

little modified on ascertaining the true organization of those parts from which we have hitherto drawn some of our characters; and whether, on combining the result thus obtained with the results of a more extended anatomical investigation, it is not at least possible, that the relation of supposed affinity may prove eventually to be one of mere analogy.

The only instance in which anatomical researches have been misapplied, as regards Zoology, is that in which they have led to the formation of systems based upon certain parts of the internal structure of animals, without regard being paid to the relation in which these parts stand to the rest of the organization, and the consequent degree of influence which they exert over the economy. This has been sometimes attempted by persons, who have generally been good anatomists, but ignorant of the first principles of Zoology. Yet even these systems are not entirely without their use to the naturalist.—Although worthless as a whole, they may suggest certain affinities which might otherwise have been passed over. They at least teach us the method of variation of those organs upon which they are established; acquaint us with their true value; and throw more or less light upon the real relations which subsist between those characters which anatomy furnishes, and those with which Zoology is more immediately concerned.

But it is time that we pass from these remarks to inquiring into the present state of the science, as regards that, in which the scientific Zoologist is so much interested,—a natural classification of animals.

And here we must state, that it is not our intention, neither would it be practicable within the limits of this article, to analyze in detail any of the numerous systems which have been brought forwards of late years. We simply purpose making a few observations on the views which naturalists seem to entertain on this subject. The most important feature in the present times is—the endeavour, now made almost universally, to refer the affinities of animals to some fixed principles of arrangement,—some general plan determined by certain laws. We can hardly doubt of there being such a plan, upon which the Great Author of Nature has formed the innumerable creatures which people the earth. When we see the harmony which pervades the rest of his works;—when we become acquainted with the beautiful laws which have been discovered in other sciences;—when, especially, we call to mind the principles established in those which border most closely upon Zoology;—we can hardly but conceive, that this science also admits of some generalizations, and that animals are as much under the influence of given laws in respect to their affinities, as they are in respect to their structure. Now it may be true that the first idea of

the existence of such a plan belongs to a much earlier period than the present ; and that Linnæus, and even the older naturalists, had some faint notions upon this subject ; but, until the close of the last century, there was hardly any attempt made to elucidate its principles ; and only quite recently has this attempt, with some few exceptions, become general. The reason is obvious. The science had not made sufficient progress to warrant the endeavour. And for the same reason, it is obvious we must await the arrival of many years to come before we can hope to see that endeavour perfected. We are not, then, surprised to find, that although naturalists are everywhere pursuing the same object, they are following very different roads in the hope of attaining it. We should wonder if it were otherwise. When we think of the immense field which Zoology lays before us,—of the comparatively small portion of that field as yet explored,—and of the impediments which arise to make our path difficult ;—when we reflect further, how much is required to determine the exact relations of a single group,—and how often we are left to mere conjecture and analogy, in the absence of facts, which can alone establish our reasonings on a sure basis ;—we are prepared to meet with much variety of opinion upon such a subject. It is, indeed, more than probable that the classification of animals is destined yet to undergo, at least in part, great and important changes.* The views of naturalists will differ, according as they have paid most attention to this or that department of the science, in each of which, from the unequal progress of our knowledge, we appear to recognize the influence of distinct principles. Their minds, too, will receive, unconsciously to themselves, a slight bias, arising from the nature of their other studies, or of those which led them to the particular study of which we are here speaking. It is only time which can do away with the erroneous conclusions of a partial or a prejudiced judgment. Contrariety of opinion originating in the above sources, is for a season unavoidable ; and we must wait patiently till we have received sufficient light to determine those questions, with respect to which naturalists are so much at issue.

But independently of what has been above-mentioned, there is another and distinct ground of difference observable in the attempts which have been made of late years to arrange animals upon some uniform plan. In fact, as has been recently said,† there are two distinct prin-

* We may mention in this place, that a new arrangement of the Animal Kingdom has been recently brought forward by Professor Ehrenberg of Berlin. It is divided into 29 classes founded on the organization, and on the general existence of one type of structure, as respects the system of sensation, circulation, locomotion, nutrition, and reproduction.—See *L'Institut*, 1835, p. 305.

† *Lam. Hist. Nat. des An. sans Vert.* (2d edit. 1835.) tom. i. p. 336.

ciples upon which we may proceed, each being in accordance with certain obvious relations existing in nature. We may either take for our guide the principle of the subordination of characters, successively grounding our divisions upon modifications of structure becoming less and less important as we proceed downwards; or we may seek to distribute animals into as many principal groups as there are well-marked series, each of these series being characterized by a peculiar type of organization gradually becoming more and more simple in its descent. The former of these principles is that which was first developed, and so strenuously upheld, by the great Cuvier, and which is still adhered to by a large proportion of the naturalists in France and in our own country. The latter may be distinguished in many of the systems which have appeared in Germany, as well as in some which have emanated from other quarters. It may be said, that, as there can be but one natural system strictly speaking, it is impossible that both these principles can conduct to true results. This would be correct if animals exhibited only one *kind* of relation to each other. But we know that they exhibit more than one; and hence we are not without hope, that, notwithstanding the opposite nature of these principles, the day may come, in which it will be found possible to reconcile the views to which they have respectively given birth. We, in fact, are inclined to think, that some slight approach to this reconciliation already shows itself in the theories of those naturalists who distinguish between *relations of affinity* properly so called, and *relations of analogy*. It does not follow, that the theories themselves, by which it is attempted to explain these relations, and to refer them to given laws, are necessarily correct. Which, or whether any, of them can be so regarded, is a matter for time to determine. We would here simply draw attention to a point which may hereafter prove in some measure a bond of union between two conflicting opinions of the present day, and conduct at length to truths of which it will be then found that each party had some faint glimmerings.

There is another circumstance which tends to confirm our hopes that naturalists are approaching gradually to some just and uniform results, and which merits notice. And that is, that, notwithstanding the diversity of their views respecting the details of systematic arrangement,—there may often be observed certain features of resemblance (not of identity) in the general principles from which they set out. This resemblance may be especially traced in two very different schools established in Germany and in our own country respectively. Thus, for instance, the principle first laid down by Oken, that the classes of the animal kingdom are severally characterized by the particular develop-

ment of some one of the animal functions, and that these characters are continually reproduced in the subordinate divisions, causing each group to contain representations of those above it together with its own peculiar type ;—this idea offers some analogy to the “ Theory of Representation” of Mr Swainson, * which is itself only an extension of one of the views adopted by Mr Macleay. †—We may also refer to the idea, that the subordinate divisions of every natural group are controlled by some fixed number,—as another point of resemblance in the two schools. This principle, indeed, seems to follow from the one just mentioned as a necessary consequence, since if two groups do not contain exactly the same number of divisions, it is impossible that the larger number can be all duly represented by the smaller.

We must observe in this place, that, in making the above remarks, we do not wish to be considered as upholding the particular views of either school just alluded to ; much less the details of those systems, which they respectively advocate. We are simply adverting to a certain similarity of principle which pervades them both. That animals do appear, in a multitude of instances, to represent, as it were symbolically, others, with which they are connected by no immediate affinity, must have struck the most inattentive observer. It is also in the highest degree probable, that these relations, as well as all others, are in accordance with some fixed laws. But whether these laws will be found in the end exactly as they are laid down by either of the parties above referred to,—whether by making some slight modification in either of their peculiar theories, or by combining their principles in any manner,—we may be able to attain ultimately to some result, not very different from the results at which they have respectively arrived ;—these are questions, which, as we said before, time only can determine. We are aware that this assertion will appear somewhat strange to Mr Swainson, who has recently developed more at large his modifications of Mr Macleay’s theory, ‡ and who considers his own first laws as established upon incontrovertible evidence. But with all deference to the talents of that distinguished naturalist, and with every wish to receive the truth, when it shall be proved to the satisfaction of unprejudiced minds, we cannot entirely accede to this opinion. Mr Swainson does not seem to be aware of the existence of a school of naturalists in Germany, § who profess equally with himself to refer the classification of

* *Faun. Bor. Am.* part 2, p. xlvi. ; and *Classific. of An.* p. 236.

† *Hor. Entom.* part 2, p. 518.

‡ See his *Preliminary Discourse on the Study of Natural History.* Lond. 1834. Also his *Treatise on the Classification of Animals,* 1835.

§ We infer this from the circumstance that no notice whatever is taken of this

animals to some *general* principles, and who are equally satisfied with him, that they have attained to a knowledge of these principles in the main. We mention this, in order that it may not excite his surprise, if those who attach themselves to no particular party should consider one set of principles as much entitled to their regard as the other, and should withhold from both, at present, their unlimited confidence. We should be sorry to be thought to offer any opposition to those which Mr Swainson advocates. We simply wish to see them better substantiated, than, in our opinion, and, we believe we might add, in that of a large proportion of the naturalists of this country, they are at present. With respect, in particular, to the law of circular affinities, it is undoubtedly true, that many groups, which have all the appearance of being natural, evince a decided tendency to such an arrangement, and, reasoning from analogy, it seems highly probable that this principle may extend throughout nature; but let us investigate the affinities of animals more closely, of those especially, which, being low down in the scale of organization, have as yet obtained but slender attention from naturalists, before we consider this point as resting upon anything like "*mathematical certainty*."—In fact, the supposed proof of this principle is involved in the fate of two others:—that which assigns a definite number of subordinate divisions to each group,—and that upon which depend those symbolical representations of which we have before spoken.—The whole theory may, in short, be said to rest upon relations of analogy conforming to an apparent law, these relations themselves, however obvious in certain cases, being, when remote, so obscure as to be hardly palpable, when very near, so striking, that, according to Mr Swainson's own shewing, there is danger of confounding them with relations of affinity.* We would not willingly assert, that Mr Swainson has suffered his own judgment to be warped by an active imagination. But certainly we must say, that some of the relations which he has pointed out are such as we conceive few besides

school, either in his Treatise on the "Rise and Progress of Zoology," (Prelim. Disc. part 1.) or in a subsequent Treatise on the "Rise and Progress of *Systematic Zoology*;" (Classific. of An. part 2.) We are inclined to think that the Germans are equally strangers to the school founded in England by Mr Macleay, since we find M. Agassiz adverting to the "philosophic naturalists" of Germany, as those who *alone* have sought after *general* principles.—See Notices of Commun. to the Brit. Assoc. 1835, p. 67.

* Speaking of analogical relations, Mr Swainson observes;—"In proportion as groups approximate, other dissimilarities of course become less, so that when we descend to genera which follow or come very close to each other, it is impossible to decide, at first sight, whether the relationship be one of analogy or of affinity."—Prelim. Disc. &c. p. 215.

himself can distinctly appreciate ; that, at any rate, points, with respect to which there exists any ground for difference of opinion, can hardly be advanced in proof of a theory, whatever there may be to render that theory *probable*.

In fact, we believe the time is not yet come, in which it is possible to *demonstrate* the truth of any theory whatever. We may shew the apparent influence exerted by certain principles which seem capable of being deduced from the data already in our possession ; but we conceive that we must be more acquainted with the structure and affinities of animals, before we can establish such principles upon a sure basis. Nevertheless, we would not discourage persons from making the attempt. We are rather inclined to let all naturalists hold their own opinions on this subject, convinced that truth will work its way in the end, and that, if not found in exact agreement with what they had anticipated, it will at least have received some light from their researches.

In the meanwhile, those who keep aloof from speculations of the above kind should endeavour to enlarge the bounds of our positive knowledge as regards this science. Let them investigate some of those departments, to which so little attention has been as yet given, and try to place the several ramifications of the great system of nature upon a more equal footing. By so doing, they will supply sound materials for such as choose to speculate upon the exact plan on which that system is constructed, at the same time that they help the science forwards to that point, which, when arrived at, the plan will in some measure develop itself. For it should be remembered, that there are two distinct objects to be attained, as regards the natural system :— “ first, the arrangement of all animals according to their true relations ; and then the discovery of those general principles (assuming that there are such) by which these relations are governed.”* Now had we already effected the first of these objects, which can only be the result of a rigid analysis, extended to all the groups of animals with which we are acquainted, we should probably have little difficulty in evolving the latter. At present, however, this has been but very imperfectly done. And, perhaps, we are more likely to proceed rightly in this matter, when we go to work unfettered by any theoretical views, which in our anxiety to uphold, we are liable to have our judgment biassed to a degree to which we ourselves are totally unconscious. It is, undoubtedly, at all times agreeable to true philosophy to ascend by inductive reasoning from known facts to general

* The above passage will be found nearly similar to what Mr Swainson has expressed at p. 200 of his Preliminary Discourse.

principles : It is undoubtedly allowable, without waiting for the acquirement of all possible facts that can be obtained, to try any theory which explains those already in our possession, by applying it to such as may be observed afterwards. But the danger is, in the science now before us, especially when we have to deal with analogical relations, that we *mistake for facts*—points, which may certainly appear as such to us, but which are of that nature that they *must* infallibly strike different observers in different lights according to the impressions upon the mind at the time of viewing them. Hence it is that we conceive, that we are more likely to see these relations truly, when we have no theory to support ;—when there is nothing which is likely to warp our judgment. We believe that if we made it our first endeavour to arrange all animals according to their best ascertained affinities, at the same time noting any other less obvious relation ; and if we then drew lines of separation between such groups as appeared well characterized, taking care to assign to each a rank proportioned to its true value ; we should gradually arrive in this manner at as just a conception of the true order of nature as, perhaps, it is possible to attain.* For, after all, it becomes a question, whether, assuming that there is some definite plan in nature grounded upon fixed principles, we can ever hope to understand more than part of it. When we consider how much is requisite to complete the history of a single species, and that we need to be acquainted with this history, not only in the case of all existing animals, but of all lost ones also,—we may conceive how vast must be the task of tracing the relations which one species bears to the others. We can scarcely do more than make some approximation to the truth.—We can only arrange our groups in such a manner, that there be no other known ones more nearly allied to be brought in between those which stand next each other. And the system which does this may be called natural, † although it may not

* The above will be found nearly in accordance with Lamarek's judicious observation on this subject, which it may be well to repeat here. He says,—“ Nous avons senti que, pour réussir à établir une bonne *distribution* des animaux, sans que l'arbitraire de l'opinion en affaiblisse nulle part la solidité, il était nécessaire, avant tout, de rapprocher les animaux les uns des autres, d'après leurs rapports les mieux déterminés ; et qu' ensuite, l'on pourrait, sans inconvénient, tracer les lignes de séparation qui détachent les masses classiques, ainsi que les coupes subordonnées, utiles à établir, pourvu que les rapports ne fussent nulle part compromis par la composition et l'ordre de nos diverses coupes.”—Hist. Nat. des An. sans Vert. (2d edit.) tome i. p. 285.

† This remark is Cuvier's but we are unable to refer to the exact place in which it is expressed.

serve to elucidate all the laws by which the exact plan of nature is regulated.

We would here briefly remind those who enter upon the investigation of the natural affinities of animals, of the importance of being guided by certain acknowledged principles,—quite independent of those general laws above alluded to, indeed independent of all theory whatever, and equally deserving our regard, whether we incline to any theory or not. They respect the *value of organs* and the *subordination of characters*. Except we set out in our enquiries with some definite ideas on these points, we shall be perpetually falling into error. In order to discover the true affinities of animals, it is not sufficient that we compare their respective characters,—and then note what marks of resemblance are to be traced between them :—we must first ascertain what is the relative value of the different organs, which furnish those characters. And how is this to be determined?—By observing which organs exercise the most important functions, or are of most general occurrence. It will be found on such an enquiry, that some appear to take the lead of others,—that while some are constantly present, others are often wanting,—and that even among the former, we may trace, in different instances, very different degrees of variation. Now by paying regard to these circumstances, we establish gradually that subordination of characters, to which we have before alluded as first employed in this science by Cuvier, and which must be the basis of all natural classification. Our limits will not allow us to enter at any length upon this subject, or we might advert to certain rules, founded—partly upon observed facts—partly upon common principles of reasoning, which have been drawn up with reference to these enquiries, and which ought to be well studied by the Zoologist. * We regret, indeed, that, in some instances, they have not been more attended to. Had it been otherwise, we should not see so many conflicting opinions respecting classification ;—we should not see groups of very unequal value placed exactly upon the same footing,—genera founded upon the most trivial distinctions, and such

* Many of these rules apply equally well, or with some slight modification may be made applicable, to the two sciences of Zoology and Botany. On these grounds, we strongly recommend to the attention of naturalists some portions of the *Théorie élémentaire de la Botanique* of M. Decandolle, where they are laid down with more clearness and precision than in any zoological work with which we are acquainted. The student, however, may consult with advantage the seventh part of Lamarck's *Introduction to his Hist. Nat. des. An. sans Vertèbres*, divesting it of those peculiar theoretical views which pervade the whole of that volume.

as are connected with no peculiarity whatever of habits or economy,—in short, so little attempt made to ascertain the limits of variation in the case of those characters to which recourse is had for the purpose of systematic division. There is one observation in particular we would wish to make, the truth of which must be apparent to all who have paid the least attention to the structure of animals ; and that is, —that organs of the same kind, and performing exactly the same functions, do not necessarily afford characters of the *same* value in *different* groups. This circumstance has been much overlooked. We not unfrequently see naturalists taking as the basis of their arrangement a system of organs, which, in certain classes, are universally allowed to be of the first importance, but which, in the group under their consideration, are subject to such modifications, as clearly indicate the subordinate influence which they exert over the economy. We might point out several instances in which this error shows itself. We shall, however, content ourselves with referring to the *pulmonary* and *trachean Arachnida*, which have been made distinct classes by some naturalists, on the ground of differences in their respiratory organs, which they would seem to think entitled to as much regard in the case of the Annulose animals, as all allow them to deserve in that of the Vertebrate.

The subject of the value of characters is closely connected with a question of great importance, and which, in the present state of the science, calls for particular investigation. What are the limits of variation assigned to *species* ? We conceive this to be a problem, to which, in these days, we shall do well in directing the attention of naturalists. We need hardly say how general has been the complaint of late years, that species are now multiplied to an excess, quite overwhelming to those who wish to enter into the details of the science, and bidding fair in time to exhaust all the resources of a pure and correct nomenclature. It is undoubtedly true, that to distinguish and point out even the slightest variation of character in animals is of importance, not merely as helping others to identify those which have been the subject of our observation, but as a necessary step to a knowledge of their true affinities. We are, however, only half completing this matter, if we stop there, without proceeding to enquire whether these variations are due to a specific difference, or whether they may have resulted from the action of external or other causes. The evil above alluded to has taken deep root enough in our own country ; but in Germany it would seem to have attained to a yet greater pitch. M. Brehm, an ornithologist well known for the extent to which he has multiplied the birds of Europe, a few years back

attempted to establish no less than *seven* species of *Loxia*, or true Crossbill, as natives of Germany.* The same observer has made three species out of *Columba livia*; three out of *Emberiza nivalis*; two out of *Anas glacialis*; two out of *Larus marinus*; the same out of two other species in that genus; two out of *Procellaria glacialis*; besides many more which it would be tedious to enumerate. When we find opinions of this nature entertained, and we imagine it would not be difficult to adduce similar examples in the other departments of Zoology, can we believe that due attention has been paid to those variations of character, which we see constantly arising from local or accidental circumstances in other species, and which in these last we can indubitably trace back to such causes only? With respect to birds in particular, we cannot be too cautious how we place reliance upon mere variations of plumage; over which we know so great an influence to be exerted by age, sex, season, and have ground to suspect, in certain instances, also by climate. We know, moreover, that some of the changes arising from these causes are themselves liable to irregularity from any accidental circumstances affecting particular individuals. This is especially the case with those due to differences of age, as has been proved by rearing birds in confinement. It has been observed that, under such circumstances, an interruption often takes place for a longer or shorter period, possibly in some few instances an entire suspension, of laws, which would otherwise operate uniformly for each species respectively. Thus it is mentioned by F. Cuvier,† that *Gulls*, which in a state of liberty would naturally mature their plumage at the end of the second or third year, have been known in captivity to require one or two years more for that purpose. Analogous facts have been observed in our own country. With respect to the influence of climate in bringing about a permanent difference of character in certain species, as compared with the same species found in other latitudes, it is a subject upon which little positive information has been yet obtained. We may, however, fairly contend that the thing is possible, if not highly probable, till some direct arguments be adduced against it. Assuming, as a fact, that season and temperature cause periodic changes in the plumage of some species; it seems highly probable, that any thing which occurs to disturb the regularity of the seasons or to affect their general character, may affect, in like manner, the regularity of such changes, or at least modify the extent to which they are carried. Now any particular season departing from its usual character in this manner, may

* Bull. des des Sci. Nat. 1828, tom. xiv. p. 259.

† Ann. du Mus. tom. xi. p. 285.

exhibit, while it lasts, the *permanent* character of a distinct climate, in which may occur the same species of bird with its plumage, consequently, from the action of that climate, *permanently* modified. We regret that, in relation to this subject, we have it not in our power to consult a memoir which is said to have been presented nearly two years since, by M. Gloger to the Berlin Academy, treating expressly on the modifications induced by climate on birds. We may, however, quote a remark by M. Jacquemin, which leads us to think that the above idea is far from problematical. He says, in allusion to M. Gloger's memoir, that its author "has demonstrated in such a manner as to leave no doubt, that individuals of one and the same species of bird present different arrangements of colouring according to the climates which they inhabit, and that one and the same individual, amongst the birds of passage, changes during nearly the whole year the colours of its plumage, according to the different climates through which it passes."* If this be true, the question is, indeed, nearly at rest. Or it is reduced simply to an inquiry into the *amount* of influence produced by such a cause; and till some endeavour has been made to assign the limits, the fact itself may well make us sceptical respecting many of the species found in distant latitudes, which, although presenting *the closest affinity* to some in Europe, have been considered as distinct by naturalists.†

We have dwelt so long on the above subject, that we can hardly pursue the question first adverted to, as regards the limits of species, through any of the other classes. In some of these, we may observe variations not of colour only, to which, in the case of birds, such variations are generally restricted, but, to a less extent, of form and sculpture also, which may be due to causes of which as yet we understand little or nothing. Generally speaking, differences of this nature are more to be depended upon than those of mere colour; but even these cannot always be trusted as indicating a distinct species, until after examination of a large number of individuals. We shall then see how far such variations retain their constancy; and whether there may not be found specimens of an intermediate character serv-

* Ann. des Sci. Nat. 1834, tom. ii. p. 279.

† These allied species have been considered different, principally on the ground of their inhabiting such different geographical positions. But there is reason to believe that too great importance has been attached to this circumstance, and that the power of acclimation possessed by many birds is more considerable than naturalists are inclined to suppose.—See a few remarks on this subject, accompanied by a list of Birds common to various parts of the world, by Lieut.-Col. Sykes. Notices of Commun. to the Brit. Assoc. 1835, p. 69.

ing to connect the more extreme cases, and thus clearly proving all to be of one species.

From experiencing the great difficulty which attends the exact discrimination between species and varieties, especially in certain groups in which variation of character is very frequent, some have imagined that there was nothing definite or constant in species themselves, but that they passed insensibly into one another. To this opinion, although embraced by many eminent naturalists, we are strongly opposed. It may be true that hitherto no definition of a species has been brought forwards which will meet all cases; but until our means of observation shall have been more extended than they have at present, this simply proves the imperfect state of our knowledge on this subject. For let us consider how much is wanted to enable us to fix such a definition with certainty. It has been observed with reference to this point, that “we ought to see species in all their several localities from north to south; to get together all the varieties of age, form, colour, and size, in order to construct from all these modifications a table representing one well-known species, and to establish as many of these tables as there are true species of organized beings. By these means we should probably arrive at some law which would fix the limits of the species in its modifications, and serve consequently as the basis of an exact definition.”*

Having brought our remarks to this point, we shall simply, before concluding, say a few words for the direction of those, who may be entering on the branch of science we are here considering. We have more than once alluded to the immense field which Zoology brings before us. We have also noticed the great partiality shown by naturalists towards certain parts of that field in preference to others. Now what we would recommend to such as really desire to advance its progress,—is,—that they restrict their chief attention to some given department, and, when practicable, to those particular groups which have been least studied. It is utterly out of our power to become acquainted with all the existing species of nature. The longest life, added to the enjoyment of the most favourable opportunities, will not suffice for acquiring more than a very limited knowledge of the details of their history. It must, then, be by division of labour, that we try to perfect the science, so far as human researches can perfect it. We must bring our observation to bear on those parts of it which are behind the others,—we must apportion such parts amongst us according to the respective circumstances in which we are placed, and

* Lam. Hist. Nat. des An. sans Vert. (2d edit.) tom. i. p. 166.—Note by the Editors.

then, by combining afterwards the results obtained separately in this manner, we may one day be in a position to form some judgment of the true natural system. But, before selecting our particular field of observation, it is very desirable that we take, as far as possible, a general view of the whole subject. This is requisite, in order that we may get some idea of that uniformity of plan which pervades more or less the entire animal kingdom, and of that complicated chain of affinities by which its several divisions are held together. Hardly any group is circumscribed by such absolute boundaries, as to admit of being viewed correctly, except in connection with all others to which it bears any kind of relation. It may not be necessary to study these others in detail, but without some knowledge of them, we shall hardly prosecute successfully our own department. Except we have some acquaintance with other structures, we shall hardly set a right value upon those modifications of structure which come before us; and except we see something of the affinities of other animals, we shall hardly arrange according to their true relations those with which we are concerned. We shall especially overlook those approaches to the organization of contiguous groups by which is effected a passage from one to another, and those partial resemblances, or relations of analogy, which may be so often (some think always) traced between corresponding parts of two parallel series.

With respect to the particular groups, which, in the present state of the science, call most loudly for the attention of naturalists, we can only make a few remarks, supplementary to those which we have already made elsewhere.* We would observe, generally, that it is towards the bottom of the system that their exertions are most wanted. We as yet know but little of the classification of the *Invertebrate* animals, below the *Mollusca* and *Annulosa*. We may think that we can, with some appearance of truth, point out the leading divisions, or even proceed to the arrangement of the more subordinate groups; but every day is bringing to light some fresh discoveries calculated to make us mistrust those views which are founded more upon *a priori* reasoning than upon actual observation. We are so in ignorance of the real structure of certain families; we find others established upon characters of general resemblance, but offering internally such differences of organization;—that we can hardly say at present, with reference to these animals, on what systems of organs we can most rely as the basis of a natural classification, or indeed, in many instances, what organs are present. It is a question of dispute at this moment,

* Report on Zoology, p. 249.

whether those microscopic marine shells, which have been hitherto arranged with the *Cephalopodous Mollusca*, and in which class they stand as D'Orbigny's order of *Foraminifera*,—whether, after all, they do not belong to animals possessed of the very simplest organization, and such as brings them into close alliance with some of the lowest forms among the *Infusoria* of Muller.—Such at least is the opinion of M. Dujardin, whose memoir on the structure of these animals, published recently, is deserving of attention.* The same observer has been led by his researches to question the accuracy of even the brilliant discoveries of Ehrenberg with respect to the *Polygastrica*. He is inclined to think that the supposed alimentary sacs seen by that naturalist are simple vacuities formed spontaneously in the midst of a peculiar gelatinous substance † which enters into the composition of these animals, and through which the water is imbibed into these vacuities. He considers the existence of a mouth and anus as a mere illusion. Whether these opinions be right or not, they show that our knowledge of the structure of these groups, which is the only sure guide to their classification, is not yet upon a sure footing. And how many others are there to which the same remark is applicable? We might speak of the *Physaliæ*, by some classed with the *Acalepha*, by others with the *Mollusca*; ‡ or of the *Diphyæ*, those anomalous beings which we are at a loss whether to consider as simple or compound animals. We might allude to the strange discovery, recently announced by Mr Thompson, § that the genus *Comatula* is, during its young state, a species of *Pentacrinus*, to shew that even in tribes with whose structure we are better acquainted, there is yet much to be learnt as regards their history. Or, lastly, we might dwell on those extraordinary productions, which seem to baffle all our conjectures respecting their true nature;—which we know not whether to arrange with plants or animals, and which some have even regarded as forming an intermediate kingdom between the two. || Who does not see, in these instances, and in many others which might equally be brought forward, an ample and almost untrodden field, in which the acquisition of only a few facts may lead to the most important discoveries.

* See Ann. des Sci. Nat. 1835, tom. iv. p. 343.

† To this substance, which is said to be found in all the lower animals intermixed with the other elements of their structure, M. Dujardin gives the name of *Sarcode*.—See Ann. des Sci. Nat. 1835, tom. iv. p. 364.

‡ Referred to the *Mollusca* by Blainville.—See his Manuel d'Actinologie, (1834,) p. 112.

§ Edin. New Phil. Journ. No. 40, (April 1836,) p. 295.

|| See the article ARTHROIDES in the Dict. Class. d'Histoire Naturelle.

We are not without hope that the above and other obscure tribes of animals, which have of late years attracted much notice from a few eminent observers, will continue daily to be brought more to light. We would also hope, that as there are already some in our own country who have devoted themselves to these departments, there will not be wanting others to join in the inquiry. The shores of Britain offer great advantages for the study of the marine *Invertebrata*. Those, especially, who are resident on the coast, have it in their power, not only to examine these animals in a recent state, but to follow up a series of observations on the same individuals, so as trace with accuracy the successive changes through which they pass before attaining to maturity. There are, however, many groups inhabiting fresh water not less deserving the attention of the inland naturalist. The field may be here more limited, but it is still extensive. Our rivers, streams, and ditches, abound with various forms of animal life; of which, if some, belonging to the higher classes, have been well investigated, there are others, low down in the scale of organization, offering a rich harvest to the attentive naturalist. It is here that we meet with the *Planariæ*, so remarkable for their singular modes of propagation;—the *Hydræ*, which have already immortalized one observer;—the *Vorticellæ*, imitating in exact miniature the most elegant forms of vegetable life;—the *Rotatoriæ*, so called from that astonishing mechanism by which they secure their prey, and to which it would be difficult to find anything parallel in other classes. It is here that we have the opportunity of searching into the real structure of the *Polygastric Infusoria*: it is here, especially, that we may investigate, in all their details, those ambiguous productions, to which we have already alluded, as oscillating, if we may so speak, between the two great kingdoms of organized beings.

In expressing a hope that before very long, we shall have much increased our acquaintance with the lower animals,—we have been guided by the circumstance, that many groups are now attended to by naturalists, which, till recently, have been very little thought of. A spirit of inquiry has arisen up amongst us, which is no longer content with treading in the old beaten paths. If Entomology, perhaps the most attractive branch of the science under our consideration, still usurps an undue share of the attention of naturalists, it is at least studied in all its several departments, and with a degree of zeal no longer restricted to the mere desire of accumulating rare specimens. It was observed five years back,* that the *Coleopterous* insects en-

* Bull. des Sci. Nat. 1831, tome 27, p. 102.

gaged alone two-thirds of the European Entomologists. We are inclined to think that there is no longer any ground for such an assertion. Sure enough in this country, whatever may have been the case formerly, it is now otherwise, Judging from the elaborate memoirs which have appeared lately, in illustration of some of the most minute and obscure families in different orders, it would seem as if each order had its own admirers, and as if they were all in the way to a progressive development of their contents. There is further a desire manifested to study certain groups which in bygone days the Entomologist seemed to think beneath his notice; as well as to extend inquiry to all the other classes of the *Annulosa*. We need only refer in illustration of this remark, to Mr Templeton's recently published memoir on the *Thysanura* of Ireland *—to Dr Johnston's on the *Myriapoda* of Berwickshire †—and to sundry memoirs on the *Crustacea* of the British Islands, to one especially on the *Entomostraca* by Mr Baird, ‡ which we believe to be nearly the first attempt that was ever put forward in this country to investigate the native species of this very interesting group. We hail these essays as indicating a very different spirit from that which prevailed amongst us a few years back. They lead us to anticipate the day, in which there will be found no lack of labourers willing to enter upon any portion of that immense field which Zoology presents to us, and which requires to be explored in its very darkest recesses, before we can make much approximation to the exact plan upon which it has been mapped and laid out by the Great Author of Nature.

It is encouraging to those who engage in the pursuit of Natural History, to think—that, however restricted may be their situation or circumstances, however limited their opportunities and means of observation, they have it in their power to do something for the advancement of this science. They have only to direct their researches with care and accuracy, with patience, § with judgment, and with a never-failing regard for truth. Let them do this, and it matters not what department they take up. They may select any group, which they consider as best adapted to their means of study, or to which their inclination prompts them most strongly. It is, indeed, not easy to estimate the advantages that would accrue, if each individual were

* Entomol. Trans. part 2, p. 89.

† Loud. Mag. of Nat. Hist. vol. viii p. 486.

‡ Proceed. of Berwicksh. Nat. Club. p. 95.

§ We have a striking instance of patience exemplified in Ehrenberg, who, we are told, was *ten years* in conducting his experiments, before he succeeded in selecting a fit colouring matter to serve as nutriment for the *Infusoria*.—See Edinb. New Phil. Journ. 1831, p. 209.

to do nothing more than to glean such facts as offered themselves to his notice. * It is observed by the great Cuvier,—that the “natural sciences are but collections of facts, and our theories only formulæ for embracing a large number of them: hence it follows, that the smallest fact, if well-ascertained, ought to be preserved; since if new, it may serve to modify our most approved theories: the most simple observation may overthrow the most ingeniously-constructed system, and open our eyes to a long train of discoveries, which had previously been concealed from view by received formulæ.”†

With the above words,—the words of one whose judgment and great experience entitle him to be heard on such a subject, and who himself appreciated as it deserved even the smallest endeavour made to promote the advancement of the particular science we have been here considering,—we would conclude these remarks. It was in conformity with such views that he himself acted. He was not opposed to theory; ‡ but he knew enough to mistrust the theories of his own day, and to see the necessity of knowing more to establish their validity. He sought truth only; and truth is what we also should propose as the end of our researches. We may not all attain to the same brilliant reputation which he earned; or enlarge as widely as he did the bounds of science. But we may do enough to entitle us to the thanks of those who take a common interest with ourselves in these pursuits. We may throw our mite into the common stock. We shall at least reap the satisfaction which never fails to attend the study of Nature; and even if our labours should not conduct to any great or very important results, they will yet have been the means of opening to us an inexhaustible fund of rational occupation and amusement.

Swaffham Bulbeck, April 21, 1836.

* Many facts of great importance relating even to the most common animals, remain yet to be observed. Thus M. Prévost has thrown much additional light lately upon the habits of the *Cuckoo*, and shown that, notwithstanding the degree of attention which this bird had received from naturalists, there was yet much to be learnt respecting its anomalous history. See *L'Institut*, 1834, p. 418. See also an article “on the Importance of preserving Facts connected with the Natural History of Animals,”—by Mr Swainson, in the *Lond. Quart. Journa. of Science*, (New Series) vol. i. p. 83.

† *Hist. des Prog. des Sci. Nat.* tome i. p. 5.

‡ It has been well said by M. Laurillard, that “he who, in the introduction of his work on fossil bones, remarked,—*Why may not natural history one day have its Newton?* could not be the enemy of theory.”—See some remarks by that writer, in his *Eloge on Cuvier*, in reply to those who have taunted Cuvier with being a mere collector of facts.—*Edinb. New Phil. Journ.* vol. xvi. p. 360.