

DOCUMENT 25/4

Questions arising in connection with the naming of Orders and taxa
of Higher Rank

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(Editorial Note : In a letter dated 20th November 1957 Dr. Dougherty explained that, in conjunction with Dr. Benjamin G. Chitwood, he had recently been engaged on work on a re-classification of the Nematodes and that in the course of this work Dr. Chitwood and he had found themselves in disagreement on certain questions relating to the naming of Orders and taxa of higher rank. Dr. Dougherty explained that he had set out his views in a series of documents which had formed enclosures to a letter which he had recently addressed to Dr. Chitwood. These papers, he suggested, might be published in the *Bulletin of Zoological Nomenclature*. It has been judged that the most convenient course would be to present this documentation to the London Congress for consideration in connection with Section 1 of Article 12 of the Draft *Règles* (1957, *Bull. zool. Nomencl.* 14 : 92). The following extract from Dr. Dougherty's letter, together with the enclosures to that letter, has accordingly been allotted the Congress Number Document 25/4 and is reproduced below. (Intl'd. F.H. 23rd January 1957)

(Extract from a letter, with enclosure, dated 20th November 1957)

Dr. Chitwood and I have reached a fundamental impasse with respect to the criteria by which names of higher taxa of the Order/Class and Phylum Groups are to be reckoned for the purposes of the Law of Priority. I am enclosing some appendices (II-V) to a recent letter to Dr. Chitwood. If any of this material seems suitable for the *Bulletin* (with appropriate recasting, of course), please let me know.

In the first appendix of my letter to Dr. Chitwood (of which an extra copy was not made), I indicated that I planned to send you copies of Appendices II and III ; subsequently to writing that, however, I recast the material a bit so that it came to be four appendices, instead of but two. A copy of this letter goes to Dr. Chitwood by way of explanation of this fact.

APPENDIX I

(not furnished to the Office of the Commission by Dr. Dougherty)

APPENDIX II

Determination of Names for Higher Zoological Taxa

A. Present Rules

In our recent letters we have been wrestling with problems that, in important respects, Copenhagen left unsolved. The volume *Copenhagen Decisions* (1953) has a Section D (pp. 38-43), which is entitled: "Proposed adoption of rules for the naming of Orders and Higher Taxonomic Categories".

One thing is immediately evident: the scheme for arriving at lists of recommended names for taxa of the Order/Class- and Phylum-Groups in the Animal Kingdom, as outlined in Decision 62 (pp. 38-40), has yet to be implemented. Certainly the suggestion (Decision 62(6)) that "Specialist Committees" have such lists ready and published before "the Linnean Bicentenary in 1958" was hopelessly optimistic. No one at the Copenhagen Colloquium gave any indication of understanding the enormity of the problems involved. (Certainly I did not realize it; only through our joint efforts, in fact, have I come to appreciate fully this situation.) The fact that, to my knowledge, Francis Hemming has not actively sought to have "Committees of Specialists" formed is, I feel, partly due to the intrinsic difficulties involved.¹ No doubt another factor has also played a critical role: he has, I believe, been overwhelmed with a flood of problems of all sorts, whose extent the Colloquium also failed to recognize. The implementation of the Copenhagen provisions with respect to names of higher taxa (i.e., those above the Family-Group) has, I surmise, been forced to a position of relatively low priority by the imperative nature of more urgent problems. The participants of the forthcoming London Colloquium will, I am sure, have a more realistic understanding of the time necessary for the realization of the goals set at Copenhagen (some of which will, I believe, be modified).

But, if I start with the body of law enacted in 1953 for deciding on names of higher taxa, I can, I feel, illuminate some of our problems rather more adequately than has been done so far by either of us.

¹ See Document 25/1, paragraph 3.

As regards higher-taxon names already proposed, I draw your attention first of all to main Decisions 63 (p. 38) and 62(1) (pp. 31–34) and quote from the following : “ [Decision 62] The Colloquium recommends . . . Decision 62(2) . . . [that the] Commission should be asked to invite the Committees of Specialists, when selecting names to be included in the recommended lists, to give first consideration to weight of current usage, and, when usage affords no clear basis for choice, to other considerations, such as priority . . . ”. This is the ruling on which I base my preference for Nematoda as a Class or Phylum name. It implies, of course, a “ popularity contest ”, of which you have been bitterly critical. If enough other zoologists feel as you do, this rule can be changed. I happen to agree with it on principle ; but I fully realize that its application has many pitfalls.

Let us, for the sake of argument, decide that this is not a good rule—that, instead, some rule based on the Law of Priority should be substituted for it. Now we are faced with the problem of just what way in which to formulate such a rule.

Over the past months you have gradually clarified your thinking on these issues—partly, I am sure, under pressure from me. I believe that you have been primarily drawn to your present position by your desire to preserve Nemata as the phylar name for the nemas (=nematodes) and that most other aspects of the problem have been subordinated to this aim.

B. Your Proposal for a Rule on Acceptance of Names of Higher Taxa

In your last letter (of Nov. 14th) you enunciated a principle (hereinafter referred to as “ [your] Rule ”) that is quite clear—I quote : “ The only formula we can arrive at is that the stem of the name used by the man who made the final logical exclusions should be accepted if he used that name at the rank we use today ”. At first glance, this would seem a fair enough rule, but, if ever formalized, it could lead to endless confusion as I can immediately show.

Before going into the more important objections, I should point out that, from your standpoint, it would have one effect that you may not have perceived and would, I believe, not wish : namely, it would ensure the preservation of a name with the stem *aphasmid-*, whether at the Subclass or Class level, for certainly Aphasmidia (subclass) and Aphasmeida (class) are the first names used for taxonomically rational taxa at those levels (Adenophori having followed Aphasmidia at the Subclass level and Anenophorea being far junior to the Aphasmeida at the Class level). (Secernentea as a Class name would, on the other hand be valid under your Rule since it would be a replacement for the homonymous name Phasmeida.)

The difficulties with your Rule are more deep-seated than this, however ; they derive from two main facts. First, its implementation would require an intrusion of taxonomy into nomenclature far more than any formal provision of the Rules now provides for—with the all-important exception that, fundamentally, the “popularity contest” provision, of which you are so critical, would allow full operation of taxonomic (or systematic, if you will) ideas to operate in reaching ultimate decisions on nomenclature. (Incidentally, this principle is also extended to names of the Family-Group—see Decision 45 [p. 33].) Second, your Rule would require junking what I should call the “Principle of Co-ordination” of taxa of the Order/Class- and Phylum-Groups (see Decision 66 [pp. 41–42]).

C. Nomenclature *vs.* Taxonomy

One of the most difficult problems in the nomenclatural aspect of systematics at the level of higher taxa is that of deciding to what extent use of names should vary according to taxonomic concepts. Given the *Règles* as amended at Copenhagen and general zoological traditions in nomenclature, going far back of Copenhagen, I contend that one should strive, in settling on rules of nomenclature for higher taxa, to minimize the obtrusion of taxonomic concepts.

Your Rule could, however, embroil zoology in an endless confusion in many cases. In effect, it would abolish the fundamental usefulness of the type system for higher taxa (type genera in these cases). You may be inclined, I should anticipate, to dispute that it in any way would interfere with the type system, but I think that it can be fairly shown that it would. Types are useful primarily as anchors for names when there are, as is inevitable in our growing state of taxonomic knowledge, changes in taxonomic systems.

Let us examine what would be the full implication of a rule requiring that that name be used that was first applied to a group after “logical exclusions” [= a “natural” group] had been made. This brings us full-tilt into taxonomic issues that I feel strongly should be left out of nomenclature.

Look what would happen in an extreme case if it were necessary (as it would be in the strict application of your Rule) to accept any name change associated with a change of content in a given taxon ; in such cases, it is obvious that the type would stand for little. Let us imagine a higher taxon *X* of a given rank that, when originally named, contained subordinate taxa (orders, families, genera—it doesn’t matter much for the sake of this discussion which they were) ; let us call these subordinate taxa A, B, C, D, E. Now, by

your Rule, any of the following would result in a condition in which a different name, if proposed, would be binding :

- (1) A, B, C, D, with E removed (with name change of group to *Y*) ;
- (2) A, B, C, with D and E removed (with name change to group to *Z*) ;
- (3) and so on through many permutations.

A worker accepting concept (1) would be bound by name *Y* ; a worker accepting concept (2) would be bound by name *Z* ; etc. Similarly, the introduction of any other subordinate taxon (*F*, *G*, etc.) into *X* would require that any name-change undergone by *X* be binding. The ultimate consequences of this are ridiculous. And what is a valid group anyway ? In fact, who are we to say that a group is valid ?

No, I think we need the type system for higher taxa, just as we do for species, genera, and families and taxa subordinate to these. We need to keep names that were originally proposed for largely reasonable groups and to follow the same system of restriction as we do for names in the Species-, Genus-, and Family-Groups.

The one escape mechanism is the " popularity contest " provision. With it, totally irrational groups can be ignored and, in general, prevailing usage maintained.

D. Coordination of Taxa

I admit that I have been critical of the idea of coordination of names of the Family-Group and that, to be consistent, I should also object to this principle at higher levels. The Botanists in their Code eschewed coordination from the very first—from the species level up. Concomitantly with this, however (and perhaps with wisdom) they have steadfastly refused to legislate the application of the Law of Priority for taxa above the level of order ; they have gone so far as to exclude such taxa explicitly from Priority. In many ways it is clear that the Botanists are twenty years ahead of the Zoologists in the perfection of their Code—although it is perhaps not fair to judge the two Codes in this way, for the plants strike me (on the basis of my recent studies of their evolutionary interrelationships) as posing less difficulties of classification than the animals. However, it may well be that, all this admitted, the Botanists have been more unified and clear-headed ; and their rejection of coordination strikes me as a good example of clear thinking. The fact is that they do have a better Code than we in Zoology. (In fact we have been essentially without a Code—in the sense of codification—since the revolutionary

changes of 1948 at Paris, for, at that time and subsequently, much vital innovation has been made [as in the Copenhagen volume and also as published in the *Bulletin of Zoological Nomenclature* and in various of the *Opinions, Declarations* and *Directions* published by the International Commission in the series *Opinions and Declarations Rendered . . .*.]

I have done some soul-searching the last few weeks and especially the last few days, and have decided that, since the Principle of Coordination, as applied to species and genera, was a part of zoological nomenclature with the earliest official Code (1900) and since this concept has already been legislated for higher taxa as well, at Copenhagen, it perhaps serves best the interests of nomenclatural stability in zoology to accept this extension of the concept in question all the way up the hierarchy of taxa. So I am now prepared to accept it at the Family-Group level too. If, however, the Principle of Coordination is to be reversed at the Family-Group level, the same should be done at the Order/Class- and Phylum-Group levels as well. But you will, I hope, see that, in your Rule, you are asking for a reversal of this long standing zoological tradition.

E. Summary and Conclusions

I pointed out that I regard your Rule as contravening two basic principles of zoological nomenclature—that of the type concept and that of coordination. It is ironical that, at present, at least, you must look in the Code to the very ruling of which you appear to disapprove most strongly, for a source of support. As far as I can see, every other provision is designed to keep taxonomic (or systematic, if you prefer) ideas out of nomenclature as much as possible.

Frankly, I think that the most undesirable feature of your Rule lies in the fact that, at the same time that it demands radical departures from much past nomenclatural tradition, it relies basically on another nomenclatural tradition. Thus, on the one hand, it would, in effect: (1) circumvent the type concept (type genus in the case of families and higher taxa); and (2) at the same time, reject the time-honored process of exclusion, which has traditionally not affected the names of species, genera, and families, and whose application, as a result of the 1953 legislation, would appear to have been extended to higher taxa. On the other hand, your Rule rests squarely on the Law of Priority. In other words, you propose sweeping aside certain important traditions and yet at the same time requiring that the essentially new concepts be supported by the Law of Priority.

Well, all this is possible, but I doubt that it is desirable. Personally, I should prefer not to indulge in radical departure from current rules, but rather to work with them insofar as I can in good conscience. It is a strange

predicament. On the one side I support the *Règles* as they are constituted, with full appreciation that the "popularity contest" provisions for higher taxa run counter to many of the traditional concepts of the earlier Code (but I accept the "popularity contest" provision because I realize that the *Règles* before 1953 did not have any explicit provisions for determining the names of higher taxa; except a few for families and subordinate categories thereto. Thus the very promulgation of such rules was a radical departure; and it does not disturb me if, in their promulgation, essentially new concepts are introduced). On the other side you would, in effect, replace the legislation of 1953 with concepts that are, in their way, as novel as the "popularity contest" provisions.

I am willing for the sake of our paper to go along, in the main body of the text, with your Rule and to express my demurrals in footnotes. But I hope that after reading and digesting what has been written in this appendix you will abandon some, at least, of your position. There are certainly many moral points to back you; but, as I have said to you often, nomenclature as it has generally evolved has operated in certain vital respects independently of taxonomy and systematics. It has traditionally been a means of getting stable names—not of honoring people. The adding of author's names and dates has been (properly) regarded as an abbreviated bibliographic device. You are highly ambivalent on this matter—at one time you state that authors' names should be left off of higher taxa in order to discourage the incentive for personal glory; another time you say that, for a given taxon, that name should be used that was applied to it at the time the group was first accurately characterized at the level accepted by you and that this is only right because it honors the person responsible (von Linstow and Cobb being two of your heroes in this connection]. I can only say that, to me, these are scarcely consistent viewpoints.

But I say what I said before—to me this is a non-Aristotelian world. Therefore, I do not object to multiple logical systems; but I do like to know what I am doing and to be able to recognize where I am applying one set of logic and where another. I want you to do the same. Otherwise you will not be adequately prepared to meet the challenge of those whose systems of logic differ from yours.

APPENDIX III.

Conditions Causing Homonymy between Names of Higher Taxa

At the outset, let me explain what I believe the International Congress of Zoology means with respect to homonymy of names of higher taxa (see *Copenhagen Decisions* . . . , p. 42, Decision 68). In the Copenhagen volume it is stated that [in addition, by implication, to the fact that two names are

homonyms when they are of identical spelling] two names that differ only in "termination" are also homonyms. You in effect raised the question of how this ruling is to be interpreted—in your letter of the 7th. Having been at Copenhagen, I can say that a lot of discussion went into the rulings later published as the *Copenhagen Decisions* . . . , but that in some cases the published version fails to do full justice to those discussions. In the particular case before us I can say that it is quite clear to me that what the Colloquium had in mind with respect to "terminations" were only the common neuter pleural adjectival endings *-a*, *-ea*, *-ia*, *-ida*, *-ina* (*-oidea* is a special case, which I discuss further on).

By contrast with the foregoing, when a compound word is made by combining the appropriate parts of two latinized Greek words (or two Latin words, or a Latin plus Greek or Greek plus Latin word in hybrid combination), a different word is formed, and the second part cannot be considered as a "termination" in the sense of the Copenhagen decision in question. Instead the stem (or root) of the compound word consists of both parts up to the declensional (usually adjectival) ending (*i.e.*, *-a*, *-ea*, etc.). [Since I first wrote the foregoing paragraph, it has become evident that you independently arrived at the point of view there expressed (*vide* P.S. to your letter of Nov. 9th).]²

As an example, let us take the case exemplified on the one hand by the series that, in Pearse's system (1942), ran *Spiruria-Spirurida-Spirurina*. Here the difference lies in the *-ia*, *-ida*, *-ina* suffixes, which are merely adjectival in force and do not contribute any other meaning to the word than indication of the rank for the respective taxa. But, on the other hand, take the word *Spiruromorphina*, which I have suggested as a replacement for *Spirurina*: this consists of combinations of three Greek words, *σπειρα ούρά*, and *μορφή*, plus the Latin adjectival ending *-ina*. To be more exact, the word is made up as follows—from:

- (1) *σπειρα* → the stem *spir-*;
- (2) *ούρά* → the stem *ur-*;
- (3) the connecting vowel *-o-* ;*

² The letter here referred to was not furnished to the Office of the Commission by Dr. Dougherty.

* This is normal for Greek when two words are combined to give a compound word and the stem of the first one and the derivative of the second one begins with a consonant. For compound Latin words in the same situation the proper vowel is *-i-*. With hybrid (Greek-Latin or Latin-Greek) words the connecting vowel is *-o-* if the first part is Greek and *-i-* if the first part is Latin. (This last rule explains why *nematocide* [Greek-Latin] is to be preferred to *nematicide* [where the Latin combining vowel is used with a Greek stem]. Despite Cobb's contention, it did not usually make any difference, in the best Classic Latin, if a word had already been adopted from Greek; it still kept its "*-o-*" connecting vowel in hybrid compounds. A good classic example is *thermō-pōto* [*< θερμός*, *hot*; and *pōto*, *to serve drinks*—hence, *to refresh with hot drinks*].

- (4) $\mu\omicron\rho\phi\pi \rightarrow$ the stem *morph-* ;
 (5) and, finally, the adjectival ending *-ina*.

Result : *Spir-ur-o-morph-ina*. The stem of this new compound word is *spiruromorph-*, and names based on the new stem *spiruromorph-* should pose no problems of homonymy with words based on the old stem *spirur-*. The contrary view, to my way of thinking, does violence to good linguistic sense.

I must admit, however, that the suffix *-oidea* is an embarrassment here. It is the neuter plural of Latin *-oideus*, *-oidea*, *oideum*, which in turn derives from the uncontracted Greek adjectival suffix *-οειδης*, which in its turn derives from the Greek word *εἶδος* (*form*). Viewed in this light, it might be held to have as much right to conferring independent status as should (I believe) *-morph(ina)*. There is this difference, however : *-oidea* has been adopted in an adjectival sense for superfamilies and made homologous with the endings *-idae* and *-inae*, which apply, of course, to families and subfamilies respectively. (As indicated in an earlier letter [Nov. 4th—sent Nov. 7th],³ *-idae* is a Latin noun suffix [masculine plural] and *-inae* a Latin adjectival suffix [feminine plural]). But I believe that the neo-Latin usages of all these endings have reduced them to the same function (adjectival in force) as those of higher categories and that, for purposes of determining homonymy, they should not usually count as parts of stems. I think that exception should be made, however, where the Greek word *εἶδος* is specifically given as the basis of word formation (just as I should propose in the case of $\mu\omicron\rho\phi\eta$).

No doubt the International Commission needs to go over these points and spell them out more clearly.

With respect to the ending *-acea* and your contention about it in your letter of the 5th, yes—I know that it is adjectival in force, being a Latin adjectival ending (like *-inae*, etc.) and that it has been used in Botany in the feminine [sic] plural form, *-aceae* [sic] (agreeing in number and gender with the kingdom *Plantae*), for families of plants. However, I doubt that it will find a place in any system of uniform zoological endings and therefore should prefer to leave it alone in the case of the word *Gordicea*. Your emendation *Gordea* does, it is true, preclude any possible future conflict in this respect. I should point out, however, that, in this sense, the stem of *Gordicea* is *gordi-* and that *Gordiea* is a better emendation than *Gordea*. (Contraction of vowels was, of course, done in both Greek and Latin, but in the case of the neo-Latin names for the Family-Group, it has become the rule that the endings *-idae*, *-inae*, etc., should be added to the complete stem—thus the family *GORDIIDAE*).

³ The letter here referred to was not furnished to the Office of the Commission by Dr. Dougherty.

The same rule has been applied to the higher taxa of plants. It therefore would seem logical to follow this practice for names of higher zoological taxa—at least in making emendations where a stem of a higher taxon has already been established—as *gordi-* in Gordiacea.

My discussion of the status of compound words that include, as a suffix, a derivative of *μορφή* should, I feel, explain why I hold that it is improper to create a word *Archescoleca* or *Archaeoscoleca* and attribute it to Huxley. The name of Huxley's taxon *Scolecida* was derived only from *σκώληξ*; your alternative names are derived from *ἀρχε-* or *ἀρχαῖος* and *σκώληξ*. Linguistically they are hardly equivalent to *Scolecida*; nor can they reasonably be so nomenclatorially.

APPENDIX IV

A Summary of My Personal Views on Determining Names of Higher Taxa

1. General Principles

(1) I believe that the first consideration to be given to deciding the name of a higher taxon is universality and stability of usage; to me—as in the present *Règles*—this takes precedence over considerations of priority, etymology, etc. if it appears to me that a given name is important and most widely used as between two or more contenders, I don't give a hoot or holler whether it is historically etymologically, or philologically legitimate; where there is no overriding consideration of usage, however, I feel that priority is the best guide;

(2) I believe that, once a name is decided upon, its documentation should be determined: I hold that, as much as possible, this should be accomplished as an exercise of nomenclature, with minimal obtrusion of taxonomic concepts;

(3) I subscribe to the following further principles with respect to deciding the author and date of the name of a given higher taxon:

(a) the author of a higher taxon (*i.e.*, of the Order/Class- or Phylum-Group) is the first person to use the name at any level above the Family-Group (and for this purpose the adjectival group-endings can be ignored—it is the stem [or root] that counts); the date is that of first usage; it makes no difference whether the original grouping was unnatural, as long as it held a genus that would fit as a modern type genus of the taxon;

(b) where the original nominal group was taxonomically unnatural, I do not believe that, from the point of view of nomenclature, any recognition of the restricting author need be extended by analogy with the treatment of lower taxa (promotion or demotion of rank can, however, usefully be indicated by the device of parentheses ; and changes in spelling [of the ending and, to a limited extent, of the stem] can be indicated by the device of square brackets) ;

(c) if, however, it were desired to indicate that some other than the original author were responsible for the taxonomic concept of a given nominal group, this could be indicated by using "*sensu*" followed by author and date.

2. The meaning of §, #, and ‡, of another symbol † and of the word "*partim*"

(1) By § I mean that a name, as originally used, contained groups not now in the group bearing the name, or, in the case of a synonym, not in the group with which it is synonymized ; from the nomenclatural standpoint, however, I regard such groups as more or less co-extensive and as having the same type genus ;

(2) by # I mean that a name, as originally used, did not contain a group or groups known at the time the name was proposed and now included in the group bearing the name, or, in the case of a synonym, now in the group in which the synonym is listed ; from the nomenclatural standpoint, I again regard such groups as more or less co-extensive and having the same type genus ;

(3) by ‡ I mean that a name, as originally used, applied to a group now entirely included within, but comprising only a part of, the group with which it is synonymized ; such groups have a type genus different from that of the group with which the included group is submerged, but the type genus of the included group is considered as not being separable, at least for the time being, from that of the including group ;

(4) with "*partim*" I had wished to convey a quite different concept (I now feel I should abandon this proposal) ; what I had intended was that, where two or more groups were originally united under a name, but would at present be regarded as not belonging together and where no one had restricted the name to one of the natural group originally included and, furthermore, I should not wish to restrict it, I should list the name in the synonymy of each of the originally included groups, but qualify it in each case with "*partim*" ; this was meant to indicate immediately that the name in question would be found in the synonymy of more than one group (you have, I believe, misunderstood this ; the fault is, however, mine for not being clear) ; I now think that another symbol should be used and propose † (which, appropriately enough, also means dead) ;

(5) I now have decided that "*partim*" should be restricted to the sense in which it is usually applied in generic synonymies—*viz.*, to qualify a group name that, when first proposed, included that nominal group (or members of that group) in whose synonymy the name is being listed, but that, as now treated, does not or should not contain the former (or members of the former).

All these symbolic devices are designed to convey taxonomic concepts in what is otherwise a formalistic nomenclatural system.

3. Possible Modification and Amplification of Existing Rules

I have given what I consider the most reasonable analysis of the *Règles* as they apply to higher taxa. However, I am not strongly wedded to any one nomenclatural system. I can see some virtue in the argument that nomenclature and taxonomy should be brought somewhat more into line and that the rules for crediting authorship for higher taxa might be somewhat different from those used for families, genera, and species—or, what is implied more basically, that the choice of a name for a given taxon should be governed by considerations of systematic naturalness of the Group. But this, I feel, would be an exceedingly difficult thing to implement as an explicit provision of the *Règles*, requiring, as it would, a new departure in nomenclature, with, I am sure, unpredictable ramifications.

It may be admitted, however, that considerations such as the taxonomic naturalness have obviously played a decisive role in deciding in many zoological groups the names that are generally used. By consequence, such names will doubtless find their way on to the lists of names for zoological taxa such as are called for by the Copenhagen decision in this connection. Therefore, from this standpoint, the departure, mentioned in the immediately preceding paragraph, from past nomenclatural practices will, after all, play an inevitable role. But I believe that the Copenhagen provision that results in what you call a "popularity contest" handles this problem neatly and avoids a most difficult impasse—*viz.*, the obtrusion of taxonomic considerations into nomenclature to an extent that would greatly reduce nomenclatural stability.

I am sure that it is precisely because of this difficulty (which was perceived, but not exhaustively discussed) that the Copenhagen Congress on Zoological Nomenclature decided to have "lists" of names for the higher taxa of organisms prepared by "panels of specialists". The issue of applying priority was specifically by-passed in this situation, although priority was declared binding in determining the status of names for higher categories created in the future. The effect of this is that experts in each group have a chance to express preference for a given name or set of names on any basis they desire. This the

taxonomic problem tends to be divorced from the nomenclatural; for nomenclature would be fixed only after experts had decided the names in each group on whatever grounds they regarded as suitable.

A further effort to avoid, as much as possible, the intrusion of taxonomy into formal nomenclature was the Copenhagen decision to require type genera for higher taxa. Such type genera are meant to anchor names (*i.e.*, nomenclatural entities) into taxonomic systems.

In essence, the points that we have been arguing back and forth these past months are ones in which we have failed to agree on the relative roles of nomenclature and taxonomy (or systematics, if you will) in determining the choice of names.

I repeat—I hold that a nomenclatural system that depends as little as possible on taxonomic concepts—*i.e.* is “automatic”—is the best. Otherwise one is continually beset with problems of whether a given name should or should not be used because the taxonomic concept originally embraced by it is not that accepted today.

But what we have been arguing over is certain to reach the International Commission. We could no doubt serve a valuable function by organising our thinking, including our conflicting views, and presenting all for the Commission's consideration.

APPENDIX V

Application of the “Règles” to the names

Nemata vs. Nematoda, etc.

With the background of Appendices II–IV we can now conclude with a discussion of the relative status of *Nemata* and of *Nematoda* and its variants.

First of all, let us make clear the taxonomic (and systematic) problems. Both of us recognize that, in the nemas and horse-hair worms, we have two groups of independent phyla. We obviously need names for these taxonomic entities. So far there is, I am sure, no disagreement.

I believe we also agree on the essential historical facts—the earlier ones at least. Originally the name *Nematoidea* was given to an order containing both nemas and horse-hair worms—though primarily the former. This nominal order was, we feel, unnatural. Subsequently the *Nematoidea* (and various

linguistic equivalents : Nematodes, Nematoda, etc.) fluctuated from the level of Order down to that of Family and up to that of Phylum, in accordance with the views of various investigators. Although the horse-hair worms were taken out of the nominal group Nematoidea by von Siebold in 1843, certain nemas (especially mermithids) were confusedly placed with the horse-hair worms until Vejdovský in 1886 clearly distinguished between the two groups and segregated the latter (as in the order Nematomorpha) from the former.

A point that we have never discussed is that Vajdovský, at the same time as he made a logical grouping for Nematomorpha, restricted the vernacular term "Nematoden" to an "Ordnung" for the nemas and thus created a completely natural nemic taxon. I do not know who was the first person to translate Vejdovský's concept into a formal neo-Latin word, but it is to be noted that Grobben (1909) had "Ordnungen" Nematodes and Nematomorpha in his Klasse Aschelminthes. So it seems clear that at least one variant of the word "Nematoda" was validly restricted to the nemas before Cobb created his phylum for them in 1919.

Now it appears that, under your Rule, the correct name for the Phylum of nemas would be Nemata. But what do the present *Règles* require ?

Insofar as they provide a guide, the following are the interpretations that appear to me to fit the *Règles* most closely :

- (1) Nematoidea Rudolphi, 1808, is coordinate with all taxa of that name above the level of the Family-Group ;
- (2) Nematoda Diesing, 1861, an orthographic variant of Nematoidea, is the name that would win, hands down, under the *Règles* "popularity contest" provision ; B. G. Chitwood is almost alone in favoring Nemata ;
- (3) Nematoda as a taxon was validly restricted to the nemas—at least by Grobben (1910) and probably earlier ;
- (4) in its promotion to phylar rank it must be reckoned as having maintained the same priority that it had at a lower level (see *Copenhagen Decisions . . .*, 1953, Decision 66 [pp. 41–42]) ; at the phylar level it therefore has priority over Nemata Cobb, 1919, even though it was promoted subsequently by Potts.

Now, I don't hold that all these rules are necessarily good ones. I tend to question, for example, that a promoted name should have priority over another name if the promotion was done after the latter was proposed. This is a point that the International Commission should, I think, re-examine. If they reverse themselves on it, then, of course, Pott's promotion of Nematoda would not affect Nemata Cobb, 1919.

However, we are still left with Lankester's phylum Nematodea. By designation of a nemic genus as type, this would automatically become the name of the phylum of nemas under the Law of Priority even if coordination were done away with. To invalidate Lankester's group, the type system for higher taxa would have to be junked also.

In sort, I now conclude that I must point out in a demurring footnote in our paper that Nemata can only be validated either by changing the present *Règles* drastically, or by appealing for its preservation under the "popularity contest" provision.

My position is, as I have stated consistently, that Nematoda is the name of choice. If we accept this merely for the sake of argument, then how should it be documented? Using the device of parentheses to allow indication of the person to promote it to the rank in which I accept it and of square brackets to fix responsibility for the spelling now used, one would have:

Nematoda ([Rudolphi, 1808] Diesing, 1861) Lankester, 1877.

Now, if it were to seem desirable to indicate also the sense in which this name is being used—*i.e.*, the person responsible for the taxonomic concept associated with the name, this could be indicated with the device of "*sensu*". One should then have:

Phylum Nematoda ([Rudolphi, 1808] Diesing, 1861) Lankester, 1877, *sensu* Potts, 1932.

An even more extended version, which would indicate the fact that Lankester did not use the spelling Nematoda, would be:

Phylum Nematoda ([Rudolphi, 1808] Diesing, 1861) Lankester, 1877] Potts, 1932.

By this one would know that Rudolphi was responsible for the original word but did not spell it Nematoda, Diesing was the first one to use the present spelling, Lankester was the first to use it for a phylum, but not with the spelling Nematoda, and Potts was the first to use the spelling Nematoda at the phylar level. All of these are primarily nomenclatural facts and are not meant to document the historical sequence of taxonomic concepts that have been meant by Nematoda and its variants. The one taxonomic fact that would have to underly all, however, is that, in all its permutations, nomenclatural and taxonomic, Nematoda would be conceived as having the same type genus.

I realize that this leaves Cobb out, but to me the purpose of giving names and dates is, first and foremost, to document the nomenclatural facts. The taxonomic concepts are subordinate to these nomenclatural facts. It would, I

believe, be incorrect to put Cobb's name in the foregoing series because his word *Nemates* (or, emended, *Nemata*) is of different classic origin and thus should be treated as nomenclaturally different from *Nematoidea* and its variants, including *Nematoda*.

Lists of author's names such as follow *Nematoda* in the foregoing examples would obviously not be used except in places where detailed nomenclatural documentation would be needed. In most cases, one could write merely the "Phylum *Nematoda* Rudolphi, 1808" or possibly the "Phylum *Nematoda* Rudolphi, 1808, *sensu* Potts, 1932".

In finishing this difficult discussion, I might cite a few examples of the way in which I should apply the symbolism—§, #, ‡ and †. The examples can appropriately center around *Nemata*, etc. Since I have agreed that in the long paper your views should prevail in the text, I must assume first of all that the phylar name *Nemata* is to be used for the *nemas*. I believe that the main entry should be :

Phylum *Nemata* [Cobb, 1919] Pearse, 1936

The synonymy would be as follows :

† *Intestina* Linn., 1758 (Ordo-p. [n.v.]) [here † is used in the sense proposed in this letter ; "† *Intestina*" would also need to be listed in the synonymy of the subkingdom *Amera*.]

§ *Nematoidea* Rud., 1808 (Ordo—pp. 197, 198) [here the § means that organisms (*viz.*, certain horse-hair worms) were originally in Rudolphi's order, but we exclude them ; it also means, however, that the type of genus of Rudolphi's order is reckoned as the same as that of the phylum *Nemata*].

. . . [sundry names].

Gordiacea von Siebold, 1843 (Ordnung—pp. [362], 303), *partim* [here "*partim*" is used in the sense in which it is redefined earlier in this letter ; Gordiacea is included in this form in the synonymy of *Nemata* because it originally included certain *nemas* (mermithids), but no longer does].

† *Nematalmia* Vogt, 1851 (Klasse—pp. 174, 175) [here the † is used instead of "*partim*" in accordance with the revision proposed earlier in this letter].

Nematoidei Vogt, 1851 (Ordnung—p. 181) [here the # is used because Vogt's order did not include all *nemas* (*i.e.*, the mermithids were excluded)].

The foregoing examples should serve, I feel, to indicate how the symbols would function. I think, however, that using them for names in the Family-Group would be too complicated (at least at this time) for an enormous amount of checking would be required. I therefore propose that, if we are to use these symbols for the higher taxa, we nevertheless not use them for names in the Family-Group (except perhaps for ‡) and that this fact be specifically stated.



Dougherty, Ellsworth C. 1958. "Questions arising in connection with the naming of orders and taxa of higher rank." *The Bulletin of zoological nomenclature* 15A, 523–539.

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