Of course you may have some ulterior purpose for publishing such a falsehood respecting this organization, but I give you the opportunity to make a correction, assuring you at the same time that I should have more respect for you and your paper if before publishing a falsehood of this sort you would make some effort to ascertain the truth.

Sincerely yours,

Melville E. Stone,
General Manager

We regret having published a communication attributing to the Associated Press the story concerning Marchette's comet. We are glad, however, to find that the Associated Press guards so carefully its reputation for accuracy in its scientific news.—Editor.

SPECIAL ARTICLES

THE FIRST SPECIES RULE FOR DETERMINING TYPES OF GENERA—HOW IT WORKS IN ORNITHOLOGY

As a further contribution to the discussion of methods of fixing types of genera in zoology, an exposition of how the first species rule works when applied in ornithology may be of interest to other zoologists.

It is evident to every one familiar with the intricacies of nomenclature that the uniform enforcement of this rule would result in eliminating many generic names that have become, through a long period of nearly universal and unquestioned use, almost household words in the current literature of zoology, or in their transference to wholly new and more or less repellant associations. So frequently would this happen in the case of Linnæan genera that the promoters of the first species rule are obliged to make, as one of their first conditions for its adoption, an exemption clause for Linnæan genera. It can readily be seen that such an exemption clause would work charmingly in the case of North American birds, and many American ornithologists may be persuaded to swallow the sugar-coated pill thus so thoughtfully prepared for them; but it is hardly probable that such action would be followed by ornithologists at large, and quite improbable that it would meet with approval in other departments of zoology. But no way has been suggested for saving many other genera, equally as well established and as universally current.

As an illustration of how the first species rule would work when applied without restriction, a few Linnæan genera may be cited. Of the seventy-five valid Linnæan bird genera, fourteen are fortunately monotypic, and the type of some thirty-four others is by common consent (in nearly all cases by elimination) the first species. This leaves about one third of the total number with the currently accepted type some other than the first, ranging from the second to the thirty-fourth. To take the first species in these cases would create nomenclatural chaos. For example, the type of the genus *Fringilla* would be *Dolichonyx oryzivorus,* the bobolink, a bird of a different family, thus transferring the family name Fringillidæ from the finches to a wholly different group, rendering a new name necessary for the finch family; the type of the genus *Psittacus* would be *Ara macao,* a large long-tailed American species instead of the familiar gray parrot of Africa, and involving also the transference of the family name as well; the type of *Anas* would be *Cygnus cygnus,* a swan instead of a duck; the type of *Scolopax* would be an ibis instead of a snipe; and so on through the list. The same confusion would result in the case of mammals, fishes and reptiles, and doubtless in other classes. As, however, Linnæan genera are tabooed in this connection, non-Linnæan genera will be considered later on in this communication.

In Mr. Stone's second paper on this subject[1] he states that in my reply[2] to his former article[3] I relied 'mainly upon general state-

[1] 'The First Species Rule versus Elimination,' Science, N. S., Vol. XXV., No. 630, pp. 147–151, January 25, 1907.

[2] "The 'Elimination' and 'First Species' Methods of Fixing the Types of Genera," Science, N. S., Vol. XXIV., No. 624, pp. 773–779, December 14, 1906.

[3] "The Relative Merits of the 'Elimination' and 'First Species' Method in Fixing Types of Genera—with Special Reference to Ornithology," Science, N. S., Vol. XXIV., No. 618, pp. 560–565, November 2, 1906.

ments' and did not prove any of his 'facts or figures to be inaccurate.' Such facts and figures are of a kind one is not apt to carry around in one's vest pocket, or to have pigeonholed ready for immediate use. Mr. Stone had the advantage of six months or more for preparation, and presented what seemed to be —and to many really was—a convincing array of statistics. From my general knowledge of the subject I felt confident that both his statistics and conclusions were misleading. I was so strongly convinced of this that I determined at once to make a thorough examination of the case, primarily for my own satisfaction as to the real truth of the matter. After three months or more of pretty continuous application to the subject, I am glad of the occasion Mr. Stone's second article affords to make public the results.

But first a few words in reference to some of the 'points' he has endeavored to make in his rejoinder.

1. In regard to his attempt to explain away his original statement that "Elimination has never been practised in Europe and does not seem to be understood there," it seems a pretty small loophole of escape, in the face of Dr. Bather's reply[4] to this phase of his paper, to say (see foot-note to p. 148) that he meant 'simply that they did not *interpret* the method in the way Americans have done.' He cites the case of *Passerina* and *Sarcorhamphus* as the 'sort of name shifting' he 'claimed to be not understood abroad'; and says further, that where a first reviser had failed to fix the type on the first species "subsequent authors have frequently ignored them and have selected the first species as the type." This is unfortunately true of Mr. Stone and his first species rule associates, but is not true as a general statement of how things have been done in the past, either in Europe or in America. It would be easy to fill columns of SCIENCE with evidence in disproof of such an assumption.

2. It would take up too much space to reply in detail to the many points wherein he seems to have misunderstood or placed a forced construction upon my statements; yet one or two points may be referred to as an illustration of the hopelessness of attempting to diffuse light where light is not desired. If he is unable to see that I have already shown that the first species method is not always so simple in application as he has claimed, and is able to exclaim with sincerity: "Surely to ascertain the first species mentioned by an author in describing a new genus we have only to look at his original description!" and with the intention of implying that this is all there is to do under any circumstances in determining types by the first species rule, reiteration of evidence already given, and the presentation of other like evidence, seem a useless waste of effort. Fixing types implies the determination of the validity of genera, as this is the whole purpose of the work. It is one thing to look up a genus and see what is its first species, and another thing to determine whether this first species has not already been the first species of some other genus. This was the import of my remarks and illustrations, and they were open to no other construction.

3. Mr. Stone says, on p. 149, that "if the types of two or more genera happen to be the same by elimination the later genera become pure synonyms of the earliest"; which is quite true, but it has no bearing on the point at issue, which is that a later genus must become a synonym of any earlier one that has the same first species, no matter how different may be their constituents as a whole, while by elimination a heterotypic genus can be restricted so that the name, instead of being reduced to synonymy, may be conserved for some part of its original constituency. As an illustration we may take the genera *Limosa* Brisson, 1760, and *Actitis* Illiger, 1811. As originally constituted, *Limosa* contained 8 species, representing 3 modern genera; *Actitis* contained 4 species, representing 4 modern genera. Of these 12 species, only two were common to both genera. The first species was the same in both, namely, *Scolopax limosa* Linn., which by tautonomy is the type of *Limosa,* and by first species rule is also type of *Actitis.* By elimination the type of *Actitis* is *Tringa hypoleucos* Linn., the fourth and

[4] "Elimination in Fixing Genotypes,' by Dr. F. A. Bather, SCIENCE, N. S., Vol. XXIV., No. 625, pp. 809, 810, December 21, 1906.

last species. *Actitis* is currently recognized as a valid genus, with *Tringa hypoleucos* as type, but Mr. Stone, without hesitation, would relegate the name *Actitis* to synonymy and bring in some other name for the genus commonly known as *Actitis*.

4. Mr. Stone says his "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." In the opening sentence of his second paper, Mr. Stone says that the 'extravagant statements of the probable revolution that would be thus occasioned by the adoption of the first species rule in our nomenclature' are what led to his preparing a 'statement of the matter based on fact and not on theory.' If anything more 'extravagant' than his repeated assertions about the 'diversity of results' from elimination and 'the almost infinite variety of ways' in which it is conducted have found their way into this controversy I have yet to be apprized of them. He proceeds to illustrate this infinity of ways by citing two methods which he assumes to be in current use, one of them with two subdivisions, making in all, we will say, four ways of conducting elimination. There is always a common sense way of doing things and other ways. He says:

(*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.

I am glad that Mr. Stone has put these several 'methods' on record, for it throws great light upon his possible points of view of elimination, and also goes far toward explaining how his 'facts and figures' were compiled. I may here say, at the outset, that I first became aware that there was any such method as his method '*a*' only some six months ago through correspondence with Mr. Stone, or that any one could take 'congeneric' in this connection from any other standpoint than that of the eliminator! To me both of these propositions are unthinkable, for I do not see how any results—at least, any rational results—can be obtained if "we interpret congeneric to mean congeneric from the standpoint of the author of the genus." The suggestion is on its face an absurdity, as it would permit of no elimination whatever; and we must credit the author of a genus with putting an assemblage of species into a single genus which he knew were only in part congeneric and in part really belonged somewhere else. Of course, an author often states that certain species are referred to a given genus provisionally, or are given as doubtfully belonging to it. In all such cases the rules of our standard codes prohibit the taking of any such doubtfully referred species as the type of a genus.

5. In criticizing my treatment of the genus *Vultur* and the genera into which it became subsequently divided Mr. Stone says: "I fail to see why we have to ascertain the types of the involved genera when we eliminate *Vultur*." In determining the type of *Vultur*, or of any other heterotypic genus, each of its specific components must be traced to its final generic resting-place. It is thus necessary to determine first the types of all the genera to which species of *Vultur* were successively removed. As the involved genera were also good illustrations of the working of the two methods of determining types, each was taken up in historic sequence, bringing out the fact that the status of neither *Sarcorhamphus* nor *Gypagus* could be determined by looking at the description of the genus to see what was the first species; in other words, that a knowledge of the literature was necessary to get correct results in nomenclature even under the first species rule.

Mr. Stone, in his criticisms, has properly enough taken advantage of a pure blunder on my part in the elimination of *Sarcorhamphus*—an incomprehensible slip which, through haste in preparing the paper for an occasion other than its publication in Science, I overlooked and failed to observe in revising the proof. This warrants his statement that I have really, in this case, "interpreted 'con-

generic' to mean congeneric from the standpoint of the original author, and not from that of the eliminator." Nothing, however, was further from my thought or intention, for I do not admit the possibility of such an interpretation of the term 'congeneric' in connection with its use in elimination. It was an 'unconscious' slip, which most of us have now and then to regret. In reality *Sarcorhamphus,* by the method of elimination is a synonym of *Vultur,* as it is by the first species rule. If I had put the case in my usual manner of formulating elimination cases, it could not have happened. It leaves, however, the cases of *Vultur* and all of the other involved genera without change. They may be more clearly restated as follows:

Genus *Vultur,* 1758

6 noncongeneric species, representing 6 modern genera and two modern families, as follows:

1. *gryphus,* type of *Gryphus* Duméril, 1854.
2. *harpyia,* type of *Harpyia* Illiger, 1816.
3. *papa,* type of *Gypagus* Vieillot, 1816.
4. *aura,* type of *Cathartes* Illiger, 1811.
5. *barbatus,* type of *Gypaëtus* Storr, 1784.
6. *percnopterus,* type of *Neophron* Savigny, 1808.

Type, by elimination, *Vultur gryphus* Linn., the last species to become the type of a later genus.

Genus *Sarcorhamphus,* 1806

3 noncongeneric species:

1. *gryphus,* type of *Gryphus* Duméril, 1854.
2. *papa,* type of *Gypagus* Vieillot, 1816.
3. *auricularis,* type of *Otogyps* Gray, 1841.

Type, by elimination, *Vultur gryphus* Linn. *Sarcorhamphus* is thus a synonym of *Vultur.*

Genus *Cathartes,* 1811

2 noncongeneric species:

1. *papa,* type of *Gypagus,* 1816.
2. *aura.*

Type, by elimination, *Vultur aura* Linn.

Genus *Gypagus,* 1816.

2 noncongeneric species:

1. *papa.*
2. *gryphus,* type of *Gryphus* Duméril, 1854.

Type, by elimination, *Vultur papa* Linn.

6. Mr. Stone emphasizes the difficulties of elimination by calling attention to two genera I have overlooked, namely, "*Rhinogryphus,* 1874, and *Torgos,* 1828, which, respectively, antedate *Œnops* and *Otogyps,*" but which, he adds, fortunately do not alter the results of my eliminations. Space for a few words must be taken to place these 'sins of omission' in their true light. As to *Torgos,* he fails to give the author or place of publication. *Torgos* is not in 'Scudder's Nomenclator Zoologicus' (1882–1884), nor in Waterhouse's 'Index Generum Avium' (1889), nor in Richmond's "List of Generic Terms proposed for Birds during the years 1890 to 1900, inclusive, to which are added Names omitted by Waterhouse in his 'Index Generum Avium,'"[5] nor does it appear to have been before cited since its original publication. It is one of the recent discoveries of overlooked names that have rewarded the commendable zeal of some persistent name-hunter who has not yet imparted to the public the latest results of his labors.[6] As to *Rhinogryphus* and *Œnops,* they were both published in the same year, and for the incidental use I made of *Œnops* it did not occur to me to find out which has priority, as neither is at present in current use.

At this point (p. 150), Mr. Stone devotes a paragraph to what *might* have happened *'if'* the dates of certain genera had been earlier than they really were. He raises the hypothetical possibility that "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 sub-

[5] *Proc. U. S. Nat. Mus.,* XXIV., pp. 663–729, May 2, 1902.

[6] Since writing the above I have discovered by accident the place and manner of publication of *Torgos,* which it seems worth while to make public. It occurs in *Isis* von Oken, Bd. XXI, Heft 11, p. 1143, Nov., 1828, in a paper by Kaup entitled, 'Ueber Hyaena, Uromastix, Corythaeolus, Acontias, von Kaup.' Under the 'Gattung *Hyœna* Cuv.' is the remark: "Diese Gattung repräsentiert die Gattung *Torgos* (*Vultur auricularis*) mihi." So here is *Torgos,* a monotypic genus, with *Vultur auricularis* Daudin as type, in a paper devoted mainly to reptiles, in a journal with a nonalphabetic 'Inhalt' and no index.

stitution for the two current names having the same types." In reply to this it is only necessary to recall that in the case of *Vultur* two overlooked names did not in the least affect my elimination of types. Furthermore, Mr. Stone knows, and I and some others know, that since the publication of the last supplement to the A. O. U. Check-List in 1904, it has been found that more than thirty of the current generic names of North American birds will have to be replaced by others solely on the ground of priority, or will be carried back to other authors and to earlier dates, without affecting the type of any of the genera involved. We can imagine almost anything. But such hypothetical speculations are hardly to be looked for from one who especially deprecates 'extravagant statements,' and relies so emphatically upon 'facts and figures.'

7. In regard to the 'action of revisers,' it must be noted that there are all sorts of revisers, who in times past have revised in all sorts of ways, even to designating as types of genera species not originally contained in them, and even transferring names to groups wholly different from those for which they were originally proposed. This was pretty commonly practised prior to about 1850; yet where genera were restricted and a type properly designated, that is, in conformity to the requirements of modern codes of nomenclature, it is of advantage to accept them, and often a distinct aid in settling complicated cases, like the large genera of early authors. If a reviser selects his type in contravention of generally accepted rules his work is not of course entitled to recognition.

8. In this connection, Mr. Stone refers to the fixing of 'the types of the genera *Cathartes, Sarcorhamphus* and *Gypagus* by Mr. Ridgway in 1874, and independently by Dr. Bowdler Sharpe in the same year,' and adds that it is interesting to note (foot-note, p. 150) that both authors "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." But Mr. Stone fails to give us the subsequent history of this piece of work. Ten years later Mr. Ridgway and Dr. Stejneger, to whose excellent work in fixing the types of the North American genera of birds in the A. O. U. Check-List we are so greatly indebted, reversed the work of Mr. Ridgway in 1874, making *aura* the type of *Cathartes* and *papa* the type of *Gypagus,* as they have since stood in Mr. Ridgway's 'Manual of North American Birds,' as well as in the Check-List. Dr. Sharpe in 1902, in his invaluable 'Hand-List of Birds,' reversed his position of 1874, making *aura* the type of *Cathartes* and citing both *Rhinogryphus* and *Œnops as synonyms* of *Cathartes,* giving also generic recognition to *Gypagus* with *papa* as its type. Thus my recent independent determination of the types and the use of these genera is in harmony with current usage by the best authorities on both sides of the Atlantic. If Ridgway and Sharpe determined the types of these genera by the first species rule in 1874, they have done otherwise since. Evidently when Mr. Stone cited this case he really had seen 'too many Vultures' to clearly discern the present nomenclatural conditions of the group—conditions which were evidently not reached by the strict application of the first species rule.

9. Mr. Stone quotes an eminent zoologist as saying that "elimination is absolutely dead and ought not to be revived in any code or thought of in any connection." It is a suggestive 'coincidence' that another zoologist, especially eminent in invertebrate zoology and a recognized authority in several classes of animals, has expressed to me the same sentiments in practically the same language about the first species rule!

10. Great emphasis is placed by Mr. Stone upon the fact that the first species has so often become the type, even where the type has been determined by elimination. I have stated that this has often resulted by 'coincidence' rather than from a conscious reservation of the first species in the process of subdividing polytypic genera. There is abundant evidence that such is the case, but space can not here be taken to cite examples in detail.

In the case of Brisson's genera the type, by

tautonomy, is nearly always the first species. In the case of Stephens's numerous bird genera, a species is generally figured to illustrate the genus, and this species nearly always heads his list. Naturally, in subdividing these heterogeneous groups, the illustrated species is taken as the type. In other instances it frequently happens that where an author proposes a genus to which he refers several species, all the species except the first were already the types, or congeneric with the types, of other genera. Sometimes the diagnosis shows that the author based his genus primarily on the first species, and subsequent revisers, in dismembering the group, have had the good sense to restrict the original genus to this species. In the non-monotypic Linnæan genera the currently recognized type in 44 per cent. of these genera is some other than the first species. These facts suffice to show that the type may be quite often the first species by 'coincidence,' or without the conscious application of the first species rule.

11. In place of 'general statements' a few facts and figures may now be presented respecting the comparative number of name changes rendered necessary through the strict application, respectively, of the 'elimination' and the 'first species' rules in bringing the nomenclature of the genera of the Check-List of North American Birds to a proper standard of accuracy. Mr. Stone stated in his first paper[1] that the number of changes would be practically the same under each, namely, fifteen by elimination and sixteen by the first species rule. As said above, I have spent a large part of the last three months in determining the types of the genera and subgenera of the Check-List by both elimination and the first species rule. The Check-List was taken as it was left by the publication of the last 'Supplement' in July, 1904. The changes later found necessary by the Check-List Committee, being as yet not officially published, are not considered. Also, in order to show just how much truth there is in the allegations that no two eliminators ever reach the same results, owing to the 'almost infinite variety of ways' in which elimination may be applied, I have eliminated from the same basis as the original A. O. U. Committee in preparing the first edition of the Check-List, and their successors in preparing the second edition and its subsequent supplements; that is, I have taken the genera at the dates and from the same sources as they took them, even in the few cases where later investigation has shown that they originated earlier and with a different constituency. These later discoveries are considered, however, in making up the statements for comparison with Mr. Stone's statistics, with a view to treating both phases of the subject with perfect fairness.

(*a*) First as to the difference in results reached by different eliminators. I disclaim any knowledge of just *how* Mr. Ridgway and Dr. Stejneger reached their surprisingly accurate results. I followed my own method, strictly and consistently, and did not check up my results with the Check-List till my work was finished. The total number of errors of elimination in the Check-List sufficiently serious to affect generic nomenclature is 3, which result in changing the names of 2 genera and 1 subgenus. One additional change, affecting two genera, is due to the application of the principle of tautonomy, a rule not formulated till many years after the publication of the Check-List, and this change of names is thus not chargeable to the A. O. U. Check-List Committee as an error of elimination. The type in five other genera is transferred from one species to another strictly congeneric with it, in three of the five cases through the application of the rule of tautonomy, but in none of these five cases is a generic name affected. There are thus, all told, five errors of elimination, only three of which affect the names of species.

As bearing on the question of alleged diversity of results through elimination it may be noted that my results not only agree closely with the Check-List, but also in every case with Mr. Ridgway's recent eliminations in 'Birds of North and Middle America' (Vols. I.–III., 1901–1904), and also almost invariably with those of the British Museum 'Catalogue

[1] SCIENCE, N. S., Vol. XXIV., p. 562.

of Birds,' so far as the genera in these several works are strictly comparable.

(*b*) As to the changes necessary to correct properly the generic nomenclature of the Check-List. Here it is necessary to take note of recent discoveries of overlooked names, and of names transferred to earlier dates, as this was doubtless Mr. Stone's basis. This is seemingly quite a different question from that of actual errors in the Check-List eliminations. Although there are some thirty of these discoveries, it is surprising to find that not any of them affect the types of genera as determined by elimination.

The changes necessary on the basis of elimination are as follows:

Podiceps (subgenus) becomes nameless.
Cyclorrhynchus becomes *Phaleris*.
Phaleris (subgenus) becomes nameless.
Ceophlœus becomes *Phlœotomus*.

(*c*) As to the results of the strict application of the first species rule. The changes that would surely follow such action are as follows:

Colymbus becomes *Podiceps*.
Podiceps (subgenus) becomes nameless.
Phaleris (subgenus) becomes nameless.
Cyclorrhynchus becomes *Phaleris*.
Dysporus (subgenus) becomes nameless.
Aix becomes nameless.[8]
Erionetta (subgenus) becomes nameless.
Melanitta (subgenus) becomes nameless.
Actitis becomes *Tringoides*.
Bonasa becomes nameless.
Tympanuchus becomes *Bonasa*.
Cathartes becomes *Rhinogryphus*.
Gypagus becomes *Cathartes*.
Conurus becomes *Conuropsis*.
Aphelocoma becomes *Cyanurus*.
Acanthis becomes *Ægiothus*.
Spinus becomes nameless.
Passerina becomes *Plectrophenax*.
Cyanospiza becomes *Passerina*.
Pœcetes becomes *Zonotrichia*.
Zonotrichia becomes nameless.

Summary.—According to Mr. Stone, the Check-List contains 124 composite genera for which no type was designated by the founder, and of which the type has been fixed by 'elimination.'

The strict application of elimination involves 3 changes of names, of which 2 are generic, affecting the names of 2 species, and 1 is subgeneric, and hence does not affect the names of species. Mr. Stone's estimate was 12 genera and 3 subgenera—an estimate over 500 per cent. greater than the reality.

The strict application of the 'first species rule' involves 16 changes of generic names not otherwise necessary, which affect 33 species and 18 subspecies, and 5 subgeneric names, making 21 changes in all. Mr. Stone's estimate was 16 changes, an under-estimate of nearly 33 per cent.

The ratio of the required changes by elimination is as 1 to 41 by first species rule as 1 to 8, or 5 times as many by the first species rule as by elimination. A number of other genera that would be relegated to synonymy by the first species rule are saved only by the rule of tautonomy.

Seven other lamentable changes in the names of numerously represented genera of American birds come under the spirit of the first species rule and partly under its scope, although urged on the basis of priority, in disregard of a hitherto almost universally recognized principle of nomenclature—the designation of types by the founder of the genus. It has been customary in selecting types to consult the intent of the author, and to accept his types even if only inferentially designated. In 1827, Swainson published two papers on birds, one a list of a collection of birds from Mexico, the other describing many new genera; this more general paper was sent to the *Zoological Journal* for publication long before the other was sent to the *Philosophical Magazine*, which latter, however, was unfortunately published a few months before the other. In the *Zoological Journal* paper were described five new genera represented in the Mexican collection reported upon in the *Philosophical Magazine*. In referring species to these unpublished genera he made a cross-reference to *Zoological Journal*, No. 10, where they were not only described but had their

[8] That is, if *A. sponsa* and *A. galericulata* are considered as noncongeneric, as is done by various late authorities.

types explicitly designated; and these types had been accepted by all subsequent authors, down to 1904, or for over 75 years, when it was proposed to take the first species associated with the generic name as the type, instead of the type designated by the author a few months later in the same year. In each case the generic name is transferred to a wholly different group, and different names, some of them new, have to be substituted for the groups long known under the displaced names. These names are:

Ammodramus, changed to *Coturniculus*.
Coturniculus, renamed *Ammospiza*.
Euetheia, changed to *Tiaris*.
Tiaris, renamed *Charitospiza*.
Helminthophila, changed to *Vermivora*.
Dendrornis, changed to *Xiphorhynchus*.
Xiphorhynchus, renamed *Xiphornis*.

These changes affect 18 species and 10 subspecies of North American birds, and about 30 species and subspecies of Mexican and South American birds.

The revised A. O. U. Code (as yet unpublished) has a rule to the effect that an author may designate the type of a new genus in any part of the work or paper in which the genus was originally proposed, and (by inference) not elsewhere. But it has happened many times in the past that an author has designated the type of his own genus in some subsequent work, and such designation has been respected as valid. It hence seems desirable to add to the new A. O. U. rule the following provision, namely: *The type of a genus designated by its author in a publication subsequent to the one in which the genus was originally proposed may be taken as its type provided that the species thus designated as type was one of the original species and had not in the meantime been made the type of some other genus.* This would prevent the ruthless overturn of such long established names as those mentioned above.

12. In conclusion, a word on the subject of methods of elimination. I fail to see in elimination but a single principle, the rule of priority. As Dr. Stiles has well said:[9] "If this principle is just when applied to generic names, why is it not equally just when applied to the generic types?" I also fail to see how there can be more than one way of applying the rule, or anything difficult or abstruse in it, beyond a proper knowledge of the literature of the subject. Experts evidently *do* reach the same results; those who try to apply the principle without thoughtful consideration of how to do it naturally meet with trouble. As said before, it is unfortunate that there has been so rarely a definite statement of the process, which should have long since been set forth in the codes of nomenclature for the guidance of the inexperienced. The statement of the method given in an earlier number of SCIENCE (Vol. XXIV., p. 777) covers the whole matter. Where trouble arises it is not from any obscurities of the method but comes from the taxonomic side, the doubts that arise in relation to the validity or value of groups that have been set aside as genera or subgenera. But this would arise equally under any method of determining types.

The convenience of the first species rule is its only asset; every other consideration, as emphasized by Dr. Stiles (*l. c.*), scores against it. It ignores all types hitherto established under any other method, in the case of genera whose types are not determinable by one of the three universally accepted rules. As Dr. Stiles has well said, when a "type is once designated, by any method whatever, 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?" As a matter of fact, the A. O. U. Committee in preparing the Check-List established the types of such genera of North American birds as had not had types previously properly designated, and why now reopen the cases except for cause? The proposal of a new rule, obviously disastrous to the stability of nomenclature, is certainly not a sufficient cause.

Dr. Stiles makes reference to the rule laid down by Linnæus himself for the determina-

[9] "The 'First Species Rule' vs. the 'Law of Priority' in Determining the Types of Genera," SCIENCE, N. S., Vol. XXV., No. 630, pp. 145–147, January 25, 1907.

tion of the types of his own genera. It is my opinion that Linnæus's rules were followed to a larger extent by his disciples and immediate followers than we are wont to recognize. But the types of Linnæan genera in ornithology are not now a source of trouble. With the exception of two or three, of which *Vultur* is one, they have long been settled in a way to meet general approval. I do not apprehend that Dr. Stiles meant to suggest that any of these cases should be reopened if it is found that they were not settled in accordance with Linnæus's own rule. Indeed, his later remarks (quoted above) seem to preclude such a suggestion.

In stating the results of my determination of types by the two methods, as given above, it is impracticable to show the steps by which they were reached; I shall, however, publish soon elsewhere not only the basis of these results, but a complete list of all the North American genera and subgenera, with their types by both methods, where the results differ; and also showing each step in the process of elimination for all the genera to which elimination is applicable.

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CURRENT NOTES ON METEOROLOGY AND CLIMATOLOGY

CUMULUS CLOUDS OVER THE SAN FRANCISCO FIRE

Mention was recently made in these notes of an observation recorded in *Nature* of cumulus clouds which formed over the fire succeeding the San Francisco earthquake of last year. Attention was at the time called to the fact that this was the first, and only, mention of such clouds which seems to have found its way into print. The publication of that comment in Science brought to the compiler of the notes a letter from Professor George D. Louderback, of the University of California, with reference to the clouds observed by him on the same occasion. As the matter is of some general interest, Professor Louderback's letter is here inserted, with the permission of the writer.

From your review of Mr. Van Norden's description of a cumulus cloud over San Francisco at the time of the great fire, and your noting that it is the only reference to that phenomenon you had seen, I have concluded that you may be interested in a corroborative observation of mine on the same occasion. I was in Nevada at the time of the earthquake, and as my parents and other near relatives lived in San Francisco I took the first train for that city, but on arriving at the Oakland pier the morning of the nineteenth I found that no one was permitted to cross by the ferries in that direction. I spent several hours on the water front looking for a launch that would take me over, and then had a slow trip across the bay and arrived in San Francisco about five o'clock in the afternoon.

The form of the rising column of smoke impressed me very strongly, and I have made a number of efforts since to find some one who might have taken an expressive picture of it, but so far, without success. The dark smoke rising from a large area of the city rather quickly gathered itself together and rose to a great height as a tall column with a low conical base. At the top it spread out in a practically horizontal layer and drifted slowly to the northwest, in which direction its limit was beyond the range of vision. In fact, the first visible indication that I had of the fire was this drifting smoke cloud beyond the coast mountains seen from the train at Benecia (north of San Francisco), and looking west along the gap of Karquinez Straits.

This horizontal cloud extended a very short distance to the south of the main column—probably not greater than the thickness of the column. Rising above its upper surface and directly over the vertical shaft of smoke was the cumulus cloud, its upper surface forming four or five beautifully regular and pure white domes. Not only was it differentiated from the rest of the visible floating material by its form and position, but distinctly by its color and luster, and I decided that the cumulus cloud was of pure water particles, uncontaminated with the smoke particles that gave their character to the horizontal stratum. I watched it, at intervals, for several hours and noticed but little change. It reminded me very strongly of the photographs I have seen of volcanoes in eruption, and especially suggested the stone pine of Vesuvius. Even the form of the volcanic mountain was presented to the eye in the conical spreading base of the smoke column.

As we approached the city the lowering sun was veiled by the cylinder of smoke, and later by the lower layers, and produced the most striking and weird absorption effects. It became a deep fiery