

CASE NO. 1

DOCUMENTS RELEVANT TO THE CONSIDERATION OF
THE PROBLEM OF SECURING GREATER STABILITY IN
ZOOLOGICAL NOMENCLATURE

DOCUMENT 1/1

Origin of the present investigation

The record of the decision by the Thirteenth International Congress of Zoology, Paris, 1948, inviting the Secretary to the International Commission on Zoological Nomenclature to examine, in consultation with interested specialists, all means which might secure greater stability in zoological nomenclature and to submit a Report thereon was published in 1950, *Bull. zool. Nomencl.* **4** : 234.

DOCUMENT 1/2

A survey of possible means for promoting stability in zoological nomenclature prepared by FRANCIS HEMMING, Secretary to the International Commission on Zoological Nomenclature, in response to the invitation of the Thirteenth International Congress of Zoology, Paris, 1948, was published in 1952, *Bull. zool. Nomencl.* **7** : 148-188.

DOCUMENT 1/3

Professor PIERRE BONNET (Toulouse) presented a proposal for the incorporation into the *Règles* of a provision recognising a " Law of Prescription " to the International Commission on Zoological Nomenclature on 15th January 1948 for consideration during the meeting to be held in Paris in July of that year during the Thirteenth International Congress of Zoology.

The provision so proposed to be inserted in the *Règles* was as follows :—

Loi de Prescription : Tout nom qui sera resté ignoré durant de nombreuses années ne pourra, pour cause de priorité, remplacer un nom de genre ou d'espèce depuis longtemps utilisé ; s'il y avait lieu de le faire les spécialistes en décideront.

For the full text of the foregoing application, see Bonnet, 1950, *Bull. zool. Nomencl.* **3** : 177.

DOCUMENT 1/4

The proposal by PIERRE BONNET (Toulouse) for the restriction, in the interest of stability in zoological nomenclature, of the Law of Homonymy as applied to generic names ; proposal presented to the International Commission on Zoological Nomenclature on 15th January 1948 for consideration during the meeting to be held in Paris in July of that year during the Thirteenth International Congress of Zoology.

The provision so proposed was that the following new provisions should be inserted in the *Règles* ;—

Article 34.—Homonymie : Tout nom générique n'est rejeté comme homonyme qui s'il a été utilisé pour un autre genre dans un même grand groupe d'animaux ou dans un même pays : les grands groupes d'animaux sont : Protozoaires, Spongiaires, Coelentérés, Plathelminthes, Annélides, Némathelminthes, autres Vers, Crustacés, Myriapodes (*s.l.*), Insectes, Arachnides, Echinodermes, Procordés, Vertébrés.

Article 34bis.—On ne reviendra pas sur les changements faites jusqu'en 1920 ; mais à partir de cette date seront maintenus contre le nom nouveau proposé tous les noms ayant déjà une grande vogue et qui depuis de nombreuses années étaient employés sans la moindre confusion avec un homonyme méconnu d'un autre groupe.

Article 34ter.—Cependant, à partir de 1940, il ne sera plus toléré qu'on utilise pour un nouveau genre un nom déjà existant et se trouvant dans le *Nomenclator Zoologicus* de Neave (1939-1940).

For the full text of the foregoing proposal, see Bonnet, 1950, *Bull. zool. Nomencl.* **3** : 178-179.

DOCUMENT 1/5

By the late LODOVICO DI CAPORIACCO
(University of Parma, Italy)

Statement dated 19th January 1948

LAW OF PRESCRIPTION : COMMENTS ON PROFESSOR P. BONNET'S
PROPOSITION No. 2

Proposal No. 2 of Professor Bonnet (proposal to add a new Article (Article 27 bis : Law of Prescription)) : "No name which has remained ignored for many years can, for reasons of priority, replace a generic or trivial name which has been utilized for a long time : if such a case arises, specialists will decide."

I agree with the intention of the proposal, but this seems to me formulated in too vague a manner : *many years* is subject to personal, i.e. variable, interpretation ; I think it is necessary to fix the time which gives the right to prescription : for instance, one might fix 30 years, or, say, 50 consecutive papers in which, instead of the oldest name, a later one has been used. It seems to me impossible to agree with the proposal to let "specialists" decide in dubious cases, because how can we decide *who* is a specialist ? Dubious cases must be decided by the Commission, who, of course, listens to the opinion of specialists.

DOCUMENT 1/6

By the late LODOVICO DI CAPORIACCO
(University of Parma, Italy)

Statement dated 19th January 1948

COMMENT ON PROFESSOR P. BONNET'S PROPOSITION No. 3
RELATING TO RELAXATION OF THE LAW OF HOMONYMY IN
RELATION TO GENERIC NAMES

Proposal No. 3 of Professor Bonnet (proposal to modify Article 34) : "A generic name must be rejected as a homonym only if it has been utilized for another genus in the same major group of animals or in the same country," etc.

I am absolutely *against* this proposal, which would be perhaps convenient in purely systematic works, but would cause great trouble and confusion in faunistic or other papers. For instance, a genus may be discovered in a country where another genus of the same name does not exist ; then the name of the discovered genus, though identical with the name of another genus, would be valid ; but afterwards the "homonym" genus may be found to extend also to the country where species of the first-named genus occur ; then the second genus would have to change its name. That would only cause confusion. Moreover in the case cited by Professor Bonnet, the name *Argyrodes* (Araneae) ought to be changed in any case, even if the proposal of Professor Bonnet were to be accepted since *Argyrodes* (Lepidoptera) is found in Hungary, which is a part of Europe, where *Argyrodes* (Araneae) is found also.

DOCUMENT 1/7

Proposal submitted by Dr. HENNING LEMCHE on behalf of a large group of Scandinavian Zoologists

Law of Prescription

The proposal (received on 7th June 1948) for the incorporation into the *Règles* of a Law of Prescription submitted by HENNING LEMCHE on his own behalf and on that of a large group of Scandinavian zoologists for consideration by the International Commission on Zoological Nomenclature at the meeting to be held in Paris in July of that year during the Thirteenth International Congress of Zoology.

The provision so proposed to be adopted was the following :—

A name of a genus or species, given before the year 1850 and not used in scientific literature since 1st January 1850, is to be rejected in favour of such other name which has been in general use for the genus or species in question since that date.

For the full text of the foregoing proposal, see Lemche, 1950, *Bull. zool. Nomencl.* **3** : 158-161.

DOCUMENT 1/8

Statement furnished by

SMITHSONIAN INSTITUTION COMMITTEE ON NOMENCLATURE

Letter from Dr. CURTIS W. SABROSKY dated 25th June 1948

On the use of the Plenary Powers to Establish Nomina Conservanda

I am transmitting herewith, on behalf of the Smithsonian Institution Committee on Nomenclature, a statement and petition "On the Use of the Plenary Powers to Establish Nomina Conservanda." Because of delay in publication in *Science* and the consequent impossibility of thus securing many reactions to the petition in time to transmit them to you before the Paris Congress, we distributed mimeographed copies in order to secure representative opinions on the matter. All original material—signatures, comments, and letters—received in response to this solicitation are enclosed herewith, together with a copy of the mimeographed circular and a reprint of the statement as it appeared in *Science*.

In order to assist you in interpreting the signatures, I have prepared a list of the persons, grouped by institution or organisation, with the general field of study indicated for each person as Zoology, Paleontology or Entomology.

In summation, 156 (Group I) approve the petition as it stands (or with minor comments or reservations that do not seriously modify their general approval), 7 (Groups II and III) would either go much farther than this petition in restricting and limiting Suspensions, or would eliminate Suspensions altogether, and only 5 (Group IV) oppose the petition and stand strongly in favour of Suspension. One of the latter, Dr. Forbes, perhaps does not favour Suspension, for his opposition is on a different basis. Two reactions are difficult to classify.

Annexe to letter submitted on behalf of the Smithsonian Institution Committee on NomenclatureReprinted from *Science*, 21st May 1948, Vol. 107, No. 2786, pages 543-544**A PROPOSED PETITION TO THE
INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE**

The numerous applications now before the International Commission on Zoological Nomenclature, requesting exercise of the Plenary Powers for Suspension of the International Rules and establishment of a number of *nomina conservanda*, are commanding considerable interest among zoologists. The large number of applications, it is true, has resulted in part from the long hiatus in nomenclatural activity followed by a postwar revival of interest in nomenclature. Nevertheless, the number has become overwhelming, both for the Commission members who must study them and pass upon their merits and for zoologists who should give them thorough consideration in order that the Commission may have the benefit of various points of view. Some applications, moreover, are too briefly presented and inadequately justified, though the burden of proof should rest on the applicant.

It is generally recognised that letters from scattered workers are a less effective means of demonstrating approval or disapproval than expression by groups of workers. Accordingly, the undersigned subcommittee of the Smithsonian Institution Committee on Zoological Nomenclature, composed of taxonomists of the U.S. National Museum, the Bureau of Entomology and Plant Quarantine, the Geological Survey, and the Fish and Wildlife Service, has drawn up the following statement, in the hope that it will crystallise opinion and serve as the basis for representations to the Commission. The full Committee has approved the petition and directed the sub-committee to publish it for consideration by other zoologists.

Zoologists are invited and urged to send their reactions to the petition to the undersigned as soon as possible, with the understanding that the Committee will transmit this petition together with all names and comments, both supporting and disagreeing, to the International Commission. All communications at hand will be sent to the Commission in time for the International Congress of Zoology at Paris in July 1948. In order to aid the work of the Committee and to avoid misinterpretation, it is requested that, if possible, a brief and unambiguous statement of approval or disapproval be given, though as much other comment may be added as the individual wishes.

On the Use of the Plenary Powers to Establish Nomina Conservanda

It is not the immediate purpose of the undersigned to discuss the merit of individual proposals now pending, but to present their views on the general question of the use of the Plenary Powers by the Commission.

We believe that the International Rules of Zoological Nomenclature offer a sound foundation for ultimate stability and uniformity. In theory, at least, a standard to which there is no exception is a more solid basis than a standard whose application is weakened by numerous exceptions. In most cases, taxonomists could apply the Rules strictly, with little or no confusion and only a little inconvenience. On the other hand, we recognise that due consideration should be given to common usage of important names by the great body of nontaxonomists, the general zoologists, physiologists, medical men, teachers, and applied zoologists of all kinds. It is reasonable to conclude that the overall objectives of stability and uniformity would be distinctly furthered if zoologists would agree upon some basic standards satisfactory to both viewpoints.

In the case of a specific or generic name transferred to a different species or genus (e.g. the transfer of the specific name *malariae* from the quartan malaria parasite to the malignant tertian, if the Rules are strictly applied), both the element of inconvenience and that of confusion are involved. Such a situation is likely to present adequate grounds for suspension of the Rules. Misidentified genotypes commonly result in transfers, though not all such cases are of sufficient importance to merit suspension.

Changes resulting from straight synonymy usually cause inconvenience rather than confusion. Experience indicates that even apparently serious changes of this nature can soon become commonly accepted (e.g. the change

from *Stegomyia fasciata* to *Aedes aegypti* for the yellow fever mosquito). Such changes, accordingly, are not grounds for Suspension unless better reason can be demonstrated.

If too many suspensions are allowed, or if suspensions are allowed on weak or unconvincing evidence, the value of the Rules as a basis for stable nomenclature would be depreciated. We are convinced that the International Commission should as soon as possible reassure zoologists that such will not happen. We therefore petition the Commission to set forth some general standards concerning applications for suspension of the Rules and respectfully suggest the following for consideration :—

- (1) Applications for suspension of the Rules should be *required to demonstrate* that strict application of the Rules would result in *far-reaching and substantial confusion* in the taxonomic use of names and/or in a lamentable change that would *greatly confuse* the literature of a related field (e.g. ecology, malariology, stratigraphic paleontology).
- (2) Demonstration of confusion should include adequate bibliographic data showing the amount of usage and the percentage of uniformity. Mere assertion of “greater confusion than uniformity,” unsupported by these data, should be considered inadequate.
- (3) The Secretary of the Commission should return for additional evidence any application that does not contain the above demonstration and data.
- (4) When an application that appears to satisfy the above requirements has been received and notice of its pending consideration has been published, the Commission should on its own initiative actively solicit—not merely passively await—the advice of specialists or other interested and qualified persons or groups from a variety of countries, museums, societies, and educational institutions.

Concerning the method to be employed by the Commission in actively soliciting advice on cases under consideration, it is suggested that, in addition to the regular published invitation to all interested individuals, use be made of existing national, society, and institutional committees on nomenclature, or that special panels in the various branches of taxonomic zoology be established for the purpose. These panels might be headed by, or liaison with them maintained by, various members of the Commission, in order to relieve the Secretary of the extra burden of these contacts.

Curtis W. Sabrosky and R. I. Sailer.

U.S. National Museum, Washington, D.C.

Editorial Note

The foregoing application reached the Office of the International Commission on 12th July 1948, and was too late, therefore, to be considered by the Commission when it met in Paris nine days later. Among the proposals which had previously been received and which were discussed in Paris was a proposal

submitted by Dr. Henning Lemche on behalf of a large group of Scandinavian zoologists in favour of the introduction into the *Règles* of a Law of Prescription, which, if then adopted, would have limited the scope of the Law of Priority to a much greater extent than did the existing Plenary Powers of the Commission which it was the purpose of the application by the Smithsonian Institution Committee on Nomenclature to curb. It was because, for the reasons explained above, that application was not then before the Commission that during the discussions in Paris, I drew attention to that application, in my capacity as Acting President of the Commission and President of the Section on Nomenclature, and supported the proposal, which was then adopted, that the whole question of the means to be taken for promoting stability in zoological nomenclature should be deferred until the next (Copenhagen) Congress in 1953 and that in the meantime the whole subject should be subject to comprehensive study with interested specialists (1950, *Bull. zool. Nomencl.* **4** : 233).

2. The application discussed above was resubmitted to the International Commission in 1952 under cover of a letter dated 16th July, from Dr. Richard E. Blackwelder, who had succeeded to the position held in 1948 by Dr. Curtis W. Sabrosky, as Secretary to the body which had in the meantime been reconstituted under the title Nomenclature Discussion Group, Washington, D.C. The note covering this resubmission which was enclosed with Dr. Blackwelder's letter will be found as Document 1/42 in the present series (*see pp. 73-75*).

(intl'd) F. H. 25 February 1953.

DOCUMENT 1/9

Views submitted in July 1948 on behalf of the ZOOLOGISCHES INSTITUT DER UNIVERSITÄT WIEN, the NATURHISTORISCHES MUSEUMS WIEN, ZOOLOGISCHE ABTEILUNG, and the ZOOLOGISCH-BOTANISCHE GESELLSCHAFT

Editorial Note: Attention is drawn to a statement urging the need for securing greater stability in zoological nomenclature which was placed before the Thirteenth International Congress of Zoology at Paris in July 1948 jointly by (1) the Zoological Institute of the University of Vienna; (2) the Zoological Department of the Naturhistorisches Museum of Vienna and (3) by the Zoologisch-Botanisch Gesellschaft of Vienna. The text of this communication was published in 1950 (*Bull zool. Nomencl.* **5**: 77-78.)

It will be seen that the request put to the Paris Congress by the Austrian zoologists was that there should be accepted a principle under which a name which had been consistently applied to a given animal should not be changed in cases where the defect in that name was solely of a technical nomenclatorial character. The following is the text of the proposition which was then submitted by the Austrian zoologists :—

Jeder heute einheitlich gebrauchte, eingelebte wissenschaftliche Tiername ist ein unschätzbare nomenklatorischer Wert, ein Verständigungsmittel, dessen Zerstörung den Zoologiebetrieb schwer schädigt. Bis zur endgültigen Regelung der Verhältnisse ist daher jede Änderung eines einheitlich gebrauchten Namens zu unterlassen, wenn für die Änderung nur formal nomenklatorische (Prioritäts-)Gründe, aber keine systematischen Notwendigkeiten vorliegen.

DOCUMENT 1/10

By L. T. S. NORRIS-ELYE

(The Manitoba Museum, Winnipeg, Man., Canada)

Enclosure to a letter dated 20th October 1948

TYPE LOCALITIES

The type localities are determined by the locality from which the first species or sub-species were named and described; among many of the old authors, this locality is not named or the locality given is virtually useless, e.g., "Hudson Bay region" or "Eastern North America."

When most of the types were established, even many of those determined today, the ranges were rarely known and may not be finally delimited for many years to come, due to the lack of wide-spread collecting followed by competent taxonomic work; this may not be so in those cases where the type specimen is represented by a large series that formerly went under another name and has been shown to be another sub-species.

Until the respective ranges can be determined accurately, it is impossible for the taxonomist to select, for comparative purposes, really typical material from the central regions of the ranges, where intergradation is likely to be absent or, at least, at its minimum.

Under our present system of selecting the typical form, our model is not necessarily the purest in form, but must be the specimen that was first described, perhaps barely within what turns out later to be its range; it may well be an intergrade in point of fact. It was determined by chance rather than knowledge; this system exists almost throughout the whole field of zoology.

Had the type of *Peromyscus maniculatus bairdii* (Hoy & Kennicott) been taken, say, at Pelican Lake, Manitoba, instead of at Bloomington, Illinois, most of the topotypes would have been quite unrecognisable by the pelts, on account of their intergradation with *Peromyscus maniculatus osgoodi* Mearns which does not become really typical for about one hundred miles to the west. At Pelican Lake, fifty specimens may well show over a dozen marked variations; such instances could be duplicated by the hundreds.

The difficulties of the taxonomist are often much increased by this situation and any device that will lessen them, without creating new problems, should be given some consideration. The rules give no protection against two types of sub-species being described within a very few miles of each other, perhaps both of them being intergrades.

With the full realization that long-standing rules in world-wide use cannot be discarded, without making the remedy worse than the disease, the writer ventures to offer a suggestion that might ensure that comparisons would be based upon a much greater factual knowledge by using only material that is the least tainted by intergradation.

It is proposed that the types be retained, largely for (1) recording data as to discovery, description, priority, etc., and (2) for comparative purposes, as at present, but only until enough range data shall have been secured to show practically the whole range, without any reasonable likelihood of the discovery of any serious extension of the ranges so far discovered.

When the ranges can be reasonably well delimited, the centres of those ranges could be determined mathematically; from such centres, radii could be established to form areas to be called "centros" or some such name and all specimens taken within that area could be called "centrotypes" or some such name. Here, priorities of nomenclature would not interfere with the name stability, except when the type name had to be changed; there would be no first-named centrotypes.

With such an accessory system, consideration might well be given to the following possible advantages:—

- (1) Centros could be established for some of the vague type localities.
- (2) It would be impossible to establish centrotypes until really adequate work had been done on the entire ranges, thus ensuring eventual stability.
- (3) The centrotypes must be more truly typical than many or most of the present types in all characters, pelage, dental and cranial.
- (4) The system should put some curb on those who rush into print to name allegedly new sub-species, before becoming acquainted with some of the most fundamental facts; these tendencies are even far worse in the case of naming new species where intergrading is not recognised.
- (5) It should lessen the difficulties of the taxonomist and greatly increase the finality of his determinations.
- (6) It would make the assessment of degrees of inter-gradation of border-line specimens much more accurate and more obvious.

It may be questioned what would happen if the "lumpers" or "splitters" got to work on some adjacent ranges; the answer seems to be that a system of centros would meet the situation immediately by means of projecting new radii and forming a new or several new centros.

DOCUMENT 1/11

By THORD RAGNAR NORDENSKJOLD

(Stockholm, Sweden)

(1) Enclosure to a letter received on 25th May 1949

PROPOSALS FOR THE SETTLEMENT OF CERTAIN QUESTIONS OF NOMENCLATURE

A name of an insect which has become the accepted term among scientists should not automatically be supplanted or replaced by another name given by an earlier author but found later in scientific literature. The term originally used—"the most usual name" of the insect—could in many cases be retained as a synonym and constitute a *nomen quasiconservandum*, to be included and preserved in a (newly created) entomological central register. This *nomen quasiconservandum*, which is always to be regarded as a complement to the author's name and is given only secondary importance, should, when the insect is named, always be given in a footnote or within brackets after the older author's name which had been discovered later and which, in accordance with the principle of priority should be regarded as the original main name of the insect—as *nomen primum auctoris*. Alternatively, it is proposed that the Congress should recommend that the lists of synonyms at the end of every future scientific work should be so constructed that *nomina prima auctoris* are placed first and *nomina quasiconservanda* second (the latter followed by "(n.qc.)") among the number of synonyms for every species of insect. Whether the traditional name can be considered to possess such strong goodwill that it is really worth while retaining it for the future and codifying it should be decided in the first instance by a body chosen by the Entomologists' Congress but functioning also between Congresses. The final decision should be made by the International Congress of Entomology.

The creation of a central entomological register is very desirable. In addition to the listing of *nomina quasiconservanda*, its task should be to compile complete lists of synonyms, including all names for every species of insect used in scientific literature. In these lists the *nomen primum auctoris* (the oldest known author's name for the insect concerned) should be printed in heavy type and the *nomen quasiconservandum* in spaced-out heavy type.

The adoption of *nomina quasiconservanda* could alternatively be of a provisional character and be retained in each individual case only until the re-discovered original author's name has gained sufficiently wide acceptance. One might also consider a 50-year provisional arrangement, to remain in force until a firm new principle has been established regarding the use of *nomina quasiconservanda*. It should be noted that, in certain cases, it may be easier to reach agreement about the adoption of two names (a *nomen auctoris* and a *nomen quasiconservandum*) than of one (a *nomen primum auctoris* only). For the sake of uniformity, it would be better to use two names simultaneously for an insect than for different authors to use different names for the same insect in scientific works.

In the hope that some entomologist will adopt my proposal and that it will be considered as the basis for further discussions and to some extent improved upon, I herewith submit it for consideration to the International Nomenclature Commission.

(2) Letter dated 20th December 1952

In May, 1949, I wrote to you about the stabilization of zoological nomenclature (in respect of the species). I was mainly concerned with the nomenclature of the insect groups and my intention was to make a contribution towards solving the "nomina conservanda" question.

In this letter, which I hope will be put together with my earlier one—already, as I understand, in the dossier for new suggestions for nomenclature—I wish to develop further my earlier and perhaps somewhat confused line of thought and to some extent amplify and adjust it (in order to avoid difficulties in the practical application of my suggestions).

Assuming that an insect has been named in a certain way for a century or so, but that a subsequent discovery has shown that an author some fifty years earlier for instance has satisfactorily described the insect in question and given it a different name, causing this last-mentioned *primum nomen*, in accordance with the law of priority, to be raised to the insect's *nomen auctoris*, then the name that has been in general use during the hundred years or more should, in accordance with my 1949 suggestion, be retained as a subordinate attribute (a *nomen quasiconservandum*) in conjunction with the author's name for at least fifty years after the discovery of the "new" author's name.

A binominal system with regard to the insect would thus be involved to some extent. To this the following objections may be raised :—

- (1) This new system would be clumsy and unwieldy.
- (2) An insect can never be designated by more than *one* name. A double naming would be an absurdity in itself.
- (3) Owing to the accurate indications of synonyms provided and supervised by present-day specialists on different animal genera and owing to the existing practice of setting out, often in parentheses, during a certain transitional period, the earlier-used insect name after the "new" author's name, the proposed arrangement is superfluous.
- (4) In certain cases it would be difficult to decide upon *one nomen quasiconservandum*. Here, strictly speaking, *two* or even *three nomina quasiconservanda* would have to be added as attributes to the *nomen primum auctoris*. Such an arrangement would obviously be preposterous.

In this connection the following may be said :—

The binominal system, which of course is not absolute but only conditional, can be adopted in the lists of synonyms given in the scientific works. This method would prevent unwieldiness in the text of scientific works where an

insect's name recurs frequently, would reduce editorial difficulties, would bring down costs and would avoid any great deviation from current usage in the production of scientific works.

In that case one thing would be essential: the *nomen primum auctoris*, as such, would have to be indicated in some way *typographically* and preferably also *acoustically* (when spoken). Here, then, it would be appropriate to print the Latin word of the insect name in *spaced-out* type and (or) to add to it the word (*prim.*), an abbreviation of (*primum nomen auctoris*). This (*prim.*) would have the same effect as an "NB," as a "Warning." It would be an urgent request: "Look at the synonym list in this book."

In such a system the synonyms, as hitherto, would be arranged by priority, with one possible exception; viz. the *nomen* interimistically authorised as the *nomen quasiconservandum* by the International Nomenclature Commission or any of its special committees would always—even irrespective of its priority, by virtue only of its actuality—be placed immediately after (closest to) the oldest name of the insect and would by its fat type be specially characterised and distinguished among all the other synonyms as a *suprasynonym* (provisionally for only fifty years, let us say, if the entomologists so desired), after which the pure, *simple*, original nomenclature would be restored. The "new" *nomen primum auctoris* of the insect could then be regarded as having gained a sufficiently permanent footing to be able to stand alone by itself. The *nomen quasiconservandum* could be entirely dispensed with as being superfluous in the future.

The specialists of today perhaps may not find the above line of thought particularly obvious or desirable. Nevertheless, I venture to express this my opinion not because I am in a modest way a specialist on the Hemiptera and Heteroptera, but in the first place because I am a practical entomologist, a plant protection entomologist and in the second place a zoologist (general) and biologist. I present my proposal with the following motto: "A stabilized nomenclature for the species is a just and reasonable demand on the part of the practical entomologist or zoologist and biologist generally." With the present arrangement the latter cannot get a proper survey, even if the specialist can just manage to do so.

What would be the real innovation, the real change, if my proposal were adopted?

Clearly, those who wished could consistently employ the binomial system in all connections, written as well as spoken.

The use of the "double name" would, however, only be definitely imposed, if one can use such a term at all in the case of scientific congresses, in the following circumstances:—

- (1) When using in speech the name of the insect belonging to the category in question.
- (2) On the insect labels in scientific collections where there is no clear reference to the list of synonymns in a particular work (i.e. where the entire collection is not arranged according to some definite, clearly indicated, scientifically systematic work).

DOCUMENT 1/12

By W. D. LANG, D.Sc.

(formerly of *British Museum (Natural History)*, London) (*Bridport, Dorset, England*)

Paper dated 13th June 1951

SOME COMMENTS ON THE CODE OF ZOOLOGICAL NOMENCLATURE APPROVED BY THE INTERNATIONAL CONGRESS.

(1) The object of the Code is to get uniformity in Zoological Nomenclature, and so to frame the rules that they can be applied in only one way, thus leaving no loophole for a subjective interpretation.

This I hold to be impossible of attainment. However much the Code is simplified, the whole subject (to judge from a life-time's experience) is bound to be so complicated and to leave so much to individual interpretation, that anything like complete agreement among zoologists (apart from those who are unwilling to submit to any rules imposed from without) must for ever remain, in my opinion, only an ideal and an aim practically unattainable.

(2) If this is conceded, the only alternative to the present state of affairs is to add dictatorial powers to the International Commission, that is, to convert their present function of standardising certain names on regulated appeal, to the duty of ultimately standardising all names. A zoologist could then simply apply to the Commission for the standardised name of a given species. I think that most zoologists would gladly accept such an innovation, which, during their lives, would save them months, if not years, of time; and would grieve only those who regard the authorship of a name, not as a matter of general convenience, but as involving a moral right.

(3) Should such a change be made, I admit that the practical difficulties might be great, but should not be great enough to condemn the proposal without over-mastering reasons.

(a) First, it would greatly increase the work of Commission, and ways and means would have to be found of increasing the staff. Since the proposal benefits zoologists, it would be up to them to produce the means.

(b) In giving the Commission dictatorial powers, it is understood that in deciding what is the proper name for a genus or species, the Commission would be guided by the general principles inherent in the present Code.

(c) To decide upon the proper name of a species pre-supposes that the author who originally gave the name applied it to one species only; or that where the original name covers more than one species, that either a type-specimen was originally designated, or, if not, that a lectotype has since been chosen, or, if the type-material has been lost, a neotype has been instituted. On the other hand, this might not have been done, and in that case the Commission itself should designate a lectotype or neotype. There would then be no uncertainty about the type specimen, and therefore about the species to which the standardised name is given. It would make the identity of a species a *matter of fact*; for whatever is, or is not, a given species, the type-specimen,

by definition, is that species ; and whether or not any other specimen belongs to that species, must remain a *matter of opinion* varying with the different views of individual zoologists. The standardised name must be applied to the type-specimen, also standardised, if necessary.

(d) As with the species, so with the genus. The Commission should standardise all generic names with reference to the type-species (itself founded upon the type-specimen). More often than the specific type-specimen, has the type-species not been designated, either originally or subsequently, and in many cases has proved impossible of determination, except on debatable grounds. A dictatorial ruling on the generic name to be used and the type-species to be accepted would be a boon to zoologists, probably to a very large majority.

DOCUMENT 1/13

By CH. FERRIÈRE

(Genève, Switzerland)

Paper submitted through H. BOSCHMA (Leiden) under cover of a letter dated 9th August 1951

(This paper was presented to the Ninth International Congress of Entomology, Amsterdam, August 1951)

LE FARDEAU DES VIEILLES ESPÈCES INCONNUES

Dans plusieurs groupes d'insectes il existe un grand nombre d'espèces décrites par des auteurs anciens et qui sont restées depuis leur époque complètement inconnues. Leurs descriptions incomplètes ne permettent pas de les reconnaître et les types n'ont plus été étudiés. Des exemples nombreux de ces espèces qui encombrant les catalogues sont indiqués parmi les Hyménoptères Chalcidoidea, à l'occasion d'une étude monographique entreprise pour les espèces d'Europe. Des centaines d'espèces décrites par Fabricius, Spinola, Dalman, Fonscolombe, Walker, Westwood, Nees, Förster, Ratzeburg, et d'autres, sont impossible à reconnaître et sont le plus souvent placées dans des genres auxquels elles n'appartiennent pas. Que faire de ces espèces, pour la recherche et l'étude desquelles une vie humaine ne suffirait pas ?

Nous voudrions voir adopter par les systématiciens les propositions suivantes :

1. Toute espèce qui a été décrite il y a plus de cent ans, n'a jamais été retrouvée depuis lors et ne peut pas être reconnue d'après sa description, ne doit plus être prise en considération.

2. Si le type est perdu, cette espèce doit être rayée des catalogues et traitée comme si elle n'existait pas.

3. Si le type existe encore, il ne pourra être rendu valide qu'après avoir été redécrit dans le genre auquel il appartient vraiment. Le nom du premier auteur est alors mis entre parenthèse et le nom de l'auteur qui a fait la nouvelle description est indiqué à la suite.

4. Dans le cas de la redescription, par un auteur récent, d'une espèce restée inconnue et vieille de plus de cent ans, la question de la priorité en cas de synonymie est à discuter.

DOCUMENT 1/14

By the late K. W. DAMMERMAN

(Rijksmuseum Van Natuurlijke Historie, Leiden, The Netherlands)

Paper submitted through Professor H. BOSCHMA (Leiden) under cover of a letter dated 9th August 1951

(This paper was presented to the Ninth International Congress of Entomology, Amsterdam, August 1951)

PROPOSALS CONCERNING THE NOMENCLATURE OF FAMILY NAMES AND OF NAMES OF ECONOMICALLY IMPORTANT INSECTS

- I. 1. To restrict the ever continuing changes of names and to arrive at a greater stability in nomenclature, it is advisable to obtain as soon as possible a fixation, in the first place of family names of insects and of the names of the most important insects of economic interest (noxious as well as beneficial species).
2. In this fixation the names that were in general use at the time of the beginning of the *Zoological Record* (1864), and that generally were in use till the introduction of the International Rules (1905), as far as possible are to be stabilised. Moreover, it is very advisable that attention should be paid to the *nomina conservanda* proposed by Apstein (1915. *Nomina conservanda*; *Sitzb. Ges. naturf. Fr.* **5**: 119) and by Handlirsch (1915. In: *Schröders Hand. d. Entomologie* **3**: 79). (See also Dammerman, 1950, *Continuity versus priority in Nomenclature*, *Tijdschr. v. Entomologie* **92**: 34.)
- II. 1. To arrive at results within reasonable time, the following procedure is recommended:

During three years, starting with 1952, or as many more as will appear necessary, an attempt shall be made to fix at least ten names of families belonging to each of the following groups of insects: Hymenoptera 10, Coleoptera 10, Lepidoptera 10, Diptera 10, Rhynchota 10, Orthoptera, Thysanoptera, Mallophaga and Pediculina together 10, other Orders of insects 10;

Moreover, at least ten names of economically important insects belonging to each of the seven above-mentioned groups, and for each of the six following geographical regions: Europe (or, as the case may be, the Palaearctic Region), Africa, the Oriental Region, Australia, North America, and South America.

2. In the case of family names a beginning shall be made with the names that were in common use about 1864.

- III. 1. The present Congress is requested to appoint an international committee for stimulation and co-ordination of the work referred to in paragraph I above. This committee shall solicit co-operators or organisations to assist in this task. Individual investigators willing to co-operate shall be free to apply to the committee.
2. Co-operators shall observe the following rules :
- a. If an economically important insect is at the same time the type species of a genus from which a well-known family name is derived, the reviser of this family name shall communicate directly or through the above-named committee with the person who is charged with the fixation of the name of the insect concerned, in order to arrive as soon as possible at a unanimous proposal.
- b. The same procedure shall be followed in the case of an insect which is of great economic importance in more than one of the main geographical regions.
- IV. Any entomologist or zoologist who detects a new name which on the basis of priority would involve a change of a well-known family name or of the name of an insect of economic importance, shall abstain from introducing that name, and shall immediately communicate with the committee referred to above. This committee shall send the proposal for further consideration to the co-operator concerned.
- V. 1. Every year the names dealt with by the co-operators shall be published, if possible after having obtained concurrence of the majority of an existing national committee on nomenclature or of an existing national organisation of entomology.
2. The present Congress will be requested to obtain permission for publication of the proposals mentioned above in the English language in the *Bulletin of Zoological Nomenclature*.
- Moreover by announcement in the above named *Bulletin* or in whatever journal the present proposal is first published all other journals suitable for the purpose shall be requested at least to publish the proposed names, with a reference to the number of the *Bulletin* or to the other publication concerned.
3. One year after publication of the proposals, when the opportunity for discussion may be deemed to have been sufficient, the above-mentioned committee shall take a decision concerning the proposed names. All names having obtained the concurrence of at least two-thirds of the members of the committee shall be published at once, and zoologists will be requested to use in future these names, whilst the International Commission on Zoological Nomenclature will be requested to place these names as soon as possible on the *Official List*.
4. Names that have not obtained a majority of two-thirds of the votes of the members of the committee, shall not immediately be rejected, but shall remain under suspension during one year. The committee shall then again consider these names. These names shall be rejected only when the committee can recommend alternative names to be used as the valid names in their place.

DOCUMENT 1/15

Proposal by RUDOLF RICHTER

(Senckenbeigische Naturforschende Gesellschaft, Frankfurt-a-Main, Germany)

The proposal dated 18th October 1951 submitted by RUDOLF RICHTER (Frankfurt a.M.) that all names placed upon the "Official List" should be validated under the Plenary Powers (1952, "Bull. zool. Nomencl." 6 : 184-185)

In the above paper Professor Richter advocated the adoption of measures to promote stability in zoological (and palaeontological) nomenclature and suggested that all names accepted in the forthcoming international *Treatise on Invertebrate Paleontology* should be placed on the *Official List*. The specific proposal submitted by Professor Richter was the following :—

Alle Namen, die auf die Offizielle Liste gesetzt worden sind, sollen als endgültig geschützt gelten, unter Suspension der Regeln.

DOCUMENT 1/16

By D. SHENEFELT and J. T. MEDLER

(Department of Entomology, University of Wisconsin, Madison, Wisconsin, U.S.A.)

Paper submitted by the senior author under cover of a letter dated
9th November 1951

(This paper was published in *Science* on 10th August 1951)

**SHOULD A "LAW OF RECENCY" BE ADDED TO THE
INTERNATIONAL CODE OF ZOOLOGICAL NOMENCLATURE ?**

Proposal, counterproposal, and debate on zoological nomenclature have been presented in numerous zoological periodicals during recent years. Basically analysed, the literature appears to represent a clash of opinion between two groups : those who feel that it is desirable to follow strictly Article 25 (the law of priority) and limit the commission's use of plenary powers, and those who advocate more extensive use of the plenary powers by the commission, with less strict application of Article 25.

Individuals in both groups admit that, at present, the procedures used are confusing and unwieldy. Each side concedes that there is some merit in the other's case. It is not the intention of the writers to enter into this debate ; opinions of the two schools are perhaps irreconcilable in this generation.

The writers do feel, however, that constructive thought should be given by all zoologists to ways of preventing similar confusion in the future, especially by eliminating the words, too frequently found in the *Opinions* and the proposals in the *Bulletin of Zoological Nomenclature*, that such and such action "would clearly result in more confusion than uniformity." The debates being waged in all zoological literature about the status of names clearly indicate that confusion is not entirely a matter of the future, but a real concern of the present.

An examination of the sources from which this confusion has arisen reveals that the important ones are :

- (1) Lack of knowledge of the existence of a publication ;
- (2) Lack of knowledge of the exact date of publication ;
- (3) The "dragging-out" of questions by different schools of thought arising from differences in interpretation of the Code and its applications ;
- (4) Deliberate refusal to give up a name once it has been learned—i.e., deliberate failure to apply the law of priority and maintaining that its application will create "confusion."

Perhaps errors made in the past can be corrected only by rulings such as those being made by the commission under its plenary powers. But what about the future ? Is the commission to continue to use its plenary powers indefinitely (or be forced to do so by allowing the same causes of confusion to continue to exist) ? Is the commission to rule on its rulings as it apparently must do with the Meigen 1800 paper ?¹

It is evident that something is wrong when the commission has to use its plenary powers so often. There is a need for practical working procedure in nomenclature to avoid multiple use of names, especially at generic and suprageneric levels. The writers believe that if stability is ever to be obtained in the use of scientific names a "law of recency" must be added to complement the law of priority. Once a name is rejected it should not be used again as a valid name unless there are definite reasons for so doing and they are given in publication. The proposed "law of recency" might be stated as follows :

The names used in the most recently published article dealing with the taxonomy or nomenclature of a group or species shall be regarded as authoritative *until further publication is made* reorganising or otherwise disposing of the names involved and *giving the reasons why the changes are made*.

The need for such a law can be amply demonstrated. For instance, in 1915 Gahan² definitely synonymized many generic names with *Opius* Wesmael. In 1943 Hincks³ wrote :—

Bracon carbonarius Nees, 1834, is designated as the genotype of *Opius* by Westwood (1840). This species is also the type of the monobasic genus *Biosteres* Foerster, 1862. For the present it will be better to transfer the whole of the species now grouped under *Biosteres* to the genus *Opius* until such time as an examination of the species leads to a regrouping. There are rather a large number of generic names available in this section, but it is evident that Gahan (1915) regards the divisions as of little generic value.

While Mr. Hincks' action appears to be correct, it illustrates the necessity of the proposed "law of recency," which would *compel* the transfer of the names in *Biosteres* to *Opius* and the acceptance of the synonymy given by Gahan until such time as the group was revised and *the reasons published* for changing the system used by Gahan.

The confusion that has existed with regard to *Bracon* Fabricius, *Cremnops* Foerster, and *Microbracon* Ashmead also illustrates the case in point. In 1917 Gahan⁴ transferred the name *Bracon* to replace *Cremnops*. This name change was accepted in North America but rejected in Europe. Finally, in 1935, the

question was decided by the International Commission, but the results were not published until 1943.

Although illustrations have been drawn from the family Braconidae, numerous other examples could be given—e.g., *Agrion* versus *Calopteryx*.⁵ Undoubtedly any taxonomist can readily cite similar cases in his particular field of interest.

At the present time, refusal to accept published synonymy is responsible for differences in accepted nomenclature between different groups or individuals. Such refusal and the resulting difference in accepted names mean that the cataloguer must constantly change species from one generic name to another and back as new articles appear—or carry the synonymy where he wishes, using his own interpretation in order to place the species in the “proper” place in his catalogue. The other alternative is to carry the species under several genera at the same time. In either case the situation produces much confusion and additional labour rather than stability.

Where does this leave the cataloguer? In the first illustration mentioned, is Gahan's synonymy to be accepted? Or are we free to ignore all but the original descriptions? Should the individual have the right to be “conservative,” reject the published work of others (perhaps by merely ignoring it or dismissing it by stating that it is incorrect) and continue to use names that have been synonymized by others? For example, is Whiting's continued use of *Habrobracon juglandis* (Ashm.) valid?

The writers believe that the adoption of such a “law of recency” would aid in lessening the confusion created by sources (3) and (4), since points of disagreement would be brought to the fore where they could be acted upon by the International Commission when necessary, resulting in a much more rapid stabilization of names than occurs under the present system.

Incidentally, the lone expression “in the opinion of the writer” does not convey the extent of similarity or difference existing between organisms and perhaps should not be regarded as a valid reason for changing the established status of a name. The same objections may be raised against “new synonymy” or “new combination” when used without descriptions of similarities or differences.

To eliminate sources (1) and (2) it is suggested that names and their changes be considered as fully validated only after appearing in the *Zoological Record*, that for validation purposes the author be required to send a copy or facsimile reproduction of his publication to this source, giving the date on which the article in question was distributed (i.e., “published”), and that this date be included after the reference in the *Zoological Record* under the “Titles.”

Such a procedure would eliminate the question of the date of publication serving as a source of future trouble. That the publication date is still a source of confusion is illustrated by the fact that Sabrosky⁶ found it necessary to publish a note regarding the date of “publication” of an article distributed as late as 1948.

It also appears desirable to provide for continuing international support (perhaps through Unesco or some similar means) for the *Zoological Record*

and to ensure sufficient funds so that an adequate staff may be maintained to produce a new volume within the year following that with which the volume deals.

It would be the authors' suggestion that the plenary powers of the International Commission be terminated at the point where the names and dates are first validated in the *Zoological Record*, i.e., that the plenary powers should not extend to names to be published in the future and that a date be set at which the application of the law of priority becomes rigid.

Since it is only through stabilization of the International Code and the procedures to be followed that nomenclature can be placed on a sound basis, the suggestions given are presented as a constructive criticism rather than from any derogatory motives. The authors would welcome both private communications and published discussions of the ideas herein submitted.

References

- ¹ Hemming, F. *Bull. zool. Nomenclature*, **1** (7), 119 (1945).
- ² Gahan, A. B. *Proc. U.S. Natl. Museum*, **49** (2095), 65 (1915).
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- ⁴ Gahan, A. B. *Proc. U.S. Natl. Museum*, **53** (2197), 196 (1917).
- ⁵ Schmidt, E. *Entomol. News*, **49** (8), 197 (1948); Longfield, C., *et al.* **50** (6), 145 (1949).
- ⁶ Sabrosky, C. W. *Proc. Entomol. Soc. Wash.*, **52** (6), 315 (1950).

DOCUMENT 1/17

By RICHARD MEINERTZHAGEN, D.S.O.
(London)

Need for Power to fix Type Localities

Extract from an application to the International Commission on Zoological Nomenclature received on 21st November 1951, relating to the type locality to be accepted for the Ostrich (Meinertzhagen, 1952, *Bull. zool. Nomencl.* **9**: 97).

4. It is a serious weakness in the *Règles* that they contain no provision for regulating the selection of type localities thus for preventing confusion of the kind described above from arising. It is very much to be hoped that this omission will be repaired by the Copenhagen Congress in 1953, when it resumes the discussions begun in Paris in 1948 for securing greater stability in zoological nomenclature. In the meantime effective action can be taken only by the International Commission on Zoological Nomenclature and it is for this reason that I am submitting the present application.

DOCUMENT 1/18

Document communicated by HOBART M. SMITH

(University of Illinois, Department of Zoology, Urbana, Illinois, U.S.A.)

Enclosure to a letter dated 29th January 1952

Editorial Note : The paper communicated by Professor Hobart M. Smith which appears below is a paper written by himself jointly with Dr. Richard E. Blackwelder and Dr. J. Brookes Knight, which was published in *Science* **111** (No. 2881) : 289-290 on 17th March 1950.

Categories of Availability and Validity of Zoological Names

Recent publication by one of us (Smith, H. M., *Science*, 1947, **106**, 11) of a note on the use of the expressions *valid* and *available* in describing the status of scientific names prompted the other two to write him that experience in other groups of animals might modify the conclusions that he had reached. Comparison of usage in our three widely separated fields (herpetology, entomology, and paleontology) has led to substantial agreement on a set of terms and definitions different from those previously held by any of us. It is thought that these conclusions may be of interest to others, for the categories involved are not clearly understood by all taxonomists, and the terminology is often confused in practice. Particularly confusing are the uses of *valid* or *validly* by different writers for several of the categories.

Zoological names appear to fall into four categories in respect to their nomenclatural status. (1) All names that have appeared in print (in the broadest sense) must be considered for possible acceptance into scientific nomenclature. (2) Printed names that meet all the publication requirements of the *International Rules of Zoological Nomenclature* are automatically accepted into nomenclature. (3) Names published in full accord with the *Rules* are nomenclaturally acceptable if they are not pre-occupied by another name of the same spelling. (4) From among the nomenclaturally acceptable names, there is only one which, because it is the oldest or has been judicially accepted, can be properly used to the exclusion of all others under a given set of circumstances.

The first of these categories generally has not been given a name, although *printed*, *published*, and *occupied* have all been used. We believe that *printed* is not sufficiently descriptive since a printed label should be excluded, and *occupied* implies "in nomenclature" and so is more appropriate in the second category. *Published* appears to be logically applicable to all names that have appeared in print (in the broadest sense). Most published names are accepted into nomenclature, but some fail to meet requirements of the *Rules* and are disregarded in nomenclature; examples are vernacular names, names without referrants (*nomina nuda*), and names printed in mediums not qualifying as scientific publications.

Names in the second category have generally been cited as *published*, but *available* has also been used. In the customary sense, however, not all these names are available for use, since some are junior homonyms; and to be exact in this usage, *published* must be modified by "under the *Rules*." The term *occupied* may be applied appropriately to those published names that do

meet the requirements of the *Rules* as to publication. *Occupied* names include all named published according to the technical requirements of the *Rules*—all names that are accepted into zoological nomenclature, such as valid names, synonyms, homonyms, and *nomina inquirendae*.

The word *occupied* in this sense has an unfamiliar ring and may at first glance seem inappropriate. However, if one imagines that there is theoretically a niche for each possible combination of letters that could form a name under the *Rules*, and that when a name is published it would occupy its particular niche, the concept of occupation becomes clearer. Note also that the common use of the familiar term *preoccupied* makes it easier to understand the corresponding term *occupied*.

In the third category names have been described as *valid* or *available*. They are available for use, but they are not valid in the more common sense of that word, as being the one acceptable name. The numerous ways of using *valid* make it unsuitable for a sharply defined concept. We may then apply the term *available* to all names that were published in accordance with the requirements of the *Rules* (legally published) and which have not been so published previously for some other genus of animal or for some other species of the same genus. If any of them has been so published previously, the later name is said to be preoccupied and is called a homonym, or more significantly, a junior homonym, and is not available. Thus all names are available which are now properly in use or which may at any future time be properly used.

Finally the one name under which the species or genus is to be known has also been called the *valid* or *available* name. *Available* seems more appropriate in the third category, and *valid* has the disadvantage of being commonly used with several meanings. We suggest that the simple and self-expressive term *correct* be applied in a technical sense to the oldest available name for a genus or for a species within a genus. If the oldest available name has been set aside by the International Commission using its plenary powers, then the next oldest name or the one designated by the commission is the correct name.

The categories may then be defined and named as follows, according to these conclusions: *Any name that is printed and circulated is published; any published name that meets the publication requirements of the Rules is occupied in zoological nomenclature (if it fails to meet the requirements it is an outlaw name, i.e., unpublished, illegally published, or a nomen nudum); any occupied name that is not preoccupied by an older name of the same spelling is available (if it is preoccupied it is a junior homonym and is not available); the oldest available name is the correct name, unless it has been specifically set aside by the Commission under the plenary powers. (The correct name will, of course, vary with changes in our knowledge of subjective synonymy or discovery of unknown facts in the history of the names, such as homonymy and objective synonymy. An available name whose genus cannot be identified is a nomen dubium.)*

U.S. National Museum,
Washington, D.C.

RICHARD E. BLACKWELDER,
J. BROOKES KNIGHT, and
HOBART M. SMITH

DOCUMENT 1/19

By PER BRINCK

(Lunds Universitets Zoologiska Institution, Lund, Sweden)

Letter dated 18th February 1952

On Type Localities and Allied Problems

Since a citation of a locality or a host does not constitute an indication, I cannot see that a correction of a wrong citation is a nomenclatorial matter *per se*. But of course it would be desirable to have a recommendation (or other arrangement) included in the *Règles* and as to the type localities it would be preferable to have the restriction effected by means of an Article similar to Article 30.

If a type is preserved a restriction (or correction) must be performed in accordance with what is known about the occurrence or life history of the nominal species as substantiated by the type. If the species is split up into subspecies, the typical race must be restricted according to the type. Well, this is quite clear. But what to do when there is no type left, that's the problem. Let us assume that a Spanish scientist makes a revision of a widespread European butterfly, named by Linnaeus. He found it worth while to distinguish a lot of races, named as subspecies by him. No Linnaean type is left and the locality is given as "Habitat in Europa." So far as I can see he can select as the typical race what he wants—even an isolated population in the Central Spanish mountains—in spite of the fact that it is quite impossible that Linnaeus had specimens from there when describing his species. Therefore, it would be desirable to have the restriction made so that it agrees with what the original author might have meant when describing his species. But this is not always so easy to fix and I suppose some trouble would arise from such restrictions. In the case of parasitic species a restriction (or a correction) ought to be performed in the same way, i.e. the revising scientist should choose a type host (etc.) which could have been given by the original author. A Mallophagan described by Nietzsche from *Corvus corone* but later found to be a typical parasite of herons, should have the type host corrected to a common European heron (as common and widespread as possible) and not to (for inst.) the South African Goliath Heron. If no host species was given in the original description, a similar recommendation to select the most common and widespread species in the country from which the author had his material (if known or presumed with some certainty) would be useful. For Denny, England. For Nietzsche, Germany. For Linnaeus, Sweden, etc.

DOCUMENT 1/20

By E. BERIO

(Museo Civico di Storia Naturali "Giacomo Doria," Genoa, Italy)

Enclosure to a letter dated 3rd March 1952

THE RULE OF PRIORITY IN ZOOLOGICAL NOMENCLATURE

According to Mr. Dammerman, who has criticized the so-called Law of Priority in *Tijdschr. Ent.* 92 : 34 (1949), after 50 years the hopes of reaching stability through the promulgation of the International Rules for Zoological Nomenclature have been disappointed, to the great distress of non-systematists who never know the proper name to apply to a given animal and the reason for this failure lies primarily in the retroactivity of the Law of Priority which, though introduced in 1905, operates retrospectively back to 1758, although authors prior to 1905 applied it only with reference to the most well-known works.

In the light of this view, it would be necessary to abolish such retroactivity, by starting the validity of the aforesaid Law of Priority from 1905 and by adopting, for the period prior to that year, the principle of continuity and prescription proposed by other people too.

Accepting Dammerman's proposal would lead to the adoption of the following principles :

- (1) the Law of Priority must apply only to names subsequent to 1905
- (2) names in general use between 1864 and 1905 are not to be replaced.

These are :

- (a) firstly, names used in monographs or catalogues published between 1864 and 1905, which should be fixed by international agreement ;
 - (b) secondly, names in general use during the period above, this condition to be ascertained by an international commission.
- (3) in doubtful cases, the Commission's opinion to be sought.

A list of catalogues and monographs (together with further details and instances) is given by Dammerman, whose premises, conclusions and proposals require careful examination.

I

Instability of nomenclature is not too serious an evil and its effects must not be exaggerated. Seeing an animal called by several different names may be vexatious and involve researches : no damage arises, however, so long as correct identification is possible.

One must recognise that even a not officially correct name fulfills its task perfectly, so long as there is no doubt what is meant ; non-observance of the International Rules calls for sanctions, but cannot prevent a name from designating an entity when its content is clear.

Thus, non-systematists need never trouble themselves about changes in designation, for they can use what names they like for designating the entities

they are dealing with, provided the name is clear, even if not orthodox or unobjectionable in relation to the International Rules for Nomenclature.

But systematists also are often perplexed at finding a well-known entity called by a name never seen previously, whether a re-exhumed name or a newly created one. In practice, when an author is compelled to change the status of a well-known denomination, he generally gives the synonymy for a certain time. Thus systematists keep up-to-date and non-systematists, after all, have but to do the same.

As will be seen later, there is one cause of nomenclatorial instability which does not depend upon strictly nomenclatorial conventions, but is due to systematic exigencies. Not even non-systematists can avoid these needs, although they often show a tendency to consider systematics as a quite subsidiary and negligible science and confound its field with that of true nomenclature which really is, on the contrary, a "handmaid of taxonomy."

II

But is the retroactivity of the Law of Priority really the main reason for continuous changes in denominations and names (1) ?

Generally speaking, changes of designation (2) take place in the following cases :—

I. Ascertainment of a change in the validity of a name

A name thought valid (3) may come to be thought invalid, and *vice versa*, owing to :—

- (A) Information about the bionomiality of the work in which it was published ;
- (B) Information about its synonymy ;
- (C) Information about the date of its publication prior to or subsequent to 1758.

II. Ascertainment of a different status of employment (4)

- (A) Ascertainment of the date, within the limits of the period subsequent to 1758, of the book in which a given name was published.
- (B) Ascertainment of a content different from that commonly understood (5).

III. Rejection of a name

- (A) Ascertainment of primary homonymy (6).
 - (a) On ascertaining that two generic names are identical, the more recent one must be discarded.
 - (b) On ascertaining that two original specific denominations are identical in their components, the more recent specific name must be discarded.
- (B) Ascertainment of primary synonymy (7).
 - (a) On ascertaining that two generic denominations have the same type-species (8), one must be discarded. This happens when, by using the Code's Rules to fix the type-species, more than one reviser is found who has chosen a given species as the type-species for different genera, or because the species fixed

as type is found not to be the one which the quoted references imply.

- (b) On ascertaining that two specific names have been erected for the same entity, one must be discarded (9).

(C) Formation of secondary homonymy by transfer.

When a species is transferred to a genus including another species which possesses an identical specific name, two identical names occur, and one of the two names must be discarded as long as the two species are regarded as congeneric.

(D) Formation of secondary synonymy by transfer.

- (a) When two species, each being the type-species of a generic denomination, are thought to be congeneric, one of the two generic names must be discarded, so long as the two species are regarded as congeneric.
- (b) When two forms possessing different names are thought to be conspecific or identical, one of the names must be discarded for use as a component of the specific denomination (10).

IV. Rehabilitation of names

(A) Rehabilitation of a reputed primary synonym.

When it is ascertained that two genera possessing different names which had previously been regarded as primary synonyms, possess different type-species (see causes at item III, B, a), the later-published of these names is exhumed.

(B) Rehabilitation of a reputed secondary homonym.

When a species possessing a name which had previously been discarded on the ground of secondary homonymy, is removed from the genus, in which the secondary homonymy had occurred, the name which had been discarded is exhumed.* A similar situation arises when a species possessing a name, which led to the discarding, as a secondary homonym, of the name of another species in the genus is itself removed to another genus.

(C) Rehabilitation of a reputed secondary synonym.

- (a) When a species which is the type species of a genus which has been rejected as a synonym of the name of another genus is removed to another genus, the name of the genus of which that species is the type-species is exhumed.
- (b) When it is ascertained that two entities previously regarded as belonging to a single species belong to different species, the later-published of the two names—which had previously been discarded—is exhumed.

(D) Rehabilitation by reference to priority.

When a case of secondary homonymy arises, the oldest valid

**Note by Editor:* Under a decision taken in Paris by the XIIIth International Congress of Zoology, a trivial name once rejected on grounds of secondary homonymy and replaced can never again be used for the same species. (See 1950, *Bull. zool. Nomencl.* 4: 121-124.)

synonym of the name rejected on account of secondary homonymy is exhumed.

V. *Creation of names*

- (A) Renaming. In the absence of a valid synonym, it is necessary, on the case of secondary homonymy arising, to erect a new name in replacement of the name then discarded.
- (B) Separation of two entities previously confused with one another. When an entity formerly regarded as conspecific with another entity is found to be distinct, a new name must be erected for that species, provided that there is no valid existing specific name available for application to it. A similar situation arises when two genera formerly regarded as identical are found to be distinct, i.e., when the respective type-species of those genera are found not to be congeneric with one another.

VI. *Composition of the denomination of a species*

When a species is transferred to a genus in which it has never previously been placed, a new denomination for that species comes into existence, through the combination of the specific name of that species with the name of the genus to which it is transferred.

On examining the causes specified above, we at once observe that Nos. I (A, B, C), II (A, B), III (A, B) and IV represent changes of names which are due solely to nomenclatorial reasons, while in all the remaining cases the changes specified arise from necessities of a systematic nature.

We thus see that nomenclatorial instability may arise from two quite different causes, the one destined in time to cease, the other unavoidable, quite apart from the Law of Priority.

A first cause of instability is that specialists are still in the phase of research as to the priority of names, and they will continue to be so for a long time to come.

There are, for example, publications the date of which has not been established as yet, diagnosis or figures which have not been identified exactly, and works which have not been considered carefully, owing to their extreme rarity.

But these reasons of uncertainty are destined to cease sooner or later. When all the old names have been placed correctly both as regards date of publication and content, there will no longer be strictly nomenclatorial or formal causes for changes. Duplicate names will be known and such obstacles at last be over.

The highest stability will be reached in this direction by discarding definitively primary homonyms and synonyms, by ascertaining in the same way the validity, employment and content of names (by fixing without further uncertainties the type-species for generic names, and the type-specimen for names of species).

Against all the arguments about the time necessary to reach such stability must be set the knowledge that enforcing the Law of Priority in its official

manner must lead inexorably to such stability for, after all, the unique valid oldest name exists, and it only remains to find it.

A second and more important cause of instability in denominations and names arises from the connection of nomenclature with systematics.

Among the main principles of nomenclature there is one (see item VI above), arising from Linnaeus, which constitutes his particular innovation and colours the whole modern nomenclatorial conception, that is, that the binominal name applied to an entity must reveal the hierarchic systematic conception of the Author at that moment.

From this standpoint, nomenclatorial instability, as exemplified in cases III (C, D); IV (B, C, D); V and VI, is linked unavoidably to the flexibility of the system, being subject to continuous improvement and complication, and to differences of Authors' and Specialists' points of view.

The Law of Priority only affects such cause of instability as a last and logical consequence, for the changes of systematic position involve a change of denominations which, in its turn, leads to changes due to adaptation.

But this instability is fatal and incurable, for it is inevitable that both the system and systematics should evolve continuously and that no nomenclatorial rule can prevent instability from these causes, *so long as the rule of formal interdependence between names remains in force.*

III

The method of continuity and prescription which some people advocated even before the adoption of the present International Code and which Mr. Dammerman has now put forward with definite suggestions for its application could release nomenclature neither from the instability arising from the connection between nomenclature and systematics nor from instability arising from laborious research into the availability and use of names. Indeed, if such a method were now to be adopted after fifty years of the enforcement of the Rule of Priority, its adoption would undoubtedly add to the existing confusion.

Furthermore, as we could not in any circumstances cease to take notice of primary and secondary homonymy, the discovery of homonymies between the names used in catalogues, monographs and those used in other works would involve research of the same kind as that required under the Rule of Priority. But the task would be more complicated, for, in addition, there would be the difficulty of establishing whether authors who—acting logically—did not enforce a law of continuity when citing particular names, had intended to refer to the same entities as those to which those names were applied in the catalogue or monograph which had been adopted as the standard for the purpose of determining continuity or whether that they had applied those names to other species, considering that previously they had been misapplied by reason of erroneous identifications of the entities concerned.

In particular, the method of prescription could do nothing to prevent instability arising from changes in the system and in systematics of the kind referred to at the end of the preceding section. For, like the Rule of Priority, the principle of continuity and prescription leads exclusively to the selection of names which are used only as single elements in the compound denominations

used as the systematic names of species. The importance of this consideration becomes evident when it is recalled that systematic categories are subject to constant change and even at any one moment vary from one author to another. On this subject, however, it might be possible (as will be seen later) to obtain nomenclatorial stability, notwithstanding the instability of the system, if it were to be decided to keep the two elements separate from one another.

Turning back to the group of modifications in nomenclature which are independent of systematics, it will be noted that, among the many cases cited in Section II of the present paper, differences in the interpretation of the content of a name represent by far the commonest occurrence. That this should be so brings out clearly that the application of the principle of continuity would lead to exactly the same uncertainties as those arising under the Rule of Priority. The identification of primary homonyms and synonyms is relatively easy, either by research into the literature or by reference to such registers as Sherborn's *Index Animalium*, which are of great value for this purpose. But the chief cause of discordancy in the use of names arises from differences in their interpretation, for a novel interpretation may lead to a change of synonymies—often extensive in character, owing to the interdependence between entities and names. That this is so is well known to systematists, for everybody is aware that such inter-linking is the commonest reason for the changing of names.

In cases of this kind what assistance would be provided by the adoption of the method of continuity? Evidently none, because either the interpretation of the catalogues and monographs accepted as the reference standard would involve uncertainties similar to those involved in the interpretation of the old names, or their interpretation would depend upon the sources cited in these works, in which case the situation would be the same as it is today.

Catalogues and monographs generally give, for purposes of identification, both bibliographical references and synonymies. This, for example, is done in the Staudinger-Rebel Catalogue of 1901 which Dammerman advocates should be taken as the starting point in the case of the Lepidoptera. But catalogues do not indicate under each specific name the fixation made for the type specimen of the species in question, and it would still be necessary, therefore, to go back to the diagnoses given by the original authors or to the type specimens, just as it is at present. Thus, no change in this matter would result from the adoption of the principle of continuity and prescription.

IV

The selection of monographs and catalogues as standards of reference for the purpose of applying the principle of continuity would thus not only involve injustices but would also give rise to uncertainties. In addition, difficulties would arise in ascertaining the so-called "general use" of a name during a certain period of time.

Finality and stability in such a matter could be reached only by rejecting all indeterminate concepts. The expression "general use" leaves room for a number of different shades of opinion and is thus open to objection. If applied in the most rigorous sense, this expression would require that within the specified period no author should have used another name to denominate

a given entity. The research involved in determining whether this condition was satisfied would give rise to a conflict between various possible interpretations of the content of a given denomination according to differences in the views held by individual authors, though, in practice, difficulties of this type might prove to be relatively infrequent.

The concept of duration in relation to the principle of prescription would also give rise to disputes in regard to continuity or discontinuity of usage in particular cases. This would be especially liable to happen in the case of names of species in those groups which, being less known, arouse no interest over long periods and possess only a scanty literature.

Finally, it would be impossible ever to lay on one side those works which are still models of perfection and a source of wonder. It would not be practicable to fix for all time an equivalence between the "official names" and the oldest names for given entities by means of a sort of synonymy between the two, for causes of uncertainty could not be abolished in this way.

To sum up, it appears that uncertainties in nomenclature arising from research into the question of the names which are employable under the Rule of Priority back to 1758—a class of investigation still in process of development—would be greatly increased by substituting some other rule in its place, especially a rule which would abolish the so-called retroactivity to 1758 which during the last fifty years has provided the basis for a definitive and concrete fixation of employable names. A change of direction at the present time would subvert recent monographs which represent the nearest approach to stability that is attainable in our present state of knowledge.

The present criticism of Mr. Dammerman's proposals gives me an opportunity, in my turn, to advance a proposition which has long kept me busy on the problem of zoological nomenclature and which, if adopted, would tend to remove one of the many elements of uncertainty in the zoological nomenclature of today. As will be seen later, this proposition provides some fixity for the names of species as against the constant changes necessitated by the requirements of systematics. It has, moreover, the merit of being, in practice, quite a simple remedy. As I have already pointed out, a complete freeing of nomenclature from systematics would provide the ideal means for stabilising nomenclature, for in the main names are changed for reasons of a systematic nature.

After ascertaining that, to take a particular example, the first name given to a particular species was "*Noctua chrysitis*," that name having been so given by Linnaeus, there could be no uncertainty if everyone were to cite that species only under the name "*Noctua chrysitis*," and, if subsequent changes in the systematic position assigned to that species were to be indicated without abolishing the original binominal denomination of "*Noctua chrysitis*." In other words, the binominal name "*Noctua chrysitis*" (which merely means the species *chrysitis* described by its original author—Linnaeus—as belonging to the genus *Noctua*) would be given a mere binominal of identification, if views were to change regarding the systematic position of the species so named. Thus, if, for example, the foregoing species were, on systematic grounds, to be transferred to (say) the genus *Plusia*, all that would be needed would be a

means for indicating briefly that "the species originally named *Noctua chrysitis* by Linnaeus belongs to the genus *Plusia*." What is required is a unique formula, as short as possible. For this purpose, the two formulae (the nomenclatorial and the systematic) would need to be brought together. This could be done in any of the following ways:—

1. *Plusia chrysitis* *Noctua* Linn.
2. *Plusia chrysitis* (*Noctua*) Linn.
3. *Plusia chrysitis* (*Noctua* Linn.)
4. *Plusia* (*chrysitis* *Noctua*) Linn.
5. *Plusia* (*chrysitis* *Noctua* Linn.)
6. *Plusia chrysitis* Linn. *Noctua*
7. *Plusia chrysitis* Linn. (*Noctua*)
8. *Plusia chrysitis* (Linn.) *Noctua*
9. *Plusia chrysitis* (Linn. *Noctua*)
10. *Plusia* (*chrysitis* Linn.) *Noctua*
11. *Plusia* (*chrysitis* Linn. *Noctua*)
12. *Plusia* *Noctua chrysitis* Linn.
13. *Plusia* *Noctua chrysitis* (Linn.)
14. *Plusia* (*Noctua*) *chrysitis* Linn.
15. *Plusia* *Noctua* (*chrysitis* Linn.)
16. *Plusia* (*Noctua chrysitis*) Linn.
17. *Plusia* (*Noctua chrysitis* Linn.)

The relative merits of the alternative positions 1-5, 6-11, and 12-17 and those of bracketing one or more terms depend upon the extent to which these methods bring into prominence the distinction between the systematic formula and the nomenclatorial formula. When an author's name applies both to the specific name and to the generic name, it may be useful for it to be placed after those names. Moreover, under the Code, where two generic names are cited in a given specific name, the second such name indicates the subgenus to which the species is referred; accordingly, it may be appropriate for the original generic name to follow the specific name, rather than the systematic generic name. Since the most important element in the formula is the indication so given of the systematic position of the species and since there can be no doubt that the first term must be that of the systematic genus, it is evident that the best order to be given to the various terms is one or other of those comprised in the group 1-5.

It seems necessary to adopt bracketing to separate the two parts of the proposed formula. Moreover, the Code states that the name of the author of a specific name should be placed in brackets, when that species is transferred from the genus in which it was originally described to some other genus. Accordingly, a formula should be chosen in which the author's name is placed in brackets.

There remains therefore a choice between Formula 3 and Formula 5. Of these, there can be no doubt that Formula 3 is the better, for it brings into evidence the systematic formula at the most important point and in the most unitary manner, while at the same time making possible the addition of other indications recommended by the Code.

If Formula 3 were to be adopted, the position as regards (say) the species

described by Hübner in 1805 as *Noctua puta* and later transferred to the genus *Agrotis* Ochsenheimer, 1816, would be cited as :—

Agrotis puta (*Noctua* Hübner, 1805) Ochsenheimer, 1816.

By this notation, which is quite simple, nomenclature would be redeemed from systematics to some extent, for it would be possible to vary the generic name to any extent and as often as might be required on systematic grounds, but the second part of the formula would not be subject to change and would thus provide for securing the identification of the entity cited. The following are additional examples of the way in which the formula would operate. There are cases where the extended form shown in the second of these examples is really necessary.

(1) *Euxoa puta* (*Noctua* Hübner, 1805) Warren, 1909

(2) *Euxoa* (*Agrotis*) *puta* (*Noctua* Hübner, 1805) Dufrane, 1932.

This method of notation would be particularly useful where the same name is given to a number of non-congeneric species in the same work, as was often done, for example, by Bethune-Baker who published the following names in this way : *Lasiosceles pratti* : *Epa pratti* : *Zethes pratti* : *Targalla pratti* : *Tibiocillaria pratti* : *Ingura pratti* : *Carea pratti* : *Sphingiforma pratti* : *Risoba pratti* : *Zagira pratti*. Under the present method of notation, a state of complete uncertainty would arise if at any time any of these species was to be transferred from the genus in which it was described to some other genus, for it would be impossible to determine which was the species cited. For example, the notation "*Agrotis pratti* (Bethune-Baker)" could apply to any of the entities cited above.

It is to be noted that by the formula now suggested it would be possible to eliminate all cases of secondary homonymy, for in such a case, the nomenclatorial portion of the formula would differ in the two cases, although the systematic portion would be identical, as may be seen from the following example, in which for two species originally named *Phalaena nana* and *Noctua nana* respectively but both later transferred to the genus *Harmodia*, the formula for the first would be "*Harmodia nana* (*Phalaena* Hüfnagel)" and "*Harmodia nana* (*Noctua* Rottemburg)" respectively.

EXPLANATORY NOTES ON TERMS USED IN SECTION II

(1) I use the expression "name" to distinguish a unique term consisting of a single word, in contrast to the expression "denomination" which I use for a group of words applied to a systematic entity. The expression "species' denomination" is used to denote the phrase consisting of the generic name and specific name of a species; the expression "genus' denomination" is used to denote a generic name.

(2) I use the expression "designation" for any indication given for a systematic entity. It covers the expression "denominations" (see Note 1 above).

(3) I use the expression "validity" as signifying the presence of all those requisites which, under the Rules, are essential for the acceptance of a zoological name.

(4) By the expression "employment" I refer to the possibility of using a given name as a valid zoological name.

(5) This proposition is quite distinct from that given later (I, B) in Section II. In the latter place I have examined the case where a name, previously thought to be a *nomen nudum* or *nomen dubium* is stated to have an unequivocal content or where a name previously thought to be a valid name is on the contrary a *nomen nudum* or, owing to the doubt regarding its content, a *nomen dubium*, examination showing that the original author applied this name to an entity different from that previously believed. A particular case of this situation arises when it is found that a previously overlooked designation of the type species of a genus involves a change in the type species of that genus and therefore a change in its content.

(6) The situation of "Primary Homonymy" arises when the original denomination given to each of two systematic entities consists of the same term or terms. Examples: *Delta* and *Delta*: *Noctua lignosa* and *Noctua lignosa*.

(7) The situation of "Primary synonymy" arises when two different names (whether generic names or specific names) are published with the same content. In the case of generic names, this arises when two generic names are published, each having the same type species.

(8) There is a tendency to abandon the expression "genotype" to designate the type species of a genus, owing to the fact that this expression bears also a different, though more recent, meaning.

(9) This situation arises when a *nomen novum* is published in replacement of a name which is either invalid or unemployable. Such a name denotes exactly the same entity as that covered by the older name, as given to it by its original author. Where the older name is truly invalid, it must be discarded, but, if it is found to be valid and employable, it is to be retained, the substitute name, in that case, being discarded.

(10) According to the Code, names of specific rank include all those given to "species" or "subspecies." Many authors have excluded from this latter category the names applied to any category below the species, other than those given to the "subspecies" in the modern sense of a geographical entity. In my opinion, the expression "subspecies" should be interpreted in as a wide a sense as possible, in order, at least, to protect, for the purposes of priority, names which, through further investigation, may attain "specific" or "subspecific" rank, this being necessary, in order to avoid another possible cause for the changing of names.*

Editorial Note* : The point here made was met by the decision taken by the Fourteenth International Congress of Zoology at Paris in 1948, when a restricted definition was adopted for the expression "subspecific name" but at the same time a special, though separate, status was accorded for the purposes of priority to names given to entities belonging to all other infra-specific categories, means being at the same time provided for the promotion, where required, of "infra-subspecific names" to the rank of "specific name" or "subspecific name." See 1950 *Bull. zool. Nomencl.* **4 : 89-96.

DOCUMENT 1/21

Statement furnished by HOBART M. SMITH

(University of Illinois, Department of Zoology, Urbana, Illinois, U.S.A.)

Letter dated 14th March 1952

On the Question of Type Localities

I was very pleased to see the discussion accompanying your letter of 27th January. You have thoroughly set forth the difficulties and the logical solution for them. I am in complete agreement with your conclusions and method of approach, with only one reservation noted hereinafter. In addition a few modifications might be made here and there for the sake of clarity, depending upon your opinion of their necessity. These also are noted below. Dr. Edward H. Taylor has gone over the entire commentary, and his commentary is attached. He discussed the problems with a number of other systematists at the University of Kansas, and the commentaries of two are also attached. Dr. Wilson wrote his own, but I have transmitted in my own words the gist of the opinions of Drs. Taylor and Michener. They have not authorised the wording which is therefore only my impression of the news which they have expressed. My own comments follow.

I would suggest adding another paragraph between your Nos. 26 and 27, as follows :

26a. A similar situation which should be anticipated and governed by any new Rules is that of discovery, subsequent to designation of type locality under **23(7)** above, through attainment of further knowledge of geographic variation, that the type does not represent the population average at the locality formerly designated. A reasonable solution is an automatic one which permits correction of such former restrictions providing they can be shown to be sufficiently improbable that the type would not fall within the interquartile range of variation at the locality formerly designated. In such case the newly designated type locality should be so selected that the type would fall at least within the interquartile range of variation of the population at that locality. Uncertain cases, or those in which adoption of this procedure would lead to greater confusion than would some other course, should be submitted to the Commission for arbitration.

Secondly, I would recommend deletion of the last 11 lines of paragraph **28**, which would place what I believe would be an unnecessary and large burden upon the Commission, in favour of a substitute solution of more automatic nature, as follows :

“ . . . the type locality shall be determined by a system of precedence, the selection under each of the Rules of paragraph **23** having the preferential rank precisely of the order in which the rules are there listed. Thus a type locality determinable at any time by Rule **23b** shall have immediate and permanent precedence over any selection under any of the subsequent Rules. Uncertain cases, or those in which adoption of this procedure would lead to greater confusion than would some other course, should be submitted to the Commission for arbitration.”

Thirdly, would it not be well to add a paragraph making it clear that the Rules, as ultimately adopted, would be retroactive ?

In paragraph **23(5)** it might be well to make it clear that the conclusions there stated apply not only to localities doubtfully cited, but also to specimens doubtfully, or with any reservation, referred to the species. Locality data known at the time of original description, or unearthed at a later time, for such doubtfully referred specimens, should not be eligible for use in fixation of type locality for the species.

In paragraph **23(6)** it might be well to make it clear (1) that choice of a neotype or a lectotype automatically limits selection of a type locality, exactly as though it were an originally designated holotype ; and (2) that selection of a type locality automatically limits subsequent selection of a lectotype or neotype. Special cases should be submitted to the Commission for consideration.

Annexe 1 to Professor Hobart M. Smith's letter

Commentary by Dr. EDWARD H. TAYLOR (paraphrased)

On the whole agrees with the proposals made. They do not, however, clearly indicate proper procedure for two situations recently encountered, as follows :

- (1) A given species has " Guatemala " as the locality. A reviser restricts it to Tomulco, Guatemala. The original expedition can be shown not to have passed near that locality, although positive records for the species are known from there. Is the reviser's choice acceptable ?
- (2) A manuscript description of a new Mexican species is written by Dugès and sent to a colleague in Paris. This colleague has specimens from another locality of that species, from which drawings are made and published. Some years later the Dugès description is published and attributed to Dugès, but actually Dugès has never seen the specimens that served as the original " indication " of the species. What is the type locality—that of Dugès' specimens which served for the description, or that of the specimens serving as the original " indication " ?

Annexe 2 to Professor Hobart M. Smith's letter

Commentary by Dr. CHARLES D. MICHENER (paraphrased)

In general in agreement with the proposals made, but believes that too much work would be placed upon the International Commission. Wherever possible the Rules should allow the worker to make the decisions, as a matter of time-economy.

Annexe 3 to Professor Hobart M. Smith's letter

Statement by R. W. WILSON

(University of Kansas, Lawrence, Kansas, U.S.A.)

Fixing of Geologic Age and Position within Section

I do not think it is practical to set rules in respect of geologic age for nomenclatorial purposes. Such rules would aid on occasion, but the net result would be to cause more troubles than they would alleviate—particularly so if the age for nomenclatorial purposes were to be established by methods used for the selection of types.

The time scale was developed to more or less its present structure in the 19th Century concomitantly with the naming of thousands of fossil species. New divisions were added as the need seemed to arise, and these modifications extended well into the present century, if indeed, modification is not still in process. One result of the historical growth of the time scale is that the geological ages assigned to many species by the namers are not now acceptable, and in addition there were the usual number of outright errors in age assignment. As an example of the former situation what we now call Oligocene was at one time called early Miocene. As an example of the latter, the Etchegoin formation of California was originally dated as Miocene (species from this formation named *X-miocenicus* attest to this fact), but is now regarded as of Pliocene age, possibly in part early Pleistocene. Even at present, for example in mammalian paleontology, age determinations based on standard time units (e.g., late Miocene, early Pliocene, late Pliocene, early Pleistocene) are frequently so ambiguous that most American mammalian paleontologists make use of a system of provincial ages rather than the "international" time divisions (Eocene, Oligocene, etc.) of the Cenozoic.

Certainly if the proposed rules were to be used at all, the Commission would be passing judgment on hundreds of fossil species. I feel rather certain that most mammalian paleontologists would simply ignore such rules rather than appeal to the Commission for a change in "nomenclatorial" age. In this connection, perhaps it should be pointed out that many of the geological surveys (state and national) exercise rather tight control of the dates assigned to certain levels. Age assignments of strata may differ from one such organisation to another. I doubt that the surveys would be willing to assign their "rights" to a non-geological commission.

A fairly concrete example illustrates some of the difficulties if selection of age were made as suggested.

Ernestokokenia patagonica (Ameghino) : Cretaceous (Cenomanian) according to Ameghino.

No member of the Order to which *Ernestokokenia* belongs is Cretaceous in age. In fact no known Cretaceous mammal remotely approaches members of this genus in structure.

Ernestokokenia yirunhor Simpson is of late Paleocene age according to its namer. It is a smaller animal with simpler molar patterns than Ameghino's species. *E. patagonica* is actually slightly younger (early Eocene) rather than considerably older.

Ernestokokenia is a South American genus. Except for relatively recently named species, virtually the entire pre-Recent fauna of South America would have similar problems. Aside from formal dating, in some cases the stratigraphic sequence in South America is agreed upon, in others it is not—thus presenting additional problems for the Commission.

R. W. WILSON.

DOCUMENT 1/22

By H. ENGEL

(*Zoologisch Museum, Amsterdam, The Netherlands*)

Extract from a letter dated 18th March 1952

I have received the latest Parts of the *Bulletin* (Vol. 7, Pts. 1-6). You have put forward the questions admirably. I think that the best way will be to complete the *Official Lists* of names as fast and as far as possible, as you propose.

DOCUMENT 1/23

By L. B. HOLTHUIS

(*Rijksmuseum van Natuurlijke Historie, Leiden, The Netherlands*)

Letter dated 15th April 1952

I consider that the stabilization of zoological nomenclature will be best promoted by means of amplifications, clarifications and extensions as proposed in 9-30 of your (1952, *Bull. zool. Nomencl.* 7 (5/6) : 148-188) paper and by the more extended use of *Official Lists*.

DOCUMENT 1/24

By G. H. E. HOPKINS, O.B.E., M.A.

(British Museum (Natural History), Zoological Museum, Tring, Herts, England)

Letter dated 30th April 1952

Commission's Reference Z.N.(S.)359

In *Bull. zool. Nomencl.* **7**: 187 and 188 you ask for replies from specialists to specific questions. Here are my replies:—

- (1) I am opposed to an attempt to stabilize zoological nomenclature by the adoption of any novel expedient such as a "Law of Prescription", partly because I think such a law would prove almost unworkable in practice, and partly because I think the aim is more easily attainable by the less drastic methods mentioned in paras. 9–37 of your article.
- (2) I do not think that the disadvantages and difficulties can be adequately foreseen and provided against, and therefore have no suggestions for dealing with them.
- (3) I would have thought certain of the suggestions enumerated in paras 9–37 of your article to be unnecessary, but there are none to which I object. I particularly welcome the suggestions in paras. 16 to 18 for the elimination of *nomina dubia*, both generic and specific, and that in para. 21 for the proposed recognition of neotypes.
- (4) I have no additional proposals for promoting stability in nomenclature.

DOCUMENT 1/25

By FRANZ DANIEL

(*Zoologische Sammlung des Bayerischen Staates, Entomologische Abteilung, München, Germany*)

Letter dated 30th April 1952

Eine Bereinigung der zoologischen Nomenklatur ist in Weiterentwicklung der bestehendden "Regeln" nicht mehr durchführbar. Die von Herrn Professor Hering vorgeschlagenen Verbesserungsvorschläge müssen sich genau so totlaufen wie die bisherigen Anweisungen, da es praktisch unmöglich ist ein solches Gesetz wirklich eindeutig zu fassen. Eine erlösende Bereinigung kann nur durch eine radikale Abkehr von den bisher beschrittenen Weg erreicht werden. Dies mag hart klingen, wird sich aber eines Tages doch als einziger Ausweg durchsetzen. Je früher dies eingesehen wird, um so eher wird die Last von allen Beteiligten genommen einen wesentlichen Teil ihrer Lebensarbeit mit unfruchtbaren Auseinandersetzungen über Nomenklaturfragen auszufüllen.

Als möglicher Weg erscheint mir in ganz grossen Zügen folgende Verfahrensweise :

Ein IZK wählt eine Kommission für jede einzelne Tiergruppe, die innerhalb ihres Arbeitsbereichs eine nomenklatorische Unterteilung bis zur Familie vornimmt. Sie ist hierbei weder an Prioritätsregeln, noch andere einschränkende Bestimmungen gebunden, soll aber im Rahmen ihres subjektiven Ermessens bisher eingeführte Benennungen nach Möglichkeit übernehmen. Sie legt für die von ihr vorgeschlagenen Einheiten genaue Diagnosen, sowie die Typen höherer Ordnung fest. Diese Vorschläge sind von einem IZK zu prüfen und bestätigen, womit sie für die Zukunft als alleinige Grundlage dienen.

Von Tage der Anerkennung an hat die Neuaufstellung, Teilung oder Verschmelzung solcher Kategorien, sowie eine Änderung oder Erweiterung der festgelegten Diagnosen nur mehr Giltigkeit, wenn sie von einer für jede Tiergruppe permanent aufgestellten internationalen Kommission anerkannt wurde. Der Veröffentlichung ist die Genehmigungsnummer beizufügen, anderenfalls ihr die internationale Anerkennung zu verweigern ist. Alle zoologischen Fachzeitschriften sind anzuhalten nicht genehmigten Änderungen die Veröffentlichung zu verweigern. Gegen einen negativen Bescheid steht jedem Autoren ein Beschwerderecht zur nächsten IZK zu.

Dieser Beschneidung der Freiheit wissenschaftlicher Forschung müssen sich alle Kreise im Interesse einer gedeihlichen Zusammenarbeit fügen. Sie hat gewiss nichts mit Gewissenszwang in irgend einer Form zu tun.

Für die Bereinigung der Nomenklatur der Familien sind geeignete Bearbeiter zu ermitteln, die frei von allen Hemmungen durch den überlebten Prioritätsstandpunkt die Benennung der unteren Kategorien bis zur Subspecies durchführen. Sie sind gleichfalls gehalten nach Möglichkeit eingeführte Bezeichnungen beizubehalten. In ihren Diagnosen gehen sie von dem erreichbaren Typenmaterial aus, soweit sie dieses jedoch nicht beschaffen können, oder falls der Typus durch mangelnde Voraussetzungen (schlecht

Bezettelung, Alterserscheinung etc.) nicht mehr voll auswertbar ist, bestimmen sie nach freiem Ermessen einen neuen Typus. Sämtliche verwendeten Typen sind als "Typus neuer Art" zu bezetteln und soweit nötig nach modernen Gesichtspunkten unter Anwendung einer *Differenzialdiagnose* zu beschreiben. Ebenso ist, falls nicht gute Bilder verwiesen werden kann, in denjenigen Gruppen, bei denen habituelle Gesamteindruck durch die Sprache nicht vermittelt werden kann (Lepidoptera !) der Typus abzubilden. Genitaldiagnosen ist grosse Bedeutung einzuräumen, jedoch ist die in einigen Ländern eingerissene Unsitte *nur* nach Genitalmerkmalen zu urteilen und den Koppulations-Apparat allein abzubilden wenigstens bei Lepidoptera tunlichst zu vermeiden. Die Typenform ist sowohl ihrer genauesten Heimat nach, wie in Bezug auf ihr Aussehen innerhalb der modifikatorischen Möglichkeiten festzulegen. Benennungen von regelmässig wiederkehrenden Aberrationen sind weiterhin von internationalen Bindungen freizuhalten, es ist aber tunlichst darauf hinzuarbeiten für grössere Gruppen durch Aufstellung von Kollektivbezeichnungen eine vereinfachte Verständigung zu erreichen. Alle nicht mehr sicher aufklärbaren Beschreibungen, bei denen aber auf Grund der vorhandenen Unterlagen eine Einreihung als Synonym nicht ratsam erscheint, sind als *sp. dubiosa* aufzuführen. Bei späterer Klärung des Falles sind sie neu zu beschreiben, wobei der damit Betraute als Autor zu gelten hat. Der bisher fraglich geführte Name wird dabei zum Synonym.

Alle von den Familien Bearbeitern als Synonyme angesprochenen Bezeichnungen scheiden endgiltig aus, auch wenn sich später herausstellen sollte, dass diese Ansicht unrichtig war. Ebenso sind alle von den Familien Bearbeitern etwa übersehene Namengebungen automatisch gestrichen. Für beide Fälle ist gegebenenfalls eine Neubeschreibung unabhängig von den bisherigen Literaturangaben, durchzuführen.

Alle diese Vorschläge sind vom Standpunkt der Entomologie aus gesehen, wo die Verhältnisse durch den ungeheueren Formenreichtum völlig anders liegen als in den übrigen Gebieten der Zoologie. Es wäre deshalb zu erwägen ob nicht eine internationale Vereinbarung möglich wäre, die einen Teil (ich denke hier vor allem an die Entscheidung von der Familie abwärts) der notwendigen Regelungen im Insekten Bereich dem IEK übertragen würde.

Ich bin mir bewusst, dass die hier in ganz groben Umrissen niedergelegten Vorschläge heute noch keinerlei Aussicht auf Erfolg haben. Ebenso sehr glaube ich aber auch unbedingt, dass die von Prof. Dr. Hering angeregten Verbesserungs-Wünsche zu keiner Bereinigung der chaotischen Zustände unseres systematischen Gerippes führen können. Dazu gibt es meines Erachtens keinen anderen Weg als den einer völlig neuen Grundlage. Je eher der Entschluss hierzu gefasst wird umso leichter wird diese Umstellung sein. Ich denke hierbei besonders auch an das Ansehen des Systematikers, welches in den weiten Kreisen derjenigen, die dessen Erkenntnisse nur als Arbeitsunterlage benutzen, ständig sinkt—und sinken muss, da ihnen ein stets schwankendes Gerüst geboten wird, dass sie nicht mehr als internationales Verständigungsmittel anerkennen können. Mit Massnahmen, die lediglich auf eine Verbesserung nicht mehr tragbarer Grundlagen hinauslaufen ist der Krankheitsherd nicht mehr zu heilen.

DOCUMENT 1/26

By FRIEDRICH KÜHLHORN

(Museum, München, Germany)

Statement dated 2nd May 1952 communicated by Professor E. M. Hering

Editorial Note: The present communication from Dr. Kühlhorn and also the immediately following communications from Dr. Haltenorth (Document 1/27) and from Dr. Sheljuzhko (Document 1/28) were prepared by the specialists concerned by reference to a summary of the points raised in my paper of March 1952 (*Bull. zool. Nomencl.* **7** : 148–188) which Professor E. M. Hering kindly prepared for communication to German zoologists. Since Dr. Hering's summary bears numbers different from those used in the foregoing paper, the three comments in question are not intelligible without Dr. Hering's summary, which is accordingly reproduced as an annexe to the present document. (intl'd. F. H., 10th April 1953).

Stellungsnahme zu den vorschlagen zur neufassung Internationalen Nomenklaturregeln

Eine Neufassung der Regeln darf auf keinen Fall zu einer weiteren Komplizierung der zoologischen Nomenklatur führen, die trotz aller Verbesserungen immer noch genügend geschichtlich verständliche Schwachpunkte behalten werden.

Die Systematik darf nicht nur Selbstzweck sein, sondern sie muss bei der grossen wirtschaftlichen Bedeutung vieler Tiere auch die berechtigten Wünsche der Praktiker berücksichtigen, für die eine Vereinfachung eine ungeheure Erleichterung ihrer Arbeit bedeuten würde. Ich trete deshalb allein schon aus diesen Gründen für die *Beibehaltung des Prioritätsprinzips* ein, des en Schwächen durch die im Absatz VII/II vorgeschlagenen Erweiterungen weitgehend ausgeglichen werden könnten.

Annexe to Editorial Note to Document 1/26

Summary prepared by Professor E. M. Hering of the main points in regard to zoological nomenclature raised in the seven papers by Mr. Francis Hemming; papers published in 1952 in Parts 1–6 of volume 7 of the "Bulletin of Zoological Nomenclature."

Sieben Probleme der zoologischen Nomenklatur

die eine Neufassung der Internationalen Regeln notwendig erscheinen lassen, über die auf dem 14. Internationalen Kongress für Zoologie, Kopenhagen, 1953, entschieden werden soll.

In dem Zeitraum von 1901 bis zur Gegenwart hat sich die Fassung verschiedener Artikel der "Internationalen Regeln für die zoologische Nomenklatur" (nachfolgend als "Regeln" bezeichnet) als unzulänglich erwiesen.

Der Internationale Kongress für Zoologie Paris, 1948, beauftragte die Internationale Kommission für zoologische Literatur (nachfolgend als INK. abgekürzt), Neufassungsvorschläge der betreffenden Artikel dem 14. Int. Kongr. f. Zoologie, Kopenhagen, 1953, vorzulegen, der über sie entscheiden und die Entscheidungen in die "Regeln" einkörpern soll.

Das in Paris, 1948, endgültig festgelegte Verfahren der Internationalen Kommission zur Ausübung der ihr vom Kongress Monaco, 1913, übertragenen Vollmachten zur fallweisen Suspendierung der "Regeln" (im folgenden kurz als "Verfahren" bezeichnet) besteht darin, dass.

- (a) Ein oder mehrere Spezialisten stellen einen begründeten Antrag auf Klärung einer zweifelhaften Frage oder auf Aufhebung der Regeln in einem gewissen Falle, dass die INK. eine Entscheidung auf Grund ihrer Vollmachten treffen möge.
- (b) Dieser Antrag muss im Wortlaut mit der Begründung veröffentlicht werden.
- (c) Die auf diese Veröffentlichung eingehenden Stellungnahmen der Spezialisten werden ebenfalls veröffentlicht.
- (d) Auf Grund dieser Stellungnahmen und sonstiger Befragung von zuständigen Spezialisten entscheidet die INK. regelverbindlich über den Fall, wobei sie Erwägungen über Stabilität und Uniformität den Vorrang gibt gegenüber philologischen Argumenten.

Ich habe die Behandlung dieser Probleme in vol. 7 des *Bulletin of Zoological Nomenclature* in möglichster Kürze darzustellen versucht, um eine Stellungnahme der deutschen Zoologen und Paläontologen ohne die grosse Mühe des eingehenden Studiums dieses Bandes des "Bulletin" zu ermöglichen. Wo ich mich nicht klar ausgedrückt habe, bin ich gern bereit, auf Anfrage nähere Mitteilungen zu machen.

Ich halte es für ausserordentlich wichtig, dass Spezialisten sich möglichst zahlreich zu den zur Entscheidung kommenden Vorschlägen äussern, damit einmal keine Entscheidung in Bezug auf die Neufassung der "Regeln" getroffen wird, die den Wünschen der Mehrheit der Wissenschaftler widerspricht, zum andern die deutsche zoologische Wissenschaft ihre Existenz dokumentiert und aus ihrer Abseitsstellung herauskommt, die sich in gewissen Beschlüssen Int. Kongr. f. Zoologie zeigt (Paris, 1948 beschloss Ausschaltung der deutschen als offizieller Sprache für INK. und Veröffentlichungsverpflichtungsaufhebung für Ausserkraftsetzung von "Regeln" im "Zoologischen Anzeiger" die früher vorgesehen war). Abstellung solcher Mängel kann nur durch Aktivität und Beteiligung auch deutscher Wissenschaftler erreicht werden.

Da ich bis Ende Mai ein Referat über solche Stellungnahmen auszuarbeiten habe, wäre ich für recht baldige Stellungnahme des Stabes der Wissenschaftler des Museums—wie auch anderer befragter Spezialisten—zu den nachfolgend berichteten Vorschlägen für die Neufassung der "Regeln" zu Danke verpflichtet.

E. M. HERING,

deutsches Mitglied der Internationalen Kommission für Zoologische Nomenklatur.

VII. Vorschläge zur Erzielung grösstmöglicher Stabilität in der Nomenklatur

A. Die mangelhafte Stabilität zoologischer Namen hat sich in letzter Zeit zu einer grossen Erschwernis wissenschaftlicher Arbeit ausgewirkt, so dass an Massnahmen zur Erzielung einer *grösseren Stabilität* gedacht werden muss. Sie können bestehen entweder in der *Einführung ganz neuer Prinzipien* in die "Regeln," oder in der *Erweiterung und Klärung schon vorhandener Regeln*, oder im *erweiterten Gebrauch* der schon in den "Regeln" *vorhandenen Vorschriften*.

B. Vorschläge zur Besitigung der Instabilität :

I. *Einführung eines "Praescriptions-Prinzips"* (etwa dem "Konzinuitäts-prinzip von HEIKERTINGER entsprechend). Es würde besagen: Gattungs-oder Artnamen, die etwa vor 1850 veröffentlicht wurden und seitdem von keinem Autor mehr verwendet wurden, sollen zu verwerfen sein, wenn sie mit einem nach diesem Zeitpunkt dauernd gebrauchten Namen in Konflikt kommen. (Andere Vorschläge schreiben anstelle eines festen Datums von "längerer Zeit" etc.).

1. Ist ein solches Prinzip durchführbar? Es würde in besonderen Fällen das Prioritäts-Gesetz unterdrücken, müsste auch unterscheiden, ob ein *Name vielleicht einmal*, in anderen Fällen *dauernd gebraucht* würde, der *Zeitpunkt* vom Beginn der Gültigkeit müsste *exact formuliert* werden.

2. Schwächen des Prinzips: Es ist *unmöglich*, einen *negativen Begriff* (Nichtgebrauch) in die "Regeln" *aufzunehmen*. Der Nichtgebrauchsnachweis ab 1850 etwa erfordert ein *nicht*

durchführbares Studium der gesamten Literatur. Die Gewissheit zu erreichen, würde unvorstellbaren Zeitverlust bei der wissenschaftlichen Arbeit kosten. Überraschungen wie die kürzlich erfolgte Auffindung einer Schrift von Linnaeus, die bisher unbekannt gewesen ist, könnten sich auch in Bezug auf spätere Literatur erwarten lassen.

II. Stabilität durch *Erklärung und Erweiterung* der schon vorhandenen "Regeln"! Wo eine die Stabilität gefährdende Unklarheit in den Regeln besteht, ist eine Entscheidung durch Antrag an die INK. zu erzielen.

a. *Erklärungen.*

1. Die Regeln sollen eine Präambel erhalten, die die Grundprinzipien enthält, unter denen die nachfolgenden Artikel angewendet werden sollen.
2. Strittige Begriffe in den Vorschriften der "Regeln" sollen durch ausführlichere Vorschriften geklärt werden (cf. I. Problem: Emendation).
3. Die der INK. gegebenen Vollmachten (Monaco 1913, Paris 1948) geben jede Möglichkeit, Vorschriften der "Regeln" in Bezug auf einzelne Fälle aufzuheben, um Stabilität zu erhalten.
4. Wo mehrfache Interpretation eines Artikels der "Regeln" möglich ist, kann immer Entscheidung durch Antrag an die INK. erfolgen.
5. In allen anderen Fällen, wo unerwünschter, die Stabilität gefährdender Namenwechsel droht, kann durch Antrag an die INK. eine zugunsten der Stabilität ausfallende Entscheidung erwirkt werden.
6. Notwendig ist analog den bereits in den "Regeln" verankerten "Offiziellen Listen" ein "Offizieller Index" der zu verworfenden Bücher aufzustellen (die entweder nicht der binominalen Nomenklatur folgen oder anderweitig die Stabilität gefährden). Die Überführung auf diese Liste soll nach üblichem Verfahren durch Vollmacht der INK. übertragen werden, die in den "Regeln" zu verankern ist.
7. Zweifelhafte Typenbestimmung für Genera kann durch die INK! auf Antrag festgelegt werden (*Lumbricus* L. Typus *terrestris* L., *terrestris* ist Nomen dubium!)
8. Artnamen, die Nomina dubia sind, können nach üblichem Verfahren durch die INK. auf den Bereits vorhandenen "Index der verworfenen Namen" gesetzt werden. Notwendigkeit ergibt sich aus späterer Deutung früherer Nomina dubia (cf. STRESEMANN über die Gmelin-1789-Namen!).

b. Erweiterung der Regeln.

1. Begriffsfestlegung für "Typus" in Art. 31 der "Regeln" (Kongress Paris 1948) durch Anerkennung und Definition für Holo-, Syn- und Lektotypus, sowie Prozedur und Bedingungen für Schaffung von Lektotypen.
2. Die Pariser Regelung lässt Typenfestlegung unklar in Fällen, wo kein typisches Material mehr existiert und macht keine bindenden Vorschriften über nominotypische Subspecies, wenn ursprüngliche Fundortangabe zu unbestimmt ist. Hierfür sind ausführliche Vorschriften notwendig.
3. Einfügung des "Neotypus" (cf. VI. Problem).
4. Vorschriften über die typische Lokalität polytoper Artnamen, deren Fehlen zur Vieldeutigkeit bei der Aufstellung der nominotypischen Unterart geführt hat. Deswegen sind in Art. 30, 31 der "Regeln" einzufügen folgende Vorschläge:
 - a. Wo der Autor selbst die Typenlokalität bezeichnet hat (wenn er sie nicht selbst als zweifelhaft bezeichnet) gilt dies auch für die nominotypische Unterart.
 - b. Der am Holo-bezw-Lektotypus angebrachte Fundortzettel ist, wenn genauer als in der Beschreibung, für die Aufstellung der nominotypischen Unterart massgeblich, wenn vom gleichen oder späteren Autor später veröffentlicht.
 - c. Wo Art-oder Unterartname eine Lokalität bezeichnet, ist diese als topotypisch anzusehen, wenn Patria-Angabe bei der Beschreibung fehlt.
 - d. Wenn ein Art-oder Unterartname für ungültig erklärt und durch einen neuen (oder ein Synonym) ersetzt wird, soll der Ersatzname gleichen Topotypus besitzen.
 - e. Wo die Lokalität nicht genauer in der Originalbeschreibung präzisiert wurde und auch nach (b) nicht zu erhalten ist, ist Auswahl durch gleichen oder späteren Autor später massgeblich. Weitere Restriktion der Lokalität erfolgt durch die folgenden Autoren.
 - f. Wo Patria-Angabe bei der Beschreibung fehlt, aber Hinweis auf frühere Veröffentlichung des Autors angegeben ist, wird die dort angegebene Lokalität als topotypisch angesehen und geht den Lokalitäten in anderen gleichzeitig angegebenen Referenzen auf andere Autoren vor.

- g. Wo ursprüngliche Fundortangabe irrig ist oder fehlt, kann auf Antrag die INK. den Topotypus für Art oder Unterart festlegen. Wenn nach solcher Festlegung vermisste Holo-oder Syntypen wieder auftauchen, die die Lokalität in anderem Sinne klären, ist Entscheidung durch die INK. zu beantragen.
5. Bei Fossilien ist geologisches Alter und Horizont wichtiger als die Lokalität, deshalb sollen die Vorschriften einen Zusatz erhalten, dass für erste analog zu vorigen Regeln zu verfahren ist.
6. Bei Parasiten ist analoger Zusatz in Bezug auf den Wirt nötig.

III. Erweiterter Gebrauch schon bestehender Vorschriften zur Erhöhung der Stabilität.

Der Kongress von Monaco, 1913, schuf die " Offizielle Liste " der Genusnamen (für Nomina conservanda), von der bisher zu wenig Gebrauch gemacht wurde (seitdem etwa 600 Namen eingefügt !). Der Kongress von Paris, 1948, legte das Verfahren für die INK. fest und schuf die " Offizielle Liste " auch für die Artnamen, sowie den " Offiziellen Index " der zu verwerfenden Namen. Die Listen sind jetzt in den " Regeln " verankert, das liberum Veto in der Kommission wurde beseitigt. Diese Listen geben jederzeit die Möglichkeit durch Antrag an die INK. Namensänderungen, die die Stabilität gefährden, auszuschalten.

Fragen an die Spezialisten

1. Kann Stabilität besser erreicht werden durch ein " Praeskriptionsgesetz oder durch Ausbau des Systems der " Regeln "?—Wenn im Sinne der Praeskription : wie können die Schwächen nach B.I.2. beseitigt werden?—Wenn im Sinn des Ausbaues der " Regeln " : Ist man für die obigen Vorschläge, welche weiteren Vorschläge zu machen ?

DOCUMENT 1/27

By Th. HALTENORTH

(Museum, München, Germany)

Statement, dated 14th May 1952, communicated by Professor E. M. Hering

Editorial Note : For an explanation of the reference cited by Dr. Haltenorth in the following note, see Editorial Note to Document 1/26 and the document prepared by Professor Hering annexed thereto.

VII. B.II sollte bevorzugt werden, d.h. Namens-Stabilität durch Ausbau der Regeln, die bereits weitgehend für Stabilität sorgen und nach denen schon Jahrzehnte gearbeitet wird.

DOCUMENT 1/28

By LEO SHELJUZHKO

(Zoologische Sammlung des Bayerischen Staates, München, Germany)

(1) Statement, dated 16th May 1952, communicated by
Professor E. M. Hering

Editorial Note: For an explanation of the reference cited by Dr. Sheljuzhko in the following note, see Editorial Note to Document 1/26 and the document prepared by Professor Hering annexed thereto.

VII. "Einführung eines Praeskriptions-Prinzips"—möchte ich als kaum durchführbar und unzulässig verwerfen. Dagegen scheint es mir, dass die Stabilität durch Erklärung und Erweiterung der schon vorhandenen Regeln allmählich gute Fortschritte erzielen kann.

Zu dem Punkt 4d der "Erweiterung der Regel" wäre zu bemerken, dass dieser unerfüllbar ist. Ein Synonym ist ein Name, der auf bestimmtem Material basiert ist, das aus einem bestimmten Fundort stammt. Sollte solch eine synonyme Bezeichnung aus irgend-welchen Gründen Gültigkeit bekommen und den früher gebrauchten Namen ersetzen, so bleiben doch die Typen der beiden Formen mit ihren Fundorten bestehen und können diese Fundorte nicht geändert werden. Ganz anders verhält es sich, falls es sich um einen *nomen novum* handelt; dieser *nom. nov.* soll überhaupt keine eigene Typen besitzen, d.h., dass als Typen des *nom. nov.* diejenigen der Form gelten, für die er den Ersatznamen bildet.

(2) Letter dated 27th March 1953

Bei einem Ersatz eines Art—oder Unterartnamens kann es sich um zwei recht verschiedene Fälle handeln.

Im ersten Fall ist der Name nur ein *novum novum*, der als Ersatzname für die frühere Bezeichnung geschaffen wurde. Solche *nomina nova* sollten überhaupt keine Typen und müssten also solche diejenigen des alten Namens gelten.

Ganz andere verhält sich die Sache falls an die Stelle des alten Namen ein Synonym tritt. Diese Synonymy wurde bei ihrer Aufstellung auf bestimmtes Material gegründet, d.h., dass sie auch ihre Typen aus bestimmten Fundorten besitzen. Es wäre ein reiner Zufall, wenn diese Typen aus den selben Orte stammen sollten, von wo die Typen der Form stammten, für die die synonyme Bezeichnung eintreten soll. Nun glaube ich, dass es nicht möglich wäre zu fordern, dass der in diesem Fall der Ersatzname den gleichen Topotypus hat, wie es im Punkt "4.d" gefordert wird.

DOCUMENT 1/29

By W. J. ARKELL, M.A., D.Sc., F.R.S.
(Sedgwick Museum, Cambridge University, Cambridge)

Letter dated 23rd May 1952

I have read with admiration your most thorough and clear papers in the *Bull. zool. Nomencl.* Vol. 7, Parts 1-6.

I enclose comments on Z.N.(S.)359 (stability). I should be content to see everything not covered by my comments go through as expounded in your papers.

Enclosure to Dr. W. J. Arkell's letter of 23rd May 1952

I am not in favour of a Law of Prescription. In my opinion it would cause far more confusion than it would be worth, and the objects desired can be achieved by sensible behaviour of specialists in refusing to be hustled into unnecessary changes until they have submitted the case to the Commission and invoked the Plenary Powers.

My recent applications on *Pachyceras*, *Sphaeroceras*, *Pecten asper*, *Exogyra virgula*, etc., illustrate the kind of action to be encouraged before sweeping changes are accepted under the Rule of Priority.

In the past, authors have not had the chance to use the existing machinery in this way, because of the extreme slowness of publication and processing of applications. If one was ever going to publish any work in a lifetime, one simply had to ignore the Commission and follow the letter of the existing Rules. (As you know, one waited 10-15 years for a case to be dealt with.) Now that you have got all that changed, I consider it should be made widely known by a Recommendation that it is the duty of an author to investigate possibilities of adjustment under the Plenary Powers before rushing into changes of name as the result of "bibliographical discovery"—i.e., reading Neave's *Nomenclator*.

DOCUMENT 1/30

By J. R. DYMOND
(Department of Zoology, University of Toronto, Canada)

Statement received 27th May 1952

I consider that the stabilization of zoological nomenclature would best be promoted by means of amplifications, clarifications and extensions carried out within the existing framework of the *Règles* and by the more extended use of the existing provisions for stabilizing individual names through the *Official Lists*.

DOCUMENT 1/31

By CHARLES H. BLAKE

(Massachusetts Institute of Technology, Department of Biology, Cambridge,
Massachusetts, U.S.A.)

Enclosure to a letter dated 5th June 1952

Proposal of a Nomenclatorial Statute of Limitation

Preamble.—In the interest of nomenclatorial stability a statute of limitation should be enacted as set forth below. It has been the general experience of those peoples living under systems of elaborate statute laws that more harm than good flows from permitting actions to be initiated indefinitely long after the cause of action comes into existence. It has also become apparent that the unrestricted application of the Rules of Priority and Homonymy has not led to nomenclatorial stability.

The following rule or statute shall come into force six months after its enactment by the International Zoological Congress.

Section 1. Definitions.—Terms used in this statute shall have the definitions given in paragraphs (a) to (e) of this section.

- (a) The actual zoological organism or concept bearing a name is an onomatophore. It acquires prescriptive right to a name by de facto, published assignment of the name to the given onomatophore. Its prescriptive right to a name is extinguished by non-application of the name within the term of this statute.
- (b) A *nomen conservatum* is a name which has been placed on the "List of Official Names" by action of the International Commission on Zoological Nomenclature (hereafter, the Commission).
- (c) A *nomen conservandum* is a name belonging to either of the following categories :—
 - (1) The name of a genus that is the type of a supergeneric category.
 - (2) Any name which can qualify for the benefit of this statute and which has not been the subject of a final determination by the Commission.
- (d) Contested application is the application of a name to more than one onomatophore. Such application shall be deemed contested as regards all of the onomatophores.
- (e) Primary zoological literature is all published work in which the authors could have contested the applications of the names used. For the purposes of this statute literature shall be deemed primary unless there is conclusive evidence to the contrary.

Section 2. A name shall be entitled to the benefit of this statute regardless of its original rank or of the rank subsequently or currently assigned to it.

Section 3. The benefit of this statute must be expressly claimed.

Section 4. The provisions of this statute apply to all cases of priority, synonymy, and homonymy, except as provided otherwise. (Sections 3, 8, 9 and 10).

Section 5. The provisions of this statute apply to all cases of the designation of types of genera or their subdivisions or of higher rank than genera, except as provided otherwise. (Sections 3, 8, 9 and 10).

Section 6. Exclusive and uncontested possession of a name by an onomatophore for fifty years or more shall be deemed conclusive as to the correct application of the name provided, however, that the given application shall have been made on three different occasions in primary zoological literature within the period of fifty years.

Section 7. Exclusive and uncontested possession of a name by an onomatophore for seventy years or more shall be deemed conclusive as to the correct application of the name even though it shall not have been applied on three different occasions within the period of seventy years.

Section 8. The doubtful application of a name to an onomatophore or a doubtful type designation shall be deemed non-application for the purposes of this statute.

Section 9. The provisions of this statute shall not apply during any time when fraudulent concealment of the facts shall be adjudged to have occurred.

Section 10. This statute shall not apply to cases where action which could have been challenged under this statute has been taken prior to the coming into force of this statute.

DOCUMENT 1/32

By S. G. KIRIAKOFF, L.Sc.

(Zoological Museum, Ghent University, Ghent, Belgium)

Enclosure to a letter dated 5th June 1952

MEANS TO BE FOUND FOR PROMOTING THE GREATEST POSSIBLE
STABILITY IN ZOOLOGICAL NOMENCLATURE(Bull. zool. Nomencl. **7**, Pts. 5-6, 148-188)

Preliminary remark.—I am fundamentally against any restriction of the Law of Priority. In my opinion, any case of suspension of the *Règles* in favour of a more recent name on the ground that the latter has been in general use, is a solution of facility and is, moreover, nothing less than an encouragement either of (former) mistake or of ignorance. The first describer of a form has, I believe, the (mostly posthumous) moral right to have the name he has given used in preference to any younger name regardless of the fact that the latter has been in common and general use for ever so long.

The above is, however, only a declaration of principle, and, as it seems that the great majority of the zoologists (although most of the latter are physiologists, ecologists, biochemists and so on, who are not interested in Taxonomy and who often do not have a clear idea of the objects and of the meaning of the latter) are against the unrestricted use of the Law of Priority, I am quite willing to help in promoting the stability in nomenclature they are in such a hurry to find.

Answers to the questions l.c., p. 187-188 :—

- (1) I consider that a " Law of Prescription " such as it has been proposed by some zoologists (par. 4-7) would be unable to achieve any serious purpose ; for those impatient to preclude any possibility of further name changing, a means is suggested below (vide (2)). But personally I am in favour of gradually achieving the stability so desired by means of amplifications and so on, within the existing framework of the *Règles*, as suggested in para. 9-37.
- (2) The radical means suggested to achieve at once the stability in nomenclature is the following :—

An article is to be inserted in the *Règles* to the effect that—

- (a) no further changes of generic and specific names will be accepted ;
- (b) that any paper or book published before the date of publication of the present article in the *Bull. zool. Nomencl.* and containing names of genera and/or species generally known under other younger names, and consequently apt to supersede the latter, is to be considered as non-existent for nomenclatorial purposes.

Of course, the provision under (b) would apply in fact only to the (relatively few) old papers or books that have remained unknown for perhaps a century or longer, and that are being periodically unearthed by people (*bona fide* or otherwise) with as result necessary but, to many workers, irritating changes in nomenclature.

The means suggested above makes it unnecessary to make up the proposed list of invalid publications.

- (3) I fully agree with the various suggestions enumerated in para. **9-37** of your inquiry.
- (4) See under (2).

DOCUMENT 1/33

By ERICK M. HERING

(Abteilungsleiter am Zoologischen Museum der Humboldt-Universität, Berlin)

Statement received on 7th June 1952

Stellungnahme zu den Vorschlägen zur Sicherung der Stabilität in der zoologischen Nomenklatur

Das in den Para. 4-7 im *Bull. zool. Nomencl.* vol. 7 behandelte "Praeskriptions-Prinzip" wird nach Mitteilung von Dr. Walter Forster von der Zoologischen Sammlung des Bayerischen Staates mit 11 von 12 Stimmen des Zoologen-Stabes des Museums München unbedingt *abgelehnt*. Die Vorschläge zur Sicherung der Stabilität durch Erklärung und Erweiterung der "*Règles*" werden *unterstützt*.

Der Zoologen-Stab des Museums Berlin lehnt das Praeskriptions-Prinzip ebenfalls ab und *unterstützt* die Vorschläge zur Sicherung der Stabilität durch Erklärung und Erweiterung der "*Règles*" mit folgenden Ausnahmen:

Zu Para. 18 (p. 168-170): Der Vorschlag, dass *nomina dubia* auf Antrag der Spezialisten durch die International Commission on Zoological Nomenclature auf den "Offiziellen Index der zu verwerfenden Namen" gesetzt werden können, wird mit 13 von 14 Stimmen *abgelehnt*.

Zu Par. 23 (4) wird für den Vorschlag der Zusatz für nötig gehalten: "wen die Spezies oder Subspezies an dem Ort vorkommt oder vorkam, den der Name bezeichnet."

DOCUMENT 1/34

Statement of the views of the NOMENCLATURE COMMITTEE established jointly by the DEUTSCHE ZOOLOGISCHE GESELLSCHAFT, the PALAONTOLOGISCHE GESELLSCHAFT and the SENCKENBERGISCHE NATURFORSCHENDE GESELLSCHAFT

Editorial Note : The following document which was published in the issue of 15th June 1952 of the serial publication *Senckenbergiana* (**33**(1/3): 193-196), deals with a number of questions, of which some only are directly concerned with the problem whether provisions and, if so, what provisions should be inserted in the *Règles* for the purpose of promoting stability in zoological nomenclature. It is published here *in extenso*, since in the circumstances this appears to be the most appropriate course, having regard to the fact that the authors of this document base all their arguments upon the need for promoting stability in zoological nomenclature. At later points in the present volume backward references will be made to those portions of the present document which refer to subjects which form separate items on the Agenda for the Copenhagen Meetings, in order thereby to obviate the risk of the views expressed in regard to those matters in the present document from being overlooked by reason of not having been submitted in the form of separate statements (intl'd) F.H. 10th April 1953.

Antrag an die Internationale Kommission für Zoologische Nomenklatur

Die unterzeichneten Gesellschaften haben zur Prüfung der laufenden Fragen eine Nomenklatur-Kommission eingesetzt und diese zu folgender Erklärung ermächtigt.

A. Aufschub jeder Änderung der Regeln bis nach dem Kongress von 1953

Die auf dem Zoologen-Kongress in Paris 1948 gefassten Beschlüsse zur Änderung der geltenden Internationalen Regeln der Zoologischen Nomenklatur sind, vor allem in Nord-Amerika, auf den Widerspruch vieler Forscher und grosser Körperschaften gestossen¹). Die formellen Einwände richteten sich gegen die Unrechtmässigkeit des Verfahrens, besonders gegen die ungenügende Vorbereitung und das nicht einwandfreie Zustandekommen der Beschlüsse.

¹Veröffentlicht sind besonders scharfe Proteste der Nomenclature Discussion Group in "Science," **110** : 594, 1949 und **112** : 27-30, 1950. Diese Gruppe besteht aus 71 Zoologen (Neo- und Paläozoologen) folgender Anstalten in Washington : Smithsonian Institution, U.S. National Museum, Paleontological Branch of the U.S. Geological Survey, Bureau of Entomology of the U.S. Dep. of Agriculture, Fish and Wildlife Service of the U.S. Dep. of the Interior, U.S. Public Health Service. Ja sogar der Präsident der Internationalen Nomenklatur-Kommission selber, der in Paris nicht anwesende Dr. J. L. Peters-Cambridge (Mass.), hat sich diesem Protest gegen die Pariser Beschlüsse seiner Kommission (d. h. der dort in Erscheinung getretenen Kommission) angeschlossen und hat den unterzeichneten Vertreter der Paläontologischen Gesellschaft brieflich zu einem ähnlichen Vorgehen aufgefordert.

Für wertvolle Anregungen danken wir Dr. R. E. Blackwelder (Secretary der Nomenclature Discussion Group, Washington) und Dr. E. Mayr (American Museum of Natural History, New York).

Die sachlichen Einwände betonen die Beunruhigung der Nomenklatur durch die drohende und unnötige Änderung sehr vieler Namen.

Die Zoologen (neontologische und paläontologische) ganzer Länder sind zu dem "Internationalen" Kongress in Paris 1948 weder eingeladen noch zugelassen worden. Sie dürfen erwarten, dass ihnen ausreichende Gelegenheit zur persönlichen Beteiligung an allen Beratungen und Beschlüssen gegeben wird, die eine internationale Anerkennung finden sollen.

Erst der Internationale Zoologen-Kongress in Kopenhagen 1953 wird diese Möglichkeit bieten. Vorgreifende Entscheidungen würden die Gefahr eines Schismas der Nomenklatur heraufbeschwören.

B. Sachliche Einsprüche gegen die Pariser Beschlüsse von 1948

Die Pariser Beschlüsse widersprechen in vielen Punkten dem obersten Grundsatz der Nomenklatur, nämlich dem Prinzip der Stabilität. Ohne damit die übrigen zu billigen, wird hier gegen folgende Punkte Einspruch erhoben:

1. Allgemein: Neue Bestimmungen sollen mit rückwirkender Kraft eingeführt werden, z.B. bei der Zeilen-Priorität, bei Emendationen und in einer Reihe weiterer Fälle. Es sollen also veröffentlichte und bisher gebrauchte Namen nachträglich geändert werden, wenn sie den neuen Bestimmungen nicht entsprechen. Das wäre gegen den Sinn und die gesunde Entwicklung der Nomenklatur zur Stabilität.
2. Die private Veränderung veröffentlichter Namen aus nicht-zoologischen Gründen soll, statt verhindert, begünstigt werden. Eine Reihe von neuen Bestimmungen fordert zu "Emendationen" aus philologischen und ähnlichen Erwägungen geradezu heraus. Das Prinzip der Stabilität verlangt dagegen, dass ein Name in der veröffentlichten Form grundsätzlich unantastbar bleibt²). Ist in einem bestimmten Falle die Änderung der Form eines Namens begründet, dann kann nur die Kommission eine endgültige und obligatorische Emendation vollziehen. Die Erlaubnis oder gar die Verpflichtung zu privaten Emendationen, die oft in mehr als einer Weise möglich sind, führt zu einer dauernden Beunruhigung der Namen.
3. Die Priorität soll verschärft werden zu einer "Seiten-Priorität, Zeilen-Priorität und Stellung-in-der-Zeile-Priorität." Es soll also, bei einer taxonomisch nötigen Vereinigung von Gattungen oder Arten, der Revisor nicht mehr denjenigen Namen konservieren dürfen, der sich durch Gebräuchlichkeit oder Sicherheit der Unterlagen am besten für die Wissenschaft eignet. Vielmehr soll der Revisor gezwungen werden, den Namen zu wählen, der in der betreffenden Veröffentlichung durch Zufall an erster Stelle gedruckt worden ist. Damit wird das Prioritäts-Prinzip, das doch nur als Hilfsmittel für die Stabilität eingeführt worden ist, missverstanden und wird der Stabilität als Selbstzweck übergeordnet³). Dieses von den geltenden

²Vgl. Rud. Richter: Schutz der Tier-Namen vor Emendation.—Senckenbergiana, 32: 357-366, Frankfurt a. M., Februar 1952.

³Vgl. Rud. Richter: "Seiten-Priorität" ist keine Priorität.—Senckenbergiana, 33: 73-74, Frankfurt a. M., Juni 1952.

Regeln abgelehnte und nun neu hervorgeholte "Prioritäts"-Prinzip soll sogar rückwirkend für alle Veröffentlichungen bis 1758 durchgeführt werden! Alle seitdem benützten Namen sollen geändert werden, wenn sich dabei erweist, dass sie der neuen "Seiten (usw.)-Priorität" nicht entsprechen. Die Kommission erbieter sich zwar, "Namen von Wichtigkeit" auf besonders zu begründenden Antrag ausnahmsweise durch Aufhebung der Regeln zu retten; aber die Möglichkeit zu solchen umständlichen Einzel-Verfahren im Ausnahmefall fällt nicht ins Gewicht gegenüber der mutwilligen Änderung aller Namen im Normalfall.

4. Subjektive Homonymie soll einen Art-Namen für immer illegitim machen und zu einer Änderung des Namens zwingen. Nach den geltenden Regeln schläft ein Art-Name nur, solange er zu einer Gattung versetzt wird, in welcher derselbe Name schon mit einem früheren Datum vorhanden ist; aber er bleibt legitim und tritt unverändert wieder in sein Recht, sobald er der betreffenden Gattung nicht mehr zugerechnet wird⁴). Nach den Pariser Beschlüssen dagegen wird ein Art-Name für immer tot und daher ersatz-bedürftig, wenn er zu irgendeiner Zeit, vielleicht nur für ganz kurze Zeit und von einem Unkundigen (wenn nicht gar einem Tauf-Gierigen), in eine Gattungs-Kombination gebracht worden war, in der er vorübergehend ein subjektives Homonym gewesen ist. Für eine Flut von neuen Namen wird damit absichtlich ein Tor aufgetan⁵).
5. Nomina nuda und Manuskript-Namen sollen als legitimiert gelten, wenn sie in einer Synonymie-Liste erwähnt worden sind, und zwar auch dann, wenn der Autor der Liste einen solchen unfassbaren und daher gefährlichen Namen durch die Erwähnung unschädlich machen wollte. Er hat einen Stolperstein aus dem Weg geräumt, und die Literatur ist ihm dankbar gefolgt. Der gefährliche Name schien damit vorsorglich beseitigt zu sein. Bereits die geltenden Regeln bedürfen in diesem stabilisierenden Sinne eine noch klarere Fassung als bisher⁶). Stattdessen richten sich die Pariser Beschlüsse gegen die Stabilität: Jede Synonymisierung soll als Legitimierung gelten! Die wohlüberlegte Absicht des Revisors soll in das Gegenteil verkehrt werden. Gewiss, für die Zukunft wird man sich dagegen schützen können, indem niemand

⁴Vgl.: "Die endgültige und zeitweilige Verwerfung homonymer Art-Namen" in: Rud. Richter, Einführung in die Zoologische Nomenklatur durch Erläuterung der Internationalen Regeln," 2. umgearbeitete Auflage, S. 195-202, Frankfurt a. M. 1948.

⁵Obwohl die Pariser Beschlüsse noch keinerlei Rechtskraft besitzen, haben sie in der kurzen Zeit seit 1948 schon Schaden genug angerichtet. Ein Beispiel aus der Ornithologie, auf das man in Amerika aufmerksam gemacht hat: Ein Autor hat sich in Katalogen planmässig alle Arten zusammengesucht, die einmal durch vorübergehende Zurechnung zu einer bestimmten Gattung subjektive Homonyme geworden sind und veröffentlicht daraufhin Listen neuer Namen.

⁶E. Stresemann & E. Mayr: Über den Rechtsstand in Synonymielisten enthaltener Nomina Nuda.—Senckenbergiana, 32: 211-218, Frankfurt a. M. Juli 1951.

mehr Nomina nuda und Manuskript-Namen erwähnen wird. Aber für die Vergangenheit soll es keinen Schutz geben. Denn auch dieser neue Beschluss soll rückwirkend sein und allen als synonym erwähnten, mit Recht vergessenen Nomina nuda und Manuskript-Namen legitime Rechte verleihen auf Kosten der gebräuchlichen Namen.

C. Notwendigkeit einer Präambel für die Internationalen Regeln

Eine Präambel sollte den Internationalen Regeln als Richtschnur vorangestellt werden und folgende übergeordnete Sätze enthalten :

1. Der oberste Zweck der Regeln ist die Stabilität der Namen.
2. Wenn eine Bestimmung der Regeln mit dem Prinzip der Stabilität in Widerstreit steht, so ist es die Pflicht der Internationalen Kommission die Änderung der betreffenden Bestimmung herbeizuführen. Denn die Namen sind wichtiger als die Nomenklatur.
3. Die Zuständigkeit der Internationalen Kommissionen beschränkt sich auf die Nomenklatur und erstreckt sich nicht auf Fragen der Taxonomie.

D. Notwendige Beschleunigung auf dem Wege zu einer stabilen Nomenklatur

1. Die Regeln sollten zum Schutz gegen die Exhumierung vergessener Namen eine Verjährung der Priorität einführen. Nach Vorschlägen, die bereits in Nord-Amerika gemacht worden sind, etwa in folgender Weise : Ein Name verliert seine Priorität, wenn er während 50 Jahren in weniger als 5 % derjenigen Veröffentlichungen benützt worden ist, die sich mit der betreffenden Einheit beschäftigt haben. (Formulierung von E. Mayr, New York.)

2. Zusammenfassende Werke sollten im Block mit allen darin enthaltenen Namen der Gattungen und deren Typus-Arten endgültig als Nomina conservata auf die Offizielle Liste gesetzt werden. Es gibt solche Werke, in denen anerkannte Spezialisten die Gattungen (mit den Genotypen) grösserer Gruppen zusammenfassend dargestellt haben. Ausschüsse von Spezialisten, berufen von den wissenschaftlichen Gesellschaften, könnten solche Werke der Internationalen Kommission empfehlen und den Schutz der darin enthaltenen Namen beantragen.

Als Beispiel sei der "Treatise on Evertbrate Paleontology" genannt, an dem Spezialisten fast aller Länder ihre Gruppen bearbeiten. Die Anerkennung des Treatise in diesem Sinne, wie sie bereits bei der Kommission beantragt worden ist⁷⁾ würde ein märchenhaft erscheinendes Ziel mit einem Schlage verwirklichen : Praktisch sämtliche Gattungs-Namen (mit Genotypen) der fossilen Wirbellosen würden jeder künftigen Erörterung enthoben sein. Ein gewaltiges Kapital an Geld und noch mehr an Zeit wäre dadurch für wirkliche Forschungs-Aufgaben gewonnen. Ein solcher Präzedenz-Fall würde die Entwicklung der Zoologischen Nomenklatur auf einen neuen Weg und schnell zum Ziele führen.

⁷⁾Rud. Richter : Antrag.—*Bull. zool. Nomenclature*, 6 : 184, London, May 1952.

Sollten sich dann wirklich für den einen oder anderen Namen begründete Bedenken ergeben, dann ist es die unvergleichlich geringere Mühe, diese einzelnen Fälle als Ausnahmen durch die Kommission mit Hilfe der Suspension zu lösen. Die Kommission würde also nicht sofort völlig arbeitslos werden.

Wenn sich dagegen auch in Zukunft die Kommission darauf beschränkt, jeden Namen einzeln in seiner Historie zu untersuchen, und zwar oft unwichtige und nur durch Zufall aufgetauchte Namen, und diese dann nach jahrelangen Sonder-Verhandlungen und in jedem Einzelfall mit grossem Aufwand an Druckwerk zu schützen, so werden noch Hunderte von Jahren vergehen, bis das Ziel erreicht ist, zu dem man einst hoffnungsvoll die Regeln und die Kommission geschaffen hat.

3. Eine kurze und klare Fassung aller Regeln ist erforderlich und wäre leicht möglich.

Für die geltenden Regeln, die noch den Geist von R. Blanchard und Franz Eilhard Schulze atmen, wäre der logische Einbau aller etwaigen neuen Bestimmungen einfach. Für die Neu-Fassung einiger Artikel sind bereits von verschiedenen Seiten Vorschläge veröffentlicht worden. Grundsätzlicher Änderungen bedarf es dabei nirgends; solche wären auch nicht im Interesse der Kontinuität und Stabilität.

Dieselbe Forderung wird erhoben für die "Gutachten" (opinions) und für alle Verhandlungen der Internationalen Kommission. Die Grundsätze und Ausführungs-Bestimmungen sind an sich logisch und einfach; sie können, wenn man es ernstlich will, in der schlichten Sprache des Lebens ausgedrückt werden. Leider ist die Kommission in den letzten Jahren zu einer umständlichen Juristen-Sprache übergegangen, welche die Klarheit nicht erhöht, aber die Verständlichkeit so erschwert, dass kaum ein Zoologe davon Notiz nimmt. Die ständigen Wiederholungen unnötiger Floskeln machen diese Veröffentlichungen dickleibig; der Preis wird so hoch, dass sie nur an sehr wenigen Stellen zugänglich sind.

Die wissenschaftliche Welt ist daher über die Vorgänge und Pläne auf dem Gebeit der Nomenklatur ohne Unterrichtung geblieben. Daher kann auch dieser Einspruch gegen die Pariser Beschlüsse erst heute, im letzten Augenblick vor dem auf den 1. Juli 1952 festgesetzten Termin, erfolgen.

Im Auftrag folgender Gesellschaften :

Deutsche Zoologische Gesellschaft :

E. STRESEMANN, K. GÜNTHER, O. KOEHLER, H.-J. STAMMER

Paläontologische Gesellschaft :

RUD. RICHTER, R. DEHM, J. GÖRGES, W. GROSS, H. HILTERMANN, O. KÜHN,
O. H. SCHINDEWOLF, HERMANN SCHMIDT.

Senckenbergische Naturforschende Gesellschaft :

R. MERTENS, E. FRANZ, RUD. RICHTER, W. SCHÄFER, HERTA SCHMIDT,
J. STEINBACHER, W. STRUVE, E. TRIEBEL, A. ZILCH.

DOCUMENT 1/35**Views of MEMBERS OF THE SOCIETY OF SYSTEMATIC ZOOLOGY
on the question of the Law of Priority**

Letter, with enclosure, dated 19th June 1952, from Professor CHARLES D. MICHENER, former Chairman of the Nomenclature Committee of the above Society (letter transmitted by Dr. W. I. FOLLETT, Professor Michener's successor as Chairman of the foregoing Committee)

The Nomenclature Committee of the Society of Systematic Zoology has recently made a study of a number of nomenclatorial problems. In some instances it has sent a ballot to members of the Society in order that its opinion and recommendations to you would correctly reflect the viewpoint of American taxonomists.

The material on the enclosed sheets deals with the results of such ballots. It is transmitted to you with the request that the members of the International Commission on Zoological Nomenclature be notified of the stand of the Society on the problems concerned, and that, where rules or opinions are involved, this stand be considered in connection with the proposed revisions of the *Règles*.

Editorial Note : *The document here reproduced was the only one of the enclosures to Professor Michener's letter which was concerned with the problem of stability and the Law of Priority and is therefore the only one of those enclosures reproduced at this point (intl'd F.H. 10th April 1953).*

Enclosure to Professor Michener's letter**Law of Priority**

It appears that the Law of Priority, which many taxonomists have regarded as the cornerstone of nomenclature, is now under strong attack in some quarters (cf. Hemming, 1950, Minutes of the Meeting of the ICZN, 1948, in *Bull. zool. Nomenclature*, vol. 4, p. 233. "No member of the Section spoke in favour of the continuance of the present system under which the Law of Priority not only failed to promote uniformity but was itself actively instrumental in leading to confusion and instability in nomenclature").

In order to determine the views of American taxonomists on this matter, the ballot shown below was sent to members of the Society of Systematic Zoology.

1. I favour the STRICT application of priority, with no exceptions.
2. I favour REASONABLY STRICT application of priority, with few exceptions, that must be virtually universally approved (e.g., *Musca domestica*, *Plasmodium*, etc.), where confusion would result in other fields as well.
3. I favour MODERATE APPLICATION of priority, with more exceptions than in No. 2, but applications for these exceptions must be carefully and fully documented and must receive general approval. Generally, exceptions should be made only for cases of real confusion, i.e., not for straight synonymy but to prevent REVERSAL OF USAGE in important names.

4. I favour PRIORITY as a general principle, but with reasonably free exceptions, to be made in any group whenever the specialists in that group so agree, regardless of how important the case is and whether confusion or mere synonymy is involved.

5. I favour USAGE as the guiding principle, regardless of priority.

The voting on these choices was as follows :—

- (1) 33 (Strict application of priority).
- (2) 119 (Reasonably strict application of priority).
- (3) 187 (Moderate application of priority).
- (4) 29 (Free exceptions).
- (5) 3 (Usage as the guiding principle).

In view of these returns, and also in view of the consensus of opinion among the members of the Nomenclature Committee of the Society of Systematic Zoology, it is recommended that the International Commission of Zoological Nomenclature consider setting aside the law of priority only when carefully and fully documented cases are presented to it, and then only (1) when virtually universally approved names (e.g., *Musca domestica*, *Plasmodium*) are to be conserved or (2) when real confusion such as reversal of usage would result from strict application of the law of priority, or (3) when changes in subfamily or family names would result from strict application of the law of priority.

CHARLES D. MICHENER,
*Chairman, Nomenclature Committee,
Society of Systematic Zoology.*

DOCUMENT 1/36

By ANGEL CABRERA
(*Eva Peron, F.C.N.G.R., Argentina*)

Statement dated 22nd June 1952

ON THE LAW OF PRESCRIPTION. Z.N.(S.)359

I *do not* consider that a Law of Prescription would be the best means of promoting the stabilisation of Zoological Nomenclature. On the contrary, such a law would seem to me the best way to go back to the chaotic situation which existed prior to the adoption of the *Règles*. Personally, I am somewhat sceptical about stability of Nomenclature. Every human thing is unstable, specially so laws and regulations voted in international congresses, in which by reasons of distance and travel expenses the influence of a local majority is exaggerated. Whether our efforts will end in relative stability remains to be seen, but common sense tells us that we can approach such an ideal only through objective, impersonal means, and not by subjective and changeable ways. The Law of Priority is not perfect, but it is at least a sound, objective

law, resting on the neutral and impersonal basis of the date of the beginning of binominal nomenclature. A Law of Prescription—apart from the weak points so aptly pointed out by yourself—would be a subjective law, based on an arbitrary date and therefore open to constant modification. I suppose the date 1850 in Dr. Lemche's proposal has been selected with a view to the difficulties about old zoological literature. Since no less than forty-four Danish zoologists advocate this proposal, it is possible that it may be approved in the Copenhagen Congress. Well, the next Congress, or the one following the next, may transfer the Prescription date to 1950, or to any other year, on the ground that the two World Wars involved a serious drawback for scientific research and scientific intercommunication. The principle of Prescription has not yet been discussed and we already have two or three different opinions about its regulation. That suffices to show that, in whichever way we see the matter, it is not by falling into the complications and discrepancies of Prescription that we can escape the disadvantages of Priority.

DOCUMENT 1/37

By ANGEL CABRERA

(*Eva Peron, F.C.N.G.R., Argentina*)

Statement dated 22nd June 1952

ON "NOMINA DUBIA." Z.N.(S.)359

The *nomina dubia* question is more taxonomical than nomenclatorial, but I think it absolutely necessary to have in the *Règles* some provisions about such names, this being one of the cases in which taxonomy and nomenclature are inseparable. The suggestions about the treatment to be given to generic *nomina dubia* of classes (1) and (2) are quite acceptable. As for class (3), I dare to suggest the inclusion in the *Règles* of a provision ruling that, when a nominal genus based on an unrecognisable species has been accepted by specialists during the fifty years following its publication to include species supposedly congeneric with the unrecognisable type, that nominal genus must be declared valid, and its new type should be the first recognisable species selected as such either by the author of the genus or by another author. As to trivial *nomina dubia*, I think that in cases where the specialists are utterly unable to identify the corresponding taxonomic species, it is necessary to distinguish two classes: (1) names of species unrecognisable because the identification is impossible without seeing the type material, and this type material is lost (e.g. *Simia apedia* Linnaeus, 1758, not determinable from description and based on a lost specimen); and (2) names established on a fantastic or imaginary basis (e.g. *Megophias megophias* Rafinesque, 1817, based on an imaginary sea serpent). In my opinion, names in class (1) must be rejected for the purposes of the Law of Priority, but not for those of the Law of Homonymy, whereas names in class (2) must be rejected for the purposes of both laws. In fact, these names, although sometimes accepted, fall under the scope of *Opinion* 2 and of the Recommendation agreed to in Paris (*Bull. zool. Nomencl.* 4: 144), as they are not based on actual things.

DOCUMENT 1/38

By JOSHUA L. BAILY, Jr.

(San Diego, California, U.S.A.)

Statement received on 24th June 1952

REFERENCE Z.N.(S.)359 : *Bull. zool. Nomencl.* **7**: 174, Paragraph 23(4)

Examples of specific trivial names which are also the names of types localities are *Sonorella mustang* Pilsbry and Ferriss, from the Mustang mountains, and *Vespericola columbiana shasta* Berry, from Mt. Shasta.

Personally I disapprove of such names, and would like to have the Commission issue a ruling that when a geographic name becomes a trivial name it should first be Latinized (unless it already has the form of a Latin noun) and then either given the genitive form or an appropriate termination such as “-ensis” or an equivalent.

I would caution against adoption of a rule declaring such localities as type localities. Generally they are such, but not always. Sometimes such names are given as the result of a misunderstanding of the facts. *Pecten magellanicus* does not come from the Straits of Magellan but from New England. *Cassis madagascariensis* does not come from Madagascar but from the West Indies. *Unio peruvianus* does not come from Peru but from the Ohio River.

Whether these names should be changed by suspension of the rules is a question that will someday come before the Commission. Until that time it would seem to me wiser to defer the action contemplated by this application, desirable as it may be in most instances.

DOCUMENT 1/39

Statement furnished on behalf of the

SCIENTIFIC STAFF OF THE ROYAL ONTARIO MUSEUM
OF ZOOLOGY AND PALAEONTOLOGY, TORONTO,
CANADA

Letter, dated 26th June 1952, from F. A. URQUHART, Director

As you are no doubt aware, the Royal Ontario Museum of Zoology and Palaeontology is rapidly becoming the centre for the study of systematic zoology in Canada. In view of the fact that we now have a trained staff of systematic workers, I thought it advisable to bring the various matters contained in the publications of the International Commission on Zoological Nomenclature to their attention. After due consideration we held meetings to discuss the various problems upon which you have asked for advice.

I most humbly submit to you some suggestions and conclusions which we as a body of systematic workers have drawn up for your consideration.

I do sincerely hope that we may have added clarification, rather than confusion, to some of the problems which are now being considered by you for presentation at the next annual congress. I realise only too well the many difficulties that are involved. I also realise that it would be quite impossible to formulate any set of rules and expect to get a unanimous approval on all of them. It is, however, my sincere hope as Director of one of the largest systematic institutions in North America that we shall eventually arrive at simplified rules which can be grasped by the beginning student, and rules which when followed by all workers will eventually lead to extreme clarification.

May I take this opportunity to congratulate you on the very fine work which you have been doing. The manner in which you have outlined the various problems concerned and the amount of research which you have done on these various problems is most commendable. May I wish you every success in your very great and very important task.

Enclosure to Dr. F. A. Urquhart's letter of 26th June 1952

We unanimously agreed that the stabilization of zoological nomenclature could best be secured by means of amplifications, clarifications and extensions carried out within the existing framework of the rules and by the more extended use of the existing provisions for stabilizing individual names through the *Official Lists*.

We felt that it would be helpful if a preamble introduced the entire code, making clear the purposes for which rules of zoological nomenclature have been established.

We agreed with the suggestion that, as part of any general plan for the promotion of stability in nomenclature, the plenary powers provision should be reworded by the omission of the phrase "greater confusion than uniformity" and having inserted in its place a condition that the plenary powers are for use where, in the judgment of the Commission, the use of those powers is clearly necessary for the purpose of promoting stability and uniformity in zoological nomenclature.

We agree with the suggestion submitted by Dr. Mayr and outlined by you in paragraph 15, page 164 of volume 7, parts 5 and 6.

We unanimously agreed with the suggestion set forth by you in paragraph 16, page 167 of volume 7, parts 5 and 6.

We unanimously agreed with the suggestion set forth by you in paragraph 18, pages 169-170. We again suggest however, that application to the Commission on Zoological Nomenclature might be made through the person or persons in charge of the Systematic Studies for a particular Order who, as previously outlined, would be in communication with other workers in this particular field.

We unanimously agreed with the provision as set forth by you in paragraph 23, page 174 of volume 7, parts 5 and 6.

DOCUMENT 1/40

By A. MYRA KEEN and SIEMON S. MULLER
(Stanford University, Stanford, California, U.S.A.)

Enclosure to a letter dated 1st July 1952

COMMENTS ON THE QUESTION OF MEANS FOR PROMOTING THE
GREATEST POSSIBLE STABILITY IN
ZOOLOGICAL NOMENCLATURE

The " Law of Prescription " seems to us to be, at this late date, not practicable. Had it been promulgated some fifty years ago, before many long-forgotten works had been exhumed, it might have been useful. Its application now would only lead to confusion.

As to means of promoting greater stability, we feel that strict adherence to the *Règles* as now in effect, or at least to the *Règles* as they will be when the clarifications adopted by the 1948 Congress are included, will lead to stability. We deplore too frequent resort to the use of the plenary powers of the Commission, for the result of such action will be to set up an esoteric body of knowledge available only to those who have access to the facilities of a large library or who can afford to subscribe to the publications of the Commission and also to those persons having leisure and inclination to read a large volume of output. Priority may at times require the dropping of a familiar name and the learning of an unfamiliar one ; but this, we feel, is much to be preferred to the uncertainty that results when suddenly one discovers that by action of the Commission, priority has been set aside to favour the wishes or convenience of some petitioner. As one of our correspondents has wryly said of such action, " This cuts the ground from under us."

DOCUMENT 1/41

Statement furnished on behalf of the NOMENCLATURE COMMITTEE AT THE AMERICAN MUSEUM OF NATURAL HISTORY, NEW YORK, U.S.A.

Extract from a letter dated 10th July 1952, from JOHN T. ZIMMER

As I wrote you before, I have had considerable difficulty in attempting to get the Committee's action on the various proposals on which you wished our opinion. As I said then, it is a bad time of year, since many of the members of the staff and of the Committee are away for the summer, and some left before the matter could be brought to their attention. Consequently, I have no full Committee action to report. However, I have drafted our reply and suggestions and have added the names of the individuals who approved them as written; some of them are not members of the Committee.

Enclosure to Dr. John T. Zimmer's letter of 10th July 1952

MEANS FOR SECURING STABILITY IN NOMENCLATURE
Z.N.(S.)359

Under this heading is the question of a "Law of Prescription" as against removal of obscurities in existing rules. The former denies priority to names that have been unused for 100 years following original publication, but its adoption and use would entail exhaustive searches through all literature, probably an impossible task. Dr. Mayr suggests 50 years and excludes mere citations in synonymy from consideration as "usage." It might be possible to require evidence of consistent usage in even moderate degree in comprehensive treatments to establish usage, not complete absence of such; a modified "auctorium plurimorum" plan with a "Statute of Limitations." Such a statute presents many difficulties of adequate formulation and should be thoroughly discussed before final adoption. It is possible that a 95 per cent. usage other than in synonymies, bibliographies, and compendiums such as Sherborn's "Index Animalium," should constitute adequate acceptance.

The removal of obscurities in existing rules should also be accomplished, but not necessarily as an alternative to the Law of Prescription, if adopted. It should include a "Sub-Title or Preamble" defining the primary purpose of the Code as to ensure stability of nomenclature.

An *Official Index* of rejected and invalid books should be useful.

Elimination of generic and specific names of indeterminate application. This should aid in stability. The names will be rejected for the purposes of priority but not of homonymy.

Elaborate rules have been proposed for fixing type localities: original designation, monotypy, restriction to originally included localities in subsequent designation, etc. Correction of erroneous original localities to be by the Commission. In subsequent fixation, if supposedly lost holotype is rediscovered

and has a different locality from that selected, the case is to be referred to the Commission. Similarly, if overlooked data are discovered in books or manuscripts by the original author upsetting the designated locality, the case will go to the Commission. Similar rules are proposed for horizons of fossil species ; also for hosts of parasitic species. On the whole, such rules ought to be fairly simple to operate, although it is questionable how much should be put on the shoulders of the Commission. Common sense should dictate the selection of type localities from among those from which the type could have come, and on discovery of error could be automatically corrected by the first reviser principle. No elaborate rules are required. The Commission should not be saddled with more work than it can accomplish without long delays.

A.M.N.H. Committee on Nomenclature

(Signed) Edwin H. Colbert

John T. Nichols

Ernst Mayr

George H. H. Tate

John T. Zimmer (Chairman).

DOCUMENT 1/42

Statement furnished on behalf of the

NOMENCLATURE DISCUSSION GROUP OF WASHINGTON, D.C.,
U.S.A.

Enclosure to a letter, dated 16th July 1952, from R. E. BLACKWELDER,
Secretary

RESUBMISSION OF PETITION REGARDING THE PLENARY
POWERS* Z.N.(S.)359

The Nomenclature Discussion Group of Washington, D.C., under its then used name of Smithsonian Institution Committee on Nomenclature, submitted to the Secretary of the International Commission on Zoological Nomenclature prior to the Paris Congress a petition on the subject of the use of the Plenary Powers to establish *nomina conservanda* (*Science* **107**: 543-544, May 21, 1948). This petition was supported by votes and comments from a number of American zoologists. The letter of transmittal submitted this material for presentation and discussion at the Paris Meeting of the Commission.

Unfortunately this petition and the supporting material were not presented at Paris. On the other hand, Dr. Henning Lemche did present a petition on a different viewpoint, on behalf of a large number of Scandinavian zoologists. Furthermore, during the Congress there was received, and at once presented, a letter from Austrian zoologists stating views similar to those of Dr. Lemche. Both of these have been published in full with all signatories in the reports of the nomenclature proceedings of the Paris Congress (*Bull. zool. Nomcl.* **3**: 158-161 and **5**: 77-78, 1950). In the light of the foregoing, the omission of our petition from both presentation at the meetings and the printed minutes of the meetings has resulted in a rather onesided appearance of the views of zoologists, an appearance which has been referred to with some emphasis (Hemming, 1951, *Trans. Soc. Brit. Ent.* **2**: 1-15; Hemming, 1952, *Bull. zool. Nomcl.* **7**: 148-188).

In view of the recent appeal for statements on the general problem of stability in nomenclature, we have examined again our petition, and find that it still outlines the position that we wish to present for the consideration of the International Commission on Zoological Nomenclature. Accordingly we hereby formally resubmit the petition for the Nomenclature Discussion Group. The petition is already in the files of the Commission, but two copies of the printed petition are appended.

We have not republished this petition, nor contacted the previous supporting signers, and accordingly we do not formally resubmit the supporting material at this time. However, we do wish to call attention to the fact that of the 170 zoologists who voluntarily responded on the original petition, 156 supported our petition, and 7 would even have gone much farther in limiting the plenary powers. It is doubtful that there would be much change in the votes at the present time.

*See immediately following Editorial Note.

Editorial Note

The document enclosed with the foregoing letter was the same as that which had been enclosed with Dr. Curtis W. Sabrosky's letter of 25th June 1948. In view of the fact that that document had been earmarked for consideration at Copenhagen when the discussion on the proposed introduction of a Law of Prescription came to be resumed from the point at which it was adjourned at Paris in July 1948 (1950, *Bull. zool. Nomencl.* **4** : 233), I reached the conclusion, when I came to prepare the present volume for publication, that the most appropriate course would be to publish the document in question, together with Dr. Sabrosky's letter of June 1948, at the appropriate point in the historical series in which it had been decided that communications received on the subject of the stabilisation of zoological nomenclature should be arranged, and to publish at the point appropriate for documents dated July 1952 the covering note enclosed with Dr. Richard E. Blackwelder's letter of 16th July 1952, while not reprinting the longer paper submitted in June 1948, since under the procedure proposed that document would already have been published in the present volume in its historically correct place, namely as the enclosure to Dr. Sabrosky's letter of 25th June 1948.

2. Accordingly, in a letter dated 17th January 1953 I put the foregoing suggestion to Dr. Blackwelder, by whom the document referred to above had been resubmitted to the Commission and who had succeeded to the position formerly held by Dr. Sabrosky as Secretary of the body which had in the mean time been renamed the Nomenclature Discussion Group, Washington, D.C. As will be seen from the extract from Dr. Blackwelder's reply of 5th February 1953, given in the annexe to the present note, he concurred in the procedure which I had suggested.

3. Accordingly, the statement prepared by the Smithsonian Institution Committee on Nomenclature, transmitted under cover of Dr. Sabrosky's letter of 25th June 1948, is reproduced as Document 1/8 in the present series (see pp. 9-11), while the covering note prepared by the Nomenclature Discussion Group, when resubmitting the foregoing statement (under cover of a letter, dated 16th July 1952, by Dr. Blackwelder) is reproduced here as Document 1/42, but without the document then resubmitted, that document appearing (as explained above) in its place as the annexe to Dr. Sabrosky's letter of 25th June 1948.

(intl'd) F. H., 25th February 1953.

Annexe to Editorial Note

Extract from a letter dated 5th February 1953, from Dr. Richard E. Blackwelder, Secretary Nomenclature Discussion Group, Washington, D.C.

Perhaps our "resubmission" of the application on the use of the plenary powers, and the wording used in referring to the original documents was not as clear a statement of our intention as we should have made. The original (1948) application was submitted for consideration by the International Commission on Zoological Nomenclature and is presumably still before them.

The resubmission was intended merely to call attention to it and reiterate our belief in the contents. Our inability to speak again for the original supporters in no way affects the original proposal or their support of it.

It appears to us that the original application is the one to be considered, with our resubmission as possible supplementary matter. No doubt it would also be useful to include the pertinent paragraphs from Mr. Sabrosky's letter of June 25 1948, because they constitute a summary of the attached papers.

DOCUMENT 1/43

By C. W. WRIGHT

(*London*)

Enclosure to a letter dated 18th July 1952

ANSWERS TO QUESTIONS RAISED IN PARAGRAPH 39 OF THE PAPER BY THE SECRETARY TO THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE (1952, *Bull. zool. Nomencl.* **7** : 187-188)

Question (1) A Law of Prescription would be a confession of defeat. I see no reason, taking a long view, for pessimism. The Rules must be amended if necessary to deal with every type of problem that crops up. Given common sense and the Rules difficulties will be resolved. The great peril to nomenclature lies in the fact that many authors are both ignorant and selfish. Several prominent palaeontological monographers of my acquaintance either do not trouble to apply the Rules or if they do try to apply them they misunderstand them.

(3) The main solution to the difficulty of ancient works is for authors to get into the habit of considering the possibility and advisability of applying for such works to be placed on the *Index* before starting to use names excavated from them.

DOCUMENT 1/44

By the NOMENCLATURE COMMITTEE AT THE AMERICAN MUSEUM
OF NATURAL HISTORY, NEW YORK, U.S.A.

Letter, dated 18th July 1952, from ERNST MAYR

I am sending you herewith an additional petition to the Commission concerning
a Preamble to the *Règles*

Enclosure to Dr. Ernst Mayr's letter of 18th July 1952

It has been criticised by numerous taxonomists that the present version of the *Règles* does not contain an adequate statement of the object of the rules of nomenclature. To meet this criticism we propose that the following preamble of the Rules be adopted.

PREAMBLE

The object of the Rules of the Zoological Nomenclature is to establish the basis for a uniform and stable set of zoological names. All individual provisions of the Rules are subservient to this ultimate goal. If the application of the Rules in an individual case threatens the stability of nomenclature such a case shall be brought to the attention of the International Commission for possible exercise of its Plenary Powers. If the application of a given Rule consistently disturbs the stability of nomenclature, it shall be the duty of the Commission to modify said rule in such a manner as to restore stability.

Signed :

The Nomenclature Committee at
the American Museum of Natural
History, New York.

Ernst Mayr

John T. Zimmer

G. H. H. Tate

C. H. Curran

DOCUMENT 1/45

By ERNST MAYR (*The American Museum of Natural History, New York*)

Enclosure to a letter dated 18th July 1952

The Commission is herewith petitioned to redraft Article 25. The provisions now included in Article 25 should be separated into two articles, one dealing with the availability of names and one with the validity of names. In the former article all provisions concerning proper publication of names should be included.

The article on validity of names should contain provisions concerning synonymy and homonymy, as well as a statute of limitations. It should

include a statement on priority as well as rules specifying under which conditions priority may be set aside. A suggested draft of such specifications is attached.

Enclosure to Dr. Ernst Mayr's application of 18th July 1952

RULE OF VALIDITY

If two names are available for the same taxonomic unit, the earlier published available name ("senior synonym") is the valid name unless it is invalid owing to homonymy or unless the later published name ("junior synonym") has special rights as *nomen conservatum* or *nomen conservandum*.

A *nomen conservatum* is a name that has been preserved by a special decision under the plenary powers of the International Commission and which has been placed on the appropriate "List of Official Names."

A *nomen conservandum* is any name belonging to either of the following categories of names :

- (1) Any name on one of the *Official Lists of Names in Zoology* (excluding lists of names not available) but concerning which the Commission has not taken action under its plenary powers.
- (2) Any name in current usage for the conservation of which a petition to the Commission is pending.

A *presumptive nomen conservandum* is any name belonging to either of the following categories :

- (1) The name of any genus that is the type-genus of a super-generic category.
- (2) Any name (excluding a binomial or trinomial combination) that has been applied exclusively or virtually exclusively to a taxonomic unit for a period of fifty years or more. Such a name must have been used in at least ten separate publications in order to qualify. This period may be shortened to not less than 30 years for names that have been used in 100 publications or more during that period.

The law of priority does not apply to *nomina conservata*. It shall not be applied to *nomina conservanda* or presumptive *nomina conservanda* in such a manner as to disturb current usage, or the user's view as to what is current usage, unless the Commission so directs. When a taxonomist discovers that the application of priority to a presumptive *nomen conservandum* would disturb usage, it shall be his duty to present the case to the International Commission, but to conserve current usage pending their decision.

When a presumptive *nomen conservandum* is thus laid before the International Commission, that body shall give it due consideration and shall determine whether the name in question shall become a *nomen conservatum* or whether the *Règles* should, in this case, take their normal course.

DOCUMENT 1/46

By HENNING LEMCHE

(Universitetets Zoologiska Museum, Copenhagen, Denmark)

Letter dated 20th July 1952

(See also Document 1/7)

To the main question about stabilizing nomenclature, viz. the proposed Rule of Prescription (we have not and cannot have real laws), I would like to express my regret that you seem to have misunderstood the way in which it is intended to act. Especially, I felt a little disappointed when I read your suggestive heading to your par. 7: "The weakness of any Law of Prescription arising from the negative character of the test imposed." There is no weakness here, as the whole proposal is positive.

The thing to do is to rule that every case where an old name is found and an author wishes to use it, he will have to cite *both* the old quotation and at least one newer record indicating that the name has been used more recently. If he cannot find any newer record, he has no right to *alter* current usage. So, contrary to the rule of priority, the burden of looking through literature is entirely on the man who wishes to overthrow current usage. The weakness of the priority rule has always been the impossibility to state that a name is really *the* first one. Therefore, your main objections to my proposal could be applied with their full force against the rule of priority, but not against a rule of prescription where the proof that the name is available lies in the presentation of a later quotation (from a place where it is used, *not* in any synonymy).

The proposed rule is exclusively for future use, so that no new disturbance can be started. Moreover, no library difficulties can be involved, because if the author has the book and finds the old name, he must *not* use it, he will have to keep to *current usage*, and let the man near a great library have the sole chance to disturb nomenclature. If, in future, an author revives an old name, other authors should not follow him if he gives no newer citation, because the procedure is incorrect and the change invalid.

To your alternative proposals, I totally agree that it is very good to make the *Règles* as definite and sharp as possible. But, again, we come to our main difficulty: the difference between your deductively trained brain and the inductively trained ones of the zoologists who are to use your text.

I prefer, a *single*, definite article about the plenary powers, but no other references thereto. If we add such references to some articles, there might be authors believing therefrom in that the plenary powers can not be used concerning any other articles. So, we will fall in the pit we are trying to escape.

The *Official Index of Rejected and Invalid Books* seems a good idea. An asterisk could indicate volumes, the names of which would be "sympathetically regarded" by the Commission for use of the plenary powers.

DOCUMENT 1/47

By JOHN H. LOCHHEAD

(Department of Zoology, University of Vermont, Burlington, Vermont, U.S.A.,
and Marine Biological Laboratory, Woods Hole, Massachusetts, U.S.A.)

Enclosure to a letter dated 21st July 1952

COMMENTS ON THE MEANS TO BE FOUND FOR PROMOTING THE
GREATEST POSSIBLE STABILITY IN
ZOOLOGICAL NOMENCLATURE

Since the comments on the *Limulus* versus *Xiphosura* controversy which I am enclosing herewith have some bearing on the general question of how best to promote uniformity and stability of taxonomic names, you may wish a separate statement from me on this latter topic.

Some general conclusions that seem to me to emerge from the *Limulus* versus *Xiphosura* case are the following. 1. The *Official Lists* must be published and made easily accessible to all zoologists. 2. The legal force of the *Official Lists* must be made clear and widely known among zoologists. 3. In particular, a name once on an *Official List* should not be removed because of new discoveries in the literature, except when the Commission may decide that such removal really would be in the best interests of taxonomic stability and uniformity. 4. The *Official Indexes of Rejected and Invalid Names* also should be published and made easily accessible to all zoologists. 5. An "Official Index of Rejected and Invalid Books" (rather than "Books" I would say "Publications") should likewise be published and made widely accessible. 6. There should be full publication of the arguments involved in each case considered by the Commission.

All of the above proposals have, of course, already been acted on by the Commission or are set forth in Secretary Hemming's report. It is my belief that had they all been in force some years ago, we would not have had such disputes as that involved in the case of *Limulus* versus *Xiphosura*.

It will be seen that I favour developments carried out within the existing framework of the *Règles* rather than a Law of Prescription. As pointed out by Secretary Hemming in paragraph 7, the latter proposal has the same weakness as does unyielding adherence to the Law of Priority—namely that it demands a knowledge of the entire literature before the stability of a name can be assured.

In reference to discoveries in the literature which under the rules would require the changing of long established taxonomic names, it seems to be assumed (in paragraphs 34-36 of Secretary Hemming's report) that the specialists who make such discoveries will voluntarily apply to the Commission for validation on the *Official Lists* of the established names, and rejection on the *Official Indexes* of the newly discovered competitors. Perhaps I have overlooked a proposal already made, but at any rate I would like to suggest that some sort

*The reference here is to Case Z.N.(S.)506, for which see 1951, *Bull. zool. Nomencl.* 2:319-322.

of obligation be placed on zoologists to consult the Commission before displacing a long established name on a merely legal technicality. The main objection might be that publication of taxonomic papers would be held up while awaiting a decision by the Commission. However, it would seem to me that pending such a decision a genus or species could be referred to by its established name, with a note to the effect that the name is one currently under consideration by the International Commission. As a non-taxonomist, I cannot help feeling that perhaps some taxonomists take a wicked delight in changing names, a pleasure that should be denied them so far as possible under the *Règles*.

In regard to the proposal in paragraph 15, of an "Official Index of Rejected and Invalid Books" [or "Publications" ?], might there not also be use for an "Index of Publications partly or wholly approved by the Commission"—with references to the discussions and *Opinions* regarding them ?

DOCUMENT 1/48

By E. RAYMOND HALL

(*Department of Zoology, University of Kansas, Lawrence, Kansas, U.S.A.*)

Enclosure to a letter dated 22nd July 1952

Avoid extending list of *nomina conservanda* and employ instead the rule of priority with 1758 as the controlling date.

DOCUMENT 1/49

Statement furnished covering the views of three members of the NOMENCLATURE COMMITTEE OF THE SOCIETY OF SYSTEMATIC ZOOLOGY, U.S.A.

Letter, dated 27th July 1952, from W. I. FOLLETT, Chairman

STABILITY IN ZOOLOGICAL NOMENCLATURE

I enclose herewith comments on this subject, received from members of the Nomenclature Committee of the Society of Systematic Zoology.

Annexe 1 to the Chairman's letter of 27th July 1952

Extract from a letter, dated 20th May 1952, from HENRY TOWNES, *North Carolina State College of Agriculture and Engineering of the University of North Carolina, Raleigh, North Carolina, U.S.A.*

(7) I would suggest abolishing all exceptions to the rule of priority as the first step.

Annexe 2 to the Chairman's letter of 27th July 1952

Statement by JOHN T. ZIMMER
(*American Museum of Natural History, New York*)

359. Most of the proposals in this section are incorporated in previous sections.

A "Law of Prescription" might be highly desirable if it can be formulated in a workable condition. I do not believe it possible to insist on a name having been "unused" for 100 years or even 50 years following original publication, even excluding citations in synonymy from the category of usage. It is always possible for an obscure and utterly overlooked paper to be found that would cancel the non-usage provision. Possibly some sort of "auctorum plurimorum" principle could be devised that would recognise some general acceptance of a name for lengthy periods even if an occasional author adopted another available name. If the Commission were not so overworked, the *Official List* would offer an acceptable solution for this problem of stability.

I believe a "Sub-Title" of "Preamble" should be printed with the new Code, defining the primary purpose of the Code as the establishment of stability in nomenclature.

The proposed *Official Index of Rejected and Invalid Books* is desirable.

Elimination of generic and specific names of indeterminate application should promote stability. The names should be rejected for purposes of priority but not of homonymy.

The proposed rules covering fixation of type localities are too elaborate. They should be on a first reviser principle, with the requirement that the locality selected must be reasonable—a place from which the type could have come at the time it was presumably collected, a place on the itinerary of the known collector, a place from which the original author is known to have received material, certainly a place in the known range of the form, etc. Such designation is to be subject to revision if overlooked data are discovered that permit greater accuracy. Any disputed data may go to the Commission for action, but such cases should be few if the evidence is conclusive.*

**Extract from a letter dated 21st July 1952, from Francis
Hemming, Secretary to the International Commission on
Zoological Nomenclature, to Dr. John T. Zimmer**

Type localities ; I discussed this matter with Dr. Erwin Stresemann, when I met him in Lappland two years ago, and he then told me that in birds this did not give rise to difficulty. That in some cases it does is shown however by what has recently happened in the case of the name of the Syrian Ostrich, where, as there is at present no provision in the Code, Colonel Meinertzhagen has thought it necessary to make a special application to the Commission (shortly to be published in the *Bulletin*)¹, which would have been entirely unnecessary if the Code had contained rules on this subject. Speaking as a lepidopterist and not as a member of the Commission, I am quite certain that the lack of rules relating to this subject is one of the principal remaining obstacles to stability in nomenclature. The European fauna contains a much larger number of cases where species possess no type localities than any other, owing naturally to the fact that Linnaeus and the other early authors dealt with a relatively much larger number of European species than they did from any other part of the world—probably more than all other parts of the world put together. This problem is therefore one of special urgency and importance to European zoologists. It is clearly, however, not confined either to European workers or to the Lepidoptera, as may be seen, for example, in the arguments raised by the recent attempt by two American herpetologists to catalogue the type localities of the Reptiles and Amphibia of Mexico².

* See also in this connection, Document 1/42.

¹See Meinertzhagen, 1953, *Bull. zool. Nomencl.* 9 : 96-97.

²See Smith (Hobart M.) & Taylor (Edward H.), 1950, *Sci. Bull. Univ. Kansas* 33 (Pt. 2) (No. 8) : 313-380.

**Extract from a letter, dated 6th August 1952, from
John T. Zimmer**

I have delayed in replying to your letters of July 21. . . The difficulty has been that members of other Departments of the Museum are away on vacations and field trips. . . .

As regard type localities, we admit the desirability of fixing type localities. In my own case, I have proposed many such fixations or finer restrictions. The rules, however, should be simple and not necessarily rigid, if the reviser uses good judgment and selects a locality where the collector (if known) is known to have worked; or a locality from which material was available to taxonomists in a possibly early time when the creature was originally described; or a locality not antagonistic to any data given by the describer; or a locality from which material is now available showing agreement with the existing holotype, etc. However, future evidence may show the basis to have been faulty, in which case it should be remediable. Many times this will result in no confusion, although sometimes it may do so. Evidence for alteration would need to be positive and proof would have to be supplied by the would-be reviser.

As a case of what I mean, I may mention one of the humming birds discussed in one of my papers. Originally described from the "Upper Amazon" with certain characters given for it that are definitive. A more recent taxonomist proposed "Bolivia" as restricted type locality. I objected on the ground that Bolivia is not the "Upper Amazon" and Bolivian specimens lack the characters mentioned in the original description, which examples from localities on the Upper Amazon show well. Accordingly, I proposed a new restriction to an Upper-Amazonian locality, in all probability the exact place visited by the supposed collector of the original specimens. To necessitate holding to the first proposal of "Bolivia" would be in disregard of the weight of evidence, and yet it would have been difficult to outlaw such suggestion in advance. Our suggestion to require common sense in the selection of type localities by revisers would have done so much as elaborate rules. In this case two subspecies were involved.

Another case I have in mind. Count Berlepsch proposed a restriction of type locality for one of Spix's Amazonian birds to a certain locality. Hellmayr rejected Berlepsch's proposal on the grounds that Spix had never visited this place. I happened to come across a brief note in Spix to the effect that he had spent a night there. Consequently, I reverted to Berlepsch, but in this interchange there was not the slightest effect on the identity or validity of Spix's species. The same form occurs throughout the area in question.

In any case, we believe any rules for fixation of type localities should be simple, though authority for such fixation may well become a part of the new Code.

Annexe 3 to the Chairman's letter of 27th July 1952

Statement by W. I. FOLLETT

(California Academy of Sciences, San Francisco, California, U.S.A.)

With considerable diffidence, I venture to suggest the futility of our attempting to eliminate minor causes of instability in zoological nomenclature, while we continue to wink at the major cause.

As long as our rules continue to saddle nomenclature with the burden of expressing purely subjective notions of generic relationship, there can be no stability in nomenclature.

In order to avoid transitional inconveniences, we adhere to the system of the past 200 years, in disregard of stability and the requirements of the next 200—and 2000—years.

Supplementary statement by Dr. Follett in response to a question raised by the Secretary to the International Commission

Letter dated 18th February 1953

Your letter of 4th February 1953, concerning my comments submitted under date of July 27th 1952, has just arrived. With the exceptions hereinafter noted, I would answer your queries in the affirmative.

It was not my intention to express disapproval of measures to promote stability, such as those considered in volume 7, parts 5/6, of the *Bulletin of Zoological Nomenclature*. Instead, I wished to emphasise the apparent indifference of zoologists to the major cause of nomenclatorial instability, and to express concern that the frequent sharp protests should so often be directed against minor factors such as the law of priority rather than against that major cause.

I am convinced that the number of changes in nomenclature that have resulted from purely nomenclatorial factors, including priority, homonymy, emendations, position-precedence, and all changes that have been effected in the International Rules, comprise but an insignificant total in comparison with the number of changes in nomenclature that have resulted solely from shifts in generic evaluation. These shifts are based on subjective considerations, and are accordingly subject to continual modification.

In other words, the binominal system of Linnaeus itself appears to be the major cause of instability in nomenclature.

Theoretically, a uninominal system would therefore seem capable of eliminating a substantial element of instability, even though it would not eliminate variations inherent in the definition of species.

It appears unlikely that a uninominal system will be acceptable to zoologists, because of the transitional inconveniences that would arise from so drastic a change, and to a greater degree because of the prevalent desire that nomenclature express relationship.

I am unable, however, to avoid the conclusion that nomenclatorial expression of relationship is in irreconcilable conflict with the attainment of nomenclatorial stability.

DOCUMENT 1/50

By J. CHESTER BRADLEY
(Cornell University, Ithaca, N.Y., U.S.A.)

Statement dated 29th July 1952

(**Note :** The numbers cited in this statement refer to the paragraphs in the paper published by the Secretary to the International Commission on Zoological Nomenclature, 1952, *Bull. zool. Nomencl.* **7** : 148-188).

PROMOTING STABILITY IN NOMENCLATURE

A. Answers to specific questions in Section 39

(1) and (2). I consider that the stabilisation of zoological nomenclature would be importantly promoted by the introduction into the *Règles* of a *principle* of prescription. I do not say a law, because I do not see how it can be made so objective as to be automatically operative. Nevertheless, I think that great good would derive under the following provisions. (This suggestion is essentially and in brief a plan suggested by Dr. E. Mayr.)

(a) That any generic or trivial name that has been virtually exclusively in use for a given taxonomic unit for at least a specified number of years and has been used in at least a specified minimum number of publications shall be presumed to be a *nomen conservandum*.*

(b) That when any taxonomist observes that what he deems to be such a name is not tenable under some provision of the *Règles* he shall not disturb usage, but shall present the case to the Commission.

(c) That the Commission acting not under plenary powers, but by way of putting into operation the principle of prescription, in precedence over other provisions of the *Règles*, shall place the name upon the appropriate *Official List* as one to be conserved, provided in their judgement it qualifies, and such action is not for some reason contra-indicated.

By the preceding plan the subjective element expressed by the word "virtually" is not interpreted by the perhaps conflicting views of different taxonomists, but by central authority instead, and that is just where the existence of central authority provides efficiency.

3. Prescription is not an alternative to developments within the present framework of the *Règles*. Everything must be carried out that will contribute to stability.

*Note that *nomen conservandum* means a name that ought to be conserved, as contrasted with *nomen conservatum* meaning a name that has been preserved, i.e., by official act of the Commission. This distinction of definition has been pointed out *in litt.* by Dr. E. Mayr and has been employed by Stresemann, Richter, Mertens and others (1952, *Senckenbergiana* **33** : 193-196). [J. C. B.]

4. *Particular suggestion for promoting stability.*

A regulation to be inserted in the *Règles* to the effect that: Whenever the status of a name having been correctly determined under the *Règles* and *Opinions* in force at the time, it has come into use as so determined, but some amendment to the *Règles* made subsequent to 1907 would enforce a change, it shall be presumed to be a *nomen conservandum*. It shall be the duty of every taxonomist becoming aware of such a name to present the facts to the Commission, who shall, without having need to apply their plenary power, decide the course to be pursued in the interests of continuity and stability. It is thus to be understood that no amendment made since 1907 is designed to overturn continuity or prevent stability, and is *not operative* in any case where in the opinion of the Commission it would do so.

B. Comments on numbered section of the Secretary's Paper

11. *Preamble.* A preamble is desirable.

I have seen the preamble suggested by the Committee on Nomenclature of the American Museum of Natural History. I like its brevity, and the way that it touches significantly upon just the right points. Its last sentence seems to me not to go far enough.

Before I read their suggestion I had written out some of my own, a copy of which I attach. Mine is far too long, nevertheless I think that some thoughts in it, especially clarification of the fields of nomenclature versus taxonomy are important and ought to be included.

In the Secretary's paragraph 11, I suggest that in the 7th line from the bottom, the word "objective" should be inserted before "species," and that the word "trivial" should be inserted before the word "name." (Let no impression get abroad that it is an aim to freeze specific names, i.e. generic and trivial combinations.) I should add at the end of the section that the system refrains from trespass upon the field of taxonomy. With these changes I fully endorse section 11.

12. *Plenary powers.* I strongly endorse this section. I should reword the last three lines to read "powers are for use where, in the judgment of the Commission, they will promote stability, continuity and uniformity in zoological nomenclature." As previously worded the last sentence places emphasis on whether it is clearly necessary to use the plenary powers to secure the objectives named, or whether they might be secured in some other way. My modification is to place emphasis on the objectives themselves as the important factor. I have added the word "continuity" because stability alone might be thought to mean stability from the time of action, without necessarily including continuity with the past.

13. *Obscurities and lacunae in the Règles.* Agreed.

14. *Stability despite emendation.* I strongly support this section. Instead of lengthening the *Règles* by frequent provisions of the sort indicated, each in connection with some particular instance, would not the drafting be greatly strengthened by a single general provision, somewhat along the lines suggested above under Section A (4).

15. Official Index of Rejected and Invalid Books in Zoology. I support the proposal for action. Since we do not wish to give the public impression that we are casting any stigma upon these books, or rejecting *en masse* as unreliable the biological or anatomical detail that they contain, would not the title better be "Official Index of Books Rejected or Invalid for the Purpose of Zoological Nomenclature" ?

In rejecting a work we reject every zoological name in it, unless special exception are made. Does this not imply that each such name should be entered in the appropriate *Official List of Rejected Names* ?

17. Suppression of nomina dubia at the specific level which no specialist claims to recognise. The fact that no specialist claims to recognise a name does not mean that no specialist can recognise it. No one may ever have had occasion to make a serious attempt to study the type. That is continually found to be the case, *ex. gr.* with North American insects the types of which are housed in Europe. We are confronted not only with what the specialist of the past or today has or can do, but what the specialist of tomorrow may do when he has opportunity. Instead of the provision in Article 31 (B.Z.N.4 : 76 g.2) would not provision along the following line better serve such a case :

If the name in question has consistently had the status of a *nomen dubium*, and no type specimen is known to exist, the Commission may, upon request, suppress the name under their plenary powers.

If a type specimen is known to exist and the Commission is satisfied that adequate study has been given to the type, and that all are agreed that identification is hopeless, the Commission may, under their plenary powers either (a) suppress the name, or (b) cancel the type, thus paving the way to selection of a neotype, according to which may be the better course in the individual case, from the standpoint of continuity of nomenclatorial usage.

If the name has been traditionally employed to represent a certain taxonomic species, but due to the non-existence of a type, or failure to study the type, or unrecognisable condition of the type, doubt is now cast upon its actual identity and it is threatened with becoming a *nomen dubium*, the Commission may if necessary, cancel the type. The way will then be clear for establishing a neotype which will reflect the traditional sense in which the name has been employed.

18. Nomina dubia which specialists cannot identify. Of the alternative actions suggested for the Commission, I agree to (1) suppression of the name. I cannot agree to (2) in the form stated because that is a taxonomic question. The Commission is not competent to rule directly as a matter of fact that a disputed name belongs to a particular taxonomic species, because they may be wrong, and they may not force any taxonomist to accept as a fact what he believes is not a fact. However, the same desired result can better be attained indirectly. They can cancel any existing type material. This paves the way for recognition of a neotype in the sense desired. That done, no question longer remains of whether the original author did or did not describe a particular taxonomic species. The neotype thereafter determines the application of the name. For the same reason Conclusion 11(2) (g) (3) B.Z.N. p. 76, affecting Article 31, is wrong in principle and must be changed if it implies a direct

taxonomic decision. It is perfectly correct if the Commission acts by either suppressing the name, cancelling the type, or recognising a neotype.

19. See my comments on Article 31, on lectotypes and on neotypes.

20-28. *Type Locality.* I am perplexed by the Secretary's discussion of this topic. What is a type locality? We must agree upon a definition before our minds can reach a common groove.

To my mind (but clearly not the Secretary's, who may perhaps reflect a view peculiar to Lepidopterists) the type locality is the spot on the face of the earth where the holotype, lectotype or neotype was living a natural life (unaffected by artificial transport) at the time of its capture. If it were a bred specimen, it would be the spot where its nearest free ancestor was captured. That spot of "locality" can only be known to the person who "collected" the specimen in nature, or from some record left by and derived from its captor, and in many cases cannot be known at all, because no record was kept, or if kept has been lost, or an erroneous record substituted. It is, however, a matter of fact, of truth, just as much as the size of the type, or the number of scales or something else that it possesses is factual. The only method of determining the size is to measure the type, of the number of scales is to count them. These things cannot be determined by the decision of a commission, no one of whom ever saw the type itself.

The Secretary's views appear to be that the type locality is a nomenclatorial fiction, divorced from reality, that a commission may establish.

In paragraph **26** we read "a species based upon an erroneous type locality occupies a position very similar to that of a genus based upon an erroneously determined type species." But a species is not based upon a type locality, it is based upon a type specimen and solely that. It is impossible to have *two* criteria for determination of the application of a trivial name.

Species and subspecies are, by the *Règles*, co-ordinate, and whether a species is polytypic with two or more subspecies, or each of these populations is a distinct species is purely a matter for taxonomy to decide. It is a matter upon which taxonomists hold diverse views. When the holotype, lectotype or neotype determines the taxonomic form to which a specific name belongs it equally determines the form which is the potential nominotypical subspecies. For those who regard the species as polytypic, it is the nominotypical subspecies. Each of the other subspecies (which may be species in the eyes of some taxonomists) has its own type and from the standpoint of nomenclature must be regarded indifferently as species or subspecies.

There may be subspecies individual specimens of which can be differentiated only by those taxonomists who know where the specimen came from. Usually we expect subspecies to be sufficiently distinct for the taxonomist to be able to perceive from where the individual did come. That is not always the case because the criteria of differentiation may regard percentage of the population, rather than individuals. In any case it is purely a taxonomic problem. If nomenclature tries to solve it or to set up artificial standards, it will get its fingers burned.

What then to do, if we have a type with no record, or with an incorrect record, of the locality from which it emanated? What, one might equally

ask, to do, if we have a type that has lost its head, where all differentiating characters are to be found? Certainly we cannot, in the latter instance, permit any commission to rule that the type had six, or seven supraorbital setae, or what not. No commission saw the type, none knows, and they probably would not have been capable of making an *objective* determination if the unmutilated type were before them. Equally, in the former instance, they cannot establish a fictional locality, for the taxonomist wants fact, not fiction, and especially not fiction established arbitrarily by those who know nothing about the taxonomy of the group concerned. In either case, we are dealing with an imperfect type specimen, and if that imperfection is so great that taxonomists in the group concerned find it useless as a criterion for correctly applying a specific (or subspecific) name, they may seek relief by petitioning the Commission to cancel the type, and thus to pave the way for the establishment of a neotype which has preserved not only the necessary physical characters, but an accurate record of its place of capture, which will then be, without arbitrary action, the factual type locality.

A type locality is not, or is only rarely, essential to the interpretation of a species (subspecies). The fact to be filled in by taxonomists as rapidly as possible is the area and physical conditions within the area that the population inhabits in nature. The center of abundance is much more important than the incidental spot where the type was captured. But none of this concerns the nomenclaturist at all until (occasionally) the point is reached where the taxonomist can not determine his material until he knows where it came from. It follows that the usefulness of stating a type locality, if factually correct, is to serve as a check on the geographic differentiation of populations. If fictional and erroneous it would have to be disregarded by any taxonomist as soon as the fact became known.

Two instances will illustrate the absurd results that could and would arise if the Commission were empowered artificially to designate type localities *without cancelling the status of the type specimen* :

(1) Paragraph **26**, deals with type localities originally erroneously cited. If the actual locality is unknown, it is suggested that the Commission shall, under certain circumstances, designate whatever locality they consider to be most appropriate.

A species of *Campsomeris* was described as from China. The Oriental species of *Campsomeris* have been critically studied by a recent author who recognised the fact that no such species occurs there. That author listed it as a *species inquirenda*. Suppose that, on the contrary, wishing to clear his list, and thinking to have recognised the form in an African species, although he had no critical knowledge of the Ethiopian fauna, he had recommended to the Commission that the type locality be designated "Africa." There would at that time have been no taxonomist who knew anything to the contrary, and the Commission (had the proposed rule been in effect) would doubtless have acceded.

Actually the type is a specimen of a common South American species, a fact that no one but the writer happens to know, and that by reason of having studied the type and compared it in the South American material. Would he be keeping faith with truth and science if he were to accept the arbitrary ruling that the type locality (and therefore the holotype) were African? In this

instance there is no need to establish the type locality. When the writer was able to identify the holotype with an actual living species, he was able to state the geographical distribution of that species, and where the holotype happened to have been caught was not even of academic interest.

(2) In paragraph **23**, suggested rule (7), it is proposed in brief that where the type locality has not been designated or indicated it shall be the first subsequently selected as such.

Let us assume a species "a" described from New York, Carolina, Jamaica. The holotype has no indication as to which place it came from. Three subspecies are known to exist, one inhabiting the Appalachian Mountains (hence including New York State and the mountains of North Carolina), one the coastal plain (hence including Long Island in New York) and the third the West Indies. A taxonomist, concluding that the holotype could have been any of the three, sets Jamaica as the type locality. Later taxonomists, with more precise discrimination, are able to definitely determine that the holotype belongs to the Carolinian coastal plain form. Then we have the type continental, the type locality insular, where the subspecies represented by the type does not occur. That is the sort of absurdity that may result from establishing two criteria that may conflict for application of a name.

From such considerations I cannot support the proposed rules, which appear to me a sort of shadow-boxing, a method of setting up taxonomic fictions to pose as fact, and which in the end do not concern nomenclature.

29 and 30. *Fossil Species : Parasitic Species.* The principles conform with the preceding and the same remarks apply.

34-37. *Official Lists. Nomina conservanda.* Perhaps some feeling exists that these are intended only for very widely applied names and not for the host of minor names known chiefly to specialists. A strong publicity drive might be very successful in enlarging the *List* rapidly.

In the Hymenoptera lists of all generic names with type-species have been issued for Chalcidogastera, Ichneumonidae, Chalcidoidea, Cynipoidea, Ants, Pompilidae and Bees. All establish the types believed by the authors to be correct under the *Règles* as then formulated. If they could be re-examined and changed wherever contrary to current interpretations of the *Règles*, and cases at variance with continuity brought to the attention of the Commission for decision, we could soon add the major block of generic names employed in this great order to the *List*. It would be an example for others. All the names in "Generic Names of British Insects," if incorporated into the *Official List* would themselves make an even greater block.

I have also long felt, that in cases where all generic names in a group are too numerous for any taxonomist or group of taxonomists to handle, that the important results desired could be largely attained by a study of all generic names up to about 1825 or some other period varying according to publication of some early basic work in the particular field involved. Here again a little publicity might instigate such investigations, and the mere fact that the results would be incorporated in the *Official List* would be a strong stimulus. People may say that it is extremely difficult to be sure one is right in a large list of old names. But correctness in every detail is far less important than a definite decision for the future, in conformation with usage where that has been clearly established.

Annexe to Statement furnished by Professor J. Chester Bradley**NOTES TOWARD AN INTRODUCTION TO THE INTERNATIONAL
CODE OF ZOOLOGICAL NOMENCLATURE**

These International Rules of Zoological Nomenclature are a set of laws designed to govern in an orderly manner the application of names to all of the categories involved in the classification of animals. They are enactments of the successive International Congresses of Zoology, but their authority lies only in the extent to which they interpret and express the will of zoologists in whose conscience their enforcement lies. The penalties for their fracture are not material but are expressed in the disservice done to zoology.

While based on principles, they recognise none as paramount to their fundamental aim, which is to provide the maximum stability and continuity in nomenclature compatible with freedom of taxonomic thought. They seek to provide *the* name which every zoologist, under whatever circumstances may be imposed by his personal taxonomic judgment, shall apply to any given kind or group of animals. They especially seek to provide that, under the same circumstances, that name shall be permanently the same.

They refrain from impinging upon taxonomic judgment, which must not be made subject to regulation nor restraint. Harmony with taxonomy, however the latter fluctuates, is secured by the device of types. Each name is conceived to be based on a type of inferior category, which for nomenclatorial purposes defines it objectively. Thus the name of a species is ultimately defined by the characters of an individual—its type, that of a genus by its type-species, that of a family by its type-genus. From the viewpoint of nomenclature species, genus or family consists each of its type plus all the other individuals, species or genera that any given taxonomist holds to belong to it. The limits of each are questions of taxonomy, ignored by nomenclature. Nomenclature accepts as objective synonyms only those units that are based on the same type; but it is prepared at the same time to accept or to reject subjectively as synonyms units based on other types, in the sense that it provides the proper name for the taxonomist to use, whichever course his taxonomic judgment prescribes.

Equally nomenclature does not determine the rank accorded to any group of organisms, but it does provide the name that shall be applied to whatsoever rank any taxonomist may wish to assign it.

From these considerations it follows that the complete binomial name of a species can be stabilised only for the type-species of each nominal genus, since the generic placement of all others is a matter of fluctuating taxonomic judgment.

Conceiving nomenclatorial rules as tools useful only to the point where they provide the maximum stability compatible with taxonomic freedom, certain measures have been adopted to prevent their becoming tyrannical, and actually destructive of their own usefulness.

The first of these provides that the rules may be suspended by an authorised body in any case where their operation would cause change and confusion.

The second provides that where practice has established a certain name under the International Rules as they existed in 1907 or later, that name shall not thereafter be changed to conform to subsequent revision of the Rules.

The third provides that long established usage shall, under certain safeguards, take precedence over priority in case where application of the latter principle would overthrow such usage.

The fourth provides that where an author discovers that a well-established name must suffer a serious change under the rules, he shall bring the matter to the attention of the International Commission on Zoological Nomenclature for a ruling before instituting such change.

(Note : Provisions 2 and 3 above are "wishful thinking." They are tentatively inserted with the conviction that such rules should exist, and the hopes that they will.—J. C. B.)

DOCUMENT 1/51

Statement of the views of the ENTOMOLOGISCHE GESELLSCHAFT, BASEL

Extract from a letter dated 30th July 1952 from M. HENRY BEURET

Sur ma demande vous avez eu la grande obligeance de m'envoyer les parties 1-8 dy vol. 7 du *Bulletin of Zoological Nomenclature* contenant divers problèmes de nomenclature ainsi que les déductions et propositions qui devraient être soumises au Congrès international qui aura lieu l'année prochaine à Copenhague.

Tout d'abord je tiens à vous remercier cordialement d'avoir bien voulu me transmettre ces publications et ensuite à vous féliciter chaleureusement pour l'immense travail que vous avez accompli en étudiant à fond les problèmes qui nous intéressent.

L'Entomologische Gesellschaft, Basel, qui s'intéresse vivement à toutes ces questions de nomenclature a étudié vos exposés et m'a chargé de répondre aux diverses questions que vous avez posées.

Je suis heureux de vous dire d'emblée que, dans les grandes lignes, nous voyons la résolution des divers problèmes sous le même angle que vous-mêmes. Comme nous avons un grand intérêt à ce que le Congrès de Copenhague fasse du bon travail, nous n'avons nullement l'intention de créer des difficultés mais nous voudrions plutôt *vous appuyer dans vos efforts*. C'est dans cet ordre d'idées que nous avons examiné vos propositions ; nous ne ferons donc que les remarques suivantes :—

Pages 152 et suivantes :

No. 1- 3 Rien à objecter.

No. 4- 7 Une " law of prescription " est dangereuse et nullement nécessaire pour arriver à une solution satisfaisante du problème soulevé sous No. 1-3 !

No. 8-15 Rien à objecter.

No.16-17 D'accord, mais le terme " General body of workers " devrait à notre avis être mieux précisé.

No. 18 D'accord. (" general body of workers " éventuellement à préciser).

No. 19-22 Rien à objecter.

No. 23

Rules a, b, c, d, e, f: D'accord.

Rule g: Si l'auteur de la description originale choisit ultérieurement lui-même une localité comme "type locality" parmi celles qu'il avait mentionnées dans la description originale, nous ne voyons rien à objecter:

si un autre auteur fait ce choix, n'y aurait-il pas lieu de l'obliger de choisir la première localité mentionnée dans la description originale? En effet, il nous semble que la localité citée en premier lieu a plus de poids, ce qui ressort d'ailleurs très souvent des descriptions originales, sans que leurs auteurs aient expressément mentionné qu'il s'agit là de la "type locality."

Doit-on admettre que l'auteur d'une forme ou une autre personne puissent ultérieurement choisir comme "type locality" une localité qui ne figure pas parmi celles mentionnées dans la description originale? (Le cas n'est pas exactement le même, lorsque la description originale ne mentionne aucune localité, ou seulement une indication très imprécise comme par exemple "dans les Alpes" !)

No. 24 Rien à ajouter.

No. 25 Why only a "Recommendation" and not a rule?

No. 26-28 Rien à objecter.

No. 31-37 Rien à objecter.

No. 39 (1) "Law of Prescription": **Non!**

(3) "stability by means of developments carried out within the existing Règles": **Oui!**

.

Dans vos propositions vous utilisez souvent les termes suivants "specialist" ou "general body of workers." Si ces expressions doivent être admises dans le texte officiel des *Règles*, il y aurait, nous semble-t-il, lieu de donner une définition précise de ces expressions. Qui est "specialist" ?

Lorsque les "specialists" ne seront pas d'accord sur un problème quelconque, est-ce l'opinion de la majorité qui décidera ? Il se peut fort bien que suivent la minorité ait raison.

En ce qui concerne les *noms douteux*, nous estimons qu'un tel nom cesse d'être douteux si par n'importe quelle méthode on arrive à reconnaître avec certitude la forme désignée ou décrite sous ce nom.

J'espère vivement que le grand travail que vous avez accompli soit suivi par des décisions qui marquent un grand pas vers l'assainissement complet de la nomenclature.

Extract from a letter, dated 13th August 1952, from Francis Hemming, the Secretary to the International Commission on Zoological Nomenclature, to Henry Beuret

The point raised on the last page of your letter in regard to the meaning of such phrases as "specialist" requires, I agree, careful consideration. In the context in which these phrases were used in my papers in volume 7 of the *Bulletin*, this expression was intended to mean that what was wanted in regard to whatever might be the subject under consideration was a statement of the views of specialists (=workers in, or students of) the group concerned; it was intended to exclude comments from zoologists whose work was not directly affected by the question at issue. I fully agree that, if any expression of this sort is used in the *Règles*, it must be so used that its meaning is entirely unambiguous. So far as the *Règles* are concerned, this problem of drafting arises chiefly in connection with such questions as the use of the plenary powers, the determination of *nomina dubia* and the like. Here it is the views of persons concerned with the group and the views of no one else which are directly relevant. On the question which you also raise as to the position when in any given group some specialists hold one view and others another, what I had in mind was that any interested specialist in the group concerned should be at liberty to put his views before the Commission and that the Commission, after taking into consideration any comments, either for or against the proposal in question, which might be elicited by the "advertisement" by the Commission of the proposal concerned, would itself decide what action ought to be taken. Reverting to the first of your two points, I think that it will be important to avoid using in the *Règles* any expression in this connection which might later be interpreted in too restrictive a sense. By this I mean that, while it is important to secure the general feeling of specialists in any given group, it is often important also to take account of the views of other interested workers, e.g. workers in the fields of applied biology who may be deeply concerned with the maintenance of a given name but who are not "specialists" in the particular group in question, so far as its systematics are concerned. Moreover, no expression should be used which implied that, for a problem arising in a given group, the only persons whose views were desired or who were authorised to raise a matter with the Commission were professional zoologists working on that group, for it often happens that independent zoologists (i.e. what in this country we call by the rather misleading title of "amateur") have an important contribution to make.

DOCUMENT 1/52

By W. E. CHINA, D.Sc.

(*British Museum (Natural History), London*)

Extract from a letter dated 20th August 1952

REF. Z.N.(S.)359 (STABILITY IN ZOOLOGICAL NOMENCLATURE)
ANSWERS TO QUESTIONS ON PAGES 187-188 (PARAGRAPH 39) IN
THE PAPER BY THE SECRETARY TO THE INTERNATIONAL COM-
MISSION ON ZOOLOGICAL NOMENCLATURE

1. I consider that a properly stated Law of Prescription would be of great help in stabilising nomenclature.
2. It should not be made retrospective otherwise all the changes in the 40 years which have now been more or less generally accepted will be invalidated and a further series of changes be necessary. It should become operative at some future date to be advertised. Ideally each case should be reported to the Commission and the old names discovered should form part of the Official List of invalid names.
3. At the same time developments within the existing framework of the *Règles* should be promoted and would form an additional safeguard for future work.
4. One of the factors bringing about instability in names which is not dealt with in your report is the uncertainty of the exact date of many publications e.g. Laporte's Easai Class Syst. Hemipt. 1832 or 1833. Research into dates often brings about a change of date which results in changes of generic and even of family names. I suggest that the Commission should publish a complete list of literature prior to 1900 giving (arbitrarily if necessary) official dates of publication and at the same time indicating those works which are officially regarded as invalid.
5. As an example of an entirely new scheme, one of the novel expedients mentioned in paragraph 3, I propose that which has long been suggested and discussed by workers at this Museum. It is the proposal that there should be an International Museum and that no species should be valid unless the type is deposited in this museum. The law of priority would hold only so far as type specimens were available. Species without types would be invalid. Until such time as the International Museum could be established, its place would be taken by the National Museums. Each country would maintain specialists at the International Museum who would gradually built up authoritatively named collections for their own National Museum. This scheme would have the advantage of close contact between specialists of various nations working on the same group, and the elimination of the majority of dubious species described in the old literature. If necessary, neotypes could be established in this International Institution for all those older specific names which it was desirable to maintain.

**Extract from a letter, dated 19th September 1952, from
Dr. W. E. China to the Secretary to the International Com-
mission on Zoological Nomenclature**

I agree about the difficulty of enunciating a satisfactory Law of Prescription to cover all Classes and Orders. It would almost certainly be necessary to have a different starting point for each group, to date from the first real monograph of the group, this work to be agreed upon by the specialists in that group.

Another method would be to advance the starting point of Zoological Nomenclature from 1758 to say 1900, giving the workers operating at that date the authorship of all the pre-1900, names. This of course would create a precedent and would deal a death blow to the law of Priority since similar "purges" could be expected every few generations when the nomenclature again became chaotic.

Official List of publications :

I should prefer a list, to be compiled by someone with the assistance of specialists, of all the taxonomic works, say up to 1900 which are to be officially regarded as valid*. The generally accepted dates should be given unless already proved to be otherwise, and all dates on the list arbitrarily fixed. I would suggest allowing a few years in which changes could be made during which specialists should be encouraged to make application for the addition of titles and alteration of dates. After that no change in the list should be allowed on any account.

I can think of no one, other than yourself, with sufficient energy and enthusiasm to compile such a list.

International Museum : I agree that this will be impracticable for many generations but the same idea could be adopted using the leading National Museum. Private collectors would, naturally be against such a scheme, but what I am anxious to do by this method is to invalidate all the old species based on lost or doubtful types. This would result in a more rational and less unstable nomenclature. At the moment the types of very many species are lost or the location unknown and such species are really based on unofficial neotypes in such institutions as the British Museum and other National Museums where identifications have been continually circulated for many years. The real types are the "neotypes" not the specimens actually seen by the original authors. My scheme would merely legalise the present set-up.

*This list could be based on Sherborn up to 1850.

DOCUMENT 1/53

By the
NOMENCLATURE COMMITTEE OF THE AMERICAN MUSEUM
OF NATURAL HISTORY, NEW YORK

Enclosure to a letter, dated 28th August 1952, from Dr. ERNST MAYR

We propose that in the ruling, based on the Monaco Resolution, which grants Plenary Powers to the Commission, the wording should be altered to provide that these Powers are to be used for the purpose of preventing confusion and of promoting uniformity and stability in zoological nomenclature.

Signed :

The Nomenclature Committee at
the American Museum of Natural
History, New York,

ERNST MAYR,
G. H. H. TATE,
JOHN T. ZIMMER,
C. H. CURRAN.

DOCUMENT 1/54

By K. H. L. KEY

(Commonwealth Scientific and Industrial Research Organisation, Canberra, Australia)

Extract from a letter dated 4th September 1952

I have studied your proposals in connection with the other six questions referred to you by the Thirteenth Congress,* and in general I find myself in hearty agreement with them. I do not like the definition of "subspecies" adopted by the Thirteenth Congress; I hope to be able to submit a short paper on this later on.

DOCUMENT 1/55

Statement furnished by the

AMERICAN SOCIETY OF PARASITOLOGISTS

Extract from a letter dated 8th September 1952

The American Society of Parasitologists at its meeting in November of 1951 appointed a committee to investigate the controversy that arose following the announcement that "far-reaching decisions in regard to zoological nomenclature (had been) taken by the Thirteenth International Congress."

In undertaking its assignment the *Bulletin of Zoological Nomenclature* was studied and the requests for advice in Volume 7 were noticed. The committee reached a unanimous decision on only three points.

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(3) The committee agrees that a preamble to the International Rules such as you suggest would be valuable.

In submitting these comments to you the Society wishes to express its gratitude to you and the entire International Commission for the considerable effort that has been expended on behalf of zoological nomenclature.

Signed for the Society by its Committee,

ALLEN McINTOSH,

D. H. WENRICH,

G. W. WHARTON, Chairman.

*i.e. the questions other than that relating to the emendation of names on which Dr. Key submitted a separate statement. This will be published later in the "Document 5" Series,

DOCUMENT 1/56

By JOSHUA L. BAILY, Jr.
(*San Diego, California, U.S.A.*)

Extract from a letter dated 8th January 1953

Stability and the Plenary Powers

As I have so often said to you, the most helpful step you can take in stabilising nomenclature is to suspend the rules whenever necessary to preserve a name universally understood, where the application of the rules would compel recognition of a name that has never been used.

DOCUMENT 1/57

Statement furnished by the WIENER ENTOMOLOGISCHE GESELLSCHAFT

Letter from Dr. HANS REISSER, dated 7th March 1953

Editorial Note: This letter is concerned mainly with an individual case (that of the name *Papilio adippe* Linnaeus), but is included here because in it the Society states its view on the general question of the use of the plenary powers.)

By our member, Mr. Schwingenschuss, we have got your inquiry from 20th September 1952, about the question of nomenclature of the species *Argynnis adippe* L. We beg to excuse the delay in answering.

Of course we agree with great pleasure that the name of *adippe* should be conserved. This case illustrates that the application of the strictest priority and exhumations of obsolete names only produces such a trouble that it becomes necessary to use the vulgar denominations instead of the scientific ones in order to signify the real species meant! Our Society would prefer the application of Heikertinger's "principle of continuity" and we should be very glad if it would become possible for a resolution of the authorities, treating with nomenclature problems, to introduce this principle into practice.

DOCUMENT 1/58

By CYRIL F. DOS PASSOS, LL.B.

(Research Associate, The American Museum of Natural History, New York)

Statement enclosed with a letter dated 2nd April 1953

Shall the " Règles " be amended so as to regulate the fixation of type localities and if so upon what terms and conditions?**Introduction**

This problem has been considered recently in a very able review of the subject by the Secretary of the Commission (1952, *Bull zool. Nomencl.* **7**: 172-180), as part of a general study on the means to be found for promoting the greatest possible stability in zoological nomenclature. Secretary Hemming concludes that instability in nomenclature at the species-name level arises chiefly out of the lack of provisions in the *Règles* for determining the locality to be accepted as the type locality of a given nominal species, and he suggests provisions for putting an end to instability for this cause in the names of subspecies due to inadequate information regarding type localities. Most of the Secretary's recommendations cover recent species, but he has also considered parasitic species and fossil species. Most suggestions, it is believed, will meet with the general approval of zoologists.

The fixation of type localities is not a new procedure in the zoological sciences. It has been the well-established practice of entomologists, mammalogists, ornithologists, and probably other zoologists for many years. My colleagues, Dr. George H. H. Tate, Curator, Department of Mammals, and Dr. John T. Zimmer, Curator, Department of Birds, both of the American Museum of Natural History, have kindly furnished me with many examples of this procedure in their respective branches of zoology. In mammalogy may be mentioned papers by Merriam (1901), Allen (1916), Kloss (1921), Sanborn (1930), and Hershkovitz (1947). In ornithology there is a lengthy paper by Zimmer (1951) in which many type localities are suggested and fixed. In entomology it is necessary to refer only to a paper by Comstock (1944), one of my colleagues in the Department of Insects and Spiders of that institution, and to one by the present author in collaboration with Grey (1947).

Some zoologists have objected that in a small percentage of cases the fixing of type localities is a nomenclatorial fiction. That may be true where no type locality is given in the original description, or where a type locality is erroneous. But those situations demand correction, even if the remedy is fictional. There are other fictions in nomenclature. The lectotype designated to replace a holotype that has been lost or destroyed may, in a small percentage of cases, be another species! But by selecting it stability is obtained, because the name is tied to an object. The same may be said of neotypes, if authorised by the coming Congress. Stability in nomenclature cannot be obtained until many type localities are fixed, and is so important that the possibility that some errors may occur, or a few fictions result, cannot be allowed to prevent its attainment.

It does not seem necessary to the present author to argue too strenuously for rules governing the fixation of type localities. The reasons for incorporating such rules in the Code has been presented fully and ably by the Secretary in paragraph 22 (1952, *Bull. zool. Nomencl.* **7** : 172-173). It seems necessary only to add that zoologists for many years have fixed type localities from sheer necessity in their systematic work when dealing with polytypic species, because without fixing the type locality of the nominotypical subspecies, how can any other subspecies be named without the risk that one of them will be a synonym of the first subspecies named? Which that one may be will be uncertain until the type locality of the oldest name has been fixed.

The problems before the International Congress of Zoology in 1953 in studying this subject are to determine first, whether provisions for the fixation of type localities shall be incorporated in the *Règles*, and if so, secondly, what these provisions shall be. This paper will consider the second problem, because the first seems to have been pretty well settled, and the main efforts should be directed to the codification of the present practice.

Preliminary Considerations

The phraseology used by the authors mentioned above in fixing type localities has not been uniform, and in a few cases could have been more definite. For instance, Comstock (1944 : 541-542) states "Therefore . . . is hereby fixed as the type locality of . . ." while Zimmer (1951 : 6, 16) has used a variety of expressions ranging from "For this reason I have selected it as the restricted type locality for . . ." to "I suggest . . ." to cite two examples only. Perhaps if rules covering this subject are adopted, it would be well to provide for a definite formula to be used in the future, with a somewhat milder formula for those type localities already selected.

Furthermore, different words have been employed from time to time by authors in fixing type localities, and it would be well to select one for the rules on this subject. Some of those appearing in the literature are "designate," "fix," "restrict," "select," "suggest," etc. In view of the fact that "designate" or "indicate" have been used in connection with a holotype (1950, *Bull. zool. Nomencl.* **4** : 186), "designate," "indicate," and "select" with the type species of a nominal genus (1950, *ibid.* **4** : 179), "designate" with the type specimen of a species (1950, *ibid.* **4** : 187-188), "select" with a lectotype (1950, *ibid.* **4** : 186), and that "designate" has been proposed for neotypes (1952, *ibid.* **7** : 137), maybe it would be well to choose one of the other words. In the proposed rules hereinafter set forth, "fix" has been used, because it is rather usual to employ that word in the United States of America, but the only matter of importance is uniformity. Any of the other words would be equally suitable.

Consequently, I proceed to the formulation of proposed rules. Three different situations arise which should be covered by the *Règles*. First, where no type locality is mentioned in the original description, secondly, an erroneous type locality, such as India for a species that never occurred there, but does occur in America, and, thirdly, where the type locality mentioned is so indefinite

that present day knowledge shows it to be insufficient for all practical purposes, i.e. America, Europe, etc. All other cases where it appears necessary or desirable to fix a type locality would appear to fall within one of these three main divisions.

Credit for most of the proposals must be given to the Secretary of the Commission, but in the concluding paragraphs of this paper some instances have been pointed out in which our agreement is not complete. Certain drafting changes and omissions have been made also in the Secretary's proposed rules which, however, are not believed to affect their meaning substantially.

Proposed Rules for the Fixation of Type Localities

DEFINITION.—The type locality of a species, subspecies, or infra-subspecific form (all hereinafter included in the word "species"), shall be that place where the species was taken or found in nature (unaffected by artificial transportation), unless for good and sufficient reason, and in accordance with the provisions of the following rules; (1) an author shall select subsequently a different type locality for the species, or (2) the Commission, upon application, shall otherwise direct.

RULES.—(a) Where no type locality is given in the original description, an author may fix a type locality where the species did occur at that time.*

(b) Where no type locality is given in the original description, but the author cites bibliographical references which mention one or more localities where the species occurs, an author may fix one of said localities as the type locality of the species.

(c) Where the type locality given in the original description is erroneous (being a place where the species did not occur at that time), an author may fix a type locality where the species did occur at that time.

(d) Where an author in the original description indicates a single type locality, that locality, if not erroneous, is to be accepted as such.

(e) Where an author indicates several type localities, the locality where the holotype was taken or found, or from which a lectotype is designated, shall be accepted as the type locality.

(f) Where the label attached to the holotype, or as the case may be to the lectotype, gives a more precise indication of the locality where the species was taken or found than that given in the original description, the more detailed locality so given is to be accepted as the type locality of the species concerned, when a subsequent author publishes the particulars thereof.

*It is a question whether a name proposed in an original description that does not contain a statement where the type was found should not be considered a *nomen nudum*, since a type locality is an essential ingredient of a good original description, but perhaps such cases are not very frequent or much worse even than those in which the type locality is entirely erroneous.

(g) Where an author finds that the fixation of the type locality as specified in Rules (a), (b), (c), (d), (e) and (f) is not sufficiently definite, he may fix the type locality more definitely by publishing such fixation with his reasons therefor, in which event the restricted type locality so fixed shall be accepted as such.

(h) The localities to be regarded as having been originally cited for a given nominal species shall be (i) the locality or localities cited in the original description of the species (including any more detailed locality ascertainable from the label attached to the holotype, or where no holotype was designated the syntype later selected to be the lectotype), or any restricted locality comprised within one of the localities aforesaid, and (ii) in default thereof the locality or localities indicated in any previously published work cited by the original author as applying to the species in question, but shall not include any locality only doubtfully cited by the original author, but preference shall be given to any localities given in earlier works by the author of the specific name in question when bibliographical references to such earlier works were cited in the original description of the species in question.

(i) When the original locality or one of the original localities cited by the author of the name of a given species is found to be incorrect, such other locality or localities indicated as being the locality or one of the localities in which the type material or part of it was obtained, is to be deemed to be such locality.

(j) When the name of a species is found to be invalid and a new name is proposed for it, the nominal species so established shall have the same type locality as the nominal species for which it has been established as a substitute.

(k) Where the type locality of a species has not been fixed under any of the preceding rules, the type locality of that species shall be whichever of the originally included localities is first definitely fixed as such at a later date, either by the original author or by any other author.

(l) When after a type locality has been fixed under the foregoing rules it is found that the restricted type locality so selected is not sufficiently precise, it shall be open to an author to restrict further the type locality of the species concerned by fixing some place or area comprised within the previously selected type locality to be the restricted type locality of the species in question, and if necessary a succession of restricted type localities may be fixed, each situated within the area comprised in the "type locality" last previously selected.

(m) In fixing the type locality of a parasitic species, the species to be accepted as the host species shall also be determined for nomenclatorial purposes.

(n) In fixing the type locality of a fossil species, the geological age of the rocks and the horizon in which the species was found shall also be determined for nomenclatorial purposes.

(o) If subsequent to the fixation of a type locality new evidence is discovered, either from the rediscovery of the holotype, syntype, or from manuscript notes showing the true type locality of the species in question, the matter shall be referred to the Commission, which shall have power to fix the type locality.

(p) A type locality fixation made prior to the adoption of these rules, if in substantial compliance therewith, and evidencing an intent on the part of the author to fix a type locality, shall be recognised as valid.

(q) After these rules take effect, the expression "fix a type locality" is to be strictly construed, and to exclude the mere mention on a later occasion of a locality as one in which the species occurs.

(r) The Commission, upon application, for good and sufficient reason, may cancel or alter the fixation of any type locality of a nominal species made by an author hereunder, or prior to the adoption of these rules, except where the original author cites one type locality only, unless that locality is erroneous, and fix a new type locality.

(s) Whenever the Commission is requested to fix a type locality, or otherwise act hereunder, it shall give public notice of the receipt of such application in like manner as that prescribed in cases involving the use of the plenary powers, and shall prescribe that a period of twelve months shall elapse from the date on which such public notice is given and the date on which the Commission may take its decision thereon.

(t) In the fixation of a type locality, the Commission shall select one which harmonises best with current nomenclatorial practice, except where the Commission on the advice of specialists is of the opinion that such practice is erroneous and that its perpetuation would lead to confusion, in which event the Commission may designate as the type locality of the nominal species in question whatever locality it may consider to be the most appropriate.

RECOMMENDATION.—Authors are urged in their original descriptions to fix the most precise type locality possible for a species, and to give at least the name of the country, state, or province, as well as the county or district, and the city, town, or village where the species was taken or found, and if the locality is remote to give the latitude and longitude. It is also advisable for authors to add the altitude and describe briefly the faunal zone. In the case of parasites, the name of the host, if known, should be given, and in describing fossil species the name of the geological formation should be added.

Conclusion

In preparing the foregoing rules an effort has been made to keep them as simple as possible, and to cover most foreseeable contingencies. Also, it has been assumed that the primary responsibility for fixing type localities should rest upon the author. Recourse to the Commission should be had only when authors find themselves in disagreement. However, at that point full and complete authority should be vested in the Commission to solve all the problems submitted to it as it may believe best. We now proceed to a few matters in which I do not find myself in complete accord with the Secretary's proposals.

I do not concur with the Secretary's Rule proposed in paragraph 23 (d) (type by [absolute or virtual] tautonymy) (1952, *Bull. zool. Nomencl.* **7**: 174), because such type localities as those given as illustrations, i.e., *zermattensis*, *adriaticus*, and *altaianus*, are too indefinite to be of much value, and sometimes similar type localities are false, or at least misleading, i.e., *britannicus* for an Irish insect (1950, *ibid.* **3**: 115). In one case the name *Argynnis atlantis canadensis* was proposed for a Newfoundland insect by the author of the present paper (1935: 85), not because the insect occurred only in that country, but because it occurred in that faunal zone. In another case the name *Parnassius phoebus manitobaensis* was proposed by Bryk and Eisner (1935: 55), and the type locality was given as "Manitoba, End-Mountains," but there are no "End-Mountains" in Manitoba, nor does the insect occur in that province of Canada. However, there are End Mountains in Alberta, where the insect does occur. These few illustrations show the danger of having anything to do with tautonymy in fixing type localities. Consequently, no rule has been drafted to cover such cases.

Neither do I agree with the Secretary's suggestion (1952, *Bull. zool. Nomencl.* **7**: 175) that a rule governing the subsequent fixation of a type locality shall provide "... that no locality can be validly selected as the type locality of a species, if it was not included among, or comprised within one of, the localities cited at the time when the species in question was originally described." Such a rule would prevent the proper fixing of some place in North America as the type locality of an insect erroneously described from India, where it never did occur, i.e., *Papilio genutia* Fabricius, 1793. However, the Secretary's proposal would be quite proper if it were qualified, as he may have intended, by substantially the following phrase "except when no type locality was cited in the original description, or that locality was erroneous."

It does not seem necessary, as proposed by the Secretary in paragraph 26 (1952, *Bull. zool. Nomencl.* **7**: 178) to provide for the case when one of the original localities cited by the author of the name of a given species is found to be incorrect, because the other correct locality would govern such a case. It is to be assumed that an author would not fix an incorrect type locality.

It is accordingly urged that the *Règles* be amended by the Fourteenth International Congress of Zoology at Copenhagen in 1953 so as to include a new article governing type localities. The suggestions herein made are offered as a basis for the discussion of that problem.

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