THE LENGTH OF THE SMALLEST KNOWN SIRENIAN FETUS; GYRE PREFERRED TO "CONVOLUTION"

TO THE EDITOR OF SCIENCE: In the October issue of the *American Naturalist,* under "Scientific Exhibits at the Seventh International Zoological Congress," on p. 633, the fetal manatee shown by me is said to be "approximately an inch and a half long." Since this is supposed to be the smallest known Sirenian fetus it is proper to state that from the root of the flexed tail to the top of the flexed head, allowing two mm. for the depression caused by the removal of the brain, the present length is 53 mm., say two and one-eighth inches. It has shrunk about two mm. since the measurements recorded in the *American Journal of Science and Arts,* August, 1875, where also various aspects are represented upon Plate VIII. In accordance with the criterion applied to the developing human being by Minot ("Embryology," p. 391) this specimen should be called *fetus* rather than *embryo,* because the tail is unmistakably that of a manatee notwithstanding the marked flexures of both ends of the body and the resemblance of the head to that of a horse or other ungulate.

On p. 662 it is said that I "exhibited photographs of human cerebral convolutions." Do not think me over-particular in disclaiming responsibility for the last word. I would no more use "convolution" for *gyre* than "conflagration" for *fire.* The choice was made deliberately in 1881, and published on page 133 of the issue for March 26 of SCIENCE, (the original periodical of that name, of which only three volumes were published). It has been declared upon several subsequent occasions. It constitutes one of many cases of identity between my neural terms and the B.N.A., although the framers of the latter had not the grace to acknowledge the priority of nearly the fourth of a century.

BURT G. WILDER

ITHACA, N. Y.,
April 7, 1908

AMETHYSTINE GLASS

IN connection with the interesting information given in SCIENCE, February 7, p. 239, it may be worth while to call attention to the fact—first pointed out to me by Mr. Irish, of the normal school at Tempe, Arizona—that ordinary bottle glass, when left exposed on the ground out-of-doors in the arid southwest, assumes a strong purple or amethystine color. This I have observed in Arizona, New Mexico and Colorado. Dr. Shedd, when professor of physics at Colorado College, examined some of this glass which I had collected at Colorado Springs, and found that the color was discharged by heat. Many years ago, Faraday made some experiments with glass, noticing a similar change of color; but I have not seen any recent literature upon the subject.

T. D. A. COCKERELL

UNIVERSITY OF COLORADO

SPECIAL ARTICLES

SOME NOTES ON MALACOLOGICAL NOMENCLATURE

Two years ago I published a small paper on this matter.[1] On some points my eminent and most competent friend, Dr. W. H. Dall, does not agree with me, and I hope that these lines may contribute to concert between us. This would be the more desirable, as Dr. Dall without any doubt is actually the first among all the living malacologists whose knowledge extends to actual molluscan faunas as well as to extinct ones. Also the divergence of Dall and myself is not one of principle, but only caused by a different interpretation of the international rules.

Dr. Dall during the last years has made many efforts toward establishing names of ancient authors, more or less forgotten. The question in this case can not be that of the more or less convenience in accepting such names, but whether it is necessary to have no other rules for our manner of proceeding, than consequence and logic. Therefore we accept the most disturbing changes, but only in the case of true necessity, and this is not the case at all with such authors as Martyn, Humphrey, Link, Bolten, Mörch and others.

The international rules say in article 25:

The valid name of a genus or species can be only that name under which it was first desig-

[1] *Nachrichtsbl. der Deutsch. Malacozool. Ges.,* 1906, pp. 1–12.

nated in the condition: (*a*) that this name was published and accompanied by an indication, or a definition, or a description; and (*b*) that the author has applied the principles of binary nomenclature.

This rule confirms what has been accepted by zoologists a long time ago, that is to say, that *nomina nuda* shall not be regarded as established. Concerning the genera, all names given in catalogues without any definition are worthless. I am of the opinion that, also, species figured without the addition of any description can not be accepted. With respect to the generic names, Dollfus[2] is of the opinion that the worthless names rest without value up to the date that they are accepted in a valid publication of another author. This may be correct, but the examples given by Dollfus for the greater part must be refuted. He says that the names of Bolten and Link first acquired good standing in the catalogue of the Yoldi collection published by Mörch in 1852. This catalogue, however, having no definitions, is as completely worthless as the two others just mentioned.

All the generic names of this catalogue, and even some new ones, are given without diagnosis or any definition. Add to this that this catalogue is not at all a scientific publication, but the enumeration of a collection of shells which were to be sold in public auction in 1852. The catalogues also of Humphrey and Bolten are nothing but dealers' auction lists. It is not from such dim sources that science gets its fundamental elements. Quite in the same way, we might consider newspapers scientific publications.

The catalogue of the collection of Rostock studied by Link has never been published. The entire edition has been destroyed by fire and some proof-impressions have had the undeserved destiny of getting to be the object of scientific communications. The catalogue has never been reprinted, and even if it had been published, we could not attribute to it more value than to the similar elaborations of Bolten and Mörch. This opinion of the auction catalogue of the Museum Boltenianum has been expressed long since by the publications of P. Fischer,[3] W. Keferstein[4] and C. Semper.[5]

Dr. Dall has furnished no new data which could justify the rehabilitation of these rejected names. During my recent journey to Europe I have ascertained that my colleagues in this respect retain this opinion.

Another author who has been favored with the rehabilitation of his names by W. H. Dall is Martyn. As early as the year 1861 E. von Martens[6] asserted that the "Universal Conchologist" of Martyn has no authority, because the author is not strictly binomial. And I can find no reason to agree with Dr. Dall in thinking otherwise, being convinced that his arguments are unsuccessful. In this respect I have myself had an important experience. Working in ornithology, I have been impressed like almost all American ornithologists with the admirable publications of Brisson and I have endeavored to rehabilitate his names, but finally I have convinced myself that my endeavors must remain without success. Actually, I am completely sure that the international rules are perfectly in the right in driving from our nomenclature all the authors who are not strictly binomial. I regret this much in the case of Brisson but not in that of Martyn.

In my opinion, we not only have the right, but even the obligation to reject all the generic and specific names which are not formed exactly after the rules contained in article 25, and I therefore wish that Dr. Dall would give up his isolated position and follow the example of our other colleagues. I believe that he can do this so much the more easily, since nobody has contributed more to elucidate the systematic nomenclature than himself. Of all his works, nothing will be lost in this case, as these publications have been elaborated in such a conscientious manner that we need only eliminate the names of Bolten, Link, etc., and

[2] *Rev. Crit. Palaeozoöl. de Cossmann*, IX., 1905, p. 55.

[3] *Journ. de Conchyl.*, 1858, p. 206 ff.

[4] *Malacozool. Blatter*, X., 1862, p. 164 ff.

[5] *Verhand. d. Vereins f. natur. Unterhaltung, Hamburg*, V., 1875, p. 121.

[6] *Malacozool. Blatter*, VII., 1861, p. 141 ff.

take the next name proposed by a binomial author which is accompanied by a definition.

HERMANN VON IHERING

SÃO PAULO,
March 7, 1908

WITH the feelings expressed in the letter of my friend, Dr. von Ihering, I have much sympathy, as a quarter of a century ago I myself experienced similar feelings, expressed similar opinions and accepted as exact similar statements by older authors in regard to the publications of Link and some other authors, which greater knowledge and more experience have led me to regard as more or less erroneous.

To be brief, Dr. von Ihering asks us to reject all names unaccompanied by a diagnosis or definition.

But this is not the requirement of the international code of nomenclature. Excluding "diagnosis" and "definition" as universally accepted, the code also admits names "accompanied by an indication." Now, if an indication is not a diagnosis or a definition, what is it? Plainly, a recognizable figure or reference to another work where a figure, or definition, or both, will be found. I believe I am historically accurate in saying that the word "indication" was intentionally inserted in the code to cover exactly such cases. If it were otherwise it is clearly a superfluity in the paragraph of the code just referred to.

It can not be denied that the possession of an exquisitely accurate drawing of an animal, like those of Martyn, is calculated to give a far better knowledge of what the author had in mind than the three or four lines of dog-Latin, without a figure, often used by the ancient binomial authors.

And, if an author refers to page, plate and figure of a standard iconography, for the illustration and diagnosis of the animal he has in mind, in what respect is the diagnosis less effective than if it had been reprinted?

I am, therefore, confident that my friend's major premise is not well taken, but that the code expressly provides for such cases and properly accepts such indications.

We must remember that authors, from Linnæus to Lamarck, and even later, did not feel themselves obliged to accept the prior "nomen triviale" given by another author, but altered and changed to suit their own notions. Thus Gmelin, Lamarck and others borrowed Martyn's names, sometimes retaining his specific name, sometimes adopting his generic name, but rarely giving full credit. The binomiality of Martyn was not questioned by Gmelin or Lamarck, or other contemporaneous writers. I have shown that his names are quite as binomial as those of Linnæus. And von Martens, cited by Dr. von Ihering as rejecting Martyn, proposed to accept his *specific* names, while rejecting his *generic* names, because the latter conflicted with those which were more familiar. Martens himself was not above altering a prior generic name because it did not agree with his assumptions, and, though a most able and learned man, is not to be regarded as an authority on matters of nomenclature.

When a scientific man prepares an original systematic catalogue of a collection in his custody, is the scientific character lost because when that collection comes to be sold, this catalogue is printed for the use of buyers? I do not see why. An "auctioneer's catalogue" implies a catalogue made by an auctioneer and not by a scientific man. But Bolten, Hwass, Link and Mörch were scientific men, or even professors of zoology. Why then reject their work? I think each case must stand on its own merits.

In the case of Link, I formerly believed the tradition that the whole edition of the work had been burned, but I now think this tradition erroneous, on account of the number of copies of Link which have turned up. We have two in the National Museum library, and references to his system, which was taught to his students in the University of Rostock, are not very infrequent in the publications of his contemporaries. At any rate, Link, whose work was published as a university program, gives diagnoses and references to iconographies; so in that respect he is qualified to meet Dr. von Ihering's standard.

I have always been of the opinion that consistency required the rejection of Brisson's

names except when they had been validated by a binomial author.

I have rejected the names in the catalogue called the Museum Calonnianum, because it is anonymous, and because it was not issued by any named publisher, though we know it to have been based on a manuscript of Hwass and distributed by George Humphrey.

I have not found that accepting the international code in its full meaning and intention has isolated me from the majority of active workers in the line of my specialty. On the contrary, nearly all those who have had much experience with nomenclatorial matters have, like myself, with some reluctance, arrived at the conclusion that half measures will not do, that a thorough revision is the only way to arrive at permanency, and that each of us must do his share toward this desired result.

Wm. H. Dall

U. S. National Museum

P. S.—In order to make sure that my understanding of the rules is correct, I have applied to the secretary of the International Committee on Nomenclature, whose reply is appended.

Dr. Wm. H. Dall,
Smithsonian Institution,
Washington, D. C.

My dear Dr. Dall:

Referring to your letter relative to the points raised by Dr. von Ihering in regard to Article 25 of the International Code, I would invite your attention to a recent opinion rendered by the Commission, namely:

"*The meaning of the word* 'indication' *in Art.* 25a.—The word '*indication*' in Art. 25a is to be construed as follows:

"(A) with regard to *specific* names, an '*indication*' is (1) a bibliographic reference, or (2) a published figure (illustration), or (3) a definite citation of an earlier name for which a new name is proposed.

"(B) with regard to *generic* names, (1) a bibliographic reference, or (2) a definite citation of an earlier name for which a new name is proposed, or (3) the citation or designation of a type species.

"In no case is the word '*indication*' to be construed as including museum labels, museum specimens or vernacular names."

This opinion seems to cover the point of principle under discussion. It does not of course cover the point whether the particular authors mentioned by Doctor von Ihering are to be interpreted as coming under the principle in question. This latter point is dependent upon evidence, but I do not understand that you have asked me to express an opinion on the evidence.

Very truly yours,

C. W. Stiles,
Secretary, International Commission on Zoological Nomenclature

Washington, April 27, 1908

A NOTE ON THE ECOLOGICAL FORMATIONS OF PITTSBURG AND VICINITY

It has occurred to the writer, after reading a recent article in Science,[1] that a generalized classification of the vegetation of Pittsburg and vicinity, as presented recently before the Biological Section of the Pittsburg Academy of Science and Art, might be of interest to many botanists, especially teachers, who may be situated in regions whose land forms are similar to that of Pittsburg or whose vegetation may be classified in a similar manner.

Pittsburg is situated in a region whose soils are quite uniform so far as their chemical composition is concerned, being mainly derived from sandstones and shales, with a few thin strata of limestone, and it takes but little field work to convince one that the all-important factor in the formational structure of the vegetation is to be found in the different conditions of ecological habitats as brought about by physiographic processes. For this region, physiography, in the narrow sense of the term,[2] furnishes an efficient basis for an ecological classification of the vegetation; the details, but not the principles, of classification differing from those of "physiographic ecologists" elsewhere.

The main features of land form which need

[1] Ramaley, Francis, "Plant Zones in the Rocky Mountains of Colorado," Science, N. S., 26:642–643, November 8, 1907.

[2] Geomorphology—that part of physical geography, devoted to the form of the land—coordinate with oceanography and meteorology. See Davis, W. M., "Current Notes on Land Forms," Science, N. S., 25:70–71, January 11, 1907.