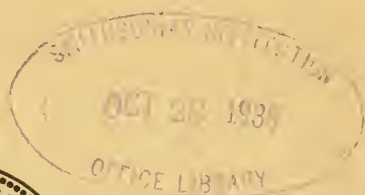


SMITHSONIAN MISCELLANEOUS COLLECTIONS

VOLUME 73, NUMBER 8

OPINIONS RENDERED BY THE INTERNATIONAL
COMMISSION ON ZOOLOGICAL
NOMENCLATURE

OPINIONS 124 TO 133



(PUBLICATION 3395)

CITY OF WASHINGTON
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OPINION 124

LINNAEUS, 1758, SUBDIVISIONS OF GENERA

SUMMARY.—The various Subdivisions of genera published by Linnaeus in 1758 are not to be accepted as of this date (1758) as of subgeneric value under the International Rules.

STATEMENT OF CASE.—Several zoologists have requested the Commission to make a definite ruling in regard to the status of the subdivision of genera found in Linnaeus, 1758*a*. One case is before the Commission at present (*Bulla*) which makes a ruling on this point very desirable and at least one other case is likely to be submitted to the Commission in the very near future.

DISCUSSION.—Considerable difference of opinion exists among zoologists as to the status of the subdivisions of genera used by Linnaeus, 1758*a*.

On account of the situation presented, the Commission has made a page by page study of the tenth edition of the "Systema Naturae" and has tabulated the subdivisions into various categories. A result of this tabulation shows conclusively that it is impossible to look upon all these subdivisions as definitely named subgenera, and if one attempts to grant subgeneric nomenclatorial value to certain of these categories and to deny it to others it is found to be exceedingly difficult, in fact impossible, to present a plan which is free from objection.

The subject was laid before the Commission in Circular Letter No. 137, series 1928, and this Circular Letter with the text of the tenth edition was studied by the Commission during its meeting in Padua in August and September 1930.

As a result of this study the Commission adopted the following paragraph in its Minutes for August 30, 1930:

After a discussion of the so-called subgenera in Linnaeus, 1758*a*, the Secretary was instructed to prepare an Opinion to the effect that these are not sub-

genera, but if any group of specialists finds that because of the literature on said group this Opinion will produce greater confusion than uniformity, the Commission is prepared to take up individual cases under arguments which may be submitted.

Pursuant to these instructions, the Secretary presented the draft of this Opinion for formal vote.

The adoption of this Opinion automatically settles the case of *Bulla* now before the Commission, i. e., the alleged subgenus *Bulla* Linn., 1758, insect, is not a subgenus under this Opinion and therefore does not affect in any way the standing of *Bulla* Linn., 1758a, mollusk.

Even in absence of this Opinion the case of *Bulla* would be settled under the following amendment to Article 36 (on homonyms) adopted at Padua, 1930:

When homonyms are of the same date, whether by the same or by different authors, then any name proposed for a genus takes precedence over a name [its homonym] proposed for a subgenus. The same principle is applicable to homonyms of species and subspecies of identical date.

The Secretary has the honor to recommend that the Summary as given above be accepted as the Opinion of the Commission.

Opinion prepared by Stiles.

Opinion concurred in by thirteen (13) Commissioners: Apstein, Bather, Chapman, Cabrera, Pellegrin, Horvath, Ishikawa, Jordan (K.), Stephenson, Silvestri, Stejneger, Stiles, Stone.

Opinion dissented from by no Commissioner.

Not voting, five (5) Commissioners: Bolivar, Handlirsch, Jordan (D. S.), Richter, Warren.

OPINION 125

Borus HERBST, 1797, AND *Borus* AGASSIZ, 1846, VS. *Borus* ALBERS, 1850

SUMMARY.—*Borus* Agassiz, 1846, is an emendation of, and therefore an absolute synonym of, *Borus* Herbst, 1797; *Borus* Albers, 1850, is a dead homonym.

STATEMENT OF CASE.—Dr. H. A. Pilsbry, of the Academy of Natural Sciences of Philadelphia, submits the following case for Opinion:

In *Archiv für Naturgeschichte*, Jahrg. 92 (for 1926), Abth. A, 8 Heft, July 1928, p. 66, E. Strand proposes to reject the name "*Borus* Albers, 1850", on account of *Borus* L. Agassiz, *Nomencl. Zool.*, 1846, in Coleoptera, and to replace it by *Corus* Jousseume, 1877.

Borus was suggested by Agassiz (*Nom. Zool. Index Univ.*, p. 49) as an emendation of *Borus* Herbst, 1797. Under present conditions the names *Borus* and *Borus* would be considered sufficiently different (Opinion 25 of the International Commission). In my opinion the original spelling of each name is all that need be considered; subsequent variants or emendations having no status in nomenclature. According to this view *Borus* Albers will stand.

"*Corus* (*Bulimus*) *valenciennensi*" (sic) was mentioned with other snails by Jousseume (*Bull. Soc. Zool. France*, vol. 2, p. 311, 1877), but without any intimation that the name was new. In the same paragraph and elsewhere in the same communication, new names proposed are so designated, and moreover are printed in heavy face type. It is clear, therefore, that "*Corus*" was a pen error or printer's error for *Borus*. Such an error seems the more likely as there are two mistakes in the name "*valenciennesi*" (a well-known species of *Borus*) in the same line. I do not think that such an evident error is available as basis for a new name.

Megalobulimus K. Miller, *Malak. Blätter*, vol. 25, p. 172, 1878, for *Borus* *garcia-moreni* Miller (= *B. popclairianus* var. *thammianus* v. Martens) is available for the *Borus* group in case *Borus* is rejected.

DISCUSSION.—This case was studied independently by Commissioner Bather, by the Secretary, and by Dr. Paul Bartsch of the United States National Museum. The opinions prepared by all three are in agreement. The opinion as worded by Commissioner Bather reads as follows:

By Art. 19, the name *Borus* Herbst should be preserved unless an error of transcription, a *lapsus calami*, or a typographical error is evident. Since the name is obviously the Greek *βόρος* none of these is evident.

But by Art. 8, Recommendation *a* and Appendix *f*, Herbst "should" have written *Borus*. Since this recommendation is based on the previous usage of both classical scholars and the early systematists (who were for the most part scholars), Agassiz was within his rights in emending to *Borus*.

If his right be disputed, then, since there is no possible question of an error of transcription, etc., *Borus* Agassiz is a synonym of *Boros* Herbst.

Borus Albers, it can hardly be doubted, is also a transliteration of *Boros*. If a correct name, it is a homonym of *Borus* Ag. If incorrect, it should be written *Boros* and so becomes a homonym of *Boros* Herbst. Art. 36, Recommendation, does not apply to this case.

Therefore according to strict application of Art. 34, and Opinion 83, *Borus* Albers is to be rejected.

The Secretary recommends that the Summary, as given above, be adopted as the Opinion of the Commission.

Opinion prepared by Bather, Stiles, and Bartsch.

Opinion concurred in by fourteen (14) Commissioners: Apstein, Bather, Cabrera, Chapman, Horvath, Ishikawa, Jordan (K.), Pellegrin, Richter, Silvestri, Stejneger, Stephenson, Stiles, Stone.

Opinion dissented from by no Commissioner.

Not voting, four (4) Commissioners: Bolivar, Handlirsch, Jordan (D. S.), Warren.

Commissioner Richter adds:

Ich stimme der Opinion zu.

Zur Discussion, Absatz 3, habe ich aber grundsätzlich zu bemerken: *Nichtbefolgung eines Ratschlags bei der Aufstellung eines Namens gibt kein Recht, den Namen nachträglich im Sinne dieses Ratschlags zu ändern.* Herbst, 1797, "should have written *Borus*"; wenn er aber *Boros* geschrieben hat, so hat Agassiz, 1846, nicht das Recht, *Boros* in *Borus* zu ändern.

OPINION 126

NEW NAMES IN D'ORBIGNY'S, 1850, "PRODROME" ARE NOMENCLATORIALLY AVAILABLE

SUMMARY.—On basis of evidence and expert advice of outstanding specialists, the Commission does not see its way clear to declare the new names in d'Orbigny's, 1850, "Prodrome" as unavailable or as *nomina nuda* under the Rules.

PRESENTATION OF CASE.—The following case has been submitted by L. R. Cox and W. J. Arkell:

That the new specific names published by A. d'Orbigny in his "Prodrome de Paléontologie Stratigraphique Universelle" (3 vols., Paris, 1850) shall be considered as *nomina nuda* and shall have no status in nomenclature, unless they are accompanied by a reference to a figure or description published by some previous author.

As specialists in the Mesozoic Mollusca, we are of the opinion that the suppression of these names is desirable in order to avoid numerous changes in current nomenclature, while few, if any, changes would result from such suppression. The "Prodrome" purported to be a complete synopsis of the fossil Invertebrata known to the author at the time of its compilation (1847). Besides listing all species which had been described prior to that date, and providing new specific names in cases of preoccupation, etc., it includes a great number of new names given to previously undescribed species; most of these came from French localities and were represented in the author's own collection. In each case the horizon and localities are given, and a brief comment is made on the species, but this rarely occupies more than two lines and is quite inadequate as a specific diagnosis. Examples: "*Teredo antiquatus* d'Orb., 1847. Espèce à tubes très-longs. France, Thouars (Deux-Sèvres)" (vol. 1, p. 251); "*Lucina sarthacensis* d'Orb., 1847. Espèce très-comprimée, presque circulaire. France, Pizieux, Chaumont" (vol. 1, p. 339).

If these names are discarded as *nomina nuda*, as here suggested, d'Orbigny's species will only be valid as from the date of their earliest description by a later author. Example: *Astarte socialis* d'Orbigny (vol. 2, p. 60) will date from its description by De Loriol in 1867 (Mém. Soc. Phys. Genève, vol. 19, p. 60), and will be referred to as "*Astarte socialis* de Loriol ex d'Orbigny." In most cases the first descriptions of d'Orbigny's species are in a work by M. Boule and others now appearing in installments in the "Annales de Paléontologie", and figuring the supposed types. In a few cases d'Orbigny's species have been guessed at and misinterpreted by later authors; such misinterpretations, if accompanied by proper descriptions, will be accepted as having the status of original descriptions. In most cases later workers have necessarily ignored d'Orbigny's species, and many of them have been described under other names, which are now familiar in the literature. Names proposed by d'Orbigny as substitute-names, etc., will of course remain valid, since they are accompanied by references to descriptions in previous literature. D'Orbigny's new genera will not be valid if the only

species referred to them are those suppressed as *nomina nuda* (e. g., *Sowerbya* d'Orbigny, vol. 1, p. 362, will be rejected in favor of *Isodonta* Buvignier, 1851, in accordance with current practice); in most cases the new genera include previously described species, and genotypes will be available.

DISCUSSION.—The decision on this case is obviously one of far-reaching importance, and is likely to be cited more or less frequently by various authors in reaching decisions on similar cases. It seemed wise, therefore, to obtain expressions of opinion from a number of specialists in different parts of the world before preparing a formal opinion to be submitted to the Commission for vote. In response to invitations to specialists to discuss the case, the following replies have been received.

L. R. Cox states :

In submitting the question of d'Orbigny's "Prodrome" names to the International Commission, our primary object was to obtain a *definite ruling* upon a matter in which uncertainty has always existed, the majority of authors having deliberately rejected these names as being accompanied by absolutely inadequate descriptions. It seemed to us that it would be unreasonable to revive his names, with the resulting disappearance of familiar ones, without obtaining some opinion on the matter, and our recommendation was made in the hope that it might be possible to avoid such changes.

The main objections to our recommendation are :

1. It would be a dangerous precedent to create, since the validity of several early authors might similarly be questioned. Also, a description which now appears inadequate may have been quite sufficient at a time when fewer species were known.—D'Orbigny, however, writing so late as 1847, cannot be classed with authors half a century and more before him. Descriptive terminology was very well advanced by his time, and in his other works he gives good descriptions and figures, showing that his "Prodrome" descriptions were not intended very seriously.

2. The "Prodrome" is a work of great merit, and Professor Boule protests against a proposal to set it aside so lightly.—The value of this work for the purpose for which it was compiled is not questioned, but in the Introduction (p. lvi) d'Orbigny says: "En publiant notre "Prodrome de Paléontologie Stratigraphique" nous n'avons pas eu en vue de décrire des espèces." The new names were probably merely introduced in the same way as *nomina nuda* often get published in lists prior to description of the species, and it is quite certain that d'Orbigny intended to publish proper descriptions in the "Paléontologie Française", later on.

3. Even if his descriptions are valueless, his types have always been accessible in Paris.—The idea that the publication of a description is an unimportant formality, the preservation of a type specimen being the chief thing, seems to be current in some quarters, but fortunately not among paleontologists in general. We might just as well accept *nomina nuda*, where a type specimen is extant.

I realize that this is an important test case and it may prove discreet for the Commission to rule once and for all that no specific name published, even with only a single word of comment is to be rejected on the grounds of inadequate description.

In a letter to Dr. Bather, W. J. Arkell discusses the case as follows :

Cox's letter to you on the subject of d'Orbigny seems to me to be rather too unconcerned. To say "that our primary object was to obtain a definite ruling", as if it did not matter much one way or the other, is too mild a statement for my view of the case, so may I give my reasons more fully?

Dr. Stiles, in the last paragraph of his letter (herewith), says "but in this particular instance it is not clear to me how many names are involved or how much of an upset would occur." At the outset, therefore, I should like to make it clear that I am in favor of the suppression of d'Orbigny's "Prodrome" names, not because of any prejudice against d'Orbigny or his work, but solely to prevent just such an "upset" of a very large number of familiar species.

I am fresh from trying to compile a monograph of the Bathonian Lamelibranchs, and it has been vividly brought home to me in the course of this work what a revolution in nomenclature the recognition of the "Prodrome" names would bring about. For the "Prodrome" was published in 1850, and Morris and Lycett's "Monograph on the Mollusca from the Great Oolite", from which nearly all our familiar names are drawn, was published in 1853-4. Morris and Lycett, who described and figured the species so well, very rightly gave up the attempt to interpret the "Prodrome" species, which they regarded as virtual *nomina nuda*. In the few instances where they thought they recognised one of d'Orbigny's species they were always wrong. For instance, *Trigonia cassiope* [of] Lycett is not *T. cassiope* d'Orb., which has since turned out to be a synonym of *T. pullus* Sow. The original diagnosis was as follows: "Espèce voisine du *T. Costata*, mais plus longue et pourvue sur l'area anale de trois grosse côtes saillantes crenelées indépendamment des côtes intermédiaires: Luc. Vézelay, etc." On this Boule comments in the "Types du Prodrome", 1913, p. 145: "Cette diagnose a donné lieu à des interprétations diverses. Lycett a décrit et figuré sous ce nom des échantillons qui doivent être pris comme types (Suppl. Mon. Moll. Gt. Ool., pl. 37, fig. 10, et Mon. Brit. Foss. Trig., pl. 32, figs. 1 and 5). La collection d'Orbigny renferme sous ce nom des échantillons variés; les uns sont indéterminables, tels que celui de Vézelay, la plupart des autres sont des *T. pullus* Sow., ainsi que l'a reconnu M. Bigot."

Again, with regard to *Myoconcha actaeon* d'Orb., Boule writes: "L'échantillon de la collection d'Orbigny est très mauvais; il faut prendre comme type la figure de *M. actaeon* donnée par Morris et Lycett. . . ."

You will notice that in both these quotations there is a tacit assumption that it is only d'Orbigny's *type specimen* which could give the name validity, but when this has to be rejected Morris and Lycett's species should be regarded as the types. There is no suggestion that d'Orbigny's *descriptions* should give the species validity.

If we reject some of d'Orbigny's names on the ground that the type specimens are unsatisfactory, it seems to be introducing an arbitrary factor in the form of personal opinion, and I do not see how anyone is to pronounce finally whether the type specimen of any species is satisfactory or not. Anyone's work is liable to be overturned at any moment by the expression of a different Opinion about the d'Orbigny collection in Paris. I have referred to this collection in a few cases myself, and know there is plenty of scope for different interpretations. The species in many of the boxes are composite.

How little thought d'Orbigny bestowed on the assigning of his names is shown by the system on which he worked. He gave all the species of one genus fantastic names with the same initial letter, after the manner of naming a class of warships or liners, e. g.: *Lima harpax*, *L. hellica*, *L. hippia*, *L. hille*; *Arvicula jason*, *A. janassia*, *A. janira*, *A. jarbas*, *A. jonthé*, etc. Many of the names so lightly assigned are scarcely worthy of varietal distinction. For instance five trivial varieties of forms in our familiar Great Oolite "*Cyprina*" *lowcana* Morris and Lycett appear in the "Prodrome" as *C. antiope*, *C. alcyon*, *C. amphitryton*, *C. arion* and *C. arethusa*. All these names have priority over Morris and Lycett's *lowcana*.

As far as my work has taken me, the recognition of d'Orbigny's names would involve the following changes in the Great Oolite alone:

Arca eudesii Morris and Lyc. would become *Arca eudora* d'Orb. *Arca tenuicosta* M. and L. would become *Arca electra* d'Orb. *Cucullaca clathrata* Leckenby would become *C. euryta* d'Orb. *Mytilus subreniformis* M. and L. would become *M. galanthus* d'Orb. *Trigonia cassiope* Lycett would require a new name. *Pecten hemicostatus* M. and L. would become *P. rhetus* d'Orb. *Astarte rustica* Lyc. would become *A. vesta* d'Orb. *Cyprina lowcana* M. and L. would become *C. antiope* d'Orb. *Protocardia stricklandi* M. and L. sp. would become *P. cybele* d'Orb. *Protocardia buckmani* M. and L. would become *P. luciense* d'Orb. (?) *Unicardium parvulum* M. and L. would become *U. ovoideum* d'Orb. *Corbula agatha* Lycett would require a new name.

In the Corallian:

Nucula oxfordiana Roeder would become *Nucula hellica* d'Orb. *Myoconcha texta* Buv. would become *M. radiata* d'Orb. *Astarte subdepressa* Blake and Hudn. would become *A. pasiphac* d'Orb. *Astarte nummus* Sauvage would become *A. pelops* d'Orb. *Astarte contejani* de Loriol would become *A. phyllis* d'Orb. *Isocyprina cyreniformis* Buv. sp., would become *I. dimorpha* d'Orb. *Unicardium excentricum* (d'Orb.) Dollfuss would become *U. aceste* d'Orb.

Further research will probably bring many other changes to light, and where it will end can only be determined by prolonged study of the d'Orbigny collection in Paris. The names in the "Prodrome" being for all practical purposes *nomina nuda*, it seems only fair that they should be officially recognised as such in theory.

B. B. Woodward (London) writes:

I am entirely in accord with Mr. L. R. Cox and W. J. Arkell in considering that the new specific names published by d'Orbigny in his "Prodrome de Paléontologie Stratigraphique Univ." should be regarded as *nomina nuda* unless accompanied by a reference to a figure or description published by some previous author.

M. Boule, Professor of Paleontology at the Muséum national d'Histoire naturelle, Paris, and Curator of the d'Orbigny Collection, presents the following considerations:

Il est de mon devoir de protester contre la proposition de MM. Cox et Arkell de traiter aussi légèrement l'oeuvre considérable et si utile d'Alcide d'Orbigny et de considérer, d'ores et déjà, comme inexistantes (*nomina nuda*) les espèces du "Prodrome", en arguant du fait qu'elles n'ont pas été figurées.

Il faut remarquer tout d'abord que beaucoup de ces espèces ont été réétudiées sur place, d'après les échantillons eux-mêmes par divers paléontologistes qui en ont figuré un certain nombre dans leurs propres travaux.

De plus, la figuration très soignée des échantillons types ayant servi aux courtes descriptions de d'Orbigny dans son "Prodrôme" a été précisément entreprise par mes soins, dès 1906 dans les "Annales de Paléontologie", pour satisfaire aux desiderata exprimés de tous côtés et pour remédier dans une certaine mesure à la complication croissante et déplorable de la nomenclature.

En 1923, l'ensemble de cette publication formait un premier volume illustré de 34 planches en phototypie et de dessins dans le texte où se trouvent citées ou décrites près d'un millier d'espèces (Silurien-Bathonien), avec rappel des publications antérieures relatives à ces espèces.

Depuis 1923, ce travail se continue régulièrement dans les "Annales de Paléontologie." Les espèces des étages Callovien et Oxfordien ont été figurées, celles de l'étage Corallien sont en cours et la publication se poursuivra avec le plus de célérité possible.

Je proteste également contre l'affirmation de MM. Cox et Arkell que les échantillons figurés par nos soins sont des types supposés. D'abord beaucoup de ces espèces sont représentées par un exemplaire unique. Dans les autres cas, le type est celui qui figure en tête de l'énumération du Catalogue manuscrit de d'Orbigny. Ce n'est que dans des cas très rares qu'il peut subsister quelque doute. MM. Cox et Arkell parlent de la collection d'Orbigny sans la connaître. Le jour où ils voudront la consulter au Muséum, où elle est à leur disposition, leur opinion deviendra certainement plus favorable.

La proposition de nos confrères anglais et américains s'explique par une application du principe du moindre effort. Il est en effet plus facile de donner à des fossiles des noms nouveaux que de se livrer à de longues recherches pour les rapporter à des espèces déjà connues. Non seulement une telle manière de procéder n'est pas conforme à l'équité, mais encore elle a pour effet d'augmenter précisément les complications de nomenclature qu'on voudrait éviter.

J. F. Pompeckj, Geologisch-Paläontologisches Institut und Museum der Universität, Berlin, reports:

besteht die Gefahr, dass auch andere alte Autoren, wie z. B. Baron v. Schlotheim ähnlichen Ausnahme Bedingungen unterworfen werden.

Meiner Meinung nach müssen die d'Orbigny'schen strittigen Namen nach den Internationalen Regeln der Zoologischen Nomenklatur behandelt werden (Art. 25, a and b).

Ich kann daher dem Vorschlage der genannten Herren nicht zustimmen.

Dr. Rudolph Richter, of the Senckenbergische Naturforschende Gesellschaft, Frankfurt a. M., expresses the following opinion:

1. Hinsichtlich der Beschreibung, durch die ein Artnamen gültig wird, verlangt der Codex (Artikel 25) nur das *Vorhandensein* in der ursprünglichen Veröffentlichung. Über die *Qualität* oder *Quantität* der Beschreibung werden keine Vorschriften gemacht. In demselben Sinn hat sich Opinion 52 ausgesprochen.

Nach der *lex lata* besteht also *kein Zweifel über die Gültigkeit* auch solcher Namen in d'Orbigny's "Prodrôme", deren Beschreibung so kurz ist wie in dem angeführten Beispiel von *Lucina sarthacensis*.

2. Aber auch wenn man von der *lex lata* absieht und nur prüft, ob eine *lex ferenda* zweckmässig wäre, kommt man zu demselben Schluss:

Schon heute genügen die meisten Diagnosen der älteren Literatur nicht, um zu erkennen, welche Species der Autor gemeint hat. Zu ihrer Zeit hat eine Diagnose vielleicht völlig dazu ausgereicht, auch wenn sie nur aus zwei Worten bestand. Heute aber sind nicht nur viele Arten hinzugetreten, gegenüber denen damals noch nicht unterschieden zu werden brauchte, sondern vor allen Dingen, es sind neue Gesichtspunkte für die Systematik massgebend geworden. In dieser Richtung wird die Entwicklung weitergehen. Nehmen wir an, dass die Zoologie die Artbegriffe nach Serum oder Blutgruppen abgrenzen würde oder die Paläontologie die Abtrennung ihrer Arten nur nach röntgenographisch erkennbaren Strukturen vollziehen würde, so würden sämtliche früheren Diagnosen ungenügend werden. Wenn dann ein Chaos der Nomenklatur vermieden werden soll, so geht es nur auf den vom Codex verfolgten Wegen: Der Typus jeder Art ist nach dem neuen Gesichtspunkt zu untersuchen und neu zu beschreiben; aber an jedem Typus hängt der Artnamen unabänderlich.

Wenn er auch heute so schlimm noch nicht ist, so muss man doch oft genug den Typus untersuchen, um die ursprüngliche Beschreibung richtig zu verstehen. Die Unbequemlichkeit, die die persönliche Untersuchung der Typen nötig macht, und die gelegentliche Änderung von Namen in Fällen, wo die Vorgänger diese Pflicht versäumt haben, rechtfertigen aber nicht, das segensreiche Prinzip des Codex aufzugeben.

Denn wenn man einem späteren Autor das Recht gäbe, den Namen eines früheren Autors dadurch ungültig zu machen, indem er die ursprüngliche Beschreibung als "nicht ausreichend" anerkennt, so würde das die Subjektivität quo ante codex wieder einführen und jede Stabilisierung der Nomenklatur unmöglich machen.

Schluss: Es würde unheilvolle Folgen haben, wenn man für d'Orbigny's "Prodrome" Ausnahmebestimmungen zulassen sollte.

Dr. Wolfgang Adensamer, of the Naturhistorisches Museum, Vienna, reports:

Es scheint mir sehr wünschenswert die zahlreichen unzureichend beschriebenen Artnamen in d'Orbigny's "Prodrome de Paléontologie Stratigraphique Universelle" (3 Bde. Paris; 1850) zu eliminieren! Ich schliesse mich ganz der Ansicht der Herrn Kollegen Dr. L. R. Cox und Dr. W. J. Arkell an, dass die nicht oder unzureichend erläuterten Artnamen des d'Orbigny'schen "Prodrome" in der Nomenklatur nicht berücksichtigt werden sollen. Am Schluss der Ausführungen von Cox und Arkell heisst es: "D'Orbigny's new genera will not be valid if the only species referred to them are those suppressed as *nomina nuda*; . . .". Falls derartige Genera hinreichend beschrieben sind, halte ich es aber nicht für zweckmässig sie auszuschalten! Hier müsste die Ansicht der jeweiligen Spezialisten eingeholt werden. Auf alle Fälle ergibt sich nicht durch das Ausscheiden aller d'Orbigny'schen Artnamen eines d'Orbigny'schen Genus das unberücksichtigt lassen dieses Genusnamens! Hier müsste eine eigene Bestimmung solche Genusnamen eliminieren.

W. C. Mendenhall, Acting Director of the United States Geological Survey, submits the following:

The proposal of Messrs. L. R. Cox and W. J. Arkell that the new specific names published by A. d'Orbigny in his "Prodrome de Paléontologie Strati-

graphique Universelle" (3 vols., Paris, 1850) shall be considered as *nomina nuda* and shall have no status in nomenclature unless they are accompanied by a reference to a description or figure published by some previous author has been considered by the paleozoologists of the Geological Survey who are now in Washington. A review of the individual opinions submitted indicates, with one exception, general agreement in the view that each of d'Orbigny's new species published in his "Prodrome" should stand on its own merits and that those that have been or can be identified should be accepted as valid. The Survey paleontologists who subscribe to this view are Charles Butts, C. Wythe Cooke, George H. Girty, W. C. Mansfield, John B. Reeside, Jr., P. V. Roundy, T. W. Stanton, and L. W. Stephenson. A dissenting view is expressed by Edwin Kirk, who states that he thinks that the proposition submitted by Messrs. Cox and Arkell is sound and he concurs in the stand they take.

R. S. Bassler and Charles E. Resser, paleontologists of the United States National Museum, wish to be recorded as in favor of the majority opinion given above.

Dr. Paul Bartsch, United States National Museum, submits the following opinion:

I cannot see how by any stretch of the imagination these names could be considered *nomina nuda* if they are accompanied by short descriptions. Furthermore, these descriptions, it would appear to me, will be found probably in almost all instances recognizable when one has ample collections from the locality in question which, as the two authors state, is always cited.

I have read, at times, through pages of descriptions, and have found it quite difficult to pull out the few things that differentiated the species or subspecies in question from another form closely allied to it, and I have frequently longed that the author would give just a few brief diagnostic characters.

If specialists, working with the fauna in question, are unable from the short description and the name to fix upon a proper candidate for the name, then it seems to me that the species in question will have to be relegated to the unrecognizable group and left there until some wise man is capable of rescuing it from that limbo.

Dr. H. A. Pilsbry, Academy of Natural Sciences of Philadelphia, reports:

The new names in d'Orbigny's "Prodrome" are not all so curtly defined as the examples given by Messrs. Cox and Arkell. Some are sufficiently defined by comparative characters for recognition and have been generally recognized. To reject all these names as *nomina nuda* would be inexact. Moreover, such an Opinion might open the question of adequacy of definition in enough other cases to swamp the Commission.

I believe it the wiser course to leave new names in d'Orbigny's "Prodrome" to be dealt with individually by the paleontologists interested.

These documents were submitted to Commissioner Bather, who has prepared the following discussion of the case:

The application by Messrs. Cox and Arkell raises many difficult questions. This must be my excuse for a somewhat long discussion before proceeding to submit an Opinion.

The expression *nomen nudum* does not occur in the Rules or Recommendations. It may occur somewhere in the Opinions, but repeated search has failed to find it. In the absence of a definition by the International Commission, it seems necessary to take the literal meaning of the words, which corresponds with general usage, viz., a generic or specific name unaccompanied by any word of definition, diagnosis, or description, by any figure, or by any reference to previous definition, etc. or figure. A statement of locality and geological horizon does not of itself prevent a name from being a *nomen nudum* (Opinion 52). Reference to a type specimen or type specimens by the register or catalogue number of a museum or collector does not of itself prevent a name from being a *nomen nudum*; *à fortiori* the mere existence of a type specimen has no bearing on the question (Opinion one).

It is plain that the new names introduced by d'Orbigny in the "Prodrôme" are not *nomina nuda* in the sense here defined, and no ruling of the International Commission can make them so.

This conclusion has the support of Dr. Bartsch, but the other colleagues do not seem to have dealt with the precise point.

The application of Messrs. Cox and Arkell is not, however, to be dismissed because of a loose use of terms. They proceed to request that the "Prodrôme" names "shall have no status in nomenclature."

The meaning of this phrase, as used by the applicants, is ambiguous. There are two kinds of status: 1. availability; 2. validity.

1. A specific name may be unavailable for various reasons, e. g., because it is pre-Linnean, unpublished in the sense of the Code, non-binominal, as well as the reasons already discussed.

2. A specific name may be invalid for various reasons, and these reasons are of two kinds—*a*, nomenclatural; *b*, zoological.

a. Invalid because a preoccupied homonym, or because established on the same type specimen or other indication as a pre-existing species, i. e., a nomenclatural synonym.

b. Invalid because held by the reviser(s) to belong to a species previously named, i. e., a zoological synonym. Invalid because the definition, figure, etc., are held by the reviser(s) to be incapable of interpretation, or, in so far as capable, then palpably incorrect and misleading.

Now the International Commission is competent to pass an Opinion on all questions raised under 1 and 2*a*, because these are questions of pure nomenclature. It is not competent definitely to decide questions under 2*b*, because these involve zoological points, and these points are not so much of zoological fact as of subjective interpretation. The Commission is, however, competent to pass an Opinion on the nomen-

clatural consequences of zoological assumptions. It is, for example, entitled to say to a zoologist; "If you honestly believe that *Cidaris weissmanni* Desor, 1846, is the same species as *Cidaris spinosa* Agassiz, 1841, you must, other things being equal, adopt the name *Cidaris spinosa*."

Now it is on zoological grounds that Messrs. Cox and Arkell base their application. They say of the new names for previously undescribed species in the "Prodrome" ". . . in each case . . . a brief comment is made on the species, but this . . . is quite inadequate as a specific diagnosis." This apparently means that the applicants, whose expert knowledge must be admitted, are unable to recognise the species from d'Orbigny's sentences. They are entitled to their opinion, and justified in applying the Rules accordingly. The names will, so far as Messrs. Cox and Arkell are concerned, be invalid. But, as they point out, this will not stabilise the nomenclature, for other experts may hold a contrary opinion. Further, they say, the application of the Rules will result in upsetting a considerable number of names in current use. This must, it appears, be the result whatever view be held as to the validity of the names, and they claim that the only way to avoid both instability and confusion is to make the names nonavailable. This can be effected only by suspension of the Rules.

A specific instance of the difficulties may be given: *Trigonia cassiope* d'Orb. ("Prodrome", vol. 1, p. 308).

Lycett (1863) took over this name without comment and described British specimens as *T. cassiope* d'Orb. Others, however, have interpreted d'Orbigny's diagnosis differently.

Reference to the original specimens shows that, in the words of M. Boule, "La collection d'Orbigny renferme sous ce nom des échantillons variés; les uns sont indéterminables . . . la plupart des autres sont des *T. pullus* Sow." (1913, "Types du Prodrome", p. 145.)

It is open to Professor Boule to say that *T. cassiope* d'Orb. cannot be recognised from the description, and so to regard the name as invalid; or it is open to him to say that *T. cassiope* d'Orb. is a synonym of *T. pullus* Sow. But he continues; "Lycett a décrit et figuré sous ce nom des échantillons qui doivent être pris comme types." Clearly they cannot be the types of *T. cassiope* d'Orb., for they were not part of d'Orbigny's material. Is then the name *T. cassiope* Lycett available? Certainly not if *T. cassiope* d'Orb. is recognisable as a synonym of *T. pullus* for then *T. cassiope* Lycett is a homonym of later date and is to be rejected under Article 35.

But if we admit Professor Boule's other conclusion that *T. cassiope* d'Orb. is unrecognisable, then it cannot be said definitely to represent

any species, whether the same as *T. cassiope* Lycett or not the same. Therefore Article 35, if taken strictly and literally, does not apply, and *T. cassiope* Lycett can be used.

[Article 35.—A specific name is to be rejected as a homonym when it has previously been used for some other species or subspecies of the same genus.]

This interpretation of Article 35 has never been discussed, but a casual phrase in the discussion of Opinion 54 indicates that the opposite view would have been taken by the Commission in 1913. It is there said, "If *Phoxinus* Rafinesque, 1820, is unidentifiable it becomes a *genus dubium*, but the name preoccupies *Phoxinus* Agassiz, 1835." That was not the question before the commission, so that the remark is an *obiter dictum*. Nevertheless, such an interpretation would have its value in extending the principle of Article 35 and so promoting stability. Thus, in the example chosen from the "Prodrôme", *T. cassiope* d'Orb. may stand as a valid species or as a synonym of *T. pullus*, in which cases *T. cassiope* Lycett, if different, must have a new name. Or *T. cassiope* d'Orb. may be a species *dubia*, and still *T. cassiope* Lycett must have a new name.

If, as claimed by the applicants, many other names of the "Prodrôme" have been similarly misinterpreted by subsequent writers and have come into general use for species that are not those intended by d'Orbigny, then there is a *prima facie* case for considering suspension of the Rules. It becomes necessary to discuss this proposal in more detail, and to consider the arguments adduced by the applicants and by the colleagues whose opinion has been asked.

Let us take first the opinions unfavorable to the application:

Professor Boule, as Keeper of the d'Orbigny Collection, claims foremost attention. He assumes that Messrs. Cox and Arkell are unacquainted with the d'Orbigny Collection. This is not the case: Mr. Arkell has examined some of the originals for himself and finds that in some instances more than one species is included under a single name. This observation probably explains the phrase "supposed types", to which M. Boule naturally objects. If, as M. Boule implies, the holotype is fixed by d'Orbigny's MS. Catalogue, then the phrase is certainly unwarranted. It may, however, be recalled that De Loriol occasionally doubted whether the alleged type really was the type.

The valuable work being done on the collection by M. Boule or under his direction does not seem to bear on the point at issue. The absence of figures from the "Prodrôme" was not specially given by Cox and Arkell as a reason for rejecting d'Orbigny's definitions;

and it was known to them, and so stated, that several of d'Orbigny's specimens had been described and figured by later authors, notably by M. Boule.

The opinion expressed by Mr. W. C. Mendenhall and many paleontologists of the United States Geological Survey and the United States National Museum is not perfectly clear. It says that those of d'Orbigny's species " that have been or can be identified should be accepted as valid." This may mean either identified on the basis of d'Orbigny's diagnosis or identified by reference to the type material. The distinction is important, as will appear further in the discussion of Dr. Richter's letter.

Dr. Richter is the only colleague who defends his position by relevant argument.

1. He maintains that, according to Article 25, a species name is validated by a description. Now Article 25 does not say this. It says that a name cannot be valid unless " accompanied by an indication, or a definition, or a description." " Ueber die Qualität oder Quantität der Beschreibung werden keine Vorschriften gemacht " (Richter). Opinion 52, cited by Richter, says " It is not feasible for the Commission to issue an opinion upon the question: What constitutes an adequate description? "

All that follows from this is that a name accompanied by a description should be considered, but whether the description is sufficient to validate the name is a question to be decided by the reviser. " It is ", to quote the discussion of Opinion 52, " entirely a zoological not a nomenclatorial question."

Opinion 52 has, however, a direct bearing on d'Orbigny's " Pro-drome ", because it states that the type locality " is to be considered as an important element in determining the identity of species." If in this we intercalate the words " and/or type horizon " we have a restatement of the principles on which d'Orbigny worked, as fully explained in the introduction to the " Pro-drome."

2. Richter says very truly that a diagnosis which would be inadequate to-day may have been adequate when it was drawn up. This is a view that I have urged repeatedly. But it does not follow that the diagnosis *was* adequate.

On the assumption that a diagnosis even today may be inadequate, Richter concludes that examination of the holotype is essential. I should not like to say anything that would seem to suggest the contrary. "An jeden Typus hängt der Artname unabänderlich ", is a principle that cannot be urged too strongly; but it must not be taken to relieve authors from the necessity for drawing up adequate diagno-

ses. Some diagnoses have been unintelligible to the author's contemporaries, and have been proved by subsequent reference to the type specimens to be misleading and even incorrect.

The object of a definition or diagnosis is to furnish contemporary fellow-workers with the characters by which they can distinguish the species from others already known or diagnosed at the same time. It is not (as is a description) intended to furnish evidence by which the species may possibly be distinguished from all others hereafter to be discovered. It is when extension and precision of the original diagnosis are necessitated by further discoveries that recourse to the holotype is incumbent on the reviser. If contemporaries could not understand a definition apart from the holotype, it is surely plain that the definition was inadequate from the outset. Since there always was and must be type material of some kind, the logical consequence of inclusion of the holotype itself within the definition would be to deprive the rest of the definition of any significance. One need say no more than: "A charming species, rather large, Holotype: Nat. Mus. Ruritaniae, No. X999."

Dr. Richter supports his thesis by an appeal to the "subjectivity" involved in any interpretation of the diagnosis. A bad diagnosis undoubtedly opens the door to subjectivity; but a diagnosis is good in so far as it eliminates subjectivity. After all there may be as much subjectivity in the interpretation of a holotype (especially if it be an obscure fossil) as in the reading of a diagnosis. (See next Section, argument No. 6.)

The arguments in favor of the proposal are contained to some extent in the original application (C. and A.), but still more in letters subsequently received from Mr. Cox (C.) and Mr. Arkell (A.). They are:

1. The comments of d'Orbigny are inadequate as specific diagnoses (C. and A.).
2. D'Orbigny's species have been misinterpreted by later authors, or have been ignored and described under other names (C. and A.).
3. The names, whether d'Orbigny's or new, used by later authors are familiar and current, and it would breed confusion to disturb them (C. and A. and A., who gives many examples).
4. D'Orbigny was a competent describer, not to be compared with writers 50 years before him, and he himself says that it is not his intention to describe the new species in the "Prodrome"; he would have described them later in the "Paléontologie Française" (C.).
5. Reference to a type specimen should not be a permissible substitute for an intelligible definition (C.).

6. To retain or reject a species according as the type specimen is considered satisfactory or not is to introduce personal opinion (A.).

7. In some cases, as admitted by Boule, and as testified by Arkell, d'Orbigny's type specimens are not satisfactory.

8. D'Orbigny's names were often fantastic and given without thought.

On the preceding arguments, the following comments may be made:

1, 2, and 4. Undoubtedly d'Orbigny did not intend his remarks as "descriptions," but it is not so sure that he did not intend them as provisional diagnoses, sufficiently clear to enable the species to be identified. Whatever his intentions may have been, the fact is that he fulfilled the requirements of the Code.

The question of confusion does not necessarily depend on the inadequacy of the "Prodrome" diagnoses; still the applicants make that so large a part of their argument that the justice of the charge must be considered. It has been pointed out that the adequacy of a definition must be decided with regard to the knowledge of the time, and the applicants attempt to show that contemporaries could not understand the "Prodrome" diagnoses. Their examples are all drawn from the Oolitic Mollusca and from Morris and Lycett. Even were they justified in this regard, it does not follow that other groups and other specialists were in similar case. I have therefore looked into some of the echinoderm species, as well as into the molluscan.

First, it does not appear what steps Morris and Lycett took to understand the "Prodrome." D'Orbigny lays great stress in his introduction on horizon and locality, and it has already been decided by the Commission that such details when given are to be taken into account. Did Morris and Lycett attempt this? In nearly every case where they adopt one of d'Orbigny's new names, they do so without comment; only under *Opis pulchella* d'Orb. do they indicate that they have made the necessary comparison, and they say: "The experience derived from a multitude of examples leaves no room to doubt that . . . *d'Orbigny* has correctly indicated its distinctive characters in the brief sentence above quoted."

Morris and Lycett took over d'Orbigny's names in enough instances to show that they did not regard his diagnoses as inadequate; they did not, so far as I can see, express any opinion on the matter. There is no evidence, except that just quoted, that they ever troubled to examine specimens from the type locality.

The evidence bearing on the new echinoderm species of the "Prodrome" is far more satisfactory.

For the echinoids we have Desor's "Synopsis", which appeared within a few years and obviously considered d'Orbigny's names. Some were accepted without comment, some were accepted on evidence of specimens, some were adversely criticised, and some were passed over in silence presumably as inadequately defined. Thus: *Diadema subcomplanatum* d'Orb., p. 319, *416, is accepted. Wright also accepts this and mentions specimens. *Hemicidaris luciensis* d'Orb., p. 320, *422 is accepted after examination of specimens from Luc. Wright also accepted this. *Diadema calloviensis* d'Orb., p. 346, is accepted, but apparently on the evidence of a paratype. *Diadema Jobac* d'Orb., p. 290, *513. "Espèce voisine du *D. subangulare*, mais avec les tubercules intermédiaires tout autrement disposés". Desor ("Synopsis", p. 17) says with justice "la diagnose ci-dessus ne suffit pas pour identifier une espèce." Finally *Cidaris jarbus*, *C. jasius*, and *C. itys* d'Orb., p. 222, are not mentioned in the "Synopsis", perhaps because they were based only on radioles; the definitions seem to me adequate. *Holactypus corallinus* d'Orb., vol. 2, p. 26, was accepted by Desor and by Cotteau (1854). Cotteau also (1854) found no difficulty in identifying d'Orbigny's *Dysaster suprajurensis* in the field, although he did not regard it as distinct.

Turning to the Crinoidea we find De Loriol in "Paléontologie Française" exercising a similar discrimination, accepting or rejecting. His approach to the "Prodrome" differs from that of the echinoid specialists mentioned because he had the type material before him. He refrains none-the-less from accepting a name merely because he can identify the holotype. He accepts *Cyclocrinus precatorius* (vol. 1, p. 320) and *Millericrinus rotiformis* (vol. 1, p. 346) without criticising d'Orbigny's definitions. Of *Millericrinus bachelieri* (vol. 1, p. 346) he says: "la diagnose n'est pas compréhensible", and the material in the d'Orbigny Collection does not enable him to interpret the species. There are seven specimens in the collection labelled *Millericrinus pulchellus* from the type locality "dont quatre seulement correspondent à la description du Prodrome" (vol. 1, p. 346), from which statement one infers that the holotype is not always so easily ascertained as Professor Boule implies.

Several species are described by De Loriol from the type material and he adopts d'Orbigny's names, although he either asserts or implies that the "Prodrome" definition was inadequate or misleading. See for instance his remarks on *Pentacrinus oceani*, *P. marcousanus*, *Millericrinus convexus*, and *Pentacrinus buvignieri*, which last he makes a synonym of *P. nicoleti* Desor, solely on the evidence of types of both

authors. In such cases it seems to me that the names should be quoted as "de Loriol ex d'Orb.", for there is nothing in Article 35 to prevent a name being used for the *same* species.

In the following instances De Loriol's remarks may be quoted more fully because they bear directly on the point at issue.

"Prodrome", vol. 1, p. 241, *248 *Pentacrinus liasinus* d'Orb., 1847. Espèce voisine du *pentangularis*, mais plus grêle encore et plus uniformément lisse [3 locc. are given].

There is no such name as *P. pentangularis* in d'Orbigny; perhaps *P. pentagonalis* is meant. If so, d'Orbigny is comparing Liassic and Oxfordian, a procedure which he criticises in the Introduction. De Loriol, on examining the syntypes of *P. liasinus*, rejects the name, as well as *P. cylindricus* Desor *nom. nud.*, in favor of the later *P. subteroides* Quenstedt, because the latter is "le seul réellement connu dans la science, puisque le premier ne l'est que par une simple mention, et le second par une phrase du 'Prodrome,' qui n'est pas même exact."

"Prodrome", vol. 1, p. 321, *?433 *Pentacrinus nodotianus* d'Orb., 1847. Espèce voisine du *P. briareus*, mais ayant ses verticilles moins comprimés.

De Loriol ("Paléontologie Française", 420 sqq.) explains how he was quite at a loss to interpret this until he discovered the type, which belonged to *P. dargniesi* Terquem and Jourdy, 1869. His concluding remarks put the case clearly:

Maintenant quel nom lui donner? Celui de d'Orbigny a la priorité d'années, mais, en vérité, il est impossible de prétendre que la simple mention du "Prodrome", que j'ai citée, et qui, encore, n'est pas exacte, soit suffisante pour dire que l'espèce a été publiée par d'Orbigny *antérieurement* à MM. Terquem et Jourdy. Ce sont ces derniers qui, par une description et de bonnes figures, ont réellement fait connaître l'espèce, dont personne, d'après la phrase de d'Orbigny, ne pouvait avoir la moindre idée, sauf que c'était un *Extracrinus*. Je crois donc que le nom de *P. nodotianus* doit être définitivement abandonné, parce qu'il était impossible de savoir quelle espèce il représentait, et que, in réalité, avant MM. Terquem et Jourdy, l'espèce n'avait pas été publiée.

With these remarks of De Loriol I entirely agree.

To sum up these enquiries into the adequacy of the "Prodrome" diagnoses.—It appears that, while some are clearly inadequate, others have been found adequate by specialists who took all the facts into consideration. In this respect the "Prodrome" does not seem to me worse than many works which have always been accepted. Among relevant facts I do not include the existence of a type specimen; at the same time it may be pointed out that, although d'Orbigny indicates by an asterisk the existence of specimens in his collection, he nowhere

fixes on any specimen or specimens as holotype or syntypes. In fixing the holotype it is no doubt advisable to regard the locality and, if the specimens therefrom are individually listed, to select the first on the list as holotype. The holotype as thus fixed may confirm the interpretation of the diagnosis, or, as Professor Boule and others have shown, it may be equally unintelligible; or again, the diagnosis may be quite clear and may correspond with specimens from the type locality although the lectotype happens to be obscure.

The adequacy of the "Prodrome" diagnoses is not to be judged by their length, for a single epithet may be sufficient. Nor can the names employed have any bearing on the question, especially as d'Orbigny (Introduction, § 66) insists that names which have no meaning are often the best.

Thus examination of the "Prodrome" leads to the conclusion that it is possible to consider each of the new species on its own merits and to accept as valid those that have been or can be identified.

The plea of the applicants is that such a course would lead to confusion, and Mr. Arkell in his letter gives a respectable number of instances in which familiar names would have to go. It does not appear that there is or would be any particular difficulty in echinoderms. My colleagues in the Geological Department of the British Museum take essentially the same view in regard to corals, Polyzoa, and brachiopods.

In these circumstances it seems out of the question for the Commission to sweep away all the names proposed for new species in the "Prodrome." It is by no means certain that such action would not produce a converse state of confusion in some groups.

The chief difficulty, or at any rate the most annoying change involved by following the Rules, seems to be that exemplified by *Trigonia cassiope* and *Myoconcha actaeon*. Here it is generally admitted that d'Orbigny's diagnoses are inadequate (even the type specimens do not elucidate them). Yet it seems to be thought necessary to reject the *T. cassiope* and *M. actaeon* of Morris and Lycett as homonyms of d'Orbigny's species. This conclusion does not appear to be necessitated by the rules. I have already maintained that a name cannot be a homonym when given to the *same* species. But can it be said (in the words of Article 35) that *T. cassiope* d'Orb. was used for some other species than *T. cassiope* M. and L.? *Ex hypothesi* it cannot. If it were proved that *T. cassiope* d'Orb. did represent a distinct species, then that name would stand, but it has not been proved, and, one gathers, cannot be proved. Morris and Lycett were not founding

a new species; they believed that their specimens belonged to d'Orbigny's species. If the contrary cannot be proved, surely the name may be left.

Many of the difficulties arising out of the "Prodrôme" and similar works would be largely smoothed away if the Commission could agree to the following:

A name that rests on a diagnosis unintelligible in itself and not explained by the type material, shall not prevent the use of the same name for a species from the same locality and horizon, when subsequently diagnosed in proper form.

To meet the undoubted difficulties I have endeavored to frame an Opinion that would be of general application, but without success. I therefore submit the following for the approval of the Commission.

Opinion.—There are no grounds for treating d'Orbigny's "Prodrôme" differently from other works containing preliminary diagnoses. In all such cases the decision whether a diagnosis is adequate or no must be made by the systematist and not by the Commission.

If the diagnosis is held to be adequate, the ordinary rules regarding priority and homonyms apply.

If the diagnosis is held to be inadequate, the publication of the name will not prevent any author from subsequent description and establishment under the same name of the same species (as recognised from the holotype, if any); further, if the holotype be wanting or undecipherable, subsequent description and establishment under the same name of a species from the same locality and horizon is permissible. In both these cases the date for purposes of priority shall be the later date, and if the later author (say Brown) is not the same as the earlier author (say Green) then the name shall be quoted as "Brown ex Green". If, however, the holotype attached from the beginning to the earlier use of the name with inadequate diagnosis be clearly of a different species from the holotype attached to the later use, then the later use is a homonym as defined by Article 35 and is to be rejected.

On the question of generic names, also raised by the applicants, Dr. Adensamer considers that a genus if properly diagnosed will be valid although the species referred to it may be suppressed as *nomina nuda*.

This seems rather a contradiction in terms.

If there is only one species, the diagnostic features of the genus, which *ex hypothesi* are adequate, will also distinguish the species. If neither they nor the characters of the species are adequate, then both genus and species must fall. (Cf. Opinion 43.)

If there be more than one species, one of them either was, or must now be, selected as genholotype. That will then be distinguished from all species previously known by the diagnostic characters of the genus. The names of the remaining species may be treated as synonyms of the genholotype, or as *nomina nuda*.

Opinion prepared by Bather.

Opinion concurred in by fourteen (14) Commissioners: Bather, Cabrera, Dabbene, Handlirsch, Horvath, Ishikawa, Jordan (K.), Pellegri, Richter, Silvestri, Stejneger, Stephenson, Stiles, Stone.

Opinion dissented from by no Commissioner.

Not voting, five (5) Commissioners: Apstein, Bolivar, Chapman, Jordan (D. S.), Warren.

Stone adds:

I agree with paragraphs 1 and 2 of the Opinion but paragraph 3 is so far reaching that it should be definitely embodied in the Rules rather than be considered in an Opinion on a single case.

I agree that a genus based upon *nomina nuda* has no standing.

Richter adds:

Ich stimme der Opinion zu, jedoch mit Ausnahme des Absatzes 3, dem ich nachdrücklich widerspreche. "If the diagnosis is held to be inadequate", ist eine Frage, die mehr als andere der Subjektivität unterworfen ist. Es ist daher nicht nur eine unnötige Neuerung, sondern sogar ein gefährlicher Anreiz, einem Autor zu erlauben, seine Autorschaft mit einem älteren Namen zu verbinden, weil dessen ursprüngliche Diagnose "nicht ausreichend" sei. Der bisher in Zoologie und Paläozoologie übliche Gebrauch, Autorschaft und Prioritätsdatum bei der ursprünglichen Veröffentlichung zu belassen und den Autor der späteren Diagnose nur in zweiter Linie zu nennen, hat seine guten Gründe und sollte nicht geändert werden. Beispiel: *X-us albus* Green, 1900; emend. Brown 1920. Denn: lässt Green's Diagnose die Möglichkeit zu, dass *albus* Brown damit identisch ist, so besteht kein Grund, diese Identität zu bezweifeln. Solange diese Identität aber nicht bezweifelt wird, ist *albus* Brown sowohl als Homonym wie als Synonym von *albus* Green zu betrachten.

Ich bin mit einem Absatz der Opinion gar nicht einverstanden, nämlich mit der Erlaubnis, zu zitieren "Brown ex Green", wobei das Datum der Priorität dem späteren Autor zugesprochen werden soll. Ich würde es sehr begrüßen, wenn dieser Absatz aus der Opinion entfernt werden könnte. Im übrigen ist Bather's Discussion von wundervoller Klarheit. Aber in jenem Satz scheint mir die Commission nicht nach der Konsequenz ihrer eigenen Grundsätze zu handeln.

Stiles adds:

It would be well to consider whether the difference of opinion as expressed by Bather and by Richter is not settled by Art. 24 concerning division and restriction of a species.

OPINION 127

SUSPENSION OF RULES FOR *Lepidocyclina* GÜMBEL, 1868, TYPE
Nummulites mantelli

SUMMARY.—Complying with expert advice from specialists in the group involved, the Commission herewith Suspends the Rules and places *Lepidocyclina* Gümbel, 1868, type *Nummulites mantelli*, in the Official List of Generic Names, with *Cyclosiphon* Ehrenberg, 1856, type *Nummulites mantelli*, as objective synonym. The consultants agree, almost unanimously, that to apply the Rules in this case would produce greater confusion than uniformity.

STATEMENT OF CASE.—Commissioner Chapman of Melbourne, Australia, recommends that the Rules be suspended in the case of *Lepidocyclina*, 1868, vs. *Cyclosiphon*, 1856.

DISCUSSION.—According to the evidence verified by the Secretary the nomenclatorial premises in the case of *Cyclosiphon*, 1856, versus *Lepidocyclina*, 1868, are very clear.

Cyclosiphon Ehrenberg, 1856, Ueber den Grünsand, K. Akad. Wiss., Berlin Abhandl., für 1855, p. 145, is monotypic, being based solely upon *Nummulites mantelli*.

Lepidocyclina Gümbel, 1868, Beiträge zur Foraminiferenfauna der nordalpinen Eocängebilde, K. bay. Akad. Wiss., m.-p., Cl. Bd. 10, no. 2, pp. 689 and 717, was originally published as a subgenus of *Orbitoides* and contained three species, i. e., *L. mantelli* Morton, *L. dilatata* Michelotti, and *L. burdigalensis* Gümbel. No type species was designated, indicated or intimated, directly or indirectly.

Douvillé, 1898, Bull. Soc. Géol. France, ser. 3, vol. 26, p. 594, definitely designated *Nummulites mantelli* as genotype, as correctly stated by Galloway, 1928, Journ. Paleontol., vol. 2, p. 65, and as accepted by Vaughan, 1929, p. 29.

As both generic names are based upon the same type species they are objective synonyms regardless of any subjective interpretation in respect to their structure (we name objects, not our conception of those objects). On this account Galloway, 1928, pp. 46-64, logically accepted *Cyclosiphon* in preference to *Lepidocyclina*.

The Commission is now requested to suspend the rules and to validate *Lepidocyclina* in place of *Cyclosiphon*.

On account of the general adoption of *Lepidocyclina* and its importance in paleontology the Secretary has referred this case to various

specialists for expression of opinion, and in reply has received the following:

J. A. Cushman reports:

I have little to add to the debate on these two names [*Lepidocyclina* and *Cyclosiphon*]. I should try to be consistent and use *Cyclosiphon*, but as noted in Vaughan's paper here appended, it is a very great doubt as to what was meant by Ehrenberg, and his types are certainly not at all helpful. On account of the very great uncertainty, I would advocate the retention of the name *Lepidocyclina* in this case.

When in Berlin in 1927 I examined the material of *Cyclosiphon* in the Ehrenberg collection there and found it to consist of various things, mostly glauconitic casts, a considerable portion of which did not even belong to the family Orbitoididae. Of the material which could be referred to an orbitoid none was of sufficient completeness even to be specifically identifiable.

Evidently Ehrenberg from his description of *Cyclosiphon* had not seen the *Nummulites mantelli* which he referred to as his generic description would exclude that species from the genus *Cyclosiphon*.

It seems to me very clear from the evidence that no good purpose would result from trying to revive the name *Cyclosiphon* with all the attendant confusion that would necessarily arise. I, therefore, urge most strongly the retention of the name *Lepidocyclina* with *Nummulites mantelli* as the type species of both the genus and the typical subgenus.

T. W. Vaughan, "A Note on the Names *Cyclosiphon* Ehrenberg, 1856, and *Lepidocyclina* Gümbel, 1868", Journ. Paleontol., vol. 3, no. 1, March 1929, pp. 28-29, reviews the case of *Lepidocyclina* and concludes that:

Because of confusion surrounding *Cyclosiphon*, it appears to me undesirable, even unfortunate, to revive that name, and it seems that the use of the name *Lepidocyclina*, with *Nummulites mantelli* as the type-species of both the genus and the typical subgenus, should be continued.

Letter from Dr. George Otis Smith, Director of the U. S. Geological Survey, Washington, D. C.:

The proposition for suspension of the Rules in zoological nomenclature for the purpose of retaining the two generic names *Lepidocyclina* and *Nummulites* has been considered by all of the Geological Survey paleontologists now in Washington whose work involves the use of zoological names. While the workers of this group subscribe to the rule of priority for general use they are unanimous in their recommendation that the rule should be suspended in its application to the two names above mentioned so that they may be continued in use.

The signed statements of the several paleontologists are attached.

Letters from Survey paleontologists:

In the case of a generic name which has been in long and general usage there seems nothing to be lost and much to be gained by retaining it, even though some one may discover that an older, practically unknown name has priority over it.

I therefore recommend that *Nummulites* and *Lepidocyclina* be given validity by the International Commission. I feel, however, that exceptions should be made only in extreme cases such as the ones here presented.

Signed: L. W. Stephenson.

"I concur in the above statement." T. W. Stanton.

"Concur." Edwin Kirk, C. Wythe Cooke, W. C. Mansfield, Chas. Butts.

"Agreed, both as to making exceptions only in extreme cases and as applied here to *Nummulites* and *Lepidocyclina*." George H. Girty.

I believe that the substitution of *Camerina*, almost entirely unused and unknown, for *Nummulites*, extensively used for over a century, is a useless bit of hair-splitting legal procedure. It will lead to more confusion than clarity. Much the same is true with respect to *Cyclosiphon* and *Lepidocyclina*. I can see no profit whatever in going back into the literature of the dim past to dig up names that have only the legal show of validity and using them to replace widely used and well understood terms [irrelevant personal opinion-C. W. S.]. Let us keep *Nummulites* and *Lepidocyclina*.

Signed: John B. Reeside, Jr., Jan. 25, 1920.

"I agree with the above statement." P. V. Roundy, Feb. 5, 1920.

"Amen and again Amen." Chas. Butts.

In cases in which the confusion arising from the resurrection of an older name is obviously to the disadvantage of the science [relevant testimony-C. W. S.], especially as in the cases under consideration in which no good save the restoration of questionably earned rights to Ehrenberg and Bruguière appear to offset the ill it would do the science, I am opposed to replacing a well known and generally used name by an older one that never attained common usage. Therefore I am in favor of retaining *Lepidocyclina* and *Nummulites*.

Signed: E. O. Ulrich, Jan. 20, 1920.

Letter from Edward Willard Berry, of the Johns Hopkins University, Baltimore, U. S. A.:

I understand that there is pending before the International Commission on Zoological Nomenclature the decision whether to retain the generic use of *Nummulites* and *Lepidocyclina*. I wish to go on record as being in favor of retaining these two genera in the Classification.

The following are expressions of opinion from Australian specialists:

Prof. Walter Howchin, F. G. S.:

I am heartily in accord with you for the retention of the generic names *Nummulites* and *Lepidocyclina*. These names have become so thoroughly incorporated in the literature of the Foraminifera that their substitution would involve serious inconvenience and confusion, priority notwithstanding. I hope that the exceptions you suggest will be agreed to.

W. J. Parr, F. R. M. S.:

I think that the genera *Nummulites* Lamarck and *Lepidocyclina* Gumbel should be retained as *nomina conservanda* in place of the earlier *Camerina* Bruguière and *Cyclosiphon* Ehrenberg.

I am generally opposed to the Suspension of the Rules, but unlike the other foraminifera genera which have been superseded recently, *Lepidocyclina* and *Nummulites* have been much used in general geological literature and a change to the older genera would certainly lead to much confusion which it is desirable to avoid.

Robert A. Keble, F. G. S. Paleontologist :

I am in thorough agreement with the retention of *Nummulites* and *Lepidocyclina*. By doing so the literature becomes intelligible at a glance and unconfused by the rules of nomenclature. Expressed in terms of time saved, such [word omitted] has a true economic value; confusion and uncertainty must obviously accompany a reversion to the strict order of priority.

There remains, then, the question of sentiment. Bruguière and Ehrenberg, the aggrieved authorities, have long passed away, but there is no question of depriving them of their priority. These unselfish pioneers would not have condoned for a moment the waste of time and confusion that would ensue in establishing their presumed right of priority.

Miss Irene Crespin, Paleontologist :

As far as the two genera, *Nummulites* and *Lepidocyclina*, are concerned, I would emphatically support the retention of these names by a suspension of the Rules.

A. C. Collins, student of the Victorian Tertiary Foraminifera :

I should like to express my personal opinion that the generic names *Lepidocyclina* Gumbel and *Nummulites* Lamarck should be retained in preference to earlier names. As these names are so widely used in stratigraphic references, their alteration would, I think, create confusion amongst nonspecialists in the group, and I see no useful purpose to be served [in these cases] by the rigid application of the rules of nomenclature.

Frederick A. Singleton, M. Sc. :

My formal opinion concerning *Nummulites* and *Lepidocyclina* is that both should be placed on the official list of *nomina conservanda*, and it is impossible to reject one and not the other, *Cyclosiphon* having stronger claims than *Camerina*.

The case was submitted to the Commission for informal ballot. The resulting vote stood six (6) for Suspension, four (4) for enforcement of the Rules.

With his informal [affirmative] vote Commissioner Bather transmits the note :

Professor A. Morley Davies, Mr. Heron-Allen, Dr. H. Dighton Thomas, and Mr. A. Wrigley advocate the suspension of the Rules in favor of *Lepidocyclina*. Mr. C. P. Chatwin, on the contrary side, writes: "The question is: do we know what Ehrenberg meant by '= *Nummulites mantelli*'? In my opinion we do." In my opinion, from the evidence of Vaughan and Cushman, we do not. That is just the point in dispute. I may remark that C. D. Sberborn, 1893, "Index

to Foraminifera", quotes "*Cyclosiphon?* Ehrenberg., Abhandl. K. Akad. Wiss. Berlin, 1855, p. 168", and adds "*Orbitoides* fragment, referred elsewhere by Ehrenberg to *O. mantelli*." Obviously this high authority on foraminifera, bibliography, and nomenclature hesitated to accept *Cyclosiphon*.

From a strictly nomenclatural standpoint I agree with the Secretary that this uncertainty has no bearing on the incidence of the Rules; but this only shows how ridiculous adherence to the letter of the law may sometimes be.

It is not clear to me what confusion would be caused by substituting *Cyclosiphon* for *Lepidocyclina*, but I gather that the latter name has long been in general use, whereas no one seems to have used *Cyclosiphon* between Ehrenberg (1856) and Galloway (1928). It is not in the Nomenclators of Bronn, Scudder, or Waterhouse.

With his informal [negative] vote Commissioner Stone sends the statement:

The privilege of asking for a Suspension of the Rules is in danger of being abused. I should advocate it *only* in cases (1) that are so involved that various interpretations are possible or (2) that seriously affect fields and activities outside of pure zoological nomenclature. With too much leniency our whole system will become utterly inconsistent.

The Secretary has corresponded with the following persons, also, who are interested in this case and who approve of a Suspension of the Rules. Most of these workers have read the Summary of this Opinion and have subscribed to it:

R. Wright Barker, Tampico, Mexico; W. S. Cole, Columbus, O.; J. A. Cushman, Sharon, Mass.; A. M. Davies, London; S. Hanzawa, Sendai, Japan; L. G. Heubest, Washington, D. C.; H. K. Hodson, Caripito, Mexico; W. L. F. Nuttall, Cambridge, England; D. K. Palmer, Matanzas, Cuba; H. J. Plummer, Austin, Tex.; G. M. Ponton, Tallahassee, Fla.; L. Ritter, Utrecht, Holland; A. Silvestri, Milan, Italy; G. Stefanini, Pisa, Italy; J. H. F. Umbgrove, Delft, Holland; I. M. van der Vlerk, Leiden, Holland; G. L. Whipple, Puerto Mexico, Mexico; H. Yabe, Sendai, Japan.

The Secretary invites attention to the facts: (1) that the specialists consulted are agreed upon the advisability of Suspension in this case; (2) the case involves geological record, i. e., a coordinate branch of science, and zoologists should be doubly conservative in arriving at conclusions on cases of this type which may have important economic bearings and which have become thoroughly established in paleontological and geological literature.

In view of the foregoing data the Secretary recommends that the Summary given above be adopted as the Opinion of the Commission.

Opinion prepared by Stiles.

Opinion concurred in by thirteen (13) Commissioners: Apstein, Cabrera, Chapman, Fautham, Horvath, Ishikawa, Jordan, Pellegrin, Silvestri, Stejneger, Stiles, Stone, Peters.

Opinion dissented from by no Commissioner.

Not voting: Bolivar, Handlirsch, Richter.

Note: In the case of *Nummulites* eight (8) Commissioners (Apostein, Bather, Chapman, Horvath, Ishikawa, Pellegrin, Silvestri, and Stiles) voted for suspension; four (4) Commissioners (Cabrera, Jordan, Stephenson, and Stone) voted against suspension; not voting, five (5) Commissioners (Bolivar, Handlirsch, Richter, Stejneger, and Warren). Accordingly this case is tabled until the next meeting of the Commission.

OPINION 128

Nycteribia, 1796, PUPIPARA, AND *Spinturnix*, 1826, ACARINE

SUMMARY.—Under Suspension of the Rules *Nycteribia* Latreille, 1796, with *pedicularia* Latreille, 1805, as type, and *Spinturnix* von Heyden, 1826, with *myoti* Kolenati, 1856, as type, are hereby placed in the Official List of Generic Names.

The specific name *vespertilionis* of all authors is hereby invalidated for the following generic names: *Acarus*, *Acrocholia*, *Celeripes*, *Dermanyssus*, *Diplostaspis*, *Gamasus*, *Hippobosca*, *Ichoronyssus*, *Liponyssus*, *Listropoda*, *Megistopoda*, *Nycteribia*, *Pediculus*, *Penicillidia*, *Periglischrus*, *Phthiridium*, *Pteroptus*, *Sarcoptes*, *Spinturnix*, *Strebla*, on the ground that the application of the Rules would produce greater confusion than uniformity.

PRESENTATION OF CASE.—Prof. J. M. Aldrich, United States National Museum, has submitted the following case for consideration:

Latreille proposed the genus *Nycteribia* in "Précis des caractères génériques des Insectes", 1796, p. 176, mentioning only *Pediculus vespertilionis* Linn. In his "Histoire naturelle des Crustacés et des Insectes", vol. 14, p. 403, 1805, he again briefly describes the genus, and gives a partial description of *Nycteribia pedicularia*, new species, which he figures on pl. 112, fig. 14. He places *Pediculus vespertilionis* L. under *pedicularia*, apparently as a synonym.

Now it is a fact mentioned by Speiser, "Ueber die Nycteribiiden", Königsberg, 1901, p. 2, that *Pediculus vespertilionis* L., 1758, is an acarid, and not a nycteribiid in the usual sense of the term.

Latreille in 1796 evidently did not know what *vespertilionis* L. was, since his reference to long tarsi indicates a nycteribiid in the usual sense. His second reference, however, is accompanied by a figure which makes the intention clear.

Up to the present time *Nycteribia* has universally been accepted as a genus of Diptera, suborder Pupipara, and there has been no attempt within a hundred years, as far as I know, to "correct" the nomenclature by transferring the genus to the Acarini. Hence no confusion will arise if the Commission of Nomenclature shall decide upon a Suspension of the Rules in this case, and shall designate *vespertilionis* Latr. 1796 (non Linn.; *pedicularia* Latr. 1805) as type of *Nycteribia*. I request that this be done.

DISCUSSION.—This is probably the most confused case of nomenclature which has ever been submitted to the Commission for study and Opinion, and as such it calls for radical action in order to prevent further confusion.

At the request of the Secretary and under his personal supervision this case has been very carefully studied by one of his assistants, Benjamin J. Collins, M. S., who has summarized the results of his study in Bulletin 155, National Institute of Health, United States Public

Health Service, pp. 743-765, figs. 1-11, 1931. This printed article, a copy of which is mailed to each Commissioner, is hereby included as a portion of the Discussion.

The chief points at issue are the following :

1. *Pediculus vespertilionis* Linn., 1758a, 611, was described as a hexapod, namely, genus *Pediculus*, but the most definite part of the original is the inclusion of a bibliographic citation of an illustration or figure of the "Fledermauss-Lauss" of Frisch, 1728; this illustration is clearly that of an octopod. It seems highly probable that Linnaeus actually had in mind a hexapod in addition to this octopod of Frisch, and for purposes of nomenclatorial argument this is adopted as premise.

2. Scopoli, 1763, interpreted *Pediculus vespertilionis* as an octopod and transferred the species to *Acarus*. This view was adopted by Linnaeus, 1767.

3. Latreille, 1796, proposed a hexapod genus *Nycteribia*, with monotype "*Acarus vespertilionis* Linn. Fab. *Pediculus* Linn." In 1805 Latreille proposed for *Nycteribia vespertilionis* a new specific name, *Nycteribia pedicularia*, thus accepting the premise that Latreille's 1796 specimens of *Nycteribia* belonged to the Insecta, *sensu restricto*. The species *pedicularia* is objective synonym of the hexapod *vespertilionis* as of Latreille, 1796.

In 1826 von Heyden proposed *Spinturnix* as a new genus in the Acarines, with type by original designation "*Acarus vespertilionis* Scop. (non Lin.)", i. e., *vespertilionis* Linn. of Scopoli as restricted to the acarines in 1763, not the hexapod *vespertilionis* Linn. as of Latr., 1796a, which under Art. 31, International Rules, is a dead name.

Nycteribia vespertilionis remained with the insects for more than a century, but in 1902 Oudemans transferred *Pediculus vespertilionis* (namely the type species of *Nycteribia*) to *Spinturnix* (an acarine).

4. Under a strict interpretation of the Rules as applied to the foregoing premises the insect genus *Nycteribia* is based on an erroneously determined species, since *vespertilionis*, a compound species of 1758, was definitely assigned to the Acarines in 1763.

The question now arises whether *Nycteribia* should not be transferred to the Acarines, since its type species (*vespertilionis*) is an Acarine, or whether *Nycteribia* should be left in the insects on the ground that Latreille's specimens were insects. This brings up a controversial point which has produced great confusion in zoology and which is open to different interpretations. The most practical method of settling these cases is by Suspension of the Rules, the decision in each case being made upon the merits of the individual case.

From 1796 down to date the specific name *vespertilionis* combined with *Spinturnix*, *Nycteribia*, and allied generic names presents such extreme confusion in synonymy that tables of subjective synonyms are difficult to understand.

5. We have before us a practical problem to settle. If attempts be made to work this case out on theoretical grounds an agreement is hopeless. The only practical solution the Secretary sees is to settle the case under Suspension of the Rules, holding in mind the preservation of that portion of the nomenclature which is practically universally accepted and eliminating from all further consideration that portion which is hopelessly confused in subjective interpretations.

The proof sheets of Mr. Collins' study were laid before the International Commission in its meeting in Padua, and the Commission adopted the following in the minutes of its meeting for August 30, 1930:

The case of *Nycteribia* vs. *Spinturnix* was discussed on basis of galley proof by Collins (Washington) and the Secretary was instructed to prepare an Opinion in favor of Suspension of the Rules.

In harmony with the foregoing instructions from the Commission the Secretary submits this Opinion and recommends the adoption of the Summary given above as the Opinion of the Commission.

Opinion prepared by Stiles.

Opinion concurred in by eleven (11) Commissioners: Apstein, Bather, Cabrera, Chapman, Horvath, Ishikawa, K. Jordan, Silvestri, Stephenson, Stiles, Stone.

Opinion dissented from by no Commissioner.

Not voting, seven (7) Commissioners: Bolivar, Handlirsch, D. S. Jordan, Pellegrin, Richter, Stejneger, Warren.

OPINION 129

Bipinnaria 1835 vs. *Luidia* 1839

SUMMARY.—The rules are herewith suspended in the case of *Bipinnaria* 1835 vs. *Luidia* 1839, on the ground that “the strict application of the Règles will clearly result in greater confusion than uniformity.” *Luidia* Forbes, 1839, with monotype *fragilissima* 1839 (subjective synonym of *Luidia ciliaris* 1837), is hereby placed in the Official List of Generic Names. The names *Auricularia*, *Bipinnaria*, *Brachiolaria*, and *Pluteus* are hereby excluded from availability as generic names and are reserved as designations of developmental stages.

STATEMENT OF CASE.—Mortensen submits his argument in “Annals and Magazine of Natural History”, vol. 10, pp. 350-351, Oct. 1932, and his presentation is herewith made a part of this Opinion.

DISCUSSION.—Article 37*b*, quoted by Dr. Mortensen, has an interesting history.

The original draft of the International Rules provided an exception to the Law of Priority for certain animals undergoing metamorphoses and change of host, and this exception was included in the rules as adopted by the Moscow Congress in 1892. This same provision was retained in the draft prepared for the Cambridge Congress in 1897. In the 1901 Meeting in Berlin, Commissioners Blanchard and Stiles argued for the retention of this exception, but were overwhelmingly defeated in the final vote and they conceded the point for the sake of harmony.

The parasitic worms, particularly Trematoda and Cestoda, were the first groups to accommodate themselves to the Berlin decision in so far as generic names are concerned; although many specific names are involved, fortunately few generic names come into consideration.

The case of *Bipinnaria* vs. *Luidia* is the first one to come before the Commission for Opinion. The essential data, as made out by the Secretary on basis of Mortensen, 1932, and Sherborn's *Index* are as follows:

Bipinnaria Sars, 1835, Beskr. Bergenske, Kyst Dry, p. 37 monotype *asterigera* Sars, 1835, *ibid.*, p. 37.

Luidia Forbes, 1839, Mem. Wernerian Soc., no. 8, p. 123, monotype *fragilissima* Forbes, 1839, *idem*, p. 123.

Bipinnaria asterigera has been identified as the larval stage of, and therefore a subjective synonym of, *Luidia sarsi*.

Luidia fragilissima has been identified as a subjective synonym of *Luidia ciliaris* (Philippi, 1837, [*Asterias*]) Gray, 1840. p. 183.

Accordingly, *Luidia* 1839 becomes a subjective synonym of *Bipinnaria* 1835 and the name of the larval stage becomes the name of the genus. Further,

Luidia sarsi is an adult stage. Furthermore, *Bipinnaria asterigera* 1835, the name of a larval stage, becomes the name of the species now known as *Luidia sarsi*, since the latter is a subjective synonym of the former.

The effect is that a larval form (*asterigera*), in which various organs important for classification are not yet developed, becomes the type of a genus, in connection with which it is essential to know these undeveloped organs in order to determine the genus and to classify the species, and we have not even the benefit in this case of objective synonyms but only subjective synonyms. Accordingly, the case is much stronger than one would first assume from Dr. Mortensen's presentation.

Furthermore also, in the echinoderms are recognized various larval stages, *Auricularia*, *Bipinnaria*, *Brachiolaria*, *Pluteus*, the names of which have become current in general zoology and embryology. To grant to these names the availability as generic names is to assume the risk of confusion (to an extent which cannot possibly be foreseen) in the nomenclature of the echinoderms in systematic zoology and in geology as influenced by paleontology. Here again the case is much stronger than one might assume from a casual study of Dr. Mortensen's presentation.

The Secretary recommends that the Commission adopt as its Opinion the Summary given above.

Opinion prepared by Stiles.

Opinion concurred in by thirteen (13) Commissioners: Apstein, Bather, Chapman, Fantham, Horvath, Ishikawa, Peters, K. Jordan, Richter, Silvestri, Stejneger, Stiles, Stone.

Opinion dissented from by no Commissioner.

Not voting, five (5) Commissioners: Bolivar, Cabrera, Handlirsch, Pellegrin, Stephenson.

OPINION 130

Lytoceras SUSS, 1865, PLACED IN THE OFFICIAL LIST OF GENERIC NAMES

SUMMARY.—Under Suspension of the Rules *Lytoceras* Suess, 1865 (genotype, *Ammonites fimbriatus* Sowerby) is hereby placed in the Official List of Generic Names.

STATEMENT OF CASE.—The following cases have been submitted by Dr. L. F. Spath:

Ophiceras was proposed by E. Suess in June, 1865, (Anzeiger K. Akad. Wiss. Wien, p. 112) for the "fimbriati" (i. e., group of *Ammonites fimbriatus* Sowerby) but was afterwards thought to clash with *Ophioceras* Barrande (May 1865, in explanation to plates, = *Ophidioceras* Barr., in text, 1867) and was replaced later in 1865 by *Lytoceras* Suess (Sitz. B. Akad. Wiss. Wien, vol. 52, p. 78). This last has ever since been in universal use.

A second *Ophiceras* was proposed in 1880 (Griesbach, Rec. Geol. Surv. India, vol. 13, p. 109) for a Triassic group of ammonites, and (Suess' original *Ophiceras* being forgotten) it has now also become universally accepted.

The resuscitation of the original *Ophiceras* according to the Rules of Nomenclature would cause great paleontological confusion. *Lytoceras* and the family Lytoceratidae are now given in every textbook, *Lytoceras* being one of the two fundamental ammonite genera, persisting from the base of the Lias to the Upper Cretaceous. *Ophiceras*, also recorded in most textbooks, is Lower Triassic in age, so that from stratigraphical considerations, also, it would be advisable to secure stabilization of the present use of these two genera by the International Commission as follows:

Genus *Lytoceras* Suess, 1865 (genotype: *Ammonites fimbriatus* Sowerby; Min. Conchol., vol. 2, pl. 164, 1817).

Genus *Ophiceras* Griesbach, 1880 (genotype: *O. tibeticum* Griesbach, 1880, p. 109, pl. 3, fig. 4).

DISCUSSION.—These cases were referred to Commissioner Bather for special study. He reported upon them as follows:

I have gone into this case carefully and consider it to be eminently one where adherence to the rules would produce nothing but confusion. I therefore recommend as the Opinion of the Commission: That, to prevent confusion, the law of priority be suspended as regards *Lytoceras* Suess, 1865 (genotype, *Ammonites fimbriatus* Sowerby) and *Ophiceras* Griesbach, 1880 (genotype, *O. tibeticum* Griesbach) and that these two names be added to the Official List of Generic Names.

The documents in question were then submitted to Dr. B. B. Woodward, and to the following Museums: United States National Museum, Washington, D. C.; Senckenbergische Naturforschende Gesell-

schaft, Frankfurt a.M.; Zoological Museum, Berlin, Germany; Natural History Museum, Vienna; Musée nationale d'Histoire naturelle, Paris; Zoological Museum, Copenhagen; Field Museum, Chicago, U. S. A.; American Museum of Natural History, New York City, U. S. A.; and to the United States Geological Survey.

The experts consulted have reported as follows:

Paul Bartsch of the United States National Museum:

While I do not favor exceptions to the Law of Priority, this case appears to be one in which abiding by the rules would produce greater confusion than the suspending thereof. I therefore favor Doctor Bather's opinion.

W. C. Mendenhall, Geological Survey, Washington:

The proposition now before the International Commission on Zoological Nomenclature to suspend the Law of Priority in the case of two generic names of ammonites, *Lytoceras* and *Ophiceras*, has been considered by the paleontologists of the Geological Survey now in Washington who are concerned with zoological names—

C. Wythe Cooke, George H. Girty, W. C. Mansfield, J. B. Reeside, Jr., P. V. Roundy, T. W. Stanton, and L. W. Stephenson state:

That they concur in the recommendation of Dr. F. A. Bather that the two names *Lytoceras* Suess and *Ophiceras* Griesbach should be added to the list of "nomina conservanda" under suspension of the Law of Priority.

Edwin Kirk joins in this recommendation so far as *Lytoceras* is concerned but thinks that the retention of Griesbach's *Ophiceras* would be unfortunate because Suess' prior use of that name has been noted by Marshall in 1873 and by subsequent bibliographers.

R. Spärck of the Universitetets Zoologiske Museum, Copenhagen:

I absolutely recommend the proposition to suspend the Law of Priority in the case of the two above mentioned generic names. Dr. Ravn, Head of the Department of Paleontology, joins the recommendation so far as *Lytoceras* is concerned, but is of the opinion that the retention of Griesbach's *Ophiceras* would be unfortunate.

Rudolf Richter, Senckenbergische Naturforschende Gesellschaft, Frankfurt a.M.:

Suspension der Regeln soll eine sehr seltene Ausnahme bleiben, weil die häufigere Anwendung dieses Rechtes zu schlimmen Folgen für die Nomenklatur führen würde.

Im Falle von *Lytoceras* Suess und *Ophiceras* Griesbach ist aber *Suspension* das allein Richtige.

B. B. Woodward, London:

I am of opinion that *Lytoceras* should be placed with "nomina conservanda", but that *Ophiceras* Griesbach, 1880, should not be accepted, Suess' earlier name having passed into literature.

There is unanimity of opinion regarding *Lytoceras* among the experts consulted, and an overwhelming affirmative majority in regard to *Ophiceras*. In view of the foregoing data the Secretary recommends the adoption of the Summary given above as the Opinion of the Commission.

Opinion prepared by Bather and Stiles.

Vote on *Lytoceras*:

Opinion concurred in by thirteen (13) Commissioners: Apstein, Bather, Cabrera, Chapman, Horvath, Ishikawa, K. Jordan, Pellegrin, Richter, Silvestri, Stiles, Stone, Stephenson.

Opinion dissented from by no Commissioner.

Not voting, six (6) Commissioners: Bolivar, Fantham, Handlirsch, Peters, Stejneger, Warren.

Vote on *Ophiceras*:

Opinion concurred in by ten (10) Commissioners: Apstein, Bather, Chapman, Horvath, Ishikawa, K. Jordan, Pellegrin, Richter, Stiles, Stephenson.

Opinion dissented from by three (3) Commissioners: Cabrera, Silvestri, Stone.

Not voting, six (6) Commissioners: Bolivar, Fantham, Handlirsch, Peters, Stejneger, Warren.

Accordingly, *Lytoceras* is placed in the Official List of Generic Names and the case of *Ophiceras* is tabled until the next meeting of the Commission.

OPINION 131

THE TYPE SPECIES OF *Tromikosoma* MORTENSEN, 1903

SUMMARY.—The type species of *Tromikosoma* is *T. koehleri*.

PRESENTATION OF CASE.—Dr. Mortensen, of Copenhagen, has presented the following case for Opinion:

Pomel, in his paper "Classification méthodique et Genera des Echinides vivants et fossiles", 1883, p. 108, established a genus *Echinosophoma*, citing the species *Phormosophoma uranus* A. Agassiz and *Phormosophoma tenuis* A. Agassiz as belonging to that genus without designating any of them as the genotype.

In my work "Echinoidea I. The Danish Ingolf Expedition", vol. 4, no. 1, p. 62, 1903, I adopted the said genus of Pomel, referring to it the same two species as did Pomel, but no genotype was designated. In this same work I established the genus *Tromikosoma*, with the single species *Tromikosoma koehleri* n. sp., which is accordingly the genotype of that genus.

A. Agassiz and H. L. Clark, in their work "Hawaiian and other Pacific Echini. The Echinothuridae" (Mem. Mus. Comp. Zool., vol. 34, no. 3, p. 160, 1909) designate *Phormosophoma tenue* A. Agassiz as the genotype of *Echinosophoma*, which is made to include also my genus *Tromikosoma*—which I agree to be correct.

The name *Echinosophoma*, however, was preoccupied, no less than three times: by Audinet-Serville, 1839, for an earwig; by Wollaston, 1854, for a beetle; and by Semper, 1868, for a Holothurian. Accordingly, it cannot be used for the echinoids, and the name *Tromikosoma* must take its place.

Which species is now to be the genotype of *Tromikosoma*, *Phormosophoma tenue* A. Agassiz or *Tromikosoma koehleri* Mrtsn.?

I would think the latter ought to remain the genotype of *Tromikosoma* also in its extended sense. But the matter does not seem to me quite clear, so it would seem better to have the Commission give its Opinion about the case, and to give it a more general form. I may then put the question thus: When an older genus proves to be a synonym of a later genus, which species is then to be regarded as the genotype, that of the older or that of the later genus?

DISCUSSION.—This case was submitted to the Commission in Circular Letter No. 252 for informal expression of Opinion and informal vote. As a result the following ten Commissioners registered their view that the species *koehleri* is the correct type: Apstein, Bather, Chapman, Fantham, K. Jordan, Peters, Richter, Silvestri, Stiles and Stone. The following two Commissioners view the species *tenue* as the type: Ishikawa and Pellegrin.

The informal votes were accompanied by the following views:

Chapman remarks: "*Tromikosoma koehleri* is monotypic and founded by Mortensen in 1903. Therefore that species has priority [as genotype-C. W. S.] over *tenue* (*Phormosophoma*) selected by Agassiz and Clark in 1909."

Ishikawa remarks: "I consider the specific name *tenue* is to be used for the species, even when the generic name was changed. The reason is the older name has the right of priority in the present case when the *kochleri* and *tenue* are used for one and the same species."

K. Jordan remarks: "*Tromikosoma* has absorbed an older generic concept which has no valid generic name. The genotype of *Tromikosoma* thus extended remains the same as before, *T. kochleri*. *Tromikosoma* was not proposed as a substitute for the preoccupied name *Echinosoma*."

Peters remarks: "*kochleri* is the type of *Tromikosoma* by monotypy; it was not one of the originally included species of *Echinosoma* and is of course excluded from consideration in determining the type of the latter genus."

Richter remarks, "*kochleri* ist der Typus von *Tromikosoma* Mortensen, 1903; *tenue* ist nicht der Typus von *Tromikosoma* Mort., 1903, sondern von *Echinosoma* Pomel, 1833;—gleichgültig, welches das Verhältnis der Genus-Namen *Tromikosoma* und *Echinosoma* zu einander ist. Da die Gattung *Tromikosoma* Mortensen, 1903, bei ihrer Aufstellung *monotypisch* war, ist *kochleri* ihr Typus. *Daran ändert sich nichts* durch die Frage, ob der Name *Tromikosoma* (unabänderlich mit dem Genotypus *kochleri* verbunden) an die Stelle eines anderen Gattung-Namens zu treten hat (z. B. an die Stelle von *Echinosoma* mit dem Genotypus *tenue*) oder nicht. In Übereinstimmung mit meinem Kollegen Dr. R. Mertens."

Silvestri remarks: "because the genus author designated that newer and not another."

Stiles remarks: "*kochleri* is the type species of *Tromikosoma*, and this point is not influenced by any restriction or by any broadening of the generic concept."

Stone remarks: "When two genera are united, such action in no way affects the type of either. The broader genus thus formed will take the oldest available name based on any included species, as its name; and such name retains the type previously established as its type."

On basis of the foregoing informal vote and the arguments presented, the Commission adopts as its Opinion the following: The type species of *Tromikosoma* is *T. kochleri*.

Opinion prepared by Stiles.

Opinion concurred in by twelve (12) Commissioners: Apstein, Bather, Cabrera, Chapman, Esaki, Fantham, K. Jordan, Peters, Richter, Silvestri, Stiles, Stone.

Opinion dissented from by two (2) Commissioners: Ishikawa, Pellegrin.

Not voting, six (6) Commissioners: Boliver, Handlirsch, Horvath, Stejneger, Stephenson (successor Calman).

Cabrera adds:

This case is clear. *Tromikosoma* being a monotypic genus, its single species, *kochleri*, is the type without any shadow of doubt. The question if *kochleri* is or is not the same species as *tenue*, is quite a different point, and one to be discussed, not by the Nomenclature Commission, but by echinodermatologists.

OPINION 132

STATUS OF THE "GATTUNGSBEZEICHNUNGEN" OF SOBOLEW, 1914

SUMMARY.—The "Gattungsbezeichnungen" published by Sobolew, 1914, are of the same nature as the designations published by Herrera; namely, formulae, not generic names, and have no status in Nomenclature. See Opinion 72.

PRESENTATION OF THE CASE.—Prof. O. H. Schindewolf of the Preuss. Geol. Landesanstalt, Berlin, Germany, presents the following case for Opinion:

Die Nomenklaturkommission bitte ich ergebenst um einen *Beschluss*, der die 1914 von D. Sobolew in seiner Publikation "Skizzen zur Phylogenie der Goniatiten" (Mitt. d. Warschauer polytechn. Inst., Warschau, 1914) eingeführten zahlreichen neuen "Gattungsbezeichnungen" für nomenklatorisch ungültig erklärt.

Sobolew ist zwar Anhänger der binären Nomenklatur, steht aber insofern nicht auf dem Boden der Nomenklaturregeln, als er alle früher gegebenen Gattungsnamen verwirft und durch "rationelle" Namen, d. h. Formeln für Merkmalskombinationen, ersetzt. Zur Kennzeichnung seiner Methode zitiere ich aus seiner Schrift die folgenden Sätze (pp. 136-137):

"Statt der 'Gattungs'-Namen werden Benennungen eingeführt, welche das Entwicklungsstadium der Suture und die Gruppe und Reihe, zu denen die Kombination gehört, angeben. Das wird auf folgende Weise gemacht.

Auf dem *Simplicissimi*-Stadium stehende Goniatiten werden

Protomero-ceras genannt

Auf dem *Simplices*-Stadium stehende Goniatiten werden

Monomero-ceras genannt

Auf dem *Duplices*-Stadium stehende Goniatiten werden

Dimero-ceras genannt

Auf dem *Multiplices*-Stadium stehende Goniatiten werden

Pliomero-ceras genannt.

Eine entsprechende Vorsilbe am Anfang jedes Namens wird die Gruppe anzeigen, zu der die Kombination gehört. Gomi-monomero-ceras (= Tornoceras p. p. auct.); Goma-monomero-ceras (= Tornoceras p. p. auct.); Oma-monomero-ceras (= Cheiloceras Frech + Prionoceras Hyatt + ? Aganides P. Fischer). Auf dieselbe Weise kann am *Duplices*- (und *Multiplices*-) Stadium die isomere Reihe bezeichnet werden: α -Oma-dimero-ceras (= Praeglyphioceras Wedek. + Glyphioceras p. p. Hyatt + Gastrioceras p. p. Hyatt); β -Oma-dimero-ceras (= Sporadoceras Hyatt); γ -Oma-dimero-ceras (= Dimero-ceras Hyatt); β -Goma-dimero-ceras (= Maenoceras Hyatt); α -Omi-dimero-ceras (= Manticoceras p. p. auct., Crickites Wedek.); α -Gomi-dimero-ceras (= Gephyroceras Hyatt. em. Holzapf.); γ -Gomi-dimero-ceras (= Tornoceras p. p. auct. + Posttornoceras Wedek.),"

Es ist klar, dass alle die oben genannten neuen Namen ungültig sind und in die Synonymik der in Klammern aufgeführten alten Gattungen fallen. Ich halte es indessen für empfehlenswert, die *sämtlichen* von Sobolew eingeführten Namen als nomenklatorisch nicht existierend zu erklären, da der Autor den Boden des Prioritätsprinzips verlassen hat und seine Bezeichnungen keine Gattungsnamen im Sinne der Nomenklaturregeln sind. Ein solcher Beschluss bringt den Vorteil, dass in Zukunft die Listen der Synonyma von den wertlosen Namen Sobolews entlastet werden und dass ferner langwierige Untersuchungen fortfallen, ob für eine später als neu erkannte Gattung etwa einer von Sobolews Namen verfügbar ist.

DISCUSSION.—This case was submitted to the Commission in Circular Letter No. 249. Reports from Commissioners were submitted in Circular Letter No. 292, No. 312, and No. 320.

Jordan reports :

Die von Sobolew veröffentlichten "Namen" für Goniatiten sind durch Opinion 72 (Herrera) erledigt. Rhumbler legte ein ähnliches Verfahren der Sektion für Nomenklatur in Graz vor.

Peters reports :

It seems to me that Sobolew's "names" are not generic names in the sense of the spirit of the Rules. In my opinion they are practically formulae and as such have no standing or availability. I think they can be declared invalid on the basis of Opinion No. 72.

Richter reports :

Die von Sobolew eingeführten Bezeichnungen sind keine Gattungsnamen, sondern Definitionen einer wissenschaftlichen Auffassung. Da sie somit dem Wechsel der Auffassung unterworfen sind, kommen sie für die Nomenklatur nicht in Betracht. Vgl. auch Opinion 72. In Übereinstimmung mit Dr. Rob. Mertens.

Stiles reports :

On basis of the premises presented, I interpret these designations under Opinion 72.

On basis of the premises presented by Professor Schindewolf, the Commission adopts the following Opinion: The Gattungsbezeichnungen published by Sobolew, 1914, are of the same nature as the designations published by Herrera; namely, formulae, not generic names, and have no status in Nomenclature. See Opinion 72.

Opinion concurred in by ten (10) Commissioners: Cabrera, Esaki, Fantham, K. Jordan, Peters, Richter, Silvestri, Stejneger, Stiles, Stone.

Opinion dissented from by no Commissioner.

Not yet voting, six (6) Commissioners: Apstein, Bolivar, Calman, Hemming, Horvath, Pellegrin.

OPINION 133

Urothoe DANA AND PHOXOCEPHALIDAE SARS

SUMMARY.—Under the Rules, the type of *Urothoe* is *U. rostratus*. The original author of a family name is free to select any contained genus as the nomenclatorial type of that family. It is not necessary to select the oldest included genus as type genus for the family. Under the present premises it is unnecessary to substitute the newer name Urothoidae 1932 for the earlier Phoxocephalidae.

PRESENTATION OF CASE.—Dr. Jean M. Pirlot of the University of Liéges requests an Opinion on certain points of nomenclature which he has raised on pages 61-62 in an article¹ published in February 1932, involving the generic name *Urothoe* Dana, 1852 and 1853, vs. *Pontharpinia* Stebbing, 1897, and the family name Phoxocephalidae vs. Urothoidae.

DISCUSSION.—I. Type of *Urothoe*. Dana (1852, p. 311²) in an extensive key summary, down to and including genera, describes *Urothoe* Dana, with generic diagnosis but without mention of any species. This appears to be the original publication of the generic name.

The following year, Dana (1853, p. 921³) discusses *Urothoe* and cites two species (*U. rostratus* [which is given unconditionally] and *U. irrostratus* [which is clearly given sub judice⁴]). This is apparently the first allocation of any species to this genus.

Under Article 30cβ⁵ of the Rules, *U. irrostratus* is excluded as type, and *U. rostratus* automatically becomes type regardless of the fact whether one dates the genus from 1852 or 1853. Compare Opin-

¹ Les Amphipodes de l'Expédition du Siboga, deuxième partie. Les Amphipodes Gammarides: I. Les Amphipodes fouisseurs, Phoxocephalidae, Oedicerotidae. Leide.

² On the classification of the Crustacea Choristopoda, Amer. Journ. Sci., ser. 2, vol. 14, no. 41, Sept.

³ U. S. Expl. Exped., vol. 13, pp. 920-923.

⁴ "The occurrence of the individuals of this species with the preceding leads us to suspect that the two may be male and female. Yet the great difference in the front is not like any sexual difference noticed; moreover, the superior antennae differ much."

⁵ e. The following species are excluded from consideration in determining the types of genera.

β. Species which were *species inquirendae* from the standpoint of the author at the time of its publication.

ions 35 and 46. For determination of this point it is not necessary to follow the literature further and the fact that *U. irrostratus* has been used as type by some authors is irrelevant as the case now stands.

2. Family name. A complication has arisen because of the fact that *U. irrostratus* has been used as type⁶ of *Urothoe*.

Stebbing (1906, *Das Tierreich*, vol. 21, p. 131) retains *U. irrostratus* in *Urothoe*, family Haustoriidae, and classifies (idem., p. 146) *U. rostratus* in *Pontharpinia* Stebbing, 1897, mt. *pinquis*, family Phoxocephalidae. Thus a typical "transfer case" is presented.

Pirlot raises an important question in regard to Phoxocephalidae, namely:

1. Must the oldest included generic name be taken as type for the family name? To this, the answer is in the negative.

Article 4 of the Rules reads: "The name of a family is formed by adding the ending *idae*, the name of a subfamily by adding *inae*, to the stem of the name of its type genus."

This rule does not prescribe how the type genus of a family is to be selected; and in the absence of restrictions covering this point it is to be assumed that, in accordance with custom, the original author is free to select as type genus any generic unit which he prefers. This is in harmony with the spirit of Article 30 which obviously leaves an original author of a genus entirely free to select as type species any species he wishes thus to designate. If the original author of a family (or of a genus) were compelled to select as type the oldest genus (or the oldest species) in the proposed family (or genus), this might confine his choice to a little known and very rare taxonomic unit—a restriction which would obviously be contrary to the interest both of taxonomy and of nomenclature. In this connection it is to be recalled that the "type" selected is the nomenclatorial type as distinguished from the assumed anatomical norm.

Since (with the exception of isolated instances by early authors) family names are based upon the name of the respective type genus, such family name constitutes, *ipso facto*, a definite designation of the type genus. For instance, *Musca* is definitely and unambiguously designated generic type by the use of the family Muscidae, *Homo* of Hominidae, *Ascaris* of Ascaridae, etc. It would be a nomenclatorial *reductio ad absurdum* to consider any other genus as type of any of these families. The concepts of a given family are not identical as adopted by different authors and if the rule obtained that the oldest

⁶ Stebbing, 1891, on the genus *Urothoe* [etc.], *Trans. Zool. Soc. London*, vol. 13, no. 1, p. 10: "This, which has become the type species of this genus."

genus must be the type genus of the family, the family name would be constantly subject to possible change according to the subjective ideas of authors from year to year; accordingly, even relatively stable nomenclature for family names would be hopeless, and synonymy in family names would be potentially indefinite and chaotic.

Accordingly, if *Urothoc*, type *rostratus*, is classified in Phoxocephalidae Sars it is not necessary to change this earlier family name to the later Urothoidae 1932.

In formulating this Opinion, the Commission has considered only the question of the formal application of the Rules and has not considered the question whether it would be wise to "Suspend the Rules" in this case. The data on which this latter question should be judged have not yet been placed before the Commission in sufficient detail.

In view of the foregoing premises the Secretary recommends the adoption of the following as the Opinion of the Commission:

Under the Rules, the type of *Urothoc* is *U. rostratus*. The original author of a family name is free to select any contained genus as the nomenclatorial type of that family. It is not necessary to select the oldest included genus as type genus for the family. Under the present premises it is unnecessary to substitute the newer name Urothoidae 1932 for the earlier Phoxocephalidae.

One of the points involved in this Opinion was voted upon by the Commission in the meeting at Lisbon, when the following interpretation was adopted:

Article 4 of the Code, which relates to the naming of families and subfamilies, does not require that the oldest generic name in the family or subfamily concerned must be taken as the type genus of the family or subfamily.

This point was concurred in by Commissioners Calman, Hemming, Jordan, Pellegrin, Peters, and Stejneger, and by the following alternates: Amaral *vice* Cabrera, Oshima *vice* Esaki, Chester Bradley *vice* Stone, Beier *vice* Handlirsch, Arndt *vice* Richter, Mortensen *vice* Apstein.

Opinion prepared by Stiles.

Opinion concurred in by seventeen (17) Commissioners (or alternates): Apstein (in part), Beier (in part), Cabrera, Calman, Chapman, Esaki, Fantham, Hemming (in part), Jordan, Oshima (in part), Pellegrin (in part), Peters, Richter, Silvestri, Stejneger, Stiles, Stone.

Opinion dissented from by no Commissioner.

Not voting, two (2) Commissioners: Bolivar and Horvath.

Apstein agrees in so far as concerns *Urothoe* but not in so far as it affects Phoxocephalidae.

Stone adds:

I concur in the Opinion that the first author to fix a type genus for a family is free to select any contained genus as the type, but in case the name then used for that genus is found to be untenable the family name changes in accordance with the change in the generic name.

For example, the American Wood Warblers were named Sylvicolidae by Gray, based on the genus *Sylvicola* (type *Parus americanus* Linn.), but *Sylvicola* was found to be preoccupied in mollusks and as a substitute *Compsothlypis* was proposed, and the family name changes to Compsothlypididae. If this were not done we might have *Sylvicola* for mollusks and Sylvicolidae for Birds!

Sylvestri states:

I agree perfectly with the opinion of Commissioner Stone as expressed in the Circular Letter No. 333 (Series 1936).