and he controls the statements of the book by his observations in the laboratory; and where the latter are incomplete they can be supplemented by reference to the former.

The continued usefulness of this little book is an object lesson in embryological pedagogy by which the writer tried to profit in writing a new "Development of the Chick" which should bring the subject matter up to date, and serve as an introduction to embryology. On the whole it seems improbable that the chick will be displaced as the favorite subject for laboratory practise in embryology, because the material is of universal occurrence and available at all seasons of the year without great expense. Moreover, the technique is as simple as that of any other form, at least after the egg is laid; and the knowledge of its development, while yet incomplete, is certainly more considerable than that of any other animal with the possible exception of man himself.

Professor Metcalf's objections to the use of the chick for introducing students to the subject of embryology[1] do not appear to me to be well grounded. He complains that the embryo chick is "highly specialized" and "distorted from the general vertebrate type," and that "the space relations of the organs are distorted by secondary influences." For these reasons he prefers the frog, and wishes that there were an embryology of this form. I echo this wish and hope that Professor Metcalf will undertake to write one. I am afraid, however, that the inconsiderate agriculturalists who domesticated the hen and taught her to lay the year around have conferred on her an unfair advantage; and it appears to me better for the elementary student to study living hens' eggs than preserved frogs' eggs. This is indeed the main advantage that I see on the side of the chick. But I believe that the objections on account of "specialization" and "distortion" are more deeply rooted in tradition than in nature.

But whether the student uses the frog or the chick, or some other form, he needs a fairly complete and modern book of reference of the same form, if not to replace, at least to supplement, the comparative text-books. In this contention I think Professor Metcalf will agree. We need above all objectivity in the teaching of embryology; we must require some basis of exact facts to support generalizations, and keep the distinction between the two clear in the student's mind.

[1] Science, N. S., XXIX., May 7, 1909, pp. 738–739.

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GENERA WITHOUT SPECIES

In Science for February 26, 1909 (pp. 339, 340), Professor Cockerell discussed the "controversy" concerning "genera without species," pointing out the difficulty of dealing with such cases, since they are not distinctly provided for in the International Code of Zoological Nomenclature. Apparently each case should be dealt with solely on its merits. A few illustrations may help to make this point clear. In 1799 Lacépède proposed the genus *Picoides,* giving a short diagnosis of it, but omitting to refer to it any species. The diagnosis clearly indicates a woodpecker having only three toes. The only species of woodpecker at that time known with only three toes was the three-toed woodpecker of northern Europe, *Picus tridactylus* Linn. This species being clearly the basis of the diagnosis, it may be taken as the type of the genus *Picoides,* now, for a long time, current for the group containing this and other closely allied species.

In the same way, and at the same date, Lacépède proposed the name *Astur* for a genus of short-winged, long-legged hawks, giving for the genus a wholly inadequate diagnosis, without mentioning under it any species. In 1806 Froriep published a German translation ("Analytische Zoologie") of Duméril's "Zoologie analytique," adding to it, *passim,* much new matter, including the mention of examples under Duméril's genera, which, for the most part, were originally proposed by earlier authors, without, of course, the designation of types. As an example of *Astur* Froriep gave the species *Falco palumbarius* Linn., which thus may be taken as the type of *Astur.* But the genus should date

from Lacépède, 1799, and the type from Froriep, 1806. The genus has been current for more than a century, during which period the same species has been repeatedly and independently designated as its type by various subsequent authors.

Forster in 1788 published a work ("Enchrid. Hist. Nat.") in which he gave diagnoses of the genera of birds (and other animals) then known to him, but without referring to them any species. Some of them were for the first time characterized and named, among them the genus *Gavia*. His diagnosis, with the context, shows unequivocally that *Gavia* was proposed for the loons, a group comprising, as now known, some half-dozen species, all strictly congeneric, and so different from all other birds as to constitute a distinct superfamily. *Gavia* was, furthermore, the first generic name proposed for the group. It only remained for some one later to select some one of the loon species as its type.

Muscivora was proposed by Lacépède in 1799, for a genus of tyrant flycatchers, but he referred to it no species. It was not satisfactorily determinable till a species was referred to it by G. Fischer in 1813.

Cuvier in 1800 published ("Leç. d'Anat. comp.," tab. ii.) a considerable number of genera now currently accepted from this source, without giving either diagnoses or other basis for them beyond citing their equivalent vernacular French names, which names are, however, identifiable from a slightly earlier work ("Tabl. élém. de l'Hist. nat.," 1798) of the same author where these vernacular names are coupled with their proper technical designations. In a few cases his generic names are not thus identifiable, and are hence to be ignored.

These examples, selected from many that are available,[1] seem to show clearly that "genera without species" should be dealt with according to their individual merits. They seem also to fully answer Professor Cockerell's question, "Who can define a genus except as including species?"

It may be noted further that while this question is not considered in the International Code, it is fully discussed and provided for in the A. O. U. Code, where a diagnosis is recognized as a valid basis for a generic name, with the provision, however, that "a name resting solely on an inadequate diagnosis is to be rejected, on the ground that it is indeterminable and therefore not properly defined." This ruling is based on general usage for nearly a century, as well as on common sense; to reject it would result in the overthrow of many generic names that have been current in vertebrate zoology for almost a century. It may be added that while the A. O. U. Code of Nomenclature and the International Code of Nomenclature are in perfect accord in respect to principles and spirit, the A. O. U. Code is much fuller and more explicit than the International, taking up in detail a large number of questions not included in the latter. This may well be the case, inasmuch as the A. O. U. Code is a document of some fifty pages while the International Code is comprised in a dozen pages.

Postscript.—Since the above was sent to SCIENCE, Professor Cockerell has returned to the subject of "Genera without Species,"[2] giving abstracts of replies received by him from a number of correspondents in response to a suggestion to that effect made in his former communication.[3] These replies are not only interesting, but possess some importance as showing the opinions on this question of a number of entomologists and botanists. The twelve gentlemen here represented seem to pretty unanimously agree with Professor Cockerell that (to quote from one of them) "generic names published without any mention of included species are to be regarded as invalid"; or, as otherwise stated, "are nomina nuda." This remarkable unanimity seems to me to be due either to limited experience in this difficult field, or to a lack of knowledge of

[1] For example, Lacépède, in his "Tableau . . . des Oiseaux" (1799), recognized 130 genera of birds, of which 19 were here first proposed, all solely on the basis of diagnoses; of these 11, or more than one half, are now, and always have been, in universal use; the others were homonyms or preoccupied names.

[2] SCIENCE, May 21, 1909, pp. 813, 814.

[3] SCIENCE, February 26, 1909, p. 340.

the history of nomenclature; in other words, as off-hand opinions as to what seemingly ought to be, regardless of the actualities of the case.

Nomenclature (both zoological and botanical) has attained its present stage of comparative orderliness by slow stages of development. For the first seventy years of its history such a concept as a "genotype" appears to have been rarely, if ever, thought of; and it was not until the first quarter of the nineteenth century had passed that types of genera began to be considered as a necessary part of the proper basis of a genus. Prior to 1810 hundreds of genera now in current use were proposed solely on the basis of a diagnosis; although they were accepted and have been in use from the date of their proposal, many of them were without designated types for half a century. Yet the authors of this early period were in substantial agreement as to what groups of species these generic names were intended to include. From the modern viewpoint these genera were (usually) heterogeneous groups, each comprising several modern genera. In the process of division a type was sooner or later, by restriction or by actual designation, assigned to the original genus. Not till then did the genus, from the modern viewpoint, become properly established. Many other genera of this early period, similarly proposed, are unidentifiable. I can not agree that these two categories should have the same treatment. Nor can I agree that a long-accepted genus must date from the author who, long after it was originally founded, "validated" it by designating a type for it; but rather, as indicated in the first part of this communication, that the genus should date from its founder. Otherwise nearly all of the early genera for birds would date from about 1840, after many of them had been in general use for one half to three fourths of a century. In the case of mammals, many of the early genera were not thus "validated" till many years later than those of birds. To take genera from the date of "validation" would obviously establish a new source of trouble in relation to priority of names.

It is now the custom of a large number of nomenclators to make a distinction between a nomen nudum and a name that is for any reason unidentifiable;[4] the former can be employed by a later author, from whom it must date; the latter can not be again used, the attempt at a diagnosis, however brief or inadequate, precluding its subsequent employment. Hence a name founded on a diagnosis, and subsequently validated, can not be taken from the validating author, but must date from the founder, if this rule be followed. Furthermore, to call a genus a nomen nudum when based on a diagnosis is a misuse of language, and entirely contrary to usage.

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THE ORIGIN OF THE MOON

In his inaugural lecture delivered in Columbia University, November 3, 1908,[1] Dr. Albrecht F. K. Penck, the Kaiser Wilhelm "Umtausch" Professor, spoke in part as follows concerning the geographical and geological similarities between the eastern coast of North America and the western coast of Europe:

These similarities between Europe and peninsular North America are not merely superficial ones. *In a very remarkable way, these two sides of the Atlantic repeat the same structural features;* there is an astonishing symmetry, as Eduard Suess has shown so clearly. The northeast of Canada and Labrador on one side, and Scandinavia with Finland, the region of Feno-Scandia, on the other, are both composed of the oldest rocks we know of. These have a very complicated structure, being intruded with many eruptive rocks, and in a secondary way only, the surface features of the above regions are dependent on their structure. Both regions had already been leveled down before Cambrian times, and they sink gently down under a cover of horizontal Paleozoic strata. Both were called by Suess shields. The resemblance between these shields is the more conspicuous because both were covered during the last ice age by a glaciation which molded their surface in a similar way. In Sweden and Finland we find the same rounded

[4] See Revised A. O. U. Code, Canon XXXIV., and the explanatory "remarks."

[1] For the whole lecture see Science, February 26, 1909.