THE INTERNATIONAL CODE OF ZOOLOGICAL NOMENCLATURE: RESULT OF VOTE ON PROPOSALS FOR SUBSTANTIVE AMENDMENTS (FOURTH INSTALMENT) Z.N.(G.)185

By the Secretary, International Commission on Zoological Nomenclature

This report presents the result of the Commission's vote on the proposal to introduce "hapantotypes" into the Code. This proposal was put forward as a means of solving difficulties peculiar (at present) to workers in parasitic protozoology in typifying species with complex life cycles. It was published in *Bull. zool. Nom.* vol. 35, pp. 200–208 (May 1979), was reported to and approved by the Special Session of the Commission at Stensoffa, Sweden, and was reported to and approved by the Section on Zoological Nomenclature at Helsinki. The proposal had been endorsed by the International Commission on Protozoology and the International Congress of Parasitology.

A supporting paper by Garnham, Bray & Killick-Kendrick was published in *Bull. zool. Nom.* vol. 36, pp. 17–21, and a comment by Dr R.B. Williams in vol. 37, pp. 137–139. Professor Gordon Bennett (*Memorial University of Newfoundland*) contributed to the discussion by correspondence.

On 16 June 1980, the members of the Commission were invited to vote under the Three-Month Rule on Voting Paper V.P.(80)17 on the proposal in the following form (paragraph B.27 of the Commission's report to the Section on Zoological Nomenclature at Helsinki):

Code Article	Commission Report to Section on Zoological Nomenclature at Helsinki, 1979, Section B
72	27. To provide that in extant species of protozoa, when a taxon cannot be differentiated by a single individual (or a single preparation – B26 above), a suite of several preserved preparations of directly related individuals representing different stages in the life cycle may be designated as a holotype or neotype, or selected as a lectotype. Such a group of preparations would have the status of such a type (not syntypes). The term hapantotype is proposed to describe this category. The change proposed to Article 72 is an extension of that in B26 above. The proposal results from consulta-

tion with the same bodies (*Bull. zool. Nom.* vol. 35: 200).

The following note on the historical background was sent out with the voting paper.

CALL FOR A VOTE ON THE INTRODUCTION OF "HAPANTOTYPES" IN THE CODE

The proposal by the Committee on Typification of Species of Protozoa (*Bull. zool. Nom.*, vol. 35, pp. 200–208) to introduce a new kind of type in the species group, termed a 'hapantotype', represents a radical new step in the progressive development of the Code. That proposal is now put to you for a vote. It is explained in this accompanying note.

It is a feature of the system of regulation of zoological nomenclature that infractions of its rules can neither be prevented by the Code nor punished by the Commission. The only pressure on zoologists to conform to the Code is peer pressure. When this is found to be ineffective, as in the present case, and when the Commission's help is sought, it is incumbent on us to see whether the infractions in question arise from a real problem or not; and, if so, to seek ways of solving or removing the problem.

In 1976 Professor P.C.C. Garnham (*U.K.*) and Professor Norman D. Levine (*U.S.A.*) drew my attention to the work of a group of German scientists on parasitic protozoa (for full references, see Frenkel, J.K. *et al.*, 1979, *Zeitschr. Parasitenk.*, vol. 58, pp. 115–139). The group consisted of veterinary surgeons and an electron microscopist, but included nobody with a specifically zoological, still less a taxonomic, qualification. Before they started work, it had been believed that species of *Sarcocystis* and related genera were homoxenous parasites, that is, that they infected only a single host species in which they passed through all the stages of their life cycle. The German group showed, by some brilliant research, elegantly conducted, that the species were heteroxenous, that is, they infected two host species, both of which were necessary to the completion of the life cycle of the parasite. *Sarcocystis* Lankester, 1882 was a well-known parasite of farm animals and man; and in those species there was never any sign of a sexual phase in the life cycle. The German workers showed that each species of *Sarcocystis* also infects a carnivorous host (fox, dog, raptorial bird) in which a sexual phase takes place. They also showed that not one species, *Sarcocystis hirsuta* Moulé, 1888, but three infect cattle, and that each has a different final host – cat, dog and man.

It was at this point that the German group met their nomenclatural problem. To which of the three species in the ox should the name *S. hirsuta* be given? Could any of the names regarded as synonyms of *S. hirsuta* (e.g. *S. fusiformis* Railliet, 1897; *S. blanchardi* Doflein, 1901) be used as valid names? The sheer size of the technical strides made by the German group rendered the original descriptions useless as a standard of reference, and none of the original preparations survived. They therefore decided to reject the old names altogether and to propose new ones. They named the three cattle species *S. bovicanis, S. bovifelis,* and *S. bovihominis,* and extended this practice to other species of *Sarcocystis* and to species of related genera (*Frenkelia, Hammondia,* etc.).

The procedure adopted by the German workers evoked two different reactions among their colleagues. Some pointed out that it was not only contrary to the letter of the Code, but also to its spirit in that long-established names for species of great economic importance were rejected in favour of junior synonyms, thus upsetting stability of nomenclature. Others, however, welcomed the new names, particularly those that combined elements of the names of the two hosts – though the apparent simplicity and directness of this procedure has been somewhat spoiled by the discovery that there is more than one ox-dog species of *Sarcocystis*. Since 1975, when these new names began to be proposed, their use has spread and is gaining ground, in spite of protests from distinguised protozoologists, who continued using the old names.

When the Commission's help was first sought, my reply was that the Code was there to be applied, and that its application in the present case was perfectly clear: the new replacement names were invalid; they must be rejected, and the old names restored to use. Having ascertained that no original material of the early-named species was known, I asked why the problem could not be solved by the use of neotypes. The answer was that no single individual would serve the purpose of a type, namely, to provide a standard of reference for the application of a name. It was explained to me that it is now necessary to examine all the stages of a life cycle before a species can be correctly recognised. Thus the Code, by insisting that a type in the species group can only be a single individual, actually prevented a solution to the problem being found within the framework of the Code.

It was at this point that I was able to discuss the problem personally with Professors Garnham and Levine (who wished to retain the old names) and the members of the German group at the International Congress of Protozoology in New York in July 1977; it was there that the International Commission on Protozoology set up the committee whose report is referred to in my first para-

graph. Meanwhile, Frenkel *et al.*, 1979, are asking the Commission to suppress the older names by the use of the plenary powers, on the grounds that they are *nomina dubia*. You are asked to read that paper, if possible, as well as the Committee's report and the enclosed separate of my reply to Frenkel *et al.* before voting.

After a year's intensive work by correspondence in the Committee, and after further direct discussions at the International Congress of Parasitology at Warsaw in 1978 and subsequently, I am personally wholly convinced that the problem arising from the work of the German group can only find a solution if the Code is amended so as to allow the species concerned to have as their types a suite of exhibits demonstrating the successive phases of the life cycle and, where appropriate, zymograms or isoenzyme prints. The number of species involved is not small, for it includes other parasitic protozoa, most notably the malarias, in addition to *Sarcocystis* and its allies. The gravity of the problem arises from the medical and veterinary importance of the species concerned. The Commission cannot condone the continuance of confusion and discord in the nomenclature of these animals.

At the same time, if the concept of hapantotypes is to be incorporated in the Code, the strictest controls must be imposed on its application. This view is strongly held by the parasitologists with whom I have discussed the subject. They see as clearly as anybody else that there is bound to be a risk of hapantotypes including material of more than one species, and that every precaution must be taken to ensure their purity. They also understand the radical nature of the proposal. In effect, it reveals a conflict between the classic concept of the objectivity of the unique type and the practical necessity to have types that serve some useful purpose as standards of reference. The Code, after all, admits that species may be based on syntypes; and a moment's reflection will show that in many groups, types are already of a multiple nature – not only in colonial animals, such as corals, polyzoa, graptolites, but also in vertebrates, where an entire body with all its parts, or an entire skeleton may be a type. The conceptual difficulty of accepting hapantotypes therefore does not seem to me very great. It is the fact that it must comprise physically independent cells which cannot be treated as syntypes that poses difficulty. I hope the fact that the German group is persisting in its course of action and that confusion and ill feeling are growing, will induce you to overcome that difficulty.

You are therefore asked to vote for or against adding a provision to Article 72 of the Code to allow that, in extant species of protozoa, if the name cannot be interpreted by reference to an animal or part of an animal, the type may be a number of directly related individuals, either:

[(1) in a single preparation (type slide); or]
(2) a suite of preparations representing differing stages in the life cycle (hapantotype).

(I have placed proposition (1) in brackets because it has already been accepted in V.P.(80)1, Point 10.)

The proposed Glossary definition of 'hapantotype' is: 'the suite of directly related individuals, including where necessary directly related evidence of their work, that together form an onomatophore [name-bearing type] in certain extant species of protozoa.'

OBJECTIONS RECEIVED

On 20 September 1979 Professor Holthuis wrote to express disquiet at some of the major changes to the Code accepted by the Stensoffa meeting. On hapantotypes he said:

'My objection to this concept is that until now a holotype is an objective standard of reference for a species, since it can, by definition, only be a single specimen. Both an individual and a clone cannot belong to more than one species. The moment a type series of different individuals, and a hapantotype is such a type series, is given the status of a holotype, the holotype concept loses its value as an objective standard.

'I cannot see what is wrong in designating a type series of various stages as syntypes. If they all belong to a single species they are as good a standard of comparison as when one calls them a holotype. If they do not belong to a single species then a lectotype can be chosen and the standard of the name can be preserved. My advice would be that we recognise lectosyntypes, i.e. that from a series of syntypes we could remove certain specimens that prove to belong to a different species from the rest, and still keep a series of lectosyntypes instead of a single lecto(holo)type. In this way one would still have the advantage of being able to use a (purified) type series.

'A hapantotype is far more vulnerable than a syntype series. If a hapantotype proves to represent more than one species, what do you do? Reject the whole thing and leave the species without a type? Or can you make a lectohapantotype? And what if two authors differ about the homogeneity of a hapantotype? Who is to decide how many stages have to be represented to make a hapantotype? Can you have hapantotypes of two individuals? I am afraid

that here too all the consequences of the introduction of the term hapantotype have not been thoroughly considered, and that in introducing the concept in such a hurried way we shall do the Commission and the Code more harm than good.'

I replied on 26 September 1979: 'In considering the difference between a hapantotype and a series of syntypes, have you considered the arguments advanced by Garnham *et al.* in *Bull. zool. Nom.* vol. 36, pp. 17–21? I agree that a hapantotype is vulnerable – that is the nature of the case – but you cannot deny the necessity for a suite of preparations showing successive stages of the life cycle. The nearest we can bring such a suite to the objective ideal is to insist that all the components of it are directly related. But the whole suite is indivisible and no single specimen or preparation can be designated from it as holotype or lectotype, because no single specimen can serve the function of a type.

'Steyskal has come up with the suggestion of the term "plethotype" which could be used to designate a provisional standard of reference for a protozoan species for which a hapantotype could not, or not certainly, be provided. But there might be a succession of plethotypes, each rejected in turn as a more nearly complete suite of stages was prepared, before a true hapantotype could be prepared. The point here is, as I am sure you can see, that you cannot go on breeding directly related individuals from one or more dead preparations; so each earlier plethotype would have to be rejected. This is, perhaps, a grave disadvantage.'

'Of course, if a hapantotype is found to contain material of more than one species, the whole thing must be rejected and a fresh start made with new material.'

On 11 October 1979, Professor Holthuis asked three questions:

'(1) How can you prove that organisms that pass through various stages in different hosts are directly related?

'(2) How can a type be an objective standard if you can reject the whole thing and start again with fresh material?

'(3) What happens if one zoologist claims that a hapantotype consists of specimens of the same species while another says that more than one species is involved? Does the species then have two holotypes? The original and the new hapantotypes? Who is to decide which is the correct one?'

I replied on 29 October:

'(1) Direct relationship through different hosts is demonstrated by cross-infection tests using laboratory-bred, sterile hosts.

'(2) A hapantotype would only provide an objective standard of reference if it consisted of true-bred parasites. If it proved to contain a mixture, then obviously it must be rejected and a fresh start must be made. It would not be possible, as it would with a series of syntypes, to choose a lectotype because the hapantotype would consist of dead, fixed cells from which it would be impossible to restart the life cycle. [The ground of argument on this point is obviously changed if living culture material in cryopreservation is admitted.]

'(3) Any author who claimed that a hapantotype represented more than one species would have to prove his case, by cross-infection tests, by isoenzyme analysis, or by some other means. If his case is accepted, then the hapantotype would have to be rejected and a fresh start made.'

On 16 November Professor Holthuis wrote, on these same points:

'(1) In that case I cannot see that many (or perhaps any) direct hapantotypes can be established if before describing a new species one has to do cross-infection tests on laboratory-bred sterile hosts, especially if the hosts are feral, not domestic, species.

'(2) This second requirement makes it even more impractical to establish hapantotypes, and it makes the fitness of a hapantotype as a standard even more hazardous.

'(3) You say that an author who thinks that a hapantotype is heterogeneous has to prove his case. As you remarked yourself, the hapantotype cells are dead and preserved; therefore it cannot be proved that the various stages in the hapantotype series are different species. But he can make it likely by raising from a stage of what he takes to be the same species as the corresponding stage in the hapantotype a series of stages that differ from the remaining stages in the hapantotype. You say "if the case is accepted", but by whom? By the Commission? But the question is taxonomic, so the Commission has no say in it. By the zoological public? How do you find this out? In most cases there will be a difference of opinion, which will clearly show the fact that this "type" is anything but an objective standard.'

Professor Holthuis also referred to a paper by Tadros & Laarman, 1976, *Acta Leidensia*, vol. 44, in which it is said that "the oocyst of eimeriid Coccidia is the most stable and reliable basic criterion for a workable scheme of classification" and

suggested that an oocyst might serve as a holotype.

It does not appear that I answered that letter, but it may be pointed out that Professor Holthuis has altered the ground of his objection by including in it the impracticality of preparing hapantotypes. On point (1) it is fair to point out that there is no compulsion on authors to designate types of new species. On the suitability of oocysts as holotypes, it is unfortunately a fact that no successful method of preserving them undistorted has yet been found.

On 8 January 1980 a meeting took place at the Imperial College Field Station at Ascot between, on the one hand, Professor Holthuis, Professor Bayer and the Secretary, and, on the other hand, Professor Garnham, Dr. Bray (members of the Committee on Typification of Protozoa) and Dr. Killick-Kendrick. The following note of the meeting was circulated. (Professor Holthuis's reservations are recorded below.)

'The discussion made it possible, first, to explain one of the main arguments in favour of hapantotypes: namely, that in a protozoan species with a complex life cycle, each stage taken individually might be indistinguishable from the corresponding stage in some other species, although the complete sequence of stages taken as a whole was peculiar to that species. Hence, any standard of reference that represented less than the complete life cycle would be incapable of serving the prime function of a type and would be useless. This point was accepted by Professor Holthuis.

'Professor Holthuis then raised the question of a hapantotype found to be incomplete. Here an analogy was drawn with imperfect or incomplete types among Metazoa; if necessary to the stability of nomenclature, an application could be addressed to the Commission for the setting aside of the imperfect type or hapantotype and the designation of a neotype by the use of the plenary powers.

'Professor Holthuis's main objection was to the multiple nature of a hapantotype. For him, the essence of the type principle when applied to species was the objective uniqueness of the type specimen. In the case of a species based on syntypes, if these were later thought to represent more than one species, it was possible to designate a unique lectotype to represent the species originally based on the syntypes. Although he accepted that this could be ineffective in protozoa with complex life cycles, he still sought reassurance on the problem of a hapantotype found to comprise representatives of more than one species. Although in some cases it might be enough to rely on the description and illustrations, this would not always be the case. He accepted the general point that it is impossible to require, as a matter of legislation, that descriptions or illustrations must satisfy certain predetermined criteria.

'The protozoologists pointed out that the likelihood of an author knowlingly basing a new species on composite material was equally small in protozoa and Metazoa. With modern techniques using laboratory-bred hosts, it was extremely unlikely that composite hapantotypes would in practice be prepared, although they agreed that the possibility could not be excluded. At the same time, they pointed out that even in a hapantotype found to be mixed, every stage of the species based on the hapantotype would be certainly represented, even if individuals of another species were also present. It would therefore be possible for a subsequent zoologist to restrict the original hapantotype by indicating the cells that he considered to belong to some other species, without thereby destroying the completeness of the hapantotype as a sequence of representatives of every stage in the life cycle. Professor Holthuis was satisfied with this argument.

'Lastly, it was agreed to propose the following revised Glossary definition of "hapantotype";

> "The suite of directly related individuals, including where necessary directly related evidence of their work, that represents successive stages in the life cycle of extant species of protozoa and that forms the name-bearing type (onomatophore) of such a species. If a hapantotype is found to be mixed or composite, it may be restricted, but no lectotype can be validly designated from among the specimens comprised in it. The term may be prefixed by "holo-", "lecto-", "neo-", "para-" or "syn-" as appropriate".'

Professor Holthuis wrote as follows on receiving this report (his letter was dated 25th January 1980):

'I do not remember having raised the question of "incomplete" hapantotypes. Does a hapantotype, in order to be "complete" have to consist of all stages of a species? It does not say so in the proposed definition. If this is not so, what then do you take to be a "complete" or an "incomplete" hapantotype? Is it "complete" when there are enough stages to make it possible to recognize the species? In some cases a single stage would suffice, and I would not call that a hapantotype. I will return to this point when dealing with definition.

'I fear that you misunderstood me on composite hapantotypes. If a hapantotype proves to consist of more than one species, the description and figures, whether or not they deal with only one of the species, is or are immaterial. The type material is and remains heterogeneous.

'What I meant was that if there is a homogeneous type series (syntypes or hapantotype), the fact that one of the specimens is made the lectotype does not make the species any less recognizable.

Consider a type series consisting of a specimen of each of stages A, B, C and D (if the species has four stages) and none of the stages is by itself characteristic of the species, but only a combination of some or all of them. If specimen C is selected the lectotype of the species, that single specimen does not characterize the species, but the fact that stages A, B and D belong to the same species does. To be of importance, these specimens do not have to belong to a hapantotype. If they become paralectotypes and if, but only if, they are conspecific with the lectotype, they play exactly the same role as they would if they were part of the hapantotype. The condition is that they are conspecific with the lectotype, but the same condition applies to a hapantotype. By the same token, an author can indicate a single specimen of a single stage as the holotype of a species and in his description describe all the stages of the species. In this way his species is recognizable, even if the single holotype in itself does not show enough characters to make the specific identity certain.

'In taxonomy there are lots of cases in which the type itself is not sufficient to recognize the species (e.g. where the type is damaged or lost, or where a non-morphological character such as sound, movement or locality, are essential) but where outside evidence (e.g. original or later descriptions, paratypes, etc.) is needed for the identification of the species.

'I agree with the definition, except for the last sentence. But it does not cover statements in the preceding paragraphs:

(a) the definition is such that any two or more stages of a species (even if it contains more than that number of stages) can be made into a hapantotype. There is no mention of a complete set of stages. Personally I do not see the need for a complete set, as (1) it might be possible to recognize the species from a few stages only, so that a complete set would be unnecessary, and (2) it might be difficult to ascertain that a set is complete, certainly with a new species, so that a hapantotype would be disqualified the moment an "intermediate" stage is discovered.

(b) what is meant by "directly related"? If it is the sequence "mother-daughter-granddaughter" and nothing else, it would be extremely difficult if not impossible to prove, and thus hapantotypes will become extremely difficult if not impossible to establish.

(c) in the definition it is said that a hapantotype may be divided or restricted. I fully agree to the broad sense in which this is expressed, for a hapantotype could then be restricted to a single specimen, which would be equil-

avent to a lectotype designation. However, from the previous paragraphs I get the impression that a hapantotype, even if restricted, has to represent all the stages of a species. Hence if one stage proves to be represented by a different species from the rest, the hapantotype is invalid.

(d) as to the last sentence of the definition, I see the hapantotype as a special kind of syntype (from which no lectotype should be selected, but which can be restricted if necessary), and just as there are no holo-, lecto-, syn- and para-syntypes, I would not recognize such categories for hapantotypes. The only thing that I would recognize would be a neohapantotype.

'I believe Dr. Bray objected to the restriction of the use of hapantotypes to the protozoa. I fully agree with him. If there is a need for hapantotypes in other groups, it seems pedantic of the Commission not to allow its use there. Nowhere in the Code is there, I believe, a rule that applies only to one taxonomic group. As different authors might interpret the limits of such a group in different ways (and taxonomic freedom cannot be limited by the Commission), this would mean that different nomenclatural rules might legally be used for the same taxon, depending on the taxonomic views of an author.'

[It seems to me that Professor Holthuis shows, in this letter, that he has not fully understood the grounds for the hapantotype proposal and that, in consequence, he falls into inconsistencies. In his last paragraph he seems to suggest extending its application, which is not in line with his previous arguments. I have discussed the proposal with zoologists working in various multi-stage animal groups, and in every case I have been told that species can be adequately represented by a single holotype taken at an appropriate stage (usually, but not invariably, the mature adult). I therefore believe that the concept should be applied only where an urgent need has been demonstrated, and that it is for interested zoologists in other groups to make their own case to the Commision for its extension.]

Professor Bayer wrote on 25 February 1980 as follows:

'I have to repeat my conviction that the Code must serve the sciences that need it, and therefore take into account the situation where a species cannot be characterised by a single specimen or single stage in a complex life history. We have got round this problem in macroinvertebrates for years without introduction of a new category of type-specimens, but it appears that in the case of protozoans with complex life cycles the situation is so difficult that new procedures are needed. Nevertheless, I consider it a

mistake to let new provisions established for such special cases diminish the effectiveness of the Code for use in other animal groups, so I concur with Dr. Holthuis's view while acknowledging the needs of researchers in parasitology. Given that the Code must not only retain its integrity for application in the vast majority of animal groups, but also fill the needs of scientists working with animals having a complex life cycle, the problem is to find an acceptable common ground. I think that this was achieved under your guidance at Ascot, and in my opinion there remain only matters of detail, largely semantic, to be clarified.

'If memory serves me aright, it was the potential heterogeneity of hapantotype preparations that drew the strongest objection from Dr. Holthuis, as noted in his letter of 25 January 1980. Evidently, heterogeneity remains possible even with the most refined laboratory techniques, although chances of its occurrence are slight. The provision that a hapantotype sequence can be culled of extraneous components without jeopardy to its status seems to circumvent this difficulty by leaving only conspecific organisms in the hapantotype array. However, if one of the stages of the hapantotype were found not to be conspecific (as opposed merely to being contaminated by cells that can be excluded from consideration), it was my understanding that the whole hapantotype would be deemed invalid, requiring a new sequence to be prepared. Do I recall correctly that this point was raised several times in the discussion? It seems to be a potentially sticky point and, other than in the possible term "neohapantotype", it is not addressed in the definition (and perhaps ought not to be; it is procedural and should be covered in the pertinent Article).

'It seems to me that the question regarding the expression "directly related individuals" raised by Dr. Holthuis is a semantic one. I sense that they are ontogenetically related but not necessarily "mother-daughter-granddaughter". Having been cultured in a presumably clean host from a presumably pure inoculate (or whatever they call it), all would be conspecific even if not derived from the same parent (clonal). Haven't the parasitologists some term that could serve in place of "directly related"?

'I have difficulty in determining how the combining term with prefixes "holo-", "lecto-", "para-" and "syn-" are to be used. As I read the definition, the hapantotype functions as a holotype. If the hapantotype serves as a holotype, I can't see the need for a term "holohapantotype". All of the stages could be on one slide, or they could be on separate slides (as I assume would often be the case in blood parasites), or in separate vials or bottles. One or all of these could contain contaminants excludable under the Code. Are the conspecific cells remaining after exclusion of the contam-

inants the "lectohapantotypes" and those excluded the "paralectohapantotypes" (God help us!)? I assume that it is also possible for the parasitologist to make several slides of each stage from the blood samples that he takes from the host. Are the several sets of all the stages the "synhapantotypes" and one of the sets the potential "lectohapantotype", the others then becoming the "paralectohapantotypes" (again God help us!)? To gain a different perspective on this question, assume a theoretical case of a scyphozoan requiring not only the adult medusa but also the scyphistoma and the ephyra for adequate characterization of the species. It would seem logical to consider one set of (reared?) scyphistoma/ephyra/medusa the hapantotype; additional sets would then be parahapantotypes. Obviously, if the establishing author had several sets but did not indicate one of them as the hapantotype, then the compound syn-, lecto-, and paralecto- terms would come into play, but I don't see the need for "holohapantotype". Whatever is intended, the meanings should be clarified.

'At first, I agreed with the opinion of Drs. Bray and Holthuis that the application of hapantotypes not be confined to protozoa. On further consideration, however, I have come around to the view that the hapantotype provision should, in daily practice, be limited to these special, probably unique, organisms. In those metazoans with complex life histories that include several dissimilar stages (and there are many: coelenterates, annelids of several sorts, insects, crustaceans, mollusks, echinoderms, even fishes, to mention the most obvious), the species usually can be characterised by a single stage; once the life cycle is worked out, all the stages are recognisable and identifiable. In some instances, one or more stages in the life cycle may be morphologically identical in several related species, and cannot be identified unless reared to some stage or condition that is morphologically unique. The hapantotype provision in no way alters the situation. It does not make it possible to identify unknowns, and it does not make the basic definition of a new species any more useful. If applied to certain crustaceans, for example, it might preclude describing a species until all stages are obtained by rearing, and it would preclude having a lectotype in the usual sense. I do not think anything is gained by it. Therefore, I would prefer to see hapantotypes restricted to those protozoan parasites where they are necessary for definition of the species, because opening them up to all animal groups would open up Pandora's Box in ways that we might well regret'.

Professor Holthuis wrote again in February 1980, but without adducing any fresh arguments.

The hapantotype proposal represents a large, new and radical step in the direction of adapting the Code to the needs of present-

day zoology. I therefore make no apology for providing this extensive documentation with the Voting Paper. The proposal as here presented is the fruit of work extending over three years, and I hope you will consider it worthy of careful consideration.

At the close of the voting period on 16 September 1980 the state of the voting was as follows:

Affirmative votes – fifteen (15) received in the following order: Melville, Nye, Corliss, Brinck, Hahn, Halvorsen, Willink, Kraus, Mroczkowski, Starobogatov, Trjapitzin, Vokes, Binder, Sabrosky, Ride

Negative Votes – Holthuis, Alvarado

Dupuis and Bayer abstained. No voting papers were returned by Bernardi, Habe, Heppell, Cogger, Tortonese and Welch.

The following comments were sent in by members of the Commission with their voting papers:

Hahn: 'I vote for the introduction of hapantotypes, but only if this sort of type is strictly confined to protozoa. In the comment of Dr Williams I see a tendency to extend it also to Cnidaria and perhaps other phyla. If the medusae of several related species are indistinguishable, as in *Obelia*, polyps may be selected as types (and vice versa).'

Vokes: 'Reluctantly.'

Willink: 'I fully agree with the clear, consistent and exhaustive explanations given by the Secretary to the different objections presented.'

Bayer: 'It is with regret that I abstain from voting in this case. I cannot vote against the principle involved, but neither can I vote for the complex terminology proposed for what is nothing but a set of syntypes. If adopted, "hapantotype" and its numerous possible polysyllabic combinations with lecto-, para-, etc. inevitably will bring the Code into ridicule and invite the contempt of many zoologists who already consider it too complex.'

DECLARATION OF RESULT OF VOTE

I hereby declare that the votes cast on V.P.(80)17 were cast as set out above, that the proposal contained in the voting paper has been duly adopted by the required two-thirds majority, and that the Commission will incorporate the proposed amendment 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.

R.V. MELVILLE, *Secretary*
International Commission on Zoological Nomenclature, London, 7 Oct. 1980

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