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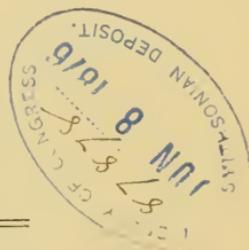
ENTOMOLOGICAL SOCIETY

OF

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APPENDIX.

On Entomological Nomenclature, and the Rule of Priority.
By W. ARNOLD LEWIS, F.L.S.

[Read 1st February, 1875.]

SINCE last the subject of Nomenclature came before us, a great deal has been done. The movement in favour of retaining universally accepted names, which originated at one of our meetings, has received efficient support in widely different quarters. One prominent entomologist on the Continent of Europe has declared himself free from obedience to the rule of priority; another in the New World has replied to the proposed changes of names that his motto is "*Resist innovation*;" while at the present time the entomologists who are his fellow-countrymen are formally re-considering the laws of Nomenclature at the instance of those who support the reform in question. The discussion has covered many pages; and the same reasoning which was put forward here has occupied large numbers who have shown an interest in the subject. The author of one of the chief synonymic lists has come forward to render explanations of some points urged against the observance of the strict priority rule, and based on discrepancies apparent from his own important work. From the signs which appear, it is high tide now on this question. The arguments have reached an advanced stage; yet I think there is reason to fear that some who have expressed themselves as adverse to the restoration of forgotten names, have nevertheless failed to seize an essential point, and are in danger of missing the object aimed at. For taking up the thread again, reviewing some part of what has been said and done, and making clear the questions in dispute,—for pointing out (I venture also to hope) some considerations which are of importance,—the present appears a fitting time. With these objects at all events, I offer the remarks which are to follow.

Now what are the main points relied on by those who desire the preservation of names in use? I think they can be taken up successively in such a manner as to make them clear to everyone.

The Meaning of the Rule of Priority.

In the first place, we ask those interested in the discussion to examine with us the circumstances under which the laws of

our Nomenclature were made, with a view to the inquiry what meaning the Rule of Priority was intended to bear.

Linné, who invented Nomenclature, and Fabricius, who first formulated rules for Entomological Nomenclature, did not invent, and never heard of a rule of priority. Linné and Fabricius were very far from being guided by priority; and, as is well known to every investigator, both of them changed names as and when they chose. For some forty years after the death of Linné, a general principle of priority seems never to have occurred to anyone; that is to say, all the writers who described species in the infancy of our science did so before "priority" was born or thought of. On the one hand, then, they disregarded no law when they "re-named" a species; and on the other hand, they did not pen their descriptions in any reliance on a rule which, in their time, had no existence. Thus they were in no respect wrongdoers; but neither had their work the sanction of the law, which otherwise it might plausibly be urged we should be wrong now to modify to their disadvantage. This fact will supply some useful considerations when we come to consider the element of "justice" to the first nomenclator. The authors who gave specific names under no law of priority, were, besides Linné and Fabricius, De Geer, Poda, Scopoli, Schoefier, Hufnagel, Schrank, Fuessly, Sulzer, Cramer, Stoll, Knoch, Esper, Engramelle, Scriba, and Borkhausen; and all from whose works the disused names are to-day being disinterred.

Latreille is credited with originating the *principle* of maintaining the prior name, and the proposal appears to have been made shortly previous to 1825. The proposal, when he made it, came as a perfect novelty, for the reception which it met with shows plainly that "priority" was a strange thing to all. Dejean, who at this period commenced his descriptive work on the whole of the *Coleoptera*,* takes notice of the new suggestion only to scout it, and (thus early in the bibliography of the science) declares himself as deciding questions of nomenclature on the principle of upholding names generally employed. In 1834, Lacordaire wrote an essay† of an elaborate character, in which he set himself to prove that to endeavour to decide the priority of names was from the infirmities of the old descriptions impossible and a mere waste of time; and he summed up his arguments in one objection, that the plan was completely and radically "impracticable in the application." In these noteworthy observations he was warmly supported by Silberman;‡ and I think there is little doubt about the fact that Lacordaire had the suffrages of entomologists. Although the principle had been started some twenty years before, I believe it is the fact that until the British Association Rules of 1842, "priority" to intents and purposes remained a theory. Only

* *Species Général* (1825), vol. i. p. x.

† *Silb. Revue*, vol. iv. 233.

‡ *Silb. Revue*, vol. iv. 241.

in 1840 Boisduval published the second edition of his "Index Methodicus," in which he expressly declined to supersede names in use by names which had been forgotten.*

What was the state of things here in 1842? Entomological science, though not still in its infancy, was yet receiving the attention of a limited number. The movement which has since made entomology the most popular of all the sciences was, however, near its birth. Stephens' "Illustrations" and Curtis' "British Entomology" were (so far as they ever were) one quite, the other nearly, completed; but neither do those authors communicate any information on the state of development which entomology had attained in other countries, nor does it appear to have been the case that English naturalists (whether authors or readers) had any knowledge worth speaking about on the subject. England, however, was the country which had the deepest interest in securing a uniform nomenclature. The position of isolation, which to-day remains as a geographical fact alone, was in 1842 a real separation from community in study and language, in entomology as in everything else. The countries of the Continent rubbed along together, but if English naturalists were to be *au courant* with the state of science, or (vulgarly speaking) were to have a finger in the pie at all, it was imperative that they and the naturalists of the Continent should use the same nomenclature. What difficulties met them at the outset? Here is the well-known description in the language of Mr. Strickland:—

"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."

These facts being recognized, the English naturalists set themselves to find the remedy. There was little doubt that in the majority of cases where the English names differed from the Continental names the former were wrong. The British Association† appointed a committee, and the committee (adopt-

* Index Methodicus, 2nd ed. vi.

I have discovered in the Entom. Mag. vol. i. p. 225 (1833), the phrase "the now universally received law of priority;" but the writer's wish was, it would seem, father to the thought. Instances to the contrary might be multiplied; it is worth while to mention Denny's *Monographia Anoplurorum Britanniae*, published in 1842. An inspection of this work seems to show that the author could never have heard of "priority" at all.

† The meeting in 1842 was held at Manchester. The rules are sometimes called the Manchester Rules.

ing the principle of priority as the basis) drew up rules which had for their first result the suppression of hundreds of names in use in this country, and in this country alone.

The object, then, of the British Association Rules was to reconcile the nomenclature of England and the Continent. The need to be supplied was agreement on scientific names *in the cases where the authors in use differed*. I take this point to be clear, partly from the surrounding circumstances at which we have glanced, and no less so from the interpretation which the rule of priority for many years on all hands received.

It would appear not to have occurred to those who framed the priority rule that neither the names in use on the Continent nor the names in use here should be correct; and in this confidence they unsuspectingly formulated their Rule I., that "the name originally given by the describer of a species should be permanently retained to the exclusion of all subsequent synonyms." For a period of years the rule received the interpretation which (as above indicated) it seems certain that it was meant to bear. The construction of it to mean that the earliest discoverable name shall be adopted to the displacement of all names in use, never, I believe, originated in this country, but has been caught up, as it seems, by some English entomologists from those on the Continent who had invented that construction. It is quite unbelievable that for twenty years the priority rule enacted by the British Association was *misunderstood* in the country where it originated, and where those who took part in framing it were continuing their scientific labours.

The Position since the Rule of Priority was made.

But let us suppose that by the law of 1842 it was intended to enact that the earliest discoverable name should supersede all others. Well, the originators of the law had not the advantages which we have. In the first place, their agreement was come to in comparative private. There was no endeavour to take into the account the practical students who might be conversant with special aspects of the case; and, as a bald fact, the matter received no sort of public discussion, of which all may satisfy themselves by searching the magazines and journals circulating at the time. I rest no especial weight on this circumstance; it is a good thing that naturalists of eminence were found to agree on a solution of the difficulty which was sadly wanted, and which did effect a practical settlement of our nomenclature. But I should omit an important consideration if I were to miss pointing out the great difference between 1842 and 1875.

In 1842 the domain of entomology appears to have been parted out on something of the feudal pattern; and the followers of Curtis and Stephens respectively were not only well content

to acquiesce in, but eager to further, the ascendancy of one or the other of these leaders. I have once before quoted Mr. Stainton's words on "the extreme seclusion" in which our entomologists lived. "Except a few of the leaders,"* he says, "literally no one knew anything." It is a fair argument which should give offence to no one, that a rule imposed when our science was in this obscured condition may well be open to review to-day, when a very large class of entomologists is, as I at least will assert, competent to form a sound and independent judgment on this matter.

But, with these reservations left aside, it must never be overlooked that we are viewing this question in the light of the *fresh experience* of more than a generation, and a generation, moreover, which has surpassed in results—and consequently been more prolific of experience than—the whole preceding period. The legislators of 1842 had made the discovery that the names employed here were different from those employed elsewhere, and they enacted a rule to cure the evil. The discovery which we in our turn have recently made is, it seems to me, as fresh a matter as that which opened the eyes of those who promoted the rules. We discovered between three and four years ago that the bare rule of priority (as construed now) has let in practices which promote and do not dissipate confusion. I put this as a discovery, and that word implies that in my judgment the truth of it is established. The main point, indeed, I rely on not as a prophecy or a predilection, but as a fact. But we have at present to consider a little further the historical aspect of the case.

In 1845, the British Association Rule was adopted by the American Association of Geologists and Naturalists. They seem to have merely "followed suit," and I think we are well justified in assuming (what appears to be confirmed by the present position of the question in America) that the rule of priority in America meant whatever it meant in Europe—no more and no less.

Naturalists who confined their attention to the British Fauna had little temptation to concern themselves with foreign books; they would have had to pick out the British species from a crowd of non-British. The central European Fauna is, however, in no respect limited by political boundaries, and the descriptive work which dealt with German insects answered pretty well for French. To the interchange of communication and common circulation of some descriptive works it is to be attributed that the position of nomenclature on the Continent gave less trouble than did ours; and to this circumstance in turn it may be owing that no rules for nomenclature were made on the Continent until many years later. By the year 1858, however, many on the Continent awakened to the circumstance

* Ent. Weekly Intelligencer, vol. v. p. 113.

that disagreements on nomenclature had arisen. In 1857 M. Guenée issued the last volume of the *Species Général des Lépidoptères*, in which work he undoubtedly brought forward some names unwelcome to the entomologists of Germany. Whatever the cause may have been, the German entomologists in 1858 called a Congress to establish rules for nomenclature. It duly met at Dresden, and its rules* were shortly afterwards published.

There is one great difference between the Manchester rules and the Dresden rules, and it is not a little instructive to remark it. The framers of our rules no doubt had before them only the object of reconciling the prevailing disagreements. The object of those who framed the Dresden rules, however, was to supply a standard of perfect accuracy, and the laws which they framed they intended not only to be of permanent authority, but also to comprehend all the aspects of the questions. Those who took part in the Congress were fully awake to the circumstance that names in use everywhere might be "wrong," for they had had some recent experience of the fact. How did they deal with the case? They first agreed in enacting "priority," in much the same language as our own rule; but by another rule, passed at the same time, they provided—what? "*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.*" Where entomologists had an eye to the point that no name in use might be the "prior" one, it is striking that the decision arrived at was—not the imperative acceptance of the prior name—but that the choice between the names should follow "the greater or less degree of *convenience.*"

We are coming to consider wrong and right, and it is not beside the question to recall that this code of rules is in operation in Germany at the present moment, while nevertheless it is from Germany that we are visited with the systematic intrusion of the first name on strict "priority" grounds. The very writers to whom we must attribute a familiarity with the Dresden code are the most unsparing in throwing all "degrees of convenience" to the winds, and even (it must be said) treating with derision those who have all the time this statute in their justification.

Immediately on the publication of the Dresden code, the Entomological Society of France had the matter before it, and M. Amyot, who took a leading part in the discussions which followed, formulated a set of rules.† It is only important to quote the one which provides that "*usage may consecrate injustices in the priority of names.*" There is no code drawn

* Berlin. Ent. Zeitsch, vol. ii. app.

† Ann. Soc. Ent. France, 3rd ser. vol. vii. 606.

up by a committee (so far as I am aware) in operation in France, but a translation of the Dresden code was printed and distributed with their Proceedings by the Entomological Society of France and the Entomological Society of Lyon.

The next important event after the Dresden code of 1858 was Dr. Staudinger's Catalogue of 1861, which I (in common with others) take as the starting-point of the modern objectionable practices. Before this, forgotten names had been here and there brought up in their monographs by different entomologists, and on no system in particular. *Nemo repente fit turpissimus*; I do not say Staudinger commenced all of a sudden a practice totally unheard of. But Staudinger's 1861 Catalogue was the first example of the resurrectionary literature which has since become familiar. It is striking to find this work published at Dresden in only the third year after the promulgation of the code.

From 1861 to 1871 the tide went in the direction of restoring the earliest discoverable names. It is sufficient to mention the names of Gemminger and von Harold, the late Mr. Crotch, Mr. Scudder, and Mr. Kirby to recall that the practice of "resurrection" resulted in the production of several volumes. In 1865 the British Association appointed a committee, which enacted with some slight alterations the rules of 1842 over again. These 1865 rules, however, were not the justification of practices which commenced in 1861, and it is well known that no reconsideration of the priority rule took place. The attention of the framers of the rules was given to the settlement of certain minor details.

If, however, the case had been otherwise, and we had to look upon the rules of 1865 as confirming "priority" pure and simple, which from the known opinions of some who took part in framing them we should be wrong in doing, yet the consideration remains that 1865 was too early to see this subject as it now is. Gemminger and von Harold were yet to publish their Munich Catalogue (not to mention other Coleopterological lists which had not then seen the light). Staudinger and Wocke's second Catalogue was yet to gild the fine gold of their first edition, and this work, with Mr. Kirby's Catalogue of Diurnal Lepidoptera, had not then proved how the early descriptions baffle the operation of "priority." Especially was it not then discovered that the early nomenclature itself is less contradictory and discordant than the commentaries and practice of editors of catalogues and other writers on synonymy. For it has only recently become apparent that the same reasons which make one author accept a name as "prior" make another reject it, and that this action on opposite or conflicting principles is producing irretrievable injury to the stability of our nomenclature. The question could never before be seen in the light in which now we see it, and such reasons have of late years

become operative in one direction, that there is a pressing necessity, as we urge, for a re-settlement of the priority rule in the interests of our science at large.

The Principle which regulates Nomenclature is Convenience; and Convenience requires that Accord shall be upheld.

Having glanced at the historical side of the question let me proceed to the next points. We invite those interested to consider with us the principle in dispute, and to join us in investigating one question of fact.

In recent years entomologists have set themselves to work to discover the earliest name for every species, for this has been the practice, as we have seen, of writers from 1861 to 1871. In the words of one * authority "a generation arose who knew nothing of, or overlooked the circumstances connected with its original proposal, and who took the letter of the rule as their guide. And gradually there has sprung up a class of authors who have devoted themselves with enthusiasm to exploring ancient works and forgotten publications of all sorts in the hunt for the earliest recorded name to every species by which to replace the name or names in use." As another † writer remarks, "A little band of so-called reformers discovered the 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."

What I may term the general argument put forward on the undesirability of this practice has not, I submit, been met with a satisfactory answer. I refer to that embodied in the axiom *Communis error facit jus*. Although the literature of this controversy has now grown to a considerable bulk, I conceive that the argument founded upon convenience and expediency remains as strong as (if it has not grown stronger than) it was when first put forward. Rather early in its history, the maxim had to encounter the opposition of those who misconstrued *communis*; more recently it has had to meet a criticism founded upon a fancy rendering of *jus*. It may therefore be worth while to expound the meaning which I give to this apparently troublesome aphorism. As to what it does *not* mean, "*communis error*" signifies "a mistake which is universal," and not "a vulgar error." *Jus* simply means "legal right," and that is all about it. The English of the maxim therefore is that "An error which is universal makes the legal right." I apply the maxim to the solemn question of nomenclature.

* Mr. Edwards; Canadian Ent. vol. v. p. 22.

† Mr. Newman; Zoologist, 2nd ser. 2877.

clature in this sense ; that there are cases where it may be we are in error in using a given name as the first name, but in those instances where we are all in the same error, right is done. The maxim does great credit to its inventor, who showed his sympathy with a just and natural human sentiment, at the same time that he threw into a proverb the *modus vivendi* which controls every one of us in daily life. In matters of positive enactment (*not* of course of scientific truth), what all, whether rightly or wrongly, are agreed on, is the law ; and I should have supposed this did not call for much explanation. There are enthusiasts who from time to time work their way into courts of justice who cannot, I grant, be prevailed on to acknowledge this axiom. They, I doubt not, would press on you the opinion that the unanimous consent which they encounter is a universal error in favour of their opponents and against themselves. To such the reply may be applicable : "To please you we will say we are wrong ; but we are all equally wrong together. At all events we are *agreed*."

Some pretty phrases (which certainly caused amusement if nothing else) found their way into print,* about the very essence of the studies of the naturalist being "the exposure and obliteration of error," and that there could, "in an *exact* science, be no 'common error.'" It certainly can never have occurred to some that there is a profound difference between facts in natural science, towards which men, after infinite study, occupy the position of mere learners, and the trumpery bye-laws of naturalists. When there was a universal agreement that the sun moved round the earth, I agree that it was necessary to abandon the universal error. The name of the Clouded Yellow butterfly is a matter on which universal agreement makes the right. The notion of an eternal right and wrong about the names of bugs appears to me a misconception ; and the allusion to "exact science" defeats me still as much as ever.

The law of priority is a means to an end, and the end in this case is accord or common agreement on a name. If you have agreement on a name, and that agreement can be made secure and permanent, you have already that which the law was designed to provide. The object of the law is the important thing ; not the law, which is only machinery. Then, if all names, save one for a species, are obsolete, you *are* enjoying agreement on the name. What is wanted you have ; better off you cannot be, and the most that can be done is to disestablish an accepted name in favour of one which at best has to fight for its position. One complaint therefore against those who insist on subverting accord by "priority" is that they put the means above the end sought.

* Entom. Monthly Mag. vol. viii. 41.

Accord on a name is not to be desired, as I shall take it, on principles of eternal truth; but it is imperatively necessary on the score of *convenience*. Those who use the names are men and not machines; the subject they have to deal with is enormously vast, and cannot be called easy; life is short. Cross purposes about names, and the trouble necessary to clear up or avoid them, are a serious matter for those who have their hands full already; but that is a small part of it. If names are continually changed, inter-communication is embarrassed, and the work of others becomes available only at a ruinous sacrifice of time and labour, which may frequently have the result of making an important work a closed book. Convenience cannot of course be paramount to the direct advantage of science, and if that could become an element in the discussion convenience would take the second place. But, as the case stands, there will be few who do not hold with me that convenience is the be-all and end-all of nomenclature.

I said just now that "if agreement on a name can be made *secure and permanent*," you have already that which the law of priority was designed to provide. If the agreement be not permanent it is illusory; and the only way in which it can be made permanent is by establishing it on a principle. Although, therefore, convenience is, I say, the be-all and end-all of nomenclature, it is none the less necessary to have a strict law. I argue that convenience requires that *accord* shall be upheld. The law to be aimed at, then, is a law to protect and render permanent names which are everywhere in use.

From the very first of the discussion this was the ground taken up. The independent assertion of wishes and predilections has formed no part of the battle. Where the object sought *ex concessis* is agreement, to stand wilfully on an original tack defeats the object; and the evils of this very course (and the hopeless prospect of agreement which it holds out) have been sufficiently dwelt upon. Those who have favoured the proposal which I put forward have done so on a principle which was plainly stated.

Being conscious of all this, I think those who have agreed with me have some reason to complain that Dr. Sharp has considered himself entitled to write of them as he has done. When undertaking a review of the question I should be wrong in passing by without notice his "Object and Method of Zoological Nomenclature," for several reasons. Dr. Sharp, after mentioning the evidence of identification which has to be collected, observes* :—"This will undoubtedly be a slow process, but it will be a sure one; and I may remind the impatient ones, who proclaim that we must have a way of settling such things right off, that they are, if they have any just voice in this matter, men of science as well as collectors, and, as such, they

* Object and Method of Zoological Nomenclature, pp. 31, 32.

will readily appreciate the association of the words 'ohne hast' with 'ohne rast.' Let them recollect that in these disputed points we wish to obtain a decision that shall be absolute, and not one that may be reversed on the first appeal. To enable us to do this, we must in each case carefully collect the evidence, and consider it under the light of reasonable and admitted principles."

Dr. Sharp must decide for himself how far Candèze, Leconte, Westwood, Wallace, Bates, Edwards and others (with whose names I can supply him from the list of "the impatient ones") merit the appellation of "men of science." That, however, is his matter. I am content to speak as one of the "collectors," and therefore desire explicitly to assert that everyone, who comes forward with coherent reasons, has "a just voice in this matter;" and the reasons which he may adduce, be they bad or good, are (it seems to me) not vanquished by terming their sponsor "an impatient one," nor even by charging that he "proclaims we must have a way of settling things right off." It is pretty evident from the rest of the passage that Dr. Sharp has never made himself adequately acquainted with the tenets of those he was addressing in this peculiar vein. The proposal to which Dr. Sharp alludes was and is founded on principle and supported by reasons; and to my surprise I find that Dr. Sharp's answer amounts to the statement that he thinks differently. That is scarcely, as I submit to entomologists, a good foundation for comments like the one just quoted, which have a strong family likeness to an avowal that those of Dr. Sharp's opinion are men of science, and those of the contrary opinion something different. I venture to think that when the reasoning on which they rely has been demolished, it will be time for Dr. Sharp to indulge in the inquiry whether those who meet him in argument are "men of science." While their reasoning remains unassailed, that seems little relevant to the matter under discussion.

The Oldest Descriptions are unrecognizable.

Having endeavoured to point out that the movement—being based on the sober and strictly-defined principle of convenience upholding accord—does not owe its birth to "impatience" or frolics of any description, I pass on to the question of fact which must be satisfactorily settled before we can do good by these discussions.

Having to bring forward the oldest name that they could find, entomologists searched the oldest books in which to find it. Their task was to see what insects the names in those books represented. From the way these old books are sometimes regarded, one could understand anybody who was innocent of all acquaintance with them, contracting the notion that they are grand old works, containing stores of valuable facts,

with sterling descriptions of species, original and masterly and systematic. The old books do not all merit this description. Perhaps, to avoid misconception, it is better to say at once that they all merit a different description. Let us start with this, that the knowledge of species which those writers possessed was restricted to comparatively a limited number in the case of each. When the author had but a small number before him for description, he would use only such of the characters of the species as served to distinguish each of them from others then known to him; and the better describer he was the more certain he would be to do it. But what use can now be made of descriptions so drawn up? This objection speaks for itself, and the truth of it must be plain to every one. So simple a matter did the "differentiation" of species at first appear, that the whole description was the insect's name. All the characters which separated a species from all others were conveyed in its name alone! The specific name (*nomen specificum*) in Linné's earlier works was, as has been clearly pointed out, "what to-day is called diagnosis."* This afterwards had to be discontinued, but a few Latin words (more often than not falling short of three lines of print), formed the usual "Linnean description" of a species. As to the extent of this objection: Linné described but 780 Lepidopterous insects, the number now known cannot be less than 30,000. Dozens of allied species all equally fit numbers of the old descriptions; and such descriptions are now necessarily of no value. On this ground alone, an enormously large proportion of the oldest descriptions are at the present day *unrecognizable*; and, since the discussion began, declarations have come from all sides establishing what I venture to consider is the agreement of entomologists on that point.

The discovery, however, is a very old one indeed, and appeared in print more than sixty years ago, from which it appears that the oldest descriptions became strictly unrecognizable very soon indeed after they were written. Schönherr even (1810) remarks † on "the incomprehensible and little available descriptions of the older writers." Lacordaire ‡ (1834) remarked that Linné and Fabricius were at that day "unintelligible without tradition." In the time of J. F. Stephens § "confusion arose primarily from the difficulty there was of ascertaining the first name given, from the description being so vague and indefinite as to preclude the possibility of accurately determining the species intended." M. Reiche has affirmed || that if the rule rejecting tradition were taken *au sérieux* the

* Hagen, *Can. Ent.* vol. vi. p. 165.

† *Synon. Insect.*, pref. iii.

‡ *Silb. Revue*, vol. iv. 234.

§ Stephens' *Cat. British Insects*, p. iii.

|| *Ann. Soc. Ent. France*, 3rd ser. vol. vii. 609.

Linnean and even the Fabrician species "would have to disappear from our nomenclature." Von Harold insists* that the greater part of Linné's, Scopoli's and Fabricius' descriptions, with others of the same period, are "plainly and beyond question insufficient for identification," and "fail entirely to differentiate the species." "Who," he demands, "is in such a position that he can with certainty point out in the works of Linné, Herbst or Fabricius, anything more than that a given beetle is a *Harpalus*, a *Halitica*, a *Nitidula*," etc.? Dr. Standinger agrees† that "names given by the old authors belong to such and such species only by a sort of tradition." Mr. Edwards remarks‡:—"The old authors had described but a few hundred species, and their descriptions were of the briefest. How brief an average example from Linnæus will show—'*Papilio Troilus*; wings tailed, black; fore wings with pale marginal spots, hind wings beneath with fulvous spots;' a description applicable perhaps to fifty species of *Papilio*."

It would only be overlaying the case to cite more instances after this "average example." A chief objection to restoring names attached to the oldest descriptions, then, is that as a body those descriptions are unrecognizable, and, in consequence, the names brought forward on the faith of them as a rule are of doubtful accuracy.

No person can demonstrate whether a given identification is wrong or right. Guesses decide the matter; and nothing can compel the list editors to make the same guess. Thereupon they make different guesses; you have a confusion that does not admit of being reconciled either by proof or persuasion.

The infirmity we have just been considering attaches to the best of the old descriptions, and for that matter will be found to attach in all probability to many of the descriptions published at the present day. Von Harold has this further passage§ on the point:—"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 perchance in the future depend on for distinguishing from some closely-related species one which first makes its appearance later, which we do not know in nature, but only from authors' description. Anyone who has occupied himself with drawing up analytical tables knows right well that separation of species 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."

* Coleopterologische Hefte, vi. pp. 45, 46.

† Cat. 1871, pref. xvi.

‡ Canadian Ent. vol. v. p. 23.

§ Coleopterologische Hefte, vi. p. 50.

See some similar observations by Professor Westwood in Mag. N. H. vol. ix. p. 561.

The works of the old authors present other characteristics, which are important to be considered. Many of the old authors were very ignorant persons indeed, and the problem of species and variety was not less confounding to them than to others. Many described varieties of all shades as separate species; on the other hand they not unfrequently described two nearly allied species as one insect, and it is a common thing to find the list-writers who scrutinize these descriptions coming to different determinations on this account. There are some proved instances of those authors describing species from damaged examples, and endowing the species with characters due to old age or rough treatment of the specimen. Then (as I fear) the old writers were not all what we term "conscientious;" and many copied copiously from others. The comparative isolation in which each author flourished perhaps made detection unlikely; and one of these borrowers would appear not to have held before his mind the notion that a rule of priority in the future would pry out his failings on the score of doing justice to him as a nomenclator. To quote Mr. Edwards once more:—"Besides the brevity of the old descriptions, many are defective from other causes. Often the two sexes received different names; often varieties were described as species; often damaged and broken specimens were described as perfect, the defects being cured by imagination; often figures were made by unskilled artists who omitted the specific characteristics; or the figures were coloured so poorly as to be incapable of identification; or were copies from copies, or copies from memory; and often descriptions were made from unreliable figures instead of from the insect."

Mr. Edwards takes as an example of insufficient description the *Papilio Troilus*, Linné. I recommend the *Satyridae* and *Lycenidae* to any one who desires to satisfy himself what some of the old descriptions are good for. These are large groups, each with a strong superficial likeness among the species; both families contain a number of common European insects which lend themselves to observation; the ocellated spots on the wings furnish characters sufficient to produce a glorious *farrago* of confusion, which the old authors were not the men to miss. But whether the illustration be sought among the *Papilionidae*, *Satyridae* or *Lycenidae*, or elsewhere, the thickest confusion is of course supplied in those groups which contain a number of closely-related species; and the genus *Limenitis* and its allies supply some instances which I shall advert to in another connection.

A solitary species like *Nemeobius Lucina*, for instance, which was not fairly open to be confused with others, has never as a fact had bestowed upon it any name but its own. In cases like this, synonymic list-writers have no service that they can do us.

The fatality is that the cases where a change is made are, from the necessity of the thing, cases of a species which has many allies, and there one change most frequently necessitates several. It begins by some writer (whom the rest have followed) mis-identifying an original description and ascribing the name accompanying it to the wrong insect. This, of course, occurs most frequently where there is a real similarity which misleads. But the writer (whom the rest have followed) has most often not made one mistake of the kind alone. If he has taken species A. to be indicated by the description meant for B., he has of course ascribed some different name to B., which is thus also wrongly named; and the correction of the first error involves the correction of the second error as well. Very lucky are we, if it ends there. More frequently there is a much longer chain of "rectifications," each furnishing ground for fresh differences of opinion and consequently fresh confusion.

Small wonder that, under these conditions, Mr. Newman remarks that "the object of names is frustrated;" or that another writer makes the comment that "undertaken to make an end of confusion the synonymic lists have done nothing but augment it;"* or that a third (Mr. Edwards), after considering the facts as they are, should sum up the prospect in these words:—

"The result of all these efforts at stability, for that is the avowed object of the advocates of rigid priority of date, is extreme confusion, instead of the agreement hoped for when the code of the British Association was adopted, and students of one branch of entomology at least are at a loss to know where the nomenclature stands to-day, and are very certain that under the present order of things there will not be a name familiar to them that twenty or fifty years hence will not be supplanted under the claims of priority."

A fourth † has observed: "The rule of absolute priority, adopted as paramount law by a few investigators, has already brought about such a state of things, and alone is capable of continuing it Whatever the strict law of priority theoretically should accomplish, we have seen but the beginning of the permanent confusion in which its practice results, and which its continuance as the fundamental law will hand down to the remotest generation."

On questions of this kind it is well to give chapter and verse, and there is no authority, I presume, better than a very prominent descriptive writer who has paid much attention to synonymy.

Let me reproduce a few sentences selected from similar ones

* Dr. Albert Breyer; *Ann. Soc. Ent. Belg.*, vol. xiv.; pp. cxxxi, cxxxii.

† Mr. Mead; *Canadian Ent.*, vol. v, 108, 109.

in M. Guenée's *Noctuélites*.* Of *Scopoli*, writes M. Guenée:—“His method has very little of the natural about it; his descriptions are for the greater part unintelligible, and his names completely arbitrary or wrongly applied. This writer then we must take small account of; for the rest he is little consulted, and no one has followed him.” Of *Schoeffer*, he says:—“His figures are as badly coloured as they are coarsely engraved, and in order to be recognizable had great need of the explanatory text of Panzer. Even with this addition his iconography is scarcely of any use save to clear up some passages of the ancient authors.” Of *Fabricius* himself, M. Guenée writes:—“The greater part of the species do not possess the characters of their section, and the 380 *Noctuae* which he has described are in reality thrown together without any order, and without any correlation between them. This makes the works of Fabricius an entirely unarranged repository, and much less useful than people have been accustomed to think it. You are obliged, in fact, to neglect a crowd of species which he created and named in visiting the different cabinets of Europe, because, after all the attention possible, you result almost always in uncertainty, or in finding over again a *Noctua* already given under other names.” Of *Goeze*, M. Guenée remarks:—“His work is not original in any respect. The considerable time which this voluminous compilation required by no means finds any justification in the utility of the book, and it is much better worth while to have recourse to the same sources as the author than to follow him in his errors and repetitions.”

Of *Esper*, M. Guenée says:—“This collection, extensive as it is, is at this day much neglected, and the work sells at an insignificant price, which must be attributed in the first place to the imperfect character of the figures, which are, in fact, the roughest for the age, and of which a certain quantity are unrecognizable As for the text, it is scarcely anything but one long compilation, to which is added a synonymy swelled by the diagnostical phrases, and sometimes by the old description of the authors whom he cites, but often applies wrongly. . . . Esper opened a disastrous road for science. I refer to the numerous varieties which he has figured as separate species, and to which he has given names which come forward to complicate our works without any use.”

Of *De Villers*, M. Guenée says that “his additions to the “*Systema Naturæ*” might have been used if he had taken any care to assure himself at the outset, that the species he had before his eyes were really those of Linné; but he has sometimes committed in this respect the wildest mistakes, so that

* See the chapter entitled “Classification et Bibliographie des *Noctuélites*” in vol. i. pp. xlix—xc.

we do not know at this day to what species his observations apply."

Of *Borkhausen*, M. Guenée writes:—"As to the specific portion it is very unequal. The descriptions of moderate length are faithful enough for the species which the author has seen in nature, but it is to be wished that all were in this position. Inspired by the desire to give a *complete* work Borkhausen took all the *Noctuae* which appeared to be wanting in his collection from authors who had preceded him, and described them on trust. You can tell what a wonderful muddle was bound to result from this exploit; it is not rare in fact to find the same *Noctua* under two, three, and even four different names. Happily, it is pretty easy to distinguish these borrowed descriptions, though the author had not the frankness to acknowledge them; but this research renders very troublesome the study of his work, which owes to this circumstance as well as the imperfection of its plan the neglect into which it has fallen."

Of *Illiger*, M. Guenée remarks, that in discussing the synonymy of different authors, "Illiger has not always been any more accurate, and it would be difficult to say whether he has cleared up or mystified the most."

It would be tedious to prolong such quotations. The mere titles of some of the works disclose the circumstance that fugitive productions of several countries are bearing a part in overturning our nomenclature, being vouched for some obsolete names bestowed without any system and under circumstances which surely do not merit that points should be strained in their favour. M. Breyer has remarked that "*the greatest number* of these untimely changes came about from investigating or rather from bringing again into memory works without serious scientific merit."

Mr. M'Lachlan, to whom I probably do no wrong in styling him the most uncompromising of my opponents, agrees that the writers who bring up the old names "in their reverence for old names raise ghosts, not entities; in other words, they seek to overthrow names thoroughly substantiated to give place to others, nine-tenths of which have the merest shadow of a right to the superior position their admirers would allot to them—names that should sink into oblivion or rest quietly in the list of *species indeterminatae*."*

* Entom. Monthly Mag. vol. viii. p. 40. See also Mon. Brit. Caddisflies; Trans. Ent. Soc. Lond. 3rd ser. vol. v. p. 2, note. [In "Trichoptera of the European Fauna," p. 100 (May, 1875), Mr. M'Lachlan remarks of "several not identified species of *Phryganea*, described by authors from the time of Linné up to 1830":—"It is just possible that some of these may hereafter be made clear, but for the majority I consider it hopeless and useless to indulge in speculations as to what may have been intended."]

The works which have been mentioned and others such as those, it is said, must be ransacked and scrutinized with the object of disinterring the names found there, to replace the names in use! Now that we are fresh from examining a few of these books can we treat with gravity such a proposition as this? What could prompt the framers of any rules to set our entomologists to such House of Detention work? Have our most laborious writers nothing better to occupy themselves with than the puzzling out of these conundrums? What shadow of obligation is there that author after author should sharpen his wits to form a theory about the meaning of this or that third or fifth rate author's bad descriptions?

Things to me somewhat incomprehensible have been written on this point. It is said that "the existence of synonymy is too often owing to what are actual *crimes* against science," and that "when an entomologist describes an insect as new, without using every endeavour that is humanly possible to discover whether it be not already described, he commits one of the greatest crimes against science." I have elsewhere* described this language as of the high-falutin order, and must confess to experiencing some impatience at having soberly to reply to such declarations. Why should anybody be required to wade through "a chaos of blunders" before he is permitted to give to the world his own elucidations (or opinions for that matter) on a subject he may have investigated? Considering that this kind of *travaux forcés* has been in fact shunned by a crowd of prominent writers, I confess to further impatience when, at this time of day, "crimes" are constructed out of the practice which has been prevalent during all but the most recent period of modern entomological literature. The character of the old works has been examined; and before we censure those to whom we are beholden for the more modern (from which, in fact, we derive the degree of enlightenment we possess), for myself, at least, I should like to hear some good reason adduced. If every writer were forced to guess for himself the riddles provided for his entertainment by the first nomenclators, many would stop there and never get any further.

The Demand for "Justice" to the first Nomenclator cannot prevail.

Let me now pass on to take notice of an objection, viz., that if we deviate from absolute priority *per se*, we are wanting in "justice to the first nomenclator." At a very early stage of the controversy (in the course of some remarks published in the Entomologist's Monthly Magazine)† I said that, being an *ad populum* argument, I feared this might prove an influential

* Discussion of the Law of Priority in Entomological Nomenclature, p. 5, &c.

† Entom. Monthly Mag. vol. viii, pp. 1—5.

one with many. The history of it shows how one may be mistaken in estimating the strong points of an adversary's case ; for the argument, which made me anxious, is one which nearly all those who have come forward on the matter have agreed in condemning, and which, in two noteworthy instances, writers on the opposite side have repudiated. Amidst what I may term the chorus of agreement on this matter a discordant voice arises. Dr. Gray announces that the protest which has received the signatures of a majority of this Society "is decidedly against all proper treatment of our predecessors," and Mr. E. C. Rye apparently considers that, by quoting this statement, he can administer such a knock-down blow to all of us that he copies it out after his manner in the *Entomologist's Annual* for 1873.

Dr. Gray on this reason brings himself to the conclusion that the protest which we signed "can only have been put forward by mere butterfly collectors who have had no proper scientific training." It would not be worth while to take seriously what I believe to be a purely characteristic flourish ; the more so as the list of names appended to the protest (which speaks for itself) was published in the same volume.* But I think we shall see before getting much farther how much this "justice to predecessors" is worth as an argument.

It seems necessary to observe that this is a matter on which one entomologist with a head on his shoulders is, when he knows the facts, as good a judge as another. The contrary notion, *i. e.*, that a strongly-worded phrase or two from an experienced entomologist can countervail good reasons adduced by an inexperienced amateur, may have arisen naturally enough out of the associations of years, but I submit will not bear examination. This is not a question of zoological science. If it were, some might perhaps hesitate before putting forward an opinion opposed, for instance, to Dr. Gray's. It is only because the influence of personal authority on such a point as this is of the lightest, that I do not vouch on the other side the names of entomologists who say the opposite. The number of those names is large, and their authority (on matters where authority has weight) is of the highest ; but I shall not, on my part at least, turn aside to bring forward opinions merely as such. It may be that this question is eminently one which those who are not themselves nomenclators should take part in deciding ; for circumstances, I think, show that those who have described species themselves may see these questions from a point of view which is not always that of entomologists at large. Authors alone, indeed, are little likely to arrive at a settlement ; and most of them have works which make their writers tender on controversial points. However that may be, two or three

* See *Ent. Annual*, 1873, *ad finem*.

sharp sentences from any quarter cannot influence the conclusion at which those who weigh the reasons may arrive.

The plea for "justice to predecessors" seems to be founded on the doctrine that the author who has first named an insect has a personal and individual right to have the name given by him retained. We are concerned with old authors; and I think we must be already satisfied that in upholding, regardless of consequences, a divine right in the first name-giver, we should be paying a reverence which would be somewhat laughable. It is entertaining for a little while to trace out the odd variety of accidents which combine sometimes to establish the names of the old authors. One author names a species (in one of the large genera) giving it an original name; that name had been hit on by some one else for a different species in the same genus (= group), which was not rare in times when gods and goddesses gave all the names to butterflies. The first nomenclator thus goes to the wall because his name is "*nom. præocc.*" A later author comes and mis-identifies his description with a different species altogether, which he accordingly publishes to the world; by this time the genus has been divided, and consequently the name destined for species A., and which was in error taken for species B., stands for species B., and not for the one to which it was originally given; the blunderer obtains immortality, and his friends importune us for "justice." This is by no means a rare kind of mistake, and there are many others quite as humorous. It constantly happens that a man's uprightness works his fall, but his backslidings put him on his legs again. But though sufficiently amusing, the subject is really very much beneath discussion. Although the old writers were most often little acquainted with what others had done, the works of Linné and Fabricius would appear to have fallen in the way of most of them. It is instructive to observe how the early writers themselves got on with the descriptions even of Linné; and I think we here reach a point in the discussion where we get a strong independent light on the facts, and our argument receives a good deal of assistance.

When Linné completed his labours he had (as we have said) described but 780 species of *Lepidoptera*, and of those a large portion were day-flying insects inhabiting Europe. Those who immediately succeeded Linné also described numbers of day-flying species inhabiting Europe. Now, investigation shows that these writers *then* ascribed the Linnéan descriptions to widely-different species. Linné by no means described all the European day-flying *Lepidoptera*; but, perhaps, from a belief that he had done so, many of the writers who immediately succeeded him seem to have managed if possible to find somewhere in Linné's works the species they had under description. Thus when they had a butterfly with black wings and white markings on them they went, say, to the "*Systema Naturæ*" and

promptly identified their insect. The Linnean description being made without a knowledge of allied species was vague enough to be applicable to the insect which the author referred it to. If one only of the authors did this, little confusion came out of it. But it frequently happened that several authors went independently, and respectively arrived at different identifications. At this point we are not dealing with inferences or opinions however distinguished or well supported, but with facts. Let any unprejudiced investigator examine the history of the following names (in the present genus *Colias*) *Edusa*, *Electra*, *Hyale*, *Helice*, *Chrysothème*, *Myrmidone*, *Palæno*, *Europome*; (in the genus *Polyommatus*) *Alexis*, *Agestis*, *Icarus*, *Argus*, *Alsus*, *Thetis*, *Corydon*, *Meleager*, *Acis*, *Argiolus*, and any of the old species; (in the genus *Satyrus* and its allies) *Mæra*, *Pamphilus*, *Tithonus*, *Tiphon*, *Iphis*, *Aleyone*, *Actæa*, *Hero*, *Amyntas*; and do not let him leave off before he discovers cases where it seems old authors confounded a *Polyommatus* with a *Satyrus* (!), so pleasantly comprehensive was the description of the "first nomenclator." The selection is pretty impartial, and will be found to illustrate several different authors.

It is the case that through all the very early literature of entomology many diagnoses of Linné and other describers of that date were found open to different interpretations. They were found thus vague by Linné's and the other writers' *immediate public*. What would be the attention paid to-day to descriptions which left it open to those who used them to apply the same one description to several different species? We should not wait long for the comment that the author's descriptions were "unrecognizable." These are, in truth, in the greater number of instances, descriptions no more of one species than of another (or many other) species; and this is not the discovery of some pert critic in our go-ahead era, but was a fact, experienced by those who were in part or altogether the writers' contemporaries, and a fact, moreover, productive within the span of a very few years, of the very confusion and disagreement which has continued ever since.

Perhaps, however, the truth is, the earliest descriptions were excellent, and those who came to opposite interpretations of them showed their incompetence? Well, if that line is the one to be taken, it illustrates the argument even better. If the entomologists who immediately succeeded Linné (who wrote, in fact, all the "old" books) were unable to read aright Linné's descriptions—when they were plain—is it the authority of *these* writers, and to preserve their work, that we are asked to do "justice" to the first nomenclator? And, in truth, I think that there is plenty of evidence that the fault lay on this side as much as on the other. On the one hand, the Linnean descriptions did often suit widely different species; on the other hand, his successors were very often wrong through their own

mistakes. Linné's descriptions are vague; but they certainly did not always admit of being interpreted as his successors did interpret them. In those cases it was open to a later investigator to correct the wrong interpretation, and that has long ago been done; but in other cases where the successors of Linné came to different identifications, no one can say who was right. No one could say at the time, and what could not be decided at the time has generally not become any clearer since.

We are now on the consideration of "justice" to the nomenclator. I understand that phrase to mean giving to the nomenclator as much as he deserves, not to mean falling down and worshipping the oldest describers. The Ashantees, when they abase themselves before a fetish, (and subject themselves to a vast deal of inconvenience in the service,) no doubt consider they are doing "justice" to the fetish. That, however, only takes place while the fetish is credited with the possession of authority and other dignified attributes. When the course of events has convinced the devotee that his fetish is only remnants and rubbish, the Ashantee is reported to lose all respect for his fetish, and, indeed, to ignore him altogether. But the Ashantee is a barbarian of Africa,—and acts on principles essentially different from those of some entomologists in Europe.

Sweeping assertions are now-a-days always cavilled at; and, as my object is not to say things which excite cavil, the sweeping statement of fact which it is necessary to make shall be made in the language of an opponent. Baron von Harold thus characterizes the early literature of entomology: *—"The longer and more thoroughly 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 which hardly had a beginning has been made."

It would be silly to enlarge on this, because it stands to reason. If the fact were not that the nomenclature of the end of the last century and the beginning of this century is a mass of blunders, an infinity of corrections, so-called, could not now be brought up. (I say "corrections so-called," for I dispute that it is possible satisfactorily to elucidate at the present time the points which have led astray for this long period after author; and which have done so because the questions are in truth obscure.) Well, but it is the literature which we are asked to do "justice" to that is "a chaos of arbitrariness, inconsequences and blunders," for the *misunderstander* and the *misunderstood* make up together the band of "first nomenclators"! Justice does not go by guess-work; but who is now

* Coleopterologische Hefte, vi. p. 37.

able to decide the right and wrong of these questions, and point out where the fault lay? Conjecture and speculation are the only resort; and conjecture and speculation (though we often have to act on them in other ways) have naught to do with claims for "justice," and justice has naught to do with them.

Justice implies the giving to each his due; and when each gets what he ought to have, we say that justice is done. I believe that systems of jurisprudence provide for the ascertainment of rights and also the due chastisement of offences; and it is a question whether the justice demanded should not be meted out under the latter branch. I never heard before the cry of "justice to our blunderers," and to me such a demand suggests retributive justice alone. There is such a thing as the *lex talionis*, but a fair compromise which leaves the old works undisturbed on the top shelves, will supply the best basis of settlement, least vexatious to the living, and likely to bring least into question the merits and demerits of the long departed.

Are we in doing "justice" to one person (be he the first nomenclator or the last), to be regardless of the injustice which we may do to other people? And are there not other people entitled to consideration? Which is the more important figure in science, the man who publishes stray descriptions, or he who masters the natural history of a group? We are on questions of sentiment, although the sentiment is of a good kind—the sentiment of respect to predecessors. Well, whose claims for "justice" present the stronger appeal? While the describer is as likely as not to be a man of small attainments, we have in the case of a monograph a guarantee that we are dealing with the work of one who, to some extent, must be a naturalist, and who comes to us with his title to respect made out. If it be the case (which I should regret to believe) that those who write books look for perpetuation of their memory in the names they bestow, then it seems to me that our sympathies should be enlisted on behalf of those who have done most for us. It comes to this: if we do not retain the names bestowed by the first describer it is because we *do* perpetuate the names bestowed by a monographer; while if we do not retain the names bestowed by a monographer, then (on the principle which is asserted) a quantity of the good work goes unrewarded. Wherever this is carried to the point of superseding names bestowed in a monograph for a name that is doubtful, most will agree with me that there is no "justice" at all in the case, but a flagrant injustice is committed. But, as I have already insisted, the cases where any names of the date we are considering can be brought forward which are not doubtful are either none at all or so exceedingly few that they do not materially affect the question. In nearly every case where a modern name is now superseded for an old one the case is one

of doubt. The list-writers now seize on points of identification which a few years back all persons agreed were insufficient.

Another consideration remains with reference to the "justice" of the case. Some have dealt with this question as though authors were the only people concerned. I may be right or wrong, but I have grown up in the belief that authors do not write books for their own satisfaction or enlightenment, but to enlighten or satisfy other people. I have not yet learned that authors write to enlighten or satisfy other authors alone; for I believe they do not put out of view the large number of readers who are content themselves to publish nothing. Now, if the nomenclator has rights in the matter, so have other entomologists. To subject the whole entomological world to inconvenience and disgust without necessity is itself an "injustice" of a bad kind. I ask what we have done to deserve that our beautiful science should be made a battle-ground for the upholders of different fancy systems of synonymy? "Priority" has been aptly termed "a hobby." The bulb mania and the old china mania, and others, have their day; and there are fanciers in various departments which attract the notice of those who are blessed with leisure and have the special taste developed. But we have an interest in suppressing the fashion of synonymy-fancying; and I entertain the hope (which is brighter than it was) that this description of industry will soon not be worth following.

In parting from the subject of "justice to the nomenclator," I cannot pass by the trenchant writing of Dr. Leconte*:

"It would seem from some expressions of opinion I have seen, but which I forbear to refer to more definitely, that there are those that believe that one main object of descriptive natural history is to give the authors a sort of proprietary interest in the species to which they affix names . . . Such ideas are really aspersions on the notions of the great professors of unremunerative labour, upon whom science chiefly depends for her advancement. The good and true labourers are many; the small and mean minds, who feel honoured at being quoted in synonymy, are few."

Again:—

"It is only in descriptive natural history, the lowest and most routine work that a man of science has to perform, that any association of names with results is possible. In all other and higher departments of knowledge, such as Newton with gravitation, &c., or, to exemplify from our own departments, Linnæus, Jussieu, Cuvier and Geoffroy, all these men are historically eminent for their labours far more than for attaching their names to the objects of their study. With such examples of high and honest effort, to be imitated by us in proportion to our respective abilities, it is surely an ignoble ambition, and certainly an uncommon one, that would aim at distinction by

* Canadian Ent. vol. vi. p. 203.

having the name printed in association with a weed, or a bug, or a bone." *

Strict Priority cannot settle our Nomenclature.

It is contended that the strict application of "priority" will give us certainty in nomenclature. We shall see, I think, how this is.

Dr. Sharp, who (in the pamphlet already mentioned) concludes that "to abandon the rule of priority is to abandon the only foundation possible," has the following observations on "the very important point" whether a description applies to a species:—

"This is a very much more difficult problem than the ascertaining of a date, and it can only be properly dealt with by a complete consideration of the evidence in each particular case, and this evidence is of three kinds. 1st. The description itself and the complementary evidence accompanying it (such as locality of occurrence, statement of habit or peculiarity of modes of life, &c.); 2nd. Tradition; and 3rd. The existence of the individuals from which the description was drawn up, or of other individuals alleged to be authentically named. The evidence under the first of the heads is the most important, and if it be of itself satisfactory no other evidence is necessary; if the description accord satisfactorily with the characters of a particular species, and if it be ample and well-drawn up, and especially if it be accompanied with a well-executed figure, the question is decisively settled. But if the description be so deficient in any or all of these points as to leave doubt in the opinion of a skilled or expert inquirer into these matters, the evidence should be sought under the other heads. And if it be found that scientific treatises dealing with the matter have declared or cited the questioned description as belonging to some ascertained species, and if the number and importance of the treatises in which this is declared be considerable, then also this evidence is important. As for the evidence of types, it is clear that this must not be exclusively or even strongly relied on." †

And this is all! Those who expected, as I did, to derive assistance from Dr. Sharp's treatise must have felt no little

* I remarked (ante, p. xix), that in two noteworthy instances the claim of a "right" in the first nomenclature had been repudiated by those who yet favour absolute "priority." The writers referred to are Mr. Scudder and Dr. Sharp. The former writes (*Am. Jo. Arts & Sc.* 1872): "In systematic nomenclature the object is to register titles, not to gratify pride, and the names of authors are appended for convenience, not fame; the question of justice or injustice has no place here."

Dr. Sharp (in *Nature*, v., 341) lays down that the author's name placed after a species "should always be that of the first describer of the species; *not because he has any right in the matter*, but as an additional means of certainty, and as a security against change."

† *Object and Method of Zoological Nomenclature*, pp. 30, 31.

disappointment, for he most serenely gives the go-by to all our difficulties. To all who know the subject (whether expert inquirers or otherwise) the remarks above quoted are harmless platitudes; and when they are read and agreed to, it seems to me that the case is left exactly as it was before. The descriptions of the old authors do not "accord satisfactorily with the characters of a particular species" and are not "ample,"—therefore they do not furnish material for Dr. Sharp's *decisive* settlement. Then the evidence is to be sought in tradition or types. Well, in the cases which make our difficulty, "scientific treatises, which have declared the questioned description to belong to an ascertained species," are either none at all or are *not* considerable in number and importance. As to types, Dr. Sharp agrees that "very little authority can be attached to them." Then, where does all this leave us? The evidence of which Dr. Sharp speaks is not forthcoming; and it is exactly because it is not possible to obtain such evidence that it is now discovered our nomenclature cannot be settled by recourse to the old descriptions. The above passage states simply enough the "priority" *modus operandi*. What has been lost sight of is the all-important fact that the method is inapplicable to the only cases on which our discussion turns. "Priority" is baffled by the old writers, and on that ground its virtues are a matter of pure indifference. What is the good of puffing an invention that cannot be got to work?

Discretion cannot settle Nomenclature, which requires a Rule.

M. Candèze, the president of the Entomological Society of Belgium (who is engaged on a monograph of the *Elatridæ*), has placed on record his views on the question, which closely resemble those contended for in the present paper. He remarks* :—

"To-day when entomologists are divided into two camps on the question, whether we are bound to return to the names which have been long forgotten, to substitute them for those which have usurped their place and which tradition has consecrated, or whether we ought not rather to admit for scientific names a sort of prescription legitimising these usurpations—in presence of this discussion in which both sides support their opinions by excellent arguments, I thought it necessary to take a part.

"An enemy of every exclusive and absolute rule, I have not rigorously followed either of the two systems, allowing myself to be guided by one or the other, according as it appeared to me the more rational in such and such a case. Thus, while for *Adelocera atomaria*, the name before admitted by me,

* Mon. Elater. quoted Ann. Soc. Ent. Belg. 1874 : Comptes rendus, pp. 10, 11 (December, 1874).

I adopt the correction which attaches to it the name *carbonaria*, which is older by some years, I reject that of *punctata*, which it is proposed to substitute for it as the most legitimate.

"I by no means ignore the criticisms to which I expose myself by acting in this manner, but I think that moderate minds will support me, and that sooner or later a sort of compromise will rally the greater number. . . . I have always declared that if sometimes I range myself on the side of the reformers,' sometimes I abide with the 'conservators,' it has not been without reasons which I consider good."

In this passage we see the doctrine of "*Communis error facit jus*" carried into practice, and have a clear illustration of the way in which it is proposed to work the priority rule. *Atomaria* is the name in use in France, but *carbonaria*, the name in use in Germany, is older; therefore of course *carbonaria* supersedes *atomaria*. But *punctata* is older still. *Punctata* is nowhere in use, therefore *punctata* must be rejected; and we retain for the insect the name *carbonaria*, the oldest which is in use.

Thus M. Candèze treats this case as those who share my opinion would treat it. Acting on our principle he naturally arrives at our conclusion. My object, however, now is to carry this a step onward, and to show that the considerations which have conducted M. Candèze so far must take him farther.

We have seen that M. Candèze declines to bring forward a name for many years totally forgotten. He however says that we shall find him siding sometimes with the reformers and sometimes with those who are in favour of preserving the names, and on whichever side it is that he is found it will be "for reasons that he considers good." I hope not to be misunderstood in saying that I think this conclusion of M. Candèze cannot be justified. Nothing short of that declaration will serve, and it would be highly dangerous to be otherwise than explicit on the point.

What is it we are struggling to do? At the present time our nomenclature is shifting and uncertain, because successive authors change one after another the names that are in use. We are struggling to render our nomenclature certain; that is, to establish the names in such a manner that it shall no longer be in the competence of successive authors to change them. As the case at present stands the labour which A. has expended on a given identification is rendered worthless by the later labour of B. and C. Both of these again find their conclusion rejected because D. arrives at one that is different. Now these authors have determined the question by "reasons which they consider good." It is because what A. "considers" good, B. "considers" not good, that B. discards the conclusion that A. arrived at. It is because though he may consider that B.'s

reason and C.'s reason "good," he yet "considers" another reason better, that D. in his turn declines to follow the conclusion of B. and C. Where perception is the only guide and standard, A., B. and C. may fairly differ, considering that the subject-matter is so obscure as we have seen that it is.

But it does not do to forget that we are concerned with not only the independent judgment but also with the preferences of individuals. There is scope in nomenclature for the operation of a writer's preferences; and sometimes all do not agree on the value of this or that author. There are list-editors* so possessed with the paramount importance of the Fabrician nomenclature that they are committed to supporting the Fabrician names, recognizable or unrecognizable, under all circumstances. There is another author who (as we have seen) has placed on record his conviction that it is necessary to neglect altogether the great majority of the Fabrician names in the group which he specially affects, because they are unrecognizable or are found to be only synonyms for names given before.

Now the reasons which have appeared "good" to one writer and not good to another have brought about contradictory results, though each writer has pursued his investigations subject to the direction of the same principle and the area for difference is restricted to one solitary point. It is while "priority" pure and simple receives the unsparing adhesion of those who publish lists, that the reasons which each considers "good" have landed us in confusion. But M. Candèze's proposal is very remarkable. The fact that good reasons send him sometimes to the side of the reformers and sometimes to the other side is used as an argument for throwing off the *single* controlling enactment which does now supply the essentials for an agreement; and this without putting anything in its place. Under this plan then the reasons which an author may consider "good" will be reasons for following priority or ignoring it, for rejecting the first name (of which M. Candèze has given us an instance), or insisting on it, and we must now take it that this prominent entomologist has declared himself free from the rule of priority as it at present stands. M. Candèze's system seems to leave us without anchor, chart or compass. Heretofore divergences in judgment have been restricted to *modus operandi*. Henceforward, all is to be discretionary. I dread to contemplate the condition at which, as it seems to me, our nomenclature will arrive in two years if such a principle receive acceptance. To institute a parallel which holds good in some respects,—it does happen that courts

* * Amazing as this statement seems, it is unvarnished truth; see the preface to Gemminger and von Harold's Munich Catalogue, pp. x., xi. (See also Discussion of Priority, pp. 40—45.)

of equal authority come occasionally to conflicting decisions ; and that takes place in administering a fixed law when all which the judges have to do is apply it. But only conceive what condition of things we should come to if judges were to begin deciding upon rights by the light of discretion only, doing what they considered desirable and untrammelled by a positive law !

When M. Candèze has rejected a prior name on the ground that it has been long disused,—which he has shown us is a reason he considers “good,” why should another author abstain from bringing that name forward, on the ground that it is the prior name—which is a reason he in turn considers “good” ? This surely is the kind of decision which might properly be described by Dr. Sharp as one “to be reversed on the first appeal ;” but Dr. Sharp’s criticisms could not (from considerations of chronology) have had reference to M. Candèze’s proposal. Bad as I consider the present practice has proved to be for our nomenclature, I think it is only as King Log to King Stork in comparison with the principle (for such only can it be termed) of having no rule but what the author chooses. We are in search of certainty in our nomenclature. How can it be said that rejecting or bringing up names on grounds of discretion will ever bring certainty ?

I said that the line which M. Candèze adopts would take him further than the point which he reaches. It is impossible to stay where he leaves off ; and I think the rest should naturally follow. If an author’s object be sometimes to bring forward old names, and sometimes to reject them when there is *no* substantial difference in the circumstances, we must grant at once the proposition that rules for nomenclature would be out of place. But if, in bringing forward or rejecting the old names, the author acts upon a principle which admits of being stated, I cannot imagine in what respect his work is not greatly improved and advantaged by having the support of rules and reasons. It would appear to me that a compilation of synonymy not so supported remains open to be misread and misunderstood in every way, and that whenever its conclusions might be attacked the attacker would experience an easy victory, because no person could say on the author’s behalf what his grounds of procedure or *modus operandi* were. If these considerations be well grounded, the old names, if they are to be rejected, must be rejected on a principle ; and I do not gather from M. Candèze’s observations that he entertains any rooted objections to the principle of discarding names totally disused.

Irreconcilable Confusion must continue to result from the present Condition of Things.

Mr. W. F. Kirby has contributed to the “Canadian Entomologist” (vol. vi. p. 196) a short but very important paper on

“Discrepancies in Recent Lists of Lepidoptera,” which is as follows :—

“The opponents of the law of priority in nomenclature have taken occasion, both in England and America, to argue against the restoration of obsolete names, on the ground that the names employed in my Catalogue of Diurnal Lepidoptera do not always harmonize with those used in Staudinger’s Catalogue of European Lepidoptera. Although this argument looks plausible at first sight, a little reflection will probably convince many that it is baseless. We may leave genera out of the question now, as Staudinger has not attempted to grapple with the difficulties which they present; but as regards species, it must be remembered,—1st, that Staudinger starts from 1758, instead of 1767, and that I should have done the same had I investigated the question fully when I commenced my work; and 2nd, that Staudinger, working at European Lepidoptera only, was necessarily better acquainted with the special literature relating to them than myself. Had I selected 1758, and possessed Werneburg’s “Beitrage zur Schmetterlingskunde” at the time I was writing my own Catalogue, or had Staudinger’s new Catalogue been published in time for me to verify the references contained in it, I think I may say that many of the alleged discrepancies would have disappeared, although, in some cases, I may have made use of materials which Staudinger does not appear to have employed, or may have seen reason to disagree with him as to the determination of certain species. Unless two authors have exactly the same materials to work with, or one copies from the other, no rules will be sufficient to insure their absolute agreement in every case; but by the strict law of priority, the chances of disagreement are reduced to a minimum.”

I presume I am to consider myself included in the description of “opponents of the law of priority.” (The phrase is convenient as a *nomen triviale*, though it is defective as a *diagnosis*.) Now, I certainly have used the discrepancies in Kirby’s and Staudinger’s Catalogues as furnishing arguments against the endeavour to restore obsolete names founded by the early writers; and I have vouched them (with other considerations) as proving an important part of the case set up. I venture to think that if, on the appearance of the two Catalogues, I had missed drawing attention to the discrepancies as they exist, *and the causes of them*, I should have failed to seize what is really a plain conclusion; and also should have been rightly chargeable with building up a discussion of words and theories instead of dealing with facts. I repeat the expression of my opinion that the lessons to be drawn from Mr. Kirby’s and Dr. Staudinger’s Catalogues taken together are in the highest degree valuable; from the point of view therefore which I occupy, the remarks which Mr. Kirby may offer on the matter have a corresponding interest.

First of all, it seems necessary to state that the ground taken up is something different from that which the words quoted would indicate. I do not think anybody has founded arguments on the bare circumstance that the names in the two Catalogues in question "do not always harmonize." For myself I never founded on that circumstance an argument of any sort or description; and the exploit would be so entirely futile that (in the absence of an allusion more definite) I think the words have not quite accurately expressed what Mr. Kirby probably intended.

I have pointed out that Mr. Kirby and Dr. Staudinger, "having in almost every instance used identically the same references," have, in what I term a prodigious number of cases, come to different interpretations of them. In particular, I have quoted a chain of instances where these two writers have sounded every note in the whole gamut, and not only "did not always harmonize," but did always arrive at discord; and on this circumstance (with others) I have argued that a large proportion of names in the old books are not truly recognizable, even after the maximum of research and study by two of the most industrious lepidopterists. *That* is the conclusion which I sought to impress upon entomologists interested in our nomenclature.

It would have been impossible for any one who had read Mr. Kirby's and Dr. Staudinger's prefaces to their respective Catalogues to find an argument (of the kind supposed) on the bare fact that Kirby's and Staudinger's names are different, because it was plainly stated that Staudinger had gone back for his names to the date 1758, while Kirby announced that he had adhered to the names of 1767.* If Mr. Kirby is under the impression that this circumstance was lost sight of, he must himself, I think, have given little attention to the criticisms which provoked his reply.

Mr. Kirby continues:—"Although this argument looks plausible at first sight a little reflection will probably convince many that it is baseless." The argument really used is, I venture to maintain more than plausible, for not a little reflection only, but a somewhat prolonged investigation has brought me at least to the conviction that it is well founded. Two years and more before Mr. Kirby put forward this explanation, I had pointed out that the differences between Staudinger's Catalogue and Kirby's Catalogue would be *wider than they are* if the two works agreed on their starting-point; and the matter received a great deal of notice in the pamphlet, entitled "A Discussion of the Law of

* I say it is "announced." It is, however, not always the fact. Instances will be found by every one who looks for them in which Mr. Kirby starts from 1761. It is unnecessary to complicate the discussion by enlarging on this circumstance.

Priority," which I published. As I have no reason to alter the language, and the point is of the essence of the controversy, I may be forgiven for presenting the case in words then used. I said:—"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 recognize! 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."

And I gave this instance of the way in which the change of Mr. Kirby to 1758 would work:—"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 recognize* 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."

This was followed by a succession of instances (quotations and references being given) where Kirby and Staudinger came to different interpretations of a number of names all traditionally ascribed to various European butterflies with white-banded wings (which cannot be reproduced here); the summary

* Discussion of Priority, p. 21.

being :—“ ‘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. . . . 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.”

It is impossible in a paper such as this to do more than give instances to show how this change confuses the nomenclature of Kirby’s Catalogue, and Staudinger’s as well. Any one can discover for himself a large collection of similar cases among the long-discovered species; and I must not delay over this portion of the subject.

I must maintain that the difference in the date taken for starting-point does not explain the existence of discrepancies between Kirby’s list and Staudinger’s; because, although it happens that Staudinger and Kirby do print different names because of the different starting-points which they accept, yet not only does that circumstance account for comparatively speaking a very small number of the discrepancies, but in point of fact there are so many obscure names in the books of 1758—1767 that the two authors’ disagreements are largely increased in number when that period is taken into the account. We get a spice here of the “chaos” that writers on synonymy frequently mention. The period before and long after 1767 was the infancy of entomology, but before 1767 was certainly its toothless babyhood. The British Association Rules still forbid recourse to the names of that period; but the two active catalogue-writers on the *Lepidoptera* are now agreed in favour of taking 1758 as the starting-point. We are thus in the position, for the first time, of seeing what kind of names these are which have lain hid so long. I think there will not be much doubt in the mind of those who handle the question that the names prior to 1767 are of the strictest sect of the unrecognizables, and (as I have convinced myself by some very distasteful labour) that agreement in favour of using them will increase the confusion.

Mr. Kirby says :—“Had Staudinger’s new Catalogue been published in time for me to verify the references contained in it, I think I may say that many of the alleged discrepancies would have disappeared.”

I am puzzled by Mr. Kirby’s use of the phrase “alleged discrepancies.” If the discrepancies are alleged only, and not

actual, it seems peculiar to excuse them on the ground of Staudinger's better acquaintance with the literature; while the drift of the passage would appear to be that a verification of Staudinger's references by Kirby would have effected the disappearance, not of discrepancies supposed or imagined to exist, but existing in fact. However, I do not take the phrase to imply a denial that the differences are actual and substantial, because, in truth, such a contention could not be raised by anyone acquainted with the two works. Staudinger (says Mr. Kirby) was better acquainted with the special literature, and had Kirby verified Staudinger's references we should not be complaining as we are. It is invidious to look gift-horses in the mouth, and the explanation which an author may choose to furnish to critics is a gift-horse to some extent. If, however, we contemplate using the animal for stud purposes, it may be that the character of our stable for years to come will depend on its strength and soundness, and a prudent man will pocket proverbial philosophy and send for the veterinary surgeon. The works, then, which Dr. Staudinger cites—which Mr. Kirby does not cite, and which may contain identifications of "Diurnal Lepidoptera,"—do not exceed thirty in number, all told, and by far the greater part of these appear to be works of a completely trivial character,—such, for instance, as may be used for the localities in Staudinger's Catalogue, but of which he appears (though he gives them in his list of authors) to have *ignored* almost the whole number in the synonymy. In point of fact, I do not assert that these works may not here and there be responsible for differences, but the number which is thus accounted for is again insignificant so far as my researches have gone.

An odd thing about Mr. Kirby's explanation is, that in another way it does not meet the complaint. The "better acquaintance" with the literature, so far as making use of a *far greater part* of it is an indication, is shown on the part of Mr. Kirby. He has identified numbers of references which Dr. Staudinger has passed by, or (according to his own remark) has "made use of materials which Staudinger has *not* employed." Though in the majority of cases the same references have been made by both authors, and very often differently construed, yet there is no room for doubt who makes most use of the literature—that is Mr. Kirby himself, and not Dr. Staudinger.

Mr. Kirby further says that had he possessed Werneburg's "Beitrage zur Schmetterlingskunde" at the time he was writing his Catalogue, or had Staudinger's new* Catalogue been published, he "thinks he may say" many of the discrepancies

* It is worth remarking that Mr. Kirby did start on the same general lines as Dr. Staudinger. In 1862 Mr. Kirby published a Manual of European Butterflies, which he prepared when enjoying "unusual facilities for studying the literature of Entomology," but he actually based that

would not have appeared. This, I presume, means that Mr. Kirby has made errors in his identifications, and has that brought home to him when he finds that another has decided differently. This is not in any way a pleasant discovery. The superseding of names in use by others, which are abandoned when a fresh author says something different, is the very practice which has proved so serious for our nomenclature; and it is to be regretted that heretofore this occurrence has been treated so much as a matter of course. One author thinks he sees a likeness in an old description, and brings it out as identified. Three months after (it may be) some one else sees a better likeness, and that is brought out in its turn, to serve till something more attractive still is lighted on.

We have quitted the subject of the old descriptions, but perhaps it is as well to look again at them from this new point of view. What light does *all this* throw on the character of the descriptions which admit of such conflicting interpretations? We have modern author after modern author (not at all ignorant, but on the contrary, having trained himself for this special work) finding grounds for bringing out new identifications. Yet the grounds which they make sufficient for upsetting names in use are so little worth, that they are ready at the shortest notice to withdraw their identifications in favour of a new one. The reason is this, the books *do not furnish* any good grounds. I do not believe Mr. Kirby has identified any species *dissimilar* from the description. If he had done that, it would be a far different affair; but, on the contrary, I take it that Mr. Kirby had fair grounds for his identifications and read the descriptions as well, on the whole, as they truly admitted of being read. Both authors have reasons of some sort, but nearly all is mere speculation. The difficulty being caused by facts (not rules, or theories), such disagreements must continually crop up, and there is no possible way of reconciling them, while recourse to the old descriptions is permitted.

As to placing justly any reliance on Herr Werneburg, or vouching him as an authority in disputed cases, surely this is not to be entertained. Herr Werneburg has devoted himself to this study, and is responsible for a number of our irreconcilable differences; but as for Herr Werneburg's work bringing us to *certainty*, it is almost a shock to hear the suggestion made. Dr. Speyer long ago* characterized Herr Werneburg's work as having taken from "the generally unintelligible and vague descriptions and defective plates of the *Patres Entomologiæ*, identifications which hung on the most precarious holding

work on Staudinger's first Catalogue (1861), as will be found stated in the Appendix (p. 145). Mr. Kirby was not working solely by the light of nature, for he and Staudinger were starting fair in 1862; and of all Staudinger's references up to that point Kirby had the use and advantage.

* Stett. Ent. Zeitung, 1865-66, p. 51.

points," and Herr Werneburg's *Beitrag* must be considered—not a repertory of truths (as I am submitting), but an assortment of speculations—like the parts of which we complain of the two Catalogues themselves.

Before leaving Mr. Kirby, let us take a new look at certain facts. Mr. Kirby gives the explanations which have been quoted, to account for his differences from Staudinger. We will take a familiar instance, the one originally used by me three years ago,* and test these explanations. Kirby and Staudinger's Catalogues (as they stand) differ on the names of *one-seventh* of the British *Rhopalocera*. Since reading Mr. Kirby's explanation (which appeared to hold out hopes that the differences could be reconciled), I have gone through the synonymy again, and have to report that whereas, starting from different dates, Kirby and Staudinger differed in *one-seventh*, now that they both start from 1758 (taking also into account some fresh identifications by Mr. Kirby), the differences have risen to *one-sixth* of the number. When the two lists first appeared, they changed seventeen of our species' names; some additions to the "corrections" having been made, now they change twenty. The common starting-point of 1758 has not reduced the number of these differences, but added to them; and the fact will have interest for British entomologists, who would have been truly glad to see a real explanation forthcoming. As for the other reasons, in nearly all the cases of difference Kirby and Staudinger have had the same materials and made the same reference to page and plate. But where more "literature" is quoted, it is almost always quoted by Mr. Kirby himself.

I conclude that it will be self-deception on our part if we expect a reconciliation of the names, unless one author makes a simple surrender of his opinions, retiring in favour of his rival in cases where they differ. Even that step would be eventually ineffectual, for the next list-writer may be relied on to ferret out such points again.

The foregoing remarks have been directed to exposing existing evils and considering some opinions of entomologists. Therefore, whatever proposal I may found on them (and whether the expedient which I am about to suggest receive approval or disapproval), the points already dealt with will remain unaffected.

There is a Remedy in a Rule which does equal Justice.

With this preface I proceed to indicate the reform which I look for.

The object is to exclude the names which cause confusion—that is, the names which are attached to descriptions doubtfully

* Discussion of Priority, &c., p. 83.

recognizable. The descriptions doubtfully recognizable are found in the old books. Therefore any expedient which excludes recourse to the old books effects the object.

The object also is to preserve the names which are everywhere in use. Any expedient, which protects the names in use, *ex vi termini* excludes doubtful names brought up from the old books.

The objects are clear—(1) to exclude doubtful names; (2) to preserve accepted names. They are not identical, but both objects can and must be pursued together.

It has been several times suggested that the enjoyment of universal acceptance for a period of years should give a name an indefeasible title to adoption. For a purpose which I have in view, I will fill in the number “thirty” and make the proposal read thus: No name for thirty years in universal acceptance should be displaced.

We have seen that an objection which we had to meet was founded on the claim for justice to predecessors. A part of our reply was that the “justice” demanded was demanded under *ex post facto* legislation—the “rights” being laid claim to under a retrospective construction of the law.

If that answer recommends itself, then we advance a step. No rights can justly be claimed under a retrospective construction. Act on that. Deny to the law any retrospective operation. See now whether we are not rewarded for consistent conduct by seeing our difficulties disappear.

The date of the law is 1842, a year which is a good way on the right side of the “infancy of science” period, to which we owe our troubles. No name before 1842 can lay claim to priority as a right.

This would be the working of the limitation. No name could be produced now for the first time from any book bearing date 1842 or previously. Place together by themselves, labelled “Old Style,” all books of that period, and agree that they be considered as non-existent so far as new identifications are concerned. That stops the evil spreading henceforward. No author can then bring up a name from Old Style books, unless the name has been kept alive by quotation as the true name in some work since 1842. Here is a measure there is really no difficulty in applying, and its operation is simple.

Merely being quoted in the synonymy since 1842 will not do. Nearly all these names which afflict us (now brought forward as the true names) have for years past appeared in the synonymy as representing some species in some author. If that were allowed to give the old name a claim to be received, we should reap little benefit from the change.

This limitation agrees well with the proposal already mentioned, which has attracted much popularity. The names which have appeared as the true names in publications since 1842 fall

into one of two categories; either they are in universal employ themselves, or, at all events, they prevent any other name having the title of universal employ. I took the period of thirty years for the prescriptive right. The date 1842 is thirty years exactly from the date (1872) when this limitation was proposed, and both plans are thus made to come to the same thing. If a name found in a publication since 1842 be the name given as the true name in all publications, it is in universal employ. Good; it cannot be disturbed. But if in *any* publication since 1842 a name is given as the true name, then it follows that *no other* name is in universal employ. Therefore, the names which (in the phrase of a great French entomologist) "usage has consecrated" are effectually preserved by both methods. On the other hand, where *no* usage has "consecrated" the names, our principle does not intervene. Thus both plans are applicable or inapplicable together.

Suppose a question arises as to the true name of any species. The choice at present lies among all the names in all the books there are. Henceforward, it will lie only among names which since 1842 have been treated as the true names; and we have the difficulty confined within reasonable limits. It does not become impossible to find cases of sickness, but the plague is stayed. There could be no difficulty in agreeing what work is the true starting-point in the case of each Order; and operations in synonymy would soon fall into mere routine.

Here then, as I submit, we find a point where the ground of "universal employ" and the ground of logical deduction are conterminous,—at which we can unite the suffrages of two schools of doctrine; and it is so fenced around by a principle, that "the man of science" can occupy it to his advantage.

Having defined the proposal in a few words, I have to say something more about it.

There is a natural indisposition to make a sweeping limitation which may seem to serve out hard measure to the good descriptions, while it aims only at protecting us from the bad. It may be said by some who are far from unfriendly to the movement, "Suppose an unmistakably 'prior' name is discovered to-morrow in an ancient work, will you not consent to its being brought up to supersede names in use?" That is a question which deserves an answer; and it shall have the best which I can give.

There is no such thing as a category of plain cases; nor can there be one law for dealing with plain cases and another for obscure cases. Directly you admit of such a difference, see what you open the door to. Who is to decide what is a plain case, and what an obscure one? When one writer proposes to pass by an old name on the ground that it is not a plain case, another may insist in bringing it forward on the ground that

it is a plain case. Where are we then? Why, exactly where we are now; that is, depending altogether on the judgment of authors—on points where judgments cannot be made to agree. Any change of the law which provided different treatment for so-called “clear cases” would leave open the door to all the evils which we are suffering from now. Such an alteration (as I have argued in considering the proposal of M. Candèze) would be illusory altogether; and, however unwelcome in one of its aspects the scheme may be, yet it would be the worst mistake to shrink from saying that nothing but a fixed limit for admission—which involves a fixed limit of exclusion—can succeed.

But although the scheme may appear rigorous, in point of fact its operation is perfectly harmless. There are no “clear” cases, such as excite sympathy, to be brought forward; and this is the best answer to the question. Preceding list-makers and monographers have, in one work or another, brought up the “clearly” prior names and they have been already admitted; those we are now receiving are not the clear but anything but the clear instances. For the sake of uniformity (and indeed to prevent its whole object being frustrated) the rule must apply to all names, but no one need experience alarm that “clear” priority will suffer by it.

If, after all that can be said, the proposal of the date 1842 appears to those who weigh the reasons to be too sweeping, then by all means let us agree on some date farther back. I have said why I prefer the starting-point of 1842, but agreement can do anything, and I should eagerly co-operate in fixing even* 1800 as the limit. Those who agree with me in *the objects* desired, will, I think, if they give attention to the matter, bring themselves to a conclusion upon *the method* not essentially very different from the one I have expressed. This reform could only be wrought by agreement; and when those concerned meet together to form an agreement, the general plan would be found to admit of moulding this way or that, according as those who assisted might decide.

We are occasionally told that we cannot enforce performance of a rule; but I think this difficulty is somewhat of a bug-bear. The list-makers do not agree on the names they introduce; and the authority of any particular one, as well as of all together, is in a great degree neutralized by that circumstance. Then, when once we are entitled to retain the names in use I think that there will be little disposition to bring out the forgotten names—that is, I confess I think that the agreement of

* In that case it would be necessary to draw up an “Index expurgatorius” of the works to which no recourse for new identifications could be had, as the dates of a few bordering on the line of limitation cannot be clearly ascertained.

entomologists will have in a very great measure the effect of stopping that which it aims at stopping. But, supposing that it fails in securing this, it will be effective in another way. It will give us the right to disregard strange names hereafter to be brought up; and, considering that the law will be passed to supply a great need and to remedy evils whose pressure is as a fact severely felt, those who reap the benefit of it may be relied on to give it sturdy support. What reason is there to fear that, when they have a real boon of this kind granted them, people will be found renouncing it and submitting to the old disagreeables? At present we are without a lawful standpoint; and I admit that we are made to feel it. While, in answer to arguments, it can still be trumpeted that the "laws of priority" are all in all, it is the case that there is a real difficulty in declining to receive these forgotten names. But when the new law is passed, the whole advantage is the other way. Every sanction which can now be laid claim to by those who support the innovations will then be the warrant for their rejection, and it appears to me that there will be little to fear from those who might insist on continuing their resurrectionary exercises. At all events we have begged this question. We *now* proceed on the basis that laws are obligatory; and the British Association Rules and the Dresden Code bear testimony to the fact. We should be acting strangely if we forbore to alter the old law which is enforced to oppress us, for fear of not being able to enforce the new law which is to release us.

Summary of the Points contended for.

I conclude, then, that the rule of priority, as originally enacted in 1842, means that the earliest of the names in use should be adopted; or, at all events, that the consideration that the oldest name might have fallen out of use was not present to the minds of those who made the rule. That since then the circumstances are changed, and cogent reasons for resetting the law have become operative. That the paramount principle in nomenclature is convenience; and that, if all agree to forget a name, there is no "error" in the case, but right is done. That convenience requires the upholding of accord, if that can be done in a manner to render the accord permanent. That the bare rule of priority has failed to supply us with permanent accord (*i. e.* certainty), because it is not possible to decide on the earliest descriptions.

That, in order to make accord permanent, a law is necessary; and that discretion cannot effect any good. That accord can only be upheld by a law excluding recourse to the old books; and that law may take any formal shape so that its provisions be definite. That a law denying to the rule of priority any retrospective operation will exclude recourse to the old books,

and will thus make accord permanent, while it will admit of justification upon principle. And that a law which proves in truth salutary will run little danger of being disregarded.

I have sought to show that Mr. Kirby's explanations of the discrepancies between his Catalogue and Dr. Staudinger's do not encourage us to expect an effectual reconciliation of them; and to point out that M. Candèze's proposal will not improve our present position.

I have said little of the discordant rules and practice adopted by different writers on synonymy because this paper is too long. Some, who seem to say it is necessary to identify the unrecognizable and interpret the unintelligible, have surrounded themselves with rules of their own private devising which, as might have been foretold, fail altogether to save them from disagreement, but, on the contrary, supply fresh points of difference. The identity of an insect with an old description is entirely or in part matter of tradition and not of proof. In that state of things we find some (like von Harold) avowing candidly that they accept tradition as all in all, while others (like Staudinger) profess that they decline to act save on "proofs which appear irrefragable," but that certainty cannot be attained; while the point to which another (Kirby, as we have noticed) brings himself is that there is "reason to disagree on the identification of certain species," and "no rules will be sufficient to ensure absolute agreement in every case." But I say "Remove the cause and the effects will disappear!" No rules which encourage and render obligatory a scrutiny of the worthless descriptions will ever produce agreement; but *other* rules can and will yet effect it.

Conclusion.

Confusion in nomenclature is not a visitation of Providence—inexplicable, and to be submitted to with folded hands. You would almost gather from some things said about it, that a mysterious calamity had fallen from the skies, which all were helpless to remedy. On the contrary, it is the most commonplace result of human agencies—and human agencies of a not very inscrutable or venerable description. Surely we are not going to be so supine as to suffer endless inconveniences for want of a resolve to avail ourselves of the expedients which are at hand. When thirty years ago the entomologists found themselves in trouble about their nomenclature, they made a rule to help them, and got straight again. Are we not going to do the same? A grave responsibility will, it seems to me, rest on those who do not bear a part in effecting some settlement. If the blow be not struck now, our nomenclature will

fall a sacrifice ; for we have seen what is coming, and in a brief space of time we shall have lost our opportunity for good.

The rule of priority was first enacted in this country. The movement we are considering originated at a meeting, nearly four years ago, of this Society. The approval which some sentences of my own were fortunate enough to attract led to the proposal being developed and supported by reasons given at length. Some discussion followed ; and that was in turn succeeded by the signature, by an imposing number of our body, of a protest having for its object the suppression of the new names introduced. The movement is to a great extent the child of this Society ; and I was anxious at this critical time to bring it again before those with whose support it has sprung onwards as it has. I hope enough *vis* remains to carry the movement forward to the end, that it may be not merely a welcome proposal, but also an effectual success.