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THE
OBJECT AND METHOD
OF
ZOOLOGICAL NOMENCLATURE.



BY

DAVID SHARP.

~~~~~  
*"Nomina si nescis, perit et cognitio rerum."*  
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E. W. JANSON, 28 Museum Street. | WILLIAMS & NORGATE, Henrietta St.

November, 1873.

P R E F A C E

ANIMALS do not come into the world labelled with their scientific names so that if a system of these is to be established it must be by the efforts of Zoologists.

They have made the attempt, but there have recently been indications that it may possibly end in failure.

The Author of this Pamphlet offers it to Naturalists with the hope that it may contribute something to the establishment or maintenance of a system of Zoological Nomenclature, that shall be reasonable, and therefore likely to meet with general assent ; that shall be capable of application without much difficulty in the present transitional state of Zoology, and may possibly be capable of adaptation to a final system of classification,—when such shall be completed.

Thornhill, Dumfries, November, 1873.



ZOOLOGICAL NOMENCLATURE.

I.—THE OBJECT.

IT is only the educated Naturalist who can understand how vast a subject is the study of Zoology as understood in the present day.

It involves an account of the anatomy, internal and external, of each species of the animal kingdom; of the history of its life—including its embryology; of its distribution in time and space—the latter point including not only the actual limits of its geographical distribution, but an accurate account of the places of its occurrence within those limits, and whether these depend on peculiarities in the species itself, affecting its dissemination, or on external conditions. It requires an accurate discrimination of the characters which each species possesses peculiar to itself, and by which it is distinguished from other species; it requires also a complete understanding as to the points and peculiarities each species has in common with other species, for by this means only can a natural classification be arrived at. The demands of science are not even thus satisfied; for it needs an account of the differences actually existing between the individuals of each species, and it seeks to know whether these vary with the lapse of time, or are constant from generation to generation, and what are the ultimate relations of these variations to external conditions; for such questions as these are inevitably raised by the great problem of the origin of species.

Extent of
zoological re-
search.

How enormous, then, is the amount of research required to give us the knowledge needed as to even a single species.

Necessity of
names to faci-
litate study.

When we reflect on this, and at the same time consider how great is the number of species Zoology has to deal with, and that the progress of science continually suggests new questions, to which answers are required, and that these continually involve fresh or renewed observations, we see how voluminous the records of Zoology must certainly become, and how gigantic is the amount of information and observation that must and will be stored up, and how important it is that some means shall be provided by whose aid each enquirer may readily get at such portions as he requires for his particular purpose of the store already accumulated for the purposes of science. We are impressed in short with the great necessity there is for some well arranged system of Nomenclature, which shall facilitate the exchange of ideas among Zoologists, and shall enable facts to be recorded in an accessible manner. We must, it is clear to make possible our researches, give names to animals; and our names must be associated with their objects in an accurate manner, so that there shall be no mistake as to what object a particular name refers to.

Naming of
individuals im-
practicable.

Now the animal world, as it exists at present, is composed of an enormous number of individuals, and it is clear that it would be hopeless for Zoologists to attempt to carry out their researches by giving a distinct name to each individual they deal with; this course would it is true ensure accuracy, but it would soon destroy the possibility of research, because the individuals so named would soon die, and observations have to be carried on with a fresh set of individuals, requiring of course a fresh set of names, and the collating and comparison of observations would be impossible because of the enormous number of names, and the difficulty of the identification of each of these.

The naming of individuals for the purposes of science being then impossible, and not calculated to meet the requirements of Zoology, what course can be substituted for this?

Therefore
adopt naming
of species.

On looking around us we find that large numbers of individuals resemble one another extremely closely, and that these individuals are intimately associated in various ways; Zoology perceives this fact, and gives one name to the

whole of the individuals possessing this general resemblance to one another, but it only can do this with advantage when experience shews that these groups of individuals, are really separated by natural characters from other likewise circumscribed groups of individuals. Zoology takes advantage then of these groups which we find existing in nature distinctly differentiated from other groups, calls each such group a species, and gives it a name. And a Zoologist is considered to have sufficiently designated the individual he refers to, when he has called it by the name of the species. But there is more than this; the observer finds, that though the individuals forming these groups assuredly, and more or less speedily, die and disappear, they in the course of their existence have given origin to other individuals, and observes moreover that these individuals resemble their parents so that the generations succeeding one another can be spoken of under the same species name. Having ascertained these facts to be tolerably certain and definite, Zoology is clearly in a position to found a system of Nomenclature in accordance with them: and it is clear that if we can make sure that every species shall have but one name, during the whole period of its existence, and that this name shall be different from the name of every other species, we shall by means of this species name be able to facilitate greatly the future progress of Zoology.

I think these considerations justify us in concluding that the basis of Zoological Nomenclature should be a universally accepted, simple, and permanent system of species names.

Species-names
to be the basis
of Z. N.

I earnestly invite the attention of Naturalists to this point. Let each one who takes an intelligent interest in the subject of Natural History Nomenclature, commence his studies by gaining to himself a clear idea of what are the objects both immediate and ultimate aimed at thereby. For if there is not a clear idea, and a common understanding, as to what is the primary aim of a system of Nomenclature, it is hopeless to expect accord on matters of detail. And I insist on this the more strongly, because that recent writings on the subject seem to display a tendency to deal with it rather after the method of the soldier than that of the student or philosopher.

Necessity for
discussion on
this point.

II.—THE LINNÆAN SYSTEM.

First edition
of *Systema
Naturæ.*

Up to the time of Linnæus (or Linné) no recognised system of naming of animals was in general use, but in 1735 that justly celebrated man published a small work—the first edition of his *Systema Naturæ*. In this, and in many of the subsequent works of Linnæus, species were not named at all; but natural objects were classified into three kingdoms, and the animal kingdom was split by successive divisions into classes, orders, and genera, and each and everyone of the groups so formed received a distinct name.

Ninth edition
of *Systema
Naturæ.*

Successive editions of this work were published all similar in their method. Thus we find that in 1756 the ninth edition of the *Systema Naturæ* was published at Leyden under the auspices of Gronovius (an intimate associate of Linnæus.) In his earlier works Linnæus, as we have said, did not give names to the species of animals, but particularized each species by means of a generic name, and a description of the species, and in this the ninth edition of his great work, the same system was adopted; but as no new species were described, the species forming each genus were particularized by a reference to his former works; this reference being made by a word or two of description, or name being given, accompanied by a reference to page and work where full information would be found.

Tenth edition
of *Systema
Naturæ.*

In 1758 appeared the tenth edition of the *Systema Naturæ*, it was styled by Linnæus "*editio decima reformata*"; and it was in fact a totally new work, and in it a proceeding of great importance was systematically carried out. The ninth edition (the largest of all the earlier ones) consisted of 225 pages, while this, the tenth contained no less than 1384 pages, and in it, each species of every genus received a separate name. The naming of species as an essential part of Zoological Nomenclature was here for the first time (by Linnæus) put into practical operation.

It will be instructive for us to consider and comprehend the growth of this system and endeavour to obtain an idea of its rationale.

Ideas of Lin-
næus as to
nomenclature.

In the *Philosophia Botanica*, Linnæus expounds his ideas about Nomenclature, and we find them to be these:—That botany (or Zoology as the case may be) has two objects in

view: the classification and naming of plants; and that a classification being made, names are then given: "Fundamentum Botanices duplex est; Dispositio, et Denominatio . . . Dispositio est Denominationis fundamentum." (Phil. Bot. p. 97.) His practice as we have seen corresponded well with his theory; he made an analytical classification, and then instituted a system of names adapted to it. The classifications of Linnæus are dead and buried. They were eminently artificial; they could not be otherwise; they deal with the whole of natural objects, and yet with only an insignificant fraction of those objects known to the author, how could he hope to display the classification of nature? Ignorant of, and therefore undeterred by the magnitude of the task, he undertook it—completed it (as he supposed), and founded a system of Nomenclature to suit it.

But we can learn more than this by our consideration of his works. For it is manifest at once that the naming of species formed no actual part of his system of nomenclature, but was forced on him as an afterthought. To his ambitious mind genera formed the units of Nature, species were but fractions of genera, and in all his earlier works were so treated. In the course of years, however, these fractions became so numerous that Linnæus discovered the necessity of a system of names for them; and in his tenth edition of the *Systema Naturæ* he, as we have seen, gave them names. But he did not by doing so raise them to the rank of units: he still treated them as fractions. For, whereas he gave a distinct name to each genus, he did not give a distinct name to each species, but only to each species of a genus; his trivial names were allowed to recur any number of times, excepting always that any given trivial name could not recur twice in combination with the same generic name.

With Linnæus, then, genera formed the units of classification, and were made the basis of his system of nomenclature.

Generic names
with Linnæus
the basis of Z.
N.

Noting well this, let us go on a little further with our history.

Linnæus died, but his enormous energy had enlisted a number of able men in the study of Nature, and like true seekers of truth they carried on the work of examining the

classifications of their great master, and ascertaining how far his divisions were correctly made. The result that followed is instructive to us. Let us take one instance.

The multiplication and fission of genera.

In the twelfth edition of the *Systema Naturæ* Linnæus divided his order Coleoptera into 29 genera; these 29 genera comprising 891 species. His pupil, Fabricius, undertook a scrutiny of the Coleoptera, and in 1801 published at Kiel his "*Systema Eleutheratorum*" (be it noted in passing that he changed the name Coleoptera for that of Eleutherata), and he divided the order into 181 genera, comprising about 5000 species!

Thus in 35 years the genera of Coleoptera had increased sixfold, and this increase, we must observe, was not merely the result of the discovery of new objects unknown to Linnæus, but was caused in large part by the subdivision of the Linnæan genera into smaller ones, or by their entire rearrangement. It was in fact a totally fresh classification of Coleoptera, so far as their genera are concerned. The process thus instituted by Fabricius has been continued by others, and it will be sufficient for our purpose if we remark, without following its details, that the 29 genera of Linnæus, multiplied by Fabricius to 181, have now become 10,000, and in a few years will be 20,000.

How futile, then, was the hope of Linnæus that he had completed a classification, and was therefore in a position to make a permanent nomenclature!

It is clear that he did not anticipate the enormous change that would take place in his successors' ideas as to genera, otherwise he would not have based his system of nomenclature on the genus. And it is still more certain that he did not foresee that the progress of science would require that the speedy analytical system should be exchanged for, or corrected by, the slower but more exact method of synthetical classification.

Under these circumstances, it is no wonder that the Linnæan nomenclature has not been preserved in its integrity. Nevertheless, his method has been preserved at any rate to some extent, and the question arises, whether we can make use of the Linnæan system of nomenclature in the present state of scientific research, or whether it must be abandoned for a new and more precise method.

III.—THE ADAPTATION OF THE LINNÆAN SYSTEM TO THE FUTURE OF ZOOLOGY.

The reader will perhaps by this time have begun to feel alarmed, for he may very naturally suppose that I am about to propose an altogether new system of Zoological nomenclature; I am glad to be able to reassure him, and I hope to regain his confidence, by saying that I think it would not only be unwise to do this, but that it is unnecessary to attempt it. If it be found, on enquiry, that the purposes of science will be best answered at present by a system of permanent species names, apart and distinct from generic names, I think it will also be found that the Linnæan method is quite suited for giving this to us; but at the same time it will have to be admitted that our ideas as to what the Linnæan method can accomplish, as also our mode of making use of it, will each require some alteration so as to permit of their readjustment. I have already stated my opinion of the great advantage it would be to Zoology to have an unchanging Nomenclature for species; let us for a moment consider the question whether we are at present in a position to claim or obtain more than this.

I have already pointed out that Linnæus sought to make his names a means or exponents of a system of classification. Now is Zoology, we would ask, at present in such a condition that we can have our names fixed, and yet at the same time classificatory? (a horrible word, but useful for my purpose). It appears to me that it most certainly is not. Zoological classification is yet in its infancy, I think I might even say it is still in embryo. We have smiled at Linnæus for supposing he had settled classification, let us have a quiet laugh at ourselves, for, do we not always take for granted—is it not always as it were an unconscious postulate with us—that we have now got a classification, which with (perhaps) slight alterations in its details is a natural one? I am sure we all think this, and yet I believe experience will prove in the future as it has in the past, that we have got nothing of the kind.

Species-names
not to be ex-
ponents of
classification.

For example our classification is at present, mainly analytical. Now, what does an analytical classification involve? I answer a continued reiteration of the operation known as

Analytical
classification.

petitio principii. We, to a great extent, take for granted the distinction between animal and vegetable kingdom, and then proceed to divide the animal kingdom into classes : if it should prove that the division between the animal and vegetable kingdoms is not where or what we supposed it, then necessarily the limits of one or more of our classes are erroneous and will have to be altered, and with the alteration in the classes come alterations in the orders, and so on. While our classes are thus obtained in this unsatisfactory manner, so it is with our orders, and even with our more subordinate groups. Are we not always discovering some creature which sets our classification at defiance, and involves a rearrangement of our groups ?

And even suppose we have obtained the classification in a natural manner of existing species, does not the Geologist come forward and present with us a new order of insects, inevitably intermediate between two orders quite distinct at present ?

Synthetical
classification.

Suppose, again, we proceed in the more scientific course of attempting a synthetical classification, that is to say, take a species, ascertain thoroughly its characters, take another species and ascertain its characters, and so on, and then group together the species that have most points in common, and form them into a genus, will not this process be a most slow and tedious one, and will it not certainly follow that observers having different points of view will obtain different results ? Thus the Zoologist who groups his species of Coleoptera into genera, in accordance with the structure of the parts of the mouth, will arrive at quite different decisions to the observer who shall base his genera on structural characters derived from other parts. And the groups so formed may be as numerous and distinct as there are organs in the body. How many years must elapse,—how many generations of naturalists pass away—before these different attempts can all be completed and collated so as to ascertain what are really the classifications of Nature ?

It is clear, then, when Linnæus said that classification and naming were the objects of the Naturalist, and that classification being completed he should proceed to naming, that he made a grave mistake ; for in order to facilitate our

classifications,—in order to get the knowledge we require to render its accomplishment possible—we must have names, and the object of our endeavours must clearly be to name first and classify afterwards.

I think, then, I am justified in concluding that we must in the present state of science abandon the hope of accomplishing the purposes of classification and the purposes of nomenclature by one set of names. Nomenclature and the purposes of observation require fixed names—classification requires shifting ones. Let us in so far as the names of species are concerned, eliminate from our ideas all notion (or nearly all) of any classificatory power in the name: and so doing we can render them permanent, and so get a system of Nomenclature which will answer the purposes we started by requiring.

The separation of classification from species nomenclature a desideratum for Z. N.

But here I shall be met by saying that our specific names are permanent, or as permanent as they can be, and that the whole efforts of Naturalists to secure a permanent system of Nomenclature have been directed to the attempt to render specific names permanent and unchangeable. Quite so—they have been striving to seize and secure a shadow, and naturally have obtained nothing. We want, I say, permanent names for species, and naturalists have been trying to get this by rendering permanent the specific or trivial name; but that is quite a different thing. I have pointed out that under the Linnæan system of Nomenclature species are only fractions of genera, and in order to name a species we require to indicate not only the numerator but also the denominator of the fraction. Linnæus himself has said that the name of a plant consists of the generic and specific names combined, and that the specific name without the generic name is like a bell without a clapper—“*Nomen omne plantarum constabit nomine generico et specifico*” (Phil. Bot. p. 158) and “*Nomen specificum sine generico est quasi campana sine pistillo*” (Phil. Bot. p. 226). I think had he compared the name of a plant to that of a fraction, and said that to name it by the specific name alone is as impossible as to name a fraction by specifying the numerator without the denominator, his illustration would have been more forcible and accurate.

Fallacies in the use of the term “specific name.”

Indeed, I believe it is the confusion as to specific name that has rendered nugatory hitherto, the efforts of naturalists to obtain permanent names for their species. In practice the name of every species consists of the combination of two words, a generic name, and a specific or trivial name: while in their efforts for permanency of Nomenclature Zoologists have directed their efforts to the preservation of the trivial name alone, deluded by the false idea raised by the term specific name. I can insist the more strongly on this, as I myself was for long completely misled by the term "specific name," and under its influence directed my efforts for procuring permanency of names for species solely to the trivial or specific name. Let us avoid for the future entirely the use of the deceptive phrase specific name, and clearly possess ourselves of the idea, that, as an actual fact the name of a species consists of two words—a generic word, and a trivial word.

It is no wonder to me that with these sources of error unrecognized, our Nomenclature is rapidly becoming "chaotic."

Analogy between names and fractions.

Let me try and illustrate the nature of this error by an analogy. I have compared the names of species with the arithmetical system of vulgar fractions. And the two really have, so far as names are concerned, so much in common that the analogy will not I believe (as analogies generally do) mislead us. As in vulgar fractions it is the custom to specify the particular fraction referred to, by two sets of figures, a numerator and a denominator, so in our names of species we indicate a species we wish to recall by two words, a generic name, or word which corresponds to the denominator, and a trivial name, or word corresponding to the numerator of a vulgar fraction. Thus the name of a common insect was with Linnæus "*Scarabæus vernalis*." We may write it $\frac{\text{Scarabæus}}{\text{Vernalis}}$; this same species was with Latreille $\frac{\text{Geotrupes}}{\text{Vernalis}}$; with Jekel $\frac{\text{Sternotrupes}}{\text{Vernalis}}$; with Mulsant $\frac{\text{Trypocopsis}}{\text{Vernalis}}$; and yet it is our practice to consider that by keeping the numerator (I have turned the fraction upside down) unaltered, and allowing the denominator to be varied at pleasure we have kept the designation of the fraction intact. It is quite clear we have done nothing of the kind, for if Mulsant had

not expressly declared that $\frac{\text{Trypocopsis}}{\text{Vernalis}} = \frac{\text{Sternotrupes}}{\text{Vernalis}} = \frac{\text{Geotrupes}}{\text{Vernalis}} = \frac{\text{Scarabæus}}{\text{Vernalis}}$, the name itself would have given us no means of detecting the fact. The "*vernalis*" has, on the Linnæan system, no more definite value as the name of the species, than has the numerator of a fraction without the denominator, and its preservation by itself is of little or no value.

We have, in fact, entirely sacrificed the permanency of specific names to the endeavour to render them classificatory.

Or to use the Linnæan illustration, we have transferred our clapper from bell to bell, and are deluding ourselves complacently with the belief, that because we have kept the same clapper we are in possession of the same bell! We are trying to satisfy our necessity for permanent species names, by a deceptive sentiment in place of a solid fact.

I propose, then, that Zoologists should recognise that the name of a species consists of two words taken together, and with a view to secure permanent names for species should make it a practice to keep both words together unchanged.

The name of a species consists of the generic and trivial names together.

I think there is yet another consideration that will shew us the wisdom of such a proceeding. Had Linnæus pursued with his trivial names, the same plan as he did with his other groups,—that of allowing no word to be twice used—we should not have been involved in our present difficulty. But he did not do this; and the system he actually pursued, was one which was well suited to serve the purposes of an incipient or infant Nomenclature, for it greatly facilitated the recalling of an object by means of its name. J. W. Dunning has aptly remarked "in a mononymic system we should require as many separate nouns (names) as there are objects to be named; if a separate name were framed for each species, it would be impossible to recollect them all; the multiplicity of natural objects, and the weakness of human memory required, therefore, some artifice to make it possible to recollect or apply their names" (Entomologist Monthly Mag. viii. p. 274). This remark is very true, and it was no doubt one powerful reason why the Linnæan system of Nomenclature so rapidly gained the universal adhesion of naturalists. Scientific men, like other men, naturally choose the broad and easy way; but when they recognise that the easily travelled road is not getting them

Mononymic system of names for species.

to their journey's end, they must just make a cut across country and find the path, which, even if it be but painfully travelled over, will get them to their object. Now the growth of Zoology has long since destroyed the advantage derived a hundred years ago from the binomial system of Nomenclature. Linnæus, as we have seen, divided the Coleoptera into only 29 genera, and while this was the case, the use of the generic name as part of the name of the species was undoubtedly a great assistance to the memory : but the 29 genera of Linnæus are now replaced by the ten thousand of Lacordaire, and the help to the memory is consequently lost. Now, if my proposition be adopted we shall be able to regain a large part of the lost advantage of the binomial system, and yet secure the fixity and precision of a mononymic system.

Advantage to classification as well as to nomenclature.

Fixed generic names not yet desirable.

And by this course, viz., the recognising that the generic-word part of the name of a species, is not to be varied or changed, because our ideas as to genera change, we should not only secure the great desideratum of permanent names for species, but should, also, I believe, render an enormous service to classification ; for, I think, that while permanency of name has been lost because of the attempt to make names represent the classification of the moment, so, also, classification itself has greatly suffered from the attempted combination. I have already argued that classification is yet in its infancy, and that the genera of our day will not be the genera of to-morrow ; and, I believe, that naturalists cannot recognise this too speedily. For how great are the sources of fallacy arising from the false stability we have given to generic names? Under the present system of carrying on our nomenclature, we are constantly labouring under the assumption that our present genera are final or nearly so. Linnæus did this, Fabricius did this, Latreille did this, Lacordaire did this, and each and every one of us do this every day. Let us rather have classifications from every point of view, and we may then more readily discover whether our genera are really the combinations of nature, or only of books. Is it not a constant practice among Zoologists to base conclusions as to faunæ on comparisons of the number of genera common to the two countries com-

pared? and yet what exact value can we attach to the results obtained by these comparisons, when a new and more careful system of classification may at any moment reduce the number of genera, two faunæ have in common, from 100 to 10?

I think, then, we are justified in concluding that by rendering the names of species, after they have been actually established, independent of change made with a view of adapting them to classification, we are really rendering them more valuable as instruments for scientific research.

For purposes of detail we require the names of species to be permanent, and that they shall be directly associated in our minds with the objects they represent. For purposes of generalization it is important that the names of genera shall not have an air of fictitious actuality bestowed on them. Let the names of genera be then considered as quite distinct from the names of species. Let classification and nomenclature be each freed from the bondage in which they have been held by the attempt to unite them. If one of our opticians were at present to offer us an instrument with the assurance that it was both a powerful telescope and an admirable microscope, I think we should laugh at him. And yet the attainment of the objects of nomenclature and classification, by one set of names, appears to me just about as possible at present as the construction of a telescope and microscope by one set of lenses.

But we may ask, has there been no advantage gained by the changes that have taken place in the method of writing our fraction? And to this I think we must answer that the great inconvenience occasioned by the changing of ^{Scarabeus} ~~Geotrupes~~ to ^{Vernalis} ~~Vernalis~~, to ^{Sternotrupes} ~~Vernalis~~, and to ^{Trypocopris} ~~Vernalis~~ has been hitherto balanced by no actual advantage derived from the change.

And in asserting this I by no means would deny, that if we had actually attained a correct idea of what genera are, it would be an advantage that the generic part of the name of a species should be the same as the name of the genus to which it belongs: I fully admit this, but it appears to me that we should do well to delay the change till we find such a practical unanimity actually existing with regard to genera as there is and has been with regard to species. The

Names of species cannot serve two opposite purposes.

Advantage of system hitherto attempted.

generations to come may be able to reunite classification and nomenclature in the manner attempted by Linnæus, but we are not in a position ourselves to do this; let us, then, take care of ourselves; if we adopt the system most convenient to ourselves, and take care that it be so simple that it can be adapted to the classification of the future, whatever that may be, we are certainly doing better for posterity than by encumbering them with the incubus of a synonymy that will require a large part of their energy to be devoted to its comprehension.

Advantages
of system here
proposed.

By recognising, then, that the name of a species consists of the generic and trivial names taken together, and that these must each and both be retained intact as the name of the species, we should gain, I think, the following advantages:

1. Really permanent names for species, and the great facilities resulting therefrom.
2. We should put a stop to the formation of synonyms.
3. We shall free classification from the unnatural bondage in which it has been held by its union with the nomenclature of species.
4. The "laws" for the regulation of zoological nomenclature will be comparatively simple.
5. We shall possess a simple system of species' names that will probably work harmoniously with any classification that may ultimately be adopted.

While, on the other hand, should naturalists decide to continue with the present course of changing the most vitally important part of the species-name in accordance with every change of classification, they must be prepared to bear an always increasing burden of synonymy, only to find at last that the object they have had in view in so doing is not only insignificant and practically unimportant, but would have been actually attained more speedily if they had refused to bear the burden.

IV.—THE NOMENCLATURE OF THE FUTURE.

If I were to try to guess what may be the system of classification of the future, and the nomenclature adapted to it, I should probably prove to be ridiculously wrong in my estimate. Believing, as I do, that zoology is at present

as completely in its infancy as was chemistry in the days of Black and Lavoisier, and that the discovery of some generalisation similar in character to the atomic theory is possible (have we not, indeed, some of us, in the reveries suggested by "evolution," already caught vague glimpses of what might ultimately prove to be something of the sort), it is clear to me that I am not yet in a position to determine of what kind will be the classification of our successors. Nevertheless, something may be said on the point without perhaps positive disadvantage.

In "Wiegmanns Archiv für Naturgeschichte", xxxvii. (1871) pp. 24-41, will be found some details as to a system of nomenclature that shall also be a system of classification. The name of the genus is proposed to be so constructed that it shall indicate, by its mode of construction, the family, order, class, and primary division of the animal kingdom, to which the genus belongs. I have not myself seen the paper in question, but so far as I can judge from an abstract published by Von Harold in the "Coleopterologische Hefte" (ix. p. 234), it is a careful and sober attempt. The only remark I need make about it, is that it is applicable (perhaps) to a completed system of classification, not to one in process of construction. For example, supposing the limits of two families to be altered, all the names of genera in the parts shifted would have to be changed. How great then would be the confusion, if we were again to alter our views as to what are really the primary divisions of the animal kingdom. A paper published by some Owen or Huxley might at one fell swoop destroy some ten thousand or fifteen thousand names of genera, or, what is still worse, necessitate their being changed for other names. It is quite clear then that such a system must not be prematurely attempted. But, supposing some such system were adopted in the future, we can readily see how well it would work in conjunction with a system of permanent species-names, such as I advocate. For example, there is a remarkable beetle whose name is at present *Manticora Scabra*; this is according to my ideas its name as a species, and as long as it is considered a species, its name shall remain as *Manticora Scabra*, whatever alterations may take

Harting's
system.

Combination
of proposed
system with
Harting's.

place in our ideas as to the genus *Manticora*. Supposing Professor Harting's system to be in operation, the name of the genus *Manticora* would be changed to that of *Mancilerderes*. Our *Manticora Scabra* would then always be spoken of and written of by us as *Manticora Scabra*, while if we wished to treat of the group to which it belongs for any such purpose as classification, or consideration of genera apart from species, we should do it by means of the term *Mancilerderes*. The species forming the genus *Mancilerderes* would be arranged together in a catalogue, for example, under the name of *Mancilerderes*, but no species would have the term as its own name. *Mancilerderes* would be clearly understood to be a collective name: and when a naturalist wished to deal in his writings with this genus, independent of any particular species, he would use the term *Mancilerderes*, which being thoroughly understood to be a collective term, would be less likely to induce misconception than the word *Manticora*; for this word is much associated in our minds with a more individualised meaning.

I put this, however, only by way of illustration; for I believe that actually results will prove Prof. Harting's system to be quite as useless in the future as it is at present.

Harmony between species-names and a system of zoological notation.

But supposing that some system were invented that would enable us to do as the chemists do, that is to say, to designate in an unmistakable manner the particular species we wish to allude to by a combination of figures and abbreviations! This would no doubt be a great step in advance, but it would not do away with the necessity for a system of names of species to be used along with it. The fact that the chemists have discovered the correct way of designating a particular salt is $2 \text{ H O}, \text{ C}_8 \text{ H}_4 \text{ O}_{10}$ has not done away with the necessity for, and advantage from having the name Tartaric Acid also in use for it. No doubt the designation of the salt, with a view to the expression of its affinities and origin is $2 \text{ H O}, \text{ C}_8 \text{ H}_4 \text{ O}_{10}$ and the use of this formula reveals to the initiated chemist a whole host of possibilities and affinities, but to the less advanced chemist, the formula conveys no meaning, and even the most learned one still does, and probably always will use

the name Tartaric Acid for all ordinary purposes. Our present system of altering constantly the generic part of the species name, is really a vague and unconscious attempt to make the generic name serve the purposes of a system of notation. This attempt is, however, a futile one, and when this is fully recognized we may indulge the hope that an impediment is removed from the path of the future inventor of a rational system of Zoological notation.

And I think, then, we are entitled to consider that so far as it is possible for us to guess as to the classification of the future, a system of species-names made permanent on the basis of my propositions, would harmonize well therewith.

V.—THE GIVING OF NAMES.

The rest of my paper will be occupied with the consideration of some details of Zoological Nomenclature; and in this part of my subject it will be well to commence with some consideration on the making and giving of names.

Supposing that I am about to give a name to a species Making a name unknown to science, and therefore without any designation, the steps I should take are these. I must first ascertain the important fact that the species is really without name, for if it be already a species named in accordance with the system of Linnæus, it is clear I shall be introducing a grave error to science, by putting it forward as without name. Having assured myself that my species is really a new one, I have first to select a name for it, and then to make the selected name known, for the benefit of others. In selecting the name for my species I shall have several things to consider. First, I must see that the name chosen be so different from other names as to prevent the confusion arising from great similarity: I must bear in mind that the name is intended for use, both in writing and speaking, that it is intended to be used throughout the civilized world, and that it is to be created for the convenience of those using it. It is clear then, that distinctness from other names, brevity and euphony are the most important points I have to secure for my name. If I obtain these, I may congratulate myself I have done well.

No particular directions on these points can, perhaps, be offered to the describer with advantage, except that so Name not to occur twice in Zoology.

far as distinctness of name goes, he must at least secure that his name shall not be the same as any other name already in use for Zoological purposes. Some have contended that it is sufficient if the name be distinct from names already in use in the same class or order. But it appears to me this is not enough. Suppose, for example, a writer or lecturer on natural history required to sketch and contrast the embryology of a species of Vermes with one of the class Insecta, how extremely confusing it would be if it should happen that the two species most suitable in other respects for his purpose bore the same generic and trivial names!

There are, however, writers who require much more in a name than the qualities of distinctness and convenience.

Names without meaning the best.

A common demand is that the name shall be descriptive, or express some quality of the object to which it is applied. This requisition is however founded, it appears to me, on an inadequate appreciation of the object and use of names. In giving a name to a genus or species, I am not naming a simple object, but a collection of individuals possessing to some extent various qualities and in different degrees, and it is not improbable that if by the name I adopt, I recall one of these qualities, I risk giving undue prominence to a character which the progress of knowledge may shew to be subordinate, or even erroneous; so that if a descriptive word be chosen for a name it should be one of a very vague and general character. At the same time it must be admitted that I should not select an inappropriate name,—one that would tend to disconnect, rather than connect, the object with our sensorium or consciousness. Fabricius has admirably remarked “*Nomina valent uti nummi, pretio certo, determinato. Optima sunt quæ omnino nil significant. Characterem generis essentialem generico nomine indicare impossibile, nec opus est. Caractere genus distinguimus, nomine appellamus*” (Syst. Eleutheratorum preface p. viii.) He could scarcely have expressed the truth more pithily. A name is intended to be a name and not a description and if it attempt to be a description it will very possibly produce misconception. How large a portion of the difficulties of mankind is caused by good things in inappropriate places.

It has also been required by some, that names should be selected from the Greek or Latin languages; or that a word shall not be used if compounded partly from a Greek and partly from a Latin word. But is not this putting a limitation on the formation of names that is both undesirable and unjustifiable? It should be remembered that the number of distinct names required for the purposes of zoology is enormous, and that short, euphonious names are none too abundant, even when the greatest latitude is granted, as to the sources from which they may be derived. The origin of the superstitious dread that some men appear to entertain of an ill-formed (or rather what they assert to be an ill-formed) word, is to be traced to the fact that a hundred years ago a knowledge of the Greek and Latin languages was the chief test of whether a man's mind had received any training or been totally neglected, and so it has happened that an undue importance has been, and still is, attached to their study. The superstition, too, has attained a longer lease of life than it would otherwise have enjoyed, from the fact that a hundred years ago, scientific works were written exclusively in Latin, and it seemed therefore only natural that scientific names should be Latin. It will be, however, a point gained when it is recognised that the names of genera and species are intended for cosmopolitan use, and do not belong specially to any language—indeed, I might go further than this, and express my opinion that the Latin language is very ill adapted to the purposes of scientific description, and that it will be a gain when its use for them is altogether abandoned.

It is, however, very undesirable, for obvious reasons, to select words in common use for other purposes as zoological names, and to this extent a dead language is better than a living one as a source of names. All that can, I think, be said with advantage on these points is this—If a word in common use be selected as a zoological name, it is advisable it should be adapted by some slight change in termination (or other point), so that sources of confusion may be avoided. And that if a word be formed from Greek or Latin words, it is well that this should be done so as not to shock the prejudices of sensitive individuals, and offer them a temptation for “meddling and muddling.”

Classical names

Ordinary words undesirable.

The maker of a new name will also do well to define it in a clear manner, by pointing out the way its syllables are to be divided, their accentuation, and the length of the vowels used in the word; and his declaration on these points, if made at the time of publication of the name, should be held to be authoritative and final.

Choice of
generic names.

I must not altogether pass over the fact that the describer, in naming a new species, will have to give it two names, a generic word and a trivial word, and in choosing the generic-word portion of the name, he will naturally ascertain whether his species possesses the characters already assigned to some genus to which a name has been given, and, if so, he will naturally adopt that name, as the generic word in the name of his species; if he finds, on the other hand, his species to possess a combination of characters such as will not allow it to be correctly designated by an already existing name of a genus, he will have to make a new generic word for his name, and he will accompany the publication of this word with a statement as to the characters which, taken collectively, he intends this new name to represent; or if he do not think it desirable to make a new generic name, he must state distinctly that his species possesses such and such characters not reconcilable with the formula, as at present understood, of the genus whose name he gives it. Having, then, obtained the generic name of his new species, the describer proceeds to complete his work by adding to this a trivial name. Now as we have seen that it is necessary that the new name of the species should be distinct and different from already existing names, and as the new species probably has the first portion of its name (the generic word) the same as that of a number of other species, it is necessary that the second word or trivial name should be quite different from the corresponding portion of the name of any species that has the same first portion: and some research will be necessary to enable the describer to deal with this point in a satisfactory manner.

The method
of description.

When the describer has thus found a name for his new species, he will next proceed to make it known to the scientific world. In other words, he must declare what value this name is to represent, and he must do this in such a way

as to secure precision, and obtain an accurate association between the name and the particular species it is intended to represent. He must, in fact, accompany the name with as complete an account of the characters of the species it is to represent as his knowledge will enable him to draw up. And his object in making this description, he will recollect, is a different one from that of the faunist or monographer, for, whereas the latter has only to attain the provisional object of distinguishing a particular species from others equally well known to him, the giver of a new name to a species should endeavour to do his work in such a way, that the discovery of species unknown to him will not render less certain what is the particular species with which the name he gives is to be associated; and yet, at the same time, he should avoid loading his description with such details as will obscure, rather than display, the particular species intended. He should, to begin with, possess a knowledge of what are the important and really essential characters that distinguish the other species to which his new one is allied; for without this he cannot form a correct idea as to the really distinctive characters of his new species. Strengthened by this knowledge, he should gather together a fair number of individuals of his new species, and having from their study obtained an idea of the collective characters of the species, should select from the material so studied the most typical male individual he can find,—in other words, the individual that possesses most completely the collective characters of the others,—and this he should describe in detail; he should also state in what respects the female differs from the other sex, and complete his description by a statement as to the amount of variation the individuals he has studied exhibit. I advise the selection of a male individual, because as a general rule it may be stated that the males of a particular species are more completely or strikingly differentiated from the males of allied species than the corresponding females are. There are, however, some exceptions to this rule, and cases occur in which the females indicate more completely than the males the differentiation of the species; in such cases the characters of the description will of course be drawn from the female instead of the male as recommended above.

Preservation
of types.

With his description completed, the describer should label in an unmistakable manner the specimen he has made use of for the purposes of his description as being the type of his new species, and should label in a different manner the other individuals from which his more general ideas as to the species are derived. If these specimens be thus distinguished and carefully preserved, they will be a means of additional certainty to future students, who may by their examination obtain additional evidence as to the views of the describer. The description made must then receive the stamp of completeness, by publication. With the publication of the description and name, the describer's work has ended; from that moment he has no more power over name or description than has any other man. His work is a gift to the commonwealth of science.

VI.—ESTABLISHED NAMES.

There are several points demanding consideration in connection with the use and treatment of names already established: and first let us take a brief glance at some of the features of names of genera.

Though I have undertaken to consider the question of Zoological Nomenclature without restriction, yet I have, I hope, already made it evident that in my opinion the names of genera cannot be dealt with at present in a way to afford us any hopes of permanent result. Nevertheless, it is clear that we must have names for genera, for on the Linnæan system, even in the limited sense in which I think we can advantageously accept it, a generic name forms an essential part of the name of the species.

Reality of
genera.

In the early part of this paper I insisted that we are in a position to give permanent names to species, because we have arrived at a practical if not a theoretical understanding as to the limits of species as they exist around us. But with genera the case is very different, our ideas with regard to them are not only unscientific, but entirely undefinable; we have not arrived at present at any conclusions with regard to genera that would warrant us in trying to found a system of names for them. There are some naturalists, even, who doubt the existence of generic characters as distinct from

specific characters. Nevertheless, evolution supplies us with some suggestions, that render it possible if not probable that the study of genera may afford us practical results, and allow us to ascertain that there really are such things as genera actually existing around us, and to define their limits. For if species are the descendants of other species having different characters from themselves, if, moreover, of the species of a past epoch a number disappeared without leaving descendants, so that the species actually existing around us are the representatives not of all the species that formerly existed but only of a portion of them, it is clear then that we may expect to find that there are existing around us groups of species, well defined at present from other groups of species; in other words, that there are really genera. But even without this there would be still another justification for the belief in genera, in the fact that the order of evolution has been, on the whole, the change from the general to the special; for this would warrant us in the belief that our species possess, besides their recently specialised characters, others of an older date, and therefore of a more general character. If this latter, however, were the only reason for genera, we should expect genera to be distinguished from one another by only slight characters, and composed of few species. Indeed, it is perhaps in the fact that there are two distinct sources of genera, that is to be found the explanation of the puzzle that in some groups genera are well defined and yet may comprise a large number of species, while in other cases, in order to find generic characters at all, we are obliged to descend to points of comparative detail.

I mention these considerations, which are certainly of a very vague and probably of a very illusory character, merely to give force to my statement that our knowledge of genera is yet rudimentary, and that the giving of names to them at present is a question of secondary importance. I repeat that what we want to enable us to understand genera is an accurate knowledge of existing species, and a minute and accurate understanding of the characters by which they are distinguished from another; when these are known and can be eliminated from the enquiry we can then hope to comprehend genera.

The knowledge of genera can only follow that of species.

Preservation
of Linnæan
generic names.

The ideas of Linnæus with regard to genera were necessarily of a most rude and imperfect character, and naturally the groups he called genera differed greatly from ours. Under these circumstances, what has been the course pursued with regard to names of genera? When the groups of Linnæus were altered, should they have received fresh names, or should the old names have been retained and applied to the new groups? In practice opposite answers were given to this question;* some of the names Linnæus gave to genera have now entirely disappeared from use, while others of his names are still used; but in such a case it is always as the name of a group very different in extent and value from that to which Linnæus applied the name.

I do not myself intend to give any complete consideration to this question. For it is part of my argument that if we secure permanent names for species, it will be no disadvantage to science to be without fixed names for genera at present. But, nevertheless, I will call attention to one practical point in connection with this question. It is this. In catalogues or lists of species where names of genera are given it is the custom to place after the name of the genus the name of the author who first founded a genus of the name; but it appears to me that no really useful object is attained by this practice, and I would suggest that the author's name placed after the name of a genus should be that of an author who describes the genus in the sense in which it is actually used. For it should be remembered that we do not use the author's name for the purpose of commemorating or rewarding him, but with the object of securing precision and conveying a distinct meaning.

Emendations
of published
names.

It has been unfortunately the practice with many writers not to adopt the name given by the original namer of a species or genus, but to use in its place another word more or less closely resembling the original, and they make this change on the ground that the original name was incorrectly

* I recommend any one who may wish to get a conviction of the want of precision that has prevailed about genera and generic names, and therefore of the unsuitableness of these to serve as the basis of a permanent nomenclature, to refer to the *Transactions of the Entomological Society of London* for the year 1870: they will find there at p. 41, an interesting paper by Mr. G. R. Crotch entitled "The Genera of Coleoptera studied chronologically."

formed, and therefore requires emendation. Now we have seen what are the really essential points for names, and that the only demand with regard to names absolutely involved by the principle of the Linnæan system of nomenclature is, that a name shall be distinct and different from other names. But "emenders" (I am almost tempted to say cobblers) go far beyond this: they insist that the names used in Z. N. shall be framed in accordance with the laws of the Greek or Latin language (or rather in accordance with their private ideas as to these laws.) But even suppose this to be granted, and that it is desirable that names should be "correctly" formed (on this point see p. 19), still it appears to me that in altering names after they have been already established, these emenders commit the serious error of confounding the functions of critic and doer. Admirers of correctly-formed words have certainly the right, and perhaps do well in using it, to point out what they consider to be the correct way of forming names, and obtain, if they can do it by these means, correctly-formed names; but when they refuse to content themselves with this, and alter names already established, they play the part of the destroyer of permanent nomenclature rather than that of preserver. There are, however, I believe, "emenders" who would justify their practices by arguing that it is with the view of obtaining permanent names that they alter established names, for that an incorrectly formed name is always a temptation to some one to interfere with it. This argument may, it appears to me, be dealt with by a parallel. Supposing a brigand cuts a man's throat, will the officers of justice hold him to have done no harm, because he argues that if he had not done the act, it is very likely it would have been committed before long by another band?

I think, also, we should ask emenders what guarantee they offer us that their alterations will give us a permanent name? After they have done their work will come others who will also be glad to give a practical lesson in classics to the world; and it is at any rate possible, if not actually probable, that with the lapse of time different ideas may prevail as to what is the most correct way of forming a generic name, and turning a Greek word into Latin or English to those held at present.

It appears to me that interfering with and altering established names, is pursuing a course the advantage of which is at the best slight and problematical, while the disadvantage is certain and immediate. I think, in short, that the alteration of established zoological names for classical reasons is both foolish and undesirable,—foolish, for it is to lose a primary object in the pursuit of a secondary one,—undesirable, because it places the names of science at the mercy of any lad fresh from college.

Integrity of
name.

There are others who would say that, though these emendations are theoretically objectionable, yet they are practically unimportant, and that so long as the integrity of the name is not destroyed, they may be allowed to pass. But this is the argument of weakness, not of strength. The integrity of the name is the name itself,—the name, the whole name, and nothing but the name. If it be not, we may at any rate require that it shall be settled where integrity ends and fractions commence.

How objectionable is the practice of emendation that has got into vogue in this insidious manner we shall see by a slight consideration of a common instance. The older describers, in transforming a Greek word into a zoological name, were many of them in the habit of taking no account of the aspirate, which in Greek writing is indicated by a sign over another letter, while in Latin and our modern European languages it is indicated by a letter of its own. Classical purists assume that the older writers ought to have introduced the aspirate into their words (though even on this point much may be said on both sides of the question), and not content with this assumption, actually place the H where they consider it ought to have been, and they do this even where it involves the alteration of the first letter of the word. Thereby they practically substitute a new name for the old one, for it is clear that, for purposes of alphabetical registration, the name must be indexed, both with and without the H ; and we are encumbered with two words for one and the same name.

Errors of the
press.

Others, again, would limit their interference with names to the correction of errors of the press. All that need be said on this point is, it appears to me, this : If an original

describer, or namer, be so careless as to put forth for a name a word that can only be written and not spoken, others must necessarily make such alteration in the word as will adapt it for both purposes. Suppose, as an instance, that a new species were described as *Canis vittns*, it is clear that some alteration must be made—but that alteration should be as slight as will render the word a name—I should myself in such a case use *vittus*, and adopt the name as *Canis vittus*. Suppose, however, that he had published a *Canis vivalis*, or *rivalis*, I should adopt his name without any change, even though there might be strong evidence to make one believe that he intended to write *nivalis*; and I should deal just the same with generic names as the above examples do with trivial ones.

But many will ask me, what will you do in the case of the notorious *Amphionycha Knownothing**? I answer, adopt it. As a name, *Knownothing* is surely better than "*Wladimirskyi*," or "*Zakharshevskyi*," or "*Slovtzovi*," or "*Stscheglovi*," or "*Stschukini*," all of which have been proposed as trivial names for species of *Carabus*, and adopted without difficulty.

I think it is, then, quite clear that interference with names once given is unadvisable; and though it is probable that when such alterations are made some will adopt them, yet I do not believe they will ever obtain universal consent, and will probably in all cases ultimately be put aside altogether. I think therefore we are entitled to ask that they shall not be made at all.

VII.—THE "LAW OF PRIORITY."

I will now call the reader's attention to another point in connection with nomenclature; it is one which, though really of a subordinate character, has received an undue share of attention.

Let me introduce it by saying that one of the postulates

* It may be worth while to note here that *Knownothing* and *Copperhead* have been proposed as trivial names by American authors. No doubt it was intended by them to allude to the political parties known by these names in the United States. Now, it would seem to me quite as unobjectionable to commemorate, by means of a trivial name, a party or group of men as a single man.

of Z. N. is that a species shall have but one name. It is, I think, unnecessary to enlarge on this point ; for I think the postulate will meet with universal assent.

In practice, however, it constantly happens that a cataloguer, monographer, or collector, finds that one and the same species possesses two or more names. In such a case, which name is the correct one? which shall he adopt? The answer (in the shape of a general rule) is very easily given to this question. For, it being admitted that no species can have two names, it is clear that we have only to ascertain the dates at which the names were given, and then decide at once that the earliest given name is the name of the species ; for if not, we admit that there is a period at which it is correct that one species shall have two names ; the inference that the oldest name is the correct name, is a corollary that follows necessarily on the assumption of the postulate that no species can have two names. The "law of priority" is therefore something much more stable than a law or convention—it is a reasonable and logical inference from an universally admitted postulate.

Though the principle is in such cases clear enough, the practical solution of the question is a much more complex and difficult affair.

In practice, this question (what is the real name of a species?) requires not only evidence as to the date at which the names in question were given, but also evidence that these names were given to certain species ; and it appears to me that, where the evidence on these two points is satisfactory, there is no other conclusion possible than that the earliest name is the correct name of the species. This simple principle has been hitherto the admitted guide in disputed cases. But it is necessary to notice that suggestions have lately been made that it would be an advantage to supplement the rule of priority by some other "simple principle"—such as, that "universal employment*" of a name shall be sufficient to establish its claim to be considered the correct name of a species, even when there is an older name. It appears to me, however, that if Naturalists

"Universal employment."

* J. O. Westwood. Proc. Ent. Soc., 1872, p. lxix.

have not been able to agree as to what is the correct name of a species when they have had but one law to apply, still less will they be likely to agree when they have two "laws" to litigate about. Moreover, I find it difficult to understand how it is intended to prove "universal employment." Indeed, does not the very fact that there are two names in question in any particular case, shew us that the test of "universal" employment cannot be applied in that case?

It has also been suggested "that the maxim, 'Communis error facit jus,' should govern scientific nomenclature." * "Communis error" fallacy.

As regards this suggestion, it is hardly necessary to remark that, if a common error does in fact make law or right, it would rather suggest the necessity of importing the scientific element into legislation, rather than the governing scientific nomenclature by a legal maxim of such a character. I have no doubt, however, that the meaning Mr Lewis has attached to the word "jus" in the above maxim has misled him; it is, I should suppose, limited to expressing a "right," or standard, by which past acts shall be judged, not future ones determined.

Accepting, however, to the full the law of priority, we often find in practice a difficulty in deciding even so simple a question as the exact date at which a name was given. In the remarks made on the giving of names, it has been pointed out that no name can be considered as given till it has been published and so made common property. It is important, therefore, that the dates of publication of scientific works should be accurately given in the works themselves; self-evident as this is, it is still necessary to insist on it, for a work containing a large number of descriptions of new species has recently been published, bearing as the date of its publication a period two years before that at which it actually appeared. Publication and date.

A man of science cannot be too precise in his statement of facts, even when they are only indirectly connected with science; and it should be understood that it is the duty of

* W. A. Lewis, *Entomologists' Monthly Mag.*, VIII., p. 1. Mr Lewis has also published a pamphlet on the subject, which not a little reminds one of the advocate who wrote on the back of a brief submitted to him—"No case: abuse the plaintiff's attorney."

scientific men to see that their works appear with the exact date of publication attached. I have lately received a letter from a describer, in which he argues that in the cases of descriptions published by scientific societies, names should take their dates from the moment at which the paper was presented to the society, the basis of my correspondent's argument being, that it is fair to the author that this should be the case. But this is not a reason for departing from the simple principle that a name can only be considered to have commenced its existence in the scientific world from the moment of its publication. It is clear that reading a memoir cannot be considered as publication, because both the author and the society have it in their power to alter name or description after the reading of the memoir. And it cannot be too strongly urged on scientific societies, that every portion of their proceedings shall bear the actual date of its publication; and it should be the duty of the secretary (or editor) to see that this is properly carried out.

Evidence
other than
date.

When we have determined the date of a description we have still to ascertain the very important point as to whether it applies to the species in question. This is a very much more difficult problem than the ascertaining of a date, and it can only be properly dealt with by a complete consideration of the evidence in each particular case: and this evidence is of three kinds. 1st, The description itself and the complementary evidence accompanying it (such as locality of occurrence, statement of habit, or peculiarity of modes of life, &c). 2nd, Tradition. And 3rd, The existence of the individuals from which the description was drawn up, or of other individuals alleged to be authentically named.

The evidence
of the descrip-
tion.

The evidence under the first of these heads is the most important, and if it be of itself satisfactory, no other evidence is necessary: if the description accord satisfactorily with the characters of a particular species, and if it be ample and well drawn up, and especially if it be accompanied with a well-executed figure, the question is decisively settled. But if the description be so deficient in any or all of these points as to leave doubt in the opinion of a skilled or expert enquirer into these matters, the evidence should be sought under the other heads. And if it be found that scientific

Of tradition.

treatises dealing with the matter have declared or cited the questioned description as belonging to some ascertained species, and if the number and importance of the treatises in which this is declared be considerable, then also this evidence is important. As for the evidence of types, it is clear that this must not be exclusively or even strongly relied on; for the specimens, unless accompanied by an actual declaration that they are the individuals described (and even then they may have been interfered with either by fraud or carelessness), may have been labelled with the name long subsequently to the description of the species, so that their value would rest solely on the accuracy of memory of the describer. Indeed, I may say that in my opinion the sources of fallacy as regards types are so numerous, that I think very little authority can be attached to them, except in such cases as they are accompanied by a declaration of the describer to the effect that they are types, and even then their value is clearly nil if they contradict the description. It is evident if a description and its supposed types are not in accordance with one another, we must either consider the description as merely imaginary, and therefore without claim for consideration, or decide that the types are valueless, and consider the case without them.

Of types.

And here I cannot but allude to the careless (I had almost written unprincipled) way in which many zoologists use the term type. It has, in my knowledge, been applied to specimens which have never been seen by the original describer; some writers, indeed, appear to consider nearly every specimen as a type, if it has been determined by some one other than themselves; and on the authority of these pseudo types all kinds of identifications are made. Naturalists will, in my opinion, do well to receive with suspicion any announcement made on such vague authorities, as "ex typ.," and insist strongly on the prime necessity of basing the identification of species on the published descriptions thereof.

What constitutes a type?

The question, then, as to the association of a particular name with a species is, we see, a question as to the evidence in each case, and this evidence must be collected in a careful manner and considered by skilled men. This will un-

doubtedly be a slow process, but it will be a sure one; and I may remind the impatient ones who proclaim that we must have a way of settling such things right off, that they are, if they have any just voice in this matter, men of science as well as collectors, and as such they will readily appreciate the association of the words "ohne hast" with "ohne rast." Let them recollect that in these disputed points we wish to obtain a decision that shall be absolute, and not one that may be reversed on the first appeal. To enable us to do this we must in each case carefully collect the evidence and consider it under the light of reasonable and admitted principles.

I will bring these remarks on the rule of priority to a conclusion by saying that it appears to me to be correct in principle, easy in application, and the only rule likely to obtain general assent. That to add to it a series of supplementary laws will, it appears to me, complicate and not facilitate our operations; and that these points appear to me, after very full consideration, so clear, that I have no doubt that to abandon the rule of priority is to abandon the only foundation possible for a fixed nomenclature.

Limit of
priority.

There is, however, one limit to the rule of priority; it is that a name, to be recognised by zoologists, must be given in accordance with the method of the system of which it is to form a part: that method we have seen to be that the names of species are composed by generic and trivial name together, and consequently all names, to meet with support, must be given in works professedly or practically recognising this principle. We have seen that the system was introduced between the years 1750 and 1760, and we consequently have here a fixed line beyond which we cannot go. This affords us with another reason why the priority rule should meet with support, for it is very clear that if we persistently adopt the oldest name as the correct name, we must very soon bring all the earlier-proposed names into use, and we have then in this limit a barrier against change, the power of which it would be impossible to equal under any other system.

VIII.—TRIVIAL NAMES.

I have, I hope, expressed sufficiently plainly my opinion

that the names of species consist of the generic and trivial name combined; and that the original combination must remain intact if permanency of name of species is to be anything more than a theoretical hobby. If naturalists, however, decide not to adopt this suggestion in practice, and to go on as they have done hitherto (holding in their theory that the generic and trivial names form the name of the species, but in their practice that the trivial name is the name of the species), they will find very numerous complications as to trivial names arising from the fact that, with changes of our views as to genera, we shift the trivial names to other generic names. Now, it often results from this that the trivial names require to be changed when this is done, because Linnæus had not foreseen that this would be done, and his system was not framed with a view to admit it. The points arising under these changes are of so subtle a character that it is scarcely to be hoped that practical unanimity will be obtained about them. I shall only here allude to them very briefly, but may take this opportunity of stating my opinion that no result of practical importance is obtained by the endeavour to make trivial names, considered independently of generic names, permanent, and that therefore these points are really unimportant. It is, indeed, straining at a gnat and swallowing a camel, to allow the generic portion of the name of a species to be subject to unlimited change, and yet to strive with all our might to render the trivial name permanent.

Some naturalists, seeing the uncertainty of our ideas as to genera, have endeavoured to make trivial names permanent by laying down in place of the rule of Linnæus that no trivial name can occur twice in combination with the same generic name, the rule that no trivial name can occur twice in the same family or sub-family. But they have overlooked the important fact, that if the limits of genera are ill defined, the limits of the larger groups are still more unexamined, and therefore more uncertain, and that if these larger groups have to be remodelled, more alterations in trivial names would then become necessary if their practice and principle were admitted. To make it a law that no trivial name shall occur twice in the same sub-family seems to me therefore

Recurrence
of trivial name

unwise, not only because of the above special reason, but also of the more general objection drawn from the argument that the introduction of subsidiary laws is undesirable, and tends to complicate, not facilitate, agreement as to names. At the same time, there can be no objection whatever to this point being strongly pressed on original describers as one they should keep in mind. But to go beyond that, and make alterations in already established trivial names, is inadmissible, and clearly indicates that those doing this have but confused notions as to the distinction between advising and doing. I shall make my meaning perhaps clearer by saying that so long as the generic part of the name is different in two species no confusion can be caused by identity in their trivial names, and that the alteration of these should therefore be left till such time as a change in the generic name renders them actually necessary.

Erroneous
citations.

Older trivial names have been rejected for more recent ones, also, in another case, such as the following: An author of a fauna or monograph describes at length some species under a generic and trivial name which have been previously used by some older author, and this he does under the idea that he is describing the same species as the older author applied the name to. In this, however, he is mistaken, and it happens that the species is in reality one hitherto undescribed. In such a case the monographer has, without knowing it, played the part of an original describer. Now it is clear if naturalists keep the name of a species intact, preserving both the original generic and trivial names as the specific name, that their course is clear in the case above pointed out. The monographer has, it is true, described a species for the first time, but he has not given it a name that can be used by others, because a previous author has given the same name to a different species. The point, however, involves in an inextricable difficulty those who hold that the trivial name is the name of the species, and devote their efforts for the stability of nomenclature to the maintaining of the oldest trivial name. For in such cases as the one under consideration it often happens that the species so named proves to be a different genus, as well as a different species from that of the earlier describer,

and as they therefore change the generic name, it happens that the trivial name no longer collides with that of the earlier describer, for it is now placed in association with a different generic name; the question then arises, are we to adopt the trivial name in this case as the proper name of the species? This question, be it first noted, can only agitate those who maintain in practice the two theses—1st, that the oldest name is the correct name of the species, and 2nd, that the name of the species means the trivial name; and the answer they are bound to give is therefore clear;—they are bound to accept the oldest trivial name. The actual practice on this point has, however, been various. In some of such cases the oldest trivial name has been used, and in others rejected, the reason assigned for the rejection being that the description was accompanied by an erroneous citation (it would be more correct to say a citation made under an erroneous impression). But it is clear that the citation is not a part of the description, and if the description be in itself unmistakable, the erroneous citation cannot invalidate either this or the date. Indeed, if the evidence independent of the citation be sufficient, the erroneous citation cannot affect the claim of the name, any more than would a slight imperfection in the description itself. The citation in such a case is only part of the evidence, and cannot by itself decide the question. There are some cases, however, in which a description is not only referred to but actually reproduced, and though in such cases the evidence of types or tradition may lead us to believe that the author who adopted the description had really a different species in view to what the first describer had, I do not think we can adopt the name; unless, indeed, he made some change in the description which would support the evidence afforded by the types, or tradition.

There is another point to which I must briefly allude, viz., the custom of adding, after the trivial name of a species, the name of an author. This is an incubus that can only be tolerated by its being proved to be absolutely necessary. It is an outgrowth or excrescence which pretty clearly indicates the inefficiency of the present method; there being at present no real accord as to names, it has been necessary

Appending
author's name
to trivial name

not only to mention the name but to give some means of identifying the name. It is an attempt to make the trivial name valid or distinct from other trivial names by means of an author's name, instead of by the generic name as Linnæus intended. When, for instance, a catalogue was made, and a generic name placed at the head of a series of trivial names, many, if not all, of which were divorced from the generic names with which they were originally associated, it became of value to add to the trivial name some special assistance for its identification. Hence the practice in question. Should naturalists, however, decide that the argument of this paper is valid, viz., that the generic and specific name in combination form the real name of the species, and that it is the interest of science they should be kept together as such—then the reason for the practice having ceased to exist, the practice itself no doubt will speedily die out.

IX.—THE NOMENCLATURE OF VARIETIES.

My notice of Zoological Nomenclature would perhaps be incomplete if it made no allusion to the naming or designation of varieties—*i.e.*, groups of individuals occupying a position intermediate between the individual and the species.

It is to be hoped that varieties will soon receive more attention than they have hitherto done; for the mode of differentiation of species in the past, can only be really scientifically studied by the light afforded us by a knowledge of the process of differentiation as going on at present. This will involve an accurate knowledge of the amount of variation actually exhibited by one or more species (selected as specially fitted for the purposes of this observation), and then observations as to whether this remains stationary from generation to generation or whether it increases or diminishes, or whether (as the writer believes) it increases in some species and diminishes in others. When this study is systematically entered on, the means of specifying any variety that may be alluded to becomes of considerable importance. The question, however, is surrounded with difficulty. The basis on which zoologists ground their claim for names for species is the fact, that observation teaches the mthat species, as existing at present, are undoubtedly differentiated from one another. But with varieties exactly the opposite is the case.

The only definite distinction that can be practically applied as to whether a given group be a species or variety is the test of actually existing differentiation. When we find (or consider we are justified in inferring from our limited observations) this differentiation to be complete, we call the groups of individuals so isolated a species, when we find the differentiation incomplete we call it a variety. The term variety, it is now well understood by zoologists, is a very vague one. It includes both groups of individuals of large extent and mere individual aberrations. Taking these points into consideration, I come to the conclusion that the giving of distinct and definite names to varieties is likely to induce fallacy in our minds, and that there is no sufficient argument by which it can be justified. It appears to me that the purposes of science will be best served by there being no names for varieties, but that it shall be left to each recorder to indicate what variety he alludes to by special means he may devise for his particular purpose. To facilitate the doing this every author who specializes varieties should do it by means of a letter or figure, or combination of the two. If a recorder then wish to give some important fact as to a variety, he can do it by mentioning (as always) the name of the species and adding "var. A. 2 Mulsant": or, if the variation be geographically determined, he can indicate his variety by the mention of locality. These methods it may be observed are vague, but I believe them to be advisable for that very reason.

X.—SUMMARY.

I shall conclude by summing up the contents of this pamphlet in a series of propositions, on which I ask the criticism of naturalists, and I shall append an extract from a Catalogue of British Coleoptera to serve as a practical illustration.

Prop. 1.—That a system of Nomenclature, which should commend itself to naturalists so as to meet with unanimous employment, would be a great means of facilitating and advancing the study of zoology.

Prop. 2.—That any system hoping to command the assent of naturalists must be able to furnish a series of names (for animals) that shall be both permanent and precise.

- Prop. 3.—That we are at present in a position justifying us in the attempt to establish a series of permanent names for species that may serve as the basis for a more extensive system.
- Prop. 4.—That our knowledge (or accord) as to genera—*i.e.*, groups of greater complexity than species—is at present so far from settled and final that their names cannot as yet form an essential portion of a system of permanent names for species.
- Prop. 5.—That, nevertheless, the system actually in use for naming species adopts the generic name as its basis, and that this system having gained the approbation of naturalists, it is unwise to attempt to supplant it.
- Prop. 6.—Therefore, recognising that the generic name is used for two distinct purposes, that we treat it in two distinct manners: or, in other words, recognise that the generic word, as forming part of the species-name, is a distinct thing from the generic name used, unaccompanied by any trivial name, as the appellation of a group of species.
- Prop. 7.—That the correct name of a species is the earliest two words (generic or mnemonic word and trivial word) that were first given in combination to it, as a specific name.
- Prop. 8.—That this earliest combination shall always (so far as the efforts of naturalists can secure this) remain and be considered the name of the species; and if it be absolutely necessary that for use in any particular language it must undergo some slight alteration, that such alteration shall be limited to its use in that one language, and not affect the preservation of the name in its unaltered form by the cosmopolitan body of naturalists.

With regard to the specimen of a catalogue of British Coleoptera given below, I shall not attempt to answer by anticipation the criticisms which I trust it may evoke; but will content myself with remarking, by way of explanation, that the first column gives the name of the species as it was originally established, and which I consider should be preserved intact and unchanged, so as to form a means of con-

necting as one whole the literature of zoology from generation to generation : while the second column represents a portion of the ideas at present put forward as to the classification, or grouping into genera, of these species: and while asking the naturalist to preserve the first column as sacred, we can, in compensation, place this second column completely at his mercy: the student may add to it, subtract from it, or alter it, just as he may consider desirable in the interest of science. Should a cataloguer think it necessary to indicate more than the arrangement of the species into genera, he can easily add this; and should he desire to give more than one system of generic arrangement, this also can easily be accomplished. On the other hand, should the object be to offer only a condensed pocket list for use by mere collectors, the first column may be given alone. To facilitate reference a full alphabetical index should accompany the systematic list.

(Column 1.) Species.

(Column 2.) Genera.

1. <i>Scarabæus mobilicornis</i> Fab.	}	Odontæus Er.
2. <i>Scarabæus typhæus</i> Lin.		Minotaurus Jek.
3. <i>Scarabæus stercorarius</i> Lin.	}	Geotrupes Jek.
4. <i>Scarabæus spiniger</i> Marsh.		
5. <i>Scarabæus foveatus</i> Marsh.		
6. <i>Scarabæus mutaton</i> Marsh.		
7. <i>Scarabæus sylvaticus</i> Panz.	}	Anoplotrupes Jek.
8. <i>Scarabæus vernalis</i> Lin.		}
9. <i>Scarabæus pyrenæus</i> Charp.		
10. <i>Scarabæus hispidus</i> Pont.	}	Trox Harold.
11. <i>Scarabæus sabulosus</i> Lin.		
12. <i>Silpha scabra</i> Lin.		

