

I can not, therefore, see in the first species rule any inherently just principle, nor can I see in the processes which it is designed to supplant any corresponding inherently unjust principle, which indicates that future generations of zoologists would abide by the rule if adopted. Accordingly, I am unable to view this proposed legislation as advisable.

All, or practically all, systematic zoologists recognize that the principle of priority is inherently just. It commands respect, even though it irritates us occasionally. We apply it to generic names, without a murmur, or at least without murmuring very loud. If this principle is just when applied to the generic names, why is it not equally just when applied to the generic types? In the one case as in the other, the author who applies it must know the literature. As a matter of fact, the status of no generic name is satisfactorily established, from the modern point of view, until the type is designated. But when this type is once designated, by any method whatsoever, so long as the species selected was an original species, valid from the original author's point of view, and unreservedly classified in his genus, why reopen the question? At that date the generic name first complied with all of the formal conditions which can reasonably be demanded of it. Why now reverse the decision of the author who took this step, even if you or I would have done it in a somewhat different manner? If he selected the type on the first species rule, or if he did so on some other rule, or on no rule at all, the point can still be objectively demonstrated that the type was actually designated. This point being established, the question should be settled once for all. A genus can not have two separate type species; if, therefore, any author has definitely designated a type species for any given genus (regardless of his method), how can we establish another type species for it? To do so, by legislation or otherwise, is to weaken the very foundation of nomenclature—namely, the principle of the law of priority.

The discussion on this very live subject in nomenclature has convinced me more than ever of the justice of a rule to the effect that no new generic name published after a given

date, say January 1, 1908, shall be entitled to consideration unless its author definitely designates a type at the time of its publication. If American zoologists approve of this proposition (several systematists have already signified their approval), I am willing to do what I can to have it inserted in the International Code. I believe it would be wiser to make such a rule retroactive (namely, to date all genera from the time their types were designated) than to adopt the first species rule at this late day.

CH. WARDELL STILES

WASHINGTON, D. C.

THE FIRST SPECIES RULE VERSUS ELIMINATION

DISCUSSIONS concerning the adoption of the first species rule for fixing the types of genera have been so generally accompanied by extravagant statements of the probable revolution that would be thus occasioned in our nomenclature that there seemed to be a need for some statement of the matter based on fact and not on theory, and my recent article in *SCIENCE* was intended largely to supply this need. I had no thought of starting a lengthy controversy, nor do I desire to do so now. As my friend Dr. Allen in his recent comments upon my paper relies mainly upon general statements and does not prove any of my facts or figures to be inaccurate, he does not impair the strength of my argument and there would be no call for a reply were it not that he claims that I have been (doubtless unconsciously) led into a few misleading statements. These so far as I gather from his article are:

1. "That elimination has never been practised in Europe and does not seem to be understood by foreign writers." I was perfectly well aware that the 'first reviser' principle was incorporated in the B A Code of 1842, and in most others, *i. e.*, "that when no type is indicated the author who first subdivides a composite genus may restrict the original name to such part of it as he may deem advisable." But I claim that so far as birds are concerned the first revisers in the vast majority of cases have restricted the original name to the first species and its allies and

that when they failed to do so, subsequent authors have frequently ignored them and have selected the first species as the type. Furthermore, European authors have not practised the kind of elimination that shifts *Passerina* on to the snowflake and *Sarcophamphus* on to the eared vulture, and this sort of name shifting is what I claimed to be not understood¹ abroad.

Moreover, when we find that out of 277² complex genera of birds the currently accepted types of only 38 would be changed by the operation of the first species rule I am forced to believe that the first species was very generally regarded as the type by the first revisers and that the result is not a mere 'coincidence.'

2. Dr. Allen states that the rules and recommendations of Dr. Stiles referred to by me 'relate only in small part to the method of elimination' and cover the whole field of the determination of generic types, including the 'four conditions' of (1) monotypic genera, (2) type designation by the author, (3) tautonomy and (4) selection of type by subsequent author.

This is perfectly true as applied to Dr. Stiles's rules as a whole, but he has twenty-four rules and recommendations and Dr. Allen will find that I referred to only nineteen, omitting those covering the first three conditions stated above. It is true that I did include the 'first reviser prerogative' which Dr. Allen in this connection implies is not elimination. It seems to me, however, to be so intimately associated with the operation of elimination

as to be inseparable from it, and Dr. Allen himself says on p. 773 that with the adoption of the 'first reviser' rule 'the elimination principle follows as a necessary corollary.' The thirteen secondary suggestions to which I referred all relate to elimination in its strictest sense.

3. On p. 775 Dr. Allen makes a statement that I fail to understand, *i. e.*, "that the first species method is 'not always so simple and direct' as I have stated and that the case of *Vultur* will show that more than one reference must be consulted even under the first species rule." I have searched in vain for any demonstration of this claim in the subsequent pages of Dr. Allen's paper. Surely to ascertain the first species mentioned by an author in describing a new genus we have only to look at his original description! Dr. Allen must certainly have misunderstood the first species method here and also at the bottom of p. 776, where he says it would conflict with the 'rule that a monotypic genus takes its sole species as its type.' If *barbatus* had been the first species in *Vultur*, as he suggests, it would of course be the type, but this would in no way affect the type of the monotypic genus *Gypaëtus* which would remain *barbatus*. *Gypaëtus* being of later date would of course be a synonym of *Vultur* just as it would have been if *barbatus* had been the only species in *Vultur* or if it had been designated by Linnæus as the type of *Vultur*. This argument simply shows that genera with the same types are synonyms and has no further bearing.

4. Dr. Allen at p. 778 calls attention to the fact that "by the first species rule, where the first species happens to be the same in two or more genera * * * all the later genera become pure synonyms of the earliest genus" and then goes on to say: "It is thus evident that Mr. Stone's statistics greatly underestimate the number of changes in names that would result from the adoption of the first species rule." This deduction is entirely unwarranted. It assumes that I overlooked the synonymizing of genera with the same first species. This I did not do and all changes due to this cause are included in my statistics.

¹ I regret that this word has proven misleading. I had no intention whatever to question the ability of our friends across the water to practise elimination as Dr. Bather supposed, but simply that they did not *interpret* the method in the way Americans have done.

² Since my paper was published I have continued my card list of bird genera to 1830. Up to that date I have 1,119 genera, of which 842 are either (1) monotypic, (2) have their types designated by their authors, (3) indicated by tautonomy or (4) are substitutes, leaving 277 with no indication of type, and in 86 per cent. of these the first species is the currently accepted type according to the British Museum catalogue.

As an argument against the first species rule this has no weight, as it applies with equal force to any method of fixing types. I might say, for instance, "if the types of two or more genera happen to be the same by elimination the later genera become pure synonyms of the earliest." *Otogyps* is suppressed as a synonym of *Sarcorhamphus* by this very method in Dr. Allen's paper.

So much for my 'misleading statements.' Turning now to Dr. Allen's elaborate discussion of the types of the Vulturine genera, which he gives as an example of how elimination should be practised and which we should be very glad to see, as it gives us an actual case or series of cases worked out by one who is a recognized expert in this method of fixing types.

My chief objection to the method (*i. e.*, elimination) is that it will give different results in the hands of different workers owing to the almost infinite variety of ways in which it may be applied. Dr. Allen, far from refuting this claim, actually shows that two different methods of elimination may (no doubt unconsciously) be used by the same author in the same paper, thus emphasizing the elasticity of the method and the impossibility of formulating rules that will meet all its varied requirements.

Any one who has practised elimination knows that there are two methods in use in successively removing the species of a genus which have been made the basis of subsequent genera.

(a) Some remove only the species which has been made the type of a subsequent genus at the date at which the genus was established.

(b) Others remove along with the type any other strictly congeneric species, and here again there are two practises according as we interpret congeneric to mean congeneric from the standpoint of the author of the genus, or congeneric from the standpoint of the eliminator.

Taking Dr. Allen's elimination of *Sarcorhamphus* at the top of p. 776, he says:

Sarcorhamphus, 1806; species *gryphus*, *papa*, *auricularis*. The species *papa* was removed to

Cathartes in 1811, *gryphus* to *Gypagus* in 1816, leaving *auricularis* as the type of *Sarcorhamphus*.

The species thus removed are not, according to Dr. Allen's conclusions, the types of the genera *Cathartes* and *Gypagus*, but they were included in these genera by their authors in 1811 and 1816, respectively. It will thus be seen that Dr. Allen adopts method 'b' (above) in his elimination and interprets 'congeneric' to mean congeneric from the standpoint of the original author, not from that of the eliminator (or the usage of the present day). Having fixed the types of the four involved genera in this way, he next proceeds to eliminate *Vultur* by removing the species at the dates at which they became the types of subsequent genera—*i. e.*, according to method 'a.'

If *Vultur* were eliminated in the same way as *Sarcorhamphus* the result would be as follows:

Vultur, 1758; species *gryphus*, *harpyja*, *papa*, *aura*, *barbatus*, *percnopterus*. The species *barbatus* was removed to *Gypaëtus* in 1784, *gryphus* and *papa* to *Sarcorhamphus* in 1806, *percnopterus* to *Neophron* in 1808, *aura* to *Cathartes* in 1811, leaving *harpyja* as the type of *Vultur*.

If we do not trouble ourselves to ascertain the types of *Cathartes* and *Gypagus* when we eliminate *Sarcorhamphus*, I fail to see why we have to ascertain the types of the involved genera when we eliminate *Vultur*.

As a further example of the various ways in which elimination may be practised, it will be noticed that Dr. Allen pays no attention to what may have been done to species prior to the date of the genus that he is eliminating. Under *Gypagus*, 1816, he says: "*gryphus* was removed to the genus *Gryphus* in 1854," but as a matter of fact it had already figured in the establishment of the genus *Sarcorhamphus*, 1806, and proves, according to Dr. Allen's demonstration, to be the type of *Vultur*, 1758. Here again very different results may be obtained according as we consider or ignore the work of authors prior to the date of the genus we are eliminating.

Dr. Allen truly says that elimination requires 'a thorough knowledge of the literature of the cases involved' and 'is therefore

not a task a novice should meddle with.' This is another great objection to the method, since we never know when we have exhausted the literature and so never know when we have our types definitely fixed, while the worker who has not an enormous library at his command is unable to attempt to settle the application of his genera.

In the *Vultur* case, Dr. Allen, whose knowledge of ornithological literature is equaled by few, has overlooked two genera, *Rhinogryphus*, 1874, and *Torgos*, 1828, which, respectively, antedate *Ænops* and *Otogyps*. Fortunately for his eliminations these are both monotypic and their dates are such that they do not alter the results. If they had been proposed some years earlier, however, they would not only have replaced the above genera, which they do in any case, but by removing their species from other genera at earlier dates they would have altered the results of several of Dr. Allen's eliminations.

If *Torgos*, for instance, had been 1815 it would have left *gryphus* as the type of *Sarcorhamphus* instead of *auricularis*, while *Rhinogryphus* at 1815 would have left *papa* as the type of *Cathartes* instead of *aura*, and by Dr. Allen's method the type of *Vultur* would then have been *harpyja*. In other words, the discovery of two overlooked genera would not only replace two current genera by reason of priority, but would by *elimination* alter the types of three other genera. With the types fixed by the first species rule the only effect of the resurrection of the old names would be their substitution for the two current names having the same types.³

The *Vultur* text invites one more comment. Dr. Allen states that by ignoring 'the fixing of a type by a later author' I have 'needlessly increased the number of open cases by from probably 50 to 75 per cent.' Now as a matter of fact the fixing of a type by a later author

has no status whatever in the eyes of those who practise elimination *unless it agrees with the action of revisers* up to the time that the type was so fixed. Therefore the cases are more open under the operation of elimination than if we settled them once for all by taking the first species of the original publication as the type. For example, the types of *Cathartes*, *Sarcorhamphus* and *Gypagus*, the three genera most involved in this Vulturine muddle, were definitely fixed by Mr. Ridgway in 1874, and independently by Dr. Bowdler Sharpe in the same year, each selecting the same species, as follows:

Sarcorhamphus, type *gryphus*.

Cathartes, type *papa*.

Gypagus, type *papa*.⁴

We might infer from Dr. Allen's statements that this settled the cases of these genera for all time, for he says: "There are four conditions, any one of which when present determines the type of a genus *beyond appeal* [*italics mine*] under current usage" and as the fourth condition he gives "4. When some subsequent author has selected one of its [*i. e.*, the original genus] species as its type."

Nevertheless, he ignores absolutely the action of these two eminent type-fixers and opens all these genera to elimination with the following results:

Sarcorhamphus, type *auricularis*.

Cathartes, type *aura*.

Gypagus, type *papa*.

It seems, therefore, that the action of a later author in fixing the type of a genus is not 'beyond appeal' and 'condition 4' needs an important amendment. Further examples of the unsatisfactory nature of elimination might be drawn from this case of *Vultur*, but I fear I shall be charged with rivaling the combined vision of Romulus and Remus on

³ In spite of what Dr. Allen says on p. 777, the first species rule will give the same relief in cases where the type of one genus depends on whether or not two other groups are regarded as congeneric or not. Cf. Jordan, *SCIENCE*, 1901, Vol. XIII., p. 500, where the first species rule as advocated in my paper is formally proposed.

⁴ It is interesting to note that both Mr. Ridgway and Dr. Sharp have in each instance selected the *first species* as the type and one would be inclined to suspect that they were following, consciously or unconsciously, the first species rule, though it may have been merely a 'coincidence' as Dr. Allen suggests in another connection.

the hills of ancient Rome in the number and variety of Vultures that I have been able to discern.

With Dr. Allen's closing statement that the first species rule 'has only here and there a disciple' or that it has ever been generally abandoned *in practise* so far as ornithology is concerned, I beg to differ.

The interviews and correspondence that I have had since my paper was published show that the adoption of the first species rule as there outlined meets with very general approval among vertebrate zoologists as well as entomologists, while botanists, as is well known, have long practised it.

One prominent entomologist in a recent publication hopes that it may be incorporated in the International Code at an early date, while one of the foremost zoologists of America writes me that "elimination is absolutely dead and ought not to be revived in any code or thought of in any connection."

A thorough discussion of this subject is desirable, but really, my friend Dr. Allen and I are of nearly the same mind on the question. He says at the beginning of his article: "I have always conceded that this [*i. e.*, the first species principle] would be the ideal method if we were at the threshold of our work * * * and my opposition to it has been * * * that to adopt it now would introduce serious confusion into nomenclature." This was exactly my view, and when upon investigation I found that serious confusion (so far as birds are concerned) would not ensue, I thought that there were no further grounds for objection. The other objections that have occurred to Dr. Allen in the later pages of his paper I have tried to dispel.

At the present time I feel more sure than ever that the zoological code that adopts the first species rule (excepting in relation to Linnaeus) will be setting an example which will in a few years be followed by vertebrate zoologists in general and, with a possible **further** limitation, by invertebrate zoologists as well.

WITMER STONE

ACADEMY OF NATURAL SCIENCES
OF PHILADELPHIA

SPECIAL ARTICLES

ON A CASE OF REVERSION INDUCED BY CROSS-BREEDING AND ITS FIXATION¹

PERHAPS the most important extension which has been made of the law of heredity originally discovered by Gregor Mendel consists in the demonstration (chiefly by Cuènot and Bateson) that certain characters are produced only when two or more separately heritable *factors* are present together. Such a character does not conform with the simple Mendelian laws of inheritance, but its *factors* do. Herein lies the key to the explanation of so-called *heterozygous* characters and to the practical process of their fixation. This same principle serves to explain also atavism or *reversion*, and the process by which reversionary characters may be fixed.

When pure-bred black guinea-pigs are mated with red ones, only black offspring are, as a rule, obtained. The hairs of the offspring do indeed contain some red pigment, but the black pigment is so much darker that it largely obscures the red. In other words, black behaves as an ordinary Mendelian dominant. In the next generation black and red segregate in ordinary Mendelian fashion, and the young produced are in the usual proportions, three black to one red. All black races behave alike in crosses with the same red individual, but among the reds individual differences exist. Some, instead of behaving like Mendelian recessives, produce in crosses with a black race a third apparently new condition, but in reality a very old one, the agouti type of coat found in all wild guinea-pigs, as well as in wild rats, mice, squirrels and other rodents. In this type of coat red pigment alone is found in a conspicuous band near the tip of each hair, while the rest of the hair bears black pigment. The result is a brownish or grayish ticked or grizzled coat, doubtless inconspicuous and so protective in many natural situations. Some red individuals produce the reversion in half of their young by black mates, some in all, and others, as we have seen, in none, this last condition being the commonest of the three. It is evident that the

¹Published by permission of the Carnegie Institution of Washington.