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Report on botanical nomenclature – Yokohama 1993

XV International Botanical Congress, Tokyo: Nomenclature Section, 23 to 27 August 1993

Herausgegeben von der Direktion des Botanischen Gartens und Botanischen Museums Berlin-Dahlem (Leitender Direktor: Werner Greuter) in Zusammenarbeit mit der International Association for Plant Taxonomy, Berlin.

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PREFACE

This is the official Report on the deliberations and decisions of the eight sessions held by the Nomenclature Section of the XV International Botanical Congress on five consecutive days immediately preceding the Congress proper. It is based, primarily, on the taped record of the proceedings, which, thanks to the modern equipment and skilled technical monitoring provided at venue, the Congress Center of Pacifico in Yokohama, was of outstanding quality and showed no gap. It is also based, whenever appropriate, on the written texts submitted by most speakers on the numbered sheets available to that effect, and on two parallel sets of handwritten notes, by the Rapporteur and the Chairman, on the sequence of speakers and on the decisions taken. With all these aids combined, we can confidently claim that the session records, as here transcribed, are accurate and complete, with all speakers correctly identified and the text of all motions and the voting results double-checked through written notes and the sound-track.

The Report was generated in a number of successive phases. Soon after the Congress, a first roughly pre-edited transcript of the tapes was produced. McNeill did this for the first and last Session, Barrie for Sessions 2 to 4, and Greuter for Sessions 5 to 7. The transcripts produced in North America were e-mailed to Berlin, where the whole lot was formatted, printed and duplicated in time for the Editorial Committee Meeting (January 3 to 7, 1994) at which the "Tokyo Code" was prepared. The further editorial refinement and the final editing of the Report was done by Greuter in Berlin, again using the original tapes and written documents, partly before and partly after the Editorial Committee Meeting, depending on the sequence in which the transcripts became available. All three authors of the Report had final or sub-final versions of it for proof-reading, and all have an equal share of responsibility for its accuracy. The laser-printed, camera-ready master copy from which this volume was produced was generated in Berlin with exactly the same equipment as its predecessor six years ago.
Needless to say, the spoken comments had to be condensed and partly reworded, sometimes drastically so. We did not therefore feel entitled to maintain a fiction of direct speech but (as for the previous Reports in Englera vol. 2, 1982, and 9, 1989) consistently used indirect speech instead. We tried nevertheless to avoid boring and sterile monotony but rather to provide a faithful and readable account of what was said, not obliterating the speakers’ wit and the originality of their comments. As Voss aptly stated in his preface to the "Sydney Report", it "is presented not merely as an official record of five days of deliberations and decisions. It is also a document in the history of nomenclature. The bare account of decisions taken cannot convey the spirit of the discussions, the arguments for and against certain actions, or the often extensive debate preceding selection of words to express a desired amendment."

The Section in Yokohama was relatively small. It consisted of 95 registered members (Appendix A) also representing 148 institutions and carrying 361 institutional votes as delegates (Appendix B), so that the total of possible votes was 456. It had to consider 321 published proposals (plus a small number of additional ones raised from the floor), which is almost as high a number as in Berlin (336 proposals), and significantly more than at the Sydney Congress (215). This was a hard chore for the Section to complete. It could be managed thanks to Burdet’s efficient chairmanship and to an improved routine for dealing with heavily defeated proposals and new motions, even leaving time for half-a-day's final housekeeping through carefully worded and thoroughly discussed resolutions.

The sequence in which the proposals were acted upon was basically that in which they were reviewed in the "Synopsis" (see Taxon 42: 191-271. 1993), which again was the same as the sequence of the provisions in the Code. For a number of reasons, however, there were several departures from this order. This Report would be hard to use, and indeed somewhat chaotic, if it were to reflect faithfully the chronology of the actual proceedings. We have therefore restored the "normal" sequence, explaining by means of bracketed notes such deviations as had occurred. This, as well as the index to speakers integrated in the list of Section Members (Appendix A), should ease the task of those looking for particular items within the Section’s debates.
The decisions taken by the Section of Nomenclature were ratified by the Congress at its final Plenary Session, on September 3rd, by the following sentence included in the first Congress Resolution: "The XV International Botanical Congress ... resolves that the decisions of its Nomenclature Section with respect to the International Code of Botanical Nomenclature, as well as the appointment of officers and members of the nomenclature committees, made by the Section during its meetings, 22-27 August, be accepted." (The full resolution is quoted in Taxon 42: 927. 1993.) The decisions themselves were recorded in print in November 1993 (in Taxon 42: 907-924) and their result will soon be incorporated in the newest edition of the Code (Regnum Veg. 131, in press).

This is not the place, and we may not be the right persons, for a detailed assessment of the merits and defects of what the Nomenclature Section decided in Yokohama. We may however venture to make what we believe is a safe prediction. The Tokyo Congress will not be remembered for its failure to adopt the proposals for stabilizing names in current use. It will not enter history as a reactionary nomenclatural event, but as the turning-point where the foundations of a new botanical nomenclature for the 21st Century have been laid (see also Taxon 42: 925-927. 1993). This Congress has introduced liberal conservation and rejection options; it has foreshadowed registration of names; and it has given its blessing to continued work on lists of names in current use. It has provided a challenge for those that aim at the stability and security of names and of the rules governing them; and it has set the scene for the next Congress in six years' time, to complete and round off what it has initiated.

In publishing this Report, we want to thank the Botanical Garden and Museum Berlin-Dahlem for having again accepted its inclusion in the serial Englera. Thanks are also due to the International Association for Plant Taxonomy for contributing to the printing costs, thereby enabling free distribution of copies to all registered Section Members.

Werner Greuter
John McNeill
Fred R. Barrie
Note: The figures given in parenthesis for each proposal, in this Report, correspond to the result of the preliminary Mail Vote (Yes : No : Editorial Committee : General Committee : Special Committee).
FIFTEENTH INTERNATIONAL BOTANICAL CONGRESS
TOKYO 1993

NOMENCLATURE SECTION

Bureau of Nomenclature

President: H. M. Burdet
Vice-Presidents: W. G. Chaloner, K. Faegri,
                K. Iwatsuki, D. H. Nicolson

Rapporteur-général: W. Greuter
Vice-Rapporteur: J. McNeill
Recorder: Z. Iwatsuki

FIRST SESSION

Monday, 23 August 1993, 10:05 – 12:25

Burdet opened the sessions of the Section of Nomenclature of the
XV International Botanical Congress by welcoming its members at
the Congress Center of Pacifico, Yokohama, Japan. He reported
that several regrets had been received, of which none had been so
moving to him as those expressed on behalf of Frans Stafleu, Rap-
porteur-général for many years and Chairman of the Section in
Berlin, who would not be able to attend the meeting in Tokyo due
to persistent ill-health. The Section conveyed to Stafleu its best
wishes and sincere thanks for all that he had done for nomenclature.

The Section would have to deal with 321 proposals, apart from the
usual Committee reports. This was twice as high a number as in
Leningrad (161), considerably higher than in Sydney (263), and just
about the same as in Berlin (336). As the number of hours at the
Section's disposal was the same as on previous occasions, all mem-
bers were requested to be concise in their remarks and to try to
concentrate on the main issues. The results of the preliminary mail ballot on the proposals, prepared by the Vice-Rapporteur and prefaced by his comments, had been made available to all those present.

Burdet proceeded by introducing the members of the Bureau of Nomenclature, including the four newly co-opted vice-presidents (see above). The Nomenclature Section would later on appoint the Nomenclature Committees that work between the Congresses. A Nominating Committee had been appointed, consisting of the members of the Bureau of Nomenclature plus Gams and Silva, to prepare a slate for approval by the Section. The Chairmen and Secretaries of the present committees were asked to provide the Nominating Committee, by Wednesday afternoon, with nominations of committee members to serve from 1993 until the next Congress in 1999. The same deadline of Wednesday afternoon applied for the submission of those Committee reports that had not yet been received.

The Berlin meeting had asked that the General Committee recommend new procedures for dealing with proposals heavily defeated in the mail vote and for introducing new motions from the floor. The Secretary of the General Committee would report on the outcome.

Nicolson added that it had also been suggested in Berlin that efforts be made to stem the tide of proposals. To this end, authors had been asked to limit each proposal to 375 words (half a printed page) and to follow a set of guidelines (see Taxon 38: 474-475. 1989); nonetheless there had again been over 320 proposals. The General Committee had been asked to consider the possibility of lowering the cut-off point for ruling proposals as defeated. By a tradition going back to the Paris Congress in 1954, a proposal receiving 75% or more "no" votes in the Preliminary Mail Ballot was not discussed, but declared by the Chair as having been defeated, unless someone moved its reconsideration from the floor. This percentage might be lowered to perhaps 66% or even 50%, but the General Committee's vote on such suggestions had been inconclusive. He therefore moved on his own that the Section maintain its traditional cut-off point of 75% "no" votes. This motion was carried.
Nicolson then considered the ease with which a proposal rejected under the 75% rule, or any new motion, could be brought up for discussion from the floor. In the past, parliamentary procedure had been followed, permitting any one person to present a motion which, if supported by a second person, must then be considered. He now moved that, whereas it would still be possible for a single Section member to present a motion, this would then require not one seconder, but four. In other words, five people would have to support a new motion, or a "deceased" proposal, for it to be discussed; or, barring this, that those in support carried at least 35 institutional votes.

Jeffrey asked whether this was also to apply in the case of amendments proposed from the floor to existing proposals.

Nicolson replied that the Chair, in the case of doubt, would rule whether an amendment was of a minor or merely editorial nature, in which case the restriction would be waived, or whether it had the same weight and substance as a new proposal, when the stringent rule would apply.

Demoulin, now at his fourth Congress, did not recall that there had ever been problems with proposals that had been brought up for discussion despite the negative mail vote. This had happened only for a minor portion of them, and the complex procedure that the Section was being asked to adopt might easily make lose more time than just sticking to tradition.

Burdet summarized the motion, which was not that complex: a new motion from the floor, or a request to discuss a proposal defeated by more that 75% of the votes in the mail ballot, must be seconded by four persons not just one, or else be supported by at least 35 institutional votes.

The motion was carried.

Burdet announced that the afternoon of Thursday would be devoted to meetings: a general meeting of the IAPT and meetings of the new Council of IAPT, of the General Committee, and of the Nominating Committee, to be followed by the IAPT dinner. He appointed Kato and Dorr to function as tellers.
Greuter made a number of procedural points. He urgently requested speakers to wait for the microphone and to announce themselves by name and city, so that the taped records of the meeting would include no gaps and no anonymous comments. All those who commented would be handed a numbered sheet of paper and were kindly requested to summarize in writing what they believed they had said. For those who were to edit the proceedings it was often helpful to know not only the spoken word as recorded on the tape, but the intended meaning; sometimes it had proved necessary to make a compromise between the two. Those who expected to speak frequently were encouraged to sit close to the front.

The voting would normally be by a show of hands, which would in most cases be conclusive so that the Chair could announce the result. If the situation was unclear, the Chair might ask for a show of cards and the tellers would make an approximate count of these. The third level was the card vote, for which one of the numbered tickets of the voting cards, to be announced by the Chair, would have to be placed in either of two boxes marked "yes" and "no". The tickets No. 6 and 9 would not be used in view of the difficulty of distinguishing between them. Members would be well advised to sign their voting cards so that they might hopefully recover them in case of loss.

The meetings were to start at 9 a.m. except on the first day; at about 12.30 p.m. there would be a lunch break, after which the sessions would resume at 2 p.m. and continue until 6 p.m. There would be a coffee or tea break each morning and afternoon.

There had been a tradition of long standing that a 60 % majority be required for a proposal to amend the Code to pass, but this was not automatically so and if no-one moved otherwise a simple 50 % would be all that was required.

Brummitt, recognizing that the 60 % requirement had always been applied in these sessions in the past and that it had worked very well, moved that a 60 % majority be required for acceptance of any proposal to amend the Code.

The motion was seconded and carried.
A motion by Greuter, that the Section recognize the "Berlin Code", as published, as the basis of its deliberations, was also seconded and carried.

Greuter moved that for the revised Code to arise out of this Congress, the Editorial Committee be empowered to change, if necessary, the wording of any Article or Recommendation and to avoid duplication, to add or remove Examples, to place Articles, Recommendations, and Chapters of the Code in the most convenient places, but to retain the present numbering in so far as possible, and in general to make any editorial modification not affecting the meaning of the provisions concerned. He added that at this point of its proceedings the Section used to have a good old friend [Cronquist], cruelly lacking this time, who stood up and asked that the interpretation of this empowerment by the Editorial Committee be such that the changes would be minimal and did not reflect the view of individual members of the Committee. This was, of course, again to be the understanding.

The motion was seconded and carried.

Burdet noted that at previous Congresses the Section had started the debates by one of the more weighty items on the agenda. This time, what was perhaps the weightiest issue of all, protection of Names in Current Use, was to come up right at the start, with the General Proposals. The Bureau had therefore seen no need to deviate from the sequence in which the proposals appeared in the Preliminary Mail Vote tabulation.

General Proposals

Prop. A (24 : 158 : 3 : 3 : 0) was ruled as rejected.


Greuter saw no objection to the proposed shortening of the title of Chapter I, which was basically an editorial point.

Prop. B was referred to the Editorial Committee.


Greuter explained that, in the mind of the Bureau, this proposal was to provide the opportunity for starting a fundamental discussion on
the whole issue of granting protection to Names in Current Use. This would not yet result in a decision, which was to be taken when Art. 15bis came up for debate. Having listened to this discussion, the members of the Section could thus talk to each other and make up their mind on what was clearly a very important matter, on which any rash decision one way or the other would be deleterious. He would have to speak at some length to introduce the topic, for which he apologized in advance.

The NCU proposals came from a Special Committee that had been set up by the General Committee at the request of IUBS, not, as was usual, upon instruction by the Nomenclature Section of the previous Congress. It consisted of nine nomenclaturally experienced plant taxonomists: Cronquist, Faegri, Hawksworth, McNeill, Nicolson, Raven, Stafleu, West, and himself. After thorough deliberation it had been unanimous in recommending the NCU principle, except that there had been one opponent [Cronquist, to no one's surprise] to extending it to the ranks below genus. Unanimous support by such a group certainly conferred some weight to the NCU proposals. What, then, was their purpose?

It had often been said that NCU protection would result in increased nomenclatural stability – and this had caused concern since stability of names, if taken literally, meant the freezing of taxonomy, something that no self-respecting taxonomist could support. The real purpose of the proposals was, however, to achieve nomenclatural security: to gain clarity on the names that were needed for use, and on the nomenclatural parameters of these names.

True, the Code already provided instruments for achieving stability, and to an extent security, of names through its conservation and rejection options. These were necessary and powerful provisions, but they had limitations beyond those of NCU protection. In particular, it was not possible to conserve a name against itself; in other words, the authorship and date of a name (and also its spelling unless the contrary was specified) were not conserved and were liable to change if new evidence came to light. The Editorial Committee, in consultation with the Permanent Committee concerned, had indeed repeatedly been forced to make such changes in App.
III of the *Code*. In the case of conserved family names of Spermato-
phyta, it had come to be known that the authors, dates and places of
publication for about one-quarter of them, well over 100 names,
would have to change unless a new scheme was found for dealing
with them. For rejected names, nothing could be taken for granted,
not even their cited type. Nor was it possible to conserve a name
against the unknown danger, since conservation worked only when
it was known that there was a concrete threat. Still there was no
question but that conservation and rejection provisions would con-
tinue to be needed even under the NCU concept.

Protecting names in current use, contrary to rejection in particular,
meant investing one's efforts in valuables, in those names one
wanted to use. There was little point to wasting efforts on unwanted,
unknown and potentially harmful names. It was for the names cur-
cently in use, those that one wanted to use, that it was worth while to
establish the authorship, date and place of valid publication, the
correct spelling, perhaps (for generic names only) the gender, and
hopefully, at least in a second round, the type. Once these attributes
had been established as reliably as possible – a labour that was
needed at any rate if one wrote, say, a monograph – they were to be
protected so that they could not any longer be changed. The present
situation was that every new worker on a group had to redo that
work, had again to check whether what his predecessors had done
was indeed correct. If previous results were confirmed, this was a
sheer loss of time. But perhaps there had been mistakes, or over-
sights, or intervening changes in the *Code* affecting the issue. If so,
the verification work might be less frustrating but the effect was
even worse: the new results were likely destabilizing. The idea of
NCU protection was to do the job once, and once it had been pro-
perly done and reflected what was needed, one would stop carping
and accept what had been found.

Names not on a list would be unprotected. They would remain
available for use but could not compete with listed names. Listed
names could compete among themselves. There was therefore no
need for the lists to reflect a single taxonomy. Names that some con-
sidered to be synonyms of older listed names, but others did not,
should be on the list; priority would then apply as before. Unlisted names would also continue to compete among themselves, of course. This would be a two-level system, with upper-class and lower-class names.

This was in a nutshell what the NCU proposals were about. They had not been developed by ivory-tower nomenclaturalists, as had sometimes been claimed, but by a bunch of working taxonomists with some nomenclatural knowledge; and they were to benefit, also the world at large to some extent, but basically the taxonomists themselves. In a time when taxonomic resources and resource allocation to taxonomy were limited – more than in the past but perhaps not yet as much as in the foreseeable future – it was important to concentrate them on real taxonomy. Nomenclature was not a science as taxonomy was, but a technique that was to serve science. It was just ridiculous, and shed discredit on the whole discipline, when research time and printed space were invested into changing the authorship and date, and in some cases the type, of slightly more than 300 familiar generic names – as had recently happened because some early but obsolete works (which had been purposely ignored by people like Dandy and Stafleu) had been unearthed. But there was no point in being emotional. The debate to follow might be heated, but would hopefully be without acrimony. Would discussants, doubtless all well informed and well intentioned, please respect the opinions of their opponents; would they try to substantiate their gut feelings with concrete arguments; and would they, please, be briefer than he had been!

Dorr had a comment and a question. First, Greuter had rather misrepresented Cronquist’s feelings on the NCU issue. Cronquist personally had expressed reservations about the NCU concept and had stated that, if in the Committee he had voted in favour of protection of generic names, it was because he had felt outnumbered and, conceding defeat, had voted with the majority. Second, how could the NCU system promote stability? The published NCU lists were replete with errors of scholarship, and the next two Congresses might well be spent arguing about mechanisms to correct these errors.
Greuter firmly challenged Dorr’s comment in view of his own personal knowledge of Cronquist and of the discussions they had had on the subject. Cronquist had definitely been in favour of the principle and convinced of the wisdom of it; and he had not been the kind of man who spoke one way to one person and a different way to another. As to Dorr’s question, it was not specific enough to be answered. First of all, while the lists had of course been made to the best of available knowledge, they had nevertheless to be carefully checked, and much work had yet to go into them. In particular, the just published list of generic names in current use, available for consultation in a number of copies in a neighbouring room, would certainly not be pushed through for adoption at this Congress even if, as he hoped, the NCU principle was to be approved: that list would thus be open for refinement and vetting for another six years. Secondly, what were errors? Factual errors, such as misspellings and the like, would of course have to be corrected (unless they reflected current use); it was certainly not misspellings that one would want to protect. If names were on the list that would upset other names in current use, or if types were found to be in conflict with the sense in which the corresponding names were being used, then these were errors that had to be corrected. On the other hand, if a listed name turned out to be illegitimate while being in general use, it might be a good thing that no one had noticed the fact before, and the name should stay on the list. All depended on the kind of errors one was thinking of.

Gams noted that according to previous discussions only names which, to the best of present knowledge, were validly published and legitimate were to go on an NCU list. This criterion should be made explicit in the rules dealing with NCU, so that names not complying with it would first have to be dealt with by the relevant Permanent Committee.

Greuter pointed out that the proposed provision would rule that NCU lists go through the Permanent Committee concerned before being presented for approval, so that this mechanism of control was already built in. Lists clearly had to be as reliable as was reasonably possible with respect to validity and legitimacy of the listed names.
Nomenclature in Yokohama

It was not the intent to list names that were not validly published, nor to list illegitimate names unless they deserved conservation; these criteria, while not explicit in the proposals, had been followed in setting up the published lists. However, the Code did not always give a clear and unambiguous answer to the question whether a name was validly published, or legitimate, in a given place. Much printed paper had in the past been wasted on this kind of futile arguments, which would become irrelevant in the future if the NCU proposals would pass.

Friis asked how current "current use" had to be so that a name would be included on an NCU list. In tropical Urticaceae, many names had hardly been used in the literature since Weddell's account in Candolle's *Prodromus*, yet they were useful and should be listed. The lists should not be restricted to names used in recent literature.

Ahti, as author of one of the NCU lists that extended to the species and varietal levels, had considerable experience with applying the NCU idea in practice. Most of the names he had listed were in full agreement with the provisions of the Code, but perhaps 8 or 10 were not, being either nomenclaturally superfluous or currently used in a sense not including their "legal" type. Many other, unlisted old names had never been used, but might be typified – perhaps neotypified – so as to upset currently used names. The NCU lists provided an easy way to overcome these problems. He had at first been very suspicious about the NCU idea, but during the course of his work he became fully convinced that it was excellent, especially in order to get rid of thousands of unused old names that were a potential threat to the established names of well-known, common, often economically important, mostly European plants and fungi. When recently compiling a list of names of plants cultivated in Finland, he had experienced vehement opposition from horticulturists to nomenclatural name changes (much less so to those reflecting a taxonomic change) that could be easily prevented after adoption of the NCU principle. Regretfully he had been unable to convince his own institute, of which he carried the votes, to support it.
Brummitt had been involved in this project almost since its beginning five years ago. It might be difficult for him to view the question objectively, but he would try hard. Some might remember that he had expressed his support of the principle at the two-day meeting on the subject organized by the International Mycological Institute, then at Kew, two-and-a-half years ago. Most botanists had come to regret the infuriating name changes that resulted from the application of the nomenclatural rules, especially the principle of priority of publication, when no complete index of plant names existed and people went on digging out names from the past. The rules on how to spell names did not give definitive answers either, such as were needed on such matters. For these and similar reasons, he had declared himself strongly in favour of the idea of Names in Current Use. However, already at the Kew meeting, as had been recorded in the published proceedings (Regnum Veg. 123: 217-223. 1991), he had expressed reservations as to how one was to proceed from approving the principle to putting it into practice. His reservations had since then increased, and he was still hesitant about which way he would vote, but he would put them forward so that the Section might come to a balanced judgement on these extremely important proposals.

At the family level, so far as the flowering plants were concerned, Hoogland & Reveal had done an excellent job; their list (in Regnum Veg. 126: 15-60. 1993) was much needed, they seemed to have dealt with all the problems, and he would have no hesitation in supporting the adoption of their list. However, granting them protection under the NCU option was not the only way in which one could adopt this list of family names. As Greuter himself had told him, if the NCU principle be rejected one could simply replace the present App. IIB with the Hoogland & Reveal list.

At the generic level there were different problems that had to be assessed. First the question of orthography, a constant thorn in the flesh and constant source of dispute, was probably the main argument now in favour of a list of generic names in current use, which would answer all the questions about spelling at one stroke. But, to be objective, his being very much in favour of such protection was conditioned by the fact that, with one notable exception, all his
favourite spellings had been adopted for the purpose of the published list. If however, as Greuter had promised, the list of generic names would be revised before the next Congress in six years time, what would happen to the 70 names for which he knew there was conflict of usage of spelling at the moment? Which spelling was to appear in the final list? There was no other answer than putting them all to a Committee vote – a formidable prospect. The second aspect of generic names concerned the bibliographic details. Again, it was annoying when these things kept being changed; but one had to be sure that the list was at least reasonably clean before it was "put into stone". Here he had grave reservations. He had worked with Greuter and knew the list well, which had been mainly Greuter's work, and an amazing amount of very meticulous work indeed. However, he did not think that one person could ever come up with a completely clean list. Greuter himself had held the opinion that no list should be protected unless it had 99 % accuracy – which was completely unattainable. Zijlstra might later give figures based on her experience with the *Index nominum genericorum*. He himself, while drafting a report on the work of the Committee for Spermatophyta over the past six years, had found that for 32 out of about 1000 presently conserved generic names of flowering plants changes to the listed entries had been approved that were sufficiently important for Committee action, and for 7 of them a formal proposal had been published. As there were 15,000 generic names on the NCU list, and if the same proportion applied, the Committee for Spermatophyta would have to adjudicate on 70 bibliographic changes each year, 450 over a six-year period, not to mention the inclusion of 8 proposals per issue of *Taxon* to correct entries. This would be a major problem. Third came the matter of types. From the very first meetings on NCU, he had vigorously opposed the protection of types of generic names simply downloaded from the *Index nominum genericorum* database. The type entries there were not all of a sufficient standard for wholesale acceptance. There were inappropriate typifications throughout *NG*, and the only way to resolve this was by Committee decisions. If all listed types were granted protection, the Committees would be inundated with work unpicking the mess. He had at first been alone to say so, but meanwhile many had realized that protecting types was just not possible. The fourth aspect
was that names on the list would take precedence over unlisted names. He had expressed doubts about this before. There was a danger of getting the wrong names on the list, so that again a conservation proposal would be needed to unscramble the mess. About half of the cases considered by the Committee for Spermatophyta over the last six years concerned the typification of generic names, a matter completely untouched by NCU lists which could in no way solve the problems of names having to be retypified, whereas only 19 cases of a name taking precedence over a taxonomic synonym had come up, not a big task for a Committee to handle. The present conservation mechanism for generic names solved many of the problems – and many that NCU did not resolve. The fifth point was the question of whether the operation of NCU protection was to be done once or whether the lists would be updated every five years, as had also been suggested. He had strongly objected from the beginning to later updates. The names in early literature that were likely to be dug out having once been omitted, no new names should be added to the list, certainly not through an arbitration system set up by, say, IAPT to judge whether a new name was good or not. If it were possible to add new names later on, there would be no way to stop people publishing new names to displace early but unlisted names under which all the required combinations already existed. In summary, at generic rank he felt that NCU might prove to be a sledge-hammer to crack a nut.

At the species level, the problems were absolutely horrific. No comparable operation to coping with a quarter million specific names of flowering plants had ever taken place. These problems could not be solved by bureaucratic committee decisions. At the species level in particular, taxonomy must precede nomenclature. The two lists of specific names so far published for the Spermatophyta, *Lemnaceae* and *Pinaceae*, had been circulated in the Committee for Spermatophyta, and the vote had been almost unanimously against them. At the species level the NCU principle would never get adequate support. However, once someone had done the taxonomy and sorted out the questions of types and bibliography, then the time would have come to take steps to ensure that things were not changed again. To be somewhat self-commercial, there were two ongoing
projects on a world scale seeking to cover all species of flowering plants: a quick world checklist, to be done in three years, which could not realistically be expected to create the foundations for nomenclatural stability; and what had been called the "Species Plantarum Project" that was just about to get off the ground and would, for those groups where the taxonomy had already been sorted out sufficiently well, put it into a single data-base and republish it as hard copy. The latter project was nomenclaturally significant. Family by family was to be worked up in full detail, so that the adopted names and their types could be protected and their synonymy established. Greuter had repeatedly stated that he liked elegant solutions to nomenclatural problems. The "Species Plantarum Project" was by far the most elegant such solution. It would yield a single publication and list of names. It would perhaps not solve all the problems (it might create some) but would be by far the best way of attacking the persistent problem of nomenclatural instability at the specific level. Later on the Section would decide on proposals to enable unlimited conservation and rejection of specific names – an option not seriously considered at the Kew meeting two-and-a-half years ago. Strong support should be given to these proposals, which introduced a completely new, much more efficient way of dealing with the problems of names in current use, doing so when they arose rather than trying to complete the task in one operation, possibly creating many more problems than were being solved.

Stearn asked how problems resulting from changes in taxonomic definition were going to be handled. For example, a taxonomic revision had led to the splitting off of Potentilla lineata, and that name was now to replace P. fulgens, a name in current use for a well-known ornamental species.

Greuter, while not familiar with that particular case, thought that in some situations a full NCU list of Potentilla would help, and in others conservation of a particular name would be more appropriate; there were, alas, a few situations where nothing could help, because different names (or the same name for different taxa) were wanted by different groups of people. In the latter case one should let the Code operate. But it should be possible to serve nomenclatural stability, whenever the case was clear, through either protection or conservation, or both.
Zijlstra spoke as a collaborator of the *Index nominum genericorum*. The *ING* database had been much improved since the books were published, and recently many more improvements had been made following comments on the NCU draft list. In order to know how accurate the list was before considering acceptance of the NCU principle, she had prepared some estimates, which were available on a duplicated sheet, of how much still needed to be done. In a number of families and family groups, about 15-30% of the entries still needed significant correction. Was the principle of NCU to be accepted by the Section when it was clear that the list itself would not be approved? Would not people who were not at the sessions be led to think that the list was fine when it still was, as she hoped, to be substantially improved in the future?

Jørgensen was very much for the NCU principle, because it would stop the counter-productive meddling around with names. The greatest problem was due to the retroactivity of the rules. A new nomenclatural compendium was required, obviating the need to dig in the old literature while using rules that none of its authors knew about. There certainly was a practical problem, however. This was in a way like writing a new *Species plantarum*. Linnaeus had needed about 15 years to produce his book – and he "became grey over it". Our problem in doing something similar now was that the number of species was very much larger. It was difficult to see how one could manage to produce a species list. More time was needed, starting with the generic and family lists, about the procedures for which he had made some complaints. He was against sanctioning these now. The principle was fine, but the lists would need a very long time.

Johnson drew attention to the fact that at this stage the Section was facing the proposal to set up a Committee on NCU. The arguments for using caution with respect to the NCU lists were fine, but, if this meeting were not to pass Prop. C, where would one be left?

Pitt, as co-author of one NCU list currently under discussion (in *Regnum Veg.* 128: 13-57. 1993), made a strong plea for nomenclaturalists to consider carefully the problems of those dealing with organisms that were important industrially and in every-day life.
They wanted continuity of names. Samson and he had produced a list of names for the family *Trichocomaceae*, in particular *Aspergillus* and *Penicillium*, since this had seemed to be by far the most effective way of getting over the problems of nomenclatural instability. They had taken great care to ensure that all other specialists working in the area had seen and critically evaluated the list. Having checked all the details of bibliography and typification, also with Greuter’s help, they had come up with what they considered as the best possible list, on which – except perhaps for a single controversial item – there was unanimous consensus. These fungi were of great importance medically, industrially, and in biodeterioration and biotechnology. Industrial people had simply no interest in nomenclatural deliberations such as were going on here. They needed stable names for these fungi. It was certainly impossible to draw up reliable lists of names in a single go for all plant species, and it might take three hundred years for nomenclaturalists to complete the whole task, but somewhere there had to be a start. Agreement should be reached now on the principle; the lists could then be acted on individually as they became available, and approved when they were ready.

Jeffrey fully agreed with Brummitt, not only because they both worked at Kew. However, when Brummitt had said that the problem could not be solved before the taxonomy had been done, he had meant, not taxonomy but nomenclature. Legislating on taxonomy was envisaged neither by the "Species Plantarum Project" nor by NCU. Every name had to be considered from a purely nomenclatural point of view, and if the limited nomenclatural manpower was to be used efficiently, it must concentrate on cases where it had been perceived there was a need. Conservation and rejection procedures, requiring Committee action, were the best and most unequivocal way to place names on an NCU list.

McNeill, answering to a suggestion by Ahti, confirmed that at this stage there would merely be a general discussion on NCU. There would be no vote taken on Gen. Prop. C until after the Art. 15bis proposals had been acted upon.

Hawksworth explained that there was widespread concern over how biological nomenclature now operated, and general awareness of the need to reduce changes in names made only for nomenclatural
reasons. This had been flagged in resolutions of the IUBS, and the
issue had thus been placed before zoologists, botanists and users of
other *Codes* alike. In zoology, phycology and mycology, the prob-
lems were much greater than for flowering plants. This broader
picture must be borne in mind, rather than focussing attention on a
small number of perhaps controversial issues. The establishment of
a Standing Committee, as proposed, was needed so that the dia-
logue could continue. Failing to do so now would be judged very
severely by the biological community at large, and might lead to
these matters being taken out of the Section's hands – which would
be most regrettable.

McNeill commented that Hawksworth's point, that a Committee be
set up regardless of the fate of the proposals on Art. 15bis, was
clearly a very sensible one, but the Committee envisaged under the
present proposal was to be assigned specific tasks regarding NCU
lists if covered by the *Code*. A committee as suggested by Hawks-
worth was a different thing, to be looked at in due course should the
need arise.

Jonsell sympathized in general terms with Brummitt's views. While
seeing the advantage of the principle, and perhaps prepared to vote
for the NCU idea in a general way but without approving the lists so
far produced, he wondered whether it would not be an awkward
situation if the *Code* was to include the general principle of NCU
when none of the lists were adopted. There would be an "empty"
paragraph in the *Code*, waiting for future implementation.

Greuter had been noting the points raised and would try to respond.
The last comment was most relevant and included the question of
what lists would be placed before the Section for approval if the
principle did pass. The NCU proposals stipulated, among other
things, that lists, before being approved and the names on them
protected, had to be recommended by the General Committee,
which in turn would rely on a recommendation from the Permanent
Committee concerned. This procedural rule would very much limit
the number of lists that could be approved at this Congress. There
was no recommendation by a Permanent Committee, so far, in
favour of any of the lists of species names, and so the General
Committee would not act on these: the Committee for Fungi and Lichens had voted for more discussion on the two fungal lists, which was entirely appropriate and showed that it took its mandate seriously; and Brummitt had previously informed the Section of the lack of support for the two species lists in the Committee for Spermatophyta. The generic NCU list had just been published so that it was not only inappropriate but technically not feasible that it be proposed for approval by the Section. This left the family name lists, for one of which there was indeed a positive opinion from the Committee for Spermatophyta. Brummitt had just said that he would like to see that list in the Code even if the NCU principle were not adopted. While this was possible, it would make a major difference. Under the present Art. 14, the dates and authorships, and perhaps the relative priority, of more than 100 names already on the list would change, whereas in their published list, Hoogland & Reveal had maintained the authorships and places of publication of the family names as now conserved, and these would be protected and thus stabilized by approval of the list. The other portions of the family name list were not ready for approval: in the Committee for Bryophyta the voting had not been completed, and the Committee for Fungi and Lichens had again voted for more discussion. The list of fungal family names certainly needed improvement, as witnessed by a distributed list of suggested additions to it. Apart from the Spermatophyta family names, one further list might be ready for adoption, and Jarvis might care to report if there was a vote by the Committee on Linnaean Typification on the published list of Linnean generic names.

Jarvis reported that the members of Subcommittee 3C of the Committee on Lectotypification had considered the published list (in Regnum Veg. 127, 1993) and had voted (5 : 2) in favour of the statement that the List of Linnaean generic names and their types, with specified corrections, would, if given some form of protected status in the future, afford nomenclatural stability to the names included'. The corrections referred to had been placed as an insert in the published list.

Greuter added that the Section could take a decision on this list on an ad hoc basis, because the Subcommittee on Typification of Linnaean Names was not a Permanent Committee but had been given
the mandate by the Nomenclature Section, six years ago, to produce exactly what it had achieved. At this point it was necessary to explain that the proposals on NCU, as they were worded, did not necessarily imply that names on an approved list would be protected against unlisted names (which was not called for, because not all Linnaean names were in current use and some should better remain synonyms): there were other options, one of which was that only the listed types be protected, which was exactly appropriate for the list of Linnaean generic names. Jarvis’s Subcommittee was to be complimented for the very painstaking way in which, in consultation with innumerable experts, it had drawn up its list of types of all Linnaean generic names, in most cases down to the specimen level, i.e. to the type of the species name that stood for the generic type. Those who had experienced past endless debates on Linnaean typification would realize what an enormous step forward protection of this list and consequent stabilization of the application of all these names would be.

What, then, would happen to the lists that were not yet mature for approval? Would they just leave an "empty" paragraph, as Jonsell feared? Certainly not. There was one important aspect of the NCU proposals that was easily overlooked. Not only did they give guidelines for the future, as to what could be protected and what steps were needed to that effect, they also foresaw that a list under consideration would have a sort of special status *ad interim*. A recommendation was incorporated, that so long as a list was under study, botanists were encouraged, and thereby authorized, to postpone application of the *Code* in individual cases if the consequences would be destabilizing. As long as a list had not been approved it could be easily refined, which answered Zijlstra’s concerns. The generic list was to be sent back to the Committee for improvement, and if in six years’ time the next Nomenclature Section had convinced itself that it was ready it could then be approved, but if again the feeling would prevail that the work had not been done up to a reasonable standard, the list could be sent back to the Committee once more. Pending further scrutiny, nomenclatural "terrorists" would be told to stay put.
Friis had raised the question of the definition of current use. What the Committee had understood by "current use" had been published. This was a flexible definition that admittedly did not answer every question in detail; also, the way in which it had been followed by various persons concerned with preparing the lists might not have been quite uniform. Names in Current Use were those that one wanted to use, or might want to use in a foreseeable taxonomic context. Any name that was thought useful by contemporary botanists should be on the list, unless another name competed for the same taxon and had a still better claim. There were a few cases in which a dual nomenclature was currently in use, and if so, a decision had to be taken, normally just by applying the Code. Sometimes, e.g. for the 70 cases of spelling conflict mentioned by Brummitt, a Committee decision was needed. This was not an additional chore that would become necessary by adopting NCU; since there was dual usage there would have to be a decision taken, regardless of NCU's being passed, but NCU offered a more elegant way to solve the problem. The supplementary labour for Committees that NCU would cause had been much overemphasized. The NCU principle not only required Permanent Committee expertise, it also added very significantly to the instruments of which the Committees disposed. Many of the present proposals would become unnecessary under the NCU option, or could be much more easily handled. The large majority of problems that would come up under the NCU option were those that existed anyhow, that the Committees would sooner or later have to address at any rate. If entries now placed on the list were later found to be disturbing, they were most probably of a nature that would make them equally disturbing under the present Code. Problems due to changes in taxonomic opinion occurred now just as they would occur in the future. Botanists would, for the best of reasons, continue to fuse genera of very unequal size where the smaller-sized genus had the older name. This was a problem neither caused nor to be solved by NCU, both names now being in current use. Only conservation could help, and conservation would at any rate remain essential in the future.

Brummitt feared that his earlier comment on the Committee for Spermatophyta vote on the family list might have been slightly ambiguous. The question that had been asked, at Greuter's request,
was whether the Committee members thought that the list was a good one, and the answer had been positive. It had not been asked whether the list ought to be approved under the NCU option. Indeed, some members of the Committee who were known to be strongly opposed to the concept of protecting names in current use had still voted for the family list. The general view on NCU that had come back from the Committee for Spermatophyta, for all three taxonomic ranks, was distinctly negative, although it was not possible to substantiate that statement by figures at generic level because no vote had been taken.

Burdet ruled that the formal vote on Prop. C would take place after the vote on the Art. 15bis proposals. [The following discussions and action did in fact take place on the following day, during the 4th session, when the other NCU proposals had failed.]

Hawksworth, in the light of the negative vote on the NCU principle, suggested an amendment to the wording of Gen. Prop. C, to read as follows: "The section to authorize the appointment of a Standing Committee on Lists of Names in Current Use, to initiate, assist, coordinate and vet the production of lists and of updatings of the existing lists of NCU, and to report to each subsequent International Botanical Congress." Greuter accepted this as a friendly amendment.

Funk asked for just a minute of respite to figure out what changes had been made and what exactly the proposal now meant, before taking a vote.

Greuter explained that the changes were mostly deletions. He read out once more the amended proposal, pointing out the main changes: the phrase "Names in Current Use" had replaced "Protected Names", "approved" had been changed to "existing", and the last nine words had been deleted.

Barrie wondered why a Standing Committee to deal with NCU lists was needed when there were no provisions for NCU in the Code.

Jørgensen would, unless it did confuse the issue, have another sentence added, perhaps: "While this work is going on, no changes resulting from the nomenclatural rules should be done on names." This was very radical, but reflected what Zijlstra had said during the
discussion. Changes of names should not go on while work con-
tinued on the lists. Creating the proposed Committee would be a
commitment to continue the work on the lists. They were very valu-
able, whatever their future status would be. While this happened,
one should stop muddling around with names, at least for the next
six years. Some better weapons were now available to avoid chan-
ges, and probably some more would be added, but there should not
be six more years of digging up names. The NCU principle would,
hopefully, eventually pass.

Greuter felt that Jørgensen’s was a completely different, additional
proposal. It should be presented in writing, and if seconded, it could
be brought up later, after some possibility for others to consider it.

Hawksworth supported Jørgensen’s idea. It matched the conclusion
he had reached when writing the introductory chapter of the Stability of names volume on the meeting held at Kew (Regnum Veg. 123,
1991). One certainly should not take up any names that would com-
pete with those on the published lists. To do otherwise was gross
irresponsibility and would confirm his opinion of some who called
themselves taxonomists but really were nomenclaturalists, not scien-
tists.

Lack asked why the amendment included deletion of the last words,
"through the appropriate Permanent Committee and the General
Committee".

Greuter replied that as there was no provision for Names in Current
Use in the Code, lists could not be proposed for protection. The
mechanism for channeling lists through the appropriate Permanent
Committees and the General Committee would have to be inserted
again if, at a later stage, protection of lists of Names in Current Use
would be approved.

Demoulin pointed out that the earlier lists had been communicated
to the Permanent Committees. There was no reason not to leave the
last nine words in.

Greuter wished to dispel a misconception. This was not a proposal
to amend the Code. It was a proposal to set up a Committee. Com-
mitees, while operating under the authority of the General Com-
mittee, had always had some leeway in interpreting and carrying out
their mandate. There would certainly be constant liaison between the proposed Committee and the Permanent Committees, but there was no a reason to formalize this by a statement that might be interpreted by some as giving too much weight to the new Committee.

**Brummitt**, like Barrie, was puzzled as to why a "Committee to coordinate and vet the production of further lists" was needed when NCU was not in the *Code*. This seemed to rest on the assumption that NCU was going to get through at the next Congress in six years' time – as maybe it would, and as many others would like it to happen. But was it really appropriate, again, to circulate lists of species, taking up botanists' time and promoting a whole bureaucracy?

**Faegri** thought that Brummitt had misquoted the amended proposal. The word "further" had been deleted.

**Greuter** did not think this made a difference. Additional lists were covered, because later on the updating of existing list was explicitly mentioned. He reread the amended text.

**McNeill** reminded the Section that the proposal was not an amendment to the *Code*. It would set up a Committee, for which only a simple majority was required.

**Prop. C**, as amended, was **accepted** by a card vote (54.9% in favour, 230:189).


**Silva** asked whether, by referring this proposal to the General Committee, that Committee would be given a clear mandate for action or would just be authorized to arrange or not to arrange for the preparation of a glossary. Was not the Editorial Committee the appropriate body to prepare such a glossary? Of course the Editorial Committee would not feel authorized to coin new definitions, but the definitions were already included in the *Code*. If there were discrepancies – as was indeed the case – the Editorial Committee had the authority to reconcile them. There also was at least one inanity: that one special case of a new name was called a *nomen novum*. Just think of a translation of the *Code* into Latin, when users would be asked to differentiate between "nomen novum" and
nomen novum. The Editorial Committee could, as a part of its mandate, substitute nomen substitutum for nomen novum. No week went by without his receiving a phone call asking to define some term used in the Code. A glossary was clearly needed.

Hawksworth drew attention to a resolution of the IUBS General Assembly in Amsterdam in 1991, asking that a general glossary for biological nomenclature be produced. He recommended that the General Committee should liaise with the IUBS initiative.

Greuter thought that what Silva had proposed would require a modification of Div. III of the Code, which specified that the task of the Editorial Committee was the preparation of the Code. As to-be chairman of the next Editorial Committee he strongly objected to that Committee’s mandate being widened. In his experience, the Editorial Committee was heading for a full week of very hard work, after which no-one would be able to bear the thought of nomenclature for at least two more weeks. Also, the Editorial Committee was hardly the right body to smooth away contradictions in the Code when the Section had not dealt with the matter. There were plenty of truly editorial points, but terminology questions tended to be so delicate and critical that delegating them to the Editorial Committee was clearly inappropriate. The reason why the Rapporteurs had suggested that the General Committee might accept such a mandate was that it represented the Section between Congresses. This was a matter for which the Section itself was responsible but which could not be handled during its meetings. If the task was felt to be worthwhile, the General Committee would doubtless be prepared to examine the best way of achieving it. Would the Secretary of the General Committee, Nicolson, care to comment?

Nicolson felt that a glossary would have to be prepared by a Special Committee, which could either be created by the Section or appointed by the General Committee to work under its authority.

Burdet asked Silva whether his proposal to refer the task to the Editorial Committee stood firm.

Silva wanted assurance that the General Committee, if charged with the matter, would not have the option to simply scrap the idea. Could the Section be asked to show, by a vote, its interest in having a glossary or not having a glossary?
McNeill drew attention to the Rapporteurs' comments, in the "Synopsis", that a "G. C." vote was to favour production of a glossary. If the Section favoured the idea (irrespective of whether it was to be carried out by a couple of persons, or a group of people, or perhaps in conjunction with the IUBS as suggested by Hawksworth) a vote for reference to the General Committee was appropriate, if not, the proposal should be defeated. Some might wish to abstain. A strong positive vote would give a clear indication to the General Committee that it had to take action toward producing a glossary.

Stearn asked for a clarification on the exact scope of the proposed glossary. There already existed a Glossary by McVaugh & al. (Regnum Veg. 56, 1968) that could be revised with little effort. Extending the scope beyond that would lead into an absolute morass.

McNeill felt that this was a question for the General Committee to assess.

Greuter, in order to clarify the situation, moved an amendment to the proposal, so that it would authorize the General Committee to prepare a new edition of the Annotated glossary by McVaugh & al.

Silva was opposed to the amendment, since it would mean that the glossary would not appear as a part of the Code but as a separate unit, perhaps in Regnum vegetabile.

Greuter's motion was seconded and carried.

Prop. D, thus amended, was accepted.

**Preamble 1**

Prop. A (64 : 70 : 45 : 4 : 0).

Hawksworth noted that there was still confusion over what groups were covered by the botanical Code. One still encountered mycologists who were amazed that fungi were included, particularly when molecular data now showed that they were closer to animals than to plants. To forestall a proliferation of Codes, of which at least one additional one was in preparation, an up-front clarification of coverage was needed.
Greuter drew attention to the linguistic problems noted by the Rapporteurs in their published comments. Might he take it that those voting in favour would authorize the Editorial Committee to take care of the matter? [He might.]

Prop. A was accepted.

Prop. B (21 : 153 : 6 : 4 : 0) was ruled as rejected.
SECOND SESSION

Monday, 23 August 1993, 14:00 – 18:15

Preamble 7


Greuter noted that in addition to the favourable mail vote, the proposal received a positive vote in the Committee for Fungi and Lichens: 10 yes, 2 no, and 2 undecided.

Prud'homme van Reine favoured the proposal, but recommended several changes in the wording. Firstly, "cyanobacteria or blue-green algae" should be changed to "cyanobacteria as blue-green algae". Although these were but different names for the same group of organisms, as cyanobacteria these organisms fell under the bacteriological Code; only as blue-green algae were they covered by the botanical Code. Secondly, "chromists" was a "non-word", not to be used, and "photosynthetic chromists" should be replaced by "photosynthetic protists and taxonomically related non-photosynthetic groups". For instance, Noctiluca, an organism with no colour at all, was part of the Dinophyceae and therefore covered by the Code.

Faegri [perhaps addressing Prop. B] warned against including footnote 2 into the Code because the term "fossil" was highly ambiguous. There were many so-called "sub-fossils", particularly in palynology where it was impossible to determine whether or not a deposited pollen grain was derived from an extant genus or species. The footnote should be left as weak as it stood, without a futile attempt at precision.

Demoulin had been instructed by his Belgian colleagues to support the principle, but to propose two (hopefully editorial) changes. Firstly, the term "cyanobacteria" should be removed, because only as blue-green algae was the group covered by the Code. Secondly, it should be made clear that the list was not exhaustive but only mentioned examples.
Hawksworth confirmed that the listed groups were intended as examples, which was why he had used the word "including". He had no problem changing "chromists" to "protists", unless that would bring in groups that caused problems. The cyanobacteria were a different case. As had been noted by several bacteriologists, the group could not be adequately dealt with under the current bacteriological Code. There had been a proposal put to the International Commission on Systematic Bacteriology that they permit the names to remain validly published and accepted under the botanical Code. Most people today called these organisms cyanobacteria and should have the option of using that name under the botanical Code. One advantage of calling them cyanobacteria was that one was more likely to be funded as a microbiologist than as a botanist.

Prud'homme van Reine pointed out that he had not proposed exclusion of the word "cyanobacteria", just making it clear that, when recognised under the botanical Code, the group was to be termed blue-green algae.

Greuter wished to have a clear record of what was being voted on. Did Hawksworth accept the suggestions as friendly amendments, that is, the replacement of "or" with "as", between "cyanobacteria" and "blue-green algae", and the replacement of "chromists" by "protists"? [He did not.]

Demoulin wanted at least to see the "blue-green algae" first; "cyanobacteria" could be retained parenthetically after them. Reversing the sequence was just a matter of principle, but people were sometimes very attached to such principles. The decision could be left to the Editorial Committee.

Jeffrey thought that with the proposed introduction of the word "traditionally", a term not now in Pre. 7, the listed examples of groups became superfluous and the whole parenthesis could therefore be deleted. This was put forward for discussion, not formally moved as an amendment.

Hawksworth replied that deleting the list would defeat the purpose of the proposal, which was to make it very clear, early in the Code, exactly which groups were covered by it, and which were not.

Jørgensen asked whether it had been agreed to change the word "chromists" to "protists". His understanding was that the original
wording was designed to exclude groups Prud'homme van Reine did not think should be excluded.

Hawksworth had only felt nervous, as a non-specialist, because changing the term might unintentionally broaden the coverage of the Code to include groups not currently covered.

McNeill noted that, if members of the Editorial Committee interested in this issue, having done their homework, felt that the term "protists" was correct, it would be in, but if it were thought to be dangerous, some other term would be used.

Prop. A was accepted.

Prop. B (3 : 171 : 4 : 3 : 2) was ruled as rejected.

Principle I

Prop. A (173 : 7 : 4 : 3 : 0) was accepted.

Article 2


Trehane objected to the proposed misuse of the word "fundamental". Ideas and theories could be fundamental, but concrete things such as taxa could not. The Editorial Committee might give some thought to that.

Greuter pointed out that proposals referred to the Editorial Committee were never considered to be set in stone. The Committee always felt free to incorporate them or not (when on afterthought the proposal turned out to be an unhappy suggestion). It was important to bear in mind that an "Ed. C." vote was not absolutely binding. If there were editorial changes which the Section was determined to see in the Code, then a "Yes" rather than "Ed. C." vote was appropriate.

Prud'homme van Reine, being uncertain whether fundamental, basal, or basic was the most suitable term, had referred to the largest Oxford English dictionary he could find – where they were treated as absolute synonyms.

Prop. A was referred to the Editorial Committee.
Article 3

Prop. A (53 : 38 : 91 : 0 : 0) was referred to the Editorial Committee.


Greuter had solicited an opinion from the Committee for Hybrids on this issue, as well as others concerning hybrid nomenclature. There had been no expression of opinion, however, nor any other sign of life from the Committee for Hybrids in the last six years.

McNeill added that the Rapporteur’s remark referred to the second component of the proposal. The first component was a reversal of sequence and would be treated by the Editorial Committee by analogy to Art. 3.1. If the proposal was referred to the Editorial Committee, it would likely choose to take no action on the second issue.

Prop. B was referred to the Editorial Committee.


Demoulin, by respect of tradition, spoke against the proposal, also in the name of the institutions he represented. While it was not as objectionable as earlier similar ones, it still would be a pity if the botanical Code were to be the first to introduce the term "phylum" when its use came from zoological textbooks. As long as the zoological Code did not use the term, it was not clear why the botanical Code should. Phylum was a term with several meanings, its usage often being synonymous with what some people called clades. It was a bad word for a taxonomic category.

Jeffrey confirmed that the term "phylum" did not occur in either the zoological or bacteriological Code. Why then should botanists have it in theirs? He had been one of the authors of a similar proposal, in 1978, precisely because of the reason mentioned by Demoulin, that the term was so enshrined in textbooks, not only zoological but also general biological ones. The botanical Code was the only code that set out to govern nomenclature at such a high rank.

Brummitt wondered for what sort of groups in the plant kingdom one was to use the term phylum. Were the bryophytes to be a phylum, or the different colours of the algae?
McNeill replied that if Brummitt had read the proposal he would know that "phylum" and "division" were being proposed as alternative terms. This proposal finally seemed to bring a solution to what had been an infuriating matter for users and authors of textbooks, and all those working in the real world of communicating with students. The word division was an antiquated term that the botanical Code used, that he had grown up with and learnt to like, but that was totally out of step with the rest of biology. Here there finally was a sensible proposal that worked for the benefit of general users but also covered those few instances where authors had used both "division" and "phylum" simultaneously (an aspect to be covered later, by Art. 16 Prop. A). It was time to stop insisting upon the use of "division" when everyone outside botany used "phylum".

Demoulin argued that the confusion expressed by Brummitt was an example of why phylum was a lousy term. People had used it for taxa of higher rank than division; for example, Feldman in the algae had used it for groupings of several divisions.

Prop. C first passed on a show of hands, then failed on a show of cards (55.6% in favour, 149: 119), and was finally accepted by a card vote (62.3% in favour, 218: 132).

Recommendation 3A

Prop. A (16 : 151 : 8 : 2 : 1) was ruled as rejected.

Article 4


Greuter explained that the "Ed. C." votes had obviously been cast in support of the Rapporteurs' editorial suggestions, made in the "Synopsis".

Jeffrey noted that Art. 4 already had "supplementary terms", "supplementary ranks", "subordinate ranks", and now "secondary ranks" were going to be added. Could all this be sorted out, please?

Taylor wondered about the implications for subtribe and subspecies under the proposal. Were they secondary ranks or not?

Silva explained that the problem with Art. 4, if one read it carefully, was its complete circularity. One could add supplementary terms
and then add "sub-" to the supplementary terms, ending up with "sub-" to the nth degree. His purpose had been to reduce the number of "sub-subs" that one could have.

Greuter viewed Prop. A in the context of Prop. B. Subspecies and subsections would thus be "modifications" of a primary and secondary rank, respectively. What Silva had attempted, and perhaps partly achieved, was to bring a coherent logic into the hierarchy of ranks – where hierarchy meant, not the taxonomic hierarchy but a classification of ranks by their importance. Referring the proposal to the Editorial Committee was preferable to adopting it straight away. The Editorial Committee would then evaluate these suggestions and incorporate into the Code those which were improvements.

Prop. A was referred to the Editorial Committee.


McNeill noted that the heavily negative mail vote was doubtless linked to the proposed introduction of the prefix "super-" for rank designations, something new in botanical language.

Greuter added that introducing mention of such ranks here but not in Chapter III was awkward. Would Silva agree to withdraw reference to "super-" and have the rest of the proposal referred to the Editorial Committee?

Silva agreed, having introduced "super-" only to reflect current usage. It had become standard practice for almost every paper treating higher taxa to use "super-" somewhere along the line.

Prop. B, thus amended, was referred to the Editorial Committee.

Prop. C (126:151:14:1:0) was ruled as rejected.

Article 5

Prop. A (10:166:5:1:0) was ruled as rejected.

Article 6

Prop. A (14:166:3:2:1) was ruled as rejected.

Prop. B (14:166:3:2:1) was ruled as rejected.
Prop. C (15 : 163 : 5 : 2 : 1) was ruled as rejected.

Prop. D (3 : 173 : 6 : 1 : 1) was ruled as rejected.


McNeill noted that the Rapporteurs had expressed doubt as to whether the suggested changes were indeed advantageous.

Henderson accepted the Rapporteurs' suggestion to replace "formally cited" with "appear in print". On their second point, he thought the fact that autonyms were not mentioned in Art. H.3 (as they should be) was no reason for not adding a reference to that provision in Art. 6.8.

Faegri thought the Rapporteurs' proposed wording was unfortunate. The Committee on Registration had attempted to find out what "in print" meant these days, and had found that it had no meaning.

Prop. E was referred to the Editorial Committee.

Prop. F (8 : 81 : 94 : 1 : 0) was withdrawn.


Greuter agreed that "description" and "diagnosis" had, for nomenclatural purposes, exactly the same effect. The proposed definition of description was, however, too wide. "A listing of its [a taxon's] characters" meant listing all its characters.

Silva accepted, as a friendly amendment, the Rapporteurs' suggestion to downgrade this and define a description of a taxon as "a statement of any of its supposed characters".

Brummitt preferred the Code as presently worded, with "description or diagnosis", as these words meant something. "Any of its supposed characters" brought to mind Fosberg's claim that a species name was validly published because it had a perennial sign after it, which was a "supposed character".

McNeill noted that this was a proposed new provision, providing a definition for the term "description".

Stearn upheld the conservative view of people born before the present generation, like himself, who were perfectly aware of the difference between a diagnosis and a description. The present
generation, not having read a book of his called *Botanical Latin*, might be unaware of that distinction. No harm whatever would be done by just keeping "description or diagnosis", without putting in these extra and possibly confusing definitions.

McNeill reiterated that nothing of substance would be altered in the *Code*. Whether or not the proposed addition was necessary was for the Section to determine.

**Prop. G was rejected.**

**Article 7**

**Prop. A (22 : 152 : 7 : 2 : 0)** was ruled as **rejected**.

**Prop. B (32 : 30 : 123 : 0 : 0)** was referred to the **Editorial Committee**.

**Prop. C (10 : 96 : 82 : 1 : 1)**.

Perry, consequent to the Rapporteurs' comments, wished to amend her proposal slightly and replace the word "element" (meaning specimen or illustration) with "specimen" in lines four and five, and with "specimen or illustration" in line six.

**Brummitt** was desperately trying to come to grips with the proposal. It took out the words "used by the author" from the present rule, leaving only "designated by the author." Was that one of the main points?

Perry explained that the whole idea was to shift the emphasis from the material seen by the validating author to the material used by the author of the validating description, as in more cases than was sometimes realized this was not the same person. Other changes made no real difference, just making it easier to decide what a holotype was.

**Brummitt** was concerned about taking out "used by the author". A holotype had not necessarily to be designated explicitly. For example, Jacquin had published a precursory work in 1760, in which he cited no material but was implicitly describing his own collections from the West Indies.

**Perry** felt that this situation was covered by the clause "specimen or illustration upon which the validating description of the name was based".
Greuter cautioned against accepting a proposal on an article as vital as Art. 7, on typification, when refinements were being sought in a debate on the floor between attending members. The risk that an unwanted change might sneak in was too high. How many of the 44 proposals concerning Art. 7 were clear cases for a yes or no answer, all of their implications having been understood? Relatively few of the proposals came from the pertinent Special Committee, most were from individuals. In the end, what had become a tradition by now might again be the answer: to establish a Special Committee on Lectotypification – it had been a long time since nomenclature had lived without one.

Johnson feared that the proposal as worded would allow authors to designate holotypes that they had never seen.

Perry pointed out that Art. 7.3 and Note 1, even now, did not imply that an author had to have seen the specimen he designated as the holotype.

Brummitt confirmed that many such cases were known.

Dorr was unclear about how Perry had changed her proposal, and requested that the amended text be read out – which was done.

Brummitt was happy with Perry’s explanation and now agreed with her proposal.

Greuter wondered whether he interpreted the proposal correctly, in that it would effect two things: make a distinction between material used by the validating author and that used by the author of the validating description, either of which could be the holotype if only one specimen was cited; and remove the phrase "used by the original author", so that there would no longer be a holotype when no specimen was cited, even though demonstrably only one element had been used.

McNeill explained that even when no specimen was cited the holotype would be the one specimen or illustration upon which the validating description was based, as in the Jacquin example of Brummitt.

Perry saw no difference between the present wording, "the one specimen or illustration used by the author" [in writing the description], and her proposed one: "that specimen or illustration upon which the validating description was based".
Jeffrey doubted that it was always clear whether an author had used a certain element, and whether the validating description was based on it. He knew of instances where it was clear which specimen had been used, but the description as published was so erroneous that no one would normally have concluded from it that it was based on that type.

Barrie questioned the author's own modification of clause (a). What if the name was based solely on an illustration?

Nicolson felt completely lost. The mail vote (and he himself) favoured referring the proposal to the Editorial Committee, which might or might not act on it.

Perry explained that the Editorial Committee vote favoured the Rapporteurs' suggestion of adopting the proposal without clause (a). She was not happy with that suggestion because clause (a) was an integral part of her text.

Greuter agreed that it was inappropriate for the Section to send the proposal to the Editorial Committee. Some of the proposed changes were not editorial, and the Editorial Committee would not take the responsibility of implementing them.

Stearn requested that the amended proposal be read out once more - which was done.

Demoulin asked why, in the revised wording, "element" was replaced by "specimen" in clause (a), but in clause (b) by "specimen or illustration".

Perry explained that the change was due to problems with the typification of Linnaean names. Linnaeus had often cited illustrations but not habitually specimens. It would be dangerous to rule that the single illustration he might have cited must be the holotype.

Prud'homme van Reine asked for a definition of "illustration". Could it be a series of photographs, for instance?

Taylor supported Perry's point concerning Linnaean names, which applied also to other names. One prolific author in Cactaceae, who was known not to have deposited type specimens because he did not believe in them, invariably cited (or published) illustrations. When forced to accept these illustrations as holotypes, one would have to accept all his names, now largely abandoned.
Stearn observed that the word "designated" implied a deliberate act. In Webb & Berthelot's *Histoire naturelle des îles Canaries*, a number of names had been first published on plates with analyses. The plates were not designated as types, the names just existed on them, but in fact typification by the plates was implied in many cases.

Johnson apologized for being linguistically pedantic, but "were" in lines 3 and 5 should be corrected to "was". If one could substitute "where" or "when" for "if", in English, then the verb was not in the subjunctive mode.

Friis asked about holotype status in cases when a single element was described or used by the validating author, but that material was later split up – new names by Hochstetter and Welwitsch being such examples.

Perry knew that splitting so-called holotypes after a description had been published had always been a problem, but this had not been addressed in her proposal.

Brummitt added that this point would come up later, in Prop. T-V.

Greuter reiterated his warning. It appeared to him that, for instance, a name validated solely by an illustration with analysis, when no material was known to exist, would no longer be holotyped under the present proposal, as the illustration would not qualify as a holotype. Clauses (a) and (b) would not apply, because there was no validating description. He might be wrong, but if so, then obviously Nicolson was not the only one in the room to be confused.

Faegri pointed out a further ambiguity. There were faithful illustrations, but many older ones were idealized pictures, for which a specimen had never existed. Should one accept them as a holotypes?

**Prop. C was rejected.**

**Prop. D (24 : 117 : 43 : 0 : 0) was withdrawn.**

**Prop. E (18 : 78 : 88 : 0 : 0) was withdrawn.**

**Prop. F (10 : 165 : 7 : 2 : 1) was ruled as rejected.**

Jeffrey, as its author, wished however to stress that his use of the word "protologue" had been intentional. It did not stand for "validating description" as the Rapporteurs had for some reason surmised. The proposed wording had been modelled on Art. 8.1.
Jeffrey thought the proposal to be just logical. Prop. W showed the same misconception as the Rapporteurs' comments on Prop. G. This was not a question of the Code "implying" anything, but of its recognizing reality. There was no way of knowing a priori whether an "individual taxonomic whim" was going to be a better or worse hypothesis than a "serious search for taxonomic consensus".

Brummitt supported the proposed wording, which was a distinct improvement, despite what the Rapporteurs had written.

Prop. G was rejected.

Prop. H (23 : 155 : 6 : 0 : 1) was ruled as rejected.

Prop. I (7 : 172 : 6 : 1 : 0) was ruled as rejected.


Greuter reported negative opinions of three Permanent Committees. The Committee for Pteridophyta vote was 0 : 8; The Committee for Fungi and Lichens vote, 4 : 6, 3 undecided; the Committee for Bryophyta vote, 0 : 4.

Prop. J was rejected.


Hawksworth had been invited to co-author the proposal but had not responded in time. He supported it, but would prefer the term "pragmatype", which had been used in the zoological literature (Disney in Nature 326: 251. 1987), to "protype".

Barrie, as one of the authors, explained that the proposal came primarily out of work with typification of Linnaean names, where the original material was often too inadequate to decide to which taxon it belonged. The "protype" concept would retain typification by original material as now required by the Code, but would fix the interpretation of the element chosen as the type. It seemed the most reasonable answer. His home institution at St. Louis was worried that people might designate "protypes" willy-nilly, thus destabilizing types which were not ambiguous and perhaps upsetting usage, problems which would be covered by Art. 8 Prop. T. He did not like their idea that the proposal be referred to a Special Committee for further study.
Greuter was convinced of the soundness of the concept, which was new but needed and was an improvement over what the Code presently permitted. Terminology was a different issue. The term "prototype" was inappropriate because "pro" meant "before" and what was intended came after and instead of the type. Semantics and terminology were however second in importance to the concept itself. Was the "pragmatype" suggestion a friendly amendment, accepted by the authors of the proposal?

Barrie supposed that while it was not as euphonious, it would be acceptable.

Greuter noted that the Rapporteurs had also suggested "metatype", which meant "coming after the type". Would the Section be willing to vote on the proposal with the understanding that the choice of the most appropriate term would be left to the Editorial Committee? [Barrie agreed.]

Brummitt had made the same proposal, except for the term, at the Seattle Congress in 1969, when he had disastrously used "neotype". The proposal was a very good one, covering many well established cases like Mimosa pudica and Butyrospermum paradoxum. He did not want to mix in a linguistic battle with Greuter, but would not "a type that comes before" be a "pretype", while "protype" meant "on behalf of the type", exactly what was intended?

Greuter thought that, thus defined, the word would be a linguistic hybrid, "pro" being Latin and "type" being Greek. He objected on principle to this kind of chimaeras when they could be avoided, although he used "television" at home sometimes.

Jeffrey had three questions. First, what was meant by "demonstrably ambiguous"? Should not "demonstrably" be replaced by "considered to be"? Secondly, what did " precise application" mean? To genus? To species? To infraspecific taxon? And, thirdly, what about uncertainty over the nominotypical subordinate taxon, when species identification was possible but not subordinate taxon identification?

Thulin asked what would happen if one person demonstrated that a type was ambiguous but another person later demonstrated that it was not ambiguous and could be identified. Would the "protype" then still take precedence over the primary type?
Barrie knew that such situations might arise when one discovered new defining characters for a taxon and the type specimen, previously considered ambiguous, could then be identified. Art. 8 Prop. T made it clear that the person first designating a "protype" must be followed. Once the application of a name had been fixed, even if later investigation proved that the primary type (holotype, lectotype or previously designated neotype) differed taxonomically from the "protype", the latter would stand, and for nomenclatural purposes the primary type would still be defined in the sense of the "protype". Changing the "protype" every time new characters were discovered would not do. Fixing the application of a name was the real goal.

Jørgensen supported the "protype" notion as a way of stabilizing nomenclature in cases when the type was ambiguous, of which he had seen too many. Even if other techniques and better taxonomists later came to a definite, different interpretation of the original material, it was worth maintaining the "protype" supporting it.

Ahti favoured the term "pragmatype" which had been used by zoologists. There was no reason for coining a different term. The choice should not be left to the Editorial Committee.

Johnson much preferred "metatype" because it was well formed. "Pragmo-" was not Greek, it should be "pragmato-". Just because zoologists were barbarous about language was no reason for botanists to be. He would prefer not to vote on the term, but let it be decided by the Editorial Committee.

Prop. K was accepted, subject to review of the term "protype" by the Editorial Committee.

Prop. L (126 : 41 : 15 : 2 : 0) was accepted.

Prop. M (148 : 13 : 24 : 2 : 0) was accepted.

Prop. N (108 : 15 : 61 : 0 : ) was accepted.

Prop. O (51 : 98 : 31 : 0 : 0) was withdrawn.


McNeill reiterated the Rapporteurs' comments. Although many people might have thought that the proposed definition matched the
definition of "syntype", it was not what the Code now said, and a change might be destabilizing.

Demoulin felt that it was important to have illustrations treated like specimens. He was in favour of the proposal.

Perry pointed out that the subject had been discussed by the Committee on Lectotypification, which considered that a change would be unwise.

Brummitt stressed that the important issue, in this proposal, was to allow for syntypes which were not cited. At the moment, there were no syntypes when no specimens were cited, so that in such a case one did not know what to do. He was in favour of the proposal, also of its first part, adding "illustrations".

Perry drew attention to Art. 7 Prop. Q, by the Committee on Lectotypification, which would remove illustrations from the definition of paratypes and thus bring it into line with the definition of syntypes.

Nicolson noted that the definition Fosberg was putting forward was the same as was in the Glossary of botanical nomenclature by McVaugh & al. It was with some surprise that he had found that the definition in the present Code was really quite different. He supported the proposal because it would bring back into the Code the definition that he and perhaps others had been using.

Jeffrey said that the difference between the wide Glossary definition and the narrow Code definition of "syntype" had been considered by the Committee on Lectotypification. If the present narrow definition were changed to the wide one, some present neotypes would likely have to be replaced by lectotypes such as illustrations, or specimens used but not cited by the author. The implications for nomenclatural stability were questionable.

Barrie pointed out that changing the definition of syntype would have implications for the illegitimacy of names under Art. 63.1, resulting from inclusion, in the named taxon, of "all syntypes ... of a name which ought to have been adopted...". If this was going to mean all of the material that had been seen by the author of the earlier name but was not cited by him, then many names now considered superfluous would no longer be so.
Demoulin felt that this was a major reason for accepting the proposal, especially since most of the proposals concerning Art. 63 had been rejected in the mail vote. Art. 63 was a terrible source of instability, especially in the fungi and lower plants, and anything that could limit its range of application would be welcome.

Lack favoured the first proposed insertion, "and illustrations", but not the second: "or known to have been studied". Could the discussion on these issues be split? This was agreed by the Chair.

Greuter cautioned once more against accepting seemingly innocuous and perhaps logical modifications when basic provisions were concerned and one was not fully aware of the consequences, which apparently no one had studied. Chance examples where the proposed change would be stabilizing or destabilizing might here be put forth, but no one knew how many lectotypifications would be affected by changing the definition of "syntype". Those who had taken the Code seriously and applied the definition of syntype as it now stood, like Jeffrey, would feel frustrated by a change, while those like Nicolson who had used the broad definition of syntype because they had not looked into the Code might feel elated. Art. 7.4 now mandated a choice between syntypes (i.e. cited specimens), if any existed, for purposes of lectotypification. Art. 9.3 gave clear precedence to specimens over illustrations when lectotypification was concerned. This rule had, in its roots, been in the Code as far back as Vienna. Although, e.g., macrofungal mycologists tended to disregard it and in some cases preferred illustrations, in many other cases the principle had been applied. Moving away from it now might bring about much more change than one might believe. Similar arguments applied to the second part of the proposal. If the proposal had real merits, which might well be, then the Section should decide to set up a Special Committee and refer the proposal to it, to report in six years' time on the exact implications of accepting it.

Prop. P, first clause, was rejected.

Demoulin (since Jørgensen was unwilling to explain the subtleties of the Omphalina case, under heated dispute within the Special Committee for Fungi and Lichens) referred but briefly to the typification
of a name in *Omphalina*. An illustration was disputed as type because it was not cited in the protologue. There was indirect evidence that Linnaeus had seen the illustration and some, like Jørgensen and himself, therefore believed that it was eligible. Others disagreed. Acceptance of the second part of Prop. P would settle the argument.

Nicolson asked Jarvis or Barrie to speak on the typification of Linnaean names in terms of material used (but not cited) by Linnaeus. Were such specimens neotypes, or could one call them lectotypes?

Jarvis replied that the concept of original material, defined in Art. 7.5, was relevant in this situation. It enabled one to regard illustrations cited by Linnaeus, as well as specimens that had in all probability been used by Linnaeus, as elements from which a lectotype could be designated. Many had originally believed that cited illustrations were syntypes, and up to the point when they knew better had so designated them.

Barrie observed that essentially the proposal was redefining syntype to be virtually synonymous with original material. Lectotypes were defined as elements selected from the original material, so that changing the definition of syntype would not expand the options to designate a lectotype. There was some utility to having a restricted definition of syntype.

Stearn, having been doing Linnaean typifications for nearly 50 years, had often found illustrations to be of more value than specimens, particularly with succulent plants.

Jeffrey underlined Barrie's earlier statement on the relevance of any change in the definition of syntype for illegitimacy under Art. 63.1, and agreed with the Rapporteur's warning in view of the dubious nomenclatural consequences of the proposal.

Prop. P, second clause, was also rejected.


Demoulin qualified the proposal as part of the conspiracy against the use of illustrations as type material. It had just been stressed how important illustrations were as type material, not only in higher fungi but also for the typification of Linnaean names. Illustrations must be eligible in all categories of types.
Jeffrey assured Demoulin that no conspiracy against illustrations was involved. The proposal was to bring the wording of Art. 7.8 logically into line with the definition of syntype. There was no intention or implication that illustrations should be excluded from among the original material.

Demoulin would have wanted Fosberg's proposal accepted. First illustrations had been excluded from the syntype definition, now they were to be removed from the paratype definition, and next one would remove them from the definition of original material.

Greuter noted that this proposal, for once, came from a committee, not an individual, and doubtless its implications had been considered carefully. The Committee vote had been 5:1 in favour – not bad for a Committee in which very different spirits were represented. Acceptance of the proposal was a logical consequence of the prior decision not to add illustrations under Art. 7.7.

Prop. Q was accepted.

Prop. R (3:172:4:0:1) was ruled as rejected.


Jeffrey objected to the last clause of the proposal. The illustrations, being part of the protologue, could not logically be something the protologue was based on.

Greuter noted that Prop. R and S were mirror images relating to the same problem. Prop. R having been defeated, the logical consequence was to accept Prop. S. If an illustration was published as part of the protologue, the proofs, and certainly the original drawing on which the illustration was based, must have been seen by the author when drafting the description. It made no sense to exclude the published illustration from the original material, thus having to go back to the proofs which had likely been thrown away, or to the original drawing which might also have disappeared. The published figure was part of what the author had had in mind.

Jeffrey knew cases in which the protologue was nothing but an illustration with a name underneath it. If the material upon which the figure was based was no longer in existence, did the protologue then become the type of itself?
McNeill replied that it became the type of the name. If Jeffrey's concern was with the wording, it could be taken care of by the Editorial Committee.

Prop. S was accepted.


Brummitt felt that the comments from the Rapporteurs had negatively influenced the mail vote. They had found the proposal commendable, but believed its effect would be just the contrary of the intent. He could not quite see what they had meant, but perhaps, to get around any objection, the text might better read "A lectotype is a single specimen or any one of a set of duplicate specimens..." It was surprising that after having applied the type method for so long, one still had no adequate nomenclature for the types themselves. Authors often designated a whole collection, say, Smith 1234, as the lectotype without specifying a herbarium. The proposal wanted to allow this to be effective lectotypification, whereas the Code now said that a lectotype was "a specimen" (singular). The Committee for Spermatophyta had recently voted that a collection could be a lectotype. Then, logically, provision for a second stage of lectotypification was needed, to choose one specimen out of the whole set. If the Rapporteurs could come up with a better wording, he would be grateful.

McNeill advised replacement of "one" by "any", not just add "any" – with which Brummitt declared he was happy.

Greuter thought it unfair to expect that the Rapporteurs should come up with a wording of their own. The problem was obvious, it was unsolved, and it would not be solved by accepting Prop. T. It would rather be exacerbated by clarifying what was now, perhaps purposely, loosely worded. Under the proposal, lectotypification would only be effected if the exact specimen was mentioned; citing the whole collection would not be effective lectotypification. A two-step lectotypification was now a normal procedure, when a whole collection was at first designated, and later one of its duplicates. One should not then be allowed to discard the first-step lectotypification as being ineffective. Since the mail vote included 22 unsolicited votes for sending the proposal to a Special Committee, he felt
encouraged to move that a Special Committee on Lectotypification be again set up, to look specifically into Prop. T-V, and perhaps also into proposals related to Art. 7 that had been rejected earlier.

Brummitt wondered whether the Rapporteurs were divided, because the Vice-Rapporteur had just come up with a wording that would answer the question. [McNeill: but only the one question, not necessarily the others.] In practice there was a problem. When an author wrote "Type: Smith 123" and as long as there was only one specimen known to exist, this was effective lectotypification. But if 10 years later someone found a duplicate in another herbarium, that discovery would immediately make the lectotypification inoperable. He was still in favour of the proposal as it stood, but if it was to be sent to a Special Committee, then so be it.

McNeill felt that Brummitt had not understood the Rapporteurs' comments. The proposed wording said "a single specimen or one of a set...", and so did the very reverse of what the authors intended. The present wording was sufficiently vague for the Committee for Spermatophyta to legitimately interpret it as it did, but if the proposal were to be passed, it would no longer be able to do so. His own alternative suggestion was not necessarily ideal, but highlighted the defects of the original proposal. The present position was sufficiently vague so that one could live with it (one might e.g. consider that the particular specimen seen by the designating author was ipso facto the lectotype).

Brummitt maintained that the best wording was "A lectotype is a single specimen or any of a set of duplicate specimens". The motion to set up a Special Committee on Lectotypification was seconded but defeated.

Brummitt read out again the revised text of the proposal. "Lectotype" was a singular noun, so it was not sensible to define it in terms of a plural concept. "Any of a set of duplicate specimens" meant [he thought] that if there were duplicates in the lectotype collection, they were all lectotypes.

McNeill noted that Brummitt was damning himself by his comments.
Friis had pointed out earlier that there were situations when a single collection, described by the validating author, was subsequently split up. There was a problem there.

Prop. T was rejected.


Brummitt explained that this was similar to Prop. T, but concerned a different sort of type. Prop. U and V were complementary. At the moment, duplicates of the same type collection were not isotypes but syntypes according to the Code. To most, however, "syntypes" implied different collections. A decision was needed.

Ahti explained that there were two kinds of syntypes. The second kind, duplicates of a single collection, was very common in cryptogam exsiccatata, published before 1953 as perhaps 50 identical sets, without an indication of type. There would then be 50 syntypes, and one of them must be designated as the lectotype. In his NCU list for Cladoniaceae, for instance, there were about 30 such cases.

Prop. U was rejected.

Prop. V (29: 102: 26: 0: 20) was withdrawn.


Dorr thought that vascular plants were one thing, but what about diatoms, for example, where it was impossible to ensure that there was a single taxon in one vial?

Greuter responded that this was exactly the corner from which the proposal came. Individuals of other species than that of which the name was to be typified, although on the type slide, must not be considered as type material.

Jeffrey reiterated an earlier point, made under Prop. G: what was "mixed" was a matter of opinion. A gathering could be considered mixed, but could not be proved to be mixed. As this was a matter of interpretation, which might differ, he did not support the proposal.

Barrie wondered if the wording would create a problem if collections initially assumed to be a single taxon were later determined to be mixed.

Greuter cited Art. 9.2 in response.
Nomenclature in Yokohama

Stearn had been shown slides of diatoms at the Natural History Museum, and confirmed that it was very unusual that they were not a mixture. There were usually little marks on the sides of the slide to indicate the individual diatom that was meant.

McNeill feared that the thrust of the proposal was being misunderstood, which was to define the word "duplicate" as "part of a single gathering of a single species or infraspecific taxon". This did not preclude the existence of mixed gatherings. The subsequent comment "However, the possibility of a mixed gathering must always be considered...", would remain.

Prop. W was accepted.

Prop. X (157:12:11:2:1) was accepted.

Prop. Y (87:60:26:0:6).

Zijlstra had written in her notes that this should be considered after Art. 9 Prop. A, but could not remember why.

McNeill replied that the Rapporteurs’ position was that the proposal could stand independently, although it certainly was linked to Art. 9 Prop. A.

Prop. Y was rejected.

Prop. Z (20:154:5:0:0) was ruled as rejected.

Prop. AA (78:80:18:0:0) was withdrawn.


McNeill noted that this was a proposal from the Committee on Lectotypification that the Rapporteurs believed was largely redundant.

Jeffrey replied that the Committee on Lectotypification had considered the proposed Note a useful clarification.

Greuter explained that Notes in the Code always stated the obvious, which was to say that the meaning of the Code would not change if they were deleted. As the proposal was offered as such a clarification, it could be referred to the Editorial Committee that would then decide whether or not the Note, in that place, was useful.

Prop. BB was referred to the Editorial Committee.
Prop. CC (6:170:5:0:0) was ruled as rejected.
Prop. DD (9:156:16:0:0) was ruled as rejected.
Prop. EE (9:143:30:0:0) was ruled as rejected.
Prop. FF (7:171:4:0:0) was ruled as rejected.
Prop. GG (7:141:30:0:1) was ruled as rejected.
Prop. HH (11:165:4:0:1) was ruled as rejected.
Prop. II (6:160:11:0:1) was ruled as rejected.
Prop. JJ (12:152:12:0:1) was ruled as rejected.
Prop. KK (64:48:66:0:0).

McNeill observed that this was one of a set of proposals from the Rapporteur, of which Art. 11 Prop. B was another. They involved a restructuring of the Code and were primarily editorial. The mail vote was fairly positive.

Greuter suggested the proposal to be referred to the Editorial Committee, which would take care that no change of substance would result. This was a cautionary measure to be taken in such a case, when no change was intended.

Prop. KK was referred to the Editorial Committee.

Prop. LL (7:168:3:1:1) was ruled as rejected.


Jeffrey had distributed sheets with written comments on this and several other proposals by the same Subcommittee. This proposal would remove the present anomaly under Art. 7.13, by which a name could be nomenclaturally superfluous, yet heterotypic with respect to the name which ought to have been adopted. It would thus resolve a case of conflict between Art. 7.13 and 63.1. The alternative solution, to make such names legitimate but incorrect when published under Art. 63.3, was offered by Art. 63 Prop. J when combined with Prop. MM, or by Zijlstra's Art. 63 Prop. K taken alone. This alternative might be more acceptable to workers in cryptogamic and fossil groups, who had tended to ignore the "superfluous nonsense" of Art. 63. Under Prop. MM some typifications of
superfluous and illegitimate names would change, and homonymy might be introduced where it did not exist before, but this was the cost of making the articles of the *Code* more harmonious.

**Prop. MM** was rejected.

**Prop. NN** (28:136:9:1:2) was withdrawn.

**Prop. OO** (0:100:77:0:0) was referred to the Editorial Committee.

**Prop. PP** (55:30:96:0:0) was referred to the Editorial Committee.

**Prop. QQ** (8:148:12:7:1) was ruled as rejected.

**Prop. RR** (6:76:95:0:0) was withdrawn.

**Recommendation 7B**

**Prop. A** (15:89:75:1:0) was withdrawn.

**Recommendation 7D**


**Barrie** took issue with the Rapporteurs' statement that Prop. A introduced a superfluous advice. One of the dangerous temptations of the "protype" concept was to designate "prototypes" when they were unnecessary. One way of limiting abuse was to recommend strongly that authors discuss their reasons for designating a "protype".

**Prud'homme van Reine**, while in favour of the "protype" idea, suggested the set-up of a Special Committee on "Prototypes". Much discussion on this principle was still needed.

**Greuter** stated that the Rapporteurs were generally not enthusiastic about adding new recommendations to the *Code* – often a sheer waste of printed space. When designating a "metatype", to most it would be clear that they must give some justification. People to whom this was not obvious would not do it anyway, and would not therefore be penalized. If it were made a rule that a "metatype" could be designated only if one did what was recommended here,
that would at least have some teeth. However, Prud’homme van Reine was probably right that a committee, even an informal one, should study the question first.

**Prop. A** was rejected.


Brummitt had found it impossible to propose this as an article as it was a battle one could not win. It had therefore been worded as a recommendation. In many cases when a single type collection was cited the author did not state where the holotype was based. The practical answer as a rule of thumb was to accept the herbarium of the institution where the author was known to have worked, as the place where the type was conserved. When a duplicate was later found elsewhere, this was not a holotype after all. In this case, the Rapporteurs and the mail vote were both supportive.

Burdet noted that the Rapporteurs had suggested fitting this into Rec. 7B. Would Brummitt agree? [He did.]

Henderson understood, from Art. 37.5, that the new recommendation would apply only to names published prior to 1 January 1990, because after that date the herbarium that held the type had to be specified for the name to be validly published.

McNeill confirmed this – but that still left a lot of names.

Brummitt added that names not validly published under Art. 37.5 were not names and did not have to be taken into account.

Greuter commented that in this case the advice to be given had not been generally followed and might not be obvious to all, so he had no objection to the proposal.

**Prop. B**, to be incorporated in Rec. 7B, was accepted.


Greuter reported that the Committee for Fungi and Lichens had voted 7:2, with 4 undecided, in favour of the proposal.

Kirkbride said that the U.S. National Fungus Staff opposed the proposal because its premise was wrong and it went against the idea that types should be kept in herbaria.

Prud’homme van Reine suggested that the proposal, like that on the "protype" concept, should be sent to a Special Committee.
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Hawksworth replied that the proposal addressed an issue that was quite different from the "protype" situation. Currently, mycologists working in groups where herbarium specimens were of little use blatantly ignored the Code on this point. DNA techniques might now be seen as the answer for certain fungi, but depending on the way the material was treated, one could not guarantee retrieval of DNA in a usable form from dry material more than ten years old. One had to legislate for what a large number of mycologists, particularly those working with yeasts, actually did, to end an argument that had been going on for many years.

Greuter asked for a vote of either "no", or "Editorial Committee" by those in favour. Should the related proposal, Art. 9 Prop. G, be defeated, the recommendation would make no sense and the Editorial Committee should be free to delete it.

Prop. C was referred to the Editorial Committee.

Article 8

Prop. A (23 : 146 : 6 : 1 : 1) was ruled as rejected.
Prop. B (32 : 141 : 3 : 1 : 3) was ruled as rejected.

Perry reported that the Committee on Lectotypification was of the opinion that lectotypification based on a largely mechanical method of selection should remain a reason for overriding a lectotypification, but that the example did not illustrate this. However, taking it out would make it very difficult to know whether or not American Code lectotypifications could be overridden.

Greuter explained the status of so-called voted examples which, confusingly for many, were not explicitly designated as such in the Code. Addition or removal of an example was normally at the discretion of the Editorial Committee, but for a "voted examples" it was not. they had a kind of legal force by rendering provisions in the Code applicable that would otherwise be unintelligible or ambiguous. Art. 8 Ex. 1 was one of them. It was the one example that illustrated the relevant rule, Art. 8.1(c) on the supersededability of American Code lectotypifications, which had been the subject of much
heated debate in the past. If the example were deleted, that rule would become meaningless and impossible to apply – which some might think a good thing. It would have been more logical to propose deletion of both Art. 8.1(c) and the example, since leaving meaningless stuff in the Code led to fruitless and endless debates. This, however, had not been proposed. The whole issue was an old-timer and all the arguments for and against had been told and published over and over again. The present proposal, just to delete the example, was the worst possible solution.

Jeffrey emphasized, like Perry, that the example did not illustrate the point it was supposed to and that to retain it would be misleading. Apart from that point, he agreed entirely with the Rapporteur's remarks.

McNeill, who had convened the first of the many Committees on Lectotypification, knew that the criterion of a "largely mechanical method of selection" did not apply to the vast majority of lectotypifications made under the American Code. Art. 8.1(c) had been approved at Seattle so that American Code lectotypifications, deemed annoying, could be superseded. Whether that was sensible or not was now irrelevant – in fact, it had likely been foolish, and it would have been much better to take a later starting date for lectotypifications, such as 1935, or simply use conservation. To tamper with this provision now, after 24 years and without yet having a list of names in current use, would be extremely destabilizing.

Prop. C was rejected.

Prop. D (10: 46: 121: 0: 1) was referred to the Editorial Committee.

Prop. E (27: 147: 7: 0: 2) was ruled as rejected.


Jeffrey, replying to the Rapporteurs' comments, explained that the proposal related to errors in citation of the name which was the subject of the act of typification.

Greuter still wondered what was meant by "errors in citation". Was it a misspelled name? or the mis-citation of the type? In the former
case, the proposal appeared trivial because no one had ever thought that misspelling a name would invalidate an act of typification.

**Prop. F** was rejected.


Brummitt, since this brought back the question of whether or not a lectotypification was effective when somebody cited a whole collection as lectotype, insisted on his earlier opinion that such a lectotype should then still stand. He was therefore opposed to the proposal.

Greuter held the same opinion. After Brummitt's proposals on the matter had been defeated, it would be extremely unwise to accept the present one, because it would render many presently accepted lectotypifications ineffective. The present wording was flexible, though vague, and should remain so until a clear and satisfactory solution to the problem had been devised.

**Prop. G** was rejected.

**Prop. H** (30 : 147 : 3 : 0 : 1) was ruled as rejected.

**Prop. I** (26 : 142 : 5 : 3 : 3) was ruled as rejected.


Brummitt and his Committee for Spermatophyta had been continually getting into a mess over what constituted lectotypification, and not all the many decisions they had taken recently had been consistent. It was very difficult to predict what the practical effects of Prop. J would be, but if the Linnaean typification group, having dealt with far more cases than anyone else, felt that the date 1953 was going to be advantageous rather than deleterious, he was willing to accept their advice.

Barrie had often been obliged to accept as effective lectotypifications designations of an element as "type" (without specifying "lectotype") when it was clear that the author intended "original material", meaning one of the elements that could be designated as the lectotype, perhaps with the implication that it might turn out to be the holotype. Sometimes an element selected in this way did not even belong to the taxon for which the name was used, thus disrupting usage. Art. 8.3, as now worded, essentially created *post-facto*
lectotypifications not intended by their authors. The suggested starting date coincided with the introduction into the Code of the specific terms for various type categories.

Jarvis added that the Berlin rewording of Art. 8.3, requiring use of "the term 'type' or an equivalent", had followed from a discussion of generic names, where it made absolutely no problem. But its application to species names was problematic. Prior to Berlin, when authors explicitly designated "lectotypes" in some cases and merely stated "type" in others where they did not want to formally designate a type, this was acceptable as a clear distinction between an explicit typification and the mere citation of original material. Now the situation was changed, as described by Barrie. A distinction between generic and specific names was desirable.

Ahti pointed out that authors, after 1953, had sometimes misused the word "holotype", and it had been assumed that such misuse could be corrected to "lectotype". The proposed wording might prevent that interpretation.

McNeill supported the proposal. Clearly the process of lectotypification was very different between generic names and species or infraspecific ones, so that the proposed distinction seemed logical. It was a firm rule in the Code that first lectotypifications, or neotypifications, be followed. It was therefore extremely important that lectotypification be conscious and deliberate, not fortuitous. His impression was that the implications of tightening up this rule would not be too destabilizing, and he was quite impressed by the great deal of experience of the proposers in working at the species level.

Jonsell strongly supported the proposal, based on his experience with the typification of Linnaean species names, particularly concerning the Nordic flora. Together with Jarvis, he had often found cases that would have been effectively solved by a paragraph like the one proposed here.

Greuter was well aware of the problem, a real one, that would have to be solved. Many who worked with names of species and lower ranking taxa resented having to accept lectotypifications made fortuitously in Floras and the like. In the Caryophyllaceae treatment of Davis's Flora of Turkey, care had been taken not to term Linnaean
specimens cited in support of the application of a name as types, so as not to rashly fix their application. But the *Flora of Libya*, by Jafri & al., copying this information on original specimens, used the term "type" and so effected lectotypification. This was clearly undesirable. However, was the proposed remedy really good? It came from workers who had much experience in the field of phanerogam botany, but in other groups the problems were often quite different. Even without going into the intricacies of group-specific nomenclatural traditions, there were problems with the proposal in a number of concrete cases. What would happen when someone had written, perfectly correctly, "type (designated here)" after 1953? It would not be an acceptable lectotypification because the term "lectotypus", or a modern equivalent, had not been used. What would happen when someone, under the old *Code*, cited those Jafri & al. typifications as "lectotype (Jafri & al., Fl. Libya ...)"? Would that be effective lectotypification or a misquotation? Such things happened frequently, and would continue to happen. They were the usual kind of semantic intricacies and whimsicalities that could not be taken care of fully by proposals that had not been thoroughly tested by case studies. He would not, a second time, propose a Special Committee to study the matter, but he would gladly second any such motion – while warning against accepting the proposal itself.

Hawksworth spoke strongly against the proposal, which might be quite straightforward for flowering plants, but as a mycologist made him feel very nervous. Elias Fries, one of the most prolific producers of names in mycology, had used the term "typus" – but not, it had been argued, in the normal sense of the word but just meaning "a typical example of".

Stearn, perhaps anticipating a later discussion, wished that a statement be included in the list of Linnaean generic names, that all of the typifications in it were provisional. While some of these typifications were very careful, others had come from authors he did not trust completely.

Brummitt had tried to resist the temptation to suggest a Special Committee, but yielded to it in view of Greuter’s comments and of the opposition to the proposal, and particularly of the problems
faced by the Committee for Spermatophyta, not with the new proposal but with the present Art. 8.3. His Committee was totally split on what was meant by "the term 'type' or an equivalent", and on the sorts of paraphrases of the word "type" which were acceptable. The Committee was, to put it bluntly, in a state of disarray about what to do, and desperately needed an answer, whichever this would be. He moved that a Special Committee on Lectotypification be set up, and his motion was seconded and carried.

Prop. J was referred to the Special Committee on Lectotypification.

Prop. K (68: 92: 5: 0: 16) was referred to the Special Committee on Lectotypification.

Prop. L (97: 71: 8: 0: 2).

Jeffrey commented that the previous Committee on Lectotypification had been proposing (a) guidance on how past acts of lectotypification should be interpreted, and (b) rulings as to how future acts of lectotypification were to be effected. While one could only give guidance for the past, one could legislate for the future. Within the confines of its time, the Committee had not considered everything it might have, and since a new Committee was going to be set up, this proposal might well be referred to it.

Demoulin objected to the introduction of yet another starting date into the Code, for a relatively unimportant matter. The proposal should be rejected, so that the Special Committee need not bother.

Faegri recalled that his own Special Committee had received a bunch of proposals from the Berlin Congress, some without recommendation, some with a positive recommendation. This form should again be used here, and this proposal should be recommended to the Committee which would then be obliged to take it seriously and integrate it into a comprehensive set of proposed amendments. He had heard so many proposals on typification that he felt completely confused and unwilling to vote for the proposal as such.

Greuter brought up a point which the Committee should bear in mind. Generic names, despite what was generally believed, had no lectotypes, let alone neotypes, as these terms were defined in the Code. Lectotypes were specimens, but so-called lectotypes of supra-
specific names were species names, standing for their ultimate type. The term lectotype, unless redefined, should not be applied at the generic level.

Prop. L was referred to the Special Committee on Lectotypification.


Funk complained that there had been several proposals in a row sent to a Committee, some of which she liked and others not, but there was no way of sensing how the group felt beforehand, except for the individuals who got up and spoke. Was there an informal, non-binding means of exploring how the Section felt about a proposal before it went to a Committee?

Greuter felt that what was useful to the Special Committee was to know the reasons why one was for or against a given proposal. Straw votes at this stage were not really helpful.

McNeill added that the proceedings of the meeting would be available to the members of the Special Committee.

Prop. M was referred to the Special Committee on Lectotypification.

Prop. N (57: 120: 4: 0: 1).

Zijlstra suggested that this, too, be referred to the Special Committee, although support for it was stronger than for prior similar proposals.

McNeill thought it logical to refer the proposal to the Committee, which would however note the negative mail ballot and the fact that residual typifications had been prohibited for the past six years. Any change now would be quite destabilizing.

Demoulin was very much in favour of the proposal (the fact that it included a new starting date notwithstanding) and had been instructed by Brussels to vote for it. Brussels felt that the decision of the Berlin Congress had been unfortunate and that the Special Committee should note that many regretted it.

Hawksworth urged a positive vote on the issue, which was very important. The new provision had not been in operation very long
and had not yet been implemented retroactively. For mycology, the current position was destabilizing and should be reversed.

Barrie argued that the proposal effectively gutted Art. 8.3. There probably had been very few residual typifications proposed after 1958. The vast majority were from the late 1700's and the 1800's. The proposal should be voted up or down, not referred to the Special Committee.

Jeffrey had been asked by someone who could not be present to point out that many typifications in bryology had been effected by a strictly formulated residue method, which would conform to this proposal.

Brummitt spoke very strongly against the proposal. Searching for a deliberate, published typification statement might be difficult, but at least it was a concrete item one could look for. The residue method required finding out who first circumscribed a name excluding this specimen and that specimen, and was completely unworkable.

Greuter noted the Section's preference for a "yes or no" vote, but added that even a defeated proposal could be taken into consideration by the Special Committee concerned. While not instructed to do so, as by explicit referral, it would still be entitled to look into the matter. Contradictory decisions taken at successive Congresses were most deleterious for the credibility of nomenclature. A similar proposal had been defeated by the Berlin Congress, and although Zijlstra had noticed a slightly increased mail vote support since then, reversing the previous decision now would not be easily understood.

Zijlstra had read in the introduction to the NCU generic name list that many of the type entries in ING likely needed revision. Indeed they would if the residue method were again rejected, but if it was accepted then many types that had always been agreed would stand. In a recent report of the Committee for Spermatophyta, a residue type had been accepted for Utricularia caerulea (see Taxon 38: 302. 1989), so the concept must still be alive in that Committee.

Prop. N was rejected.

Prop. O (149 : 24 : 8 : 0 : 0) was accepted.

Perry understood that the words "definitely accepted by the typifying author" in Art. 8.3 were meant to rule out ING typifications, though few seemed to recognize this. If the statement were taken at face value it would also mean that Hitchcock & Green's "standard species" would have no standing as types, because they were not "definitely accepted" but proposed for future acceptance. A clear indication was needed in the Code as to whether or not ING type entries could qualify as priorable lectotypifications. As to the Rapporteurs' comment about the card version of ING, it should be noted that many institutions had thrown the cards out.

Taylor was not sure the proposal's final sentence was correct. There was another clause in the ING introduction, which he did not have available, that tended to contradict the quoted statement and suggested that some lectotypifications in ING were intentional.

Perry added that this statement referred to lectotypifications made in the card edition, which were to be accepted. There was an appendix which gave the dates of such lectotypifications.

Greuter stated that the proposal was not an explanatory clarification, as suggested by the term "Note", but would introduce a change. Under the Code, ING itself did not contain effective lectotypifications if there was an explicit disclaimer. The cards did include lectotypifications. Some of these had been accepted, others had perhaps been ignored. As the exact consequences were in doubt he preferred to have the proposal referred to the Special Committee on Lectotypification for an examination of the pro and con.

Zijlstra noted that not all ING cards were the same in this respect. Some had an explanation, clearly indicating why a given type was chosen. In most cases, however, only the designation "type" was used, the authors not realizing that this might be understood as lectotypification.

Demoulin enquired about the distribution of the ING cards, which he had never seen.
Greuter replied that a fair number of sets had been distributed. They were printed cards, which demonstrated they had a certain spread. As far as he was aware, major botanical institutions all got their set.

McNeill added that there was no question as to the cards' effective publication, nor, equally, as to their effectiveness for typification purposes. There was a small number of cards where the word "prov." [provisional] appeared; these were obviously to be disregarded for lectotypification purposes. A rule writing off all lectotypifications in that particular medium might perhaps be beneficial, but the implications should first be considered. If some libraries or herbaria had chosen to destroy the cards, this was most shortsighted.

Demoulin had to admit that Brussels botanists were in favour of accepting the cards, probably because they had them. Personally, he found it paradoxical that the books, of which thousands of copies had been distributed, should not be acceptable, while the cards, which might be limited to tens of sets, were. An argument could be made for accepting ING typifications. But if this proposal was not accepted a contradiction would remain.

Prop. P was referred to the Special Committee on Lectotypification.

Prop. Q (21:57:100:0:1).

Greuter noted that, although the mail vote favoured sending the proposal to the Editorial Committee, it was linked to Prop. M, which had been referred to the Special Committee on Lectotypification.

Prop. Q was referred to the Special Committee on Lectotypification.


Jeffrey requested debate on Prop. R and S, feeling that the Rapporteurs' comments had been misleading and in view of the strong support for them in the Subcommittee on Retroactivity of Lectotypification; his request was seconded. A minor point was whether an act could be retroactive: of course it could, as anyone who had ever
received a retroactive salary increase would know. The Subcommittee had felt that the performance of nomenclature was impossible without retroactivity of typification. Unless typification was retroactive, many nomenclatural acts would be impossible to effect because one could not tell whether or not a given name, relative to other names, was homotypic or heterotypic. It was implicitly clear that the words "the type", whenever used in the Code, meant the element that served as the nomenclatural type. If typification were generally non-retroactive, Prin. II and Art. 7.1 would imply that up to the date of their typification names had no application and therefore remained inoperative in matters of nomenclature. Priority of names should not however be dependent on the date of their typification. There were examples that could clarify the implications of these statements for nomenclatural practice, dealing with superfluity (Art. 63.1), homonymy (Art. 64.1), orthographic variants (Art. 75.4), creation of a later homonym (Art. 48), and the general question of homotypy and heterotypy which all of these cases exemplified. Further examples touched on the question of valid publication (Art. 32.1, 42.1 and 75.1) and the typification of autonyms (Art. 22.2, 26.2, and implicitly Art. 11 Prop. A). They were all available on five pages of notes on the Subcommittee 3B proposals, of which 25 copies had been prepared. To give the Section time to read them, the Chair might agree to adjourn the meeting until the following morning. This was granted.
THIRD SESSION

Tuesday, 24 August 1993, 9:00 – 12:20

Article 8 (continued)

Prop. R (continued).

Jeffrey summarized the points illustrated by concrete examples in his handout "Notes on the proposals by Subcommittee 3B". His first example *Neohymenopogon* concerned superfluity under Art. 63.1, when a replacement name had been proposed for what was thought to be a later homonym but was in fact merely an orthographic variant of a yet earlier, legitimate name; it was the latter therefore that had been replaced, and under Art. 7.11 its avowed substitute had the same type as the replaced name. To find out whether the replacement name was illegitimate, one must discover whether the holotype, all syntypes or the previously designated lectotype of the earlier name had been definitely included under its replacement, which was seldom possible when the replaced name had neither a holotype nor a previously designated lectotype, and was inapplicable when it was only based on uncited original material. Unless a holotype was involved, the identification of a name as a later homonym, defined in Art. 64.1 as a name spelled exactly like a previously published name but based on a different type, depended on typification being retroactive to the date of publication of the original name. As for orthographic variant status (Art. 75.4), one had to make sure that the putative variants were really homotypic before declaring them to be such. The Code considered two names *Geaster, Geastrum* to have the same type although neither was formally lectotypified until 1984. In order to ascertain whether or not an author had published a new combination (Art. 48), one had to know the type of the putative basionym at the time of the author's action in order to decide whether she or he had included or excluded it. Unless and until a name had a type, it was impossible to know if it was correct or incorrect, as considered in Prin. II and Art. 7.1, 7.2,
51-53 and 69. In the absence of an operative type, there could be no application or misapplication of a name which would warrant rejection under Art. 69. All these examples addressed the question of whether names were homotypic or heterotypic. If acts of lectotypification and, by implication, neotypification, were not retroactive, how could one interpret a nomenclatural act which was predating typification? Valid publication (Art. 32.1) was affected by typification. The example *Senecio bodinieri* was of a species name with three varietal names. Until the names were lectotypified, and unless typification was considered retroactive, it was impossible to know which of the three varietal names had the same type as the species name and therefore would, by this act of typification, no longer be considered validly published. The establishment of autonyms was exemplified by *Fumaria bulbosa*, originally published with three varieties. Unless lectotypification was retroactive, there could be no autonym formation: Art. 26.2 would not apply as there would be no way of telling which subordinate taxon included the type of the name of the species. When there was original material in common between two names, the later of which fell under Art. 42.1, validity of publication of the latter might depend on an act of typification of the former, which had to be retroactive if the status of the name was to be at all determinable. The effect of typification on the nomenclatural status of names other than the one typified was unavoidable and was implicit in every nomenclatural act. In practice, the problem might be overcome by replacing Art. 8 Prop. R-S and Art. 11 Prop. A with a general principle: "The operative date of any effective act of typification is the date of valid publication of the name it typifies."

Zijlstra tried to illustrate the problem by an example on the board. When published, genus *A* consisted of two species, *Aa* and *Ab*. Later, *A* was discovered to be a later homonym, and the substitute name *B* was proposed. One of the original species was included (*Bb*) and designated as the lectotype, the other transferred to another genus. A further species, *Bc*, was added. Later still, another author proposed a substitute name, *C*, for genus *A*, and included the two original species. Of course *C* was superfluous as *B* was available. But was it illegitimate if one did not accept the retroactivity of
lectotypification? Art. 63.1 suggested that that the author would have had to include "the holotype or all syntypes" of the original name, but the author of B did not include species Aa, which he had removed. Should one say, therefore, that C was legitimate? Was it not simpler to say that it was illegitimate because it was proposed as a substitute name with the same type as A?

Greuter pointed out that the example was irrelevant: C was illegitimate because the genus included the designated type of the legitimate generic name B. Jeffrey's explanations, which he hoped everyone had well understood, demonstrated how complex the matter was. Although the wisdom and fairness of the Rapporteurs' comments had been questioned, they were quite to the point. As to the semantic issue, not really important, an act could not indeed be retroactive, although its effects could be. But should the effects of lectotypification really be retroactive? There were two aspects to that question. First, a name had a type, and was tied to that type: the Code said so. Philosophically, that type was there from the onset, even if it had not yet been designated, even if it were never to be designated. The second aspect, that mattered in practice, was whether or not the designation of a lectotype (or neotype) should be allowed to retroactively affect the nomenclatural stability of other names; whether by a later act of typification they should be bound to move from a status of validity or legitimacy to that of an invalid or illegitimate name. The effect of retroactivity of lectotypification, if granted, on the creation of homonyms was particularly relevant and had not been sufficiently discussed. Most would agree that the creation of artificial homonyms was a nuisance. In the old days very often a basionym, when a new combinations or a transfer in rank was made, was not quoted from its original place of publication but from a later one – e.g., not from the first but from the second edition of Linnaeus's Species plantarum, it being more up to date. What if someone chose as the type of a 1753 Linnaean name an element no longer included in the 1762-1763 concept? Granting retroactive effect of lectotypification, the 1762-1763 name would automatically become a later homonym of the earlier name. This would bear on the priority and perhaps on the application of the later combination which would then be based, not on the legitimate 1753 name but on
the later homonym. It was similarly undesirable to permit creation of illegitimacy post factum, or loss of the status of a validly published name under the autonym rule (e.g. Art. 26). These problems had prompted the Rapporteurs, and presumably most of those participating in the mail ballot, to vote against the proposal.

Jeffrey was familiar with the difficulty with Linnaean names, and mentioned the example of *Matricaria chamomilla*, controversial exactly for the reason Greuter had mentioned. As for altering the status of names, by lectotypification, from validly to invalidly published, this could be remedied by special legislation. This had been considered by his Subcommittee, and he had himself put forward two proposals (Art. 32 Prop. I and Art. 63 Prop. D) which, taken together, would declare validly published and legitimate a name that should have been an autonym provided it was heterotypic with respect to that autonym. Other proposals (Art. 22 Prop. A and Art. 26 Prop. A) would declare as validly published both heterotypic and homotypic names which would otherwise be rendered invalid by retroactivity of lectotypification. So there were two options for making legitimate either some or all such names. Under Art. 42.1 and 75.1, the possibility would still remain that a name be retroactively invalidated by lectotypification. One implicitly assumed retroactivity when thinking of types of names, and there was apparently no principle in the Code which stated that a name might not be changed in status by typification, even though it was probably desirable to minimize such effects. After all, typification inevitably could change the status of a name from correct to incorrect by changing its application.

Prop. R was rejected.

Prop. S (13:153:8:1:3) was withdrawn.


Thulin saw the advantages with the "protype" concept, but felt there were definite problems with it as proposed, because there was no mechanism by which a "protype" could be superseded. Once it had been designated, even if the original type later proved to be identifiable and might turn out to differ taxonomically, the "protype" would still stand. One could also easily misuse the concept
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and designate completely unnecessary "protypes". A mechanism to supersede "protypes", as there was for lectotypes and neotypes, was necessary.

McNeill drew attention to the previous adoption of Art. 7 Prop. K. In that debate it had been made clear, and agreed, that it was not desirable that there be provisions for superseding a "protype" except in the situations specified in the present proposal. Thulin's comment was therefore not any longer relevant, the point having been dealt with. Thulin was however free to raise additional material from the floor, once the Section had dealt with Prop. T.

Barrie explained the reason for not allowing "protypes" themselves to be superseded. Such a practice would perpetuate instability and ambiguity. If there was a conflict between the protologue and a lectotype (or neotype) supported by a "protype" (which would include the characters of the "protype" itself) then the lectotype could be superseded under Art. 8.1 (or 8.5). This implied changing the lectotype or the neotype, but would not permit maintaining the primary type and flipping through a series of "protypes". There had to be some kind of stability for the "protype" itself or else it would not have the desired stabilizing effect.

Demoulin supported the "protype" concept but would also support an amendment based on Thulin's comments. As long as a type was not interpretable there would be no reason to change the "protype". But this was probably never definitively so. In the case of Callithamnion (see Taxon 42: 521-530. 1993) it was necessary to count the cellular nuclei to correctly identify the taxa, but a method had been developed to count the nuclei on old herbarium specimens and reinterpret the types. Similar situations would occur elsewhere as new techniques were developed. Thus a "protype" might seem suitable by present standards but need to be rejected in the future when a new technique proved that it differed taxonomically from the primary type.

McNeill found that Demoulin's argument convincingly demonstrated that under no account should one ever permit to replace a "protype". When a new technology exposed a discrepancy between
the "protype" and the primary type, only maintenance of the "protype" would keep the name in its present application – and this was the purpose of a type, not to provide for historical accuracy. The decision of the previous day should in no way be reversed. Anyhow, an amendment as here foreshadowed would have to address Art. 7 Prop. K, not the present proposal that was mainly technical: it provided the necessary parallelism between "protype" and other kinds of types, indicating that a "protype" which stood for a lectotype was to be treated like any other lectotype, and one which stood for a holotype, like any other holotype. In the same way as a holotype, if lost or destroyed, could be replaced by a lectotype, a "protype", when lost or destroyed, could be replaced by a new "protype". This was a logical extension of the decision reached previously.

Thulin found it illogical that there was no mechanism for superseding "protypes" when it existed for lectotypes and neotypes. If stability was so desirable, why not prohibit the supersession of lectotypes and neotypes, too?

Jeffrey strongly supported McNeill’s view, which put the issue in a nutshell, but asked what would happen if a "protype" was supporting a holotype and some new technique showed that the two differed taxonomically. Was there any mechanism for conserving the "protype" over the holotype?

Barrie replied that a "protype", while not replacing the primary type, was a means of defining its identity. When a holotype was ambiguous at the time the "protype" was designated, future sophisticated methods might indeed uncover its true identity; but it was perhaps more likely to happen (and certainly a more serious threat) that someone should pretend: "I can recognize characters in the primary type no one else can, and just by looking at it can tell that it is this species rather than that, therefore the application of the name must change." This was exactly what one was trying to avoid.

Brummitt had much sympathy with what Thulin had said. Whether or not the original type could be "critically applied" or was "demonstrably ambiguous" were very subjective decisions. If new techniques permitted the identification of a primary type, it would be inappropriate to stick to the "protype"; one should then certainly go back to the primary type.
Demoulin felt that while it might be convenient to fix the application of a name and never change it again, this would completely negate the type concept, in which there was some historical justice included that could not be ignored. A procedure to overrule history in the interest of stability did exist: conservation (hopefully to be widened to the species rank). When necessary, conservation permitted to change a type, but this must remain exceptional. It was unaccept- able that "prototypes" should benefit of such extraordinary protection, much stronger than other kinds of types. He moved an amendment, to add at the end of the first sentence of Prop. T "... or if new evidence allows a different interpretation of the type". The motion was seconded.

Barrie thought several people were having problems with the "pro- type" concept. A "protype" did not replace the original type, it was merely a means of defining how that type was to be interpreted. If new evidence came to light which proved that the identity of the primary type differed taxonomically from that of the "protype", the consequence of changing the "protype" would be to destabilize the name by changing its application. Was this really what was wanted? Historical accuracy was one thing, but at some point one had to stop, say that this was the best one could do at the moment, and fix the application of the name permanently.

Greuter offered a procedural suggestion, provided Demoulin could agree: to postpone voting on the motion (then treated as a separate proposal) until after Art. 14 had been acted upon, when one would know the fate of proposals to broaden the range of conservation. This would also permit discussion and perhaps refinement of the present motion.

Demoulin insisted on having his motion voted on immediately.

Jörgensen asked for the meaning of "a different interpretation". Was this to be a matter of opinion, or should it be based on evidence from a new technique? This seemed very ambiguous.

Demoulin said that "new evidence" was required, not just a new opinion; this clearly meant new facts uncovered by a new technique.

Brummitt reiterated his belief that the unamended proposal was open to gross misuse. Anyone who did not like a holotype could
simply find the specimen ambiguous and choose a "protype" which
could not be reversed. The holotype would be discounted, which
was extremely dangerous.

Demoulin's motion was apparently carried on a show of hands, but
was eventually defeated by a card vote, after an initial miscount
(49.8 % in favour, 203 : 205).

McNeill advised the Section to bear in mind that the proposal just
brought the substance of Art. 7 Prop. K, approved the day before,
to line with the provisions for other categories of types. To vote
against it was somewhat illogical.

Demoulin saw nothing illogical in refusing to support Prop. T
having accepted Art. 7 Prop. K. Without Prop. T, if a holotype,
lectotype or neotype were no longer demonstrably ambiguous, the
"protype" would automatically lose its status as there was no longer
a reason for its existence. Prop. T, now the amendment had failed,
would confer to "protypes" the exceptional protection status that he
could not accept.

McNeill took Demoulin's point, but felt that other interpretations
were equally possible. If the proposal were defeated this would
leave a certain ambiguity.

Prop. T was rejected by a card vote (48.5 % in favour, 199 : 211),
then explicitly referred to the Special Committee on Lectotypification.

Article 9


McNeill noted that Prop. B was essentially an amendment to Prop.
A. Supporters of Prop. B might wish to support Prop. A in the first
place.

Brummitt recognized that for many the problem might never have
arisen, but to those working in a family like the palms where it might
take five herbarium sheets to make a specimen of even part of a
leaf, or the petaloid monocots where flowers and leaves might
emerge at different times, the present definition of a specimen pre-
sented a serious problem. The compilers of Index kewensis had
often found holotypes cited as two herbarium sheets, one sheet being inadequate to make a specimen. The proposal had been extensively discussed at Kew and outside, and had many signatories. As one of the proposers he introduced an improvement to the wording: on line 6, instead of "For a large plant", it was to read "For a plant too large to be accommodated on one herbarium sheet or equivalent preparation." This was more objective a statement.

Nicolson was concerned that the "little guys" were not accounted for, such as the Lemnaceae, with many plants on a single sheet. Had they been lost?

Faegri supported Nicolson's point, which had been mentioned on the previous day in regard to diatoms. The proposed wording had "a specimen", which in the case of Lemna minor meant any of a big bunch that had been pitched out of the water and put on a herbarium sheet. There was no mechanism for recognizing this properly. The proposed wording, and not it alone, was in itself contradictory.

McNeill pointed out that the Code in Art. 9.2 dealt with mixed gatherings of, say, Lemna.

Hawksworth confirmed that the proposal had obviously lost something from the old Art. 9.1, and in view of the suggestion about permanently preserved cultures that would come up later he suggested that the matter be referred to the Editorial Committee if it was supported in principle.

Greuter, while having considerable sympathy for the proposal, was reluctant to recommend that it be accepted. Obviously the wording was not ideal. An author's amendment had already proved necessary, and even that amendment might pose some questions: was a Kew-size or a Vienna-size herbarium sheet intended, or one from Leiden that might beat the record? A Special Committee to look into questions of specimens, collections, etc. had already been set up. This matter would fit nicely into its mandate, and if the proposal was referred to it, it might in six years' time come up with a well balanced, definitive solution also covering the problems of crypto-gams.

Brummitt pointed out that for some people this was an urgent problem. There was something to be said for referring it to a Special Committee, but not before having tried to vote it in straight away and without having to wait for six more years.
Stearn thought this to be a very sensible proposal when taken in conjunction with the examples. Anyone who had ever collected a palm (as he had) knew that they were so huge that you had to cut them into pieces even to put them on the British Museum herbarium sheets; it was like collecting an elephant, one could not preserve it in one bit. If necessary, the proposal could again be amended on a later occasion.

Demoulin was in favour of accepting the proposal, which had the unanimous support of Belgian botanists, rather than burdening a Committee with it. In a proposal of his own to the Sydney Congress he had placed more emphasis on cryptogamic practice, while the present wording emphasized phanerogamic problems, yet he could live with it as it stood, perhaps seeking a better balance through additional cryptogamic examples.

Prud\'homme van Reine reiterated his earlier question of what exactly an illustration was to mean; did a series of photographs of the same plant qualify? Somewhere, perhaps in a glossary, there should be a definition of "illustration".

Burdet ruled that a "yes" vote would mean acceptance the proposal in principle, with a request to the Editorial Committee to amend it in the light of the comments from the floor; whereas a "no" vote would refer the proposal to the Special Committee on Lectotypification.

Johnson, having obtained confirmation that Brummitt's amendment (he being a co-author) was now part of the proposal, was worried. What mattered was what had actually been done, not whether there was an objective reason for it, like specimen size. He much preferred the original wording.

Brummitt replied that one could not please everybody, but that he was ready to accept whatever people thought was the most practical. Had anyone else an opinion as to the appropriateness of his amendment?

Jeffrey preferred a practical approach, and suggested that the phrase to be amended should read: "For a specimen that is accommodated on more than one herbarium sheet, the type may consist of more than one herbarium sheet or equivalent". After all, one was
not dealing with future specimens but with ones that already existed, and it was not for the users of the *Code* to define whether a plant was small enough to be accommodated on a single herbarium sheet.

Faegri agreed to the idea of Brummitt's amendment but found the wording unfortunate. Strictly speaking, perhaps 50% of the Norwegian flora consisted of plants too large to be accommodated on an ordinary herbarium sheet. There was no need to go palm-hunting in the tropics, the same problem was posed by *Heracleum*.

Brummitt decided that he would stick with the amendment he had made.

Nicolson supported Jeffrey's amendment, and had meanwhile discovered that, in its third sentence, the proposal did cover the point of specimens consisting of more than one plant or pieces of one or more plants, thus taking care of the "small guys". He would therefore vote for referral to the Editorial Committee.

Briggs, feeling Jeffrey's suggestion to be very practical, wanted it to be incorporated.

Jeffrey was not prepared to move a formal amendment. He had indicated the thrust of such an amendment, but to find the exact words it in the haste of a meeting was inappropriate. He hoped that the Editorial Committee would take note of his remarks if the proposal was referred to it.

Zijlstra suggested that the word "plant" be replaced by "individual".

Greuter objected to the Editorial Committee being asked to decide between variants of a proposal that bore on its substance and meaning. This was matter for consideration by a Special Committee, which was the right body to deal with a proposal when it was not yet properly balanced and there was no agreement in the Section as to the most appropriate meaning and the best way to express it.

Jeffrey apologized for having said "Editorial Committee"; he had meant Special Committee.

Prop. A was rejected and thereby referred to the Special Committee on Lectotypification.
Prop. B (44 : 55 : 73 : 1 : 8) was referred to the Special Committee on Lectotypification.


Silva was disappointed at the negative mail vote. In the algae, as in other groups in which there were no hard preservable structures, the term "iconotype" had been used almost universally. At least 750 names of species or infraspecific taxa each year were provided with an iconotype. Unfortunately the term was used in two different senses, for an illustration of the type and for the type itself (as was permissible under Art. 9.3 of the Code in cases when it was impossible to preserve a specimen). As an aside, there was strictly speaking no case in which it was impossible to preserve a specimen, although doing so might be a nuisance, which was why workers in all these groups were using iconotypes. He had a backlog of several thousand names he could not process for his Index nominum algarum because of the ambiguous use of the term iconotype. There was no locality or collector to be cited when the picture itself was the type. Would the Section please provide a definition of the term "iconotype" in one way or the other.

Greuter thought that if this were not a technical term but the name of a plant it would be rejected under Article 69 as having been widely and persistently used in a sense excluding its type (perhaps a potential neotype). If the term were now fixed in one sense this would leave the ambiguity in past literature. The only wise solution would be to create a new, unambiguous term for each usage.

Prop. C was rejected.

Prop. D (18 : 135 : 29 : 0 : 0) was rejected.


McNeill noted the possibility, pointed out in the Rapporteurs' comments, to reject Prop. E and instead rephrase Rec.9B Prop. B. There had been considerable support for that position in the mail ballot.

Prop. E was rejected.

Prop. F (37 : 115 : 11 : 7 : 13) was rejected.
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Greuter noted support of the proposal by the Committee for Fungi and Lichens (7 : 3, 4 undecided).

Prop. G was accepted.

Recommendation 9A


Greuter reported the Committee for Fungi and Lichens’ favourable vote (11 : 0, 3 undecided).

Prop. A was accepted.

Recommendation 9B


Brummitt expressed preference, by him and others, for referring the proposal to a Special Committee, but whether this should be the Special Committee on Lectotypification or a Special Committee on the Definition of a Specimen was unclear. He favoured the latter option.

Greuter advised that since the Section, following tradition, had authorized the set-up of a Special Committee on Lectotypification, the question of whether it was appropriate to define subcommittees within it be left with the General Committee. The General Committee had so decided for the last Special Committee on Lectotypification, that had also been assigned very different tasks.

Prop. A was referred to the Special Committee on Lectotypification.


Stearn favoured the Recommendation. Linnaeus had founded many of his species on Sloane’s Historia and Plukenet’s Phytographia. Often the very specimens from which these illustrations were drawn could be found in London’s Natural History Museum. An example was Evolvulus, where the pubescence, crucial for species identification, could be easily seen on the specimen from which the illustration had been made. As a Recommendation this would do no harm.
Greuter noted that the Section had just defeated a parallel but unconvincingly worded proposal to introduce this as an Article. The Rapporteurs, in their comments, had asked that those in favour of considering this an Article rather than a Recommendation express this by voting "Ed. C.", and the mail ballot showed considerable support for that suggestion. If this was to become an Article, the words "should be used" would have to be replaced by "is to be used". The Section might first want to debate whether this was to become an Article and, once this was decided, vote on the proposal itself either as it stood or as amended.

Perry would rather replace "should be" by "may be", lest people be forced to use the specimen to identify the illustration.

Briggs had a similar concern. As reworded, the proposal would send people on a long chase for specimens. Perhaps ", if known," could be inserted editorially after "based".

Greuter agreed, Perry's point being well taken. The wording would thus be "may be used" if it was to become an Article, and "should be used" if it would stay a Recommendation.

Jeffrey also felt it essential that "may" be used if this were turned into an Article, but his Committee preferred it to be a Recommendation.

The Section first voted that Rec. 9B Prop. B should stay as a Recommendation, after which, Prop. B was approved.


Hawksworth, since this was a major concern to phycologists, suggested an amendment, to change "iconotype" to "illustration" – after which such a recommendation might be quite useful. Having edited papers with descriptions of new algae, he was aware of the difficulty. His motion to have the proposal discussed, in spite of its rejection by the mail vote, was seconded.

Greuter confirmed that one of the concerns expressed by the Rapporteurs, which might have negatively influenced the mail vote, was the use of the word "iconotype". However, he had a second objection. Under Art. 9.3 of the Code, an illustration was acceptable as type only if no specimen could be preserved, which was extremely
rare. According to Silva, many phycologists ignored this provision and selected icons as types although a specimen could have been preserved. Technically such names were invalid. The proposed Recommendation might encourage the bad practice of invalidly publishing names in phycology.

Silva did not fear that the Recommendation would encourage the use of iconotypes. He agreed with Hawksworth that it would be useful to recommend that, when an illustration was used as type, the collecting data of the illustrated specimen be given. Otherwise, there was no way of knowing the type locality.

Prud’homme van Reine added that the ocean was full of nanoplankton, which at the moment it was barely possible to collect in such a way that one could look at it, let alone preserving a type. Sometimes it was possible to produce a drawing of it and publish it as the type, which was the only way of dealing with one of the most commonly occurring plant groups of the world.

McNeill wondered whether, in that case, the word "specimen" was appropriate. If it was not preserved, was it a specimen? Should it not rather be called "material"?

Silva accepted both Hawksworth’s and McNeill’s suggestions as friendly amendments.

Taylor, referring to Art. 9.3, asked how one was to decide when it was impossible to preserve a specimen.

Demoulin explained that the provision limiting the use of illustrations as types to cases when it was impossible to preserve a specimen had always been in the Code, and it had always been tacitly accepted by a large group of phycologists working with unicellular algae that their specimens were impossible to preserve. This might have been true 50 years ago but was certainly untrue today when there were fixation media such as glutaraldehyde. However the tradition was strong. Should one now insist that this rule be enforced and that all those names were invalid? This would create a big problem. No species name in Euglena, for example, had a type specimen. Personally, he disapproved of the practice, but one had to cope with reality. Until a rule was found that would legalize past practice while perhaps enforcing the rule for the future, one had to live with the situation. The recommendation would do no harm.
Faegri noted that Art. 9.3 did not only apply to algae but also to fossil material, where specimens were often destroyed when investigated and thus could not be preserved. Even if there were now methods to preserve algae, the provision would not become superfluous.

Greuter pointed out that Art. 9.3 did not apply to fossil plants.

Jeffrey added that Art. 9.3 not only applied when it was impossible to preserve a specimen but also when a name was without a type specimen. Art. 7.3 said a holotype was a specimen or illustration used or designated, whether in the past or in the future. He had recently designated an illustration as the holotype of a new species name because the specimen in existence was far less informative than the illustration. This seemed, to him, perfectly logical.

Prop. C, as amended, was accepted.

Article 10

Prop. A (5 : 178 : 2 : 0 : 1) was ruled as rejected.

Prop. B (3 : 178 : 3 : 0 : 0) was ruled as rejected.

Prop. C (12 : 168 : 2 : 0 : 1) was ruled as rejected.


Brummitt asked for discussion of the proposal, which was seconded. The Rapporteurs had commented that Prop. C and D had a parallel intent, which he doubted. Prop. C would have added to the Article "or if such a choice would be in serious conflict with the protologue", which was a major change to which he objected. However, Prop. D had seemed to him to be simply a clarification of Art. 10.2, the second sentence of which, particularly, had always been obscure to him. When he had read the proposal by Wijnands & Zijlstra, he had thought it a much better wording than the present one.

Zijlstra thought the proposal necessary to eliminate a conflict between Art. 10 and Art. 7.3, which stated that the holotype was a specimen or illustration used by the author. If the proposal was rejected, original material, which sometimes still existed, could not
be used as the type, and a neotype had to be selected, an alternative she disliked. The comments of the Rapporteurs, that the proposal would cause many changes of typification, were unwarranted.

Jeffrey agreed. As Secretary of Subcommittee 3B he had had considerable correspondence with Wijnands over the proposed wording and was convinced that it was a distinct improvement and clarification over the present one. Zijlstra’s point about a conflict with Art. 7.3 was well taken, and the proposal should be supported.

McNeill objected that it was quite incorrect to suggest that this was merely a clarification of the existing article. It was a definite change. It had been decided in Sydney that the type of a name of a genus was to be the type of a name of an included species, regardless of whether or not that type was part of the original material for the generic name. This proposal would, on the contrary, require that the type must be original material in those cases in which the author had not cited a species name.

Brummitt conceded that perhaps he was being stupid, but he could not see a change. The proposal referred only to situations in which there was no species name given in the protologue.

McNeill explained that in such cases, under Art. 10.1, the type now was the type of another species name, not referred to and likely unseen by the author of the generic name. This had been one of the major objections made at Sydney by those who had felt it was improper that, say, a Jacquin name could be ultimately typified by a specimen collected long after he died. Nevertheless this had been the decision.

Brummitt remained unconvinced.

Greuter explained the situation once more. The Section at Sydney, and the Committee which had drafted the proposals on Art. 10, had been faced with the alternative, expediency or historical faithfulness. Early Codes [starting from Stockholm] said that the type of a genus was a species. This was changed [at Seattle] to “the type of a name of a genus is a species” [also the original Cambridge wording]. But a type could not really be a species, meaning a taxon, it had to be an objectively defined element. When this was changed at Sydney, it was decided to accommodate established tradition by writing “the
type of the name of the genus is the type of a name of an included species. If no name of a species had been mentioned (as e.g. by Miller, Adanson, and later Thouars), this was to be the type of a name of a species to which, one assumed, material included in the author’s original generic concept did belong. The proposed change would, in these cases, mean that herbarium material seen by the author of the generic name must be inspected, which meant having to know and prove what material had been seen by, e.g., Miller – a notoriously difficult task. It would also overturn previous lectotypifications which had almost invariably been carried out and recorded by mention of a binomial. The present rule, under which a specimen that was not the type of a species name could be the type of a generic name only by conservation, did work. To overthrow it in the interest of historical faithfulness, which was the only interest at stake, would be a backward step and cause a lot of work that was completely useless.

Zijlstra finally understood why the Rapporteurs had said that many generic names were concerned. The proposal was meant to address only those cases, perhaps a few dozen, in which a generic name had not later on been identified with a named species, and yet there was identifiable original material. A large majority of generic protologues included elements that had later been identified with a named species, and then of course the corresponding binomial should continue to provide the type.

Prop. D was rejected.

Prop. E (128 : 26 : 25 : 0 : 2) was accepted.

Prop. F (141 : 17 : 23 : 0 : 2) was accepted.

Prop. G (134 : 38 : 7 : 0 : 2) was accepted.


Jeffrey thought that the proposed change was a bad idea. The concept here was that of a nominal genus, not a taxonomic genus, thus the difference was far from editorial.

Prop. H was referred to the Editorial Committee.
Article 11


Zijlstra wished to amend the proposal by limiting it to those cases for which it was really necessary. Not yet having the new text available, she requested that the debate be postponed, which was granted. [The following discussion actually took place on the next day, at the beginning of the Fifth Session.]

The request for discussing the heavily defeated Prop. A having been seconded, Zijlstra moved an amendment to it [also distributed, with supporting arguments and examples, on a duplicated sheet]. She had come to realize that the proposal would be advantageous only in the case of rejected earlier homonyms. She now also thought that this point should be covered, not in Art. 11 as proposed nor in Art. 14, but by means of a new provision to follow after Art. 6.4: "A name that has been rejected against a later homonym, however, has to be treated as illegitimate from its beginning." The 13 names she had listed as examples (11 of which were in current use) were, she thought, presently illegitimate due to inclusion of the type of a name meanwhile rejected as an earlier homonym. They could not be used unless conserved. One such case was that of Goodyera R. Br., based on the same type as Epipactis Ség. (1754) [by subsequent typification – Ed.] now rejected against Epipactis Zinn (1757). She felt that, in such cases, conservation should save not only the conserved name but also the one threatened by the rejected name. After some hesitation the motion was seconded.

Nicolson spoke in favour of the motion. At Berlin he had made a similar proposal [Art. 6 Prop. F] which, however, would have created more havoc than it would have cured. Zijlstra had identified and eliminated its weakness, and her examples were convincing. Under his earlier proposal, a replacement name like Paxillitriletes would have become illegitimate, retroactively, upon rejection of the earlier homonym of the replaced synonym, Thomsonia Wall., as a taxonomic synonym of conserved Amorphophallus. Zijlstra's proposal, limited to homonyms of conserved names, took care of that point.
Jeffrey drew attention to the previous rejection of Art. 8 Prop. R and S, to which the original proposal was tied, so that accepting it in isolation would have been unwise. The amended proposal, however, concentrated on a particular aspect relevant to nomenclatural stability, and would make explicit what, in practice, had been assumed in the past. Also, it was a logical corollary to Art. 14 Prop. C. To his mind, conserved names were retroactively made legitimate from the date of their valid publication, not from the date of their conservation. The last five words of Art. 6.4 were superfluous and misleading, and should be deleted.

Greuter reiterated his earlier warnings against rash adoption of motions from the floor that had not been critically considered by a larger audience. If a good example was needed, there it was. Jeffrey's previous statement, to the effect that acceptance of the proposal would make explicit the status quo, was plainly incorrect. Art. 14.9, at present, stated that rejected earlier homonyms were not made illegitimate by their rejection, and adoption of this proposal would introduce a conflict into the Code. Art. 14.9 had likely been applied in the past, and rejected earlier homonyms had been used as legitimate basionyms for names of subdivisions of genera.

Zijlstra objected that the quoted provision under Art. 14.9 was quite recent, having been added at Berlin [Art. 14 Prop. G, amended]. Acceptance of her new proposal would indeed require deletion of that clause.

Brummitt acknowledged that Zijlstra was correct in saying there was a problem, but such cases had come up from time to time and were then always dealt with by means of conservation. Hoffmanns-eggia and Forestiera, the latter quite recent, were cases of this kind.

Demoulin viewed this as a confirmation of the absurdity of Art. 63 and the concept of illegitimacy, the most obnoxious provision of the Code. Perhaps the time was still not ripe for deleting that Article, which he hoped would be the case in ten or twenty years. Since for the time being one had to live with it, one might try to improve it. The problem addressed by Zijlstra was parallel to the one now covered in Art. 63.3, and this might be a more appropriate place for dealing with it. The names she wanted to save might be defined as
merely incorrect when published. Rather than dealing with the problem under Art. 6, a new wording might be found, to be introduced under Art. 63.

Zijlstra saw this as a problem, since Art. 63.3 referred to names initially incorrect that could become correct, whereas her concern were names now illegitimate that should become legitimate.

Demoulin, looking at the whole Art. 63.3, not only at its last sentence, still felt there was a good parallelism. This provision could well be extended to cover the cases addressed by Zijlstra's proposal. Yet, he did not want to come up with a wording of his own.

The motion was defeated, and Prop. A was withdrawn by Jeffrey.


McNeill stated that the proposal was related to Art. 7 Prop. KK, which had been referred to the Editorial Committee on the understanding that there be no change of substance. It seemed appropriate to do likewise in this case.

**Prop. B** was referred to the Editorial Committee.


Jeffrey requested permission to bring up the proposal, in spite of the negative mail vote, on the grounds that the Rapporteurs' comments showed they had not understood its import, and also that it was being proposed as a package along with Art. 24 Prop. A, Rec. 26B Prop. A, and Art. 57 Prop. C, which had not received as unfavourable a mail vote but could hardly stand without the present proposal. If this were to pass, he foreshadowed an amendment from the floor to Art. 6.7 and Ex. 2. The request was not however seconded.

**Prop. C** was ruled as rejected.

**Article 13**

**Prop. A** (130: 25: 23: 0: 5).

Friis noted that the proposal was related to App. V Prop. B, with which there was no conflict. The works proposed for rejection could either be enumerated in the Article or listed in a separate section of the new Appendix. This could be left to the Editorial Committee, depending on the Section's decision.
Greuter agreed. The proposal could stand on its own. The Editorial Committee would take care of transferring the matter to the new Appendix, if established. If not, it would stay as proposed.

Barrie moved an amendment, to add another work to the list. Unfortunately he did not have the exact citation. It included generic names published by John Hill in an encyclopaedia published in November 1753, which predated some 250 of Miller's generic names, and was referred to in a recent article in Adansonia.

Friis asked whether Barrie had checked that the work was not listed in Art. 20 or 23. [He had.]

Stearn explained that the reference related to a work which hardly anybody had ever seen and which was absent from major botanical libraries. It would be well to add it to the list, to prevent many changes.

Hawksworth had proposed a new Appendix for exactly this sort of situation. There would be a continual flow of proposed additional rejected works, and it seemed better to list them all in one place and thus to minimize tinkering with the body of the Code. If there would be an Appendix, works could be proposed for rejection by analogy with names, by being submitted to the General Committee and, subject to its recommendation, subsequently added to the Appendix. In the Hill case, it might be prudent to go the General Committee route rather than taking an immediate decision.

Greuter reported that the motion concerned in fact two items. One were Hill's entries on natural history in A supplement of Mr. Chambers's Cyclopædia: or universal dictionary of arts and sciences, edited by Scott in two volumes and published in 1753; the other, published in 1754-1755, were again Hill's entries on natural history in A new and complete dictionary of arts and sciences, published anonymously in four volumes by a Society of Gentlemen. These two sources of generic names had recently been dug up and were quite disturbing. He wanted to second Barrie's motion to have these added to the present list. It might admittedly look funny (though quite feasible if the Section so ruled) to treat a work published in 1755 as pre-Linnaean, and transferring these titles to an Appendix later on, the Section permitting, would be more elegant.
Dorr stated [erroneously] that the second Hill reference was discussed in Art. 20 Prop. B, but the first one was not.

Jeffrey felt that the Hill works, in view of Greuter's remarks about the date, should be added to the list of Art. 20 Prop. B as works to be rejected as sources of validly published generic names. He would not object to Hawksworth's proposal to place all proscribed works in an Appendix if it was to be subdivided into sections, so that works to be rejected under Art. 13, 20 and 23 would be in different sections because they were rejected for slightly different reasons. This might well be a matter for the Editorial Committee to decide.

Greuter agreed that the placement of the rejected titles, whether in the appropriate provision or in an Appendix if the latter was approved, was an editorial matter. Presumably Art. 20 was indeed the proper place. What was definitely not editorial was the decision to have these titles added.

Barrie, having had his motion seconded, now thought that the references should better be added under Art. 20 Prop. B.

Greuter advised not to postpone the decision now the matter had been placed before the Section. The Editorial Committee could make the appropriate transfer.

McNeill noted that there might be a difference. If the works were defined as pre-Linnaean they would not count for names at any rank, not just for generic names. This made a difference if species names were included [which was not of course the case].

The two additional items having been read out again, Barrie's motion was carried.

Prop. A, thus amended, was accepted.


Jeffrey pointed out that the proposal was part of a package including Art. 20 Prop. A, Art. 37 Prop. C, Art. H.3 Prop. A-B, Art. H.6 Prop. A-B, and Art. H.9 Prop. A. The aim was to remove condensed formulae and their equivalents from competition for priority with names of non-hybrid taxa. The proposals would make it clear that condensed formulae were not names and could not compete with the names of non-hybrid taxa for the purposes of homonymy.
They needed not conform to the criteria of valid publication of names of corresponding non-hybrid taxa, had no type, and could not be applied to a non-hybrid taxon. They were, potentially, almost limitless in number: for \( x \) generic names one could form \( x \) into \( x-1 \) over 2 formulae (1,846,000 for the Compositae alone, even if no duplications were admitted).

Greuter had always, personally, hated those hybrid formula names; but before deciding to get rid of them as names competing with normal names in matters of homonymy, one had to be certain of the consequences. At least some such formulae had been listed in ING. His cautionary note was due to lack of advice from the Committee for Hybrids. A member of that Committee who was present, perhaps Trehane, might be prepared to comment.

Trehane replied that he was not actually a Committee member, though Baum was. Asked for an opinion, he felt that it was about time the Committee for Hybrids did some work.

Stearn, as a founder member of the Committee for Hybrids, strongly opposed this potentially dangerous proposal. These formula names had been listed, in particular for the orchids where everything could hybridize with everything.

Greuter advised that this only available advice from at least a former member of the Committee for Hybrids be taken very seriously.

Jeffrey, to cover Stearn's point, foreshadowed an amendment he was to move from the floor to yet another part of the same package, Art. 64 Prop. F: the proposed words "... one may be conserved over the other, or a replacement for the condensed formula may be adopted" were to be replaced with "... the use of one may be sanctioned over the use of the other by a procedure analogous to conservation", in order to safeguard against any clash of so-called homonymy between condensed formulae and typified names. So modified, his proposals would not cause a threat to any names adopted for hybrids, but they would free botanists from having to consider them in the same category as names which had types, which was illogical.
Greuter announced that, since the Committee for Hybrids obviously did not function, and since it had no routine function like other Permanent Committees which advised on the conservation or rejection of names, there would later be a new proposal by the Bureau of Nomenclature, to delete the Committee for Hybrids from Division III of the Code. It would be appropriate, however, that in replacement a Special Committee be set up, to which all proposals affecting hybrid nomenclature could be referred for study and for reporting back to the next Congress. This could save a lot of the Section’s time and spare it the frustration of having to decide on specialized proposals without expert advice. He therefore moved that the Section authorize the set-up of a Special Committee on Hybrids, to report to the next Congress. The motion was seconded and carried.

Prop. B was referred to the Special Committee on Hybrids.

Nicolson moved a New Proposal on behalf of Hoogland & Reveal, to amend Art. 13.1 by adding, after clause (f), the following new text: "Family and ranks between family and genus: (g) ALL GROUPS, 4 Aug 1789 (Jussieu, Genera plantarum)". The motion was seconded. A duplicated memorandum from which the proposal was taken was available, and the complete explanatory text could be found there.

McNeill noted that the new text should not be placed as proposed but should follow clause (e), because it would not apply to fossil plants for which the starting date was 1820. This was merely an editorial point.

Nicolson added that in App. IIB of the Code there was already the statement that 1789 had been used as the starting-point date, but there was nothing in the body of the Code to authorize this. The proposal would simply make formally correct what was informally accepted.

Prud’homme van Reine hated new proposals brought up during the meeting, and also disliked new starting-point dates.

Demoulin shared these feelings. Having had as one of his primary goals in nomenclature the elimination of a later starting point [in fungi], he would not accept introducing an additional one, especially when the proposal came from the floor. It might be tolerable if
people had used Jussieu’s book as a baseline for the practical purpose of setting up the current list of conserved family names. Yet he definitely objected to having this formally introduced as a rule into the Code.

Hawksworth challenged the argument that this would merely reconcile App. IIB with legality. The statement in App. IIB only referred to Spermatophyta while the proposal would extend application of the date to all groups, which was a change in principle. If there was to be either a protected NCU list of family names or an extended list in the Code, there was no need for any date.

Greuter shared the expressed misgivings regarding new proposals raised from the floor when they were not a matter of urgency. Should the NCU principle fail, there might however be urgency, and he would therefore ask the Section to authorize the Editorial Committee not to change the authorships and dates of family names listed in App. IIB. This would at least temporarily stabilize the situation without a need to tamper with the Code in undue haste.

Nicolson’s motion was defeated.

Greuter moved that the Section empower the Editorial Committee to leave the listed authorships and dates in App. IIB unchanged. The motion was seconded.

Brummitt asked whether, assuming the NCU principle would fail, adopting this motion would mean that App. IIB would have to stay unchanged.

Greuter explained that the motion would achieve that names presently listed in App. IIB would be listed as before, without change of authorships and dates. The reason for the motion could be found in Reveal & Hoogland’s memorandum, where over 100 changes to App. IIB were foreshadowed.

Brummitt commented that this seemed to prejudge the NCU issue. One should not take such a step at this point merely as an outlet in case NCU should fail.

Nicolson said that there were many family names published by Adanson and Batsch before 1789, and for these the authorship and date would change without the proposed authorization.

Greuter’s motion was defeated.
Article 14


Jeffrey pointed out that Art. 69 Prop. B, by Reveal, was complementary and not adversary to Art. 14 Prop. A-B. The three together would make for greater flexibility by making both the conservation and rejection options widely available. However, Reveal's proposal alone could not save an illegitimate name that was not proposable for conservation under the present rules.

Brummitt introduced this all-time classic among proposals, to open conservation to specific names. This highly significant issue had been discussed at Congress after Congress, and he was sure it would get through some day, hopefully even now. The argument against had been that by permitting conservation of specific names, or indeed as here proposed of names at any rank below family, the floodgates would be opened. In 1981 in Sydney, the gates were edged slightly open by allowing conservation of names of species of major economic importance. In the following six years, only two species names had been conserved, *Triticum aestivum* and *Lycoper-sicum esculentum*. Over the next six years and up to the present, two more names were being added under that provision, *Erica carnea* and *Bactris gasipaes*. At Berlin, conservation was opened a little bit more by allowing, through a combination of Art. 69 and Art. 14, that misapplied species names could also be conserved, and in the six years since then the Committee for Spermatophyta had recommended approval of twelve such proposals, failing to recommend sixteen. All this hardly indicated that the floodgates had been thrown wide open. It was a sensible procedure, some very critical names had been saved, and the Committee had coped well. He therefore did not think that it would be a rash decision if conservation of specific names were now opened completely. The proposal was a great chance to show the rest of the world that botanists took the stability of nomenclature seriously. If it failed once more, this would be held against nomenclaturalists by those who believed them to be unsympathetic to the needs of users of names.

Friis, while working in the Committee for Binary Combinations, had encountered many names, published between 1753 and 1800, that
were validated by very short descriptions, lacked a type specimen, and had never come into use. An example, mentioned in Art. 23 Prop. E, was Forsskål's "Sida parva, flore albo". Such names, when they threatened established names, could be ridded by accepting this proposal.

Barrie stated that in St. Louis there was a great deal of sympathy for liberalizing the options for conserving species names, but much less for names of varieties, formae, or subdivisions of families. Retention of the current wording of Art. 14.1, when combined with acceptance of Art. 14 Prop. B, would permit conservation of species names without widening that possibility to names in ranks other than family, genus and species.

McNeill clarified what had been a general discussion, ranging across Art. 14 Prop. A and B. As Barrie had explained, Prop. A would permit conservation at all ranks but did not of itself extend the options for conservation. Prop. B, while retaining conservation at the ranks of family, genus and species, would remove the restrictions that currently existed on conservation of species names.

Hawksworth strongly supported Prop. B, but as a supplement, not an alternative to the NCU principle. Conservation procedures just took too long. The common pattern, certainly in mycology, was that people first made new combinations or changed names, then somebody else proposed conservation, which became stuck in debates in the Committee for Fungi (as he hoped it would be called shortly), a process that in one case had lasted for 12 years without a decision having yet been reached.

Demoulin felt that, whatever might happen with the NCU issue, unrestricted conservation of species names was needed. Belgian botanists felt the same as those in St. Louis, they did not support Prop. A, not wanting to burden Committees with proposals dealing with intermediate ranks, but they very much wanted Prop. B.

Mabberley strongly supported Brummitt's views. Having had to compile a dictionary of plants and plant products, he had found it extremely frustrating that it was not normally possible to conserve specific names. True, conservation often took a long time, and one should consider accelerating the system, perhaps by creating a series of committees concerned with different aspects of conservation rather than leaving it to a couple of overworked people.
Greuter reported the opinions on the proposal of various Permanent Committees. This was of special significance since one of the major arguments against widening the conservation options had been that committees would be overloaded. While the Committees for Spermatophyta and for Algae had not voted and the Committee on Bryophyta had not completed its vote (it stood 2:2), three committees had handed in complete results: the Committee for Pteridophyta supported Prop. A by 6:2; the Committee for Fungi and Lichens supported it by 8:3, 3 undecided; and the Committee for Fossil Plants supported it by 10:2.

Jeffrey thought that Prop. A also related to Art. 18 Prop. A, which might be considered superfluous if the former were to pass.

Brummitt aimed at obtaining clarity, his own comments having perhaps been ambiguous. A vote for Prop. A was for opening conservation to everything, including subspecies, varieties, sections, the lot; whereas Prop. B would restrict conservation to the names of families, genera, and species. Was this correct?

McNeill replied in the negative. Prop. A taken alone would indeed open widely conservation at all ranks except species, which under the restraint in Art. 14.2 could still be conserved only if the species were of major economic importance or fell under the provisions of Art. 69. If Prop. A was to pass, it would of course be extraordinary if Prop. B did not pass also. Prop. B without Prop. A, on the other hand, would remove any restriction from the conservation of species names, so that families, genera, and species would be treated alike, but no other ranks would be involved.

Prop. A was rejected by a card vote (42.8% in favour, 178:238).
FOURTH SESSION

Tuesday, 24 August 1993, 14:00 – 16:30

Article 14 (continued)

Prop. B (80: 89: 4: 2: 5) was accepted.

Chaloner, consequent to the approval of Prop. B, drew the attention of the Editorial Committee to the fact that the emasculated residue of Art. 14.2 was almost meaningless. It was just an optimistic declaration about the desirability of stability in names, which was covered in the Preamble to the Code. He hoped the Editorial Committee would think of removing that pointless anodyne statement.

Prop. C (84: 24: 72: 0: 0) was accepted.

Prop. D (62: 30: 86: 0: 0) was referred to the Editorial Committee.


Brummitt observed that this provision was already in the Code as Art. 7.17, as Greuter had agreed. The new proposal might be better than the present 7.17, but it did, in fact, simply repeat what was already there.

Greuter agreed that this was editorial and boiled down to the transfer of Art. 7.17 to a new position where one could find it. Funnily enough, neither the proposer nor either Rapporteur had noticed the present provision, which did indicate that it was not ideally placed.

Prop. E was referred to the Editorial Committee.

Prop. F (115: 43: 18: 1: 1) was accepted.


Demoulin had submitted a written request to the Chair, duly seconded, to take up the heavily defeated Prop. H and, since it was more far-reaching, discuss it ahead of Prop. G which would provide a fall-back solution should Prop. H be defeated.
Prop. H (34: 139: 5: 1: 1) was, as requested, discussed first. Demoulin and his co-proposer wanted to answer some of the Rapporteurs' comments which had negatively influenced the mail vote. Prop. H had resulted from his twelve-years' experience, both on the Editorial Committee and the Committee for Fungi and Lichens. Work on the list of conserved names of fungi was extremely unsatisfactory under the current system – which had already been partly dismantled by the acceptance of Prop. F. The former Art. 14.8, which made it mandatory in many cases to conserve a "pseudohomonym" (a later usage of the name or a taxon in which the conserved type was included) was illogical and not at all in line with modern concepts of types and names. He had discovered that the system had crept into the Code rather obliquely, without having been properly discussed and accepted at a Congress. He had discussed the matter earlier (in Taxon 38: 83-87, 1989), and also privately with Greuter, McNeill, Nicolson and others. The proposal, foreshadowed in 1989, had been formally submitted by him and Perry in 1991. For practical reasons, a less encompassing rule than was here proposed, that enabled a choice between conserving a name with a new type either without changing its authorship and date or from a later date (when the former option would overturn what had been done before) might be preferable: such was Prop. G. He believed however that the reasons stated by the Rapporteurs for opposing Prop. H, that it would create a tremendous amount of committee work, were exaggerated. Application of the new rule would be straightforward: one would not change the authorship or date of a name when its type was conserved. Out of 150 fungal names now conserved, he had found only about 10 that, at first glance, might be affected. Three of them had soon turned out not to be involved; three, all fairly recent, were presently conserved under the form of "pseudohomonyms" but this could easily be changed, and the last three cases (including two lichens) still remained to be checked. Six cases out of 150 did not represent an "enormous amount of work", and for his part of the Code he was ready to do it. In the phanerogams, Perry and Brummitt had spotted about 70 cases that might be troublesome, but this would require further investigation. No practical consequences were expected to result
from changing authorships or dates of some conserved names. Prop. H was much more logical and straightforward, but if it were defeated, he would support Prop. G.

Perry had checked through the Spermatophyta in App. IIIA. She had found about 80 cases of homonymy which needed to be investigated: all those in which a rejected homonym belonged to the same family as the conserved name. The actual number of changes would be smaller. The work involved was manageable and would result in greater simplicity.

Brummitt knew that this matter affected the work of all Permanent Committees. For both generic and specific names the type was often not what everyone had thought it was and so had to be changed by conservation. The choice was between conserving the name from the original publication with a new type or from a later publication, thereby changing the date and author of the name. That question had again been raised in a paper just published (Brummitt & Perry in Kew Bull. 48: 413-421. Aug 1993 [which he had been told was the most unreadable paper ever published in the Kew bulletin]), reprints of which were available. Yet another proposal on the same issue was included there, and he apologized for having to raise it from the floor – but the whole picture had emerged only after the other proposals and the Rapporteurs’ comments had been published. It suggested a third option, making a difference between names of genera and of species. The present situation, when a change of type made it necessary to conserve a name from a later author and date, worked at the generic level: one looked in Index kewensis, or in the Index of fungi, for the author of the first appropriate combination, who in most cases was appropriate as author of the name to be conserved. At the specific level, however, just recently introduced, the situation was different, because the type was not a specific name but a specimen, and it was hard to say when such and such a specimen was first included in the species. This had not really been thought about when one started conserving specific names. In fact, those proposing a change of type of a specific name chose a different specimen, an early or even a modern one, without changing the author and date of the name. It might look nonsensical to have a type specimen collected after the name was published, but it was practical.
He had therefore proposed to maintain the status quo for generic names but at specific level to conserve names with a new type from the original author and date.

**Greuter** wondered whether **Brummitt** had in fact been speaking on Prop. H.

**Brummitt** had been trying to explain the whole context. The Section could not vote on any of these interrelated proposals until all the options were apparent. At the appropriate time he would ask for permission to consider the new proposal.

**Burdet** asked for a vote first on Prop. H, then on Prop. G, and finally on the new proposal by Perry & Brummitt, if it was seconded.

**Prop. H** was rejected.

**Greuter** wished the Section to be fully informed of the issues before being asked to vote. Prop. G had been brought forward as a compromise between the present rule and Prop. H. The *Code* had been far from clear. Was one to create artificial later homonyms for conservation purposes, or conserve names from their original author and date but with a type not mentioned in the protologue, i.e., a kind of a neotype? Both solutions were unusual, but then, acts of conservation were always exceptions to the rules. Past practice had been somewhat inconsistent, but only slightly so. Conserving a name from the original author and date, although with a different type, had the advantage of avoiding a loss of priority for the name to be conserved. Under the current practice, it often happened that names had to be listed as rejected synonyms that were later than the original publication of the conserved name, but earlier than the name as conserved. Some such additional rejected names were in current, but less general, use. Committees had often been faced with proposals to add such synonyms that had previously been overlooked. All this was a nuisance. However, it would not be really helpful, as Prop. H would have required, to undo previous work and aim at the purists’ solution. What was to be gained by changing author citations of conserved generic names when they were now consistently used? Prop. G would provide the option of leaving untouched what had been done (while not ruling out retroactive changes if a Committee felt they were useful), and for the future it would offer the possibility, but not the obligation, to conserve names
from their original publication and with their original author. Contrary to Prop. H, Prop. G would not cause a delay in the publication of the next Code, since conserved names could all stay as they were; it would however provide new flexibility for the Committees in the future. The new proposal by Brummitt & Perry, if seconded, would not become redundant if Prop. G was approved, since its last half could be combined with Prop. G, which he would be happy to support.

Brummitt admitted that under Prop. G it was possible to put into practice the procedure he preferred; the proposals were not incompatible. However the new proposal gave authors of proposals clear guidance on what they should do, whereas under Prop. G they would be faced with two options. What Perry and he were proposing was exactly what had always been done.

Jeffrey noted that the Brummitt & Perry proposal did more than give clear guidance, it was mandatory as an article. He preferred the greater flexibility offered by Prop. G.

Demoulin agreed. While he of course would have preferred Prop. H, he could live with Prop. G – but not with the revised Perry & Brummitt proposal which would make mandatory the present objectionable system for generic names. It was possible, as had been suggested by the Rapporteur, to amend Prop. G so as to make conservation from the original place and date of publication mandatory for species names. But this was probably unnecessary because no one would be so absurd to use the present system for species names.

Prop. G was accepted.

The Perry & Brummitt proposal was withdrawn before it could be seconded.

Prop. I (19:102:57:1:1) was withdrawn.

Prop. J (28:110:37:2:1) was withdrawn.

Article 15bis


Barrie suggested that, given the complexity of the proposed Art. 15bis and of the whole question of NCU, it might be wise to have a
vote on whether to consider Art. 15bis at all before going into each of its individual parts. NCU should be voted up or down as a package.

Greuter, having considered the question of how one could vote for or against the principle of NCU without going into details, advised that the whole set of proposals be split into sub-packages. The first such sub-package, to stand for the basic idea of NCU reduced to its bone, might consist of Art. 15bis Prop. A, W, and Rec. 15bisA Prop. A. Its adoption would place the principle of NCU into the Code without yet permitting its implementation, since it would not provide the necessary guidance as to how to implement it in detail. The next question might be whether to extend the range of application of NCU beyond the ranks of family and genus, meaning Art. 15bis Prop. B. The third step might be to provide for implementing NCU protection for the single list that was, on the basis of committee recommendations, mature: the list of spermatophyte families. To this end, a further package of proposals would have to be accepted: Art. 15bis Prop. C, D, F, L, P, R, and, optionally, G, H, and J. Proposals concerning the sanctioning of fungal names did not depend on the NCU issue but were just a question of fungal nomenclature. They were Art. 15bis Prop. I, K, M, O, Q, T, V, Y, and Z. All the proposals he had not mentioned would be useful in the future, when lists other than the one presently before the Section would come up. Their present adoption was not essential, although they would have to be passed before other lists could be approved. Was it in the mind of the Section to first concentrate on the three essential proposals, Art. 15bis Prop. A, W, and Rec. 15bis Prop. A?

Barrie agreed that this would be an efficient use of the Section’s time.

Dorr requested, as a procedural issue, that the Section vote on whether or not to discuss Art. 15bis in separate packages of proposals, and that that these packages be outlined on the chalkboard so that all would understand what was being voted. The procedural motion to discuss Art. 15bis package by package was carried.

Hawksworth had been involved in what was to become the NCU initiative since 1986, well before it had been made publicly known. It
was part of a general concern about how biological nomenclature operated. The fact that the Section had over 300 proposals to consider showed that a satisfactory Code had not yet been produced after some 130 years. One had to accept that the current way of operating had failed, one had to face up to that and not just keep hiding behind history. NCU was part of a broader initiative. The International Commission on Zoological Nomenclature was doing what botanists were proposing to do. They were producing a list of protected names in association with BIOSIS and planned to introduce provisions to that effect into their Code, the drafts of which were due to be completed by Christmas. IUBS was sponsoring a meeting of representatives of the different Codes in March 1994 to which delegates from zoology, cultivated plants, bacteriology, and protistology had already been nominated. A delegate would be appointed from the International Commission on Virus Nomenclature, freshly agreed last week, since viruses would now receive formal Latinized names.

There was thus a major move to improve the system. Comments by Jeffrey in particular had made it abundantly clear that many illogicalities remained in the botanical Code, but that nomenclature was so embedded in history that any attempt to correct this with retroactive effect would have unforeseen and potentially disastrous consequences. A system was needed to get away from this situation and obtain a much neater Code, that non-specialists could actually work with. The current Code did not provide unambiguous answers, and never would, to many questions of, e.g., legitimacy and typification. Many of those present had wrestled with such problems over the years, and come up with an answer - but should one now continue to rework the same issues? And what when there was no clear answer at all? He drew attention to examples given in a recently published paper (in Bot. J. Linn. Soc. 109: 543-567. 1992). Priority was perhaps the largest issue. It was impossible to be sure that one had, in a given case, found the earliest name. Arthur Cain had told his students that it was like looking for water-babies. "You cannot say there are no water babies existing until you have seen water-babies not existing" was a Charles Kingsley quotation. In this context, it meant that unless one had looked personally at every piece of literature plus the original material, there was no way to be sure one had
the correct name according to the rule of priority. In mycology this was an absolute minefield, and the same was probably true in phy- 
cology. There were huge numbers of names, never typified, never sorted out, just waiting for taxonomists to find the relevant original specimens. For flowering plants, as in the case of Hill’s works that had been referred to earlier, there were many such names, some of which had been deliberately left out of the original volumes *Index kewensis*. Many taxonomists could not afford spending hours and weeks on nomenclatural work while employed as scientists and sup- posed to produce revisions, do original scientific work, produce phylogenetic hypotheses, etc. He had conducted a poll of a cross- 
section of botanists in the U.K. and found they devoted, on average, about 20% of their research time to nomenclature. This varied from less than 5% to several in the 75% category. He had also recently done some calculations for a paper (in press) on the cost of biological nomenclature world-wide, including zoology. Using even a 10% average, it worked out at roughly 22 million dollars the bio- 
logical community was currently investing into the history of names, asking questions to which often there were no answers. One had to get away from these diversionary activities.

The proposals on NCU, that had been put together after extensive debates and thorough consideration, foretold a clear way ahead. There were admittedly still some problems in the published NCU lists, but there was much support for the basic issue, including funding support already received for the preparation of the generic list. UNESCO was keen to become active in biological nomenclature, and projects such as IOPI, originating from the "Species Plantarum" project and aiming to prepare a spermatophyte species list, might stand a better chance of being funded if linked to this move. Many in the Section carried votes from institutions, but had those responsible for these institutions really understood what was in- 
volved? This was not an attempt to legislate taxonomy but to save taxonomists’ time by producing working lists that, when their quality was satisfactory, could then be settled. Suggestions had been made (in *Taxon* 41: 622-623. 1992) as to how lists might be corrected and updated in the future. Dorr had earlier asserted that Cronquist had not endorsed the NCU principle, which was absolutely wrong.
Having had a long session with him in San Antonio at the AIBS in 1991, Hawksworth knew first-hand that, while he was unhappy with the concept for species, for families and genera he was not. The general decline in funding for taxonomic biology was partly due to the taxonomists’ image of dabbling with history and pseudolegalistic arguments rather than getting on with their science. In mycology there was so much science to do that he did not like his staff having to spend much of their time on such exercises. Linked to the funding problem and to the negative image of taxonomy was the fact that few young people were entering into the field. The Section’s age structure conformed to the U.K. pattern of taxonomists: if they were a nobler species of vertebrate animals they would now be on the endangered list and down for captive breeding. NCU were a very important issue. The Section should not follow an initial gut reaction but act cautiously; it should not rush into adopting anything it felt unsure about. He had often personally opposed changes in the Code until their implications had been properly assessed. But if the NCU principle was placed into the Code, even without any list being yet approved, this would put out a clear message to the broader biological community that taxonomists were concerned and wanted to take action to put their house into order, and also that they were able to respond to what users of plant names perceived as their needs.

Jeffrey had a vision of a simple Code with a modern starting point date, free from the imponderables of priority, legitimacy, superfluity, typification, first reviser principle, and orthography, with which it was at present burdened by history. This, of course, was of the future, but good taxon lists were the essential starting point if it were to become a possibility. Hawksworth had given good reasons as to why one should try to make it a possibility.

Brummitt, commenting on one particular point referred to by Hawksworth, felt that considering its protologue and possible typification, the name "Species Plantarum Project" had become a nomen ambiguum. There were two operations involved under that designation. One was IOPI, which was to produce a world checklist of vascular plant species within three years. As he saw it, such a
A checklist would be utterly bad, and much too quickly produced to serve a nomenclatural purpose. The other project was the original "Species Plantarum Project", which would take a more cautious view, compile proper flora accounts, and would not be published before the necessary work had been completed. On this point, his feelings about Names in Current Use clashed with those of many others. Hawksworth had mentioned taxonomists wasting their time on repetitive work, but Brummitt knew firsthand from personal experience that the NCU operation created an enormous amount of work for an awful lot of people. It was premature to go ahead with creating this vast bureaucracy. A more cautious approach was preferable, that would produce proper flora accounts and proper revisions. Stability in nomenclature could not be achieved before the taxonomy had been properly sorted out. Approval of the NCU proposals would place an enormous weight of responsibility on the existing Committees, and would multiply Committee work by many times. While agreeing with much of what Hawksworth had said about the need for some action, he was scared by this prospect. A major problem for himself and other members of the Committee for Spermatophyta was the impossibility to properly assess the names on the published NCU lists. They were just names on a piece of paper, giving no idea as to whether they referred to good species or bad ones.

Pitt had been working in taxonomy, but not nomenclature, for some 30 years. Thus, in a sense, he came as an outsider holding an extreme view, which he nevertheless wanted to put forward. Being chairman of the International Commission on Penicillium and Aspergillus (which operated under the Mycology Division of the International Union of Microbiological Societies and had no status under the botanical Code nor in botanical nomenclature, but had been given the specific task of clarifying and stabilizing the nomenclature and taxonomy of Penicillium and Aspergillus – two genera of industrially important fungi) he wished to emphasise that these genera were of great importance in the real world, in the industrial sense. They were the source of penicillin, citric acid, and nearly all of the industrially used enzymes. All enzymes that Novo Nordisk, a huge company, produced for detergents and anything else one could
think of came from *Aspergillus*. Some of the most important fungal toxins and pathogens came from these genera. *Aspergillus flavus* produced aflatoxins, the most potent liver carcinogens known. In the United States alone, 100 million dollars had been spent on research on these toxins over the past 25 years. These genera were worth hundreds of millions of dollars a year, and they also caused costs of hundreds of millions of dollars a year.

No substantial taxonomy had been done on *Aspergillus* in the past 30 years, and no taxonomically correct nomenclature for *Aspergillus* did exist. There was one very good reason for this. The experts in the field shied away from touching the matter because they knew of hundreds of unused names linked to specimens lying around in European herbaria, which in many cases could be identified if one would make the effort of looking at them. Probably most of the names currently in use were incorrect, some of them perhaps four or five times, as these were quite conspicuous fungi that had been studied for hundreds of years and for which many specimens existed. The names now in use were critically important to those who used them. If one was to change the name *Aspergillus flavus*, half of the world's regulatory systems, which were concerned with this fungus because it produced toxins, would not know what to do – or, more likely, they would totally ignore the change. It had happened a number of times in recent years that someone had taken up a new name, having used all the correct nomenclatural techniques – but half of the world simply went on to ignore it because they did not understand why this had to be done, perhaps grumbling once more about nomenclaturalists and taxonomists, and saying "We do not need these people, we got a name and will continue to use it." It was very difficult to enforce changes in the names of taxa, be they fungi or plants, that people were using all of the time, and trying to do so only led to confusion. Conservation was a possible solution, and had just been successfully proposed for *Penicillium chrysogenum* and *Aspergillus niger*, But this was a lengthy procedure, and in the meantime different names came into use, causing confusion, so that the literature became crowded with incorrect information. By consequence, trying to make sense of published data and sort out which species made which enzyme or toxin, or grew under particular conditions in food spoilage, now took a great deal more of his
time than he could spend on taxonomy. Brummitt’s assessment that one should get the taxonomy right first and then the nomenclature eventually would fall into place was wrong. There was not enough time. There were more than a thousand incorrect names in *Penicillium* and very close to the same number in *Aspergillus*, most of which had never been looked at critically. He had done a superficial survey for *Penicillium*, avoiding to look at herbarium specimens and preferring to assume that they could not be recognized. But in *Aspergillus* they probably could, and modern PCR and similar techniques it would in a few years permit the identification of all these species from a few well-preserved spores. Chaos would be the result.

A mechanism to stabilize currently used names in *Aspergillus* and *Penicillium* was urgently needed. The experts in the field had met, formed a committee with international status, and had done what Brummitt said was so difficult. They had looked at the names in current use, had developed a consensus on those names, and had totally ignored the nomenclatural problems that were known to exist in the hundreds. It had taken months of work. The names had to be typified and nomenclaturally checked. Greuter and Hawksworth had helped sort out the nomenclature in a number of cases. In the end, a list had been produced on which there was general agreement, with a single name for each species. What was needed now was a mechanism permitting use of these names without the old names and problems continually cropping up. While he did not want to see that happen, if the Section should side-step this issue there would doubtless be a great deal of pressure on him as chairman of this commission to simply bypass the *Code*. The list had been published and could easily receive official status irrespective of what this Section decided and the *Code* ruled. However, he would much prefer to achieve the same goal through the legal procedures of botanical nomenclature.

Hawksworth, as chairman of the International Commission on the Taxonomy of Fungi which operated under the IUMS, added that there was no reason why this list could not be adopted and recommended to the general assembly of the IUMS Mycology Division which would meet in July 1994 in Prague. Such action would certainly be taken with the *Trichocomaceae* list if it were not accepted
here as part of the NCU initiative, and would set the start for the
disintegration of the current procedures governing the botanical
Code.

Jørgensen reiterated his supported of the NCU principle, which was
sound. He had had problems with the way in which the lists had
been made, and was glad to note that few if any were going to be
accepted there and then. A wider discussion of individual items was
desirable than had so far been allowed. On the family and generic
levels, it would be possible to produce some very useful books by
the next Congress. Perhaps a 99 % accuracy was too ambitious a
goal, but he was willing to swallow quite a number of camels as long
as they were known as such. As Brummitt had said, the problem was
that the lists, as they went to the Committees, were naked lists. One
had to hunt for the problem cases, or for the reasons why a given
name had been preferred to another one, or why a certain solution
had been chosen. Those reasons might be perfectly good, but it
would be helpful for a Committee, and would avoid unnecessary
extra work for Committee members, to know them before being
asked to vote on a list.

He, too, had seen some problems when coming down to the species
level. It was not the labour he was afraid of: the lists would mean a
lot of work, but once it was done, they would be worth it. Having to
do the same work over and over again was certainly worse than
doing it once. He had been afraid of the very long time it would take
to produce species lists – perhaps 50 years – so that in the meantime
a lot of counter-productive shifting around of names could take
place. Now that unrestricted conservation at the species level had
been agreed, he was not that afraid any longer. Disruptive changes
of well-established names could now be counteracted while work
continued on the species lists. He was left with few if any reserva-
tions on the NCU principle.

Johnson warned that the Section ought better take careful notice of
what Pitt and Hawksworth had said: traditional botanical nomencla-
ture and the bodies that governed it would otherwise be bypassed.
He could not agree with Brummitt: it was quite wrong to wait for
getting "the taxonomy right" before dealing with names in current
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use. These were names that existed. The idea behind NCU was to set up lists of those names which one needed for use and did not wish to have upset for trivial and non-scientific reasons, for reasons that contributed nothing to understanding. As for "getting the taxonomy right", that was a pipe-dream. There was no such thing as a "right taxonomy". An enormous number of changes in taxonomy was going to happen with the increase of knowledge and the development of new methods, particularly in the micromolecular field. He would not accept anything produced by Kew or a committee on "Species Plantarum" as the "right taxonomy", or as a basis for NCU lists. NCU lists were to prevent nuisance activities.

Perry quoted the following statement from the introduction to NCU-2: "There is one way to use the lists that should be explicitly discouraged. No one should waste his or her skill and efforts in trying to prove them faulty. They are deliberately wrong in some cases. Abandoning well established names on the grounds of nomenclatural technicalities does not further scientific knowledge, and worse, using them in an altered meaning is a major impediment to taxonomic progress. Therefore my plea: let sleeping dogs lie – until they die and be buried." What sort of wrong things, she wondered, were actually to be sanctioned by approval of these lists?

Taylor feared that if the principle of NCU were voted into the Code it would to some extent stifle taxonomic progress. If a draft list of names was being prepared, one would not know how long it would be before it became, as it were, law: perhaps at the next Congress, or at the following one, or at the one after that. What would happened if, working on a revision, one knew that various names were going to be affected? Would a journal accept the work for publication? This was going to create a lot of uncertainty for taxonomic research, that research which could, in the end, give a new understanding of the organisms concerned.

Mabberley entirely agreed with Pitt. However, protection of species NCU lists would come too late for next July, when Pitt intended to put forward his kind of UDI [unilateral declaration of independence]. The change accepted that same morning, to increase the range of names that could be conserved at specific level, met Pitt’s
requirements more readily, without requiring any further changes to the Code. Others who had an interest in having names fixed, whether they be for industrial or horticultural plants, should similarly be able to submit substantial lists to the existing Committees. As everybody knew we was not in favour of NCU and was opposed to introducing new provisions into the Code but mechanisms should be developed for beefing up rapidly Committee responses to those who, like Pitt, wanted to have species names conserved.

Ahti, as author of one of the species NCU lists, felt that it was most important to make rough, not necessarily complete lists with the names of those taxa that were presently recognized. The Pinaceae list by Farjon, for instance, was not complete, as many species of Russian authors had not been included any many names remained untypified. Yet, even though it was in a way provisional, it could stabilize the most important names that everyone wanted to keep unchanged. To give a couple of examples, the name Betula alba L. could be typified by anyone at any time and taken up to displace the name of the common European birch, B. pendula Roth, that had recently supplanted B. verrucosa Ehrh. Nobody had yet done so, but it would be completely correct. In eastern N. America, Rhus typhina L. was a very common shrub, but had recently been renamed R. hirta (L.) Sudw. Such changes could be avoided by drawing up short, rough NCU lists.

Faegri could not understand Taylor. If a group was working on the nomenclature of some major taxon, they did not do it in secret. It was a public, open operation. And if other taxonomists were working on the taxonomy of that same group, how on earth could they avoid collaboration?

Briggs had come to the Section meeting because of her belief that there was, in the botanical Code, something that was imperfect but worth perfecting. The same applied to the NCU principle. In adopting it, one would embark on a process of developing lists and mechanisms for perfecting them, for incorporating changes which would happen as taxonomy progressed. This was a familiar process and one that would work well in the future under the NCU rules as proposed.
McNeill addressed what Taylor had referred to and what, he felt, was of general concern to North American taxonomists in particular, which might have influenced some of the presently committed institutional votes: that the NCU principle might inhibit taxonomic research. This was not in any way true. In that respect this principle differed from other proposals toward stabilization that had been made over the years, such as the standardized lists of names that horticultural bodies, forestry organizations and other groups of users of plant names had produced. These were fixed lists that would push taxonomy into a straightjacket, and which taxonomists had rightly objected to. But they came from professional groups who were of vital importance to plant taxonomy, basically an applied science that was supposed to provide the basic reference tools for an understanding of plant diversity. Defending the freedom of scientific development and the stressing the importance of increased knowledge for the practical purpose of understanding biological diversity was fine, but it did not mean indulging in changes of names solely for nomenclatural reasons. Taxonomic change would inevitably take place as increasing knowledge led to a better understanding of the circumscription of taxa. But, again, this was no reason for continued name changes for purely nomenclatural purposes.

The NCU principle would deal with this problem in a different way. If it should fail, then this might not solely result in a mycological UDI: other users of names might also say "to hell with taxonomists" and proceed with those standardized name lists that were a genuine restraint for science. As a member of the Committee for Spermato phyta, he had not supported any of the extant lists. All were agreed that the Section was not in a position to consider any of them, with the probable exception of the family name list. But there were areas where the spade-work had been done, where lists existed which, like that for Penicillium and Aspergillus, were ready to go ahead. Frankly, if the Section did not lend support the NCU principle, this would bring great discredit on its function as the regulatory body for botanical nomenclature.

Brummitt had been slightly misquoted by Johnson. He had not said "one should get the taxonomy right" (there was indeed no "right
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taxonomy"), he had said: "one should get the taxonomy sorted out". [Laughter.] As Friis had pointed out, there were vast groups where the taxonomy was still in the 18th Century. One had no idea what effect NCU lists would have in such cases.

McNeill retorted that Johnson’s point was of course perfectly correct. Taxonomy had been done when there was a species name one wanted to preserve in order to apply it to a particular circumscription. This did not mean that every conceivable synonym had been looked for, which was not a matter of taxonomy. Listing of NCU meant looking at the names as they were used, typifying them accordingly, stabilizing them, and going on from there.

Barrie had walked into the Nomenclatural Stability Meeting at Kew mildly sceptical about the NCU concept but thinking that, if it could be worked out, it might become a useful tool for working taxonomists and reduce their workload. Over the week, however, he had become convinced that NCU, while its goals were laudable, would not achieve this. One reason was that, for reasons Brummitt had so eloquently given, it was almost impossible to vet the lists accurately to the point where one could be confident that it comprised just the names one wished to protect. True, applied workers wanted taxonomists to come up with a list of names for them to use, but it had also become clear at the Kew Meeting that most of them were unable to make the difference between nomenclatural and taxonomic name changes. Compiling such lists together might therefore, in the end, be a disservice to taxonomy. Others would become even more irritated seeing taxonomists change names they supposedly recognized on lists enshrined in the Code. Some other proposals accepted by the Section, especially those liberalizing conservation and prescribe certain disruptive publications, or hopefully to be accepted, as the generalization of Art. 69, would together address almost all of the problems that NCU was supposed to resolve and would do so in a more timely and efficient fashion. When problems came up and were recognized, they could now be solved more quickly and in a way that was easy to recognize and understand. With NCU one would have a huge number of listed names, the validity of which would have to be taken on faith. No one could assert with any confidence that a list of 35,000 generic names was all right, because
no one was capable of evaluating a list of that size. That part corresponding to one's area of expertise might be fine, but one would still have to accept on faith that the rest was fine as well. He knew from discussions with various botanists, both in the U.S. and Europe, that they were unhappy with the nomenclature listed for their groups but were unwilling to suggest improvements because it would take them three or six or twelve months of work that they could not spare from their research time — so they just disapproved. If this was the common situation, how could one ever hope to achieve a list in which one might confide? Unless and until it had been demonstrated that high-quality lists could be generated, NCU did not belong in the Code.

Forero, in answer to some of the previous comments, pointed out that the mail vote showed consistent opposition to NCU, particularly from Western hemisphere botanists, but also from Europeans and others. It had been inferred that, perhaps, many had not looked carefully at the proposals. He could not accept that. Many botanists in the United States and elsewhere had considered the proposals carefully and had concluded that they could not be accepted now. He agreed with most of what Barrie had just said. If botanists were to actually try to have the correct names listed, it would take months of the research time that they would allegedly save. No one was more aware of funding problems than were North Americans, yet they were opposed to the proposals as presently worded. The Section should not rush into a decision. It would be very dangerous to put provisions of this kind into the Code at this point, particularly in view of the strong opposition in several parts of the world. Also, the splitting of the proposals into groups was hard to accept. The Section was a small group, whereas the proposals as submitted had been looked at by hundreds of botanists. It was not appropriate for the Section now to look at the proposals in a different way. This was dangerous, and would be difficult to justify back at one's home institution.

Henderson took the point, repeatedly expressed, that the NCU proposals would not stop name changes and stabilize names against taxonomic changes. Protecting names against newly found, earlier names seemed to be the main reason behind the NCU proposals.
What worried him, and others alike, was the tremendous amount of work involved in the production of these lists. Once they had been produced and accepted, they might check name changes, but they had to be produced in the first place.

Hawksworth commented that Barrie’s point about users not making a difference between nomenclatural and taxonomic name changes might be valid in some cases but was not in others. He had dined the night before with a representative of Ciba-Geigy, Japan, who showed keen interest in getting the correct taxonomy, was quite happy with names changes that might improve predictions of where certain enzymes might be found. Scientists did make the difference between this kind of name changes, which they would accept, and those made for quasi-legalistic and historical reasons. Nomenclature in other branches of science had taken a different route. There was an international, large-scale body that had ruled on the terminology for chemical compounds, and another, on electronic units. Experience at the IMI had shown that taxonomic work, when linked in with major international initiatives, was more likely to get support. Someone monographing a family and in the same time generating a list for NCU might find it easier to get funded.

Johnson, supporting Hawksworth’s argument, noted that he and others had often had to explain name changes to non-taxonomists, and of course there were those who objected to any name change, whatever the reason. If told that a name change was necessary for reasons of nomenclature they wouldn’t understand, but if one could explain that there were scientific reasons behind the change, they would in general be more sympathetic. Apologies were due to Brummitt if he felt he had been slightly misquoted, but this had not affected the argument.

Taylor was sure almost all were agreed that the names of economically important plants and other organisms must be stabilized. Taxonomists were remiss if they did not. But under the NCU proposals the problems were to be attacked at the level of a given rank, so that all other, poorly understood groups at that rank had to be included. That was going to take away time from work on the more serious problems of economically important taxa, problems that could be
dealt with by applying the options to conserve these names that were now in the *Code*. Time and efforts should better be devoted to making conservation proposals.

McNeill was glad that Taylor had given him the opening he had been waiting for. He was delighted that the Section had liberalized conservation of species names. But did not address the same kind of questions as the NCU proposals. It did not help Pitt, who ignored the names over which those now in use would have to be conserved, merely knowing which names mycologists wanted to use. Tucked away in European herbaria were countless types of names that were likely to displace any or all of the listed names – but one did not know which ones. The procedures for conservation, as presently practised, were quite different from what Mabberley had suggested they were. There had to be an analysis of the typification of the name to be conserved and of its competitors. It was not possible to just recognize the names one wished to use, essentially giving approval to the work of a specialist committee – which was what NCU would permit. In fact it was not necessary, nor was it desirable, that every name at a given rank be on the list. He personally felt that there were far too many names on the NCU lists he had seen. It was not essential that the name of a genus of two or three species from, say, Canada or Mongolia be listed, even if it was in current use, unless one would really care if it were to change. What was important was to make sure that all names which it would be very undesirable to see changed were on the list, the ones widely referred to in textbooks and in economic works. Of course, taxonomy must be done and one had to know which species one wished to recognize and which name was widely used for it.

Zijlstra was afraid of adopting the principle while the list (the generic NCU list) was still in such a rough state. The NCU idea was good, but it was too early to accept it. There was the alternative of either first have the principle accepted and then prepare lists, or first prepare lists and then add the principle. Adoption of the principle had now been proposed. There was also a list, which could not however be accepted at present. For many plant groups, it was clear which names should be on the list, and these were on the published list, but in many other groups the selection that had been made would cause endless confusion. Since *ING* had been published in
1979, many corrections to it had been made and more correcting was needed, yet many people treated the ING books as though they contained the ultimate truth. The same would happen again with the NCU list, which was in a much better shape than the original ING volumes. Accepting the principle now, when the NCU list was still rough, would encourage such unwarranted conclusions.

Pitt fully appreciated the obsession of nomenclaturalists with correctness, having done enough nomenclature himself. In the minds of most of those present, a name could only be established and kept if it was fully correct. An NCU list was designed specifically to bypass that requirement. The names in NCU lists were those which, as the experts in the field had agreed, were the most commonly used in the world at the present time, and the ones they wanted to continue to use. Once a name was so listed, its typification was established so that it correctly reflected usage, and the date of valid publication was verified. All this had nothing to do with nomenclatural correctness.

McNeill added that it did not matter if an earlier publication could be found once a name had been agreed upon. It would have been perfectly reasonable to accept the references given in ING, since they were technically accurate. However, to make everyone happy, some revision of these references had been permitted for NCU purposes. Once something else had been accepted, there was no reason to insist for going back to the earliest publication.

Faegri was astonished at the complete negativity which had emanated from some of the previous pronouncements, e.g. Zijlstra's. She had claimed it was too early for accepting the proposals, but what was her positive alternative? Send back the whole matter to the NCU Committee? One could not expect that Committee to rework it over and over again. Either an alternative was shown up, or the argument that the idea was premature really meant that one was against it. If that were true, it would be more honest to say so.

Zijlstra's alternative was to declare that the idea was a good one and to make a commitment, for taxonomists to stop digging out old publications, trying to find names that had never been in use, and searching through old herbaria for specimens not previously recognized as types but which as such would cause the displacement of
familiar names. Meanwhile one could start working on the NCU lists and have the principle accepted in six or twelve years.

Stearn had spent at least half of his life dealing with practical people in the realm of horticulture. They always asked the question: "Why do you botanists keep changing the names that we use?" When this was explained to them on scientific grounds, they took the point. They did not take the point that the new name had been published in an earlier book than the name they had used. One must not overlook the extraordinary stabilizing influence of some books that existed. In the field of trees and shrubs, Rehder's *Manual* and his *Bibliography* had formed the basis of trade lists etc. for nearly 50 years, and when one checked their data one found that they were nearly always correct. He was in favour of adopting the NCU principle, but then get on with preparing lists for particular groups of economic importance, without going further. All of the basic information needed for this kind of lists was already available, unless one were going to nit-pick over typification. Incidentally, most of the vast number of his own typifications had preserved the name in current use.

Dorr, while personally opposed to NCU, pointed out that one of the reasons Rehder's *Manual* had been so stabilizing was the huge amount of scholarship invested in preparing it, while what seemed to be noticeably lacking from most of the NCU lists was the scholarship.

McNeill reminded the Section that what it was looking at was the principle, not at the specific lists, on some aspects of which all appeared to be unhappy.

Barrie disagreed. It was not merely the principle of whether or not to have Names in Current Use that was being voted on, not just the idea; it was far more than that. The vote would be on the mechanism for putting NCU into the *Code*. With all due respect to those who had spent an extreme amount of time and effort putting these proposals together, he could not support them. Once the mechanism would be in the *Code*, the Section would be asked to authorize a bureaucracy to maintain it, a Standing Committee to deal with these lists.
McNeill objected that the Section was not, at this point, considering a Standing Committee. It was really considering the basic Prop. A, which would require any list to come to a future Botanical Congress, on the recommendation of the General Committee.

Greuter wanted, before the vote was taken, to respond to a few questions that had been raised but had not yet been answered by others. First, this was to be a vote on the principle, not yet on introducing the concrete mechanism for NCU protection into the Code. The package of proposals under discussion included no such mechanism. It spelled out what NCU lists were, defining them and giving the rationale behind them. It went on recommending (or at the final stage, ruling) that no name changes adverse to stability be effected pending study (or after eventual recommendation) of proposed lists. This would take care of Zijlstra’s concern, that there should be something in the Code to discourage people from meddling with old literature and bringing up the sleeping dogs pending work on drawing up lists, and he could not understand why she opposed it. The first three proposals would not bring in any lists, but would authorize the work on such lists under the Code and would protect such work against brigands who might lurk on the roadside.

Second, the point of workload and bureaucracy had been raised. In reality, nothing in what was being proposed, neither in the first, general package nor in those that might follow, had to do with bureaucracy. One new Committee was requested, when eight were already in existence, and it was known that there would be enough persons willing to serve on it if it was authorized. One more Committee was not a bureaucracy! As to the workload, the concern that all of the names in a given rank would have to be done in a single go was not justified. At the generic level, an attempt had been made to provide a complete list of names for all groups treated as plants. The reason was that, for names of genera and higher-ranking taxa, homonymy operated throughout the plant kingdom(s). It would have been unfair to try and stabilize, say, fungal generic names, only to discover later that an earlier homonym of a protected fungal name was in current use for a genus of mosses. True, generic names of fossil plants were not now included in the published NCU-3 list, but they had been considered at the initial stage so as to avoid the
risk of killing any legitimate name of a fossil genus by proposing for protection a later homonym in current use for an extant plant genus. At other ranks, species for instance, there was no such constraint. For binary names homonymy operated within a genus, and it was therefore possible – and indeed desirable – to produce NCU lists by genera or, when the task was manageable, families, as had already been done. Efforts should concentrate on groups of plants of economic importance and on those on which research was going on and experts were available. Specialists could easily tell which names were in current use, and they had a vested interest in drawing up NCU lists. What they disliked was the prospect of doing all that work, which they had to do anyhow, knowing that it would have to be redone after them, over and over again. No wonder Brummitt favoured stabilization for his "Species Plantarum" names, and these treatments would obviously be prime candidates to serve as a basis for NCU lists, although it was hardly appropriate to grant them the right of exclusivity (as even Brummitt might agree, if only for the reason that the "Species Plantarum Project" was not covering the whole plant kingdom(s) but only the phanerogams, for the time being). The nomenclatural work was a labour that would have to be done if it had not yet been done. As soon as it had been completed for a group, an NCU list could be drawn up quite easily, so that the same work need not in future be done anew. Eliminating waste of labour was one of the purposes of the NCU principle. There was also no time pressure involved in the production of such lists, which would arise as the opportunities and the need presented themselves.

Third, the General Committee which represented the Section between Congresses, and also the IAPT, were part of the International Union of Biological Sciences, already mentioned by Hawksworth. One of the major concerns of this Union was to prevent the provisions of the biological Codes from drifting farther apart than they already had. It had been recognized that it was not possible to unify biological nomenclature this stage. Lists of names in current use had now been decided and would become a reality in zoology: the International Commission on Zoological Nomenclature had, in 1990, opted for introducing the principle of NCU lists in zoological nomenclature. The principle here proposed was, among other things, a means not to let the Codes diverge even further.
Last, having had the pleasure and privilege to work in the NCU Committee, he had come to know the roots of this novel effort. There was a general discontent with the present situation, both within and outside the Committee. The ideas and proposals of the NCU Committee had been publicized much more widely than was usually the case, on the justified assumption that the issue was of so fundamental importance that only after having been adequately discussed should they be put into practice. This publicity had raised fears, particularly within part of the present audience, but also the widespread expectation that the reasons for discontent among biologists could and would now be taken care of. If not even the basic NCU principle was to be approved, many who had placed their confidence in this meeting would be disappointed. (This was not a question of approving the extant lists, perhaps not mature and certainly, as the present experience had told, in need of a good quarantine period in a published, generally accessible form that everyone could check and criticize.) The Nomenclature Section was well known for valuing its autonomy very highly and for its dislike of being bullied by threats from the world outside. It was probably counter-productive to say so, but as a Rapporteur he felt obliged to point out that decisions of the Section had to be ratified by the International Botanical Congress as a whole: about 3,500 botanists, most of them non-taxonomists, while the Section members were only about 90. This had up to now been a mere formality, but let the thwarted expectations burst out in public and there might be a real danger of the decisions of this Section – all of them, not merely those concerning NCU – failing to be recognised by the Congress. Rather than to conclude on that apocalyptic note, Greuter did so by thanking all those who had, with enormous effort and much skill, energy, perseverance and abnegation, worked on the NCU lists. These lists had a twofold value, being useful in themselves and demonstrating that the operation was feasible.

Clemants asked whether Gen. Prop. C, action on which had been postponed the day before, was to be included in the present package or voted on independently.
Burdet explained that it would be taken up after the current vote. The first vote would be on Prop. A. [Following a protest from Dorr:] The request had been to vote packagewise, but the Section's decision had been to discuss, not to vote, package by package. Each proposal would be acted on individually.

Prop. A was rejected by a card vote (54.98 % in favour, 231 : 190). Thereupon, Greuter withdrew all other NCU proposals except Gen. Prop. C, which was up for consideration [see Session I].

Prop. B (50 : 129 : 1 : 0 : 6) had been withdrawn.

Prop. C (62 : 107 : 4 : 0 : 7) had been withdrawn.

Prop. D (58 : 113 : 3 : 0 : 6) had been withdrawn.

Prop. E (59 : 112 : 3 : 0 : 6) had been withdrawn.

Prop. F (63 : 106 : 3 : 0 : 6) had been withdrawn.

Prop. G (66 : 106 : 3 : 0 : 6) had been withdrawn.

Prop. H (65 : 103 : 4 : 0 : 8) had been withdrawn.


Demoulin observed that this and the related proposals were provisions for integrating what was presently in Art. 13 concerning sanctioned names into a new Art. 15. He did not think there should be a new Article just for sanctioned names. If there was something new, then fine; but if it was just an editorial shift he would be opposed.

Greuter frowned at this comment from a member of the Committee for Fungi and Lichens. Demoulin had considered this proposal as a Committee member, and there had been a favourable Committee vote on these proposals as reported in the Rapporteurs' published comments. The proposals certainly did include new matter, insofar as they clarified a number of points on which the current sanctioning provisions were not explicit but which were current practice in sanctioning. The matter was perhaps too technical for discussion by the full audience, but had been considered thoroughly by mycologists. It concerned chiefly the ranks in which combinations, as opposed to single-element names, were used.
Demoulin had of course worked on these issues in the frame of the Committee, but felt unable to rethink, in two or three minutes, what was then considered important. He could not immediately tell that such point was editorial and such other point involved change of substance. [Rapporteur's interjection: "you are getting old!"]

Gams noted that the Committee for Fungi and Lichens had considered the proposals for their content, not with regard to their position in the Code, and had favoured them simply as they were. He had no preference one way or the other for having a new article, perhaps in anticipation of future protection for names in current use.

Jørgensen was also a member of this Committee and he, too, was getting old – so he had brought his notes. They told him that the proposals were an improvement and a simplification. Since a most of the Art. 15 proposals had failed, one might be uncertain whether the matter should be placed in that Article or somewhere else, but this was an editorial question. The contents were clear and all right.

Burdet observed that all eight remaining proposals started with the phrase "Add a new Article 15bis...". The Section should act on them as they were, and eventually an amendment regarding their placement elsewhere than in a new Article could be considered.

Greuter admonished the Section not to confuse Editorial Committee business with the substance of the proposals. The Committee on Fungi and Lichens had proposed a reorganization of the rules on sanctioning and their completion in view of what they were intended to mean. Those who had been in Sydney might recall that the Section there was confronted with the very interesting and quite novel idea of sanctioning fungal names rather than maintaining the later starting point for fungal nomenclature. The Section knew so little about the matter, and cared so little, that it just followed the advice of fungal specialists and voted "Yes". After a while it had turned out that what had been proposed was not quite what mycologists had intended to propose: or perhaps they had not dared to propose it all because they felt that the matter would then become too complicated for the Section to vote. Now they wanted to have in the Code what they really needed, and to have it in one place so that...
they might use it rationally. With but little effort, the Editorial Commit-


Demoulin supported the substance of the proposals, his question
merely concerned their position in a new Art. 15. Perhaps they
could be left in Art. 13, or, better still, in Art. 14.

Greuter wondered whether Demoulin intended to withdraw from
the Editorial Committee. If not, he could raise the point there [as
doubtless he would].

Zijlstra wondered whether the members of the Committee on Fungi
and Lichens who were present might be willing to work on this issue
overnight, so that the Section could get a better impression of what
was left of the proposal. [Her suggestion was not taken up.]

Jeffrey wondered what might be meant by "recognizing a name".
One could adopt a name or recognize a taxon.

Greuter assumed [correctly] that this came straight out of the pres-
ent wording.

McNeill reiterated the point that only the content of the proposals
mattered, which involved modification of provisions now in Art. 14.
The Editorial Committee would put the new provisions where it
deemed best, and there would be mycological representation on
that Committee. The fact that "Add an Article 15bis" prefaced a
proposal did not bind the Editorial Committee.

Prop. I was accepted.


Prop. K (53 : 89 : 27 : 0 : 8) was accepted.

Prop. L (67 : 99 : 6 : 0 : 7) had been withdrawn.

Prop. M (58 : 86 : 27 : 0 : 8) was accepted.

Prop. N (53 : 111 : 5 : 0 : 7) had been withdrawn.

Prop. O (60 : 95 : 13 : 0 : 8) was accepted.

Prop. P (68 : 100 : 4 : 0 : 6) had been withdrawn.

Prop. Q (56 : 87 : 27 : 0 : 7) was accepted.
Prop. R (62: 108: 2: 0: 7) had been withdrawn.

Prop. S (55: 112: 4: 0: 7) had been withdrawn.


Jeffrey suggested that the Editorial Committee reword this slightly to avoid giving the impression that a rank had an epithet.

Prop. T was accepted.

Prop. U (69: 100: 2: 0: 7) had been withdrawn.

Prop. V (71: 89: 10: 0: 7) was accepted.

Prop. W (61: 109: 4: 0: 7) had been withdrawn.

Prop. X (39: 101: 34: 0: 6) had been withdrawn.

Prop. Y (46: 93: 33: 0: 5) was accepted.

Prop. Z (45: 85: 48: 0: 1) was accepted.

Recommendation 15bisA (new)

Prop. A (70: 103: 2: 0: 6) had been withdrawn.

Article 16


McNeill noted that this was a desirable corollary to Art. 3 Prop. C that had been accepted earlier on. It indicated what should be done in those few cases in which phylum and divisio both appeared.

Prop. A was accepted.

Article 18

Prop. A (134: 22: 26: 1: 0) was accepted.

[The following item was brought up and acted upon belatedly, at the end of the Seventh Session.]

Greuter (since no one yet claimed to be hungry) moved a New Proposal for the Section to consider. This was one of the proposals that had been submitted to the Section for consideration in a duplicated
memorandum by Hoogland & Reveal. It suggested a welcome improvement by correcting what was an obvious omission in the present *Code*. It would add the following sentence to Art. 18.2: "Names published in the same context with their rank denoted by the term ‘suborder’ (*subordo*) instead of subfamily are treated as having been published as names of subfamilies". Cross-reference to this, in the form of a new Note, was to be added under Art. 19. The present provision of Art. 18.2, that names intended as names of families but designated as orders were to be treated as family names, was frequently used and very important for the nomenclature of families. At present, the *Code* lacked a parallel provision for names of subfamilies designated as suborders. Taking this literally one might argue, in relevant cases, that families were subdivided into suborders, contrary to Art. 3, names for both being invalid under Art. 33.4. There was a strong case for eliminating this disparity of treatment and remedy to this obvious omission. The motion was seconded.

Nicolson was glad that this had been brought forward. He had in a way been commissioned to do so himself, but after his first attempt had failed he had felt so badly burnt that he had left Hoogland & Reveal’s Memorandum in his room. He entirely supported the proposal, which he felt was uncontroversial.

The motion was carried, and Hoogland & Reveal’s New Proposal was thereby accepted.

**Article 19**

**Prop. A** (33 : 140 : 5 : 0 : 2) was ruled as rejected.


Greuter hated to explain his own proposal, but it might not be immediately obvious what it implied. It addressed the rules operating at the ranks above genus, where names were not combinations but comprised a single element. The name of a subfamily consisted of a root, taken from the genitive of the generic name, plus the termination -oideae. It was not a combination of a family name with a subfamily name. He had considered, with others, to suggest that they
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become binary combinations, but there were cogent reasons against this. At Sydney autonyms had become priorable names, but at the higher ranks there was virtually no way of establishing when such a priorable autonym had been created. It happened the first time a valid subfamily or tribal name was used under a given, legitimate family name, which use then automatically established the autonym corresponding to that family name. Tribal and subfamily names had almost invariably been created within a specified family, but often that family was designated by a name that was not validly published, a descriptive name like "Synanthereae" for instance, or one that was illegitimate being based on an illegitimate generic name. The subordinate names could then at any time be placed under a legitimate name of a family and establish an autonym. There were other problems associated with the question of autonyms at those higher ranks, which he had discussed in some detail when making his proposal. The proposal would maintain the mandatory use of names of the same form as autonyms for the subdivision of a family that included the type of the correct name of that family, only such names would not be autonyms any longer and could not be automatically established. They might even be followed by an author citation, if one so wished, but that was a rather unusual practice after names at those ranks. The proposal would not introduce a change with respect to what one was used to, but would solve the problems that presently existed.

Brummitt, as ex-Secretary of a Committee on Autonyms, had looked at the proposal very closely and, as one might expect, had found it to be perfectly logical and correct.

Prop. B was accepted.


Henderson disagreed with the Rapporteurs' published comments. His proposal was not aimed at purifying the Code. The important question was whether a wording was correct or incorrect. A binominal could indeed be used, not only in two but three different senses: in the sense of the name itself, in the sense of the taxon concerned, and as the name of an individual plant or specimen. It could certainly be used in two different senses in the same sentence, but one could not switch sense in the particular situation he had ad-
dressed. In the *Code*, a type was always a type of a name, not of a taxon. The intent of producing his proposal had been to provide guidance to the Editorial Committee, and he therefore asked that it be referred to the Editorial Committee. The same applied to Art. 57 Prop. D.

Greuter agreed that the proposal was editorial. But since past Editorial Committees had been knowingly happy with the present wording, the next one might be reluctant to take up this suggestion, if was referred to them, unless there was a strong feeling by the Section that a change was needed. A "yes or no" vote would provide such an indication. A "yes" vote would instruct the Editorial Committee to consider the proposal favourably; if the vote was "no", the Editorial Committee would still be free to consider it – since it was editorial matter – but perhaps less favourably.

Prop. C was rejected.


McNeill invited anyone who knew better to explain why it was important to retain this Article in the *Code*. It seemed redundant to the proposer.

Prop. D was accepted.

**Article 20**

Prop. A (9 : 90 : 16 : 0 : 55) was referred to the Special Committee on Hybrids.

Prop. B (126 : 30 : 18 : 0 : 9).

Prud’homme van Reine had been asked those working on *Flora malesiana* to have Rumphius (ed. Burman) removed from the list. It was a small book published in 1755 in which Burman had used Linnaean taxonomy and nomenclature, and there was no reason why it should be banned.

Friis said that the work had been listed because it had been excluded by Dandy (in Regnum Veg. 51: 7-8. 1967) as a source of generic names. Dandy’s list had no standing under the *Code*, and the present proposal aimed at giving it such status. He could not tell what grounds and consequences the opposition from Leiden against listing that work might have.
Jeffrey seemed to remember that the only part of this work that was relevant was Burman’s key to Rumphius’s work, not the work itself as edited by Burman. The part which the Flora malesiana staff wanted would thus not be outlawed.

Prud’homme van Reine had been told that the rest of the book was pre-Linnaean, and so was excluded anyway.

Stearn emphasized Dandy’s extreme care in discussing every one of these publications. Dandy was very reluctant to publish his reasons, but they were always very good reasons. Stearn did not know the exact details in this particular instance. Perhaps botanists at Leiden knew better, but he would rather accept Dandy’s position because not only had he had a very intimate knowledge of the Code, but also a very fine intellect in the bargain.

Greuter added that Dandy’s list had been used by many as their basic guideline with respect to validity of pre-1775 generic names. It had for many years been a kind of bible for most people working with phanerogams.

Mabberley was sure that botanists at Leiden were aware of the scholarly attitude of Dandy. However, he thought that it would be inappropriate for this Section to overrule the wishes of some users who had explicitly asked that this title be taken out.

Demoulin asked for a vote on the amendment (provided it was seconded) to remove Rumphius (ed. Burman) from the list. He did not know the book, but thought that if there was disagreement, and in the absence of detailed arguments, it might be more prudent not to leave it on the list. It could still be added later.

Prud’homme van Reine formally moved the amendment, and the motion was seconded.

Greuter, referring to Demoulin’s comments, wondered whether it was safer and more stabilizing to leave on the list a title that might perhaps have to go, or to remove it, perhaps to reinsert it later. Both processes, addition and removal, were of equal weight and equal ease or difficulty. What, then, was more consequential? One title too many on the list meant avoid having to consider generic names in that work as being validly published. Removing the title meant introducing names that might be disturbing.
Jørgensen wondered why the Committee had not consulted Leiden, and why Leiden had not drawn attention earlier to this problem. The proposal had been published in *Taxon* the year before.

Friis admitted that the Committee had not consulted Leiden in this case. If they had consulted all botanists that worked on all geographical areas dealt with in all these works, it would not have finished its report on time. The proposal dealt only with generic names, and if Leiden had wanted to use some such names earlier, he could not understand why there had been no conservation proposals.

The motion was defeated.

Hawksworth noted that if App. V Prop. B was to be approved this list would be placed in the new Appendix.

Prop. B was accepted.

**Article 22**


Jeffrey commented on Prop. A and Art. 26 Prop. A, both by Greuter, in the light of his own Art. 32 Prop. I and Art. 63 Prop. D. Greuter's proposals went further in that they sought to make validity of publication independent of typification, whereas his would validate publication of a name adopted in place of an autonym, and make it legitimate, only if the autonym were heterotypic with respect to that name. What would be gained, for example, by according validity of publication to a name such as *Marchantia polymorpha* var. *hispida*, which upon lectotypification of the name *M. polymorpha* must be an obligate synonym of *M. polymorpha* var. *polymorpha*? It was mere nomenclatural clutter we could well do without. It was impossible anyhow to make validity of publication independent of typification. Greuter had not dealt with another situation besides autonymy in which the validity of publication of a name could not be seen from the publication itself. This might arise under Article 42.1 when it was uncertain until one or more names had been lectotypified whether no other name at any rank had previously been published based on the same type. Riding's discussion of the *Tubiphytes obscurus* case (*Taxon* 42: 71-73. 1993) was relevant.
Greuter pointed out that the general issue had already been addressed repeatedly and at some length: the retroactive effect of lectotypification on the nomenclatural status of other names. Should names be allowed to become invalid upon lectotypification of another name? His answer was a clear "no", not only because one needed to know whether or not a name was valid – and many names existed that might potentially invalidate other extant names but had not yet been typified – but also because so much uncertainty surrounded the status of many lectotypifications. Nomenclatural stability would be very adversely affected if one let it happen that, whenever an earlier lectotypification turned up, another name became invalid instead of the one thought not to be validly published under the previously accepted lectotypification. This was not theoretical reasoning, it happened often, even more so when the lectotypification requirements were changed at almost every Congress.

**Prop. A** was accepted.

**Prop. B** (41:130:5:0:2) was withdrawn.

**Prop. C** (4:164:11:0:1) was ruled as rejected.

**Prop. D** (22:117:32:1:3) was rejected.

**Prop. E** (46:19:114:0:0) was referred to the Editorial Committee.

**Article 23**

**Prop. A** (144:21:11:0:3) was accepted.

**Prop. B** (150:17:10:0:2) was accepted.

**Prop. C** (143:23:10:0:3) was accepted.

**Prop. D** (149:17:11:0:3) was accepted.


Jeffrey explained that the intent of Prop. E was to define what was not an epithet consisting of two words.

McNeill was unclear whether Jeffrey was suggesting that the Rapporteurs had misinterpreted the intent of the proposal.
Greuter asked whether the Rapporteurs' suggestion was acceptable as a friendly amendment. This would mean that the initial half of clause (cc) and Ex. 9bis alone would stand.

Stearn, referring to the Linnaean example, pointed out that Linnaeus himself had rejected these names and provided normal specific epithets for them in the second edition of *Species plantarum*.

Friis agreed that Ex. 9ter, which had been found to affect some Cavanilles names, and the second part of clause (cc) could be deleted, as had been proposed by the Rapporteurs. As one could now conserve species names that might be threatened, the example was not as important as it was before.

McNeill did not personally know the situation with Cavanilles, but the suggestion had been that the provision and example, meant to address Forsskål, had undesirable effects in at least one other case.

Greuter noted that these examples were to be voted examples, so that they had the force of law. That was why the point was important. Usually, exclusion or inclusion of examples was at the discretion of the Editorial Committee. He was glad that Friis had taken up the Rapporteurs' suggestion as a friendly amendment. What was up for a vote now was Prop. E as amended in conformity with the Rapporteurs' comments.

**Prop. E, so amended, was accepted.**

**Prop. F (138: 27: 13: 0: 2) was accepted.**

**Prop. G (46: 25: 107: 0: 2) was referred to the Editorial Committee.**

Greuter reminded the Section that an Editorial Committee vote had been given a special meaning by the Rapporteurs in their published comments. He took it that the Section had kept this in mind when voting.

Demoulin asked whether, given the nature of the cases involved, if it was not appropriate to treat these examples as "voted examples", especially those with *Trichomanes* and *Melilotus*.

Greuter agreed. The way in which this had been voted was formally not quite correct. The "Ed. C." vote followed from a suggestion, by
the Rapporteurs, to modify the examples, which should then be accepted as voted examples. If Friis accepted that suggestion, the Section vote could stand as an affirmative vote taken in the light of the Rapporteurs' comments.

Friis confirmed that it had not been his Committee's intention to reject the names *Asplenium dentatum* and *Trifolium indica*, but to delete the words *Trichomanes* and *Melilotus*. This was what the statement that *Asplenium trichomanes dentatum* and *Trifolium melilotus indica* were not validly published was meant to imply, in conformity with traditional usage.

Jeffrey made a plea, related to Prop. G, that those who might feel that the Committee had got things slightly wrong give feedback to the Editorial Committee. Did the examples really reflect established custom? The Committee had tried to make sure that they did, but its members had not expertise in all branches of systematics.


Ahti wondered about the status of new varietal names published in some of the works listed, when they were linked to previously validly published species names. To get rid of them, the proposal should perhaps be changed to read "of validly published infrагeneric names". Also, the second volume of Gilibert was actually published in 1782 not 1781, so that the dates should be cited as "1781-1982".

Stearn suggested that the Editorial Committee reinvestigate the dates of Hill's *British herbal*, which was not published in 1756, but over a period of years.

Friis [misunderstanding Ahti's suggestion] knew there had been good reasons for distinguishing between works outlawed for generic names, and others for species names.

McNeill explained that Ahti wanted names of both species and infraspecific taxa to be covered. Would Friis accept this as a friendly amendment?

Friis asked Ahti whether he knew of any examples of infraspecific names being mentioned in these books.

Ahti was not 100% sure, but Isoviita had checked the Gilibert book and found varietal names. Further items were certainly to be added
to the list later, such as the first volume of *Flora danica*, and some of them would contain varietal names, or names of other infrageneric taxa.

McNeill made sure that "Species and infraspecific taxa" [not infrageneric] was what Ahti meant.  
Ahti's motion to amend Prop. H was seconded and carried.  
Prop. H, so amended, was accepted.

**Article 24**

Prop. A (15 : 84 : 77 : 0 : 0) was rejected.

**Article 25**

Prop. A (3 : 103 : 72 : 0 : 0) was withdrawn.

**Article 26**

McNeill explained that this proposal paralleled Art. 22 Prop. A, just accepted, but dealt with infraspecific rather than infrageneric auto-nyms.  
Nicolson was uneasy about the last sentence. Could the proposer state the meaning of "explicit indication"?

Greuter explained that if, instead of citing the type, one cited "var. typica", this was to be treated as if one had included the type. The type variety, however designated, stood for the typical element of the species.

Prop. A was accepted.

Prop. B (34 : 137 : 6 : 0 : 2) was ruled as rejected.

Prop. C (3 : 161 : 16 : 0 : 1) was ruled as rejected.

**Recommendation 26B**

Prop. A (7 : 95 : 77 : 0 : 1) was withdrawn.
Nomenclature in Yokohama

FIFTH SESSION

Wednesday, 25 August 1993, 9:05 – 12:20

Article 32

Prop. A (52:41:86:0:1) was referred to the Editorial Committee.


Nicolson, at the risk of a kiss of death for everything he supported, underlined the advantage for indexers being relieved of the obligation to track down all the names appropriate for being indexed. This burden should be shifted to the authors of names, and perhaps to their publishers.

Greuter introduced registration as one of the key issues before this Section. It had first been raised six years ago in Berlin, when adoption of the concept was felt to be premature but a Special Committee had been appointed to study it further and to submit new proposals if appropriate. This the Committee had done, coming up with a unanimous solution, even though one of its members, Cronquist, had been an active opponent in Berlin. Cronquist, whose absence was badly felt, had in fact been at the origin of the presently proposed wording. With respect to the Berlin version, it included one major and in the mind of many perhaps crucial change: registration offices would have no authority to decide on the nomenclatural status of names submitted for registration, although they might give an opinion. They might for instance state "type not indicated", but they might not declare that a name was not validly published. Their duty would be to register and date the names submitted to them.

Funk drew attention to the negative mail vote. The major objection, at least at the Smithsonian Institution, was not to the principle but to the question of the date (Prop. D). The proposed solution was unfair to botanists in developing countries, and did not address the problems of electronic communication. Could Prop. D be dealt with first?
Greuter saw the basic principle and the question of dating as two independent issues, and advised that the latter should be debated once the decision of principle had been taken. He further reported a favourable vote (10:2) by the Committee for Fossil Plants.

Dorr endorsed Funk's concerns. Prop. B-D had been qualified as "a coherent package" in the Rapporteurs' published comments. It seemed appropriate to discuss the consequences of accepting Prop. B before discussing Prop. C and D separately.

Faegri provided the historical background of the registration proposals. Registration was not in itself a new idea. It had already been discussed at the Amsterdam Congress in 1935, but the conclusions reached there were never published due to the Second World War. Extensive debates on effective publication at Sydney in 1981, and consequent establishment of a Special Committee on Effective Publication, had not yielded concrete results. Recent developments of printing technology had made reckless publication of names ("nomenclatural terrorism", as David Ride had called it) more and more easy, so it was natural that the initial efforts had been directed toward improving the rules on effective publication. But this proved to be unworkable. Defining effective publication had become almost impossible, and what technological changes one would face by the year 2000 was unpredictable. Instead of trying to patch up Art. 29-31, a new Special Committee had therefore decided to pursue the idea of tightening the rules on valid publication by introducing registration. The Committee for Registration, appointed in 1986, had come up with well considered and well worded proposals, which fell victim to the uncoordinated presentation of other, partly similar and partly competing proposals, mainly relating to Art. 29-31. As a result, all such proposals were defeated and referred back to a new Special Committee on Registration, with two Subcommittees which did not however function independently and had been tacitly merged. Their respective secretaries, Gunn and Hnatiuk, had done an excellent job, but neither could unfortunately be present. The Committee had found it impossible to come up with detailed procedural rules, but had chosen to present a concise statement of the basic principle. As the Rapporteur had just stated, this principle did deliberately exclude any possibility of censorship.
Also, the present way in which the Code operated was left untouched. The provisions on effective publication, while still unsatisfactory, would lose much of their importance once registration had been implemented. Registration offices as here proposed were not limited in number or location but could be recognized in as many places as was practicable. Nor was there any constraint on the way in which the registration system was to be implemented. The set-up of a machinery presupposed that its scope be known. Postponing a decision on the principle until the machinery would be in place was equivalent to entering a vicious circle. In some respects, the principle as now worded looked immature; but then, did the type method spring to light fully mature like a Greek goddess out of Zeus's head? Or was the Code itself mature when it got started, in 1867 and 1905? All new ideas had to start in a relatively simple way and had then to develop. The important point was that the new proposed provisions must not include any obstacle to further development. In view of all this, it was appropriate to rediscuss the proposed date, 1 Jan 1995, which was completely arbitrary. There should certainly be a date, but it might be wiser to place it subsequent to the next International Botanical Congress. However, acceptance of the principle now was absolutely essential.

Brummitt objected to the present weakness of the proposal. Without details, it was impossible to consider it. He was unwilling to support something of which he had no idea how it would operate. Prop. C gave some clue as to what was intended, but what did it mean? Was the statement "comb. nov.", in a printed text, sufficient to "clearly identify" a name to be registered? Many might have thought that more than that was required. Having published a paper, did an author have to wait for the reprints in order to submit them for registration? Or would the publisher submit a printed copy directly? Unless this was clear, both or neither might do it. Was a covering letter required with the submission? And if so, what should it specify? Since the Committee had deliberately opted for registration of names rather than publications, presumably each name had to be specified in some way. These were details of major importance for the registering centres. The compilers of the Index kewensis, who already had problems with the Code as it stood, felt that these problems would immediately be doubled. An important technicality was
whether Xerox copies of individual pages out of a larger publication would be acceptable. Perhaps his main objection was that, if under the present Code one had claimed that two copies of printed matter were sufficient for effective publication, under the registration principle, one copy would be enough. Typing a validating text into a word processor, printing it out and faxing it to the registration centre would be sufficient to validate any new name within, say, ten minutes. With the present revolution in communication technology, e-mail would be the next thing.

Faegri retorted that one could always find arguments if one looked for them, but Brummitt had perhaps dug up the most improbable ones. Prop. C specified that "printed matter" was to be sent, and that the names to be registered had to be "clearly identified". There was no need to specify the colour of the pencil used to identify these names. People working at the registration centres would not be so stupid as not to understand. Besides, Brummitt's remarks rested on a complete misconception of registration. This was a purely mechanical process, and indexing was a separate issue that could build upon registration. Since registration, if approved, would be a requirement of valid publication, authors and publishers would no doubt be keen to comply without being cajoled into it. An important point had not yet been made: it was not specified who had to submit a name for registration. It could be the author, or the publisher, or, failing both, anyone who wanted to use the name. Multiple submissions of the same name would do no harm since only the first would matter.

Kirk, as the only indexer present, contradicted Brummitt's statement that registration would have an adverse effect on the functioning of the indexing centres. For the Index of fungi, registration would have only advantages.

Chaloner supported the proposal on behalf of the Committee for Fossil Plants, representing perhaps a very marginal group of taxonomic botanists. The Committee vote now stood 11:2, due to a late return from Vladivostock. Palaeobotanists had a special problem since many of their taxa were published in papers on the outer fringe of botanical literature, e.g. in obscure bulletins of geological surveys, absent from even the major botanical libraries. Registration
had therefore particular significance for palaeobotanists, who would warmly embrace this concept. He could well see the problems of detail mentioned by Brummitt, but with Faegri, he agreed that these could be tidied up later. If the principle were not to be adopted now, it would never get on the way.

Jeffrey, while not unsympathetic to the idea of the registration of names or publications, was weary of the present proposals. Incorporating the principle of registration might be fine, but not as prerequisite for valid publication from a specified date. Details could not yet be worked out in such a way that it would be clear whether or not a name was validly published. Speaking on Prop. D, when would the botanical community be informed of the existence of a registered name? At present, looking up the protologue would give the answer; if registration offices were expected to publish lists at specified intervals, they would need funding—which might be discontinued any time. The belated palaeobotanical response from Vladivostock illustrated the problem of postal delays, with letters now taking up to six months to arrive at destination. Could one in fairness insist on the requirement of "printed matter" in view of present progress in electronic mail and electronic publication? By the time of the next Congress, the options for publication in the 21st century would be much clearer. Incorporating the principle now into the Code was fine, but without any date and without any commitment of linking it to valid publication. True, bacteriologists had been successful with their registering procedure, but they lived in a small world. Botany was more complex and would require much more time.

Dorr shared Brummitt’s misgivings, which came from one of the major indexing institutions. The mechanisms were very important. If a registration committee failed in its function, we would be in serious trouble. He urged some clarification of the ways in which the principle was to be implemented.

Demoulin felt that some of Brummitt’s comments were not relevant, since registration was proposed to operate under Art. 32 and did not concern the issue of effective publication. Others of the supposed problems would be easily solved. Journals in which many new names were published would presumably be sent immediately and
directly to the registration office, and would specify this on their cover, so that authors would not have to bother. Problems with the requirement for printed matter submission might arise in the context of names published in books, perhaps expensive ones, where the submission of photocopies should be acceptable. Some amendments of detail would presumably be required. However, the principle in itself was necessary.

Ahti moved to change the year 1995, stated in the proposal, to 2000. The date should not be left open. This was accepted as a friendly amendment by Faegri.

Henderson spoke in favour of the proposal. The principle of registering plant names was not new. Registration authorities existed for cultivated plants. That mechanism worked and could be made to apply for registration under the botanical Code.

Clemants opposed the principle to be written into the Code as long as no mechanism was explicit.

Jørgensen asked for a guarantee, from the side of the IAPT, that the system would be made to function. By accepting this proposal, one would be stuck with registration. Was the economic background secure?

Briggs supported this as an important principle, of which the mechanisms would have to be worked out. But what were its teeth? What would be the status of a name that was not registered but otherwise validly published?

Barrie asked the same question. Taking for granted that registration offices could not rule on validity, it was nevertheless inappropriate to dissociate the registration process from the requirements for valid publication of a name. This was confirmed by McNeill. As the proposal now stood, registration would be a requirement for valid publication, starting with the year 2000.

Hawksworth, as director of one of the indexing centres, knew that it would be much easier to justify index production, an ongoing major investment of institutional resources, if it had international recognition. Financially speaking, the Index of fungi was a disaster, if one accounted for the full cost of staff involved. Just as the palaeobotanists, mycological indexers had to track down a body of literature
that was not represented in major botanical libraries: medical, biochemical, microbiological, and molecular biology publications were increasingly including fungal nomenclature. Looking up such references in other libraries was time-consuming and labour-intensive. The International Mycological Institute would consider making registration information, including dates, available to the botanical community by means of electronic communication networks almost immediately, which would be a great benefit for workers active in the mycological field. To ensure speedy registration at an early date, registration offices should be set up in individual countries whenever possible. Once the principle was approved, efforts could be undertaken to that effect, and the necessary funding could be secured.

Forero wanted it put on record that registration was unfair to botanists working in developing countries, who had not been invited to participate in the deliberations of the Registration Committee. Yet, tropical countries housed most of the biological diversity of the world and had large botanical communities mostly publishing within their own countries. To them, registration would cause serious problems. Many large U.S. institutions, such as New York, Missouri and the Smithsonian, being sympathetic to the needs of botanists in the tropics did, for the most part, oppose registration.

Faegri retorted that he had spent half a year trying to get nominations for Committee membership from academies etc. in tropical countries – and had never got a single reply.

Demoulin, contrary to Forero, felt that registration was in the very interest of botanists in developing countries. It would provide them easy and direct access to the information they needed, rather than forcing them to search through the world literature, which for them would be an impossibility.

Brummitt accepted the need for something like this in perhaps twenty years. But how should it operate now? If authors had just to send in a publication, without a covering letter specifying the names to be registered, this was no more than registration of publications. If lists of names were required, and an editor left out one name by mistake, would then the registering centre be allowed to register it?
And would indeed, as he had asked earlier, production of a single copy be sufficient for the purposes of valid publication? If so, registration would open the pathway for the grossest misuse. Photostats of proof sheets could be used to achieve valid publication.

McNeill patiently explained that the requirement for effective publication was not in any way affected by the new proposals. Prior to submission for registration, all present conditions for valid publication (including of course effective publication) would have to be fulfilled. To judge whether they had been fulfilled was not, however, the responsibility of a registering centre. The proposal was in fact very clear in this respect. Registration was an additional hurdle, but did not alter the prior hurdles on the way toward valid publication of a name.

Barrie, while agreeing with Forero in deploiring the absence of tropical botanists from the Registration Committee, also shared Demoulin’s belief that, in the long run, registration would be in their best interest. Especially now that library budgets were being cut, it would continue to bring their publications to the notice of botanists in the industrialized world. While sympathetic to the principle, he thought it unwise to introduce it into the Code as long as concrete data on, e.g., the location of registration sites, the sources of financial support, etc. were not available.

Dorr worried over the question of dates. Would the date of effective publication (Art. 30) and the date of valid publication (Art. 32) now differ? And which would take precedence? He was sympathetic to the principle but uneasy about the mechanism. Forthcoming changes in the way one published might, before long, make much of this argument moot.

Stearn favoured the principle of registration but wanted its implementation delayed into the far distant future in view of the practical difficulties. He had himself written the original Code for the nomenclature of cultivated plants, where the idea of registration authorities for different groups of cultivated plants was introduced. The difficulty was to coordinate the action of those different registration authorities. How did this presently function?
Zijlstra recalled that the registration idea had originated from dissatisfaction with the rules on effective publication. Why were the proposals on registration now placed under Art. 32 rather than Art. 29? Placing them in Art. 29 would also solve the date problem mentioned by Dorr.

Leslie, in reply to Stearn, explained that cultivar registration depended largely on the co-operation of the persons involved. In some groups, e.g. orchids, it had a very high coverage of perhaps 95%, but in other groups breeders would not co-operate and the system was failing.

Trehane added that there was no compulsion to register cultivar names. In addition, international registration authorities lacked infrastructure and funds - which the IAPT should note.

Faegri explained that, while publication of lists of registered names was indeed envisaged, an enquiry addressed to the registration centre would always be possible in the meantime to find out whether new names had been registered, say, in a given group. Demoulin's point about sending it in whole books was of course valid and would have to be taken care of. Practical solutions could always be found when problems presented themselves. As to Dorr's point, yes, names would have two dates that had nothing to do with each other: the date of effective publication and the date of registration. As to the date of valid publication, it was yet another question. Registration would become a condition for validity of names, but was not an element of validity as presently defined. If the Section agreed to the principle, one would, among others, contact the powerful associations of science editors who could provide most valuable support.

Barrie had understood that the proposals would result in priority of a name starting with its date of registration, not of its publication.

Greuter explained that the date of a name was and would still be the date of its valid publication, which under the registration principle would, however, be coincident with the date of registration. Prop. D which, as the Rapporteurs had written, was to a degree independent of the others, provided a definition of the date of registration and, therefore, of the date of valid publication. Other definitions were perhaps possible, but this should be discussed when
Prop. D came up. At present, the starting point date for mandatory registration was the one that mattered.

Barrie was unconvinced that writing to the registration centre, perhaps weekly, to find out whether anything new had come in was the final answer. There should be other mechanisms to disseminate information on the registered names at frequent intervals to the botanical community. Someone would have to pay for that service.

Mabberley did not feel it was realistic to expect editors or publishers to take responsibility for registering names. There were many journals that were not primarily taxonomic, especially in developing countries. It seemed inevitable that, for registration, it would be up to the author to do what was needed. What, then, would happen if someone should die after writing a paper and prior to its publication? Would the executors have to take care?

Pitt, as a mycologist, made the strongest possible plea for support of this proposal. As had been pointed out, the mycological literature was extremely diverse, both in terms of geographical origin and discipline. Even in a country like Australia it was impossible to keep up with the current literature without the aid of Index of Fungi, whose production would be greatly facilitated by adoption of the registration proposals. Having a registration system was the greatest possible asset for mycologists in Australia and the countries of the developing world. Submitting a name for registration was certainly easier than writing a paper and submitting it for publication.

Henderson reiterated the plea for keeping the principle separate from the mechanism. As to the latter, there had been no suggestion of a single registration authority with world-wide responsibility. Why could there not be a registration authority in each country? The option of quick smart contact, in their own language, with an address located at a major botanical institution of their country would answer the concerns of botanists. So much for the practicalities, which could be dealt with, provided the principle was supported.

Stuessy, recognizing both the apparent interest in the registration principle and the obvious problems of its uncertainties and loose ends, moved that Prop. B be transformed into a recommendation, with the verb "must" changed to "should". If this was carried, he would then propose to incorporate Prop. C and D into the same
recommendation. He could see no way in which this could pass as proposed, in view of its being tied in with the basic principle of valid publication. The motion was not, however, seconded.

Faegri wondered why Stuessy, as a member of the Committee on Registration, had not brought up his point before.

Hawksworth pointed out that the International Mycological Institute was largely owned by developing countries, and production of the Index of Fungi was one of the institute's services to mycologists in the tropics. Presently, many botanists in developing countries were reluctant to publish in their local journals, fearing that their new taxa might pass unnoticed. Registration would be a great boost for many journals in the tropics who could then state "new names published in this journal are registered under the Code". This would even encourage botanists in industrialized countries to describe new taxa in journals of the country in which these organisms actually grew.

Greuter observed that no one had contested the actual need for registration. Objections that had been raised either addressed details or, more often, the lack of detail. As Faegri had explained, omission of details at this stage was deliberate. Registration required an infrastructure, perhaps not a heavy but a widely spread one, and this also meant funding. In view of the obvious utility of registration, funding prospects were promising. However, the set-up of an infrastructure, the negotiation of agreements, and applications for funding were only possible if there was a relevant rule in the Code. Requesting that first the structure should be in place and afterward decide whether it would be needed was asking the impossible. A commitment had to be made before negotiations could start. Postponing the date of the registration requirement by five years was a very wise move, not only because the year 2000 was easily memorized, but mainly because the next Congress was thus given an option to cancel the registration provisions if, contrary to his expectation, the system had not by that date been properly implemented. In that case it would be easy enough to obtain the majority required for deletion. But if registration was not put into the Code now, it would never be. Comments to the effect that in six years we would better know the publication procedures of the next
century were exhilarating: in the year 2000, we would be less able than today to predict the technological developments of the next six years. Objections of this kind could in effect only mean objection to the principle itself, and it would be more honest to say so. Some of the questions asked, however, were justified, and he would try to provide answers. Who was going to be entitled to register? The reply was: anyone. As under the present Code, the authorship of the name would remain with the author of the publication. The person submitting the name for registration was unimportant: it could be the secretary, or the widow of a deceased botanist, or the publisher of the journal. Hopefully the latter would become the rule, in the journals' own interest. Authors would have the option of publishing either in a journal taking care of the registration procedure or in a perhaps more parochial or less botanical journal of his or her choice, when registration would be the author's responsibility. Concerning the purposely vague phrase "any registration office", the idea was indeed that there should be very many such offices throughout the world. Foreseeably, developing countries would be keen to set up their own registration offices, at very moderate cost, where incoming submissions would be stamped, dated and acknowledged. On average, postal delays in developing countries would not be longer than in, e.g., the United States. To Jørgensen's question, he could respond as Secretary of the IAPT. The IAPT officers had considered the registration proposals and felt very positive about them. They were able and willing to set up the necessary registration system, but they would on their own have asked for a delay of two additional years. Now that no less than five years were going to be added, the IAPT felt confident that the system could be made to work. The present proposals on registration were perhaps undetailed, but not vague. They were fit to be accepted now. In six years' time, there would be a possibility to vote them out again or, based on the gained experience, to add details of implementation, as might then be felt necessary.

Barrie was willing to support Prop. B if a statement could be incorporated requiring renewed approval by the XVI International Botanical Congress. The mere option of reversing the present decision in six years' time was not enough. An explicit proviso would satisfy those who, while sympathetic to the principle, were nervous
about the way in which registration was going to happen. The mere certainty that there would be a Nomenclature Section at the next Congress did not suffice. He therefore moved an amendment to add, at the end of the text of Prop. B, the words: "If an appropriate system of registration offices is created and a means of disseminating lists of registered names is created, and the mechanism is approved by the XVI International Botanical Congress". The motion was seconded.

Hawksworth suggested turning the proposed addition into a footnote, to avoid making the provision too clumsy [which, the Rapporteurs felt, was an editorial matter].

Nicolson enquired whether, to the mind of Hawksworth and others, addition of such a footnote would help or rather hinder the search for funding. Hawksworth's reaction was positive.

Chaloner could not see the point of the amendment. A similar proviso could, logically, be appended to each and every change of the Code here decided. The option of reversing a decision after six years was the metre of the game.

Jørgensen, while sharing the feeling that this move was unnecessarily cautious, could live with the amendment if it made the proposal more acceptable to others.

After an intervening break, Forero urged that, in the event of the amended proposal being approved, plant taxonomists from Latin America, Africa and Asia be actively involved in the set-up of the registration system. He would be willing to help involving botanists from Latin America and Southeast Asia.

Hawksworth asked on behalf of mycologists present to reintroduce the original 1995 date [which would have made the amendment pointless]. The Rapporteur objected that, even from the point of view of the IAPT, this would be too short a period for implementing the registration system; mycologists had to be patient.

Jørgensen also contradicted Hawksworth. Some among the mycologists present would even favour the date 2001. The question had been asked whether decisions of a Nomenclature Section took effect immediately or upon publication of the subsequent Code. If the former was the case, there was no objection to the year 2000.
Greuter replied that delay in publication of the Code was mainly due to the inclusion of the Appendices which made up two-thirds of its total bulk. Should the Section authorize reduction of the Appendices, e.g. by limiting them to additions and changes, publication of the Code could be further speeded up. Irrespective of this, decisions of the Section became effective immediately.

Clemants pointed out that, under the proposed amendment, a 60% majority would once more be required at the next Congress to confirm the principle of registration. Without the amendment, 41% of the votes would suffice. Barrie's motion was not unnecessarily cautious.

Greuter was not afraid of a second 60% threshold. If in six years' time the system was set up and was seen to be functional, and if money had been invested into it, the Section would certainly once more endorse registration. He rather objected to the motion's clumsy wording. He offered a simpler alternative, the addition to read: "subject to approval of the XVI International Botanical Congress".

Jeffrey expressed his strong preference for the shorter version, which was succinct and not grammatically flawed. On a show of hands, the Section shared his opinion. The motion, thus amended, was carried.

Prop. B, with the date 1 Jan. 2000 and as subsequently amended, was accepted.

Prop. C (76: 100: 4: 0: 4).

Demoulin still took exception to the explicit requirement of submitting "printed matter". Earlier in the discussion, it had been stated that photocopies would be acceptable. Was this covered by "printed matter", or was an amendment necessary?

Greuter replied that, should an amendment prove necessary, it might better be proposed in six years' time. This needed some more thought. While the point of not having to send in large books was well taken, Brummitt's concern about possible submission of photocopies of proofs should also be taken into consideration. True, by the latter action one would take a considered risk, since under Art...
45 Prop. A a name submitted for registration prior to its date of effective publication would remain invalid. This should prove a strong deterrent from fraudulent misuse of the registration procedure.

Funk suggested that Prop. C and D might better be withdrawn, and a committee asked to work out the details of registration procedure ahead of the next Congress. She mainly objected to Prop. D.

Brummitt, being still concerned over the details, concurred with Funk. There were now six years available for working out the minutiae. A precise statement of how registration was to operate should be presented at the next Congress.

Jeffrey questioned the Rapporteur’s statement on the effect of Art. 45 Prop. A. As he read it, a name once registered would not have to be re-registered, but would become valid as soon as the other requirements of valid publication had been met.

McNeill, responding to Brummitt, felt that it was essential for something like Prop. C to be approved, since it defined the meaning of registration. Prop. B did not make sense on its own. The issue addressed by Prop. D was of a different kind, and opinions on its present desirability might diverge.

Forero supported Funk in her plea for withdrawal of Prop. C and D. It was still not clear, even with the explanations given, where the registration offices were going to be. This was part of the procedure that had to be worked out in the next six years.

Barrie suggested that the provisions of Prop. C and D might become a Note, to follow after Prop. B. This would explain how the mechanism was envisioned at the moment, but would not be as binding as an Article would.

Chaloner saw it as the much tidier solution to incorporate these two items into the Code. Surely they were safely locked up with the obligation of reconfirming registration in six years, as per Prop. B. This was a very brief explanation of what registration meant. Having accepted Prop. B, we should also accept to two ensuing ones.

Greuter felt that it would be unfair, not towards those present who had listened to the debates, but to other readers of the future Code,
just to mention registration without stating what it was. The Code was to be self-explanatory. The present proposals contained the minimum information that was owed to users of the Code.

Henderson requested that the words "any registering office" be explained. Without an explanation, the proposal did not make sense.

Greuter pointed out that they were defined by the qualification "designated by the International Association for Plant Taxonomy".

Jeffrey reiterated his question on the relation between registration and the present Art. 45. If registration was, chronologically, the last condition to be fulfilled, how did the requirement of "explicit acceptance" fit into the picture?

Jørgensen could not see problems with Prop. C, since it was so openly worded that it should be acceptable even to those who were not completely happy with the basic principle, Prop. B. There might be justifiable uneasiness over Prop. D, which brought in the critical question of the date of a name.

Stuessy asked the Editorial Committee, in the event of the proposal being accepted, to change the present order of words by placing "to any registering office designated by the IAPT" at the end.

Mabberley offered a second editorial suggestion, to replace the word "designated" by "approved".

Henderson still wondered how a user of the Code could find out which were the "registering offices designated by the IAPT".

Greuter replied that, of course, the whole system once in place would have to be amply advertised before registration became mandatory. The message would have to be spread not only through Taxon but through all major journals for the different taxonomic groups. At that stage, details of procedure would have to be clearly specified. Otherwise, one would create a situation in which many unregistered names would be a major nuisance for plant nomenclature. To him, the need for ample publicity had been an important point in support of the postponement of the date to the year 2000.

Prop. C was accepted.

Funk was strongly opposed to including this kind of provision into the Code, when detailed procedures had to be worked out ahead of the next Congress. Discussion amongst those present had shown that there were other possibilities than the one here suggested.

Greuter tried to explain how the Code would function without the proposed clause. The date of a name, being the date on which the last requirement for valid publication was fulfilled, would still be the date of registration, which would not however be clearly defined. This question had previously been raised by Jeffrey when he referred to Art. 45. The proposal would define the date of registration as the earliest possible date, the one on which a name entered the periphery of the registration system, perhaps long before it was accessioned to a central register, index or database. Unless thus defined, the date of registration might even be taken to be the date of publication of a corresponding list of registered names. By its proposal, the Committee had wanted to dispel possible fears and to define the date of registration in such a way that it could be influenced and, to some extent, verified by the submitting author.

Funk, and some of her Latin American colleagues, preferred a completely different approach. The date of a name could remain the date on which it was published, after which a window of perhaps 6 to 12 months would be open during which a name would have to be registered. This was a concept that not everyone might like off-hand, but it should at least be fully discussed before a decision was taken. It would be premature for her to move a concrete amendment now, but it would be equally premature to accept Prop. D. When one would know how registration was to function, and how dense the network of registering offices would be, one would see how urgently a change of the date concept was needed.

Chaloner could not sympathize with Funk's concerns. Approving this proposal did not mean ratifying it with instant effect. This was a proposal which, if approved, would have again to be ratified in six years' time and, in the meantime, would act as an explicit working hypothesis. If a more appropriate solution was produced, surely this could be ratified by the next Congress.

Forero felt that, for exactly these reasons, it was not urgent to approve the proposal now. It could very well wait.
Taylor concurred that by putting this provision now into the Code one would prejudice future discussion of the matter.

Johnson, while strongly supporting registration, shared this particular concern of his American colleagues. Adopting Prop. D now would, in fairness, be prejudicial to further discussion of questions of dating names for purposes of priority.

Briggs suggested that a similar clause could be added, in the case of Prop. D, as had been incorporated in Prop. B, making it explicit that the option would have to be reconsidered and reconfirmed by the next Congress.

McNeill disliked the idea of cluttering the Code with further temporary clauses of this kind. Since so much concern had been voiced against introducing now a provision whose effects would depend on structures not yet implemented, it might be wiser to defeat the proposal. As the Rapporteur had pointed out, in the absence of such a clause the Code would operate through its other provisions and the date of a name, in the future, would be the date of registration. Clearly it would have to be defined, but this could be left to the next Congress.

Greuter begged to differ. Briggs's suggestion was exactly appropriate. It was only fair to tell readers of the Code who had not followed these discussions what solution was being envisaged. Otherwise they would just wonder, and not react. In order to be definitely accepted, the proposal would need to obtain a 60% majority a second time, in six years.

Jørgensen wondered whether the Rapporteur, with his comments, might have scared unnecessarily some people. Since this had anyway to be revoted in six years, why accept it now? Would voting it down have really serious effects?

Briggs, at the invitation of the Rapporteur, moved that the same proviso that had been added to the text of Prop. B be included in Prop. D. The motion was seconded.

Forero felt that, even with the amendment, this sentence should not go into the Code since it would only scare people.
Zijlstra spoke in favour of the amendment, whereby the present proposal and other solutions would be given equal chance in the future.

The motion was carried, but Prop. D, thus amended, was rejected by a card vote (54% in favour, 223: 190).

Prop. E (34 : 138 : 7 : 0 : 1) was ruled as rejected.


Hawksworth strongly supported the proposal. The example given spelled out current practice, and there were other similar cases in mycology.

Demoulin confirmed that, for the past 30-35 years, indirect basionym reference had indeed been assumed in the case of Kummer. Whether the provision was really necessary might be open to doubt, but he would support the proposal. Should it be defeated, at least the example should be taken up.

Brummitt offered a further example. There was a spiralled horticultural form of Juncus effusus, var. spiralis. Next, J. effusus f. spiralis had been published, without explicit reference back to the earlier varietal name. Unless one accepted this as a new combination, it would be an illegitimate later homonym under Art. 64.4, but would in turn preclude subsequent transfer of the epithet spiralis to the rank of forma, and a new epithet would be required at that rank. This would be nonsensical.

Greuter pointed out that this was not so, since the combination Juncus diffusus f. spiralis could be validated at any time, based on the varietal basionym. [Brummitt still disagreed; but see the example in Art. 64 Prop. E.] As the Rapporteurs had written, the proposal had indeed merits, and the Kummer example was most appropriate. However, all cases so far mentioned, and perhaps all that had been thought of by the proposers, concerned names that would have to be accepted as validly published anyhow, either as the names of new taxa or as new combinations. Many other names that had always been considered as nomina nuda, perhaps published in lists and without explicit indirect basionym reference but the epithets of which had previously been used in validly published names, might arguably become valid new combinations under this provision. As
long as the proposal did not limit itself to names that were in all events validly published, its effect would be highly destabilizing.

Taylor disputed the statement that such lists of names had always been ignored as potential sources of valid new combinations. Jeffrey gave an example. The name for the angled luffa, *Luffa acutangula*, had merely been listed by Roxburgh, and the only reference to its assumed basionym, *Cucumis acutangulus* L., was the use of the same epithet. That name had always been cited as *L. acutangula* (L.) Roxb., and the Rapporteur’s statement that nude combinations were not validly published appeared to be contrary to most current practice. Therefore, the intents of the proposal were laudable, although the proposed wording, by legislating for the application of common sense, might not always be helpful. The common-sense interpretation, while perhaps endorsed by botanical usage, had recently led to unpleasant consequences in a case in *Scaevola*.

Zijlstra could see the danger that names that had always been considered as nomina nuda might, under the new provision, be picked up as validly published names. But in such cases one should apply Pre. 9 and follow established custom. She had recently encountered a good example in support of the proposal, a moss handbook published in parts, with many new generic names first appearing, without authorship statement, in a diagnostic key, when in subsequent parts it was made explicit that they were based on earlier, validly published names of subgenera. Treating these as the names of newly described genera would be contrary to established custom.

**Prop. F** was rejected.

Stearn, upon that rejection, asked that the Editorial Committee introduce an example from Miller’s *Gardeners dictionary*, ed. 8 (1768), to illustrate indirect reference as defined in Art. 32.4. In that work, indirect references to Linnaeus as the basionym author were plentiful, both in a general way in the preface and in the discussions of individual genera.


Henderson challenged the Rapporteurs’ statement that spelling an epithet formed from a modern personal name with a single *i* was possible under the *Code*. Art. 23.10 made corrections in conformity
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with Rec. 73C.1 mandatory, with no stated exception. In so far as it was contrary to that rule, Rec. 73C.2 was not applicable. He had distributed a memorandum from which it appeared that publication of *Andropogon martini* by Roxburgh was valid in 1814, spelled with -ii, so that the spelling used in probably all recent grass treatments must stand anyway. The Editorial Committee should take care of this point.

Nicolson had campaigned for a corresponding change in the Editorial Committee but had failed. He had now used the new CD-ROM version of the *Index kewensis* to assess the relative frequency of the epithet spellings martini and martinii, and had found 98 single Martinis as opposed to 32 double Martinis [general hilarity].

Demoulin, having been at the origin of the present Rec. 73C.2, knew that it would be inapplicable unless reference was made to it under Rec. 73C.1 - but of course there was: "(but see Rec. 73C.2)". The Editorial Committee had been perfectly correct in adopting the -i spelling in the present example, provided that Roxburgh's earlier use of the name, spelled with -ii, was invalid. That assumption would now have to be checked. He was pleased to note that, as revealed by Nicolson's search, most botanists as he himself preferred the -i spelling.

Prop. G was referred to the Editorial Committee.

Prop. H (37: 25: 120: 0: 0) was referred to the Editorial Committee.

Prop. I (12: 156: 6: 0: 2) was ruled as rejected.

Prop. J (8: 164: 8: 0: 0) was ruled as rejected.

Prop. K (4: 79: 98: 0: 0) was referred to the Editorial Committee as to the example [which, as the Rapporteur explained, implied rejection of the proposed new provision].


Greuter pointed out that the "Ed. C." vote had a special meaning, explained in the Rapporteurs' published comments.

Hawksworth had discussed the issue with members of the Committee on Binary Combinations, and they had felt that in the proposed new Appendix V the suppressed works might best be listed alpha-
betically by authors, with the ranks concerned given in a second column. Replacing "Names" by "Names in specified ranks", as suggested by the Rapporteurs, would make this possible. He therefore accepted the suggestion as a friendly amendment.

Friis agreed, and wanted it to be clarified whether the new Appendix would take effect immediately as from this Congress, or only at the subsequent Congress as now suggested by App. V Prop. B.

Greuter explained that an amendment of the latter proposal, when it came up, would no doubt be appropriate. Since the proposer himself agreed, immediate effect might by now be assumed.

Prud'homme van Reine opposed the addition of a new Appendix to the Code.

Prop. L was accepted as amended.

Prop. M (44: 76: 51: 1: 4) was accepted.

Recommendation 32B

Prop. A (11: 147: 18: 1: 0) was ruled as rejected.

Recommendation 32E


McNeill asked for a "yes or no" vote, since the proposed deletion might not be merely editorial. Could mycologists comment?

Demoulin explained why the incriminated sentence was relevant. Cedar was Cedrus in Europe, but Juniperus in North America.

Prop. A was rejected.

Article 33

Prop. A (9: 173: 4: 1: 0) was ruled as rejected.

Article 34

Prop. A (2: 170: 11: 1: 0) was ruled as rejected.

Prop. B (5: 109: 66: 1: 0) was rejected.

Prop. C (3: 172: 6: 1: 0) was ruled as rejected.
SIXTH SESSION

Wednesday, 25 August 1993, 14:00 – 18:00

Article 36

Prop. A (25 : 145 : 18 : 0 : 0), although heavily defeated in the mail vote, was brought up for discussion through a motion from the floor by Hawksworth, duly seconded. [An amendment to it had been moved by Chaloner (see below), but the chair ruled that, by inversion of normal procedure, the original proposal would be acted upon first and the amendment would be considered if and when that proposal failed.]

Hawksworth regretted that the proposer, Chaudhri, could not be present to defend his proposal. In a general way, the decision process used in nomenclature was open to challenge. As Stuessy had pointed out in a recent paper in Taxon [42: 316. 1993] there were probably between 1000 and 2000 active working plant taxonomists in the world. Of these, 202, i.e. 10-20 %, had responded by way of the mail ballot. The mail ballot showed a geographical imbalance, being skewed toward those who could afford to be personal IAPT members. The Section, worse, had barely 70 members and lacked representatives from Latin America, most of Africa, India, Indonesia, Spain, Italy, France, and several other European countries. Decision-making on the botanical Code was thus not really democratic. The General Committee should be mandated to explore ways to significantly enlarge mail ballot participation, particularly on key issues like the present one but perhaps generally, e.g. by circulating ballots and off-prints of the "Synopsis of proposals" to a wide range of plant taxonomists who were not personal members of the IAPT. As to the proposal itself, he felt that eventually there was no getting round allowing English as an alternative for Latin, it was just a question of how long this was to be delayed. If there had not been Stearn's Botanical Latin, this might have happened long ago. That book did indeed promote precision in the description of flowering
plants, for which Latin morphological terms were carefully defined. When trying to use Latin to describe apical structures of asci, mitosporic fungi and the like, one would find this to be quite impossible. Furthermore, Latin was hardly being taught anywhere today. If Linnaeus were still alive he would certainly be writing in English. Chaudhri's concern, which he shared, was that by continuing to enforce Latin one was putting an enormous millstone around the necks of young taxonomists in countries like China or Brazil, forcing them to learn Latin in addition to English. None of the other biological Codes of nomenclature enforced the use of Latin, while the botanical Code made an exception for fossils and made Latin mandatory only from 1935 onward, or from 1958 for the algae. The continued use of Latin in botany also perpetuated the image of plant taxonomists being out of touch with the real world. Being strongly supportive of the proposal, he asked for a card vote.

Stearn had found his own language to be very imprecise. He had been asked, during his long life, to translate very many English descriptions written by his contemporaries into Latin, but he now refused to do so unless he could examine the plant described and so make sure what the description was intended to mean. The act of translating was above all an act of discipline and made people understand what they were trying to say. Having realized the great difficulty besetting people who tried to use Latin, he had spent twenty years writing a textbook on the subject so as to keep open this important channel of communication, botanical Latin. How would taxonomists ignoring Latin be able to use past standard works as those of Saccardo and De Toni? There was an enormous body of information written in botanical Latin (rather than in classical Latin), and knowledge of that language was necessary to gain access to that information, much more perhaps than for writing one's own diagnoses.

Demoulin regretted that this matter had again been brought up in spite of the heavily negative mail vote, specifically since, in this case, there was nothing to object or to be added to the Rapporteurs' published comments. The question had been discussed at length in Berlin, and there were no new arguments now. Changing the present mail ballot system might result in a most arbitrary procedure. How to redefine the circle of those entitled to vote? At present,
they were those who had sufficient interest in taxonomy and nomenclature to maintain IAPT membership. How could one ask that a small group of 95 people should act against the advice of a much larger group of 202, that was certainly no less representative than the Section?

Rather than reiterating what he had said in Berlin, he would just read out a statement by the botanists in Meise (BR) by whom he was mandated: "Latin diagnoses present at least two important advantages: (1) they clearly show the intent of an author to publish a new taxon, and (2) they force taxonomists to keep a minimum of contact with the language in which the classical treatises of their science are written. If one were to admit one modern language, why not also Spanish, German, Russian, Chinese or French?" The latter point was particularly relevant since botanists speaking Romance languages were so obviously underrepresented in the Section. Italian, French, Portuguese and Spanish, especially the latter that was understood from the southern tip of South America to New York City, were languages spoken by a very large number of people that were generally in favour of Latin and opposed to the new importance being given to English. Adding English as the single modern language to Latin might cause very adverse reactions and would lead to requests for adding other modern languages as well. Incidentally, there were no major problems with Latin for mycologists, as recently witnessed by one of his students, untrained in Latin, who with the help of Stearn's *Botanical Latin* had prepared an adequate Latin description of a new taxon.

Ahti felt certain that, were a zoologist present, he would laugh at Demoulin's arguments. He himself had changed his mind since the last Congress and now supported the proposal, as one representing the non-Indo-European languages. No one under forty, in his country, had any knowledge of Latin — and he had got tired of translating all his colleagues' descriptions into Latin. At the last Congress, Russians would have been upset if English but not Russian had been allowed; but this again had changed. The French would probably still mind, but they might be given a chance at a later Congress. The proposal might perhaps not make it this time, but it would come up again and again with the progressive decline of Latin knowledge.
Jeffrey doubted that Stearn's argument of English being too imprecise was relevant. The application of future names, to which the new provision would apply, was determined by the nature of their type. Since a holotype must now be designated, the description really was irrelevant. A description was still needed for valid publication under Art. 32, but it was not there defined. There was no requirement for it to be the description of the type, nor even of the taxon to which the name was applied, so why should one ask it to be accurate? As to English being just one of many modern languages, this might be true but was apparently not the opinion of the Committee for Fossil Plants.

Orchard saw the obligation to provide a Latin description as a very useful filter discouraging the frivolous description of new taxa. In Australia, it had certainly prevented the rash naming of many variants thought by some to be separate species.

Gams, one of the few Latinists left and who had therefore to translate one description a week into Latin, confirmed the function of Latin as a useful filter. If the proposal were to be seriously considered, it would need an amendment, to the effect that an English description or diagnosis would have to be clearly identified as such. The reader should not be forced to screen whole texts for possible descriptive elements. [This amendment was not formally moved.]

McNeill drew attention to corollary proposals to the forerunner of the present one that had been submitted to the Berlin Congress, one of which would take care of the point raised by Gams. It was, however, in the form of a Recommendation [an additional sentence under Rec. 32B.1]. If the Section were to go so drastically against the mail ballot as to accept the present proposal, this might then be raised again.

Jørgensen commented on the widespread failure to make a clear distinction between description and diagnosis. Many authors had their full English descriptions translated into Latin, but this was a misunderstanding. A Latin diagnosis could be a short text which it was not terribly difficult to write. For him as a non-Latinist, it was a useful exercise to sit down and write a concentrated text in Latin telling the important characters. If it were to become possible to write such a diagnosis in English, one would need a book on "Botanical English".
Briggs felt that, if allowance for diagnoses in English were to be made concurrently with mandatory registration of names, the filtering role of the Latin requirement might be taken over by the latter. Greuter knew that it was superfluous to give the Section advice: by now, everyone had made up his or her mind. Demoulin's statement that the mail vote had not been influenced in any way by the Rapporteurs' comments was certainly accurate, be it only for the reason that their personal opinion on this issue diverged. Indeed, one of the Rapporteurs (the other one!) had co-authored a similar proposal to the Berlin Congress. The outcome of the mail vote was clear enough, and the Section should feel guided by it, but it was not binding. If the proposal should fail, the IAPT would receive with an open mind suggestions for setting up a translation service into Latin, perhaps in conjunction with UNESCO so as to make it affordable for botanists from the developing world.

Prop. A was rejected by a card vote (21.7% in favour, 91:328). Chaloner introduced his amended version of Prop. A, whereby the new provision would apply only to "fossil plants", not to "plants" in general. The present Code accepted diagnoses in any language for fossil plant taxa. In support of his impassioned plea for restricting the former latitude to the use of Latin and English only, he would limit himself to four basic points: (1) the change concerned the nomenclature of fossil plants only, while making palaeobotanical literature available to a much greater degree to non-fossil botanists, obviating the need to try and wrestle with diagnoses in Chinese or Russian or other entertaining and inscrutable languages from all over the world; (2) the Committee for Fossil Plants was heavily in favour of the proposal, with only one member dissenting, which was quite a change since the Committee had been rather divided on this issue in the past; (3) this would be a restrictive rule for palaeobotanists, fitting them into the straightjacket of either an English or a Latin diagnosis; (4) the idea that one was depriving the younger generation of the obligation and benefit of knowing Latin, while relevant for the main proposal, did not apply here, since there were no Latin classics in palaeobotany where the basic texts were mainly in English, French or German.
Stuessy wondered how the broader community of palaeobotanists really felt about this. What was the percentage of diagnoses presently published in English?

Chaloner was unable to give an exact figure, but estimated that at least half and probably a great deal more of the diagnoses published in the last decade were written in English. The remainder were mainly in Russian and Chinese. There had been virtually no Latin. While he did not of course know the feelings of the whole palaeobotanical community, he had listened to many of its representatives. The Committee for Fossil Plants was well balanced geographically, and both the Russian and the Chinese member on it had expressed support. The only to dissent was a South American speaking English and Russian impeccably, who had written: "I would accept English and a Latin derivative language". It was indeed becoming a handicap for English speaking students that they had no longer to learn languages to communicate with their colleagues from abroad, since English had become so prevalent world-wide.

Chaloner's motion was carried, and the amended Prop. A was thereby accepted.

Recommendation 36A

Prop. A (28:64:88:0:0) was rejected.

Article 37

Prop. A (168:8:6:1:1) was accepted.


McNeill explained that the "Ed. C." votes were cast in favour of incorporating the new matter of the proposal into Rec. 7A.

Greuter, noting that there was hardly any support for accepting this as a rule, as evidenced by the mail vote, moved an amendment, to word it as a recommendation. His motion was seconded.

Faegri wondered what "open access" was to mean in this context. Did Hawksworth imply that Russian herbaria had an open access policy? Had he worked in any of them?
Hawksworth's main concern was about the depositing of types in private herbaria. Whereas Russian herbaria had at least some kind of policy of access, this was not necessarily the case with private collections. Type specimens were part of the communication system of biology and ought always to be in the public domain. The proposed new Rec. 37B was designed to cover the situation where to-be type material was privately owned, when the owner was encouraged to take steps to ensure that the material would eventually reside in a public collection. At present, the Code was too lax in this respect.

Demoulin liked this as a recommendation but thought it was impracticable as a rule. How would such a rule apply in a case like the Cryptogamy Department of the Paris Museum, certainly a public collection with an open access policy – except that the director, Heim, had denied access to his own collections, including the types of his names, during his lifetime.

Johnson was concerned about the word "open", which was unnecessary and could be misinterpreted; "policy of access" was preferable. Was this acceptable as a friendly amendment to the amendment?

Greuter explained how the proposed amendment would work. As suggested by the Rapporteurs in their published comments, it would instruct the Editorial Committee to incorporate some or all of the new matter of the proposal into the present Rec. 7A. This was a place in the Code that was much more appropriate for this topic than Art. 37, concerning valid publication.

Prud'homme van Reine spoke for the present wording, "policy of open access". A policy of access could also be a policy of non-access! If this were to pass as a rule, many large herbaria would no longer qualify as repositories of types, so that it should certainly be no more than a recommendation.

Dorr was worried by the reference to Index herbariorum since, as far as he knew, the criteria for inclusion of institutions in that index were completely arbitrary.

Greuter drew attention to the Rapporteurs' published comments, that Index herbariorum did not recognize institutions but just listed them. Before entering into details, he asked for a vote on his motion, which was carried. He then recommended that the proposal,
thus amended, be referred to the Editorial Committee for appropriate action.

Jeffrey asked for an assurance that no reference of any kind to *Index herbariorum* would be incorporated. There were large institutions omitted from that *Index*, such as the Institute of Biological Problems of the North in Magadan, with a herbarium of 160,000 specimens. Encouraged by the Rapporteur, he moved deletion of the last six words of the proposal. His motion was seconded and carried.

Jørgensen still feared that the Editorial Committee would have a difficult job in finding a suitable wording, but since this was to be a Recommendation it did not matter so much. He mentioned the example of a colleague who, being afraid that he might no longer be allowed to deposit types in his private herbarium in view of proposals made to the Berlin Congress, had named that herbarium an "Institution" – yet it still was a private collection.

Prop. B, as amended, was referred to the Editorial Committee.

Prop. C (11: 157: 5: 1: 9) was ruled as rejected.

**Recommendation 37B (new)**


McNeill pointed out that the proposal depended on Art. 37 Prop. B, of which parts were now to go under Rec. 7A. The present proposal was thus superfluous.

Hawksworth pleaded for its substance being included as a second part under Rec. 7A. It did address a separate issue, admittedly not belonging under Art. 37.

Greuter was opposed to having this issue referred to the Editorial Committee, since too much judgement would be needed on what to place where. As it was worded now, the proposal depended on acceptance of Art. 37 Prop. B as an Article, i.e., on the assumption that names whose types were deposited in a private herbarium would not be validly published. Whether a different recommendation was now appropriate instead, and how it should read, had yet to be figured out.

Prop. A was rejected.
Article 39bis (new)

Prop. A (10 : 152 : 15 : 1 : 1) was ruled as rejected.

Article 41

Prop. A (151 : 22 : 9 : 1 : 0) was accepted.

Article 42

Prop. A (32 : 144 : 7 : 1 : 1) was ruled as rejected.


Silva qualified the Rapporteurs' published comments as inappropriate. Oral descriptions could not be used to validate names.

McNeill explained the Rapporteurs' point: the word "written", qualifying "description", was not semantically redundant.

Prop. B was rejected.

Article 45


Barrie doubted that, in view of what had been decided on the registration proposals, this was now relevant. Should it not better be considered at the next Congress?

Greuter agreed that this was a rule that would be needed when registration came into effect. But since the point appeared to be uncontroversial, it was just as well to clarify it now. The date would of course be editorially changed to the year 2000.

In reply to a question by Dorr, McNeill explained that the change of date was indeed an editorial point. Since the Section had changed the date in Art. 32 Prop. B, the Editorial Committee would of course see to it that the date be changed here accordingly.

Forero saw this proposal as part of the process that was to take place during the six years to come. There was no need to approve it now.

Faegri admitted that, during the next six years, this would not be a very active provision. It answered one of the questions that arose...
naturally under the registration concept. Since it could do no harm as long as it was not operative, it might just as well be accepted immediately. Barrie agreed.

Prop. A was accepted.

**Article 46**

**Prop. A** (11:150:6:0:6) was ruled as rejected.

**Prop. B** (8:150:5:0:8) was ruled as rejected.

Henderson, the author of the two foregoing proposals, did not want to bring them up again but made two points. (1) Even though the proposals were rejected, an author validating future new combinations based on an autonym would have to cite the author who validated that autonym and the place of its valid publication. How this was to be done would have to be worked out in the future. (2) Citation of the author of a species name within an infraspecific autonym [as presently practised in the Code and elsewhere] was both illogical and nonsensical. That author was not usually the author who had validated the autonym. Citing the author of a species name within the name of an infraspecific taxon made as much sense as citing the author of a generic name within a species name – which nobody did.

**Prop. C** (2:154:3:0:26) was ruled as rejected.


Brummitt introduced the "in and ex" matter generally. It was most unfortunate and confusing to have two independent sets of proposals, and it should never have happened. At Berlin, an "in and ex" Committee had been set up to deal with the unresolved problems addressed by a number of proposals that had been submitted by Taylor and himself. Not being a Committee member, he had been associated to its deliberations by the Secretary, Karttunen, who had started very actively to circulate materials, comments, and proposals for the members to vote on. Unfortunately, activities thereafter went completely dead, and successive deadlines for submitting Committee reports went by. Eventually, Nicolson had invited him to submit a set of proposals, which he had done – with the
result that within a week Karttunen came up with his own Committee report. Brummitt had prepared and distributed a sheet of equivalences of "in and ex" proposals to the Tokyo Congress. He suggested that the chair might wish to deal with the proposals in the sequence of that sheet. Group 1, of simple cases of when to use "ex" after the names of new taxa, comprised Prop. C, D and N, where C was an alternative to the latter two and reflected an extreme view, held by one of the Committee members whom he valued highly as a former Rapporteur. It had received virtually no support in the mail vote (only two positive votes) and had been rightly ruled as rejected. Prop. D and N were complementary to each other. Whereas Art. 46.2 and 46.3, dealing with the use of "in" and "ex" respectively, were not now mutually exclusive but partly overlapping, Prop. D and N were contrasted sharply and thus gave clear guidance on whether "in" or "ex" was appropriate. Prop. D would replace Art. 46.2, and Prop. N, Art. 46.3, both being aimed at supporting current practice while clarifying the Code. They reflected the view that what mattered was not the person to whom the name had been credited in the original publication, but the one who had done the hard work, meaning, in the case of a new taxon, the writing of the validating description.

Greuter advised that the sequence of proposals in the "Synopsis" be followed since it had been carefully considered by the Rapporteurs, who had endeavoured to give equal treatment to the two competing sets of proposals. Fortunately, Brummitt's sequence and that of the "Synopsis" were rather similar. However, relegating Prop. E to the end was clearly inappropriate. It defined the term "ascription", used among others in Prop. D, and was in a way the backbone of the whole series of proposals.

Ahti admitted that the whole topic of Art. 46 was rather unimportant, yet it had significance for those preparing lists of names. At present, Art. 46 was indeed highly unsatisfactory. Having gone through carefully the examples proposed by Brummitt, he had convinced himself that his was a carefully considered approach covering all major issues. While there might be no perfect solution, he wanted to lend his support to the Brummitt proposals which he considered as superior to the Committee proposals.
Greuter drew attention to the fact that Prop. K, which the Rapporteurs had stated to be equivalent to Prop. D and J combined, had been favoured by the mail vote over the latter two.

McNeill added that, while Prop. D could be accommodated within the present article, Prop. J would require Prop. D to be passed.

Prop. D was accepted.


Brummitt had found Karttunen's definition of "ascription" rather baffling. To him, ascription was a simple and unambiguous term, meaning the name of the person that appeared behind the name of a plant in the original publication. He had the feeling that Karttunen had been confusing a lot of issues in his proposal, which he himself did not support. The first sentence by itself, however, was all right.

Greuter expressed concern that not everyone present might at this point be fully aware of the issues. There was a risk that, with successive proposals being accepted, the Article under consideration would constantly change its shape. The term "ascription" had been used in the now accepted Prop. D, and would again be used in proposals concerning Art. 46.3 and the names of authors preceding "ex". Taking Brummitt's straightforward definition of ascription literally, one would end up with innumerable "pre-ex" citations that had been avoided carefully in the past because they rested on erroneous assumptions. Many names, when first used in different combinations or at different ranks, had been attributed to the basionym author – whose name, unless ascription was redefined, would now be cited twice: within parentheses, and following the parentheses to precede the "ex". The Karttunen definition here proposed, and it alone among the extant proposals, would avoid this. Brummitt could not conceivably favour such a proliferation of redundant author citations, so that his opposition to the Karttunen proposal was difficult to understand. Unless this proposal was accepted, Art. 46, and the option of citing "pre-ex" authors, would become completely discredited, and no one would use it any longer – which was perhaps not such a bad thing after all.
Demoulin urged that attention be given to the situation of names with later starting-point dates, which constituted the vast majority of cases in which the "ex" citation was used. Phanerogamists had a tendency to neglect this issue. As long as specialists of these groups did not follow the example of mycologists and revert to the 1753 starting point, it was vital for them to maintain the link between the validating author and the pre-starting-point original author of a name. Acceptance of the Karttunen proposal would lead to a wholesale change of author citations in, e.g., blue-green algal names, and would result in a major catastrophe.

At the request of Brummitt, Greuter explained his point again. If, say, Nyman ascribed the name of one of his subspecies to the author of a species name that served as basionym (say: Boissier), the author citation might become "(Boiss.) Boiss. ex Nyman", which was clearly undesirable. Under Karttunen's definition, however, "reference to a basionym or a replaced synonym" was not ascription.

Nicolson disliked the examples in Prop. E, because they failed to distinguish between "in" and "ex" in a suitable way. The first example treated "in Wilson" as part of the author citation, whereas in fact it should be part of the bibliographic citation. The first sentence of the definition was appropriate, but the second sentence should be left out.

Greuter explained that the wording of the examples would have to be modified editorially if, e.g., Prop. K were to be adopted later. It was perfectly appropriate for Karttunen to have worded the proposed examples in agreement with the present rule, without taking acceptance of his other proposals for granted. This had no bearing on the second part of the proposed definition, the suppression of which would completely emasculate it.

Johnson recognized that the point raised by Greuter was important, but asked that the Editorial Committee should look critically at the wording of Karttunen's proposal if it was adopted.

Greuter assured that the Editorial Committee would have a close look at the wording, in particular of the examples. If any or most of the present proposals were to pass, Art. 46 would presumably be
the toughest morsel of the whole Code to edit. If the Section shared his feeling that the situation had become too confused for taking sound, definite decisions, it might be well advised to appoint a new Special Committee to study the matter. This was not an elegant solution when such a Special Committee had been operating for six years and had submitted a report, but it might well be the only reasonable option. Was it not so that the Section sat patiently listening to the divergent opinions of perhaps three main speakers without fully grasping their arguments?

Upon a question by Brummitt, McNeill tried to explain the divergent points of view. If "ascribed" had a standard English meaning that was satisfactory in the context of Prop. N, then no special definition as by Prop. E was required. Greuter's point was that the current meaning of "ascription" led to difficulties under Prop. N unless a special definition like the present one was accepted.

Brummitt, following renewed explanations, agreed that there was a problem and that Prop. E should indeed be seriously considered.

Funk suggested that, since divergent views had been expressed and opinions were shifting, decisions on Art. 46 should better be postponed to the following day. Giving the discussants some more time might enable them to reach consensus. At present, she felt at a loss to make up an opinion of her own and would just have to abstain. However, having to wait another six years was too long.

Brummitt having agreed, and a number of volunteers having declared their willingness to serve, an ad-hoc Committee was mandated to discuss the proposals on Art. 46 and correlated Recommendations, and to report on the following morning.

[Action and discussion reported hereunder actually took place at the beginning of the 7th Session, on 26 August.]

Greuter reported that the ad-hoc Committee [Ahti, Brummitt, Demoulin, Greuter, Kirk, McNeill, Nicolson, and Zijlstra] had worked through the intricate tangle of proposals relating to Art. 46 and correlated Recommendations. Those looking forward to a heated debate might be disappointed to learn that unanimous consensus had been reached on every issue, with the restriction that in a
single case, Prop. T, the consensus was to abstain from making a recommendation for or against. The Section might want to concentrate its energies on that particular issue. The previous acceptance of Prop. D was not to be challenged, and the group's recommendations on the other proposals would be read out case by case by the Vice-Rapporteur. The Section willingly abided by that advice, as detailed below.

Prop. E was accepted.

Prop. F (14 : 135 : 4 : 1 : 32) was withdrawn.

Prop. G (60 : 84 : 5 : 0 : 33) was accepted.

Prop. H (69 : 77 : 3 : 0 : 33) was accepted.


Prop. J (48 : 86 : 17 : 0 : 31) was withdrawn.

Prop. K (75 : 62 : 13 : 0 : 32) was accepted.

Prop. L (74 : 62 : 15 : 0 : 31) was accepted.

Prop. M (42 : 91 : 17 : 0 : 31) was withdrawn.


Brummitt drew the attention of the Editorial Committee to a bibliographic error in Ex. 4ter that had been pointed out to him by Egorova (details to follow).

Prop. N was accepted.

Prop. O (23 : 120 : 6 : 0 : 29) was referred to the Editorial Committee.

Prop. P (13 : 134 : 4 : 0 : 26) was accepted.

Prop. Q (51 : 54 : 51 : 0 : 27) was accepted.

Prop. R (36 : 45 : 71 : 0 : 29) was referred to the Editorial Committee.

Prop. S (37 : 60 : 55 : 0 : 29) was referred to the Editorial Committee.

Brummitt admitted that he normally hesitated to rule on unique cases, as the present one was, in the Code, but he and Zijlstra had found the Bentham & Hooker case to come up so often that it deserved such treatment. According to the title page of Genera plantarum, Bentham and Hooker were co-authors. They later published a note stating that they wanted the text to be considered their joint work, but then continued to give the individual authorship for each family. Names of their own new genera they invariably credited to the single family author. This was not the case for names ascribed to others, that were either newly validated there or transferred to a new rank, of which there were hundreds. In the literature, these names were sometimes credited to either Bentham or Hooker alone, or to Bentham & Hooker jointly. Even the Index kewensis was inconsistent in this respect, without a marked preference for one or the other approach. The same applied to ING and NCU-3. A decision was needed, and logic favoured a single author's name, which was the explicit preference of the authors themselves. This would go against the new provisions of Art. 46, and a special rule was therefore needed.

Jeffrey pleaded for support of this proposal. One of the largest family treatments in Genera plantarum was that of the Compositae by Bentham, who had also written all the backing papers alone. Jeffrey, as an editor or reviewer, had constantly to delete reference to Hooker in the authorship of Bentham's generic names in that family. Bibliographic precision, if required, could always be achieved by adding "in Bentham & Hooker" after Bentham's name.

Demoulin wondered whether this case was really unique. Was there not a similar situation with "DC. in Lamarck & Candolle", and also with Humboldt, Bonpland & Kunth?

Trehane mentioned Stafleu & Cowan's TL-2 as a work giving specific advice on which authors to cite or not to cite in cases like Lamarck & Candolle. There were numerous such cases of joint authorship for which there had been a traditional consensus on the way in which to cite. This whole set of proposals, that had been rather bulldozed through, would have some severe implications for
the editors of indexes throughout the world. The Bentham & Hooker case was so vast a problem that Brummitt's proposal should be strongly supported, simply because it would remove an awful workload from an awful lot of people.

Stearn had gone into this problem when writing the preface to a facsimile edition of Bentham & Hooker's Genera plantarum, and he fully supported Brummitt's proposal. Author citation was of some consequence in questions of typification, since the handwritings of Bentham and Hooker could be easily distinguished in their respective herbarium annotations.

Greuter deplored the absence of a real debate on this question. There had been five speakers, four of them from the U.K., and all four naturally in favour. The matter was not perhaps of much consequence, since there appeared to be an equal balance in usage of joint vs. single authors. Jeffrey's complaint of having to correct so many double author citations made one suspect that these might in fact rather predominate. Under the new, clear and consistent rule, double authorship would indeed be correct. Introducing a specific rule for a single case was being unkind to the users of the Code. Such ad-hoc rules took unnecessary space, confused people, and might frustrate many who had to deal with similar situations that were just not important or well-supported enough to warrant an exception. Since the meeting was held in Japan, Siebold & Zuccarini might perhaps be mentioned, for whom similar problems existed. Since the new rule was unambiguous, why not apply it? Bentham and Hooker had made it publicly known that they wanted to be considered jointly responsible, if not for the merits then at least for the possible defects of their work, so that it was only fair that when they credited a name to another person they should stand as joint authors. When they had a real feeling of paternity for a name, they had made that explicit by adding their single name to it. Perhaps more importantly, a Bentham & Hooker citation referred unambiguously and directly to that one work, Genera plantarum, which either name alone did not. The option of adding "in Bentham & Hooker" after the author's name, mentioned by Jeffrey, had now been limited to those instances in which the bibliographic source was cited anyway. Adoption of the Brummitt proposal would therefore, in many cases, result in a loss of information.
Hawksworth also preferred to keep the Code simple. There were some similarly complicated, essential works in mycology. Perhaps the Committee for Spermatophyta might be asked for an opinion on the present case.

McNeill made it clear that no such opinion was required. The new rule was perfectly unambiguous. Without this provision, the joint authorship of Bentham & Hooker would have to be cited. Perhaps what was seen as a problem was that this applied in the case of new combinations, but not of new taxa for which a single author had been specified.

Brummitt agreed. Without this additional provision, names of genera newly described by either Bentham or Hooker would appear side by side with other, perhaps newly combined ones that would have to be credited to Bentham & Hooker jointly. This was nonsense and would be contrary to these authors' own wishes.

Jeffrey felt that this special case could, if in the Code, provide guidance in other, similar cases. For example, he had often to correct editorially "Humboldt, Bonpland & Kunth" to "Kunth" alone. There again there was no consistency of treatment and some guidance was needed.

Faegri shared the Rapporteur's concerns on losing the bibliographic reference when abandoning the joint "Bentham & Hooker" citation.

Jørgensen was worried at the thought of other examples being brought up as additional exceptions to the rule in the future. He could think of some large French works in cryptogamy where a special ruling would make things easier. Was the case of Bentham & Hooker really that exceptional?

Zijlstra wondered why all those who feared that other examples might have to be added had not voted against Prop. Q and S. She was convinced that there was a solid tradition for attributing names published in Genera plantarum to either Bentham or Hooker alone. There were very few instances of joint author citation now left in ING. Wiersema, in comments on the NCU draft list of generic names, had stated that tradition favoured single authorship in these cases. Comparison of Bentham & Hooker with Siebold & Zuccarini was inappropriate.
Brummitt replied to Faegri that, even without the special provision, many genera were and would continue to be attributed to either Bentham or Hooker alone. For the great majority of such names, this proposal made no difference. The other examples alluded to were probably not exactly comparable, since the circumstances for Genera plantarum were quite unique.

Dorr pointed at the conserved name Thymopsis, in App. III of the Code, that was credited to Bentham alone.

Greuter retorted that this case was not affected by the present proposal, the name having been credited to Bentham alone in the protologue.

Prud’homme van Reine felt that, had this been a French speaking group, the decision might have been different. (He was proved wrong by the vote.)

Prop. T was rejected by a card vote (39.3% in favour, 169: 261).

Prop. U (86: 53: 11: 0: 28) was accepted.

**Recommendation 46A**

Prop. A (6: 171: 1: 1: 0) was ruled as rejected.

**Recommendation 46C**

Prop. A (53: 63: 63: 0: 0) was referred to the Editorial Committee.

**Recommendation 46E (new)**

Prop. A (27: 140: 11: 0: 2) was ruled as rejected.

Prop. B (30: 137: 8: 0: 5) was ruled as rejected.

Prop. C (34: 120: 21: 0: 5) was rejected.

Prop. D (51: 99: 24: 0: 5) was rejected.

**Recommendation 46F (new)**

Prop. A (8: 136: 23: 5: 8) was ruled as rejected.
Article 48

Prop. A (3 : 65 : 111 : 0 : 1) was referred to the Editorial Committee for action in conformity with the Rapporteurs' published comments ("definitely" to replace "explicitly").

Prop. B (23 : 126 : 11 : 0 : 16) was rejected.


Jeffrey warned that the phrase "all its syntypes", in the present proposal, would cause the same difficulties as the same phrase now in Art. 63.1 did, which had caused much worry in Subcommittee IIIB – a point that he would further explain when Art. 63 came up. If modified as proposed, Art. 48.1 might be more difficult to apply than it was now.

Perry explained that at present Art. 48.1 did not make it obvious whether or not an author who adopted an existing name but excluded an element subsequently designated as the type of that name had, upon such lectotypification, published a later homonym. If one accepted the principle that lectotypification was not to affect the nomenclatural status of names other than the one being typified, then Prop. C should be adopted.

Greuter reminded that the alternative Prop. B, just defeated, and Prop. C had been made because the effect of the present Article was not clear. However, the consequences of either clarification were unknown. Therefore, both proposals had been heavily defeated in the mail vote, which gave no cue as to which of the two was preferable. Positive action on either proposal would obviously be premature.

Prop. C was rejected.

Prop. D (8 : 29 : 141 : 0 : 0) was referred to the Editorial Committee.

Prop. E (25 : 14 : 140 : 0 : 0) was referred to the Editorial Committee.
Article 49

Prop. A (3 : 115 : 57 : 0 : 3), depending on action on Art. 46 Prop. P, was [on the following morning] referred to the Editorial Committee.

Recommendation 50B

Prop. A (29 : 27 : 124 : 0 : 0) was referred to the Editorial Committee.

Article 57


McNeill thought that the present rule was clear as to its intent but was somewhat ambiguously phrased. The attempt to achieve greater precision had led to a tremendously long text, which was certainly the reason for the many "Ed. C." votes.

Perry was embarrassed by the length of the example but had found it impossible to cut it down. She wished the Editorial Committee good luck in doing so.

Prop. A was accepted.

Prop. B (46 : 33 : 101 : 0 : 0) was referred to the Editorial Committee.

Prop. C (10 : 104 : 66 : 0 : 0) was withdrawn.


Henderson felt that many of the negative votes might reflect opposition to author citation for autonyms, heavily defeated elsewhere, rather than to the proposed correction of wording. If the Editorial Committee accepted that taxa had types, then the present wording was satisfactory. If it accepted that names, not taxa, had types, then the present wording was indefensible. The proposal should be considered by the Editorial Committee.

Prop. D was referred to the Editorial Committee.
Article 60


Zijlstra asked for consideration of this matter to be postponed. Since the mail vote was heavily negative for both Prop. A and B, offered as alternatives, she had drafted a compromise solution that she would present on the following morning.

Jeffrey pointed out that Zijlstra's proposals concerned one special case (transfer of a taxon of subordinate rank to generic rank), whereas the problem was more general and included transfer from one subordinate rank to another. He suggested that, if a proposal on the special case, as revised by Zijlstra, should pass, the more general case should then also be considered.

Greuter drew attention to the Rapporteurs' published comments, and to the close analogy of the situation with the one that had arisen under Art. 48. Here as there, an alternative clarification of an Article had been proposed without the consequences of either change being known. Here again, both proposals had been heavily defeated in the mail vote without either being preferred over the other. The advice was clearly to leave the Code as it stood, and, in case of doubt, to apply the too often ignored Pre. 9 and follow established custom. The Section should decide whether a compromise proposal on so uncertain an issue had a chance to be acceptable and, if so, postpone decision; or whether in all events maintaining the status quo was the safest answer, and in that case, clear the desk by rejecting the proposal now. Might the Section be asked to express its preference by a show of hands?

Brummitt spoke for postponement of action. The issue was of practical relevance for quite a number of names, and if at all possible a decision should be reached at this meeting rather than being postponed for another six years.

Zijlstra explained that her compromise solution would involve application of Pre. 9, as the Rapporteurs themselves had suggested. Thereupon, the Section agreed to postpone the matter until the following morning.
Nomenclature in Yokohama

[The following debates took place during the 7th Session, when action on Art. 46-49 had been completed.]

Zijlstra presented her New Proposal as a compromise that had been agreed upon by a group of five late in the previous night and was to replace Prop. A and B: "When a taxon of the rank of a subdivision of a genus is raised to generic rank and the author of the generic name created a name that is derived from the infrageneric epithet, the basionym epithet and the generic name have to be treated as variants, one of which is considered to be correctable. For the choice of which epithet/name has to be corrected, established custom should be followed. If, however, the epithet of a purported basionym has the form of a plural adjective rather than the same form as the generic basionym [sic!], a nomen novum is considered to have been published."

She had prepared a list of more than sixty names concerned by this issue, and many more did certainly exist. Three examples were given to illustrate the kinds of situations that were found. When the (later) spelling originally adopted for the generic name was currently used, the former Prop. B would have been appropriate; perhaps 80-90% of the cases belonged to that category. For the two other categories (10-20%), Prop. A would have worked: when usage had immediately reverted to the spelling of the subdivisional epithet; or when it had first followed the modified generic spelling but had later been corrected back to the original form. The new proposal would effect that in every one of these cases the spelling that was now current could be maintained. The examples quoted were Phyllostegia sect. Haplostachya vs. Haplostachys, for the first category; Filicites sect. Sphenopteris vs. Sphaenopteris, for the second category; and Polygonum sect. Acogonon vs. Acogonum, for the third category.

Greuter favoured the intent of the proposal, which was clear to him. However, the proposed text would, if approved, require complete rewriting by the Editorial Committee. If the Section was willing to place confidence in that Committee, then it could accept the present proposal. The Editorial Committee would act on the understanding that it was a rule – not a recommendation, as use of the verb "should" might make believe. If the intent of the proposal was
felt to be unclear, or if the Section was worried about the possible implications of editorial rewording, then Zijlstra might prefer to have action again postponed until the following day when she could come up with an improved text after consultation with some Editorial Committee members here present. A third option was, of course, to leave the matter for the next Congress to consider.

The proposal addressed the situation in which generic names were based on earlier names of subdivisions of genera, which were then considered as basionyms (although this was not, strictly speaking, covered by the present basionym definition). If such epithets were plural adjectives, they could not be used as generic names, and for that case Zijlstra's proposal only made explicit the present situation. If however they had the same form as a generic name (i.e., if they were nouns in apposition), they could then be so used. Often when this had been done the spelling had been changed (as, by analogy, might have happened upon transfer of an infraspecific epithet to specific rank). Since no name had priority outside its own rank, one could argue that the transferring author was free to change, perhaps to intentionally correct, the spelling, and the generic name might then be interpreted as an avowed substitute name, without parenthetical author citation; but one could also treat the generic name as a correctable orthographic variant, with the spelling of the "basionym" (whose author was then parenthetically cited) prevailing. The Code was not clear on this issue. Zijlstra's new proposal offered a third, more flexible approach, considering all such generic names as transfers, not as nomina nova, but ruling that the spelling to be adopted (both for the subdivisional epithet and the generic name) was to be the one that best suited current usage.

Jeffrey had prepared an amendment to the original proposals, which he hoped the Editorial Committee would take into consideration if the new proposal was accepted. The proposal as it now stood was still relevant to a wider set of situations than it addressed: to all transfers from one infrageneric supraspecific rank to another such rank, or from generic rank to such a rank. Also, Art. 60, dealing with limitation of priority to a given rank, was hardly the appropriate place for the proposed new provision. (Incidentally, the present wording of Art. 60, implying that names had a rank, was defective: it was taxa that had ranks, not names.) The proposed provision was
not an exception to Art. 60, but was relevant to Art. 73.1, and was also an exception to Art. 75.1. To make these points clear, here was the wording of his drafted amendment to Art. 60 Prop. A or B: (1) Renumber Art. 73.1 as Art. 73.1(a). (2) Add a new article 73.1(b): "However, when a taxon of a rank subdivisional to the generic rank is raised to generic rank and the author of the generic name published a name that is derived from the epithet of the name of that taxon, the epithet of the basionym and the generic name are to be treated as variants, both validly published, one of which is considered to be correctable. In choosing which variant to correct, established custom should be followed. If however the epithet of the putative basionym has the form of a plural adjective, no correction may be made and the epithet as such may not be adopted as a generic name (Art. 20.1). The same applies when a taxon of such a rank is changed in rank within the ranks subordinate to the generic rank, except that in such cases the constraint imposed by Art. 20.1 does not apply." (3) In Art. 75.1, delete "and" before "Art. 32.5", and after "terminations)" insert "and epithets treated under Art. 73.1(b), which are otherwise validly published". Also relevant were Art. 6.6, 21.1-2, and Rec. 21B.

Greuter had not grasped the meaning of everything that had been read, nor might the Section, but since it was present in writing the Editorial Committee might look into it at leisure, and would certainly take into account all points in it that were strictly editorial and did not affect the meaning of the New Proposal. Jeffrey was obviously right in stating that the new provision would be misplaced in Art. 60 since that Article did not exist any longer but was to be incorporated in Art. 11. A placement under Art. 73 appeared to be sensible. The question of transfers from one rank to another, within a genus, was perhaps not so crucial as to require an immediate solution; but could not at least the reverse situation to the one presently addressed, the downgrading of a generic name to a subdivisional epithet, be covered? It would be easy just to add the words "and vice versa" in the appropriate place.

Zijlstra had no objection to the latter suggestion, but had hardly any experience with the reverse process. Placing the provision under Art. 73 with a cross reference under Art. 60 was possible, although she had, upon consideration, preferred it the other way round.
Jeffrey added a further editorial suggestion, that since Art. 75.1 concerned valid publication, reference to it should be included under Art. 32.1.

Silva preferred Zijlstra’s original Prop. A, because it avoided the subjective phrase "established custom". In a case known to him, in the algae, 40% of the publications used one spelling and 60% the other, so that it was "established custom" to follow either of the two spellings – and one was where one had started from. Besides, established custom did not guarantee linguistic correctness.

Greuter admitted that there would always be a few borderline cases, but was it not worth while to solve unambiguously a great majority?

Zijlstra knew that some problems remained, but the small group that had met on the previous day’s night had felt that doubtful cases could always be referred for an opinion to the relevant Permanent Committee. She was not convinced that the original Prop. A would be the more suitable alternative even for the algae, of which she had but few examples; but for flowering plants it would be far more disruptive than Prop. B. The new compromise proposal would suit all cases equally well.

Hawksworth felt nervous about this proposal, and was uncertain on whether it would help or hinder fungal nomenclature. In many cases, authors had gone back to the original (basionym) spelling in the last 10-20 years, and there were still two current usages. If this was to pass there should be a clear recommendation added, that if there was dual current usage the relevant Committee should be consulted. Or might one perhaps agree that, in such cases, the Dictionary of the fungi was to be followed?

Demoulin was reluctant to apply such a new rule as long as it was not unambiguously clear that there was an established custom going one way or the other. As the Code now stood, the essential provision was in Art. 75, viz., that only that form of a name that appeared in the original publication was treated as validly published, with a few stated exceptions. This proposal would provide for a further exception to the obligation to respect the original spelling. At present, one had to deal either with nomina nova or with transfer between ranks. If circumstantial evidence showed that a transfer was being made, there was no valid reason for not adopting the original spelling.
Jeffrey had in practice applied the Code in the same way as Demoulin had just outlined. However the question was: which was the original spelling? One could always argue that a change was deliberate, to the effect that the supposed basionym would become a replaced synonym, in which case the nomen novum had its own original spelling.

McNeill pressed this point. The weakness of Zijlstra's proposal was that it did not make the distinction between transfer and replacement, treating all cases as if they were transfers, although this might be contrary to the intent of the author.

Jørgensen also felt that the proposal, with which he had much sympathy, had two weak points: the one just mentioned and the other, its reliance on "established custom". In most cases he could remember, there was no such thing as an established custom. More likely, there were two different schools, one American and one European, quarrelling endlessly over the correct spelling. Usually such cases had by now been solved, but many unresolved ones doubtless remained. While he could not think of a better wording (the Editorial Committee might), he felt uneasy about the proposal.

Demoulin agreed with the proposal on one point: transfer of plural adjectival epithets to generic rank was not possible, these were all cases of nomina nova.

Greuter summarized his understanding of the present state. The Code had a clear ruling, that in the case of new combinations the original spelling was not to be altered; but in the same time it left complete freedom of choice of spelling in the case of a nomen novum. The Code gave no guidance as to whether cases in which a transfer in rank was associated with a change in spelling were to be treated as new combinations or nomina nova. Even the stated intent of the original author, that it be a transfer, was not necessarily sufficient if in the same time he also deliberately changed the spelling: there were then two simultaneous deliberate actions that were in conflict. Statements such as "stat. nov." or "nom. nov." had no binding force under the Code if the context demanded otherwise (see Art. 33 Ex. 8). What mattered was what authors had actually done, not what they thought they had done. When an action resulted in a conflict, as here discussed, a choice had to be made; and
as long as the Code, as was now the case, gave no guidance this choice would continue to be made individually on an ad-hoc basis. The present proposal would give the required guidance, viz., to treat all such cases as transfers while simultaneously setting aside the obligation to maintain the original spelling in cases when this would be disruptive. These were its merits; its shortcomings had also been pointed out, and the Section would now have to decide whether the former were sufficient to outweigh the latter.

Zijlstra replied to McNeill that there were many cases in which the deliberate intent of the author to change the spelling was obvious. Together with Aconogonum, several other sectional epithets ending in -on had been used as generic names ending in -um. In other cases, the generic name and subdivisional epithet were deliberately spelled differently by the validating author, perhaps even repeatedly, in the context of the protologue. She accepted Greuter’s suggestion, to introduce "and vice versa", as a friendly amendment.

Zijlstra’s New Proposal was rejected by a card vote (45.7% in favour, 196 : 233).

Prop. A had been withdrawn.

Prop. B (40 : 119 : 9 : 0 : 13) had been withdrawn.

Prop. C (24 : 131 : 9 : 0 : 14) had been withdrawn.

Prop. D (15 : 141 : 9 : 0 : 12) had been withdrawn.

Article 61

Prop. A (2 : 179 : 1 : 1 : 1) was ruled as rejected.

Article 63

Prop. A (12 : 162 : 4 : 1 : 1) was ruled as rejected.


Jeffrey, feeling that the problems associated with the present Art. 63.1 had been severely underrated in the Rapporteurs’ published comments, moved that this proposal be brought up for discussion, which was seconded. He then explained that Prop. B would bring
about a reversion to the pre-Berlin provision and would thus rescind a previous decision by a Nomenclature Section. Some of the difficulties inherent in the present wording had already been indicated during discussion of Art. 8 Prop. R and S. In order to ascertain what the phrase proposed for deletion meant, one had to combine it with the text of Art. 63.2, unchanged in Berlin, which was not always easy. The post-Berlin situation was very peculiar. A new name published as an avowed substitute had by necessity the same type as the replaced name. If the replaced name was legitimate, however, or if there was an earlier legitimate avowed substitute for it, then illegitimacy would only result if the holotype or syntype or a previously designated lectotype were definitely included. Accepting the narrow definition of syntypes given in the Code, one could seldom say that a name was superfluous and illegitimate when the replaced name was based on original material that was neither holotypic nor syntypic nor known to have been the subject of prior lectotypification. And how about inclusion of a previously designated neotype? In practice, there was no absolute and objective distinction between a neotype and an element chosen from authentic original material. Above the rank of species, the distinction between neotypes and lectotypes was more a matter of interpretation than of fact. At the lower ranks, especially with early authors, it was often impossible to draw a sharp line between holotype, isotype, isosyntype, or syntype, nor was the status of parts subsequently separated from a holotype quite clear. Then there was the question of the status of earlier designated lectotypes that were later superseded under Art. 8.1; and that of mandatory lectotypes that were in serious conflict with the protologue. There might also be the case in which all original material was included, none of which however was in the nature of syntypes. Apparently, as long as he did not mention "the name itself", an author was free to publish a legitimate replacement name for a legitimate name, that could then be typified from the original material of the earlier name. This situation was in conflict with current practice, and also with Art. 51.1. Even citation of the original phrase name associated with the earlier name would not cause illegitimacy, since it was not a name in the sense of the Code. To be fair, these difficulties existed also under the pre-Berlin wording of Art. 63. All
syntypes might be cited, but under more than one new name, and
the replaced name might either not be mentioned at all or only as a
pro-parte synonym, in which case all of the replacement names
would be legitimate while in clear conflict with Art. 51.1 and 53.1.
Inclusion of an automatic lectotype (Art. 22.4) led to conflict with
Art. 63.1, a conflict to be resolved through other proposals (Art. 63
Prop. H and I). If the inclusion of the type of a conserved name did
not by itself cause illegitimacy this would conflict with the spirit of
conservation, since a conserved type took precedence over a type of
any other category defined in Art. 7 (see Art. 14.3-4, 14.9 and 7.17).
The retroactive effect of conserving a type, now consciously negated
in Art. 63, should definitely be admitted. All this made it amply
clear that the remark by the Rapporteurs, that they had found the
Berlin version of Art. 63.1 to work without problems, must have
been based on too superficial a consideration of its implications.
While Prop. B might not be the ideal solution, in practice the Art-
icle so amended would be easier to interpret and less in conflict
with other fundamental provisions of the Code.

Zijlstra resented the Berlin Section having set up a Special Com-
mittee while in the same time deciding positively on the most relev-
ant issue. The provision proposed for deletion, that might perhaps
work at the species level, was difficult to apply at the rank of genus.
The example she had given on the previous day, though faulty
through omission of a sentence, was in fact relevant: an author had
proposed a substitute name for a later homonym, but that substitute
was superfluous because another avowed replacement for the same
name existed already; since he did not include "all syntypes" of the
replaced name, how could one apply Art. 63.1 and declare the
second substitute name to be illegitimate?

Greuter was unconvinced that the Rapporteurs' comments had
been quite as unqualified as had been claimed. He would disagree
with Jeffrey on several of his points, but these being highly technical
he would rather discuss them in private and would here stick to the
main issues. Reversing a decision taken at the previous Congress
was something one should never do lightly, certainly not when the
mail vote was so heavily negative. It was strange that Jeffrey, having
just let lapse his proposal to do away with Art. 63 altogether (Prop.
A), now fought for widening its application. There were many
names which only thanks to the Berlin amendment were not now illegitimate. One remark in the Rapporteurs' comments might be felt to be cryptic, referring to editorial rephrasing "along the lines suggested in Art. 22 Prop. A", since the corresponding text had not been reproduced in the "Synopsis" although it was in the original Prop. (219) [Taxon 41: 783. 1992]. That proposal having been accepted, the Editorial Committee had been given mandate to replace the present text, in Art. 63.1, by "... definitely included the holotype or original type, or all syntypes (Art. 7.7), or all elements eligible as types (Art. 10.2), or the previously designated type ...". That rephrasing took care of the difficulties caused by the definitions of the various type categories in Art. 7 and their non-applicability at the higher ranks, governed by Art. 10. This was a problem of terminology, and all those who in applying Art. 63 until now had used their brains rather than going into the semantics of type category definitions knew what its meaning was. In practice, at the level of species and below, illegitimacy was almost exclusively caused by "citation of the name itself". At the higher ranks, it could also be caused by inclusion of all elements eligible as types. This was indeed the way in which the Article had been applied in the past, and recognition of this fact had induced the Berlin Section to so readily accept the Rauschert proposal while in the same time setting up a Special Committee to look into other, yet unsettled matters relating to retroactivity and illegitimacy.

Jeffrey had thought it fair to the members of his Subcommittee to bring to the notice of the Section some of the points they had raised but could not be included in the printed report. It was true that returning to the former wording would make more names illegitimate, but then, the Section had now decided to widen the option for conserving names of species. The clarified wording read out by the Rapporteur was welcome, although it might not remove all problems entirely. Yet, he was now willing to let the matter rest there.

Prop. B was rejected.

Prop. C (16 : 156 : 5 : 0 : 1) was ruled as rejected.

Prop. D (16 : 146 : 6 : 1 : 3) was ruled as rejected.

This proposal had, as McNeill explained, been accidentally omitted from the printed mail ballot, which only a minority of those voting had noticed. The estimate he had provided had only indicative value and should not be used to rule the proposal as rejected.

Funk mentioned that the Smithsonian Institution staff members had voted yes on this proposal on the understanding that Nicolson would move an amendment to it from the floor.

Nicolson explained that the intent had been to combine Prop. C and E into a single proposal but, in view of the heavily negative mail vote, he had abandoned the idea.

Prop. E was rejected.


Zijistra had made a terrible typing error, writing "legitimate" instead of "illegitimate". She did not wish, however, to have the proposal picked up again.

Prop. F was ruled as rejected.

Prop. G (1:171:4:0:1) was ruled as rejected.

Prop. H (2:168:5:0:2) was ruled as rejected.

Prop. I (5:148:21:0:2) was ruled as rejected.

Prop. J (7:161:8:0:1) was ruled as rejected.

Prop. K (27:142:8:0:1) was ruled as rejected.

Prop. L (4:133:8:5:20) was ruled as rejected.

Prop. M (7:131:7:5:20) was ruled as rejected.

Article 64

Prop. A (71:78:27:0:0) with the friendly amendment fore-shadowed in the Rapporteurs' published comments, to add the word "family", was accepted.

Prop. B (11:155:10:0:1) was ruled as rejected.
Silva accepted the Rapporteurs' suggestion, to transform the last sentence of Art. 64.4 into a Note rather than deleting it, as a friendly amendment.
Prop. C, thus amended, was accepted.

Prop. D (26:68:81:0:1) was referred to the Editorial Committee.

Prop. E (10:32:134:0:0) was referred to the Editorial Committee.

Prop. F (0:141:13:0:22) was ruled as rejected.

Article 65

A motion to bring this defeated proposal up again having been seconded, Hawksworth explained that it belonged to a package aimed at bringing the biological Codes closer together. One problem that the Rapporteurs had not mentioned in their published comments was that of homonymy in databases. Although one could cope with such homonymy by specifying the main groups of organisms, one did not necessarily do so unless one knew beforehand that there was a homonym in existence. The main purpose of the proposal, however, was to give some hope to workers in protistan groups, who were even considering having a Code of their own and for whom homonymy was the main problem. Although the proposal was not a panacea for the past, it would help for the future. By the end of the Century, there would certainly be machine-readable generic lists for all groups of organisms, that could be easily searched for potential earlier homonyms. The proposal should therefore be approved, but with an additional clause, similar to the one decided for the new registration procedure (Art. 32 Prop. B): "subject to the approval of the XVI International Botanical Congress".

Greuter pointed out that, whereas some of the Rapporteurs' concerns would be taken care of by such a clause, others would not. As worded, the provision would still appear to tell zoologists what they
had to do, which was no business of the botanical Code. The matter was of course important, and there was a risk for it to be neglected by the Section due to the absence of specialists working on those funny marginal groups shifting to and fro between the kingdoms. Would it not be a better idea to authorize the setting up of a Special Committee on these problems, rather than writing anything into the Code? Such a Committee could, in contact with representatives of the other Codes, explore avenues of harmonization and come up with compromises that could be proposed in parallel for implementation under the different Codes.

Hawksworth had had in mind to suggest that the General Committee should look into ways of achieving a greater harmonization between the Codes. If the suggestion now was to establish a Special Committee with a broader remit than just looking into the homonymy question, then he would certainly support it, and accept having his proposal referred to that Committee.

Greuter agreed that the Special Committee should be given a broad mandate, to investigate all borderline problems between the biological Codes, and the special problems of all borderline groups, and eventually all questions of harmonization of the Codes that were felt to be soluble.

Hawksworth was happy to move the establishment of a Special Committee on Harmonization of the Codes, with the above mandate, and his motion was seconded and carried.

Prop. A was referred to that Special Committee.

**Article 68**

**Prop. A (27:26:127:0:0).**

Henderson failed to understand why the Rapporteurs, when he and Pedley had merely proposed to replace one adjective by another, had suggested that they had their vocabulary wrong. He was however happy with the wording suggested by the Rapporteurs, and agreed to leave the final decision with the Editorial Committee.

Ahti supported the wording as originally proposed. A combination in any infrageneric rank (subdivision of a genus, species, or infra-
specific taxon) could be legitimate even when its final epithet was placed under an illegitimate generic and/or specific name. This was implicit from the definition in Art. 6.4, since there was no provision in the Code defining such combinations to be illegitimate. Actually, Art. 68.1 and 68.2 were hardly more than Notes, although for practical reasons their present status was preferable.

Greuter expanded on the reasons behind the Rapporteurs' published comments. Art. 68.1 and 68.2 were parallel and designed to be complementary. The first dealt with specific names in which the epithet was associated with an illegitimate generic name. Infraspecific epithets could not be associated directly with a generic name, but only with a species name, and that situation was covered by Art. 68.2. What was wanting in Art. 68.1 was a reference to epithets in names of subdivisions of genera. Adding this reference was not strictly speaking editorial, since it widened the application of the rule, but such a wider coverage had been taken for granted by everybody in the past, so that this change was certainly welcome. The proposal should be accepted with the amendments suggested by the Rapporteurs, and which Henderson had accepted as friendly amendments.

Prop. A, thus amended, was accepted.


Ahti asked for discussion of this heavily defeated proposal, and his motion was seconded. He explained that the parenthesis "autonyms excepted" was indeed superfluous. Art. 22 and 26 made it perfectly clear that under illegitimate generic and specific names no autonyms were formed. Since they did not exist, their mention in Art. 68 was unnecessary.

Henderson, on the contrary, agreed with the Rapporteurs' published comments and accepted the negative mail vote. There was, however, a second element in the proposal that the Editorial Committee should take care of, to add the word "species" after "illegitimate".

Prop. B was rejected.
Article 69

Prop. A (3:172:2:0:5) was ruled as rejected.
Prop. B (58:106:14:0:1).

Greuter announced the results of Permanent Committee votes on this proposal: Committee for Fungi and Lichens opposed by 4:5; Committee for Pteridophyta opposed by 0:8; Committee for Bryophyta, 2:1 in favour for the time being.

Brummitt saw this proposal as an essential complement to what had been decided under Art. 14. There had been a heavy vote in favour of opening conservation for any specific name, and one might wonder why one still needed the present proposal. However, allowing for some deficiencies in the proposed wording, the idea of simply rejecting a name had several advantages. There were a number of untypified names hanging about in the literature, and others whose type was ambiguous. Such cases had been submitted to the Committee for Spermatophyta that, while in sympathy with them, was unable to reject the names because the Code did not permit it. An example was Nymphaea pentapetala, neotypified by Reveal on the basis of circumstantial evidence by a plant with many yellow petals when the original description had five white petals. It was to be hoped that the Section would agree to provide a facility for getting rid of such silly names. There were other cases in which names had become ambiguous since their type had been interpreted in different ways. Whereas it was now possible to conserve the desirable name against the ambiguous one, there was no mechanism for fixing the type of a rejected name, which might still be in dispute, so that ultimately the rejected name had perhaps to be reintroduced for a different taxon. Acceptance of the present proposal would make Prop. C-H irrelevant, the wording of which had proved to be confusing for many. Prop. B certainly had for it the advantage of simplicity.

Greuter explained the way in which Art. 69 presently operated, since this was often misunderstood – perhaps because it was, for historical reasons, arranged in an illogical sequence. The most essential and most powerful provision was Art. 69.4, which should logically stand at the beginning. In short, an ambiguous name was
not to be used under the *Code* unless there was a formal Committee decision to the contrary. The only way to bring an ambiguous name into use was by submitting a proposal to formally reject it to an unsympathetic Committee that would reject the proposal. Prop. B consisted of two halves, the first of which, corresponding to its first sentence, deserved full support. It would enable to propose for rejection any name that would cause a disadvantageous nomenclatural change. The second sentence of the proposal was going one step too far, postulating that names proposed for rejection were, pending a decision, not to be used. Anyone disliking a name for any reason might then make a proposal, and pending its consideration the name would be banned from use irrespective of the merits of the case. Maintaining Art. 69.4 as presently worded (since it was unchallenged), and adding after it the first sentence of Prop. B, would result in an extremely powerful, very flexible and completely adequate Article. Since the proposer was not there to accept a friendly amendment, a motion from the floor to delete the second sentence of Prop. B would be appropriate. Such amendment was moved, seconded and carried.

**Prop. B**, thus amended, was accepted.


**Perry** pointed out that, since Art. 14 Prop. B had passed, there was no need any longer for Prop. C, nor for the following proposals, nor indeed for the whole paragraph they affected. In the past, Art. 69.3 had only had a function in the case of specific names, since for generic names Art. 14 had sufficed. She was therefore happy to withdraw Prop. F.

**Jeffrey** was uncertain as to the effect of previous decisions on his Prop. C. The proposed deletion of rank designation would widen the option, not only to reject but to conserve names. If the provision remained unchanged, its wording would be too restrictive now with regard to rejection, Prop. B having been accepted. If Prop. C was accepted, this point would be covered, but a conflict with Art. 14 would arise as far as the conservation option was concerned, Art. 14 Prop. A having been rejected. He was uncertain whether his proposal was now superfluous, but if not, some amendment of it was perhaps required.
McNeill could see no possible conflict, Prop. C being entirely dependent on the fate of Art. 14 Prop. A; the reference to rejection under Art. 69.3 referred to rejection against a conserved name under Art. 14. Conservation, however, was still restricted to names in the ranks of family, genus and species.

Prop. C was thereupon withdrawn.

Brummitt detailed the proposals which, he thought, had become superfluous consequent to adoption of Prop. B. Under the unrestricted option to reject nomenclaturally harmful names, Prop. D, dealing with a very special case, was certainly unnecessary, and so was his own Prop. E. The other proposals might require careful consideration, perhaps by the Editorial Committee, unless a decision could be postponed to the following day.

Perry reiterated her plea for deletion of the whole Art. 69.3. Brummitt concurred that it was now superfluous, and so did McNeill. A motion to delete it was seconded and carried.

Brummitt wanted it to be clarified that deletion included Note 1, since this depended on Art. 69.3. It was agreed that the Editorial Committee would take care of this.

Demoulin, being a Belgian (a people whom the French considered to be slightly stupid and slow-moving), disliked the rushed deletion of a whole provision upon a proposal from the floor. In particular, he spoke for maintenance of the footnote linked to Art. 69.3, which referred to a post-Berlin situation on which not every Committee might yet have completed its homework.

[The following additional proposal was presented, discussed and acted upon towards the end of the final session, between the Stuessy Resolution and the Stuessy Motion.]

Barrie moved a New Proposal, to modify Art. 69.2 by adding the words "including considerations of typification" at the end of the second sentence. The major surgery on Art. 69 had had one unfortunate side-effect, to eliminate the requirement for typification of names being proposed for conservation or rejection. The proposed addition would instruct authors of rejection proposals to discuss types or type material. He had at first envisaged to make previous
typification of names proposed for rejection mandatory, but had weakened this on the Rapporteur's advice since it was not always possible (or sensible) to formally typify such names. However, forcing authors of proposals to at least discuss typification would ensure their having done their homework properly, would improve the quality of proposals, and reduce the number of spurious ones. The motion was seconded.

Greuter commended this late addition. It was in the interests of the Nomenclature Editor of Taxon, of Nomenclature Committees, and of nomenclature in general, to have good, well-presented, well-argued proposals.

The New proposal was accepted.

Prop. D (40 : 126 : 13 : 0 : 1) was rejected.

Prop. E (128 : 42 : 8 : 0 : 3) was withdrawn.

Prop. F (36 : 131 : 12 : 0 : 1) had been withdrawn.

Prop. G (35 : 132 : 12 : 0 : 1) was withdrawn.

Prop. H (26 : 140 : 11 : 0 : 1). Perry drew attention to the amended version of this proposal that had been published elsewhere (in Kew Bull. 48: 419. 1993). There was, however, no formal motion to bring up again this heavily defeated proposal.

Prop. H was therefore ruled as rejected.


Stearn confirmed that the proposal stated current practice. However, the Editorial Committee should transfer the names now included in the provision to an example. The provision should not legislate on these specific cases only, others might be added.

Greuter asked whether Stearn's point could be taken care of by replacing "These" by "Such", at the beginning of the second sentence.

Stearn however moved that the names in the second sentence be transferred to an example, so as not to give the impression that these were the only names concerned.
Zijlstra was afraid that more and more cases might be treated under such a provision, and preferred to limit it to the presently stated names.

Nicolson enquired whether the "protype" concept would apply in the case of these names.

McNeill replied negatively. The intent was that these names, which had been used for apomictic or autogamous complexes, should not be applied to any definite taxon at the microspecies level. Tradition indeed supported use of these old names at the aggregate level only. But while it was possible to mention the four concrete cases in an example, this should then definitely be a "voted example", so that future additions would have to be explicitly proposed.

Greuter asked whether Stearn's motion was seconded. This being the case, discussion concentrated on the proposed amendment.

Forero quoted from a letter of Voss to McNeill: "I vote yes but would welcome the 'more useful' alternative provision suggested by the Rapporteurs. I am uneasy about ruling that 'These names are ...', implying a closed list. Does the celebrated case of Polygonum aviculare fit here, too?"

Brummitt was not strongly opposed to the amendment. But the original reason for the proposal, whose main author was Kent, had been the specific case of Euphrasia officinalis which some author had chosen to use at the level of microspecies, publishing subspecies under it. Kent, realizing that something had to be done, had then gone through all the cases he knew and believed that the four ones here mentioned were the only ones. Addition of Ranunculus auri-comus had been suggested since, and both Kent and he agreed that this was appropriate. Limitation to specified cases had thus been deliberate, and certainly the Polygonum aviculare example was of a different kind, since that name had been applied at the microspecies level in various floras.

Barrie opposed the amendment. Unless the names concerned were definitely listed, there would be cases in which some authors might arbitrarily choose to limit the use of a name to the aggregate level and take up a later name for the "typical" microspecies.
Faegri warned against making this a definitive list without some more consideration. Apart from the *Ranunculus auricomus* example, there was the perhaps most classical of all, *Erophila verna*.

Ahti supported a definite listing of the names concerned in the *Code*, otherwise there would be no end of them. If this was an open-ended list, numerous cryptogamic examples could be added, when now normally one used qualifications like *sensu lato*.

Jørgensen recalled the case of *Lecanora subfuscusca*, which lichenologists would have liked to maintain in an aggregate sense but which, since that option did not exist, they had had to reject. Perhaps an open-ended list was indeed desirable, but then there should be a mechanism for adding names to it.

McNeill observed that, under the unamended proposal, this mechanism existed in the form of a proposed amendment to the *Code*. Perhaps it was the wisest thing, under the circumstances, not to make additions too easy.

Demoulin agreed with McNeill. Replacing "These" by "Such" would encourage people to use their individual judgement, as under past wordings of Art. 69. Of course, making a new Appendix, parallel to that for rejected works, was also a possibility.

Brummitt accepted the addition of both *Ranunculus auricomus* and *Erophila vema* to the enumeration as *friendly amendments*. However, the list should be restricted at present to these six names.

Stearn clarified his proposed amendment: the proposed new provision should be limited to the general statement (its first sentence). The names now in the second sentence should be enumerated in an example, reflecting current usage, to which everyone was free to add. Leaving the second sentence in, to begin with "Such names", was not a good form of presentation.

Johnson disliked the idea of having non-committal examples and leaving the application of the rule open-ended. However, one of the Rapporteurs' published comments was spot-on: the proposal was extremely Eurocentric. Who was to say that there were no situations similar to those having worried European botanists in many other parts of the world? *Poa caespitosa*, of Australia and New Zealand,
was a case in point that might throw nomenclature of that group into chaos. Certainly each case would have to be legislated upon individually, if there was to be a rule of this kind.

Greuter could accept the proposal with Stearn’s amendment, but not its original form. The latter was clearly biased, providing special legislation for a limited number of phanerogams, all of them centred on Europe, when it was now clear that cryptogamic groups and plants in other continents presented exactly the same problems. The original selection of four names had been worked out by two competent botanists who thought it to be representative for their limited domain of experience, yet two suggested additions from that same domain, casually made here from the floor, had already been found also to qualify – a 50% increase. Just asking a few fellow-botanists not in this room would doubtless bring to light additional cases. *Stipa pennata* might qualify, again a Eurocentric phanerogamic assemblage. With Stearn’s amendment, it would be explicit that the rule was to apply to the given examples but it would also be clear that it had, potentially, a wider application. This being a voted example, additions to it, and therefore extension of the rule, would have to be submitted as published proposals and accepted by a Nomenclature Section.

Demoulin did not resent the bias in the present selection. The first two "nomina specifica conservanda" were the names of wheat and tomato which were both cultivated phanerogams, but everybody accepted that *Agaricus bisporus* might be added later on. The important point was not balance in geographical or taxonomic coverage, but a satisfactory procedure. The choice was between having a rule illustrated by some examples but which everybody could apply at his or her own judgement, or a provision requiring that each case in which it was to apply be listed individually after having been examined and approved by a Committee. The question of where to list these names, in an Appendix or in an example or in the provision itself, was less important. To avoid the impression that the selection was final, a list in an Appendix might be a good thing.

McNeill disagreed with Greuter. It was vital to restrict this provision to specific cases, otherwise there would be a clear conflict with Art. 53, to which the proposed provision would be an exception. Some
change of the proposed wording was however desirable, to make it clear that the present, biased selection was open to later additions. Making an exception to Art. 53 was justified only for names that had been used exclusively in an aggregate sense but which, consequent to their typification, would also be the correct name of a segregate species, creating instability of usage.

Greuter pointed out that the Section had just agreed to delete Art. 53 [as by Art. 11 Prop. B].

Jørgensen felt confused, as others might feel, and asked for a clarification. Stearn was right that this should be a general rule; but what was the status of the listed examples? Was the field to be wide open for applying the rule to any similar case? There had been much support for a definite list, but if this was the Section’s mind, there should also be a mechanism for adding to that list. Leaving the options wide open was dangerous.

Greuter referred to the parallel case of works now to be listed as rejected under Art. 23, or in a separate Appendix. In previous editions of the Code such names were mentioned in voted examples, without anyone having tried to widen the rule to other than the explicitly mentioned works. Under Stearn’s amendment, it would have to be made clear that the example gave an exhaustive list of those cases which, for the time being, fell under the rule.

Ahti wondered whether it was not now feasible, under the new unrestricted conservation option for specific names, to conserve the name of the segregate species against the old aggregate name, e.g., *Alchemilla acutiloba* against *A. vulgaris*, so that it would be clear that the name *A. vulgaris*, when still used, referred to the aggregate. The same applied to *Taraxacum officinale*, the type of which belonged to some Lapland agamospecies of the *T. croceum* group.

Faegri noted this to be a good example in parallel to registration: a new concept to which one could not give an absolutely appropriate form from the beginning. When this developed and when one could see how wide the field really was, an example would probably not be the appropriate format, and a separate list might become appropriate. For the time being, Stearn’s proposal of an example was ad-
equate. Something might be said for the European bias of the present selection: they were all names that had been in use for a very long time, and it was their being typified now that caused the confusion.

Barrie was afraid that Stearn's amendment might open the provision to abuse. Some might use it to reject familiar species names on the excuse that they also applied to an aggregate. In the long run an Appendix was presumably needed, and a mechanism by which names could go on it – but a quarter to six in the evening might be the wrong time to start working on this. One should now vote on the present amendment and proposal, and look for ways of widening the field later. Having modified the Code so much during the past days, one should not worry too much about having to modify it again in the future.

Taylor questioned the Eurocentric nature of these species. Their nomenclatural types were perhaps from Europe, but some were weeds occurring elsewhere, and Flora writers in other countries would welcome the option of using the aggregate names without having to identify the actual microspecies they were dealing with.

Johnson, by the term Eurocentric, did not want to imply that those species were of no concern to botanists in other parts of the world, but certainly they had been described from and studied in Europe, and were a preoccupation for European botanists. The same might now happen elsewhere in the world, and this proposal might open the floodgates.

Briggs favoured the provision being kept restricted to legislated names, as originally proposed, but suggested that a minor editorial rewording might make it sound less Eurocentric: "This applies to" would be preferable to "These names are".

Stearn's motion was defeated, and the discussion reverted to the original proposal.

Silva wondered whether the word "Certain" at the beginning of the proposal did really imply, as one was led to think, that there were names that had long been used for aggregates but might nevertheless be used for a segregate species.
McNeill explained that, indeed, this was exactly the intent. The case of *Polygonum aviculare* had been mentioned by Forero, quoting Voss. There was an aggregate with two segregate species and no problems of typifying the name. Comments so far had indicated that a distinction was to be made between aggregates with numerous, ill-defined components, in which it was inappropriate to use the aggregate name for the segregate including its type; and other cases like *P. aviculare* where there was no such confusion and one of the few segregates could bear the aggregate name.

Jeffrey, while not opposed to the proposal in general, objected most strongly to listing *Taraxacum officinale*. Much work had been put into typifying the name *Leontodon taraxacum*, being the replaced synonym of *T. officinale*, so that one now knew how to apply the name and was moving away from its old misapplications. Including the name here would discourage the search for the correct name at specific rank for what was known as the widespread, weedy *T.* sect. *Ruderalia*. A second difficulty was that the proposal referred to use "only in this broad sense", whereas *T. officinale* had been used in a fairly considerable number of different broad senses. The biological entity referred to under this name in e.g. the *Flora of South Africa* did in no way coincide with that for which it was used in Britain or Western Europe. It was inappropriate to maintain a name, even for an aggregate, when its information content was so problematic and unfathomable. The motion to amend the proposal by deleting *T. officinale* was not, however, seconded.

Faegri remarked that there was no better way of making nomenclature the laughing stock for the rest of the biological world than by telling, say, gardeners that their weed was not *Taraxacum officinale* because that was a little plant of Lapland.

Stearn had three segregates of *Taraxacum officinale* growing on his lawn, which he could distinguish but which still were *T. officinale*. In Britain at least 200 segregates had been named, and perhaps an equal number in Scandinavia. As an aggregate name, this was certainly needed.

Jeffrey reiterated his point that *Taraxacum officinale* was ambiguous even at aggregate level. If names were to go on that list at all, they
should be correct names having a definite meaning. The name \textit{T. officinale} did not convey any information on the properties of the plant group so named. Following this comment, the necessary number of seconders for the \textbf{motion} to delete \textit{T. officinale} from the proposal were found.

\textbf{Demoulin} spoke against the amendment. \textit{Taraxacum officinale} was among the first names one would wish to include in a list such as here proposed. It was no real problem that local floras used the name for just the chance selection of microspecies found on their territory. What mattered was that applied botanists could use a familiar name in a familiar aggregate sense, even though they might apply it to only a subset of all segregates.

Jeffrey's \textbf{motion} was \textbf{defeated}.

\textbf{Demoulin} pointed out that, unless the Section had also agreed to delete Art. 62 [which it had not], there was still a provision in the \textit{Code} conflicting with the newly proposed one, which instructed one to reject some names merely because they were inappropriate or disagreeable. This was an important exception that had to be very clearly stated and limited.

\textbf{Prop. I}, as \textbf{amended} by the addition of \textit{Ranunculus auricomus} and \textit{Erophila verna}, was \textbf{rejected} by a card vote (50.5\% in favour, 206 : 202).
Article 72

Prop. A (6:168:3:0:1) was ruled as rejected.

Prop. B (10:66:102:0:0) was referred to the Editorial Committee.

Article 73


Taylor introduced Prop. A-F. The Berlin Congress had declined to set up a new Special Committee on Orthography. He and Brummitt had therefore collected examples as they came up, mainly from staff and visitors at Kew, of which they had presented a synopsis (in Taxon 39: 298-306. 1990). Correspondence with a considerable number of botanists ensued, of which the examples now proposed were the result. Hopefully, including them in Art. 73 would give the necessary guidance on what to correct or not to correct. Lack of clarity of Art. 73.1 had been of concern to many, and some had even chosen to ignore the qualifications in it that followed after the first comma and did not accept any corrections of the original spelling at all. The mail vote reflected that some of the proposed examples were more controversial than others.

Greuter assumed that the idea behind this series of proposals was to find out, by way of a poll, which corrections were generally felt to be acceptable and which ones were not. In this way, through a series of voted examples, users of the Code could learn what the majority of those working in the field felt to be correctable misspellings. Attempts at legislating exhaustively, in Art. 73, on the correction or maintenance of spellings had failed in the past, and would hopefully fail in the future. Clarity would gain even more if some examples of what was not correctable were added, and the Section might
consider whether those of the proposed examples that were not accepted should not then be incorporated as examples of non-correctable spellings.

*Prop. A* was accepted.


Johnson believed that the proposal was based on an imperfect knowledge of Greek, but there was no time to debate this question here. Pending clarification of the question, the proposal should be rejected.

Jeffrey held matters of derivation of second in importance to the question of convenience for users. The corrected spelling had, in this case, come into general use in literature on African *Compositae*.

Johnson explained that there were two linguistic aspects: first, the absence of the *h* after the *c*, which was a plain error; second, however, there was a Greek prefix *pollacho-* so that the correction to *polytoma* was far from obvious. This could be debated at length, but perhaps not here. Since the example was dubious, it should be left out.

Demoulin suggested that the example might be referred to the Editorial Committee, which would then decide on the basis of evidence submitted by both Johnson and the proposers.

Greuter disliked the idea of burdening the Editorial Committee with such responsibility. The Section should vote yes or no, and the Editorial Committee would still be free to select an exactly equivalent but more appropriate example instead. Johnson’s point sounded convincing: such examples should certainly be unequivocal.

Chaloner pointed at the two levels of argument in the discussion. Jeffrey had supported correctability on the basis of usage in the literature concerning that concrete case. Others spoke of examples of the kind of corrections that were allowed in the spelling of names in a general way. Certainly, the latter aspect was the one that was relevant here.

Zijlstra, on the contrary, thought that the time when the correction had been introduced was most relevant in the particular case. If the correction had been made virtually immediately, she would accept it; if it was introduced after 10 or perhaps 100 years, she would not.
Hawksworth suggested to Jeffrey, either to produce his own list of names in current use in the *Compositae*, or to propose the desirable spelling for conservation, which was now possible. It was inappropriate to introduce this confused case as an example into the *Code*, and he would certainly vote against it.

Prop. B was thereupon withdrawn.

Prop. C (70:83:26:0:0).

Faegri objected that, whereas in the other examples the correctable spellings were plainly erroneous and in themselves meaningless, *callistegioides* was a correct, meaningful word. The case that it was a misspelling in the context of this concrete example was not convincingly argued.

Greuter agreed that, while in the concrete context *callistegioides* might result from a misspelling of the generic name *Calystegia*, it was not by itself incorrectly spelled. It might even result from a deliberate correction of the spelling of the generic name.

Brummitt confirmed that, indeed, the author of *Bignonia callistegioides* had explicitly so named the species in view of its similarity with "*Callistegia*", thus misspelled (not "corrected", as the Rapporteur had said).

McNeill suggested an amended wording, by replacing "an incorrect spelling" by "a misspelling". Brummitt accepted this as a friendly amendment.

Demoulin was somewhat afraid of introducing this example. Should it be disallowed to form an epithet from a generic name and use a spelling that, while grammatically correct, differed from the nominally correct spelling of that name? Without judging on the present case, he could think of instances in which this would be appropriate. In his (considerable) experience with questions of spelling he had found that each individual case had its own merits, and that before correctability of an original spelling could be decided on the argument would normally run over several pages. This proposal, again, should be referred to the Editorial Committee.

Nicolson pointed out that mycologists already had their particular provision, Rec. 73H, that insisted on correcting epithets to make them agree with the correct spellings of the corresponding generic
names. In a sense, the Section was now voting on whether or not this policy should be extended to groups other than fungi.

Zijlstra mentioned a large group of names, in fossil plants, that were derived from generic names by the addition of a second element like -xylon. Sometimes incorrect variant spellings had been used in compounding, e.g. Anona instead of Annona, but as far as she knew the original spelling of the fossil name was not therefore altered.

Brummitt observed that these were generic names, whereas all the present examples referred to specific epithets. He had cautioned very strongly against correcting generic names in his earlier paper.

Chaloner disliked the example. There were also many epithets in fossils that had been derived from generic names that had variant spellings. Allowing by an example the correction of such variant spellings, that were not in themselves incorrect, to fit the nomenclaturally correct spelling of the generic name would be harmful. All manner of plays had been made on how to derive epithets of fossils from the names of extant plants.

Brummitt took (he thought) Chaloner’s point and rejected the former friendly amendment, reverting to the original wording. Calystegia, a conserved name, had only one correct spelling, and Callistegia was indeed incorrect. However, when different variants of a name were possible one should not make a change.

Demoulin, while disliking Rec. 73H (like all other Recommendations turned into a rule), could live with it because it addressed the very specific case of parasitic fungi living on a given host plant, with which they thus had a strong and immediate link. In the general case of epithet formation, the link with the name-giving plant was quite loose, and there was no good reason to start correcting such epithets and give raise to the usual controversies that would ensue.

Greuter welcomed Brummitt’s reversal to the original wording, that qualified Callistegia as an incorrect spelling. The question now clearly was whether to introduce an incorrect example into the Code.

Prop. C was rejected (and later referred to the Special Committee on Orthography).
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Chaloner raised the point that had been initially made. Since this had been rejected, would it not be a good example of a correction not to be made? Just such examples were needed, they were the other side of the coin. He so moved, and the motion was seconded.

Johnson objected that the mere fact that something had not been accepted did not necessarily make it objectionable. He still felt that Bignonia callistegioides was correctable, for exactly the reasons given by Brummitt & Taylor, that it was a mistake by the author and not a deliberate deviating spelling, nor a case in which variants existed from which to choose.

Greuter explained that the motion, as he had understood it, was not just to take the present wording and put it into the negative, as by merely changing "is to be corrected" into "is not to be corrected". The example would have to be rephrased, and a new rationale given, something like "Bignonia callistegioides is an intentional and linguistically correct spelling, and although this differs from that of the nomenclaturally correct spelling of the generic name Calystegia, it is not to be corrected."

Taylor noted that this would be contrary to Rec. 73H. Incidentally, through one of the examples given there, the Code had in effect ruled that the spelling Albizia was to be accepted in preference to Albizzia, a matter that had been very controversial before.

Barrie asked for a clarification of the motion.

Chaloner, rather than offering an explicit wording, preferred his motion to instruct the Editorial Committee to introduce the example as something that was not to be corrected, and to rephrase it accordingly.

Funk felt that an example that caused so much disagreement between people who knew so much about the Code had no place in the Code.

Nicolson agreed, and asked the proposers whether they viewed their examples as mirror-image cases, to be disposed of in one way or the other, or would rather see them lapse unless accepted.

Greuter asked whether the Section, at this stage, might wish to reconsider the former negative vote, at Berlin, against authorizing
the set-up of a Special Committee on Orthography. Six years without an Orthography Committee had been a long time, and the present set of proposals appeared to result from a corresponding feeling of frustration.

Demoulin, having been Secretary to the previous Committee on Orthography, had declared in Berlin (after having worked hard for six years and handled kilograms of documents for very little concrete outcome) that he would never again work on an Orthography Committee. But if there were still persons interested in matters of orthography, they would certainly profit by taking into account the work done by that post-Sydney Committee. They could come to Liège and consult the archives there, or carry them away if they brought along a big enough suitcase. He thought that Art. 73 was a hopeless matter, a sleeping dog that one should let lie, and although there were still examples that could arouse him he could see little merit in a new Special Committee being set up.

Taylor explained that these proposals were the result of questions raised by colleagues, and of his endeavour in helping them to reach a decision on individual cases. Even rejection of the proposed examples was helpful, because it helped to draw a line between where to correct and where not to correct.

Ahti thought that botanists tended to neglect the basic rule, implicit in the Code, that names should not be spelled incorrectly. He felt there was a general tendency not to correct an error if it was big enough. He therefore favoured inclusion of the proposed examples in the Code.

Johnson, while sympathizing with Demoulin's feelings, knew that questions of orthography, however trivial and annoying, arose all the time and caused a great deal of trouble. Brummitt & Taylor's exercise was useful in itself, but a meeting of this kind was not the right forum to debate and decide on the appropriateness or otherwise of individual examples. In spite of Demoulin's fine job, the last Committee on Orthography had not worked very well. There was indeed a good case to be made for again establishing a Special Committee on Orthography, and if there were persons willing to serve he would be happy to so move.
Chaloner, on an afterthought, had increasing misgivings about his own motion. It looked as though grabbing a positive proposal from Kew and turning it round and hitting them over the head with it. He now wavered on whether this was a good example of what not to do, and therefore withdrew his motion. Perhaps the Editorial Committee could come up with a few examples of what clearly should not be done.

Johnson’s foreshadowed motion to authorize the set-up of a Special Committee on Orthography was seconded and carried.

Greuter suggested that, except for the clearly accepted Prop. A and the withdrawn Prop. B, all other proposals on Art. 73 (including Prop. C, already disposed of), having received a very split mail vote, should go to the new Special Committee, except for Prop. L, M and N on which direct action might be appropriate.

Prud’homme van Reine asked for an individual vote on each proposal, having his institutional instructions on how to vote. This might give some guidance to the new Committee, in addition to that provided by the mail vote.

Trehane pointed out that Prop. G also required special consideration, which was granted.

Prop. D (77: 78: 23: 0: 0) was referred to the Special Committee on Orthography.

Prop. E (88: 66: 27: 0: 0) was referred to the Special Committee on Orthography.

Prop. F (91: 64: 23: 0: 0) was referred to the Special Committee on Orthography.


Trehane explained that the diaeresis and the ligature were pronunciation devices and were not technically a part of orthography. He was strongly in favour of the removal of both the ligature and the diaeresis from Latin plant names. They were a wretched nuisance, disliked by editors and unused by authors. A rule outlawing the use of both these devices would provide stability and be welcomed by many.
Greuter explained that what was new in the proposal was the second sentence. This included the verb "should" and was thus worded as a Recommendation. Following the voting instructions by the Rapporteurs, a yes vote (which had prevailed) indicated that this should be a Recommendation. The (minority) "Ed. C." vote favoured the maintenance of the sentence in the proposed place, with "should be" replaced by "are to be". In view of the mail vote, and unless an amendment was approved and carried, the Section would vote on introducing the second sentence as a new Recommendation.

Brummitt, as the proposer, asked that the position of the sentence in the rule be maintained, with "should be" replaced by "must be".

Greuter, accepting that this was now the proposal before the Section, moved to amend it so as to turn it into a Recommendation, such as the mail vote would support. His motion was seconded, but since the vote was unclear and a card vote would have been needed, he chose to withdraw it.

Barrie wondered what had happened to Trehane's suggestion of getting rid of the diaeresis. This had not, however, been taken up.

Demoulin, while recognizing that this was a matter of typography not orthography, stressed the usefulness of such signs for indicating the correct pronunciation, particularly at a time when many people where not any longer familiar with Latin. Should one allow the cheap word processor to rule one's behaviour? While he himself could live without the ligatures, he saw no good reason for preventing those who liked them, as Linnaeus did, from using them.

Brummitt viewed the ligatures as a menace to typesetters. The proofs of his proposal had come back to him with a blank space in place of the ligatures, since the typesetters could not cope with them (Taxon had obviously found a solution to the problem since). Since there were always funny people around that would wish to use ligatures, one should positively rule to get rid of them.

Prop. G was accepted.

Prop. H (69:42:69:0:0) was referred to the Special Committee on Orthography.
Prop. I (69 : 44 : 66 : 0 : 0) was referred to the Special Committee on Orthography.

Prop. J (8 : 90 : 82 : 0 : 0) was referred to the Special Committee on Orthography.

Prop. K (74 : 27 : 77 : 0 : 0) was referred to the Special Committee on Orthography.

Prop. L (18 : 143 : 22 : 0 : 1) was ruled as rejected.

Prop. M (140 : 32 : 8 : 0 : 1) was accepted.


Demoulin was sorry to have to reiterate what he had said 12 years earlier, at Sydney, when a similar proposal was discussed. There was no plausible reason why one linguistic group, the English speaking, should want to deprive other linguistic groups, such as the Romance, Celtic and some African, from using correct spellings in the Latin names derived from their languages. The French example of l'Héritier had been discussed in Sydney, where the apostrophe had the positive function of replacing an elided e. Accepting such a provision would be a discrimination that would be strongly resented by linguistic groups that were not well represented here, and whose interest in nomenclature might decline even further if this kind of attitude was to prevail in the Code.

Prop. N was accepted. [A belated request for a card vote, by Demoulin, was deferred to the following day and eventually not acted upon.]

Recommendation 73B

Prop. A (35 : 16 : 131 : 0 : 0) was referred to the Editorial Committee.

Recommendation 73C

Prop. A (125 : 36 : 19 : 0 : 0) was accepted.

Recommendation 73D

Prop. A (6 : 136 : 35 : 0 : 1) was ruled as rejected.
Rec. 73G-Art. 76 - 215

Recommendation 73G


Johnson, while unwilling to bring up this heavily defeated proposal for consideration, felt that it included matter that would be of interest to the Special Committee on Orthography, to which it should be referred.

Demoulin supported this view. Manara’s proposal indicated a growing dissatisfaction of those knowing Latin with the way in which the Latin language was being treated in the Code. It should not be dealt with as something strange or uninteresting; it was extremely well written and represented, much better than the present Recommendation did, good practice at a time when botanists knew Latin better than they did now.

Prop. A was referred to the Special Committee on Orthography.

Article 76

Prop. A (46:8:124:0:0) was referred to the Editorial Committee.

Prop. B (142:13:24:0:0) was accepted.

Stearn moved a New Proposal, to include Onosma among the names to be treated as feminine irrespective of classical usage. The Code ruled that modern compounds ending in -osma were feminine, but forgot that there also was a classical compound in -osma. Treating Onosma as feminine would secure uniformity, perhaps a small matter but a matter of consequence for, e.g., editors.

Demoulin made it clear that this was more than a mere editorial improvement, it was a full proposal [see Taxon 42: 679-681. 1993]. On this particular point, he for once disagreed with Stearn. Onosma was not a modern compound but a classical word that in ancient times had been consistently treated as neuter, and so it had been in botanical usage.

McNeill explained that, although this was closely related to Prop. B and might be considered as the author’s own amendment to it, since Prop. B had just been passed without that amendment being formally made, this should now technically be considered a new proposal raised from the floor. Stearn’s motion was seconded.
Stearn felt it would be sheer, pedantic stupidity to make an exception for Onosma by treating it as neuter, when all other generic names in -osma, the Latin rendering of the Greek word for smell, were feminine. Manuscripts that he was to edit for a Greek journal variously treated the name as neuter or feminine, Onosma being a Mediterranean genus of some importance. A classical scholar whom he had asked, Amigues, thought that neuter treatment in ancient Greek reflected the latent influence of the neuter noun "phyton", meaning plant. However, ancient Onosma had fallen into disuse until Linnaeus took it up again, independently from its classical application (which was unclear in both Pliny and Dioscorides), so that for practical purposes it was just a modern generic name. Treating it differently from the other compounds in -osma would be a great inconvenience for many people.

McNeill added that Linnaeus himself had treated the name as feminine.

Demoulin stressed the importance of botanical tradition, which favoured neuter gender. One of the very first plants he had been proud to see flowering in his rock garden had been Onosma tauricum. So, even not being a phanerogamist, he had always known that Onosma was neuter. If this was to be changed, the Code should include a full sentence, stating that all names ending in -osma, whether ancient or modern, were feminine. Would one go so far in stabilizing gender by termination, to state that all names ending in -us were masculine, and all those in -a, feminine? As long as one admitted that tree names in -us were feminine, there was no logical need to declare Onosma to be feminine.

McNeill assured that the Editorial Committee would take care to incorporate this point, now suggested in general terms, most proficiently in Art. 76.

Greuter had lately, together with Nicolson, considered a great many generic names as to their gender, in the context of NCU-3. Nicolson had undertaken a complete review of all names ending in -ma, which continued to cause problems: While most other names ending in -a were feminine, a majority but not all of those ending in -ma were neuter. This was the case for final word elements such as
comma (genitive: commatos) but not for osma (genitive: osmēs). The one general rule Nicolson and he had found to be in good overall agreement with botanical tradition, was that names ending in the same element should have the same gender. He had until now, perhaps naively, taken the view that what Art. 76.2 termed "modern compounds" referred to all scientific names of plants, which had all been validly published in or after 1753. This was why he had originally thought Stearn's motion to be merely editorial, and also why he had always treated Onosma as feminine by abidance to the Code as he understood it. As far as he was aware, this was now the generally agreed convention: to regard Art. 76.2 as applicable to all scientific generic names of plants. To make this unambiguous, he moved that the word "modern" be deleted under Art. 76.2(a)-(c), three times. Stearn accepted this as a friendly amendment to his motion, covering all relevant points. The motion, thus modified, was again seconded, and assurance was given that the wording of the examples would be corrected accordingly if it would pass.

Demoulin warned against accepting a change the consequences of which were not fully understood. The Committee on Orthography had, after Sydney, worked on this for six years, with tons of papers produced. Perhaps the idea was good, but he was worried to have a change accepted within a few minutes on which a whole Committee had been unable to agree after six years. Could this not introduce contradiction between Art. 76.2 and 76.1? This should be referred for study to the Special Committee on Orthography.

Stearn's motion, as modified by Greuter, was carried and the New Proposal thereby accepted.

Division III


Greuter announced the result of a vote by the Committee for Fungi and Lichens on this proposal: 8.5 in favour, 3.5 against, 2 undecided.

Hawksworth added that those members of the to-be new Committee that were present in Yokohama had met on the previous day and unanimously supported the proposal. The mention of "fungi and lichens" was, in logic, comparable to "spermatophytes and halophytes", putting a taxonomic and a biological concept together.
This was confusing and perpetuated an 18th Century idea. The correction, probably first suggested at the Stockholm Congress in 1950, was long overdue.

Gams pointed out that former opposition against the proposal resulted from a conservative attitude with respect to a well-established name and its acronym, CFL. There were now no strong feelings about it in the Committee, nor was anyone confused about the taxonomic situation.

McNeill had never been really confused – but often puzzled – by the coincidence of the acronyms of the Committee for Fungi and Lichens and the Canadian Football League.

**Prop. A was accepted.**

Greuter moved a New Proposal, by the Bureau of Nomenclature plus Nicolson, that had been foreshadowed in earlier comments. In view of the complete inactivity of the Committee for Hybrids and of the fact that this Committee had no permanent task such as advising on the conservation or rejection of names, and as a logical corollary to the decision to authorize the set-up of a Special Committee on Hybrids that would report to the subsequent Congress, deletion of the Permanent Committee for Hybrids from Division III of the Code was proposed. The motion was seconded and carried, and the New Proposal thereby accepted.

**Article H.3**

Greuter suggested that all proposals relating to Appendix I, in the absence of a recommendation by the defunct Permanent Committee for Hybrids, be referred to the new Special Committee on Hybrids.

Henderson asked for an exception in the case of Art. H.11 Prop. A, which was editorial.

**Prop. A (3 : 45 : 47 : 1 : 55) was referred to the Special Committee on Hybrids.**

**Prop. B (2 : 54 : 80 : 0 : 20) was referred to the Special Committee on Hybrids.**
Prop. C (12 : 111 : 18 : 0 : 14) was referred to the Special Committee on Hybrids.

Article H.6

Prop. A (5 : 65 : 67 : 0 : 20) was referred to the Special Committee on Hybrids.
Prop. B (12 : 74 : 28 : 0 : 38) was referred to the Special Committee on Hybrids.

Article H.7

Prop. A (2 : 137 : 9 : 0 : 9) was referred to the Special Committee on Hybrids.

Article H.9

Prop. A (1 : 60 : 77 : 0 : 21) was referred to the Special Committee on Hybrids.

Article H.11

Prop. A (2 : 29 : 126 : 0 : 2) was referred to the Editorial Committee.

Appendix II

Prop. A (19 : 78 : 69 : 0 : 2) had been withdrawn.

[See also motions by Nicolson (p. 95) and Greuter (p. 96), both presented and defeated during the Third Session; and McNeill’s motion (p. 241) discussed and carried during the Eighth Session.]

Appendix III

Prop. A (26 : 96 : 42 : 0 : 2) had been withdrawn.

Appendix V


Jeffrey moved an amendment to the proposal. The text of the amendment was the same as that of Art. 7 Prop. J, as published in
the "Synopsis". In their comments, the Rapporteurs had written: "Prop. J is put forward as a fallback solution should the NCU proposals ... fail." Meanwhile, the NCU proposals had failed, and Art. 7 Prop. J, previously rejected, should be reconsidered. The motion was seconded, after which, at Greuter's request, Jeffrey read out the Rapporteurs' published comments in full.

Greuter added that, as he had stated previously, the proposal was opposed by the Committee for Pteridophyta (0:8), the Committee for Fungi (4:6), and the Committee for Bryophyta (0:4), Zijlstra commenting that, should the proposal pass, she would immediately resign as a Secretary of the latter Committee.

Nicolson supported the proposal on the grounds that meanwhile the "protype" concept had been accepted, so that there were no longer names that could be left quiescent. Any old, dubious name could now be picked up and "protypified" if someone wished to do so. Jeffrey's proposal provided for a graveyard that would, in a stable way, prevent so many corpses from getting up and walking around.

Brummitt pointed out that the proposal was quite unnecessary now that Art. 69 Prop. B had been accepted, permitting rejection of any disturbing name. Such names would presumably be listed in the already existing App. IV.

Jeffrey's motion was defeated.

Demoulin could not see a contradiction between the Reveal proposal having been accepted and the proposal of either Jeffrey or Fosberg. The latter would provide for the rejected names to be listed in an Appendix, which was normal procedure. A list was certainly needed, and where to place it was in a way merely editorial.

McNeill explained that the Fosberg proposal related to a particular kind of rejected names, nomina dubia. As Brummitt had pointed out, there was no need to treat them differently, by providing for a separate Appendix, from those to be rejected under the Reveal proposal.

Prop. A was rejected.

Prop. B (107:43:12:1:10) was accepted.
EIGHTH SESSION
Friday, 27 August 1993, 9:00 – 12:45

Burdet announced that the Section would now consider new motions by its members, of which he understood there would be at least four, by Jørgensen, Hawksworth, Greuter & al., and Dorr. Thereafter, the report of the Nominating Committee, the various Permanent Committee reports, and the report of the General Committee would be received and acted upon. A final resolution from the Section, and closing remarks, would be the last items.

The text of two motions were on display. The first, by Jørgensen, read: "While work continues to find further ways to reduce changes of well-established names, until the next IBC (1999) such names should not be displaced by resurrection of long-forgotten ones."

The second motion, by Hawksworth, consisted of two items: "The Nomenclature Section, having voted 55 % in favour of accepting the principle of protected Lists of Names in Current Use, and having authorized the establishment of a Standing Committee to initiate, assist, coordinate and vet the production of Lists and updating the existing Lists, urges all botanists to refrain from either taking up names at the ranks and in the groups covered that would compete with those on existing Lists, or performing other nomenclatural acts that would affect the status or application of those names, until this issue has been addressed by the XVI International Botanical Congress.

"This is particularly so for the draft List of names in Trichocomaceae that has already been approved by the International Commission on Penicillium and Aspergillus of the International Union of Microbiological Societies."

Jørgensen Resolution

Jørgensen introduced his proposed resolution. The intention was to stop the digging up of old names, while the process of finding out
what names were to be used continued. One might wonder whether this was necessary when the options for conservation and rejection of names had been substantially improved. He felt it still was. The resolution could be used against attempts to rouse sleeping dogs. It was worded in a very general way, and he hoped it would be acceptable irrespective of whether one was in for or against the NCU principle.

Johnson was very much in favour of the spirit of the motion but, if it passed, care should be taken that it would not be interpreted as preventing, or discouraging, the adoption of old names for newly recognized taxa not at present bearing commonly accepted names. This might be a truism for those present, but perhaps not for all outside the room. The motion was to address only purely nomenclatural resurrections, not those resulting from changes in taxonomy.

Chaloner liked the sentiment of the motion, and also of that displayed on the board to be proposed by Hawksworth. They overlapped, this one being somewhat weaker but more general and the Hawksworth motion stronger but more specific. Did the other two motions also bear on the same topic? If so, could they be made known, in order that the discussion need not track over the same ground unnecessarily? Could the Chair or the Rapporteurs suggest how the overlap might be handled?

Burdet was willing to combine the discussion on both motions. If the Section agreed, Hawksworth might present his motion even though the Jørgensen one had not been dealt with completely.

Greuter asked where exactly these motions were to go and what they were to achieve. He understood that the Jørgensen motion was intended to be presented as a resolution at the Final Plenary Meeting of the Congress. It would not go into the body of the Code although it might be mentioned in its Preface, or published by any journal that so wished. But where was the Hawksworth motion to go? The Section must have a clear idea of what it was discussing.

Jørgensen explained that his was a general proposal, independent of NCU, whereas Hawksworth’s proposal concerned the future of the NCU lists.
Hawksworth supported Johnson's wish that the Jørgensen proposal be clarified. He suggested that after "... changes of well-established names" the words "for purely nomenclatural reasons" be inserted. [This was accepted as a friendly amendment.] It was not the intent of any of the proposals to discourage botanists from doing taxonomy, and that should be made absolutely clear. He took it that Jørgensen's proposal, if adopted, would be forwarded to the Resolutions Committee of the Congress, and as the Rapporteur had pointed out it could also be referred to in the introduction to the Code. The two-part proposal he had drafted could, if approved, go directly into the introduction to the Code, so as to provide guidance to its users on how the existing NCU lists should be treated. Its first part merely reiterated what the Section had decided in setting up a Standing Committee on Names in Current Use. The second, more specific part reflected the concern that the authority of the Section be maintained in the eyes of some groups of mycologists. The draft list of Trichocomaceae had already been approved by the competent international body.

Greuter, upon request, explained that the motion he was to present jointly with McNeill and Nicolson did not overlap with the present ones. It was to address Permanent Committees and urge them to make full use of provisions newly introduced into the Code. Quite a different public was being addressed, and the issue should be discussed independently later.

Jeffrey proposed a second amendment to Jørgensen's motion [which again was accepted as a friendly amendment], to add at the end, after "... long-forgotten ones": "... nor by changes in their application through typification."

Dorr, at the request of Lack, explained that his own announced motion pertained to the formation of a Special Committee and was not germane to the resolutions on the floor.

McNeill added that there was to be a fifth motion which again dealt with stability; but it related strictly to App. IIB of the Code and so was not immediately relevant to the two proposals now before the Section.
Trehane asked that the reference to 55% in the Hawksworth proposal be removed, being irrelevant. It only weakened the case.

McNeill objected that, as the Section’s rules for making a change in the Code required a 60% majority, the figure was needed for clarity.

Barrie was not happy with Jørgensen’s proposal. It ignored the provisions that would now be in the Tokyo Code, which were to permit conservation and/or rejection of almost any name, whether long-forgotten or well-known but problematic. It might discourage serious workers from making full use of the new mechanisms until NCU lists would be available. Moreover, it would hardly stop those who made a career of resurrecting old, unused names; it would rather slow down action by those doing serious work.

Zijlstra thought that Jørgensen’s proposal, as amended by Hawksworth and Jeffrey, was just what was needed. The first part of the Hawksworth motion went one step too far. There were many groups in the published NCU lists for which it was quite uncertain whether they included all names that were needed; many might have to be added. This was the main reason for her having opposed the acceptance of the NCU principle. The second part of the proposal by Hawksworth was fine, and for some other groups a comparable statement could perhaps be made. For example, in the bryophytes, the generic list of hepatics was quite satisfactory, while that of the mosses was not.

Funk felt that she was being assaulted by proposals to bring in by the backdoor something that the Section had already rejected, without a chance of getting any input from the botanic community. 68% of the mail vote had been cast against NCU – a figure that should be mentioned along with the 55% Section vote. She strongly objected to the specific reference to lists that had been said again and again to be full of mistakes.

Gams underlined that the NCU principle was controversial. The Committee for Fungi had been strongly divided, and a majority had voted against it. The Jørgensen proposal excluded the controversial issues and therefore had the unanimous support of the Committee.
for Fungi members present. This was needed, specifically its inclusion into the Introduction of the Code. He was much less happy with the Hawksworth motion, unless reference to the vetting and production of lists, and to the whole NCU issue that one might again consider after six years, were completely removed.

Stuessy felt that the Hawksworth proposal had two objectives: keeping the NCU issue alive and announcing to the general botanical public what had been enacted to promote greater stability for names. These should be separated out. The NCU issue had been disposed of, at least for the time being. The second point might better be covered by a new, independent motion.

Brummitt was very sympathetic to the principle of the Jørgensen resolution, but he hoped that it would not be seen as an argument against conserving and rejecting names. The words "while work continues to find ways to reduce changes of well-established names", when one very powerful means of doing this had been found at this meeting, might suggest to some that the latter need not be applied.

McNeill assured Brummitt that the forthcoming proposal to which the Rapporteur had referred would deal with his concerns, because it would specifically charge the Permanent Committees and the General Committee to use the tools that had been provided.

Hawksworth wondered whether Stuessy was unaware of the Section's having approved the establishment of a Standing Committee on Lists of Names in Current Use. The first lines of his proposal only cited the remit of that Committee. It was plainly erroneous to state that the NCU issue was dead!

Chaloner, noting that the discussion was flipping between two motions, requested that the Section look solely at the Jørgensen motion, and that the amendments that had been incorporated into it be projected on the screen.

Burdet accepted these requests.

Jeffrey proposed a third amendment to Jørgensen's motion, to insert "further" before "ways". [This also was accepted as a friendly amendment.]
Greuter explained that the Section need not go into editorial details, since the motion would go to the Congress Resolutions Committee that would clean it up editorially before submitting it to the Congress. Amendments of substance should, of course, be made.

Demoulin was sympathetic with the Jörgensen motion when it was drafted a few days ago, but he could no longer support it with the addition regarding typification. Introducing this would really freeze the whole taxonomic procedure and set aside the most important aspect of the Code. Since the beginning of the NCU discussion, he had expressed reservations to the lists mentioning types.

Greuter made it clear that, as a Congress Resolution, this would have a twofold effect. On one hand it would tell the world at large that the Section took nomenclatural stability seriously; that even although the NCU proposals had not achieved a 60% majority, the Section was aware of the need for greatly reducing, if not abolishing, name changes for purely nomenclatural reasons. On the other hand, this was not, and could not be, a motion to just set aside the provisions of the Code. It was an injunction to all working taxonomists to use fully the new provisions in the Code for the conservation and rejection of names. This was too technical a point to be made explicit in a general resolution of the Congress, but it was certainly appropriate to underline, when referring to this resolution in the Code, that in effect it instructed working taxonomists to make conservation or rejection proposals before changing a name.

Jeffrey pointed out that, in his amendment to the Jörgensen resolution, he had deliberately limited the typification issue to the case of well-established names by using the word "their". If this was liable to be misinterpreted as a constraint on typification in a general way, he would share Demoulin's misgivings.

Hawksworth supported Demoulin's view that typification should not be referred to specifically, for two reasons: typification was already covered by the addition of the phrase "purely nomenclatural reasons"; and the term "typification" was too technical in the context of a general Congress Resolution.

Trehane agreed that, in a "clarion call going out to the big wide world", not to botanists alone, inclusion of concepts like typification would only cloud the issue.
Zijlstra wondered whether the words "until the next IBC (1999)" should not better be deleted.

Jørgensen was not opposed to that deletion, which would make the motion, already general in scope, even more general. Would the Section agree going so far? His primary intent had been to urge stability after the NCU principle had failed and until such time as it might be rediscussed.

Greuter believed that Zijlstra had made an excellent point. The Section wanted to encourage people to make full use of the Code, and the Code was not just in operation until the next International Botanical Congress, so referring to that Congress muddled the issue. He supported deletion of "until the next IBC", and certainly of the date (which it was not the business of the Section, nor of the Resolutions Committee, to fix). He also supported (and moved) deletion of a technical aspect such as typification in a motion addressed to a very general public.

Zijlstra's amendment motion was seconded and carried. Greuter's amendment motion, to delete the reference to typification, was also seconded, it having been clarified that only the words "through typification" were to be deleted but "by changes in their application" would remain. That motion, too, was carried.

McNeill, in reply to a question by Lack, confirmed that this was to be a resolution to the Congress, and that only a 50% majority was needed for approval.

Demoulin offered an amendment to the Jørgensen motion, incorporating the important issue of the Penicillium and Aspergillus list but avoiding mention of the controversial term NCU: "While work continues to find further ways to reduce changes of well-established names, such names should not be displaced by resurrection of long-forgotten ones for purely nomenclatural reasons. An important example of such names to be protected against changes is a draft list of names in Trichocomaceae, which has already been approved by the International Commission on Penicillium and Aspergillus of the International Union of Microbiological Societies (IUMS)."
Greuter pointed out that the word "typification" had just been removed from a prospective Congress resolution because it was too technical for a general audience. He doubted that "Trichocomaceae" was a better example of a word to be included.

Demoulin interjected that, though the Trichocomaceae were not well known, Aspergillus and Penicillium definitely were. Demoulin's amendment failed to be seconded. Jørgensen's motion, as amended to read "While work continues to find further ways to reduce changes of well-established names, such names should not be displaced for purely nomenclatural reasons, whether by change in their application or by resurrection of long-forgotten names", was carried.

Hawksworth Resolution

Prud'homme van Reine reiterated his point. The Section itself had decided that, if less than 60% voted yes, a proposal was not accepted. The 55% figure was totally irrelevant and, if this were at all possible, would make him even more strongly opposed to the motion. Asked from the chair whether he was moving deletion of that figure, he retorted that the appropriate amendment would be to delete the proposal completely. [Laughter and applause.]

McNeill, who had earlier noted that under the current wording reference to 55% was needed for clarity, pointed out that an acceptable amendment would be to delete all the words following "The Nomenclature Section having" down to and including "... authorized the establishment of", replacing them with "established".

Hawksworth accepted this as a friendly amendment. The text of the motion (first clause) now read: "The Nomenclature Section, having established a Standing Committee to initiate, assist, coordinate and vet the production of Lists and updating of existing Lists, urges all botanists to refrain either from taking up names at the rank and in the groups affected that would compete with names on existing Lists, or performing other nomenclatural acts that would affect the status or application of those names, until this issue has been addressed by the XVI International Botanical Congress." This was, if accepted, to go into the introduction to the Code.
McNeill clarified that this was to be a resolution of the Section to which the editors of the *Code* would want to refer; it was not something being "put" into the *Code*.

Barrie regretted that the second part of Hawksworth's motion, as worded, appeared to discourage people from applying the provisions for conservation and rejection voted into the "Tokyo Code", leaving unsolved any problems affecting the names on the NCU lists, although the latter had no legal standing. In addition, to which lists was the motion referring? There were thousands of lists in existence, few of which anyone at the meeting had been considering – Steudel's *Nomenclator*, for instance.

Hawksworth explained that the Lists (with a capital L) were of course those in which the Standing Committee was to be involved.

Kirkbride agreed with Funk that the motion was a backdoor effort to incorporate NCU into the *Code* and could be construed as instructing the next Section to approve the NCU principle. This Section had expressed its desires concerning NCU, and its having authorized the set-up of a Standing Committee must suffice. The motion should be split into two parts, to be acted upon independently.

Brummitt believed that the Section had voted against the principle of NCU, not so much because it disliked the idea in general but largely because it distrusted the lists. Now the Section was being asked to approve the actual lists, going beyond what had been proposed previously. This seemed quite amazing.

Jeffrey noted that Demoulin's proposed amendment to the Jørgensen motion, while inappropriate for a Congress resolution, would be ideally suitable in the new context.

Friis feared that the proposed resolution would discourage taxonomic work. He could think of the need to unite taxa with different range sizes, where the taxon with the more restricted range had the older name.

Stuessy hoped that the first part of the Hawksworth motion would be defeated – he was not so sure about the second part – and if so,
he would offer a motion for a new resolution, underlining the positive aspects of what the Section had achieved, to read: "The Nomenclature Section of the XV IBC is pleased to announce that significant progress in stabilization of names, through providing broader avenues for conservation and rejection, has been achieved by formal changes in the Code. These changes will greatly aid in the maintenance of names in current use."

Greuter conceded that much of what had been said against the first part of the Hawksworth motion was justified. In particular, lists which — although not "full of errors" as Funk had claimed — were not exempt of faults should not now be treated as something almost sacrosanct. It would be far more appropriate to encourage people to contribute to correcting the lists rather than applying them uncritically. Yet, the Section should bear in mind that the traditional consensus on a 60 % majority rule presupposed due respect for simple majorities. By the 55 % vote on the NCU principle, the Section had expressed considerable support for the idea and encouraged its continuance, although it had felt that it was not yet sufficiently mature and generally acceptable to be enacted. Stuessy's suggested motion was a bit Mephistophelic: it wanted to make the world at large believe that now that quite appreciable measures toward stabilization had been taken, NCU was no longer needed — certainly a misrepresentation! Hawksworth, however, might better withdraw the first part of his motion and move to consideration of its second part which, obviously, had considerable support.

Zijlstra, even after deletion of the phrase relating to the 55 % vote, could still not accept this proposal. Even though she had not yet had the opportunity to check thoroughly the generic NCU list, which during the last few years had been a big part of her life, she had a strong impression that several families were very incomplete, including the palms, Aizoaceae, Apocynaceae, and Cactaceae.

Chaloner, although sympathetic to the spirit of the motion, felt with the Rapporteur that it gave the impression that the lists were sacrosanct, and that indeed it implied that the next six years would go by leaving the lists untouched. Surely the whole point in postponing
NCU was that the lists should be looked at critically. He, therefore, proposed to slightly weaken the motion so that, instead of "all botanists to refrain either from ...", it would read: "... all botanists to exercise the greatest caution before either ...". [This was accepted as a friendly amendment].

Stuessy felt this was a negative type of resolution, hanging out the Section's dirty laundry to the rest of the botanical community. A more positive presentation of the Section's achievements was needed.

McNeill reiterated that this was to be a resolution of the Section, not of the Congress as a whole. It would, if accepted, appear in the Report of the meeting of the Nomenclature Section in the November *Taxon* and in the Preface to the *Code*. Even if not passed, it would be quoted in the full Report on the nomenclature sessions, to be published in 1994.

Jørgensen asked the blunt question: what status, if any, had the NCU lists?

Jeffrey added a further query: what lists were to be covered?

Brummitt noted that the NCU lists had no status at all. Some had been considered by Committees, such as two species lists submitted to the Committee for Spermatophyta, which had been rejected almost unanimously.

Demoulin, encouraged by Jørgensen's support, proposed a new text, to be added to the already adopted resolution: "Important examples of such names to be protected against changes are to be found among the lists of names in current use published by the IAPT and the draft list of names in *Trichocomaceae*, which has already been approved by the International Commission on *Penicillium* and *Aspergillus* of the International Union of Microbiological Societies (IUMS). Taxonomists should be aware that new provisions for wider use of conservation or rejection have been adopted by the Nomenclature Section of this Congress, and their use should be encouraged." The Stuessy suggestion might have some undesirable implications, yet mentioning the positive decisions taken was appropriate.
Greuter recommended that further discussion be deferred in the hope that some more widely acceptable compromise motion might be achieved. This might incorporate his idea of encouraging feedback on the printed lists and co-operation in future lists.

[The following discussion took place subsequent to the coffee break, after action had been taken on the two next motions.]

Burdet opened discussion on the new compromise version of the first part of the Hawksworth motion, which read as follows: "The Nomenclature Section, having established a Standing Committee on Lists of Names in Current Use, encourages all botanists to: (a) contribute to the improvement of the published lists (Regnum Veg. 126, 128, 129. 1993); and (b) exercise caution before taking up names that would compete with or change the application of names on those Lists."

Funk noted that the motion still included mention of the NCU lists. These lists had been the major objection against the NCU principle, all that week long. Many had stood up to say that they had worked in the Committee and were in favour of the concept but were not prepared to endorse these lists yet. Now the Section was being asked at the last minute to give credit to these lists, disregarding the objections of the botanical community at large reflected in the postal vote as well as their failure to get approval at the nomenclature meetings.

Chaloner respected the sentiment of his American colleagues, but would have hoped that through this change of phraseology the motion became acceptable to them. It now simply suggested that caution be used before chucking out a listed item. The new motion did not in any way treat the lists as sacrosanct, or formally recognize them; on the contrary: it encouraged their improvement. The lists were regarded as a beginning for the future, to be knocked about when necessary – but, please, gently.

Hawksworth thought that the addition, following Greuter's suggestion, of the new clause (a) would have helped Funk to overcome her concern. These lists did exist, with an IAPT imprint, and botanists would want to know how they were to be used. The motion did just
that, asking them (a) to contribute to the improvement of the lists and (b) to exercise caution before displacing the listed names or changing their application.

**Barrie** realized that the additional clause included an acknowledgment that the lists were not perfect. However, given the questions that had been raised about the quality and lack of reliability of the lists, the second clause might just as well read: "exercise the greatest caution before accepting names on the lists".

**Brummitt** reiterated his personal view that, although he endorsed the concept of the generic list and would like to see it improved, he was unhappy about the way in which such lists operated at the specific rank. The lists submitted so far were bare lists of names without apparent taxonomic concept. The very principle of how lists at specific rank were produced needed rethinking; a different approach was needed.

**McNeill** followed up on what Brummitt had said about species lists, and what others had hinted at. He had been a member of the Committee for Spermatophyta who had voted against recommending approval of the two Spermatophyta species lists by this Congress. This was because he did not think they were ready for approval, not because he thought they were bad. In fact, they were extremely good lists; they were not perfect (and some might claim that they could never be perfect, as they objected to species lists on principle), but to suggest for a moment that most of the names on them were not the right names to use was absolute nonsense. The proposal on the overhead dealt with the situation very clearly and very well: it encouraged work toward improving the lists, making it quite clear that they were not in a finished form, but it also recognized that they represented a good deal of very fine work, such that one should exercise caution before adopting a name that was not on them. He completely disagreed with Barrie's comment on this.

**Jørgensen** had been critical of the procedure in developing these lists, particularly at the species level, but the *Aspergillus/Penicillium* list was an exception. It was a small wonder that specialists in these groups had come together and managed to agree, and that should be recognized.
Forero asked why this motion was necessary. It went against the wishes of a large number of members of the botanical community. The lists were there, and working on them would of necessity be part of the task of the new Standing Committee. People who had not been present and had not listened to or participated in the Section’s discussions would, anyway, take the lists as the final word.

Jeffrey thought that clause (b), as worded, was too vague. He suggested replacing the phrase "taking up names that would compete with" by: "adopting as correct names that would displace". [This was accepted as a friendly amendment].

Barrie regretted if his earlier remarks had suggested that he did not appreciate the quality of work, time and effort that had gone into the lists. However, he felt that even as amended the wording of (b) would discourage the necessary critical evaluation of names on the lists and would make users assume that the lists were more accurate than they actually were.

Kirkbride supported Forero’s statement. He feared that botanists outside of the Section might misinterpret clause (b) as extending special protection to the lists. Besides, did this motion extend to additional lists to be published by IAPT in the future?

Greuter replied that only the extant lists were meant, that would be specified in a footnote. The Committee on NCU would hopefully see to the publication of other lists before the next Congress, but one could hardly formulate recommendations in their regard before having seen them. He liked the reworded motion, especially the order of (a) and (b); the former was in fact closer to his heart, since critical feedback on the lists was most important. He knew that there must be errors in the published lists, but despite what had been said about their being full of errors, no-one had yet brought even a single concrete mistake to his attention. This motion was a clarification over saying nothing about the Lists. Some might think that the lists had been defeated by a 45 % minority, but were what the assenting 55 % would have adopted. This motion made it clear that the lists were thought useful but were not thought of as the Bible, not even by the 55 % supporting the NCU proposals.
Ahti suggested a further small, hopefully friendly amendment, to change "botanists" to "taxonomists". One would hardly think of medical mycologists, for instance.

The reworded first portion of the Hawksworth motion, as amended, was defeated by a card vote (43.1 % in favour, 158: 209).

Hawksworth read out the second part of his motion, still on display, but indicated that the introductory wording would need some revision, irrespective of whether or not the main motion would fail. The approval of this second clause was especially important in order to avoid further disintegration of the decision-making process in biological nomenclature. Failing this, the IUMS would be asked to take the matter into its own hands.

Greuter suggested that, since the wording would change depending on the fate of the main motion, consideration of the matter be postponed until the result of the card vote would be available.

[The following discussion actually took place subsequent to action on the McNeill Motion.]

Hawksworth read out the reworded second part of his motion: "The Nomenclature Section, noting that the list of 'Species names in current use in the Trichocomaceae' (Regnum Veg. 128: 13-57. 1994) has already been approved by the International Commission on Penicillium and Aspergillus of the International Union of Microbiological Societies (IUMS), urges taxonomists not to adopt names that would compete with and not to change the application of any names on that list."

Demoulin, while strongly supporting the motion, was somewhat concerned by the implication that the special status to be granted to that list was conditioned by its prior approval by an IUMS committee. The IUMS should not be given greater credit in nomenclatural matters than, say, the IAPT. A more neutral wording might have been preferable, perhaps omitting the causal link implied by "noting that".

Faegri's point was the same as Demoulin's. IUBS, under whose auspices the Section worked, and IUMS were different Unions, and it might be dangerous to admit that both had a say in botanical nomenclature.
Hawksworth said that these were exactly his concerns. The wording carefully pointed out that the list had been adopted by an IUMS Commission, not by the IUMS itself. If there was not some positive signal from this Section, such as he had moved, what Faegri feared would in fact likely happen: the matter would be taken out of the hands of this Section, and of the Biological Union. The IUMS Commission, here represented by Pitt, was prepared to take this issue to the higher levels of IUMS. Adoption of the motion would probably permit to keep this nomenclatural issue within the IUBS.

Chaloner asked what the Committee for Fungi felt about this matter. If they supported the motion he would gladly endorse it.

Gams explained that this was a somewhat exceptional situation. The Pitt & Samson list had been endorsed by a Committee of 16 specialists comprising all the experts on this particular group. Only few members of the Committee for Fungi were familiar with that group, and they too were in favour.

Kirkbride asked where the proposal would be placed in the Code, and what would be its nomenclatural standing and effect.

Greuter understood that this would be in the introduction to the Code. It had not been suggested that it should enter the body of the Code, where there would be no appropriate place for it.

Hawksworth added that this would be promulgated through the proceedings of the Section as well as through the Commission itself and its members.

Faegri thought that, as similar cases were likely to come up in the future, it might be expeditious for the IAPT to establish a trans-union Commission, for nomenclatural purposes, between the IUBS and IUMS.

Greuter drew attention to the fact that the Committee for Fungi had agreed to meet at the next International Mycological Congress – which, however, also fell under the IUBS purview.

Jørgensen underlined once more the great importance of this proposal. It was a small wonder that specialists known to always have quarreled over nomenclatural questions had come up with such a list by common consent. He begged American and other botanists
resisting NCU to vote for the motion, also because it concerned an economically very important group.

The motion was carried.

Greuter, McNeill & Nicolson Resolution

Greuter recognized that the Section had introduced some very valuable changes into the Code, providing instruments for greater stability. But in order for these to be implemented and fully utilized, two things had to be ensured: first, that before changing names botanists should make a proposal to avoid the change – which was the topic of the two first resolutions, addressed at plant taxonomists and botanists generally; and second, that Committees considering such proposals act in the spirit that had been expressed by this Section. Committees had existed whose members were sometimes reluctant to exploit fully the legal opportunities in the Code. A few Committee decisions had been so awkward, to put it mildly, that the General Committee, which usually just rubber-stamped Committee recommendations, had felt compelled to decide otherwise or refer the matter back for reconsideration. The motion to be presented addressed in the first place the General Committee, the body that represented the Section between Congresses, and through the General Committee it addressed the Permanent Committees that dealt with conservation and rejection proposals. He could not resist quoting what might be called the farewell words of Stafleu at the last Congress, in Berlin, when he commented on the General Committee's decision to overrule a negative Permanent Committee recommendation and conserve the scientific name of the tomato – one of the two cases that had tilted the balance in favour of conservation of names of species of major economic importance, in Sydney. Stafleu had "confessed to having been shocked to see that a Nomenclature Committee was not really willing to listen to an enormous community of plant users in the world. The conservation provisions in the Code were there just in order to avoid a group of nomenclaturalists – who were perhaps not all that worldly-wise – taking decisions that conflicted with the needs of the great community of plant users", and he had asked all those present to "bear in
mind, in the future, that the Code had not only been written for themselves. The Code and the Committees were to serve Humanity."

McNeill presented the text of the motion: "The Section urges the General Committee and through it all Permanent Committees to make full use of the options that the Code now provides in order to ensure nomenclatural clarity and stability. The General Committee is requested to establish procedures to secure this end." The motion was seconded.

Funk was bothered by the second sentence of the proposal. She wondered what type of "established procedures" were involved? Was one going to hang people by their thumbs in a dungeon or just slap their hands? Maybe it was because she was from the U.S.A., but she did not like being told what to do. This smacked of the General Committee giving itself increased power — which, historically, usually led to dictatorship.

Greuter explained what was intended by that sentence. Up to that point the General Committee had used to take specific action only on proposals recommended for adoption by the Permanent Committees, and had not considered those that did not achieve sufficient support for a positive recommendation. The idea now was that, in future, the General Committee should also look at proposals that were not favoured by the Permanent Committees, because the problems usually were with those. This being said, and unless the Section should object, the last sentence of the motion could be removed, because the General Committee would know what to do.

Zijlstra now finally understood the intention of the motion; before, she had wondered if it had any meaning at all. If it were to suggest that Committee members did not try their best to apply the Code, this would be wrong. Therefore the explanatory sentence should remain in the motion.

Stuessy acknowledged that this was a well formulated proposal, but still could not see why it was needed. This seemed so self-evident.

Barrie was quite happy with the proposal. If the second sentence created problems, and since it was redundant, it should be removed.
Brummitt had almost decided not to comment, but he found the motion somewhat superfluous although it expressed sensible sentiments. Committees were there to use their judgement on each individual case. There was a hierarchy of Committees and they could try to influence each other, but any attempt to dictate to individuals in Committees was, as Funk had rightly said, likely to be counter-productive.

Faegri agreed with Brummitt with one proviso - Committees had a mandate, and this motion was to become part of their mandate.

McNeill would have liked to think, with Brummitt, that the motion was redundant, as indeed it should be. But past comments within the Committee for Spermatophyta had shown that some members tended to act as though recent changes in the Code had not taken place. One of the merits of the motion was to draw attention to the very substantial changes in provisions for rejection and conservation of names that had just taken place. There were key members, long-standing members, thoughtful members on nomenclatural Committees who were not at this Congress, and this motion would underline to them the much broader role that the Committees would have in the future.

Kirkbride asked whether the proposal was an instruction to committees to follow the Code, or rather an empowerment to the General Committee to overrule Permanent Committee decisions when it did not like them. If so, then there was no reason to have Permanent Committees. The General Committee, rather than reverse decisions of a Permanent Committee, should abolish it or replace its members.

Jørgensen felt that the problem was that not all members of Committees always followed the Code. The nomenclatural Code was a kind of law. Members of Committees were in a way judges. If a judge did not follow the law - he should go!

Greuter had accepted the suggestion of deleting the last sentence as a friendly amendment. The General Committee was well represented at this meeting and would know what it had to do.

The motion, thus amended, was carried.
Dorr Motion

Dorr moved that the Section authorize the appointment of a "Special Committee to examine the impact of electronic publishing and databasing on stability of nomenclature and to make proposals to the next Botanical Congress." The motion was seconded. Throughout the sessions there had been statements and allusions to the fact that the way scientists had done business since Gutenberg invented the printing press was rapidly changing. The Code at present was only capable of dealing with the printed word. If not by the next Congress, certainly by the subsequent one information communication in science was going to be radically different. Unless one started now to anticipate this, there would be provisions in the Code that were essentially dinosaurs.

Faegri supported the proposal, which was not entirely new. The same concern had been the basis of the registration proposals. The pertinent Special Committee has soon found it advisable to shift its emphasis from the requirements of effective publication to those of valid publication. A study of the effect of changes in publication techniques was commendable, but changes in the Code's provisions on effective publication (Art. 29) were risky because of the very rapid changes in technology. A committee of study, as here proposed, was a sensible answer.

Greuter welcomed the idea itself but was doubtful about the reference to stability of nomenclature. The latter was mainly a problem of the past and did not seem to be much influenced by new technologies. Was this not more a matter of application of provisions of the Code, including particularly Art. 29?

Dorr was willing to accept a friendly amendment, incorporating Greuter's suggestion, but did not wish to specify which Articles of the Code would be addressed by the new Committee.

Nicolson suggested that, if approved, the new Committee make sure that its members participate in the work of international committees already existing and concerned with the same subject.

Chaloner pressed Nicolson's point. In the real world outside such questions had an enormous impact, especially in the areas of copy-
right and publishing on paper, closely related to non-paper publica-
tion. This was a very complex matter on which botanists could not
work in isolation; many, especially in the legal field, had published
extensively on it.

Jeffrey echoed the concern of the Rapporteur about the remit of the
new Committee. He proposed changing "stability" to "the prac-
tice".

Dorr preferred to replace "stability of nomenclature" with "the
Code".

The motion, thus amended, was carried.

Greuter explained that Special Committees, having been authorized
by the Section, were set up by the General Committee. Their mem-
bers were not appointed at the Section meeting because many inter-
ested and knowledgeable persons who might wish to serve were not
present. Members of the Section should make known to Nicolson,
the Secretary of the General Committee, names of persons (inclu-
ding themselves) who they thought would be willing and competent
to serve on any of these Special Committees. Sheets of paper for
each of the now six Special Committees were available outside the
room for this purpose.

McNeill Motion

McNeill introduced the following new motion: "The Section author-
izes the Editorial Committee to retain in the 'Tokyo Code' the
authorship and places of publication of the family names listed in
Appendix IIB of the present Code." This would permit the Editorial
Committee to avoid tampering with the present list of conserved
family names in App. IIB, and he hoped it was non-controversial.
The reason why this was needed was that Reveal and Hoogland had
dug out a large number of earlier places of publication for the
names that had been conserved since approval of the Bullock list by
the Montreal Congress. It would be quite destabilizing to make the
changes, but without a special authorization of this sort, the Editor-
ial Committee would have no option but to change the entries for
some 100 or so names.
Greuter liked McNeill's assumption that this would not be controversial, since he had moved the same motion before (under Art. 13) when it had been voted down!

Brummitt asked whether there was any possibility of pursuing the line not to amend the present App. IIB but to replace it by a reference to the published Hoogland & Reveal list (in Regnum Veg. 126: 15-60. 1994). Some might [indeed!] think of this as of a way of slipping through one NCU list. But since most agreed to consider that Reveal and Hoogland had done a good job, this might save the Editorial Committee the rather formidable task of making all these changes.

Greuter saw no normal way of doing so because neither in a report of the Committee for Spermatophyta nor in the General Committee report was reference made to the Reveal & Hoogland list. But even if the Section, which was omnipotent, should decide to approve the Reveal & Hoogland list in place of the present App. IIB for the Spermatophyta (or for all vascular plants, even though the Committee for Pteridophyta had failed to consider the list), it would not thereby authorize setting aside the Code and maintaining the familiar dates of publication and authorships for about 100 presently conserved family names for which different, earlier ones had come to be known. Adoption of the present motion would still be required in addition.

Demoulin had read the Hoogland & Reveal memorandum that had been made available to the Section, but failed to see the nomenclatural consequences of correcting the date and authorship of conserved family names. Authorship and dates of conserved names were mere bibliographical markers that the Editorial Committee should, in co-operation with the relevant Permanent Committees, try to maintain as accurately as possible. He did not wish to oppose the motion, for it might indeed be premature to make the changes now when the need for still further ones was foreseeable. But as to the nomenclatural impact, he asked that the Rapporteur should say whether he had perhaps missed something.
Greuter explained that of the family names of Spermatophyta presently listed in App. IIB, according to Reveal & Hoogland's distributed memorandum (not their published NCU list), about 20% would have their authorship and date changed. They had been listed with their newly established, different authors and places of valid publication, and since this had been officially brought to its attention the Editorial Committee could hardly do anything but abide by the rules and implement these changes. This might in some cases, which he understood were but few, result in changes of names of well-known families under a wide family concept, because of a change in relative priority of conserved, synonymous names. Even if no changes of names were implied, the credibility of the nomenclatural system would not be greatly enhanced by changes in the parameters of publication of over 100 well-known family names for no good reason. Perhaps Brummitt might explain his opposition to the motion that he had expressed earlier on, or else, state that he now was happy with it.

Jeffrey would support the motion if it was understood that it would not prevent the Editorial Committee from making necessary changes, bearing in mind, for example, Reveal & Hoogland's remarks under Donatiaceae. There were several cases in which family names were not validly published in the place listed in App. IIB, and these entries should be corrected.

McNeill pointed out that this was clearly covered; the verb was "authorizes", not "requests". Where it was inappropriate to retain the present entry, the Editorial Committee could make the change.

Stern suggested adding the phrase ", it being understood that this authorization does not preclude the correction of purely bibliographical errors". The motion at present read like the law of the Medes and Persians, which it was impossible to apply. [This was accepted as a friendly amendment].

The motion, thus amended, was carried.

Stuessy Resolution

Stuessy moved the following resolution, to be presented for adoption by the Congress: "The Nomenclature Section of the XV IBC is
pleased to announce that significant progress in stabilization of names, through providing broader avenues for conservation and rejection, has been achieved through formal changes in the Code. These changes will greatly aid in the maintenance of names in current use." It would be beneficial to present a positive image of the Section's achievements. There had been impressive gains that others should be told about. The motion was seconded.

Barrie thought that changing "the maintenance of Names in Current Use" to something more general would be appropriate, as the liberalization of conservation and rejection procedures had an effect on all names, not merely those currently used. Perhaps "enhancing nomenclatural stability", or something to that effect, would be more appropriate.

McNeill pointed out that Stuessy's "names in current use" were written with a lower-case initial letter and were perhaps not meant to be the NCU of the foregoing debates.

Chaloner thought it sounded like saying that motherhood and sliced bread were still good, just like they were at Berlin, but we had sharpened the knife since we last cut the bread. He could not quite see the point of the motion. Its gentle and anodyne nature made it seem as though the Section had just been sitting around for the past four days.

Gams suggested adding a strengthening sentence like: "Taxonomists are encouraged to submit proposals to this effect."

Greuter offered a comment of detail and a comment on principle. The former concerned the lack of acknowledgement of the achievement of establishing a Standing Committee on Names in Current Use. He supposed this was a mere inadvertent slip. [Laughter.] The general point was that this was no resolution. It was not something that could be submitted to a Final Plenary Session of a Congress. Should the Congress tell the Nomenclature Section that it was "a good boy"? There were no conclusions, no recommendation of actions to be taken. The Resolutions Committee would just have to discard it as inappropriate. The intent might be good (especially if the omission was corrected), but this was a non-starter.
Stuessy took the point, but asked how, then, the message he wanted to convey could be appropriately spread so as to reach all interested parties.

Greuter thought that a text of this sort might be a news item in Nature or Science.

Mabberley spoke for the Gams amendment. Exhorting botanists to submit names for conservation and rejection would turn this into a real resolution.

Faegri asked whether the daily Congress Bulletin would not be the right place for publication of such a text.

Stearn observed that it did not matter a damn to the world whether the Section was pleased or not pleased. The resolution should just read "The Section ... announces ..." [This was accepted as a friendly amendment.]

Greuter wondered whether Stuessy had also accepted his small insertion as a friendly amendment. [Laughter.]

Stuessy had not given up the thought that his text could be reworded as a resolution. The impact might be very positive if it was presented in the right fashion. He did not have the wording ready, but it would be something like: "In view of the important international concern for the stabilization of plant names, ... let it be resolved that greater stability has been achieved at this Congress through new conservation and rejection techniques."

Greuter did not think so because "let it be resolved" that something had already been done was also hardly an appropriate statement. The normal mechanism for presenting resolutions to Congress, that would presumably be set out in detail in the first daily Congress Bulletin, required either a minimum number of individual signatures or support by a Symposium or Section meeting. Since the resolution text was not yet ready, Stuessy could still submit it personally together with the necessary number of co-signatories.

Stuessy asked that the motion, if passed, rather than going to the Congress, be disseminated by the Rapporteurs in whatever form they thought appropriate.

The motion was defeated by a card vote (46.0 % in favour, 171 : 201).
Stuessy Motion

Stuessy moved that the Section authorize the set-up of a Special Committee on Infraspecific Categories and Names, to report and perhaps present amendment proposals to the next Congress. The mandate of the Committee would be to investigate inconsistency of use of varietal and subspecific concepts (a taxonomic concern), suggest possible means of reducing or eliminating this inconsistency, and (the nomenclatural aspect) indicate such nomenclatural consequences as might arise from the solutions that were to be recommended. There was a big inconsistency in the application of hierarchical categories under the Code. As pointed out by Hamilton & Reichard (in Taxon 41: 485-498. 1992), botanists world-wide tended to recognize one major infraspecific unit, either subspecies or variety. Only few used both. One major morpho-geographic unit was being recognized routinely, but was named inconsistently. Dealing with this question retroactively would be a bit of a problem, but perhaps greater uniformity might be achieved for the future. Past failures to resolve the matter should not discourage a new effort. The motion was seconded.

Demoulin disapproved of the idea. This was a matter of taxonomy not nomenclature. The Section was there to deal with nomenclature, not to tell people how to classify. In mycology few infraspecific ranks were in use, so he did not have a personal interest in maintaining many ranks, but there were phanerogamists in Europe who used many more different ranks than was customary in the United States. Nomenclature should not be permitted to dictate taxonomy.

Hawksworth would support a Special Committee whose brief was to investigate the nomenclatural implications of utilizing a single infraspecific rank. That would be of interest, especially bearing in mind that the bacteriological and zoological Codes, with varying degrees of success, utilized a single infraspecific rank.

Jeffrey asked if this could not be part of the remit to the Committee on Harmonization between the Codes.

Stuessy enquired about the nature of that Committee. If it were appropriate to refer the matter to it, he might withdraw his motion.
Greuter explained that this Committee was to address discrepancies between the Codes, and differences in the number of recognized infraspecific ranks were part of its field of study. He was not enthusiastic about the proposal, not because there was no problem, but because he could not see how a Special Committee could usefully operate without having a basis from which to start. Not a single proposal relevant to the issue had been placed before the Section, nothing had been discussed that could be referred to such a Committee. It would seem better that those interested get together informally and perhaps come up with some proposals for the next Congress. If defeated, such proposals might then perhaps be referred to a formal Special Committee for study.

Stuessy felt that there was scope for developing a new perspective for dealing with the Code in a more managed fashion. The past need not be an absolute guide for the future. The Code prescribed the number of available infraspecific ranks, but offered the strange option of leaving out one and jump to the one below. He had no ready solution but knew the bewildered look of students who were told that after a hundred years of codified nomenclature there was this gross inconsistency at the infraspecific level. It would be worth considering this in a formalized way and request a Committee report so that something for sure would have to be achieved.

Stearn knew from personal experience with wide-ranging taxa that more than a single infraspecific category was necessary in taxonomy. Because Flora europaea had admitted subspecies as the only infraspecific rank, some authors had had to elevate taxa in rank, despite their inner conviction, and turn them into subspecies in order to get them into the Flora. He objected to this imposition of a taxonomic straightjacket.

Taylor considered that the proposal for a Special Committee of this nature was perhaps a business for the IAPT, but not for the Nomenclature Section. Non-nomenclaturalists were already confused about the Code, thinking it was a botanical textbook on how to "do" taxonomy, which it was not.

The motion was defeated.
Report of the Nominating Committee

Nicolson presented the report of the Nominating Committee on the office of Rapporteur-général and on the membership of the Permanent Nomenclature Committees (see Div. III of the Code) for the period up to the XVI International Botanical Congress, scheduled for 1999. The Secretaries of the Permanent Committees had indicated, at his request, which of their members were willing to continue, and had suggested additional members to fill vacancies. From this information the Nominating Committee had attempted to form full and balanced Committees. Copies of its report had been made available to the members of the Section, unfortunately with two dozen (correctable) orthographical errors reflecting his lack of familiarity with Japanese keyboards; one name (of a 15th member to the Committee for Algae) was to be added. [The full and corrected report appeared in Taxon 42: 923-924. 1993.]

The report of the Nominating Committee was approved.

Report of the Permanent Nomenclatural Committees

Nicolson explained that these would be activity reports presented to the Section, not the usual reports with recommendations on conservation and rejection which went to the General Committee.

Committee for Algae

In the absence of Silva, Greuter noted that, of the 15 members of the Committee for Algae, one had resigned and two, having consistently failed to respond, had been considered to have resigned. Of 50 proposals to conserve family names inherited at Berlin, 21 had been withdrawn, 10 were not recommended, and 19 had been recommended. Of 8 old and 18 new proposals concerning generic or specific names, 22 were recommended and 3 were not. Action on a proposal concerning a species name had been deferred. Two reports of the Committee for Algae had been prepared since the last Congress; the first had just been published (in Taxon 42: 699-710. 1993), the second had been made available at the meeting and was to appear later (in Taxon 43(2), 1994, in press). The General
Committee had already approved the recommendations in both reports.

The report was received by the Section.

Committee for Fungi

Gams reported that over the past six years the Committee for Fungi and Lichens, as it then was called, had mostly comprised 15 members representative of the major groups of fungi and of different parts of the world, although Europe had the strongest representation and Asia the weakest. Only two vacancies had had to be filled at this Congress, and he was pleased to welcome two new Committee members, Kirk and Sipman, as well as the newly appointed chairman, Parmasto. He had been Secretary of the Committee since 1991 when he had succeeded Korf who had had to resign on account of illness. Because of limited attendance of mycologists at International Botanical Congresses, it had been suggested by Hawksworth that the mandate of Committee members be confined to periods between International Mycological Congresses. This was not possible under the provisions of Div. III of the Code, but Committee members present had agreed that the Committee would meet at the next International Mycological Congress in Vancouver, where potential new members might be recruited, the authority of the International Botanical Congresses over Committee appointments notwithstanding. Over the six-year period since the last Congress, three reports on the conservation and rejection of names had been published (in Taxon 37: 450-463. 1988; 41: 99-108. 1992; and 42: 112-118. 1993), and two informal reports had been circulated. The formal reports had occasioned some critical remarks by the General Committee, and the corresponding issues had been returned for renewed consideration by the Committee for Fungi – but the outcome had not substantially changed. Recommendations on proposals in the three published reports and the second informal one (to be published in Taxon 43(2), 1994) had been approved by the General Committee. Committee opinions had been given on several proposals to amend the Code, including a set of provisions on sanctioning made in the context of the NCU proposals. After limited
discussion these had been recommended by a large majority (see Taxon 40: 678-680. 1991), although a clear majority did not yet favour NCU protection. However, members present at the Congress had considered the published lists of names in current use in the Trichocomaceae and Cladoniaceae and recommended that, in the spirit of Rec. 15A and irrespective of the fate of the NCU proposals, authors should respect presently used names as listed there. The report was received by the Section.

Committee for Bryophyta

Zijlstra indicated that the Committee had been active since the last Congress. In addition to the two published reports (in Taxon 39: 289-292. 1990; 42: 683-686. 1993), a third one, recommending a further conservation proposal, had been submitted to and approved by the General Committee at the Congress (see Taxon 43(2), 1994, in press).

The report was received by the Section.

Committee for Fossil Plants

Chaloner tabled the 14-page report of the Committee, meanwhile approved by the General Committee (published in abbreviated form in Taxon 42: 869-872. 1993). Of three conservation proposals one had been recommended and two not. The Committee had lent strong support to the proposal requiring an English or Latin description for the valid publication of new names of taxa of fossil plants. He noted that since Berlin the Committee had worked much more effectively thanks to the use of fax, though not yet e-mail, both of which he perceived as important for the future work of all international nomenclatural Committees.

The report was received by the Section.

Committee for Pteridophyta

In the absence of any representative of the previous Committee, Nicolson noted that the Committee for Pteridophyta had submitted one report which had been duly published (in Taxon 42: 119-120. 1993) and approved by the General Committee. The previous sec-
retary, Lellinger, had stepped down without handing in a formal activity report to the Section, which might hopefully accept this informal statement instead.

The report was received by the Section.

Committee for Spermatophyta

Brummitt had prepared a synopsis of the Committee's work on conservation and rejection of names from 1987 to 1993, which would be submitted for publication (in Taxon 43(2), 1994, in print). The Committee had dealt with 14 cases involving family names, 126 cases of generic names (of the 20 proposals to conserve a generic name against an earlier taxonomic synonym, 19 were recommended and one had been found to be unnecessary; similarly, 8 out of 9 proposals of conservation against an earlier homonym had been recommended), and 57 cases of species names (including 4 proposals, all recommended, of conservation against an earlier synonym; 28 proposals to conserve with a new type, of which 12 were recommended, the others being mostly deemed unnecessary; and 25 proposals to reject a name, of which 10 were recommended). Apart from this summary report, 7 reports had been prepared, of which 4 had been published (in Taxon 37: 444-450. 1988; 38: 299-302. 1989; 39: 293-296. 1990; and 42: 687-697. 1993) and three were forthcoming (in Taxon 42: 873-879. 1993; 43: 113-126. 1994; and 43(2), 1994, in print), all having been approved by the General Committee. The Committee, meeting that week, had considered ways of speeding up the decision process, bearing in mind the principle that conservation or rejection proposals should be published in Taxon to permit comment from anyone around the world. Having discussed whether publication prior to Committee consideration could be dispensed with (the text of proposals being sent directly to the Committee secretaries for circulation and outside comments being invited on the published Committee report ahead of its being approved by the General Committee), the Committee had felt this to be impractical. It had instead preferred the route of speeding up the processing of proposals – which, as Secretary, he would endeavour to achieve.

The report was received by the Section.
At the request of Prud'homme van Reine, it was put on record that no report by that (defunct) Committee had been submitted to the Section.

Editorial Committee

Greuter presented to the Section the "report" of the Committee, which was the "Berlin Code". This had been formally approved by the Section earlier in the week and would now achieve official recognition by the Congress. He drew attention to two asterisked entries of conserved names that had been misspelled in App. IIIA; these were Chascanum, which appeared as "Chacasanum", and Pycnoporus in the Fungi, which read "Pycnophorus". Unless there be protest, the Editorial Committee would feel authorized to correct these in the "Tokyo Code".

The report was received by the Section.

General Committee

Nicolson noted that the General Committee was the Section's "face" between Congresses and so its decisions were subject to the ratification of the Section and of the Congress. One of the major actions had been the setting up of a Special Committee on Names in Current Use, to develop such lists and prepare corresponding proposals, at the request of the IUBS. The Committee had published five reports (in Taxon 37: 440-441. 1988; 38: 662-663. 1989; 40: 461-662. 1991; 42: 110-111 and 431-434. 1993). This very week, it had in addition dealt with 11 Permanent Committee reports involving conservation and rejection proposals (see Taxon 43(2), 1994, in press). At Berlin, the General Committee had shaken off its previous inertia and overturned the Committee for Spermatophyta's negative recommendation on conserving the name of the tomato, so that conservation had been ratified by the Berlin Section. The General Committee had used its prerogatives very sparingly, but a further, recent example was as follows. A proposal to reject Fumaria bulbosa L. having failed, a modified proposal to conserve Corydalis
*solida* (L.) Clairv. against *F. bulbosa* had instead been recommended by the Committee for Spermatophyta; however, since doubt had arisen as to the type of *F. bulbosa* and its belonging to *C. solida*, the General Committee had decided to go back to the original proposal and reject *F. bulbosa*, irrespective of the nature of its type; in so doing it had overruled the Committee for Spermatophyta's recommendation. There was a second example of the same kind, but this was exceptional. More usual was the situation of the General Committee referring a case back to the appropriate Permanent Committee together with suggestions or instructions for reconsideration.

The Report of the General Committee, inclusive of the General Committee's actions on *nomina conservanda et rejicienda proposita*, as published (in Taxon 42: 435-446. 1993) and with the addition of decisions taken in Yokohama (see Taxon 43(2), 1994, in print), was approved by the Section.

**Other business**

**Burdet** read out the text of a motion to ensure ratification by the International Botanical Congress of the decisions taken by the Nomenclature Section: "The Section instructs the Rapporteur-général to present a resolution to the Resolutions Committee of the XV International Botanical Congress, to the effect that the Congress should approve the decisions taken by the Nomenclature Section." This motion was carried.

**Stuessy** considered the returns in the mail ballot to be disappointingly low. This might be due to the fact that the important issues were buried among lots of picayune proposals, which most people simply did not take the time to go through. Would it not be possible to highlight in some way the most critical decisions, say on Names in Current Use? Perhaps even to make a separate ballot on the five or so "hot" proposals? Could this be discussed here, or at least referred to the Rapporteurs in view of the next Congress?

**Greuter** noted that the present Rapporteurs would have hated to have such empowerment or instruction.
Burdet expressed, on behalf of the Section, thanks to the various persons from Tokyo who had assisted so effectively in the operation of the proceedings. [Applause.] The members of the Section had much appreciated the carrying around of the microphones, the collecting of the votes, the tape recording, and the coffee and tea so generously dispensed. He would also like to thank especially the Rapporteurs, the Recorder Z. Iwatsuki, and his assistant Kato, for all the work they had done. Theirs had been a splendid job, especially if one considered the enormous bulk of material that had to be brought under control. He wanted also to thank Nicolson, the Nomenclature Editor of *Taxon*, for preparing the material for publication, as well as all the members of the Section for the excellent way in which they had co-operated with each other and with the Bureau in order to get through the agenda. They all had certainly understood that, if he had at times been a little pushing, this had been justified and had served a good cause. This meeting had greatly benefited, among others, of the presence of two senior members, Faegri and Stearn, who had attended all Nomenclature Sessions since Stockholm in 1950, and in the case of Stearn, even the Cambridge Congress in 1930. It was wonderful that they had been present, and even more wonderful was their invaluable active contribution to the Section's work. [Prolonged applause.]

Stearn asked that the Section express its appreciation to the Editors of *Taxon*, Zimmer and Greuter, who had most accurately prepared matters relating to nomenclature for the print. They deserved special thanks. Those, like himself, who were engaged in editing knew only too well the enormous amount of unappreciated effort that went into producing a scientific journal for the public. [Applause.]

Chaloner asked to grab one last word to thank the Chairman for his very efficient, his calm, and above all very good-humoured handling, which had contributed so much to the meeting having worked in a very pleasant way. [Loud applause.]

Burdet declared the Nomenclature Section’s meetings closed.
Appendix A

List of registered members of the Nomenclature Section

This list is, in the same time, an index to speakers as recorded in the preceding report, with page references to all relevant entries.

Ahti, T., Finland - 18, 24, 48, 55, 63, 114, 136, 143, 162, 170, 193, 194, 200, 202, 211, 235
Akiyama, S., Japan
Anton, A., Argentina
Barrie, F. R., USA - 29, 44, 46, 47, 48, 49, 51, 55, 58, 62, 67, 75, 76, 77, 92, 93, 98, 104, 105, 116, 121, 143, 145, 146, 147, 149, 152, 168, 169, 197, 199, 203, 210, 213, 224, 229, 233, 234, 238, 244
Baum, B. R., Canada
Blackmore, S., UK
Briggs, B. G., Australia - 81, 84, 114, 143, 155, 164, 203
Burdet, H. M., Switzerland - 9, 10, 11, 13, 29, 32, 59, 80, 103, 125, 126, 221, 222, 225, 232, 253, 254
Chappill, J., Australia
Chayamarit, K., Thailand
Clemants, S., USA - 124, 143, 151
Deguchi, H., Japan
Demoulin, V., Belgium - 11, 30, 35, 36, 38, 39, 44, 49, 50, 51, 52, 65, 66, 68, 69, 75, 77, 78, 80, 85, 90, 91, 95, 98, 100, 101, 104, 125, 126, 127, 132, 135, 142, 144, 151, 156, 158, 159, 161, 166, 172, 175, 185, 186, 197, 200, 201, 205, 207, 208, 209, 211, 213, 214, 215, 216, 217, 220, 226, 227, 228, 231, 235, 242, 246
Dorr, L. J., USA - 11, 16, 43, 55, 93, 105, 121, 139, 142, 145, 166, 168, 178, 223, 240, 241
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Ershad, D., Iran
Faegri, K., Norway - 31, 35, 41, 45, 65, 79, 81, 86, 114, 120, 139, 141, 144, 146, 148, 165, 168, 177, 200, 202, 204, 208, 235, 236, 239, 240, 245
Forero, E., USA - 117, 144, 150, 152, 154, 155, 168, 199, 234
Friis, I., Denmark - 18, 45, 55, 91, 92, 97, 131, 133, 135, 136, 159, 229
Funk, V. A., USA - 29, 66, 138, 152, 154, 173, 191, 210, 224, 232, 238
Furuike, H., Japan
Furuki, T., Japan
Gams, W., Netherlands - 17, 126, 163, 218, 224, 236, 244, 249
George, A. S., Australia
Givnish, T. G., USA
Greuter, W., Germany - 12, 13, 17, 22, 25, 26, 29, 30, 31, 32, 33, 34, 35, 36, 37, 38, 39, 40, 43, 44, 45, 46, 47, 50, 52, 53, 55, 56, 57, 58, 59, 60, 61, 62, 63, 65, 66, 67, 68, 69, 73, 77, 79, 81, 82, 83, 84, 86, 87, 90, 92, 93, 94, 95, 96, 99, 100, 103, 105, 122, 125, 126, 127, 128, 129, 131, 132, 134, 135, 137, 138, 139, 146, 148, 151, 152, 153, 154, 155, 156, 158, 159, 164, 165, 166, 167, 168, 170, 171, 172, 173, 176, 178, 179, 181, 182, 184, 185, 186, 189, 192, 193, 194, 195, 198,
Hawksworth, D. L., UK - 24, 29, 30, 32, 33, 36, 37, 46, 60, 64, 66, 79, 84, 92, 96, 98, 105, 111, 118, 133, 143, 148, 150, 156, 158, 160, 166, 167, 177, 185, 192, 193, 208, 217, 223, 225, 226, 228, 229, 232, 235, 236, 246
Henderson, R., Australia - 41, 59, 117, 130, 143, 147, 153, 157, 169, 180, 193, 194, 218
Hiepko, P., Germany
Hirabayashi, H., Japan
Holsinger, K., USA
Hu, Shi-lin, China
Ito, M., Japan
Iwatsuki, K., Japan
Iwatsuki, Z., Japan
Jamzad, Z., Iran
Jarvis, C. E., UK - 26, 51, 63
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Johnson, L. A. S., Australia – 23, 43, 45, 48, 80, 112, 118, 155, 166, 172, 200, 203, 207, 210, 211, 215, 222
Jonsell, B., Sweden – 25, 63
Kato, M., Japan – 11
Kinoshita, L. S., Brazil
Kirk, P. M., UK – 141
Kirkbride, J. H., USA – 59, 229, 234, 236, 239
Kurosawa, S., Japan
Lack, H. W., Germany – 30, 50, 223, 227
Leslie, A. C., UK – 146
Lin, Su-juan, China
Mabberley, D. J., UK – 98, 113, 132, 147, 153, 245
Malhotra, S. S., Canada
Murata, J., Japan
Nagamasu, H., Japan
Ohashi, H., Japan
Orchard, A. E., Australia – 163
Peng, Ching-I, China (Taiwan)
Perry, G., Australia – 42, 43, 44, 45, 49, 60, 68, 84, 102, 113, 179, 180, 196, 197, 198
Pitt, J. I., Australia – 23, 109, 120, 147
Prud’homme van Reine, W., Netherlands van Reine – 35, 36, 37, 44, 58, 59, 80, 85, 95, 131, 132, 159, 166, 178, 212, 228, 252
Riedl, H., Austria
Silva, P. C., USA – 31, 32, 33, 39, 40, 41, 82, 85, 168, 185, 192, 203
Skog, L. E., USA
Smith, G. F., South Africa
Solomon, J., USA
Sriboouma, D., Thailand
Stuessy, T. F., USA – 147, 153, 165, 225, 229, 231, 238, 243, 245, 246, 247, 253
Sugiyama, J., Japan
Takahashi, M., Japan
Tateishi, Y., Japan
Taylor, N. P., UK - 39, 44, 68, 85, 113, 118, 155, 157, 203, 206, 210, 211, 247
Thin, N. N., Vietnam
Thulin, M., Sweden - 47, 74, 76
Trehane, R. P., UK - 37, 94, 146, 175, 212, 224, 226
Ueda, K., Japan
Ueda, Y., Japan (Chiba)
Ueda, Y., Japan (Yokohama)
Utech, F. H., USA
Vorster, P., South Africa
Wagner, W. R., USA
Wang, Xiao-ying, China

Wilson, K. L., Australia
Wyk, A. E. van, South Africa
Wyk, B.-E. van, South Africa
Yadav, S. R., India
Yamaguchi, S., Japan
Yamaguchi, T., Japan
Yi, Yang-zhong, China
Yoshida, T., Japan
Zhuang, Wen-ying, China
Zimmer, B., Germany
Appendix B

Institutional votes

A revised list of institutional votes was drawn up and approved as follows by the Bureau of Nomenclature and the General Committee, in accordance with Division III of the Code. This replaces the list in the Berlin report (Englera 9: 223-228. 1989). Institutes are briefly identified by the name of the city in which each is located, followed in general by the herbarium symbol from the *Index herbariorum, Herbaria*, ed. 8 (Regnum Veg. 120. 1990). An asterisk (*) indicates that the institution was represented at the Nomenclature Section in Tokyo. The number of votes allotted to each institute is given in the right-hand column.

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