

OPINION 126

NEW NAMES IN D'ORBIGNY'S, 1850, "PRODRÔME" 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, "Prodrôme" 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 "Prodrôme 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 "Prodrôme" 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 actacon* d'Orb., Boule writes: "L'échantillon de la collection d'Orbigny est très mauvais; il faut prendre comme type la figure de *M. actacon* 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 tenuicrta* 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. phillis* 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 " Prodrôme ", 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 " Prodrôme."

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. *Hemicidarid 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 *Cidarid 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.