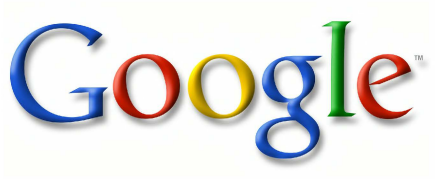

A DISCUSSION

OF THE

LAW OF PRIORITY IN ENTOMOLOGICAL NOMENCLATURE;

WITH STRICTURES ON ITS MODERN APPLICATION;

AND

A PROPOSAL FOR THE REJECTION OF ALL DISUSED NAMES.

BY

W. ARNOLD LEWIS,
F.L.S., M. Entom. Soc. Lond.,
BARRISTER-AT-LAW.

ALSO CONTAINING

A Paper, by the same, read before the British Association (Section D) on August 7, 1871;

And a Second, by the same, intended as a Contribution to the Discussion in the 'Entomologist's Monthly Magazine.'

London:
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PRICE ONE SHILLING.

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ADVERTISEMENT.

The paper "A Proposal for a Modification of the strict Law of Priority in Zoological Nomenclature in certain cases," was read at Edinburgh in Section D of the British Association, on August 7, 1871. Mr. Stainton, F.R.S., kindly took charge of the paper in the writer's absence.

The short paper "Synonymic Lists and Certainty in Nomenclature" was sent to one of the editors of the 'Entomologists' Monthly Magazine' in September, 1871, and after two months' consideration returned by him as rejected.

All the parallel passages copied in the foot-notes were discovered by the writer after the two papers had left his hands.

A DISCUSSION

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LAW OF PRIORITY IN ENTOMOLOGICAL NOMENCLATURE;

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Confusion in Nomenclature has increased, IS INCREASING, and ought to be diminished.

LINNÉ invented Scientific Nomenclature; but did not invent or acknowledge any law of priority. On the contrary, Linné changed specific names with freedom, and gave, in the later editions of the 'Systema Naturæ,' new names to a great many species which had been named before. It is desirable at the outset to disabuse our minds of the notion that a law of priority comes to us invested with traditions to make it incontrovertible or sacred from modification. It has no such sanctions.* The law was the invention of a time when,

* A set of rules for entomological nomenclature was first published by Fabricius, in his Philosophia Entomologica (chapter vii.). His clauses on the subject of uniformity in specific names are very short: the following apply to the present discussion.

§ 38. "Multiplied trivial names for the same species produce confusion, and therefore are by all means to be avoided. The trivial names of my predecessors, especially Linnæus, I have preserved as sacred."

§ 45. "Trivial names are never to be changed without the most urgent necessity. Every change of names becomes a cause of confusion; they are therefore by all means to be preserved.

"It would be supererogatory to substitute for a vague name another vague name, as for the most part they are. Therefore *let them never be changed, unless the food or habit or an essential character can be expressed.*" This amounts to a recommendation to change the names where they can be thus improved; and to that extent supports the remarks of Mr. T. H. Briggs, 8 Ent. Mo. Mag. 94.

The Philosophia Entomologica was published in 1778. Linnæus, in the Philosophia Botanica (1751), has no rule whatever on the subject, though he lays down a great many rules concerning names (chapter vii.). In 1767 he had written: "For all insects trivial names taken, where they can be, from the plants from which they draw life (and which they devour and decrease), are of the first rank and to be placed before all the rest" (reliquis omnibus anteponenda); Syst. Nat. ed. xii. p. 767, *note*.

Fabricius was "in relation to names created by others not at all conscientious" (Berlin. Ent. Zeitsch. vol. 2, app. p. xix.), and changed them with the greatest freedom.

as compared with the present, the study of Natural History had few votaries. Such as it is with us, that law was agreed on by leaders in zoological science; but leaders of science were at the time quite disconnected from the persons fond of science who were not leaders. A few distinguished persons coming even to an ill-advised agreement, without forecasting the results of their legislation, seem to have been then able to bind all their fellow-students. We are safe in saying that in Natural History matters "the connection between the governing and governed" was in 1842 not so close as at the present time; and laws were then made by others for the persons who now-a-days are fully well able to take part in making them for themselves.

It is probably a startling view of the case, to the minds of some self-complacent critics, that this law of priority was itself agreed on in the obscure days of natural science, when those who knew anything were few in number, and when their authority was in consequence nearly absolute. Such is undoubtedly the fact. It is interesting to conjecture what number of individual minds took part in fixing the law of priority of 1842. You will scarcely find a word of discussion on the subject in any journal or magazine of that period; and you will only waste time if you search for any indications of what was the general opinion of entomologists on the matter. The code of 1842 was made without correspondence with a class of practical students, conversant with the requirements of a scientific nomenclature. There was then no such class in existence; entomologists were untrained and ignorant. In the words of Mr. Stainton, "few can realise to themselves the extreme seclusion in which the entomologists of this country lived twenty or thirty years ago; except a few of the leaders, literally no one knew anything; the reader of Stephens swore by Stephens—the reader of Curtis swore by Curtis."* It was while entomological science was in this obscured condition that our law of priority was adopted. It would be strange indeed if (good reason arising) the large class of competent entomologists of the present day were to abstain from questioning a rule thus imposed, for the conditions were not those in which a wise determination could be looked for.

Moreover, the originators of the law of priority could not see the question as we see it in all the light of another whole generation's experience. Those respected gentlemen could not realise in 1842 the condition of entomological nomenclature in 1872. They were perhaps not better qualified to show us the way through present difficulties in nomenclature than was Franklin to prescribe directions for the repair of the Atlantic Cable. Both Franklin and the British Association Committee were highly skilled and worthy of all confidence, but the scientific circumstances in which we find ourselves are new, and would have been strange to both one and the other. If our time produces new complications and necessities we must not shirk doing fresh work. Does anyone seriously maintain that we

* 'Entomologist's Weekly Intelligencer,' vol. v., p. 113.

must use no means for clearing a present block in nomenclature but those which the last generation invented?

The persons who agreed on the law of priority certainly did not imagine that out of that law would come the complications under which we suffer. The law was enacted in the first consciousness of the inconveniences arising from the different nomenclatures in use in different countries, and those who enacted it intended to make the way plain and easy. They did not contemplate the displacement of names universally agreed on, but intended that of the different names in use, one only, and that the earliest, should be adopted. Picture, indeed, the lawgivers of 1842 sitting down to enact the resurrection practices with which 1871 has made us familiar! Moreover, the rule first received the resurrectionist construction within the last very few years; and this interpretation was coincident with the appearance on the scene of that section of the entomological world which publishes Synonymic Lists. So that here is a test. Is it believable that for many years after 1842 the law was *misunderstood?* Or, is it the fact that for all that period there was a plain law well understood as requiring acceptance of the earliest discoverable name, although no one followed its precepts? Why, if this was all along known to be the law, was not the uprooting of established names begun at once? How, is it that we were suffered to languish on till 1871 in the noisome atmosphere of an inexpurgated nomenclature? The reforms of nomenclature undertaken in 1847-1850 by Mr. Doubleday were confined to replacing a later name in use here by an earlier name *in use* elsewhere. It was not until 1861 that the resurrection men made their first move, and it seems pretty clear that the law of priority was in fact originally *understood* to have the common-sense meaning. But again, was it likely that the originators of the priority law had in their minds the resurrection of names then wholly dead and buried? Did the occasion call for such a step? What could have been the motive for then making such a rule? The guide to an interpretation of any new law is to look at what was the state of things before it, and what was the mischief which the law was to remove. Apply that test here; the old state of things was merely that different names were in use for the same species in different countries, and what was required was merely the removal of *that* mischief. There was no outcry for a perfect theory of nomenclature; and the actual need to be met was all that came within the purview of this law. That need was the establishment of a uniform nomenclature *in cases where the different authors were at the time not agreed.* This was the law of priority as promulgated by English naturalists. If we turn to notice what has been done by foreign entomologists, we shall see that the corresponding legislation abroad illustrates our own. The law of priority was decreed at Dresden in 1858, that is to say sixteen years after our own British Association rules were made. The proceedings of the Dresden Congress were subjected to abundance of criticism—the mark of our sixteen years' advance of Science, in

which the rank and file had been catching up and coming level with the leaders. As a consequence, the law of priority decreed by the Dresden Congress was tempered by the contradictory clause that the law is "not absolute, and the choice between two names remains free." The code is found translated and commented upon in the Annals of two at least of the French Natural History Societies; and opinions of different shades are there put on record.

But I do not impugn in any way the principle of priority when reasonably and properly applied. It is the application of it by modern (that is, very recent) authors to which all my objections are directed. In their hands "the law of priority" becomes a hateful instrument; and the title itself has accordingly attracted much of the odium which should strictly have been visited upon those who misapply the law.

Latreille had instituted the priority rule; but it found more opponents than friends. Dejean, alike in his 'Species General' (1825) and in his 'Catalogue des Coleoptères' (1837) took for his principle the preservation of the name most generally employed, and argued stoutly against Latreille's invention. Lacordaire in 1834 published an elaborate essay attacking the priority principle on all points; and Silbermann wrote in support of Lacordaire. Any entomologist who desires to argue the priority principle on its merits has only to refer to the writings of these authors in order to find ready to hand the case made out against it. If those who object altogether to the priority rule be in error, they err in distinguished company. Those who have adopted a disparaging tone towards these objectors have plainly done so in ignorance that the authors I have mentioned were part of their number. The priority principle, by reason of the opposition and neglect which it met with, remained a theory until 1842. But some law was wanted to set the nations at one in their nomenclature; and the law of priority appears (notwithstanding the obscured condition of entomological science at the time when it was adopted) to have been a just law enough, and serviceable for that purpose. The application of the law was, it seems, intended to be this. Different names for the same insect being in use, one of them only was to be chosen, and that one the name first given. The law has, however, been *applied* to introduce the first name ever given to the insect at all, even to the displacement of all names in use. Then, owing to the insufficient and untrustworthy character of the old descriptions, authors have, in hunting for the earliest names, come upon a vast number not really recognisable. The consequence is that some authors accept these, and others reject them, while they accept other names which the first authors in their turn reject; and confusion reigns supreme. Again, the application of the law has been hampered by a variety of checks and restrictions, the scope and extent of which afford more ground for disagreement. These things have made nomenclature seem a difficult matter; and this last result has, more than anything, tended to put entomologists in a worse and worse plight. Nomenclature being thought

to be so difficult, its mastery has been the object of comparatively a very few. Nomenclature is now, in fact, almost a craft of itself; for the devotion which its professors have shown to it has resulted in many nice refinements not attractive to Natural History students. Hence those authors have appeared too learned for entomologists at large; and no one has arisen to cope with them or curb their extravagances. Their work proceeds until (as it has been truthfully expressed) the entire object of names is frustrated.

But it seems, indeed, that they are now put on their defence. The changes of names familiarly and universally known have recently aroused much feeling; and the entomological world is, I believe, ready and eager to press for a reconsideration of the law by which such intolerable inconveniences are justified. It is only to assist in the expression of this feeling, and as a step towards this reconsideration, that I have ventured to join battle with the list-makers. From those who express strong disapproval and are unsparing in continuous protests, it is fair to ask what they themselves propose. So far as I personally am concerned, I know clearly what I propose; and in the course of these remarks a simple suggestion will be put forward. I have no right to speak for others, and do not assume to do so. The remedy I look for and the reasons for it I leave to unfold themselves in due course.

The existence of different names for the same insect is chiefly occasioned by a cause which entomologists have no occasion to regret, viz., that different workers have independently investigated the same ground. It is very much for the advantage of Science when such independent investigations take place, and I have never been able to agree with those entomologists who launch dreadful diatribes against the new describers of an old insect. Nature is more important than her describers; and so long as the student brings us to an acquaintance with facts, he may be held excused if he omit from his record of them the stock *rechauffée* of authorities from a library. True it is a very great advantage for entomologists of different countries mutually to understand each other, and the writer who makes inter-communication easy deserves well of his fellows: but so also does the independent investigator; and I could not let fall any censure on an entomologist who erroneously describes as new an insect which he has lighted upon for the first time in his own discoveries. That this act should be represented as a "*crime* against Science,"* or call forth high-falutin declamation of any kind, can only take place where the critic has forgotten scientific language in contracting the *cantilena* of small scientific authors.

What we have to deal with is simply the fact of the accumulation of many names for one insect. Our task is to choose one name from among them; managing, as far as possible, to cause no inconvenience

* See 8 Ent. Mo. Mag. 41. Contrast Mr. Dunning's opinion in 8 Ent. Mo. Mag. 215, where the "sanctity" of the first nomenclator is rudely offended.

or confusion. This we may certainly do by any rule or by no rule at all. We are not bound to choose the first name or the last, the longest or the shortest; but it is expedient that there should exist some rule so that we may all arrive at the same choice.

Now, we of this generation find a rule all cut and dried for us; and we agree no doubt that this rule is reasonable and efficacious. Moreover, it is the only rule which has been acted on for securing uniformity; and, independently of its merits, the principle has now received very extensive acceptance. Therefore, we are not put to invent a rule for ourselves, as we are fortunate enough to inherit a good one. But we of this generation make a discovery. We find that this rule is being applied in a manner to re-introduce the confusion which it was invented to dissipate. We find that names in use nowhere and entirely forgotten are brought up to supersede names universally agreed on; and we find that upon the new names themselves there is no sign or semblance of agreement between those who support their introduction. This is a new matter entirely. Our cut-and-dried rule will not serve us here; and it seems that we are called upon to invent something to meet the difficulty. This we are fully entitled, and indeed, if we are worth our salt, are *bound* in our turn to do.

Let us shortly examine some of these difficulties, and the causes which have occasioned them. We shall soon be able to judge whether our old rule will be sufficient in time to overcome them; or whether it is not necessary for us to come forward and devise for ourselves some new expedient. For this purpose I propose to take note of the opinions of those who hold that the old rule will be sufficient to help us.

In the first place it is necessary to remark that every resurrectionist author has his own particular views. There is no such thing as a complete agreement among them all, or even between any two of them. The partisans of absolute priority differ radically among themselves on both the two questions,—*when* our nomenclature is to be taken as beginning, and *what degree of identification* is to be required before a given name is accepted; or, in other words, they differ *in toto* as to the application of their principle. They likewise differ on the questions how to arrive at a name: when a species is described by the discoverer more than once; and in the case of names nonsensical or not properly constructed. I will touch very lightly on these several matters.

First of all, then, the old-priority partisans differ completely as to when our nomenclature begins. The priority principle, they say, requires the acceptance of the earliest name; and it is, therefore, of the first importance to discover how far back your investigations are to go. A short examination into the position of affairs will be quite enough to satisfy entomologists that there is no reasonable hope of an agreement being arrived at on this head.

There are, at present, at least four different dates, each of which is

set up by different living entomologists as the date when scientific nomenclature began:—

1735. Mr. Crotch (for genera).
1751. Dr. Thorell.
1758. Staudinger and Wocke; Gemminger and Von Harold.
1767. Mr. Kirby (Catalogue); British Association.

In addition, Mr. W. F. Kirby has shown that specific names were first given in 1746; so that this new date must be added to the list of those claimed as the commencement of our nomenclature.* Some of the names of 1746, rejected by Linnæus himself, were, it seems, subsequently adopted by Esper, Retzius, and others;† so that there is abundant reason for contending that those Fauna Suecica names should be upheld. The other dates, 1751, 1758, and 1767, are each and all found supported‡ by arguments, into whose merits, as they are foreign to my subject, I do not now travel.

The importance of this question can hardly be over-rated. A great many species bore in 1767 different names from those which

* See 8 Ent. Mo. Mag. 42. † See 8 Ent. Mo. Mag. 142.

‡ I quote the following authorities for these dates:—

"As our subject-matter is strictly confined to the binomial system of nomenclature, or that which indicates species by means of two Latin words,—the one generic, the other specific,—and as this invaluable method originated solely with Linnæus, it is clear that, as far as species are concerned, we ought not to attempt to carry back the principle of priority beyond the date of the twelfth edition of the Systema Naturæ. Previous to *that* period naturalists were wont to indicate species not by a name comprised in one word, but by a definition which occupied a sentence," &c.—Rules for Zoological Nomenclature, authorised by Section D. of the British Association, 1842, p. 9. This passage is, of course, founded on a mis-statement. The extraordinary want of information here displayed bears testimony to the pertinence of some of the writer's remarks on p. 2 *suprà*.

"Species ought to be designated by a double Latin name, of which Linné has given the first example in the tenth edition of his Systema Naturæ. We have up to the present time thought that all denominations anterior to the twelfth edition of Linné ought to be regarded as not met with, and that the names given in this edition (whether they be those of Linné himself, or come from another author, or be the result of a change of name made by Linné) ought to be upheld. This way of acting is illogical, and endangers the fixity of names in Natural History. It is illogical because it does not commence at the commencement, it is dangerous because it starts with an exception and with a denial of justice. If we allow to Linné the right of changing names and replacing them by others, we accept a precedent which can be imitated by other writers in Natural History."—Dr. Staudinger, pref. to Cat. 1871, pp. x., xi.

"We leave unnoticed all works published previously to the year 1751, when Linné's Philosophia Botanica appeared, in which his new system of nomenclature was first fully and distinctly propounded. . . . It appears from this that we ought not, as in some quarters has been proposed, to fix upon either the tenth, or still less the twelfth edition of Linné's Systema Naturæ as the starting-point from which priority in specific names is to be reckoned."—Dr. Thorell, On European Spiders, pp. 7, 8.

As to the date 1735: Mr. G. R. Crotch, Trans. Ent. Soc. Lond. 1870, p. 41; Cistula Entomologica (pars. iv.), p. 60.

they bore in 1758; both again being different from those they bore in 1751 or 1746. Those authors who decline (following the British Association) to go behind the twelfth edition of 'Systema Naturæ,' will always give the insects under the names they bore in 1767, and ignore all previous ones. Those who go back to the tenth edition (as does Dr. Staudinger) supersede the 1767 names in all the cases where those differ from the ones of 1758. The authors who follow Dr. Thorell, or act upon Mr. Kirby's discovery, will, in like manner, supersede as synonyms the later names of the species which they find described in 1751 or 1746 respectively. Until, therefore, the question, when our nomenclature is to begin, is determined, we have not a source of potential difference, but a cause *ensuring* confusion. The two most recent synonymic works accept different starting-points. Mr. Kirby's Catalogue of Diurnal Lepidoptera treats 1767 as the starting point; Dr. Staudinger's Catalogue of European Lepidoptera begins with 1758. This partly (but only partly) accounts for the widely different conclusions at which the two accomplished authors have arrived. I shall have presently to notice the very remarkable proofs that the disagreement between these authors would be actually as wide, or wider, if they accepted *the same* starting-point.

The tendency of recent opinion seems to be to shift the commencement as far back as possible. This at least is exhibited in an uncomfortable degree by Mr. Kirby's different utterances. First, in summarising his views on this head (in Journ. Linn. Soc. Zool. vol. x. p. 502), he says:—"It seems clear that we must either take the earliest or the latest works of Linnæus to begin with. To admit the claims of any author, *previous to the year* 1767, would simply be to introduce an element of additional and very serious confusion. . . . . The danger of making any exceptions to the rigid limit of 1767 in adopting specific names is so great, that it appears most desirable to refuse the claims of all previous authors." And in Trans. Ent. Soc. 1870 (p. 133), read in March of that year, Mr. Kirby terms the twelfth edition of the 'Systema Naturæ' (1767) "the lawful commencement of our nomenclature." Next, in the preface to his Catalogue of Diurn. Lep. (p. iv.), dated March, 1871, Mr. Kirby merely remarks, that though "some difference of opinion exists" as to whether our specific nomenclature should commence in 1758 or 1767, he has adopted the latter date. But after this point Mr. Kirby began to parley with, and finally went over to, the foe. In June, 1871, he writes, that 1758 "will probably be fixed as the commencement of our scientific nomenclature;" and* in August, 1871, he declares "*the date of* 1767 *cannot be defended*," and proceeds to give his reason for the view! Now I do not desire to press Mr. Kirby unduly with this alteration of his opinion, although it is very hard upon us to be obliged to master these vexatious changes (published in several successive papers), only to be informed afterwards by our

* 8 Ent. Mo. Mag. 142.

instructor that we have been taught the wrong thing. But see where this change of opinion leaves us. Mr. Kirby has printed his Catalogue of *Rhopalocera*, and the names are arrived at by fixing the date 1767 for the commencement of our nomenclature. Yet, *before the work reaches the hands of his subscribers*, Mr. Kirby has sent to an entomological journal a confession that his leading principle has been wrongly applied! This, even to me, administered a nasty shock; and it may, perhaps, suggest reflections to those who have indulged themselves with the hallucination that list-making brings finality.

Mr. Kirby in August, 1871, gave up the stand-point, which up to March, 1871, he had defended against all comers. What period of time will elapse before Mr. Kirby notifies that the date 1758 "cannot be defended"? When in a few months more or less the hour for that disclosure has sounded, there is the next cry all ready for Mr. Kirby and the true believers. As "we must take the earliest or the latest works of Linnæus to begin with," and as Mr. Kirby has convinced himself the beginning is with the Fauna Suecica, the word will next be passed to stand by 1746! That step is already prepared. Mr. Kirby seems now to have taken up ground which he declares all the time to be untenable; for the date 1758 is not that of the "earliest" or the "latest" of Linné's works.

Next, the numerous authors differ as to what degree of identification is to be required before a given name is accepted; and here we have the most fruitful source of disagreement.

The divergences of opinion on what is an adequate and proper definition of a genus, and on the method of applying the priority law to the care of genera are more wide, if anything, than the same differences with reference to species; and the questions raised in respect of generic names are also more novel. As the controversy concerning generic names is a question by itself, I conceive that I do right to pass it by here. Everyone who has paid attention to recent expressions of opinion will feel satisfied that there is a sufficiently strong feeling against the suggested innovations in generic nomenclature to prevent them from being accepted for a long time to come; and it seems likely that they will hardly be seriously entertained unless introduced in some less fugitive manner than any hitherto adopted. I will refer to the comprehensive criticism of Mr. Kirby's revision of generic nomenclature, by Mr. A. R. Wallace, in the Presidential Address to the Entomological Society of London (Trans. Ent. Soc. 1871, Part 5), and to the even more damaging papers of Mr. Crotch (Trans. Ent. Soc. 1870, pp. 41, 213; 'Cistula Entomologica,' pars 4, p. 59) and Mr. Kirby (Proc. Ent. Soc. 1868, p. xliii.), which will furnish those desirous of studying this part of the subject with all the material they can possibly need for satisfying themselves of the outrageous character of the "reforms" to which we are asked to accede.

I must confine my notice of the generic question to this statement. Mr. Crotch has in Cist. Ent. pars 4, p. 59, &c., shown us that

Mr. Kirby is wrong in the names of twenty-seven genera defined before 1817. Mr. Crotch has, as yet, gone no further than 1816 in his published reforms of the genera of *Lepidoptera*. The spectacle of one "absolute priority" champion making waste paper of the freshly issued octavos of his fellow "reformer" is peculiarly gratifying to a mere outsider; and we must all wish speed to Mr. Crotch in his good work of demonstrating how many different ways at once those of his party want to lead us.

The question what is sufficient identification of a name with a species has often before set entomologists by the ears; and there are various expressions of opinion to be found concerning some aspects of the question, which we must presently consider.

It is not necessary to draw a new picture of the confusion in specific nomenclature. "The longer and more thoroughly," writes* Baron von Harold, "that I occupy myself with the subject, the more the conviction forces itself upon me that a good part of our nomenclature, in so far as it has reference to the literature of the end of the last and beginning of the present century, is nothing more than a protracted and fixed chaos of arbitrariness, inconsequences, and blunders, to the sifting and correct dealing with which hardly a beginning has been made. . . . We see almost daily every monograph, and every working entomologist who has looked a little into the older literature, introduce obsolete and disappeared but correct names in the place of those hitherto in use. . . . It is clear that from this constant struggle after truth, which may be considered as an emanation of the correctly-founded principle of priority, the stability of the *present* nomenclature is injured." Our nomenclature of the end of the last and beginning of the present century is "nothing more than chaos." Both sides, then, are agreed upon that, and I need not waste words in proving it. It *is*, as I for one have always urged, the plain truth, that if we surrender the nomenclatnre now in use we plunge at once into disagreement and confusion. "Chaos" is the term chosen by the list-makers to describe this. Baron von Harold, Dr. Staudinger, and Mr. W. F. Kirby, all hit upon the word; and I can do no less than accept it. Now these gentlemen base upon this fact the conclusion that the said disagreement must be scrutinised and methodically cleared up. Those on the other side draw the conclusion that the said disagreement is best let alone, and allege that it becomes hurtful to science only when meddled with and brought to notice.

It is urged by the authors I have named that the acknowledged disagreements can be reconciled by the application of rules. If, they say, we forego our present stability (which is an abiding in error), we shall, in course of time, possess a better and true stability, which will be accuracy itself. It appears to me that one very patent consideration entirely disposes of this reasoning. *The rules applied by different authors are discordant; they are applied by them in contradictory*

* 'Coleopterologische Hefte,' vi. p. 37.

ways; and the result is not the reconciliation of existing disagreements, but the exaggeration of those, and the creation of new differences as well. This answer is so complete that, in my judgment, the case of our opponents is annihilated by it. But, inasmuch as I place such entire confidence in this answer, it will be worth while to explain myself before proceeding.

If I take as text two assertions of the opposite case, my commentary will be easily understood. After the passage above cited, Baron von Harold continues:—"It is clear that these changes must eventually lead to final stability." Dr. Staudinger varies this:—"The changes are the only means which can furnish us with fixity." Now, I am not going to be clever or captious; but I note that Baron von Harold does not allege that stability is attainable by no *other* method. Dr. Staudinger, on the other hand, confines himself to alleging that no other method *can* furnish it, and leaves to mere implication that his method will do so. My answer consists in the assertion of the matters which these learned authors respectively omit; and I draw attention, in passing, to both of them. I contend that the method advocated by these authors will fail to give us stability; and I allege, besides, that another method will succeed. All that it is necessary to do, in order to upset the reasoning put forward, is, however, to establish the former position.

It might show devotion to science to abandon the certainty we have for ultimate certainty of another sort, even at the expense of an intervening period of confusion. I do not share that view; but it is intelligible. We must know, however, that we can get the ultimate certainty; or at the least be satisfied that there is a reasonable chance of it. Now, is stability in nomenclature attainable by the application of Baron von Harold's principles? Is it attainable by the application of Dr. Staudinger's? Stability means agreement everywhere; the possession of a nomenclature which *all* shall stand by. Now, as Baron von Harold knows perfectly well, all even of the authors do not agree on his principles; and therefore all even of the authors will not accept his results. He is aware of this; for he explains his own principles, "less," he says, "to increase by fresh discussions the already *existing controversies*, than to make known the principles on which *we* think it necessary to proceed."* As for Dr. Staudinger, he is deeply impressed with the effect of existing dissensions upon certainty in nomenclature. "*The diversity in the laws* that ought to govern nomenclature" is to produce the veritable chaos "*unless* we attain to the establishment of fixed laws." Here again, then, there is nothing left for me to prove; for both sides are again agreed; and I adopt the same line as these authors. Baron von Harold can only give stability to those who accept *his* principles: and he is aware that those persons are not the whole but a section of the Coleopterists. Dr. Staudinger occupies a like position, and shows us that he knows it; and the case is the same with all list-makers.

* Catalogus Coleopt., Einleitung, p. x.

For instance, Mr. W. F. Kirby (though he has prefaced his Catalogue by a very scanty statement of his principles) cannot any more than Dr. Staudinger give stability to those who side against him. He gives stability to those who believe his identifications and grant his postulates, shutting their eyes and ears against all others: this is the utmost which any list-maker gives in our state of utter disagreement. I am unable to see how stability of this kind differs from the stability which the older entomologists enjoyed when they looked only to *their* single author. We are getting to be in a position like theirs; but we shall in truth be worse off. Upon exactly the same data, four list-makers may choose four different names and take their final stand upon them, each triumphantly proving himself to be right. As he has arrived at his names by great labour, and on favourite principles created by himself, no one of these list-makers will ever be found abandoning his work, or espousing the principles of some one else. The only hope seems to be for entomologists at large to intervene before this battle is set in array. I ought perhaps to notice some declarations (I do not like to term them "opinions") that the more lists we have the nearer we come to achieving certainty. The proposition is, in my judgment, ridiculous. People going in opposite directions will finish by reaching different places. If anybody is unable to see this without argument, I am not ambitious of convincing him.

The result, I am bold to predict, will be a repetition of the state of things before list-making began. The original complaint was that people, from a belief in their established author, held to his name right or wrong. They will soon be driven to take this course again. It was easy, however, to ascertain the first in date of all the names in use, and, choosing it, to abandon all the others. In future, each name being proved by its sponsor to be incontestably right, mere predilection must decide everything. A polyglot author, or one who writes in the language most general, will stand the best chance of obtaining followers; and this man may be the most unreasonable of the whole number. Or, we shall see entomologists of the different countries supporting their own respective list-makers; and at this highly edifying result I cease my prophesies.*

But suppose that it were not (as I submit that it is) certain that nomenclature will get more instead of less confused, if the resurrection practices be continued. Suppose that authors who have invented one set of principles in the end convert the authors who have invented opposite ones (a violently improbable assumption). What has yet to be gone through before stability will be announced? Why, the mock

* When this takes place, there will be a call for one gigantic List. We shall require a concordance giving each species under the names it is called by in all countries, *i. e.* the oldest species under some six names apiece. Each of these will be the true name of the species, and none will be synonyms; for each will have been proved to be right. I grant that by this means clear-headed men (with a powerful memory) may avoid real confusion in the end. Only this seems to be not what entomologists at present look for, and can I think scarcely be termed the enjoyment of uniformity in nomenclature.

stability which any one list-maker can provide for his own followers is to be had only after a fierce ordeal, as we are openly told. We must go back into "chaos" before one list-maker, using his own rules in his own way, attains a certainty which satisfies *himself*. "Scarcely a beginning" of the attack on this "chaos" has yet been made. When can we expect the end? But, supposing an agreement on principles, what time must elapse before all agree in the *application* of them? We shall find that, wide as are the differences in the principles, the latter are yet a smaller source of disagreement than are *divergences in the judgment* of individual authors—which do not depend on principles at all. Agreement on principles being conceded, the application of them by all authors so as to secure a uniform nomenclature is, I believe, still completely out of the question.

The divergences in judgment (to which I now refer) arise in the attempts to identify a species with an old figure or an old description. Thus it commonly happens that one author will fail to detect a resemblance where his brother sees a likeness quite sufficient. More frequent instances are supplied by the attempts to identify a figure or description of some species which has others closely allied to it. Here an author who finds the description (which he sees is certainly intended for one of three or four allied species) is prevailed on to choose the name for no. 1, which he then persuades himself it suits. The next who finds it persuades himself that he recognises no. 2; and there is no end to the possible differences in judgment until all species with the remotest similarity have received the same name in turn. It is with some list-makers a *mission* to use up the old names; and when the case arises that a name is passed by without being introduced for *any* species, it may be concluded that the authors have found none in nature which suits it ever so remotely.

The worthlessness of such attempts at identification is patent; but I will briefly mention some of the reasons which prove this. Many old authors were very ignorant; indeed all of them were deficient in comparison with a first-rate entomologist of the present time; for in Entomology there has been a real and very rapid advance. The old authors showed their ignorance in two principal directions. They described varieties of all shades as separate species: they, on the other hand, often described several nearly allied species as only one. The consequence of the first error is, that the description (even if adequate) applies only to a special form and is truly recognisable only by an author who happens to try it for that form. The consequence of the second error is, that the description is a description of an imaginary insect not existing in nature at all, but possessing in itself the characters of several; and therefore there is no wonder that in such a case authors differ *ad infinitum* upon the species indicated. The work of the old describers is furthermore reduced in value by the restricted knowledge of species which all (including the best) of them enjoyed. In consequence, a given description, which in 1767 (for instance) may have fully identified one insect, does not now identify

it at all; because entomologists of the present day are acquainted with a batch of allied species all of which suit the description. This last deficiency is made apparent by our advance in knowledge; and I of course do not make on that account any reflection upon the old authors. But, moreover, they sometimes described from worn and bad examples, without indicating that they did so; and therefore, while their descriptions apply only to bad specimens (and are not recognisable for good ones) authors land themselves in irreconcileable differences through their persistent attempts to identify them. Lastly, it is the opinion of many that some old authors not seldom described species which they never saw; copying from and garbling the descriptions of others. As descriptions of this class might be purposely altered in order to render them unlike the pirated originals, here again we need feel no wonder that authors find themselves disagreed in their attempts at identification.

We need not feel surprise at these results. But I must and always shall feel surprise that the authors go into those questions as they do. Worthless in great part the old descriptions now are from one cause or another; and whether they are recognisable or unrecognisable concerns not a living soul. But if they were the perfection of scientific labour, and contained truths of world-wide importance, the old books could hardly be more rigorously studied. The "chaos" referred to by authors is a "chaos" created by and only now existing in these worthless descriptions; and there is not a shadow of obligation to touch that chaos at all in nineteen out of every twenty cases where it is touched. Those who bring us back to that "chaos" and disturb our nomenclature with the results of their speculations on it, are themselves responsible for the condition of things which (so far as it exists) is *of their own wilful introduction.* We have long been quit of these ancient unrecognisable descriptions. We do not use them nor want to use them, because we have new and good ones. We have in universal employment names fully identified; and about which we are in no doubt or confusion of any kind. Yet on the strength of their speculations upon these descriptions, whose infirmities I have stated, a number of authors are now endeavouring to overturn the accepted names. Their justification is the "priority" rule providing that the first name given to an insect shall be adopted in place of all later ones. By the help of this rule and a collection of principles peculiar to himself, each of these authors—unsolicited, irresponsible, and completely uncontrolled—is now at work introducing the names attached to the descriptions which I have characterised. His pretext is that a law has been passed requiring the first name to be found and adopted. No such law, as I believe, ever has been passed. But, if it has, then the power which passed it has the fullest control over it still; and since its provisions have been made use of to work harm, they must now be modified in order to make that use of them impossible.

The modification necessary is merely such as shall prevent the dis-

placement of names agreed on; and there never was a simpler amendment. The law of priority, therefore, will (if we agree so to alter it) be that "The first name given to a species shall be accepted to the exclusion of all subsequent synonyms; *so, nevertheless, that no name in use be displaced for a name not in use.*" In the paper printed at the close of this discussion, the general considerations pointing to the necessity of this course are dealt with. I mention the exact proposal in this place, because, having just considered some of the causes of our present situation, entomologists will be enabled, having the proposal before them, mentally to test its efficacy. The old "chaos" names are out of use. The proposed restriction, therefore, will absolutely bar their introduction; and as "scarcely a beginning has been made" towards touching the "chaos," we are in time to prevent the most serious results of intermeddling. Rarely, I think, has there been so simple an escape from so great a misfortune; and I cannot but feel great confidence that entomologists will agree to use it.

The differences concerning old names arise chiefly in the case of nearly allied species, where a name doubtfully identified has been traditionally attributed to some one of them. Now, it is a rule that no species shall bear the same name as another in its near neighbourhood. Therefore to identify the name afresh with one such species is (as a consequence) to deprive another such species of its name. Bearing this well in mind we are now on the track for discovering the whole cause of synonymic confusion. Species A being declared to be the one intended by the name *Clyte*, species B, which has hitherto enjoyed the name, must surrender it. Then down come the list-makers upon species B. One declares it is *Umbra*, a second *Symposiarchus*, a third *Servus*, a fourth *Scurra;* and each proves to you that he alone is right. Now *Umbra, Symposiarchus, Servus* and *Scurra* have all severally been identified with and are used for different species, none of which is the old *Clyte*. Here is a glorious field for reform! First, the four authors supersede respectively these four names. "Nom. præocc." writes the first list-maker against the name *Umbra*. "Sed *Symposiarchus*, L., alia erat sp.," writes the second; if he have serious qualms about the identification throwing in "certo" to keep his courage up. The two others in like manner supersede *Servus* and *Scurra*. Thus the rings are getting wider, though the pebble which disturbed the water was such a very tiny one. Four species have now lost the old accepted names; and the rival list-makers are all at work scrutinising the miserable old descriptions to find four fresh ones. An observation will at this point burst from an inattentive reader. "Yes, but all the four old names are not superseded. No single author (in the case you are putting) wishes to supersede more than *one*." My critic, does that fact make the confusion less or greater? If the four names were conceded on all hands to be erroneous, we should start fair; but each one of the four has now its opponents, but also its supporters. Say, three list-makers retain it; one rejects it. With a great many

the last turkey will be the largest, and the newest extravagance be deemed the highest fashion.* But as three list-makers stand by the old name, the chances are that entomologists are divided about equally. Each of the four is choosing a new name for one species. It is therefore very likely that all—morally certain that some—of the four list-makers will fix for their new names on names likewise now in use for neighbouring species. These in their turn are cast adrift without a name; and the corresponding process is again repeated: this not only may, but does, go on now as the common practice.

These differences having spread over some little time, a list-maker next arises who has been no party to anything anyone else has ever said or done. He has got nothing to hamper him. His broom is new, and a clean sweep of our nomenclature he means to make. Is it to be expected that (the case admitting of so rank a crop of differences) he will not on some one, two or three points at least break out in an entirely fresh place? Where each man's eyes, intelligence, and perception make his law, what reason is there for expecting that the power of originating differences has been exhausted even in two or three generations? I will not weary the reader by continuing my diagnosis of these differences. I have put him in a position to realise them for himself as they are at present, and as they will be. Picture these processes repeated *in perpetuum;* imagine all the circumstance attending the appearance of each big compilation; and these appearing one after the other with all the frequency with which recent industry has produced them. Picture the same processes complicated by all the petty refinements which cheap-learning now delights itself with, and with who shall say how many similar ones to be invented in years to come! And when you have pictured this, go to Dr. Staudinger, Gemminger and Von Harold, Mr. Kirby, Mr. Crotch, and any other comforters you can think of, and solace yourself with their assurances that all this is done for the pure love of science, and that scientific studies were never before so *certain* of producing uniformity.

Some short notice of the classes of names contested, so far as they have not been hitherto dealt with, will not be out of place. These appear to divide themselves as follows:—

(1) A catalogue name; *i.e.* a name published, when first bestowed, without either a figure or a description of the insect, and without any reference to another work where that appears.

(2) A name accompanied, when first bestowed, by an insufficient description or figure, and recognisable only by reference to the author's type specimen.

* I heard it recently insisted that all changes appearing in a List are thenceforth "made" and not "proposed." I forget whether anyone took the trouble to ask whether all the discordant changes of the same name are equally "*made*"; or which one is more "made" than the others.

(3) A name identified only by an accompanying figure, description, or reference to another work, where those means leave the identification in doubt.

(4) A name which, when first bestowed, was identical with that of another species in the same genus.

Catalogue names, always, it would seem, in fact accepted till very recent times, have come to be pretty generally scouted since the resurrection practices have reached such a head. Names of the second and third classes—that is, all names doubtfully identified—furnish, of course, the greatest number of disagreements. Every author has been his own judge, and the confusion has been complete. Recourse to type specimens (which appears inconsistent and illogical on the part of those who reject catalogue names) is a fertile cause of difference.

The rejection of catalogue names is (whether justified or not) an infraction of the rule of priority. I, of course, bow to this rule loyally, though detesting the phrase "priority," as the stalking-horse trotted out to justify wholesale changes. But our opponents are, in this, not supporters of priority. What they all uphold is, indeed, a rule of priority hampered by artificial checks, which checks make necessary many more changes in names, and certainly find the list-makers in work.

Why is not a catalogue name in use as good as any other? Any name universally acknowledged should, as I contend, need nothing to be urged on its behalf. But why should not a catalogue name be as good as any other, even when not universally employed? There never was till recently (if there now be) a general observance of any prescribed method of naming; all that men cared for was to have the species named in fact.* It appears a hardship to disqualify names bestowed before the present rules of nomenclature existed, in cases where (notwithstanding the vice in their origin) they have been identified and accepted. For my part, I fail to see the need or desirability of rejecting a catalogue name. It appears to me that the use of the name to indicate one certain insect is at least as good an identification for our purposes as a figure or description, whose accuracy is open to the criticism of every fresh commentator. I agree fully that where the same catalogue name is used for two different species, it cannot stand for either. In that case there is no identification, and the name is a name unattached. But all the catalogue names which are in use for one insect only are fully identified; of this the fact that they are so in use is of course conclusive. The question naturally arises here, What is the object of scientific names? Is it to enable entomologists to indicate the insect by a word or badge everywhere understood? Or is it to glorify the first describer? If it be the latter *cadit quæstio*, the name given by him must be

* See Silbermann; 1 Silb. Rev. 132.

preserved, and all others rejected. Most of us, however, believe that the object is only to provide a badge by which the insect is at once indicated to entomologists. Now, this is as effectually done by a name bestowed in one way as by a name bestowed in another, so long as the name *indicates the species* to entomologists, *i. e.* so long as the name is in use for the insect, and not in use for any other closely allied. Why, then, shall not catalogue names in use be upheld, if they be the prior names? I refer, I repeat, only to catalogue names in use, for catalogue names not identified cannot possibly stand, whether innovators wish it or not. I am not, however, in any way concerned to argue this question, both because it is not necessary, and because the question has been well argued in print before. On the admissibility of catalogue names entomologists are thoroughly divided; and those who read M. Amyot's argument in favour of these names in Ann. Soc. Ent. Fr.,* will satisfy themselves that the practice of ignoring them is not and will not be acceded to. Works as recent as M. Guenée's Noctuélites and Phalénites, and Mr. Doubleday's Lists, accept them unconditionally; Dr. Staudinger and Mr. Kirby reject them all.

The idea of overturning a name universally agreed upon, only because when first bestowed it was not accompanied by a description, is *very* modern, if it did not originate, indeed, with the chief delinquent among contemporary Lepidopterists. The indictment against this novel practice is short and very intelligible: it unsettles nomenclature for the sake of a theory, and wantonly supersedes names which, besides being the approved names, are *ex concessis* also the earliest. No one of the innovations is more galling than this. It is impossible not to feel annoyance at the displacement of names given by the accomplished authors of the Vienna Catalogue, and adopted everywhere and always. The very large number of cases where our nomenclature has been, or is to be, unsettled, out of tenderness for this crotchet, makes it additionally important that its absurdity should be exposed; and, if I take only one instance of its working, I hope to show its authors in a foolish light.

The first species in the genus *Leucania* of Staudinger's Catalogue is "*Impudens*, Hb.:" this is the insect known to all the world as *Pudorina*, W.-V. The name *Pudorina* has been adopted by all the authors who have ever noticed the insect; amongst them being *Hübner himself*, Treitschke, Duponchel, Herrich-Schæffer, Guenée, Freyer, Stephens, and Curtis.

Now Dr. Staudinger has altered the name of this species to *Impudens*, Hb.; and the reason is that *Pudorina* was a catalogue name when first bestowed! He takes no account at all of the universal acceptance of the name *Pudorina* in all countries and by all persons (in books, catalogues, and collections) down to January, 1871. He says in effect: "The name when given was not accompanied by a description or figure; therefore the insect was never

* Third ser. tom. vii. 1859, p. 590.

named at all." And, having brought himself safely to this conclusion, he begins a rummage amongst his books to find us some name for the insect.

Now, unfortunately for Dr. Staudinger's success, all the books, old and new without any exception, call the insect *Pudorina;* the radical defect in its title not having prevented any one of the celebrated authors above named from accepting the name without reserve. A colourable way to dispose of the name was, however, discovered, although I will not pronounce upon its merits. Hübner figured a moth not of the same colour or size with *Pudorina*, and of a different cut generally; and to this (conceiving it to be a new species) he gave the name *Impudens*. He also accurately figured the male and female of *Pudorina* under the original name, and that he did so is not disputed by Dr. Staudinger. The name *Impudens* has simply not been recognised. Curtis, indeed, gives it as the (synonym) female name of *Pudorina;* and Guenée thinks it must be a variety of that species, though he, nevertheless, ranks "Impudens, Hb.," as a *separate species*, saying he has not seen it in nature. No other author appears to have noticed the name at all. Here, then, was Dr. Staudinger's opportunity; and, accordingly, he gives *Impudens*, Hb., as being "certo" our *Pudorina*. The name which *all* the world has *always* used for the insect, and which was *the first* given to it, is thus superseded, because (at best, and accepting the dubious* identity of the figure with the species) Hübner, by an error, figured a variety of *Pudorina* as a distinct species, he himself preserving the name *Pudorina* for the typical form! Dr. Staudinger has probably made no mistakes; and, therefore, this course can, by his principles, be justified. There is certainly nothing unusual about the instance, and it is desirable that entomologists should know some of the results they must expect if they trust him.

* The way in which our modern list-makers identify by scores the figures and descriptions which have been an enigma to preceding generations can only be described as wonderful. Speaking for myself, I do not believe in the accuracy of one-tenth of the new identifications; and I venture to doubt whether Mr. Kirby or Dr. Staudinger could produce a dozen Lepidopterists who are much more credulous.

The addition of "certo" after such and such a reference makes me very suspicious. All references ought to be verified "certo" or not be acted on at all. The word must signify either that the other cases are verified something short of "certo," or that this word is thrown in where there is a case of extra suspicion to quell our doubts,—and possibly the author's.

The indentification, from a mere figure, of an insect, which the naming author himself (knowing the type) considered to be something different, must generally be suspicious. Hübner, in the case discussed, was acquainted with both sexes of *Pudorina*, but considered his *Impudens* to be not that species at all. It requires a good share of self-confidence to assume to set right an entomologist having the knowledge of species which Hübner possessed, when the corrector is at so great a disadvantage. A figure of an aberrant form of the family *Leucanidæ* (of all insects in the Order) is not an object of which anything can be predicated "certo" in opposition to its author's judgment.

Dr. Boisduval, for example, would deal quite differently with this case. He writes:—"It is often impossible to arrive at a knowledge which author has first named an insect. Whenever there has been any doubt, I have given the preference to the name most diffused in collections. Where a species not yet described was known to entomologists under a certain name, I have never allowed myself to change it." Dr. Boisduval would give preference to the name most diffused. Here there was actually but one name in all the collections in the world! Dr. Boisduval will not even change a name which has been attached to an insect in collections; here the name was attached not only in collections, but by every describing author who has noticed the species. It may, therefore, be concluded that Dr. Boisduval (besides a large number of other entomologists) will not allow the rejection of accepted catalogue names. Of him and them I enrol myself as a humble follower. Dr. Staudinger, with ostentatious pedantry, alters, on grounds like this, the names universally accepted by entomologists. Dr. Boisduval, when in doubt, gives the preference to the most diffused name. On which side does common sense lie? And does not an author strangely mistake the current of modern opinion, who, at this day, flouts a common agreement to show the great lengths to which a principle will carry him?*

One other consideration remains. It was remarked so long ago as 1837, that all names had even then become in a manner "catalogue" names, inasmuch as they all depended more or less on tradition to identify them. Dejean (who upheld all museum names as well as all catalogue names) draws attention to this with much pertinence; and there is no doubt that his argument is stronger to-day than it was when first written. His words must be copied at length:—

"These authors think, then, that it is enough to give a Latin phrase and some lines of French to make us well recognise an insect; now the longest and most minute descriptions, the best

* I draw attention to the fact that this introduction of the name *Impudens* to supplant *Pudorina* is erroneous also in the opinion of others, *e.g.* Dr. Thorell:—

"Quum autem in uno eodemque opere varietates ejusdem speciei ut diversæ species descriptæ et nominatæ sunt, difficile interdum videri potest judicare, ex nomnibus datis quod retinendum sit et speciei imponendum. Definiendum est, quæ sit *forma principalis* sive *primitiva*, cujus nomen sibi adsciscat species necesse est, et cujus varietates igitur reliquæ sunt habendæ. Forma vero *principalis* ea existimanda est, quæ frequentissime invenitur in patria ejus, qui primus nomina, de quibus agitur, dedit. Si id dijudicari non potest, vel si apparet, *scriptorem illum veram formam non cognovisse: tum* primum ad alias rationes est confugiendum et ex nominibus, quæ dederit, id eligendum, quod exempli gratia magis quam reliqua in hac specie *tritum atque usitatum* sit, vel quod magis aptum et idoneum videatur—et id genus alia."—On European Spiders, p. 16, note; quoting the author's 'Recensio Critica Aranearum.'

Thus Dr. Thorell would, in the first place, adopt the name given by a writer to the typical form; he would, in the second place (if ignorant of that), take the name which has become most familiar. The name *Pudorina* is the one which answers to both of these descriptions.

figures, hardly suffice, and Entomology has come to such a point, that we are often very much embarrased exactly to determine an insect, even when comparing it with the individuals which have served for making the description. Knowledge of species becomes from day to day more a science of tradition, and I think that there is very little difference between collection and catalogue names, and those which are published every day with such activity."*

I apprehend that the fact is as stated by Dejean; and that unless tradition set us right we should now, among the infinity of descriptions and figures, be often in doubt among twenty or thirty different species when trying to name a single one. Supply a sharp but ignorant Lepidopterist with a collection of 1000 species from different parts of the world. Supply him at the same time with descriptions of every known species. Leave him by himself with the insects and the books; and tell him to name all the former. He will land himself in hopeless doubt as to more than half of the number. Dejean is right; that man is in bewilderment for want of the assistance which others get from tradition.

Therefore, authors who profess that they will accept no name which is not identified without tradition only mislead their readers. They do accept such names; and when they reject a catalogue name *on the ground* that they refuse assistance from tradition, they act inconsistently. This point has been taken up by Dr. Staudinger. He says† that where there is an "impossibility of recognising" what species the author intended to indicate, the name becomes a catalogue name. No doubt it does; I hope we all agree that it is pharisaical and absurd to allow a perfectly useless phrase of Latin to effect the salvation of any name. You grant the name must be dropped, if it have not the Latin phrase; you grant the Latin phrase is useless; but you uphold the name because it has the Latin phrase! Now, what bearing has this conclusion on the question of upholding catalogue names? To me it appears very cogent. If you do already, because they have a *useless* scrap of Latin, uphold names which are not recognisable; is it not clear that you must also uphold names equally well identified which have not the useless scrap? To be consistent you must do this; and I honour the writer (if there be one) who acts on the plain doctrine that a name with an unrecognisable description is a name without any description. You will never succeed in uprooting all names with an unrecognisable description; that is merely hopeless. If you wish to achieve consistency you

* Cat. des Coleop. (1837), p. xi. See the above observations, mentioned with approval by Silbermann (4 Silb. Rev. 241).

† Cat. 1871, pref. xx—xxi. This had also been pointed out by the authors of the Dresden Code. (2 Berlin. Entom. Zeitsch. app. p. xvi.). See also Ann. Soc. Ent. France, ser. 3, vol. vii., 1859, pp. 590-592, where M. Amyot (who consistently upholds collection and catalogue names) assails the Dresden reasoning from the strict priority point of view. M. Reiche (id. p. 610) relapses into inconsistency; disagreeing with M. Amyot on catalogue names, *and* disagreeing with the Dresden rules on insufficiently identified names.

must do it in the other way, and uphold all the names which are in as bad a plight. Your policy must be "levelling up;" that is to say, to preserve at once the old names and your reputation for consistency, you must accept the catalogue names identified by tradition.*

I have endeavoured to show that the old descriptions cannot be depended upon for identifying species. But it is quite clear that the specimen described ought to settle the point; and if "the laws" of nomenclature permit recourse to types, remarks upon the fallacious character of old descriptions lose much of their weight.

Now let us see what real assistance in the way of achieving *certainty* entomologists can obtain from inspection of type-specimens. It is, I fear, only too plain that recourse to them increases instead of diminishing the doubts. The entomologist who examines an author's types may find them just as the author placed them, and bearing his labels. On the other hand, he may find them sorted anew and re-arranged, without labels or with fresh ones, mixed with other specimens, or removed altogether from the collection. He may find them damaged or cleaned. He may find the author's labels affixed to species for which they were not meant. He may find the place of the sought-for types occupied by other specimens, which thus pass for the types which they have displaced. Lastly, he may find a combination of these adverse conditions.†

Thus, it will be seen, an inspection of an author's types can by no means be relied on for a road to certainty. But (bearing in mind that we are upon the work of the older entomologists) let us consider if these be the only causes which render types of little service for clearing up difficulties. We have just noticed some vicissitudes experienced by a collection after the author has parted from it. Dr. Staudinger has this statement:—

"It happens that authors after having created species afterwards mix up in their collections, together with the originals, species which are very near to them, and that their collections present numerous errors of this kind even in their life-time."‡

Now the collection of Mr. J. F. Stephens has not undergone any tampering with since that author's decease; and it contains the type-

* Catalogue names are very generally allowed in the case of *genera*. Mr. Kirby and Mr. Crotch agree in upholding them, as virtually does Dr. Thorell. Mr. A. R. Wallace, on the other hand, following the British Association Committee, insists that they must be dropped. As before, I abstain from going into this question.

† "Wer will aber überhaupt aus einer Sammlung, die ihre Schicksale und Zufälle erlebt hat wie jedes Ding auf Erden, die oft von Besitzer gewandert ist und in ihrem wehrlosen Zustande von berufenen oder unberufenen Händen rectificirt, transferirt, restaurirt, ja ganz eigentlich metamorphosirt worden ist, die wohl noch den Namen des ursprünglichen Besitzers trägt, aber dem Geiste desselben längst entfremdet ist, wer will, sage ich, aus einem so verschiebbaren, veränderlichen Dinge Beweise für Stabiles herzustellen im Stande sein"?—Dr. J. R. Schiner, 2 Wien. Ent. Monatsch. p. 55.

‡ Cat. 1871, pref. pp. xvi.-xvii.

specimens of species described by him. If I quote the evidence of some English entomologists as to the condition of it, I shall sufficiently corroborate Dr. Staudinger's opinion above quoted. Mr. J. F. Dawson has discussed the question rather fully. He writes:*—

"Suppose Stephens's collection, instead of coming to us direct from the hands of its compiler and owner three years ago, had become antiquated like the Linnean; or suppose the question of the types to be discussed some sixty or seventy years hence, with no more definite knowledge on the subject to assist the enquirer than the Stephensian types and the Stephensian descriptions would supply; might it not be argued that the types (in the instance under discussion) must be ignored, as they never could have been *intended* to represent the true *Loppa pulicaria*, Steph., because *they are antagonistic* to the *descriptions?*"

Again:—

"Dr. Schaum invariably refers to the Stephensian types; my references are frequently given to Stephens's works, irrespective of the types. Now we are well aware that these do not always correspond, but that, on the contrary, a considerable difference is often found to exist between them."

Among several instances, Mr. Dawson cites the following:—

"I refer *P. decorus*, Steph. Mand., to *B. decorum*. It is correctly recorded by Stephens as British, though *the representatives in his cabinet consist of several species besides.* Our author (M. Jacquelin-Duval), on the contrary, refers *P. decorus* to *B. rufipes*, Dufts.; always after these delusive types collectively. But to be consistent again, he ought likewise to have referred it as a synonym in part to *B. nitidulum*—to *B. affine* (species or variety)—to *B. stomoides* (species or variety) and (with me) to *B. decorum;* because the supposed types in the Stephensian cabinet do in fact = 1 *decorum* (the first in the row, and therefore probably the *true* type), 1 *stomoides*, 1 *affine* and 3 *nitidulum!* In what a jolly mess of confusion and repetition would a synonymy founded consistently upon the types involve us."

Mr. E. W. Janson has placed on record some similar criticisms;—

"It not unfrequently happens that two, or in difficult genera more, species are mixed up in Mr. Stephens's cabinet under the same specific title."† "Stephens's description of *Omalium planum*," he also remarks, "could certainly not have been drawn up from the specimen of *O. concinnum* which now stands as the exponent of the species in his cabinet."‡

And the following passage bears testimony that specimens quite unfit to be used for identification may be lighted upon by those who do succeed in discovering the veritable type:—

"The exponent of *D. Dresdensis* in the Stephensian cabinet is a

* Ent. Ann. 1858, pp. 56-60. † Id. 1859, p. 119.
‡ Id. 1859, pp. 137, 138.

mere fragment, on a very suspicious looking pin, *sans* head, *sans* legs, in fact *sans* everything."*

Writing of the *Trichoptera*,† Mr. M'Lachlan says:—

"Another circumstance which adds greatly to the difficulty experienced in determining many species, is the bad condition of Stephens's types. These have at some time been damp and mouldy, and in attempting to clean them the hairy covering of the wings has in many instances been totally destroyed, thus rendering the specimens almost useless as types."

The vicissitudes of an old type-specimen are usually greater than those of a fresh one. If the above-cited observations apply sometimes to all collections, they do so with greatest frequency to the oldest; and it is the identification of the oldest names which is (so far as types go) most doubtful. The truth of this has been shown by the investigations which entomologists have made. The Linnean collection, for instance, which is naturally the one of greatest interest, has been the object of some strong remarks, which I will notice further on.

Dr. J. R. Schiner has written a rather humorous paper on type examples, which is cited by the Dresden Congress. After depicting the dismay of the discoverer of a new species when his name *qualifer* is overturned for the *talifer* of an old collection; he announces his intention to break a lance in support of *qualifer*. He next (slightly altering the metaphor) places himself in the position of a judge deciding on the pretensions of the "claimant" *talifer*, Sempronius; and his narrative proceeds. The specimen's history is sifted. It is stuck in Ulpian's collection, whither (through the intervention of Quintilian—in a round-about way by Rome, Sparta and Athens) it arrived at Abdera. It is reported to have been seen there, still indeed with the original Sempronian label,—which, moreover, may have been lost somewhere through an unlucky accident.

"On this intelligence we shake our judicial head doubtfully, give a look full of significance at the jury-box, and continue our High-pains-and-penalties Inquisition so as further to inform ourselves more closely concerning the *personalia* of our 'claimant.' There stands now in the *Editio princeps* the *Carabus talifer*, Semp. His shape gives more the impression of 'oblongum' than of 'orbiculare,' his shins are armed with powerful spurs, his coat is of the colour of umber, &c., &c. We compare him with the so-called description of the object of our search. *His* coat is as black as an old raven; his shape as round as a funny story; and of spurs or such-like there is

* Id. 1861, p. 69. Lepidopterists and Coleopterists seem therefore to have met similar experiences. With Neuropterists the case is no otherwise. Dr. Hagen writes (Ent. Ann. 1863, p. 9):—"The investigation of the [Stephensian] species is the more difficult, as the types are not labelled according to the Illustrations, but according to the Catalogue, and some of them do not agree with the descriptions."

Mr. M'Lachlan (Ent. Ann. 1862, p. 31) makes the same complaint.

† Ent. Ann. 1862, p. 24.

not the smallest trace to be descried. Our hesitation has reached the highest pitch. We resume the trial, and lay the case before our jury for decision. The verdict rings out 'Guilty.' The examinee is an unrighteous usurper." *

There is no doubt that scores of names in use have been superseded on evidence as untrustworthy as that adduced by *Carabus talifer*. The divergences of opinion as to types naturally add to (as I have said) instead of diminishing doubts. Whereas when the old *descriptions* alone are looked at, the authors find in this one vast source of confusion; when reference to ancient *types* is included, an additional source arises. But here as elsewhere it is not alone differences in judgment which set authors at variance; their principles differ as well. Some authors prefer to place absolute reliance on type-specimens; some refer to them with a mental reservation; others profess to reject them *in toto*.

Of the last class are the authors of the Dresden Code.† They, however, allow recourse to the types of Linné and Fabricius. The British Association Rules, which do not in terms mention type-specimens, appear also to exclude recourse to them. ‡

Dr. Staudinger is an instance of those entomologists who accept type-specimens *sub modo*. I shall presently examine his utterances on the subject of identification generally; and it will not be convenient to divide the subject in two.

Those who place absolute or nearly absolute reliance upon types are a numerous body. Gemminger and von Harold, Amyot, Reiche, Guenée, and Doubleday, are of the number; besides of course all others (such as Dejean) who uphold collection or catalogue names.

I need hardly remind entomologists of the importance of *these* differences. An author who has recourse to types has a means of identification not open to those who decline that recourse. The consequence is that names which to the latter mean nothing are to the former fully identified. In his work therefore the former restores them; in his the latter rejects them. The two works may appear side by side with the same insect under different names; and while there is no accord upon this principle it affords a cause of disagreement which of course bears its part in complicating our nomenclature.

It is certain, however, that the practice of authors in this matter is less divergent than are their professions; and consequently the disagreement is not so great a cause of confusion in names as might be expected. An author who goes to types satisfies himself of the identity of a species which he finds in the author's collection. The other author, who does not go to types, learns nevertheless the identification which his brother has effected. While he is labouring to discover the meaning of a description, the other suggests the

* 2 Wien. Ent. Monatsch, pp. 51, 52. See Dr. Schiner's paper cited 2 Berlin. Ent. Zeitsch. app. p. xvii.

† Berlin. Ent. Zeitsch. vol. 2, app. p. xvii. ‡ See p. 15.

answer to him. He sees the crabbed words in quite a new light, and straightway identifies the name. It is plain enough that the authors (if there be any) who persuade themselves they uphold no name that does not speak entirely for itself, accomplish a self-deception. There is a near approach to unanimity that the descriptions of Linné and Fabricius are not recognisable without tradition.* The authors who uphold those names rely on tradition; and in the case of Linnean names the foundation of tradition is his types.

Now the Dresden Congress, which forbids recourse to types for identification of names, makes an exception in the case of names given by Linné; and a more unfortunate and inconsistent exception could not have been made. When the door is being shut against other sources of confusion, it is a most inconsequential proceeding to keep it open for the source of confusion which is most fertile of all. Of all existing types, the Linnean types supply, I suppose, the greatest excuse for differences of opinion.

In my judgment it would of course be preposterous to disturb an *accepted* identification of a description because it is effected by reference to a Linnean type; just as it is preposterous to disturb an accepted identification because the name when first given had *no* description. I have already said that I can see no difference between the two cases. But the Dresden exception permits identification of the Linnean and Fabrician names by means the most unsatisfactory and inconclusive. Dr. Staudinger writes:†—

"It is unfortunately a certain fact that the acquirer of the Linnean collection had the deplorable idea of sometimes replacing damaged examples by fresh, and it is nevertheless to the damaged examples only that the descriptions of Linné can apply."

Another writer makes the case much blacker:—

"Before the Linnean collection was placed in its present quarters, it was so maltreated by additions, destructions, and misplacement of labels, as to render it a matter of regret that it now exists at all. Any evidence it now furnishes is only trustworthy when confirmed by the descriptions."‡

I do not know how much of the last-quoted statement is founded on fact;§ nor in particular what authority there is for saying that

* See the opinions of Schönherr, Lacordaire, Stephens, Reiche, von Harold, Thorell, M'Lachlan, Staudinger, von Kiesenwetter, &c., collected in note to the British Association paper, *post*.

† Cat. 1871, pref. pp. xvi.-xvii.

‡ Mr. R. M'Lachlan, Trans. Ent. Soc. Lond. 1871, p. 443. See also Mr. E. W. Janson, Ent. Ann. 1859, p. 136.

§ It is probable that much has been laid to the score of "maltreatment" which should be ascribed to other causes. Mr. J. F. Dawson writes (Ent. Ann. 1858, p. 56):—"This matter simply resolves itself into the question as to the amount of value we are disposed to attach to the Linnean types. There in the Linnean collection stands an example (mutilated indeed) of *B. fumigatum* labelled *Cicindela rupestris*, Linn. We may assume that it *has* stood there as a type from a period antecedent to the date at which the collection was brought to England; because although we can easily imagine that specimens

the collection was maltreated by "destructions." But it is quite enough for my purpose that a writer can use these expressions concerning the Linnean types. The result is to shake all identifications of Linnean species effected by recourse to the types. There is no doubt that many dozens of names have been thus identified; and as those types afford ground for such a battle royal among the list-makers we shall never attain stability* if we allow a departure from our present identifications.†

Recourse to types extends the field on which an author's opinion makes his law, and therefore makes more ground for the inevitable divergences in *judgment;* it also supplies an additional subject for the warfare of discordant *principles*. The latter I have already lightly touched upon; the former require no long discussion. Where types are in bad condition, one author may well see a sufficient likeness, where his brother detects none whatever, though both be men equally well instructed; and any one is free to believe in or to discredit the current statements as to interference with or displacement of types. Again, the identity of types is a matter on which the entomologist with a great knowledge of species is certain to come to conclusions different from those of one less instructed. A very slight want of knowledge, indeed, will prevent a list-maker from correctly apportioning a dozen damaged "types" among the species (and genera) to which they of right belong; and all our list-makers do not know everything. I conclude therefore that it cannot be said that the

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(particularly if unlabelled) may have been misplaced through the carelessness of parties examining them, yet this specimen could scarcely have been introduced at a more recent period, because it [the species] is so extremely rare in England that I know of but four British examples—those in the Stephensian collection. But it will be said it does not agree with the Linnean description, which states that the legs are black, and the description must be correct; I am not so sure of that. That descriptions can and do err may be shown," &c.

* One main difficulty is that the collections undoubtedly contain some species not described by the authors; and in the case (at all events) of unlabelled specimens it is doubtful whether you have under examination an insect which the author has described or which he has added to his collection after the descriptions were published. It is unreasonable to conclude that Linné's collection contained no species but those he had described; and therefore unreasonable to attribute to Dr. J. E. Smith every difficulty which a ferreting ("*fureteur*," Dr. Albert Breyer) list-maker finds to baffle him.

† The Fabrician types are not spoken of very favourably by Mr. M'Lachlan (Ent. Ann. 1863, p. 155 et seq). Thus of *Neuronia signata* we read:—

"The type in the Banksian collection certainly does not belong to this genus; it is some small species so covered with fungus that *it is impossible to fix the genus*."

Again of *N. semifasciata*:—"This must be an error, as Fabricius's description applies to *reticulata*," &c. And we read further with reference to British Museum *Phryganidæ*, "type in bad condition," "in very bad condition," "type almost destroyed." See also Dr. Schiner's remarks on the types of Linné, Fabricius, Meigen, and Zetterstedt; 2 Wien. Ent. Monatsch. 54.

existence of types brings us to certainty upon the old names—which is the matter I am concerned with.

The difficulties of the old descriptions have been already* adverted to, and it seems unnecessary to enlarge upon them: we have only too much evidence, and no argument is needed to support it. I propose, indeed, to adduce very few instances. Dr. Staudinger's Catalogue and Mr. Kirby's Catalogue both include the European *Rhopalocera*. I refer to the short paper printed last in this pamphlet for the opinions which I formed after examination of a small part of these two works. If that paper be considered as coming in at this place, that will not greatly disturb the course of discussion, and will render more intelligible some of the remarks which follow. It will be seen that upon the *identification of names* these two authors differ to a degree almost incredible; quite incredible, I feel sure, by any person not well persuaded that the old descriptions are unrecognisable rubbish. If Dr. Staudinger and Mr. Kirby were the only Lepidopterological writers in the world (instead of being two in a crowd), entomologists would now be in very great perplexity on the true names of European *Rhopalocera*. As it is, (both the two works under consideration unsettling an existing nomenclature) it can scarcely be said that three-fourths of the European *Rhopalocera* have at this moment any trustworthy names at all; and it may become necessary to name large numbers of this group afresh by common consent, and send the innovating authors to oblivion, so hideous is the disorder which, by reason of their "revisions," has taken the place of our accepted nomenclature. If this be not done, then we must, as it seems to me, adopt the course already referred to,† for in the position there described (so far as many of the European *Rhopalocera* are concerned) WE ALREADY ARE.

The cases in which Mr. Kirby and Dr. Staudinger now print different names for the same species do not by any means make up the total number of cases in which those two authors are opposed. Mr. Kirby restricts himself to 1767, and restores no names of earlier date; while Dr. Staudinger starts from 1758. Now Mr. Kirby, who does not use them, cites a prodigious number of "prior" names (given in his Catalogue as synonyms), which Dr. Staudinger does not recognise! The results are not yet felt; because, though he finds and identifies the names, Mr. Kirby at present refuses to restore them. When he shall publish a List starting from the date 1758 or 1746, there will be a terrible addition to the number of cases in which he and Dr. Staudinger are dragging us different ways. All these forthcoming complications we are just in time to nip in the bud: they arise out of the old inexact descriptions, which I have explained as being the prime cause of learned confusion. To learn beforehand that such horrors are prepared for us must surely be to forearm us against them. Though the task is distasteful, it appears desirable to illustrate these remarks by instances.

* *Ante*, p. 13. † *Ante*, p. 12.

We will start our examination at the genus *Limenitis*. The English species of this genus has been named "Sibylla" and also "Camilla;" but in all recent works (including the two lists in question) it preserves the former name. Linné undoubtedly named the two sexes of our insect differently (♂ Sibylla, ♀ Camilla) in Syst. Nat. twelfth edition (1767). Both our authors agree that the Linnean "Camilla" (of Syst. Nat.) is the same species as his, and our, "Sibylla."

Dr. Staudinger acknowledges and restores names found in the Museum Ulricæ (1764); Mr. Kirby does not. If, therefore, "Sibylla" be found described in the Mus. Ulr. (1764) under the name *Camilla*, Staudinger will accept this name, but Kirby will call the butterfly *Sibylla* still. Now Kirby goes to the Mus. Ulr., and there he does find "Sibylla" described under the name *Camilla*. It is against his principle to take names earlier than 1767, so he does not change the name, but only quotes *Camilla* as a (prior) synonym. Staudinger, meanwhile, who would adopt the name *Camilla* from the Mus. Ulr. without hesitation, fails to recognise * the species there at all! The consequence is that he likewise (in ignorance, or by choice) retains *Sibylla* as the first name. Now, supposing Kirby to be accurate, it is quite clear that Staudinger ought to have rejected the name *Sibylla*, L. S. N. (1767), for *Camilla*, L. M. L. U. (1764). When Mr. Kirby publishes a list beginning from 1758 or earlier, he will have "Sibylla" under the name *Camilla*, and thus he and Dr. Staudinger will be openly at difference; they are now disagreed, though, under present conditions, the difference does no harm. It does not signify whether the former author be right, or the latter, or neither. The *disagreement* between them does the mischief; and, wide as that is now, it seems to be not nearly so wide as it will be when the works of both agree on their starting-point.

We are, however, only at the beginning of the chapter. Staudinger having now to adopt *Camilla* for the species "Sibylla," a new name must be found for the continental species "Camilla, W.-V.," which thus loses its old designation. The next name appears, according to Staudinger, to be *Lucilla*, Esp., so that would be his name for the continental "Camilla." *Rivularis*, Sc. (1763), does not represent *Camilla* according to Dr. Staudinger, as it is a fictitious species described from two others; while Mr. Kirby seems to consider that

* Die bekanntlich oft *dürftigen und vagen Beschreibungen und mangelhaften Bilder* der Patres entomologiæ haben Hrn. Werneburg nicht abgeschreckt, Bestimmungen auch bei *nur sehr geringen Anhaltspunkten* zu versuchen; &c.—Dr. Speyer, Stett. Ent. Zeitung, 1865-1866, p. 51.

Dr. Speyer is for drawing a line. He would have the priority rule to be "restricted" thus, "Linnean names have the preference over all others; if a species be included in Linné's works under more than one name, the last (given in the 12th edition of 'Systema Naturæ') put into currency by him, remains." Such a rule he says "würde wohl von keiner Seite her ernstlichen Widerspruch erfahren." Since this, however, Dr. Staudinger has published his denunciatory preface (noticed further on); and Dr. Speyer would probably now express himself differently.

the type of *Rivularis*, Sc., is the "Camilla, W.-V.," so that at this second stage the species now called "Camilla" would be *Rivularis*, Sc. (1763), Kirby; *Lucilla*, Esp. (1778), Staud. By this name, then, Dr. Staudinger would call it; *but* the name *Lucilla* was pre-occupied in the genus, W.-V. (1776) having thus named the *Neptis Lucilla* of continental collections. Now those who admit catalogue names do not allow *Lucilla*, W.-V. (1776), to be set aside, and therefore "Lucilla, Esp." (1778), could in their view not stand. If their opinion be correct the continental "Camilla" has, according to Staudinger's Catalogue, *no name at this moment;* and—holding (as I do) that a catalogue name, when thoroughly identified, cannot be rejected—I have proposed the name *Anonyma* for this species.*

I must not, however, allow myself to drop the thread of this absorbing narration. Staudinger being driven to give to the species "Camilla" the name *Lucilla*, Esp. (1771), he has next to find a name for his "Lucilla (W.-V.), Fabr. Mant. 1787." The next and only other names for this species are, according to Staudinger, *Camilla*, Esp., and *Rivularis*, Scop., both already disposed of. Now "Lucilla, W.-V." (Staud. Cat.), has *no other name* at the present moment, and I have named it *Innominata*,† thus donning the nomenclator's purple twice over in two minutes.

Our story now brings us back to Mr. Kirby, whom we left at the point where Dr. Staudinger took up *Lucilla*, Esp., as the name for "Camilla." Kirby would not allow Staudinger to take Lucilla, Esp., for "Camilla," and for this reason:—*Prorsa*, L. M. L. U. (1764) is, according to Kirby, the absolute first name of "Sibylla," coming one page before *Camilla*, L. M. L. U.: therefore "Sibylla" becomes not *Camilla*, but *Prorsa* (according to him); "Camilla" also becomes *Sibylla*, because Dru Drury (1773) misdescribed a variety of "Camilla" under that name, and "Camilla" was first considered a species in 1776! This is precisely the sort of result the list-makers are most fortunate in securing. One is shown over and over again in these Catalogues not only that Black is White, but, *en revanche*, that White is Black as well. Dr. Staudinger has "verified" all cases by "irrefragable proofs," and used "the greatest care." Mr. Kirby, who gives me no such assurance, produces an infinity of early names, which Dr. Staudinger ignores. As I am in fairness entitled to do, I use one author against the other. By that means I could produce in a month such a Synonymic List of European *Rhopalocera* as the world has not yet dreamed of. Only let the list-makers take care that I do not put this threat into practice. But to proceed, for there is much work before us.

Dr. Staudinger had to find a new name for "Lucilla, W.-V. Fabr. Mant.," and could only hit upon pre-occupied ones. Now, Mr. Kirby all this time recognises no such species as "*Lucilla*, W.-V." (1776), at all! That name is a mere synonym, he holds, of *N. Sappho*, Pall. (1771); but Dr. Staudinger has received "irrefragable proofs" that

* 'Zoologist,' 2nd ser., no. 80, p. 3074. † 'Zoologist,' *ubi suprà*.

Sappho, Pall., is no other than the name for Kirby's *N. Aceris*, Lep. (1774)! Running both authors to ground, we find they, nevertheless, both give *N. Aceris* as the first name! How is this accounted for? Why, the authority for *N. Aceris* is Lepechin's Reise, I. p. 203. Staudinger's date for this work is 1768–1770; Kirby's is 1774, which edition, says Dr. Staudinger, is only a later translation! If Dr. Staudinger be right, *Aceris* (1768–70) is prior to *Sappho* (1771); but, then, *if* Dr. Staudinger be wrong on this point, the name of *Aceris* (according to Staudinger's proofs) will be *Sappho*, Pall.; while Mr. Kirby—believing *Sappho*, Pall., is "Lucilla, W.-V."—will stand by *Aceris*—for the present.*

Solvuntur risu tabulæ. "Sibylla" is *Camilla;* "Camilla" is *Lucilla;* "Lucilla" is *Sappho*; "Sappho" is *Aceris.* "Camilla" is *Sibylla;* "Sibylla" is *Prorsa; Prorsa* is before the commencement of our nomenclature. The early nomenclature is an exhilarating study! There is not one of the books above quoted which was not already antiquated in the time of our grandfathers.

Any idle man may run riot through these Catalogues by merely showing up the deficiencies of one by the virtues of the other; and any ambitious man may sit down a real nomenclator after executing (on his own account) a mere trifle of "conscientious" revision. I ask nothing better than that anybody who takes the trouble to read thus far should go over the names I have mentioned in the two Catalogues. I have not used any work to aid me but Mr. Kirby's 'European Butterflies,' where I find, of course, the now superseded *Lucilla, Prorsa,* &c., given as the correct names for these species. The reader will not fail to satisfy himself that my claims to be a nomenclator† are at the lowest very arguable, and I hope he will

* Any Lepidopterists who use Mr. Kirby's List should have recourse to the different parts of Proc. Ent. Soc. before they put reliance on it, for the accomplished author has there commenced a running fire of missiles directed against the acceptance of *his own* nomenclature. In the holy warfare against accepted names he spares none.

A reference to Proc. 1871, p. xliv., will establish two points: *first*, Mr. Kirby has formally recanted his adhesion to 1767, and gone back at all events to 1764, how much farther does not there appear; *second*, he is very much disturbed in his mind about the true names of the two insects which I have since re-named. *Rem acu tetigi.* I arrived at the above conclusions without seeing Mr. Kirby's latest-published opinions. Whether he has formed some later yet it is unsafe to conjecture; but as those referred to were communicated as long ago as last December, it seems probable that he has.

† Both these new names of mine cannot stand in the estimation of the same person, because the first is arrived at by upholding a catalogue name, while the second is arrived at by dropping it. But it seems to me that one name or the other is assured of immortality, and I now have much pleasure in asserting the accuracy of *both.* I will remind entomologists that, according to Dr. Staudinger and according to Mr. Kirby, it is *compulsory* to accept a name bestowed by a writer who finds a species without a lawful designation, and imposes a proper name by proper publication. Dr. Staudinger dilates on the point; and both he and Mr. Kirby have done the very thing. That there may be no doubt about proper "publication," I have secured the appearance of the new names in a scientific journal.

improve his interleaved copy of either Catalogue by inserting at his leisure the following "revised" synonymy :—

Genus LIMENITIS.

Staud. Cat. p. 15 ; Kirby, Cat. p. 236.

ANONYMA, Lewis, 'Zoologist,' 2nd ser. 3074 (1872).
Camilla, W.-V., p. 182 (1776), sed Camilla, L. M. L. U. p. 304 (1764), erat Lim. Sibylla.
Camilla recentium auctorum.
Lucilla, Esp. 38, 2 (1778? post 1776), sed *Lucilla*, W.-V., alia erat sp.
Rivularis, Sc. Ent. Carn. 165 (1763), pro parte.

Genus NEPTIS.

Staud. Cat. p. 16 ; Kirby, Cat. pp. 239, 240.

INNOMINATA, Lewis, 'Zoologist,' 2nd ser. 3074 (1872).
Sappho, Pall. Reis. (1771), Kirby, Cat. 1871, p. 239 ; sed Sappho, Pall. Reis. erat Nept. Aceris auctorum.
Camilla, Esper. 59, f. 1 (1780), sed Camilla, L. M. L. U. p. 304 (1764) alia erat sp.
Lucilla, S.-V., p. 173 (1776), n. Cat.
Lucilla, F. Mant. 55 (1787), et recentium auctorum, sed Lucilla, Esp. (1778), alia erat sp.

Thus the number of cases in which these authors now differ upon the name does not at all represent the actual differences, which will appear when Mr. Kirby acts upon his conviction that 1767 cannot be defended. This *farrago* of disagreement at present lies concealed from those who do not search for it. *But for* the circumstance that Mr. Kirby had (when he wrote his Catalogue) refused to go behind 1767, we should now be in the thick of the contentions I have just exposed, and hundreds of similar ones on questions of the same importance. *If* "Camilla" be restored for our *Sibylla*, we have the whole avalanche upon us; and the ONLY WAY now to prevent "Camilla" being introduced (by somebody or other) for our *Sibylla* is to reject it as the disused name of the species.

If I have, by wading through this synonymy, entitled myself to characterise my own work and that of my predecessors, I can only apply to it a phrase lately well known in another connection. In my judgment it is, from first to last, pernicious nonsense; securing for its compilers (whether or not that be contemplated) plenty of work and some measure of consequence, without a corresponding liability to criticism,—and insuring, without one jot of compensating benefit, confusion and disgust to other entomologists. It does not concern anyone which of these two writers is correct in individual cases; most likely each has excellent reasons for every speculative conclusion. *Delirant reges, plectuntur Achivi.* While two authors are "building up a reputation," and being patted on the back by half the emulous small fry of science, they are providing for entomologists a

never-ending worry on all manner of infinitesimal points, which ought never to have been raised.

This view might have received illustration by reference to page after page of the two offending Catalogues. To expose every absurdity to which the restless industry of the list-makers has brought us, would involve an expenditure of time on the same work which I decry as worthless. Of such an inconsistency I have not been yet guilty; but I feel very much tempted to publish a really good Synonymic List, founded on the Catalogues of Dr. O. Staudinger and Mr. W. F. Kirby. If I do it—or if any earnest entomologist set some quick-witted stripling to do it for him—the list-makers' glory will have gone for good; and this single consideration nearly induces me to advertise my List forthwith. In my judgment "corrections of nomenclature," such as I am about once more to notice, bear to Entomology the same relation that dabbling in a puddle bears to an Atlantic voyage. I sincerely apologise for having any share in such work, but it is done with a good object. I cannot expect many entomologists to follow me into instances; and I can only confess I would not follow them if they were writing for me.

Colias Edusa is one of the best-known names in entomological nomenclature, and it has represented our Clouded Yellow butterfly ever since 1787. There is no doubt whatever that this name was given to our butterfly, and on that all persons are agreed. In 1785 a gentleman, presumably fond of children, named Fourcroy, published a pamphlet in order to amuse their minds, and chose for his subject the insects caught near Paris.* The Clouded Yellow butterfly is found there; and Mr. Fourcroy in 1785 thought *croceus* an expressive name, and taught it to his boys. Now, either Mr. Fourcroy had a very small sale, or his boys never thought enough of his teaching to make themselves into entomologists. It happened, therefore, that after the boys grew up they forgot all about *croceus*, and everyone has gone on very nicely in the same ignorance for *eighty-six years*. We are, I think, justified in assuming that none of Mr. Fourcroy's original subscribers are now active entomologists, and also that his book is not now consulted even by the little boys of Paris. No other book in the world contains the name. Fabricius gave the name *Edusa* two years only after Mr. Fourcroy wrote. As Fabricius was known and Mr. Fourcroy was not, *Edusa* was the name used; and no one of this generation, or the last either, has employed any other for the insect.

In 1871 Mr. W. F. Kirby publishes a big Catalogue, and he tells us to abandon the name *Edusa* of Fabricius and take up Mr. Four-

* "Fourcroy's book, in which the author does not even adopt the Linnean genera, is a very poor pamphlet, written without pretension, and exclusively intended for tyros and boys. Such a book has no scientific claim whatever; no greater claim than a mere catalogue, for it does not *describe* insects, but merely gives a notice of them in three or four words."—Dr. H. Schaum, Ent. Ann. 1860, p. 121.

croy's *croceus!* In 1871 Dr. Staudinger publishes *his* Catalogue, and does not recognise Mr. Fourcroy's name, nor Mr. Fourcroy either,—not even quoting him in his list of authors. So far so good; we are all content for this time to hang to Dr. Staudinger, and leave Mr. Fourcroy's new pupil to amuse himself with the pretty *croceus* (which we let him have all to himself). But it is a most unfortunate thing that the latter will not rest satisfied.

Another butterfly is known to all the world as *Colias Hyale*. The name was given to some species or other (of our *Colias*) in 1761, and by universal agreement has been identified with our Pale Clouded Yellow. No one of this generation, or the last either, has called the butterfly by any *other* name; and, in 1870, if this was not the name of the Pale Clouded Yellow, then that butterfly had no name at all,—a pretty good reason, some people would think, for letting well alone. Mr. Kirby was a good deal exercised early in 1870 about the unfortunate condition of this butterfly; but in 1871 his Catalogue (doubtfully) still gives *Hyale* as the name for it. In 1871 Dr. Staudinger's Catalogue (not doubtfully) also gives *Hyale* as the name for it. Dr. Staudinger had also got a curious butterfly (which he was disposed to consider a hybrid) caught on the Russian steppes, and this insect he writes down as a variety of *Hyale*, but with eight Latin words, or bits of words, as follows:—"al. ant. marg. post. lato nigro, ♂ satur. flavus;" and, considering this to be a distinct form, he gives to it a sort of nick-name, *Sareptensis*.

Mr. Kirby has now* persuaded himself that *Hyale* is our new friend *croceus* or, in other words, our old friend the Clouded Yellow; therefore the butterfly we call *Hyale* must have a new name. No one has ever professed to name it anew; and it is the fact that it has never had applied to it any name but another Linnean one, *Palæno*, given in error. Thus our butterfly, the *Hyale* of ninety years, or nearly so, has still to be named in December, 1871! Dr. Staudinger's funny insect out of Russia, which he nick-names *Sareptensis*, now becomes (according to Mr. Kirby) the TYPE of our *Hyale*,† because, says Mr. Kirby, that is the only other name for it; and, though it is a truly absurd one, "we cannot avoid adopting it." Can't we, indeed? I much doubt whether six men in Europe are so cracked as to follow this lead. We don't take an accidental variety (aberratio, Staudinger) for our type just yet, whatever list-makers may bring us to in time.

Now the ruthless author, in making this new one, most unhappily disposes of his other great discovery. Linné, he now says, knew our Clouded Yellow butterfly, and named it *Hyale* in 1761; so that we don't lose the name, but transfer it to another insect. But the new-found *croceus* goes back into oblivion after its brief months of garish daylight. Poor Mr. Fourcroy!

Now I, for one, cannot lecture over follies like these. *Edusa* and

* Proc. Ent. Soc. Lond. 1871, pp. xlv., xlvi.

† For the normal butterfly I propose the name of *Kirbyi*, VAR.

Hyale are the respective names of our two *Colias*, and such they will remain, long, long (I daresay) after Mr. Kirby has forgotten that he ever heard of *croceus*, or made that funny *Sareptensis* a species.

Let me now advert to the work of Dr. Staudinger. The English way is rather to provide for cases as they arise, than to restrict our choice of expedients by laying down principles at the commencement. The latter is, however, the German method. The inconveniences of it are that it allows an author no new means of escape from new and *unforeseen* difficulties; and he has no course but to stand by and persist in his principles, whatever absurdity they may lead him on to. The author becomes, in consequence, the slave of his principles. He must not, it seems, acknowledge that any person not proclaimed a numskull can withold his testimony to their merits. At all events, Dr. Staudinger's principles domineer over Dr. Staudinger. This is what his principles have driven him to write of those who do not agree with them:—

"To refuse to restore the old name on the pretext that that name is entirely unknown, and that its introduction is inconvenient, *would be the mark of an obtuse and, I might say, almost egotistical intelligence.*"*

This is a very pretty anathema.† Any person who holds contrary opinions is *ipso facto* a conceited noodle; and the learned writer has, no doubt, prepared himself to meet assailants as troublesome as that class of controversialists usually supplies. At all events, let us see what we can make of the opinions put forward by Modest Intelligence.

Dr. Staudinger's preface sets forth all his principles and explains their operation. He devotes a paragraph‡ to explanations of his leading canon:—"Every species ought absolutely to keep the name under which it was first described in a proper manner, upon condition that this name is in conformity with the rules established by the nomenclature of Linné." What follows admits many of the most essential facts on which the *communis error* argument is based. The passage might well have been written by Lacordaire himself, so completely does it serve the turn of those who resist the resurrectionists.

"There is a question of priority to establish first of all, but there

* Cat. 1871, pref. pp. xvi., xvii. Churchill has a couplet in point; but I trust

> "All don't agree upon the rule
> That folly's proved when he calls fool."

Dejean, Lacordaire, Silbermann, Delaharpe, Schaum, Newman, Breyer, Wallace! All self-assertive dullards and stupid coxcombs? It is a sad world.

† But it is more than matched by his previous declaration. "There will always be, as there have always been, authors who adopt other rules *contrary to all reason.*" (Id. pp. x., xi.)

‡ On pp. xvi., xvii.

is often very great difficulty in establishing clearly the species which the author has wished to indicate by the name given to the description or figure. There are names given by the old authors which belong to such and such species only by a sort of tradition. This tradition cannot, however, be accepted if the description or the figure is contradictory to the form of the species. It is not even possible always to remove analogous doubts by inspection of the original example in the preserved collections of the authors. It is unhappily a certain fact that the acquirer of the Linnean collection conceived the deplorable idea of sometimes replacing damaged examples by fresh, and it is nevertheless to the damaged examples alone that the descriptions of Linné can apply. It happens, on the other hand, that authors, after having created species afterwards mix up in their collections, together with the originals, species which are very near to them, and that their collections present numerous errors of this kind, even in their lifetime. It is therefore to be understood that *it will always be difficult to fix the names of certain species in an absolute manner;* and it has followed from this that distinguished modern authors have finished by interpreting certain descriptions in a manner altogether different from that of entomological tradition. It is this point which appears to me the most troublesome in connection with the absolute fixation of the names. For my part I have always verified these old names and their interpretation with the greatest care, and I have only accepted them on proofs which have appeared to me irrefragable; but *I am far from pretending to have disposed of these questions in an absolute manner.*"

He goes on to state other difficulties in the way of securing certainty by means of his rules. Thus, the dates of many works are not known; and the dates of others are wrongly given. Then, the date of actual publication has to be ascertained in cases where a paper was read before a learned body; and so on.

Dr. Staudinger says nothing in support of his application of the priority principle; he does not even refer to any work whose opinions concerning it he endorses. Why not? Will anyone say that is because there is an agreement in the principle; and it is everywhere understood? What do I read in one of the chief guides (perhaps the chief guide) to entomological nomenclature? "The principle of preserving the oldest of the names given to the same insect is not absolute; the choice between them, following the greater or less degree of *convenience*, remains free." Dr. Staudinger and Dresden are not strangers; and this is the 15th rule of the famous Dresden Congress of 1858. Perhaps, no man being a prophet in his own country, Dr. Staudinger returns the compliment and takes no account of the Dresden Congress; but I need scarcely say more to show that the absolute application of the law of priority is not generally agreed on in Europe. I will refer, however, to the rules formulated by a French entomologist, M. Amyot,* among them being that "usage

* In Ann. Soc. Ent. Fr., 3rd ser., vol. vii. p. 606.

may consecrate injustices in the priority of names" as well as faults in their composition. That the application of the law is disputed by English entomologists is not in danger of being overlooked. Dr. Staudinger* makes profession of "justifying himself" by expounding his principles. He even says that he knows beforehand that he will be accused of uselessly applying them; and yet of his application of the main principle (without agreement on which his catalogue is mere waste paper) he writes only what I have quoted. It must be confessed that this is a bad beginning.

The passage above copied is a confession that the resurrection practices do not give us certainty; in other words, do not give us the one thing which they are supposed to give us. I adopt what Dr. Staudinger has written,† and call him as a skilled witness on my behalf. The wonderful thing is that (with all this confession) Dr. Staudinger yet hardens his heart against those who would willingly receive him into their friendly embraces. For, strange as it must appear, the very next sentence which follows is the one already quoted, denouncing the refusal to take a name so curiously hit upon as the mark of an egotistical person! I am denounced as "obtuse" and "egotistical" because I have thought out for myself the very results which Dr. Staudinger admits occur; and because I act upon the conclusion! Those who take Dr. Staudinger's data as a way to arrive at his conclusions are more truly the "obtuse." They lack the refined perception which would show them that data have nothing to do with great principles. Only I admire the audacity of our author. Dr. Staudinger first lays down a conclusion in the teeth of all his reasons; and then derides those who do not see the affair with his eyes. This is not egotism. I am glad that I am egotistic; for never before was it made plain to me what I escaped being when nature forebore to make me modest.

Pray let us apply our minds to this subject, meaning to master it for ourselves, and with our own wits. Suppose that Dr. Staudinger in the course of his labours discovers a score of old names all agreed upon by everybody as indicating a certain score of well-known species. He sets about to test the accuracy of that agreement by his method.

* Pref. pp. x., xi.

† I am equally pleased to acknowledge the corroboration which my arguments receive at this stage from Baron von Harold. These two authors (as shown on p. 11) really find a great part of my arguments for me. Baron von Harold (Col. Hefte, vi. p. 50) writes:—

"A description absolutely sufficient, availing for all time, I hold generally, in the greatest number of cases, to be an impossibility; for one can never know beforehand what character or what individual distinction we shall possibly in the future depend on for distinguishing, out of some closely-related species, one which first makes its appearance later, or which we do not know in nature, but only from another's description. Anyone who has occupied himself with drawing up analytical tables knows right well that with the assorting of species it often depends on this or that character, which in the best descriptions frequently remains unnoticed, so that such a species cannot be classified at all."

What is his method? I have above set out Dr. Staudinger's whole explanation; and the reader is in full possession of it. He "verifies the old names with the greatest care," and "accepts them only on proofs which appear to him irrefragable," but he is "far from pretending to have disposed of these questions in an absolute manner"! What does this mean? Was there ever a more fugitive explanation? We are to understand, it seems, that Dr. Staudinger has exercised a discretion, sometimes strictly applying his principles and sometimes not; or that he has given the balance an inclination, and not been too rigorously just! Is not this mere trifling with the settlement of an important question? What does an author mean by telling us that he has not applied his rules in an absolute manner? Is it a proviso intended to cover errors hereafter to be detected? Next, what are his "irrefragable proofs"? We are left utterly in the dark. He seems to say tradition will do, *unless* the description or figure contradict it. Has tradition then been adopted by Dr. Staudinger sometimes as "irrefragable proof"? He says it is not possible *always* to remove doubts by inspection of the examples preserved. Has then Dr. Staudinger removed his doubts by this means sometimes? How has the careful verification been conducted? In short, what have been the means of identification? It is impossible to say how these questions would be answered. Of the twenty names with which we started, half may be rejected and half preserved—for all we know, on *opposite* grounds. Dr. Staudinger mystifies instead of clearing up the subject; and provides no explanation of the *modus operandi* at all. It is of importance to know these things, and I do not make the demand for the sake of cavilling. The facts one way or another whether an author has accepted mere tradition, or has been guided by type specimens, may for a large number of his readers decide for good or bad the value of his work. The greatest point of all is to know what names are decided on by one means and what by another: without this knowledge we are blindfold, and the author leads us where he wills.

We are thus without a plain statement of the method adopted. Two or three methods are suggested which may or may not have been used to aid in revolutionising our nomenclature; but we are not told whether they *were* used. The work, therefore, is proof against definite criticism; for it is impossible to contend on fair terms with an author so reticent. General expressions of "the greatest care" having been taken and "irrefragable proofs" required should put us on our guard against him. *Dolosus versatur in generalibus*, and you cannot fix him. Yet with only this worse than useless explanation of it, it is remarkable with what cool superiority Dr. Staudinger mentions his work. He says:*—

"It is necessary that all should accept these changes on condition that the names displaced are the result of errors demonstrated and that the new names are legitimate. The changes are the only means

* On pp. x., xi.

of giving us fixity in the names. We have only applied known laws in a rigorous manner and consistently; and we are sure beforehand of being exposed to many criticisms. It was the proper course in these cases of two evils to choose the least; and inattackable names have appeared to us preferable in the interests of the natural sciences, whether or not those names clash or not with habits, and the rules of philology."

Yes; but, putting aside for the present the interests of natural science (taken into consideration elsewhere), let us keep to Dr. Staudinger himself, and see whether he has put himself in a position to use this language. It seems to me that he has not; and that such gratulations as these are in his mouth out of place. Dr. Staudinger rests his claim to be considered a benefactor to science upon the fixity which nomenclature derives from his strict application of known rules. But, when he comes to the point, he not only draws off from the strict application, but actually leaves us in the dark upon the rules themselves. Dr. Staudinger has used his rules (whatever they may be) to unsettle the names we are agreed upon; but when he wishes to use them in order to give us names which shall be the right ones, his rules appear to fail him. Then what does he do upon this? To my mind his work nearly resembles the capricious production of some absolute authority. He does not stay his hand upon discovering that he cannot apply his rules. Superseding the names we use, he brings forward *other* names—arrived at he does not say how; which he does not say are by rule the right names; but as to which he does say that they are not arrived at in the only way he has any right to arrive at them. And all this is under the pretence of giving us finality! Dr. Staudinger surely cannot believe that he fulfils his own conditions. No one would be found so silly as to say that a batch of names arrived at by "not determining questions in an absolute manner" secures finality. Then have Dr. Staudinger's labours brought finality? To those who are personal believers in Dr. Staudinger, it is probably enough that the trusted author has "verified" and required "irrefragable proofs." That cannot be enough for entomologists at large. Many of them, I think, would consider they were stultifying themselves to adopt (in the confidence that they are final) names determined on not by rules, not by agreement,—but by suspending rules and in the teeth of adverse agreement. I am content to let the names "restored" in Dr. Staudinger's Catalogue be judged by the author's own declarations. It is necessary, writes Dr. Staudinger, to accept the changes "*on condition that the new names are legitimate.* The changes are the only means of giving us fixity in the names." Well, the new names of Dr. Staudinger are not legitimate, and do not assume to provide fixity. Let us wait at all events until Dr. Staudinger does absolutely judge the questions. To adopt these new names now is only to learn what in a few years we shall receive orders to forget again. So plain a declaration that the accuracy of these names is an open question will certainly be

followed by an attempt to close it, made either by Dr. Staudinger or some one else. The questions (we are fairly told) have not yet come to be decided; we are now virtually asked to adopt the names *ad interim*.

In the last preceding remarks, I have been crediting to Dr. Staudinger that his method of dealing is at present universally approved. It is, however, on the contrary, peculiar to one section of entomologists. Let us, for instance, note the differences between his plan and that of Gemminger and von Harold. Dr. Staudinger professes to require "irrefragable proofs" of the identity of a name with a species. The list-makers just mentioned regard the difference between a sufficient and an insufficient description as "incapable of being carried out." They accept not only traditions and type-specimens (about which Dr. Staudinger's utterances are so strange), but they go further and accept as conclusive a tradition concerning a type-specimen! Lastly, they avow their determination to uphold the Fabrician names at any cost, where any "*plausible*" identification of them has been suggested. It may be thought that by comparison with Gemminger and von Harold, Dr. Staudinger does his spiriting gently. In my view, however, the unsettling of an accepted nomenclature on principles not communicated, and applied capriciously, demands our most vigorous opposition; whilst authors who plainly declare that they *will* save the early names from oblivion are (without intending disrespect) well-meaning enthusiasts, and not the objects of serious blame.

The following passage gives in a compendious form the opinions of Gemminger and von Harold:—

"There arise by various real or only assumed gradations, into which these specific names are divided as sufficiently or insufficiently described (as varieties or collective conceptions), very complicated combinations, which have led to the most various interpretations. . . . . In by far the great majority of cases it is the pretence of the insufficiency of the older descriptions or the uncertainty in the meaning that seeks to do away with the oldest names and to introduce newer ones in their place. Since, for us, a decision between sufficient and insufficient description, as already stated, seemed practically incapable of being carried out, we have, in all cases when the identity of such an alleged badly described species with a newer better described one was shown, preferred the older name and introduced the younger one in its synonymy."

Then, after speaking of recourse to type-specimens:—

"How little decisive this last circumstance in itself is, is proved by the examination (almost always necessary in case of monographs) of the typical examples, even if they were described by the most approved authors of modern times. . . . . Such an identity as that above mentioned is not always placed beyond doubt, but in very many cases it rests on an assumption based entirely upon grounds of plausibility. In this position we find ourselves, especially, for

example, with regard to many names of Linné, Fabricius, and Scopoli, whose types have been lost or so ill-treated by time that they give no information. Here we have, if at all possible, unconditionally accepted explanations once proposed, especially if they have already been naturalised. Thus with us *Aphodius conflagratus*, Oliv., is brought forward as *A. scybalarius*, Fabr., although this view, defended by Erichson, rests only on probability, and the last-named species may well be *A. rufescens*. Here the object was not to allow the Fabrician name to be lost;* any plausible explanation of it commended itself to our acceptance, and if such an explanation was once given the question as to the genuine *A. scybalarius* appeared to us only more irrelevant in the interests of stability."†

There is great *naïveté* throughout this passage. Messrs. Gemminger and von Harold's position may be expressed as follows:—"The old descriptions are such that to talk of some species as sufficiently and others insufficiently described is to draw an unreal distinction. The same descriptions have led to the most various interpretations. The identity of many of the names of our oldest authors is accepted on nothing more than plausible grounds; and to prove the identity is not possible. Therefore we accept plausible grounds, and also accept unconditionally grounds that have appeared plausible to any predecessor. If this be not our stand-point, it will be impossible to use the old names; and use them we must and will." This is both the spirit and substance of the passage I have quoted; and supplies the key to Messrs. Gemminger and von Harold's synonymic work.

The authors thus proceed on one principle; and to it they make everything subservient. The oldest names they say *must* be utilised; and if the only way to do that be to accept guess-work in lieu of proof, then guess-work they accept. If this be not enough, then hearsay must come in; and if no hearsay satisfies them, then recourse must be had to the guesses or hearsay which have satisfied some previous writer. The authors exhibit such a single devotion, that it would be evident this principle is a child of their own, even though it were not the fact that they are the only declared supporters of it.

The consequence of an author making all his work subservient to one absolute principle of course is that, unless that principle be accepted, the whole work is useless. Now suppose that it appears not at all necessary to discover or accept the dubious names of the

* To obtain an idea of the position in which this principle would land the Lepidopterists, consult M. Guenée (Noct. vol. 1, p. lix.):—

"The greater part of the Fabrician species do not possess the character of their section, and the *Noctuæ* which he has described are in truth heaped up without any order and without any correlation between them. . . . . *We are obliged, in fact, to neglect a crowd of species which Fabricius created and named* in visiting the different European cabinets, *because, after all the attention possible, we finish almost always with an uncertainty* or with finding over again a *Noctua* already given under other names."

† Catalogus Coleopt., Einleitung, pp. x., xi.

oldest authors. Suppose that entomologists decline to be thrown adrift among the "various interpretations" and "complicated combinations" for the sake of a mere doubt. Suppose (above all) that we will have nothing to do with "grounds of plausibility"; and decline altogether to give up the names we have, for any piece of guess-work new or old. What becomes then of the whole fabric of Messrs. Gemminger and von Harold's dissertation? Is it not the case that their argument does proceed on a supposition entirely baseless? Entomologists have never at any time decided that it is necessary to find species for all the earliest names. They have never at any time agreed to accept guesses or hearsay with that object; much less to accept (as a new light) the guesses or hearsay which have satisfied a preceding author, but which, *ex hypothesi*, they themselves have previously ignored. I will copy here the words of another Coleopterist, whose opinion Messrs. Gemminger and von Harold respect.* Dr. Schaum, in a paper on the nomenclature of British *Carabidæ*,† writes:—

"I am much opposed to the adoption of these obsolete names, substituted for the well-known and generally adopted appellations in right of priority. Such a right can be admitted only when it can be proved *to evidence*, that the species in question were indeed those described by the old authors. . . . . If we cultivate Entomology for the sake of knowledge, and not for the sake of nomenclature, I can see no benefit arising from an inquiry into the data of the synonyms compiled (and very often erroneously compiled) by Schönherr, but, on the contrary, a waste of time, which can be better employed in exact observations. What we want for the sake of knowledge is *stability* and *uniformity* of nomenclature, not an upsetting of it by the substitution of old forgotten and *very doubtful* names published in works without or with very little scientific merit. . . . . We are at least bound when we overthrow a universally adopted name to furnish evident proofs from the descriptions of the authors, that they had *really* the species in question and *only that* before them; we must not dare to rely on tradition alone, which is always subject to doubt and critisism."

This extract is worth a whole battery of new arguments. Coleopterists, then, do not accept the principle of finding species for the old names at any cost. There are some who spurn the "grounds of plausibility," and they refuse to "upset nomenclature by the substitution of old, forgotten, and very doubtful names," for anything short of *evidence*. This position needs no more proof; but it is useful to quote the opinion of an English Coleopterist. Mr. E. W. Janson‡ had already written:—

"It is not to be supposed that the entomologists of the continent will consent to the banishment of names familar to them as household

* Notwithstanding the soreness which the latter still displays. See Col. Hefte, vi. p. 50.

† Ent. Ann. 1860, pp. 121, 122.

‡ Ent. Ann. 1859, p. 119.

words, and embalmed in the laborious and conscientious works of Gyllenhal, Erichson, and a host of worthies too numerous to mention, *unless full and unquestionable evidence is adduced* of our right to substitute for them names equally cherished by us,—precious legacies bequeathed to us in the writings of Kirby, Spence," &c.

It is since the publication of those opinions that Gemminger and von Harold have promulgated their principle. It is unnecessary to set out here a catalogue of the evils attending on attempts to restore the old names by the method of these authors; because I have been repeating them, and shall repeat them, in one form and another in arguments extending over many pages. In substance the matters alleged are:—that avowedly those names are not certain; that they cannot be made certain, do all we can; that they are strange to all now living, and that there is no sort of need to restore them; that we shall never be all agreed on them while the world lasts; that to restore them magnifies confusion where that exists already, and introduces it where now there is none; that the discussion of them occupies our minds with other men's blunders, instead of with the works of nature; and that such studies are no true part of Entomology at all. Such I conceive to be, shortly and simply stated, the objections to the resurrection practices. Of these practices Baron von Harold is perhaps the foremost and most doughty champion. I present him with the case which we make out against him. I concede that Baron von Harold is a good fighter. In his paper on nomenclature (in 'Coleopterologische Hefte'), this author shows here and there that he knows how to harry his opponents,—sometimes certainly at the sacrifice of strict justice. But I am happy to know he has said nothing at all that reaches a feather's weight concerning the subject of *this* controversy. This is something above and beyond all his disquisitions upon *modus operandi*. No amount of inconsistencies (such as Baron von Harold has made a point of parading), if committed by those who share my opinion, could diminish by a single iota the one solid overwhelming argument, that the resurrection practices cause confusion, and do not dissipate it. No amount of reasoning that the principle is good or just will ever wipe out the proved fact that its operation is disastrous; no amount of scientific authority, in those who spend their time on this work, will prevent the reflection that the work is *not trusted to when done*. The first thing after a Catalogue has appeared is for its author's friends to pick a hundred holes in its nomenclature; and each instalment of finality is thus made the direct means of showing that the work produces no such thing. The reason is that the names *are* restored from guesses and from hearsay. Certainty is a thing which it is not possible to refute; and GUESSES and HEARSAY will be open to refutation as long as the world endures.

The changes which Gemminger and von Harold introduce are, these authors tell us, brought about by leaving proof for guess-work, and guess-work for hearsay, or, in default, for the guess-work or

hearsay of others. The result which they aim at is to secure for as many species as possible a name taken from the earliest author, although (they say) that name may be in reality the name of a different insect, and always (on account of its want of identification) open to "different interpretations." It is by these means, then, that they intend to bring order out of the "chaos of arbitrariness, confusion, and blunders." Is my language disrespectful when I repeat that these authors are well-meaning enthusiasts? If so, I confine myself to quoting Mr. H. T. Stainton. "Unfortunately the striving for infinite perfection is so intense amongst many of our German contemporaries, that they discard useful stepping-stones in *the vain attempt* to arrive at some firmer and more solid foundation which shall endure for ever."* In the judgment of many, Messrs. Gemminger and von Harold's work (as described by themselves) will have only these results:—"blunders" crystallised and made perpetual, and "confusion" worse confounded; while "arbitrariness" is confessedly a dominant and necessary feature.

Baron von Harold has as many protests against other people as though his own conclusions would bear reasoning upon, his dissertation being (as usual) made the excuse for little cuts at the unbelieving crowd. Thus:—

"From the outset I must protest against the objection that the interests of the stability of our nomenclature demand the cessation of all disagreeable corrections. . . . The conservative retention of what is erroneous and incorrect in the domain of Entomology is as rotten as in that of politics or of the other sciences."†

Now Baron von Harold does no more than publish this protest. I, for one, make "the objection that the interests of the stability of our nomenclature demand the cessation" of these corrections. I also note the incidental admission that Baron von Harold has knowledge of the objection, while he says nothing to combat it, and confines his notice of it to a protest. This is not the day when an author's protest will suffice for us in place of his reasons. Entomologists who decline to surrender certainty in exchange for the old names fished out by Baron von Harold are next charged with "the conservative retention of what is erroneous and incorrect." This, I must say, is a feeble accusation. Granted, for the sake of argument, that we do retain what is incorrect; still the charge (to be worth anything) must mean that we retain this, when it is in our power to have accuracy instead. Is that in our power? What is it, pray, that is to be given to us when we have made this surrender? Only the oldest names arrived at by the process we have had explained to us? Does Baron von Harold presume to call the use of such names *accuracy?* While there remain any old names not identified, all names later in date have (in his view) an insecure title. The name is only certain when it is so old that there are no unidentified names behind it! Therefore the use of an old name for an insect doubtfully identified

* Ent. Ann. 1859, pref. iv.

† Col. Hefte, vi. pp. 37, 38.

with it *is* accuracy, and any question as to what insect the name really refers to is "irrelevant in the interests of stability"! This is a plain statement of this author's opinion, and not in any respect a travestie. We hold, then, "the incorrect," if Baron von Harold pleases to think so. But to imply that in refusing to abide by his speculative guesses we also reject the *accurate*, is a ludicrous charge; and as such I leave it. Baron von Harold also writes:*—

"In by far the great majority of cases it is the pretence (Vorwand) of the insufficiency of the older descriptions or the uncertainty in their meaning that seeks to do away with the oldest names, and to introduce newer ones in their place."

The allegation that the old descriptions are insufficient is a "pretence"? The uncertainty in their meaning is a "pretence"? Do the same insufficiency and uncertainty, which to Baron von Harold are a scientific fact, grow then into a pretence when made use of in argument by his opponents? This insufficiency and this uncertainty are made to justify Baron von Harold in his recourse to guesses and hearsay. This it is which is made the very foundation of his remarks about "plausibility" and "naturalisation." Those who share his opinion on the facts—and only differ from him concerning the proposals, which he bases upon them—are making use of a pretence; therefore it is superfluous to ask what Baron von Harold makes use of. Men impressed with the sovereign importance of some one principle are often, I suppose, unjust to opponents.

I have now almost done with Baron von Harold. Since a decision between sufficient and insufficient descriptions is not possible, he has, "in all cases when the identity of such an alleged badly-described species with a newer better-described one *was shown*, preferred the older name, and introduced the younger one in its synonymy." Now, here I am thrown back on the same ground where Dr. Staudinger placed me. The clause, "when the identity was shown," begs the question, and concludes all criticism. By what means, may I ask, are we to understand that identity *is* shown? It is not, I trust, wasting time to insist once more that writing of this kind will not do. In that one clause there lies matter enough for pages upon pages of argument and explanation; and when the object is nothing less than to revolutionise our nomenclature, that argument and explanation *must* be forthcoming. Anyone professing to be a man of science must, if he want his work to be permanent, supply us with the fullest means of testing his conclusions. When, on the other hand, the acceptance of them is left to depend on personal confidence in the author, that man is doing nothing towards the true settlement of any question which he touches.

An endeavour has been made in one (not important) quarter to fix upon me the stigma of proposing to glorify popular mistakes. It is necessary to explain that *communis error* means a "universal" (*not* a "vulgar") error. But Baron von Harold justifies the proposal to

* Catal. Coleopt., *ubi suprà*.

perpetuate error, in cases where that may be neither universal nor general. He will "especially" not disturb any "naturalised" explanation of a description. When any plausible explanation has once been given, he will adopt that, although at the time of adopting it he be satisfied that the identification is arbitrary or a guess, and that other given identifications are equally plausible. He winds up by declaring that the question, whether the species bearing the name be or be not really the species intended, is *irrelevant*, and he declines to entertain it! This is, at least, explicit. Fortunately, we have already seen that Baron von Harold has opponents.

There is one suggestion for the termination of differences which deserves notice. Dr. Thorell* says:—

"The species of the older writers are, as is well known, often difficult, sometimes impossible, to determine with certainty. With respect to them I have, in applicable cases, laid it down as a rule to preserve the determinations accepted by modern arachnologists *who have lived in the country where the species described by the author in question have been collected.* It is evident that a French naturalist has the best opportunities for studying the French spiders, &c.... as, also, we Swedes ought to be best acquainted with the Swedish forms described by Clerck, Linné, and De Geer. Tradition has here a significancy that must not be undervalued. It is only in cases in which I have supposed myself able to show that an evident mistake has been made, that I have deviated from this rule."

It strikes one that Dr. Thorell has rather obscured the sense of this passage by bringing in a reference to "tradition," besides raising the spirit of opposition in those who will not have tradition at any price. On this head I can only repeat that the doctrine about a scrap of Latin effecting the salvation of the name accompanying, is a mere salve to conscience, and that tradition must either be good for everything or good for naught. Those who adopt a score of names, identifying them solely by the light of "tradition," and who refuse by the same light to identify a score of others, are only inconsistent, and not methodical, as they probably suppose.

Dr. Thorell has made it a rule to accept the interpretations of local entomologists, in cases where the old authors named are not clear. It is impossible not to recognise in this a very intelligent proposal; and I wish that I could feel any assurance that the rule is workable,—or was persuaded, indeed, that the reasons given justify it. It is, in strictness, enough to mention the "rule" as one more element of disagreement; for here we again come upon a principle peculiar to one author, and not assented to by others. But, in truth, I think Dr. Thorell's careful writing deserves that all who find themselves with the opportunity should explain their opinions upon it.

The objection to this "rule" consists, to my mind, in the word, *Why?* Why are the Swedish entomologists best able to determine on the Swedish insects described by Linné? Only because (I assume)

* 'European Spiders,' p. 15.

they know what insects are or are not likely to be intended. But, how is that the case more with them than with others? The author's reasoning properly carries us thus far, that *putting out of mind all species not indigenous* to the country, the number from which the identification has to be made is reduced. No insects shall be held to be intended, by 'Fauna Suecica' descriptions, but such as are now found in Sweden.* But the knowledge, whether or not a given insect is found in Sweden, is not confined to Swedes. The basis once agreed on, an English or German entomologist is, it seems to me, as likely to make the right decision as a Swede; and the mere fact that he, like the insect, is an aboriginal, is removed from the question. That the "rule" is not workable for others, would, I suppose, be conceded. The list-makers of Germany, for instance, would have stayed their hands long ago, if they had ever intended to be checked by reflections that other people had done their work better. Would the least opinionated "reformer" keep back his new identification of a Linnean name because the Swedish entomologists differed from him?

But, after all, Dr. Thorell's rule does not go all the way. He does not suggest that an author's fellow-countrymen are his best interpreters. He only contends that Swedes ought to know best what *Swedish* insect was intended by the old Swedish authors. Now, in how many cases do we know that Linné, for instance, was describing a Swedish insect? Putting aside all the species comprised in 'Fauna Suecica,' are we not even entitled to assume that the others described in 'Systema Naturæ' were at the time *not* Swedish, to the author's knowledge? It seems, at the first blush, that this conclusion must be drawn. Then where does Dr. Thorell's rule come in? It is clear that on this ground we cannot use that rule for 'Systema Naturæ' descriptions, and its operations would, therefore, be very limited, so far as Linné is concerned; while for interpretation of the European Faunas of Esper, Borkhausen, and others, it is of no use at all.

If this rule be meant to inculcate that a Swede can always find a species for every 'Fauna Suecica' name,—and it goes, at all events, some length in that direction,—it ought to meet with uncompromising opposition. A more certain road than this to the resurrection of *unrecognisable* names there could not be. Moreover, the attitude of the different obscure authors' fellow-countrymen would soon grow to be highly defiant, and they would brook no contradiction of their identifications, however wild. I look with much dread to any extension of this patriotic element in science. As it is, nobody dares criticise York Minster to anyone born within the county; and when "Linné lived in Sweden, and so do I," counts for an entomological argument, we shall arrive at a pretty pass. I by no means wish to imply that Dr. Thorell, or any Swede, would be capable of adopting that tone.

I have mentioned† another cause of difference among authors, viz., the frequent occurrence in the old books of "a name which, when

* See Baron von Harold, Col. Hefte, vi. p. 50.

† *Ante*, p. 17.

first bestowed was identical with that of another species in the same genus."

The old genera were very different things from the genera of this age. All the Butterflies, for instance, were till 1801 included in the genus *Papilio* (the divisions with which Scopoli amused himself not being acknowledged). Now before the year 1801 it was very much the fashion to take specific names from the roll of demi-gods, or persons who flourished in the heroic age.* This list being limited, and the authors having their predilections, it happened that the names of some popular personages were borrowed more than once. *Ulysses* with round wings and *Ulysses* a swallow-tail, when both had the generic name *Papilio*, were not by name identified sufficiently; nay, they were actually confounded. Hence the rule that no insect shall bear the same name as another in the same genus; and it is a regulation that such a name if given must be changed for the next name, or for a new one. However, the old entomologists (though often fanciful on paper) were in practice no great sticklers, and two or more species in the large genera not rarely received and bore the same name. Now arise the "reformers," and as their laws are all retrospective as well as prospective, some assume the *right* to alter every specific name which when bestowed was identical with another in the genus. From the previous sketch,† the consequences to our nomenclature of such an interference can be imagined. To the honour of Dr. Staudinger be it said that he curbs himself here to an extent which is really remarkable. The large genera of Linné (in the *Lepidoptera* at least) are all now minutely subdivided; and a species may now find itself far removed from its namesake. Dr. Staudinger will not‡ alter the later name where the insect has not received any new name during the time when the two species *were* included in the same genus. Thus were *Ulysses* to have continued the only name both for the swallow-tail and the other

* Not, as is now the practice, from the Post-Office Directory. See the 'Zoologist' for January, 1872, p. 2897, for some remarks on the fifteen *Boisduvalii*, and fourteen *Hewitsonii* (!!) of our present nomenclature. Upwards of one-fifth of the British *Tortrices* are named after some man. Some names of this kind were, however, given by Linné and Fabricius; and the sort of "immortality" thus conferred is shown to be not very extensive. Thus the authors of the 'Accentuated List,' in their shots at the meaning of names, have been regularly puzzled by *Grotiana* and *Dipoltana*. The former they go so far as to say is "probably in honour of *an entomologist of the name of Grote;*" but, not choosing to risk so much as a guess at the root of *Dipoltana*, they suggest that *that* name "is probably in honour of *some naturalist*"!

If I ever have the luck to catch a new anything, I shall certainly name it after the dealer who made my collecting-box; and, as I have in use a number of different ones, it will be exciting when the time comes to decide whose "best cork-line ditto" shall receive the captive. Something, however, depends on the thing's genus; *Cookiensis* may best suit one generic name, *Gardnerosa* another.

† *Ante*, pp. 13-15.

‡ Cat. 1871, pref. pp. xviii.-xxi.

until the two found themselves in separate genera, Dr. Staudinger lets *Ulysses* stand for both. But the rule works untold mischief even when thus restricted.

The old genus *Phalæna* or *Geometra* was not touched till Dr. Leach began to subdivide it; and his first genus was instituted only in 1814. Let us watch the operation of this rule in the case of one *Geometra*, viz. *Asthena sylvata*.

Donovan, in 1810, figured our *sylvata* under the name *testaceata;* and Dr. Staudinger found that out in 1871. The trifling circumstance that the moth had been called *sylvata* for about a century before (1776 to 1871) and that Donovan's name has not been copied by any single author in the world, does not touch Dr. Staudinger, of course. The tempting feat is to force Donovan's name on a reluctant public, and by the help of this rule Dr. Staudinger does the trick.

The name *sylvata*, W.-V. (1776) is first disposed of in the usual funny way: "n. Cat." writes Dr. Staudinger against the universally employed *sylvata*, and thus makes a promising start. Hübner, however, in or about 1800 likewise gave the insect as *sylvata*, so, unless that name also be superseded, Donovan's name cannot come in. Now the list-maker invokes his rule; and his rule serves him nobly. *Sylvata*, Hübner (1800) is shunted because Scopoli (in 1763—which is before our nomenclature begins, according to some*) thus named another *Geometra*, viz. *Abraxas ulmata* of our collections. Here is another case for "reformers" to rejoice over. Neither *sylvata*, Scop., nor *testaceata*, Don., has been used by any single writer since the day when it was published. Both *sylvata*, W.-V., and *ulmata*, Fabr., are in universal employ and have been for about a century.

Mr. Kirby would preserve both these names; at least it appears that he does not recognise Dr. Staudinger's rule. Mr. Kirby gives no statement of his rules, and I have not yet been at the pains to go all through his Catalogue in order to tabulate them for myself. But I will take an instance which shows (if it show nothing else) that Mr. Kirby is once more at difference with Dr. Staudinger. *Charaxes Jasius*, a species once reputed to be caught in Ireland, was named by Linné in 1767. So also was *Papilio Jason;* but the latter name was first published in 1764. Linné† gives the former insect under the name *Jason*, but corrects that into *Jasius* in his Addenda to the same volume. Mr. Kirby, for some reason,—which it is useless to inquire, because Mr. Kirby does not state it,—ignores this correction and gives the butterfly under the name *Jason*.

Therefore (according to Kirby) we had in 1767 two species *Jason*,

* The British Association are no longer of this number, because, by a set of revised rules,—which have been kept dark, and are neither known nor followed,—they ridiculously make an exception from the date of 1767 of the works of Artedi and Scopoli, published earlier! This extraordinary provision bears out what has been said (*ante*, p. 8) that "the tendency seems to be to shift the commencement as far back as possible;" but it certainly answers to Mr. Wallace's description of it, viz., an "illogical compromise."

† In Syst. Nat., 12th edition, p. 749.

both in the genus *Papilio*, at the same time. They are now in separate genera; *Jason* (1764) remains in *Papilio; Jason* (1767) belongs to *Nymphalis* of Kirby's Catalogue or *Charaxes* of other people. If, before this separation took place, the later (*Charaxes*) *Jason* had received another name, that later name must come in—according to Staudinger's rule.

Hübner did name this *Jason* afresh in 1794; for in that year he figured the butterfly as *Papilio Rhea*. The genus *Papilio* was not subdivided till seven years later. Therefore the rule applies; and *Nymphalis Jason* (Kirby) ought to be *Nymphalis Rhea*. Mr. Kirby leaves both insects under the name *Jason:* I conclude therefore that Dr. Staudinger's rule is not his.

Now let us look at Dr. Staudinger, who says he does adhere to this rule. Why, there at p. 15 reposes *Charaxes Jasius* as serene as if no Dr. Staudinger existed in all the world! This is another case, then, where the two writers disagree on identifications. Kirby has recognised the *Jason* L.M.L.U. (1764): Staudinger has not. Kirby has once more shown that Staudinger, by Staudinger's rules, ought to do what Staudinger thinks he ought not. Staudinger has once more retorted that Kirby is all under a mistake; insisting that Kirby's *Jason* out of M. L. U. is not recognisable and has not any "proofs." It is the bare truth that a single minute's dip into these pretentious Catalogues brings you to contradiction after contradiction of this kind, which no one can determine; and the slightest comparison of the two works forces one into noticing them. But at all events it appears that Mr. Kirby could not supersede *sylvata* for *testaceata* of Donovan; and though that is a small mercy, I for one am heartily thankful for it.

I hope it is not necessary to take any more instances. The "rule" is of great importance, and its effect on the stability of nomenclature is disastrous, because, like the other changes, it sets the ball rolling which on its way knocks over no one can say how many accepted names. The rule appears to me artificial and *quite unnecessary;* and I regard its application as the very cream of pedantry. I hope Mr. Kirby does ignore it; but (as he does not state the rules which he observes) it is impossible to say whether he takes a given course in obedience to rule, or through mistake. His work, therefore, eludes my criticism; but I will trust that he has in the case mentioned preserved both names designedly, and because he discards the "rule."

Two other causes of difference were mentioned:* the naming of a species twice over by its author in the same work; and the occurrence of names nonsensical or not properly constructed. Both these are minor matters, but both afford plenty of ground for disagreement, if not for inconsistency.

The commonest case of double naming (by the same author) is supplied by a salient difference in the *sexes* of an insect. The

* *Ante*, p. 6.

Dresden Congress, and a number of writers,—including Professor Westwood, Dr. Staudinger, and Mr. Kirby,—have expressed themselves in favour of upholding the name given to the male. Dr. Knaggs has made profession of preferring that given to the female. Mr. Kirby, however, since publishing his opinion, has altered it on this point also, and now stands by the name which is "prior." Dr. Staudinger is also inconsistent; and I cannot say to which side the balance of authority now inclines. I have remarked slightly on Dr. Knaggs' application of his principle,* mentioning some cases in which he is not consistent. Dr. Staudinger writes:†—

"If a species have been named differently under the male and female forms, the species ought to keep the name of the male."

We will go straight to page 56 of his Catalogue, and take the case of *Nemeophila russula*, Linn. Remark what concessions to *communis error* learned writers are sometimes forced to make. Linné named the male *sannio*, and the female *russula*, both in the same work, 'Systema Naturæ,' tenth edition. The male name *sannio* is, moreover, before *russula* in the order. Strange to say, Dr. Staudinger calls the insect *russula*, his practice being thus in defiance of his principle, and of priority itself. Mr. Kirby would not do this for he gives the Meadow Brown butterfly under its female name, *Jurtina*, because that name comes one place before *Janira* in the order. As I have said, Mr. Kirby had previously declared for the male name‡ against the prior name. At the time his Catalogue was printed he declared for the prior name; and what name he now declares for I do not venture to surmise.

It is quite unnecessary to notice these differences at length. There is no sort of agreement among authors, the majority, no doubt, not having any rule at all. The Order *Hymenoptera* is probably the one in which specific sexual differences have led the writers most astray; and as to them I may quote from the review of Mr. F. Smith's recent Catalogue.§

"Mr. Smith sometimes adopts the trivial name of the male, sometimes that of the female. . . . Out of ten cases it will be seen that Mr. Smith adopts the ♂ name in five, and the ♀ in five; and I believe a like impartiality will be found to have been exercised, if all the instances of the kind which occur in the Catalogues were tabulated. Priority of place in the volume manifestly has not had any weight; and what the principle of selection is I cannot discover."‖

* Trans. Ent. Soc. 1871, p. 345; see also Mr. T. H. Briggs, 8 Ent. Mo. Mag. 94.

† Cat. 1871, pref. pp. xx., xxi.

‡ Prbc. Ent. Soc. 1868, p. xliii.

§ 8 Ent. Mo. Mag. 223.

‖ Mr. Dunning adds:—"though I have no doubt that a good reason exists for each particular selection." The reason, I venture to hope, is the all-

An author who has most consistently named the same insect more than once is Linné; and it is amusing to observe the muddle which resurrectionists have got themselves into about his names. The first name, we are told, is identified for ever with the insect, and the insect with the first name. If this be the "law," why on earth should it not be applied to the Linnean names? Before absolute priority men were born to us, entomologists had agreed to use the *later* names of Linné; and, when the new doctrine arose, it met with a great difficulty. It appeared so monstrous to reintroduce names abolished by common consent (where the common consent could be shown so clearly), that the resurrectionists paled before the attempt, and ingeniously "drew a line" at 1767. That is to say, the holders of the doctrine of strict priority found at the very outset what, in their creed, was a universal error. That error was too big for them; so they "drew a line," and retired behind it with what dignity they could muster. In that instant they acknowledged that *communis error facit jus*. To draw any such line *is*, in my judgment, not only to "yield the main point in dispute,"* but to surrender the principle contended for. Dr. Staudinger, moreover, thinks the same. "If," he says, "we allow to Linné the right of changing names and replacing them by others, we accept a precedent which may be imitated by other writers in Natural History."† Considering that, with scarcely an exception, the earlier Linnean names were changed by Linné,‡ and that the later names are everywhere in use, we have (now that the list-makers are awakened to the desirability of restoring the first) a pretty number of "rectifications" to look forward to. When they come, we shall be bound to accept them; if we do not determine to make a stand at once and reject those now proposed to us.

Another class of names on which authors are divided comprises those which convey false information; those which have no appropriate Latin form; and those which display philological inaccuracy in their formation.

First, as to names conveying false information. The British Association rules, and Dr. Staudinger's last preface, put the two views in contrast; and we need not look any further. The eleventh rule of the British Association Committee (1842) is:—"A name may be changed when it implies a false proposition, which is likely to propagate important errors." In the preliminary explanations the Committee advise the rejection of the name *nigra* for a species which

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sufficient one that the name preferred is that *in use*. If so, there could not be a better one.

I would willingly supplement each attempt at criticism with professions that I believe in the good reasons of the author. I only do not follow this course, because it would make the pamphlet read like one long apology, and that could not fail to depress my readers.

* Mr. W. F. Kirby, 8 Ent. Mo. Mag. 142.

† Cat. 1871, pref. pp. x., xi. See Dr. Speyer's view, *ante*, p. 29 (note).

‡ Mr. Kirby, 8 Ent. Mo. Mag. 42.

is not black. They also "feel justified in cancelling" names derived from an accidental monstrosity, as *Picus semirostris*, Linn. The Committee remark that the privilege of rejecting such names is "very liable to abuse, and ought therefore to be applied only to extreme cases and with great caution"—a proviso which deprives the rule of the weight it might otherwise have had. It seems that the Committee would have done much better to leave alone a subject on which it is evident they could not make up their minds. Dr. Staudinger, if he could have his way, would permit no change at all on such grounds. And so far I believe Dr. Staudinger has the great body of entomologists entirely with him.*

Names which are nonsensical, and have not a Latin look, the Dresden Congress and Dr. Staudinger repudiate altogether. Dr. Wocke, however, does not agree with them, and Gemminger and von Harold accept the names (inconsistently—as Staudinger points out). There has been an overwhelming amount of discussion on the rejection or acceptance of such names as (*Calodera*) *Mech* and (*Thais*) *Cerisy;* and it is not safe to decide on which side the balance of opinion is. Enough to say that some entomologists will not alter such names at all, but claim strict priority for all of them; others will Latinise the original name; and it appears to be a very general opinion that a proper correction of such names is allowable and a duty. This question only concerns me to a very slight extent; namely, so far as the dissensions of authors concerning it tend to make nomenclature additionally uncertain. I confess that I rest no great weight upon these differences: the "rectifications" are in the vast majority of cases only slight, and nearly all the names "rectified" are recognisable after the process. Those who wish to go into the subject at length will find as much reasoning as they can want, if they consult the following references:—Dejean, Cat. 1837, p. xii.; Berlin. Ent. Zeitsch., vol. 2, app., pp. xii.–xiv.; Ann. Soc. Ent. Fr., vol. vii. pp. 577–582, 602–604, 608, and Bull. lix.–lxxiv., cxcvii.–ccv.; Gemminger and von Harold, Cat. Coleopt. pref. pp. xvi.–xviii.; Thorell, European Spiders, pp. 12–14; Staudinger, Cat. 1871, pref. pp. x.–xiii.; 4 Ent. Mo. Mag. 259, 280; 5 Ent. Mo. Mag. 181, 182, 186; 8 Ent. Mo. Mag. 254, 294. I think any entomologist who reads all the above-cited papers will not fail to come to the conclusion that there is a great deal to be said on both sides.

A considerable number of small points of difference also arise, though it is not worth while to occupy time in discussing them. Thus, some authors will not allow two similar names in the same

* It is amusing enough to be told that we may alter *nigra* into "olivacea" if we choose. As every season witnesses the introduction of a "new colour" (with a new name to correspond), the old *rufa* and *purpurea* would soon find critics to say that the real shade is more exactly expressed by *mauvea* or *Magentica!* The very "olivacea" indeed which stands for the rejected *nigra* might two years ago have been called *ægri-Bismarcica;* but then what is the "lateinisch oder latinisirt" form of "sang de Prusse"?

genus. Of this number is Boisduval; Staudinger, on the other hand, rather likes such things, and prints *aqueata* and *aquata* both in the genus *Cidaria*. The case (such as *Selene* and *Selenis* in one genus) will also be found mentioned in Guenée's Phalénites, vol. 1, p. xxxv. and elsewhere. Again, where a specific name has been taken for the generic name, as in the case of *Cossus*, the authors are greatly bothered how to work the priority rule. See, for instance, Bdv. Lep. vol. 1, p. vi.; British Association Rules, pp. 15–16; Staud. Cat. 1871, pref. xx.–xxi.; Thorell, Eur. Spiders, 11 note.

Finally, there is one not infrequent cause of learned-looking disagreement, which it is necessary to notice in its turn, although no large amount of discussion upon it is desirable. I refer to *quackery* in an author when "revising" synonymy, or identifying old names. The name (*Atropos*) *pulsatoria* has recently been superseded (by two writers) for reasons of one sort and another, highly unsatisfactory in their nature, but not proper to be examined here. The history of the same insect furnishes, however, such an insight into the astonishing chicanery of which even an established writer can be guilty, that I feel bound to notice that among the causes of disagreement upon names.

Dr. Hagen (in Ent. Ann. 1861; see pp. 21–22) gave a synopsis of the British *Psocidæ*. The Linnean *pulsatoria* he there placed in the genus *Atropos*, which he thus defined:—"Eyes slightly prominent; ocelli wanting; antennæ with about fifteen joints, the two basal joints more robust; thorax flat; wings wanting; posterior thighs much thickened; tarsi tri-articulate." Dr. Hagen, therefore, had before 1861 satisfied himself that *pulsatoria* had "Antennæ with about fifteen joints" and was without wings; also that it had "posterior thighs much thickened." I expressly draw attention to the fact that *pulsatoria* is the only species of the genus *Atropos* there given; the species used for a type must therefore be this and no other.

In 1861, Dr. Hagen placed *studiosa*, Westw., in *Clothilla*, Westw. Of this genus Dr. Hagen gave the following (among other) characters:—"Antennæ consisting of about twenty-seven joints, the two basal joints more robust; in the place of the anterior wings two small leather-like scales, without veins, fringed on the margin; legs not thickened." I expressly draw attention to the fact that *studiosa* is the only species of the genus *Clothilla* there given; the species used for a type must therefore be this and no other.

Now the *Atropos* of 1861 is the *Clothilla* of 1865.* The insect which had a bare back, 15-jointed antennæ and thickened thighs, has now leather-like winglets, 27-jointed antennæ and "legs not thickened"! In addition, the genus *Clothilla*, Westw., defined in 1861 as having "legs not thickened," has in 1865 (when it has to receive the Linnean *pulsatoria*) "femora dilated"! But this is not all by any means. According to Dr. Hagen's (and Mr. R. M'Lachlan's) identification, *pulsatoria* is a *Clothilla*, and is moreover synonymous with Westwood's *studiosa*. Therefore the same insect is described by

* 2 Ent. Mo. Mag. 122.

Dr. Hagen twice over on two adjoining pages with opposite structural characters! That is not bad to start with.

Now take the case of *Termes fatidicum*, Linn. Dr. Hagen says;*—"I do not know this species. Linné says that it is twice as large as *T. pulsatorium* (*Clothilla*), which species is larger than *A. divinatoria*; otherwise one would consider it to be the latter species. Habitat: Southern Europe; in dried plants received from Rolander." This is all Dr. Hagen can gather of *fatidicum*—an insect which he never saw himself, and which no one he has ever spoken to ever saw (or identified), and of whose structure or peculiarities no one has ever given a description. Now will it be believed that Dr. Hagen—after the severe fall which he had already given himself—has the temerity to place "*A. fatidica*, Linné," in a genus with all the following characters:—"Meso- and meta- thorax united; antennæ with seventeen joints, thread fine; without wings; femora dilated; second joint of the tarsi short"(!!!) Was there ever a more RIDICULOUS FARCE? It may be said that Dr. Hagen has a good reputation among entomologists. I am not concerned with that. Until I detect more responsibility in an author, I hold myself entitled to ignore all the changes he may introduce. To give up the name *pulsatoria* (which all the world is agreed on) for the conclusions of a writer who does his work in such a reckless way as this, would be a sign of continued belief in his accuracy. I have given my reasons for holding a belief which is quite opposite.†

Now a few words on the way in which this important question has been treated. The majority, as I believe, of leading entomologists are strongly adverse to the resurrection practices, but with some of them it is true that discussion is not in their line, and the really scientific have shown no disposition to waste their time over a question of names; so that it has fallen to the writer of these hurried sentences to deal as best he may with the inventors and advocates of the resurrection system. In the first place, then, they take up a very uncommon controversial attitude for men who conceive they are furthering the solution of a scientific question. With the solitary exception of Mr. Kirby, who used several arguments really directed to the matters in dispute, nobody has put forward on the side of the list-makers any

* 2 Ent. Mo. Mag. 121.

† Under this head it seems proper to include the case, such as lately occurred at the Entomological Society, of an entomologist publishing a conclusion which half an hour afterwards he turns his back on. Few things are more ludicrous than Mr. R. M'Lachlan's "Identification of *Myrmeleon*" as printed in Trans. Ent. Soc. 1871, p. 441, taken in contrast with the paper as read before the Society. The accomplished writer's positive language ("I absolutely refuse," &c.) is—considering that the paper as read arrived at a conclusion exactly opposite to that of the paper as printed—almost too funny. The "synonymic labours" of an author who publishes conclusions so little digested must be worth a great deal less than nothing. It is, I presume, owing to some oversight that the Proceedings of the meeting of November 20, 1871, do not mention the discussion of Mr. R. M'Lachlan's paper. It is the more unfortunate, because Mr. M'Lachlan happens to be at present himself the Secretary.

answer worthy of the name. To Mr. Kirby's points I will reply respectfully as fully as I am able; and what I complain of is that there is not more to answer. Others, who support the resurrection practices, ought to have known better than to suppose that a proposition supported by arguments would die a natural death if only passed over in silence. The grounds of argument put forward in June, 1871, have already formed the basis of similar opinions, powerfully expressed since by several well-known entomologists; and by their countenance my "views" are abundantly vindicated.* But what am I to say of those who, professing to see through the hollowness of my arguments, nevertheless have from first to last denied themselves the gratification of exposing it? They have exercised a noble self-restraint; and deserve that entomologists should admire their magnanimity. Thus, Mr. E. C. Rye,—after a foot-note of two lines† designed apparently to dispose of the whole question,—in Ent. Ann. 1872 (p. 24), prefaces his sixteen pages of copied synonymy by mentioning "the question of 'resurrection' upon which so much energy has been expended by certain of his fellow-students during the past year"—and there he stays his hand. Mr. Rye has always taken much interest in questions of nomenclature, and no doubt has views of his own upon them;‡ and it would have been very satisfactory to know how Mr. Rye disposes of his fellow-students' arguments. He evidently either took alarm at their "energy"; or was hurt because they had usurped the same virtue which distinguishes the priority champions. §

But Mr. Rye's "erudite countryman," Mr. G. R. Crotch (after actually contributing to the printed controversy upon a minor point) in January, 1872, prints the bouncing assertion that "the laws of priority are of course assented to tacitly by all"!‖ Perhaps Mr. Crotch has since advanced his erudition by adding to it an acquaintance with other men's ideas; and he may have learnt that the "laws of priority" have

* Mr. T. H. Briggs in 8 Ent. Mo. Mag. 93; Mr. A. R. Wallace, Pres. Ent. Soc., in Proc. 1871, pp. lviii.-lxviii.; Mr. E. Newman, Zoologist for January, 1872, pp. 2893-2896; Dr. Albert Breyer, Ann. Soc. Ent. Belg. vol. 14, pp. cxxxi., cxxxii.

† In 8 Ent. Mo. Mag. 42.

‡ Mr. Rye's articles in the Entomologist's Annual, for instance, are stuffed full of nomenclature, which gives to his portion of the work an interest all its own. His notice of Mr. Crotch's 1863 Catalogue, wherein "it is the exception and not the rule for *any* species to remain unaltered, either in position, value, name, or parentage," is very entertaining reading now: see Ent. Ann. 1864, pp. 73—78.

§ It happens to be already on record that "Dr. Staudinger is a young Lepidopterist of *extraordinary* energy" (Ent. Ann. 1857, p. 126). Mr. Crotch likewise appears to have shown it. Mr. Rye, in 1864, called him an "energetic worker," and in 1866 "the energy of Mr. G. R. Crotch" was again the subject of comment. (See Ent. Ann. 1864, p. 73; Id. 1866, p. 47.) Mr. Rye should make allowances for this, and consider the great temptation, in opposing such redoubtable gentlemen, to give them a taste of their own quality.

‖ Cistula Entomologica (pars iv.), p. 59.

not been "assented to by all," either tacitly or loquaciously, at any single moment since first they were formulated.*

Mr. Edward Saunders† communicates his hope "that few will be willing to adopt" the *communis error* principle; and there he leaves the subject. Now such expressions as the above do no cause any harm. They all leave the subject exactly where they find it; but the issuing of hostile manifestoes is puerile, in a case where arguments are offered on the other side. An entomologist, who feels so much interest in a controversy that he cannot resist the temptation to publish his bare opinion, ought to be ready with his reasons when that opinion is challenged. Holding off from an argument raises a vigorous suspicion that the recalcitrant is not blessed with much confidence in the stability of his judgment. It is not rare to find a man already half convinced, who is ready to explode with assurances that his opinion is unchangeable. Those who affirm and re-affirm the divine right of the first describer are no doubt in this situation; but their manifestoes are not any more valuable as aids to an important controversy, and the time when they should cease to be issued has, perhaps, arrived. Entomologists have expressed themselves at length, and exposed their arguments to full criticism: it is rather a curious way of meeting them to make remarks about their "energy," or to "trust" they will not receive support.

Mr. Kirby (whose papers appeared in July and November, 1871‡) deals first with the question when our nomenclature begins. He repeats his declaration that "we must take the earliest or the latest works of Linnæus to begin with," adding that by the earliest work he means the first edition of the 'Fauna Suecica' (1746). Why Mr. Kirby thus prepared the ground for a voluntary immolation of himself, I cannot imagine, for it is hardly possible that he failed to see the effect of his sentences. If, says Mr. Kirby, we leave the twelfth edition of 'Systema Naturæ' (1767), we must go back at once to 'Fauna Suecica' (1746). That is all intelligible enough. Mr. Kirby then very cogently states the disadvantage of going back to the names of 1746: "With scarcely an exception these names were changed by Linnæus himself." The conclusion therefore seemed to be pretty obvious, but it turns out that it was nothing of the kind. "We must not leave 1767, unless we go to 1746," says Mr. Kirby, "but 1746 will land us in chaos."§ Therefore, let us not leave 1767?

* As Mr. Crotch has failed to discover any passages expressing dissent from the laws of priority, I have no objection to start him with the following, as a commencement merely:—Dejean, Spec. Gen. avert., p. x.; Lacordaire, 4 Silb. Rev. 223-239; Silbermann, 4 Silb. Rev. 239-242; Dejean, Cat. 1837, avert. pp. x.-xii.; Delaharpe, Faune Suisse, Phalén. p. 8. In addition (that he may satisfy himself about the *assent*) I refer him to some rules of a Dresden Congress (1858) and some other rules of a British Association Committee; also to all the Lists and Catalogues he can get a sight of.

† 8 Ent. Mo. Mag. 161. ‡ Id., 41, 142.

§ 8 Ent. Mo. Mag. 142. "We should really find ourselves in chaos." I do not know the meaning of this expression here. The phrase appears entirely misapplied. Mr. Kirby would seem to mean no more than this,—that we

Not at all: therefore let us eat our own words, and stay at 1758! If Mr. Kirby chooses to hold two inconsistent and irreconcilable opinions, it is certainly as well that he should publish both (as in the present case) at the same time and on the same page. But I have already adverted to this point* and will not discuss it any further. Mr. Kirby ingeniously supposes that the difficulty is "caused by the doubt about the dates 1758 or 1767 being the starting-point," remarking that it is limited and can be got over. But, as I have endeavoured to show, the question is not thus limited; and the difficulties are increased and not diminished by the remarkable and embarrassing pliability of an entomologist who has made these questions his special study.

Mr. Kirby says: "I cannot admit that synonymy is of less use now than formerly." This was in answer to the following expressions of my own: "The function of synonymy now is not to supply a concordance for entomologists, by which those using different works may mutually understand each other. That was a benevolent office for which the originators of synonymic lists deserve our thanks. All that is left for the lists now to do is the miserably different work of displacing names on which all are agreed, or proving the whole world is wrong and only the list-maker right." No-one has disputed this statement, and I adhere to it; I think, moreover, that I can show not only in what way synonymy is of less use than formerly, but also that other entomologists entirely share my opinion. The authors of the British Association Rules of 1842 published a preface in which they described the then position of zoological nomenclature. It argues forcibly the need which there was for uniformity:—

"If an English zoologist visits the museums and converses with the professors of France, he finds that their *scientific* language is almost as foreign to him as their *vernacular*. Almost every specimen which he examines is labelled by a title which is unknown to him, and he feels that nothing short of a continued residence in that country can make him conversant with her science. If he proceeds thence to Germany and Russia he is again at a loss; bewildered everywhere amidst the confusion of nomenclature, he returns in surprise to his own country and to the museums and books to which he is accustomed."

Thus wrote Mr. Strickland in 1842, when under the smart of this unendurable state of things, steps were determined on for securing uniformity. Does Mr. Kirby wish us to believe that this confusion exists now? Surely it is well known that the state of things which made synonymic lists necessary on this ground has passed away. My contention was and is expressed in the following sentences:

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should be restoring names of Linné, which Linné himself afterwards changed. I do not see the connection between this operation and chaos. It seems a thing simple enough, not at all different from restoring Linnean names changed afterwards by other authors.

* *Ante*, pp. 8-9.

"Synonymy does not any longer answer its former function. All entomologists use one name in the vast majority of cases. There is no real confusion even if different names are used; as, in the very few cases of doubt, entomologists know and use both the names (e. g. *Davus* and its synonyms), and no list-writer would be much of a guide in such contested cases as those," and this Mr. Kirby does not contradict. Again, if synonymy is no less useful than formerly, how comes it that Mr. Doubleday's List has from its last edition *omitted large numbers of synonyms?* Anyone who compares the edition of 1847–50 with that of 1866 will find that almost the whole of the earliest synonyms have disappeared from the latter entirely (!) The synonyms are omitted, because they have become (what Mr. Alexander Agassiz calls them) useless lumber. No one any longer wants to know the erroneous name by which a species was called in Germany fifty years ago, and Mr. Doubleday sensibly omits it. There could be no better test; Mr. Doubleday does not go on publishing what is useless, knowing perfectly well that the correct names are known and used, and the incorrect names are already forgotten.* Mr. Kirby, however, urges that "no one can have access to all the books in any branch of Entomology, and if he have a limited library, and identify an insect by a name which has been overlooked by later authors, it is useless to him." The statement here implied is a statement of what used to occur, not of what does occur. Mr. Kirby studies the *Lepidoptera*, and the "branch of Entomology" he more particularly intends is no doubt that one in which he has himself earned distinction, and in reference to which his remarks will carry most weight. Now, I must assert that in that Order the case Mr. Kirby puts does not hold. If any Lepidopterists now used Stephens' 'Illustrations' (for instance) for their text-book, the case would hold; but they do not use that, nor rely on any single book written before nomenclature became uniform. There are no Lepidopterists, except the students of local faunas, unacquainted with Guenée's Noctuélites, Deltoides, and Phalénites, and those works furnish an almost unexceptionable index to modern Lepidopterological nomenclature. The students of local faunas, on the other hand, now have their nomenclature triple-refined for them; and the case (for instance) of two English Lepidopterists being at cross purposes, because one uses names which are strange to the other, simply does not happen. The Micro-Lepidopterists (or at least the Tineinists) have their synonymy worked up, I suppose, to the finest point of accuracy. The picture, then, drawn

* Another less satisfactory discovery will also be made on comparing these editions. Some old names printed (as synonyms only) with a mark of doubt in 1847-50 are now printed as the true names, and without a mark of doubt. The pressure of the recent tendency towards resurrection is no doubt the cause of such changes. A last century's description was not in 1866 any clearer than in 1847; and what was doubtful at the earlier date was so also at the later one. We need a dash of the uncompromising spirit which arrives at conclusions for itself and holds to them afterwards.

by Mr. Kirby of the isolated student sturdily grubbing on and naming his species from an out of date text-book, while he waits in despair for the next Catalogue, represents nothing now met with in real life. Neither does this, as I am informed, occur in the other Orders, the Catalogues and other works recently published having already supplied a concordance between the books which are in use.

Mr. Kirby's real point, however, I take to be that "One great object of synonymy is to attempt to utilise the whole of the accumulated literature of Entomology." Whether this be its "object," I much doubt; but I cordially agree with Mr. Kirby, that if synonymy *effect* this, it does some good service. Only that point is far distant from the question under discussion, viz., whether or not, when discovered, the first name shall always be restored. By all means, so far as my argument is concerned, utilise the accumulated literature of Entomology. But when your speculations on the early descriptions are concluded, and you have "utilised" to your heart's content every line of print you have come upon, do not on that account revolutionise our nomenclature. The things have no necessary connection. By all means print a Catalogue giving, if you please, every name by which our *Pieris rapæ* was, might, could, would, or should have been called; but, when you have done that, please nevertheless to call it *Pieris rapæ* still. By this means no difficulty whatever will be experienced in "utilising" many times over great portions of the early literature of Entomology, as the same name and description will sometimes fit several dozen species. At the same time we shall all be enjoying certainty in nomenclature; so that the busy labourer at synonymy will do his work without disgusting anyone, and all for the pure love of it. This in fact is the very work which Mr. A. R. Wallace proposes our list-maker should confine himself to. "It is even questionable" he says* "whether the author of a Catalogue is not going beyond his province in making any corrections or alterations of the names in use, for any reason whatever. It may be said that he should simply record the facts, adopt the nomenclature in use, whenever there is uniformity among living authors, and point out if he likes in foot-notes his belief that such a name should be altered for certain reasons." This view expressed by an entomologist of such mark as Mr. Wallace I entirely adopt; it is indeed the thing which I contend for. Mr. Kirby has himself given an example of this very work. In his recent Catalogue, names of earlier date than 1767 are utilised in this way; that is, they are duly referred to as synonyms, but are not made to supersede names in use.

Mr. Kirby argues, "If the law of priority were rescinded, no one would any longer take the trouble to identify a species he intended to describe as new, and we should soon have twenty new names for every old name which would otherwise have been restored." This passage has been attended to by Mr. Briggs, who† satisfactorily

* Address, Proc. Ent. Soc. 1871, p. lxiv.
† 8 Ent. Mo. Mag. 95.

disposes of it. "No one," says Mr. Briggs, "ever proposed that the law should be rescinded." Mr. Kirby appears to think that twenty new names would arise if we pass a law that an agreement on one name shall *prevent* the introduction of any others, which I fail to understand.

Lastly, Mr. Kirby says, "The controversy hinges mainly on the question whether the knot of synonymy should be cut or untied." If this to my mind were the question, I should support the project of cutting the knot. To untie knots when you accomplish all you want by cutting them, seems (if I may repeat myself) fatigue duty of an exceptionally useless kind. But the controversy appears to me to hinge on no such question. In the cases to which alone my remarks have been directed, there is no knot of synonymy at all; and the endeavour of those who share my opinion has been to prevent the list-makers from tying one. Confusion is, I presume, "the knot of synonymy." We are asked to look on now at the tying of that knot, because it may be that hereafter somebody will be so clever as to get it undone!

I have, in mentioning these points of Mr. Kirby, noticed all the adverse arguments which my first paper elicited. Mr. R. M'Lachlan printed a communication entitled "Some considerations as to Mr. Lewis's views concerning Entomological Nomenclature," but it does not contain arguments. Any gentleman may (if he think fit), without any remonstrance from me, oppose my opinions in a contribution not containing arguments; but I must not be expected to return answer to his manifesto.*

* Mr. M'Lachlan does, however, make one or two statements which I will merely reproduce. They show the degree of intelligence with which this important subject is canvassed in some quarters. In the communication mentioned, Mr. M'Lachlan says:—"Mr. Lewis must take to his studies the predispositions of the amateur, rather than the calm investigation of the naturalist; he must be of those who, having mechanically spaced out, labelled, and arranged their cabinets and collections, feel wrath at any audacious individual who may suggest to them that neither nomenclature nor sequence is correct." "It is expedient there should be no more crime, no more deceit in the world, and, as a consequence, no more prisons, police, and lawyers. But the evils exist, and the other *necessary* evils are required to keep them in check." All this appears slightly personal, but is not otherwise very remarkable.

Mr. M'Lachlan also has the following:—"Having commenced my entomological studies as a lepidopterist, though possibly only as an amateur, it needs no great amount of discernment to make obvious to me the fact that British Macro-lepidopterists stand urgently in need of a thoroughly scientific monograph. Mr. Lewis's criticisms, in his paper in our last number, and at the Entomological Society, show that he should possess the acquirements necessary for its production. Let us hope our lepidopterists are tired of the degrading publications that have been recently submitted to them; works in which descriptions and advertisements are unblushingly and inextricably blended. If, then, he will prepare such a work (and include synonymy) he will obtain the gratitude of his fellow-labourers; or, at any rate, by being able to arrange his collections *after his own method* he will be spared the annoyance originating from the change effected by, and the want of unanimity

The proposal to accept no name which is not in use only goes the length of superseding priority in the cases where (as Mr. Dunning has said) the rule, *being strained beyond the reason for the rule*, becomes a nuisance and produces intolerable evil.* Entomologists want certainty in nomenclature, and they want it now. They are not so feeble as to fold their hands in resignation and say they do not "hope to see it in their time."† They do hope to see it, and indeed to take steps to secure it. There is no descriptive work yet in existence which uses the new names. They are therefore now only known to the curious; and, so far from its being an inconvenience not to know them, the whole body of entomologists use the old ones and the old ones only. Dr. Staudinger's Catalogue and Mr. Kirby's Catalogue are dead letters now; it ought to be the business of the entomologists, who do not live for nomenclature alone, to see that (so far as the new names are concerned) they remain so. The other innumerable discordant reforms, "suggested," "brought forward," "proposed" or "made" in Trans. and Proc. Ent. Soc. and in the Cistula Entomologica and elsewhere, are happily not dangerous; no one knows anything of them.

If we are to have a conflict of opinion on this question, it is consoling to remember that the winning is all on one side. We may be well content to let one reformer pay another in his own coin, and to see each go on proving *ad infinitum* that every name but his particular one is wrong. We may let them fight it out, assured (as we are) that the more lists and catalogues, reforms, and emendations

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in, the works of others." I have been entirely unable to fathom the meaning of this amazing passage; but I have no objection to examine it now. Mr. M'Lachlan's hypothesis that I should prepare a degrading work in which descriptions and advertisements are unblushingly blended, no doubt had its rise in some temporary confusion of mind, or a too original interpretation of the laws of composition; and I do not suppose the suggestion was intended. But what does the rest mean? What connection has the law of priority with writing a scientific monograph? Can no-one who has not prepared a "work" arrange (if he please) his collections after his own method? And what has arranging collections to do with an argument upon priority? A censorious person would, I fear, say that Mr. M'Lachlan has gone out of his way to make rather ill-natured remarks.

Mr. M'Lachlan's paper is, however, very important if only for one statement. He writes, "The application of Mr. Lewis's legal maxim is the greatest affront that could possibly be offered to an *exact* science." Mr. M'Lachlan uses the italics, which certainly give much strength to the assertion. That Entomology is now "an exact science" cannot, I am sure, be too widely or too quickly known. Conceive the intense emotion of a common Lepidopterist at learning he is the follower of an exact science! I (and perhaps some others) had contracted the notion that Entomology is not more an "exact" than an "occult" science; and that polygamy or fly-fishing have about an equal claim with Entomology to either one or the other designation. But that mistake is now set right; and it is no small matter that we have the statement plainly—and in italics.

* 8 Ent. Mo. Mag. 215. † Id. 40.

they issue, the more certain do our opponents make it that the end will be soon. If only a few more conflicting catalogues see the light, the common sense party is morally certain to receive such an accession that its numbers will include all but the list-makers themselves.

The plan proposed will, it seems, accomplish all that is really necessary or desirable. We wish to prevent the resurrection of names absolutely buried and forgotten, and not in use anywhere. We do not wish to preserve the names in use with us, where a different and prior name *is* in use. This latter would not be justifiable, and has never, that I am aware of, been proposed. The plan does no injustice. It has been well insisted by, among others, Dr. Albert Breyer* that these changes which afflict us are effected on the authority of works without real scientific merit. But even if the case were far otherwise, the choice between the general advantage and deference to a forgotten author could not be doubtful. The proposal made is a proposal to simplify the study of entomological science. As such it stands in no need of indulgence, though it is certain that it might be (and I hope that it will be) supported by more and better arguments.† By adopting the course advocated we shall rid Entomology of the single unattractive feature with which inconsiderate persons have invested it, and it is impossible but that our science should be greatly advantaged in consequence.

The Order *Lepidoptera* is the one on which the resurrection experiments are first made. I trust there will be no mistake in the reception they meet with. Lepidopterists have been the subject of many childish sneers on the part of those who ought to exhibit a different demeanour. Lepidopterists are dubbed "collectors," and their opinions are uniformly disparaged, by a great many of the lovers of disagreeable insects. That treatment by the select few of the class which includes the vast majority of their fellow-students has indeed

* In 14 Ann. Soc. Entom. de Belgique; comptes-rendus, pp. cxxxi., cxxxii.

† Concerning "an appropriate nomenclature" Mr. Alexander Agassiz writes (American Naturalist, August, 1871; copied 8 Ent. Mo. Mag. 96):—"In the hands of Linnæus it was the expression of vast erudition. . . . In the hands of his followers and disciples it has become too often the end instead of the means; and of late years the laws requisite for the establishment of the correct name of an animal, or of a plant, have become often as difficult to establish as the most intricate legal question." I think Mr. Agassiz goes astray when he terms "followers and disciples" of Linné those persons with whom nomenclature "has become the end instead of the means"; for I strongly suspect Linné would have declared them to be no disciples of his. As to nomenclature equalling in difficulty the most intricate legal questions, I can only say that if I had thought so, I should have left the former to be dealt with by those to whom the latter are more familiar.

The article (as it appears in 8 Ent. Mo. Mag. 96) is entirely on our side, except only the portion there printed in italics. Mr. Agassiz, among other things, declares it to be impossible "to lay down general instructions intended to be retrospective and prospective." I heartily agree with him, and to his generalisation add one sentence of my own. *Never was the truth of this so plainly shown as in the recent attempts to make retrospective the "law" of priority.*

become the fashion, and every weakling sniggers and conforms to it. But the Lepidopterists have it now in their power to confer upon science an immense and permanent benefit. The battle has been brought here; and the arena of conflict between pedantry and common sense is ground on which we are at home. To the Coleopterists we may with confidence appeal. *Tua res agitur, paries quum proximus ardet.* While the lists of *Lepidoptera* are being "revised" on discordant principles, the Coleopterists cannot afford to look complacently on. On the day that the priority crusades are successful against the *Lepidoptera*, the fate of Coleopterological nomenclature is sealed. When the first instalment of a real opposition Catalogue leaves the press,* those who now glorify Gemminger and von Harold will quickly change their note; and deplore it if they have not joined the Lepidopterists, and agreed to reject all totally disused names.

What then is the practical result of the opinions I have urged? Supposing (as I confidently hope will happen) that it receives a predominating support, how will this doctrine operate in the first instance? The answer to these questions is, I venture to think, a very simple affair; and I will conclude this imperfect essay with an indication of the manner in which the priority question may be readily solved in accordance with the principle I have contended for.

We are asked, How do you show a universal accord? *We do not assume to show it; but throw it upon the list-maker to show that his new name is somewhere in use.* It is ingenious to call on those who are in possession to prove their right; but this has been tried before, and the common sense of mankind has ruled against the attempt. Possession is everywhere nine points of the law; and a clear title must be made out by those who want to dispossess us.

The *modus operandi* of our restriction is therefore the simplest thing conceivable. The list-maker being allowed to supersede any name only by another name *in use somewhere*, all he has to do is to show that he fulfils this condition, i. e. point out where the new name is in use. A universal agreement can be easily disproved; while to prove it affirmatively is impossible. Therefore the way to ascertain whether there is or is not universal agreement is to try and find out a place where there is disagreement. I should think this is so plain that the most jaunty resurrection men will understand it without mental fatigue.

If a resurrectionist meet with difficulty in establishing that his new name is in use, of course that is a matter which concerns him. The

* Coleopterists must be getting restiff as it is. Mr. Crotch, we learn, has recently published a list of *Geodephaga* and *Hydradephaga*, in which all species are re-named whose names were inadmissible when first employed. The author has there amused himself by naming afresh in 1871 some half-dozen beetles all named in the last century and now known by the old names. See Ent. Ann. 1872, pp. 26-29. This is precisely what I have promised to do (only on a far grander scale) in my Synonymic List of European *Rhopalocera* —advertised on p. 33 *ante*. Mr. Crotch therefore will be just the man to appreciate that conscientious work when it appears. I shall be pleased to know how many copies he will take.

rest of us will be only too happy when he succumbs to that difficulty; and I certainly would do nothing to help him to prove his case. As, however, this is a proposition which, arising from the necessities of the time, is new in shape, perhaps it will be as well to indicate the sort of proof to be demanded of a list-maker before we accept any change of name. Thus, any descriptive work published within a few years back (and coming from any quarter of the globe), which contains the insect in question under a different name from that in use with us, will clearly be sufficient to disprove universal agreement; and upon citing it the list-maker entitles himself to pursue his task and establish the right name by the law of priority. He must, however, supersede our name by no name but a *name now in use;* and it rests with him to satisfy his public. The whole *onus* being upon him, he must show to our complete satisfaction that the new name for which we are to surrender our name is a name which other entomologists are now using for the same species. It is not only a fair thing, but necessary for the advantage of science, that where more names than one are in use, those who are in error should be set right. But the use by the whole world of one name is all that is required. *Communis error facit jus.**

This proposal is a proposal to return to the laudable practice of the entomologists who flourished before the resurrection era. The following passage from Dr. Boisduval's Index Methodicus states in plain words what (in the view of those who share my opinion) is still the reasonable and only proper method:—"Vetustis quidem et exoletæ jam memoriæ nominibus abstinui, quibus species planè notæ designantur; eorum vero quæ, nuper creata, nuper repertas species designant, nullum neglexi, eo quod primum tempore erat, assumpto semper et usurpato."†

A mere descriptive work on insects, which has lien unknown for ten years, may well lie unknown for the rest of time, so far as its effect on nomenclature is concerned; and a name attached to a

* The suggestion of a new limit, beyond which synonymic researches shall not go, has been made before. Thus writes Dr. Speyer (Stett. Ent. Zeit. 1865-1866, p. 51):—

"Es wäre sogar, wie schon Herr von Kiesenwetter bemerkt hat, für die Stabilität der Nomenclatur sehr wünschenswerth, noch eine zweite so anerkannte Autorität, wie Linné, zu besitzen, um ihr nächst diesem eine Ausnahmestellung einraümen zu können. Bei der Wahl einer solchen, sowohl für die Entomologie im Ganzen, als für die Lepidopterologie im besondern, würden aber die Stimmen so auseinandergehn, dass es gerathener ist, ganz darauf zu verzichten."

The proposal made above is a proposal of the very thing which Speyer and von Kiesenwetter so much desire for the stability of nomenclature. It establishes "a second acknowledged authority"—the authority of universal employment. My proposal being assented to, no name not in use on June 1, 1872, can be brought up to supersede any other. Instead of the authority of one man, what I propose to exalt is the authority of all together—certainly THE ONLY AUTHORITY which it will ever be possible to agree upon. The blessed results of such an agreement I shall, I doubt not, live to see.

† Index Methodicus, second edition, p. vi.

description or figure so inadequate, that for ten years it is not identified, deserves to be forgotten,—even if, through collateral proof, its identification has afterwards been effected. No reasonable excuse exists for an author whose names are not anywhere in use for ten years after their publication; and if his light has been hidden under a bushel,* he cannot claim priority for his descriptions. We cannot allow ourselves to pay to describers a deference which has the effect of hindering progress. Whether or not, within moderate limits, an original describer deserves that his name shall be adopted, it is plain that the progress of science cannot stay for a nice adjustment of his rights. Names are but names, and must not be allowed to occupy time and attention beyond the minimum quantity which will suffice to protect us against confusion; for we cannot, if we are in earnest, spare for them more than the intervals of scientific studies. The notion (which seems to have been once fostered by authors) that a man is entitled to immortality because he has first named an insect is, apparently, altogether exploded. I have already† taken into account this element of "justice to the first nomenclator," expressing my view that the divine right of nomenclators is a fiction; and there are two very recent opinions entirely on all fours with my own. Mr. A. R. Wallace, Pres. Ent. Soc. 1871, in his Address‡ has the following expressions:—

"The idea of justice to the first namer or describer of a species is sometimes appealed to; but the law of priority is founded on no such expressed idea, but rather on the universal practice of mankind, which always upholds stability of nomenclature and requires cogent reasons of convenience or beauty to sanction an alteration. . . . The proper rule to adopt would have been unchangeability of names in use, rather than priority of date, which latter rule ought only to have been brought in to decide on the claims of two or more names in use, not to revive obsolete names never in use or long ago rejected."

Mr. J. W. Dunning§ expresses himself to the same effect:—

"I have no respect for a nomenclator simply as such: the fact that he has been the first to name and describe an insect or a plant gives him, in my eyes, no title to immortality, does not even invest him with the faintest halo of sanctity. I use the name he has given, not as a recognition of any merit in him, not as an admission of any right in him, but solely from considerations extraneous to him. The rule of priority in nomenclature I hold to be a good rule within its proper limits; it is not an unmixed good; and priority, like any other hobby-horse, may be ridden too hard. When the rule is strained beyond the reason for the rule, it becomes a nuisance,—nay, more, it produces intolerable evil; but, when reasonably applied,

* Le premier auteur réclame son droit de priorité; j'en suis fâché pour lui; il a mis la lampe sous le boisseau, qu'il en porte la peine; je le condamne sans appel.—Lacordaire, 4 Silb. Rev. 234, and see G. Silbermann, Id. 240.

† 8 Ent. Mo. Mag. pp. 2, 3.

‡ Proc. 1871, p. lxvi.

§ In 8 Ent. Mo. Mag. 215.

it produces more convenience than inconvenience. I accept it, therefore, as a rule of convenience, and nothing more; a rule adopted for the benefit of science, not for the glorification of name-givers. And the sooner the better that we are rid of any such notion as that the law of priority is established *in piam memoriam fundatoris*, or that there is any 'divine right' of the nomenclator."

If I quote in this place two or three other brief extracts which support my proposal, I shall have done all that remains for me. Mr. T. H. Briggs, in an argumentative paper contributed to Ent. Mo. Mag.,* thus explains his view:—

"The law—as I have always hitherto understood it—is, that when different individuals have described the same insect at different times under different names, the name first given shall have priority over all subsequent names; but, like all laws that lay down a general precept only, it must be construed in the spirit in which it was made, which is, as I urge, only as a means of determining a right to a name when there is no accord. . . . Both sides agree that the accord of entomologists is the ultimate desideratum, but the resurrectionists seem to consider that fishing out the most ancient name and repealing all the subsequent, is a better way of arriving at that result than by letting a name accepted by common consent stand, and abrogating the obsolete! I hold, as I have before stated, that the law of priority is not that the oldest name of an insect is invariably its right one, as the resurrectionists now insist, but that, in cases of dispute, the prior name is to be preferred, and in such cases only; and that any attempt to subvert accord cannot be done under the law of priority, but we must make a new law,—the law of antiquity, say."

Dr. Albert Breyer, in Ann. Soc. Ent. Belg.,† gives the resurrectionists a hard time of it. He thus delivers himself, speaking of the Dresden code and the lists published in pursuance of it:—

"From the literary point of view these works are not without merit; but from the point of view of Natural History they are completely sterile. More, undertaken to make an end of confusion, they have done nothing but augment it. Instead of judging,—of deciding between two, three or four contradictory denominations,—they have established an enquiry into all the names given since the time of Linné. It is only insects discovered in the last twenty years which have been able to escape the zeal of the Anabaptists. There is a German name to designate this mania,—*principienreiterei*,—which I have translated by 'aller au dada sur un principe.'" . . . "When the Catalogue of 1861 brought us the first fruits of the principle of absolute priority, there were certain names changed, and among them the names best known, and on which, for years, there was agreement come to between the English, French and German authors. And the greatest number of these untimely changes came about from the discovery, or rather

* Vol. viii. pp. 93-96.

† Vol. xiv. pp. cxxxi., cxxxii., before cited.

the bringing again into memory, of some works without serious scientific merit."

Mr. Edward Newman, reviewing Mr. Kirby's Catalogue* and writing of the priority rule, says:—

"A little band of so-called reformers discovered this law, and talked it over, and gave it another meaning. They said:—'This shows us that we ought to investigate every name, and see if we cannot find another and older name.' They went at it tooth and nail, and changed every name that could be changed for another name. Thirty years have they been busied in this work, until the entire object of names is frustrated." He scouts the idea that "a name given seventy years ago, and which has been totally neglected and utterly forgotten, should ever be revived and re-introduced."

So, the tide has turned. Common sense has asserted itself against pedantry; and 1872 is not the era when the latter can expect any victories.

* Zool. Jan. 1872, pp. 2877, 2878.

A PROPOSAL

FOR

A Modification of the strict Law of Priority in Zoological Nomenclature in certain cases.

In this paper I propose to consider (shortly, and in one aspect) the law of priority in nomenclature; and to urge upon your attention a certain proposal for a reform of it in one particular.

I address myself to the subject without the claim to be heard which can be set up by many present, because there is only one branch of Natural History to which I have paid attention, Entomology. While, however, that single study may not entitle its adherents to raise their heads very high in this assembly, it is nevertheless the one department in which the laws and practice of nomenclature are constantly called into exercise. Synonymy, a thing hardly known, as we know it, in some of the other branches, assumes in Entomology a most unpleasant importance. The naming of species among the Insecta is now proceeding on a scale so wholesale as probably to be quite outside the experience of students of the less numerously populated classes; and it can scarcely be said that any other branch of natural science gives its students the same intimate acquaintance with the difficulties attending nomenclature. I claim, therefore, for the entomologists, your attention for a short time to this matter. It is in their studies that questions affecting nomenclature, priority, synonymy and the like, make their importance felt. It is in support of a movement originating with entomologists for a reform in one particular of our laws governing nomenclature, that I have undertaken to read this paper.

One reason obliges us to come here with this proposal. The movement has been begun elsewhere,* but it is felt that the British Association supplies a forum in which also this question must be mooted. In 1842, Rules for Zoological Nomenclature were published, with the authority of Section D of the British Association, sitting at Manchester; and those rules, though not strictly adhered to by any means even in this country, furnish nevertheless the standard of accuracy. A proposal for a modification of the law of priority is a proposal to modify the rule adopted by this section, albeit a generation ago, and I hope I may be deemed justified in bringing the subject now before you.

The law of priority, as set out in Rule One of the British Association Rules of 1842 is, so far as is material to my purpose, that "The name originally given by the describer of a species should be permanently retained, to the exclusion of all subsequent synonyms."

This law of priority is *a means to an end.* The end to be secured is ACCORD, *or common agreement on a name;* and the way prescribed is to seek the earliest name, and when it is found to use that only.

There is no better rule in the majority of cases. Where more names than one are in use, we must devise some method for choosing between them, and this rule is then often very useful. But let us put to ourselves the question, *When we enjoy accord already,* what need is there of a guide to enable us to attain it? We have something much better —the result. The scientific names we are *agreed* on surely need no other sanction. Adopted by all, they require no machinery of rules to make them acceptable. To cut down a tree is the first indispensable step towards constructing a

* See Trans. Ent. Soc. Lond. 1871, part 3, p. 341. Also 'Entomologist's Monthly Magazine,' vol. viii. p. 1.

The article last mentioned contains in epitome a great portion of the above paper. It is amusing to recall the criticisms delivered upon it. First, a Dipterist told me in confidence that the whole affair was so absurd he had difficulty in understanding how the editors came to print it. Second, a Hemipterist made, as I was told, such uproarious fun out of my legal maxim in connexion with an "Ortoni" and a "Tichborni," that when they came to me with the story I was sorely tempted to abandon all "views" on the instant. Third, a Neuropterist quite overbore me by proving (ten times over, without a check) that I was doing more harm than good; and finally was impudent enough to say that I had "commenced" my studies at the wrong end. Lastly, an erudite Coleopterist announced, without reserve, that, come what might, he meant to take no notice of Mr. Lewis,—a manifesto not necessarily unwelcome, and perhaps uttered under feelings of compassion. But, then, these critics were all collectors of disagreeable insects!

bench, but those already comfortably seated would be foolish to undertake such labour. Yet this is the task many persons now set themselves. They rout amongst old books to find what name is earliest for a species, though all the while men of science are agreed, and there is no difficulty or dispute.

Let us suppose that there is universal agreement among naturalists on a certain name; that a species has been called, let us say *gracilis*, for fifty, thirty, or twenty years, in all countries and by all persons. In books, in many languages, it has been described under that name. Perhaps allied species have been named after it, and the names *gracillimus*, *subgracilis*, *graciloides*, and the like, have clustered round it. Now, if to-morrow in some obscure old work (*a*) a busybody can hit on the species *gracilis* described under a different name, and this work was a month earlier in date than the first of its more popular successors, the whole world must give up *gracilis*, and every book and list from that day contains an error. The universal agreement through a series of years on the name *gracilis* goes for nothing at all, and perhaps *tetra* has to be accepted instead.

Now, one thing cannot be disputed. The common convenience is utterly sacrificed (*aa*) when such an event as this takes place. None but very important considerations, *founded on a direct benefit to science of the change of name*, could justify such an over-riding of a universal agreement. Yet this effect has persistently, by rigorous "reformers of nomenclature" been ascribed to Rule One of this Association, and no one has ever yet asserted that science is benefited a jot. There is no good reason for construing the law of priority in this sense, or for pushing its effect to such a length, and while a modified application can be made

(*a*) Un nom aura beau être inscrit dans cent ouvrages différents, tous signés des maîtres de la science et classiques; si par hasard on vient à découvrir, dans je ne sais quelle obscure dissertation imprimée il y a soixante ou quatre-vingt ans que la même espèce y est décrite sous un autre nom, ce dernier doit prévaloir, et celui inscrit dans les cent ouvrages en question être mis au néant. Or, faites cela et voyez ce que devient l'Entomologie. . . . Le trouble qui en résulterait dans la science est si palpable, que les partisans les plus prononcés du droit de propriété reculent alors devant son application.—*Lacordaire, Silb. Revue*, vol. iv. p. 225.

(*aa*) Il me semble qu'il faut toujours se conformer à l'usage, et qu'il est nuisible de changer ce qui est généralement etabli. . . . . Je demande serait-il convenable de rejeter un nom généralement adopté, donné par un de nos grands maîtres dans un ouvrage marquant qui se trouve dans toutes les mains, pour lui substituer un nom inconnu, donné par un auteur ignoré dans quelque recueil periodique ou journal academique que personne ne lit, parce que ce nom aurait été mis au jour quelques mois avant le premier.—*Dejean, Spec. Gen. des Coleoptères*, vol. i., p. x.

of its provisions, beneficial in effect and working no inconvenience, it is our business to contrive that they shall receive such an interpretation. There are excellent reasons in favour of a modified application of the rule, and by noticing in turn some results of its strict application (in the sense advocated by "reformers") I expect to show you that no little detriment has resulted to science from that cause.

It is surely the proper reading of this rule that *where there are more names than one in use*, then the earliest shall be preferred. The rule was devised to settle *differences* upon a name, and could never have been intended to have the foolish effect of reviving an obsolete name, forcing on us its acceptance to the displacement of one universally agreed on. Consider whether, if the rules had to be made now, thirty years later than they in fact were, a more enlightened spirit would not prevail. I am confident that no majority of this section would be found to sanction such a law as this has been construed to be. The law of priority, I repeat, could only have been intended to apply to cases where more than one name was in actual use, and it was necessary to determine on the proper one—not to cases where *all names save one were already obsolete.* But if this was not its intention, then the law meant a thing very unreasonable, and it is high time its terms were altered and a more reasonable practice was introduced.

Nothing could justify such a rule but its importance to the advancement of science; but what service is done to science by the restoration of a forgotten name? I have asked in vain for an answer to this enquiry, and it is clear to me none can be given. So far as Natural Science can be said to be concerned with names at all, which in fact it is not, the alteration for any cause of any name is a hindrance, because such an event tends to introduce confusion. But the whole matter of what name a species has borne, and when and by whom that name was given, is outside the domain (*b*) of Science. The choice of a name for a species is not a question of science at all, but of convenience and

(*b*) Il est souvent impossible d'arriver à connaître quel est l'auteur qui le premier a nommé un insecte. . . . . *Cette connaissance ne forme à mon avis qu'une partie secondaire de la science.*—Boisduval, Lépidopt., tom. 1, page vii.

La science n'est pas dans la nomenclature.—*G. Silbermann, Silb. Revue*, vol. i. p. 133.

common sense (*c*). To settle rules for the trammelling of common sense is a useless task, for common sense is ever a rebellious subject; and the fact that Rule One, if it means what is generally supposed, enjoins a flat abnegation of common sense (*d*), strongly supports my belief that it was meant to have only the modified application I have contended for. When all the world is agreed on a name, no reasoning or rule should have the power to alter it. Accord, agreement once secured (*e*) should be fostered by every means. If all the world is not agreed on a name, go to your rule and thus arrive at an agreement. The rule is meant to supply *a winnowing process* by which all names but one shall be got rid of; but where the progress of time and the agreement of naturalists have already done the work, it is folly to ignore that result. The common acceptance among naturalists of any name, for at all events twenty years, should give that name an indefeasible title to adoption. The restlessness of the writers I am going to mention will never be allayed but by the imposition of a restriction of this kind. The actual period of limitation should be, like the rule, the result of an agreement among naturalists.

In the few observations which follow, I shall be obliged to draw my facts and illustrations from Entomology, but, for the reasons mentioned, I make no apology for so doing.

(*c*) A côté il y a la langue que nous créons pour les exprimer et rendre leur connaissance transmissible entre nous. . . Elle n'est que l'accessoire; elle n'est que notre ouvrage tandis que les faits sont l'ouvrage de la nature et impérissables comme elle. Ou pourrait être un excellent et profond entomologiste sans connaître un seul nom d'insecte.—*Lacordaire, Silb. Revue*, vol. iv. p. 237.

Cette éternelle et sèche terminologie, à laquelle se réduit malheureusement toute la *science* de tant d'*entomologistes* qui oublient que ce n'est là que l'A.B.C. de la langue Entomologique.—*G. Silbermann, Silb. Revue*, vol. iv. p. 241.

These rules of nomenclature *make no part of Zoology*; they are in their nature purely arbitrary and dogmatical; their only legitimate object is *convenience*.—*W. Ogilby, Mag. N. H.* vol. ii. N. S. p. 150.

Those who study nature for her own sake, who employ names as we employ our own names, simply as a necessary, or, if you will, a convenient distinction, will certainly discard these puerilities as utterly unworthy the expenditure of time and thought.—*E. Newman, Zoologist*, Second Series, No. 76, p. 2894. (Jan. 1872.)

(*d*) Voyons jusqu'à quel point celle-ci est conforme à la raison et supporte l'examen. (*Lacordaire, Silb. Revue*, vol. iv. p. 224.) Ainsi, que de perplexités, que d'impossibilités, si l'on veut suivre dans toute sa rigueur ce principe, si cher à beaucoup d'entomologistes!—(*Id.*, p. 229.)

(*e*) Si l'un de ces mots est reçu, adopté par la majorité des entomologistes, *le but est atteint*, et à quoi bon vouloir lui substituer un autre mot, parce que celui-ci a quelques mois ou quelques années d'existence de plus?—*Lacordaire, Silb. Revue*, vol. iv. p. 236.

When the number of known insects was small, a short and comparatively general description served to distinguish each species. The number of Lepidopterous insects now described and named is estimated at 30,000. The number described and named by Linnæus is but 780. So that now exact and lengthy descriptions are necessary to express the differences between the allied species, our knowledge of which has been, by later discovery, so vastly extended. In fact, the old descriptions are in great part worthless. It is not possible, in hundreds of cases(*f*), to say to what insect the description was meant to apply; and many names have been

(*f*) Die unvollständigen und wenig nützlichen Beschreibungen älterer Schriftsteller. . . . Ob es nicht schon zureichend sey mit Vorübergehung jener ältern, bloss und allein diejenigen neueren aufzuzählen, deren Beschreibungen nur irgend etwas vollständig genannt werden können.—*Schönherr, Synon. Insect, pref.* iii.

Linné et Fabricius seraient inintelligibles aujourd'hui sans la tradition.—*Lacordaire, Silb. Revue*, vol. iv. p. 234.

Confusion arises from several causes; primarily from the difficulty there is in many instances of correctly ascertaining the name given by the first describer, from the description being so vague and indefinite as to preclude the possibility of accurately determining the species intended.—*J. F. Stephens, Cat. British Insects*, p. iii.

Wollen wir nur sogenannte genügende Beschreibungen anerkennen und die vermeintlich ungenügenden ausschliessen oder zurücksetzen, so bemerke ich, dass weitaus die grösste Zahl der Linne'schen, Scopoli'schen, Fabricius'schen, u. s. w. Beschreibungen zum sichern Erkennen des gemeinten Objectes faktisch und unbestreitbar ungenügend sind, dass fast sämmtliche älteren Autoren nach den heutigen Anforderungen der Wissenschaft unzureichende Diagnosen gegeben haben, und dass dasselbe bei allen Arten aus solchen Gruppen oder Gattungen der Fall ist, die seit längerer Zeit nicht mehr in guten Monographien revidirt worden sind. Wer frage ich, ist im Stande, wenn er nicht schon anderweitig instruirt ist, aus den Werken des Linné, des Herbst, des Fabricius auch nur einen *Harpalus* eine *Haltica*, eine *Nitidula*, etc., mit Sicherheit zu bestimmen? Fast ansnahmslos sind die Arten dieser Autoren nur mit Zuhülfenahme der Typen, der ferneren Angaben ihrer Zeitgenossen und sonstiger erfinderischer Hilfsmittel gedeutet worden, ein Verfahren, das auch ich von meinem Standpunkte aus volkommen billige, welches aber nur beweist, dass die Beschreibungen allein den Zweck der Kenntlichmachung der Thiere absolut verfehlen.—*E. von Harold, Coleopterologische Hefte*, vi. pp. 45, 46.

Cette règle est trop absolue, car ce qui est reconnaissable pour un entomologiste ne l'est pas pour un autre, et, si elle était prise au sérieux, les espèces de Linné devraient disparaître de la nomenclature comme celles mêmes de Fabricius.—*L. Reiche, Ann. Soc. Ent. France, 3rd Series*, vol. vii. p. 609.

Mr. W. F. Kirby has (in Trans. Ent. Soc. Lond. 1870, p. 133) expressed a contrary opinion, but there is a chorus among the writers which supports the view put forward in the text. M. Guenée, however, entertains Mr. Kirby's view: see Noctuélites, vol. i. p. liii. In support of the present writer's opinion may be quoted (besides the above cited and some other writers) Dr. Staudinger, Cat. 1871, pref. pp. xvi, xvii; Mr. M'Lachlan, Ent. Ann. 1865, p. 29; Mr. Rye, Ent. Ann. 1869, p. 53; the Dresden Congress (von Kiesenwetter), Berlin, Entom. Zeitsch. vol. ii. app. xvii.; and Dr. Thorell, European Spiders, p. 15.

saved from oblivion, only by the actual type specimens being preserved. At the time the description was written the case was often different. The insect, the subject of the description, was sufficiently "differentiated" (as the phrase goes), from all species then known by the line or two of Latin recording its characters. But now we may be acquainted with as many as a dozen acknowledged species, any one of which may be the insect for which the old description was intended.

I do not clamour against the old authors for this result. It is a natural thing to expect, and if the science of Entomology was to have made no advance since the time of Linnæus, many lives would have been lived in vain. On the contrary, we are thankful to the writers who with their opportunities did their best for Science, and who, not leaving us, their descendants, to start afresh, recorded and published to our advantage the results of their observations. The failure of their descriptions is soon compensated by a number of new ones, the work of naturalists with more extended knowledge. Our characters of genera and species are found in works much more modern, much more free from errors, much more complete. There is no use in not acknowledging that the works of the early writers might well, for all the practical uses which they serve, be left on the topmost shelf, but to be reverently dusted now and again by the hands of the grateful naturalist. Thus, having honourably played their part, and being honourably superseded by better work, they would remain a testimony of great things done with few materials, and retain the regard of their writers' posterity. But this repose the strict application of the law of priority forbids them.

It is alleged, by those who ought to know, that a writer regards it as a feather in his cap to describe a species, that to take rank as a nomenclator is to don the purple, and that to achieve this distinction entomologists are ready to be dishonest; that they would describe old species as new, naming them afresh; and that Rule One of this Association alone prevents this practice becoming general. I am not in a position to dispute the whole of this statement; but if a writer is ready to be dishonest,—to secure an advantage to his reputation, which is what we are so briskly informed—he will not be *prevented* by the law of priority. What the aspirant to fame will do is simply this; he will bring out A SYNONYMIC LIST, containing a selection from the old names

no one has been able to fit to any species, all fitted neatly to species already described and known by later names, which of course are thenceforth superseded. And he will have his reward just as he would have if he took rank as a nomenclator. Brown's new list is quite as great a fact as Dümmer's three new species; and Brown and Dümmer may fight out between themselves the battle for empirical ascendancy. The old inferior descriptions make the task of Mr. Brown a very easy one, while Dümmer's pretended novelties are pretty sure to undergo, and as a fact constantly do undergo, a speedy detection and exposure. Of the two empirics I would rather be relieved from the list-maker.

On this point it has been remarked (see p. 8 of the British Association Rules), that to allow the erasure of an original name under any circumstances "opens a door to obscure pretenders for dragging themselves into notice at the expense of original observers." But the door is now agape, as I insist, for pretenders as "obscure" as those against whom the passage is directed, who drag themselves into a notice much more notorious. The ideal poor describer, who brings out an old species under a new name, gains thereby what he seeks, a little adventitious importance, and there his influence stops; he has done science no harm whatever. Science is supremely indifferent about names, and if this section should agree to pronounce every Latin name backwards and scratch out all the vowels, the injury to Science would be *nil*. The mere wrongful supplanting of an old name by the new one of the obscure pretender is not a matter about which Science troubles itself at all. But the pretender of the other class which I have already mentioned, for whom the law of priority pushes wide the door, is a person whose influence is indeed to be dreaded, and the occupation by *him* of a conspicuous place among men of science is a misfortune, little short of a calamity. The person I refer to is the *List-maker*, or Catalogologist.

The usual way in which old or re-discovered names are brought to the notice of naturalists is by the publication of corrected lists of species, in which the "prior" names appear instead of the ones in use. These lists of names, publications of no intrinsic merit, and supplying absolutely no test of their author's worth, are, I say unhesitatingly, the publications which are in Entomology regarded as of most importance and are now most widely studied. The author of one of these publications is the person I mean by a list-maker.

These lists, and their unnatural importance, furnish matter for grave consideration for those who have at heart the interests of science. Now, lists of names must observe some certain order; and the order of the names is also the order of *the species*. The list-maker, then, cannot publish his paltry work (*h*) without at the same time trespassing on the great department of Classification. It is notorious that most important changes in classification are continually introduced by synonymic Lists, and these list-writers are actually pitch-forked into the position of founders of systems of arrangement! The one glaring instance of this in Entomology must be known to many present. A French author actually preferred, though himself a prolific book-writer, to give to the world his ideas on classification through the medium of a Synonymic List, intended for labelling collections, and published in London. That list, bought for labelling by a thousand collectors, spread far and wide the new order of arrangement of the Lepidoptera, and Mr. Doubleday, its author, as if by magic found himself the founder of a system, in defence of which neither he nor anyone else ever penned an article. This List utterly subverted all existing or previous arrangements, and its history furnishes an only too significant proof that Entomology is at the mercy of list-makers. But, generally, the publication of a list of names, unaccompanied by reasons, furnishes an easy and tempting opportunity for airing a crotchet. Consequently, no man's list follows exactly the work of any one else; and it is scarcely straining language to say that there are as many systems of arrangement as there are lists of names! The changes in arrangement thus introduced may be, and have been exposed as being, unfounded, mistaken, happy-go-lucky changes. But changes so made are, it is only too true, made effectually and endure.

A list of names, I have said, finds very wide circulation among practical entomologists. They will always, with the strict law of priority raging like Sirius over their devoted heads, be compelled to buy this trash in order to keep pace with what is facetiously termed the advance of science, that is, to learn the new names. The advance of science, save

(*h*) "His paltry work." See Lacordaire's opinion of the list reformers of his day (Silb. Revue, vol. iv. p. 231). He styles their performances L'ERUDITION À BON MARCHÉ; our phrase is "cheap learning."

the mark! Can it advance science to alter the name (*i*) of a species, or to occupy time and words in frivolous discussion on a name, when there is such a field (*j*) for observation not explored? The works written when our science was in its infancy are ransacked by these "resurrection men," with the avowed and sole object of bringing out a list of old names. What possible service these persons persuade themselves that they do for us or for science I am at a loss to surmise, but I am not at a loss to know that they are on their own quest after notoriety. Here you detect the plotting of your real "pretender." Brown can make himself vastly more important by using in his list a new order, as well as new names. Besides earning the reputation of an industrious book-worm, he gets perhaps the renown of a spirited reformer of classification: so a new order he without loss of time invents. This new order once started is fated to secure a wide reception; entomologists buy the lists, and find they can only use the names by adopting the families and genera of the author, and so they go with him the whole way. The world has then been saddled with "Brown's arrangement," and I wish the world joy of its acquisition.

To be serious, how monstrous a thing it is that a bare array of names shaken into a certain order shall found a system of arrangement! How detrimental and degrading to Science that the great department Classification is directed by persons whose fitness to direct is so manifestly untested; and all this arises from a heedless interpretation of the law of priority! If it is possible to restore to this branch

(*i*) Je me creuse en vain la tête pour découvrir quelle vertu particulière lui donne la vétusté quand l'usage ne l'a pas consacré; je ne peux voir là qu'un hommage rendu à la vanité des individus, et qu'importent les individus en comparaison des intérêts de la science? Et à ce sujet, je concevrais l'ardeur avec laquelle certains entomologistes maintiennent leur droit de priorité, si la matière manquait et que les insectes fussent sur le point d'être epuisés; mais loin de là: il y en a pour qui veut en prendre, et il y en aura encore pour nos neveux et nos arrière-neveux.—*Lacordaire, Silb. Revue*, vol. iv. p. 236.

(*j*) A PERDRE MON TEMPS à décider la question de priorité.—*Lacordaire, Silb. Revue*, vol. iv. p. 236.

The individual who sits in his library . . . turning out descriptions of improbable species, at the rate of so many per hour, is apt to imagine that *his* occupation constitutes Entomology, and, as a consequence, he too often looks down upon the poor fly-catcher with something like contempt; but, for all that, the despised collector often, of the two, does the more for science, by which is here meant . . . not the art of piling up a synonymy for the bewilderment of future generations.—*Dr. H. G. Knaggs, Lepidopterist's Guide*, 1st edit. p. 119. The writer of this spirited sentence very soon afterwards himself published a List, and announced another as in preparation. I appeal to Philip sober from Philip "piling up a synonymy."

of science a healthy condition, you will do your best to secure that result, and I confidently urge that nothing is necessary for that end but a small modification (if modification be required) of this rule. It would be infatuation to serve blindly under every word of a code drawn up thirty years since, if the long interval which has elapsed shows to your satisfaction a point on which its working can be improved. The proper course, and the scientific course, is to amend it in that particular. By agreeing on this amendment we shall be doing Science a real service.

I propose *that where there is now* (Aug. 1871) *a universal agreement on a specific name, the name shall not be displaced on account of any prior name being discovered.* As to the names on which all are agreed, the worst that can be said is that the world is now in universal error, and *communis error facit jus.*

We have seen that, names and name-finders having assumed an extravagant importance, the literature of Entomology has run to catalogues. Year by year, entomologists are harassed by the announcement of some catalogue or list merely published to change specific names, while, asI have said, they wait and wait in vain for the appearance of descriptive or general books written on the science. The result is that *Classification*, for instance, is utterly neglected. Any new views on classification are introduced by their originator through the transposition of two or three groups in the next list; and people follow the new order, or do not follow it, according to their belief in the writer's leadership. To give *a reason* for a change in the position of a genus or family never occurs to these writers, and they never do give a reason. A barren list is made to serve the function of *a treatise;* and entomologists are left to guess the reasons for themselves.

The result in turn of this condition of affairs might easily have been foretold. As the authors of these productions give no reason for their schemes of classification, *no criticisms of them are delivered. No comparison of their merits is effected.* Each scheme receives some support, and soon the writer of each has his band of followers. The entomologists have never had explained to them the reasons for their leader's plan; they follow blindly, content to declare themselves the disciples of a writer from whom they learn nothing. The spectacle presented is this: half the students of the order of insects most studied (the Lepidoptera)

follow one scheme of classification, half follow another. Neither party can tell why or wherefore (*k*); they follow unreasoning wherever they are led. You have here a kind of degraded hero-worship very pitiable to meet with in Science.

If the law of priority be declared to have the modified interpretation contended for, the *raison d'être* of the lists will disappear. There will be no acceptance then for resurrection men, or their unsavoury labours, and a more wholesome atmosphere will be enjoyed by all.

The overturning of names we are agreed on at the bidding of the unbending law is a needless annoyance, and the work of correction has brought in its train the great abuses which I have detailed. There is nothing in the spirit of Science to require a heedless subservience to this supposed divine right of a nomenclator; and the present amendment is urgently called for, for the reasons stated. Let me not be misunderstood as to the application of this amendment. The law of priority will apply with all strictness to names hereafter given. Only let us refuse attention to any more resurrection men. Let the art and practice of exfodiation perish; let us have no more obscure and doubtful names forced on our acceptance, to the displacement of names we are all agreed upon, and no more paltry contention about old names at all. Common sense requires this provision. Names and words have grown obsolete in the realm of Science as in other realms; it is folly to seek to restore them. There is no reason requiring this to be done, where universal agreement has accepted a different name.

The lists and catalogues which do such infinite harm are only published to bring out these forgotten names, and will cease to be published when the forgotten names are refused acceptance. We shall begin to have books instead of catalogues, and entomologists will take to advancing the Science as it is at present, instead of harking back to investigate the period of its infancy. Relieved of the incubus of trifling and inferior works, the literature of Entomology will become worthy of the really advanced

(*k*) British Entomologists are apt to take the position of groups for granted, and to create imaginary links of exotic species to fill up any gap, however wide.—*Dr. R. C. R. Jordan, Ent. Mo. Mag.* vol. vi. p. 152.

The writer has already expressed his views on this unpromising state of affairs, in *Trans. Ent. Soc.* 1871, part 3, pp. 342–343, and 346—352.

condition of that science. The frivolous occupation of changing names being denied them, even the busy list-makers may produce something worthy of their industry. When names, which have taken up so much attention (*l*), occupy it no longer, the whole body of entomologists will find other and profitable fields for study, observation and research, and immediate advantage to Science must be the satisfactory result.

(*l*). Ces questions de mots qui nuisent à l'étude exclusive des faits et des idées.—*Guenée, Lepidopt.* vol. 9, p. xxxi.

Les empiétements et transformations continuelles qui sont le plus grand fléau de notre époque. . . . La fixité et la consistance sans lesquelles notre science finira par devenir une fatigue au lieu d'un délassement.—*Id.* p. xxxvi.

"Synonymic Lists and Certainty in Nomenclature.

"The appearance of Mr. Kirby's 'Synonymic Catalogue of Diurnal Lepidoptera,' just as the controversy on the law of priority is at its height, has prompted me to make my further contribution to the discussion at the present time instead of waiting (as I had intended to do) until after more entomologists have given us the benefit of their opinions. It is not too much to say that, since the publication of this work, the aspect of the question for us has wholly altered. Arguments which have been put forward on either side appear now weak and beside the question in face of the serious conclusion to which we must find ourselves driven. Hence I abstain from an endeavour to answer the different points made by those who have taken a view opposite to mine. They and I have to deal now with such fresh considerations as will, I prophesy, make converts to the good cause neither few nor feeble.

"Mr. Kirby's Catalogue coming on the top of Dr. Staudinger's Catalogue is in time to teach us a most salutary and useful lesson. The least willing scholar must now be forced to learn that *never by means of synonymic lists shall we attain to certainty in nomenclature;* and we shall be wise indeed not to neglect the warning.

"Let us review the circumstances in which we find ourselves placed. After many years of tinkering and alteration, it was known that our lists of the *Rhopalocera* were undergoing a studious revision at the hands of two thoroughly competent Lepidopterists. These authors were working independently and simultaneously; Dr. Staudinger upon the European *Macro-Lepidoptera*, Mr. Kirby on the Diurnal *Lepidoptera* of the World. Each author has, as the results have shown, devoted to his work the very utmost research. No hole or corner remains unexplored; we have achieved the *maximum* of discovery.

"The result to be expected from the labours of these gentlemen was at all events not less than this,—that we should ascertain with certainty what names are, *according to the*

law which they recognize, the right names for our most familiar butterflies. The result is that upon this matter we are more hopelessly and *irremediably* at a loss than ever before in the history of Science. I do hope entomologists will appreciate the gravity of the situation; and that I shall not be deemed a trespasser if I occupy some little space in pointing the moral which it teaches.

"The work common to both authors is the revision of the European *Rhopalocera*. I must content myself with examining the results with reference to the 65 British Butterflies. This modest group is better known than any other equal number of insects; and if upon the names of these familiar objects there is a hopeless disagreement, we have the grounds for forming an opinion what are the chances of our arriving at certainty in the obscurer groups. The nett results of the two lists together are:—

(1) Of the sixty-five (*m*) specific names in Stainton's Manual *seventeen* are wrong and must be abandoned.

(2) In *nine* cases Dr. Staudinger and Mr. Kirby DISAGREE on the name.

(3) In *four* cases, though agreeing that our name is wrong, they differ on the question which name shall supplant it.

"And these most extraordinary results are arrived at, notwithstanding that both authors have in almost every instance made identically the same references! The differences are not to be explained on the supposition that one or the other author has been more painstaking in his researches. Their work shows that each has used the same sources of information.

"Now, the 'law' of priority was thought by its partisans to be an infallible guide to certainty in nomenclature. Both these authors are strenuous supporters of the so-called law. Where are we to look to find the explanation of this lamentable break-down? We find it in this,—the radical unsoundness of the principle on which their work proceeds. That principle requires an acceptance of the earliest discoverable name which can be determined to represent the species. But the early descriptions are so insufficient and defective that perpetual disagreement must take place on the question to what species the descriptions and names do refer. Indeed, if the law of priority had been

(*m*) The number is sixty-six, counting *P. Artaxerxes;* the above analysis takes no notice of this name, which Dr. Staudinger sinks as a synonym.

invented by that personage whose pleasure it is to see men in everlasting dispute, it could not more thoroughly have secured the object. When discovery has reached its acme, we are deeper in bewilderment than ever. We are suffering, in this fresh disturbance of our nomenclature, an uncertainty arising from the *differences of opinion* of two list-makers,—differences of opinion, for instance, on the stirring and important scientific question, whether a faulty description can or cannot be recognized! Is it to be tolerated that our nomenclature shall remain for ever unsettled out of subservience to a fallacious 'law,' vaunted as being a sure guide to certainty, but seen to ensure a proportion of endless disagreement? It is clear that on the names on which they differ now, Mr. Kirby and Dr. Staudinger will differ for ever. Who will judge? Are we all, each for himself, to take to the work of exfodiation? Which guide are we to choose, and why? I shall be heartily glad to have this position made plain for me by some of the smart resurrection-men.

"I have abstracted the results only as to sixty-five well-known insects. Dr. Staudinger's Catalogue enumerates 2849 species to the end of the *Geometræ* alone. Does any one believe that if Mr. Kirby had brought out a list of European *Heterocera*, the work would not throughout have exhibited the same proportion of differences? The position at this moment would be that we should have two spic-and-span new lists (hot from the press, and teeming with the results of most recent and exact investigation), working a change in *one-fourth* of our specific names, and differing between each other in *one-seventh*. Monstrous and horrid result! But does anyone believe there are not cases upon cases in which there is room for a new list-maker to contend that both these authors are wrong? Would not a Catalogue by M. Guenée exhibit discrepancies on every page from both these brand-new works? Is there anyone who for a moment doubts it?

"We are now *not* in a state of transition from error to truth. We are face to face with a dead-lock. The passion for change has hurled us where we are; and its operation is traced as plainly as the track of a torrent. Authors were never satisfied with the names they found in use, and did not confine their labours to establishing which of two living names was preferable. They set to work to search the old and inexact descriptions, till they reached some so unrecognizable that one or the other could not bring

himself to accept them. Now they differ, and we are left at their mercy (*n*).

"But there is still a remedy. Refuse acceptance to these new names, one and all. Treat them as the things which for the most part they are, a jumble of letters not accurately referable to any certain species. Let us adhere to the *accepted* names, approved by universal consent, which we are accustomed to use. Preserve the living names, ignore the dead. So only shall we achieve, in spite of the mischievous (*nn*) stalking-horse, 'priority,' that certainty in nomenclature, the chance of which through it we have nearly lost for ever. Besides accomplishing this we shall have gotten for ourselves an even more valuable result. We shall have administered a check to a class of publications which tend more than any other agency to impoverish the literature of science. While authors find the roll of fame is supplied by the wrapper of a catalogue, the best may be content to inscribe their names only on that worthless sheet. The hot quest of ephemeral notoriety once made profitless, we may expect that more energies will be directed to the production of serviceable books. Rely upon it, to encourage innovating synonymic lists is, in every way possible, to retard entomological science (*o*). This branch of natural history, from the small differences which divide its subjects and the inequality of nearly all descriptions accurately to

(*n*) Que faire alors? On a dans ce cas, me direz-vous, le droit de choisir. Mais, si j'adopte le nom de Pierre, et mon voisin celui de Paul, qui nous mettra d'accord, et la question ne reste-t-elle pas la même? Et que sera-ce pour nos descendants, s'il leur prend envie de peser les droits respectifs de l'effroyable amas d'opuscules que nous sommes en train de fabriquer?—*Lacordaire Silb. Revue*, vol. iv., p. 229.

(*nn*) Funeste pour l'entomologie.—*Dejean, Catal. des Coleoptères* (1837) p. x.

L'entomologie est maintenant un dédale, où il est impossible de se reconnaître, et la première cause de ce mal est ce faux principe de l'adoption exclusive du nom le plus anciennement publié.—*Id., ubi suprà.*

(*o*) These expressions were objected to as "dogmatic" and "extraordinary." Mr. M'Lachlan, F.L.S., does not agree with John Curtis: "Nothing is so *likely to retard if not to overthrow Science* as encumbering it with unnecessary names."—Brit. Ent. pl. 268. Nor with J. F. Stephens: "It is *detrimental to the progress of Science* to alter a name without powerful reasons."—Illustr. Haust. vol. i. p. 45. For the opinion of the last-named author on lists of names, refer also to Haust. vol. iii., p. 90, note. See M. Guenée's opinion, Lepidopt. vol. 9, p. xxxiii. For an opinion on the effect of recent changes upon the advance of Science, see 5 Ent. Mo. Mag. 210, and Proc. Ent. Soc. for Dec. 7, 1868; also the 'Zoologist' for January, 1872, p. 2894. And Ann. Soc. Entom. de Belgique, vol. 14, pp. cxxxi., cxxxii., comptes rendus.

express such differences, baffles (*p*) the 'law' of priority to fix its nomenclature; and sooner or later that conviction must force itself upon all.

"W. ARNOLD LEWIS.

"Temple, Sept. 9, 1871."

(*p*) Je n'ai qu'une seule objection à lui faire: c'est qu'il est complétement et radicalement inexécutable *dans l'application.—Lacordaire, Silb. Revue*, vol. iv. p. 225.

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