

of the equator. The motion of a satellite-like meteor, passing through the upper atmosphere, presents a serious dynamical problem, and the observations of the procession were so difficult to reconcile with the kind of path that the fireballs were supposed to have taken that several investigators actually questioned the reliability of the data. The fireballs were seen by many people in Ontario, and the height at which they passed was definitely found to be 26.4 miles. It was questioned at the time whether a swarm of meteors passing Canada at so low an altitude could persist in its motion near the curved surface of the earth for three thousand miles; moreover, some of the observations made from ships fell by several miles on the wrong side of the projected great circle path that best represented all the northern observations.

Dr. Fisher has recently examined the data again, and he concludes that the observations can now be satisfactorily interpreted. The swarm of fireballs, he considers, was an extensive one, so that the lower members seen in Saskatchewan and Ontario fell into the Atlantic near the coast, and their companions, which passed unobserved over Canada at greater altitudes, were the bright fireballs observed from ships in the southern Atlantic. Dr. Fisher has succeeded in interpreting the observations by considering two factors previously neglected; the earth's equatorial bulge and its daily rotation. It is the existence of the equatorial bulge that draws a satellite meteor towards the earth's surface as it rushes through the atmosphere. The effect of the earth's rotation is perhaps more striking; a meteor entering the atmosphere moves relative to the earth as a whole, not to the rotating surface, and therefore the projection of its path is not a great circle—it turns out in this case to be a curve, concave (in the northern hemisphere) to the southwest, and having a turning point at the equator. All the reliable observations of the swarm of February 9 are satisfactorily represented by such a curve, and their satisfactory interpretation marks an advance in the study of the dynamical problem of the motions of meteors.

In his researches on meteors, Dr. Fisher is effectively studying the fundamental cosmic problem of the composition and motions of the population of interstellar space, and a knowledge of the dynamics of meteors is one of his more important tools. If other fireballs, or swarms of fireballs (the so-called minor comets) could be observed as widely as the swarm of 1913, and with greater accuracy, the study of interstellar space could become an individual science. But like all other sciences, it requires a basis of accurate and systematic observation, and at present the student of interstellar visitors is dependent for his

facts upon an unprepared and uninstructed public. To educate them is perhaps his foremost task.

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## FAMILY NAMES

THE International Code of Zoological Nomenclature dismisses the subject of family and subfamily names with two very brief pronouncements:

Art. 4. The name of a family is formed by adding the ending *idae*, the name of a subfamily by adding *inae*, to the root of the name of its type genus, and

Art. 5. The name of a family or subfamily is to be changed when the name of its type genus is changed.

A very serious difficulty arises from two points of view, each extensively employed by taxonomists, as to what shall constitute the type genus of a family.

The one point of view is that the oldest contained genus, *ipso facto*, regardless of other considerations is the type genus of the family. The other school considers that the first author to employ a contained generic name with a plural ending, with the significance of a group higher than genus (whether he called it family, subfamily, tribe, cohort, legion, phalanx or what-not) by that fact established the genus in question as type of the higher group and that his action is not subject to change.

The principle of establishing a type, whether of a specimen for a species, a species for a genus or a genus for a higher group, is the same. It is founded upon recognition of the fact that authorities disagree and have an inalienable right to disagree as to the limits of groups—whether species, genus or family. Therefore, when an author proposes a new species or a new genus or a new family, he is not at liberty to bind the future as to the limits which the group shall assume—no code recognizes his right to do that, for that is a matter of taxonomic fact and of personal judgment, not subject to fiat. All he can do (so far as the codes of nomenclature are concerned) is to establish a nucleus for his group—the type specimen or type species or type genus, as the case may be, and all that a code of nomenclature can do is to establish, in case the original author did not make it clear, *what* that nucleus is, and having once established that, then they proclaim that in the future said nucleus or type together with all other individuals that are considered conspecific with it or species congeneric with it or genera belonging to the same family, as the case may be, shall always and forever be called by the group name which the original author proposed, provided he met certain requirements as to form of name and was not anticipated in his action by others.

This principle is very fully recognized for genera and their type species. The code enters at length into the really very intricate matter of determining just what species shall be accepted as type in the case of the legions of genera that have been proposed only from the point of view of their *limits* (that is, by diagnosis) and not from point of view of their *nucleus*, that is, by type designation. It is one of the most important matters of nomenclature and involves a great deal. Owing probably to the greater difficulty of dealing with concrete objects, the International Code does not refer to the type specimens of species. But the principle is well understood and in general use in that connection.

There seems to be neither logical reason nor practical reason why the same principle should not be applied to groups higher than genera. And in their cases the matter is more simple, for there is never any question as to the intent of the original proposer of a subfamily or family as to its type. His intent is definitely established by the stem of the genus that he employs to which is added a patronymic or plural ending.

The only question is, shall we rule that the original proposer of a group higher than genus was not at liberty to choose a type genus to represent it, but must perforce use the oldest contained genus as type, and if he failed to do so that his work was invalidated?

The very great danger of such a procedure is that it is a departure from the principle of *nucleus* toward the principle of *limits* and thus a serious trespass in a nomenclatorial question upon the precincts of taxonomy. For it is obvious that, under that ruling, if Smith includes in his new family only A-us 1850 he must call the family A-idae. But if Brown includes both A-us 1850 and B-us 1825 in the family he must call it B-idae, for to him the type genus must be the older B-us, and if Black, with still different taxonomic views, includes also C-us 1800, to him the type genus would be C-us and the family name C-idae. And yet all might be contemporaneous workers and with perfectly reasonable but altogether different convictions as to the proper limits of that family.

Is it not, therefore, obvious that insistence upon the principle that the oldest contained genus is *ipso facto* type destroys the whole idea of a type or nucleus, for it utterly disregards the intent of the proposer of the group, as expressed in the name he used, and sets up as a standard for the family name a base that may fluctuate with every realignment of the family limits. If that practice had been contemplated by the framers of the code, they would have worded Article 4 to read, "The name of a family is formed by adding *idae*, the name of a subfamily by adding *inae*, to the root of the name of its oldest

contained (instead of type) genus." The very fact that they used the word type genus and went into no further detail or explanation leaves the warrantable assumption that type genus implies a meaning and application of the term type similar to that which is well understood and fully discussed in the code in connection with the type species of a genus.

When an author recognizes for the first time the taxonomic affinities existing between certain genera and proposes to group them together as a group of greater rank than genus, he in so doing performs an act of at least equally great taxonomic significance with that performed by the describer of a new genus. He has founded a potential family, regardless of the rank that he assigns his group, just as the proposer of a "variety" founds always a potential species. If in proposing the group he establishes for it a name, he is performing not only a taxonomic act, but also a nomenclatorial act, of at least equal importance to the coining of a new generic name. If the proposer of a generic name does not meet certain arbitrary conditions that we have laid down, as, for example, if his name is not uninomial, the name is outlawed. Likewise, if the proposer of the new group name does not meet certain conditions, specifically if he coins a name instead of using the stem of a generic name from within his new group plus a plural termination, his name is outlawed. But if he *has* met those conditions, then from the standpoint of nomenclature and of taxonomy he has founded both a potential family and a family name (save only possible modification of termination to suit the rank), and it makes no possible difference (nomenclatorially) what rank he assigned it, for that is again a question of taxonomic perception and not of nomenclature.

I wish to propose for the consideration of the zoological profession and for eventual transmission to the International Commission on Zoological Nomenclature for consideration as an amendment to the code, the following paragraphs:

(a) The type genus of a family or subfamily shall be the contained genus of which the stem of the name was first employed in combination with a termination in Latin plural form to designate a group higher than genus. If any termination was originally used other than provided for in Article 4 of the code, said termination shall be changed to bring it into conformation with that article.

(Older authors rarely used the terminology to-day required.)

(b) The name of a family or subfamily shall date from the time it was first proposed as a group higher than genus, provided it was based on a contained generic name.

(The older authors used many terms to indicate groups equivalent from a nomenclatorial standpoint to what we now call family and subfamily.)

(c) *Recommendation.* When erecting a subfamily or family, an author should choose the oldest valid contained genus as type, whenever feasible; but no family or subfamily name is to be changed because its type is not the oldest contained genus.

#### UPON THE CHANGING OF FAMILY NAMES

If a family consists of its type genus and all other genera that any given taxonomist considers should be associated with it, then that type genus may never be changed without nullification of the principle of type and of priority; and since the *name* of the type genus can not be changed, unless it is a homonym, the *name* of the family can not be changed, except in the same case.

If common usage is based on the misapplication of the name of the type genus of a family, then common usage will also be misapplying the family name to a group of genera that actually should not come under it. The restoration of the name of the type genus to its correct sense under the code will involve the application of the family name to an unfamiliar group of genera to which current usage has not applied it, and will leave the group of genera to which it has been incorrectly applied (since its type genus is not one of them under the code) under the necessity of being fitted with a different name and type genus. That is in no sense a change in the family name nor that of its type genus. It is a corrected application of each.

To make this quite clear, let us assume that A-us type of A-idae is currently used as though Y (not an originally included species) were its genotype, and consequently the family A-idae as though it consisted of A-us (A-us y) + B-us + C-us (B-us and C-us being two genera of the same family group as A-us y). But under the code the only originally included species A-us z must be type of A-us, and A-us z is not of the same family group as A-us y, or B-us or C-us. Therefore, under the code A-idae really consists of its type genus A-us (but with species z, not y) + such other genera as belong to the same family group as A-us z, let us say E-us and F-us. This leaves the genus containing A-us y without a name, and the family group A-us y + B-us + C-us without either a name or a type genus.

It follows that the only case in which the name of the type genus of a family can be changed is in case it is a homonym. I, therefore, wish to propose the following modification of Article 5 of the code, in the interests of precision and clarity:

Art. 5. When the name of the type genus of a family or subfamily is found to be a homonym, it must be changed to correspond to the change of the name of its type genus.

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#### SCIENTIFIC EVENTS

##### THE BRITISH EXPEDITION IN EAST AFRICA

THE trustees of the British Museum announce, according to the *London Times*, that a valuable work in scientific research, which already has added considerably to knowledge of fossil remains, is likely to be checked owing to lack of funds to carry it through.

In 1924 the trustees sent an expedition to the Tendagura district of Tanganyika Territory, which is particularly rich in fossil reptiles, and especially in forms whose nearest representatives are to be found, it is believed, only in North America. Before the war several German expeditions collected in the district much material relating to the dinosaurs, but they left many gaps, and it was to fill those gaps that the British expedition went out under the leadership of W. E. Cutler. Mr. Cutler's assistant was L. S. B. Leakey, an undergraduate who could speak Swahili fluently, but Mr. Leakey had to return after a few months in order to resume his studies at Cambridge. Mr. Cutler carried on without an assistant, but died of malaria at Lindi in August, 1925. F. W. H. Migeod then went out, accompanied by Major T. Deacon, and they returned to England in 1926. Early in 1927 Dr. John Parkinson was appointed leader and Major Deacon went back with him.

As a result of the expedition over 500 cases of specimens have been received at the museum, and much work has been done in mapping out the geology of the Tendagura district. Dr. Parkinson has also visited the site at Koru, in Kenya, where interesting fossils have been found. It is desired that the line extending westwards of Tendagura towards Lake Nyasa should be explored to ascertain whether dinosaurian and other fossil remains occur along the course of the ancient river.

The cost of the expedition has been met partly from the reserve fund which had been accumulated by the trustees and partly from a special fund to which well-wishers subscribed in 1924. The former fund is low and the latter will soon be exhausted; without further help the expedition must be brought to an end next December. About £3,000 is the annual cost of the expedition. This is considerably more than the trustees