

forth in the single paragraph in the second column on page 602 of SCIENCE.

For the past six years my whole time has been given up to work relating to investigations as to the probable origin and physical structure of our sidereal system. In the course of these investigations the question, *What is the present surface-temperature of the sun?* has recently given me much trouble, for the results of different investigators vary all the way from twelve hundred degrees up to eighteen million degrees centigrade!

With the aid of recent observations, made with a mirror which I figured about three years ago, and which, for this kind of work, is by far the most powerful telescope ever constructed (aperture two feet, focal length three feet) I finally deduced the simple, fundamental, *theoretically exact* expression given below.

This equation proves that if Professor Poynting's value for the temperature of the "small black particle" is correct the sun's surface temperature is twelve million degrees instead of only six thousand.

In my approximate determination of the absolute temperature of space with the aid of the mirror, no allowance has yet been made for absorptions and reflections due to ponderable matter in the space between the sun's surface and the focal point of the mirror. Professor Poynting's value for the absolute temperature of the "small black particle" is 300° ; my uncorrected value for the same particle is $0^{\circ}.5+$. So that according to my results the effective surface temperature can not be less than twenty thousand degrees centigrade.

If t is the temperature of the "small black particle" at the distance r from the center of the sun, and t_0 is the effective temperature of the surface of the sun at the distance r_0 from the sun's center, then my *theoretically exact* formula is simply

$$t_0 = t \left(\frac{r}{r_0} \right)^2,$$

a Newtonian expression which, according to the assertions of modern astrophysicists, can not be used for determining the effective surface-temperature of the sun; so far as I can

learn this stand has been taken mainly for the reason that the very high resulting temperatures heretofore obtained seem to be inadmissible.

I had intended to defer the publication of my present views regarding the probable origin of our stellar and solar systems until more definite observational and more theoretical data had been deduced; but as repeated reference to a theory should be accompanied by some evidence bearing on the question "Is the theory tenable?" I will shortly forward for publication in SCIENCE a very brief statement of the results so far obtained.

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ANN ARBOR,
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ARTICLE 30 OF THE INTERNATIONAL CODE OF ZOOLOGICAL NOMENCLATURE

THE new article 30 of the International Code of Zoological Nomenclature, adopted by the International Congress of Zoologists at its recent meeting in Boston,¹ is beyond question a great step forward in providing definite methods for determining genotypes in zoology. Although the old article 30 is canceled, the new article 30 includes all of the principles of the old one, of which it is virtually an extended amplification, embracing seven distinct "rules," and thirteen additional "recommendations," the former numbered *a* to *g*, and the latter *h* to *t*. The recommendations have relation to the selection of types for genera still typeless, but one of them, numbered *i*, and relating to "virtual tautonomy," might well have been transferred to the "rules." The "cases" are wisely separated into two categories: "I. Cases in which the generic type is accepted *solely* upon the basis of the original publication." "II. Cases in which the generic type is not accepted *solely* upon the basis of the original publication."

The first class includes: (*a*) all those genera, the founder of which designated the type at the time of founding the genus; (*b*) those genera, the founder of which used *typicus* or

¹ See SCIENCE, N. S., Vol. XXVI., pp. 520-523, October 18, 1907.

typus as a new name for one of the species he included in it when founding it. In both groups the type designated by the founder "shall be accepted as type regardless of any other considerations." (c) A genus proposed with a single species takes that species as its type. (d) Any genus founded without a type being provided for it under one or the other of the above conditions, but which "contains among its original species one possessing the generic name as its specific or subspecific name, either as valid name or synonym, that species or subspecies becomes *ipso facto* type of the genus."

It is safe to claim that 70 per cent. of all generic names in ornithology, and probably in vertebrate zoology, are determinable upon the original basis of publication, by one or the other of the methods above prescribed—methods, too, which everybody accepts. The other 30 per cent. are provided for by rules *e* to *g*, rule *e* designating the conditions upon which rules *f* and *g* must rest—namely, that the species alone available are those that were included in the genus when it was originally published, of which, however, none is available that was indicated by the author as, from his standpoint, either of doubtful status or of doubtful pertinency to the genus. With this useless rubbish cleared away, rule *f* provides:

In case a generic name without originally designated type is proposed as a substitute for another generic name, with or without type, the type of either, when established, becomes *ipso facto* the type of the other.

This is a sensible innovation that may now and then prove extremely useful. But the grand stroke is rule *g*, as follows:

If an author, in publishing a genus with more than one valid species, fails to designate (see *a*) or to indicate (see *b*, *d*) its type, any subsequent author may select the type, and such designation is not subject to change.

This last rule, as old as the B. A. Code, completes the rules for type determination, and provides essentially only four methods, which are designated:

(1) "Type by original designation" (rules *a* and *b*); (2) "Monotypical genera" (rule

c); (3) "Type by absolute tautonomy" (rule *d*); (4) "Type by subsequent designation" (rules *e*–*g*). By a wise stroke of diplomacy, the word "elimination" is not mentioned; yet elimination is the basis and the method, and necessarily always has been, of any sound work by a first reviser.

To rule *g* is added:

The meaning of the expression "select a type" is to be rigidly construed. Mention of a species as an illustration or example of a genus does not constitute a selection of a type.

This seems explicit, but is far from being so; while it will tide over some difficulties, it will open up others. Not only this, but must the designation of a first reviser always be accepted, right or wrong, or only when made in accordance with fundamental rules of nomenclature that have been extant in all codes since the publication of the British Association Code of 1842?

One need not have had very extended experience with the work of "first revisers" to have learned that it is of all grades of quality from absolutely pernicious to unqualifiedly beneficent, having been often done by systematists who knew nothing of rules of nomenclature, or else disregarded them. One need not go very far back in the history of even American ornithology—less than half a century—to find that species have been taken as types of genera that were not described till long after the genera were founded; or that genera have been taken from pre-Linnæan authors when they became tenable only from Linnæus or from some much later author; or that types thus designated for certain genera had long before properly become the types of other genera and were not available as types of entirely different genera.

That the new article 30 is not intended to countenance such work is clearly indicated by the first section of rule *e*, which states that no species can be taken as the type of a genus that was not included in it at the time of its original publication. Again, if a reviser, ignorant of the literature of his subject, or merely neglectful of rules, chooses as the type of a genus a tautonomic species of an earlier

genus, or the type of a previous monotypic genus, or a species some earlier reviser has properly chosen as the type of some other genus, rules *a* to *d* clearly show that his work must be construed as void. Evidently an earlier monotypic genus can not be canceled by the act of some blundering reviser who chances to seize upon its only species as the type of some other genus; nor can a genus with a "type by subsequent designation" be canceled because its type was later made the type of another genus. This would seemingly all go without saying were it not that some systematists assume that the designation of a type by a first reviser is sacrosanct and must stand regardless of any other considerations.

This emphatic reaffirmation of the principle of "type by subsequent designation" is exceedingly gratifying. Yet, for reasons in part already stated, it is to be regretted that the International Commission did not define the manner of its application. This doubtless did not seem necessary; but there is apparently nothing so uncertain as the point of view from which any problem in nomenclature may be approached.

The great utility of the "type by subsequent designation" rule as an aid in establishing genotypes is not at first apparent; and in recent years it appears to have been to a great extent overlooked, it having been regarded by many as vague and illusory, and difficult to apply with certainty and precision. That the principle was formerly respected and extensively and effectively employed is evident from a study of nomenclatural progress during the last half century. My recent investigations in an attempt to show how the types of the genera of North American birds were determined,² to which Mr. Stone has recently directed attention,³ resulted in disclosing the extent to which the currently accepted types of polytypic genera in ornithology have been fixed by "subsequent designation."

As Mr. Stone has well said (*l. c.*):

² "The Types of the North American Genera of Birds," *Bull. Amer. Mus. Nat. History*, Vol. XXIII., pp. 279-384, April 15, 1907.

³ *SCIENCE*, N. S., Vol. XXVI., pp. 444-446, October 4, 1907.

Much of the chaos in generic nomenclature which has become intolerable to the systematist of to-day has been brought about by the failure of many writers to explain by what process they have determined the types of old polytypic genera. Had they been more explicit upon this subject, we should have been able long ago to see the weaknesses in our codes and should have abandoned methods which were neither definite nor final in their operations.

In fact, it is only about thirty years since it became the practise for even monographers to give types for genera founded by previous authors except sporadically, and rarely has the method of their determination been stated, except in the case of types determined by elimination, beginning with the A. O. U. Nomenclature Committee in 1886. There is merely the bare statement that the type of a genus is a certain species. Usually it is necessary to trace back the literature to ascertain whether the genus was originally monotypic, or whether the type was designated by the founder, or determined in some other way. Nor can this be fully shown until, in addition to giving the author, date and place of description, is also given the original constitution of the polytypic genera, with a list of the species and their final generic disposition.

My purpose in preparing the paper above cited was to ascertain for my own satisfaction two things: (1) whether it was true, as alleged, that no two investigators could reach practically the same results in type determination by the method of so-called elimination; (2) to determine the relative number of changes necessary in the generic names of North American birds by elimination and by the first species rule. An entirely independent, or *de novo*, application of elimination resulted in only three changes chargeable exclusively to elimination, equal to about three fourths of one per cent. of the total number of genera and subgenera involved; twenty would be necessary from the enforcement of the first species rule, *with all the Linnæan genera excluded*, eighteen of which have received the approval of the A. O. U. Committee, acting tentatively under the first species rule.

As Mr. Stone has said some very pleasant things in his notice (*l. c.*) of this paper, I regret that he seems to have so imperfectly understood its scope and methods as to have been misled into some erroneous criticisms of it. For instance, its scope is distinctly stated to be "the genera and subgenera of the second (last) edition of the A. O. U. Check List and its several supplements, for the purpose of showing how the types, as now currently accepted, came to be so recognized." Only two of the eleven genera Mr. Stone states to have been omitted from this paper are embraced in the Check List and its supplements. He evidently has confused the unpublished decisions of the committee with those actually published. Of the two subgenera omitted, one has been abandoned by its author and the other has lost standing; so both were purposely (but perhaps unwisely, as now appears) left out of consideration.

He also charges me with using various methods to reach my results, as "elimination," "subsequent designation," "general consent" and "restriction." As a matter of fact, I had a right to use each of these methods, under my proposition to show how the types as now recognized were determined, in any case where they were evidently employed. Obviously elimination (in its restricted sense) can not apply to (1) genera based solely upon a diagnosis, (2) to genera containing originally only congeneric species, nor (3) to genera containing two or more congeneric species after the non-congeneric species have been removed. I gave the results of elimination where it applied, and added, as matter of presumable interest, the other information. For instance, as stated in my introduction (p. 286), I became impressed in the course of my work "with the great frequency with which the types of genera and subgenera as designated by him [Gray] in 1840 to 1855 are still the currently accepted types." And added: "The agreement was of such striking frequency that finally after my manuscript was typewritten and revised for publication I compared my results with Gray's designations, and interpolated, as an afterthought, 'type

as designated by Gray,' " etc. It is therefore rather surprising to be informed by my reviewer that I am so inconsistent as sometimes to accept Gray's type designations and at other times to ignore them, or even deliberately reject them; and that owing to my following so many different methods "my conclusions with regard to the types of many of the older polytypic genera will hardly be accepted."

Mr. Stone's criticisms as a whole show how a strong mental bias may blunt one's perceptions. Space can not be taken here to point out his misstatements in detail; nor would any notice be taken of them were it not that my paper can have only a limited circulation, and is thus likely to be judged by Mr. Stone's presentation of it rather than on its actual merits, or demerits, as the case may be. In a work of this character mistakes of various sorts are inevitable; it is impossible to reproduce in print thousands of citations without clerical or typographical errors, or without some errors of omission. In addition to the several actual errors pointed out by Mr. Stone, for the indication of which I am grateful, there are others that he fails to mention.

In regard to Gray's work as a first reviser, I stated (*l. c.*, p. 286):

Of the genera published prior to 1855, the types, as now recognized, are the same for about 90 per cent. of the genera as those indicated as the types by Gray in 1855; in about half of the remaining cases Gray took as type a species not originally included in the genus. The discrepancy in the other cases is due to Gray's point of departure for generic names, since in twenty instances in the case of North American birds alone he took genera from Ray (1676), from Moehring (1752), or from Linnæus prior to 1758 (1735-1748).

In all such cases his type designations have been consistently repudiated by subsequent systematists, while all those made in accordance with the essential rules of all nomenclatural codes have been adopted and form a part of our standard nomenclature.

Gray was a pioneer in the work of designating types of genera, and his great influence in reducing the nomenclatural chaos of his day to some degree of order and stability ap-

pears to have received heretofore very little formal recognition. He began his work before there was any authoritative code of nomenclature; the basis of his decisions was, as he states, "the inflexible rule of priority," strictly enforced, which he employed without any of the modern restrictions as to when it should begin to be operative. He was handicapped, as he especially complains, by the lack of the works of continental authorities; and his knowledge of the world's ornithology at this early date (1840-1842) was grossly defective, judged even by his own later standards. When preparing the first three editions of his "List of Genera," hundreds of the genera of his predecessors were unknown to him; many were still omitted from his greatly enlarged 1855 edition, and some few escaped him altogether, as shown by their absence from his wonderfully complete and invaluable "Hand-List of Birds" published in three volumes, 1869-1871. Nor is this surprising, since old names by the score are even now being brought to light. But his early omissions and his early point of view regarding the value and relations of groups named by his predecessors have an important bearing upon the validity of many of his earlier type designations; and also upon the application of rule *g* of article 30 of the International Code, and, I may add incidentally, upon Mr. Stone's strictures upon my alleged treatment of Gray's type designations. A few illustrations of the haphazard manner in which Gray, Lesson, Vigors, Swainson and others designated types from about 1824 to 1845 would make clear the impropriety of taking their work too seriously, but space for the purpose can not well be taken in the present connection.

Mr. Stone says, there are "two methods of type fixing, either of which will yield definite and final results—the first species rule and type by subsequent designation." In as much as the first species rule has been rejected, in effect if not formally, by the Nomenclature Commission of the International Zoological Congress, this is hardly an ingenuous statement, coupled as it is with the further assertion that the Zoological Commission has "re-

pudiated the elimination method." As a matter of fact, the elimination method includes "type by subsequent designation"; a careful canvass of some 500 bird genera shows that the results by the two methods are practically identical, as would be expected on the principle that the greater includes the lesser.¹

Under a common sense construction of article 30, a species not originally included in the genus can not be taken as its type; neither can the original species of a monotypic genus, a tautonymic species, nor a species that is the type of a genus by original designation, be subsequently taken as the "type by subsequent designation" of some other genus. This being conceded, it is safe to say that the emphatic and unequivocal affirmation, in euphemistic phraseology, of the long-standing "first reviser rule" will ensure the permanency of the types as now recognized of virtually all the genera of vertebrates, and probably of many other groups of animals. To illustrate, the authors of the various volumes of the British Museum "Catalogue of Birds" (1874-1898) assigned types for all of the bird genera known to them, whether valid genera or synonyms, while nearly all of the later published genera have had their types designated by the founder. In a few cases the authors of the British Museum "Catalogue of Birds" assigned as type of a genus a species not originally contained in it, or otherwise made a few improper designations, but such mistakes are fortunately few. It thus happens that probably 98 per cent. of the genera of birds will be found to have already types that conform to the provisions of the new article 30 of the International Code.

It remains now simply to hope that the good sense of systematists will lead them to adhere strictly to the International Code.

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¹ Cf. D. W. Coquillett, "The First Reviser and Elimination," *SCIENCE*, Vol. XXV., pp. 625, 626, April 19, 1907.