have, nevertheless, dealt with the subject with sufficient fullness, I hope, to convince you, if you were not already convinced, that the fundamental problems of pathology and embryology are alike, not only in being problems of cell life, but also in being similar and even identical problems of cell life. Widely as the two sciences differ, they rest on a common foundation.

To complete our subject it would be necessary to summarize our present knowledge as to the causes of cell differentiation. Physiological morphology is a new science; we have barely crossed its threshhold, and are not yet at home in it. To the physician this new science promises to far surpass in practical importance even the bacteriology of our time, since it is not presumptuous to hope that when we understand the physiological factors, thermal, chemical stimulant, mechanical and other, which bring about structure, which cause cytomorphosis, we can acquire control over cellular differentiation, and ultimately be able to prevent some of the most formidable diseases, over which we now have little or no power. The diseases which we may attack in the future in this way are diseases which may be designated as morphogenetic, because they are due to errors of morphological differentiation. At this vast topic it is impossible now to more than hint.

Here we may stop, not because all the great host of relations between embryology and pathology have been marshaled before us, but because enough of these relations have passed us in review to present a conclusive body of arguments. As we follow their march, we find ourselves led to the attack upon the problem of the causes of the specialization of cells, of histogenesis. To conquer this problem our only hope lies in the junction of all our forces.

Before closing, a personal word: first, of sincere thanks for the honor you have conferred upon me both by your invitation and by your attention, and then a word to express the great diffidence with which I have undertaken to deal with pathological phenomena. A man of science ranks according to the number of details which he has mastered, and his ability to drill them into coherent battalions. By no such system of ranking can I hope to be included among pathologists. I offer, therefore, only the thoughts of an outsider, derived from the long pursuit of a cognate science. Such external suggestions, being independent to some degree of pathological tradition, may contribute to vivify the conception of the unity of the biological phenomena and, therefore, of all forms of biological investigation. It will be a service rendered if my words recall the great truth that biology is not a congery of sciences, but a single science, which we artificially divide and subdivide until the parts are commensurate with our mental capacity. In the truest sense we are fellow-workers. Let us, therefore, work together.

CHARLES SEDGWICK MINOT.

THE DETERMINATION OF THE TYPE IN COMPOSITE GENERA OF ANIMALS AND PLANTS.

To the older naturalists a genus was a subdivision of an order containing a number of species, each standing in like relations to the genus. The genus was a pigeon-hole into which species of similar characters were thrust.

In the modern conception a genus is a group of related species, associated about a single one which is the type of the genus. In theory this type should be the central species or the most primitive one. In the exigencies of nomenclature, it is the one which was in point of fact first associated with the generic name. Modern writers recognize this grouping of species about the generic type, and to each new genus of most recent writers a type is definitely assigned by the author of the genus. In modern rules of nomenclature the definition of a genus may be altered or even reversed, but the generic name must adhere to the original type.

The most serious difficulty in connection with the matter of nomenclature lies in the reduction of the ancient conception of the genus to the terms of the modern one. It lies in the assignment of a type species to a group in which the original author had no conception of the need of such a species.

In the subdivision and fixation of the ancient genera, various methods have been followed, with varying results. In other words, these methods have lacked the one important element of inevitableness. A rule of nomenclature has little value unless it lies in the nature of things. If it is artificial, it will be discarded.

In general, three methods have been followed in fixing the types of the early composite genera:

1. To follow the arrangement of the author who first subdivides the genus subsequent to the work of the original author.

In this many difficulties have been found in practise. The first restriction is often in obscure publications. It is often obscurely done. In other words, a genus is often subdivided in such a way as to leave no clear idea as to what the author would leave in the original group. Sometimes he leaves nothing at all, as in the case of the Linnæan genus, *Sparus*, for which no place was left after its subdivision. As a matter of fact, this system leaves the proper application of many generic names in doubt, and necessitates a profitless investigation of the opinions of early authors who wished to improve Linnæan nomenclature, but who worked on too small a scale to accomplish much.

A second system derived from this is the method of elimination. The genus of the eighteenth century corresponds roughly to the family of the nineteenth. The family may contain several genera. These may be withdrawn from the original genus in chronological order, and the old name left with the final residue. But this residue will generally consist of foreign species or species unidentified or unidentifiable. To meet this difficulty the method of elimination in birds has been applied to European species only, that generic names based primarily on European forms may not be forced out of the European fauna. To make the system workable a variety of other minor rules must be invented, as a little change in the point of view as to some obscure author will make an entire change in the final result. The final result is the only matter of interest.

The ornithologists have found this scheme workable and it is incorporated in the rules of the American Ornithologists' Union. But even here it has not yielded stability of nomenclature, as several generic names (as of owls, loons) have been more than once altered in obedience to its dictates. But in American ornithology any rule has the great advantage of the imposition of authority. The ornithologists of America agree to stand by their committees, and any decision these may make is final for them and their associates, that is for most ornithological work in America for the present generation.

Other branches of science have no such authority behind their verdicts, and without it the determination of generic types by elimination is a failure. Often two men working independently cannot reach by the same rules an identical result. It is not always easy for the same man to reach the same result twice.

Let us take a concrete problem. The genus *Clupanodon* of Lacépède (1802) containing those herrings which have no teeth includes several modern genera.

It was based originally on six species, *thrissa*, *nasica*, *pilchardus*, *sinensis*, *africanus*, *jussieu*.

In 1810, Rafinesque proposed to substitute *Thrissa* for *Clupanodon*, presumably because the latter name is badly formed. Presumably again, *thrissa* would be the type of this genus of Rafinesque, who again presumably took it, as the first species mentioned, as the type of *Clupanodon*.

In 1820, Rafinesque founded the American genus, *Dorosoma* (Chatoëssus), and to this genus *nasicus*, and afterwards *thrissa* were referred; *pilchardus* was long left in *Clupea*, which is older than *Clupanodon*, but in 1860 a related species (*pseudohispanicus*) became the type of the genus *Sardinia* of Poey. *Africanus* has teeth and does not conform to the definition of *Clupanodon*. It was made, in 1839, the type of a genus *Platygaster*, Swainson, but this name is preoccupied. Afterwards *Ilisha* (Gray, 1846) and *Pellona* (Valenciennes, 1847) were based on a species of the same type, the former without definition. *Sinensis* and *jussieu* were placed, in 1847, in a genus *Clupeonia*, by Valenciennes. Finally in 1900, Jordan and Snyder established the genus *Konosirus* on a Japanese species (*punctatus*) which proves identical with *thrissa*, and to which group *nasicus* also belongs.

In their first consideration of this generic name, Jordan and Gilbert succeeded in convincing themselves that *Clupanodon* should take the place of *Clupeonia*. Eliminating *Pellona*, and the earlier names *Dorosoma* and *Clupea*, *Clupanodon* was left for the remaining species, *sinensis* and *jussieu*.

But in 1896, Jordan and Evermann recognized that if *Sardinia* were a distinct genus, the rule of elimination required them to transfer to it the name *Clupanodon*, as *Sardinia* is of later date than *Clupeonia*.

In 1900, Jordan and Snyder showed that *Dorosoma punctatus* was the type of a distinct genus, which they called *Konosirus*. Later it became evident that *thrissa* was identical with *punctatus* and by the law of elimination the name *Clupanodon* must supersede *Konosirus* as *thrissa* was the last of its species to be removed to a genus of its own. By this system the old generic name can never come to rest, but must be held in readiness to replace any new genus which may be formed from species included in its original content.

It was possible to defend in turn the use of *Clupanodon* in place of *Clupeonia*, *Sardinia*, and *Konosirus*. Should *nasica* ever receive a distinct generic name, *Clupanodon* must again move forward to replace it. On the other hand, writers called 'conservative' will reunite *Konosirus* with *Dorosoma* and *Sardinia* and *Clupeonia* with *Clupea*. In such case *Clupeonia* must fall back on *Ilisha*, a group originally included in *Clupanodon* by error. It is evident, that in this case no fixity is possible by the method of elimination, unless imposed by the temporary authority of some ichthyological union or mutual agreement among writers.

In default of such the present writer will use *Clupanodon* in place of his own genus, *Konosirus*, not on account of the results of elimination, but because the type of *Konosirus* is the first species named by Lacépède under his account of *Clupanodon*. If he should grow more 'conservative' he might reunite *Clupanodon* with *Dorosoma*. In such case he would call the whole genus, *Clupanodon*, because the name is prior to *Dorosoma*.

The third method of determination of generic type is through consideration of the work of the author of the genus in question, without regard to the views or work of any subsequent matter.

This we do in accepting as the type of a genus the species indicated as such by the author. Such a statement cannot be reversed by any later author. In recent days, the type of a genus usually is indicated once for all in so many words. With earlier writers who did not take this method we may be allowed to read between the lines. A leading ornithologist (Alfred Newton, if I am not mistaken), suggests that in the case of Linnæus we be allowed to ask the author what type he would have chosen if the modern problem were to be presented to him. As to this we should not be often left in doubt. If we are in doubt however, there is a very simple rule followed widely by naturalists, notably by Bleeker, the most voluminous writer on fishes. This is the selection, as type, of the first species named under the genus by its author, when other indications fail. This rule gives fixity, the sole essential thing. It gives justice. It saves a profitless overhauling of bibliography, and it is a clear way out of confusion. It is the only possible clear way.

I suggest for consideration the following provisional rules for the application of this method:

1. The type of a genus is the species designated as such by its author.

2. If no type is designated by the author, either explicitly or by clear implication, then the first species referred to the genus or the species standing first on the page, shall be considered as its type. A generic name should have no standing, if resting on definition alone, nor until associated with some definite species.

3. To this rule the following provisional exceptions may be made. The type of each genus of Linnæus as stated by him is 'the best known European or officinal species' it contains. In case of doubt in the application of this rule, the species standing first may have the benefit of the doubt. Unlike most subsequent authors, Linnæus usually placed his type species near the middle in the list of species. Cuvier made it his 'chef de file.'

4. In case of genera based on old specific names (*Belone*, *Achirus*, *Trachurus*) the species thus furnishing the name, if actually mentioned by the author of the genus, may be regarded as its type.

5. Possibly, to avoid confusion, it may be well to retain old generic names, restricted by common consent to a species not the first mentioned by the author, provided that such restriction antedates any modern names for the same genus. Thus it may be well to retain *Centropomus* for *Oxylabrax*, instead of *Lucioperca*, *Cheilodipterus* for *Paramia*, instead of *Pomatomus*, *Pomacanthus*, for *Pomacanthodes*, instead of *Zanclus*. But I doubt the wisdom of this exception, and I shall not be surprised to see future writers following Bleeker in the use of *Oxylabrax* and *Paramia*, leaving the generic names of Lacépède and of all writers since Linnæus, to the first species named by their author.

DAVID STARR JORDAN.

---

*NOTE ON THE NUMBER OF PARTICLES IN THE SATURATED PHOSPHORUS EMANATION.**

IN a series of experiments made by passing air ionized to saturation by phosphorus through a slender tubular condenser (60 cm. long, radii of air space, .30 cm. and .16 cm.), I showed that the electrical current radially through the condenser for a given potential difference, and the volume per minute of the ionized air sent longitudinally through it, were rigorously proportional quantities. At the same time the color of the steam tube observed on passing the air from the condenser into it, was invariable no matter whether the condenser was charged or not, *cœt. par.* Hence only an insignificant part of the particles producing condensation takes part in the electric current even with radial fields of 2,100 volts per cm., the highest safely admissible. I have estimated that less than 5 per cent. of

* Preceding experiments in SCIENCE, Feb. 9, 1900, the above note being a sequel. I there gave relative values for the absorption velocities, absolute values being given in the *Am. Journ. of Science*, March, 1900.