

THE INTERNATIONAL CODE OF ZOOLOGICAL
NOMENCLATURE
RESULT OF VOTE ON PROPOSALS FOR SUBSTANTIVE
AMENDMENTS (FIFTH INSTALMENT). Z.N.(S.)1973

By the Secretary, International Commission on Zoological
Nomenclature

This report presents the result of the Commission's vote on the proposal to admit names proposed for ichnotaxa and parataxa to zoological nomenclature and to provide that such names do not compete in priority with names proposed for animals themselves. These proposals were first published in *Bull. zool. Nom.* vol. 36, pp. 11-14. They formed part of the Commission's report to the Section on Zoological Nomenclature of IUBS at Helsinki (vol. 36, pp. 211-212), part B, paras 1-4. They were approved by the Special Session of the Commission at Stenoffa, by the General Meeting of the Commission at Helsinki; they were also approved by the Section on Zoological Nomenclature and the Division of Zoology at Helsinki.

On 4 July 1980 the members of the Commission were invited to vote under the Three-Month Rule on Voting Paper (1980)18 for or against the proposals presented in the following form:

Code Article	Commission Report to Section on Zoological Nomenclature at Helsinki, 1979, Section B
2 (Sixth Draft)	<p>1. To provide that zoological nomenclature applies to the names of fossils of the work of animals or their traces (but not secretions), even though they have not been related to any organism in the animal kingdom that caused them. The term ichnotaxa is used to describe such entities. Article 1 of the Code provides for fossils of the work of animals and it is implicit in that Article that they must be regarded as representing taxonomic units of animals. Since some such fossils have never been related to the organism that have have caused them the Code should state explicitly that zoological nomenclature applies to their names (<i>Bull. zool. Nom.</i> vol. 36: 11-14).</p> <p>2. To provide that names given specially to ichnotaxa do not compete in priority at genus-</p>
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group level with names given to nominal taxa of recognized organisms in the Animal Kingdom and that names given to ichnotaxa at the level of the genus group be treated as the names of collective groups. Names given specially to ichnotaxa would be treated at genus level in the same manner as collective groups and at any level, notwithstanding Art. 24 b (iii), they must not compete in priority with names given to taxa of the animal that made the work or traces (*Bull. zool. Nom.* vol. 36: 11-14)

3. To provide that zoological nomenclature applies to names given to fossils of fragmentary or detached parts of animals that are classified in artificial taxa as though they were genera and species. The term parataxa is used to describe such entities. At present Article 1 excludes from zoological nomenclature names that are not applied to "taxonomic units of animals known to occur in nature". Since dual nomenclatures exist in practice the matter would be made explicit in the Code (*Bull. zool. Nom.* vol. 36: 11-14).

4. To provide that names given specifically to parataxa do not compete in priority with names given to nominal taxa of recognized organisms in the Animal Kingdom. As in the case of ichnotaxa (2 above), and notwithstanding Art. 24 b (i), the names of parataxa would not compete in priority.

The following background paper was sent out with the voting paper. (Subsequent objections to the proposals concerning parataxa were received from workers in conodonts: Dr Bergström (*Geological Survey of Sweden*), Dr Lennart Jeppson (*University of Lund, Sweden*) and Professor Walter Sweet (*University of Ohio, Columbus, Ohio*) but these were either too late for circulation to the Commission, or presented difficulties that could not be resolved in time.)

V.P.(80)18 – APPENDIX

Background to, and comments on, the proposals

Paragraphs 1 and 2. Ichnotaxa (trace fossils)

EC:	recommended
Stensoffa:	recommended with one contrary vote

The proposals now before the Commission originated in an application by Professor Otto Kraus and the late Professor Walther Häntzschel for the regulation of names given to trace fossils after 1930. This was published, together with a number of related comments, in *Bull. zool. Nom.* vol. 29, pp. 137–141, 1972. Two later comments in support (by Professor E. Voigt and Dr. Ellis Yochelson) and one in opposition (by the late Dr. Lemche) were published in *Bull. zool. Nom.* vol. 30, pp. 69–71. The underlying cause for this application was that names for trace fossils are available under the Code if published before 1931, but not if published after 1930. Thus, in Part W of the Treatise on Invertebrate Paleontology (Second Edition), 1975, roughly 118 generic names are available and about 124 are not, among those treated as 'valid'. Some action is clearly called for to rectify this anomaly, which arises from a decision of the Paris (1948) Congress (*Bull. zool. Nom.* vol. 4, p. 255) to treat a description of the work of an animal as an indication for the purposes of the Code.

The rapid and extensive growth of such geological disciplines as sedimentology and environmental geology has led to a corresponding growth in the study of trace fossils as indicators of environmental conditions and changes in them. Although some of the earliest workers named what they took to be traces of plants, nearly all are now, on good analogical grounds, ascribed to animals (and there is, of course, no intention of applying our Code to organisms now considered not to be animals). There is an undeniable need for a set of scientific names for use in communicating ideas about these trace fossils, and for a set of rules to govern those names.

Until 1972, most workers in this field ignored the Paris ruling referred to and applied the zoological Code in naming trace fossils – with one important reservation: a number of genera of trace fossils was described without any included species, and hence without any originally designated type species (indeed, many genera are without included species to this day). Such names are doubly unavailable.

An important – and, for zoologists, a disturbing – development occurred in 1973. Sarjeant & Kennedy, *Canadian J. Earth Sci.*, vol. 10, pp. 460–475, published a draft code of nomenclature for trace fossils that was modelled closely on the International Code of Botanical Nomenclature (Professor Sarjeant is a palynologist), because that code was found to be more adaptable and receptive than ours. Unfortunately, this draft code proposed to introduce into the nomenclature of elements of the animal kingdom certain nomenclatural concepts that are quite foreign to our Code. Its formal adoption was proposed to the International Union of Geological Sciences at the 25th International Geological Congress,

Sydney, 1976; but the Secretary-General of IUGS assured me that no final decision on it would be taken without consultation and discussion with IUBS.

In the event, the Sarjeant & Kennedy proposals did not find favour among ichnologists as a whole, most of whom continued to treat their objects of study as elements of the animal kingdom. In 1977 I was approached by Dr. Paul Basan, editor of the *Ichnology Newsletter*, with a request for news of progress with the original Kraus and Häntzschel application to the Commission.

At the time all my spare energy was taken up with the work of the Committee on Typification of Species of Protozoa, and it was not until July 1978 that I was able to present the subject to the Editorial Committee, during one of its meetings in London. The results of that meeting, incorporating the matters on which you are now asked to vote, were published in *Bull. zool. Nom.* vol. 36, pp. 11-14, July 1979. Put in simple terms, the Editorial Committee saw no need for a separate code of 'ichnonomenclature'. All that was necessary to adapt our Code to the requirements of ichnologists was to provide (1) that names given to trace fossils as such should be available regardless of date, but that they should not compete in priority with names given to the causative organisms, and (2) that the provisions of Article 13b should not apply to them. These proposals were reported to, and warmly welcomed by, Dr. Basan (*Palaeogeogr. Palaeoclimatol., Palaeoecol.* vol. 28 (1-2), September 1979). (He did not approve either of the publication of the Sarjeant & Kennedy draft code alongside his paper, or of that code itself.)

Meanwhile, in November 1979, I received an enquiry from Dr. Richard Bromley (*Copenhagen University*) on the state of the Commission's examination of this problem. This resulted in a joint paper by him and Dr. F. Fuersich in *Bull. zool. Nom.* vol. 37, pp. 6-10. This paper demonstrated a need for the extension of the proposed provisions to cover traces of extant animals. However, the Special Meeting of the Commission at Stensoffa decided not to recommend inclusion of a provision governing all names based on the work of animals regardless of date. The Editorial Committee's proposals were also welcomed by Professor Anders Martinsson, Chairman of the International Commission on Stratigraphy and of the International Palaeontological Association.

In February 1980 I received a letter from Dr. W. Struve (*Forschungsinstitut Senckenberg, Frankfurt, BRD*). I find it difficult to decide whether this letter really attacks the proposals as strongly as it appears to do, for in some passages it seems to accept them. In fairness to Dr. Struve I reproduce his letter in full, so that members of the Commission may make up their own minds:

'In the course of years, nomenclature apparently estranged so much from its original intention that even a loyalist is going to lose inclination to open the Code book. Therefore, the ideas of enriching the International Code of Zoological Nomenclature by introducing Parataxonomy and Paranomenclature and Ichnotaxonomy and Ichnonomenclature are alarming and point out the short memory and fast-moving time in science. As I remember, about a quarter of a century ago the scientists of this house (among them commissioners resp. ex-commissioners) were relieved that the "para"-ideas had sunk into oblivion and that a schisma breaking up through nomenclature had been avoided. It is deplorable that valuable time has to be detracted from actual research in order to be spent for this matter again.

'In principle, everybody is allowed to classify everything he wants and to mark or name every unit and element of his system as he likes — by pictography, by letters, by numbers or even by actual words or "names". And everybody may publish and distribute these achievements freely, too.

'A large group of scientists has agreed by good reasons to establish a(n artificial) system of order and naming, called the "Linnean" one and being subject to distinct rules.

'The actual problem is now, to prevent the para-taxonomies and -nomenclatures from entering the Linnean System and the Linnean Nomenclature. Once introduced in the Code and becoming reputable, the "paras" would spread like metastases.

'That means that the International Code of Zoological Nomenclature has to stay restricted to all Taxa and Names that are subject to the Linnean System and the Linnean way of Nomenclature as intended by the first author traditionally or maybe expressis verbis in future. It is irrelevant in this connection whether the Linnean System is a natural or artificial one.

'The introduction of a new taxon and a new name must not be made dependent from

- the state of preservation and percentage of body available for research,
- the sex,
- the ontogenetic stage of the respective object, etc.

'Since there are actual differences between recent and fossil documents, non-obedience of those principles will provoke a schisma between zoological and palaeozoological nomenclature.

'My opinion is substantiated as follows:

- (1) Completeness of material: Quite generally, fossil material is incomplete or fragmentary. The possibility of discrimination and determination of fossils does not depend upon percentage of available body or skeleton parts but

from systematic value of the documents which is a matter of knowledge, experience and intuition. The systematics of fossil mammals is based largely on teeth (and exceptionally by more complete dentitions). A substantial group of Carboniferous trilobites can be defined and determined the best and the easiest by means of their genal spines (BRAUCKMANN, 1978), being with about 2% of hard parts much more than a mammal tooth, but the remaining 98% of the carapace being largely the nice carrier of the small decisive "rest".

- (2) Sex: Even in case of complete knowledge about sexes in the respective recent groups it may be difficult to identify males and females among their fossil predecessors. In several important fossil groups there is a discussion but no compelling knowledge about sexual dimorphism (e.g., trilobites, brachiopods).
- (3) Ontogenetic stage: The eminent importance of early ontogenetic stages in taxonomy and systematics of several fossil groups is well known since long, e.g. by the embryonic chambers of ammonoids and dacryoconarids. Many ammonoids are characterized by the details of early coils, others by the properties of late adult to gerontic coils. The extraordinary importance of protaspid and meraspid stages of ontogeny for discrimination and determination of Middle Cambrian trilobite species has been pointed out convincingly by SNAJDR (1958).
- (4) Characters of soft body and of body fluids being so important for several recent animal groups are (with almost no exception) unknown from fossil animals.

'I believe that the unity of zoological and palaeozoological nomenclature is not only desirable but even absolutely necessary. This unity can only be maintained if the decision upon the possibility of discrimination and the reasoning for the introduction of a name is reserved for the sound discernment and the risk of the specialist.

'A special comment seems necessary concerning "ichnotaxa" and nomenclature of those. I see no reason to treat them separate from ordinary taxonomy and nomenclature:

'In palaeontology there is no sharp boundary between "body fossils" and "trace-fossils". The difference is especially spoiled between external casts of body fossils and repose imprints (Cubichnia). As the most famous example, I name the Pre- or Eocambrian *Xenusion auerswaldae* which is included promptly and legitimately as well in "Protarthropoda" (vol. O) as in "Trace Fossils" (vol. W) of the "Treatise on Invertebrate Paleontology"

[but not in the Second Edition, 1975, of Vol.W. R.V.M.]

'This problematics touches especially the exciting chapter of Pre-cambrian life on which both palaeontologists and zoologists will have great interest to discuss and to use one nomenclature only. Special attention is drawn to the large group of so-called "worms" and worm-like trace fossils which in part are documents of highbred "palaeopsychic" efficiency and will allow better definitions of fossils especially by consequent application of computer analysis than does a study of a cast of some kind of segmented or non-segmented hose-like body.

'I believe it is no tragedy if an extinct animal bears different names both for its body and for its manifestation of life, or even if the animal gets the name of its trace because of the law of priority. In my opinion the frequency of (temporary!) different names for body parts, sexes, larvae, ecological forms and traces is generally over-dramatized. Synonymy of this provenience has a share of less than 5%, probably less than 1%; such a small share is praiseworthy of a science as palaeontology displaying an explosive development in exploring extremely difficult and imperfect documents.

'As experience shows, the greatest share of "synonymies" in palaeozoology and in part also in zoology is caused by quite different reasons:

- different scientific approach to taxonomic valuation of species, subspecies and formae;
- overlooking of pre-published literature;
- shifting of meaning of species or subspecies contents by wrong determination.

'Furthermore, the actual dangers for taxonomy and nomenclature threaten from the great inadequateness of efforts to clear up the avalanche of knowledge in a simple, intelligible and reproducible way.

'One important approach towards improvement of this situation will be to cut down the code of nomenclature to simple and clear prescriptions and to avoid any load which might be likely to shunt nomenclature from a handy tool of scientific communication to an end in itself.

'In our institution is striven since many decades to obey the code of nomenclature strictly. However, it becomes more and more difficult to convince scientists and authors of the advantages of an unambiguous nomenclature if such praised qualities vanish under a tangle of confusing and turgid regulations, exceptions, deadlines etc.. It should be emphasized that the Commission bears a high responsibility for enabling all responsible editorial staffs of relevant literature to work economically.

'Therefore, I hope that the present Commission on Zoological Nomenclature will avoid to add new burden to the Code but engrave itself on the annals of nomenclature to have provided zoology and palaeontology with the simplest, clearest and shortest nomenclature ever made.'

Dr. Struve's letter covers both ichnotaxonomy and parataxonomy, considered in the next section of this report. In reply I pointed out that the degree of complexity of the Code merely reflected the complex behaviour of zoologists. I said that it is not a question of preventing parataxonomies and paranomenclatures from entering the Linnean system; they were already there before the 1905 *Règles* were written. Our business is to find a way of regulating these parallel nomenclatures (which reflect parallel taxonomies) without causing chaos.

Dr. Kerzhner (*Academy of Sciences, Leningrad*) also showed a misunderstanding of the purpose of the Editorial Committee's proposals and thought that ichnotaxa and parataxa could be provided for by a single rule. He thought it would be better to proceed case by case, by Opinions, in deciding what fragments or traces of what animals do not compete in priority with names purporting to be based on the animals themselves, and that an Official List of Parataxa should thus be built up. I fear that he grossly underestimates the extra work that this would inflict on the Commission's already overstretched resources.

Dr. Holthuis (July 1979) expressed disquiet at the introduction of proposals concerning ichnotaxa and parataxa after the publication of the Sixth Draft of the Third Edition of the Code. As has been seen, however, those most concerned with ichnotaxa welcomed these proposals; and it will be seen that the volume of comment on parataxa (though revealing widespread misunderstanding) shows that those concerned were well aware of the proposals published in July 1979.

Dr. Holthuis (June 1980) expressed his opposition to the introduction of the concepts of parataxa and ichnotaxa into the Code as follows:

'(1) The Editorial Committee published the proposal to introduce these concepts into the Code as late as July 1979, i.e., more than 1½ years after the draft for the third edition of the Code had been presented for comment to zoologists (in November 1977). Neither concept was mentioned in the draft and the 1979 proposal could not be considered a comment on any part of the draft: it was adding entirely new matter to it. Zoologists who bought the draft (both before and after July 1979) were not automatically informed that new fundamental additions to the Code had been proposed, even less what these additions amounted to.

Only those, unfortunately very few, zoologists who regularly consulted the Bulletin of Zoological Nomenclature would know of them. Although the procedure adopted in handling this matter is fully legal, it might give the impression of not being quite ethical, the more so as, in order to enable the insertion of the new concepts into the new Code by 1980, the whole procedure was rushed and voting is taking place after the shortest possible interval. That interval in this very fundamental issue is far shorter than those that as a rule occur in the most simple applications to the Commission.

'(2) The term parataxa, although not mentioned in the circulated draft of the new Code, was not new to nomenclature. In the well-known (1957-1958) Bradley draft of the first edition of the present Code, the introduction of this term had also been proposed. However, after long (and often heated) discussions during the Colloquium on Nomenclature of the XV International Congress of Zoology at London (1958), at which numerous zoologists were present, the inclusion of the concept parataxa into the Code was rejected with a substantial majority. The Commission should avoid that zoologists will get the impression that an effort is made now to get this item, which was definitely rejected in 1958, into the Code through a back door, something which in no way can be the intention of the Editorial Committee or the Commission.

'(3) The introduction of a special nomenclature for parataxa and one for ichnotaxa, together with the existing one for collective groups, makes for three nomenclatures parallel with the normal rules. Comparing the three, one finds that nomenclaturally the generic names on the parataxa-, ichnotaxa- and collective group-levels are fully identical: in all three cases the generic names are to be treated as normal generic names, except for the fact that they do not compete for Priority with other generic names. While in ichnotaxa and collective groups the specific names are not different at all from normal specific names, in parataxa the specific names do not compete for Priority with other specific names. This means that the generic names of all three categories and the specific names of parataxa are provisional names that can be used as long as the life stage has not been identified with the adult stage (in collective groups), the trace has not been linked to the causative organism (in ichnotaxa), or the assemblage of parts has not been linked to the complete animal (in parataxa). As soon as the links are established the provisional names have to give way to the names of the adult, causative or complete organisms. It would be sufficient to use a single term for this type of provisional name (e.g., collective genus name and collective species name), regardless of whether it

is used for (1) a stage in the life cycle, (2) a trace, (3) an assemblage of parts that cannot (or not yet) be definitely assigned to a natural taxon, or (4) any other artificial taxon. To decide whether a certain taxon needs a provisional name is the task of the taxonomist, not that of the Code. According to the Preamble the Code may not "restrict the freedom of taxonomic thought or action", it can allow the use of such provisional names and define them nomenclaturally.

'(4) The present problem is too complicated to be decided upon without a more careful consideration of its various aspects, and a decision at this moment seems ill-justified.'

Dr. Holthuis's final objection may be answered as follows: it is not at all the purpose of the Editorial Committee's proposals to decide whether a certain taxon is to be given a name in one nomenclature or another; once a taxonomist has taken the relevant decision, it is for the Code to provide an orderly means whereby he can communicate that decision within the framework of the Code. It is, moreover, unrealistic to suppose that names given to ichnotaxa and parataxa are provisional. In nearly all cases their permanence is as certain as anything can be in the world of nomenclature.

Paragraphs 3 and 4. Parataxonomy and paranomenclature

The problem that is addressed here is, as already mentioned, the long-continued existence of parallel taxonomies in certain animal groups, reflected in parallel nomenclatures. Whenever a taxon in one of these taxonomies (at species-group, genus-group or family-group level) is matched with one or several taxa in the other taxonomy, Article 24b of the existing Code comes into play: i.e., one of the names must be rejected in favour of the other. This causes no problem when a soundly-based one-to-one match is established between one taxon in one taxonomy and one taxon in the other; the Law of Priority can be applied and, if this causes confusion, recourse may be had to the Commission. But this is a very unusual event, even if it has ever occurred. It is much more common for a single parataxon to match with a number of 'natural' taxa (i.e., taxa based on the animals themselves). If, in such a situation, the parataxon was named first, then *all* the names of the taxa based on the animals themselves become junior synonyms. But when the latter (if species) belong in different genera, or (if genera) in different families, the consequence is nomenclatural chaos. The problem goes further than that, however: the Code, by forcing the name of the parataxon as the valid name of a number of taxonomically separated animal-based taxa, constricts taxonomic freedom and thus exceeds its own authority. These points do not appear to have been made in the earlier discussions of this subject,

but it is hoped that they will show the urgency of the problem.

The following are examples of groups in which parallel taxonomies and nomenclature exist:

Class	Order	Comment
Gastropoda		Fossils are classified only on the shell. Extant forms are classified on the radula, genitalia and operculum as well as the shell. Assigning fossil forms to taxa based on extant types thus entails a measure of speculation.
Cephalopoda	Ammonoidea	Aptychi (whether opercular or radular in function) can be classified in genera and species that do not match the genera and species based on phragmocones. In cases of supposed sexual dimorphism, taxa of presumed females (macroconchs) do not always correspond to taxa of presumed males (microconchs) below generic level.
Holothuroidea		Only detached spicules are known fossil. The genera and species in which these are classified bear no relation to those of the extant forms.
Pisces	Selachii Batoidea	Only the teeth are known fossil. Their classification does not match that of the extant forms, based on body form, number of vertebrae, etc.

These dual taxonomies reflect pragmatic necessities imposed by the nature of the evidence with which zoologists and palaeontologists have to deal (Dr. Struve's 'difficult documents'). The removal of those necessities by improved research is an ideal which can be approached only asymptotically. So long as they persist, the dual taxonomies will persist, and so will the corresponding nomenclatures. In the conodonts, for example, there is a slow movement towards an apparatus-based classification. But conodont workers do not seem to understand that the valid binomen for an apparatus-species must combine the oldest element-based generic name with the oldest element-based epithet, and that this produces names that represent no coherent taxonomic concepts.

The Editorial Committee's proposal — essentially that these parallel nomenclatures should not compete with each other for the

purposes of the Law of Priority — restores to taxonomists the freedom that the rigid application of Article 24b would remove. The decision as to when a parallel taxonomy is a pragmatic necessity in a given group is still left to the scientists concerned, as is the allocation of taxa between those taxonomies and their gradual reduction to one.

Comments on the Committee's proposals have mostly expressed disquiet at the possible introduction of a general licence to set up separate systems of nomenclature for any fragment of an animal. This is, of course, not the Committee's intention. Such disquiet can be set at rest by careful drafting, and the Committee will pay special attention to this.

Those who have expressed disquiet of this sort include Professor G. Hahn, Dr. Heinz Malz (*Forschungsinstitut Senckenberg, Frankfurt, BRD*), Dr. R.W. Huddleston (*Chevron Oil Field Research Co., La Habra, California*) (in press, with my reply, in *Bull. zool. Nom.* vol 37; copies of the proofs are enclosed), Dr. Jan Bergström (*Sveriges Geologiska Undersökning*) and Dr. Lennart Jeppson (*University of Lund, Sweden*). I have replied to all these correspondents explaining the Committee's proposals more fully, and none has sought to prolong the discussion.

The matter is therefore now referred to the Commission for a vote, on the understanding that the final drafting remains a matter for the Editorial Committee.

On 11 September 1980 (when 13 votes had already been returned) Dr C.W. Sabrosky sent the following letter to all members of the Commission:

‘Ichnotaxa and parataxa

‘The proposed rules on these two items have been before us for some time, and you may already have voted. However, recent discussions with paleontologists here in Washington and the receipt of the signed communication from the Pander group (conodont specialists) have convinced me that we have not fully explored the attitude of paleontologists on the subjects. I was astonished at the depth and intensity of opposition, especially to parataxa, and I have concluded that it would be wise not to adopt that proposal at this time but to consider more thoroughly what is involved.

‘I know that Secretary Melville, himself a paleontologist, warmly and strongly supports the proposals. But I find paleontologists of equal repute strongly and even bitterly opposed to them. As a bystander who has no need or use for either parataxa or ichnotaxa, I am puzzled, and I am unwilling to proceed until I am sure that overwhelming or at least majority opinion among paleontologists is in favour of the steps. I would not wish to find the Commission storming the enemy's fortress only to look around and

find only sparse troops behind us.

'It does seem to me that ichnotaxa and parataxa present different situations, and I will discuss them separately.

'Ichnotaxa

'It is clear in the discussion that the real problem of the ichnologists lies in the rule that names based on the work of animals are unavailable after 1930. Remove this, as we have done by removing it from indications, and their major problem is solved. I would treat the genus-group names as collective-group names, as most probably are. However, I would just as soon apply priority at the species-group level. It is rare that there will be any complication.

'I was impressed by the three comments by Martinsson, Teichert and Frey (*Bull. zool. Nom.* vol. 29, pp. 140-141). I therefore approve paragraphs 1 and 2 of V.P.(80)18, except for species-group names being exempted from priority.

'Parataxa

'Re parataxa, I received, both directly and through the Secretary, the statement from the Pander group, as well as telephone calls from Professor Sweet of Ohio State University and Dr Repetski, U.S. Geological Survey. Dr Repetski, at my request, arranged a discussion meeting and I subsequently had long discussions in depth with paleontologists Ellis Yochelson and Porter Kier. I was astonished at the strong feeling against the proposal and the — to me — cogent arguments that were advanced. Most of these were along the lines of Struve and Holthuis (chiefly his point 3). They pooh-poohed the idea of chaos. They believed that approval of dual nomenclature would cause neglect of, and even inhibit, solid zoological studies. Conodonts were used as an example of a group in which advancing knowledge had overtaken earlier and vaguer knowledge and the group is being put on an ever firmer footing, without resort to parataxa. Most hold that individual and unidentifiable parts, such as holothurian spicules or crinoid columnals, should not be named at all, but referred to, if needed, by some other system.

'I believe that the concept and practice of collective groups can also be extended to parataxa. The difference is, of course, that species in the collective groups of parasitologists can be identified, and ultimately their life cycles worked out and the specific name taken into the regular zoological classification, either as a valid name or as a synonym. Presumably this will seldom be possible for the "species" of parataxa, which may have come from various species of animals. But the genera can be considered as collective groups.

'I am not sure that Article 1 can be correctly said to exclude parataxa. It depends how one reads "names given to taxonomic

units of animals known to occur in nature", the animals or the taxa (taxonomic units)? If parataxa are considered artificial taxa that do not occur in nature, then how about collective groups such as *Cercaria*? and how about genera that are regarded as probably polyphyletic? Anyway, are not generic and family names, etc. merely names for our concepts for what occurs in nature? I would rather interpret Article 1 to say that we deal with names of taxa, the taxa being arrangements of *animals that occur in nature*, whether the taxa are artificial or natural. It seems likely that in most cases outside of conodonts, the names would remain as collective groups, but if, as in the conodonts, there is some day a zoological *dénouement* of the problem, then let priority apply. I predict that the problems will be few.

'I am therefore persuaded to vote against parataxa as presently proposed, on the ground that Article 1 already can include them and priority should apply. I would have no objection to adding such phrases in Article 1 as "including parataxa" or "including ichnotaxa" for clarity and precision.'

The communication from the Pander Society mentioned by Dr Sabrosky had already been circulated to all members of the Commission. It read:

'The undersigned members of the Pander Society, the international organisation of conodont workers, representing 25 countries, are meeting in Vienna and Prague from 4 to 9 August 1980. We have carefully considered the text of the subject amendments. This was the first opportunity since their publication to discuss the amendments in an international forum. We unequivocally reject these amendments to the Code and urge you in the strongest possible terms to vote against them.'

'The purpose of formal zoological nomenclature as expressed in the International Code (1961, 1964) is to promote stability and enhance precision of scientific communication. This is done by an almost absolute adherence to the principle of priority. As a nomenclature for parataxa would legalise a dual system of names, paronymenclature is antithetical to the purpose of the Code.'

'Additionally, almost all fossils may be considered parataxa, because preservation of the entire organism is an extremely rare phenomenon. There is a complete gradation from (1) indeterminate isolated skeletal parts (e.g. crinoid stems, certain scolecodont maxillae, certain conodont ramiform elements) to (2) more complete associations (polychaete jaw apparatuses, of which hundreds of articulated specimens have been discovered in the last twenty years, and similar numbers of *in situ* conodont multielement associations on bedding planes and in fused clusters) to (3) preservation of the complete skeleton or shell. In almost all instances, soft

parts are lacking. Thus, for example, a complete ammonoid shell (phragmocone) represents in reality a parataxon. Accordingly, it is extremely difficult if not impossible to differentiate between fossil taxa and parataxa. We are convinced that the adoption of the amendments concerning parataxa would undermine the stability of nomenclature of all fossil animal groups.

'Prior to the mid-1960's conodonts were considered in effect as parataxa (or form taxa), as almost all conodont workers dealt exclusively with isolated skeletal elements. Since that time, however, tremendous progress has been achieved in basing taxonomy on reconstructions of the conodont apparatus, which represents the entire skeleton. In the resulting nomenclature, strict application of the International Code (especially Articles 23 and 24) has been followed. This taxonomy and nomenclature is now being used in the Treatise on Invertebrate Paleontology (1980, in press), in the Catalogue of Conodonts (vols 1 to 3, 1973-1977; vol. 4 is in preparation), and by virtually all recent conodont workers. This contradicts point no. 5 in the discussion of Paranomenclature (*Bull. zool. Nom.* vol. 36, p. 12, 1979). The Pander Society has held several meetings at which the adoption of the above-mentioned procedure was advocated to promote stability of nomenclature. General agreement has been reached since the 1971 meeting in Germany to follow Articles 23 and especially 24. The re-introduction of parataxonomy into conodont taxonomy would destroy all of the progress towards a natural taxonomy. The negative effects of these amendments will similarly produce nomenclatural chaos among other groups of fossils.

'We, the undersigned members of the Pander Society meeting in Vienna and Prague and representing conodont workers from 25 countries, emphatically support the position expressed in this letter.'

'The above letter bore the following signatures:

<i>Australia</i>	Dr Ruth Mawson Mr Stephen Carey	<i>China (People's Republic)</i>	Dr C.Y. Wang
<i>Austria</i>	Dr M.F. Buchrothner Dr Fritz Ebert Dr F. Neubauer Dr H.P. Schönlaub	<i>Czechoslovakia</i>	Dr R. Mock Dr J. Papsova
<i>Belgium</i>	Dr Pierre Bultynck Dr R. Dreesen Dr E. Houilleberghs	<i>France</i>	Dr C. Cygan Dr M.F. Perret
<i>Bulgaria</i>	Dr K. Budurov	<i>Germany (Federal Republic)</i>	Prof. Dr H. Beckmann Dr H. Reguadt Dr D. Stoppel Dr K. Weddige
<i>Canada</i>	Prof. C.R. Barnes Dr P.H. von Bitter Prof. Brian Chatterton Prof. Lars Fahraeus Dr David Kennedy	<i>Germany (Democratic Republic)</i>	Prof. Dr W. Ziegler Dr H. Kozur
		<i>Hungary</i>	Dr S. Kovacs

<i>Iran</i>	Dr B. Hamdi	<i>United Kingdom</i>	Dr R. Aldridge
<i>Israel</i>	Dr F. Hirsch		Dr R. Austin
<i>Italy</i>	Miss G. Bagnoli		Dr M.J. Reynolds
	Miss M.C. Perri	<i>U.S.A.</i>	Prof. S. Bergström
	Miss C. Spalletta		Dr J. Baesemann
<i>Japan</i>	Prof. G.B. Vai		Mr T.R. Carr
	Prof. H. Igo		Prof. David Clark
	Prof. T. Koike		Prof. R.L. Ethington
	Dr Y. Kuwano		Prof. Gilbert Klapper
	Dr A. Shuko		Dr H.R. Lane
<i>Libya</i>	Dr A.W. Ghaziry		Prof. J. Miller
<i>Netherlands</i>	Dr M. v.d. Boogaard		Prof. G. Merrill
<i>Poland</i>	Dr J. Dzik		Prof. M.A. Murphy
	Dr W. Bednarczyk		Dr John Repetski
	Dr H. Szaniawski		Dr C.A. Sandberg
<i>Romania</i>	Dr E. Mirauta		Prof. N. Savage
<i>South Korea</i>	Prof. H.Y. Lee	<i>Yugoslavia</i>	Prof. W. Sweet
<i>Spain</i>	Dr C.A. Mendez		Miss T. Kolar
	Dr R. Menéndez		Prof. A. Ramovs
<i>Sweden</i>	Dr L. Jeppson		Dr M. Sudar
	Dr A. Löfgren		

The following non-members of the Pander Society, but interested palaeontologists studying other fossils, have also signed:

<i>Australia</i>	Prof. J. Talent
<i>Canada</i>	Prof. P. Lespérance
<i>Czechoslovakia</i>	Dr Ivo Chlupac

The Pander Society's communication was circulated to the members of the Commission with the following note from the Secretary:

'First, I admit that my knowledge of the taxonomic progress of conodont studies was out of date when I wrote the report in *Bull. zool. Nom.* vol. 36 (paragraph 5 on p. 12 is referred to). It is clear that the two parallel taxonomies that formerly existed in conodonts are gradually being reduced to one over most, if not quite all of the field.

'Secondly, it is still true that the nature and taxonomic position of the conodont-bearing animal is unknown. It cannot be stated as a certainty that a conodont apparatus represents a whole animal, or only a detached organ of a whole animal.

'Thirdly, the argument that all fossils are parataxa is a piece of casuistry. In practice, the "complete gradation" mentioned is not found. There are fossils — the vast majority — that can be confidently taken to represent an animal as such; and there are others — not a few — that can with equal confidence be said not to represent the same taxa as those in which the fossils of the

former group can be classified. Yet the fossils in this latter group can be classified in genera and species of their own, that do not correspond, one-for-one, with the genera and species of fossils taken to represent the animals themselves. These parallel taxonomies (some of which are much older than the first, 1905, *Règles internationales de nomenclature zoologique*) are set up to meet a practical need for communication, and they will continue to do so as long as the need exists. Article 24 could be used to destroy the nomenclatures in which these taxonomies are expressed by applying the Law of Priority case by case. In such groups as ammonite phragmocones and aptychi; nautiloid phragmocones and rhyncholites; fossil holothurian spiculea and extant holothurians; fossil and extant elasmobranch fishes; such use of Article 24 would not only produce chaos in both paranomenclature and "orthonomenclature" — it would also inhibit communication between workers on the parataxa in the only framework that makes practical sense for them, because there would no longer be a nomenclature in which they could express the only taxonomy that they could use. Thus the instrument set up to facilitate communication among zoologists would be put to a perverse and contrary use.

'If the conodont workers do not need two taxonomies and two nomenclatures, nobody can compel them to use them. Workers in other groups have such needs.'

DECISION OF THE COMMISSION

On 4 July 1980 the members of the Commission were invited to vote under the Three-Month Rule on Voting Paper (1980)18 for or against the proposals set out in paragraphs 1-4, Section B. of the Commission's report to Section on Zoological Nomenclature as set out in *Bull. zool. Nom.* vol. 36, pp. 211-212. At the close of the voting period on 4 October, 1980 the state of the voting was as follows:

Vote 1

Affirmative votes — fifteen (15) received in the following order: Corliss, Halvorsen, Welch, Holthuis, Kraus, Mroczkowski, Vokes, Alvarado, Melville, Willink, Nye, Sabrosky, Starobogatov, Brinck, Ride

Negative votes — three (3): Hahn, Binder, Tortonese

Vote 2

Affirmative votes — thirteen (13) received in the following order: Corliss, Halvorsen, Welch, Hahn, Kraus, Mroczkowski, Vokes, Alvarado, Melville, Willink, Sabrosky, Starobogatov, Brinck

Negative votes — five (5): Holthuis, Nye, Ride, Binder, Tortonese

Votes 3 and 4

On 14th January 1981 I received a request from Mr. Heppell under Bylaw 27 to defer publication of the results of the votes on points 3 and 4 of V.P.(80)18 (point 4 had by that time been presented for a second vote under Bylaw 35) and V.P.(80)39. I accordingly took action as requested under Bylaw 24. Mr. Heppell said:

'I seek the Secretary's approval under Bylaw 27 to defer publication of the decision taken by the Commission on points 3 and 4 of V.P.(1980)18 and of that taken on point 4 when re-submitted as V.P.(1980)39 on the grounds that the application presented was incorrect. This incorrect presentation of the proposals relating to parataxa may be established by comparison with those relating to ichnotaxa (V.P.(1980)18, points 1 and 2). In the case of ichnotaxa these taxa had generally been regarded as equivalent to the work of an animal and, as such, their names were unavailable under the Code if published after 1930. It was therefore necessary for the Code to be changed in two ways before the names of such trace fossils could adequately be regulated by it. First, to make it explicit that trace fossils were to be treated for purposes of nomenclature as taxa and not as the work of an animal, so that their availability was assured even if published after 1930. Second, in the special circumstances of their taxonomy as trace fossils impinging on the taxonomy of body fossils, to ensure that neither the ichnotaxon nor the taxon would disappear into the synonymy of the other, regardless of the relative priority of the two names. As in the case of a collective group, the name of an ichnotaxon would remain available for use within the ichnotaxonomic system even if some of its components had been identified with "whole-animal" taxa. Point 1 of the vote established the availability of ichnotaxa as a special kind of taxa. Point 2 established their "parataxonomic" nature in relation to "orthotaxa". It could be argued that point 2 was no more than a special application of the proposal embodied in point 4 but there is nothing intrinsically incorrect in its presentation. Although the form of words used in points 1 and 2 differs considerably from those used in this submission, my argument confirms that the underlying concept of each point is a separate though related substantive change in the existing Code. In the case of parataxa, however, the two underlying concepts are conjunct and cannot logically be subject to separate (and possibly opposing) votes. As Nye has claimed in his comments, "ichnotaxa, parataxa and orthotaxa all merge". The Secretary, in an Appendix to V.P.(1980)18, indicated a number of parallel taxonomies recognised in various phyla. In any such case the taxa of each of the parallel taxonomies are parataxa in relation to the other. As an example, if genera based on gastropod opercula cannot be related on a one-to-one basis to genera based on gastropod shells, not only are the opercula-genera parataxa in relation to the shell-taxa but the shell-genera are parataxa in relation to the opercula-taxa. Thus the concept of parataxa must be independent of any actual classification of assemblages of fossil fragments as no one system of taxonomy is necessarily more "artificial" than another. It seems to have been a failure to appreciate this, that has led to most of the argument against parataxa being recognised in the Code. It is thus evident that parataxa are not absolute but relative. Therefore, as all parataxa are taxa within their own taxonomic systems, the

Code already recognises them and no substantive change is either indicated or possible in this respect. But in order to avoid competition between taxonomies (in practice a very rare event but in theory, as made evident in the Secretary's Appendix, liable to cause nomenclatural instability of considerable magnitude) it is necessary for the Code to be emended so that a taxon and its corresponding parataxon do not compete for priority. As it has been demonstrated that there is no objective criterion for the recognition of a parataxon it can be stated that parataxa are taxa which exist in independent taxonomic systems and consequently do not mutually compete for priority. This assertion makes the necessity for such provision in the Code self-evident and demonstrates that the two aspects of parataxa embodied in the proposals of V.P.(1980)18 are indivisible and that a separate vote on each point is not acceptable.

'I do not object to the manner of presentation of the case to the IUBS General Assembly at Helsinki as the nature of parataxa had to be indicated in a way that would be readily understood. Also, it was not unreasonable to outline those areas in which parataxa are liable to be used, just as one might explain that the use of collective groups is normally confined to helminthology. In approaching the subject from a somewhat different direction, however, I hope I have been able to show that the relationship between taxa and parataxa is essentially philosophical and that their non-competition with each other for priority is implicit in the concept and thus is not susceptible to consideration as an independent issue'.

DECLARATION OF RESULT OF VOTE

I hereby declare that the votes cast on V.P.(80)18 were cast as set out above, and that proposals 1 and 2 contained in that voting paper have been duly adopted. The Commission will incorporate the proposed amendments into the Code, in accordance with the authority given to it by the Division of Zoology of IUBS at Helsinki, in words to be prepared by the Editorial Committee for the Commission's approval. Publication of the results of the votes on proposals 3 and 4 has been deferred under Bylaw 24.

R.V. MELVILLE

Secretary

International Commission on Zoological Nomenclature

London

26 January 1981



Melville, R. V. 1981. "The International Code Of Zoological Nomenclature: Result Of Vote On Proposals For Substantive Amendments (Fifth Instalment). Z.n.(S.) 1973." *The Bulletin of zoological nomenclature* 38, 30–48.
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