.

ULMACEÆ.

ULMUS *campestris*. Woods and hedge-rows, common.
——— *montana*. Hedge-rows, scarce.

AMENTALES.

CUPULIFERÆ OR CORYLACEÆ.

QUERCUS *Robur*. Woods and hedge-rows, common.
FAGUS *sylvatica*. Woods, &c., common.
CASTANEA *vulgaris*. Woods, common.
CORYLUS *Avellana*. Woods, hedge-rows, &c., common.

BETULACEÆ.

BETULA *alba*. Woods, &c., frequent.
ALNUS *glutinosa*. Frequent in low damp woods.

SALICACEÆ.

POPULUS *nigra*. Woods, occasionally.
——— *alba*. Hedge-rows, &c., frequently.
——— *tremula*. Woods, &c., occasionally.
SALIX *fragilis*. Marshes, occasionally.
——— *cinerea*. Woods and thickets, occasionally.
——— *viminalis*. Osier grounds, common.
——— *alba*. Marshes, common.

MONOCOTYLEDONES.

HYDROCHARACEÆ.

HYDROCHARIS *Morsus-ranæ*. Ditches by the side of the Gipping river, common.

ALISMACEÆ.

SAGITTARIA *sagittifolia*. Ditches near Ipswich; river Gipping, common.
ALISMA *Plantago*. Ditches near Ipswich, common.

BUTOMACEÆ.

BUTOMUS *umbellatus*. River Gipping; ditches, frequently.

JUNCAGINACEÆ.

TRIGLOCHIN *maritimum*. Sides of the Orwell, frequently.
——— *palustre*. Sides of the Orwell and Gipping, occasionally.

ORCHIDACEÆ.

ORCHIS *Morio*. Pastures, common.
——— *mascula*. Woods, common.
——— *latifolia*. Pastures, rare.
——— *maculata*. Pastures, occasionally.
HABENARIA *bifolia*. Meadows, local.
OPHRYS *apifera*. Fields, local.
LISTERA *ovata*. Woods and thickets, on chalky soil, occasionally.
——— *Nidus-avis*. Woods, rare, but occasionally found.

IRIDEÆ.

IRIS *Pseudacorus.* Ditches, common.

DIOSCORIACEÆ.

TAMUS *communis.* Woods and hedges, frequently.

LILIACEÆ.

SCILLEÆ.

ORNITHOGALUM *luteum.* Woods, local.
HYACINTHUS *non-scriptus.* Woods and thickets, common.
MUSCARI *racemosum.* Sandy fields, occasionally.
ALLIUM *ursinum.* Moist woods at Freston, common.
——— *vineale.* Banks of the Orwell, common.

TULIPEÆ.

FRITILLARIA *Meleagris.* Very common about Stoneham, ten or twelve miles from Ipswich.

TYPHACEÆ.

TYPHA *latifolia.* Ponds near Freston, local.
SPARGANIUM *ramosum.* Sides of the Gipping, &c., common.
——— *simplex.* Sides of the Gipping, local.

ARACEÆ.

ARUM *maculatum.* Woods, sides of shady lanes, &c., common.

FLUVIALES, OR NAIADACEÆ.

POTAMOGETON *natans.* } Ditches, frequently.
——— *fluitans.* }
——— *gramineum.* Ditches, occasionally.
ZOSTERA *marina.* Orwell river, abundant.

PISTIACEÆ.

LEMNA *minor.* Ditches, extremely common.
——— *trisulca.* Ditches, local.

JUNCEÆ.

LUZULA *campestris.* Fields, common.
——— *pilosa.* Woods, frequently.
JUNCUS *conglomeratus.* Marshes, common.
——— *lampocarpus.* Moist heaths, occasionally.
——— *bufonius.* Shady wet places, frequently.

CYPERACEÆ.

SCIRPUS *lacustris.* Sides of Gipping, frequently.
——— *maritimus.* Salt marshes, near Orwell river, common.
CAREX *intermedia.* Sides of the Gipping, common.
——— *stellulata.* Sides of the Gipping, occasionally.

CAREX *arenaria.* Walton and Felixtow shore, common.
——— *pendula.* Woods, occasionally.
——— *panicea.* Moist parts of heaths, occasionally.
——— *cæspitosa.* Sides of the Gipping, frequently.
——— *riparia.* } Banks of the Gipping, &c., frequently.
——— *ampullacea.* }

GRAMINACEÆ.

ANTHOXANTHUM *odoratum.* Meadows, woods, &c., common.
NARDUS *stricta.* Heaths, common.
ALOPECURUS *pratensis.* Meadows, common.
——— *geniculatus.* Meadows by the side of the Gipping.
PHALARIS *arundinacea.* Sides of ditches, &c., common.
PHLEUM *pratense.* Meadows, common.
——— *arenarium.* Sea shore at Walton and Felixtow, common.
MILIUM *effusum.* Fields and woods, common.
AGROSTIS *Spica-venti.* Fields, local.
——— *vulgaris.* } Fields and road-sides, frequently.
——— *alba.* }
AIRA *cæspitosa.* Fields, &c., common.
——— *flexuosa.* Fields, occasionally.
HOLCUS *mollis.* } Field-sides, &c., common.
——— *lanatus.* }
ARRHENATHERIUM *avenaceum.* Field-sides, common.
MELICA *uniflora.* Woods, occasionally.
POA *fluitans.* Sides of the Gipping, &c., common.
——— *aquatica.* Sides of the Gipping, occasionally.
——— *distans.* Meadows, frequently.
——— *rigida.* Old walls, &c., in several places.
——— *trivialis.* Meadows, frequently.
——— *pratensis.* } Meadows, common.
——— *annua.* }
BRIZA *media.* Meadows, frequently.
DACTYLIS *glomerata.* Meadows, &c., very common.
SPARTINA *stricta.* Banks of the Orwell at Walton, common.
CYNOSURUS *cristatus.* Meadows, &c., common.
FESTUCA *duriuscula.* Meadows, occasionally.
——— *pratensis.* Meadows, common.
BROMUS *sterilis.* } Woods, common.
——— *asper.* }
——— *mollis.* Meadows, &c., common.
AVENA *pratensis.* Road- and field-sides, common.
ARUNDO *Phragmites.* Sides of the Orwell and Gipping, common.
HORDEUM *murinum.* Meadows and road-sides, common.
——— *pratense.* Marshes, common.
TRITICUM *repens.* Fields and road-sides, common.
——— *junceum.* Sandy beach at Walton and Felixtow, common.
BRACHYPODIUM *sylvaticum.* Road-sides, &c., common.
LOLIUM *perenne.* Meadows, road-sides, &c., common.

GYMNOSPERMS.

CONIFERÆ.

PINUS *sylvestris.* Woods, common.

EQUISETACEÆ.

EQUISETUM *arvense*. Meadows, by the side of the Gipping, common.
——— *palustre*. Moist and boggy places, frequently.
——— *fluviatile*. Moist meadows, occasionally.
——— *limosum*. Ditches, common.

ACROGENS, ACOTYLEDONOUS, OR CRYPTOGAMIC PLANTS.

POLYPODIACEÆ.

POLYPODIUM *vulgare*. Road-sides and woods, common.
ASPIDIUM *cristatum*. } Boggy heaths, common.
——— *spinulosum*. }
——— *aculeatum*. Road-sides, rare.
——— *angulare*. Road-sides, occasionally.
——— *Filix-mas*. Road-sides, woods, &c., common.
——— *Filix-femina*. Moist woods, local.
ASPLENIUM *Adiantum-nigrum*. Woods, local.
SCOLOPENDRIUM *vulgare*. Road-sides, local.
PTERIS *aquilina*. Heaths, very common.
BLECHNUM *boreale*. Woods, local and scarce.

OPHIOGLOSSACEÆ.

OPHIOGLOSSUM *vulgatum*. Meadows, local.

CHARACEÆ.

CHARA *vulgaris*. Boggy pools and ditches, common.

ART. III.—*On the Variation of Colour in Wild Plants.* By ARTHUR ADAMS, Esq.

VARIETIES among the plants cultivated in our gardens, where they are exposed to every unnatural influence which the ingenuity of man has invented, to divert them from their usual mode of growth, have received all the attention so wonderful and interesting a subject demanded at the hands of the cultivators of Botany. But even among those lovely productions whose only nurse is the gentle Flora, we occasionally meet with aberrations from the normal structure, departures from the laws that ordinarily govern the world of vegetables. Of these, the most numerous are alterations in the colouring of the floral envelopes, and deserve our consideration.

The causes which give rise to, and modify the production of colour in plants, and which dispose their tints to arrange themselves in a determinate manner, are for the most part still very obscure. We know that however diversified the tints of a flower may be, yet there is, in reality, no actual intermixture of colour; each hue is pure, distinct in itself, and accurately defined, although they frequently deliciously harmonize and soften into one another: take, for instance, a petal of the tulip, or the party-coloured ranunculus of our gardens, where the distinctions of tint are beautifully seen. Here we should find, on a minute examination, that their brilliant and varied hues are owing to the deposition of a colouring matter on the inside of the cellules, of which their tissue is composed. The tissue, being colourless and transparent, allows the colouring matter to shine through it, and produce the dazzling effect we witness. It appears, however, now, to be pretty well ascertained, from the researches of Macaire, that all the various colours of flowers may, for the most part, be ascribed to different degrees of oxygenation of the chromule, or colouring matter contained in the vesicles of which they are composed. Why green should be the colour chiefly confined to the foliage of plants, and various other colours to the petals, which are constructed on precisely the same plan, seems not yet determined. It is curious to observe, however, what a striking tendency various parts, in the neighbourhood of the floral leaves, have to clothe themselves also in "coats of many colours." The *calyx* of the *Fuchsia*, for instance, is of a bright scarlet, and the bracts of the *Hydrangea* are often blue. The oxygenation of which we before spoke, appears to be effected by the agency of solar light; and it may be stated as a general rule, that the brightness of colour in plants is in the direct ratio of the amount of solar light to which they are exposed. The changes of colour which the leaves of different plants assume as autumn approaches;—the red dress, for example, in which the goddess of Botany, at that period, clothes the pear, the vine, the sumach, and Virginian creeper;—has been ascribed by Macaire to the same oxydising effect already mentioned.

Schubler and Funk divide the colours of flowers into two classes, the Oxydised (Xanthic of De Candolle), and the Disoxydised (Cyanic of De Candolle). The first, or Xanthic class, has *yellow* for its type, and the flowers belonging to this series are capable of passing into red or white, but, according to those authors, never into blue. To this statement, however, there are certainly some exceptions. *Viola lutea*, for example, has been observed, both by myself and Mr. Moore, of York, with yellow and purple, or wholly purple, flowers; and *Myosotis palustris* has both yellow and blue flowers. My friend Mr. Dickson, of Jersey, has also found a specimen of *Œnothera biennis,* with eight flowers on it; six of the usual yellow colour, one purple, and the other blue. The second, or Cyanic class, has *blue* for its type, which can pass into red or white, but never into yellow, Schubler and Funk consider green as a sort of neutral colour, intermediate between these two classes. Most purple or blue flowers may have red or white varieties; rose-coloured flowers seem the next most liable to variation; while yellow rarely change: *Glaucium luteum* has been gathered, however, by Mr. Dickson, with flowers of a white colour; and several other varieties in this type of colour have already been mentioned. The cause of the non-oxygenation of white varieties is not well understood. It cannot be ascribed to the absence of solar light, as many of them are found growing in very exposed situations: nor are the subjects of this variation weakly and frail, like plants that have been submitted to the process of blanching. It cannot be owing to the same cause that produces a white colour in many animals of the northern regions, namely cold, as many of these white specimens are found in the summer, and all of those mentioned by us, were observed in the mild temperature of the British Islands. We are constrained, therefore, to assign them to certain unknown causes, which induce in the floral envelopes of plants a white colour, perhaps somewhat analogous to those which produce albinoism in the animal kingdom.

The following *white* varieties met with in wild British plants, belong to the Cyanic series of De Candolle; that is, their floral envelopes are naturally coloured blue, rose-coloured, or purple. They have been observed both by myself, and also by Mr. O. A. Moore, of York. I have thought it better to throw the plants into their natural orders:—1. Ranunculaceæ, *Ranunculus Flammula; Anemone nemorosa* (every stage, from purple to white).—2. Violaceæ, *Viola odorata* (various shades from dark blue to white).—Polygalaceæ, *Polygala vulgaris* (also pink).—4. Caryophyllaceæ, *Lychnis dioica* (red, rose-coloured, and white).—5. Geraniaceæ, *Geranium pratense; G. phæum; Erodium cicutarium.**[1]—6. Ericaceæ, *Menziesia polifolia*;* *Andromeda polifolia*;* *Erica Tetralix*; *Calluna vulgaris.*—7. Leguminaceæ, *Ononis arvensis; Trifolium pratense.*—8. Rosaceæ, *Geum rivale* (also yellow).—9. Circæaceæ, *Circæa lutetiana* (white and pale red).—10. Me-

[1] The varieties marked with an * I have not met with; they were kindly supplied by Mr. Moore.

lanthaceæ, *Colchicum autumnale* (greenish-white, elongated, and abortive).—11. Boraginaceæ, *Myosotis palustris*; *Pulmonaria officinalis.**—12. Labiaceæ, *Lamium maculatum; L. intermedium;* Betonica officinalis;* Galeopsis Tetrahit; Ballota nigra; Origanum vulgare.**—13. Apocynaceæ, *Vinca major*; *V. minor.*—14. Primulaceæ, *Primula farinosa.**—15. Convolvulaceæ, *Convolvulus arvensis.*—16. Solanaceæ, *Solanum Dulcamara; Atropa Belladonna* (gathered in Netley Abbey, by Mr. Dickson, of a dirty white colour).—17. Gentianaceæ, *Gentiana verna;* G. Pneumonanthe.**—18. Scrophulariaceæ, *Digitalis purpurea* (common in gardens, of a white colour; we have also found it in Hampshire, wild, in that state).—19. Campanulaceæ, *Campanula rotundifolia.*—20. Compositæ, *Cnicus palustris;* Centaurea Cyanus* (every shade of blue and pink, to pure white).—21. Orchidaceæ, *Orchis maculata.*—22. Araceæ, *Arum maculatum* (yellowish or purple, white, spotted, and without spots).—23. Polygonaceæ, *Polygonum Persicaria.* I shall add a few more varieties that influence the colour of wild flowers, equally interesting:—*Cratægus Oxyacantha*, naturally white, (rose-colour); *Oxalis Acetosella*, naturally lilac, (blood-red); *Scrophularia nodosa*, naturally purple, (green); *Anagallis arvensis*, naturally scarlet, (white); *Jasione montana*, naturally blue, (dark purple); *Viola lutea*, naturally yellow, (purple).

While I am on the subject of varieties in wild plants, perhaps a few remarks on those aberrations in their form and mode of growth, which I have observed, may not prove unacceptable to those who give this notice a perusal.

The development of the flowers of plants in a wild state is sometimes irregular, either on account of abortion of some of their parts, their exuberant growth, or from certain other causes not well understood. *Linaria vulgaris*, naturally a labiate flower, is reduced, for instance, in one variety, to a regular form, where the *corolla* is regular, five-cleft, with five spurs, and where there are five equal stamens. This is owing to the circumstance of two of the petals, which are usually suppressed, attaining a development equal to the other three. In *Geum rivale*, also, the axis of growth is sometimes found prolonged beyond the petals; and the flowers of *Festuca vivipara*, *Bellis perennis*, and *Polygonum viviparum*, are often proliferous. Varieties and transformations among the foliage of plants, are innumerable. *Fraxinus heterophylla* has leaves, ternate, simple, serrated, and compound; *Paris quadrifolia* has often three, five, and even six leaves in a whorl, although, as indeed is indicated by the specific name, four is the normal number. The leaves of *Ranunculus aqua-*

tilis, and *Polygonum amphibium*, vary in a remarkable manner, according as they grow in or out of the water. *Solanum Dulcamara* and *Clematis Vitalba* are very variable as regards their leaves. In *Helleborus fœtidus*, the leaflets are sometimes joined together, forming one large and undivided leaf. Spotted leaves are sometimes seen in *Hieracium sylvaticum, Hier. murorum, Arum maculatum, Lamium intermedium*, &c. Besides these, there are numerous other interesting varieties to be met with in the vegetable kingdom, an enquiry into which would amply repay the labour expended in the search after them.

ART. IV.—*Remarks on the Botanical System of Professor Perleb.* By Sir EDW. Ff. BROMHEAD, Bt., F.R.S., Lond. and Ed.

PERLEB'S 'Clavis' appeared in 1838, and must be considered a work of very great value; no Scientific History of the Higher Botany can pass over his large contributions towards the natural grouping of the families. He refers to his 'Lehrbuch,' published as early as 1826, indicating by a mark (†)[1] the additions made in the 'Clavis,' and he complains that Burmeister, in his 'Handbuch,' adopts thirty-three out of forty-four from his groups, without once naming the 'Lehrbuch' or its author. This is unquestionably true, and few writers have such just grounds for complaint against his successors, as Perleb; but Burmeister's work is a general digest of the whole of Natural History, and does not profess to adjudicate authorship, even in the families, as monographs and systems should do; neither does it servilely copy Perleb, but freely deviates, sometimes for the worse, oftener for the better. Lindley also, Endlicher, and indeed almost all writers, except Perleb himself and Meisner, omit the founders and synonymes of the Botanical Alliances; an omission partially supplied in 'Phil. Mag.,' Sept., 1837, and to which some additions are now made from Perleb and Burmeister.

In the 'Clavis' are several matters worthy of imitation. In page 8, he gives a Table of Abbreviations, used for indicating the Authors of genera, &c. Such a Table, constructed on uniform stenographic principles, and uniformly carried through a Botanical Conspectus, would add greatly to the clearness, brevity, and symmetry of the whole; it should, moreover, enable us to ascertain by suitable marks, not merely

[1] || is substituted here.

the originator of the Name, but the real Author of the assemblage, whether produced as a section or otherwise, and also the writers who afterwards correctly limited the assemblage. Striking injustice has been committed on these points, and is properly complained of by Agardh; the priority even of Name, is still extensively violated, nor can Linnæus always escape that censure, in having acted under colour of arbitrary rules, made imperative and retrospective, instead of being recommended for future adoption. The spirit of equity, and the labour of Reichenbach, in matters of Nomenclature, merit all praise.

In page 46, Perleb gives a useful Table of the Names and Synonymes of Tribes, Families, &c. To such, the rules of priority cannot so conveniently apply, the divisions being properly provisional, and it being possible hereafter to construct a Nomenclature on some fixed principle, so as to indicate the dignity of the assemblage by its termination, and its place by the name of a genus. These advantages the names of Genera cannot possess, unless some bold speculator shall hereafter reduce to uniformity, the terminations of all genera within the same family.

At page 52, there is an Index of Genera, referring (from the nature of the work), not to the page, but by number to the Family in which that genus lies. Every Conspectus should contain a double Index, one of the Tribes, with a reference to the page of the work,—the other of the Genera, each followed by the name of the Tribe in which the genus lies. This list of genera would suit every edition and every work, would avoid incredible labour and endless errata, and would directly supply, in most cases, without further search or trouble, all that the reader requires. Some additional space might be required, but small type is scarcely objectionable in cases of mere occasional reference.

Perleb uses the words 'Class' and 'Order' (terms correctly applicable to artificial systems), to indicate his *Primary Divisions*, and the *Alliances* which they contain. His Classes are neatly named and set out, but do not differ materially from those of his predecessors :—

Protophyta.—	Cellulares	Aphyllæ.	
Muscosæ.—	———	Foliosæ.	
Filicinæ.—	Vasculares	Endogenæ	Cryptogamicæ.
Ternariæ.—	———	———	Phanerogamicæ.
Monochlamydeæ.—	———	Exogenæ	Incompletæ.
Thalamanthæ.—	———	———	Monopetalæ.
Calycanthæ.—	———	———	———.
Calycopetalæ.—	———	———	Pleiopetalæ.
Thalamopetalæ.—	———	———	———.

As to the above Scheme, it is quite certain, that *Ternariæ* are out of place. They should follow *Thalamopetalæ:* The *Filicinæ* are not Endogenous, as supposed by some, and should be immediately followed by the *Gymnosperms* and *Amentaceæ; Cycadaceæ* resemble *Palms* in the mere trivial circumstance of being unigemmate.—The *Monochlamydeæ* seem to be composed of two Sections, lying widely apart, and normally distinguished by Burmeister and others as,

Apetalæ diclineæ lepidanthæ,
Apetalæ monoclineæ chromanthæ.

The succession would also be more natural on the whole, if the *Calycopetalæ* included Families with a Disk round the Ovary, whether the Disk adheres to the Calyx, or rises freely from the *Torus*; and if, moreover, the Class, so modified, stood between the two Apetalous classes, thus:—

Protophyta;
Muscosæ;
Filicinæ;
Apetalæ diclineæ lepidanthæ;
Peridiscantheæ;
Apetalæ monoclineæ chromanthæ;
Monopetalæ thalamanthæ;
Monopetalæ calycanthæ;
Thalamopetalæ;
Ternariæ;
Rhizantheæ.

This arrangement (as not consisting of two parallel series), cannot place all the Alliances in immediate natural sequence, but may present a series always either connected, or closely analogous.

Perleb's Table of *Classes* is followed by a Table of his *Orders* or *Alliances*, with their several distinctive characters; the Families are then tabulated in the same manner, with their differential characters, under their respective Alliances. The whole exhibits great precision and extensive powers of generalization, and will amply repay a careful examination; but I cannot venture here to do more than exhibit those cases, in which Perleb seems entitled to be quoted as the founder, or among the Synonymes of the Alliances. The writer, who has assembled more than half (say three out of five) of the families of an Alliance, ought to be deemed the founder, unless its distinctive aspect is lost in the crowd of spurious additions.

Such additional Synonymes as I have met with since the former publication, and some which have arisen from dividing a few of the larger alliances (chiefly the *Cellulares*), into others of less extent, may be laid hereafter before your readers.—With the view of comparison, my own Table is subjoined, embracing, as far as possible, the latest discoveries of Endlicher, Meisner, and other writers of the first order.

Race of the Algæ.

CHARALES.—The VERTICILLATÆ (vasculares acotyledoneæ caulocarpeæ) of Burmeister are *Characeæ, Equiseteæ.*

PIPERALES.—Perleb gives PIPERINÆ *Bartl.* c. add. *Pb.*, as containing *Lacistemeæ,* || *Chloranthece, Piperaceæ,* || *Saurureæ ;* || *Podostemeæ* also being mentioned in a note, as near Saurureæ: "Infloresc. cylindrico-spadicin. v. amentaceo-spicat., perig. 0 v. incompl., embr. inverso ad apicem albuminis, fol. petiolat. vaginant. v. stipulatis."

HALORAGALES. — Burmeister's CALYCOPETALÆ AQUATICÆ are *Callitrichineæ, Ceratophylleæ, Halorageæ* (*Hippuris, Trapa*).

ROSALES.—Under ROSACEÆ *Tourn.* e. e. *Pb.*, Perleb includes the great body of *Œnotherales, Myrtales,* and *Rosales,* not well arranged. He adds Calycantheæ, and omits Rhizophoreæ, Vochysieæ; among his sequences, Salicariaceæ follow Onagrarieæ; Pomaceæ follow Myrtaceæ; Rosa is included in Sanguisorbaceæ.—The ROSACEÆ § 2 (stipulate, &c.) of Burmeister are *Chrysobalaneæ-amygdaleæ, Pomaceæ, Spiræaceæ* (Suriana)*-roseæ-potentilleæ, Sanguisorbeæ.*

SAXIFRAGALES, PORTULACALES.—Under SUCCULENTÆ *Linn.* e. e. *Pb.*, Perleb includes the greater part of the *Saxifragales* and *Portulacales* loosely arranged. He omits Bruniaceæ, Fouquieraceæ, Silenaceæ; he adds Stackhousieæ, Halorageæ; among his sequences are *Crassulaceæ, Ficoideæ, Portulaceæ, Illecebreæ;* Stackhousieæ, Philadelpheæ, Galacineæ are marked (? ||).

CHENOPODIALES.—Perleb gives OLERACEÆ *Ag.* c. add. *Pb.*, as containing *Chenopodieæ,* ||*Phytolacceæ, Amarantaceæ, Polygoneæ,* Begoniaceæ.—Burmeister's OLERACEÆ are *Chenopodieæ, Phytolacceæ, Amarantaceæ,* Paronychieæ, *Scleranthea, Polygoneæ.*

POLEMONIALES.—Perleb, under his LURIDÆ, throws together (unnamed) *Cobæaceæ, Polemonieæ,* ||*Hydroleaceæ,* as "Corollâ non plicatâ, embr. recto, corollâ ad faucem non squamigerâ."

GENTIANALES-APOCYNALES.—Perleb unites these, loosely arranged, under the name PICROCHYLEÆ, omitting (probably well) Lygodysodea.

CINCHONALES-SAMBUCALES.—Perleb gives RIGIDÆ *Batsch.* c. add. *Pb.*, as containing *Stellatæ, Coffeaceæ, Cinchonaceæ, Cephalantheæ, Lonicereæ, Sambucinæ.*

CORNALES.—Perleb includes under UMBRACULARIÆ *Batsch.* c. add. *Pb.*, *Umbelliferæ*, *Araliaceæ*, *Corneæ*, *Lorantheæ*, ||Rhizophoreæ, ?||*Hamamelideæ*, ?||Alangieæ.

GERANIALES.—Under SARMENTACEÆ *Vent.* c. add. *Pb.*, Perleb gives Viniferæ, *Oxalideæ*, Lineæ, *Geraniaceæ*, *Hydrocereæ*, *Balsamineæ*.—Burmeister properly adds *Tropæoleæ*.

BRASSICALES.—The CRUCIFLORÆ of Perleb are *Cruciferæ*, *Capparideæ*, *Fumariaceæ*, *Papaveraceæ*.

NYMPHÆALES. — Perleb in adjusting the characters of MULTISILIQUOSÆ, gives the following (unnamed) as "herbæ aquaticæ, palustres:"—? || Sarraceniaceæ, *Nymphæaceæ*, *Nelumboneæ*, ||*Cabombeæ*, *Podophylleæ*. To these are added, without arrangement, the Menispermales, mixed with Ranunculaceæ, Magnoliaceæ, and Dilleniaceæ.

AGROSTIDALES.— The arrangement of the characters of Perleb's GLUMACEÆ, throws together (unnamed) *Cyperaceæ*, *Gramineæ*, " Staminibus pistillo non junctis, perigonio glumaceo, plantæ culmiferæ plque. herbaceæ, fol. alt. simpl. integerr. parallelinerv. vaginant., ovar.—1-ovulat., ovariis solitariis."—Burmeister's MONOSPERMÆ (bracteatæ glumaceæ) take the same range.

ARECALES.—Perleb adopts the PALMÆ of Linnæus, dividing them into six families. Linnæus left the Palms as a natural group, which he was unable to distribute under his artificial system; Botany would have presented a very different aspect, had he, on the contrary, delivered his artificial system as a supplement to contain plants, which he could not form into such natural groups as the Labiatæ, Cruciferæ, Umbelliferæ, Leguminosæ, &c.

TYPHALES.—Perleb includes under SPADICINÆ *Ag.* c. add. *Pb.*, ||*Phytelephanteæ*, *Pandaneæ*, *Typhaceæ*, ||*Acoroideæ*, *Cyclantheæ*, *Aroideæ*, Naiades (||Callitrichineæ, ||Hippurideæ, ||Ceratophylleæ, ||*Naiadeæ*, ||*Lemnaceæ*): " Staminibus cum pistillo non junctis, perigonio nullo vel squamæformi."—The SPADICINÆ FLUVIALES of Burmeister are *Aroideæ* (*Acorus*), *Typhoideæ*, *Potamophilæ*, *Lemnaceæ*.

Race of the Fungi.

AURICULARIALES. — Perleb adopts the HYMENOMYCETES *Fr.* (olim). Fries, in his 'Epicrisis,' 1838, removes the Elvellaceæ, under the name of Discomycetes, from this alliance, and stations them next to Pyrenomycetes. —Von Martius opens his system with the Algæ, and terminates it with the Fungi, as VEGETATIO SECUNDARIA.

LYCOPERDALES.—Perleb, for the purpose of distinguishing the characters of Fungi, places together (unnamed) the GASTEROMYCETES, *Fr.*, and PYRENOMYCETES, *Fr.*: "E peridiis globosis primum clausis, demum varie dehiscentibus, formati, sporidiis repleti."

JUNGERMANNIALES. — Perleb's MUSCOSÆ are *Entocarpæ*, ‖*Anthocerotæ*, ‖*Marchantiaceæ*, ‖*Jungermanniaceæ*, ‖*Andræeaceæ*, *Bryaceæ.* — The MUSCI of Burmeister take the same range.

LYCOPODIALES.—The MARSILEACEÆ of Brown, as adopted by Perleb, include *Salviniaceæ*, *Marsileaceæ*, ‖ *Isoeteæ.*—The FOLIOSÆ (vasc. acot. caulocarpeæ) of Burmeister are *Rhizocarpeæ (Marsilea, Salvinia-Isoetes)*, *Lycopodiaceæ*, and he refers to Bischoff.

RHAMNALES.—The RESINARIÆ *Batsch. e. e. Pb.*, shadow forth the *Rhamnales*, though much alloyed. These are Juglandeæ, *Anacardieæ*, *Pistacinæ*, *Sumachinæ*, *Spondiaceæ*, *Burseraceæ*, Connaraceæ, Amyrideæ, *Knth.*, *Chailletiaceæ*, ‖Staphylæaceæ, Celastreæ, *Rhamneæ*, Bruniaceæ, Empetreæ.

ÆSCULALES.—Perleb gives us under TRIHILATÆ *Linn. e. e. Pb.*, Erythroxyleæ, ‖Hippocrateaceæ, *Acerineæ*, *Malpighiaceæ*, *Hippocastaneæ*, ?‖*Rhizoboleæ*, *Sapindaceæ*, Tropæoleæ, ? ‖Vochysiaceæ.

HYPERICALES-LIMONIALES. — Perleb throws together the body of these under HESPERIDEÆ *Batsch.* c. add. *Pb.*: *Ternstrœmiaceæ*, ? ‖Olacineæ, *Aurantiaceæ*, *Guttiferæ*, *Hypericineæ*, ? ‖Reaumurieæ, ‖*Marcgraaviaceæ*, *Meliaceæ*, *Cedreleæ.* — Burmeister's HESPERIDEÆ § 2 (not monadelphous) are Aurantiaceæ, *Guttiferæ*, *Marcgraavieæ*, *Ternströmiaceæ*, *Hypericeæ*, Chlenaceæ.

PASSIFLORALES.—The PEPONIFERÆ of Perleb are Cucurbitaceæ, ‖*Papayaceæ*, *Passifloreæ*, ‖*Malesherbiaceæ*, ‖*Belvisiaceæ.*

ELÆAGNALES.—The LAUREOLINÆ of Perleb, are ? Myristiceæ, Laurineæ, *Thymelææ*, ‖*Penæaceæ*, *Proteaceæ*, Aquilarineæ, *Elæagneæ*, Myrobalaneæ, *Santalaceæ*, ‖*Anthoboleæ*: "Perigonio subpetaloideo." The LAUREOLÆ of Burmeister, are Myristiceæ, Laurineæ, *Proteaceæ*, *Penæaceæ*, Hernandiaceæ, *Thymelææ*, ‖*Elæagneæ*, *Santalaceæ*, *Olacineæ;* including the whole *Elæagnales*, and substantially the *Laurales* intermixed.—‖The *Chenopodiales* and *Elæagnales* form the passage to the MONOPETALÆ, through *Mirabiliaceæ* and *Olacaceæ*, and here we must ascertain the true nature of the Monopetalous Structure. It would seem, that in the progress of development, the outer envelopes assume the functions of the inner, which disappear; the Calyx becomes Corolla, and

Bracts become Calyx,—a law from which wider conclusions may be deduced.

LAMIALES.—Perleb's PYRENACEÆ, *Vent.* c. add. *Pb.*, are ‖*Stilbineæ*, ‖*Selagineæ*, *Myoporineæ*, *Verbenaceæ*, Oleineæ, Jasmineæ.—Burmeister throws these under his TETRACARPÆ, properly removing Oleineæ and Jasmineæ.

ERICALES.—Perleb's ATAXÆ are Sapoteæ, Ebenaceæ, Styraceæ, *Rhodoraceæ*, *Ericeæ*, *Pyroleæ*, *Monotropeæ*, *Epacrideæ*, *Vaccinieæ*.—Burmeister's ATAXÆ make, moreover, a sound approach to *Myrsinales*, including *Myrsineæ*, *Sapoteæ*, *Ebenaceæ*, *Ilicineæ*, *Styraceæ*, &c.

CAMPANULALES.—Perleb's RAPUNCULEÆ contain *Campanulaceæ*, *Lobeliaceæ*, ‖*Brunoniaceæ*, ‖*Scævoleæ*, ‖*Goodenovieæ*, *Stylidieæ*. Sphenoclea he omits wholly; perhaps it is allied in structure to Pentaphragma.

ASTERALES.—Perleb's SYNANTHEREÆ *Rich.* c. add. *Pb.* are ‖Calycereæ, *Cynarocephalæ*, *Discoideæ*, *Perdicieæ*, *Radiatæ*, *Lingulatæ*.

DIPSACALES.—Burmeister's AGGREGATÆ are *Valerianeæ*, *Globularineæ*, *Dipsaceæ*.

RUTALES.—The RUTARIÆ of Perleb contain Polygaleæ, ‖Tremandreæ, ‖Pittosporeæ, Brexiaceæ, *Zygophylleæ*, *Rutaceæ*, *Diosmeæ*, *Xanthoxyleæ*, *Simarubaceæ*, *Ochnaceæ*, ‖Coriarieæ.

MALVALES.—The COLUMNIFERÆ *Linn. e. e. Pb.* of Perleb are ‖*Elæocarpeæ*, *Tiliaceæ*, *Buttneriaceæ*, *Bombaceæ*, *Malvaceæ*, *Dipterocarpeæ*, ? ‖ Chlenaceæ.

ORCHIDALES-ZINGIBERALES. — The GYNANDRÆ of Burmeister include *Orchideæ* (*Vanilla*, *Cypripedium*), *Scitamineæ*, *Musaceæ*.

AMARYLLIDALES.—Perleb, in arranging his characters of LILIACEÆ, throws together (unnamed) ‖*Hypoxideæ*, *Amaryllideæ*, *Tacceæ*, *Hæmodoraceæ*, *Burmanniaceæ*, Irideæ: "Stam. cum. pist. n. junctis, perigonio simplici 6-fid. vel. 6-petalo, ovario infero, flor. hermaphr."—The leaves of Endogens seem generally of the nature of Phyllodia or dilated Petioles, such as are found in Mimosaceæ.

THE RACE OF THE ALGÆ.

[(+) Diatomeæ [8], (+)desmidieæ [8], NOSTOCACEÆ [9]; (+)batrachospermaceæ; (+hygrocrociaceæ);

Chordariaceæ, dictyotaceæ, sporochnaceæ, laminariaceæ, FUCACEÆ,
Lichinaceæ, furcellariaceæ, polyidaceæ; SPHÆROCOCCACEÆ-(thaumasieæ), halymeniaceæ;
Caulerpeæ-ULVACEÆ, vaucheriaceæ, lemaniaceæ; oscillariaceæ, confervaceæ;]
Ceramiaceæ, ectocarpaceæ; CHARACEÆ; equisetaceæ, (sigillariaceæ),
Ophioglossaceæ, danæaceæ, OSMUNDACEÆ, gleicheniaceæ, polypodiaceæ,

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Cycadaceæ, zamiaceæ, gnetaceæ, EPHEDRACEÆ, casuarinaceæ,
[Myricaceæ[8], plataneæ, MORACEÆ [8], celtideæ[8], putranjiveæ[8], antidesmeæ[8], urticaceæ[8],
Artocarpaceæ[8], cannabaceæ, (datiscaceæ), ULMACEÆ [8], scepaceæ, (+) hensloviaceæ[9],
Garryaceæ, lacistemaceæ[9], chloranthaceæ,] PIPERACEÆ, saurureæ, podostemaceæ,
Ceratophyllaceæ, hippuridaceæ [3], callitrichaceæ, HALORAGACEÆ, trapaceæ, (gunneraceæ[8]),

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Circæeæ-ŒNOTHERACEÆ-montinieæ, [lythraceæ, (+)rhizophoraceæ, vochyaceæ, combretaceæ,
Alangiaceæ, melastomaceæ, memecylaceæ [9], lecythidaceæ-barringtonieæ, MYRTACEÆ-puniceæ,]
Pyraceæ, amygdalaceæ, +chrysobalaneæ-+sanguisorbaceæ, ROSACEÆ-+potentilleæ, (+)spiræaceæ [9],
[Cunoniaceæ [9], SAXIFRAGACEÆ, (francoeæ), philadelphaceæ-aristoteleæ, escalloniaceæ, bruniaceæ,
Ribesiaceæ, cactaceæ [8], CUCURBITACEÆ, (begoniaceæ), loasaceæ,

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(Fouquieraceæ), crassulaceæ,] mesembryanthaceæ[8], PORTULACACEÆ [8], silenaceæ[9]-alsineæ,
Scleranthaceæ, +CHENOPODIACEÆ [9], +phytolaccaceæ[9], +polygonaceæ, mirabiliaceæ-salvadoreæ,
Staticeæ-plumbaginaceæ, POLEMONIACEÆ, cobæaceæ, diapensiaceæ, hydroleaceæ,
Hydrophyllaceæ, BORAGINACEÆ, heliotropiaceæ, ehretiaceæ, cordiaceæ,
[Nolanaceæ [8], SOLANACEÆ [8], (retziaceæ), convolvulaceæ [8], cuscutaceæ,]

[Voyraceæ[2], craufurdieæ, GENTIANACEÆ [6], menyanthaceæ [6], spigeliaceæ,
Loganieæ[8], strychnaceæ [8], asclepiadaceæ [8], APOCYNACEÆ [8],] lygodysodeaceæ,
Gardeniaceæ, CINCHONACEÆ, hameliaceæ, coffeaceæ, spermacocaceæ,
Operculariaceæ, loniceraceæ, SAMBUCACEÆ, adoxeæ, (galiaceæ),
Apiaceæ, (+)araliaceæ, [hamamelaceæ [9], loranthaceæ, CORNACEÆ-hedereæ, +vitaceæ,]

+GERANIACEÆ, (surianaceæ), limnanthaceæ, tropæolaceæ-balsamineæ, oxalidaceæ,[6]
[Ledocarpaceæ [9], linaceæ[9], CISTACEÆ, reaumurieæ-(tamaricaceæ),] resedaceæ, polygalaceæ,
Tremandraceæ, capparidaceæ-cleomeæ, BRASSICACEÆ, fumariaceæ, papaveraceæ,
[NYMPHÆACEÆ, nelumbiaceæ, (cephalotaceæ[6]), cabombeæ, (podophyllaceæ), pæoniaceæ [5],
Cimicifugeæ [5], clematideæ,+ranunculaceæ, sarraceniaceæ, ARISTOLOCHIACEÆ, nepenthaceæ,]

(Lemnaceæ[8]), +hydrocharaceæ, ALISMACEÆ, butomaceæ, pontederaceæ,
Commelinaceæ, (+)philydraceæ [9], xyridaceæ, eriocauloneæ-RESTIACEÆ, desvauxiaceæ,
Cyperaceæ, caricaceæ, panicaceæ, AGROSTIDACEÆ, bambusaceæ,
Calamaceæ, borassaceæ, arecaceæ, COCOACEÆ, sabalinæ[8]-phænicaceæ[8],
Cyclantheæ-pandanaceæ, TYPHACEÆ, [+naiadaceæ, triglochinaceæ, araceæ [8],]

[(Lophophytaceæ, cynomoriaceæ [8], helosiaceæ [8]), (CYTINACEÆ, rafflesiaceæ),]

THE RACE OF THE FUNGI.

[Æcidiaceæ, tuberculariaceæ, botrytaceæ-(byssi[8]), MUCORACEÆ, cephalotrichaceæ;

Tremellaceæ, clavariaceæ, AURICULARIACEÆ, hydnaceæ, polyporeæ-agaricaceæ;
Tuberaceæ, LYCOPERDACEÆ, trichodermaceæ, sclerotiaceæ; sphæriaceæ[9]; elvellaceæ[9];]
[Calyciaceæ, sphærophoreæ, USNEACEÆ[1], graphidaceæ, endocarpaceæ;]
Ricciaceæ, marchantiaceæ, JUNGERMANNIACEÆ, andræeaceæ, bryaceæ,
Salviniaceæ, marsileaceæ, isoetaceæ[8], LYCOPODIACEÆ, lepidodendraceæ[8],

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Salisburiaceæ[3], taxaceæ, CUPRESSACEÆ, pinaceæ, araucariaceæ,
Liquidambraceæ, salicaceæ, BETULACEÆ, carpineæ[2]-corylaceæ, juglandaceæ,
Anacardiaceæ-spondieæ, burseraceæ, chailletiaceæ, nitrariaceæ-(neuradeæ), RHAMNACEÆ,
[Coriariaceæ, (+)EUPHORBIACEÆ, empetraceæ-stackhousieæ, celastraceæ[9],] erythroxylaceæ,
Malpighiaceæ, aceraceæ[6], ÆSCULACEÆ, millingtonieæ-sapindaceæ, caryocaraceæ,

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+Clusiaceæ, marcgraaviaceæ, HYPERICACEÆ, (ochrantheæ)-carpodontaceæ, +camelliaceæ,
Rhodolænaceæ, [humiriaceæ, (canelleæ), meliaceæ-(hugonieæ)-cedreleæ, LIMONIACEÆ,]
Amyridaceæ, +connaraceæ, mimosaceæ-detarieæ, swartzieæ-FABACEÆ, geoffroyeæ-cæsalpiniaceæ,
Moringaceæ, [(wormskioldiæ[2]), frankeniaceæ, sauvagesiæ, parnassieæ[8], droseraceæ[8], VIOLACEÆ,]
+PASSIFLORACEÆ, malesherbieæ, turneraceæ, papayaceæ, napoleonaceæ, patrisieæ, flacourtiaceæ,

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Bixaceæ, hydnocarpaceæ, samydaceæ, HOMALIACEÆ, aquilariaceæ,
Daphnaceæ[8]-penæeæ, +proteaceæ, ELÆAGNACEÆ, santalaceæ[9]-antholeæ, olacaceæ,
Oleaceæ[9], columellieæ-gesneraceæ, pinguiculeæ, ACANTHACEÆ, bignonieæ[9],
Stilbaceæ, selaginaceæ, myoporaceæ, verbenaceæ, LAMIACEÆ-ocimoideæ,
[(+)Antirrhinaceæ[9], gratiolaceæ[9], (+veroniceæ[8]), RHINANTHACEÆ[9], orobanchaceæ,]

Monotropaceæ, pyroleæ, clethreæ[6]-+ERICACEÆ, epacridaceæ, arbuteæ[6], vacciniaceæ,
CAMPANULACEÆ-lobelieæ, stylidiaceæ, sphænocleaceæ, goodeniaceæ-scævoleæ, brunoniaceæ,
ASTERACEÆ, cichoriaceæ, mutisiaceæ, cynaraceæ,(+) arctotidaceæ[9],
[Valerianaceæ, calyceraceæ, DIPSACACEÆ, globulariaceæ,] +plantaginaceæ,
Primuleæ-(+)MYRSINACEÆ, achrasaceæ, styraceæ-diospyraceæ, ilicaceæ-brexieæ, pittosporaceæ,

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Zygophyllaceæ, RUTACEÆ, xanthoxylaceæ, simarubaceæ, ochnaceæ,
Dipterocarpaceæ, elæocarpeæ-tiliaceæ, byttneriaceæ[6], MALVACEÆ, sterculiaceæ[6],
Myristicaceæ, hernandiaceæ, illigeraceæ, cassythaceæ, LAURACEÆ,
Atherospermaceæ, monimiaceæ, calycanthaceæ, illicieæ-MAGNOLIACEÆ, dilleniaceæ,
Schizandraceæ, +anonaceæ, +berberaceæ-(nandineæ), lardizabalaceæ, MENISPERMACEÆ,

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[Smilaceæ, dioscoreaceæ, (+)CONVALLARIACEÆ[9], paridaceæ[8], asparagaceæ[8],
(+)Aloaceæ[9], +anthericaceæ[9], +JUNCACEÆ[9], +hyacinthaceæ[9],] melanthiaceæ[8],
Iridaceæ, [apostasiaceæ, cypripediaceæ, ORCHIDACEÆ, vanillaceæ,]
Globbaceæ, ZINGIBERACEÆ cannaceæ, musaceæ, [agavaceæ[8],
+AMARYLLIDACEÆ, +bromeliaceæ, +hæmodoraceæ, burmanniaceæ, (taccaceæ),]

[(Rafflesiaceæ, CYTINACEÆ), (helosiaceæ[8], cynomoriaceæ[8], lophophytaceæ),]

THE BOTANICAL FAMILIES.

Revised from Edinb. Ph J., Apr., 1836*; Phil. Mag., July,* 1837*; Ed. Ph. J., Apr. and July,* 1838.*—See also Phil. Mag., Sept.* 1837*; and Mag. Nat. Hist., Apr.* 1838.

A series of Families in immediate and continuous affinity with each other, is called an ALLIANCE, and is indicated by a termination in *ales* :—Ex. OSMUNDALES are the *Ferns*.

The Alliances of the two Races at the same distance from the initial alliance of each, are said to be *Parallel;* Parallel Alliances are called FORMATIONS, and are indicated by a termination in *osæ* :—Ex. LAMIOSÆ include *Lamiales* and *Boraginales*.

† Indicates that the Family or Tribe may be compound.

[ ] Indicates that the order of succession among the Families so included is not settled.

( ) Indicates that the evidence for the station is more conflicting than usual.

Dr. Lindley's Work on the *Natural System* is usually referred to for the Families and Tribes.

(1) USNEALES are Coniothalami, Sphærophoreæ, Hymenothalami, Idiothalami, Gasterothalami.

(2) Here separated.—(3) Link.—(4) D. Don.—(5) Arnott.—(6) Meisner (*Fasc.* i-viii.)—(7) Fries, (*Epicrisis*).

(8) Refers to Endlicher's *Genera Plantarum* (i-xii.),the terminations being sometimes changed.

(9) Aloaceæ include...... (Xerotideæ[8]) -aloinæ[8] -agapantheæ[8].

Anthericaceæ .... [Anthericeæ[8] -conanthereæ[8] -tulbaghieæ[8] -aphyllantheæ[8] -xanthorrhoeæ[8]].

(†) Antirrhinaceæ ...... [Hemimerideæ-antirrhineæ-salpiglossideæ-digitaleæ-verbasceæ.]

Arctotidaceæ .... (Xeranthemeæ)-(calendulaceæ)-arctotideæ.

Bignoniaceæ......... Cyrtandreæ-bignoniaceæ[8]-pedalieæ.[8]

Celastraceæ .... Celastraceæ-staphyleaceæ-hippocrateæ-trigonieæ.

Chenopodiaceæ...... Amarantaceæ-†chenopodiaceæ.

Convallariaceæ ...... [Aspidistreæ[8]-philesieæ[8]-roxburghieæ[8]-eriospermeæ[8]-convallarieæ[8] (excl. Smilaceis Ldl.)—ophiopogoneæ[8]-(herrerieæ[8])].

Cunoniaceæ .... Bauereæ-cunonieæ[8].

Elvellaceæ .... Discomycetes[7].

†Gratiolaceæ ...... [Buchnereæ-buddleieæ-gratioleæ[8]].

Hamamelaceæ ...... (Helwingieæ[8])-fothergilleæ-hamamelideæ.

Hensloviaceæ ...... (Forestiereæ[8])-(henslovieæ)-(batideæ).

Hyacinthaceæ ...... Gilliesieæ[8]-hyacintheæ[8]-†tulipaceæ.[8]

Juncaceæ .... Kingieæ[8]-calectasieæ[8]-junceæ[8].

Lacistemaceæ ...... (Pseudantheæ[8])-lacistemaceæ.

Ledocarpaceæ ...... Ledocarpeæ[6]-vivianieæ[6].

Linaceæ .... Lineæ-elatineæ.

Memecylaceæ ...... Memecyleæ-myrrhinieæ[5].

Nostocaceæ .... (†)Nostochineæ-rivularieæ.

Oleaceæ .... Oleaceæ-jasmineæ-bolivarieæ.[6]

Philydraceæ......... (Rapateæ[8])-philydraceæ-(flagellarieæ[8])-(astelieæ[8]).

Phytolaccaceæ ...... Phytolacceæ-petiverieæ.

Rhinanthaceæ ...... Gerardieæ-rhinantheæ.

Santalaceæ .... Nysseæ[8] -grubbieæ[8]-santalaceæ[8]-anthoboleæ.

Silenaceæ............ Caryophylleæ[8].

Sphæriaceæ .... †Pyrenomycetes.[7]

Spiræaceæ .... Quillaiæ-spiræaceæ-(neillieæ[4]).

Art. V.—*Remarks on the Theory of Spontaneous Generation.* By Mr. J. B. Bladon.

(Continued from page 286.*)*

In a satirical rhapsody published in the latter part of the seventeenth century, entitled "The History of the World of the Sun," there is such a plain, unvarnished description of the doctrine of spontaneous generation, that I cannot forbear to transcribe it. The voyager having arrived at one of the dark spots observable on the sun, meets with a personage who instructs him in several branches of knowledge, among others is the following upon creation: "Consider well the ground whereon we stand; it is not long since it was an indigested disorderly mass, a chaos of confused matter, whereof the sun had purged himself. Now, after that, by the force of its rays, which the sun darted against it, he mingled, pressed, and compacted those numerous clouds of atoms, after, I say, that by a long and powerful coction, he separated the more contrary, and reverted the more similary parts of this bowl, the mass, pierced through with heat, sweat so that it made a deluge, which covered it above forty days; for so much water required no less time to fall down into the more declining and lower regions of the globe."——"When the waters were retired, a fat and fertile mud remained upon the earth. Now, when the sun shone out, there arose a kind of tumour, or wheal, which could not, because of the cold, thrust out its bud; it therefore received another coction, and that coction still rectifying and perfecting it by a more exact mixture, it sent forth a sprout, endowed then only with vegetation, but capable of sense; but because the waters which had so long stood upon the slime had too much chilled it, the swelling broke not, so that the sun recocted it once more; and after a third digestion, that matrix being so thoroughly heated, that the cold brought forth a man, who hath retained in the liver, which is the seat of the vegetative soul, and the place of the first concoction, the power of growing; in the heart, which is the seat of activity and the place of the second concoction, the vital powers; and in the brain, which is the seat of the intellectual, and the place of the third concoction, the power of reasoning."——"Nevertheless, you'll tell me there is no man in your world engendered of mud, and produced in that manner. I believe it; your world is at present overheated; for so soon as the sun draws a sprout out of the earth, finding none of that cold humidity, or to say better, that certain period of completed motion, which obliges it to several coctions, it turns it presently to a vegetable, or if it makes two coctions, seeing the second has not time enough to receive perfection in, it only engenders an insect." We have here the real spirit of the doctrine laid bare and open to view in its greatest absurdity; and even the most plausible expositions of spontaneous generation, have no better foundation on which to take their stand; they are all obliged to have recourse to some assisting power to help them out of their difficulties, knowing that if they rest their doctrine upon the power of matter, the universal experience of all mankind would bely it, they therefore generally, and most wisely, lay it down, that only the smaller and more simply organized beings are produced by it. The difficulty, then, in refuting it, consists in the greatly enhanced difficulties of observation. When we find that persons of eminent rank in science, are at issue respecting the organization of the minute organisms;—that they cannot coincide upon what is submitted to the observation of their own eyes;—should it not induce us to hesitate before we assert that they are positively exempt from the operation of those laws which we find to be universal and absolute in every case of an organism that is fully open to our observations in every succeeding period of its organization? There are, no doubt, many anomalies in the *modus operandi* of those laws, but one circumstance is always essential, and that is, a typical predecessor. It is a most mystical occurrence, not to be accounted for in our present state of physiological knowledge, that an organism should propagate itself by bodily division, or that one impregnation should suffice for several successive generations of *Aphides;* but from some cause not yet known (probably from exposure to cold), it requires renovation, which is provided for by the last generation always containing males. In some instances mentioned in Westwood's Introduction, of female moths having fruitful descendants without sexual impregnation, the last generation were all males; still they remained true to their type; even all the arts of mankind, exerted upon the most variable species of organisms subject to his controul, with all the diversities of soil, climate, and culture that exist upon the surface of the earth, have been unable to produce a new species; the most plastic still remains true to its type: even when he forcibly combines two species, and attempts to produce a compound of them, it succeeds not beyond the primary instance, but either remains barren, or breeds back to the type of one of its parents. Do we not perceive that here is a boundary fixed? Is not here an absolute line of demarcation drawn by some law fixed by a superintending power, who has declared, thus far shalt thou go but no farther? How can we account for it by the theory of spontaneous generation, which asserts that life and organization is only a dynamic power combined with certain imponderables acting upon each other? Here we have an organism actually in existence, possessing every function essential to life, but not to reproduction of its kind. It is admitted by all the advocates of the doctrine, that when an organism is brought into existence by the power alluded to, it is then complete in its kind, and can take its station among its fellow organisms (without further aid from its spontaneous generation), to produce its typical descendants; but we see, in the case of hybridism, a direct contradiction of those assumed powers.

Art. VI.—*Dimensions and Description of a supposed new species of* Balænoptera, *stranded on Charmouth Beach, February* 5, 1840. By R. H. Sweeting, Esq.[1]

DIMENSIONS.

	Ft.	In.
Length of body	44	0
Breadth	21	0
Breadth of pectoral fin	,,	18
Length of ditto	5	6
Length of dorsal fin	,,	18
Height of ditto	1	0
From point of lower jaw to origin of pectoral fin	10	9
From posterior edge of dorsal fin, to origin of the horizontal tail	11	0
Each lobe of tail from base to point	3	0
Full spread of lobes from tip to tip	9	0
Opening of eyelids from anterior to posterior angle	,,	6
Diameter of bony socket of eye	,,	8
Diameter of eyeball	,,	7

Colour of iris, brown; pupil circular.

The spiracles were placed longitudinally, and presented the appearance of slits or fissures in the integuments of the bones of the face, nearly meeting anteriorly, and gradually

[1] The whale is the property of John Bullen, Esq., as Lord of the Manor of Marshwood, and Lord Paramount of the Hundred.

diverging posteriorly, to a distance of about 3 inches. They were about 10 inches long, communicating with the nostrils, and opening posteriorly into the pharynx.

Colour of the head, back, tail, and outside of the pectoral fins, black; inside of pectoral fins, throat, breast, and belly, beautifully white. Integuments of the three last-named parts disposed in longitudinal folds, with the intervening spaces pink. Inside of the under-jaw black; tongue, palate, &c., pink.

Sex, female.

The under-jaw is the widest, and projects 9 inches beyond the point of the upper one. No teeth in either jaw; the upper jaw is furnished with about 250 plates of whalebone (baleen) on each side. Point of each jaw rounded; the muzzle longer and much more attenuated than in the genus *Balæna;* and the plates of whalebone comparatively short, and consequently of little or no value as an article of commerce. They resemble horn rather than bone, and are tough, flexible, and elastic if bent in one direction, but brittle, and easily split in the other. Their colour blueish-black, and yellowish-white. They are fringed at the edges with loose fibres, resembling hair or bristles, which serve to entangle and prevent the escape of the marine insects, zoophytes, *Mollusca,* &c., on which the animal chiefly subsisted.

The blubber varied in thickness, from 3 to 5 inches, and yielded three hogsheads of oil.

Total weight of the animal, about 25 tons.

SKELETON.

	Ft.
Whole length from point of under-jaw to tip of tail	41
Length of head	11
Vertebral column	30

The vertebral column consists of sixty bones; namely, 7 cervical, 15 dorsal, 16 lumbar, and 22 caudal; 52 of these are strictly spinal, the 53rd doubtful, and the remaining 7 appertain to the horizontal tail. The spinal canal terminates at the 52nd; the 53rd has a deep groove, but no spinous process, the arch being completed in the recent subject by cartilage or ligament.

Each of the principal *vertebræ* consists of a body, two transverse processes, two oblique or articulating processes, and the spinous process, surmounting and completing the spinal arch, for the transmission and protection of the *medulla spinalis* or spinal marrow.

The ribs are fourteen in number on each side; the first being double-headed, is attached to the two first dorsal *vertebræ*, the rest are each attached to the posterior edge of the transverse process of the corresponding dorsal *vertebra*.

The pectoral fin, &c., is analogous to the superior extremity of the human subject, consisting of a *scapula*, with its glenoid cavity, the *os humeri*, the fore-arm, with its two bones (*radius* and *ulna*), the *carpus*, *metacarpus* of four bones and four fingers, with their *phalanges*.

The point of the underjaw projects, in the skeleton, one foot beyond that of the upper; they are both pointed, but were rounded off in the recent subject.

Two small flat bones, representing the *pelvis* in quadrupeds, were attached (one on each side) to the first caudal *vertebra*.

The intervertebral substance consists of myriads of elastic columns or pillars; an admirable and exquisitely beautiful piece of mechanism, combining flexibility, elasticity, and strength.

The seven last bones of the vertebral column are deeply impressed laterally with pits or hollows, for the attachments of the rays or fibres of the horizontal tail.

From the discrepancy as to the number of *vertebræ*, &c., I am of opinion that this species has not been described before, and I have proposed for it the name *Balænoptera tenuirostris*.

24th March, 1840.

Art. VII.—*Catalogue of the* Entozoa *indigenous to Ireland; with Observations.* By O'Bryen Bellingham, M.D., Member of the Royal College of Surgeons in Ireland, of the Natural-History Society, the Zoological, and the Geological Society of Dublin, &c., &c.[1]

One of the objects contemplated by the Natural-History Society of Dublin on its formation, was to obtain, at as early a period as circumstances permitted, a complete catalogue of the animals, vertebral as well as invertebral, which are natives of this country. With the wish to contribute as far as lies in my power to so desirable an object, I have brought forward the following catalogue of *Entozoa*, or parasitic animals (a

[1] Read at a Meeting of the Natural-History Society, June 5, 1840.

part of the invertebral kingdom which has not hitherto enjoyed much of the attention of British Zoologists) which I have met with in this country, the great majority of them being new to the British fauna; in order to render it so far a perfect list of our indigenous species, I have included a few which I have not been so fortunate as to find myself, but which have been noticed or described by others, particularly by Dr. Drummond, the President of the Belfast Natural-History Society, whose talents have already contributed to advance more than one branch of Natural History, and who has lately turned his attention to these much neglected animals, and has described some species altogether new to science.

Under the general name *Entozoa* (derived from εντος *intus*, ζωον *animal*), are included all the animals which naturally and permanently reside in the alimentary canal, or some other part of the interior of animals. And although the habitat of any animal is not a sufficient ground to separate it from the genera or species which approach it in organization, yet as the *Entozoa* have been studied and described as a separate group by those naturalists whose authority upon the subject is the highest; and as the majority of them are distinct in organization from any animals not parasitic; and as we are as yet far from having arrived at a natural arrangement of invertebral animals, (there being some groups which though not parasitic, require to be associated with the *Entozoa*, and others which are parasitic, and which many have arranged with these animals, but of which the true situation is extremely doubtful):—it appeared to me to be more prudent to retain the term in the sense used by Rudolphi and Bremser; and on the present occasion I shall confine myself altogether to the true *Entozoa*, or those species which inhabit some part of the interior of the bodies of other animals; and I shall not enter at all upon the disputed point, as to the place which these animals ought to occupy, in a natural arrangement of the invertebral kingdom.

The animals included under the term *Entozoa*, although they have been very carefully studied by several continental zoologists, and have occupied a considerable share of the attention of several distinguished comparative anatomists, have from some cause or other been little attended to, I might almost say completely overlooked by British naturalists, even by men distinguished in other departments of the science. "While there are some branches of Natural History (as Mr. Jenyns has observed in his report on Zoology) which are most sedulously cultivated by us, there are others which have for a long time lain comparatively neglected." This remark is peculiarly appropriate, and applies particularly to the animals which form the subject of the present communication; they are commonly looked upon with disgust instead of anything of interest in a scientific point of view, and the number of individuals who have made them a study is exceedingly limited. Indeed, the little attention which the *Entozoa* have attracted in these countries will be apparent from the fact, that in the only works which contain lists of the British species, viz. 'Pennant's Zoology' and Turton's 'British Fauna,' but twenty-eight species are described as indigenous; and four of these are repeated twice under different names, leaving but twenty-four distinct species: while in the limited opportunities which I have had, I have detected and preserved upwards of 200 species, and several of these occurred in six, others in ten, and one species in as many as fifteen different animals.

The *Entozoa*, although they do not form a very numerous division of the animal kingdom, are very extensively distributed, as in almost all the *Mammalia*, birds, reptiles, and fish, which I have examined, I have detected some species, and often more than one; and there is scarcely a tissue or organ in which they do not sometimes occur.

I have found them in the œsophagus, stomach, and intestines, in the bronchial tubes, and air cells of the lungs of some animals; in the urinary bladder, in the gall and swim bladder of others; in cellular tissue and in serous membranes; in the substance of the heart, in the liver and kidney; some species inhabit the brain of animals, others their eyes, others aneurismal swellings of arteries, others the *meatus auditorius*, the frontal and maxillary sinuses, and even the cavity of the *tympanum*. In fact there is hardly an organ in which some species has not been detected, at least among vertebral animals; and if they are more rare among the *Invertebrata*, it is, perhaps, because we have not yet sufficiently sought for them.

The number of animals which I have dissected in order to complete this list, is very considerable: I possess notes of having examined upwards of 270 *Mammalia*, 360 birds, and 380 fish, exclusive of reptiles which are indigenous to this country. And here I have much pleasure in acknowledging the assistance which I have received from Mr. Richard Glennon, preserver of animals to the Natural-History Society; who most obligingly placed at my disposal the body of any animal sent to him to be preserved, which I was desirous of examining; indeed, without his kind co-operation, I could not have brought this list to its present extent.

The classification to which I have adhered in the following catalogue, is the one given by Rudolphi in his 'Synopsis,' followed by Bremser in both his works, and adopted by almost all zoologists since. I cannot see the necessity for the change in the nomenclature of the orders which has been made by Mr. Owen, in his article *Entozoa*, in the 'Cyclopædia of Anatomy and Physiology.' Rudolphi's terms are in a great measure established, having been adopted by almost every writer upon the subject since his time; and if the names of families or orders are to be altered upon trivial grounds, we should be under the necessity of giving up many of the names which have been longest established; indeed nothing appears to have a greater tendency to retard the study of Natural History, than the unnecessary multiplication of names which are already too numerous. But in the present instance, I do not think the new names are an improvement upon the old, the latter appear to me to be more expressive, and their having been adopted by the best practical helminthologists is a sufficient reason for retaining them here.

Rudolphi has arranged the *Entozoa* in five orders of families, each of which includes a larger or smaller number of genera. On the present occasion I shall only notice those genera in which I have as yet detected species inhabiting animals natives of this country.

Commencing with those most highly organized, they are *Nematoidea, Acanthocephala, Trematoda, Cestoidea,* and *Cystica.*

Order I. NEMATOIDEA.

(Derived from νημα *filum*, and ειδος *forma*.)

The order *Nematoidea* includes the *Entozoa* whose organization is the highest; the body is cylindrical and elastic, more or less attenuated at each extremity; intestinal canal complete, provided with a mouth and anus; sexes distinct; commonly oviparous, rarely viviparous. The head is continuous with the body, very rarely separated by a neck, often obtuse and sometimes edged by lateral membranes (what Rudolphi has called winged). The posterior extremity of the body is either sharp or obtuse, often curved. The male is almost always smaller than the female, and the *penis*, which is either a single or double *spiculum*, frequently projects externally. In both sexes the internal organs of generation (the ovaries in the female and the seminal tubes in the male), are in the form of long filaments, which surround the intestinal canal. The generic characters are taken principally from the shape of the mouth, or from the disposition of the tubercles which surround it; the absence or presence of lips, &c.

The genera in this order are found in all classes of animals; they inhabit almost every organ, but most frequently the alimentary canal.

Genus I.—*Filaria.*

(Derived from *Filum*, a thread.)

Body long, cylindrical, and elastic, nearly of equal diameter throughout; mouth orbicular. Male organ a simple *spiculum.*

This genus was established by Müller, and has been adopted by Rudolphi; the species inhabit cellular membrane in every part of the body, very rarely the alimentary canal; they are not uncommon in *Mammalia*, birds, and fish, they are less common in reptiles; they occur also in invertebral animals, particularly in the larva of *Lepidoptera*, and in some *Coleoptera.*

The *Filaria* have been arranged by Rudolphi in two subdivisions, according as the mouth is simple or papillary and labiate.

FILARIA.

Ore simplici.

1. Filaria *attenuata*......... Cellular membrane in abdomen of peregrine falcon.

Species dubiæ.

2. Filaria A.................. *Peritonæum* of red gurnard.
3. „ B.................. *Peritonæum* of mullet (*Mugil capito.*)
4. „ C ?............... Abdominal cavity of bee (*Bombus terrestris.*)

A. This species of *Filaria* (which does not appear to have been described) occurred under the *peritonæum* of the common red gurnard (*Trigla pini.*) The specimens which I possess are from three to four inches in length, and about the thickness of strong thread, the colour white, body cylindrical, and of the same diameter throughout. Anterior extremity obtuse and rounded, posterior acute. Mouth orbicular and very small. In removing them, some ruptured, and allowed the ovaries and intestinal canal to protrude.

B. This species (which also appears not to have been described), occurred in the *peritonæum* of the common grey mullet (*Mugil capito*): they were so imbedded in this membrane that it required considerable trouble to remove them, and some portion of the membrane continued to adhere to them, which rendered their examination difficult; they are about 4½

Genus 2.—*Trichosoma.*

(Derived from τϱιξ, *capillus.*)

Body cylindrical and elastic, of moderate length, very slender towards the anterior extremity, and insensibly enlarging posteriorly. Mouth terminal, punctiform. Male organ a simple filament contained in a sheath.

This genus was established by Zeder, under the name of *Capillaria.* The species are most common in birds, next in the *Mammalia;* they are very rare in reptiles and fish; they inhabit the stomach, the small and large intestines, sometimes the urinary bladder. The species of the genus *Trichosoma* are all exceedingly small, and resemble each other very closely (almost the only difference being a little greater or less length or thickness of the body), and as male and female are not always found together, it is very difficult to determine the species accurately; in fact, of the twenty-two species which Rudolphi has enumerated, sixteen are doubtful; and of thirteen species which I have met with nine are doubtful.

TRICHOSOMA.

1.	Trichosoma *obtusum?*	*Cæca* of horned owl.
2.	„ *inflexum?*	Small intestine of missel thrush.
3.	„ *longicolle?*	Small intestine of domestic fowl.
4.	„ *plica*	Urinary bladder of dog. Urinary bladder of fox.

Species dubiæ.

5.	Trichosoma	Urinary bladder of wild cat.
6.	„	Small intestines of weasel.

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lines in length, colour white, body slender, and of the same diameter throughout. Anterior and posterior extremity rounded, mouth obscurely orbicular; a slight prominence near the posterior extremity, at which the *anus* appeared to open.

C. This species I have met with upon several occasions in the cavity of the abdomen of the common humble bee (*Bombus terrestris*), and sometimes in very large numbers; they lived and moved about in a watch glass containing water, for a considerable time; they are cylindrical, some are smaller than others, and in these the posterior extremity is very slightly curved, in the larger (which probably are females), this part is straight. They are so small as to be hardly visible without a lens, and in consequence of this I have not been able to succeed in seeing the shape of the mouth; hence they may probably eventually turn out to belong to some other genus.

7. TRICHOSOMA	D.	Urinary bladder of rat.
8.	„	Small intestine of rat.
9.	„	Stomach of hedge-hog.
10.	„	Small intestine of horned owl.
11.	„	Small intestine of pigeon.
12.	„	*Cæca* and *rectum* of jackdaw.
13.	„ E.	Intestines of hake.

Genus 3.—*TRICHOCEPHALUS.*

(Derived from τρὶξ, *capillus*, and κεφαλη, *caput*.

Body cylindrical and elastic, anterior portion capillary and suddenly passing into the thicker or posterior part (by this it is distinguished from the genus *Trichosoma*, in which the increase is very gradual); mouth orbicular; *penis* simple, contained in a sheath.

The name *Trichocephalus* was given to this genus by Goetze, and adopted by Rudolphi. It was formerly named *Trichuris* (from τριξ, *capillus*, and ουρα, *cauda*), from the erroneous supposition that the capillary portion was the tail. The species inhabit the large intestines, particularly the *cæcum* of the *Mammalia;* they do not occur in either birds or

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D. This species of *Trichosoma* I have very frequently found in the urinary bladder of the common Norway rat; in some cases only one or two occurred, in others, six, eight, or upwards. Many were free in the bladder; others so firmly attached by their anterior extremity to the mucous membrane, that they broke across when pulled; and some even remained adherent, after having been placed in spirits of wine. They are the largest species of *Trichosoma* which I have seen, the posterior division of the body in some being so thick as in a certain degree to resemble this part in the *Trichocephalus*, from which, however, they are readily distinguished, the increase in diameter being gradual, and not sudden. They are about 8 lines in length, the body white and cylindrical, the posterior extremity rounded; in the thicker part of the body, the alimentary canal appears to be somewhat spiral, and is surrounded by the convoluted ovaries. All the specimens which I have appear to be females.

This species has not, I believe, hitherto been described, although it is very common, and I have frequently met with it. It occurs in the urinary bladder, both of the male and female rat, and is quite distinct from the species which inhabits the small intestine of the same animal.

From the thickness of the posterior part of the body compared with other species of *Trichosoma*, I would venture to suggest for this species the name *Trichosoma crassicauda*.

E. The genus *Trichosoma* is very rare in fish; Rudolphi or Bremser have never found them; the only species which has been as yet described to occur in these animals, is one noticed by Creplin, in his 'Observationes de Entozois.' This species I found in the intestines of the hake (*Merluccius vulgaris*); the longest specimen is upwards of an inch in length, the body slender, colour perfectly white. As there cannot be a doubt that it is a new species, I would venture to suggest the name *Trichosoma gracilis* for it.

fish. The genus is not numerous in species. Rudolphi has arranged them in two subdivisions ; our native species belong only to one of these subdivisions.

TRICHOCEPHALUS.

1.	TRICHOCEPHALUS	*dispar* F	Large intestines of man.
2.	,,	*crenatus* G	*Cæcum* of pig.
3.	,,	*nodosus*	*Cæcum* of mouse.

Genus 4.—*OXYURUS.*

(Derived from οξυς *acutus*, and ουρα *cauda.*)

Body cylindrical and elastic, the posterior extremity subulate in the female; mouth orbicular and terminal; male organ contained in a sheath.

The genus *Oxyurus* was established by Rudolphi ; it contains a very small number of species, only three being enumerated by Rudolphi in his 'Synopsis.' The species inhabit the large intestines of the *Mammalia*, and have not been found in either birds, reptiles, or fish.

OXYURUS.

1.	OXYURUS	*curvula* H	Large intestine of horse.
2.	,,	*ambigua* I	*Cæcum* of rabbit.

F. The *Trichocephalus dispar* has been longer known than any other species in the genus ; and although stated by several writers to be rare in these countries, in the course of my experience I have found it by far the most common species inhabiting the alimentary canal of the human subject : I have examined the intestinal canal of eighty-six individuals, who died in St. Vincent's Hospital, Dublin, of various diseases, and whose ages varied from three years to seventy; and in seventy-seven out of the eighty-six, I found a larger or smaller number of this species ; sometimes only one or two existed, sometimes they were in considerable plenty; the largest number I have met with in the same individual, was one hundred and nineteen : sometimes they were attached to the mucous membrane, more commonly they were free ; they almost always inhabited the large intestine, particularly the *cæcum* ; I have, however, met with them in the small intestine. The male appears to be as common, or perhaps more common, than the female, which is contrary to what happens in most other genera of *Nematoidea*.

G. This species resembles pretty closely the *Trichocephalus dispar ;* the capillary portion of the animal is, however, a little longer, and the sheath of the *penis* has a somewhat different shape from what it has in that species.

H. The *Oxyurus curvula* is noticed in Pennant's British Zoology, under the name of *Trichocephalus equi ;* it is the largest species of the genus, inhabiting the great intestine of the horse, and is not at all uncommon.

I. I have found the *Oxyurus ambigua* in great numbers in the *cæcum* of the common wild rabbit; the females are double the length of the

Genus 5.—*Cucullanus.*

(Derived from *Cucullus*, a hood.)

Body cylindrical and elastic, obtuse anteriorly, more attenuated posteriorly; mouth orbicular; head provided with a striated *cucullus* (from which circumstance the genus has been named); *anus* terminal; orifice of female organs a little behind the centre of the body; male organ a double *spiculum;* all the species viviparous.

The name *Cucullus* was given to this genus by Müller; the species inhabit the alimentary canal of fish, and do not occur in either *Mammalia* or birds.

CUCULLANUS.

1. Cucullanus *elegans* K......	Stomach, intestines and pyloric appendages of perch. Intestines of eel (*Anguilla acutirostris.*)
2. „ *foveolatus* L......	Intestines of plaice. Intestines of dab (*Platissa Limanda.*)

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males, and much more numerous. The mouth is orbicular, and every small; on each side of the head is a transparent membrane, somewhat resembling that of the *Ascaris vermicularis.*

K. The *Cucullanus elegans* is very common in the perch, occurring in the stomach, intestines, and pyloric appendages. The species is viviparous, as when some of the females have been accidentally cut across, I have seen the young come out in great numbers, and move about freely in water placed in a watch-glass.

The *Cucullanus elegans* is more rarely met with in the eel than in the perch; sometimes they were free in the intestinal canal, at others, attached to the mucous membrane, almost as firmly as *Echinorhynchi.* They were of a reddish colour, the head a deeper red than the body; after remaining in water for some time, every part became white except the head.

The female *Cucullanus elegans* from the eel is about 4½ lines in length, the male about 2 lines. The head is rounded, and is pretty accurately represented in the magnified figure in Bremser's large work. The *striæ* upon the *cucullus* are numerous and run longitudinally; the mouth is orbicular, in some a small *papilla* projected; the *œsophagus* is narrow at its commencement, and runs in a straight line to the stomach, which is somewhat oblong and double the diameter of the *œsophagus;* the intestine is straight, the *anus* is situated near the posterior extremity. The *vulva* in the female is conspicuous, projecting, seated nearer the caudal than the anterior extremity of the body; the *penis* of the male (in the specimens which I met with), did not project externally. In the female, the caudal extremity is rather sharp, not obtuse as Rudolphi describes it to be; it is straight in the female, inflexed in the male.

L. The *Cucullanus foveolatus* from the plaice, in some instances, adhered firmly to the mucous membrane of the intestine; very soon after being placed in water, the integuments ruptured and allowed the ovaries, &c. to protrude. I have not observed that this species is viviparous.

Art. VIII.—*On a recent species of* Crinoidea, *or* Encrinites, *constituting the type of a new genus;*—Holopus.[1]

M. Alcide D'Orbigny, in a paper read before the 'Académie des Sciences,' at their sitting of February 27th, 1837, has described a new genus of radiated animals, so interesting both to the zoologist and geologist, that we have been tempted to extract the descriptive portion of his paper from M. Guérin's 'Magasin de Zoologie' (where it is published), and to have his plate engraved for our 'Illustrations.'

The animal in question belongs to that section of the *Radiata*, the species of which being destitute of the means of locomotion, remain fixed to the ground; and is allied to the *Pentacrinus europæus*, first described by Mr. Thompson:[2] but differing in certain particulars from that species, M. D'Orbigny has been induced to constitute it a new genus, to which he applies the name *Holopus.* The species is dedicated to M. Rang, who discovered it at Martinico.

Holopus Rangii, D'Orbigny. Suppl. Pl. xvii.

Description.—*External parts.*—Root enlarged, not ramose, smooth, or but slightly marked beneath with intersecting lines, which are irregular on its margins, and assume on the under parts the form of the body on which the animal is fixed; foot or body thick, short, subquadrangular, covered with small rounded tubercles, which are most distinct on its angles; its surface, and that of all the external portions of the arms (as viewed under a lens), exhibiting on all parts a finely striated or reticulated tissue, even on the tubercles. Arms, four

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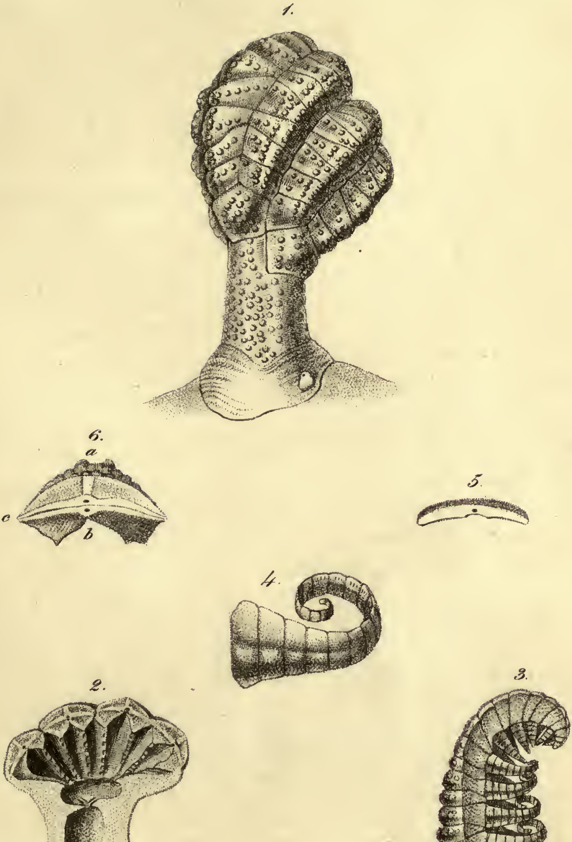
The *Cucullanus foveolatus* from the dab is perfectly white, of equal diameter throughout, except at the extremities. The females are from 6 to 6½ lines in length, the males rather less; the anterior extremity is obtuse, the posterior acute in both sexes; the caudal extremity in the female is straight, incurved in the male. The *æsophagus* is longer and wider than the stomach, it contracts suddenly where it joins the latter organ, the stomach is cylindrical, the *anus* in the female is close to the caudal extremity, in the male it is a little more anterior, and projects considerably; the orifice by which the *penis* protrudes, is seated between the *anus* and the caudal extremity. The *penis* is a double *spiculum*, very sharp, fine and white; the *vulva* in the female is situated nearer the caudal than the anterior extremity. Several of the females, after remaining for a short time in water (in which they at first moved about but soon died), ruptured, and the intestine and ovaries protruded.

[1] Extracted from an article in Guérin's 'Magasin de Zoologie,' for 1837.

[2] 'A Memoir on the *Pentacrinus europæus*,' 4to. Cork, 1827.



Holopus Rangii.

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in number, each formed at its base of a thick pentagonal plate, which is concave and irregular on the inner side, convex, and externally forming a thick, nipple-like tubercle, the margins of which, flattened at their junction with the three other similar pieces, are united in a compact manner. On the upper portion of the first piece, which has two angles, each arm becomes dichotomous; there are therefore eight arms, which are thick, strong, conical, almost twice as long as the foot, rounded and tuberculated on their mesial portion, and as it were festooned on their outer margins, alternate, and compressed at their extremity, composed of calcareous pieces, which are thick, and twenty-five in number, each piece bearing alternately right and left, a conical ramule, which is elongated, much compressed, rugose externally, somewhat concave internally, and formed of many quadrangular pieces joined together.

Internal parts.—The cavity which occupies the whole extent of the foot or body, no doubt contained the *viscera.*[1]—Mouth (as well as the *anus*) protected by four moveable, angular, stony pieces, which close the opening at the will of the animal; it opens into a vestibule, which is dilated in the upper portion of the body, separated by some ciliated and irregular excrescences at the base of the arm from a large funnel, formed of four deep grooves, each dividing into two; these are continued, though less distinctly, throughout the whole extent of the inner side of the arm.

Colouring.—The general tint of the dried animal is greenish, almost black on the body, and paler on the arms and root.

Dimensions.—The specimen described measured eight *centimètres* (three inches and two lines English measure) in total length: the foot is $10\frac{1}{2}$ lines; height of the root, $4\frac{2}{3}$ lines; diameter of root at the base, $8\frac{1}{2}$ lines; diameter of the foot, $6\frac{1}{5}$ lines.

[1] As the animal came into our possession in a dried state, it was impossible to study the conformation of these parts.

ART. IX.—*Descriptions of some new species of Carabideous Insects, from the collection made by C. Darwin, Esq., in the Southern parts of S. America.* By G. R. WATERHOUSE, Esq., Curator and Assistant Secretary to the Zoological Society.

THE insects I am about to describe belong to that great group of *Carabidæ* to which Dejean applies the name *Féroniens*, and to a section of that group, the species of which are distinguished by there being a kind of peduncle separating the *thorax* from the body,—giving to them a superficial resemblance to the *Scaritidæ*. This constricted portion between the *thorax* and *abdomen* is formed above by a produced portion of the *elytra*, which become suddenly narrow in front, and form, together with a portion of the *mesothorax*, a cylindrical neck, which apparently serves to give greater freedom of motion to the fore part of the insect. *Broscus cephalotes* affords an example of this group in which *Miscodera*, *Stomis*, *Cnemacanthus*[1] of Gray (which is the *Promecoderus* of Dejean), and *Baripus*, may also be included. These insects are most of them remarkable for the convex form of the *thorax* and *elytra*, the want of produced posterior angles to the latter, which is usually almost destitute of posterior *foveæ*, and dilated lateral margins—the dorsal channel moreover is generally very indistinct. In these respects the present group of insects affords a strong contrast to the more typical *Feroniæ*, if we may regard the species belonging to the genera *Pterostichus* and *Omaseus* as such.

In addition to the several genera (allied as it appears to me to *Broscus*) already mentioned, we are indebted to Mr. Curtis for the knowledge of three others possessing the same essential characters. I allude to the genera *Odontoscelis*, *Cardiophthalmus* and *Cascellius*, published by Mr. Curtis in the Linnean Transactions.[2]

The genus *Odontoscelis*, Curtis, is founded upon an insect brought by Capt. King from Valparaiso,—an insect which I have observed in many of our collections. In the 'Histoire Naturelle des Insectes' by MM. Audouin and Brullé[3], a second species of the same genus is described and figured as an

[1] *Cnemacanthus gibbosus* of Gray appears to me the same as the *Promecoderus brunnicornis* of Dejean, which is from Van Dieman's Land, and perhaps some other parts of Australasia, and *not* from Africa as has been stated. The genus *Cnemacanthus* of Guérin and Brullé being synonymous with Mr. Curtis' genus *Odontoscelis*, the species of which are from South America, must not be confounded with *Cnemacanthus* of Gray.

[2] Vol. xviii. part. 2.

[3] Tome iv. bis 2nd part, page 376, plate 15, fig. 4.

illustration of Mr. Gray's genus *Cnemacanthus*, whilst in the same work will be found a true species of *Cnemacanthus* described and figured under the name *Promecoderus Lottini.*[1]

M. Guerin-Méneville, in the 'Magasin de Zoologie,'[2] also figures and describes certain species of *Odontoscelis* under the name of *Cnemacanthus*, but this author perceives that the species of *Cnemacanthus* of the French authors differ in certain respects from the type of the genus figured in Griffiths' 'Animal Kingdom,' inasmuch as they have the anterior *tibiæ* prolonged externally; he states however that a small species which is found in Peru has not this external prolongation of the *tibia*, "et vient par conséquent se ranger exactement à côté du type de M. Gray. Nous ne pensons pas que cette légère différence soit suffisante pour motiver l'établissement d'un nouveau genre; nous nous en servirons pour diviser les *Cnémacanthes* en deux sections, ainsi qu'il suit." The author then proposes to distinguish those species which have the anterior *tibiæ* produced externally, by the name of *Cnemalobus*, retaining *Cnemacanthus* for Mr. Gray's species, and one other which he names *Cnem. parallelus*.

Now to those who are engaged in the study of the geographical distribution of species, it is most important to know what genera there are, species of which are found both in Australia and South America; it is highly desirable therefore that the difference in the structure of the anterior *tibiæ* of *Cnemacanthus* proper and *Odontoscelis* should be attended to, and that the *Cnemacanthus parallelus* be submitted to further examination, for Mr. Gray's genus does *not* differ *only* from *Odontoscelis* in not having the *tibia* produced externally —there are other very important differences—one of them indeed has been considered so important by Dejean,[3] that he established two great groups which are distinguishable by it, and alluded to the *tarsi* of the intermediate pair of legs being dilated in the male sex, as well as the anterior pair; such is the case in *Cnemacanthus* proper, whereas in *Odontoscelis* only the anterior pair are dilated in the males: again, in *Cnemacanthus* the tooth in the notch of the *mentum* is short, broad, and truncated, whilst in *Odontoscelis* it is long and pointed.

[1] Id. page 450, plate 18, fig. 4.

[2] Année 1838, liv. 2, pp. 9—13, plates 226 and 227.

[3] This author places his genus *Promecoderus* (which, as before stated, is synonymous with *Cnemacanthus* of Gray) in his section '*Harpaliens.*'

Genus.—*Odontoscelis*, Curtis. *Cnemacanthus*, Audouin and Brullé. *Cnemacanthus* (sub-genus *Cnemalobus*) Guerin-Méneville.

Sp. 1. *Odon. Tentyrioïdes*, Curtis, Linn. Trans. vol. xviii. part 2, page 187, plate 15, fig. D.

Cnemacanthus obscurus (?), Brullé, Hist. Nat. des Insectes, tome iv. (bis), 2de livraison, page 377.

Two specimens agreeing in all respects (excepting in having a blueish tint on the under side of the body and legs) with Mr. Curtis's description, are in the collection of Mr. Darwin; they are from Valparaiso, the same locality as that given by Mr. Curtis.

Sp. 2. *Odon. cyaneus.*

Cnemacanthus cyaneus, Brullé, as above, page 376.

Much larger than the last and of a blue colour: its length is 10 lines (French measure), whilst *Odon. Tentyrioïdes* is about 8 lines. Inhabits Chili.

Sp. 3. *Odon. Desmarestii.*

Cnemacanthus (Cnemalobus) Desmarestii, Guerin-Méneville, Magasin de Zoologie, Année 1838, livraison 2me, page 9, plate 226 of class ix.

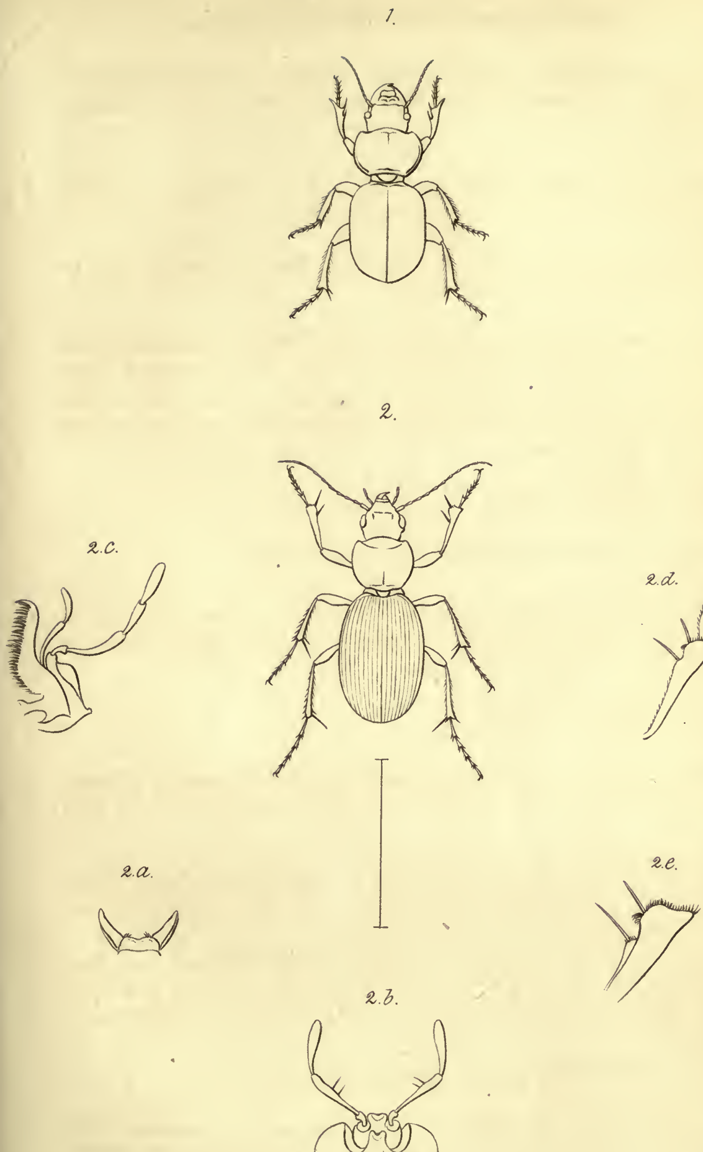
A very large species, from 26 to 30 '*millemètres*' in length. It is black above, tinted with green on the margins of the *thorax* and *elytra*. Inhabits Cordova.

These are all the species of *Odontoscelis* which I can find described; in the collection of Mr. Darwin there are four others, the characters of which I shall proceed to point out.

Sp. 4. *Odon. Darwinii.* Suppl. Plate xix. fig. 1.

Odon. latus, suprà lævis, nitidè viridis; pedibus corporeque subtùs violaceo-nigris; antennis, mandibulis, palpis, tarsisque piceo colore obscurè tinctis.

This species is from $10\frac{1}{2}$ to $11\frac{1}{2}$ lines in length, and from $4\frac{3}{4}$ to $4\frac{1}{3}$ lines in width. The upper parts of head, *thorax*, and *abdomen* are of a brilliant blue-green colour, the under parts are black, and the legs, *antennæ*, mandibles and *palpi*



1. *Odontoscelis Darwinii*
2. *Cardiophthalmus longitarsis.*

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are pitchy black; a slight blueish tint is observable in certain lights on the under parts of the body, and on the under side of the *prothorax* there is a faint green hue: a few long hairs spring from the sides of the *thorax* and *elytra*, and also from the fore part of the head and above the eyes. The *thorax* is rather narrower than the *elytra;* its upper surface is convex, the sides are rounded, and so are the posterior angles, the hinder margin is slightly indented in the middle and near the posterior angles: the breadth of the *thorax* is about one third greater than its length; the dorsal channel is very indistinct, and there is a faint, posterior, transverse depression; along the outer margins are seven or eight large punctures. The *scutellum* is large and almost semicircular. The *elytra* are very convex and almost smooth; their length is less than one third greater than their breadth; on each *elytron* is a row of punctures, which commences at the shoulder, and ends nearly at the apex of the *elytron;* excepting near the shoulder (where the punctures are close to the outer margin) this row runs parallel with and at a short distance from the outer margin of the *elytron*, and on the margin itself a second row of punctures is observable; these are rather widely separated, and each puncture gives root to a long hair; a few punctures, also giving root to long hairs, are also observable on the shoulder, and again towards the apex of the *elytra;* these are situated above the first-described row.—There are no *striæ* on the *elytra*. On each of the abdominal segments is a transverse row of punctures, each having a hair like those on the outer margins of the *elytra*. The legs are rather densely clothed with short bristly hairs, especially on the *tibiæ* of the posterior and middle pair: on the outer margin of the anterior *tibiæ* there are no hairs; these *tibiæ* are much dilated at the apex, and the outer portion is produced and pointed; in the male it terminates nearly in a line with the apex of the basal joint of the *tarsus*, whilst in the female, where it is larger, it terminates opposite (or nearly so) to the base of the fourth joint of the *tarsus:* a row of punctures is observable on the under side of all the *femora*. Length of *thorax* in the female *very* nearly 3 lines; width of ditto $4\frac{1}{3}$ lines: length of *elytra* $6\frac{1}{2}$ lines; width of ditto $4\frac{3}{4}$ lines in the female.

This species was obtained by Mr. Darwin at Bahia Blanca, North Patagonia.

Sp. 5. *Odon. Curtisii.*

Odon. latissimus, suprà lævis, niger; subtùs violaceo-niger: antennis, mandibulis, palpis, pedibusque piceo-nigris; thorace elytrisque apud marginem submetallicè relucentibus, illo subviridi, his colore purpurascente.

This species is about equal in size to the last; and, like that, has a row of widely separated punctures near the lateral margins of the *thorax;* the punctures on the sides of the *elytra*, segments of the *abdomen* and *femora*, are the same; it differs however in being rather shorter, broader, rather less convex, and in having the upper parts of the body black and almost destitute of gloss. The *thorax* is rather convex, much broader than long, and equal in width to the *elytra*, or very nearly so; the sides and posterior angles are rounded, and the dorsal channel very indistinct; behind is a slight transverse indentation on either side, and extending nearly to the mesial line. *Elytra* rather convex, about one fourth longer than broad; smooth. Legs pitch-coloured; *antennæ, palpi*, and *tarsi* pitchy red. In one female there is a transverse row of punctures on the apical portion of the last segment of the *abdomen;* in a second about four oblong indentations are observable. A very indistinct greenish tint is observable near the margins of the *thorax* and outer margin of the *elytra*, and there is a faint blue tint on the under parts of the insect.—Total length, 11 lines; length of *thorax* 3 lines; width of ditto $4\frac{2}{3}$ lines; length of *elytra* $6\frac{1}{4}$ lines; width of ditto $4\frac{3}{4}$ lines. The specimens described are females.

Brought from Port Desire, Patagonia, by Mr. Darwin.

Sp. 6. *Odon. striatus.*

Odon. mediocritèr latus, niger, subtùs levitèr cæruleo-viridi tinctus; elytris distinctè striatis; striis haùd punctatis, interspatiis paululùm convexis; elytris ad marginem externum aliquantò violaceis.

Much less than either of the preceding, but larger than *Zabrus obesus.* General colour black, the under parts of the body (especially the *thorax*) tinted with greenish; sides of *elytra* and *thorax* tinted with purple or greenish. *Thorax* convex, about equal in width to the *elytra*, broader than long; the sides and posterior angles much rounded, the anterior portion emarginated; dorsal channel indistinct; posterior transverse depression scarcely visible. *Elytra* convex, their breadth about equal to two thirds of their length; distinctly striated, the *striæ* impunctate, and the interspaces convex. On the lateral margins of the *thorax* are six or seven large punctures, and close to the outer margin of the *elytra* is a longitudinal series of punctures; besides these there are two or three larger punctures near the apex of the *elytra:* a few large punctures are observable on the under side of the *prothorax*, and on each of the abdominal segments is a transverse row of punctures; these punctures however are not found on and near the mesial line: at the apex of the last segment there are from two to four punctures, on either side, and placed in a line: numerous hairs, springing from the punctures, are observable on the sides of the *thorax* and *elytra*. Length, 8¼ lines; width, 3½ lines.

In one of Mr. Darwin's specimens the *striæ* of the *elytra* are rather less distinct than in two others from which the above description is drawn up, and the interstices are flat on the fore portion of the *elytra*;—it is moreover of a smaller size, being only 7¼ lines in length.

Mr. Darwin found this species on a sandy plain at Bahia Blanca, N. Patagonia.

Sp. 7. *Odon. substriatus.*

Odon. ater, corpore breviusculo, convexo; elytris substriatis, striis indistinctis, interspatiis levitèr convexis.

Thorax rather narrower than the *elytra*, convex; the sides rounded, posterior angles also rounded, but very slightly prominent; dorsal channel indistinct; scarcely any trace of posterior transverse impression; *elytra* convex, their width equal to more than two thirds of the length; faintly striated, the interspaces slightly concave. Colour dull black; under parts with a blueish tint. The puncturing on the sides of the *thorax*, *elytra*, segments of *abdomen*, and *femora*, as usual. The hairs on the sides of *thorax* and *elytra* numerous.—Length from 6¾ to 7⅓ lines; width from 2¾ to 3 lines.

This species is considerably smaller than either of the preceding; it is proportionately shorter than *Odon. Tentyrioïdes*, there is less space between the *thorax* and *abdomen*, and it is moreover distinguished by its striated *elytra*. *Odon. Tentyrioïdes* is smooth, or *very* nearly so; *Odon. substriatus* is rather delicately striated, and *Odon. striatus* is distinctly striated. *Odon. substriatus* is also intermediate in form between the other two species mentioned, being shorter and rather more convex than the first, and less convex than the last, from which it moreover differs in being narrower.

Genus.—*Cardiophthalmus*, Curtis.

Closely allied, as it appears to me, to *Odontoscelis*, is the genus above named,—a genus (with *too* long a name) established by Mr. Curtis, upon an insect brought from Port Famine by Capt. King.[1] I had long since determined to found

[1] Linn. Trans. xviii. part 2, page 184.

a genus upon two species of *Carabidæ* forming part of a collection placed in my hands for description by Mr. Darwin; but upon studying Mr. Curtis's paper, I found that they possessed all the essential characters of his genus *Cardiophthalmus.* The chief differences between the insects of this genus and those of *Odontoscelis*, consist in the central tooth of the *mentum* being bifid; the legs and *antennæ* much longer; the great length of the spines with which all the *tibiæ* are furnished at their apex, and the proportionately narrower *thorax*, combined with the short, ovate and convex form of the body.—The anterior *tibiæ* are somewhat suddenly dilated at the apex (in the two species before me), and the external portion is slightly produced. The anterior *femora* in the species described by Mr. Curtis, is furnished with three or four small angular projections on the under side and near the base. I find the same character in one of the specimens before me, but in two other specimens (one of which is decidedly the same species as the first) there is merely a slight unevenness on that part of the *femur*,—*possibly* therefore it may be a sexual character: in the structure of the anterior *tarsi* there is *no* difference—they are all slender, and destitute of velvet-like pads or membranous appendages beneath.

Sp. 1. *Card. Clivinoides*, Curtis, Linn. Trans. xviii. part 2 page 185, plate xv. fig. C.

Sp. 2. *Card. longitarsis.* Suppl. Plate xix. fig. 2.

Card. nitidè niger, obscurè viridi tinctus; thorace, elytrisque convexis: elytris lævissimè striatis.

Thorax elytris angustior, lateribus rotundatis sic et marginibus, at leviter, posticè aliquantò attenuatus; sulco dorsali obscurissimo et post hunc notâ transversâ insculptus. Elytra ad marginem externum seriatìm punctis, necnon segmentum abdominis ultimum, ad marginem posticum, notata.

Length, 11 lines; length of *thorax* 3 lines; width of ditto, $3\frac{1}{2}$ lines: length of *elytra* $6\frac{1}{2}$ lines; width of ditto, $4\frac{1}{2}$ lines: posterior *tarsus* very nearly $3\frac{2}{3}$ lines. Head about one third narrower than the *thorax*, the eyes but slightly prominent.—*Thorax* considerably narrower than the *elytra*, convex, the sides and posterior angles rounded; the dorsal channel indistinct; a distinct transverse impression near the hinder margin. The *thorax* is rather attenuated behind, and its broadest part is in the middle, or rather anterior to the middle. *Elytra* very convex, ovate, rounded at the apex; very faintly striated, the *striæ* impunctate. General colour black, with a very indistinct metallic gloss; *antennæ* pitchy red; *palpi* pitch-coloured. On the third, fifth, and seventh interspaces between the *striæ* of the *elytra* a few widely separated punctures are observable, but these are confined to the apical portion of the *elytra;* moreover, with a tolerably strong lens some indistinct punctures may be perceived in the *striæ:* a series of rather widely separated punctures runs along the outer margins of the *elytra,* and on the apical portion are many subconfluent punctures. On the under side of each of the *femora* is a row of punctures; the terminal segment of the *abdomen* is margined with punctures, and there are two punctures on each of the abdominal segments, one on either side of, and at some little distance from, the mesial line. The posterior *tarsi* are considerably longer than the *tibiæ;* each of the *tibiæ* is furnished at the apex with two very long spines. The *antennæ* are slender, and, if extended backwards, would reach beyond the hinder margin of the *thorax.*

This species is considerably larger than the *Card. Clivinoides;* it is of a broader form, and its legs and *antennæ* are longer, as well as the spines with which the former are furnished.

Sp. 2. *Card. Stephensii.*

Card. nitidè violaceo-niger; thorace convexo, attenuato, et posticè transversìm insculpto; corpore brevi, valdè convexo; elytris lævissimè striatis, striis vix apparentèr punctatis; interspatiis aliquantò convexis.

This species is much smaller than the last, and may moreover be distinguished from that and Mr. Curtis's species by the very convex, short, and almost rounded form of the body: its legs are shorter than in *Card. longitarsis,* and the posterior *tarsi* and *tibiæ* are equal in length. The *thorax* is short, convex, attenuated behind, and has the sides and posterior angles rounded; the dorsal channel is very indistinct; and there is a transverse depression near the posterior margin: the *elytra* are very convex, ovate, faintly striated, and the interstices are slightly convex: the *striæ* are indistinctly punctured, excepting on the apical portion of the *elytra,* where the punctures are distinct: some punctures are observable on the outer margin of each *elytron* and on the posterior margin of the apical segment of the *abdomen;* there are also two punctures on each of the other segments of the *abdomen,* as in *Card. longitarsis.*

Total length, 8½ lines; length of *thorax* 2¼ lines; width of ditto, 2⅓ lines; length of *elytra,* 5 lines; width of ditto, 3½ lines.

I have named this species in honour of the author of 'Illustrations of British Entomology,' to whom I am indebted for much information and kindness.

ART. X.—*Descriptions of some new species of Coleopterous Insects.* By EDWARD NEWMAN.

Natural Order.—*CLERITES*, Newman.

Genus.—*HYDNOCERA*, Newman.

THE genus *Hydnocera* was established in the 'Entomological Magazine,' vol. v. page 379. It approaches, in many essential characters, the genus *Tillus* of Fabricius, who probably would not have considered it as generically distinct. One of the species has been described by Germar in his 'Insectorum Species,' under the name of *Clerus humeralis;* and the same insect has also been labelled as the *Tillus humeralis* of Say, but I have no reference whatever to any description by the American entomologist. A second species (*Hyd. serrata*) has been named by myself, and recorded as captured by Mr. R. Foster, at Mount Pleasant, in Ohio: and three others are now added to the list. All the five inhabit the United States of North America.

The Count Dejean, in his 'Catalogue des Coléoptères,' has given to the genus the provisional name of *Phyllobænus;* this I learn from finding one of the species so named by Dr. Harris. I observe with regret that the plan of giving trivial names, without taking the trouble to secure them by the publication of a brief descriptive character, does not meet with that neglect and contempt which such a practice deserves; on the contrary, I have not unfrequently seen these spurious names attached at random to the genera and species that happen to stand unnamed in a cabinet; and names thus arbitrarily imposed in the first place, and subsequently applied by guess, are becoming rife in the majority of our collections of exotic *Coleoptera.*

Sp. 1. *Hyd. humeralis.* (Corp. long. ·23 unc. lat. ·06. unc.)
Clerus humeralis, Germar, 'Insectorum Species,' page 80.

Head large, transverse, black, covered with a grey pilosity, *palpi* and *antennæ* fulvous: the *prothorax* scarcely shorter than its breadth, the sides slightly tubercled in the middle, having a transverse impressed line anteriorly, punctured, black with a grey pilosity: *elytra* oblong, somewhat cylindrical, deeply punctured, black, with a slight metallic shade of green, the shoulders being prominent and of a bright testaceous colour: the body beneath is black, and clothed with a grey pilosity: the fore legs are fulvous, with the exception of a large linear black mark on the *femora;* the middle legs are black, the tips of the *tibiæ* and the entire *tarsi* being fulvous; the hind legs are black, the basal joint of the *tarsi* alone being fulvous.

Inhabits North Carolina, East Florida, and (according to Germar) Georgia.

Three specimens were taken by Messrs. Doubleday and Foster.

Sp. 2. *Hyd. rufipes.* Rugosè puncta; nigro-cyanea, oculis nigris, antennis pedibusque ferrugineis. (Corp. long. ·225 unc. lat. ·06 unc.)

Head finely punctured; black, with a slight tint of metallic blue or green; eyes black, without any metallic lustre; mouth and *antennæ* ferruginous: *prothorax* finely punctured, and having a transverse impressed line both anteriorly and posteriorly, and a slight tubercle laterally near the middle; black, with a metallic tinge: the *elytra* are deeply and regularly punctured, and have a very decided tint of metallic blue: the under parts of the body are black: the legs are ferruginous: the entire insect is clothed with a grey pilosity.

Inhabits East Florida. A single specimen taken by Mr. Doubleday.

Sp. 3. *Hyd. serrata.* (Corp. long. ·175 unc. lat. ·06 unc.)
" " Newman, 'Entomological Magazine,' vol. v. page 379.

Head finely punctured, black, with a metallic tint, the eyes being perfectly black; *antennæ* testaceous, with the apex brown: *prothorax* finely punctured, scarcely longer than broad, having an impressed transverse line anteriorly, and an obvious tubercle on each side, and being much narrowed posteriorly; it is black, with a metallic tinge: the *elytra* are deeply punctured, black, each having two large, subquadrate, testaceous markings, of which the anterior occupies the basal portion of the *elytron*, the posterior does not quite reach its apex: the apex itself is curiously serrated.

Inhabits Ohio. Two specimens taken by Mr. Foster at Mount Pleasant.

Sp. 4. *Hyd. curtipennis.* Rugosè puncta; pallidè testacea, oculis, prothoracis lineâque dorsali longitudinali, nigris: elytra abbreviata, apicibus incrassatis. (Corp. long. ·23 unc. lat. ·06 unc.)

Head finely punctured, pale testaceous, with black eyes, and, in one specimen only, a black vertical spot: *prothorax* considerably longer than broad, its sides nearly parallel, pale testaceous, with three longitudinal black lines, one on each side, and one down the centre: the *elytra* are deeply and regularly punctured, they are very obviously abbreviated, as in the genus *Necydalis,* and the apex of each is incrassated; they are of a uniform pale testaceous colour: the under parts of the body are black: the legs testaceous.

Inhabits East Florida. Two specimens taken by Mr. Doubleday.

Sp. 5. *Hyd. ægra.* Gracilis, elongata, rugosè puncta: testacea, oculis tantùm nigris. (Corp. long. ·18 unc. lat. ·04 unc.)

Head glabrous: *prothorax* nearly twice as long as wide, and somewhat attenuated posteriorly, glabrous, with scarcely discernable punctures: *elytra* elongate, linear, thickened into a kind of knob at the tip. The colour is uniformly testaceous, the eyes alone being black.

Inhabits East Florida. Mr. Doubleday took a single specimen.

Natural Order.—*LUCANITES*, Newman.

Genus.—*Cacostomus,* Newman.

Dorci facies, at corpore squamoso et mandibulis alitèr dentatis planè discrepat.

The head is small, very much narrower than the *prothorax*, and produced in an angle anterior to each eye, which is completely divided, as in *Chiasognathus* of Stephens, and several cognate genera: the mandibles are porrected, incurved at the tip, and nearly approximate at their base; they are twice as long as the head; each, on the internal side, is furnished with several teeth of irregular form, and there is little similarity between the two: the right mandible has growing from its upper margin, near the extremity, a tooth, which is directed forwards, and equals the extreme apex in length, so that the apex of this mandible may be termed bifid, while the left mandible, being without this tooth, has the apex simple:

the *antennæ* are 10-jointed, the joints from the 2nd to the 7th inclusive, are very short and of nearly equal size, the 8th, 9th and 10th are dilated laterally, and form a small but obvious mass or club: the *prothorax* is very convex; it is wider than long; its greatest diameter is about the middle, and its least diameter is anteriorly; its lateral margins are crenate: the *elytra* are rather narrower than the *prothorax:* the joints of the *tarsi* are rather elongate, and each is furnished with an obvious pilosity.

Sp. *Caco. squamosus.* Fusco-testaceus, splendore metallico modestè decoratus: rugosè ac irregularitèr punctus; squamis argenteis sparsis ovalibus omninò tectus. (Corp. long. cum mandibulis ·9 unc. lat. ·35 unc).

The colour is a testaceous brown, displaying, as the light falls on it at various angles, obvious although not brilliant metallic hues: every part of the insect is somewhat irregularly punctured, and is covered with small silvery oval scales, one of which appears to be attached in each puncture.

Inhabits ——— ? I received this insect through the hands of Mr. Bowerbank, direct from Sydney, and the pin appeared similar to those used for other insects in the same collection, evidently from that locality, yet I cannot help expressing a doubt of its country, which I should have supposed to be Assam, or some neighbouring region of continental Asia.

Natural Order.—*CARABITES*, Newman.

Genus.—*SILPHOMORPHA*, Westwood.

Silphomorpha is a genus separated by Mr. Westwood from the *Pseudomorpha* of Kirby, under which name I had unhesitatingly described the present species. Mr. Westwood has remarked of the genera *Pseudomorpha*, *Adelotopus*, *Sphallomorpha* and *Silphomorpha*, that " they possess so many characters in common, that they would doubtless be considered as belonging to the same genus, were they not carefully examined. All of them possess the same formation of legs, mandibles, *maxillæ*, minute maxillary *palpi*, truncate labial *palpi*, &c."—'Transactions of the Lin. Soc. Lond.' vol. xviii. page 416.

Sp. *Silph. maculata.* Picea, prothoracis elytrorumque marginibus lateralibus latè albidis, utriusque elytri maculæ 2 albidæ. (Corp. long. ·3 unc. lat. ·15 unc.)

Head pitchy-black, glabrous: *prothorax* pitchy-black, with a longitudinal median line of a redder hue, and the lateral margins broadly bordered with white: *elytra* pitchy-black, with a broad, white, lateral, marginal line, and two conspicuous white spots on each; of these the anterior is about equidistant from the sutural and lateral margins, and nearly touches the basal margin, at its lower extremity it is notched; the posterior is more elongated; it touches neither of the margins: the under parts of the insect, together with its legs, are pale testaceous.

Inhabits New Holland. A single specimen taken by Mr. A. H. Davis near Adelaide.

Natural Order.—*CETONIITES*, Newman.

Genus.—*Diaphonia*, Newman.

E *Cetoniâ* præcipuè differt antennarum sexubus duobus discrepantiâ; lamellis in *mare* elongatis, in *fœminâ* abbreviatis.

The sexes of the species I am about to establish are so dissimilar, that when my specimens are placed in public collections, I cannot but anticipate their being described as distinct species, I have therefore saved entomologists this labour by describing and assigning a name to each sex.

Sp. 1. *Dia. dispar*, ♂ et ♀.

♂ (*Dia. Ulysses*). Antennarum capitulo sordidè ferrugineo: caput nigrum: prothorax testaceus, plagâ magnâ medianâ nigrâ signatus: scutellum nigrum: elytra testacea, vittâ suturali nigrâ: podex testaceus, abdominis maculis 4 lateralibus nigris; cætera nigra. (Corp. long. ·9unc. lat. ·5 unc.)

♀ (*Dia. Penelope*). Nigerrima; antennarum capitulo, prothoracis marginibus anticis, singuli elytri lineâ longitudinali, basali, abdominis maculis 4 lateralibus, podicis maculis 2 magnis ferè quadratis, ferrugineis. (Corp. long. 1·1 unc. lat. ·65 unc.)

Head and *prothorax* thickly punctured, scarcely glabrous, *scutellum* having a few scattered punctures: *elytra* rugose, with numerous *foveæ* formed by large confluent punctures; each *elytron* has also three raised smooth *striæ*, the first sutural, the second at a considerable distance from the first; both these are very distinct; the interstice between the second and third, which is very indistinct, is less than that between the first and second: these *striæ*, and the rugosity of the *elytra*, at once distinguish this species from the *Cetonia dorsalis* of Donovan, which also belongs to the genus *Diaphonia*: the *protibiæ* are furnished with two strong teeth, particularly prominent in the female; the *meso-* and *metatibiæ* have also two teeth, but the upper tooth is frequently indistinct; in the males of *Dia. dorsalis* the *protibiæ* are unarmed, in the females they have one tooth only.

Colour of the male, (*Dia. Ulysses*).—*Lamellæ* of the *antennæ* dusky ferruginous; head black: *prothorax* testaceous, with a large, amorphous, discoidal, black spot, which does not reach any portion of the margin, except that immediately adjoining the *scutellum;* in a single specimen I have also seen it prolonged anteriorly to the head: *scutellum* black; *elytra* testaceous, with a black sutural stripe, which is not dilated in the middle; the *podex* is testaceous, the *abdomen* black, with a few lateral testaceous spots; legs black.

Colour of the female, (*Dia. Penelope*).—*Antennæ*, head, *abdomen* and legs as in the male: *prothorax* black, with the anterior lateral margin ferruginous; *elytra* black, with a longitudinal, ferruginous, abbreviated line on each, between the shoulder and *scutellum: podex* black, with two large and somewhat quadrate ferruginous spots.

Inhabits Kangaroo Island, Australasia. Mr. A. H. Davis has presented to the cabinet of the Entomological Club, both sexes of this insect.

Genus.—*Hispa*, Linneus.

Hispa bigeneris. Nigra: antennarum articulus basalis spinam acutam ferens: prothorax spinis 5 acutis utrinque armatus: elytra profundè puncta, margine dentato. (Corp. long. ·2 unc. lat. ·08 unc.)

This singular insect combines the spinous *prothorax* of the typical *Hispæ* of Europe with the smooth unarmed *elytra* of the North American species: the colour is black; the basal joint of the *antennæ* bears a long, acute, porrected spine: *prothorax* armed with five spines on each lateral margin; the *elytra* are without spines, glabrous, and impressed with large, deep, and almost confluent punctures, and the margin is armed with a series of short regular teeth.

Inhabits South Australia. One specimen taken at Adelaide by Mr. A. H. Davis.

Natural Order.— *CERAMBYCITES*, Newman.

Genus.—*Callichroma*, Latreille.

Call. ducalis. Antennæ nigræ; caput æneum, inter antennas gibberum, longitudinalitèr striatum: prothorax æneus, lanugine atro-purpureâ, sericeâ, mutabili vestitus: elytra attenuata, lanugine atropurpureâ vestita: abdomen læte æneum: pro- et mesofemora rubra, apice nigra; metafemorum dimidium basalem rubrum, apicalem nigrum. (Corp. long. 1·7 unc. lat. ·45 unc.)

Antennæ black: head glossy golden green, the space between the *antennæ* is considerably elevated and longitudinally striated, the space between the eyes is depressed: *prothorax* golden green, and partially clothed with a velvetty down, the colour of which is a rich, changeable, black-purple: *elytra* attenuated towards the apex, of a velvetty black-purple colour: the under side of the insect is a bright metallic green; the *pro-* and *mesofemora* are red, with the extreme apex black; the *metafemora* have the basal half red, the apical half black; the *tibiæ* and *tarsi* are entirely black.

Inhabits Navigator's Island. This splendid insect was presented by His Grace the Duke of Northumberland, to the collection of the British Museum.

Notice of a remarkable variety of Vanessa Urticæ *taken at Coventry.*—As several instances of remarkable varieties in insects have been recorded in former volumes of the 'Magazine of Natural History,' I trust I need make no apology on the present occasion, for craving a small space in your pages, to notice a very beautiful and extraordinary variety of *Vanessa Urticæ* [Sup. Pl. xv.], which was taken during a dry season in the month of August, about five or six years ago, by Mr. Gee, in his garden at Coventry, and is now in his possession. This insect presents an appearance so widely different from that of the ordinary specimens of *Vanessa Urticæ*, that at first sight, it might readily be mistaken for a distinct species. Indeed, the gentleman who first drew my attention to it, himself an entomologist, observed to me that it was unlike anything he had ever seen before, so much so, that he knew not what to call it. There is, however, generally, about these unusual varieties, a certain indescribable *something*, which enables a practised eye to detect the species to which they belong. The present insect, in spite of its dissimilarity, I feel no hesitation in at once referring to *Vanessa Urticæ*. The specimen, I should add, was in the freshest and most perfect condition when captured, having apparently but just emerged from the chrysalis. The accompanying coloured plate, which is an accurate representation of the insect, will give a more correct idea of it, than could be conveyed by the most lengthened verbal description.—*W. T. Bree.—Allesley Rectory, May* 21, 1840.

Vanessa Urticæ. var.

Swaine sc.

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NATURAL HISTORY

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AUGUST 1840.

Art. I.—*Additional Notices of Species of the Genus Equus.* By Edward Blyth, Esq.

(Vide p. 81 *et seq.*)

It did not occur to me when I wrote the paper above referred to, that I had read an elaborate essay by M. Marcel des Serres, on the animals represented in the celebrated Prænestine mosaic, wherein it is suggested that a second species of *Equus*, it would appear, according to the restricted application of this term proposed by Mr. J. E. Gray, that is, as distinguished from *Asinus*, Gray (constituted by the species with callosities on the fore-limbs only), is indicated upon that antique monument. A translation of this memoir is published in the 'Edinburgh New Philosophical Journal,' vols. xvi. pp. 160, 205; xvii. 268; and xviii. 59.

"Two species of *Solidungula*," remarks the author, "are figured upon the mosaic of Palestrina. The first represents the common Horse (*Equus caballus*, Linn.); whilst the second, under which is written the name Λυνξ, seems to be a race which is lost and destroyed [?]. Prior to the epoch of the first ages of the empire, this word would have been written Λυγξ. The animal to which this name is erroneously attached appears to be a species of *Equus*, between the Djigguitai and the Quagga. It has nothing in common with the *Lynx* of the ancients, which was the *Loup Cervier* [Qy. *Felis pardina*, Oken, the beautiful Spanish Lynx now living in the Zoological Society's menagerie ?], as has been well remarked by Perrault (*Mém. de l'Acad. des Sc. depuis* 1666, *jusqu'à* 1669, *tom.* i., *prem. part.*, p. 131.): the slightest examination, indeed, suffices to show that the animal named *Lynx* in the mosaic has solid feet, or which terminate in a single hoof, together with the body, head, and tail peculiar to the Horse. In conformity with these characters, then, this species is neither the Djigguitai nor the Quagga, and still less the Ass or the Zebra. It would therefore constitute a species which is now lost [?]; supposing that it has really existed with the form and proportions which are bestowed on it in the antique. On this point," M. Serres continues, "we may again remark that this is the more probable, since the figures of the mosaic are generally so well delineated, as to lead us to conclude that they were copied from nature."

To judge, however, from the representation of this mosaic published in the Appendix to Shaw's 'Travels in Barbary,' I should be disposed to place no reliance on the authenticity of the figure there indicated by the name mentioned, at least as representing a peculiar species; for I do not perceive in what respect it differs from a horse, excepting that no hair whatever appears on the tail. Whether Shaw has supplied an accurate copy of the original, I have no means of determining. M. Serres contends that the *Hippotiger* of Dion refers to the Zebra, and that therefore the ancients were acquainted with species peculiar to South Africa; this position is untenable, since the range of the Zebra extends northward to Abyssinia; and the Κατωβλεψ of Ælian, referred by Cuvier to the White-tailed Gnoo, is equally applicable (as suggested by Mr. Ogilby) to the Cape Buffalo, which also extends northward to Abyssinia; or supposing the *Catoblepas Brooksii* of Col. Hamilton Smith to be a true species, it may allude to this, the locality of which is unknown[1]. By the term Djigguitai (*E. hemionus*), M. Serres probably alludes to the Khur; and he observes that the animal in question was domesticated by the Greeks in many provinces of Asia. I have somewhere read that three different Hebrew appellations are rendered by the term *Ass* in the various translations of the Old Testament, and that the Asses of Saul were of the wild kind, denominated in their native region *Khur*, which sufficiently accounts for their alleged value.

"Wild Asses" of some sort, it would appear, are "common in the districts of the Thebaid" (vide Wilkinson's 'Domestic Manners of the Ancient Egyptians,' iii. 21.), and a "wild Ass" is mentioned in the narrative of Lander's expedition (p. 571.). Are these of the striped kind noticed by Bruce? Or are they of the species termed "Isabelline Zebra" by Levaillant? Or may they not be *Khurs*, and at the same time identical with Levaillant's animal? In the island of Socotra, Lieut. Wellsted mentions that "Amidst the hills over Tamarida, and on the plain contiguous to it, there are a great number of Asses, which were described to me as different

[1] For note, see the end of the present Number.

from the domestic Ass; but after repeated opportunities of observing them, I could find no reason for such a distinction. The introduction of Camels," he remarks, "having superseded the necessity of employing them as beasts of burden, they are permitted to stray where they please, and now wander about in troops of ten or twelve, evincing little fear unless approached very near, when they dart away with much rapidity." (Journ. Geog. Soc. 1835, p. 202.) Azara notices, of those which have gone wild in South America, and especially about Santa Fè de la Vera Cruz, where he states that the increasing population was fast destroying them, that those which he saw appeared to be somewhat larger than the domestic ones of Paraguay, but smaller than the common asses of Spain; nor does that large race, which is there used for the breeding of mules, exist in these parts. They also appear to have larger and stiffer ears than in my native country." Russell, in his 'Natural History of Aleppo,' p. 58, remarks, that the Levantine nations have two principal breeds of Asses; "one very large, with remarkably long ears; the other small, and much like ours in England."

Those of Upper Egypt, according to Sonnini, are particularly handsome, but they degenerate towards the Delta. Fraser states, that "the Asses of Omaun are celebrated as the best in Arabia, and individuals of the best breeds sell for very extravagant prices." (Journey to Khorassan, p. 18.) Not, however, that the Asses of warm countries are invariably superior to those of the north; for the domestic breed of India is remarkably small, and Col. Sykes states that in the Deccan they are scarcely bigger than a Newfoundland Dog. In Egypt it appears that the fine breed of that country has existed from very remote antiquity, to judge from the ancient paintings, where the cross-line of the shoulders is represented in every instance that I have seen.

There is reason, I suspect, to infer that the Persians distinguish the *Khur* from the *Goor-Khur*, *Ghore-Khur*, *Gur-Khor*, *Gour-Khor*, or sometimes simply *Gour* (which is not to be confounded with the *Gaour*, *Bos gaurus*, of India); one of these animals inhabiting chiefly the west, and the other the eastern portion of that country: the true Djigguitai is, however, clearly indicated in Morier's second 'Journey through Persia' (p. 200), as the "wild Ass" of Casvin, not far from Tabriz, "of a light mouse colour, with a dark streak over the shoulders and down the back;" and it is probably that mentioned in 'Johnson's Journey' (p. 149.), as "found in the salt plains near Tehraun, and in greater numbers towards Mansila." The Hon. Mountstewart Elphinstone, in his 'Account of the Kingdom of Caboul,' notices it, by the appellation *Goor-khur*, as an inhabitant of the deserts between India and Affghanistan. "It is called," he says, "*Gour* by the Persians, and is usually seen in herds, though often singly, straying away in the wantonness of liberty." Those exhibited in European menageries have been mostly, if not exclusively, captured in the Thurr, or great sandy desert north of Cutch. The "wild Asses" of Mesopotamia, on the other hand, and at the foot of Taurus, as mentioned by Mr. Ainsworth, those of Arabia, and perhaps of the Thebaid and elsewhere on the African continent, are probably all of the kind that has no mesial stripe down the back, as noticed in two individuals by Sir R. K. Porter. If it should turn out that the *Khur* and *Ghoor-khur* are really distinct, as also the *Kiang*[1] of Thibet, the near approximation of so many species (to which may be added the common Ass) will render it desirable that the *Equus hemionus* of Pallas should be compared with the "wild Ass" of Cutch, which latter, I believe, has never any trace of colouring upon the limbs, except a very faint tinge at their commencement, whereas the other appears to have the exterior surface of its limbs not much paler than the parts above. The "wild Ass" of M. Gmelin, also, figured with a cross upon its back in the continuation of M. Schreber's work by M. Wagner, and remarkable for the silvery white of its under parts ascending from the flanks in front of each haunch to join that on either side of the dorsal line, a particular carefully described as well as figured, might advantageously be compared with the domestic *E. asinus*. The female observed by M. Gmelin had no cross stripe over its shoulder, such as was found in the male, and is, I believe, invariably constant in the common animal; whilst in the Mongolian wild Ass, M. Gmelin was informed that the mark referred to is by no means constant (as his two specimens testified), and that sometimes there is even a double cross-band over the shoulders. Prof. Gmelin succeeded in bringing his female wild ass, together with a colt, to St. Petersburgh; and he remarks that she sometimes passed two days without drinking, and preferred brackish water to fresh: she carried her head higher than the common tame Ass, her ears well elevated, and evinced much spirit and vivacity in all her movements, such as we are accustomed to observe of the Djigguitai.

Mr. Moorcroft, the same writer from whose 'Travels' I quoted the previous notices of the *Kiang*, remarks, in the account of his 'Journey to Lake Manasurovara' ('As. Res.' vol. xii.), that "the wild Horse (*E. quagga*), the wild Ass (*Ghoor-*

[1] It should be mentioned that the red-legged Chough (*Fregilus graculus*), which abounds in many districts of the Himalayahs, is termed *Kyang* in Bhootan. Vide 'Asiatic Researches,' xvii. 16.

khur, Onagra), and I believe the mule, the offspring of these animals, are found in abundance on the mountains of Tartary"; and again,—"This day we saw more wild Horses than on any former one, also several wild Asses of the kind called *Gurkhor*, and I believe the mules. The Asses are a little less than the Horses" (p. 462.). And in another place (p. 512.), he describes meeting with "many wild Asses, and some animals which are thought more like Mules than either Horses or Asses." Here there can be little doubt three distinct species are noticed, viz. the true wild Horse, either the wild Ass or the Djigguitai, and probably the Kiang. The latter is noticed by the same author in the 'Transactions of the Royal Asiatic Society,' vol. i. p. 55, as "a nondescript wild variety [species] of Horse, which appeared to be about fourteen hands high, of a round muscular form, with remarkably clean limbs. Not more than a dozen came within view, and they were all out of shot. A native of the district was directed to lie in wait, and a suitable remuneration was offered for the skin, head, and organs of voice for dissection. The man," continues the author, "has completed his task, and I shall have these matters as soon as the Pass of Changlung will admit of being traversed." Should the executors of the late Mr. Moorcroft possess any notes by him of such examination, the publication of them would be acceptable to zoologists. In the 'Asiatic Researches,' vol. xviii. pt. II. p. 247, Mr. Gerard seems to allude to this animal, as resisting every attempt to tame it when caught, so that it has never been domesticated: and Dr. Gerard met with "great herds of Kiangs" at an altitude of 17,700 feet. Mr. Moorcroft likewise notices, in his 'Travels,' (l. 370, after at p. 311, indicating the Kiang as distinct), that "the *Ghor-khur* or wild Ass," whichever species is meant it is useless to attempt to determine, "is common in Chan-than." Finally, it may be mentioned that the word "Asses" occurs in the interesting list of wild animals inhabiting the Malay peninsula, published in the 18th volume of the 'Asiatic Researches,' p. 159; but from the context I cannot doubt that it is a misprint for *Apes*.

February 14, 1840. EDW. BLYTH.

ART. II.—*View of the Fauna of Brazil, anterior to the last Geological Revolution.* By DR. LUND.

(*Continued from page* 317.)

After this brief description of the extinct fauna of the genera and species once living here, which I have accompanied, at the end of each family, with the observations they more immediately called forth, I presume to direct the Society's attention to the general considerations that naturally arise respecting the peculiarities of that ancient fauna, and its relation to the present. Of the four orders into which mammals are divided, we find on this continent representatives both in the former and modern period. The most numerous of these orders, as well for genera as species, is now that of *Myoidea*; next to which follows that of *Acleidota*, then *Bruta*, and last of all *Quadrumana*. This relative proportion in the orders, with regard to their contents of genera and species, was not the same formerly as it is now. The order *Acleidota* was the most numerous; next followed *Myoidea*, then *Bruta*, then *Quadrumana*, as the following table will more clearly show:—

	Genera.		Species.	
	now.	then.	now.	then.
Bruta	0·18	0·27	0·10	0·20
Acleidota . . .	0·24	0·39	0·30	0·42
Myoidea . . .	0·49	0·30	0·54	0·33
Quadrumana . .	0·09	0·03	0·05	0·02

This change in the relative richness of the orders is occasioned principally by the appearance in the latter period of the numerous family of Bats; which, as far as our researches have gone, appear not to have existed in the fossil period.

Of the sixteen families into which the four orders of mammals are subdivided, nine are now found in this district, namely, the *Myrmecophaga*, *Armadillo*, *Ruminants*, *Pachydermata*, *Feræ* (predatory beasts), *Marsupials*, *Rodents*, *Bats*, and *Apes*. Of these nine, one is hitherto wanting to the list of the families belonging to the extinct Fauna, in like manner as one that played a conspicuous part formerly, the Sloth, is no longer found in these parts. Again, with regard to the comparative richness of the other families, the two periods do not entirely agree; as far, at least, as we can conclude from the confessedly imperfect list we as yet possess of the fossil species. In both periods the family of *Rodents* is the most abundant, both in genera and species; next to which follow *Feræ*; but this latter is proved to have been richer in genera, and probably also in species, then than now. The most marked difference, however, is in the family of *Ruminants*, which in the former period contained a much larger quotient, both of genera and species, than in the modern. The same is also the case, though not in so high a degree, with the *Armadillos* and

Pachydermata. The other families present too few numbers to pronounce with certainty on this point. The following table will better explain these proportions:—

	Genera.		Species.	
	now.	then.	now.	then.
Edentata . . .	0·03	0·03	0·03	0·02
Effodientia . .	0·15	0·18	0·07	0·13
Tardigrada . .	0·00	0·06	0·00	0·07
Ruminantia . .	0·03	0·12	0·07	0·13
Pachydermata .	0·06	0·09	0·04	0·11
Feræ	0·15	0·18	0·19	0·18
Marsupialia . .	0·03	0·03	0·07	0·04
Glires	0·27	0·27	0·26	0·29
Chiroptera . .	0·18	0·00	0·19	0·00
Simiæ	0·09	0·03	0·06	0·02

If now we turn to a general consideration of the genera of the extinct fauna with reference to their agreement with the present, we find that out of the thirty-two genera (leaving out the fossil *Rodent* above-mentioned, which I have not yet been able to identify) which the extinct class of mammals has hitherto afforded us, eighteen still inhabit this district, while fourteen are no longer found here. We thus see that the deeper we examine the subordinate divisions of the system, the greater is the agreement between the ancient and the recent faunas; so much so, that more than half the genera are identical for the two periods. The genera common to both may be conveniently divided into two groups, which are of very unequal importance in developing the history of our globe. Under the first group I class those genera that are common to both the Old and New World, and which are therefore not adapted to throw any light on the peculiar types of animal life belonging to this continent in former periods. These genera are only six; namely, *Cervus*, *Tapirus*, *Felis*, *Canis*, *Lepus*, and *Mus*. The second group consists of the genera which are at present confined to this quarter of the globe. It is more numerous than the first, and contains the following twelve genera:—*Myrmecophaga*, *Dasypus*, *Dicotyles*, *Eyrara*, *Nasua*, *Didelphis*, *Echimys*, *Synoëtheres*, *Anœma*, *Dasyprocta*, *Cœlogenys*, and *Hydrochœrus*. The first glance at this group is sufficient to show that "the fauna which inhabited the tropical highlands of Brazil previous to the last re-construction of our earth, was in its fundamental types the same as is now found there." This result is of vast importance with reference to the theory of the relations of development in the organic productions of our planet; and I reserve for a future opportunity the consideration of some of the weightiest consequences that result from this fact.

Turning next to the genera belonging to the extinct fauna which are no longer found in this district, we are able to separate them also tolerably well into two divisions; of which the first will contain those that have entirely disappeared from the earth's surface, and the other those which still exist, but at a greater or lesser distance from the spot where their remains are now discovered. The former of these divisions includes ten genera—*Euryodon*, *Heterodon*, *Chlamydotherium*, *Hoplophorus*, *Pachytherium*, *Cœlodon*, *Megalonyx*, *Leptotherium*, *Mastodon*, and *Protopithecus*. If now we consider more closely the genera comprised in this division, we find that far the greatest proportion of them belong to the order of sloths (*Bruta*), and that they are composed of large clumsy animals, whose extraordinary inharmonious organization seems to have contained the seed of its destruction. The second of these divisions is composed of only four genera—*Antelope*, *Camel*, *Bear*, and *Hyæna*; but the existence of these animals in the Brazilian highlands in the former period is a phænomenon of the highest interest, and calculated to awaken the most important considerations. I have shown that the fossil species of *Camel* belonged unquestionably to the under-group of *Auchenias*, and that we therefore have to seek the modern habitat of this form in the chain of the Andes; also that the fossil species of *Bear* seems in like manner most to resemble those which in our time inhabit the same mountain range. With regard to the third genus, *Antelope*, we must certainly, in the present state of our knowledge, consider it a form peculiar to the Old World. I have, however, already alluded to the possibility of a representative of this genus being eventually found also in the Andes. On the other hand, the last of these four genera, *Hyæna*, leaves us no other resource than the striking conclusion, that the plains of South America formerly sustained genera of mammals which, at the present time, are confined to the hot zone of the Old World; and we have seen that this conclusion is still further corroborated by the two subgenera of *Cynailurus* and *Speothos*; forms that, in the existent creation, are only found in the warm districts of the Old World, but which have left indubitable traces of their presence in the extinct fauna of this continent.

If we next descend to the lowest step in the subdivisions of the system, that is to species, and compare the extinct with the existing, we are again led to separate them into two divisions, of which the first contains those animals that considerably differ from all now living, while the second includes such as resemble the present order of creation. Before, however, I proceed to distinguish these fossil species, I must first put aside those of which I either possess too imperfect specimens to allow me to institute any satisfactory comparison, or else the osteology of the correspondent recent species is not sufficiently known to me. To this class belong seven species, namely, *Tapir, Cervus,* the small species of *Tiger, Fox, Coata-mundi,* and the lesser kind of *Capivar.* I do not add to this list either the species of *Mus* or *Didelphis.* Not because I want the necessary materials for comparison, but because a complete comparison of these species requires lengthened and accurate previous examination of the osteology of the recent kinds, a labour to which I have not as yet had the opportunity of devoting myself. Taking away these eleven species, there remain forty-three, which will serve for the foundation of our comparison. I begin with the species of the four genera, *Camel, Antelope, Bear,* and *Hyæna;* which, as we have seen, are indeed not now found in the Highlands of Brazil, but have not ceased to continue their existence in other parts of our earth. We have before observed that the two species of the genus *Camel* excluded, by their very size, any suspicion of their conformity with the existing species of this genus. With regard to the three other genera, *Bear, Antelope,* and *Hyæna,* nothing but direct comparison can positively decide the question as to their identity with the respective species inhabiting the chain of the Andes, and the Old World; at the same time I cannot but think that the result of the investigations I am about to detail will leave little room for doubt on this subject. In order, however, to remove all uncertainty from the comparison I am instituting, I will cast out these three also, so that only forty species remain to be considered. In the first division, which comprises all those species which differ considerably from those now living here, we may first of all class all those that belong to genera that have entirely disappeared from the earth's surface. These, as we have seen, are the following ten: *Euryodon, Heterodon, Chlamydotherium, Hoplophorus, Pachytherium, Cœlodon, Megalonyx, Leptotherium, Mastodon,* and *Protopithecus*: these genera comprise fourteen species; to which we may add the undetermined genus of Rodent, with a single species, and the lately mentioned species of Camel; in all seventeen species.

I now come to the genera that are now, as formerly, found in this district, excluding those species which either the first glance or longer examination have satisfied me to be different from the recent. These species are seventeen, viz. the gigantic *Ant-bear*, the four species of *Peccari*, the large *Tiger, Cynailurus*, the *Cave-wolf*, the *Cave-jackall*, the *Honey-glutton* (*Eyrara*), the largest species of *Spiny-rat*, the large *Cuandu* (*Hystrix prehensilis*), the gigantic *Cutia*, the three species of *Paca*, and lastly, the gigantic *Capivar*. Adding now these seventeen species to the seventeen before described, we have thirty-four out of forty that are decidedly different from existing species. The second division of the fossil species is composed of those which, from the more or less considerable portions that I possess of their skeletons, show so great a conformity to the living species, that I have not been able to discover any specific difference. These are the following six: one species of genus *Dasypus*, two species of *Echimys*, the fossil *Rabbit*, the *Perea*, and a fossil species of *Cutia*. Thus the proportion of the conformable to the unconformable species is as 6 to 34.

In this manner we have seen, as we gradually descend in our comparison of orders, families, genera, and down to species, that at each step the unconformity between the ancient and present fauna increases. This difference, indeed, is so great in the last step of all, that one may well be tempted to propose the question, whether this slight bond of connexion wherewith we see them linked together, is really fashioned by nature herself, or is only attributable to our imperfect methods and means of comparison. The decision of this question being of the highest importance to science, the Society will allow me to dwell a little longer on the subject. I would wish first to remark, that the conclusion on which the identity of the six species with the existing fauna is founded, is far from possessing the certainty of that which determines the unconformity of the other thirty-four. I do not possess a perfect skeleton of any one of these six species; and it is very possible that these fossil species, though agreeing with the recent in those portions that I possess, might differ from them in others, with which I am not acquainted. This possibility has, in fact, been proved in the case of one of the extinct genera, the *Paca*. I had in my possession a vast quantity of the remains of this genus, which showed so striking a resemblance to the living species, that I at first referred the fossil animal to it, until the examination of the respective skulls, as I have before observed, convinced me not only of their specific difference, but also proved to me the existence of two distinct species among the fossils of this genus. If, therefore, we were to allow analogy to be our guide, we ought to suppose for the other few species that are in the same circumstances as were the fossil species of *Paca*, until complete comparisons enable us to settle the question, a similar condition, that is a specific difference from the living species, to which they show a greater or less conformity.

I think, therefore, that I am supported by the highest degree of probability, approaching in most points to certainty, in confidently laying down this result, that the pre-existing race of mammals in South America, as far as regards species, was entirely different from that which now inhabits this same continent. Cuvier was led to the same result by his examination of the extinct fauna of the Old World; and the more this important conclusion has been doubted and combated by later naturalists, the more am I gratified in being enabled, by my researches in this quarter of the globe, to corroborate it.

Having thus cast a cursory glance at the extinct mammals which last existed in this district, and having next considered more closely their relation to the modern fauna that has succeeded them on the same spot, we will now advance from this foundation, and by the help of these new facts, wherewith science has been enlarged, endeavour to elucidate several important points in the history of our globe. It had been a firmly received maxim in science, that the tropical zone, at least in its lower portions, was either entirely uninhabited in the period that immediately preceded the present state of things, or at any rate was very thinly inhabited. The present inquiry has, on the contrary, proved that this zone, far from having been uninhabited at that time, displayed a richness and variety in its animal kingdom, which seem far to surpass what nature is able to maintain there in our days. We have seen that this position is certain for the greater portion of the families composing the class of mammals, and that it is true for the whole of them, as far as genera are concerned; but that it may be equally so for them, with reference also to species, no one surely will doubt when he thinks of the great number of species that have at once been discovered, upon the very first glance we have cast behind the curtain of that extinct fauna, a number so very little inferior to that of the living races. This probability must undoubtedly appear to every one so strong by itself, that I cannot but consider it almost superfluous to corroborate it by the following considerations. The extinct fauna is in the natural course of things withdrawn from our observation: only favourable circumstances, and luckily conducted explorations, have brought to light some isolated specimens of its scattered remains; while, on the contrary, the existent creation presents itself of its own accord to our eye, so that sooner or later it must be the entire property of science. We may, therefore, consider the list of existing species in that class of animals of which we are now speaking, as almost complete; whereas we may expect with every day an increase to the catalogue of extinct species.

This poverty of animal forms, that had hitherto been imagined to mark the tropical zone, was endeavoured to be explained by the supposition of an extraordinarily high temperature, that prevented the development of animal life. We now know that this supposition, and the facts on which it was grounded, are equally without foundation. The tropical zone enjoyed at that period a temperature which not only did not prevent the development of animal life, but was even far more favourable to it than that which it at present possesses. If we have a right to suppose that the richness and variety of organic productions, and their development with regard to size, are in proportion to temperature,—a proposition which is generally received, and when viewed on a large scale is confirmed by experience,—then undoubtedly we cannot but ascribe a higher temperature to that zone in ancient times than it now enjoys; but this excess of heat is yet much less than what naturalists were hitherto disposed to claim for it on the foundation of an incorrect fact.

It would be an important task to compare the extinct mammals of tropical America with the correspondent fauna of the temperate and frigid zones of the same continent. But unfortunately the insufficiency of our materials prevents us from instituting any such comparison. Our knowledge of the fauna inhabiting the corresponding zones in the Old World is much more complete; but even there several circumstances forbid a direct comparison. In the first place, the tropical zone in the Old World, at the present time, is richer in species of mammals than the same region in the New World. In the second place, the extent of country through which the fossils belonging to the former age of the earth have been sought for on the old continent, is infinitely greater than the district in which I have had the opportunity of pursuing my investigations. In the third place, the length of time during which the fossils have been collected in the Old World, and the number of scientific collectors interested therein, are both very considerable; whereas the description here attempted of the extinct fauna of S. America is the fruit of two years' labour of a solitary individual. Taking these modifying circumstances into consideration, I think that a comparison between the extinct fauna of tropical America, and of extra-tropical Europe will not be without use.

[Dr. Lund here refers to a Table, marked 4, which, as he says in a note, "represents the state of our knowledge on this point of fossil zoology at the time when Cuvier concluded his inquiries respecting it; and I am not aware that there have been any subsequent additions, of essential importance, with regard to number of species."]

If we now compare this description with the list of South America's extinct mammals, we see that the latter exceeds the other in number, both of species and genera; and if we further lay in the scale all the circumstances to which I have just drawn attention, it is very clear that we must allow to the tropical region of America a far richer fauna in the ancient period, than to the extra-tropical portion of the Old World. Such a disproportion between these two zones, with reference to their animal productions, can only be explained by the supposition of a corresponding difference in temperature; so that we see it results from these considerations as a very probable consequence, that at that epoch also, as now, the surface of our planet presented a difference of temperature, according to geographical latitude, in other words, climatic distinctions had already begun to act.

But as surely as the extra-tropical portions of our globe were already at that period inferior in temperature to the tropical, equally certain is it that they then enjoyed a higher temperature than is now allotted to them. When we see that the class of extinct mammals, found in the extra-tropical zone of the Old World, surpasses that now living there in variety and abundance of forms, as well as for the most part in number of species; when we also see that the principal of these forms are such as in the present creation are peculiar to the tropics; surely the most prejudiced person must be led to conclude that the temperature of those which are now the temperate and frigid zones, must have formerly more or less corresponded to the actual heat of the tropical zone.[1]

[1] It is not my intention here to revive the much-canvassed subject of the hairy Elephants and Rhinoceros of Siberia; but I cannot forbear remarking, how very hasty they were who endeavoured at once to draw from this phænomenon the conclusion that the climate of Siberia cannot have changed. In the first place, this hairy covering is found on a very great number of tropical animals; secondly, it has in this instance struck us so much, because it occurred in an animal that we are accustomed to see without any

A glance at our list of the extinct mammalian fauna of Brazil, will be sufficient to show that in its main features it bore the same stamp as the system that has succeeded it in the same region. This peculiarity in South America's existing fauna is in harmony with the isolated position and the form of this continent. When, therefore, we find this same peculiarity in its animal productions in that former period, we are emboldened to conclude that its general form and boundaries were the same then as now. The existence at that time of generic forms in South America, which at present are peculiar to the Old World, such as the Hyæna and Antelope, can no more weaken the force of this conclusion, than the present existence of a genus in South America, belonging to the New Holland family of Marsupials, is sufficient to prove that the two continents are now united together. On the other side, the proof of the existence of one and the same species of mammal, in the warmer regions of Europe and in South America, would be a phænomenon calculated to strengthen the uniform result to which all our examinations have hitherto led us. And exactly such a specific identity between inhabitants of the old and new continents is known to have occurred in those times: the authority for this is Cuvier. As accident would have it, the very first specimen

such protection. It is, however, certain that the Indian Elephant's want of hair arises from slavery and confinement; and that in its wild state it is provided with a hairy coat, which is most abundant on the younger animals, and which increases so much in the colder mountain chains, that it is said they are sometimes met with "as hairy as Poodles," according to the expression used by Bishop Heber. This may appear somewhat overstretched, but Mr. G. Fairholm has instituted the most rigorous investigation on this subject, and has satisfied himself of the fact. Again, if we conclude that this hairiness of the fossil Elephant afford proof of a polar climate in those regions where it dwelt, we must extend this climate over the whole of Southern Europe, throughout which, and particularly in Sicily near Palermo, have I found the bones of the fossil Elephant, with those of the Hippopotamus. And lastly, we must not forget, that even if these animals, thus provided with a thick coat of hair, could withstand the climate of Northern Siberia, such as it is in our time, still the scanty vegetation of that region could not possibly have afforded adequate nourishment to these colossal creatures, that appear to have lived there in considerable numbers; the more so, inasmuch as the Elephant, by reason of its dental provisions, is principally confined to feeding on leaves of trees; whereas not only all arboreal vegetation is absent throughout a great extent of the polar zone, where these bones are found in vast quantities, but also all vegetation whatever is suspended during a great part of the year. This last objection has been endeavoured to be removed by the supposition that they were migratory animals, which migrated southwards at the approach of winter; but it seems to be forgotten that it is precisely on the islands of the Frozen Ocean that these bones are found the most abundantly.

of South America's antediluvian fauna that was submitted to that great zoologist's notice (*Mastodon angustidens*) came under this category. It is easy to conceive that such a circumstance should excite in that deep philosopher especial attention to this continent and its ancient inhabitants; and in fact it did raise doubts in his mind of the existence at that period of the Atlantic Ocean, in its present boundary and position, at least. Meanwhile I must remark, that the supposition of this specific correspondence is founded on so few means of comparison, that we are bound, by the importance of the conclusion, to suspend our decision until further inquiries shall enable us to come to it with greater certainty: but in whatever way this question may eventually be decided, I do not conceive that this isolated, though confessedly important phænomenon can with any justice throw doubt on the correctness of the above-given result, to which all our other knowledge of the extinct fauna of this continent has conducted us.

After this glance at the ancient temperature of our globe, and the form of its continents, I proceed to consider the results, to which the facts brought forward in this paper may lead us, with reference to the nature of the great catastrophe which overwhelmed the animal race, whose remains we have been considering. I have shown in my previous communication that the surface of this district, throughout that part which I have visited, consists of loose soil of different qualities, among which a red clay is most conspicuous, that often contains beds of rolled or angular fragments of quartz; again, that this same soil fills up all the clefts and cavities in the rocks; and that it is in this deposit of soil within the caves that the bones of the extinct animals are found; that the bones lie scattered without order in the soil, often in astonishing quantities, and that for the most part they are broken or injured in various ways. Now if we collect under one point of view all these several circumstances (for the detailed consideration of which I refer to my former paper), it is evident that there is but one natural solution of them. The caverns, wherein we find these heaps of bones, served for dens to predatory animals in the ancient time; and the bones that occur there, are the remains of animals that formed their prey. The injuries to which the bones have been exposed, leave no room for doubt upon the subject. These bones then, and fragments of bones, lay heaped up on the floor of the caves, when a vast deluge which covered the whole land with the deep stratum of loose soil that we now see to overspread its entire surface, violently burst into the caverns, scattered about the heaps of bones, and enveloped them in the same sedimentary deposit with which it covered the surface outside the caves. I have traversed this district over an extent of more than 8000 square miles, and have every where discovered the indubitable traces of such a deluge. I have shown that "Canga," which exceeds the highest mountain summits in Brazil (6000 ft.), is a contemporaneous formation of the same diluvian deposit; and I therefore think I am entitled to consider it as definitively settled that this portentous catastrophe extended over the whole of this continent, with the possible exception of the very loftiest mountain tops. In the preceding communication I have shown that the red clay soil, which is the most general sedimentary soil of this district, is also that which most abundantly fills up the caverns. I have there pointed out, that in consequence of this particular position, the soil has received many extraneous additions, of which the most important are, the intermixture of angular or rolled fragments of the limestone in which the caves occur; next, the percolation of calcareous particles, which have formed crystals in its cavities, and changed it to a mass hard as stone; and lastly, the introduction of a more or less considerable quantity of bones, for the most part in fragments, and particularly white in the fracture.

No naturalist can have read the description of these phænomena without thinking of the *osseous breccia* on the coasts of the Mediterranean. I have seen many specimens of this breccia in the museums of Europe; and I can assert that so perfect is the external resemblance, that without the labels it would be scarcely possible to distinguish the osseous conglomerate of Brazil from its well-known contemporary of the Old World. The identity of the two formations, therefore, in my mind, cannot be disputed.

It will appear evident from the description I have given in the preceding communication of the loose soil that constitutes the last member in the geognostical formations of this land, that this bed is identical with those which also in Europe form the most usual covering to the surface, and which are distinguished by their greater or lesser abundance of rolled stones. I have already remarked more than once, that these beds of soil, with all their characteristic properties, are found filling up the cavities and chasms in the rocks, and that they have there undergone some alterations which, however, leave no room for doubt as to their origin. This identity between the osseous conglomerate of this country, and the loose soil of the surface, is easily recognised, and serves to throw light on a geological phænomenon in the Old World, that has hitherto been veiled in some obscurity: and I trust that the contemporaneousness of the boulder formation of Europe, and of the osseous breccia of the northern coast of the Mediterranean, may from this time be considered an incontestable fact in science. This correspondence in respect to the youngest geological formations at two points of our planet so far removed from each other, leaves scarcely any doubt as to the very general operation of the mighty catastrophe that effected these formations. If any one wish for yet another link to this chain of geological coincidences, I would refer him to the latest researches in New Holland. I have carefully compared Mr. Henderson's description of the soil that fills the caves in Wellington Valley, as also of the conditions under which the bones are there found, and I recognize the most striking conformity to the circumstances I have observed in this country. I here close this treatise, which was only intended to give a short view of the extinct fauna of this district. I have commenced with the class of mammals, as being the most perfect to be met with on the theatre of that creation, inasmuch as Man, that creation's lord, had not yet entered on the scene. I am well aware how careful we should be in founding a conclusion on a negative fact; but when this negative fact holds good so constantly as is here the case, in the midst of so many positive facts that rise around it, and serve each to confirm it, I think we cannot refuse to it an equal weight with any of them. And how, I may ask, was it possible for man to exist in a country so full of fearful beasts of prey, as was Brazil in those ages? How especially was it possible, that amid the vast mass of victims, which the first glance behind the scenes of that ancient world has shown us, so weak a creature as man should alone have escaped the necessity of yielding to physical superiority the sacrifice that so many more powerful animals were compelled to offer? I think we may conclude with certainty, that where the haunts of the Tiger and Hyæna betray no trace of human bones, our race had not appeared as an element in the composition of the organic world.

This paper having swelled to a much greater compass than I originally contemplated, I think it will be convenient if I subjoin a brief survey of the main results of my labours, so far as they are new to science.

In the period that immediately preceded the last geological revolution on the surface of this earth, the tropical zone was by no means uninhabited, or even scantily provided with animal life, as has hitherto been generally supposed, but, on the contrary, presents an abundance and variety in its fauna, which appears to have far surpassed what is now observed there. With respect to the class of mammals, the superiority of the ancient order of creation is proved in the case of genera, and it is highly probable that the total amount of species was also greater. The families of *Armadilloes* and *Sloths*, together with those of *Ruminants* and *Pachydermata*, being more numerous formerly than at present both in genera and species; there is the greatest degree of probability that this was also the case in the families of *Feræ* and *Rodents*. The family of *Simiæ* existed at that period, whereas it seems that *Bats* were wanting. The mammalian class of this continent exhibited the same peculiar stamp that now distinguishes it. At the same time, in the very midst of these peculiar South American forms, appeared some that in our day are confined to the warmer regions of the Old World. The greater portion of the genera, of which this country's mammalian fauna formerly consisted, exist there now: of those which are wanting, most are entirely extinct, others have disappeared from the whole of this continent, while a few are confined to the lofty mountain chains of the western coast.

The existing species are all distinct from the fossil: man did not exist at that period. From these results, which are nothing more than a comprehensive expression of facts, I collect the following more general consequences, which certainly seem to me to follow necessarily from the above; but which, as they rest upon conclusions that may not have equal force in the eyes of all, should be separated from the former. The form of the continents was the same in the ancient period as in the present. The temperature on the entire surface of the earth was higher than now; but at the same time it diminished from the equator towards the poles. The natural catastrophe that annihilated the numerous animals with which the present account has made us acquainted, was an universal catastrophe that embraced the whole earth. All life was extinguished on the face of our globe; a great epoch in the history of the earth was closed; and the innumerable forms under which animal life now appears are the products of a new creation. In like manner, as in an older formation (Jura limestone), we see an inferior class of animals (Reptiles) appear with an extraordinary abundance and variety of forms and enormity of bulk; so does this epoch, whose fauna has formed the subject of the present disquisition, display the culmination point of the highest class in the animal kingdom, the mammalia. Their time is now past: poor and weak does this class appear in the actual order of creation, compared with what it was in the former. Thus was it determined in the plan of Providence: the new theatre was destined to the development of a more exalted creature, the hour of whose appearance on the stage had sounded; and the inordinate proportions of animal life were reduced within their proper limits. Finally, the extreme degree of heat that had been so favourable to the increase of organic bulk, gave way to a milder temperature, that permitted the free development of intelligence; and *the Human period succeeded the Mammalian.*

In order to complete this sketch, I will, in conclusion, offer a brief view of the contributions of other naturalists to our knowledge on the subject. The first specimens of the fossil remains of South America were brought home by Dombey. They consisted of a few teeth and a portion of the under jaw of a species of *Mastodon,* which Cuvier recognized as identical with that whose remains have been found in Europe, *M. Angustidens.* This important subject did not escape Baron Humboldt's attention: he brought back with him some remains of the same genus, which Cuvier discovered to belong to two new species, besides the one already mentioned; these he named *M. Andium* and *M. Humboldtii.* Baron Humboldt found the remains of the first in Peru and Columbia, of the second in Ecuador and Bolivia, of the last in Chili. In all these countries the existence of such huge animal bones had given rise to the story of giants; which is also a very ancient tradition in Brazil, and evidently arises from the same source. Long ago Father Casale, in his 'Corographia Brasilica,' vol. i. p. 78, speaks of gigantic bones being found near the Rio das Contas, in the province of Bahia; and MM. Spix and Martius inform us that these bones belong to the above genus. So also M. A. de St. Hilaire, in his 'Travels,' vol. ii. p. 314, describes a molar tooth of *Mastodon* which was discovered in the sertão of the River San Francisco. But the most important discovery, and which excited the greatest attention among naturalists, was undoubtedly that of the remains of the monstrous and gigantic animal, to which Cuvier has given the name of *Megatherium.* A nearly perfect skeleton of this animal was brought to light in the year 1789, very near Buenos Ayres, and was sent by the governor, the Marquis of Loretto, to Madrid, where it has been set up and now remains. Besides this, a second is said to have been sent to the same city in 1795 from Lima, together with some fragments of a third from Paraguay. Since that time, in consequence of the representations of Sir Woodbine Parish, the English consul-general at Buenos Ayres, excavations have been made along the banks of the river Salado, as also of the stream Villa Nueva, and the lake Las Aveiras, which have furnished fragments of three other skeletons of the same animal, and which have been deposited in the Museum of the Geological Society in London.[1] Spix and Martius were the first who gave an account of the existence of fossil remains of Megalonyx in the caverns near the river S. Francisco. To M. Sello are we indebted for the discovery of a gigantic species of *Armadillo,* extracted from the banks of the river Uruguay; the fossils have been sent to Berlin, and have been described by Dalton; but as I have not seen his account, I can give no further information of this animal. And finally, I must not omit the extract from a letter addressed to M. de St. Hilaire by Senhor Damasio Larranaga, and published in the second edition of Cuvier's 'Recherches,' vol. i. p. 191, in which he announces the discovery of several portions of *Megatherium* in the republic of Uruguay; which, however, evidently belong to a gigantic species of the *Armadillo* family, and, as I suspect, to *Chlamydotherium gigas.*

The above is a brief account of my predecessors in this line, so far as they have come to my knowledge in my present retired position. It is sufficient to show that the subject was not new; the path had already been trodden by several respected naturalists; and the glimmering of light which their discoveries had scattered over this wide field, were in a high degree calculated to wake the attention of philosophers, and to excite the desire to see these researches extended. It was my fortunate lot to be able to contribute towards the accomplishment of this desire; but I acknowledge with gratitude, that if the facts described in the above pages have at all assisted in extending the boundaries of science, the merit thereof is due to the respected Society whose flattering encouragement gave me energy to overcome the difficulties of the undertaking, and whose generous assistance furnished me with the means of conducting my researches on such a scale as could alone lead to the desired results.

POSTSCRIPT.

Since the above communication was written, I have received a present, which, on account of its importance, de-

[1] I believe this to be erroneous; they are in the Museum of the College of Surgeons.—TRANSLATOR.

mands a supplementary notice. I have before mentioned that of each of the genera *Cutia*, *Paca* and *Capivar*, only a single species now exists in this district, whereas I have found two fossil species of *Cutia* and *Capivar*, and three of *Paca*; that of each of these genera one species is of gigantic proportions, while the remainder correspond in size to the existing species. Again, I have there observed that of the two lesser species of *Paca*[1], one resembles the living animal in its nearly smooth head, (although in the structure of its cranium it otherwise displays sufficient specific difference,) while the other species is distinguished by an extraordinary development of its zygomatic arches, as likewise by great inequalities on its cranium. I have at this moment received the cranium of a *Paca*, shot near Curvello, that exhibits all these characters of the fossil species, although a closer comparison convinces me also, in this instance, of their specific distinction. I have quite satisfied myself that this development of the zygomatic arches and unevenness of the surface of the cranium are not the effect of age, by the examination of a connected suite of the smooth-headed *Paca* from the earliest to the most advanced age, in which all the sutures have disappeared; whereas the cranium here spoken of belongs to a young animal, in which the sutures are still distinct. Instead of this, I have reason to suspect, however little such a phænomenon may appear to be supported by analogy, that this striking dissimilarity in the formation of the skull results only from a difference of sex. I rest this supposition on two principal facts; first, because, with this single exception of the crania, I do not find in all the other bones belonging to the *Paca* the least dissimilarity to betoken the existence of two distinct species; and, secondly, because the characters by which the fossil smooth-headed *Paca* is distinguished from the living, display a remarkable *parallelism* with the distinctions that characterize the rough-headed *Paca* of the same two periods.

Should this supposition be confirmed, then the two species that I have described under the names of *Cœlogenys rugiceps* and *C. laticeps*, must be reduced to one, which may preserve the name *laticeps*, and the genus *Paca* will thus possess the same proportions as the genera *Cutia* and *Capivar*.

[1] Dr. Lund does not appear aware that there are now existing in South America two species or varieties of Paca (*Cœlogenys*), the skulls of which differ precisely as above described. These differences were first pointed out by Cuvier in the 'Annales du Muséum,' tom. x. p. 203. pl. x.

Art. III.—*The Flora of Central Norfolk.* By Mr. R. J. Mann.

The county of Norfolk is, to the British naturalist, a field of abundant interest, in consequence of its forming one of the extremes of his native regions, in which the ocean marks out a defined boundary to the productions of the land, and changes, by its magic touch, the gaudy flower and waning grass into the green sea-weed. The botanic wealth of the most eastern point of England has been well displayed, in an admirable sketch of the natural history of Yarmouth, by the Messrs. C. and J. Paget. The present paper is an attempt to supply the next link in the chain of gradation, which terminates only at the shores of Sutherland. It embraces all those localities which fall within the reach of an ordinary day's march of the working naturalist from the vital centre of the district, the city of Norwich.

The substratum of Norfolk is identical with the great chalk formation of Europe, and its outcross constitutes the greater portion of the high ground of the county; its edge is marked by a straight line, a little inclined from the north and south direction, and upon the central portion of this edge is built the ancient city of Norwich. The eastern inclination of the cretaceous mass is covered by a series of beds composed chiefly of sand and loose ferruginous sandstones, mixed with gravel containing abundance of organic remains, whose characters distinctly register their ocean birth. These marine sandstones, known technically as the Norfolk crag, occupy upon the surface a broad band running parallel to the boundary of the chalk, and not frequently raised to more than a few feet of elevation above the level of the sea. The space intervening between this tract and the German Ocean is partially occupied by irregular masses of gravel and clay, formed by the action of water upon the older rocks of the more western counties, and swept thence by diluvial currents to their present localities, leaving marks of their progress at various stages of their course; at the same period that these waters were deluging the land, the then surface was channeled by some disturbing force into a series of valleys running more or less eastward, and uniting in that direction in a common termination. The next epoch in the history of these valleys must have presented them as estuaries of the German Ocean, their lower level being of necessity overflowed by its waters. In this state they appear to have remained until about the period of the Norman Conquest, when, from some uncertain cause or causes, the sea retreated to about its present bounds, and the bottoms of the valleys were raised, by alternate beds of moor and silt, to the high-water mark of the fresh streams that continue to drain through them. There are now three rivers meandering through the central tract of the estuaries that have preceded them, the lateral formations of moor and silt having been converted into valuable marsh and pasture lands; that to the northward of the three is the Bure; it is the most irregular in its course, and is connected with several shallow lakes that have been left where the silting process has been checked, or the draining less perfect. The second river is the Yare, which is rendered somewhat important by being navigable between Norwich and the sea-port of Yarmouth. The third river is the Waveney, which forms the natural boundary of the contiguous counties of Norfolk and Suffolk. At the confluence of these rivers a lake is formed, which reaches to within half a mile of the sea, then contracts again into a narrow stream, turns sharply to the south, and continues its course for three miles in that direction, separated from the ocean by a low tongue of sand only, which is overflown by the highest tides, and then suddenly empties itself into the German Ocean, where the cliffs of Suffolk commence to raise themselves above the flats of Norfolk.

The botanical stations of the county all bear a direct relation to the geological features which have been thus succinctly sketched.

In the immediate vicinity of Yarmouth the coast is a flat plain, but more to the north the sand has accumulated under the action of the wind and tide, and formed a line of sandbanks which offer a natural barrier to the encroachments of the ocean. In several positions these banks rise to a considerable height, and the labours of the plough may be seen to extend to within a quarter of a mile of the sea; the safety to the agricultural district, from its close neighbourhood to these loose and mobile sands, being provided for by a series of grasses and sedges, (the most characteristic of which are the *Carex arenaria* and the *Triticum junceum*,) which interlace their creeping roots, often extending to the distance of many yards, and thus bind into a firm soil that which would otherwise render useless to the cultivator the inland country for miles.

Somewhat more removed from the coast than these regions of sand, on the immediate banks of the lower parts of the rivers, and around the broader waters, is an extent of marsh ground entirely distinct from all other regions in its vegetable productions, in consequence of its being periodically overflown by salt water. These are known as the Salt Marshes, and are green during the summer with the wild celery, and gay in the autumn with the marsh mallow. The banks of the Yare, the Waveney, and the Bure, beyond the influence of the salt tides, are composed of marshy ground, in part putting on the appearance of loose bogs interspersed with firm tufts composed of the roots of *Carices*, rushes and grasses rising at intervals upon them. These are constantly drenched with water, and supported upon a subsoil of silt and turf of twenty feet in thickness. The marshes of Acley and Horning present characteristic specimens. Proceeding upwards along the banks of the rivers, these wet bogs gradually pass into drier and more stable meadows, in which the sedges and rushes are almost banished by the true grasses.

Some few miles to the north of Norwich is an extent of elevated heath and moor, in which are found the infant sources of the Bure. This tract is considerably higher than the level of the ancient estuaries, and in the summer assumes the appearance of a dry heath, yellow in the earlier months with the blossoms of the needle furze, but purple in August with the beautiful Calathian violet, the brilliant field being relieved at intervals by rusty spots of the sun dews, and interrupted occasionally by plantations of young pines, in which a constant war is waged between nature and man, the former in many instances appearing to have almost reclaimed that which had been abstracted from her domains. Throughout the winter these regions are almost entirely inundated and inaccessible; and if the adventurous botanist, remembering the summer gambols of the *Lacerta agilis*, and the treasures which he then reaped from the spot, should be induced to visit them at that period, they will afford to him the three species of Sphagna, and as much depth of the water in which they grow as he may please to wade into.

The edges of the elevated grounds are still covered in many places by the remains of woods and groves which have served as preserves to many of the species of plants affecting such regions.

Those districts which have fallen more immediately under the influence of man, and are employed for agricultural purposes, occupy all those portions of the chalk slopes and rising grounds which have been reclaimed from either heaths or woods, and of course constitute by far the larger portion of them. In these the native plants have been driven by the operations of husbandry into such corners as are unfitted for their purpose; the soil is formed by varying admixtures of sand, clay, lime, and the oxide of iron, the sand having been chiefly derived from the beds of crag; the clay from the diluvium formed probably by the disintegration of the lias series of the inland counties; the iron from the same diluvial beds, and the lime from the exhaustless magazine afforded by the chalk.

It will be readily imagined, that a country presenting the physical features that have been now described, must be rich in botanical productions; and that such is the case is proved by the details of wealth contained in the following List of the Flora of Central Norfolk:—

DICHLAMYDEÆ.

RANUNCULACEÆ.

ACONITUM *Napellus.* Naturalized, Whitlingham Wood.
AQUILEGIA *vulgaris.* Road-side, Porringland.
THALICTRUM *flavum.* Meadows, Thorpe.
ANEMONE *nemorosa.* Woods, Thorpe.
RANUNCULUS *aquatilis.* Ditches, common.
——— *hederaceus.* Thorpe, common.
——— *Lingua.* Horning Marshes.
——— *Flammula.* Marshes, common.
——— *Ficaria.* Pastures, common.
——— *sceleratus.* Ditches, common.
——— *acris.* Meadows, common.
——— *repens.* Meadows, common.
——— *bulbosus.* Pastures, common.
——— *hirsutus.* Waste ground, Norwich.
CALTHA *palustris.* Pastures, common.

BERBERIDEÆ.

BERBERIS *vulgaris.* Hedges, Catton.

NYMPHÆACEÆ.

NYMPHÆA *alba.* Horning, Surlingham.
NUPHAR *lutea.* Running streams, common.

PAPAVERACEÆ.

PAPAVER *Rhœas.* Corn-fields, common.
——— *Argemone.* Road-sides, common.
——— *somniferum.* Corn-fields, Framlingham.
CHELIDONIUM *majus.* Waste places, common.

FUMARIACEÆ.

FUMARIA *officinalis.* Road-sides, common.
CORYDALIS *claviculata.* Woods, Thorpe.

CRUCIFERÆ.

CORONOPUS *Ruellii.* Road-sides, common.
CAPSELLA *Bursa-Pastoris.* Waste places, common.
TEESDALIA *nudicaulis.* Telegraph Lane, Thorpe.
LEPIDIUM *campestre.* Road-sides, Blofield.
COCHLEARIA *Armoracia.* Waste places, common.

DRABA *verna*. Wall-tops, common.
KONIGA *maritima*. Around Thorpe Rosary (naturalized.)
CARDAMINE *pratensis*. Meadows, common.
——— *hirsuta*. Meadows, common.
——— *amara*. Meadows, Thorpe.
ARABIS *hirsuta*. Walls of Lakenham Church-yard.
TURRITIS *glabra*. Hedges, Thorpe, Blofield.
BARBAREA *vulgaris*. Road-sides, common.
NASTURTIUM *officinale*. Ditches, common.
——— *terrestre*. Ditches, Thorpe.
SISYMBRIUM *officinale*. Road-sides, common.
——— *Sophia*. Waste places, common.
——— *thalianum*. Road-sides, common.
ERYSIMUM *Alliaria*. Road-sides, common.
——— *cheiranthoides*. Corn-fields, common.
CHEIRANTHUS *Cheiri*. Wall-tops, common.
BRASSICA *oleracea*. Waste places, common.
——— *Rapa*. Waste places, common.
SINAPIS *alba*. Road-sides, Trowse.
——— *nigra*. Road-sides, common.
——— *arvensis*. Road-sides, common.
RAPHANUS *Raphanistrum*. Road-sides, common.

VIOLACEÆ.

VIOLA *odorata*. Hedge-banks, common.
——— *canina*. Road-sides, common.
——— *tricolor*. Road-sides, common.

DROSERACEÆ.

DROSERA *anglica*. Felthorpe bogs.
——— *longifolia*. St. Faith's Heath.
——— *rotundifolia*. St. Faith's Heath.
PARNASSIA *palustris*. Marshes, Horning.

POLYGALEÆ.

POLYGALA *vulgaris*. Heathy grounds, common.

MALVACEÆ.

MALVA *sylvestris*. Road-sides, common.
——— *rotundifolia*. Road-sides, common.
——— *moschata*. Postwick Grove.

HYPERICINEÆ.

HYPERICUM *Androsæmum*. Arminghall Wood.
——— *quadrangulum*. Road-sides, common.
——— *perforatum*. Road-sides, common.
——— *humifusum*. Telegraph Lane, Thorpe.
——— *montanum*. Tuck's Wood, Lakenham.
——— *hirsutum*. Arminghall Wood.
——— *pulchrum*. Heathy grounds, common.
——— *elodes*. St. Faith's bogs.

CARYOPHYLLEÆ.

SAGINA *procumbens*. Waste places round Norwich.
HOLOSTEUM *umbellatum*. Walls, St. Faith's Lane, Norwich.

SAPONARIA *officinalis*. Road-sides, common.
DIANTHUS *Armeria*. Thorpe Groves.
SILENE *inflata*. Road-sides, Thorpe.
——— *noctiflora*. Woods, Thorpe.
STELLARIA *media*. Road-sides, common.
——— *holostea*. Road-sides, common.
——— *graminea*. Porringland Heath.
——— *glauca*. Marshes, Thorpe.
——— *uliginosa*. Marshes, near Carow Bridge, Norwich.
ARENARIA *trinervis*. Woods, Thorpe.
——— *serpyllifolia*. Old Walls, Norwich.
——— *tenuifolia*. Corn-fields, Thorpe.
——— *rubra*. Mousehold Heath.
AGROSTEMMA *Githago*. Corn-fields, common.
LYCHNIS *Flos Cuculi*. Road-sides, common.
——— *dioica*. Road-sides, common.
CERASTIUM *vulgatum*. Road-sides, common.
——— *viscosum*. Moist hedges, common.
SPERGULA *arvensis*. Telegraph Lane, Thorpe.
——— *nodosa*. Marshes, Thorpe.

LINEÆ.

LINUM *catharticum*. Road-sides, Porringland.
RADIOLA *millegrana*. St. Faith's bogs.

TILIACEÆ.

TILIA *europæa*. Frequent about Norwich.

ACERINEÆ.

ACER *Pseudo-platanus*. Road-sides, common.
—— *campestre*. Road-sides, common.

GERANIACEÆ.

GERANIUM *pyrenaicum*. Road-sides, Lakenham.
——— *lucidum*. Road-sides, Lakenham.
——— *robertianum*. Road-sides, common.
——— *molle*. Road-sides, common.
——— *pusillum*. Waste ground, Thorpe.
——— *dissectum*. Telegraph Lane, Thorpe,
——— *columbinum*. Telegraph Lane, Thorpe.
ERODIUM *cicutarium*. Road-sides, common.

OXALIDEÆ.

OXALIS *Acetosella*. Woods, Thorpe.

PORTULACEÆ.

MONTIA *fontana*. Mousehold Heath.

CRASSULACEÆ.

TILLÆA *muscosa*. Porringland Heath.
SEMPERVIVUM *tectorum*. House-tops, occasionally.
SEDUM *Telephium*. Woods, Thorpe.
——— *reflexum*. Wall-tops, Thorpe.
——— *acre*. Wall-tops, Norwich.

SAXIFRAGEÆ.

SAXIFRAGA *granulata.* Hedge-banks, common.
——— *tridactylites.* Wall-tops, common.
ADOXA *Moschatellina.* Woods, Whitlingham.

SALICAREÆ.

PEPLIS *Portula.* St. Faith's bogs.
LYTHRUM *Salicaria.* Marshes, common.

RHAMNEÆ.

RHAMNUS *catharticus.* Hedges around Norwich.
——— *Frangula.* Horning Marshes.

ILICINEÆ.

ILEX *Aquifolium.* Woods, Thorpe.

LEGUMINOSÆ.

ULEX *europæus.* Heaths, common.
GENISTA *anglica.* St. Faith's Heaths.
CYTISUS *scoparius.* Heaths, common.
ONONIS *arvensis.* Porringland Heath.
ANTHYLLIS *vulneraria.* Catton gravel-pits.
LATHYRUS *pratensis.* Marshes, common.
——— *palustris.* Marshes, Horning.
VICIA *Cracca.* Hedge-banks, common.
——— *sativa.* Corn-fields, common.
——— *sepium.* Corn-fields, common.
ERVUM *hirsutum.* Corn-fields, Thorpe.
——— *tetraspermum.* Corn-fields, Blofield.
ORNITHOPUS *perpusillus.* Mousehold Heath.
MELILOTUS *officinalis.* Gravel-pits, Thorpe.
——— *leucantha.* Marshes, Thorpe.
TRIFOLIUM *ornithopodioides.* Mousehold Heath.
——— *repens.* Pastures, common.
——— *subterraneum.* Mousehold Heath.
——— *pratense.* Pastures, common.
——— *arvense.* Woods, Thorpe.
——— *glomeratum.* Mousehold Heath.
——— *striatum.* Mousehold Heath.
——— *filiforme.* Road-sides, common.
LOTUS *corniculatus.* Wall-tops, Thorpe.
——— *major.* Pastures, common.
MEDICAGO *falcata.* Meadows, Thorpe.
——— *lupulina.* Waste grounds, common.

ROSACEÆ.

PRUNUS *insititia.* Woods, Thorpe.
——— *spinosa.* Hedges, common.
——— *Cerasus.* Woods, Thorpe.
SPIRÆA *Ulmaria.* Meadows, common.
GEUM *urbanum.* Road-sides, common.
——— *rivale.* Marshes, Whitlingham.
RUBUS *corylifolius.* Hedges, common.
——— *fruticosus.* Hedges, common.
——— *idæus.* Woods, Thorpe.

FRAGARIA *elatior*. Tuck's Wood, Lakenham.
——— *vesca*. Hedge-banks, common.
COMARUM *palustre*. Marshes, Thorpe, St. Faith's.
POTENTILLA *anserina*. Road-sides, common.
——— *reptans*. Pastures, common.
——— *argentea*. Woods, Thorpe.
——— *Fragariastrum*. Woods, Thorpe.
TORMENTILLA *officinalis*. Heaths, common.
AGRIMONIA *Eupatoria*. Road-sides, common.
ALCHEMILLA *arvensis*. Dry pastures, common.
ROSA *arvensis*. Hedges, common.
—— *canina*. Hedges, common.
MESPILUS *germanica*. Tuck's Wood, Lakenham.
CRATÆGUS *Oxyacantha*. Hedges, common.
PYRUS *aucuparia*. Road-sides, common.
——— *Malus*. Road-sides, Bramerton.

ONAGRARIÆ.

EPILOBIUM *hirsutum*. Marshes, Thorpe.
——— *parviflorum*. Marshes, common.
——— *montanum*. Tuck's Wood, Lakenham.
——— *tetragonum*. St. Faith's bogs.
——— *palustre*. St. Faith's bogs.
ŒNOTHERA *biennis*. Waste places, Thorpe.

HALORAGEÆ.

HIPPURIS *vulgaris*. Ditches, Horning, Acley.
MYRIOPHYLLUM *spicatum*. Ditches, Thorpe.

UMBELLIFERÆ.

HYDROCOTYLE *vulgaris*. Moist spots, Thorpe, St. Faith's.
SANICULA *europæa*. Arminghall Wood.
CICUTA *virosa*. Ditches, Horning.
PETROSELINUM *sativum*. Waste ground, Thorpe.
HELOSCIADIUM *nodiflorum*. Ditches, common.
——— *repens*. Ditches, Thorpe.
——— *inundatum*. Horsford Heath.
ÆGOPODIUM *Podagraria*. Tuck's Wood, Lakenham.
BUNIUM *flexuosum*. Woods, Thorpe.
PIMPINELLA *saxifraga*. Road-sides, Thorpe.
SIUM *latifolium*. Ditches, Horning.
—— *angustifolium*. Marshes, Thorpe.
ŒNANTHE *fistulosa*. Ditches, common.
——— *pimpinelloides*. Ditches, Horning.
——— *Phellandrium*. Ditches, Thorpe.
FŒNICULUM *vulgare*. Waste places, Thorpe.
ÆTHUSA *Cynapium*. Road-sides, common.
ANGELICA *sylvestris*. Marshes, common.
PEUCEDANUM *palustre*. Marshes, Horning.
PASTINACA *sativa*. Waste places, Thorpe.
HERACLEUM *Sphondylium*. Road-sides, common.
DAUCUS *Carota*. Road-sides, common.
TORILIS *Anthriscus*. Road-sides, Thorpe.
——— *infesta*. Road-sides, Thorpe.
——— *nodosa*. Road-sides, Thorpe.
SCANDIX *Pecten*. Corn-fields, common.

ANTHRISCUS *sylvestris*. Road-sides, common.
——— *vulgaris*. Road-sides, common.
CHÆROPHYLLUM *temulentum*. Road-sides, common.
CONIUM *maculatum*. Road-sides, common.
SMYRNIUM *Olusatrum*. Waste places, Thorpe.

STELLATÆ.

GALIUM *verum*. Dry banks, common.
——— *cruciatum*. Thickets, common.
——— *palustre*. Marshes, common.
——— *erectum*. Road-sides, Thorpe.
——— *saxatile*. Heathy ground, common.
——— *Mollugo*. Road-sides about Norwich.
——— *Aparine*. Road-sides, common.
SHERARDIA *arvensis*. Corn-fields, Thorpe.
ASPERULA *odorata*. Woods, Thorpe.

CAPRIFOLIACEÆ.

LONICERA *Periclymenum*. Road-sides, common.
VIBURNUM *Opulus*. River-bank, Bramerton.
SAMBUCUS *nigra*. Road-sides, common.
——— *Ebulus*. Tuck's Wood, Lakenham.
CORNUS *sanguinea*. Arminghall Wood.
HEDERA *Helix*. Old walls, common.

LORANTHEÆ.

VISCUM *album*. Apple-trees, Porringland.

CAMPANULACEÆ.

CAMPANULA *Trachelium*. Corn-fields, Postwick.
——— *latifolia*. Road-sides, Porringland.
——— *rotundifolia*. Road-sides, common.

LOBELIACEÆ.

JASIONE *montana*. Corn-fields, Thorpe.

VALERIANEÆ.

VALERIANA *officinalis*. Marshes, common.
——— *rubra*. Wall-tops, Norwich.
——— *dioica*. Marshes, Thorpe.
FEDIA *olitoria*. Road-sides, Thorpe.

DIPSACEÆ.

DIPSACUS *sylvestris*. Road-sides, Thorpe.
SCABIOSA *succisa*. Marshes, Horning.
KNAUTIA *arvensis*. Corn-fields, common.

COMPOSITÆ.

TRAGOPOGON *major*. Waste places, common.
PICRIS *hieracioides*. Woods, Whitlingham.
SONCHUS *oleraceus*. Road-sides, common.
——— *arvensis*. Marshes, common.
PRENANTHES *muralis*. Wall-tops, Norwich.
LEONTODON *Taraxacum*. Road-sides, common.
APARGIA *hispida*. Wall-tops, Thorpe.

APARGIA *autumnalis.* Road-sides, Thorpe.
THRINCIA *hirta.* Road-sides, Thorpe.
HIERACIUM *subaudum.* Woods, Thorpe.
——— *sylvaticum.* Woods, Thorpe.
——— *umbellatum.* Hedge-bank, Plumstead road.
——— *Pilosella.* Road-sides, common.
CREPIS *tectorum.* Road-sides, common.
HYPOCHÆRIS *radicata.* Road-sides, Thorpe.
LAPSANA *communis.* Road-sides, Thorpe.
CICHORIUM *Intybus.* Road-sides, common.
ARCTIUM *Lappa.* Road-sides, common.
CARDUUS *nutans.* Road-sides, common.
——— *acanthoides.* Road-side, Blofield.
——— *marianus.* Road-sides, Kirby.
CNICUS *lanceolatus.* Road-sides, common.
——— *palustris.* Marshes, common.
——— *arvensis.* Fields, common.
——— *eriophorus.* Waste ground, Framlingham.
——— *pratensis.* Marshes, Horning.
CENTAUREA *nigra.* Pastures, common.
——— *Cyanus.* Corn-fields, common.
——— *Scabiosa.* Road-sides, common.
——— *Calcitrapa.* Road-sides, Thorpe.
BIDENS *cernua.* Marshes, common.
EUPATORIUM *cannabinum.* River-banks, common.
TANACETUM *vulgare.* Road-sides, Thorpe.
ARTEMISIA *Absinthium.* Waste places, Thorpe.
——— *vulgaris.* Road-sides, common.
GNAPHALIUM *uliginosum.* St. Faith's bogs.
——— *minimum.* Road-sides, Thorpe.
——— *germanicum.* Road-sides, common.
ERIGERON *acre.* Woods, Whitlingham.
TUSSILAGO *Farfara.* Gravel-pits, common.
PETASITES *vulgaris.* By Cringleford Bridges.
SENECIO *vulgaris.* Road-sides, common.
——— *sylvaticus.* Woods, Thorpe.
——— *tenuifolius.* Road-sides, Thorpe.
——— *Jacobæa.* Road-sides, common.
——— *aquaticus.* Marshes, common.
SOLIDAGO *Virgaurea.* Road-sides, Porringland.
PULICARÍA *dysenterica.* Pastures, common.
——— *vulgaris.* Clay-pit, St. Faith's.
BELLIS *perennis.* Pastures, common.
CHRYSANTHEMUM *Leucanthemum.* Pastures, Porringland.
——— *segetum.* Corn-fields, Porringland.
PYRETHRUM *Parthenium.* Road-sides, Thorpe.
——— *inodorum.* Corn-fields, common.
MATRICARIA *Chamomilla.* Waste places, common.
ANTHEMIS *arvensis.* Corn-fields, common.
——— *Cotula.* Corn-fields, common.
ACHILLEA *Millefolium.* Road-sides, common.
——— *Ptarmica.* Heaths, St. Faith's, Porringland.

BORAGINEÆ.

ECHIUM *vulgare.* Waste places, common.
SYMPHYTUM *tuberosum.* Woods, Thorpe.

Borago *officinalis.* Waste places, common.
Lycopsis *arvensis.* Waste places, common.
Myosotis *arvensis.* Corn-fields, common.
——— *palustris.* Pastures, common.
——— *versicolor.* Road-sides, Thorpe.
——— *sylvatica.* Woods, Whitlingham.
——— *collina.* Wall-tops, Norwich.
Cynoglossum *officinale.* Woods, Whitlingham.
Anchusa *sempervirens.* Woods, Whitlingham.

CONVOLVULACEÆ.

Convolvulus *arvensis.* Road-sides, common.
——— *sepium.* Road-sides, common.
Cuscuta *Epithymum.* Heaths, Mousehold, St. Faith's.

PLANTAGINEÆ.

Littorella *lacustris.* Felthorpe Heath.
Plantago *major.* Pastures, common.
——— *media.* Pastures, common.
——— *lanceolata.* Pastures, common.
——— *Coronopus.* Road-sides, common.

OLEACEÆ.

Ligustrum *vulgare.* Road-sides, common.
Fraxinus *excelsior.* Road-sides, common.

ERICEÆ.

Erica *Tetralix.* Heaths, St. Faith's, Bramerton.
——— *cinerea.* Heaths, common.
Calluna *vulgaris.* Heaths, common.

GENTIANEÆ.

Erythræa *Centaurium.* Dry pastures, Thorpe.
Gentiana *Pneumonanthe.* St. Faith's Heath.
——— *Amarella.* St. Faith's Heath.
Menyanthes *trifoliata.* Marshes, Thorpe.

SOLANEÆ.

Hyoscyamus *niger.* Waste places, Costessey.
Atropa *Belladonna.* Hedges near Thorpe Asylum.
Verbascum *Blattaria.* Road-sides, Thorpe.
——— *pulverulentum.* Road-sides, common.
——— *Thapsus.* Hedge-banks, frequent.
Solanum *nigrum.* Waste ground, common.
——— *Dulcamara.* Moist spots, common.

PRIMULACEÆ.

Anagallis *arvensis.* Road-sides, common.
——— *tenella.* St. Faith's bogs.
Lysimachia *vulgaris.* Marshes, Horning.
——— *Nummularia.* Moist banks, Thorpe.
——— *nemorum.* Arminghall Wood.
Hottonia *palustris.* Ditches, Horning.
Primula *vulgaris.* Pastures, common.
——— *veris.* Pastures, Thorpe.

PRIMULA *elatior.* Pastures, Porringland.
SAMOLUS *Valerandi.* Ditches, Acley.

LENTIBULARIÆ.

PINGUICULA *vulgaris.* Horsford Heath.

SCROPHULARINEÆ.

VERONICA *serpyllifolia.* Road sides, common.
———— *scutellata.* Horsford Heath.
———— *Anagallis.* Ditches, common.
———— *Beccabunga.* Ditches, common.
———— *officinalis.* Pastures, Porringland.
———— *Chamædrys.* Hedge-banks, common.
———— *hederifolia.* Hedge-banks, common.
———— *agrestis.* Waste ground, Catton.
———— *polita.* Corn-fields, common.
———— *arvensis.* Corn-fields, common.
BARTSIA *Odontites.* Corn-fields, common.
EUPHRASIA *officinalis.* Pastures, common.
RHINANTHUS *Crista-galli.* Marshes, common.
MELAMPYRUM *pratense.* Woods, Thorpe.
PEDICULARIS *palustris.* Marshes, common.
———— *sylvatica.* St. Faith's bogs.
ANTIRRHINUM *majus.* Wall-tops, Norwich.
———— *Orontium.* Corn-fields, Thorpe.
LINARIA *Cymbalaria.* Old walls, common.
———— *Elatine.* Corn fields, Blofield.
———— *vulgaris.* Hedge-banks, common.
SCROPHULARIA *aquatica.* Marshes, common.
———— *nodosa.* Woods, Thorpe.

OROBANCHEÆ.

OROBANCHE *minor.* Clover-fields, Thorpe.

VERBENACEÆ.

VERBENA *officinalis.* Pastures, common.

LABIATÆ.

LYCOPUS *europæus.* Marshes, Thorpe.
SALVIA *verbenaca.* Road-side, Thorpe.
MENTHA *piperita.* Marshes, Lakenham.
———— *hirsuta.* Marshes, common.
———— *arvensis.* Clay-pit, St. Faith's.
———— *Pulegium.* Clay-pit, St. Faith's.
THYMUS *Serpyllum.* Dry pastures, common.
ORIGANUM *vulgare.* Mackie's Dell, near Norwich.
TEUCRIUM *Scorodonia.* Pastures, common.
———— *Chamædrys.* Wall-tops, Norwich.
AJUGA *reptans.* Road-side, Thorpe.
BALLOTA *nigra.* Hedge-banks, common.
LEONURUS *Cardiaca.* Hedge-banks, Earlham.
GALEOBDOLON *luteum.* Woods, Thorpe.
GALEOPSIS *Ladanum.* Chalk-pit, Lakenham.
———— *Tetrahit.* Woods, Thorpe.
LAMIUM *album.* Road-sides, common.

LAMIUM *purpureum.* Road-sides, common.
——— *incisum.* Hedge-banks, Horstead.
——— *amplexicaule.* Corn-fields, Whitlingham.
STACHYS *sylvatica.* Woods, common.
——— *palustris.* River-banks, common.
NEPETA *cataria.* Road-sides, frequent.
GLECHOMA *hederacea.* Hedge-banks, common.
CALAMINTHA *Nepeta.* Road-sides, Lakenham.
——— *officinalis.* Road-sides, Lakenham.
CLINOPODIUM *vulgare.* Woods, Whitlingham.
PRUNELLA *vulgaris.* Pastures, common.
SCUTELLARIA *galericulata.* Marshes, Postwick.

CUCURBITACEÆ.

BRYONIA *dioica.* Hedges, frequent.

MONOCHLAMYDEÆ.

THYMELEÆ.

DAPHNE *Laureola.* Tuck's Wood, Lakenham.

POLYGONEÆ.

POLYGONUM *aviculare.* Waste places, common.
——— *Fagopyrum.* Fields, Thorpe.
——— *Convolvulus.* Corn-fields, common.
——— *amphibium.* Ditches, Thorpe.
——— *Persicaria.* Waste ground, common.
——— *lapathifolium.* Waste ground, common.
——— *Hydropiper.* Marshes, common.
RUMEX *Hydrolapathum.* River-banks, frequent.
——— *crispus.* Road-sides, common.
——— *acutus.* Moist pastures, common.
——— *pulcher.* Road-sides, Thorpe.
——— *obtusifolius.* Road-sides, Thorpe.
——— *maritimus.* Marshes, Horning.
——— *Acetosa.* Pastures, common.
——— *Acetosella.* Pastures, common.

CHENOPODEÆ.

CHENOPODIUM *olidum.* St. Magdalen's Gates, Norwich.
——— *Bonus Henricus.* Waste places, common.
——— *rubrum.* Waste ground, common.
——— *murale.* Waste ground, Norwich.
——— *album.* Waste ground, common.
ATRIPLEX *patula.* Waste ground, common.
——— *angustifolia.* Road-sides, common.

SCLERANTHEÆ.

SCLERANTHUS *anuus.* Wall-tops, common.

URTICEÆ.

URTICA *dioica.* Waste ground, common.
——— *urens.* Waste ground, common.

HUMULUS *Lupulus*. Hedges, common.
PARIETARIA *officinalis*. Old walls, common.

RESEDACEÆ.

RESEDA *Luteola*. Waste ground, common.
——— *lutea*. Road-sides, frequent.

EUPHORBIACEÆ.

EUPHORBIA *helioscopia*. Waste ground, common.
——— *exigua*. Corn-fields, Blofield.
——— *Peplus*. Waste ground, common.
——— *amygdaloides*. Woods, Thorpe.
MERCURIALIS *perennis*. Road-sides, common.
——— *annua*. Road-sides, frequent.
BUXUS *sempervirens*. Woods, Thorpe.

CERATOPHYLLEÆ.

CERATOPHYLLUM *demersum*. Ditches, Thorpe.

ULMACEÆ.

ULMUS *campestris*. Hedges, common.

ACHLAMYDEÆ.

AMENTACEÆ.

BETULA *alba*. Road-sides, common.
CARPINUS *Betulus*. Woods, Thorpe.
ALNUS *glutinosa*. River-banks, common.
SALIX *repens*. St. Faith's Heath.
——— *fragilis*. Thorpe, common.
——— *alba*. Road-sides, common.
——— *viminalis*. Marshes, common.
POPULUS *alba*. Woods, common.
——— *tremula*. Woods, Thorpe.
——— *nigra*. River-banks, common.

CUPULIFERÆ.

FAGUS *sylvatica*. Woods, common.
QUERCUS *Robur*. Road-sides, common.
CORYLUS *Avellana*. Road-sides, common.

MYRICEÆ.

MYRICA *Gale*. Horning Marshes.

CALLITRICHINEÆ.

CALLITRICHE *verna*. Ditches, common.

MONOCOTYLEDONES.

AROIDEÆ.

ARUM *maculatum*. Hedge-banks, common.

TYPHACEÆ.

TYPHA *latifolia*. Running streams, frequent.
——— *angustifolia*. Marshes, Horning.
SPARGANIUM *ramosum*. Ditches, Horning.
——— *simplex*. Ditches, Thorpe.

FLUVIALES.

POTAMOGETON *lucens*. Running streams, frequent.
——— *natans*. Ditches, Horning.
——— *perfoliatus*. Rivulet, Costessey.
——— *crispus*. Ditches, Horning.
——— *densus*. Ditches, Thorpe.
——— *pectinatus*. Ditches, Horning.

PISTIACEÆ.

LEMNA *polyrrhiza*. Ditches, Thorpe.
——— *trisulca*. Ditches. Thorpe.
——— *minor*. Ditches, Thorpe.
——— *gibba*. Ditches, Thorpe.

JUNCAGINEÆ.

TRIGLOCHIN *palustre*. Ditches, Thorpe.

ALISMACEÆ.

ALISMA *Plantago*. Ditches, common.
——— *ranunculoides*. St. Faith's bogs.
SAGITTARIA *sagittifolia*. Ditches, common.

HYDROCHARIDEÆ.

HYDROCHARIS *Morsus ranæ*. Ditches, common.
STRATIOTES *aloides*. Ditches, Horning.

IRIDEÆ.

IRIS *Pseud-acorus*. Ditches, common.

ORCHIDEÆ.

ORCHIS *latifolia*. Marshes, Horning.
——— *Morio*. Porringland Heath.
——— *maculata*. Tuck's Wood, Lakenham.
——— *pyramidalis*. Bixley church-yard.
——— *mascula*. Woods, Swainsthorpe.
OPHRYS *apifera*, Tuck's Wood, Lakenham.
HABENARIA *bifolia*. St. Faith's Heath.
EPIPACTIS *palustris*. Felthorpe bogs.
LISTERA *ovata*. Woods, Whitlingham.

ASPHODELEÆ.

CONVALLARIA *majalis*. Woods, Thorpe.
RUSCUS *aculeatus*. Woods, Thorpe.
ALLIUM *vineale*. Waste ground, Lakenham.
——— *ursinum*. Arminghall Wood.

DIOSCOREÆ.

TAMUS *communis*. Hedges, common.

BUTOMEÆ.

BUTOMUS *umbellatus*. Ditches, common.

JUNCEÆ.

JUNCUS *obtusiflorus*. Marshes, frequent.
——— *acutiflorus*. Marshes, frequent.
——— *lampocarpus*. Marshes, frequent.
——— *glaucus*. Marshes, common.
——— *effusus*. Marshes, common.
——— *conglomeratus*. Felthorpe bogs.
——— *bufonius*. Marshes, Thorpe.
——— *uliginosus*. St. Faith's bogs.
——— *squarrosus*. St. Faith's bogs.
LUZULA *sylvatica*. Woods, Thorpe.
——— *campestris*. Woods, Thorpe.

CYPERACEÆ.

CLADIUM *Mariscus*. Marshes, Horning.
SCHŒNUS *nigricans*. Marshes, Horning.
SCIRPUS *lacustris*. Ditches, Horning, Surlingham.
BLYSMUS *compressus*. Marshes, Lakenham.
ELEOCHARIS *palustris*. Ditches, common.
——— *cæspitosa*. Marshes, Thorpe.
ERIOPHORUM *angustifolium*. Marshes, Postwick.
CAREX *intermedia*. Marshes, common.
——— *muricata*. Marshes, common.
——— *vulpina*. Road-side, Bixley.
——— *paniculata*. Marshes, common.
——— *ovalis*. Moist ground, Bixley.
——— *strigosa*. Arminghall Wood.
——— *sylvatica*. Woods, common.
——— *Pseudo-cyperus*. Marshes, Thorpe, Horning.
——— *flava*. Marshes, common.
——— *binervis*. St. Faith's Heath.
——— *præcox*. Pastures, Thorpe.
——— *pilulifera*. Mousehold Heath.
——— *panicea*. Marshes, common.
——— *recurva*. Marshes, common.
——— *cæspitosa*. Marshes, common.
——— *stricta*. Marshes, Postwick.
——— *acuta*. Marshes, common.
——— *paludosa*. Marshes, Lakenham.
——— *riparia*. Marshes, common.
——— *ampullacea*. Marshes, common.
——— *hirta*. Marshes, Postwick.

GRAMINEÆ.

ANTHOXANTHUM *odoratum*. Pastures, common.
NARDUS *stricta*. St. Faith's Heath.
ALOPECURUS *pratensis*. Pastures, common.
——— *agrestis*. Road-sides, common.
——— *geniculatus*. Ditches, common.
PHALARIS *canariensis*. Naturalized in field borders.
——— *arundinacea*. River-sides, common.
PHLEUM *pratense*. Pastures, common.

MILIUM *effusum.* Woods, Thorpe.
AGROSTIS *canina.* Road-sides, frequent.
——— *Spica venti.* Corn-fields, Thorpe.
——— *vulgaris.* Hedge-banks, common.
——— *alba.* Hedge-banks, common.
CATABROSA *aquatica.* River-banks, common.
AIRA *cristata.* Mousehold Heath.
—— *cæspitosa.* Felthorpe bogs.
—— *caryophyllea.* Pastures, occasionally.
—— *præcox.* Mousehold Heath.
MELICA *uniflora.* Woods, common.
——— *cærulea.* St. Faith's Heath.
HOLCUS *mollis.* Pastures, common.
——— *lanatus.* Pastures, common.
ARRHENATHERIUM *avenaceum.* Hedge-banks, common.
POA *aquatica.* River-sides, common.
—— *fluitans.* Ditches, common.
—— *rigida.* Wall-tops, common.
—— *compressa.* Old walls, Norwich.
—— *trivialis.* Road-sides, common.
—— *pratensis.* Pastures, common.
—— *annua.* Pastures, common.
TRIODIA *decumbens.* St. Faith's Heath.
BRIZA *media.* Pastures, common.
DACTYLIS *glomerata.* Pastures, common.
CYNOSURUS *cristatus.* Pastures, common.
FESTUCA *ovina.* Hedge-banks, Thorpe.
——— *duriuscula.* Hedge-banks, common.
——— *bromoides.* Road-side, Thorpe.
——— *Myurus.* Road-side, Thorpe.
——— *loliacea.* Meadows, Thorpe.
——— *pratensis.* Meadows, common.
——— *gigantea.* Marshes, Thorpe.
BROMUS *sterilis.* Road-sides, common.
——— *mollis.* Road-sides, common.
AVENA *flavescens.* Road-sides, common.
ARUNDO *Phragmites.* River-banks, common.
HORDEUM *murinum.* Waste ground, common.
TRITICUM *repens.* Waste ground, common.
BRACHYPODIUM *sylvaticum.* Hedge-banks, common.
LOLIUM *perenne.* Pastures, common.

FILICES.

POLYPODIUM *vulgare.* Hedge-banks, common.
ASPIDIUM *aculeatum.* Hedge-banks, occasionally.
——— *Filix mas.* Earlham Grove.
——— *spinulosum.* Woods, Thorpe.
ASPLENIUM *Adiantum nigrum.* Earlham Grove.
SCOLOPENDRIUM *vulgare.* Earlham Grove.
PTERIS *aquilina.* Heaths, common.
BLECHNUM *boreale.* Heaths, Bramerton, Mousehold.
OSMUNDA *regalis.* Marshes, Horning.
OPHIOGLOSSUM *vulgatum.* Woods, Whitlingham.

LYCOPODIACEÆ.

LYCOPODIUM *inundatum.* St. Faith's bogs.

EQUISETACEÆ.

EQUISETUM *arvense.* Road-sides, common.
——— *limosum.* Marshes, common.
——— *palustre.* Marshes, common.
——— *sylvaticum.* Pastures, Thorpe.

MUSCI.

PHASCUM *subulatum.* Moist banks, Thorpe.
SPHAGNUM *acutifolium.* St. Faith's bogs.
——— *obtusifolium.* St. Faith's bogs.
BARTRAMIA *pomiformis.* Moist banks, Thorpe.
GYMNOSTOMUM *ovatum.* Road-sides, Thorpe.
WEISSIA *controversa.* Hedge-banks, Thorpe.
GRIMMIA *pulvinata.* Wall-tops, common.
DICRANUM *varium.* Chalk-pits, Thorpe.
——— *heteromallum.* Hedge-banks, Thorpe.
——— *bryoides.* Hedge-bank, Telegraph Lane.
TORTULA *fallax.* Chalk-pits, Thorpe.
——— *rigida.* Road-sides, Postwick.
——— *ruralis.* House-tops, occasionally.
——— *subulata.* Hedge-banks, common.
——— *muralis.* Wall-tops, common.
POLYTRICHUM *commune.* Banks, Telegraph Lane.
——— *piliferum.* Mousehold Heath.
——— *undulatum.* Woods, Thorpe.
——— *aloides.* Hedge-banks, Thorpe.
——— *nanum.* Banks, Telegraph Lane.
ORTHOTRICHUM *striatum.* Bark of trees, common.
——— *diaphanum.* Old trees, Thorpe.
——— *affine.* Old trees, Thorpe.
BRYUM *androgynum.* Woods, Thorpe.
——— *cæspititium.* Walls and banks, common.
——— *capillare.* Hedge-banks, common.
——— *ligulatum.* Woods, Whitlingham.
——— *palustre.* Felthorpe bogs.
——— *ventricosum.* Felthorpe bogs.
FUNARIA *hygrometrica.* Chalk-pits, common.
HYPNUM *splendens.* St. Faith's bogs.
——— *purum.* St. Faith's bogs.
——— *cupressiforme.* St. Faith's bogs.
——— *rutabulum.* Hedge banks, common.
——— *prælongum.* Woods, Thorpe.
——— *sericeum.* Hedge-banks, Thorpe.
——— *plumosum.* Hedge-banks, Thorpe.
——— *serpens.* Woods, Whitlingham.
——— *cuspidatum.* Marshes, Thorpe.
——— *cordifolium.* St. Faith's bogs.

March, 1840. R. J. M.

REVIEWS.

Art. IV.—*A Report on the Progress of Vegetable Physiology during the year* 1837. By F. J. F. Meyen, M.D., Professor of Botany in the University of Berlin. Translated from the German by William Francis, Associate of the Linnæan Society. R. and J. E. Taylor.

Our continental scientific brethren far exceed our own countrymen in the modes they adopt of making known the results of their researches to the world. Independently of a numerous and well-arranged series of journals, under the various titles of Journaux and Annales in France, and Annalen, Archiv, Repertoria, &c. in Germany, we see, emanating from the press of these respective countries, a series of scientific annuals in the shape of reports on the progress of science, edited by some scientific man, whose name in general is alone a sufficient guarantee for their accuracy. Even Sweden, under the auspices of Berzelius, is not only not behind-hand, but may be said to have set this excellent example; for eighteen years has the Jahres-bericht des Physischer Wissenschaften regularly appeared at Stockholm; and every European chemist and philosopher has for as many years hailed its appearance as a most valuable source of information, embodying, in a volume of some 400 pages, an abstract of all the researches made in the different departments of experimental science during the preceding twelve months. In like manner various annual reports or Jahrbuchs of Pharmacy, Medicine, Botany, &c. regularly appear in different parts of Germany, and among them the Annual Report on the Progress of Vegetable Physiology, by Prof. Meyen, holds a high and distinguished place. The naturalist, to whom, from ignorance of the language in which it is written, this valuable Report must have remained a sealed book, cannot fail to receive with pleasure this excellent translation from the pen of Mr. Francis. Few men are so well qualified to offer a translation of Prof. Meyen's Report to the public as Mr. Francis; for in addition to his familiarity with the German language, he possesses no mean acquaintance with the sciences to which the report is itself devoted, and he therefore must be distinguished from the great mass of translators, as feeling and understanding that which he has to clothe in an English garb. Prof. Meyen commences with a remark on the necessity of drawing a distinction between mere descriptive Botany and Phytology, or Vegetable Physiology, a distinction analogous to that which exists between Zoology and Comparative Anatomy.

"The study of Vegetable Physiology advances with rapid steps, the small number of its labourers annually increases, each year adds to the importance of its results; and we already look forward to the time when a decided separation of vegetable physiology from descriptive botany must take place, for it appears that these two sciences cannot simultaneously be pursued by one and the same botanist to such an extent as the present time demands. The number of anatomico-physiological publications of the past year is extremely great; and it is morphology especially which has engaged during that period the largest share of attention; at present a contest awaits it similar to that which previously vegetable anatomy had to undergo, where not a single observation was admitted without opposition. So also morphology must not be the work of speculation, but should be founded wholly and solely on the observation of nature; studied in this way, it will become a doctrine easy to comprehend, which will enlarge to a great extent our knowledge of the nature of plants.

"From the active interest which has been taken in vegetable physiology, and from the immense increase of materials, this report becomes from year to year a more arduous undertaking; as, however, it is of some utility for the diffusion of the science, the reader will kindly overlook those faults with which such a thankless task must always be accompanied. The interest evinced with regard to these reports both in England and in France by translations, as well as by the kind transmission of some memoirs, which otherwise would not have come so early under our view, convinces us that the naturalists of those countries will feel a greater desire to possess a more general knowledge of our very numerous German labours in this branch of science, than heretofore."

The first topic on which Prof. Meyen dilates is the series of extraordinary speculations of Von Martius, on the souls or spiritual life of plants. Here we have a specimen of that deep and alluring mysticism which our German neighbours are occasionally found mingling with rigid and demonstrative science. Witness the published opinions of Oken, Fries, Grots, Wagner, &c., or of Goldbeck, in his work on "The meaning of 0, or the first dawn of Light in the horizon of Truth." But these speculations, however curious, have perhaps too little practical interest for English readers in general.

The Professor next passes in review the researches of Ohlert, on the naked spongioles terminating the fibrillæ of roots; the researches of Dutrochet on Endosmose, and on the circulation in Chara and Nitella; and those of that very talented and excellent phytologist, Prof. Morren, of Liege, on the circulation of the sap in exogens.

Dr. Schleiden's observations on the growth of plants in water saturated with carbonic acid, are next discussed; these deserve peculiar attention, as they may perchance throw some light on the supposed paradox of seeds germinating in mere *inorganic* powders moistened with water.

The peculiarities of the vessels carrying milky sap, as in *Ficus*, *Euphorbia*, &c., are next discussed; and the microscopic investigator will read a lesson of caution, from the error into which M. Mandl fell, in mistaking the minute spiculæ present in such sap for animalcules.

The section of the report devoted to Vegetable Anatomy is extremely ample, and contains a most valuable amount of information. One or two extracts from the most novel portions will at once afford a specimen of the lucid manner in which this subject is treated, and make known some of the most interesting discoveries to our readers.

"M. Mohl has published a memoir on the structure of the porous vessels of dicotyledons, and I have also treated of this subject in my fifth chapter, but I have called these porous vessels *dotted spiral tubes*.

"M. Mohl adopts two varieties of dotted spiral tubes; in the one the walls are lined equally on both sides with dots or pores, according to M. Mohl's statement; the oak, alder, &c., offer examples; while in the other variety the tubes exhibit a totally different structure at various parts, as in the lime, the Italian poplar, and in many other woods. In the lime, the walls of these ducts, which abut on the ligneous cells, have all the appearance of spiral tubes capable of unrolling; while the other walls, by means of which these vessels cohere among themselves, exhibit the series of dots which are always situated between two convolutions of spiral fibre. It is evident, therefore, from these observations, says M. Mohl, that the dotted spiral ducts belong to the system of spiral tubes and the most essential part of their formation consists in this:—that between the coils of the spiral fibre there is a dilated membrane, on which, between every two fibres, a series of dots is situated. According to my view, all the coils of spiral fibres are clothed with a fine membrane, and the coils of fibre take some part in the formation of the dots by reciprocal cohesion. In proof that the dotted spiral tubes belong to the system of true spiral tubes, I have mentioned a case in the stem of a gourd, where at times the large spiral tubes are not metamorphosed into dotted tubes, which in this plant is otherwise very frequently the case.

"M. Mohl does not consider it as improbable that the thickening of the membrane of spiral vessels may be effected by the deposition of new layers upon their inner surface, exactly as with the thickening of cellular membrane; and I have actually observed this in several cases, representations of which are given in my Vegetable Physiology, Pl. III. figs. 15, 16.

"M. Mohl and I also agree in the explanation of the structure of the dots, namely, that they are formed precisely in the same way as the large dots of coniferous and cycadeous wood; this indeed could not but be expected with the use of such perfect instruments; for most of the incorrect observations of former times can only be ascribed to the defective microscopes of that period.

"M. Mohl compares the development of the porous vessels with that of cells, as series of thin-sided cell-like cavities constitute their base, in which the spiral fibres are then formed. M. de Mirbel had already started a similar notion, that vessels are formed from cells, and the observation of the porous tubes in the earliest stages of their development is said to prove this. About this time the individual cells are frequently found perfectly closed, and the thin membranous diagonal partitions subsequently disappear, while they remain in many cases during the whole lifetime of the plant, but take a structure quite different from that of the lateral partitions, which has already been demonstrated in various plants. I am well acquainted with the cases which might lead observers to the above views, but I also know of numerous cases in which the very opposite may be observed, where both the simple as well as the metamorphosed continuous spiral tubes separate in the course of development more or less completely by constriction, and form articulations arranged in series.

"The cross partitions of the single articulations of the metamorphosed spiral tubes are either broken through by a very large aperture or by a number of fissures and longitudinal pores: nay, even the oblique partitions of the large dotted tubes in the wood of some species of *Ephedra* are pierced, and that by the large round holes, which generally occur in them arranged in two parallel rows. M. Mohl observes that phytotomists have considered these horizontal sides as lateral sides of the tubes, which was also probably owing to bad instruments, for the inclination of these horizontal walls to the lateral is so exceedingly small, that they may even be regarded as inclined terminal surfaces of the prosenchymatous cells, with which the superposed cells stand in connexion; I at least have declared in favour of this latter opinion. The disappearance of the horizontal walls in the dotted spiral tubes is, as M. Mohl thinks, to be compared to the formation of the vessels of the latex, which also are said to originate from cells standing above one another, as M. Unger (see p. 30) has tried to render probable by a drawing. The most varied views exist however on this subject, and are as diametrically opposed to one another as those on the metamorphosis of the spiral tubes. According to M. Schultz, constrictions and the formation of articulations of the proper vessels originate with the advancing age of the plant; in the young state these vessels were still unarticulated. According to my observations, the proper vessels are neither in their youth nor old age provided with cross partitions, but exhibit in the latter state some constrictions which are independent of external circumstances. According to M. Mohl, the continuous vessels of the latex originate from cells, whose cross partitions disappear; to which, in consequence of numerous researches on this subject, I am decidedly opposed.

"The distinction between dotted and reticulated spiral tubes consists, according to M. Mohl, in this:—that in the latter the organic matter used for the further development of the vessels does not deposit itself in the form of a membrane between the coils of the spiral fibre, but goes to increase the spiral fibre itself, both with respect to its thickness as well as to its breadth. In the dotted (porous) spiral ducts of dicotyledons, on the contrary, this substance is deposited in the form of a membrane between the coils of the spiral fibre on the original membrane of the vessel.

"Neither can I concur in these statements: the thickening of the walls of both those stages of metamorphosis of the spiral tubes takes place in quite a similar manner; the principal reason for the change into reticulated and into dotted spiral tubes is to be found only in the arrangement of the individual coils of the spiral fibre. If the coils are widely apart, they may fall merely into annular tubes, or be metamorphosed into reticulated spiral tubes; on the contrary, if the coils are close together, only striped and dotted spiral tubes, and not reticulated, can originate. This is very easily confirmed in stems of *Cacti*, in the inflorescence of *Musa*, &c."

The beautiful observations of Schwann, on the presence of a fungus, *Saccharomyces*, in fluids undergoing the vinous fermentation, are too important to be passed over without notice, even in a brief review:—

"He saw in the beer-yeast most globules cohering in series; they were partly round, but for the most part oval granules of a yellowish-white colour (they are perfectly colourless in achromatic instruments.—*Rep.*), which partly occur singly, but generally in series of 2—8 or more. Usually one or more distinct series branch off obliquely on such a series. In short, the whole is an articulated and ramified plant. M. Schwann observed that the new articulations grew forth at the ends of the terminal articulations, as the articulations to form new branches were emitted laterally. During the fermentation of the expressed juice of the grape similar plants were observed, which presented but a small difference from those of the beer-yeast, only that such long threads as are perceptible in the latter were not noticed in the vinous fermentation. In the recently expressed juice nothing of these plants is perceptible; at 20° they may be observed in thirty-six hours, and M. Schwann could observe the increase of their volume under the microscope in the interval of from half an hour to an entire hour; they are here more globular, and generally only two are adherent together.

"I have also been able to observe the growth of new articulations from the ends of older ones in plants from beer-yeast, and also in vinous and apple ferments. The process is very interesting, and may be followed completely; the individual articulations subsequently separate and again grow on under favourable circumstances. Each articulation of this plant is a distinct independent plant, which immediately continues to grow when disturbed in its adherence; or each articulation must be regarded as a spore of the plant. If the single articulations, in which state the plants in the thick beer-yeast are generally found, are mixed in the mash, they continue to grow, and in thinner fluids the plants become very large; their branches extend radiately in all directions. I boiled the plants from beer-yeast for ten minutes, and yet I observed their further development when again brought under the microscope; and M. Cogniard-Latour exposed them to various degrees of cold; but even after the action of a cold 90° cent. they still retained the property of decomposing sugar.

"Similar plants occur in cider ferment; they are articulated and ramified in the same way as in the beer-yeast, but their joints are mostly three times as broad as long, and I also observed on them an increase by mere separation, although rarely.

"M. Schwann demonstrates the connexion between the fungus described and vinous fermentation: however it is probably yet too soon to explain the phænomena of fermentation from their development; first, because the formation of the fungus is carried on much earlier than the development of carbonic acid in the fermenting fluid, and then there are several other plants which are developed more or less at the same time with them in the fermenting fluid, several of which, in connexion with the former, are described under the untenable genus *Mycoderma*, Persoon and Desmazières; nay, if the supposed species of *Mycoderma* had not been figured by Desmazières, we certainly should never have had a clear notion of them. Desmazières described a *Mycoderma vini, glutinis farinulæ, malti-juniperi, malti-cerevisiæ*, and *cerevisiæ*; but here two entirely distinct things are constantly united which do not at all belong to one another; namely, the small articulated fungus previously mentioned, which we call *Saccharomyces* after M. Schwann's proposal, and form at present the species *Saccharomyces vini, cerevisiæ* and *pomorum*, occurring in all fermenting substances, together with a larger confervoid one, the formation of which is also in many respects highly remarkable. To this place belongs the confervoid fungus which Amici observed in the sap of the weeping vine, the growth of which also takes place very rapidly, so that the elongation can be observed in a few minutes. This confervoid fungus is more or less shortly articulated in various fermenting substances, frequently unarticulated and unramified for some length, and then the articulations form at the branches, frequently over the whole thread at more or less regular distances, and then again, especially towards the end of the branch, the articulations swell to a globular form, subsequently disunite, and again develope to new plants; yet rarely will two fluids be found in which these plants are quite similarly circumstanced. In the expressed juice of a Borsdorfer apple, a fungus of this kind, of great beauty, was formed together with the *Saccharomyces*; several large, almost globular flocks, frequently of the size of a pea, were present in the fluid, which were separated from one another, and always consisted of a very large, or rather innumerable coils, of such single confervoid fungi, at the basis probably adherent. I observed this very interesting form for many weeks, and placed a single flock in a watch-glass with pure water, and so that the branches came to lie close at the surface; to prevent the evaporation of the water the whole was covered with a plate of glass. In the lapse of from eight to ten days new radiate bundles were evident, and among these might be observed several which proceeded from the globular separated articulations of the original confervoid fungus, while others had developed fruit and represented nothing else than *Mucor Mucedo* when growing in water."

The report closes with an account of the Lectures of Von Martius on Morphology, the fairy-like, although most important, division of phytology, in which that philosopher luxuriates; and it is treated with a master's hand.

We must here close our review of Meyen's Report. To have occupied a smaller space in the consideration of a work of such value, would have been less than justice, whilst our limits prevent its occupying more. We cannot, however, take leave of this production without strongly recommending it to our readers, trusting that it will meet with sufficient patronage to ensure the continued appearance of subsequent Annual Reports in an English dress[1]. E. B.

A History of the Fossil Fruits and Seeds of the London Clay. By James Scott Bowerbank, F.G.S. Van Voorst, London: 1840.

Seldom has a book made its appearance in the scientific world so completely combining the attractions of beautiful illustrations with carefully accurate technical descriptions, as Mr. Bowerbank's Essay on the Fossil Fruits of Britain. The author has characterized one hundred and six species, comprising twelve genera, being a greater number than have been noticed by the whole of the previous writers on the subjects. An attentive examination of the different forms will well repay the geologist or botanist for the trouble; he will find some of them—for instance, *Mimosites*, *Nipadites*, and *Leguminosites*, closely allied to existing genera; while others are anomalous in their structure, and belong to types totally extinct. *Faboidea*, although possessing the form common to many true leguminous seeds, presents us with characters wholly dissimilar to those of any existing

[1] As it is intended to continue these Reports in the new volume of our Magazine, we may suggest to such of our subscribers as may approve it, to bind up the Report for 1837 along with the present volume.

legume that has yet been discovered. In the whole of these curious extinct legumes there exists a singular *funiculus*, passing for a considerable space beneath the *testa* before it emerges from the seed. The genus *Hightea* again presents us with a series of highly interesting fruits, resembling in a few characters some of the *Malvaceæ*, but differing from them in so many important respects, that it is a task of great difficulty to assign them their true place with anything approaching to certainty.

Mr. Bowerbank has done all that could be done by microscopic investigations, minutely and elaborately recorded, to elucidate the structure of these curious members of an extinct Flora. We particularly refer the reader to the seventh plate, and to the descriptions, from pages 25 to 31. A few true melons are given under the name *Cucumites*, and the entire structure of these fruits is described, even to the fine membranous *arillus* which surrounds the seeds. Of true *Leguminosæ* eighteen species are described, and the situation and form of every characteristic part of the seed are detailed and figured with wonderful precision: on one specimen (see p. 134) Mr. Bowerbank has even detected the attack of some pisivorous beetle, probably a *Bruchus*. The plates are beautifully executed by Mr. J. de C. Sowerby: in the one re-published in the "Illustrations of the Magazine of Natural History," are represented some most interesting Proteaceous cones, closely resembling those of the existing genus *Petrophila*, a native of New Holland.

There is one point of view in which we must hail Mr. Bowerbank's work as one of great importance, we allude to the light which it throws on the nature of the climate of this portion of the globe at the period of the deposit of the London clay: the general character of the fruits is tropical, and hence we are led to infer that our island,—if island it then was,—must at the period of their ripening have enjoyed the influence of a vertical sun.

The author has spared neither labour nor expense in getting up the work, and we hope that sufficient encouragement may be given him by the purchase of this first number to induce the speedy publication of the remainder in a style equally beautiful and complete.

SHORT COMMUNICATIONS.

NEW METHOD OF DRYING SPECIMENS OF PLANTS.

About five years since I accidentally discovered the following method of drying specimens of plants; and not having seen it mentioned in any work, and also being able to procure finer specimens by this method than by any other with which I am acquainted, I feel desirous of making it public for the benefit of other botanists. The only apparatus necessary is half a ream, or a ream of brown paper, and a quire of double-crown cap paper. I have found that size commonly called "royal," to be the most useful size for the brown paper; it should be tolerably smooth, and that of the weight of about fifty-five pounds per ream will be the best thickness. The cap paper should be rather porous in its texture, and not too thick. It may be cut into half-sheets, and each of these may be folded. The plan of proceeding will then be this:—First, lay down upon a board or table a quire of the brown paper; lay upon it one of the folded half-sheets of cap paper, between which the plant is to be laid out in the usual way; then place over it half a quire of the brown paper, and then another half-sheet of cap paper, with a plant in it, then another half-quire of brown, and so on till all the specimens are laid in. (Unless the specimens are very large, several might be laid between each half-quire.) Finally, place the remainder of the brown paper on the top of the stack. Should the number of specimens requiring to be dried at one time be very great, it *may* perhaps be sufficient to lay a quarter of a quire between the specimens; but I should give the preference to half a quire. The time which specimens will require to dry on this plan will of course vary according to the nature of the plants, and the dryness of the weather; but in general a week will be sufficient in tolerably fine weather. The great advantage in this plan appears to me to be this:—the brown paper being very flexible in every direction, exerts an equal pressure on every part of the plant to be pressed, while in the common way of drying plants (a board being used to give the principal pressure), considerable force is exerted on the prominent and more elevated parts of a specimen, such as the stem, &c., while the leaves and thinner parts frequently shrivel in many plants, the thickness of the stem, &c. preventing an adequate pressure from being applied to them. This I have frequently found to be the case in plants with a woody stem, as *Bidens tripartita*, *Senecio Jacobæa*, and *aquaticus*, *Pteris aquilina*, *Eupatorium cannabinum*, &c. &c. By the above method these difficulties are completely obviated.

Your obedient Servant, W. N.

Note on Mr. Blyth's Paper, p. 370 *of this Number.*

It is probable that the Brooksian Gnoo will prove to be the *Wadau* of Capt. Lyon ('Travels in North Africa,' pp. 76, 271), who mentions a chain of mountains to the south of Fezzan of that name, "on account of the immense number of Buffaloes to be found there, and which are of three species; viz. the *Wadau*, an animal of the size of an Ass, having very large (or, as is elsewhere stated, very long, heavy) horns, and large bunches of hair hanging from each shoulder, to the length of 18 in. or 2 ft.; they have very large heads, and are very fierce. The *Bogua-el-Wecib*, which is a kind of Buffalo, slow in its motion, having very large horns, and being of the size of an ordinary Cow; and the *White Buffalo*, of a lighter and more active make, very shy and swift, and not easily procured. The calving time of these animals is in April and May."

It is scarcely necessary to observe that the word *Buffalo* is most vaguely applied by many persons to any animal in some degree approaching to an Ox in form, but which is different from an Ox; thus, in America, the Bison of that country is so termed, and to make the matter worse, as the animal inhabits the same districts as the so-called Indian, a paragraph lately went the round of the newspapers of somebody in this country possessing several "Indian Buffaloes" in his park, whereas *American Bisons* were intended; and the large or Brahminy breed of Zebras is likewise commonly so called by English graziers. Moreover, Capt. Lyon notices (at p. 44.) that all the grazing animals of Barbary "have the power of remaining a great length of time without drinking," as indeed is the case with the upland-feeding sheep of this country. "Antelopes and *Buffaloes*," he adds, "I should conceive in some cases never touch water, none being found on the surface of the desert, and they are unable to obtain that which is in the wells," to which the *Carnivora* constantly resort. Hence it is obvious that the semi-aquatic beasts, correctly termed Buffaloes, cannot be alluded to; and with respect to those really intended, it may be remarked that unless they feed on very succulent herbage, it is impossible that they should remain long without drinking: witness the dreaded *Tack-bokken*, or migrations of the spring-bok (*Gazella euchore*) in South Africa, when the brackish pools to which they ordinarily resort are dried up; or the narration of J. Wilkinson, Esq. ('Journ. Geog. Soc.,' II. 49.), of a troop of common Gazelles (*G. dorcas*) passing through his encampment at Guttár, in the eastern Egyptian desert, which was their only route to the spring, and returning the same way. Even the Camel, if I may continue the digression, notwithstanding its peculiar organization and express training for this very object, cannot, at the utmost, endure more than five days total abstinence from water (vide Burnes' 'Travels in Bokhara,' II. 18, 179; and 'Edin. New Phil. Journ.,' 1832, 192.); unless, indeed, it meet with a supply of succulent herbage, which, if in sufficient quantity, precludes all necessity for drinking (vide Wellsted's 'Travels in Arabia,' I. 298; Russell's 'Nat. Hist. Aleppo,' 56, &c.). Thus it is that the conflicting statements of various trustworthy authorities may be reconciled without resorting to the scepticism of Burckhardt (vide 'Biography,' of that estimable traveller, prefixed to 'Travels in Nubia,' p. lxxiii.); and it is worthy of being noticed, that Buffon mentions four days as an extraordinary period for a Camel to remain without drinking.

To return, however, to the "Buffaloes" of Capt. Lyon. As it is clear that these are not *Buffaloes*, it remains to observe that the third species is obviously the White Oryx, as noticed by Col. H. Smith; the second is as plainly either the common North African Bubalio, or, more probably, an allied species with which I am acquainted, as yet undescribed; and the first is also undescribed, or at most but indicated, probably as the Brooksian Gnoo of Col. H. Smith, to which genus it would certainly seem to belong, and also as the *Pegasus* of Pliny, or "winged horse of Ethiopia, armed with horns," the *Pagasse, Pacasse, Empacasse*, &c., of various writers of the last and preceding centuries, and the *Bos? pegasus*, of Col. Hamilton Smith; its "large bunches of hair hanging from each shoulder, to the length of 18 in. or 2 ft.", according to Capt. Lyon, doubtless originating the notion of its being winged.

APPENDIX

TO THE

THIRTY-NINTH NUMBER

OF THE

NEW SERIES

OF THE

MAGAZINE OF NATURAL HISTORY:

BY THE EDITOR.

The Editor of the 'Entomological Magazine,' in terminating that journal with the completion of the fifth volume, and in voluntarily coming forward to render some account of his stewardship, tells us, he has resolved upon relinquishing on *this* occasion, the editorial *plural*, that he may address his brother entomologists in the more egotistical but less assuming *singular*. My intercourse with those who peruse the columns of the 'Magazine of Natural History' has not yet, in point of duration, placed me in the same relative position as that lately occupied by my friend, Mr. Newman: — still, the period has been long enough to make me think that, for once, I also may throw aside the attribute in question, and may venture to address the readers of this journal, not as the representative of those who contribute, or of those who subscribe to its pages, and, as such a representative, wielding a power that does not attach itself to the abstract expression of individual sentiment;—but in the character simply of a student in the glorious domain of nature, anxious, with them, to drink at the sources of scientific knowledge, and, at the same time not forgetful that to add something, however trivial, towards widening the stream as it flows from the fountain, is the sole return that can be made, for having tasted of its waters.

The position I am about to place myself in,—that of principal in a cause which will stand recorded as "Charlesworth *versus* Lyell and Owen," but which, perhaps, might be more justly designated as "Lyell and Owen *versus* Charlesworth,"—is one, that with those who may not care to sift the matter for themselves, will probably lay me open to unfavorable animadversion. I must naturally expect that in *some* quarters no exertion will be spared to affix a false construction upon the motives which have prompted my adoption of the present measure. The only contingency however, of this kind, about which I feel the slightest apprehension, is the chance of its being thought that I have come forward with the present history, under a vain conceit that details relating to the character of an Editor, in his capacity of a private individual, must necessarily be interesting from the vast importance attached to such a personage. Now, whilst I am far from affecting indifference to the possible tone of general rumour, I yet feel that the necessity for carrying out the course which I have determined on, does not originate in the relation in which I am placed to the scientific public at large, but in the relation existing between myself and that fraction only of the scientific public to which the journal under my controul owes its existence—the subscribers and contributors to the Magazine. I plead guilty to the possession of so much vanity as confidently to hope that their interest in the reputation of the individual whom they have entrusted with the editorship of the periodical in question, is such as will not allow them to rest satisfied, without a critical examination of the real circumstances which have given rise to this proceeding, at least in so far as those circumstances can be judged of from the evidence which will be placed before them: and, in committing this statement into their hands, and in some measure investing them with the separate functions of judge and jury, I do so, under the full consciousness that if the nature of their verdict be not in my favour, it is sure to be conveyed to me through *a channel which is open to no possibility of misapprehension.*

The major part of those whom I am addressing, may possibly have had their attention arrested by a notice on the wrapper of the December Magazine, (No. 36), and in that case, they will perhaps have already anticipated that the correspondence there announced for publication, is to form the chief topic of discussion throughout the present enquiry. In giving that intimation of the course which seemed to me under the circumstances the only consistent, and the most direct one to pursue, I was guided by two considerations.—First, the making known to such as might by *ex-parte* rumours hear of the matter, that publicity would be given to its details;—and, secondly, that of affording the interested parties on the other side, full notice of my determination, and an opportunity of suggesting some arrangement, should any wish exist on their part to avert the present publication. But before I proceed to the correspondence, or touch upon the immediate details with which it is associated, I must in passing, say a word upon the peculiar position occupied by the Editor of a scientific periodical; and I must refer to circumstances either directly arising from my connection with the Magazine, or which have occurred to me since the direction of that journal was first committed to my charge:—circumstances which, though they never can be wholly discarded from my recollection, I had ceased to look upon, but as of bye-gone times, only noted down in the pages of my own memory. Now however, they *must* be ushered into daylight.—They now assume an importance which makes it imperative in me not to slur them over, since, by so doing, I should leave the door open for it to be said, that though on *this* occasion I had come forward with an *apparent* vindication, there were things of earlier date which I had prudently never ventured to discuss,—never attempted to grapple with.

The Editor of a scientific journal, and the conductor of a periodical devoted to general literature, respectively occupy ground so widely dissimilar, that in the title of their avocations is to be found the main relation of agreement between them. As it regards the *degree* of intellectual acquirement, necessary to qualify them for their respective duties, they are placed on the same footing; but farther than this the parallel does not necessarily hold. The majority of contributors to the latter class of publications, transfer their ideas to paper as an honorable means of subsistence, elaborating the materials according to the greater or less demand for the supply. The great mass of readers read for self-instruction or amusement, and the object gratified they look no farther. *Their* interest in the contingent circumstances attending the publication of the periodical they purchase, only extends to its regular appearance on the first day of the month; and should it retire from the stage, the event merely gives them an opportunity of making a fresh selection from amongst a hundred others. Who the Editor may chance to be, or anything relating to him, forms as little an object of their consideration as the name of the founder who casts the type, or that of the manufacturer who supplies the paper. But in that restricted section of periodical literature, whose constitution differs from that of the general mass, inasmuch as the results of *observation* are here at a premium, and those of *imagination* at a discount, another order of things predominates. It is *here* that we find the savant who has reached the highest pinnacle of fame, and the humblest tyro destined perhaps never even to cross the threshold of her temple, alike registering the share of information which each respectively has gleaned from the treasury of nature. If it be wanted that the result of individual research, secured to its discoverer, shall rapidly circulate, and be sent forth simultaneously to every quarter in which homage is tendered at the shrine of science, *here* it is that a channel is found by which that object is made sure. It is the appreciation of this boon, on the part of those who are at work in the field of physical research, and the consciousness they possess, that these records of philosophical discovery are far from being sources of wealth to their responsible originators, which forms *here* a tie between Editor and Contributor, that elsewhere is unknown. Both are assisting, though in different ways, to further the same exalted object, and the innate satisfaction arising from this source, repays the appropriation of time and exertion which, in *one* sense, might often be far more profitably employed. It is *here* again that an Editor, in virtue of his position, necessarily becomes a party to the results which others have arrived at, before those results are communicated to the world at large; and whether their nature be one embracing simply matters of fact, or those of philosophical induction, the power of hastening or retarding the acquirement of a title to them by their rightful owners, within certain limits, is vested in himself. A knowledge of this, and of the opportunities which an Editor must sometimes have at his command, were he disposed unduly to exercise the power thus entrusted to him, brings with it the necessity for a firm belief, on the part of the contributors, that their confidence will not be abused. An individual, mixing in that sphere of society wherein the less exact sciences are professedly made the leading object of cultivation, may be willing, or perhaps even ambitious of singly incurring the risk and labor attendant upon the direction of a scientific periodical; but he will seek in vain to draw around him that class of supporters, whose contributions alone can make a journal stand high in public estimation, unless he enjoy a reputation distinct from that which forms the mere attribute of philosophical attainment.

I took the Editorship of the Magazine of Natural History at a period in the career of life which, if not the most *mature*, is perhaps the most *sanguine*—the transition from youth to manhood. I became acquainted with the later volumes of the first series whilst studying in the metropolis, with a view of legally qualifying myself for the practice of an active profession; and though their contents could not fail to interest me, I sometimes thought that a firmer tone might be given to the work, by a more careful selection of materials, and, as a general rule, the omission of unauthenticated contributions. Hearing accidentally in the latter part of the year 1836, that a gentleman who assisted Mr. Loudon in the general editorship of the magazine, was about to leave the neighbourhood of London, I obtained a letter of introduction, and voluntarily offered my services, which were accepted, and my proposal also agreed to, that a second series should be issued, to commence in January, 1837, Mr. Loudon continuing proprietor, and, of course, taking all the pecuniary responsibility incurred by the publication.[1] Popular and elementary Natural History was then being diffused through the medium of light publications, in a very cheap form; and I was convinced, both as a matter of policy and inclination, that my course was, if possible, to make the second series embrace a larger share of the philosophy of the science, and a greater amount of those descriptive details which should enhance its absolute value to practical naturalists. The prospect of success, however, was anything but flattering. Dr. Johnstone, who had long been one of Mr. Loudon's most valuable contributors, had just united with Sir Wm. Jardine to conduct a newly-established journal,—the 'Magazine of Zoology and Botany,' carrying with him several more correspondents disposed to give the preference to that one of the two Magazines, which promised to take the higher stand as a scientific periodical. And about the same time another section of Mr. Loudon's supporters, comprising some of the most frequent contributors to the Magazine, had apparently taken under their patronage a rival periodical,—'The Naturalist.' A few of the original correspondents were still remaining, but how they might feel towards a strange editor, was, of course, altogether problematical. Under these disadvantages, and without the possibility of forcing a supply of communications, by proposals of pecuniary remuneration; with no other weight attached to my name than its influence among a circle of friends, taking a warm interest in the cultivation of similar pursuits, I entered on my new avocation.

[1] At the close of the year 1838, the proprietorship of the Magazine was transferred into my own hands, and the pecuniary responsibility has from that period entirely rested with myself.

The present month brings with it the *thirty-ninth* number of the second series, and within this as yet brief career of its existence, nearly *two hundred* of those with whom Natural History forms a leading study or an occasional recreation, have registered their observations on its pages. Large as this number is, the amount itself carries with it no peculiar virtue; but considering how great is the proportion upon the list, whose scientific reputation is firmly established, and that it includes many of the most distinguished naturalists of the day, I cannot help feeling that my original ambition has been even *more* than realized. But could I *then* have known what I *now* know, of the history of *scientific* natural-history periodicals;—could I have foreseen the nerve that sometimes must be exercised, when, single-handed, and without extensive resources, an editor determines, under all circumstances, and at all hazards, to follow one undeviating course of fearless independence;—I should indeed have shrunk from what I was undertaking.

Disappointments have occurred to me, that no share of foresight could have averted; and attempts have occasionally been made to crush the Magazine, in quarters where they might least have been expected to originate: but still, I entertain the idea, that it will never prove to me a source of regret, that I should have stepped forward to carry on a periodical, of which the sphere of usefulness might otherwise have been suspended. The dark spots on the surface of the picture, are not so numerous as to obscure the brighter portions. And at whatever period the relation in which I now stand to a large and influential section of British naturalists, may be destined to terminate,—the intellectual enjoyment that I have derived from intercourse with those who, in many instances, previously unknown to me, have so cordially seconded the promotion of my object, must always form a bright page in the book of retrospection.

I have spoken of the establishment of two journals in the year 1836, devoted to Natural History, and both of these were critically noticed by me in the first number of the Magazine which was issued with my name as its responsible Editor.[1] Of the merits of the 'Magazine of Zoology and Botany' I expressed the highest opinion; at the same time freely commenting on what I conceived to be the demerits of its cotemporary, the 'Naturalist.' I believed these two periodicals to differ, also, no less in relation to the ultimate objects

[1]Of the first series, the three last numbers only were edited by myself, previously to which I had no connection with the work.

of their respective originators. The former, I have every reason to think, was really established solely with a view to the interests of science, and equally so that the latter was set on foot under an impression that the 'Magazine of Natural History' was a source of pecuniary emolument to Mr. Loudon, and that it might be supplanted by a vigorous attempt to win over its supporters to another journal. Mr. Mudie's highly entertaining article on the king-fisher, and other communications equally original, were, under these circumstances, much too good to be lost sight of; but in saying what I did, I felt that if my criticisms were unjust, I was the only party who would be laughed at:—if candid, I was doing the 'Naturalist' a service rather than an injury.

The step taken by Mr. Loudon of commencing a new series of his journal, instead of respectfully withdrawing it out of deference to the new comers, had, no doubt, the effect of greatly disconcerting the projectors of the last-named periodical; and in this emergency one of the parties prominently concerned—Mr. Neville Wood—originated the following scheme,—*to gull, if possible, some London publisher, upon the strength of a fabricated statement, which should be drawn up and furnished by Mr. Wood, into the belief that the 'Magazine of Natural History' might so easily be driven out of the field, as to ensure a highly-profitable speculation to any one who would attempt to effect this object by means of the* 'Naturalist.' Mr. Wood, having been one of Mr. Loudon's most frequent correspondents, and having also written an ornithological work for one of the most respectable publishers in London (Mr. Parker), might naturally be supposed to know something of the real facts, and the gratuitous nature of the statement he was about to concoct, would therefore be less likely to excite suspicion. Various London publishers were accordingly written to, and the substance of Mr. Wood's applications may be gathered from the following specimen, copied *verbatim*, with the omission merely of the business-details relating to the circulation and expenses of the work.

No. 1.

Campsall Hall, near Doncaster,
March 27, 1837.

Sir,

The proprietorship of 'the Naturalist' has lately become vacant; and as it would be advantageous to have the work well supported in London, and knowing your liberal spirit in such matters, I think it right to make you the first offer. * * * * If it were printed in London the sale would rapidly increase, so as to render it profitable. *Loudon's Magazine now gives universal dissatisfaction; and it is the opinion of all our best naturalists, that a spirited monthly Magazine, published and printed in Lon-*

don, would compel that work to discontinue at the close of the year. It has now very few advertisements, and Loudon has left it. * * * If you like to take it [the Naturalist], I promise to use my best efforts to encrease its sale among my numerous correspondents, and a better time there can scarce be for taking the work, when *Loudon's is so rapidly going down in public estimation.* Jardine's Magazine of Zoology and Botany is two-monthly, and so dry and scientific as to have a very limited sale, and with the exception of the Naturalist, England cannot boast of a single good Magazine of Natural History. * * If it were printed in London, I am sure it would become profitable.

I am, Sir,
Your obedient Servant,
NEVILLE WOOD.

P. S.—I had proposed to send this through an M.P., but as the contents are important, think it better it should go *direct.*[1]

To Mr. ————

Now for the reply of the party to whom the above application was addressed.

No. 2.

London, April 8, 1837.

Sir,

I have given your proposal consideration, and do not conceive it would be to my advantage to enter into any arrangement for the publication of the 'Naturalist.' I can have no wish in any way to discourage you, but I think it will be a matter of difficulty to supplant Loudon's Magazine.

Your's &c.
——— —— ——

Neville Wood, Esq.
Campsall Hall.

In the case of periodical works devoted to general literature, and which are carried on so long as they are sources of pecuniary emolument to editors or publishers, the London booksellers can form a tolerably correct estimate of their respective stability by the number of copies which pass through their hands; and hence little or no injury could be sustained by the private circulation of false information. But as it respects scientific journals, generally speaking, the number of copies sold is so limited that this criterion does not exist; and Mr. Wood's statement that the new series of the 'Magazine of Natural History' gave universal dissatisfaction, and that Mr. Loudon had ceased to have any connection with the work, might, under some circumstances, have been productive

[1] The ex-editor of the 'Naturalist' has placed himself in so critical a position by writing this, and sending other letters of a similar character, that I presume he will be very well satisfied to find himself arraigned before no other bar than that of public opinion.

of real mischief, and so far, the fabrication was judiciously planned. At the very time, however, that Mr. Wood was writing to the London publishers, and informing them that all the naturalists of eminence considered the Magazine as worthless, he had the conscience to send to me the following note:—

No. 3.

Campsall Hall, near Doncaster,
April 4th, 1837.

Mr. Neville Wood presents his compliments to Mr. Charlesworth, and would feel obliged by a notice of the 'Naturalist' in the 'Magazine of Natural History.' Mr. Wood's connection with the work commences with the enclosed number.

However well Mr. Wood's scheme might be devised, it was altogether a failure in relation to the contemplated object, for the 'Naturalist' continued to be printed in the country, and every succeeding month brought with it the names of new supporters to the Magazine which it *was* to have supplanted. The necessity for some yet more energetic measure was therefore apparent; and a plot was accordingly arranged, which, as a *master-stroke* of policy, threw the first altogether into the shade. Its nature was as follows.—*To fabricate some statement which should be in the highest degree injurious to the personal character of the new editor of the Magazine of Natural History; then to write a long notice of the second series, speaking most favorably of its scientific merits, but at the close of the article cleverly introducing these fabricated particulars. To get the whole inserted in the literary department of some provincial newspaper, and to transmit by post, copies of this newspaper to the parties contributing to the new series of Mr. Loudon's journal.*

The conception of this plot was in every way admirable. It was a hundred chances to one that the editor of the paper would conceive it necessary to peruse a lengthy scientific notice, before the article went to press, and if any legal consequences ensued, the *author* of the fabrications had nothing to fear,—the legal responsibility falling on the publishers. Fifteen or twenty copies of the newspaper might be had for a few shillings;—they would go postage-free;—and if only half a dozen of them took effect, in occasioning that number of contributors to withdraw their support, it might at that period have been fatal to the existence of the Magazine.

The project was boldly cut out, but most miserably carried into execution. For no sooner did the article appear in the 'Sheffield Iris,' the newspaper selected for its introduction, than Mr. Neville Wood reprinted the statement upon the cover of his own journal, and sent presentation copies of that one number, to the scientific societies of London, where the new editor of the Magazine being well known, the forgery was sure to be detected. A member of the Council of the Zoological Society brought the subject officially before the notice of that body, and the result was, that the then secretary received instructions to return the copy of the 'Naturalist' to Mr. Wood, and to communicate to him by letter the determination of the Council, not to admit the work into the library of their Society. The records of our metropolitan scientific institutions probably do not furnish another instance of a presentation copy of a work being rejected, under circumstances so discreditable to its author.

Mr. Neville Wood will probably say that *his* pen did not furnish the manuscript to the 'Sheffield Iris,' nor *his* hand post the copies of that paper to their respective destinations; but whether it was done *by* him or *for* him, is nothing to the purpose: that he was a party to the fabrication, it is impossible to doubt; for had he not been so, upon the return of the 'Naturalist' by the Council of the Zoological Society, he would of course have given some intimation, in a subsequent number of his journal, of there having been no foundation for the injurious statements referred to; and which statements he had reprinted, protected by the cowardly manœuvre, of quoting as an authority the columns of a provincial paper.

From the editor of the 'Iris' I received the following letter.

No. 4.

Iris Office, Nov. 7, 1837.

Sir,

I received yours dated November 1st, and immediately endeavoured to make reparation for the mistake I had unintentionally permitted to appear in the *Iris*, by inserting an apology in the most conspicuous place of the same paper of this day, a copy of which I have forwarded to you by this day's post

I regret exceedingly the misstatements, as I had not the slightest intention of doing any injury to the sale of the periodical you are connected with, or to Mr. Loudon, or to you, as a gentleman and an editor, through the pages of the *Iris*. I trust this, and the explanation in the *Iris*, will be deemed satisfactory.

I remain,
Your most obedient Servant,
JOHN BRIDGEFORD.

To what extent I may have sustained injury in quarters where personally Mr. Neville Wood and myself are alike unknown, I have no means of forming an opinion. I trusted that those who saw the article, either through an anonymously-received copy of the Iris paper, or by a perusal of it on the wrapper of the 'Naturalist,' would exercise their discrimination, and judge of the probability of its truth, from the support afforded me in my position as editor, by metropolitan naturalists immediately around me.

The Rev. W. T. Bree, whose interest in the prosperity of the Magazine I know to be undiminished, though, of late, he has been a less frequent contributor to its columns, voluntarily observes, in a letter addressed to me in February, 1838, —"I think the attempts made to decry the Magazine, have "been quite unworthy of naturalists, and, indeed, most un-"handsome; witness an article in the paper which accom-"panies this, which was sent to me by an unknown hand.— "Probably you have seen the article before this; I quite for-"get the particulars, and shall not peruse them again." It is hardly necessary to state that this paper was a copy of the 'Sheffield Iris;' and Mr. Bree being then a stranger to me, I felt sincerely his kindness in thus giving me the opportunity, had I thought it necessary, to satisfy him as to the fabricated nature of the statements which it contained. If I am blamed for not having, in justice to myself, come forward with these details at the period when the circumstance occurred, I can only say, that I preferred the chance of incurring some share of personal injury, to the task of displaying, in connection with *Natural History*, a picture of depravity so lamentable as the one now revealed; and it is only the absolute necessity that exists for my clearing the ground as I advance, that has length compelled me to withdraw the veil from that which I would a thousand times rather should have remained in obscurity.

In the seventh volume of the first series of this journal, page 476, a critical notice is given of a book, entitled,— 'Memoirs of Ichthyosauri and Plesiosauri, extinct Monsters of the ancient Earth:' by T. Hawkins, F.G.S. The author of this work had accumulated an extensive series of saurian remains from the lias of Somerset and Dorsetshire, and the collection thus formed was, in the year 1833, offered by its possessor for sale to the British Museum; and after a consi-

[1] As the name of Mr. Maund, the conductor of the 'Botanist' and 'Botanic Garden,' appeared originally on the wrapper of the 'Naturalist,' I am bound to state that I know of nothing whatever to implicate this gentleman in any of the above proceedings.

derable delay the purchase was ultimately effected for the sum of £1250. In 1835, a Parliamentary Committee was appointed by the House, to enquire generally into the state and management of the British Museum, and among other matters of which they took cognizance, were the circumstances under which Mr. Hawkins's collection was bought; more especially with reference to the minutes of certain steps taken by the Trustees, in consequence of a communication made to them by Mr. König, as to the state of some portion of the collection. I shall here make an extract from the Parliamentary Report.

"The Rev. *Josiah Forshall* called in; and examined.

"2962. DO you produce any papers which the Committee called for on a former day, with reference to Mr. Hawkins's collection?—The first paper I now produce, is a copy of a report made by Sir Henry Ellis, Mr. König, and Mr. Children, on the 2nd of February, 1829, regarding the department of Natural History.—(*See* Appendix, No. 27).—That paper does not relate at all to Mr. Hawkins's fossil remains.

"The first paper I have in my custody regarding Mr. Hawkins's fossil remains, is a minute of the 11th of May, 1833, it merely states that an offer was made by Mr. Hawkins of a collection of fossil remains; that a letter from Professor Buckland, on the subject of the collection, was laid before the Board; and that the Trustees, being ignorant of the extent of the collection and the price at which it was estimated, declined the offer.

"In the following month, June 1833, there is a second minute, that a letter was read from Mr. Thomas Hawkins, offering to the Trustees his collection of fossil organic remains for £4,000.; Mr. Hawkins expressed his willingness to dispose of the best of the Saurian animals for £3,000.; or should that offer prove unacceptable, with the rarest of them for £2,000.; a catalogue and drawings of the most remarkable specimens were laid before the Trustees, and the Trustees declined the purchase."

The next is simply a copy of a British-Museum minute.

"(11.)—MINUTE of Committee, dated 1 August, 1834.

"At a Committee,

"A Letter, dated 12th July, from Professor Buckland was read, stating that he and Mr. Mantell had separately estimated every article in Mr. Hawkins's Fossil Sauri, and that they considered the remains figured in Mr. Hawkins's work to be worth £1,025.; and that the worth of the remaining Sauri, not figured in the published plates, was £225. These last Professor Buckland and Dr. Mantell strongly recommended to be also acquired for the Museum.

"This estimate was stated by Professor Buckland to be exclusive of the value of the cases in which some of the fossils are set; these last had been valued by Mr. William Caldecott and Mr. W. Bracher, two upholsterers and appraisers, at £60 5s.

"The Secretary acquainted the Board, that as directed by the Minute of the last Committee, and under the sanction of several of the Trustees, to whom Professor Buckland's letter had been communicated, he had written to the Lords of the Treasury, recommending the purchase to the consideration of their Lordships.

"A letter, dated the 29th July, from the Treasury was read, stating that the Lords Commissioners concurred in opinion with the Trustees, that the opportunity should not be lost of acquiring possession of Mr. Hawkins's collection of fossil organic remains, to the great value of which such strong testimony is borne by most competent witnesses, and that their Lordships had directed an estimate for £1,310 5s. for the purchase of them to be laid before Parliament.

"*J. Forshall*, Secretary.

"British Museum, 22 July, 1835."

Without considering it necessary to quote farther from the Parliamentary Report, I shall merely state that it appears from the rest of the British-Museum Minutes, that shortly after Mr. Hawkins's collection had been removed to the British Museum, a communication was made to the Trustees by the Principal of the Natural History department, Mr. König, respecting some modelled parts of the two largest specimens;—that in consequence of this communication an enquiry into the matter was instituted, but that the explanations afforded were deemed so far satisfactory, that the Trustees came to the determination "that it was unnecessary to enquire farther into the circumstances attending the purchase of Mr. Hawkins's collection."

It so happened that during the treaty with the British Museum for the sale of his fossils, Mr. Hawkins had taken me, in company with a third party, to inspect his collection; and circumstances connected with this visit led me narrowly to look into the printed details of the Parliamentary investigation. Now from this perusal I certainly received an unfavourable impression of the nature of the transaction, as it regarded the seller, but I had no wish to render that opinion notorious by any public expression of it, though I most assuredly conceived myself at liberty to discuss the matter, under all ordinary circumstances,—a privilege which I presume, in common with myself, every one else would claim, in respect to any other national purchase which may subsequently have become a subject of Parliamentary investigation. In the latter part of 1838 I was dining at the house of an intimate friend, and a near relative of the late Dr. Thomas Young, whose name as a philosopher, ranks among the highest that this country can boast of, when Mr. Hawkins's name was introduced (not by myself), and some questions put to me which elicited my opinion upon the affair with the British Museum. From circumstances which then transpired, I found that the parties who had volunteered the interrogatories were personal friends of Mr. Hawkins's. Had a previous intimation been given me of this, I should either have declined answering the questions altogether, or I should have so shaped my replies that no unfair advantage could be taken of what I said. From the tenour of what passed on the occasion, I was prepared to hear farther of the matter, and a short time afterwards I was written to, and invited to retract my opinion, or to state what I could in its justification ; the party writing to me being one of those who had in the first instance questioned me. In replying to this letter I had no suspicion that legal proceedings were in contemplation. I subsequently, however, understood, that Mr. Hawkins intended to move for a criminal information against me ; and shortly afterwards I received the following letter from Mr. Robert Young, sen., the father of the gentleman at whose house I was a guest, and one of the party present at the dinner-table when the conversation alluded to arose.

No. 5.

31 Decr. 1838,
46, Nelson Square.

Dear Sir,

I have exchanged a letter with Mr. Hawkins on the subject which has lately caused so much unhappy discussion between the parties concerned, and into which I need not now enter. Mr. H. has consented to suspend the matter for a week, but he requires terms on which I wish to have some conversation with you, and if you will breakfast with me here tomorrow morning at a quarter past 8, I think we shall be likely to ascertain what is to be done better than by writing.

If you cannot come to breakfast, I will endeavour to call on you in the course of tomorrow; three o'clock to half past would suit me, if you will say where. In the mean time I am,

Yours faithfully,
ROBT. YOUNG.

That Mr. Young, sen., was actuated by the very best motives in this attempt to bring about an arrangement of the affair, there cannot be a doubt; but he had received no authority from me to enter into negociations with Mr. Hawkins, and had he consulted me previously to his doing so, I should have told him that so far as I was concerned, no advances whatever should be made to arrest the course which Mr. Hawkins proposed to take.[1] No other result therefore arose

[1] There were no grounds on which Mr. Hawkins could have obtained a rule for a criminal information; the only course open to him being that of a civil action.

from our interview, but that of Mr. Young deeming it expedient to send me copies of the correspondence which had passed between himself and Mr. Hawkins. From the letters of the latter I shall furnish a brief extract or two. The first bears date Dec. 29, 1839, and is written at Sharpham Park, near Glastonbury, Mr. Hawkins's place of residence.

No. 6.

"You must be aware, Sir, that a criminal action would cost him, [the Editor of the Mag. Nat. Hist.] at least £500, for I should follow the example of the Duke of Cumberland, and subpœna every witness that ever had anything to do with my transactions with the British Museum; and also your son's dinner. I should degrade myself by an indulgence of any thing like revenge, but it is no less my duty to crush for ever a lie, which seems to have been carefully organized and perpetrated whenever an opportunity could be found. This is the only object I have—to crush this lie for ever—it can be effected only by a public retraction, that is all I demand, and that I will, so help me God, obtain. You appeal to my lenity, I honour your feelings, and will defer instructions to my solicitor seven days from your receipt of this. If, within that time, this person *writes* an ample apology to ——————, containing a no less ample disavowal of every offensive word he uttered against myself, and if I receive the said apology within the said time, there is an end of the matter. I need not add that until one week I shall neither say nor write one word on the subject. I also need not add that you are at liberty to communicate the entire contents of this letter to *any one*. I reiterate to *any one*.

And remain, &c. &c.

T. HAWKINS.

Robert Young, Esq.

In a subsequent epistle dated Jany. 4, 1839, Mr. Hawkins proceeds to enlighten Mr. Young in the following strain.—

No. 7.

"* * * I have therefore to repeat—and I do it with regret, in that I am not able to concede anything you ask of me—that I insist on a full, ample, and most satisfactory apology to ————, and as full a retraction of anything against myself. Nothing one iota short of this shall prevent my bringing a criminal action, if I live, and there be one lawyer in England that will undertake it.

"Considering that you will not receive this until Monday next, I shall concede this person the further grace of three days, that is to say till Thursday next. On the next Friday, if I do not meantime receive all I demand the case will be no longer in my hands, but those of my attorney.

"In the case of a criminal action I should subpœna and put upon oath every person at the dinner. They may be ready or unwilling witnesses, but it matters not. * * * I feel that I have conceded to your feelings and importunity too much, but I will abide by my word and wait till next Thursday's post, for that self-damning confession which I will move heaven and earth to have, if it be not spontaneously made.

T. HAWKINS.

Robert Young, Esq.

With a view of showing that Mr. Hawkins could adopt the language of the most cringing adulation, as well as that of the coarsest bullying, just as it chanced to suit his purpose, I here insert some passages from the correspondence between himself and Dr. Buckland, written at the time during which the negociation with the British Museum for the purchase of the collection was pending.

No. 8.

"Clifford St., July 5th, 1834.

"I beg you to receive my sincere regards. This afternoon I have been told by the Revd. Secretary at the British Museum, that the Trustees expressed much regret that the sum which my collection was confessedly worth, exceeded their ability, and that it would be most agreeable to them if Government would take up the question. You have obliged me already, and this emboldens me to solicit another favour at your hands. Would you give me a letter of introduction to the ——— of ———, or any other high personage who may have influence with Her Majesty's Ministers? This, with a direct and energetic letter from yourself, would effect all that we can desire, — the preservation to the nation of my giant collection. I am aware that I take a great liberty, but I am also persuaded that you do feel some intellectual interest in the success of my design. This, with the assurance that I have of the ready attention with which the great flatter your wishes, induce me to place all my reliance upon you.

The Rev. Prof. Buckland.

No. 9.

"Bloomsbury Square, May 13th, 1833.

"Permit me to observe that I would you had yourself determined the money part as well as the rest of the question, although I must defer to the judicious—the delicate motive that induced you to decline doing so,— and to add that I have so arranged that portion of my collection purposed for the Museum, that you and Mr. Mantell could arrive at an estimate in one hour, which I am sure Mr. Mantell would spare for such an occasion. If any variation of opinion about price happen between you, let that difference be split, as the commercial world nervously calls it; for how much debate and inconvenience, loss of time, &c., the presence of a third party would occasion. In truth, my ark — my idol — is in YOUR hands; you are its most efficient keeper, and to nominate another would be unnecessary, and as humiliating to me as two co-temporaneous high priests to the Jews. Besides, I shall from time to time make such additions, (perhaps by way of donation to the British Museum) to the collection, as will—am I rash? —transmit my name with your's— the founder, the real founder of the collection there— to posterity.

The Rev. Prof. Buckland.

Mr. Young sent me copies of the letters which he had received from Mr. Hawkins, under an idea that when I saw the warlike aspect of his intentions I should be frightened, and feel the expediency of coming to terms with him if possible. The only effect however, produced, was that their perusal quite satisfied me as to the correctness of my original opinion; for had every thing been '*right*' in the British-Museum transaction, there would have been no occasion for all this display of thundering scurrility. Mr. Hawkins would have applied for a retraction, and upon its being refused, have quietly given his solicitor instructions to take such measures as, under the circumstances, he might deem expedient. In this position of affairs, conceiving that it might save the parties indirectly concerned in the matter, a great deal of unnecessary trouble and suspense, I determined to let Mr. Hawkins know the ground I was prepared to occupy, and accordingly I drew up and enclosed a letter to my solicitors, (Messrs. Richardson & Talbot, of Bedford Row), with a request that they, as my legal advisers, would forward a copy of it to Sharpham Park, in time for the Thursday's post. Its purport merely was to inform Mr. Hawkins, that *his threats had been communicated to me; that I would neither apologise to him for, nor retract the opinion I had expressed; and that I had given my solicitors instructions to take all necessary steps for meeting whatever legal proceedings he thought proper to adopt.*

The call of the postman at Sharpham Park on the morning fixed for the arrival of the "self-damning confession," I rather take it, placed Mr. Hawkins in a predicament that he was anything but prepared for. Never dreaming after he had passed safely through the ordeal of an investigation, by a select committee of the House of Commons, that I should venture to stand by my opinion, and test it before a judicial tribunal, he had pledged himself to Mr. Young, either to obtain a retraction of the imputation which had escaped my lips, or to institute a prosecution against me in a *criminal* court of justice. His letter, containing an oath to that effect, was not ten days old; and now there was staring him in the face, a rejoinder from my solicitors, accepting the challenge, in a tone that held out no chance of a loop-hole for evasion.

Some time elapsed before Mr. Hawkins mustered even courage to state his ground of action, but at length he lodged his declaration with my solicitors, laying the damages at £1000. founding the proceedings upon the letter which I wrote to the party who questioned me at my friend's table. The declaration sets forth that Mr. Hawkins "*had deservedly obtained the good opinion and credit of all his neighbours; yet the defendant* [*Charlesworth*] *well knowing the premises but greatly envying the happy state and condition of the plaintiff* [*Hawkins*], *and contriving and wickedly intending to injure him in his said good name, fame, and credit, and to bring him into public scandal, infamy, and*

"*disgrace, and to vex, harass, and oppress, the plaintiff, on*
"*the 22nd day of December, A.D.* 1838, *in the form of a letter addressed to one* ——— ———, *falsely and maliciously did compose and publish, of and concerning the plaintiff, a false, scandalous, malicious, and defamatory libel, containing among other things the libellous matter following; 'It appears by the published evidence of a Parliamentary Investigation' (meaning thereby by two printed and published reports from the Select Committee of the 'House of Commons on the British Museum, together with 'Minutes of Evidence, &c." That Mr. Hawkins (meaning plaintiff.)* * * * * * * * *
" * * * *By means of the committing of which said several grievances by the defendant, the plaintiff hath been and is greatly injured in his good name, fame, and credit, and brought into public scandal, infamy, and disgrace with and amongst all his neighbours, insomuch that divers of those neighbours and subjects to whom the innocence of the plaintiff in the said offences and misconduct so as aforesaid mentioned to have been charged upon and imputed to the plaintiff were unknown, have on occasion of the committing of the said several grievances by the defendant, from thence hitherto suspected and believed, and still do suspect and believe, the plaintiff to have been guilty of the offences and misconduct so as aforesaid, and have, by reason of the committing of the said several grievances as aforesaid by the defendant, from thence hitherto refused and still do refuse their countenance and friendship to plaintiff, and the plaintiff hath been and is by means of the premises, otherwise greatly injured and damnified to the damage of the plaintiff of* £1000., *and therefore he brings his suit,* &c."

Now if I had ever seriously entertained the slightest wish to "vex, harass, and oppress" the author of the folio on the extinct Saurians, I could have done so most effectually, without incurring the slightest personal risk, since there was nothing to prevent my bringing forward in the Magazine the subject of the Parliamentary inquiry, and there pointing out what appeared to me to be the weak points and inconsistencies in the published evidence. There were ample materials on which to exercise my ingenuity. So far from having written the letter, maliciously with the intention of injuring the plaintiff, the original expression of my opinion was drawn from me, under the most confidential circumstances, — after which I was written to, and asked what I could advance in its justification; and then the party so writing to me goes direct with my answer to Mr. Hawkins, to enable him to bring an action upon the strength of its contents, and Mr. Hawkins himself is the person to go about and make known to his friends and neighbours, what had been said or written of him, and what he had "declared" he should do in consequence.

It will readily be supposed that this trial would have been likely to excite some little interest in the Geological world; and it was not long in getting wind, that an inquiry into the circumstances attending the disposal to the British Museum, of Mr. Hawkins's fossil saurians, was to be brought forward in a legal shape. A gentleman, well known from his contributions to both the first and present series of this journal, passing through Oxford on his way to London, chanced to call on Dr. Buckland, and in the course of his visit, was somewhat startled at being informed by the Professor, that the Editor of the Magazine of Natural History was on "the brink of ruin." Dr. Buckland, after explaining to him the state of the case, and learning that the party in question was personally known to me, exhorted him to use his best efforts to induce my falling in with Mr. Hawkins's proposal, it being understood between them, that he was at liberty to communicate to me the Doctor's ideas upon the subject. Now it was a most unwarrantable assumption on the part of Dr. Buckland, to intimate to any third party, and especially to a naturalist contributing to the Magazine, that the Editor was on the brink of ruin, for no other reason than that an action was commenced against him, and the damages laid at a thousand pounds. He had no more foundation to justify his drawing that inference, than I should have had, if it had happened that Dr. Buckland had been the defendant in the cause instead of myself. The gentleman above alluded to, on his reaching London, saw me upon the subject, and I am sure that he sincerely hoped to render me a service in so doing.—Merely therefore, expressing my conviction of, and thanks for his friendly intentions, I shall pass on to a note received from Mr. Lyell.

No. 10.

Dear Sir,

Happening to meet Dr. Buckland at dinner yesterday, I found him most willing to be a mediator, and indeed he had already taken some steps, though by no means aware the affair had proceeded so far. He will, I believe, expect you to give a verbal apology of some sort before the parties in whose company you used the expressions complained of, but as he said this in conversation, I cannot of course know precisely what he will suggest. I know not where he is, probably at the Salopian coffee-house, but I believe Mr. Young knows.

Yours truly,
CHARLES LYELL.

[No date affixed, but received February 17th, 1839].

It has been often said that there is but *one* step from the sublime to the ridiculous. Here we have Mr. Thomas Hawkins, F.G.S., willing to forego the chance of his thousand pounds,—flinging to the winds all his solemn protestations,—and resorting to the pitiful expedient of getting Dr. Buckland to suggest that I should make "some sort of verbal apology."!!! Now I was very very far from courting the honour of breaking a lance with this said Mr. Hawkins, in the Court of Common Pleas, however grateful the neighbouring hotel-keepers might feel under the prospect of being intrusted with the care of half a hundred subpœnaed spectators; but I was still less ambitious of calling in the aid of a go-between, to mediate betwixt me and the Glastonbury champion. For this and other equally cogent reasons, I declined, in as courteous a manner as I could, a proposal originating with Dr. Buckland, that I should have an interview with him to talk over the matter.

The next communication I received, was from a gentleman towards whom I have reason for entertaining the highest feelings of respectful regard,—the Rev. J. Forshall, the Secretary of the British Museum.

No. 11.

Dear Charlesworth,

Dr. Buckland has called upon me in reference to some dispute between you and M. T. Hawkins. Dr. Buckland seems to think that you are ignorant of the facts of the case, and that I may possibly be able to explain them to you.

Have you any objection to come and see me?

Yours very truly,
J. FORSHALL.

British Museum,
18th Feb. 1839.

I most readily complied with Mr. Forshall's proposal, but the only result was his writing to Dr. Buckland, to inform him of the unsuccessful nature of the interview which followed.

I presumed that the idea of frightening me into the admission that I did not *mean* what I *said*, was now abandoned as hopeless. Five or six weeks had passed, and neither the *heavens* nor the *earth* exhibiting any extraordinary acceleration of their movements, I was beginning to suspect that Mr. Hawkins had had a change come "*o'er the spirit of his dream,*" when the same gentleman who was entrusted with the commission from Oxford, saw me again, and informed me that as I had persisted in maintaining my original opinion, Dr. Buckland *now* intended to make himself a party to the action, and not to suffer Mr. Hawkins to let it drop; and that in the presence of my informant and other parties, he had positively declared his intention to this effect, the previous night, at the rooms of the Geological Society. That this declaration of Dr. Buckland's was *specially* meant for my ears, I have not a doubt, though it was brought to me under a real apprehension that if I did not think better of my determination, the united forces of Mr. Hawkins and the Professor would be more than a match for me, and that I should inevitably be crushed. The composure with which I received the news of the powerful ally who had enlisted against me under Mr. Hawkins's banner, appeared, I dare say, highly philosophical to the bearer of the tidings.—The simple truth was, however, that I felt quite satisfied that Dr. Buckland had no intention of carrying his declaration into effect, because had he seriously contemplated a step of that description, he would have had the shrewdness to have kept it to himself. That this guess of mine was not a great way off the truth, was not long after confirmed in a manner that I little expected, by the following document falling in my way. It matters not who was the writer of this,—nor to whom it was addressed,—nor how it came into my hands,—its genuineness will not be called into question, or should it be, all particulars can be readily furnished.

No. 12.

"On Friday I went over to see Hawkins, and I found that the affair is suspended for some time. In fact I think that the delay is tantamount to bringing no action at all. Suspended, he tells me, until the Chanc. Exch. has given him an answer relative to his large collection, all the particulars of which must come before Parliament, before they grant the money for the purchase, and we know how long these affairs hang about before they get settled." * * * "It moreover appears that Buckland wrote to Hawkins a letter, advising him, from what I could collect, not to proceed; this letter he placed in Satchell's [plaintiff's attorney] hands, and left him, *he tells me*, to his own choice, as to continuing the action or not; so that altogether the thing will, I think, drop."

It only wanted this to make the farce perfect;—Mr. Hawkins, exulting in the certainty of my being fast in his clutches, offers me, in the plenitude of his benevolence, seven days' grace, to transmit him by letter a "self-damning confession" for the columns of the Times newspaper, or the alternative of my immediately becoming the subject of a criminal information. I defy his threats, and in a few weeks I receive a communication from Mr. Lyell, by which I am given to understand that Dr. Buckland undertakes to arrange the matter, upon my making some sort of verbal apology. I reject the overture, and the next thing I learn is, that there lies in Mr. Hawkins's solicitor's hands a letter written by Dr. Buckland, advising that the action be dropped altogether. It is hardly necessary to add, that upon the perusal of the above document, the materials I had collected for the purpose of substantiating my opinion were laid upon the shelf, where they have ever since quietly reposed.

Before quitting this subject, I must remark, that the interference of the distinguished Geological Professor at the Oxford University, in the transaction above detailed, has alone involved the necessity for the appearance of so much of this Appendix as relates to the case of 'Hawkins *versus* Charlesworth.' The part which he has taken upon himself, has given an air of importance to that, which I might otherwise have regarded as a mere piece of chicanery, aiming under a threat of legal proceedings, at a particular purpose, and about the ultimate consequences of which a moment's consideration would be thrown away. Dr. Buckland, hearing of the circumstances, was of course at liberty to enter into communication upon the subject with Mr. Hawkins, and if he felt so disposed, to supply him with the resources for carrying on the action. The step taken by the Professor wherein I conceive he has laid himself open to animadversion, was the assuming certain consequences towards myself, as the defendant in the action, and the conveying to me the nature of that assumption through the agency of other parties. If Dr. Buckland thought I stood in need of his counsel, the direct and only legitimate course for him to follow, was that of communicating with me in person or by letter; in which case, either as the President of a scientific body to which I have the honour to belong, or upon the strength of occasional intercourse which had previously arisen between us, he could, without in any way committing himself, have tendered me such advice as he might consider the occasion to warrant. I do not say that the object he had in view was illegitimate, but I complain of the machinery put in motion, by which it was hoped that object might be secured.

The sounding the alarm-bell in so many quarters has been followed, as a natural consequence, by the question being put, why have not Professor Buckland's anticipations been realized, and what has been the nature of the compromise? There is no occasion for me to hint at the manner in which this most awkward interrogation has been responded to, but so far as it may lie in my power, I must guard against its going forth to that circle in which the journal under my direction is supported, that at present I owe anything to the *intercession* of Dr. Buckland, or to the *clemency* of his protegé, Mr. Thomas Hawkins. I have been let alone, because the entrapping a person into the unguarded expression of an opinion, happily does not involve the condition of the sentiment conveyed by that expression having been unguardedly arrived at; and because my pseudo-antagonist, upon being foiled in his calculations, was much too wide awake to burn his fingers any farther.

The commencement of proceedings against me in another quarter, of a tendency far more to be dreaded than any measures of a legal description, and upon the consideration of which I have next to enter, has left me no alternative but that of coming forward and openly showing that I have not been the party to fight shy of this contest; but that hitherto, so far as it has proceeded, I have met in a straightforward and uncompromising manner, the disgraceful litigation with which I have been threatened. If by reason of this publication, and after the interval of time which has now elapsed, Mr. Hawkins should be so ill-advised as to go before a jury, in the hope of getting a farthing damages, for a pretended injury which he himself had most assuredly a hand in bringing about, it will be found that instead of simply pleading *not guilty* to the assumed libel, I have placed a *justification* on record. Having done this, unless the question shall come to a legal decision, and unless it shall then be satisfactorily shown that there were no grounds for the belief which I have been led to entertain, I occupy, in relation to the whole affair, let its merits be what they may, a position, which ought in common justice to protect me from even the whisper of an injurious imputation.[1]

Since the appearance of the first 22 pages of this appendix (a period of two months having now elapsed) I have received

[1] I avail myself of this opportunity to acknowledge the warm interest which from the first commencement of the proceedings, has been taken in the case by my solicitor, Mr. Richardson, a Fellow of the Geological Society, and a valued correspondent of the Magazine of Natural History. Immediately upon the issue of the writ, he sent to secure for me the services of Mr. Sergeant Wilde, but that eminent member of the bar had already been retained by the opposite party.

but *one* communication disputing the accuracy of any statement which I have put forward; and as in this instance the writer wishes me to correct the misrepresentation of which he complains, I cannot adopt a fairer course than to make that correction in his own words. In so doing I think it as well to remark, that before the publication of the appendix I held no communication respecting it with any one of the parties therein alluded to. I felt satisfied that I could furnish a faithful relation of such circumstances in the history, as involved a question of memory, and I therefore preferred to risk the chance of the accuracy of any part of my statement being subsequently impugned, rather than to expose myself to the being placed in the position of having to bring forward details under a protest against their publication; a position which would have been in the highest degree painful, if, as in the present instance, such a protest had come to me from a party whose share in the transaction had been irreproachable, and altogether accidental.

The letter is from the gentleman of whom I have spoken as having been the bearer of certain opinions or intentions expressed by Dr. Buckland, in relation to the action commenced against me by Mr. Thomas Hawkins.

No. 13.

" *Your statement* of the interview at the Geological Society, in page 21 " of your Appendix, is calculated to give an erroneous impression of what " took place; for though the Doctor certainly did say, in an off-hand man- " ner, " *Oh, if Hawkins won't prosecute, I will*," or words to that effect, yet " his manner of saying it did not convey to my mind anything like a *deli-* " *berate intention* of doing so, but it seemed merely expressive of his indig- " nation at the charge brought against Hawkins, and his conviction of its " want of foundation. I never therefore meant you to understand that " ' Dr. Buckland now intended to make himself a party to the action,' or " that ' he had positively declared his intention to this effect;' and I shall " be obliged if you will correct this misrepresentation in your next No."

From the confidence which is supposed to reign among the guests, at a private dinner-table, one of the party may think himself hardly dealt with, if the opinions, which on such an occasion he unguardedly expresses, are afterwards arrayed in judgment against him before the bar of a judicial tribunal; but at a public conversazione, such as those which take place after the evening meetings of the Geological Society, we do not expect ' off-hand' speeches to be made by the President, when the subject under discussion happens to be of so serious a character as an action for libel, relating to evidence published by a select committee of the House of Commons.

However sensible I may feel of the unjustifiable nature of the weapons which have been turned against myself, I think it will be seen, when this Appendix shall have been brought to a conclusion, that I have, in no instance, been forced to make good my own story, by affixing a construction to words, either spoken or written, which they were not intended to convey. Had I upon the occasion referred to, been given to understand that Dr. Buckland's was an *off-hand* speech, I certainly would not have opposed it to the contents of the document, No. 12, with which, at present, it so unhappily clashes. As it respects that document, by which it seems that a letter of Dr. Buckland's, advising the abandoning the action, was lying in the hands of the solicitors for the prosecution, (Messrs. Stevens, Wilkinson and Satchell, of Queen Street, Cheapside), it fell into my hands without the knowledge of the writer. Circumstances however, which it is unnecessary to mention, made me feel justified in making the use I did of it. Now without enquiring as to whether or not it would have been a feather in the cap of the President of the Geological Society, to have aided and abetted Mr. Hawkins in prosecuting the Editor of a scientific journal, the said journal having for its sole object the promotion of Natural History, and the said prosecution, according to Dr. Buckland's account, involving the ruin of the party about to be prosecuted,—every one I am sure must feel how desirable it would be for the reputation of the Doctor, that he should not appear to have written *one* thing and spoken *another*. If what Mr. Hawkins stated to the writer of the document No. 12, was correct, (as there seems every reason to suppose it was) and Dr. Buckland *had* advised him to drop the action, I conceive it to be in that case, a moral impossibility for Dr. Buckland to have pretended to entertain the intention which he did, without being fully conscious that he was uttering a threat, diametrically opposed to his real sentiments. When Mr. ——— in apologising for this speech, refers it to Dr. Buckland's indignation at the charge brought against Mr. Hawkins, he surely forgets that it was not I who charged Mr. Hawkins with unfair conduct in the British-Museum affair. It was the Trustees of that establishment themselves, and subsequently a select committee of the House of Commons, who brought the matter forward: all that I did was to express a belief in the charges so brought being correctly founded, the evidence which led me to entertain that belief, being before the public in a printed form.

Dr. Buckland sent me, through my friend Mr. Thomas Young, an invitation to breakfast with him on the 18th of

February 1839, that being the day on which I subsequently received from the Rev. Mr. Forshall the communication No. 11. In reply to this invitation I sent the following note:—

No. 14.

Dear Sir,

My friend, Mr. Thomas Young, tells me that in consequence of his calling to consult you respecting certain legal proceedings pending against myself for words spoken at his dinner-table, that you have proposed we should both breakfast with you to-morrow.[1] In reluctantly declining your invitation, I trust that you will not impute my so doing to any disinclination to effect an amicable arrangement of the matter in dispute, but as I suppose you will be one of the witnesses opposed to me on the trial, I do not think it would be expedient for me to enter upon a discussion of the subject with you previously to the action coming on.

I remain dear Sir,

Yours very faithfully,

EDWARD CHARLESWORTH.

Rev. Dr. Buckland.

Between the dates of my receiving this invitation to be a guest at Dr. Buckland's breakfast-table, and the Doctor's declaration that he would be my prosecutor if Mr. Hawkins would not, I had committed no crime except that of declining to compromise the action, by retracting an opinion which I could only retract at the expense of a violation of moral principle.

When I commenced this Appendix, as the action, though virtually, was not legally abandoned, I had resolved not to advance anything which might be construed into an attempt to prejudice my adversary's cause; but a reconsideration of the whole subject, has induced me to change this determination. As the case now stands, it wears an aspect which is, perhaps, hardly just to all the parties concerned, and I therefore think it the better course, to remove at once the mystery which hangs over the affair, by frankly stating some of the reasons which have led me to form a conclusion so essentially at variance with the spirit of the resolution entered on the minutes of the British Museum, by the Honorable Board of Trustees. Had I done this immediately upon my being served with the writ, and before the time arrived for my prosecutor to lay his case before a Jury, this step might, perhaps, have afforded Mr. Hawkins some reasonable ground of complaint; but after the annoyance to which I have been subjected, and as twelve months have gone by since the

[1] Dr. Buckland's interference did not originate with this call of Mr. Young's, but some time previously to it.

first commencement of proceedings against me, I think now I need entertain no scruples on the score of delicacy to the feelings of my opponent. But in thus going farther than I originally proposed, I must again remind the readers of this Appendix, that the placing this history before them involves no act of aggression on my part. The measure throughout is solely one of self protection. I find myself in a position, which no exercise of prudence or fore-sight could possibly have guarded me against; one into which I have been forced by the treachery of parties, who stand the foremost in the ranks of those who are at work in the field of philosophical research. If this position bring with it the necessity for my explaining the relation in which I have been placed to the President of the Geological Society, and that explanation be injurious either to him or to Mr. Hawkins, I have this to urge in its justification.—

That an assumed case of unfair dealing, in a purchase made by the British Museum of some objects in Natural History, having been investigated by a Committee of the House of Commons, and the evidence relating thereunto published in the usual manner, the opinions expressed in relation to that investigation, must either be favourable or unfavourable towards the party suspected of the fraud.—That having been led, in private conversation, to state my own opinion to be on the *unfavourable* side, the commencing an action against me under the idea that I would retract that opinion, rather than subject myself to a harassing and expensive prosecution, was an act of tyranny, which, if *legally*, was not *morally*, justifiable, and which I believe to be without precedent, in the history of even the law of libel.

Imagine for a moment the question divested of the fearful array of technicalities and legal quibbles, which, like a swarm of evil sprites, hover round the head of the victim entangled in the meshes of the code of libel, and nothing can be more simple than the data required to set at rest the matter in dispute.

Mr. Hawkins sells to the Trustees of the British Museum the fossil skeleton of a reptile, for 200 guineas; the Trustees having resolved upon its purchase, in consequence of the strong recommendation of many naturalists of eminence, and after they (the Trustees) have had a lithographic drawing of the specimen in question put before them. Subsequently to the purchase being effected, and the fossil deposited in the national collection, certain portions represented as genuine in the lithograph, are found to be fictitious. Now it is unreservedly admitted, that no intimation of the real condition of the fossil remain was given to the Trustees as the purchasers, or to the officer at the head of the Natural-History department, Mr. König; and this being the case, the whole thing hinges upon whether there was or was not a full and explicit understanding between the seller and the valuers, as to the precise extent of the manufactured parts; and the affirmative of that proposition being assumed, whether or not the purchasers ought to have been informed that the lithographic print submitted to their inspection, was not an honest representation of the original.

Waiving for the present the discussion upon the two-hundred-guinea specimen, I will commence by referring to page 438, vol. 2 of the Parliamentary Report, where I find the following British Museum Minute.

No. 15.

[MINUTE of the General Meeting, dated July 12th 1834.]

"A Letter, dated July 9th, was read from Mr. Thomas Hawkins, offering to the Trustees his collection of fossil remains of *Ichthyosauri* and *Plesiosauri*, for any sum of money at which they might be valued by Professor Buckland of Oxford; Mr. Hawkins stated that this offer included all the articles engraved in his published work on these fossils."

The next documents bearing upon the subject, which I select, are two letters of Dr. Buckland's, one published in the Parliamentary Report, and the other by Mr. Hawkins himself, in an appendix to his work.

No. 16.

LETTER from the Rev. Dr. Buckland to the Trustees of the British Museum.

London, 12 July, 1834.

My Lords and Gentlemen,

I have this day carefully looked over the collection of the remains of *Sauri* offered for sale to the British Museum by Mr. Hawkins, and have had the valuable assistance of Mr. Mantell, in estimating every article separately. After carefully revising our notes, taken on separate lists, without communication with one another, we found our estimates of the whole to coincide within 5*l.*; they are as below:

All the specimens engraved in Mr. Hawkins's publication (substituting a large *Plesiosaurus*, now in Adelaide-street, for plate 4, the subject of which has been sold), we value at 1,025*l.* The remainder of his collection of *Sauri*, not published in his work, but which we also strongly recommend to be purchased for the Museum, we value at 225*l.* Total value, 1, 250*l.*

In this estimate we do not include the cases of wood in which some of the specimens are framed; these are to be paid for by the Trustees, at a separate valuation to be made by two persons, one of whom is to be appointed by the Trustees, and the other by Mr. Hawkins; with reference to their umpire, in case of a difference of opinion as to the value.

I have the honour, &c.,

(Signed) WM. BUCKLAND.

The above information has this day been communicated to Mr. Hawkins under the joint signatures of Mr. Mantell and myself.

No. 17.

The Rev. Prof. Buckland, to Thos. Hawkins, Esq.

Salopian Coffee House, 12th July, 1834.

I beg to return your catalogue, and with it enclose the amount of the valuation by Mr. Mantell and myself of your entire (the Editor)[1] collection of Remains of *Sauri*. I have sent a duplicate of this valuation to the Trustees of the British Museum, inclosed to the Secretary, the Rev. J. Forshall, and through whom your future communications with the Trustees will have to pass.

I expect a Committee of the Trustees will make application to the Treasury as soon as a new Government is formed; till then you will probably hear no further of the matter.

I have much satisfaction in telling you that when Mr. Mantell and myself compared our separate valuations of the collections engraved in your book, and also of those included in your addition to them, our estimates of the total valuation of each did not differ ten pounds, and that the balance of the difference was given entirely in your favour.

I trust by the time I come again to London to find the *Sauri* all lodged safely in the magnificent gallery of the Museum, which is so worthy to receive them.

I now pass on to a portion of the evidence given by Mr. König in his examination before the Committee.

No. 18.

Charles König, Esq. called in and examined.

When was it first discovered that some of these fossil skeletons were artificial, and not entirely genuine?—I discovered it when the large specimen was put up, after the case was finished. I had no opportunity to examine it sooner.

Had you been consulted, should you have thought it your duty to make a close examination before purchasing?—Certainly.

Do you know whether Dr. Buckland and Mr. Mantell ascertained that part was artificial?—Dr. Buckland has said he knew it was artificial to a great extent. Had I been present, if Dr. Buckland had told me the specimen was not genuine, I should have considered it my duty to have examined more closely into it; but if Dr. Buckland and Mr. Mantell had said, "all this is genuine," I should perhaps, in deference to their superior knowledge, have given way, and fallen into what I consider the same mistake.

You do not mean by that to imply anything affecting the moral character of Mr. Hawkins the vender, or Dr. Buckland and Mr. Mantell, the parties on whose recommendation the purchase was made, the Trustees as the purchasers, or any of the officers of the Museum?—Certainly not. It could only effect the character of the person who made the reparation, if he had endeavoured to conceal the restoration. If I had been told the specimen was genuine, I might myself have given credit to the assertion.

[1] What do the words "the Editor" mean here?—E. C.

After the circumstance became known, did you make any report to the Trustees?—I first wrote to Dr. Buckland and Mr. Mantell, informing them of the discovery I had made. I have a copy of that letter, which I can produce; and I afterwards thought it my duty to report the fact to the Trustees, and that report is also in existence.

Can you produce a copy of that report?—I have no copy, but the report itself can be produced.

What was the date of that report?—I cannot charge my memory.

Have you had opportunities of examining large collections of Saurian remains from the lias formation?—I have seen most of the large collections.

You are aware that it is a very tender and friable formation in which the bones are found?—Yes.

Do you think, from the opportunity you have had of examining other collections, that the specimens of Mr. Hawkins are restored in a greater degree than is generally the case?—That is my impression.

In selling such articles, is it not usual and fair to specify the artificial reparations that have been made?—I should have done so.

Were these specimens sold to the Museum as perfect specimens of Saurian remains?—I do not know; I have not seen the report.

You received them as perfect remains?—I received them merely as remains.

Had you any reason to believe, when you received them, and previous to your examination, that any portion was artificial?—I never gave it a thought. As soon as I examined them I was sure of it; but not till then. Restorations were acknowledged by Mr. Hawkins in his publication, such as the restoration of the right paddle.

Were the whole of the restorations marked in the plate you have alluded to?—No.

In that plate was the tail of the specimen introduced?—No. When I began to compare the specimens with the plates, I found the *Ichthyosaurus* plate 4, was wanting, which is one of the most interesting specimens, having the *scapulæ* in their proper situation. I was alarmed at it, and wrote to Mr. Forshall, who informed me the specimen had been sold by Mr. Hawkins; but another had been substituted, and that specimen turned out to be the worst of all in point of restoration.

What authority have you for stating that?—The specimen was sold before the purchase by the Museum was made. Had the price been less, this would not much signify.

At page 443 of the Report, there is a copy of another British-Museum Minute, in which an allusion is made by Mr. König to the specimen substituted by the valuers in the place of plate 4; and a portion of which Minute I here extract.

No. 19.

MINUTE of Committee, dated 14th March, 1835.

At a Committee:

Mr. König gave it as his opinion, that the colouring of the restored parts of Mr. Hawkins's large *Ichthyosaurus*, so as to distinguish them from those which are really osseous, would be preferable to a complete abrasure of the plaster of Paris.

The Trustees directed that this method should be adopted with regard to all the restored parts of Mr. Hawkins's specimens.

Mr. König suggested that the vacant space in the case, above and below the tail of this large specimen, might be filled up by the introduction of two other framed specimens from the same collection. He likewise suggested that another specimen, 14 feet in length, which was made up, and entirely worthless, should be excluded from the gallery.

The above extracts from the Parliamentary Report, put us in possession of the following facts. That on the 9th of July, 1834, Mr. Hawkins wrote to the Trustees of the British Museum, making them a tender of his collection, and stating in the letter that his offer included *all* the saurians figured in his work. That on the 12th of the same month (only three days after the date of this offer), the Trustees appoint two parties (Drs. Buckland and Mantell) to value the collection. That the valuers, in making their estimate, find that the subject of plate 4 in Mr. Hawkins's work, had been sold, but they substitute for the missing specimen, a large unfigured *Plesiosaurus*. This is followed by the evidence of the officer at the head of the Natural-History department, Mr. König, who discovers that the *Plesiosaurus* so substituted, is made up of plaster of Paris, to such an extent as to justify his recommending to the Trustees that it be excluded from the gallery as "utterly worthless;" while on the other hand, he informs the Parliamentary Committee that the missing specimen appears by the engraving to have been one of the *most interesting* in the whole collection.

Now it is quite clear from this, that Mr. Hawkins stated that which was *untrue*, in his letter to the Trustees of July 9th, 1834, or else that within three days after sending in that letter, and before the valuation could be effected, he privately sold the subject of one of his plates. The history of this specimen, in connection with the evidence given as to the real state of the one which the valuers took as an equivalent for it, has always appeared to me the most criminatory feature in the whole transaction. Nor have I any reason to believe that in the archives of the British Museum, there will be found any unpublished document which shall modify the tenour of this part of the evidence.

But even if there be such a document, I am prosecuted for an opinion founded upon certain evidence which appears in a published Parliamentary Report, and upon that evidence alone I presume a verdict must be obtained against me. I take it for granted that Dr. Buckland, when upon his oath in the witness-box, will swear that he was privy to the condition of the specimen described by Mr. König as "utterly worthless," but how then will he acquit himself upon a cross-examination as to the wording of the valuation, drawn up by himself and Dr. Mantell? Fortunately for me, the critical construction of language does not constitute an element for the consideration of the jury. In determining upon their verdict, they are directed to receive words, whether relating to the libel itself, or to the evidence by which the libel is defended, in their ordinary acceptation. Now, the valuers tell the Trustees, that in drawing up their estimate, they substitute a large *unfigured Plesiosaurus* for a *figured one* which Mr. Mr. Hawkins had sold. Unless they meant it to be understood that the one *substituted* was equivalent to the one which was *missing*, what possible reason could they have for so wording their valuation?

Upon referring to the British-Museum Minute of the 12th of July 1834, I find a passage which bears in a very important manner upon this part of the evidence. It is as follows:

No. 20.

"The Trustees requested Dr. Buckland to send the valuation which he and Mr. Mantell might put upon these organic remains, to the Secretary so soon as it was made, and to distinguish in their valuation, the part of which engravings are given in Mr. Hawkins's work, from the other Saurian remains."

Dr. Buckland receives from the Trustees explicit directions to put, conjointly with Dr. Mantell, *one* price upon the engraved Saurians, and *another* upon those not engraved. When therefore, the valuers found the series of figured Saurians deficient, the plain course for them to follow was to word their estimate thus:—

All the Saurian remains engraved in Mr. Hawkins's work, *minus* the subject of plate 4, which has been sold, we value at ——. All the Saurian remains in Mr. Hawkins's collection which are not engraved in his work, we value at ——. Total value £1250.

Now if Dr. Buckland, when in the witness-box, states that he did know the substituted *Plesiosaurus* to be comparatively worthless, from its being in part plaster of Paris, and that he did not mean, in taking it from the series of unfigured ones and classing it with those which were figured, that the word '*substitute*' was to imply 'in lieu of,' he must then explain not only why he worded his valuation in a manner altogether irregular, but why he went directly counter to the directions given him by the Trustees.

If words are to be used in their every-day acceptation, the premises will not admit of any other conclusion, than that the Trustees were to understand the substituted *Plesiosaurus* equivalent to the one which had been previously disposed of. Dr. Buckland, on the part of Mr. Hawkins, makes no attempt to explain this matter of "substitution," and this being the case, I cannot admit the apology which has been offered for the speech at the evening conversazione at the Geological Society. The doctor must have been perfectly conscious, that any one who took the pains to scrutinize the published evidence, could not do otherwise than form an unfavourable impression of the nature of the transaction; and consequently, his threat to back Mr. Hawkins in the prosecution, could not be intended merely to convey an expression of his indignation at the opinion I had formed. I am disposed to think Dr. Buckland knew that in all probability this warlike declaration would be conveyed to me, and following in the steps of his friend at Sharpham Park, he tried the experiment of making a parade of his intentions, little suspecting his protegé would be so destitute of worldly wisdom as to betray the singularly original method adopted by his patron in carrying those intentions into effect.

Before I proceed to the letter of Dr. Buckland, in which he assures the Secretary of the British Museum that he knew the *full extent* of the fabricated parts, at the time of the valuation being made, I shall quote from the Report one or two passages which refer to the deceptive effect produced by means of the plaster of Paris, and to the unreasonableness of the sum which the public have paid for the collection.

Mr. James de Carle Sowerby, who, from the relation in which he stands to English Geologists, certainly may be supposed willing to say as much as he honestly could, in favour of Dr. Buckland's valuation, gives the following evidence.[1]

No. 21.

"Are you acquainted at all with those fossils in the Museum, which are called Hawkins's fossils?—Yes, I have seen them several times; I did know some little of them before they came to the Museum.

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[1] "I have been induced to give up my time towards forwarding the immediate objects of the leading geologists of England, by yielding them the best assistance my humble talent would permit." (Mr. Sowerby, 'Mag. Nat. Hist.,' vol. 3, new series, p. 419.)

Are you prepared to give any opinion upon their value?—It always appeared to me that above £1000 was a very high price for them, because a similar specimen or specimens, not very far inferior to the best of them, have been sold for 100 or 120 guineas.

Are you aware that some portions of those specimens have been discovered to be artificial?—I was aware of that before they came to the Museum, that a considerable portion was manufactured in plaster. It certainly required some skill to do that, but I do not think it enhances the real value of the things.

Were those parts that were manufactured so skilfully done as to deceive the eye, and apparently with the intention to deceive?—I cannot speak as to the intention, the effect was to deceive the eye. I certainly was deceived by them when I saw some of them in the Adelaide Gallery."

The following extract will show the opinion of Mr. König, the officer to whose care the Geological department of the Museum is especially entrusted.

No. 22.

" After full examination of the remains, do you think that the collection is worth £1,250?—It is a matter of opinion. My opinion was, that it was rather too much, and that is the opinion of some other gentlemen of my acquaintance; but I never stated that publicly; I had no reason to do it.

Can you favour the Committee with a statement of your opinion as to the sum which you believe the Trustees could now obtain for this collection if they were inclined or enabled to dispose of the same?—I am perfectly unable to do that; and I suppose nobody can do so.

Did you believe that the sum of £500 had been asked and given [by the Museum] for one of the specimens?—I certainly stated that.

If you then believed that the sum of £500 had been asked and given for one of the specimens, do you conceive that the sum of £1,250 was out of proportion to the assumed value of the general collection in its entirety? —I should not acquiesce in the reasonableness of £500 for that specimen, but I may be mistaken.

When you were called upon to see the collection, had you any reason to suspect that any part of it was artificially composed?—When I saw it, it was at a distance. The coach-house where it was kept was full of this collection, and other things with it; and it was impossible to go quite near it. But even had I been so close to it as to be able to examine into the genuineness of the specimen, it would never have occurred to me. I did not go for that purpose.

Have you seen the plates of the great specimens of the *Ichthyosauri*, which were published before the specimens themselves were purchased by the Trustees of the British Museum?—I have seen them; but I did not examine them or read the book at all.

Will you examine the plate of the large specimen alluded to, and point out to the Committee such parts of it as that plate indicates to be artificial in the real specimen (*the Plate being shown to the Witness*)?—In this plate the right fore paddle is represented as a restoration.

The right paddle not being shaded, but simply engraved in outline, indicates that that part is not real in the specimen purchased?—Yes.

Now, in the specimen actually purchased, are there not some parts artificial, which in this drawing appear to be genuine?—Yes; there are some such parts artificial.

Will you state to the Committee, from the drawing before you, what parts in the specimen purchased are artificial besides the right paddle, and which were not therefore honestly represented in the drawing before you? —With the exception of about 13 of them, all the processes of the *vertebræ* and several ribs are artificial.

These 13 processes of the *vertebræ*, which you have just described, appear upon the drawing to be a part of the genuine remains?—They are not represented as restorations.

And these processes, which are artificial, are also represented as genuine? —Yes; all the rest are plaster. The lias surrounding those vertebral processes which I have mentioned as genuine, is also natural; namely a patch of about 20 inches by 4, is real lias; the rest was made up of plaster of Paris with lamp-black, to imitate lias, with cracks and rifts passing through the bones; but I do not say it was done with a view to impose upon anybody, or that either Dr. Buckland or Mr. Mantell did mistake that portion for lias.

Suppose you had been called upon to purchase a specimen, an engraving of which had been shown to you, separating the artificial from the natural parts, should you have been led to suspect that other parts than those actually represented as artificial, were really artificial?—I might perhaps have agreed in opinion with the two gentlemen who made the valuation, but I am not certain of it.

That is to say, you would have been misled by a drawing which pretended to distinguish between the natural and artificial portions of the specimens purchased?---Yes."

The following two letters from Mr. Hawkins to Dr. Buckland, I extract from the appendix to Mr. Hawkins's work.

No. 23.

Clifford Street, Bond Street, June 25, 1834.

I received your most condescending favour this morning, soon after my arrival in town, for which I am exceedingly obliged and grateful. The zeal you evidence to serve me overpowers me, and I beg to coincide with every wish that you express and every suggestion. I can appreciate the delicate motive which causes you to decline the proposition made you of being sole referee, and I shall be very happy to associate Mr. Mantell (or any other gentleman you may please to name) with yourself upon this occasion, and rest perfectly content whatever be the result—proud in having my labours numbered and valued by persons so infinitely well calculated to the task. Feeling the importance of this business, which you so generously undertake, and convinced that its speedy resolution is of moment to the honour of our country and the interests of science, I hesitate not to place myself entirely at your command, and to follow implicitly your directions.

P.S. I shall remain in town that I may the better follow your instructions, which I await, anxiously.

The Rev. Prof. Buckland.

No. 24.

Clifford Street, July 9, 1834.

Most anxious to effect the final disposition of my Collection in the Museum, and conscious of the objections that a large sum of money for such kinds of purposes afford the economical---narrow-minded,--- I have after much reflection set apart for the Museum *only* those specimens of my Collection which are of primary import to the public---all the *Sauri*, the subjects of my plates, and those of my general Collection, which are really co-adjutors, and really important to the Museum.

And availing myself of your kind advice, I have sent through Mr. Forshall my proposals to the Trustees.

And I have sent the Trustees, with my work, a list of all the *Sauri*, &c. &c., that I propose for the Museum, a copy of which I retain for you, *with the several prices as well as I remember that the articles have cost me, so that there may be no manner of mistake anywhere*, and as little trouble in the estimation as possible.

And, moreover, I take the liberty to express how much gratified I feel that you and you alone determine the sum that I am to receive for that portion of my Collection which goes to the Museum, as I have not only every confidence in your judgment, but am sure that you will add the more importance to the problem which you condescend to solve, alone.

The Rev. Prof. Buckland.

I have quoted from the published Report, evidence proving the deceptive effect produced by the plaster of Paris, and also, that in the opinion of some highly competent judges, the money value of the collection was over-rated. It now remains for me to put the readers of this Appendix in possession of the document which relieved Mr. Hawkins from the imputation of "fraud or collusion," or which, at any rate, relieved the Trustees from the necessity of pressing the investigation. This document is in the form of a letter from Dr. Buckland, addressed to the secretary, the Rev. J. Forshall. I here give it *verbatim*, with only the addition of numerals to the several paragraphs.

No. 25.

Letter from the Rev. Dr. Buckland to the Rev. J. Forshall.

Oxford, 12 March, 1835.

My dear Sir,

1

In consequence of a letter from Mr. König, I called last week at the British Museum to examine the amount of restorations in plaster, of certain parts of some of the specimens purchased last summer of Mr. Hawkins; and as some misapprehension has gone abroad upon this subject, I feel it due both to Mr. Hawkins and myself to request you on my behalf to submit the following statement to the Trustees.

2

That Mr. Hawkins offered the specimens to the Museum at a price to be valued by myself, it being understood that I was to have the assistance of Mr. Mantell in the valuation:

3

That Mr. Mantell and myself made our estimates separately, and on comparing our lists found them to agree within £20 on the value of the whole collection :

4

That Mr. Hawkins never professed that there were no restorations of some defective portions of some of the skeletons ; on the contrary, I was aware of what he had been doing: many of the specimens have for three or four years past been under my observation, and I have often remonstrated against a practice which I could not prevent. On more careful examination of the specimens, I find the amount of these restorations to be much less than I had supposed ; and were I again to value the collection, I should fix a larger rather than a smaller price on it.

5

The principal restorations are in the largest specimen, which was valued only at £200 or 200 guineas; to obtain such a specimen in a perfect state is all but impossible.

6

There has been, therefore, neither fraud nor collusion on the part of Mr. Hawkins, nor want of information on my part, as to the fact of reparation and restoration of certain broken portions of the skeletons ; and provided these restored parts be pointed out (as they assuredly ought to be) by a different colour from the bones which they now resemble, no one can possibly be deluded ; the specimens will be much more intelligible to the unscientific observer than if the restorations had not been made.

7

As erroneous statements have appeared in the papers respecting this subject, you are welcome to make any use you think proper of this communication.

8

Mr. Hawkins would have done well had he indicated the amount of his restorations in his published plates; but this is a matter which affects the purchasers of his book, and not the Trustees of the Museum, who, being in possession of the specimens, can so readily remedy the existing evil by marking with a different colour the restorations.

Believe me to be, my dear Sir,
Always truly your's,
W. BUCKLAND.

Rev. J. Forshall, &c. &c.

If Dr. Buckland, when he sat down to write the above letter, could have looked forward to the present action, had he wished a verdict to be given in my favour, he could not have strung together a set of propositions so ruinous to the cause of Mr. Hawkins, as those which are contained in this epistle to Mr. Forshall. Should the action come to trial, paragraphs 2 and 3 of this letter supply the important admissions, *first*, that Mr. Hawkins offered to sell his collection without naming a price, provided his friend Dr. Buckland might be allowed to value it; and, *secondly*, that *two* valuers being appointed, both form as nearly as possible the same estimate of the money value of the specimens. The great importance of the *latter* point, in my justification of the libel, depends upon this;—that Dr. Buckland, professing to know of the full extent of the plaster of Paris, arrives at the same valuation as Dr. Mantell who did *not* know of it. That Dr. Mantell had the modelling concealed from him is a legitimate inference from the fact, that he, as co-valuer, puts his signature to the written estimate sent to Mr. Hawkins, but allows Dr. Buckland to stand alone, when subsequently vindicating Mr. Hawkins from the imputation of unfair dealing. If Dr. Mantell could honestly have joined Dr. Buckland in that vindication, he was bound by every principle of justice and honour not to remain silent. Not only does he refrain from joining in the exculpatory declaration, but it is virtually admitted in the course of the enquiry, that he was kept in the dark as to the manufacturing process which had been going forward. If the reader will turn back to the extract at page 30, he will find M. König stating to the Parliamentary Committee, that immediately upon his detecting the plaster of Paris, he wrote and informed both the valuers of the discovery he had made;—then at page 29 he is asked, "Do you know whether Dr. Buckland and Mr. Mantell ascertained that part was artificial?" to which he replies, "Dr. Buckland has said he knew it was artificial to a great extent," tacitly admitting that Dr. Mantell would not make the same assertion; and on another occasion, the same witness remarks, "According to the statement of "one" of the gentlemen who made the valuation, the restoration was not discovered by me."

Now, if it be true that Dr. Buckland, knew what Mr. Hawkins "had been doing" from his previous intimate acquaintance with the collection, and from his having remonstrated with Mr. Hawkins on the subject of the modelling, he must also have known that Dr. Mantell, engaged at Brighton in active professional practice, could not have formed this intimacy with the specimens, and that he would require to have the modelled parts pointed out to him. The most simple method of doing this, in the case of the figured specimens, was, for Mr. Hawkins to take a pen, or a chalk pencil, and to mark upon a set of his lithographic prints, those parts, which, to use the words of a member of the Committee, were not *honest representations of the originals*. So far as the *valuers* were concerned, this plan would have obviated all suspicion of intentional deception, and why it was not done I leave for Mr. Hawkins or Dr. Buckland to explain. As no clew of this sort was put before the valuers, and as Dr. Buckland says nothing about Dr. Mantell having any knowledge of the restorations, when he refers to the close agreement in their valuation, he only condemns himself, instead of benefitting Mr. Hawkins.

When Dr. Buckland says in paragraph 4, that Mr. Hawkins never professed there were no restorations, he makes use of an evasion so paltry, that every one of honourable feeling must blush to see him have recourse to it. The charge against Mr. Hawkins is not that he professed there were *no* restorations, but that he pretended in his plates to distinguish between the real and manufactured parts of the skeletons, by indicating *some* of the restorations, without indicating the whole.

In paragraph 4, Dr. Buckland also states that he finds the restorations to be *less* than he had supposed. Of all the unfortunate admissions in the letter intended to serve Mr. Hawkins, this is the most fatal and short-sighted.—It lets out the important fact, that Dr. Buckland came to no understanding with the vender, as to the extent of the modelled portions;—that he did not even take the trouble to put a question to him upon the subject, but, that in setting the extravagant price of twelve hundred guineas upon the collection, he had nothing more than vague supposition to guide him in distinguishing plaster of Paris from genuine bones, or from natural lias. By his own showing, he allows that the natural parts were not to be distinguished from those which were manufactured, for says the Doctor, "it is not the British Museum who are defrauded, but Mr. Hawkins. I ought to have put a *larger* price upon the collection.—Parts which I set down to a somewhat unusual development of the bump of imitativeness in my friend, Mr. Thomas Hawkins, I now, to my surprise, find to be the handy-work of Dame Nature herself. My estimate therefore was not a *bonâ fide* one, and, as a matter of justice to Mr. Hawkins, I ought to make a fresh valuation."

I happen to know upon more definite authority than mere rumour, the amount of the sum which Dr. Buckland is prepared to swear he would have given Mr. Hawkins over and above the sum of twelve hundred guineas, had not he (Dr. Buckland) included in his estimate of the collection a quantity of genuine remains under a belief that they were plaster of Paris. The sum in question is very considerable, and I only refrain from naming the amount, because on this occasion I purposely avoid going into details, which have not already come before the public in another shape. I may, however, just remind Dr. Buckland, that unless he quotes the name of Dr. Mantell in conjunction with his *own*, when stating what Mr. Hawkins ought to have received, the larger he makes the sum, the greater the amount of culpability resting on his own shoulders; because in calling in that gentleman to act as co-valuer, he was bound to point out to him every part which he (Dr. Buckland) believed to be manufactured.

In paragraph 6, Dr. Buckland states that the manufactured parts ought most assuredly to be indicated by being coloured differently to the parts which are genuine. How came Dr. Buckland not to request Mr. Hawkins to have this done before he and Mr. Mantell visited the collection for the purpose of setting a price upon it? This would have saved Dr. Buckland the disagreeable necessity of having to come forward, to try to make the public believe that there was no foundation for any suspicion of "fraud or collusion."—Besides which, Mr. Hawkins would have then had the *full* value put upon his specimens,—that is the additional sum which the British Museum now owes him, on account of the extraordinary blunder which Dr. Buckland pretends to have committed. The light breaking in upon Dr. Buckland, as to the urgent necessity for colouring the spurious portions, nine months after he had sent the collection to the Museum, and not until Mr. König himself happened to discover the condition of those portions, can only be regarded as forming one of the most singular coincidences on record.

When Dr. Buckland penned this defence for Mr. Hawkins, it is probable that the contents of a letter which he addressed to the Trustees a few months previously were not very fresh in his recollection, as it will be seen that upon that occasion there was no hint given to the Trustees about any want of fidelity in the published plates. The letter in question appears at p. 440 of the Report.

No. 26.

Letter from Rev. Dr. Buckland, to the Trustees of the British Museum.

Oxford, July 7, 1834.

My Lords and Gentlemen,

I beg to inform you that I have received a communication from Mr. T. Hawkins, stating that he is anxious to see placed in the British Museum his collection of gigantic fossil reptiles, found in the counties of Dorset and Somerset, and that he is ready to sell them to the Trustees at any price that I shall name.

I have declined to act alone, and have proposed that Mr. Mantell should assist me in the valuation, in case the matter should be favourably regarded by the Trustees.

I beg leave to state my opinion with respect to this collection, that it is absolutely unique, and that I consider it a matter of very high importance to the Museum to get possession of it; it is such as I could scarcely have believed it possible to make, and such as could only have been made under a rare combination of circumstances in one individual, which can never occur again.

The specimens are not only of high value in the estimation of men of science, but are also to a great degree intelligible to the unlearned; among them are several, which are in their kind, beyond compare the finest and most perfect the world has ever yet produced. I feel it would be an honour to the country to have this collection placed in the British Museum, and a national discredit if these unique productions of England should be purchased for public museums in other countries.

The specimens offered for sale are all beautifully and most accurately engraved in a folio volume, just published, by Mr. Hawkins, on *Ichthyosauri* and *Plesiosauri*, which may be considered as a catalogue of the collection.

I have the honour to remain, &c.
WILLIAM BUCKLAND.

A graphic representation of a specimen, making that specimen appear more perfect than it really is, cannot be a "most accurate" engraving of the original. Dr. Buckland on the 7th July 1834, tells the Trustees that the specimens are *most accurately* engraved in Mr. Hawkins's book, yet twelve months afterwards, when Mr. König discovers that the drawings are *inaccurate*, Dr. Buckland informs the Secretary, the Rev. Mr. Forshall, that Mr. Hawkins would certainly have done well had he indicated the amount of his restorations in his published plates, but that he (Dr. Buckland) was fully aware of their want of fidelity when he arranged the purchase for the Museum, and when he referred the Trustees to those plates as forming an illustrated catalogue of the collection. This, I think is only to be matched by the declaration at the Geological Society, and the private letter of advice to Mr. Hawkins. In paragraph 8, Dr. Buckland tells the Trustees that they are in no way aggrieved by the fact of the Saurians not being so perfect as they were represented in the plates, because they (the Trustees) having paid on behalf of the nation twelve hundred guineas, have come into actual possession of the collection, and they therefore have only to expose the defects and thereby take care that no one is "deluded."

The collection being safely deposited in the National Museum, and the money safely lodged in the pocket of Mr. Hawkins, the Trustees must have felt particularly grateful to Dr. Buckland, for waiting until that period, and then hinting to them how desirable it was that they should set their faces against every thing in the shape of delusion. Their confidence too, in the integrity and impartiality of the Doctor, must have been wonderfully strengthened by his disapprobation of his protegé's conduct in the sale of the book; disposing of the lithographic prints, upon an average, at a shilling each, and yet not giving the purchasers of those prints fully to understand that in one of two instances, the said prints were not faithful to the specimens they professed to represent. But then, as a set-off against this, it was to be borne in mind how nobly Mr. Hawkins had acted in the disposal of the originals; first offering to sell his collection to the British Museum for £4000. But that offer being declined, and Mr. Hawkins feeling that the speedy getting rid of his Saurians, and their transfer to the Museum, was "of moment to the honour of his country, and the interests of science," offers to let them go at any price that Dr. Buckland shall name; and not only this, but Mr. Hawkins colours the artificial to match the real portions so exactly, or rather, as it would seem, the real are made to look so like the artificial, that his *own* valuer, notwithstanding his long familiarity with the specimens, mistakes the one for the other, and does not give Mr. Hawkins so much by several hundred pounds, as he was fairly entitled to. Then there was the liberality of putting the modelled parts into the bargain, without saying one word about them, showing that Mr. Hawkins could not have been actuated by any mercenary motives, as in that case he would naturally have suggested that two Italian *savans* should be entrusted with the forming a separate valuation of such parts of the *Ichthyosauri* as consisted of plaster of Paris, the value of the *genuine* remains being entrusted to his own countrymen. For my own part I cannot understand why Dr. Buckland should cast a slur upon Mr. Hawkins as it respects the sale of his prints, in opposition to the sale of his specimens. I admit, says the Doctor, that the Saurians are not so perfect as they are made to appear, but this matter does not affect the Trustees, for as they have possession of the collection, they can take care no one shall be deluded, by pointing out the imperfections.

It is very true that it lay in the power of the Trustees to issue orders for the artificial parts to have a colour different to that of the parts which were genuine, but I wonder it should never have occurred to Dr. Buckland, with all his fertility of imagination, that the purchasers of the lithographic prints being the actual possessors of those prints, could inspect the Saurians in the Museum, and by the use of the same means make their copies agree with the originals. If this plan would in some measure deface the prints, the same thing may be said of colouring or otherwise marking the specimens themselves. I therefore contend that the purchasers of the specimens, and the purchasers of the prints representing those specimens, equally having it in their power to take measures to prevent "any one being deluded," upon Dr. Buckland's own inductive reasoning, must both be placed in one and the same category. Now, the Doctor himself puts forward the proposition that Mr. Hawkins may justly be called to account by the purchasers of his plates, a proposition which he must either admit to embody a fallacy, or he must come round to my view of the matter, that the buyers of the prints have cause to cry out a little, but the buyers of the originals cause to cry out much more, the difference between them being represented by the difference between one pound and one thousand.

We are told by Mr. Hawkins in one part of his folio volume entitled "Memoirs of *Ichthyosauri* and *Plesiosauri*" that there are *some* mysteries so profound as to require a period of a thousand years for their solution; and I am strongly disposed to acquiesce in the philosophy of this observation, when I find by the Parliamentary Report, the Trustees of the British Museum coming to a resolution, that the "clear and decided statement of Dr. Buckland," that is, the statement I have just been analysing—rendered it unnecessary for them to institute any farther enquiry into the circumstances attending the purchase of Mr. Hawkins's fossils.

Such of my readers as may have followed me thus far in this Appendix, will now understand why Dr. Buckland took upon himself to give out that the Editor of the 'Magazine of Natural History' was on the *brink of ruin;* and why Mr. Lyell found him so ready to offer me his services, and so willing on Mr. Hawkins's behalf to compromise the action upon my giving "some sort of verbal apology."

I appeal to the preceding pages in justification of the opinion I entertain, and I appeal to the supposed confidence with which a person replies to a question at his friend's dinner-table, in justification of my not having kept my lips sealed, when the interrogation was put to me. I *still* think the Trustees of the British Museum were not fairly treated in the purchase of Mr. Hawkins's fossils, and if Dr. Buckland deems it advisable to prosecute me for thinking so, he can make a cat's paw of his friend, and guarantee him his expenses to carry on the action. I may be put to a great deal of vexatious annoyance and expense, but if a verdict even should, under the law of libel, be entered against me, will the farthing damages which Dr. Buckland and Mr. Hawkins are looking forward to dividing,—will that farthing, I ask, recompense the President of the Geological Society for the ordeal he must pass through to obtain it? He cannot get into the witness-box with clean hands, after one day volunteering to mediate for me, and the next to change places with

Mr. Hawkins, and become my prosecutor. Nor do I believe it would be an easy matter to find a jury willing to attach weight to any statements he might depose to on the trial, after the duplicity which I have shown him to be capable of, and after the equivocal nature of the evidence laid before the Committee appointed by the House of Commons, to enquire generally into the affairs and management of the National Museum.

I shall say but few words by way of apology for having gone into the consideration of subjects which have little or no relation to the immediate cause of this Appendix being issued. The necessity for the publication, as mentioned on the cover of the Magazine for December last, arises out of matters connected with the late important discovery near Woodbridge,—that of monkeys and opossums, or at least their fossilized relics, existing in the London clay. I may, however, state in general terms, that though neither of the subjects already touched upon, was alone of sufficient importance to involve such a measure as the present; yet, having to enter the lists with Mr. Lyell and Prof. Owen, I have taken advantage of the opportunity to repel attacks in other quarters. Should there happen to be a lover of Natural History, who otherwise might have felt well disposed towards the Magazine, or the Editor who conducts it, but that he has come in contact with Mr. Neville Wood, Mr. Thos. Hawkins, or Dr. Buckland, he will *now* know how to measure the amount of importance to which their several statements or opinions are respectively entitled. No man having the slightest pretension to honourable feeling, will allege that, *privately*, to the injury of another, which *publicly* he would flinch from avowing, if openly called upon to do so. The President of the Geological Society of London is at the pains to originate a report, that the Editor of an English scientific journal is on the brink of ruin. A channel is put before him, in which, if he could, he might be expected to offer something like a pretext for having ventured on so injurious a statement, but not a syllable is advanced either to justify or palliate the act. It therefore can only be inferred, that he resorted to a gratuitous calumny, for purposes which the preceding history will have made too readily apparent.

The first London-clay mammiferous tooth obtained by Mr. Colchester, in the parish of Kingston (there called Kyson), close to the town of Woodbridge, in Suffolk, is represented by figure 1. The drawing for this engraving has been taken from the original specimen, by M. Dinkel, a natural-history draughtsman of well-known celebrity. The view is one looking down upon the crown of the tooth; and the figure is larger by half a diameter than the fossil itself. This tooth, immediately upon its passing from the hands of the finder (a lad employed in the quarry), into the possession of Mr. Colchester, was shown by that gentleman to me; and Mr. Colchester, upon that occasion, learned from my examination of it, that the tooth was that of a mammiferous animal; I also mentioned to him the high geological value it would possess, if the stratum from which it came were really what it appeared to be, a bed of the London-clay formation. I should at once have made public the circumstance in the 'Magazine of Natural History,' had it not been that no other fossils were present to aid in determining the age of the bed, and under these circumstances I left it in the hands of its possessor, with an understanding that at any future time I was at liberty to make it known.

1

Subsequently to this, Mr. Lyell became acquainted with Mr. Colchester through a note of introduction from me; and during an excursion to Suffolk last year, he borrowed this tooth for the purpose of showing it to Prof. Owen. When, however, it was removed from Mr. Colchester's cabinet it does not appear that anything was said by Mr. Lyell about his intended publication. The tooth accordingly was taken to the College of Surgeons, and Prof. Owen pronounced it to belong to one of the "mixed feeders", and ultimately decided that it was the tooth of an Opossum of the restricted genus *Didelphis*. Mr. Lyell then went into Scotland, and having possession of the tooth, and as he supposed, a knowledge of the genus to which it belonged, he resolved to announce the discovery at the meeting of the British Association, then shortly about to be held at Birmingham.

Subsequently to Mr. Lyell's going into Scotland, but prior to the Birmingham Meeting, a second and much larger mammiferous tooth, with a considerable portion of the jaw remaining attached to it, came through Mr. Colchester into the hands of Mr. Searles Wood. This second tooth was found in the same quarry as the former one, and upon being shown by Mr. Wood to Prof. Owen, he compared it with the corresponding tooth of a well-known monkey, with which, as Mr. Wood confidently asserts, he pronounced it to be identical. (See fig. 2.)

2

Natural Size.

About three weeks after this, but still before the British Association met at Birmingham, a *third* mammiferous tooth, (see fig. 3.) but widely differing from either of the two former ones, was obtained by Mr. Colchester, and given by him to me, for publication in the Magazine. The opportunity of doing this was only what I had in common fairness some claim to, because it was I who had induced Mr. Colchester to form a collection of

3

b *a*

the fossils from the numerous and rich localities in the immediate neighbourhood of his residence, and to whom he was indebted for the information that the tooth (No. 1) was that of a mammal.

Mr. Lyell went from Kinnordy, in Scotland, to the meeting of the British Association at Birmingham in August, and there announced to the Geological section, the discovery of mammiferous teeth in the London-clay of Suffolk; my own history of the third tooth was coming out in the number of my journal for the following month (September), but thinking that any information on so important a matter, would be acceptable to the Geologists assembled at Birmingham, I sent down some printed copies of the paper to the Secretary, Prof. Phillips, but not until Mr. Lyell had read his paper on the tooth (No. 1), and thereby secured to himself the credit of being the first to make known so important a step in the history of English Geology.

I now turn to the authenticated reports in the Athenæum, to make an extract from Mr. Lyell's observations upon the tooth (No. 1). This fossil has since turned out to be, like No. 2, the tooth of a monkey, and not as at first pronounced by Prof. Owen, that of an opossum. The passage I shall quote is from page 676 of the 'Athenæum' for September 7, 1839.

No. 27.

"Mr. Lyell then mentioned the discovery of the teeth of an opossum in the London clay at Kyson, near Woodbridge. This fossil, also from the collection of Mr. Colchester, was obtained, together with the teeth of fish, from the upper part of a bed of sand about ten feet thick, which is covered by a mass of London clay about seventeen feet thick. The clay is again covered, at a short distance from Kyson, by the red crag. Mr. Owen, on seeing this tooth, was clear that it could not belong to any of the decidedly carnivorous or herbivorous animals, but rather to some one of the mixed feeders, and having compared it with the teeth of the various tribes of quadrupeds included in that division, from the shrews to the monkeys, he found it to differ essentially from all of them; and he finally decided that it was marsupial, and one of the molars of a *Didelphis* allied to the Virginian opossum. Mr. Lyell immediately requested Mr. Wood and Mr. Colchester to renew their search in the same sand at Kyson, and they soon after found there a jaw and tooth, which Mr. Owen refers to a quadrumanous animal of the genus *Macacus*. The sand containing these remains is referable to the London clay, and this is the first instance of the fossil remains of *Quadrumana* having been found in a deposit of the Eocene period.'

Now, the tooth, which, in August, Mr. Lyell, on the authority of Prof. Owen, publicly stated to "differ essentially" from the monkeys, was found by Prof. Owen, in the beginning of the following October, not only to be that of a monkey, but to belong to one of the most common and best known genera of the whole monkey tribe,—the *Macacus*.

It would seem that the first charge set on foot against me by Mr. Lyell, was, that in publishing the Woodbridge fossil No. 3, (which, after a careful comparative examination, I had referred to an opossum), I stood indebted to Prof. Owen for what I had said respecting it, and that I had made no acknowledgement of that obligation, but had appropriated as my *own*, the result of *his* investigation. This, at least, was from what I could gather, the impression left upon the minds of other parties from communications made to them by Mr. Lyell. Now an imputation of this kind *privately* circulated, or an obscure hint which might admit of such a construction, emanating from so high an authority, was about the best scheme that could have been devised for doing me the greatest possible amount of injury in the fewest words: the Editor of a scientific journal, from the nature of his office, having so frequently in his hands, the unpublished observation of others, and thereby being so peculiarly obnoxious to a charge of undue appropriation.

When Mr. Lyell is taxed with having made the charge just alluded to, he evades an explanation by complaining that I had referred to the tooth No. 2, in his possession, as being like my own, (No. 3), mammiferous, but that I had not mentioned the fact of its also being the tooth of an opossum, which I ought under the circumstances to have done, having been privy to Prof. Owen's determination to that effect. Now if the comparing a fossil tooth with a series of recent teeth, and the giving an opinion as to which of the recent teeth most nearly approximated the fossil, had involved in the case of a genus so well known as *Didelphis*, any very profound knowledge of comparative anatomy, then (in the absence of good reason to the contrary), I might have been blamed for the omission. As it was however, if Mr. Lyell felt, as it is to be presumed he did, that he should be rendering the state a service by lowering me in the estimation of my fellow cultivators of Natural History, and by putting them on their guard with respect to me, he surely might have waited until I should have committed some act involving a heavier amount of culpability than the one which it is admitted he made use of to my prejudice. The tooth in Mr. Lyell's possession, which I spoke of as mammiferous, but without saying that it was the tooth of an opossum, happened all the time to be the tooth of a monkey, and what is more, Mr. Lyell knew perfectly well it was the tooth of a monkey, when he penned the letter, No. 33, complaining of my not having called it the tooth of an *Opossum*. I felt it somewhat humiliating to have seriously to combat an accusation, so absurdly frivolous in its nature, although very far from frivolous if viewed in relation to the purposes for which it had been called into being. In justification of my silence I informed Mr. Lyell that *one* of my reasons for not stating this tooth, (No. 1), to be that of an opossum, was, that Prof. Owen himself, (after Mr. Lyell had left for Scotland), suspected the possibility of his having mistaken a monkey's tooth for an opossum's, and mentioned that suspicion to myself. A monkey's jaw from the same locality as the supposed opossum's tooth, having in the mean while been brought to him, readily explaining why such a suspicion should arise. Prof. Owen upon being referred to by Mr. Lyell, and also by myself, most distinctly denies that he gave me any caution of the kind, grounding the assertion upon the statement that he felt confident the tooth, No. 1, was that of an opossum, and that he allowed Mr. Lyell to publish it as such at Birmingham, upon his authority.—Now the real point of importance at issue here is, that there being a sacrifice of truth on either one side or the other, with whom does it rest? Fortunately there is a document accessible which will help to decide this question. I quote from the Annals of Natural History, for November, 1839, the following passage to which the name of Prof. Owen stands as the author. The fossil referred to is the supposed opossum's tooth, (No. 1), which Prof. Owen now republishes as the tooth of a monkey, and of which he thus speaks:

No. 28.

"This tooth was one of the mammiferous remains from the London-clay formation at Kyson, which was submitted to my examination by Mr. Lyell, and the one which, after a cursory comparison, I observed to present a considerable resemblance with the molar of an opossum. *I should not, however, have presumed to have published a statement of its affinity to, much less its identity with the genus Didelphis, without testing the fossil by a more extended and rigourous comparison.*"

On the 14th of November (letter 38), Prof. Owen states that he could not have communicated to me before he left London for Birmingham, an intention to re-compare the supposed opossum's tooth with the teeth of the monkeys, because his first comparison made him feel confident that the tooth in question was that of a *Didelphis*, and as such he allowed Mr. Lyell to publish it before the members of the British Association; yet, only fourteen days previously to his writing this letter, Prof. Owen had declared in *print*, that his first examination of this same tooth was so cursory, that he would not have felt justified in publishing its affinities to, and much less its identity with the genus *Didelphis*. The argument, therefore, with which Prof. Owen opposes my statement, is completely falsified by his own words.

How Prof. Owen, when at the Birmingham Meeting, could authorize Mr. Lyell to publish that which, by his own voluntary admission, he (Prof. Owen) would not have "presumed" to have published *himself*, is a matter for him, and not for me to explain.

Mr. Lyell who had so committed himself in this matter as probably to feel that he must, if necessary, go any lengths in making out some case against me, comes forward with a statement, which for the boldness displayed in its concoction, could not well be exceeded. In the letter, No. 35, dated Nov. 1st, he deposes as follows:—

No. 29.

"Mr. Lyell had conversed at Birmingham with Prof. Owen, on the subject of the opossum's tooth, both before reading his paper to the British Association and afterwards, and then again in September, at the College of Surgeons in London. It was not until three weeks ago that Prof. Owen first called on Mr. Lyell to say that he began to entertain doubts, and to invite Mr. Lyell to accompany him to the College of Surgeons, where, after a careful comparison, it was decided that the tooth was not marsupial, but the molar of a *Macacus*."

Referring again to this point in his vindicatory letter, Mr. Lyell observes.—

No. 30.

"It is impossible, that during my intercourse with Mr. Owen in August and September, when conversing on this subject, I should have remained ignorant of any doubts entertained by him, of the marsupial nature of the tooth, No. 1, which he allowed me to announce on his authority, at Birmingham. It was six weeks after you wrote your paper, that the suspicion entered Mr. Owen's mind for the first time, and he immediately came to tell me that he felt some annoyance at having, after his first cursory comparison, misled me."

The intimation here distinctly conveyed by Mr. Lyell's statement, is nothing more or less than that Prof. Owen had a month or more at his disposal, during which there was no reason to prevent his testing his suspicion, if any doubt had existed in his mind as to the possibility of his having made a mistake in the matter of the supposed opossum's tooth. Mr. Lyell, moreover, making it appear that during this interval (that is, the month of September), he (Mr. Lyell), was in personal communication at the College of Surgeons with Prof. Owen. The real fact, however, is, that during the month of September, Prof. Owen was not within a hundred miles of the College of Surgeons; and during the previous month (August), Mr. Lyell not having been within a hundred miles of the same building,—it was not until the month of October that Prof. Owen could put his doubts to the test. The "September" conversations which Mr. Lyell calls in to bear against me, are therefore altogether imaginary.

Feeling how serious an imputation would be thrown upon Mr. Lyell by what I have just stated, and the great importance to myself of verifying the absence of Prof. Owen, I thought it as well to address the following letter to the Secretary of the College of Surgeons:

No. 31.

103, *Great Russell Street, Bloomsbury,*
May 23rd, 1840.

Sir,

I am under the necessity of addressing you as the Secretary of the College of Surgeons, for the purpose of obtaining (if it can be granted me) a document, which shall put it in my power to state, on more definite authority than my own personal knowledge, that the Assistant Conservator of the Museum, Prof. Owen, was absent from the College during the month of September last.

The grounds upon which I hope this request will be acceded to are these:—

That being the Editor of the Magazine of Natural History, a Journal in which the subjects of Comparative Anatomy and Physiology, in common with other branches of Science are treated upon, I have for several years past had occasional access allowed me to the Museum of the College, through the Conservator, Mr. Clift, or through the Assistant Conservator, Prof. Owen:—That I stand charged by Prof. Owen with having appropriated, or with having intended to appropriate and publish as my own, certain results, arrived at by, and belonging to himself, in relation to some extremely important discoveries in the history of English Fossil Zoology,—the said charge or charges arising, more or less, out of certain interviews between Prof. Owen and myself, at the Museum of the College, in the month of August, and certain *alleged* interviews between Mr. Lyell and Prof. Owen, also at the College, during the month of September, 1839:—That the fact of its not being possible that the *alledged* interviews could have taken place during the specified time, (namely the month of September), owing to the absence of Prof. Owen, is a circumstance of material importance in enabling me to substantiate a vindication which I am on the point of placing before the public.

I further beg to submit that the charge or charges in question, are calculated to be seriously detrimental to my reputation as a private individual engaged in scientific pursuits, but more especially as a public journalist:—moreover, that the granting the document referred to could only operate to the prejudice of Prof. Owen, upon the assumption that he has charged a visitor to the Museum with acts or intentions which that document would tend to disprove,—and consequently, that the refusal to grant it, would be placing an obstacle in the way of an investigation which otherwise might establish the innocence of the party upon whom the fraudulent imputation now hangs.

I remain, Sir,

Your most obedient Servant,

Edw. CHARLESWORTH.

Edmund Belfour, Esq.
Secretary to the College of Surgeons.

Before sending the above letter, I made a personal application to the Secretary (Mr. Belfour), on the subject: and though he did not dispute the fact of Prof. Owen's being absent from the college throughout the month of September, he held out to me but little prospect of my intended application being acceded to; and up to the present date (May 28th), I have received no reply; a result for which I was not unprepared. My own knowledge of the period of Prof. Owen's absence, arises from his having told me when he went to the Birmingham meeting, that he should proceed thence to Ireland, and remain absent during the period in question; in addition to which, I called at the College on the 28th of September, and Prof. Owen had not then returned from his excursion. Under these circumstances, it could not be otherwise than that six weeks should elapse before Prof. Owen could test the correctness of the doubt which he mooted to me; and Mr. Lyell must have been aware of this, when referring to "September" conversations, and when intimating that Prof. Owen (really absent in Ireland), was all the time within reach both of the Museum in Lincoln's Inn Fields, and the fossil which has acquired such distinguished notoriety.

Up to the date of November the 1st, I had not entertained the most distant suspicion of Prof. Owen having been a party to the charge against me by Mr. Lyell; as I felt conscious that no act of mine could have afforded the slightest foundation for any interruption to the friendly intimacy which had long subsisted between us; and the only communication on the subject which had passed between myself and the Professor, had given me the full impression that he was indignant at the course which a *third* party had been pursuing. I was, however, soon to be undeceived. The charge against me in the case of the supposed opossum, and which I fully thought to have been concocted by Mr. Lyell alone, proving a break-down, Professor Owen, in writing to deny that he had communicated to me any doubt as to his first determination of that fossil, takes the opportunity of reminding me that I had intended to rob him of his discovery of the nature of the other fossil, No. 2 (the monkey's jaw), but that he had learned from me what I was about to do, and had put a stop to my intention. After perusing the contents of this epistle, it was impossible that I could remain in the dark any longer. I saw clearly that I was to be victimized, for having taken a part in making known so important a step in tertiary Geology as the discovery at Kingston, and my reputation for integrity and candour in the pursuit of science to be destroyed: the agents resorted to for that purpose, being forgery and falsehood, and the most heartless treachery on the part of Prof. Owen and Mr. Lyell. I replied to Prof. Owen's letter in a way that left it open for him to withdraw his charge, if he had expressed that which he did not intend to convey; but hearing nothing farther from him, I determined to save both himself and Mr. Lyell the necessity of privately warning other parties of my fraudulent propensities, by publishing the correspondence, and pleading that I was charged with acts and intentions which I never had entertained or committed; thus leaving it open to any one who might read the correspondence, to believe Prof. Owen if they chose, and act accordingly. It will be seen, upon this determination being made known that the Hunterian Professor at the College o Surgeons, had not the courage to persist in his charge against me, although he well knew I could bring no witnesses to disprove his accusation. All that I could have relied was circumstantial evidence, and the great improbability that if I had intended to appropriate the discovery of another party, I should have made that party the confidant of my intention. Professor Owen however thought it prudent to shift his ground, and, like Mr. Lyell, to discuss something which I had either really done, or really intended to do; and Mr. Searles Wood having requested me to furnish an osteological description of the monkey's jaw, to accompany his announcement of its discovery, Professor Owen makes, or wishes to make it appear that my being about to do this, was what he had to alledge against me. He then goes through the farce of collecting evidence to show that I was prepared to furnish this description, and writes a letter to Mr. Wood, deliberately telling him that I had denied any such intention. He also informs Mr. Wood, that having had the civility, as one of the conservators of the Museum of the College of Surgeons, to tell him what tribe of animals the jaw belonged to, neither Mr. Wood or any one else had a right to publish a description of that fossil but himself, and that he got the fossil in question out of my hands, by insisting upon this principle. Now, Mr. Wood knew perfectly well, what it was that Prof. Owen had charged me with, and by what stratagem it was that he (Prof. Owen) had the publishing a description which otherwise would have been drawn up by myself. He therefore, at once taxes him with having shifted his accusation, and at the same time gives him to understand, that he does not believe any such thing took place during my visit to the College of Surgeons in August, as that originally stated by the Professor to have occurred. Prof. Owen finding that his position was anything but a satisfactory one, and that he was in a fair way of making as lame a business of the appropriation story in the case of the monkey, as his coadjutor, Mr. Lyell, had done for him in the case of the opossum,—judges it the safest plan to make a merit of necessity, and to state that he never meant it to be understood that I intended to do anything either *fraudulent* or *dishonourable;* in other words—that the idea of there being "fraud" or "dishonour" in one naturalist appropriating the discoveries of another, was an idea peculiar to Mr. Wood and myself, and not entertained by him.

I think any one who will read the correspondence, will have little difficulty in understanding why Prof. Owen at the eleventh hour, volunteered the admission in question. It was not a sense of justice to me that called it forth, depending on his consciousness of my having done nothing fraudulent or dishonourable, but because he found that he had outwitted himself in fabricating a charge against me which he had not the hardihood to abide by, or the tact to defend.

In proceeding to the correspondence, which includes both the charges against me, I shall in this place, merely request the readers especial attention to the letters between Prof. Owen and Mr. Wood.

No. 32.

No. 3, Great Russell Street, Bloomsbury.
October 30, 1839.

Mr. Charlesworth has heard with surprise, that Mr. Lyell, when in communication with parties to whom Mr. Charlesworth is personally known, has attributed to him the appropriation without acknowledgment of information derived from Professor Owen, in reference to a paper in the last 'Magazine of Natural History.'

Mr. Charlesworth begs to inform Mr. Lyell that the article in question did not embody any information derived either directly or indirectly from Professor Owen, Mr. Charlesworth having had an opportunity of satisfying himself as to the probable affinities of the fossil forming the subject of his paper, without availing himself of the access always readily granted him by Mr. Clift and Professor Owen to the osteological collection in the College of Surgeons. Mr. Charlesworth is at a loss to conceive the motive that has induced Mr. Lyell to attempt to create an unfavourable prejudice against him, but he trusts that he shall be able to satisfy those among his friends to whom the matter may have been named, of the entire absence of any foundation for the impression which Mr. Lyell has so anxiously endeavoured to produce.

No. 33.

16, Hart St., Bloomsbury,
Oct. 30, 1839.

In reply to Mr. Charlesworth's letter, Mr. Lyell begs to state that he believed, and expressed his belief to several friends, that Mr. Charlesworth, when he wrote on a mammiferous fossil found at Kyson (in the Sept. No. of Mag. of Nat. Histy.), had been already informed that Mr. Owen had previously examined the first mammalian tooth discovered at Kyson, and that Mr. Owen had given an opinion that it belonged to an opossum, — a result which Mr. Lyell had widely circulated. Mr. Lyell also thought that Mr. C., when expressly mentioning the first-discovered tooth in question, should have alluded to the circumstance.

But if Mr. Charlesworth had not become aware of any conclusion previously arrived at respecting the first fossil, Mr. Lyell will have great pleasure in informing the only friends to whom he has spoken on the subject, that he had laboured under a mistake.

No. 34.

Mr. Charlesworth has the honour of acknowledging Mr. Lyell's reply to his note of yesterday, in which Mr. Lyell remarks that he thinks Mr.

Charlesworth, in referring to the first mammiferous tooth found at Kyson, ought to have mentioned the opinion given by Professor Owen respecting its marsupial character.

Mr. Charlesworth felt himself at liberty to refer in his paper to the tooth in question, because, he having been the first to detect its mammiferous nature and consequent geological importance, had received from its discoverer, Mr. Colchester, express permission to figure and describe it previously to the loan of the specimen being obtained by Mr. Lyell.

The comparison, however, of the tooth, with a view to its identification with an existing mammiferous type, was undertaken by Professor Owen at the request of, and whilst the specimen was in Mr. Lyell's possession; and although Mr. Charlesworth certainly was informed of the result of the comparison, he at the same time understood that Mr. Lyell himself intended to communicate that result at Birmingham, to the British Association.

Professor Owen, moreover, previously to his quitting London to attend the Birmingham Meeting, intimated to Mr. Charlesworth, that it was his intention again carefully to examine the tooth, as he thought it might possibly be referable to a genus among the *Quadrumana*.

For these reasons Mr. Charlesworth studiously avoided stating that Professor Owen had identified the first tooth as belonging to an opossum, more especially as he himself had had no share in that identification, and consequently no right to publish the circumstance even had no doubt existed as to the fact.

From the tenor of Mr. Lyell's reply, Mr. Charlesworth infers, that Mr. Lyell admits, on his part, the having charged Mr. Charlesworth with the undue appropriation of information derived from Professor Owen; but as no circumstance named in Mr. Lyell's reply would appear to bear out, or even afford a pretext for, such a charge, Mr. Charlesworth still considers that the matter requires explanation.

October 31.

No. 35.

Mr. Lyell read, with no small surprise, in a letter from Mr. Charlesworth delivered to him this morning, Mr. Charlesworth's allegation that "he had studiously avoided stating that Prof. Owen had identified the first tooth as belonging to an opossum, because Prof. Owen, previously to his quitting London to attend the Birmingham meeting, had intimated to Mr. Charlesworth that the tooth in question might possibly be referable to a quadrumanous genus."

Mr. Lyell had conversed at Birmingham with Prof. Owen, on the subject of the opossum's tooth, both before reading his paper to the Brit. Assn. and afterwards, and then again in Septr. at the Coll. of Surgs. in London. It was not till three weeks ago that Prof. Owen first called on Mr. Lyell to say that he began to entertain doubts, and to invite Mr. L. to accompany him to the Coll. Surgs., where, after a careful comparison, it was decided that the tooth was not marsupial, but the molar of a *Macacus*.

Accordingly Mr. L. applied to day to Prof. O., and received an answer of which the following is a full and exact copy.

Royal College of Surgeons,
Nov. 1, 1839.

My dear Lyell,

I neither entertained nor expressed at any time previous to my visit to you on the 10th Oct. last, any other opinion respecting the small molar (now proved to be the second molar of a *Macacus*), than that it resembled the molar of an opossum.

Believe me,
Very truly your's,
RICHD. OWEN.

Mr. Lyell therefore concludes, that Mr. Charlesworth, at some period subsequent to the 10th Oct. last, learnt from Prof. O. that his opinion was changed respecting the small molar, and Mr. C. must, by a confusion of dates, have imagined that he had written his paper in August last, under the impression that Mr. Owen had even then arrived at new views.

Be this as it may, Mr. Lyell has always felt, that in similar circumstances, had he been first informed that Prof. Owen and another gentleman of his acquaintance had come to the conclusion that they possessed the first fossil remain of an opossum from the London clay at Kyson, and were about to publish the fact, and if he (Mr. L.), had afterwards obtained another fossil from the same place, which he also believed to be an opossum, he should not have felt at liberty to anticipate the announcement of the analogous fact, without first communicating his intention to Prof. O. and his friend.

This feeling Mr. L. expressed to Mr. Wood, and afterwards to Mr. Colchester, when begging of him the loan of the opossum's jaw first described by Mr. Charlesworth.

Mr. L. however, is willing to admit Mr. C's explanation, that he did not feel at liberty to interfere with the publication of a fact which others had arrived at, and also to communicate this explanation to the only person to whom he (Mr. L.) has spoken on the subject.

At the same time Mr. L. takes this opportunity of expressing his conviction, from the tone of Mr. Charlesworth's first letter, that the reports which Mr. C. had heard of what Mr. L. had said of him, must have been misrepresentations or exaggerations.

10, Hart St., Nov. 1st, 1839.

No. 36.

Mr. Charlesworth having quitted London on the morning of the 1st instant, to spend a week at Charing in Kent, has been unable to acknowledge at an earlier period, Mr. Lyell's letter of that date, and to which he now hastens to reply.

The subject of complaint against Mr. Charlesworth seems to resolve itself into the commission of a breach of courtesy on his part towards Mr. Lyell and Professor Owen, in not having communicated to these gentlemen his being in possession of, and his being about to publish in the September 'Magazine of Natural History,' an opossum's tooth, with a fragment of the jaw, obtained from a supposed bed of London clay near Woodbridge.

The accompanying statement (in a separate form), of the circumstances under which Mr. Charlesworth published the fossil in question, and which he trusts Mr. Lyell will favour him by perusing, will show how far the *assumed* commission of this *minor* offence is born out by fact.

The impression as to the nature of the charge preferred by Mr. Lyell against Mr. Charlesworth, upon the minds of Mr. Charlesworth's friends, in two separate instances, was widely different from the aspect which the matter now wears; but as the parties were strangers to each other, Mr. Charlesworth can hardly imagine that both of them should have fallen into the same kind of exaggeration, or misrepresentation, but would rather conjecture (as Mr. Lyell does not appear disposed to press any charge of *undue appropriation*), that they must have misunderstood the substance or the purport of Mr. Lyell's remarks.

Nov. 9, 1839.

[Statement accompanying the above letter to Mr. Lyell.]

HAVING learned in August last, through my friend Mr. Searles Wood, that a mammiferous tooth, found a considerable time since in the London clay, near Woodbridge, had been referred by Professor Owen to an opossum; and that a *second* mammiferous tooth, more recently discovered, had also been referred by Professor Owen to an existing genus of monkeys; I visited Suffolk, for the purpose of examining the spot which had produced these remains, and returned thence the latter end of the week preceding that during which the British Association met at Birmingham; bringing with me a *third* mammiferous tooth, obtained subsequently to the two teeth already mentioned, and of which the *first* discovered specimen was in the possession of Mr. Lyell, and the second in the possession of Mr. Wood. I had received permission from Mr. Colchester, the discoverer of these remains, to publish the third tooth, and I conclude either that the same permission had been given respectively to Mr. Lyell and to Mr. Wood, as it regarded the publication of the first and second, or that these gentlemen felt themselves at liberty to make that use of the specimens in their possession. I knew Mr. Lyell to be either in Scotland, or on his way to Birmingham, and Professor Owen I believed to be likewise absent from London, as upon my going into Suffolk, he had named to me the day on which it was his intention to leave, for the purpose of attending the meeting of the British Association.

Being anxious that a figure and description of the *third* mammiferous tooth should appear in the following number of the 'Magazine of Natural History,' in which number Mr. Wood was about to publish an account of the *second*, and *quadrumanous* fossil tooth, I lost no time in consulting (for the purpose of comparison), the valuable collection of *crania* in the museum of the Zoological Society; and having determined what appeared to me the affinities indicated by the tooth in my possession, I immediately placed it in the hands of the artist, there being barely time to have it drawn and engraved sufficiently soon to admit of its intended publication. I called on the following morning (Saturday), at the

College of Surgeons for a manuscript, which Professor Owen had promised to leave out for me (his description of Mr. Wood's quadrumanous tooth), and then unexpectedly found Professor Owen still in town. In the course of conversation, I named to Professor Owen my having brought up *another* opossum's tooth, which I was going to describe in the succeeding number of my Journal, and Professor Owen then observed to me that before pronouncing the *first* fossil tooth as positively belonging to an *opossum*, he intended, when it again came into his hands, carefully to compare it with the teeth of some quadrumanous genera. I am unable to state what were the exact words used by Professor Owen, but his remark distinctly implied a suspicion that the *first* tooth originally referred by him to an opossum, might possibly be the molar of a quadrumanous animal; and at the time, I naturally concluded, that this doubt on the mind of Professor Owen, originated in his having determined the generic relations of a second mammiferous tooth, subsequently submitted to his examination, to be undoubtedly quadrumanous. And as it appears that the molars of the *Quadrumana*, and those of the opossums, in some instances so nearly resemble, that without the most careful examination, they may be mistaken the one for the other, a doubt as to whether the first tooth (of which it is admitted no scrupulous comparison was made), might, perhaps, prove to be quadrumanous instead of marsupial, would seem almost of necessity to be involved in the subsequent identification of a *second* tooth, from the same locality as the molar of a monkey. However slight this doubt might be, upon Professor Owen's return to London in October, the *first* tooth having, in the meanwhile, been again placed in his hands, such a comparison was undertaken, when the fossil in question proved to be the tooth of a *monkey*, and not of an *opossum*, as he had originally supposed; and under the circumstances just referred to, it was by no means unlikely that my allusion to my intended publication of a *second* opossum's tooth should elicit from Professor Owen a casual intimation of this doubt, although at this distance of time, the fact of his having done so may easily have escaped his recollection. The possibility of any "confusion of dates" on my part, is put at once out of the question, by the circumstance of my not having seen or communicated with Professor Owen, from the 25th of August last, until my arrival in London a few days since; and also the fact of my knowing nothing whatever about the new determination of the *first* tooth, until the announcement of that determination in Mr. Taylor's journal for the present month.

I have entered into these details, in consequence of the reply sent by Professor Owen, to a note from Mr. Lyell, a copy of which reply Mr. Lyell has favoured me with; but the point involved in that correspondence may be entirely lost sight of, without prejudice to the question at issue in the discussion which has taken place between Mr. Lyell and myself; for as I have already had occasion to state the principal motive, (and one that would have induced the same course, in the absence of all other considerations), which led me, when describing a false molar in my own possession, as the tooth of an opossum, to abstain from mentioning that a true molar, in the possession of Mr. Lyell, had been previously referred by Professor Owen to the same genus, was to avoid the incurring the charge which in that case I deemed it probable might be raised against me, of having made public information which had privately come to my knowledge, and the right of publishing which, under the circumstances, was vested in Mr. Lyell.

I certainly esteem it unfortunate, that Mr. Lyell, when communicattng to other parties his unfavourable opinion of the course which I pursued, under a fallacious hope that it presented no feature which could be seized upon as a handle for animadversion, should have so expressen himself, as to be understood to impute to me an act of greater culpability than the one which I have now been called upon to defend.

EDW. CHARLESWORTH.

No. 37.

103, Great Russell Street,
Nov. 10, 1839.

My dear Sir,

Some correspondence has taken place between Mr. Lyell and myself, respecting the matter which I named to you when I was last at the College of Surgeons, in the course of which correspondence I stated to Mr. Lyell, that previously to your going to Birmingham, you intimated to me a suspicion that the *first* discovered tooth from Kingston might possibly turn out to be *quadrumanous*; from the tenour of your answer to Mr. Lyell's enquiry upon this point, Mr. L. supposes that there must have been some confusion of dates in my mind, which has led me to entertain an erroneous impression. You will, however, probably remember receiving a note from me when I arrived in Suffolk, written upon the half of a sheet addressed to Mr. Sowerby, Jun., requesting you to forward to my printer, or leave out for me your promised notice of the fossil *Macacus*, and as you had mentioned Wednesday to me as the day on which you proposed to quit London, I was surprised to find you at the Museum when I called on the Saturday for the manuscript; I was then on my way to the woodcutters in Fleet Street, with the block on which was the drawing of the *third*, and then newly-discovered fragment, and which block I showed to you, stating that I considered the fragment as marsupial, and that I was about to publish it as such. It was then that the observation fell from you to which I have alluded, and of which I have as clear a recollection as of any one circumstance that ever occurred to me.—You were busily occupied at the time, I think, upon your British Association Report, and you made no farther enquiries about the specimen, either then or the following evening which I spent at your house.

Considering the relative position in which I and Mr. Lyell have stood with respect to each other, and that there has not been on his side any disposition shown to make a return for information communicated by myself, I do not feel that there was the slightest obligation on my part, to open any negociation with him prior the publication of the *third* fossil tooth; and the opinions which he entertains, or *affects* to entertain, upon the subject, are therefore a matter of perfect indifference to me, except inasmuch as the *expression* of those opinions may injure me in the estimation of others. But as it respects yourself, if you feel (as Mr. Lyell wishes to make other parties believe you do), that my part in the transaction involved knowingly any one thing that was inconsistent with candour and courtesy, that such a feeling should exist, would be to me a source of extreme regret. The idea of anticipating *you* in any announcement, never once crossed my thoughts, for I all along looked upon the matter as a step in English tertiary geology, resting between Lyell and myself; and having, so far back as 1837, determined the mammiferous character of the *first* tooth, and examined the deposit to which it belonged, the two really essential points in its history, I felt that I had a right to have a *finger in the pie*, and seized the opportunity chance threw in my way. The subsequent location of the fossil in any one particular genus, was a matter so little affecting the abstract importance of the first fact (at least in the case of the *opossum*), that the possibility of a quarrel about priority upon that point never occurred to me. I trust that I should never attempt to grasp at scientific notoriety at the expense of creating a real foundation for the slightest shade of discord between myself and others occupied in similar pursuits; but at the same time, when having a character to deal with like Lyell's, I would never shrink when it lay in my power from securing *all* to which I felt myself legitimately entitled.

I gave Mr. Lyell a letter of introduction to Mr. Colchester, whose interest in geology solely had its origin in the friendship existing between us; and having received the greatest assistance and attention from that gentleman, Mr. Lyell writes to impress him with the notion that I had made a dishonourable use of the fossil, which, on a late occasion, he (Mr. Colchester) entrusted to me; Mr. Lyell's object being (as I think there can be little doubt), that Mr. Colchester's future discoveries should pass into more *conscientious hands*. Had the case, as it regards myself, been ever so bad, common delicacy should have kept Mr. Lyell from broaching the matter to Mr. Colchester, he being one among half-a-dozen of my private friends to whom Mr. Lyell had gone with letters of introduction from me; but under the actual attendant circumstances, I look upon the act as so utterly despicable, that henceforth, the only respect which I can entertain for the author of the 'Principles of Geology,' will be that to which he is entitled from the position he occupies in the scientific world.

I hope, in the event of Mr. Lyell consulting you upon any other particular in this disagreeable affair, that the portion of the correspondence with which it may be connected, may be placed before you, that no undue advantage may be taken of an answer given on the spur of the moment.

With many apologies for inflicting this long scrawl upon you,

Believe me,

Your's most truly,

EDWD. CHARLESWORTH.

Richd. Owen, Esq.

P. S.—Mr. Wood made a mistake about Sir James Alexander; Mr. Lyell's remark was, that the Quarterly Reviewer was a personal friend of your's. The spirit of the observation was not affected by the error as to names.

[It is hardly necessary to add that this letter was written without my anticipating the possibility of its publication. My copy of it was furnished me by Prof. Owen.]—Ed.

No. 38.

Park Cottages, Regent's Park.

My dear Sir,

With reference to the tooth which I have lately described as a bicuspid of a *Macacus*, I can only repeat, that I never suspected it to belong to a monkey till October last, when, not without some feeling of mortification, I went to Mr. Lyell, to confess that I had misled him by mistaking it for the the tooth of a 'possum. Had the case been as you suppose it, I should have warned Mr. L. at Birmingham not to speak confidently of a *Didelphys*.

The circumstance I best remember connected with your visit to the Hunterian Museum in August last, was the painful impression produced by my becoming aware of your intention to publish, as your own discovery, the quadrumanous nature of the molar which had previously been submitted to my examination by Mr. Searles Wood; but which impression was in a great degree removed, by the promptness with which you yielded to my remonstrance on the impropriety of that step.

I heartily wish that I had never seen any of these mammiferous teeth, or that you had described them in 1837, when you first became acquainted with them.

Believe me, my dear Sir,

Faithfully your's,

RICHD. OWEN.

[Post mark of Nov. 14th.]

Edw. Charlesworth, Esq.

No. 39.

My dear Sir,

Our respective impressions as to some of the circumstances attending the publication of the London-clay mammiferous remains, appear so widely to differ, that I fear it will be of little use for me to tell you that I did not intend to publish as my own the discovery of the quadrumanous nature of the molar tooth in the possession of Mr. Wood. Had I contemplated the so doing, my communicating (as you intimate that I did), that intention to yourself with whom the identification exclusively rested, would have been strange in the extreme. I presume, therefore, that your painful impression must have originated in some misconception which I will not attempt to explain: and still less will I endeavour to reconcile your observation as to the necessity *there would have been* at Birmingham, for warning Mr. Lyell not to speak confidently of a *Didelphys*, with your remark in Mr. Taylor's Journal, that your comparison of the tooth in question at that time had been so cursory, that you would not have considered yourself justified in publishing a statement of even its affinities to, much less its identity with the above genus.

To whatever genus or section among the "mixed feeders" you conceived it likely, in the absence of the opossums or marsupials generally, Mr. Lyell's specimen might be found to appertain, the necessity for warning Mr. Lyell as to the indefinite nature of the comparison you had then made, must have been equally as urgent as it would have been had your doubts been directed towards the group which I specified; and I cannot therefore comprehend the force or bearing of the observation in your letter.

I remain, dear Sir,
Faithfully your's,

EDWARD CHARLESWORTH.

Richd. Owen, Esq.

No. 40.

Nov. 18, 1839.

Mr. Charlesworth encloses to Mr. Lyell a copy of a letter addressed to Mr. Charlesworth by Professor Owen, as it contains a statement at variance with Mr. Charlesworth's own account (already in Mr. Lyell's hands), of some circumstances connected with the publication of the London clay mammiferous remains.

Mr. Lyell will see that Professor Owen advances a fresh charge against Mr. Charlesworth; not, however, of any act committed by Mr. Charlesworth, but of one which he intended to commit, had not that intention been frustrated by Mr. Charlesworth's own communication of it to Professor Owen!!!

This new charge implicates also Mr. Searles Wood, since Mr. Charlesworth could not possibly have claimed the determination of the quadrumanous fragment as his own, without that gentleman conniving at, and becoming a party to the fraud.

Before Mr. Charlesworth had seen, or before he had heard of the fossil in question, Professor Owen had compared it and pronounced it to be identical with an existing *Macacus*, and upon Mr. Wood's subsequently placing the specimen in Mr. Charlesworth's hands, Mr. Charlesworth communicated to Mr. Wood his doubts as to the correctness of Professor Owen's identification, and which doubts have since proved to have been well founded.

From the complexion which the affair has now assumed, Mr. Charlesworth plainly perceives that a determination has in some quarter been formed to affix a stigma of a dishonourable kind to the share which he has had in the publication of the London-clay mammiferous fossils; and rather than that a notion of this nature should be privately whispered, Mr. Charlesworth thinks it better that the whole subject and correspondence should be laid before the scientific public.

No. 41.

16, Hart St., Nov. 25, 1839.

If Mr. Charlesworth should think fit to print certain letters which were written to him by Mr. Lyell without any expectation of their being made public, Mr. Lyell wishes it to be known that this is done without his sanction or participation.

Mr. Lyell also begs, that in that case the whole correspondence which has passed on both sides may be published in full, and that this note may be added to the rest.

No. 42.

Mr. Charlesworth conceived that the word 'correspondence,' as used in his letter to Mr. Lyell, of the 18th inst., necessarily referred to the letters on both sides, and that it could not be construed as referrible only to "certain letters" of Mr. Lyell's.

Mr. Charlesworth willingly accedes to Mr. Lyell's request respecting the including his note of yesterday, with the rest of the correspondence.

As it regards Mr. Lyell's protest against the publication of the letters in question, Mr. Charlesworth has to observe :—

1st, That the correspondence contains no details of a confidential nature.

2dly, That Professor Owen, in the 'Magazine of Natural History' for April, 1838, spoke of M. Coste (a continental physiologist), as a pretender, who sought to appropriate to himself a discovery which had been communicated to him by Prof. Owen; and that Mr. Charlesworth having then expressed, as the Editor of the Magazine, his own unfavourable opinion of the part apparently transacted by M. Coste, is now himself charged by Prof. Owen with the prospective commission of an act involving the same culpable violation of principle.

3dly, If the charge or charges against Mr. Charlesworth be founded on truth, Mr. Charlesworth himself is the only party who has [or ought to have] anything to fear from the publication of the correspondence, as such a publication can only tend to elicit other facts or circumstances connected with the matter which may not yet have appeared. On the other hand, if the charges are destitute of foundation, Mr. Charlesworth is certainly justified in attempting to vindicate himself by the publication of the correspondence, as well as that of any circumstantial details bearing upon the subject, which it *may* be in his power to advance.

Nov. 26th.

No. 43.

16, Hart Street, Bloomsbury Square,
Dec. 11, 1839.

Sir,

Since I last heard from you, when you announced your fixed determination to print the correspondence which has passed between us, I have seen your letter to Mr. Owen, dated Nov. 10, 1839. In reply to several observations therein contained respecting me, I request you to publish the following statement. In July, 1839, I was shown, when at

Ipswich, a fossil tooth in the possession of Mr. Wm. Colchester, which he had procured at Kyson about two years before, which I recognized as decidedly mammiferous, and I therefore examined attentively the stratum near Woodbridge from which it came, in order to make up my mind whether the deposit really belonged to the London clay. Being satisfied on this point, I obtained leave from Mr. Colchester to take it to town, and to show it to Mr. Owen, who supposed it to be the molar of an Opossum. I immediately wrote letters both to Mr. Colchester and Mr. Wood, who were then residing near Woodbridge, begging them to search in the sand at Kyson, and endeavour to find other remains of mammalia. The result of their search, after they received my letters, was the discovery of two other fossils. One of these, which I shall call No. 2, was obtained by Mr. Wood, and submitted by him to Mr. Owen, who decided that it was the jaw of a monkey of the genus *Macacus*, while the other fossil, which I shall call No. 3, was placed by Mr. Colchester in your hands. Shortly after this, being in Scotland, I received letters from Mr. Wood, in one of which he told me that it was his intention, jointly with Mr. Owen, to give an account of the newly-discovered *Macacus*, No. 2. I was well satisfied to hear of his intention, but it was understood between us, that I should reserve to myself the announcement to the British Association of Mr. Owen's opinion respecting the first tooth, as well as of other mammiferous fossils from the Red Crag, at Newbourn. You have since stated that you saw the Kyson molar two years before (in 1837), in Mr. Colchester's collection, and recognized its mammiferous character. I have no doubt of the correctness of this statement, but I believe that the tooth would have remained unnoticed to this hour, and unknown to the scientific world, and I feel sure that no other mammiferous remains would have yet been discovered at Kyson, but for my visit there in July last.

At Birmingham, in the last week of August, I communicated to the British Association Mr. Owen's opinion of the marsupial nature of the grinder first discovered, or No. 1. It was not till after I had conversed with Mr. Owen on this subject, at Birmingham, that he put into my hands the August[1] number of the 'Magazine of Natural History,' con-

[1] Since writing the above, I have been reminded by Mr. Charlesworth, that the printed notices alluded to on the Kyson fossils were from the September number of the 'Magazine of Natural History,' although copies of them were put into my hands before the end of August, at Birmingham, and had been sent there in that month by Mr. C. to Professor Phillips, for distribution to members of the British Association.

taining not only the notice of himself and Mr. Wood, on the *Macacus*, No. 2, but also a paper of your's, on the fossil, No. 3, which you announced as a *Didelphis*, without making any allusion to the opinion previously given by Mr. Owen, that the fossil, No. 1, belonged to a Didelphis. I told Mr. Owen that I thought this uncandid, which led him to state how nearly you had anticipated him in the publication of the discovery of the quadrumanous nature of the fossil, No. 2, and that you had only been prevented from so doing by his remonstrance, and his insisting on his right to communicate himself to the public the result of his own prior investigations, which had already been made known to you.

A consideration of these circumstances decided me not to offer my account of the Kyson and Newbourn fossils for publication in the 'Magazine of Natural History,' where they might otherwise have succeeded Mr. Wood's paper; and I accordingly agreed with Mr. Owen, that an account of them should be inserted, with Mr. Taylor's permission, in one of the next numbers of the 'Annals of Natural History.' It then became desirable that Mr. Owen should compare the fossils, Nos. 1 and 3, both then supposed to be marsupial. I therefore sent a reqnest to Mr. Colchester, to lend me the jaw, No. 3, with which he immediately complied; and in my letter to him, I mentioned that I did not feel satisfied (for reasons already explained to you in my letters) with the manner in which you had proceeded in regard to the publication of No. 3.

In your letter to Mr. Owen, (Nov. 10, 1839), you reassert as a fact, that previously to going to Birmingham, Mr. Owen had intimated to you a suspicion that the first discovered tooth might turn out to be quadrumanous. This he has positively denied, and I repeat my conviction, that you must be entirely mistaken on this point, as it is impossible, that during my intercourse with Mr. Owen in August and September, when conversing on this subject, I should have remained ignorant of any doubts entertained by him, of the marsupial nature of the tooth, No. 1, which he allowed me to announce on his authority, at Birmingham. It was six weeks after you wrote your paper, that the suspicion entered Mr. Owen's mind for the first time, and he immediately came to tell me that he felt some annoyance at having, after his first cursory comparison, misled me.

I shall now conclude by observing, that whatever difference of opinion there may be on other points, there will, I am persuaded, be but one opinion as to the propriety of your printing this correspondence, and making your Magazine a vehicle for circulating the details of a private misunderstanding, with which the scientific public is wholly unconcerned.

I am, Sir,
Your obedient Servant,
CHAS. LYELL.

Editor of the Magazine of Natural History.

No. 44.

December 15.

Sir,

I return you your letter of the 11th instant, that before its publication you may correct an error occurring in the middle of the second page, where the words "August number" should be *September* number. If this error be merely accidental, it can be easily rectified, but if it have arisen from a mistaken belief that my paper on the fossil opossum was published in August instead of September, *that* belief will help to explain some passages in your communication to me of November the 1st.

When I wrote my notice of the fossil No. 3, I knew that whatever you might say about the history of No. 1, would, by the Athenæum report, have, by some days, priority of record over mine; and this being the case, for reasons which I have fully explained, I thought it better to say nothing more of the tooth No. 1, than what I had learned from my own examination of that fossil before it quitted Mr. Colchester's cabinet. The subsequent result, and my history of the attendant circumstances, must, I think, satisfy every one that this course was the right one to pursue. Before the termination of the Birmingham meeting, I sent to my friend Professor Phillips some separate copies of the three communications having reference to the discovery at Kyson; thinking they would be distributed and read with great interest by the members of the Geological section. I might have affixed a date to my paper, as it was drawn up on August the 24th; and this would have given me an apparent priority over your's, which is recorded as read August 28th, but I attached no date to my communication, and it therefore can only be referred to in relation to the month of September.

I have received from Professor Owen a statement to be published with the rest of the correspondence. Professor Owen, finding that I am likely to be able to refute the charge he preferred against me in the letter of which I sent you a copy, now labours hard to make it appear that Mr. Wood had no right to commit to me the drawing up a description of the fragment No. 2, and that this intention of *describing* the fossil, was the one that caused his *painful impression.* Now the "discovery of the quadrumanous nature" of a tooth, can only mean the finding out that its proper location is in the group *Quadrumana*, as opposed to the *Mammalia* generally. A construction differing from this cannot be *forced* upon the words; and it is idle for Professor Owen to calculate upon mystifiing the whole affair by the extraordinary communication he has sent me, or that the palpable inconsistencies between the contents of his first letter to me, and the documents which he has subsequently penned, will not be pointed out and readily perceived.

The subscribers and contributors to the 'Magazine of Natural History' constitute the only portion of the public with which I am immediately concerned. How far the giving publicity to a matter so intimately associated with the reputation of the individual conducting the Journal which they support, may or may not interest them, is a question which I must risk.

Whatever comments I may feel myself called upon to offer upon the correspondence itself, or upon any of the circumstances with which that correspondence is connected, I disclaim, in resolving upon the intended publication, any other object than that of openly showing that my conduct in relation to so important a discovery as the one at Kyson, has not, in my capacity of a public journalist, or that of a private individual, presented any features which could justly be seized hold of as affording grounds for animadversion.

As I did not contemplate the necessity for publication when I commenced this correspondence, you will, perhaps, upon the return of your own letter now enclosed, place temporarily in my possession the communications you have received from me, that I may carefully compare my copies with the originals.

I remain, Sir,
Your's &c.,
EDWARD CHARLESWORTH.

No. 45.

December 13, 1839.

Sir,

You have announced in the advertisement sheet of the last Number of your Magazine, your intention to publish the correspondence which has passed between us on the subject of the extinct mammiferous remains, lately found in Suffolk. In the event of your persisting in this course, I beg that you will add to that correspondence the present statement, and subjoined letters.

In my letter elicited by your's of November 10, 1839,[1] I allude to the painful impression produced by my becoming aware of your intention, in the early part of August last, to publish, as your own discovery, the quadrumanous character of the fossil submitted to my examination by Mr. S. Wood, in the preceding month.

Your answer, dated November 19th, implied a denial of your having entertained any such intention, by reference to the improbability of my having been able to frustrate it by your own communication of such intention to me. It is nevertheless a fact, that you did mean to anticipate my publication of that discovery, notwithstanding that you yourself declared it to me.

But I was aware from other sources, that after my examination and determination of the fossil in question, you had

[1] Of this letter I have forwarded you a copy.

procured it from Mr. Wood, and had had illustrative cuts of it prepared, having, as Mr. Wood has since informed me, undertaken to furnish a description of it to accompany Mr. Wood's paper. The wood-cuts of the fossil were ordered by you, according to Mr. G. Sowerby, Jun., before the 12th of August last, without my knowledge, and without the advantage which might be derived from my directions. It was not until after I had insisted on my right to describe the fossil which I had determined, that Mr. Sowerby received directions from you (dated 15th August), to call upon me with the proofs of those cuts, which were then applied to illustrate my description.

The following are the letters which have passed between Mr. Searles Wood and myself on this subject.

No. 46.

6, Park Cottages, Regent's Park,
Dec. 5, 1839.

Dear Sir,

A letter has been drawn from me under the usual confidence of private correspondence, by Mr. Charlesworth, which I observe he has advertised in the December Number of his Magazine to publish. In that letter I allude to the fact, well known both to you and myself, that Mr. Charlesworth had entertained the intention of describing the fossil tooth and fragment of a jaw of the *Macacus*, which he received from you in the early part of August last, after its quadrumanous nature had been determined by me. Mr. Charlesworth now denies that he ever entertained that intention, and as such denial implies that I had, without motive or provocation, wilfully invented a groundless charge against Mr. Charlesworth, I am compelled to request from you a statement, whether you have or have not reason to believe that Mr. Charlesworth did intend to describe the fossil in question, before I insisted on my right to describe what I had been the first to determine.

I remain,
Dear Sir,
Your's very truly,
RICHD. OWEN.

Searles Wood, Esq.

No. 47.

Bernard Street, Dec. 6, 1839.

My dear Sir,

The result of the examination, which, at the end of last July you kindly undertook for me, of the Kyson Quadrumanous Remain, led me to conclude, that as the jaw was that of a species of monkey which is so commonly carried about in this country, it might probably have been accidentally introduced into the heap of sand from which it had been sifted. I had, therefore, no intention of publicly calling attention to it; I immediately wrote to Mr. Lyell, telling him of the supposed fossil, and also mentioned my fears respecting its genuineness. A few days afterwards I showed the specimen to Mr. Charlesworth, and upon a first inspection, he assured me it was a genuine fossil, from the circumstance of its agreement in colour and general appearance with the supposed opossum's tooth which he had previously examined in Mr. Colchester's cabinet; and shortly afterwards I was confirmed in the correctness of this opinion, by his pointing out to me in what it differed from the recent jaw of a Macaque which he had at his apartments in Great Russell Street; this led me to determine to publish Mr. Colchester's fossil in the succeeding number of the *Magazine of Natural History*, but as I did not feel at home upon the subject of comparative osteology, Mr. Charlesworth undertook to furnish an osteological description, to accompany my letter, stating in what particulars he believed the Kyson fragment to differ from the corresponding part in the existing Macaci. It was, of course, my intention to mention in my letter, the obligation I was under to you, for having told me the genus to which the fragment belonged, but I cannot admit that any right of describing it was vested in you as a consequence of that determination; still, had I known that you entertained the least wish to describe the fossil, I would willingly have left it in your hands at the time I first showed it you. I think you must be in error, when you say that Mr. Charlesworth *now* denies his intention of describing the jaw; because, under the circumstances I have mentioned, he had my full permission to do so, and the illustrations were executed for his own description. I am aware, however, that Mr. Charlesworth *does* deny any intention of claiming, as his own, the *original determination* of its quadrumanous character, and this imputation I suppose to have originated in some misapprehension.

I remain, my dear Sir,
Your's truly,
SEARLES V. WOOD.

Richd. Owen, Esq.

No. 48.

6, Park Cottages, Regent's Park,
Dec. 7, 1839.

My dear Sir,

I beg to thank you for your prompt and obliging answer to my note of the 5th December, and to request that I may add it to the correspondence which Mr. Charlesworth seems determined to publish in his Magazine. I was not aware, until I received your letter, that you had mistaken me so far as to suppose that I thought your fossil belonged to a recent species. I alluded, at the time of my first comparison, to the possibility of such being the case, because we had not, in England, the jaws of every known existing *Macacus*, wherewith to compare it; and to impress upon you the necessity of obtaining the most decisive evidence of its disinterment from the Eocene stratum, in which it had been stated to you to be found. In pointing out to you the resemblance of the fossil to the *Macacus radiatus*, I wished merely to demonstrate its generic relationship, adverting, at the same time, not only to the difference in size, but in shape.

Permit me to add, in reference to the following passage in a letter from Mr. Charlesworth to Mr. Lyell, "This new charge implicates Mr. Wood, for Mr. Charlesworth could not have claimed the determination of the quadrumanous fragment as his own, without that gentleman conniving at, and becoming a party to, the fraud:" that I thoroughly repudiate any sentiment towards you, akin to such as Mr. Charlesworth has given utterance to, and that

I remain,
My dear Sir,
Faithfully your's,
RICHD. OWEN.

Searles Wood, Esq., &c., &c.

No. 49.

13, *Bernard Street,*
December 11*th,* 1839.

My dear Sir,

If I rightly understand your request it refers to the publication of my letter, in which case you are quite at liberty to make that use of it. Indeed I think for my own sake, as well as Mr. Charlesworth's, full publicity had better be given to all the correspondence that has taken place, unless some other arrangement satisfactory to all parties should be made.

Mr. Charlesworth did not show me or consult me about the letter to Mr. Lyell, from which you have sent me an extract, but I agree with him, that the intention imputed to him in your statement of what occurred at the College of Surgeons, between yourself and Mr. C., does indirectly implicate me, as Mr. Charlesworth must in that case have persuaded me to suppress the fact of my having been in the first instance to you with the fossil, and as I am sure no such intention was entertained, or even thought of, by either of us. I hope that Mr. Charlesworth's public explanation will remove any suspicion of dishonorable motives having influenced either his own or my conduct in this matter.

I remain, my dear Sir,
Yours truly,
SEARLES V. WOOD.

To Richd, Owen, Esq., &c. &c.

No. 50.

Park Cottages, Regent's Park,
Dec. 13.

My dear Sir

I beg again to disclaim the imputation of any fraudulent or dishonourable motives, either to yourself or Mr. Charlesworth, in his intended description of the quadrumanous fossil belonging to you The ideas of dishonour and fraud, as connected with that intention, are exclusively Mr. Charlesworth's.

I felt hurt when I became aware of his intention, deeming it unfriendly, and, as far as I could ascertain the circumstances under which he had taken upon himself to describe that fossil, uncandid. On one occasion only have I ever suffered an intimation of the annoyance it occasioned me to escape my lips. If Mr. Charlesworth was led to suppose that he had been the first to discover the extinct character of your quadrumanous fossil, he may have conceived himself justified, having your permission, in publishing as his own, what I again repeat was my discovery.

I remain,
My dear Sir,
Faithfully your's,
RICHD. OWEN.

Searles Wood, Esq.

I now subjoin the following answer from the curator of the museum of the Zoological Society, to a question proposed to him by me, and quoted by Mr. Waterhouse, in his letter.

No. 51.

Zoological Society, December 5, 1839.

My dear Sir,

I have but this moment received your note, and hasten to answer your question, whether Mr. Charlesworth, in the early part of August, did not avail himself of my assistance in comparing the fossil tooth described in the 'Magazine of Natural History' for September, 1839, with the recent quadrumanous *crania* in the Society's museum. Mr. Charlesworth *did* bring that tooth to the Society for the purpose of comparison. Our specimens of skulls being under lock and key, I got them out for him, and he examined them in my presence. I also examined the fossil, and compared it with the recent skulls, but I am sure I gave no opinion to the effect that the fossil formed part of the jaw of a *Macacus*.

I am, dear Sir,
Faithfully your's
GEO. R. WATERHOUSE.

To an enquiry which I made of Mr. G. B. Sowerby, Jun., as to the date when Mr. Charlesworth ordered the wood-cuts of the quadrumanous fossil, I received the following answer.

No. 52.

Dear Sir,

My only dates respecting the woodcuts Nos. 57 and 58, p. 444 and p. 447, in the 'Magazine of Natural History,' which were ordered by Mr. Charlesworth, are very vague, and refer, not to the time of the instructions being given, but of the drawings being executed. To the best of my knowledge, the magnified figures were drawn on or about the 12th of August, having been ordered two or three days before, and the natural-size figure, the latter end of the same week, or the beginning of the following.

My dear Sir,
Your's truly,
G. B. SOWERBY, Jun.

R. Owen, Esq.

This is the evidence which leads me to conclude that, had not my remonstrance on the subject of publishing my own account of Mr. Wood's fossil been effectual, the wood-cuts of it, executed by your instructions, would have illustrated your description of that fossil instead of mine.

A few words, now, with respect to your article entitled 'On the discovery of a portion of Opossum's jaw,' (pp. 448 and 450, 'Mag. Nat. Hist.,' Sept. 1839). You knew that I had previously announced to Mr. Lyell, the existence of a marsupial fossil in the London clay: Mr. Colchester and Mr. S. Wood were also aware of that fact before you published your 'Discovery.' Moreover, your own conclusion, that the jaw which you obtained from Mr. Colchester was that of an opossum, is at variance with your description of that fossil; for, if the tooth which you say (p. 450), "appears to be the one immediately succeeding the true molars," had been what you supposed it, then the fossil could not have belonged to the genus *Didelphis*, for the reasons which I have assigned in the 'Annals of Natural History' for November.

I have reason to believe that you were aware of the general result of my comparison of Mr. Colchester's London-clay fossil molar tooth, submitted to me by Mr. Lyell, in June last, before you published your own account of the opossum's jaw from the same formation. It is no justification of your silence with respect to my previous announcement of a fossil marsupial in the London clay, that I was induced to alter my opinion of it some time after the publication of your paper.

I remain, Sir,

Your obedient Servant,

RICHD. OWEN.

Editor Mag. Nat. Hist.

INDEX.

BRITISH MUSEUM Z D NATURAL HISTORY

END OF THE FOURTH VOLUME.

ERRATA.

Page 25, line 2, for *Hollowasii*, read *Hollowaysii.*
— 25, — 19, for *Conus deperditus*, read, *Conus diversiformes.*

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