These are at present as follows:

Nature of manufacture.	Number of establishments.	Number of persons employed.
Astronomical, optical, mathematical, physical and electrical instruments	500	9,200
Glass-blowing, glass instruments, glass thermometers	125	1,773
Optical instruments, spectacles, reading-glasses	165	2,652
Total	790	13,625

THE FIRST SPECIES NAMED AS THE TYPE OF THE GENUS.

IN the suggestive article on 'The Method of Types in Botanical Nomenclature,' by Mr. O. F. Cook, published in SCIENCE of September 28, 1900, is an admirable statement of the meaning of type in biological taxonomy.

A species 'is a coherent or continuous group of organisms.' Its type is the first individual on which the specific name was bestowed. The type-specimen has an especial value in fixing the name and meaning of the species.

In like manner 'a genus of organisms is a species without close affinities or a group of mutually related species.' In other words, it too 'is a coherent or continuous group of organisms.' It is essential to its definition that some one of its species should constitute its type, to which the generic name should be inseparably attached. The large genera of earlier writers, subdivisions of their artificial orders, rather than groups of species, must become each associated around a special type before they can enter into modern conceptions of nomenclature.

The first essential in nomenclature is fixity. To establish permanence we must eliminate all elements of personal choice. The fixity of specific names through the law of priority is now fairly well established. Generic names are not yet similarly fixed. The method of changing the conception of an old genus from that of a mere subdivision of a higher group to that of a group of related species associated about a type species has not yet been well determined. In nomenclature, a genus must be fixed by its type, which is definite, not by its definition, which may be amended. Some writers have insisted that the first writer who subdivides a genus has the right and the duty to fix its type. Others maintain that the type must always be fixed by the process of elimination. In this process authors who eliminated unconsciously or in ignorance must be considered, as well as those who attempted to limit and define the generic parts in a group of family rank, called by its author a genus.

The method of elimination is now generally approved, but there is great variation in the application of it. Its great defect lies in the necessary uncertainty of its definition. Too often different assumptions or different points of view give different results. Any result may be vitiated by the discovery of some note or discussion—useless in itself, which may have been overlooked at the time of the first attempt at finding the type.

Inasmuch as the thought of type is inseparable in modern taxonomy from the idea of genus or species, it is most desirable to find some way of fixing the type of an author through the words of the author himself—not trusting to the mazes of subsequent delimitation and elimination.

The most convenient and most logical method of doing this, as well as the one most practically convenient, is to fix a group name to the first individual or the first species to which the name was tenably applied. If based on specimens, the species would rest with the individual actually in hand for description. If based on a series of previous records, the one of these standing first in the list of synonyms should be the type.

In the case of the genus, if no type, central species or 'chef de file' is indicated by

the author, the first species referred to the genus by the author or by any subsequent writer ought to be taken as the type. This would ensure fixity. It has no element of injustice. The genus should stand or fall on the first species mentioned.

As Mr. Cook observes: "The selection of the first species as the type would result in no complications by reason of the Linnæan arrangement of species, and it may be confidently expected that the uniform application of such a rule would necessitate far fewer changes than would the method of elimination whereby the doubtful or unidentifiable species are often the only residue on which time-honored names could be maintained."

The practicability of this rule must be tested by different taxonomists, each by its effects in his own field of work. In ichthyology it would bring an enormous gain in giving fixity of generic nomenclature which can be attained in no other way. The process of elimination has never been consistently followed, nor can the process be so defined that it can yield fixed results in the case of the complex genera of the last century. The practice of taking the first species named as the generic type has been adopted and continuously followed by the most voluminous writer on fishes, Dr. Pieter van Bleeker, and others have used it as a guide in cases of doubt.

The really strong and perhaps conclusive argument against it is derived from its effect on the genera of Linnæus. In general, Linnæus placed his central species or type in the midst of a genus, leaving the aberrant species at either end of the list. Cuvier followed the plan of giving a full description of a type species or 'chef de file,' letting the less known or less important species follow after it. It was not until about the beginning of the nineteenth century that the thought of a type species came to be associated with the genus.

Should we adopt the 'first species type' rule in relation to genera, the following changes would result from its application to the tenth edition of the Systema Naturæ.

Raja would be transferred to *Tetronarce* (Torpedo).
Squalus would remain with *Acanthias*.
Gadus would replace *Melanogrammus*.
Echeneis would replace *Remora*.
Cottus would replace *Agonus*.
Zeus would replace *Selene*.
Pleuronectes would replace *Achirus*.
Chætodon would replace *Zanclus*.
Labrus would replace *Sparisoma*.
Trigla would replace *Peristethus*.
Cobitis would replace *Anableps*.
Silurus would replace *Parasilurus*.
Esox would replace *Spyræna*.
Polynemus would replace *Pentanemus*.
Cyprinus would replace *Barbus*.
Ostracion would replace *Lactophrys*.
Tetraodon would replace *Spheroides*.
Diodon would replace *Chilomycterus*.
Syngnathus would replace *Typhle*.
Muræna, *Blennius*, *Gobius*, *Sparus*, *Sciæna*, *Perca*, *Gasterosteus*, *Salmo*, and *Clupea* would be unchanged.

These changes in time-honored names are apparently out of the question. In ichthyology the rule, if adopted, must pass by Linnæus to take effect with his successors or perhaps only among writers of this century influenced by the Cuvierian 'chef de file' method or by the modern conception of type.

The possibility of this suggestion is worth considering. It is stated on high authority, though I have not yet verified the quotation, that Linnæus somewhere says in effect that the real type of each genus recognized by him is 'the best known European or officinal species contained in it.' It would be relatively easy to determine the species worthy of this distinction. It would be easy to put ourselves in Linnæus' place in this regard. Then taking the Systema Naturæ as a starting point, it would be possible and just to hold each genus of each author, where no type is explicitly indicated, rigidly to the first species named under it. By this ruling it would be possible to avoid

certain very undesirable changes in Linnæan nomenclature, unavoidable under the rule of elimination. Among these are the following:

Esox for *Belone*.
Syngnathus for *Nerophis*.
Polynemus for *Pentanemus*.

Meanwhile the confused generic messes of Bloch, Lacépède, Swainson, Rafinesque and others, could be definitely crystallized and made to stand or fall on the generic distinction of the first species named.

The general adoption of such means of determining types would go a long way toward stability of nomenclature, and it is possible to use it in case we may be permitted to apply another method to the genera of Linnæus. If no exceptions can be properly made, then, for one, the writer would prefer its rigid application to all authors, Linnæus included, to the present state of confusion.

In any event, the suggestion of Mr. Cook merits serious consideration and reconsideration, for it has been several times rejected by zoologists.

DAVID STARR JORDAN.

ADDRESS OF THE PRESIDENT OF THE SECTION OF GEOLOGY OF THE BRITISH ASSOCIATION.

II.

OBSCURE CHAPTER IN THE EARTH'S HISTORY.

BEFORE discussing the validity of the argument on which this last result depends, let us consider how far it harmonizes with previous ones. It is consistent with Lord Kelvin's and Professor Darwin's, but how does it accord with Professor Joly's? Supposing we reduce his estimate to fifty-five millions; what was the earth doing during the interval between the period of fifty-five millions of years ago and that of only twenty-six and one-half millions of years ago, when, it is presumed, sedimentary rocks commenced to be formed? Hitherto we have been able to reason on probabilities; now we enter the dreary region of possibilities, and open that obscure chapter in the history of the earth previously hinted at. For there are many possible answers to this question. In the first place, the evidence of the stratified rocks may have been wrongly interpreted, and two or three times the amount of time we have demanded may have been consumed in their formation. This is a very obvious possibility, yet again our estimate concerning these rocks may be correct, but we may have erroneously omitted to take into account certain portions of the Archæan complex, which may represent primitive sedimentary rocks formed under exceptional conditions, and subsequently transformed under the influence of the internal heat of the earth. This, I think, would be Professor Bonney's view. Finally, Lord Kelvin has argued that the life of the sun as a luminous star is even more briefly limited than that of our oceans. In such a case, if our oceans were formed fifty-five millions of years ago, it is possible that after a short existence as almost boiling water they grew colder and colder, till they became covered with thick ice, and moved only in obedience to the tides. The earth, frozen and dark, except for the red glow of her volcanoes, waited the coming of the sun, and it was not till his growing splendor had banished the long night that the cheerful sound of running waters was heard again in our midst. Then the work of denudation and deposition seriously recommenced, not to cease till the life of the sun is spent. Thus the thickness of the stratified series may be a measure rather of the duration of sunlight than of the period which has elapsed since the first formation of the ocean. It may have been so—we cannot tell—but it may be fairly urged that we know less of the origin, history, and constitution of the sun than of the earth itself, and that, for aught we can